Learn to Earn: The Long-Term Effects of Learning Gains in Primary Schools*

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Abstract

Tracking students over 15 years, we provide the first causal evidence linking learning gains in primary school to adult outcomes using two distinct identification strategies. Individual exposure to a school with higher test-score value-added increases educational attainment and earnings 15 years later. A randomized information intervention that led to learning gains at the village level generated similar effects. The lifetime earnings gains across all students attending a one-standard-deviation better school total US$175,000 and, for the information intervention, the marginal value of public funds is ∞ for men. These results establish the economic case for investing in foundational learning.

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1 Introduction

For over fifty years, at least since Mincer (1974), economists have relied on estimates of the returns to an additional year of schooling to argue for increased investment in expanding access to education. Now, with substantial increases in schooling attainment in low-income countries combined with abundant evidence that many children complete primary school without acquiring basic literacy and numeracy skills, the focus has shifted from enrollment to learning (World Bank, 2018). At the same time, it has become evident that children are no longer making a simple decision of whether or not to enroll in school. Rather, many children are participating in what has been termed "human capital markets" with a range of schooling options to choose from (Das and Dobbin, 2026). This shift in education policy, from access and enrollment to learning, and from single-school environments to human capital markets with considerable school choice, motivates this paper's three core research questions. First, do foundational learning gains, measured as test scores in primary school, persist into adulthood? Second, do these gains lead to meaningful changes in educational attainment? Third, do these gains translate into labor market outcomes as adults?

To answer these questions, we followed 5,865 children over 15 years from 2003 to 2018 across 112 villages in Pakistan, as part of the Learning and Educational Achievement in Pakistan Schools (LEAPS) study. We collected data through multiple surveys that included information on cognitive and socioemotional skills, educational attainment, and labor market outcomes. We now pair these data with two sources of variation in test scores during primary school to assess the long-term impacts of gains in foundational learning.

Our first source of variation leverages the expansion of human capital markets by examining differences in the quality of the school a child attends. We *define and construct* quality, or "School Value-Added" (SVA), in terms of learning, as the causal gains in test scores a child would obtain from attending a particular primary school. We then use variation in the SVA that children experience due to plausibly exogenous school switches. By construction, exposure to a higher VA school leads to greater learning gains during the primary schooling years; the question is whether these gains persist in later educational and labor market outcomes. Our second source of variation comes from a village-level randomized intervention implemented in 2003 that raised test scores for children in primary schools in treated villages (Andrabi et al., 2017). As with exposure to a higher VA school, we assess whether these improvements in foundational learning then translate into longer-run educational and labor market outcomes.

We begin by examining the long-term impacts of attending a high VA school. We have shown previously that our computational methods yield SVA measures that are forecast

unbiased (Andrabi et al., 2025). However, the unbiasedness of the SVA measures does not address potential sorting into schools, if, as is plausible, richer children select into higher VA schools. This sorting implies that regressing long-run outcomes on SVA will likely lead to biased estimates.

Our identification strategy instead relies on a conditional exogeneity argument, similar to the literature that identifies place effects on long-term outcomes using data on movers. We compare students who were first observed in the same school and then exploit variation in the average SVA experienced by these students due to school switches. In our strategy, the "first school fixed effect" acts as a conditioning variable and the validity of our approach relies on the assumption that once the first school is conditioned upon, subsequent switching is conditionally unrelated to (levels and trends) in omitted variables such as ability or socioeconomic status.

While school switching need not be exogenous, in our context we show that the data are indeed consistent with the identifying assumptions. This is because the vast majority of switching is driven by parents moving siblings to keep them in nearby schools.[1] Once parents have chosen an initial school, subsequent moves seem to be uncorrelated with variables that directly impact later life outcomes: test score and household wealth levels and trends are similar for switchers and stayers and we do not find evidence of differential attrition from the sample based on the VA of the school that students switch into.

Having established the plausibility of our identification strategy, we first establish long-term impacts on educational outcomes. The learning gains from attending a high VA school persist into adulthood. Attending a primary school with 1 sd higher average VA increases cognitive skills by 0.16–0.19 sd and functional literacy and numeracy or 'life skills' by 0.25 to 0.28 sd after 15 years. A notable result that helps assess potential mechanisms for these long-run effects is that attending a higher VA school has little discernible impact on socioemotional skills.

In addition to raising test scores in the long run, attending a higher VA school also increases educational attainment. Students attending a 1 sd higher VA school complete 1 more year of schooling, are 14 percentage points (pp) or 33% more likely to complete 10th grade, 10 pp or 52% more likely to receive a high school diploma, and 6 pp or 47% more

[1]This accords with several features of the environment. First, parents have multiple school-age children (the average child has 3 siblings, in line with the total fertility rate in Pakistan at this time), which makes sending children to far apart schools logistically challenging. Second, 50% of the switches we observe are between nominally single-sex public schools, in line with parents keeping children together or near one another even if it is technically not allowed. Third, switching does not result in any *average* change in school value-added, inconsistent with the idea that it is driven by the search for better quality schools. Finally, the plurality of parents (43%) report choosing a school based on distance, in line with their strong preference to keep commutes across multiple children who may be enrolled in different schools manageable.

likely to attend college.

We then examine the longer-run impacts of school quality on employment and earnings. Examining earnings effects in our sample is complicated by the fact that 13% of respondents are still studying (their ages range from 20–30), and enrollment is causally higher (45%) among those who attended higher VA schools. In addition, the life cycle of employment is very different for men and women. Trends in the male life cycle resemble those in high-income settings, with men increasingly switching from schooling to work as they age; 89% are working by age 28. However, the labor force participation rate for women in Pakistan is extremely low: 7% of the women in our sample engage in any kind of paid work, and after marriage that number drops to 2%. As a result, education does not crowd out employment for women in their early twenties, and employment peaks when women are in their early-to-mid twenties before collapsing later on. To account for these important gendered differences in the life cycle, we separately estimate employment and earnings effects for men and women by age, estimating earnings effects in levels to include the non-employed.

When we include all individuals in our specifications (with zero earnings for those who are not in the labor force), we find that positive and significant earnings effects emerge for men older than 26 (+30%) that grow thereafter.[2] For women, there is suggestive evidence of positive earnings and employment effects in their early-to-mid twenties, when they are most likely to be working.

Next, we follow the literature and restrict attention to individuals with positive earnings. In this subsample, attending a 1 standard deviation higher VA school increases earnings by 22 log points for the entire sample (including women), and by 25 log points among men.[3] As the earnings gains from attending higher VA schools are likely to be highest among those who ultimately attain the most education (38% of men who are not working are studying) and appear to accumulate with age, we likely *underestimate* the benefits of foundational learning for earnings over the life cycle. Interestingly, the gains from attending a better school are 5–9 times as large as the Mincerian returns to 1 additional year of schooling for these students, suggesting that the benefits of increased foundational learning are not entirely driven by greater educational attainment. We return to this point in our discussion of the benefits and costs of increasing foundational learning.

A battery of additional robustness tests assesses the validity of the identification strategy, focusing on the key outcome of educational attainment.[4] Our estimates are robust to includ-

[2] Chetty et al. (2014a) observe a similar pattern when measuring the effects of higher quality teachers on their former students, with effects only becoming positive and significant for men 24 and older.

[3] There are too few women with positive earnings to credibly estimate effects on log earnings for women separately.

[4] The robustness tests focus on the educational outcomes because our first school fixed-effect strategy is

ing rich controls for socioeconomic status, focusing on switches to worse or better schools, focusing on the most plausibly exogenous switches (those concurrent with a sibling switch due to the sibling starting or exiting schooling), and using an entirely different instrumental-variables strategy exploiting variation in the quality of nearby schools. They are also robust to a complementary "movers design" motivated by the approach in Chetty and Hendren (2018), where we restrict our sample to switchers and compare those who spend more versus less time in a higher VA school.

Our *second* identification approach complements the observational results by evaluating whether a randomized information intervention that raised test scores in these villages produced comparable long-term impacts. Beyond serving as an experimental counterpart to the observational evidence, the information treatment speaks to two further concerns. First, the experiment was implemented at the village level and boosted test scores for *the entire cohort in the village*. As a result, the estimated effects incorporate potential crowd-out at the village level, particularly for employment outcomes, and therefore better reflect what happens when learning improves at scale. Second, the intervention more directly isolates the impact of learning improvements, mitigating concerns that the SVA estimates may partly reflect signaling from attending a "better school."

Our previous research in Andrabi et al. (2017) showed that this information intervention raised test scores by 0.11 sd in treated villages, at a direct cost of ∼3 USD per student.[5] We now focus on children who were of primary school age (7–11) when the intervention was implemented in 2003 and who lived in treatment villages, and were thus exposed to the treatment. We assess long-term impacts by comparing these students with the same age cohorts in control villages.

Our education and earnings results are strikingly similar to the impacts of attending a higher VA school. By 2011, children's mean test scores in treated villages were 0.15 sd higher, and, by 2018, they remained higher at 0.09 sd. By 2018, treated children had also completed 0.54 more years of schooling and, consistent with these results, were 4.3 percentage points, or 42% more likely to still be enrolled. Earnings effects again emerge for older individuals who are more likely to have completed their education. For this older cohort, individuals from treatment villages earn 11 USD or 15% more per month. These results, especially for educational attainment and labor market outcomes, are quantitatively consistent with what we would have predicted given the estimates of the long-term impacts of going to a better

data-hungry. For education, we can use the full sample of men and women, but low labor force participation among women in Pakistan (and in our sample) implies that the sample available for earnings results is much smaller, resulting in robustness tests that are inadequately powered.

[5]The effect size is close to the median effect of educational interventions in low-income countries reported by Evans and Yuan (2022) in a systematic review.

school.

The final part of the paper addresses several additional questions and presents the benefit-cost analysis of improving foundational learning. One important question is whether the long-term benefits of foundational learning accrue primarily to the rich. We assess heterogeneity by socioeconomic status (SES) and find that children from both high *and* low SES backgrounds benefit from gains in foundational learning. A second question is whether the long-run impacts on student outcomes from attending a higher VA school are driven by gains in foundational learning itself, rather than by increases in socioemotional skills or other school characteristics correlated with SVA. We assess several alternatives and rule them out. Taken together, these findings strongly indicate that strengthening foundational learning was the key mechanism behind the long-term gains that we observe.

Our benefit-cost analysis relies on six rounds of data collection over 15 years that allow us to measure the costs of the information intervention fairly precisely. Focusing only on increased earnings, we show that the marginal value of public funds (MVPF) for this intervention is ∞ for men, with a benefit-cost ratio of 2.69–3.45 and an internal rate of return of 12.09–13.44 percent. For the full sample, the estimates are lower because we assume that women who work do so only for five years, but they remain substantial. Extrapolating from the randomized controlled trial, we scale the full-sample lifetime earnings benefit from the information intervention ($955.61 per student) by the ratio of the test score effects of attending a 1 sd higher VA school and the information intervention (0.19/0.11). This implies a lifetime earnings gain of $1,650 per student. The average public primary school in our sample has 106 children. Therefore, improving the quality of a school by 1 sd leads to total lifetime earnings gains of $175,000 for every cohort of primary school children. [6]

Studies of highly successful programs in low-income countries report benefit-cost ratios ranging from 1.5 to 5, placing improvements in foundational learning squarely among these highly successful programs. The gains to investing in foundational learning may be even more attractive because few studies have moved beyond program costs to study the full social costs of the program. In our case, including only program costs would lead to benefit-cost ratios that exceed 100, making the information intervention an extremely effective *development* intervention. Certainly, investing in foundational learning compares highly favorably to conditional cash transfers, programs focused on the ultra-poor that provide training and assets, and active labor-market programs.[7]

[6] Because the ratio between the increase in earnings and increase in test scores is almost identical in our two identification strategies, using either strategy to calculate the returns to attending a higher VA school delivers similar results.

[7] Banerjee et al. (2021) study the long-term effects of a graduation program in India to recover a benefit-cost ratio of 3.8; Barham (2011) recovers a ratio between 1.3 and 3.6 for a 17% decline in child mortality

These long-term estimates contribute to the literature in two ways. Research on the long-run impacts of educational and human capital-oriented interventions in low-income countries is rare, with notable exceptions. One set of studies looks at long-term impacts of expanding secondary schooling. Araujo and Macours (2021) show that the Progresa cash transfer program in Mexico positively affected educational attainment and labor income expectations in the long term, while Millán et al. (2020) measure the long-run effects of a conditional cash transfer program in Nicaragua. Duflo et al. (2024) and Duflo et al. (2023) estimate the long-run effects of secondary school subsidies. A second set, closer to our focus, examines the longer-run educational (but not labor-market) effects of interventions that increase learning. For instance, Buhl-Wiggers et al. (2023) estimate the impacts of a literacy program in Uganda 8 years later, focusing on cognitive skills and grade progression, and Allende et al. (2026) show that an information intervention in Chile increased test scores several years later.

The causal estimates we present on how foundational learning shapes completed schooling paths and later labor market outcomes are the first in the literature. They provide a crucial benchmark for how much low-income countries should invest to boost primary school test scores.[8] In the absence of similar evidence, it has been challenging to respond to concerns that foundational learning gains may fade out (Bailey et al., 2017); that returns could be too small given the long lag before children reach the labor market; that credit and labor market frictions may prevent high-ability children from accessing higher education and good jobs (Duflo et al., 2023); or that the typical intervention effect sizes are too modest to yield meaningful long-run improvements. Our findings help alleviate each of these concerns.

Our second contribution is to a literature on school quality and longer-term outcomes. This literature has traditionally focused on *types* of school, such as charters, private schools, or selective elite schools. The identification challenges are similar to those we address when examining the impact of attending a high VA school: researchers need data that link school type to long-run outcomes *and* a source of quasi-experimental variation in the schools students attend. Because of these challenges, much of this literature focuses on a small number of oversubscribed charter schools, where lotteries are available to provide identification (Dobbie and Fryer Jr, 2015; Angrist et al., 2016; Clark et al., 2015), or reports associations rather than causal estimates of the link between school type and longer-run outcomes (Jackson et

from Mexico's Progresa program and Alfonsi et al. (2020) report a ratio between 0.6 and 2.4 for a worker training program in Uganda.

[8] At present, low-income countries devote limited resources to education. Bharti et al. (2026) note that, "*Average public education expenditure per school-age individual (0-to-24-year-old) varies significantly across world regions, from 220€ in Sub-Saharan Africa to 9,025€ in North America/Oceania (PPP € 2025), corresponding to a gap of almost 1 to 50.*"

al., 2020). In low-income countries, Lucas and Mbiti (2014) are among the first to study the impacts of attending selective secondary schools in Kenya on test scores and progression in secondary schools, reporting null effects for both.

Our interest here, motivated by the emergence of human capital markets, lies less in school types and more in the quality of the school defined by its ability to raise test scores. Our approach is deeply influenced by a recent literature documenting substantial heterogeneity in test-score-based measures of value-added *within* every school type. For instance, Hoxby and Murarka (2009) and Angrist et al. (2013) show that school quality varies meaningfully within charter schools in the U.S., with important consequences for short- to medium-run learning. Our previous work in the same locations (Andrabi et al., 2025) documents that this is true even in the (small) Pakistani villages in our sample with an average of 678 households. Our prior research further shows that schools strategically choose their value-added, responding to exogenous shocks in school entry and competitor quality, and that households demand schools with higher VA (Bau, 2022; Andrabi et al., 2024, 2025). The extent to which selecting higher-VA schools translates into longer-run outcomes is now informative about the efficiency of human capital markets, especially in light of a set of null findings on the long-term effects of attending selective secondary schools from the U.S. and from Kenya.

The paper that comes closest to our work here is Beuermann et al. (2023), who exploit exogenous variation to estimate school value-added separately for test scores, arrests, dropout, teen motherhood, and formal labor market participation. They show that the different value-added measures are modestly correlated in the context of Trinidad and Tobago. More broadly, Chetty et al. (2014b) measure the effects of teacher quality on students' long-run outcomes. Our paper contributes to the literature by documenting effects on cognitive skills more than 10 years after students initially attended a school and estimating the effects of attending a high VA school using the full distribution of schools in a representative sample of villages with at least one private school (60% of the rural population in the province lives in such villages), with experimental estimates that are very similar to those obtained in the observational data.

The paper proceeds as follows. In Section 2, we describe the context and data. Then, Section 3 describes the switchers empirical strategy, and Section 4 reports the results from this strategy for learning, education, and earnings. Section 5 estimates the long-term impacts of the information intervention using the randomized controlled trial. An extended discussion in Section 6 addresses potential concerns and discusses cost-effectiveness, while Section 7 concludes.

2 Context and Data

2.1 Context

Economy and Labor Market Our study takes place in rural Punjab, Pakistan's largest province, with a population of 128 million in 2023. Over the period of our study, from 2003 to 2018, growth fluctuated greatly, with real GDP growth averaging 4.1% from 2000–2020, similar to the regional average for Sub-Saharan Africa but lower than India's 6% (World Bank, 2025). In the rural labor market, employment remained concentrated in agriculture and services, with most jobs in the informal sector. Within the formal sector, there is a substantial wage premium for public sector jobs, and the World Bank estimates that the public sector accounts for as much as 60% of formal wage employment (World Bank, 2023). Importantly, migration to the Gulf increased sharply over the period of our study, and migrants have become an important source of remittances for the economy.

Education Our study focuses on children who were enrolled in primary school between 2003–07. Already by this time, similar to other low-income countries, children attended schools in areas with substantial school choice: In 2003, the average village in our sample had 680 households and 7.4 public and private schools (Andrabi et al., 2025; Das and Dobbin, 2026). Pertinent to our estimation strategy, public schools were officially single-sex, but 55% reported having children of the opposite sex enrolled at some point, consistent with schools and parents using the rules flexibly to reduce commute times and/or keep primary-school-age siblings in the same school. Private schools in our sample are co-educational, primarily for-profit and non-religious (Andrabi et al., 2006, 2008). In Andrabi et al. (2025), we document substantial heterogeneity in primary school VA within and across sectors, even within the same village. Moving a student from the lowest to the highest VA school in her village would increase her *annual* test score gains by 0.43 test score sd, roughly equivalent to average annual test score gains.

The period we study was transformative in terms of educational attainment. We see this most clearly when comparing generational shifts in educational attainment. Among the children in our sample, only 24% have mothers with *any schooling* at all; 56% report fathers with (at most) some primary schooling and 6% have at least one parent who has studied beyond primary school. In contrast, the average child in our sample has more than 8 years of education, and 11% of men and 15% of women have attended some kind of (typically local) college. Across a generation, individuals have gone from almost no schooling to accessing college at sizable rates.

Despite these substantial gains in educational attainment, learning outcomes remain

poor, although with considerable variation. In Grade 3, most children in our sample could match simple words in English to pictures (such as "book"), add and subtract two-digit numbers, and recognize individual letters and combinations in Urdu. But they could not construct simple sentences in English (such as "I play" or "The water is deep"), multiply or divide, or read an Urdu passage. Despite the low averages, learning trajectories show gains of 15–30 percentage points for repeatedly tested items, but with tremendous variability across children (Bau et al., 2021). Children in the lowest decile of gains *lost* 0.49 sd, the second decile gained 0.39 sd, and the top decile gained 2.77 sd. It is this combination of poor average test scores with high variability in gains that motivates our focus on the long-term impacts of better foundational learning.

Gender, Education & Labor Force Participation Though Pakistan ranked 161st out of 190 countries in the United Nations' Gender Inequality Index (United Nations Development Programme, 2022), education and labor force outcomes paint very different pictures. Conditional on being in our sample, gender gaps in educational attainment are small: women and men complete an average of 8.3 and 8.0 years of education, respectively.[9] However, men and women have vastly divergent outcomes in the labor force. Reflecting very low rates of female labor force participation in South Asia, only 7% of women in our sample report working compared to 76% for men. This understates the true gender employment gap once schooling is completed since 11% of men are still enrolled in school at the time we observe them. Once men reach the age of 25, and only a negligible fraction are still in school, their employment rate converges to ∼90%. In contrast, for women over the age of 25, employment rates remain below 10%. See Appendix Figure A1 for more information on how enrollment and labor force participation progress with age for men versus women. Low female labor force participation rates may reflect the lack of socially acceptable jobs for women (Hyder, 2026). Indeed, a single socially acceptable occupation – teaching – accounts for 65% of employed women in our data.

2.2 Data

The LEAPS study collected a longitudinal dataset on households and schools from 112 villages in rural Pakistan. Villages were chosen randomly from 3 districts of Punjab (Attock, Faisalabad, and Rahim Yar Khan). Because one of the study's goals was to study the rise of private schooling, a village had to have a private school at baseline to be included in the

[9]Since our sample requires having enrolled in school, individuals who never attended primary school will be omitted.

sample. Hence, these villages are wealthier than average but representative of the villages in which approximately 60% of the population lives (Andrabi et al., n.d.).

Appendix Figure A2 presents a timeline of data collection. From 2003–07, all schools in the villages were surveyed, as were a random subsample of 1,807 households. In 2011, five years after the fourth round of data collection, we again surveyed households and all existing primary schools. In 2018, we tracked and surveyed individuals who were children in 2003 from the 1,807 original LEAPS households. This section describes the school and household data collection exercises, focusing on the variables used in this paper.

2.2.1 School Surveys (2003–07)

We use three types of data collected at schools: test score data from students, socioeconomic information from students, and school characteristics from owners/head teachers.

Test Scores In 2003–04, we tested a first cohort of grade 3 children and followed and tested them for four consecutive years. We included a second cohort of grade 3 students in 2005–06, who were tested again in 2006–07. Students were tested in school by our enumerators in math, Urdu (the vernacular), and English. Tests were independent, low-stakes, and designed and validated by the principal investigators (Andrabi et al., 2002). Test scores were constructed using Item Response Theory and vertically linked across five rounds of data (these four years of data and the 2011 round) using a set of common items included in each year. This results in a distribution of test scores across all five years with a mean of 0 and a standard deviation of ∼1. We use the test score data from the period between 2003 and 2007 to construct empirical Bayes school value-added (SVA) estimates.[10]

School & Student Characteristics We surveyed all primary schools in the sample villages and collected socioeconomic information on a random sample of 10 students enrolled in each school in the first year. In subsequent years, we collected information from these children and a refreshed random sample. During the school data collection, enumerators also collected information about the school type (mostly public or private, although 2.2% of schools were nonprofits or religious schools/madrassas), GPS coordinates, and grades offered by the schools.

[10]SVA information is missing for 64 schools. This occurs both (1) because of a small number of school-level refusals (7 cases), and (2) because, mechanically, SVA cannot be calculated if a school existed in 2003 but exited the village in round 2. In the latter case, this is because students are never observed in the school with lagged test scores, a key control in SVA estimation.

2.2.2 Household Surveys

In 2003, we randomly selected 1,807 households (∼16 per village) such that three-quarters of the households in each village had a child enrolled in grade 3, and one-quarter had an unenrolled child eligible for grade 3. This is consistent with the share of eligible children enrolled in grade 3 at baseline. All resident household members were surveyed annually until 2007 and again in 2011. In 2018–19, household members who were aged 5–15 in 2003 were tracked for a long-term follow-up survey with the aim of surveying them regardless of their current location. While this survey is a panel at the child rather than the household level, for ease of exposition, we refer to it as a "Household Survey" since the initial sample was identified in the earlier household surveys.

Initial Household Panel (2003–07, 2011) All six waves of household surveys collected information about socioeconomic status, educational attainment and expenditures, and importantly, between 2003 and 2007, the school and grade attended by all children aged 5–15. Thus, we observe which primary schools children attended and can match this information to the SVA estimates from the test score data. The household surveys are also used to construct control variables: assets (measured in the baseline round of 2003), the caste of the household head, and the education of their parents.[11] In the 2011–12 household survey, we also tested children who were still in the household at home.

Long-term Follow-up Data (2018–19) We identified and attempted to track 5,865 potential respondents who were 5–15 in the baseline household survey (2003) over two years (2016–2018), regardless of where they were located.[12] When possible, we surveyed them in person and administered cognitive and socioemotional tests (75%). When this was not possible, for example because they had left the village and did not return, we surveyed them over the phone or completed a shorter survey based on information from knowledgeable family members. Four villages were excluded because they fell in a high-security zone, and we did not receive permission to survey in the zone. Ultimately, in 2018–19, we surveyed 4,956 individuals for an attrition rate of 15.5 percent, or 1% per year. Appendix Figure A3 shows the age distribution for the final sample.[13]

[11] The asset index is the first component of a principal components analysis of 25 indicator variables for households' asset ownership, normalized for the household size. We follow Karachiwalla (2019) to determine whether the household head is from a high or low caste.

[12] In cases where the person had left the country, the team waited until they returned, either for a religious holiday such as Eid or for a longer spell, and then surveyed them at home. See Danon et al. (2024) for details of the tracking and long-term survey.

[13] We remove a small number of observations where repeated age observations over time suggest the individual was not actually 5–15 in 2003.

All surveys collected information on individuals' educational attainment, earnings, and labor market outcomes. In addition, the in-person surveys tested respondents' cognitive skills, socioemotional skills, and functional literacy and numeracy. Our main outcomes of interest are learning, years of education, employment, and earnings, but we also observe whether children completed important educational milestones, such as middle school, the high-stakes matriculation exam at the end of 10th grade, and a high school diploma at the end of 12th grade.

We measure cognitive skills in math, Urdu, and English using the same type of tests as in earlier rounds, with scores constructed using Item Response Theory, as in Das and Zajonc (2010), and averaged to create a single measure of cognitive skills. Our measure of socioemotional skills is the first component of a principal component analysis (PCA) of multiple measures related to the Big Five personality traits, grit, risk-aversion, and impulse control.[14] We measure functional literacy and numeracy with questions related to everyday tasks such as reading an electricity bill and understanding text messages (this was prior to the arrival of voice notes). The inclusion of these measures is inspired by functional literacy tests, such as the OECD's survey of adult skills (OECD, 2026), and by Banerjee et al. (2025), who show that children who can solve complex real-life math problems are not always able to solve similar or simpler problems in the classroom (and vice-versa). We combine these questions and again use the first component of a PCA.

2.2.3 Summary Statistics

Our sample does not include the never-enrolled and is therefore somewhat positively selected relative to the full population. Appendix Table A1 reports summary statistics for key variables for the final sample used to estimate the effects of SVA, separated by gender. The data highlight that public sector employment is low in this population (1% for women and 6.5% for men, unconditional on working) and among men, most work is informal (65% for all men or 85% conditional on working). Despite our rural sample from Pakistan's agricultural belt, only 7% of the respondents work in agriculture. Average monthly earnings are \$6 for women (\$67 conditional on working) and \$113 for men (\$148 conditional on working). Danon et al. (2024) provide more descriptive detail on the LEAPS population, highlighting

[14] Danon et al. (2024) validate both the cognitive and socioemotional skills components of the survey. They show that there are no ceiling effects in the cognitive tests. The socioemotional skills modules were extensively piloted over one year. The final modules use The Big Five and Grit as self-reported measures, similar to Laajaj and Macours (2021). The risk-aversion measure is derived from the Balloon Analogue Risk Task (BART), a task in which respondents earn money from pumping balloons, unless the balloons explode. Finally, to measure impulse control, the respondents had to complete a task on tablets in which they had to touch the screen when it changed color (i.e., the "GoNoGo" task).

the tremendous gains in education across generations, high rates of temporary and permanent migration, and the fact that employment for men is overwhelmingly informal.

3 Empirical Strategy for Long-Term Effects of Higher SVA

This section details how we estimate test score SVA, as well as our switchers' strategy for estimating the effects of SVA. Appendix B describes an additional IV lasso strategy using distance instruments that delivers qualitatively similar results.

3.1 SVA Estimation

To measure SVA for math, Urdu, and English, we use the test score panel data from 2003–07 to estimate

$$y_{igst} = \lambda_g y_{igs,t-1} + \theta_s + \alpha_g^G + \alpha_t^T + \epsilon_{igst}, \tag{1}$$

where i denotes a student, g a grade, s a school, and t a year. The outcome variable y_{igst} is student i's subject test score in year t, λ_g is a grade-specific coefficient that captures the effect of lagged performance, and α_g^G and α_t^T are grade and year fixed effects. Our estimate of a school's subject-specific SVA is the estimate of θ_s, the school fixed effect, for a given subject. Our main school-specific measure of quality is a school's mean SVA across subjects. To avoid any mechanical correlation between estimated SVA and students' long-run outcomes, we exclude all students in the 2018 household survey when we estimate SVA.[15] SVA estimates are noisy, which leads to attenuation bias if SVA is an explanatory variable in a regression. Thus, we construct empirical Bayes estimates of the SVAs, as is common in the teacher value-added literature, following the procedure described in Appendix B of Andrabi et al. (2025), and use these estimates when SVA is the explanatory variable.

Our procedure for computing SVA, including Equation (1), is identical to the procedure in Andrabi et al. (2025) and similar to specifications used by Angrist et al. (2017) to estimate SVA and Chetty et al. (2014b) and Bau and Das (2020) to estimate teacher value-added. Intuitively, a school s's estimated SVA is the predicted mean annual test score gain for a student assigned to that school. It is unbiased as long as the controls are rich enough to account for the sorting of children into schools. We validate this assumption in Andrabi et

[15] This only excludes 1,147/31,361 tested students since relatively few children in the household survey are in the same grades that we tested in the specific years they were tested.

al. (2025), where we demonstrate that the SVA estimates are forecast unbiased and pass an overidentification test for unbiasedness proposed by Angrist et al. (2017).

3.2 Switcher Identification Strategy

3.2.1 Empirical Strategy

Even though our estimates of SVA are unbiased, students are still likely to sort into schools based on SES or ability. To account for this type of sorting, we exploit variation in the average VA of the primary schools a student attends due to school switches to estimate the causal effect of attending a high VA school on long-run outcomes. While switching need not be exogenous, conditional on the first school attended, switching behavior seems to be motivated by forces orthogonal to learning and school quality in this setting.

Appendix Table A2 provides more details on switches. The first important observation is that, on average, switches generate virtually no change in the VA of the school the child attends. Rather, households have multiple children, and parents appear to prioritize keeping children's schools close together even if they do not attend the same school. We observe 727 switches, and 82% of these occur at the same time as a sibling switching schools, starting school, or completing school. This high rate of concurrent moves is not because sibling switching is very frequent; a student who does not switch herself only experiences a sibling event in the same year 42% of the time. Nor is this mainly because parents are dissatisfied with a school and switch siblings to and from the same schools (only 34% of sibling-involved switches). Instead, in 57% of sibling-involved switches, a sibling is starting kindergarten, grade 1, moving to secondary school, or graduating secondary school.[16] Another 10% of switches are due to private school exit and entry, which is common during this period. Appendix Figure A4 further shows that switches generate substantial variation in SVA–at the 5th percentile of this distribution, we see children switch to schools with 0.4 test score standard deviations lower learning gains (-2.1 sd in the SVA distribution) and at the 95th percentile, children are switching to schools with 0.38 sd greater learning gains (+2 sd in the SVA distribution).

In order to exploit the variation in SVA from switching, we estimate

$$y_{iT} = \gamma_{s_i^0} + \beta \frac{1}{N_i^Y} \sum_{t \in [2003,2007]} \widehat{SVA}_{st} + \Lambda \mathbf{X_i} + \upsilon_{iT}, \tag{2}$$

[16]The fact that only 34% of sibling-involved switches feature siblings switching from and to the exact same school may appear puzzling. However, recall the public sector is nominally sex-segregated, so parents may enroll siblings in nearby schools even if they attend different schools or have one child briefly attend a cross-sex school; 33% of switches involve switching into or out of a cross-sex public school.

where T denotes the long-run round of interest (2011 or 2018), s_i^0 denotes the first (pre-switch) school a child attends such that $\gamma_{s_i^0}$ is a first school fixed effect, and N_i^Y is the number of years the child is observed, such that $\frac{1}{N_i^Y}\sum_{t\in[2003,2007]}\widehat{SVA}$ is the average of the empirical Bayes SVA a child experiences during primary school. The vector of controls $\mathbf{X_i}$ always includes age and gender fixed effects and, in some specifications, indicator variables for whether the mother and father have some education, high caste, and the index of household assets calculated at baseline. Standard errors are clustered at the household level.

Our key estimate is β, the effect of attending a higher VA primary school on long-run outcomes. Our identification relies on the assumption that controlling for the first school a child attends fully accounts for the sorting of children across schools due to differences in SES or ability. We assume that, conditional on this control, students do not systematically switch to higher or lower VA schools in a way that is correlated with their pre-switch learning trajectories or other characteristics that affect long-term outcomes.

3.2.2 Evaluating Assumptions

Our identifying assumptions could be violated for several reasons. First, students with higher performance levels, conditional on first school attended, could switch into higher VA schools. Second, students who are improving faster (conditional on first school attended) could switch into higher VA schools. Finally, students who switch into higher VA schools could be differentially likely to attrit from the sample. We test for each of these below.

We first test if switchers differ from non-switchers in levels. In Appendix Table A3, we regress student characteristics on an indicator variable for switching, controlling for first school fixed effects. There is no directionally systematic or statistically significant relationship between switching and socioeconomic status or academic achievement. We then test if, among switchers, different types of students switch into different types of schools. In Appendix Table A4, we regress new school VA on student characteristics, conditioning on first school fixed effects. With the exception of an indicator variable for father living elsewhere (7.4% of the sample), no student characteristics predict new school VA.[17] F-tests for whether the covariates jointly predict the new school's VA cannot reject a null relationship.

Test Score Trends Even if students' time-invariant characteristics do not predict switches, students who are on different trajectories or who experience time-varying academic or financial shocks may switch into different schools. This could occur if parents observe that a child is performing well and decide to invest more in her education (e.g., by switching her to a

[17]Since most children live with their mothers, mother education is missing in far fewer cases than father education (0.5%). As a result, we do not separately break out the different reasons for missingness.

private school) or if economic stress leads a child to switch out of private school to a public school. To evaluate whether this is the case, we examine the relationship between test score and asset trajectories and changes in school quality among switchers.

Since we do not have test scores for most of the individuals in the 2018 follow-up, we use data from the panel of tested children between 2003–07 to measure the relationship between test scores and changes in SVA due to switching. Using switchers, we estimate

$$y_{icst} = \sum_{k \in \{-3,..,2\}} \tau_k event^k_{ist} \Delta \widehat{SVA}_{st} + \phi_{s^0_i ct} + \Gamma \mathbf{X_{icst}} + \epsilon_{igst}, \tag{3}$$

where y_{igst} is a student i's mean test scores in year t, k denotes event time (the years around a school switch), and c denotes the student's cohort (3rd grade in year 1 or 3). The indicator variable $event^k_{ist}$ is equal to 1 if a student is observed k years after an event, and $\Delta \widehat{SVA}_{st}$ is the change in the estimated SVA at the time of the switch.[18] The fixed effect $\phi_{s^0_i ct}$ is at the first school-cohort-year level, and the vector $\mathbf{X_{icst}}$ includes age and gender fixed effects. Controlling for $\phi_{s^0_i ct}$ is necessary because knowledge is a stock: a student's score in year t reflects the cumulative effects of the school attended up to that point. Since $\Delta \widehat{SVA}_{st}$ is defined as destination net origin VA, it is mechanically related to pre-switch school quality, whose effect on test scores will depend on the duration of time spent in the school. Failing to include these controls can generate spurious pre-trends, even with random switching.[19] For this test, SVA is measured in test score standard deviations, while for our main results, we use SVA standard deviations. This allows for easy assessment of the forecast unbiasedness of $\Delta \widehat{SVA}_{st}$ in period 0. With these units, if we have sufficiently controlled for selection, $\Delta \widehat{SVA}_{st}$ will predict test score gains with a coefficient of 1 the year of the switch.

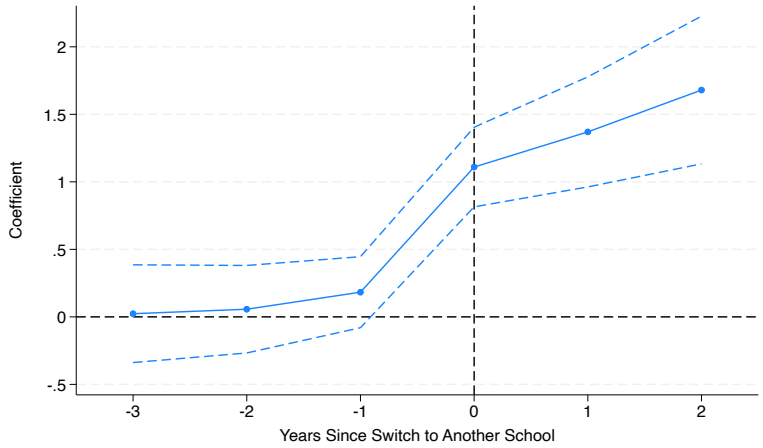
Figure 1 plots τ_k (see Appendix Table A5 for estimates). There is no pre-trend: children who are learning faster do not switch into better schools. In period 0, SVA predicts test score gains with a coefficient of ∼1, suggesting that the controls account for selection. The positive effect of attending a better school accumulates thereafter, consistent with greater exposure to a better school increasing learning.[20] Since switches to better schools may be more selected, Appendix Figure A5 plots the event study graphs using only switches to better (Panel (a)) or worse schools (Panel (b)). In both cases, there is no strong positive

[18] SVAs are estimated only using a distinct sample of non-switchers so that there is no mechanical correlation between SVA and students' outcomes.

[19] Monte Carlo simulations are available on request.

[20] This differs from what one would expect if we controlled for lagged test scores in each year, as in the value-added literature. We would then estimate annual increases rather than accumulated effects and would expect a coefficient of 1 in all post-periods.

Figure 1: Relationship Between Change in School Quality and Trends in Test Scores



Notes: This figure plots τ_k from equation (3). SVA is measured in test score standard deviations. Controls include age, gender, and cohort-by-first school-by-year fixed effects. Standard errors are clustered at the individual level since household relationships are not observed in the tested sample. The figure displays 95% confidence intervals. See Appendix Table A5 for point estimates.

pre-trend, and the coefficient is close to 1 in the year of the switch.

Assets Returning to the household sample, we use the asset index to evaluate whether switches are related to economic shocks. Since there is no mechanical dynamic relationship between the first school VA and assets, we estimate

$$y_{ist} = \sum_{k \in \{-3,..,2\}} \tau_k event_{ist}^k \Delta \widehat{SVA}_{st} + \gamma_{s_i^0} + \Gamma \mathbf{X_{ist}} + \upsilon_{igst},$$

where y_{ist} is the asset index, and the vector $\mathbf{X_{ist}}$ includes age, gender, and year fixed effects. We also measure $\widehat{SVA}_{st}$ in standard deviations of the SVA distribution, in line with the rest of the paper. Appendix Figure A6 and Appendix Table A5 show that there is no relationship between assets and the change in school quality.

Finally, in Appendix Table A6, we test for and find no evidence of differential attrition in the 2018 follow-up.

4 Results for Long-Term Effects of Higher SVA

4.1 Cognitive and Socioemotional Skills

We first present results on the persistence and evolution of initial learning gains into adult life. Motivated by a literature that suggests that the effects of educational interventions on cognitive skills fade (though they may re-emerge through socioemotional skills later on), we report the impact of attending a high VA school on cognitive and socioemotional skills in Table 1. Columns 1–2 report effects on mean test scores in the last year a student was observed between 2003 and 2007, columns 3–4 report the effects in 2011, while columns 5–6 report effects on mean test scores in 2018 (15 years after baseline).[21] Columns 7–8 report the results using measures of functional skills, and Columns 9–10 report effects on our summary measure of socioemotional skills.

Attending a high VA school has large effects on test scores between 2003–07, consistent with the valid construction and estimation of SVA. There is evidence of fade-out until 2011, but not thereafter. Exposure to a 1 sd higher VA school over the course of primary school increases cognitive skills measures by 0.16–0.19 sd in 2018–19. For comparison, a 1 sd higher VA school leads to contemporaneous annual test score gains of 0.19 sd. We observe similar effects on alternative measures of functional numeracy and literacy, with significant increases of 0.3 sd. An important result is that attending a higher VA school has little discernible impact on socioemotional skills. These results are fully robust to the inclusion of SES controls.

4.2 Educational Attainment

Table 2 reports the long-run effects of attending a higher VA school on years of educational attainment. In column 1, we estimate that attending a 1 sd higher VA school increases total schooling by 1.05 years. The inclusion of the SES controls (column 2) has little effect on the estimate, even though the controls increase the adjusted R^2 by 0.1, suggesting that they are meaningful predictors of student outcomes.

To understand at what point in the educational life-cycle we see gains, in Table 3, we estimate the effect of attending a higher VA school on each level of schooling. For every level,

[21] We exclude the students used to estimate the regressions in columns 1–4 from the sample when we calculate the SVAs used in those regressions. The samples for whom we have test scores in different years also differ. This is both because only specific cohorts were originally tested between 2003 and 2007 and because the household tracking in the 2011 round missed children who were no longer in their original household. The sample size is smaller in columns 1–2 because the sample is restricted to children in the household survey who were tested because they belonged to the cohorts we tested in schools. From 2011–12 onward, we switched to tracking the children from the household survey and testing them.

Table 1: The Effect of School Quality on Skills

	Cognitive Skills (2003-07)		Cognitive Skills (2011-12)		Cognitive Skills (2018-19)		Life Skills Index		Socioemotional Skills	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES	(7) First Sch. FE	(8) + SES	(9) First Sch. FE	(10) + SES
Empirical Bayes SVA	0.529***	0.558***	0.090	0.048	0.187**	0.159**	0.277***	0.253***	0.028	0.027
	(0.135)	(0.138)	(0.089)	(0.091)	(0.082)	(0.076)	(0.084)	(0.083)	(0.080)	(0.081)
Mean Outcome	0.339	0.339	0.765	0.765	0.135	0.135	0.178	0.178	0.027	0.027
Adjusted R^2	0.301	0.315	0.209	0.248	0.195	0.278	0.126	0.191	0.176	0.174
Observations	1132	1132	2058	2058	2764	2764	2766	2766	2766	2766
Clusters	907	907	1187	1187	1274	1274	1275	1275	1275	1275

Notes: The dependent variable in columns 1–6 is the average test score, measured in the last year observed between 2003–07 (columns 1–2), 2011–12 (columns 3–4) and in 2018–19 (columns 5–6). In columns 7–8, the outcome is a score measuring literacy and numeracy. In columns 9–10, the outcome is an index from Danon et al. (2024) measuring socioemotional skills. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age fixed effects, a control for gender, and a fixed effect for the first primary school the individual attended. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Columns 1–2 use data from children in the household survey who were tested in schools during that period. Columns 3–4 use data from children who were found and tested in their original households. Columns 5–10 use data from any children from the household survey we could track and test in person. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: The Effect of School Quality on Years of Education

	Years of Education	
	(1) First Sch. FE	(2) + SES
Empirical Bayes SVA	1.050***	0.904***
	(0.298)	(0.268)
Mean Outcome	8.473	8.473
Adjusted R^2	0.194	0.297
Observations	3093	3093
Clusters	1332	1332

Notes: The dependent variable is years of education. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age, gender, and first school fixed effects. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the effects are positive, though they become larger (in percent terms) and more significant for later outcomes. Attending a 1 sd higher VA school increases the likelihood of completing 8th grade by a statistically insignificant 7 pp (11%), 9th and 10th grade by a significant 14 pp (29% and 33%), a high school diploma by a significant 10 pp (52%), and attending college by a significant 6 pp (47%). The point estimates are not sensitive to the inclusion of the SES controls. Notably, the significant effect on educational attainment appears before students take their first high-stakes exam, the 10th grade matriculation exam. Indeed, the effects on 9th and 10th grade completion are almost identical. Hence, the effects of school quality on educational attainment do not appear to be entirely driven by an increase in the likelihood of passing high-stakes hurdle examinations.

4.3 Robustness of Switchers Strategy

In this subsection, we report tests of the robustness of the switchers strategy, as well as estimates from movers and lasso-IV strategies. For brevity, we focus on a single key outcome, years of education.

Effects for Switches to Better and Worse Schools If selection of better-performing students, or of students whose performance is improving, into higher VA schools is driving

Table 3: The Effect of School Quality on Each Level of Schooling

	Completed Grade 8		Completed Grade 9		Completed Grade 10		Completed Grade 11		High School Diploma		Went to College	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES	(7) First Sch. FE	(8) + SES	(9) First Sch. FE	(10) + SES	(11) First Sch. FE	(12) + SES
Empirical Bayes SVA	0.070	0.058	0.136***	0.120***	0.141***	0.126***	0.117***	0.107***	0.099***	0.090***	0.055**	0.049**
	(0.047)	(0.044)	(0.041)	(0.037)	(0.038)	(0.034)	(0.032)	(0.030)	(0.030)	(0.029)	(0.022)	(0.022)
Mean Outcome	0.660	0.660	0.468	0.468	0.428	0.428	0.221	0.221	0.191	0.191	0.117	0.117
Adjusted R^2	0.146	0.214	0.163	0.240	0.181	0.255	0.145	0.207	0.122	0.177	0.116	0.158
Observations	3093	3093	3093	3093	3093	3093	3093	3093	3093	3093	3093	3093
Clusters	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332	1332

Notes: The dependent variables are indicator variables equal to 1 if the individual completed grade 8 (columns 1–2), grade 9 (columns 3–4), grade 10 (columns 5–6), grade 11 (columns 7–8), high school (columns 9–10), and if they attended any post-secondary institution (columns 11–12). *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age, gender, and first primary school fixed effects. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the results, it may be more important for switches to higher VA schools. Appendix Table A7 estimates different effects of SVA by switch status (to better school, worse school, or no switch). SVA has similarly sized, large positive effects in all three cases.

Most Plausibly Exogenous Switches In Appendix Table A8, similar to the robustness tests in Chetty and Hendren (2018), we re-estimate our main specification but only use the most plausibly exogenous switches. In columns 3–4, we restrict the sample to non-switchers and students who switch schools concurrently with a sibling switching schools, or because their school closes down. We further drop switches where the siblings either start in or end in the same school in order to eliminate those cases where positively selected parents move both children to a school that is perceived as better or remove both children from a school that is perceived as failing. In columns 5–6, we instead restrict the set of switchers to those whose switch is associated with school closure or who have a sibling that is exogenously changing schools due to starting a new school level. In both cases, the estimates are quantitatively very similar to the baseline estimates that are reproduced in columns 1 and 2.

Predictive Power of Type-Specific SVA Inspired by another test in Chetty and Hendren (2018), we re-estimate the SVA measures separately by gender and SES (below vs. above median asset index). We then re-estimate the specifications from Table 2 with own and other-type SVAs as explanatory variables (Appendix Table A9). If the effects of SVA are causal, SVA for own type should be a better predictor of long-run outcomes than SVA for the other type. Selection would generate this pattern if households' enrollment decisions respond not just to mean SVA but also type-specific quality, requiring quite a bit of information and sophistication.[22] To compare estimates from consistent samples, columns 1–4 restrict to the sample in schools where both gender-specific SVAs could be calculated. Columns 5–8 restrict to schools where both SES-specific SVAs could be calculated. Own gender and own SES SVA have larger effects on students' outcomes than opposite type SVA, consistent with SVA causally affecting students' outcomes.[23]

[22]For gender, this test reduces the sample size since we can only include individuals in schools where SVA can be estimated for both genders. In cases where sex-segregation is strictly enforced, this will not be possible.

[23]If there were no measurement error in the SVAs, we would expect the opposite type's SVA to have no predictive power when both are included in the same regression. However, SVAs are measured with error since we only observe finite samples of students. Measurement error can lead both explanatory variables to have predictive power, as in column 8, if they both noisily measure a common effect across students along with a type-specific effect. The increase in the estimate for the opposite type SVA in column 8 when own-type SVA is included as a control is consistent with the suppressor effect in statistics.

Movers Strategy In a supplemental strategy, we closely follow the movers strategy in Chetty and Hendren (2018). As in Chetty and Hendren (2018), our specification only compares switchers and exploits variation in the timing of the switch, and therefore, in the length of exposure to the new school. Restricting to switchers, we estimate

$$y_{iT} = \kappa \widehat{SVA}_{i,s_i^0} + \sum_{j=1}^{3} \theta_j Avg\ Post\ Switch\ SVA \times I_i(N_i^{s^1} = j) + \Lambda \mathbf{X_i} + \eta_{iT},$$

where $\widehat{SVA}_{i,s^0}$ is the VA of the first school i attended, $Avg\ Post\ Switch\ SVA$ is the average VA of the schools the child attended post-switch, and $I_i(N^{s_i^1} = j)$ is an indicator variable equal to 1 if the child is observed for j years in primary school post-switch. The vector $\mathbf{X_i}$ includes fixed effects for gender, age, and age at the time of switch. If this strategy is valid, we expect that $\theta_1 < \theta_2 < \theta_3$ since the quality of the post-switch school should matter more the greater the student's exposure. If both this and the previous strategy are valid, we expect θ_3 to be similar in magnitude to β (the estimate from the main strategy). This is because β should be interpreted as the effect of attending a 1 sd better school for every year we observe a child (an average of 2.6 years), while θ_3 is the effect of being observed in a 1 sd higher VA school for 3 years.

Appendix Table A10 reports the results from the movers strategy. Columns 1 and 2 confirm that switching into a higher VA school strongly increases years of education, while columns 3 and 4 show that this effect increases monotonically with years in the new school. Reassuringly, the estimated impact of spending 3 years in a 1 sd higher VA school using the movers strategy is quantitatively similar to the effect of attending a 1 sd higher VA school (Table 2). The estimates – including the pattern of increasing effects with more exposure to the school – are robust to controlling for changes in assets at the time of the switch to account for SES shocks (columns 5–6). In columns 7–10, we focus on the most plausibly exogenous switches and no longer estimate separate effects by exposure due to the reduced sample size. If anything, the estimates are larger.

Lasso-IV Strategy. Finally, we evaluate whether an identification strategy leveraging cross-sectional variation in students' locations produces similar results. We exploit the fact that students are highly distance-sensitive when they choose schools, but, as Andrabi et al. (2025) argue, historical settlement patterns imply that distance to schools is uncorrelated with household characteristics *conditional* on distance to the village center. Therefore, the VA of nearby schools (conditional on distance to the center) provides an instrument for a student's SVA. Appendix B describes this strategy and the results, including identification

tests for the selected instruments (Appendix Table B1). The results (Appendix Table B2) are consistent with the switchers estimates, though larger and more imprecise.

4.4 Impact on Employment and Earnings

We next estimate the effects of SVA on employment and earnings. Unlike education, where men's and women's outcomes are similar over the life cycle, men's and women's employment are very different, both on average and by age. Table 4 therefore estimates the effects of attending a higher VA school on employment for the full sample, as well as men and women separately. Panel (a) shows that attending a higher VA school has no effect on whether an individual is engaged in a productive activity (working or enrolled in school). But this null masks the fact that students who attended higher VA schools are more likely to be currently enrolled (columns 3–4) and less likely to be working in 2018, albeit not significantly so (columns 5–6).

These pooled effects also mask important heterogeneity by gender. For men (Panel (b)), where employment rates are close to universal when out of school, there are clear positive enrollment and negative employment effects. For women, who have a very low employment rate, the estimates are imprecise but positive for both working and enrollment. There is no contradiction here: because female employment is so low, enrollment does little to crowd out employment. Indeed, while not significant at conventional levels, the employment effect for women (+3.8 pp or 56%) is large. Speculatively, this may be because education gives women access to teaching jobs, one of the few socially acceptable opportunities for women in this context.

The fact that attending a higher VA school increases education, which crowds out employment for men at younger ages, suggests that employment and earnings effects will vary over the life cycle. As has been observed in the U.S. for teacher quality (Chetty et al., 2014a), higher SVA may initially have null or even negative effects on both employment and earnings because it reduces both the likelihood of working and work experience. However, at older ages, once respondents have completed their education, we might expect the returns to human capital to dominate the difference in experience.

In Figure 2, we use our first school fixed effect strategy to estimate the effects of attending a higher VA school on employment and monthly earnings, reporting estimates separately by gender and with different cutoffs for the minimum age included in the sample.[24,25] We esti-

[24]Since the identification strategy relies on school switchers, it is quite data-hungry, making estimating a separate coefficient for each age too demanding.

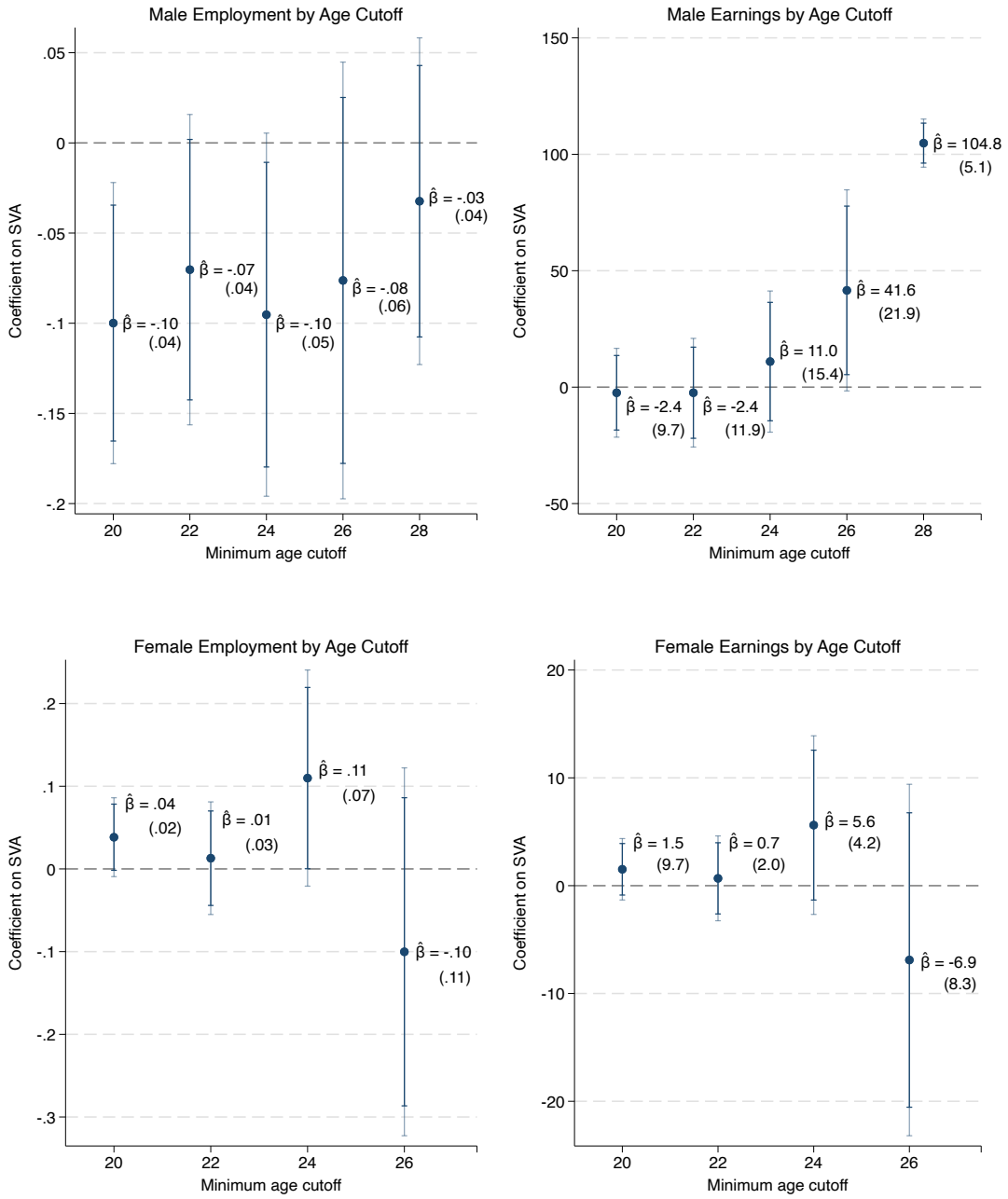
[25]In cases where an individual is self-employed (e.g., works on agricultural land), we calculate earnings as profits (revenues net of costs). The earnings measure therefore includes a small number of negative values, as well as zeros. Earnings for women are also collected using a second question which asks women who report

Table 4: The Effects of School Quality on Labor Force Participation

	Works or Is Enrolled		Is Enrolled		Works	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES
Panel A: Full Sample						
Empirical Bayes SVA	0.031	0.033	0.058**	0.052**	-0.027	-0.019
	(0.030)	(0.031)	(0.024)	(0.024)	(0.028)	(0.027)
Mean	0.560	0.560	0.129	0.129	0.450	0.450
Adjusted R^2	0.446	0.450	0.116	0.156	0.515	0.517
Observations	3093	3093	3093	3093	3093	3093
Clusters	1332	1332	1332	1332	1332	1332
Panel B: Men						
Empirical Bayes SVA	-0.038	-0.031	0.066**	0.063**	-0.100**	-0.089**
	(0.034)	(0.034)	(0.029)	(0.028)	(0.040)	(0.038)
Mean	0.864	0.864	0.108	0.108	0.775	0.775
Adjusted R^2	0.011	0.010	0.132	0.170	0.096	0.112
Observations	1635	1635	1635	1635	1635	1635
Clusters	1029	1029	1029	1029	1029	1029
Panel C: Women						
Empirical Bayes SVA	0.120*	0.101	0.086	0.062	0.038	0.039
	(0.067)	(0.067)	(0.065)	(0.064)	(0.024)	(0.025)
Mean	0.198	0.198	0.151	0.151	0.067	0.067
Adjusted R^2	0.121	0.149	0.123	0.158	0.069	0.076
Observations	1334	1334	1334	1334	1334	1334
Clusters	829	829	829	829	829	829

Notes: The dependent variables are indicator variables for whether the individual works or is enrolled in school (columns 1–2), school enrollment (columns 3–4), and working (columns 5–6). Working is defined as having engaged in an activity in the past month for which the individual received payment. Panel A includes all individuals, Panel B is men, and Panel C is women. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age, (when relevant) gender, and first school fixed effects. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. The reported sample sizes for men and women separately (Panels B and C) are smaller than the combined sample (Panel A) because *reghdfe* reports the number of observations, excluding singletons, from the first-school fixed-effect groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2: The Effects of School Quality on Employment and Monthly Earnings by Gender



Notes: The dependent variable in the left panels is an indicator variable for whether the individual reports working (engaged in an activity in the past month for which they received payment). The dependent variable in the right panels is monthly earnings (in 2018 USD). Each point estimate comes from a regression of the outcome on the individual's average empirical Bayes SVA. The sample is restricted to individuals above the minimum age cutoff on the x-axis. All regressions include age and first school fixed effects. Women aged 28+ are excluded from the women's panels since only two women work at this age. Standard errors (in parentheses) are clustered at the household level. Both 90 (dark line) and 95 (light line) percent confidence intervals are reported.

mate effects on earnings in levels (in 2018 USD) and include zeros to account for educational crowd-out, as well as the possibility that more educated individuals stay out of work longer.

For men, there is clear evidence that education crowds out working when the age cutoffs are lower, consistent with the average effects in Table 4. However, for men older than 26, the negative effects on employment are no longer even marginally significant, and for men older than 28, the magnitude of the crowd-out effect has greatly attenuated. Consistent with this, the effect on earnings levels at younger cutoffs is close to zero. Positive effects emerge for those older than 24, with effects becoming large and statistically significant at older ages. For men older than 26, attending a higher VA school increases earnings by a marginally significant 31%, and for men older than 28, earnings increase by a significant 90% (though this subsample is small).

For women, in line with differences in how employment trends over the life cycle, there is no evidence of educational crowd-out. There is some evidence of large, positive employment effects for those older than 24, which translate into positive, though statistically imprecise, effects on earnings levels (+110% in earnings). However, at older ages, these effects disappear, and we do not include 28+ in the figure because only 2 women in the sample still work at this age. We note, however, that earnings alone may be a poor measure of the benefits of education for women if there are intergenerational benefits as well (Andrabi et al., 2012; Duflo et al., 2024).

Overall, our results suggest that there are large earnings returns for men to attending a higher VA school by their late twenties. This result is consistent with positively selected individuals continuing to college. Indeed, we know that students with the highest test scores are more likely to attend college, and once we see them enter the workforce, the gains to foundational learning increase. Given the upward trend in the effect sizes and the fact that the oldest individuals in our sample are only 30, these numbers may underestimate the positive earnings effects of attending a higher VA school when men are in their prime working ages between 30 and 40. More speculatively, female employment and earnings may also improve in response to increased SVA, but these gains are concentrated during a short period when female employment peaks before marriage.

While the results in Figure 2 focus on the entire population, thus abstracting from selection into employment, the literature has traditionally focused on earnings impacts only for labor force participants. In our case, it is quite likely that men who are in the workforce are negatively selected in terms of their earnings capacity since those out of the workforce are predominantly enrolled in college. Therefore, following this literature, we next condition the

being unemployed about any earnings from home production such as from selling handicrafts or tutoring. Earnings are winsorized at the top and bottom 2% by gender, since misreporting can lead to extreme outliers.

sample on those reporting positive earnings and estimate the effect of SVA on log earnings (Table 5). We caution that these estimates average all the effects across the life cycle among working people. Because so few women work, we cannot credibly estimate these effects for women separately and only report results for the full sample and working men.

Table 5: The Effect of School Quality on Log Earnings

	All				Men			
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES	(7) First Sch. FE	(8) + SES
Empirical Bayes SVA	0.224*	0.235**			0.250**	0.256**		
	(0.118)	(0.117)			(0.118)	(0.116)		
Years Education			0.042***	0.037***			0.034***	0.029***
			(0.008)	(0.009)			(0.008)	(0.009)
Mean Outcome	4.553	4.553	4.553	4.553	4.724	4.724	4.724	4.724
Adjusted R^2	0.282	0.287	0.299	0.297	0.041	0.048	0.053	0.053
Observations	1364	1364	1364	1364	1193	1193	1193	1193
Clusters	899	899	899	899	822	822	822	822

Notes: The dependent variable is log monthly earnings. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age, first school, and (when relevant) gender fixed effects. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column 1 shows that working individuals who attended a 1 sd higher VA school earn 22 log points more (25 log points more for men in column 6). Controlling for SES yields similar effects. To identify the importance of the increase in schooling vs. learning for earnings, we replace the explanatory variable (average SVA) with years of education, and estimate a return to an additional year of schooling of ∼3–4% (columns 3–4 and 7–8). This is substantially lower than the observational estimates of Patrinos and Psacharopoulos (2020) or Gethin (2025) in low-income settings, likely reflecting our sample's young age.[26] Given that we estimate that a 1 sd increase in SVA increases years of education by ∼1 year, these estimates suggest that the majority of the returns to attending a higher VA school that we observe in columns 1 and 5 are *not* explained by increased educational attainment alone, a result that has important implications for how we should view the relative benefits of greater educational attainment versus increases in foundational learning.

[26]Estimates of the Mincerian returns to education that include older adults in previous LEAPS surveys generate numbers closer to the estimates for LMICs in the literature.

5 Experimental Evidence from the Randomized Controlled Trial

We now turn to the village-level report card randomized information intervention to again estimate the effect of test score gains at the primary level on future learning, educational attainment, and labor market outcomes. This exercise complements the SVA estimates in four distinct ways. First, it provides an alternative and experimental source of identification, further bolstering the causal interpretation of our results. Second, since the intervention directly raises test scores, it supports the case that the likely mechanism for the long-term impacts of attending a high VA school is increased learning rather than other amenities that a high VA school may provide. Third, by estimating the effect of an intervention that raised scores for all students in the village, it shows that our results hold once we account for within-village equilibrium effects. The specific concern is that people are competing for a limited number of "good jobs" (usually thought to be in the public sector, see Duflo et al. (2023)), and earnings impacts will disappear when test scores increase for the entire population. As we will see, the information intervention, which treated an entire cohort and was therefore "scaled-up," has effects that closely mirror the long-term impacts of attending a higher VA school, assuaging this concern.

Fourth, our previous results demonstrate that attending a higher VA school is causally linked to high school graduation, college attendance, and earnings. But they do not answer the question of whether interventions that increase test scores by magnitudes *typically seen in the literature* will have similar long-term impacts. One reason for skepticism is that, while naturally occuring variation in SVA may generate variation in outcomes, further efforts to improve test scores may have limited persistent effects on longer-term outcomes — this is a question of the average versus the marginal. A second reason is that the variation in SVA is quite large relative to the effects of educational interventions studied by the literature, raising questions about whether the smaller gains in learning that are typically observed in experiments can generate similar gains.

The randomized intervention that we study was implemented between 2003 and 2006. It provided schools, parents, and children with information on school- and student-level test scores. Andrabi et al. (2017) found that the intervention increased children's test scores by 0.11 sd, in line with the median education intervention documented by Evans and Yuan (2022). Using the same data as before, we now assess intermediate outcomes 8 years after the intervention (in 2011) and longer-run labor market outcomes 15 years later (in 2018–19). If the impacts are similar to attending a higher VA school, we would expect education effects to emerge for all age groups by 2018–19, but earnings impacts to be greatest for the older

students who have already completed their education and any subsequent job search. We use the same LEAPS longitudinal dataset, allowing us to retain our focus on the intervention in this section rather than the broader context and data collection.

5.1 Experimental Design

The information intervention was implemented in 56 randomly selected LEAPS villages (of the 112) with district-level stratification. Based on tests we administered in 2003–04 to grade 3 students, we constructed report cards documenting a child's subject-specific raw scores, as well as the scores and ranks for all schools in the village. The report cards were delivered to schools and parents at village meetings in September 2004, prior to the April 2005 admissions cycle. Schools and parents were also informed that children would be tested and report cards would be distributed again in 2005, which they were.

In 2006, report cards were distributed to schools in all villages, including the controls, but without community meetings or direct distribution to parents. Qualitative discussions with schools in control villages suggested that this light-touch distribution without parental engagement had little effect. To limit the scope for any contamination by the later distribution to control villages, we focus on children who were 7–11 and enrolled in a school at baseline. These children would have completed all or most of their primary schooling before 2006. In our 2003 household survey, there were 2,547 such children and we were able to survey 85% of them in 2018–19. Of these, 88.8% were surveyed in person; of the remaining, we either conducted phone surveys or obtained information from other household members.

5.2 Balance & Attrition

Appendix Table A11 shows no systematic evidence of differential attrition by treatment status, either overall or by baseline characteristics, in either 2011 or 2018. From an external validity perspective, baseline attributes alone also have little predictive power for whether an individual is observed in 2011 or 2018. Appendix Table A12 shows that treatment status is balanced on baseline characteristics for the samples observed in 2003, 2011, and 2018.

5.3 Empirical Strategy

Using the sample of individuals who were 7–11 at baseline, we estimate

$$y_{ida} = \beta RC_i + \alpha_a + \lambda_d + \epsilon_{ida} \tag{4}$$

where i indexes an individual, a indexes age, and d indexes a district. The explanatory variable of interest RC_i is an indicator variable for whether an individual lived in a treatment village, α_a is an age fixed effect, and λ_d is a district fixed effect. We include district fixed effects to account for the stratified randomization (Duflo et al., 2007) and age fixed effects since age is a particularly relevant determinant of educational and labor market outcomes. When pooling men and women, we also control for gender fixed effects.

5.4 Results

Skills Table 6 reports effects of the information intervention on skills measures in 2011 and 2018. The initial test score gains persist even though the intervention lasted only for two years (2004–2005). We find that in 2011, children's mean test scores in treated villages were 0.15 sd higher (column 1). By 2018, most children are out of school, but the effects on mean test scores remain significant at the 10% confidence level and quantitatively meaningful. The gain of 0.09 sd corresponds to 0.11 control-group standard deviations (column 2). The estimated effect on life skills is similar in magnitude to the 2018 cognitive-skills effect, though imprecisely estimated (column 3). As with attending a higher VA school, the intervention has no impact on socioemotional skills (column 4).

Table 6: Effect of Report Card on Skills

	(1) Cognitive Skills 2011	(2) Cognitive Skills 2018	(3) Life Skills Index	(4) Socioemotional Skills
Report Card	0.146**	0.094*	0.078	-0.015
	(0.062)	(0.055)	(0.050)	(0.057)
Control Mean	0.805	0.188	0.244	0.070
Control SD	0.981	0.826	0.877	1.040
Treatment Effect wrt Control SD	0.149	0.114	0.089	-0.014
Control Mean (2003)	-0.441			
Adjusted R^2	0.065	0.012	0.005	0.118
Observations	1475	1908	1909	1910
Clusters	112	108	108	108

Notes: This table reports the effect of the report card intervention on cognitive and socioemotional skills. Column 1 reports the treatment effect on the average score on tests in English, math, and Urdu in 2011, while column 2 reports average test scores in 2018. In column 3, the outcome is a functional literacy and numeracy index measured in 2018. In column 4, the outcome is an index from Danon et al. (2024) measuring socioemotional skills. For each outcome, we also report the mean and standard deviation of the score in the control group. The "Treatment Effect wrt Control SD" row reports the treatment effect divided by the control-group standard deviation for the given test score. For the 2011 cognitive scores, we also report the control group's mean test scores at baseline. All specifications include district, age, and gender fixed effects. Robust standard errors are clustered at the village level. Column 1 includes 112 clusters, while columns 2–4 include 108 clusters. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Educational Attainment Table 7 shows that there are large and statistically significant effects in 2018 (when the sample is aged 22–26). High school (grade 12) completion increases by 4.9 pp (21.6%; Column 4) and college attendance by 4.4 pp (32%; Column 5). In aggregate, treated participants complete 0.54 more years of schooling (Column 6). Strikingly, this is very close to 0.61, which is the increase we would expect from our estimates of attending a higher VA school.[27]

Table 7: Effect of Report Card on Educational Attainment

	(1) Completed Grade 5	(2) Completed Grade 8	(3) Completed Grade 10	(4) High School Diploma	(5) Went to College	(6) Years of Schooling
Report Card	0.025	0.032	0.048	0.049**	0.044**	0.539**
	(0.017)	(0.027)	(0.032)	(0.024)	(0.021)	(0.270)
Control Mean	0.872	0.683	0.447	0.226	0.138	8.723
Adjusted R^2	0.021	0.012	0.021	0.032	0.014	0.026
Observations	2152	2152	2152	2152	2152	2152
Clusters	108	108	108	108	108	108

Notes: This table reports the effects of the report card on educational attainment in 2018. Columns 1–4 report the treatment effect on completing grades 5, 8, 10, and high school, respectively. Column 5 reports the treatment effect on the likelihood of attending college, and column 6 reports effects on years of schooling. All regressions include district, age, and gender fixed effects and cluster standard errors at the village level. The specifications include 108 clusters. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Labor Market Finally, Table 8 reports the labor market effects of the information intervention. As with the prior results, we present effects for earnings in levels and logs, separately by gender. When we report levels, we include all individuals in the sample, and for logs we exclude individuals who were not working at the time of the survey. We also report results for the full sample and for the older cohort within the sample, who were 9–11 at the time of the intervention. Based on our results from attending a higher VA school, we expect lower labor force participation among treated groups who are more likely to be in school and greater earnings impacts for older individuals, who will have completed their schooling.

Column 1 (Panel A) shows no discernible overall effect on whether an individual works or is enrolled in school. Individuals in treated villages are, however, 4.3 pp (42% of the control mean) more likely to still be studying overall (Column 2, Panel A). While not significant at conventional levels, there is also evidence of a 2.3 pp reduction in labor force participation among treated individuals (Column 3, Panel A). Both the likelihood of being enrolled and

[27] This number is given by $1.05 \times .11/.19$, where 1.05 is the estimated effect of SVA on years of education, .11 is the report card effect on contemporaneous test scores from Andrabi et al. (2017), and 0.19 is the effect of a 1 sd higher VA school on contemporaneous test scores.

the reduction in labor market participation in treated villages are driven predominantly by men (Columns 2 and 3, Panel B).

Table 8: Effect of the Report Card on Labor Market Outcomes

	(1) Works or Is Enrolled	(2) Is Enrolled	(3) Works	(4) Earnings Levels Full Sample	(5) Earnings Levels Older Cohort	(6) Log Earnings Full Sample	(7) Log Earnings Older Cohort
Panel A: Labor Market Outcomes for All							
Report Card	0.004	0.043**	-0.023	2.257	10.733**	0.086	0.118**
	(0.017)	(0.017)	(0.016)	(4.133)	(5.184)	(0.053)	(0.059)
Control Mean	0.579	0.102	0.497	69.807	74.450	4.559	4.642
Adjusted R^2	0.445	0.027	0.483	0.276	0.281	0.248	0.193
Observations	2152	2152	2152	2144	1320	1053	682
Clusters	108	108	108	108	108	108	108
Panel B: Labor Market Outcomes for Men							
Report Card	-0.006	0.053***	-0.040	4.750	18.874**	0.065	0.115*
	(0.023)	(0.020)	(0.026)	(7.366)	(8.890)	(0.055)	(0.061)
Control Mean	0.863	0.083	0.798	116.295	120.264	4.761	4.784
Adjusted R^2	0.002	0.024	0.019	0.018	0.006	0.015	0.015
Observations	1217	1217	1217	1209	767	914	605
Clusters	108	108	108	108	108	108	108
Panel C: Labor Market Outcomes for Women							
Report Card	0.021	0.031	0.004	-0.146	0.112		
	(0.029)	(0.025)	(0.019)	(1.107)	(1.547)		
Control Mean	0.184	0.128	0.077	5.527	5.729		
Adjusted R^2	0.023	0.033	-0.003	-0.002	-0.000		
Observations	935	935	935	935	553		
Clusters	108	108	108	108	107		

Notes: This table reports the effect of the report card intervention on labor market outcomes and earnings in 2018. Panel A reports estimates for the full sample, while Panels B and C report estimates for men and women. Column 1 estimates the effect on working (defined as engaged in a paid activity in the past month) or being enrolled in school. Column 2 reports the effect on enrollment, while column 3 reports the effect on working. Columns 4 and 5 estimate effects on monthly earnings (in 2018 USD) in levels. Columns 6 and 7 estimate the effect on log monthly earnings. Columns 5 and 7 restrict the sample to the older cohort (individuals aged 9–11 in 2003). The remaining columns estimate effects on the full sample (those aged 7–11 in 2003). Log earnings specifications are not reported for women because too few women report positive earnings. All specifications include district, age, and (when relevant) gender fixed effects. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Positive effects on earnings (unconditional on working) are indeed concentrated among older men, with little evidence of a labor market impact for women. While there are no earnings effects for the full sample of men (column 4, Panel B), when we focus on older men who are more likely to have completed their education (those who were 24–26 years old in 2018), there is a substantial effect of 18.9 USD (+16%) per month (column 5, Panel B). Conditional on working, earnings increase by 9 log points in the full sample (column 6, Panel A) and impacts become larger and significant at the 5% level among older cohorts, a

result that is driven by men in the sample.[28] Extrapolating from the (log) earnings estimates of attending a higher VA school in Table 5, we would expect male earnings to increase by $0.22 \times \frac{0.11}{.19} = 0.13$ in logs. This estimate is again very close to what we observe for the older cohort (who are at the mean age of participants in the SVA regressions).

We conclude that the randomized intervention, which led to learning gains that are quite similar to the median of effect sizes observed in the education literature, led to sizable long-term effects. Further, these effects are quantitatively very similar to what we would have anticipated from the long-run estimates of attending a higher VA school, even though the intervention was at the village level rather than the school or child level.

5.5 Robustness

We assess the robustness of our findings in two ways. First, our experiment allows for a clear placebo group: children aged 13–15 in 2003 were not in primary school when the intervention started and should not have benefited from it. Appendix Table A13 shows no statistically significant effects for this placebo group across any of the outcomes. Second, motivated by the observation that the usual t-statistic confidence interval does not provide a coverage guarantee in finite samples for arbitrary outcome distributions, Young (2019) has advocated for the use of randomization inference tests in situations with potentially influential observations. Appendix Table A14 shows the results of the randomization inference test of Fisher's sharp null that the intervention had no effects on each individual. The same set of outcomes remains statistically significant as before.

6 Discussion: Interpretation & Cost Effectiveness

Both attending a higher VA primary school and boosting test scores through a randomized intervention lead to large long-term gains. In this penultimate section, we discuss a variety of questions raised by these results and characterize the costs and benefits of feasible foundational learning interventions.

6.1 Interpreting the Results

6.1.1 What happened to fade-out?

In contrast to a substantial literature on test scores (Bailey et al., 2017), we observe that gains in cognitive skills persist, even many years later. An insight developed by List and

[28] As with the SVA analysis, the small sample of earners among women means that we cannot provide reasonable earnings estimates for them.

Uchida (2024) may be key to understanding this result and when it is likely to hold. List and Uchida (2024) show that fade-out is related to the fact that treated children return to studying with untreated children in subsequent years. When children are tracked, or when an entire cohort is treated, there is less fade-out. In our context, attending a higher VA school keeps higher-performing children together, and the report card was a *village-level* intervention. In both cases, a child's immediate classmates receive the same treatment as the child. Our result is therefore consistent with List and Uchida (2024)'s argument that it is *precisely when interventions are scaled up* that we should expect less fade out.

6.1.2 General equilibrium effects of scaling-up

We have noted how the report card intervention, precisely because it was at the village level and therefore raised test scores for all (treated) students in the village, already incorporates equilibrium responses within treated villages.[29] In addition, to specifically test whether foundational learning increases an individual's likelihood of obtaining a rationed public sector job, in Appendix Table A15, we estimate the effects of SVA and the information intervention on public sector and formal (contract-based) employment.[30] As discussed earlier, these types of jobs are rare, and neither regressor affects these outcomes. Finally, in Appendix Table A16, we report (log) earnings results for additional specifications that control for occupation, task content of the job, and migration status (reflecting the possibility that the earnings impacts are due to increased migration, as in Araujo and Macours (2021)). These controls do not substantially change the estimated coefficients. We conclude that these results are consistent with earnings increases *within occupations*, rather than being driven primarily by occupational switching in a context with a fixed number of "good" jobs.

6.1.3 Heterogeneity by socioeconomic status

While both higher SVA and the information intervention help the *average* student, one may wonder whether these average effects may be driven by greater positive effects on high SES students, leading to higher inequality. Appendix Figure A9 estimates the effects of SVA and the randomized intervention by household assets on our key outcomes of years of education and log earnings.[31]

[29] While one could posit that there may still be GE effects across villages, this is unlikely. First, given the nature of local labor markets, they are unlikely to crowd out jobs in other villages. Second, the similarity of our SVA and randomized intervention results suggests there is little crowd-out within the village, where, if anything, we would have expected more crowd-out relative to between villages.

[30] We report results for the full sample and for men only, as the sample of women only is too small to estimate these specifications.

[31] Appendix Figure A10 verifies that the estimates are similar when we restrict to men.

In the left panel, attending a higher VA school increases educational attainment for both high and low SES households, though the effect is larger for high SES households. For the information intervention, there is only a positive effect on high SES households. In the right panel, attending a higher VA school has very similar (log) earnings impacts by SES, while the information intervention has quantitatively larger earnings effects (p=0.13) for low SES children. The lack of earnings effects for high SES households may be because they have more years of education and are therefore not at the same stage in their experience profiles or are still in college. Recall that this sample is younger on average than the sample that we use to estimate log earnings effects for SVA. Altogether, the results support a conclusion that both SVA and the intervention improved earnings for low SES children. Further, at this point in the life cycle, the estimated earnings effects for these children are at least as large as those for high SES children. Given that the information intervention did not affect low SES children's education, the results also underscore that cognitive skills themselves, not just increased schooling, matter for earnings.

6.1.4 Foundational learning or something else?

So far, we have attributed the effects of SVA and the information intervention to gains in foundational learning. However, long-term impacts could also reflect other mechanisms that are correlated with greater foundational learning. We think that this is less of a concern for the information intervention (where switching across schools was very limited and the gains were driven by increases within schools), but it is possible that other characteristics of high VA schools also increase education or long-term earnings. Guided by the literature, we consider several potential non-foundational learning channels.

Socioemotional Skills? Programs that affect children and deliver long-run benefits into adulthood often affect cognitive skills in the short-run but not the long-run, suggesting that their impacts are through non-cognitive or socioemotional skills (García et al., 2020). However, in our context, neither attending a higher VA school nor the information intervention affected socioemotional skills in the long-run. One simple explanation, that we mismeasured socioemotional skills (or measured the wrong ones) is unlikely: Danon et al. (2024) document the extensive construction and validation process that we used to measure the multiple skills that are commonly assessed in this literature. While it *could be* that there were earlier impacts on socioemotional skills that have since faded out, it seems unlikely that such early increases would have had such persistent effects on outcomes like earnings.

The next three channels relate specifically to the interpretation of the SVA estimates. Notably, the fact that the information intervention leads to results similar to those from the

SVA estimates already assuages concerns about these alternative channels. Nonetheless, we also present other tests that further address this.

Reputation & Peer Effects. One explanation for the positive effects of attending a higher VA school is that higher VA schools have better reputations, allowing students to gain admission to and attend better secondary schools and colleges (MacLeod and Urquiola, 2015; Sekhri, 2020) or providing students with better peers. Appendix Table A17 re-estimates our main switchers SVA specification controlling for peer quality measures (column 2 for years of schooling and column 6 for log earnings). The effect of SVA remains large and significant.

Private Premium. Another possibility is that SVA is correlated with private schooling (as shown by Andrabi et al. (2025)), and our estimates capture a private school premium for educational attainment rather than the direct effects of attending a school with a larger effect on cognitive skills. Columns 3 and 7 of Appendix Table A17 control for share of primary school spent in private school; the SVA coefficient remains large and significant for years of education and is similarly sized, although no longer precise for earnings.

Offering Higher Grades. A third possibility is that high SVA schools are more likely to offer grades beyond primary school. Since students do not need to switch schools, this would reduce the cost of increasing educational attainment.[32] Columns 4 and 8 of Appendix Table A17 include fixed effects for the highest level of educational grade offered by a school; the SVA coefficient is virtually unchanged.

6.2 Quantifying the Gains From Improved Learning

We now provide an accounting of the benefits and costs of improvements in foundational learning. We focus on the information intervention, since the effects on earnings, when scaled by learning effects, are almost identical in the information intervention to the effects of attending a higher VA school. The information intervention also delivers similar test score gains to many educational interventions studied in the literature. Moreover, given that the intervention was implemented by our team, we observe the direct program cost unusually precisely and have rich data with which to estimate the remaining social costs. We use social benefit-cost ratios and the marginal value of public funds (MVPF) framework of Hendren and Sprung-Keyser (2020) to measure the fiscal effectiveness of the program.

[32] About 50% of children attend schools that offer at least some middle school, though only 20% are in schools that continue through grade 10, and 2% are in schools that offer classes through grade 12.

For social benefits, we use the causally estimated financial gains to estimate the present discounted value of the earnings increase over an individual's working life (conservatively, age 26–60). This is arguably a lower-bound measure of aggregate social benefits, not only because the earnings results will likely increase as more (of the currently enrolled) men enter the workforce but also because it does not include intergenerational or health benefits (Andrabi et al., 2012; Duflo et al., 2024), or other positive externalities. Social costs include the direct cost of the intervention, lost earnings from the increase in years of schooling, and additional expenditures on public and private education by both the household and the government. We note that the $3 program cost is a trivial share of total social costs. These costs largely reflect the direct and opportunity costs of additional schooling–an important consideration for evaluations of education programs in general. The details of our estimation, including the full set of assumptions, are in Appendix C.

Table 9 reports the benefit-cost ratio, internal rate of return, MVPF, and values for the key elements (costs to government and households and earnings gains) used to construct them. The calculations in this table allow for earnings growth based on ILO informal sector wage growth profiles over the life cycle in Pakistan. The reported numbers also allow for the computation of individual-level social benefits net of social costs. We include estimates for the full sample, men alone (who drive earnings effects), and lower SES men (informing the benefits of targeting). Appendix Tables A18 & A19 report additional estimates under alternative assumptions about earnings trajectories, and Appendix Table A20 reports the range of estimates for men from households below the median baseline asset index. All calculations conservatively assume zero economic growth.

Regardless of the assumptions and metric, the intervention is highly cost-effective. The lower-bound MVPF estimate for men (column 1 of Table A18) is infinite. That is, in present discounted value terms, increased tax revenues are greater than all government costs of the intervention (including lost taxes during extended time in school and the costs of providing additional years of public education). In the full sample, the MVPF ranges from 17.9 to 73.1 (Appendix Table A19). Appendix Table A20 computes the MVPF of the intervention only for men from low SES households and again finds an MVPF of infinity for all scenarios. Hendren and Sprung-Keyser (2020) have assessed the MVPF of multiple programs in the U.S. and report an MVPF of infinity for the top 16% of all programs.[33] For men, the information intervention falls in this category. For the full sample, the most conservative estimate of 17.9 is around the 78th percentile of all policies, but around the 93rd percentile among finite MVPFs only.

[33]We compare our estimates to the 95 baseline policies assessed in Hendren and Sprung-Keyser (2020) for which there is sufficiently comprehensive evidence on willingness to pay and net government cost.

Despite our conservative assumptions, the BCR ranges from 2.1–2.7 for the pooled sample, 2.7–3.5 for men, and 8.5–10.8 for low SES men. The BCR is substantially higher than conditional cash transfers and in line with the rates for highly successful programs, such as private school voucher programs in Colombia, early child nutrition programs, and gold-standard early-childhood interventions in the U.S. (Damon and Glewwe, 2007; García et al., 2020). The BCR also shows that increasing foundational learning *has especially high social returns for low SES men*, contrary to concerns that foundational learning investments mainly benefit richer families who are able to afford secondary school or college.

It is useful to compare investments in foundational learning to widely deployed policies that seek to boost human capital and/or earnings.

- One popular class of policies is conditional cash transfers, used to incentivize continued education. A meta-analysis of 94 studies from 47 such programs in LMICs by García and Saavedra (2017) found 10 studies with data on school completion. Of these, 6 report zero impact, with an average impact across all 10 studies of 3.3 percentage points. Our estimates of 9.9 percentage points (attending a higher VA school) and 4.9 percentage points (information intervention) for school completion are substantially larger.

- A second class of policies used to boost employment and earnings consists of training and vocational programs (TVET). Carranza and McKenzie (2024) document that a new generation of training programs has yielded higher impacts, but even then, the earnings impacts average 8.2 percent. The earnings impacts for the older cohort from improving foundational learning are of a similar magnitude. Our benefit-cost ratios, for instance, compare favorably to those reported by Alfonsi et al. (2020) for a training program in Uganda.

- Finally, graduation programs that target the ultra-poor are now used in several countries. Banerjee et al. (2021) report a benefit-cost ratio of 3.8 for the long-term effects of a graduation program in India. Again, our estimates compare favorably with this benchmark.

We end this section with a simple observation. The average public primary school in our sample has 106 children. The social benefits created by improving a school by 1 sd in the SVA distribution are then .19/.11 $\times$ 955.6 $\times$ 106 = \$174,962 (955.6 is the benefit estimate from column 2 of Table 9, and 0.19 is the test score effect of attending a 1 sd better school, while 0.11 is the test score effect of the report card intervention). After subtracting the resource and opportunity costs of additional schooling, this amount drops to \$100,934.

Table 9: Benefit-Cost Ratio, IRR, and MVPF for the Report Card

	(1) Men	(2) Full Sample	(3) Low-SES Men
Panel A: Costs			
Direct Cost of Report Card	3.00	3.00	3.00
Additional Private Schooling Costs	11.69	12.69	14.16
Additional Public Schooling Costs	82.62	73.06	4.22
Additional Household Education Expenditure	115.04	94.76	26.84
Lost Earnings from 2003 to Age 25 (Baseline Ages 7-11)	369.22	220.82	181.74
Lost Taxes from 2003 to Age 25 (Baseline Ages 7-11)	27.69	16.56	13.63
Panel B: Benefits			
Additional Earnings (Ages 26–60 for Men; Ages 26-30 for Women)	1727.11	955.61	2146.78
Gained Taxes (Ages 26–60 for Men; Ages 26-30 for Women)	129.53	71.67	161.01
Panel C: Cost-Effectiveness Metrics			
Net Cost to Government	-16.23	20.95	-140.15
Total Benefits	1727.11	955.61	2146.78
Total Costs	581.57	404.33	229.97
Benefit-Cost Ratio	2.97	2.36	9.33
Internal Rate of Return (%)	12.68	10.85	23.41
MVPF	∞	27.31	∞

Notes: This table reports benefit-cost calculations for the report card. Column 1 reports estimates for men; column 2 reports estimates for the full sample; and column 3 restricts the sample to men from low-SES households (households below the median asset index in 2003). Monetary values are expressed in 2018 USD and discounted to 2003 using a standard discount factor of 0.95. Panel A reports costs; Panel B reports benefits and fiscal gains; and Panel C reports the key cost-effectiveness metrics—net cost to government, total benefits, total costs, the benefit-cost ratio, the internal rate of return, and the MVPF. Except for the direct report card cost and fiscal tax rows, cost and benefit components are constructed as individual-level present discounted values and then estimated as the coefficient on the report card treatment in regressions controlling for district, age, and (when relevant) gender fixed effects. Lost Taxes and Gained Taxes are fiscal externalities used in the net-government-cost and MVPF calculations; they are not included in Total Costs or Total Benefits. The tax calculations in Panels A and B assume a 15 percent GST rate and 50 percent compliance. Net Cost to Government equals the direct report card cost plus lost taxes plus additional public schooling costs minus gained taxes. Total Benefits equal the additional earnings gains for treated men between ages 26 and 60 and treated women between ages 26 and 30. We scale the earnings treatment effect using the gender-specific age-earnings profile for informal workers in Pakistan. Total Costs sum the direct report card cost, additional private and public schooling costs, additional household education expenditure, and lost earnings from 2003 through age 25. The Benefit-Cost ratio is the ratio of Total Benefits to Total Costs. The MVPF numerator, willingness to pay, equals additional after-tax earnings net of additional private schooling costs, additional household education expenditures, and after-tax earnings foregone while participants remain in school. The MVPF equals willingness to pay divided by net cost to the government. The Internal Rate of Return is computed from the annual undiscounted net benefit stream. For further details on the calculations, refer to Appendix C

These benefits support trialing educational interventions that are more ambitious and costly than what has been tried in low-income settings thus far. Interventions that are thought to be more 'expensive' cost ∼USD 20 per student per year in 2024 PPP terms, which for an average school in our sample would be $10,000 over 5 years if they were implemented each year. That cost is less than 10% of the 'break-even' cost for interventions that could raise test scores by 1 sd over the primary school years. We may be missing high-gain interventions because we have been stuck in a 'low-cost' mode for years.

7 Conclusion

A large literature in development and education economics has studied how to improve foundational learning in low-income countries. Yet, policymakers remain skeptical about such investments for three reasons: observed learning gains are small relative to cross-country test score gaps; these gains may fade out over time; and, even when skills persist, a scarcity of skilled jobs may limit their labor market returns. This paper is the first to causally link gains in foundational learning to longer-run learning, educational, and labor market outcomes and therefore empirically address these concerns.

We find no grounds for pessimism. Both sources of variation that we exploit – attending a higher VA school and being randomized into the treatment arm of an information intervention that increased test scores – generate substantial positive effects into adulthood. Both these sources of variation boost foundational learning in primary school, and our results suggest that it is this greater foundational learning that leads to greater cognitive skills as adults, higher rates of high-school graduation and college attendance, and ultimately higher earnings.

The earnings increases are sufficient to generate large benefit-cost ratios, internal rates of return, and a high marginal value of public funds. Further, there are multiple reasons to believe that our estimates of foundational learning's benefits are conservative. We only observe individuals at the start of their careers, which means that we may underestimate long-run earnings effects, and we do not account for the non-labor market benefits in terms of delaying fertility and marriage or the intergenerational impacts on children. Even our conservative estimates, on the merits of the labor market returns alone, confirm that the case for investing heavily in improving foundational learning in low-income settings is extremely strong. Increasing foundational learning may be among the highest-return investments currently documented in development economics.

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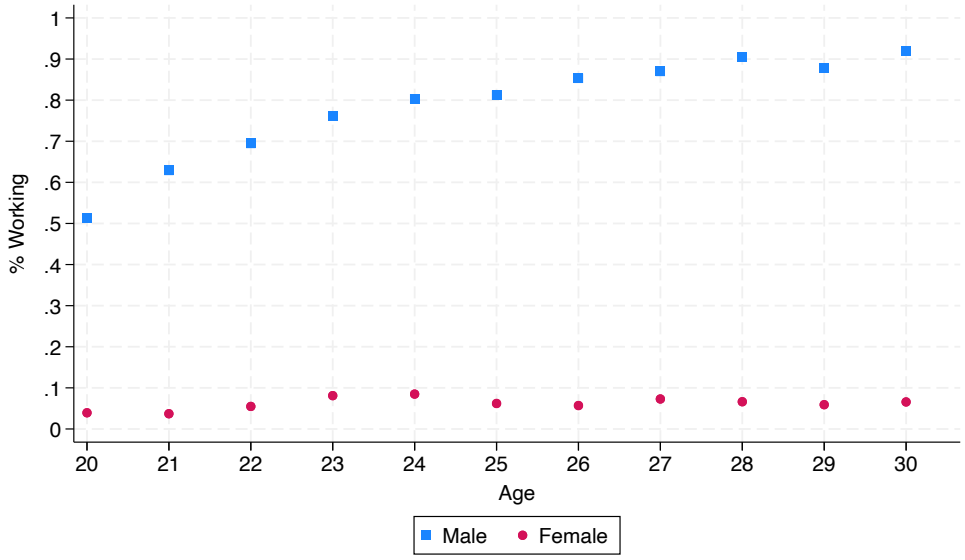
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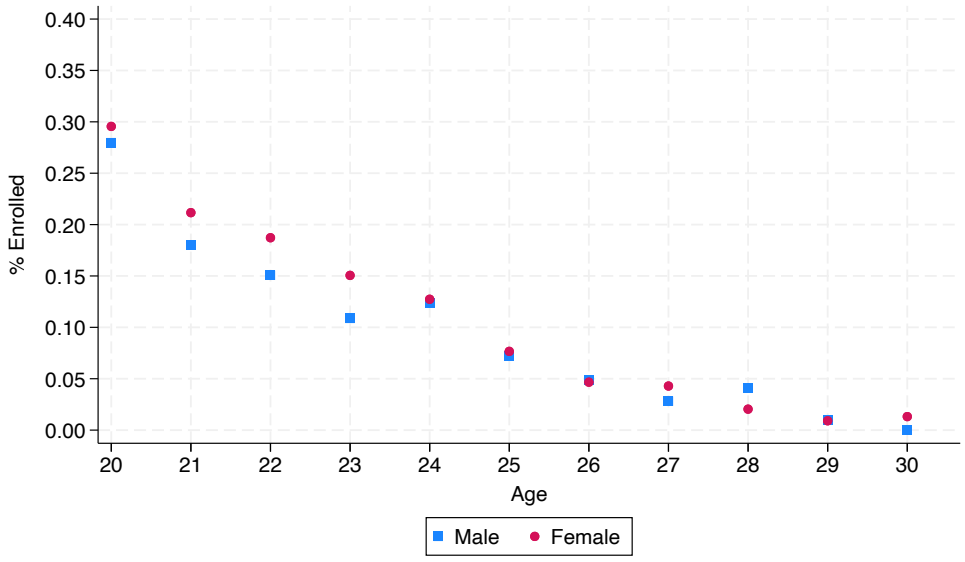
A Appendix Tables & Figures

Appendix Figures

Figure A1: Share of Individuals Employed and Enrolled in School, by Gender and Age



(a) Share Employed



(b) Share Enrolled

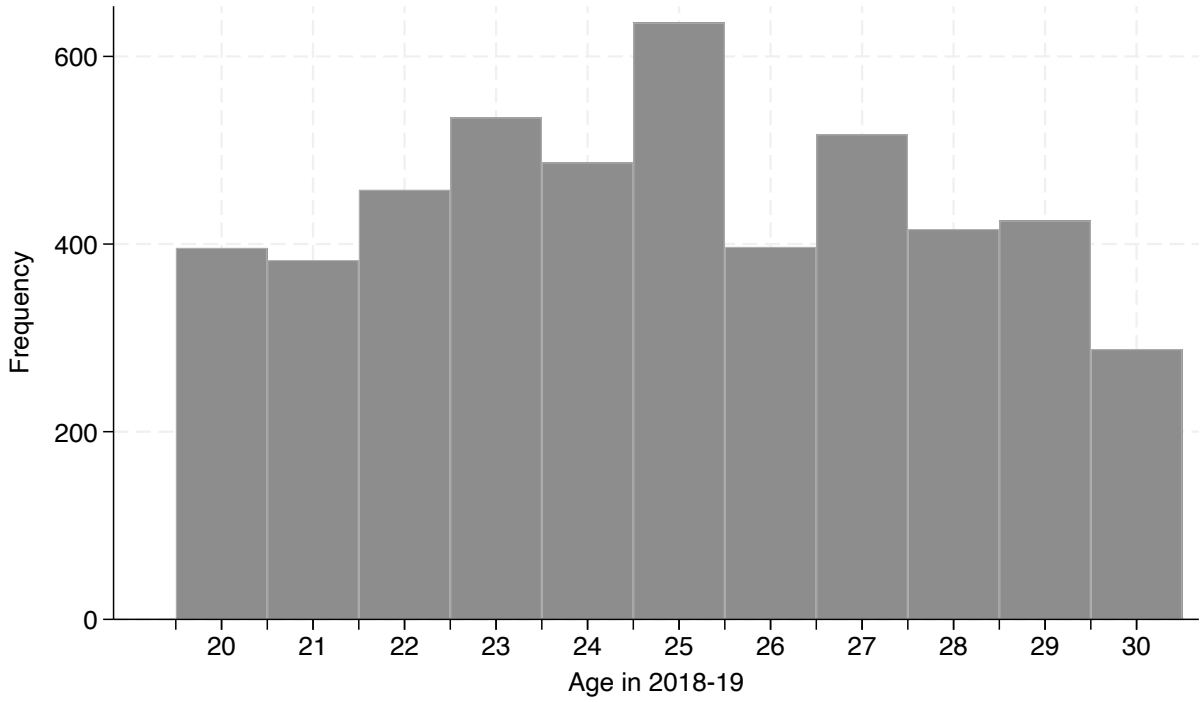
Notes: Panel (a): share of men and women of a given age who have engaged in an activity in the past month for which they have received a payment. Panel (b): share of men and women of a given age who are currently enrolled in school, including college and post-college education.

Figure A2: Timeline of LEAPS Data Collection

	2003–04	2004–05	2005–06	2006–07	2011–12	2018–19
Household Surveys	✓	✓	✓	✓	✓	✓
School Surveys	✓	✓	✓	✓	✓	–
Tested Cohort 1	Grade 3	Grade 4	Grade 5	Grade 6	–	–
Tested Cohort 2			Grade 3	Grade 4	–	–

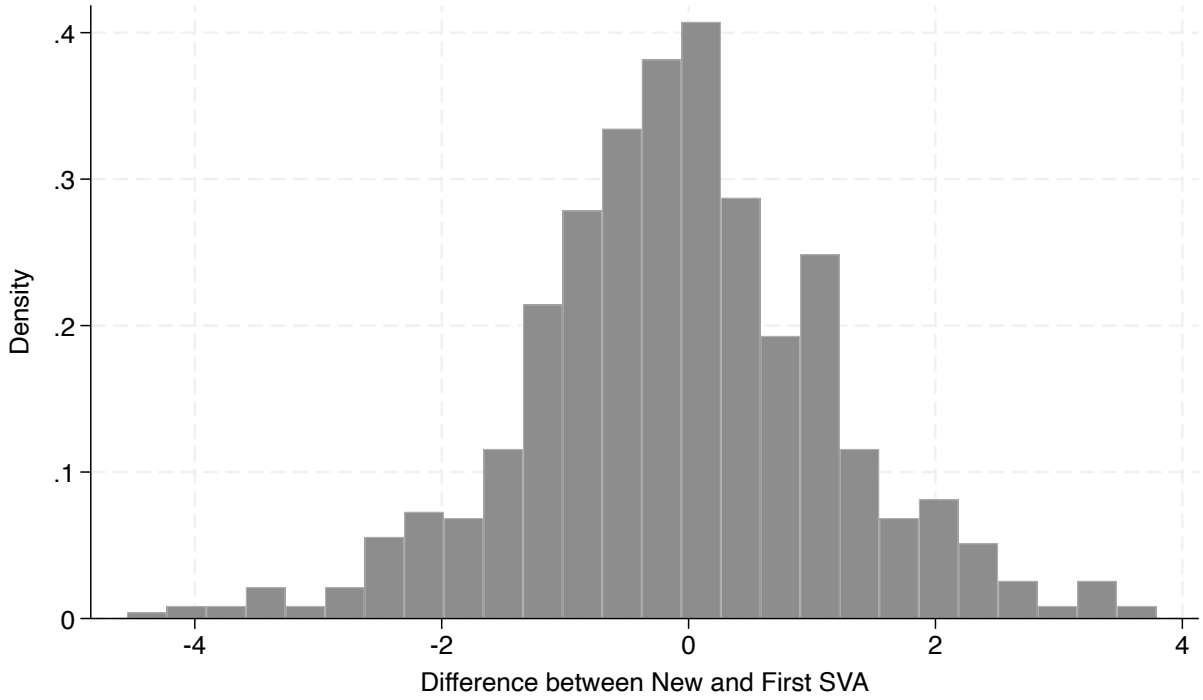
Notes: This figure displays the timeline of LEAPS data collection for household and school surveys between the first round in 2003–04 and the last round in 2018–19. Check marks (✓) mean data of a given type were collected, and a dash (−) means data were not collected.

Figure A3: Frequency of Individuals by Age in the 2018–19 Survey



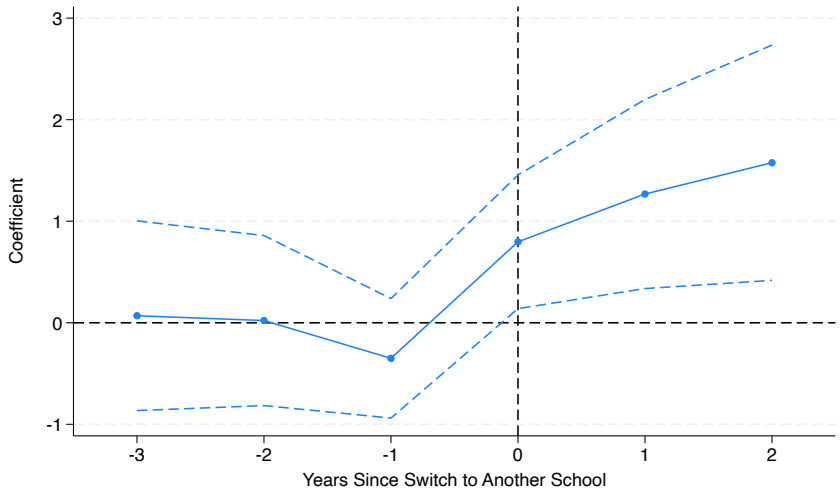
Notes: The figure displays a histogram showing the frequency of individuals who completed the 2018–19 long-term follow-up survey by age. The sampling frame consisted of individuals who reported an age between 5 and 15 years in 2003–04.

Figure A4: Histogram of SVA Differences for Switchers

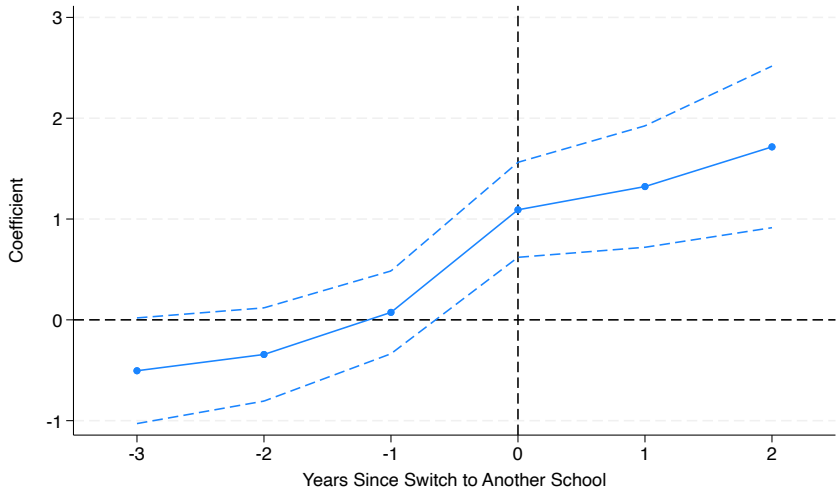


Notes: This figure is a histogram of the difference between the empirical Bayes SVA estimate of the new primary school switchers attend and the SVA of their previous primary school. SVA is measured in standard deviations of the VA distribution. The sample includes 727 switches from 563 switchers.

Figure A5: Relationship Between Change in School Quality and Test Score Trends by Switch Type



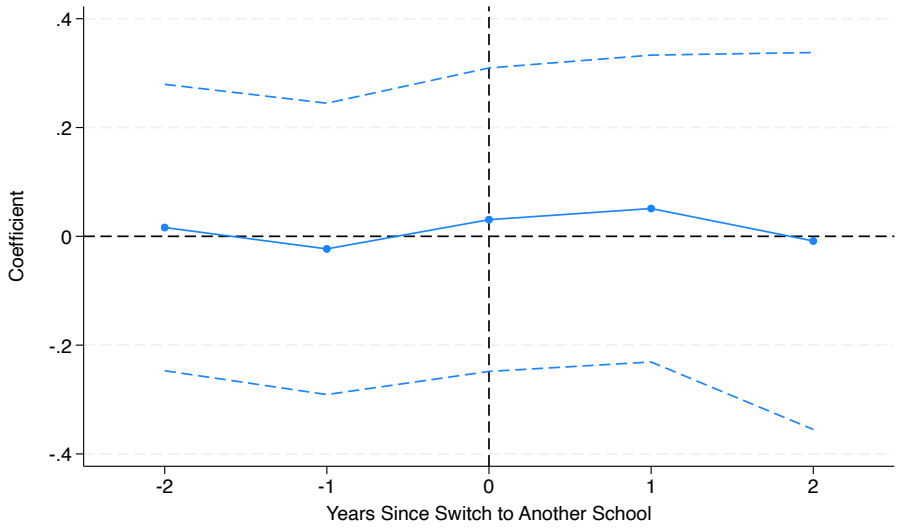
(a) Good Switches



(b) Bad Switches

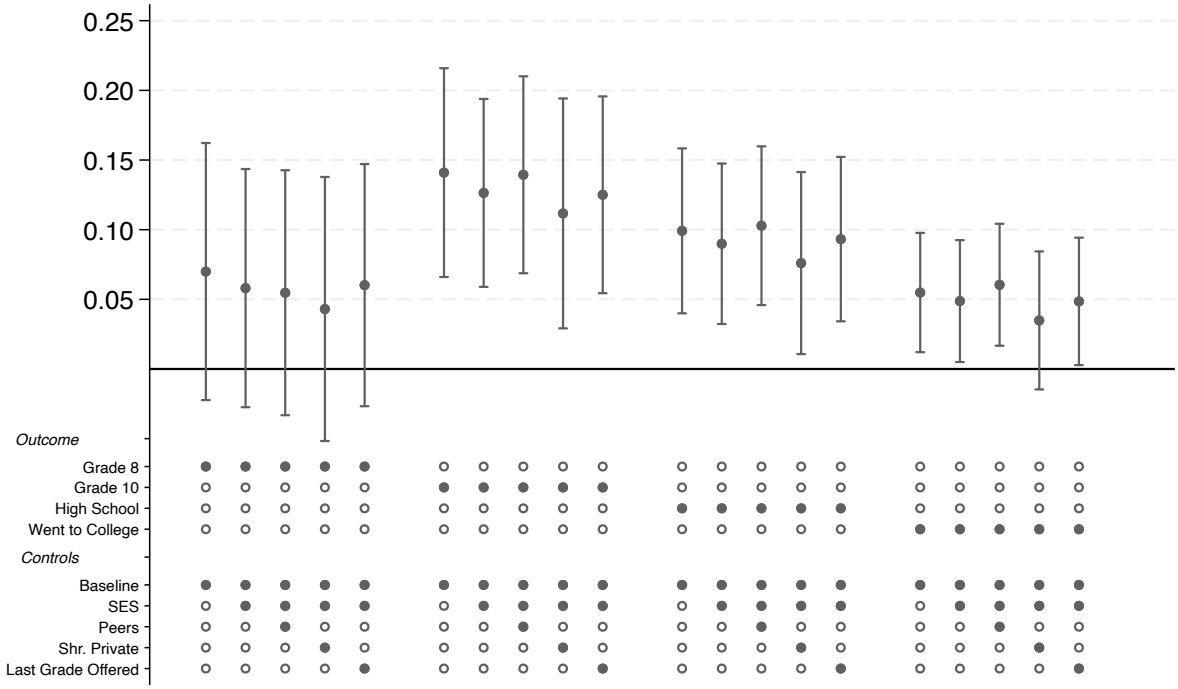
Notes: These figures report coefficients from regressions of mean test scores on indicator variables for event time prior to and after a school switch interacted with the change in mean empirical Bayes SVA. All regressions include age, gender, and first school-year-cohort fixed effects. Standard errors are clustered at the child-level (since households are not observed in the child data), and the figures display 95 percent level confidence intervals. Panel (a): sample only contains switches from a lower to a higher VA school. Panel (b): sample only contains switches from a higher to a lower VA school.

Figure A6: Relationship Between Change in School Quality and Trends in Household Assets



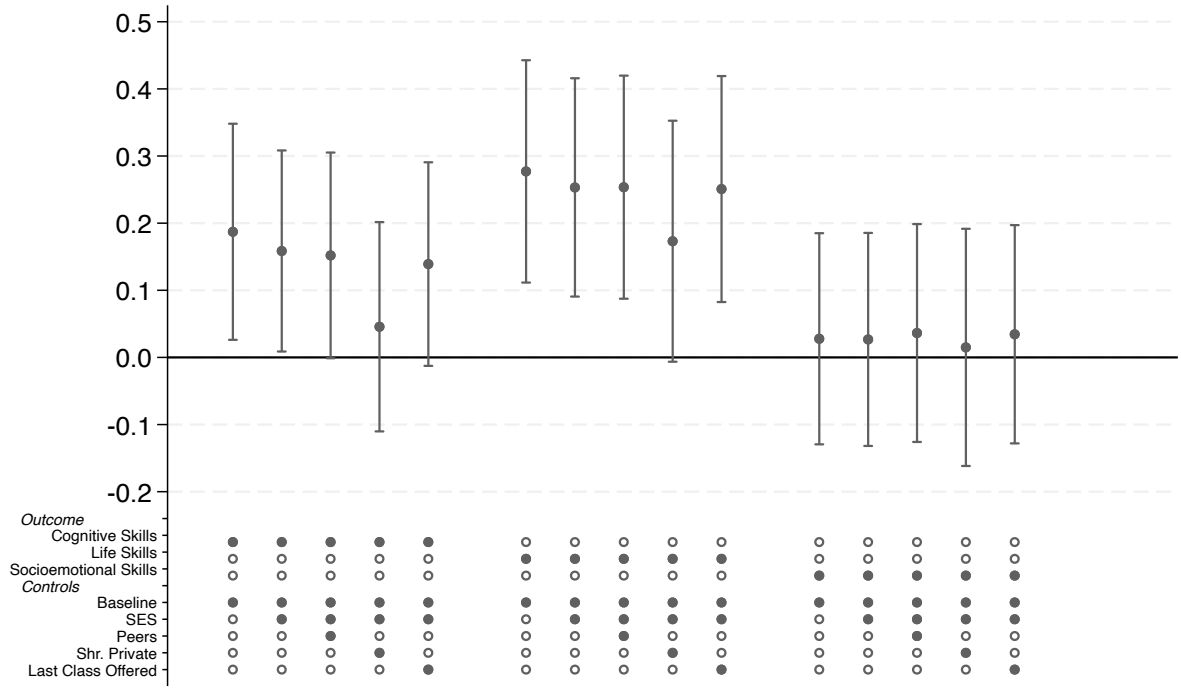
Notes: This figure plots coefficients from a regression of the asset index on indicator variables for years prior to and after a school switch interacted with the change in mean empirical Bayes SVA between the child's new and old school. *Assets* is an index of household assets using the first component of a principal component analysis. SVA is standardized by the school value-added distribution. The regression includes age, gender, year, and first school fixed effects. Standard errors are clustered at the household level. The figure displays 95% confidence intervals. Point estimates are reported in column 2 of Appendix Table A5.

Figure A7: Tests of Alternative Channels for Different Levels of Educational Attainment



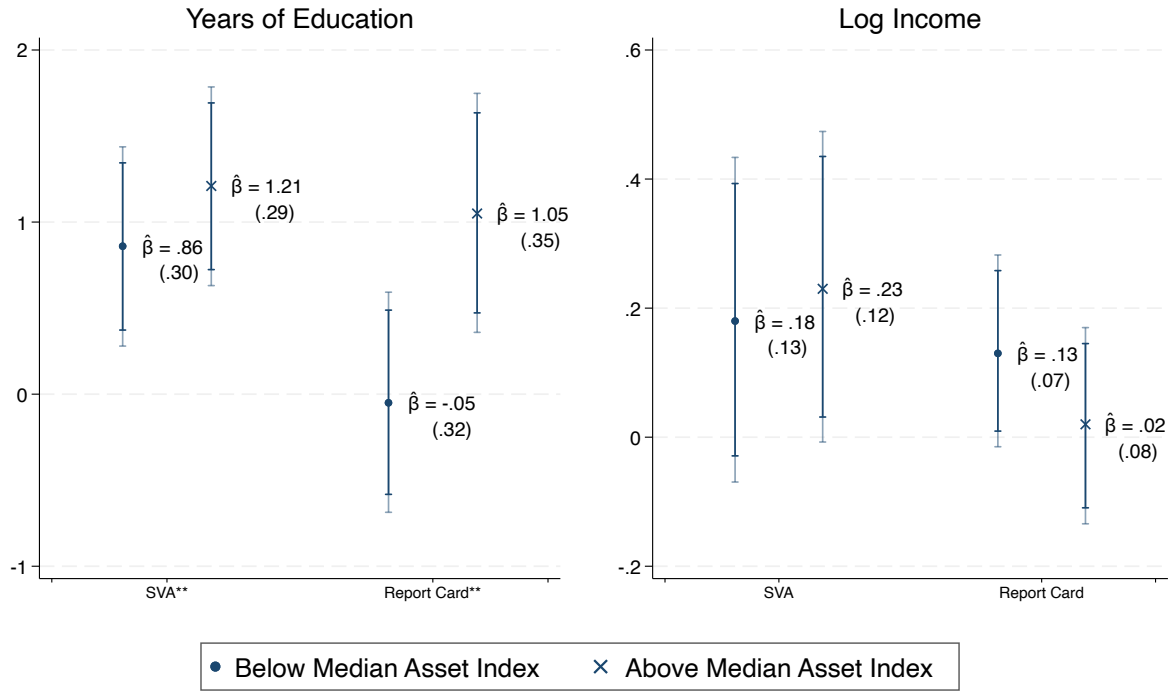
Notes: The graph plots the point estimates of the effect of empirical Bayes SVA on different measures of educational attainment, controlling for alternative channels. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. The first four rows of bullets displayed at the bottom of the graph indicate the outcome: indicator variables that equal 1 if the individual completed grade 8, grade 10, or high school, or if they attended any postsecondary institution (i.e., *Went to College*). Controls are displayed in the last five rows. Baseline controls include age fixed effects, a control for gender, and a fixed effect for the first primary school the individual attended. Socioeconomic status (SES) controls include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. *Peer quality* controls consist of three controls for the average mothers' and fathers' education and average assets of the peers in the school. *Share of private* is the share of time spent in a private primary school. *Last grade offered* is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). Standard errors are clustered at the household level, and 95 percent level confidence intervals are displayed.

Figure A8: Tests of Alternative Channels for Different Skills



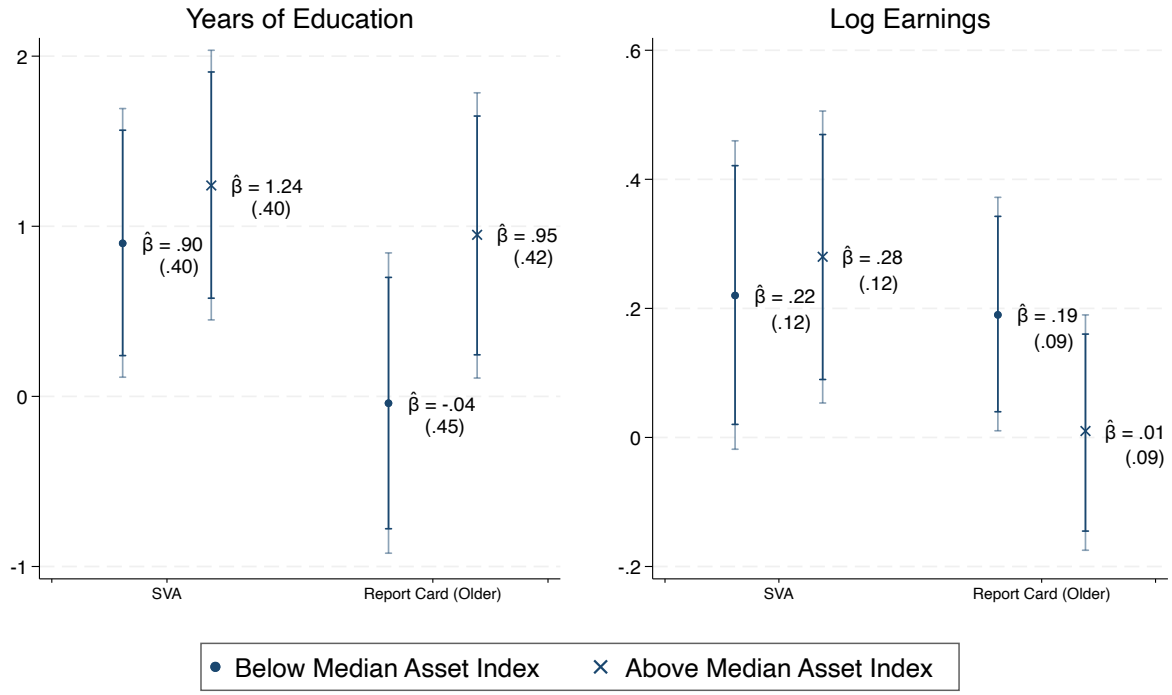
Notes: The graph plots the point estimates of the effect of empirical Bayes SVA on different skills measures, controlling for alternative channels. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. The first three rows of bullets displayed at the bottom of the graph indicate the outcome: *Cognitive Skills (2018)* is the average score from a test in math, Urdu, and English; *Life Skills* is a score measuring functional literacy and numeracy; and *Socioemotional Skills* is an index from Danon et al. (2024) measuring socioemotional skills. Controls are displayed in the last five rows. Baseline controls include age fixed effects and a control for gender. Socioeconomic status (SES) controls include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. *Peer quality* controls consist of three controls for the average mothers' and fathers' education and average assets of the peers in the school. *Share private* is the share of time spent in a private primary school. *Last grade offered* is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). All regressions include a fixed effect of the first primary school the individual attended. Standard errors are clustered at the household level, and 95 percent level confidence intervals are displayed.

Figure A9: Effects of SVA and Report Card on Years of Education and Earnings by SES



Notes: The dependent variable in the left panel is years of education and in the right panel is log monthly earnings. The figure plots heterogeneous effects of SVA and the report card intervention for those with above and below median asset indices. The asset index is the first principal component from a principal components analysis of indicator variables for household asset ownership in 2003. All SVA regressions include age and gender fixed effects and a fixed effect for first primary school. Standard errors are clustered at the household level. All report card regressions include district, gender, and age fixed effects, with standard errors clustered at the village level (108 clusters). ** and * denote that the coefficients are statistically significantly different at the 95 and 90 percent levels. Both 90 (dark line) and 95 (light line) percent confidence intervals are reported.

Figure A10: Effects of SVA and Report Card on Males' Years of Education and Earnings by SES



Notes: The dependent variable in the left panel is years of education and in the right panel is log monthly earnings. The figure plots heterogeneous effects of SVA and the report card intervention for those with above and below median asset indices. The asset index is the first principal component from a principal components analysis of indicator variables for household asset ownership in 2003. All SVA regressions include age fixed effects and a fixed effect for first primary school. Standard errors are clustered at the household level. All report card regressions include district and age fixed effects, with standard errors clustered at the village level (108 clusters). ** and * denote that the coefficients are statistically significantly different at the 95 and 90 percent levels. Both 90 (dark line) and 95 (light line) percent confidence intervals are reported.

Appendix Tables

Table A1: Summary Statistics

	Women		Men	
	(1) Mean	(2) SD	(3) Mean	(4) SD
	Baseline Characteristics			
High Caste	0.748	0.435	0.707	0.455
Assets	0.113	1.922	-0.128	1.840
Mom Has Some Education	0.255	0.436	0.221	0.415
Dad Has Some Education	0.591	0.492	0.540	0.499
	Characteristics in 2018–19			
Age	23.719	2.453	23.911	2.364
Years of Education	8.660	4.237	8.290	3.729
Enrolled	0.154	0.361	0.109	0.312
Went to College	0.142	0.349	0.097	0.295
Works	0.067	0.251	0.764	0.425
Public Sector Job	0.011	0.102	0.065	0.247
Informal Job	0.055	0.229	0.648	0.478
Monthly Earnings (USD)	6.075	24.678	113.020	208.772
Conditional Earnings (USD)	67.419	65.024	147.744	228.102
Ever Migrated	0.432	0.496	0.449	0.498
Is Married	0.473	0.499	0.256	0.437
Has Children	0.342	0.475	0.158	0.365
Observations	1426		1740	

Notes: This table shows the average characteristics of women and men in the school quality analysis sample. Standard deviations are reported in even columns. High caste, assets, and parental education are measured using the 2003 survey. All other variables are from the 2018–19 long-term follow-up survey. *Monthly Earnings* equals zero for individuals who do not work and *Conditional Earnings* is monthly earnings for those who work ($N = 96$ for women; and $N = 1,316$ for men).

Table A2: Descriptive Statistics for School Switches

Empirical Bayes School Value-Added	Mean
SVA Before Switch	-0.033
SVA After Switch	-0.123
Share of Switches by School Types	
Public to Public	0.502
Public to Private	0.140
Private to Public	0.226
Private to Private	0.132
Characteristics of Switches	
Sibling Event	0.816
Sibling Dropout	0.218
Sibling Starts School	0.324
Sibling Switched	0.836
School Closure	0.045
School Entry	0.051
Most Plausible Exogenous Switches	
Sib. Switches to Diff. Sch.	0.428
W/ Sib. Starting Sch. Level	0.528
Observations	727

Notes: The top panel reports the mean empirical Bayes SVA of the primary school attended before and after a switch. The two middle panels show the proportion of different types of switches in the data. *Sibling Event* is the percent of switches that involve either a sibling dropping, starting, or switching school. *Sibling Dropout*, *Sibling Starts School*, and *Sibling Switched* are the percent of sibling events when a sibling drops school, starts school, or switches school, respectively. *School Closure* and *Entry* are the percent of switches that involve a school closing or opening. The bottom panel shows the percent of switches classified as most plausibly exogenous. *Sibling Switches to Different Schools* are switches that involve a school closure or a sibling entering, dropping, or switching school, excluding switchers when their sibling(s) switch to the same school. *Siblings Starting School Level* are switches that involve a school closure or a sibling entering, or switching school in kachi (pre-primary), grade 1, middle school, or lower or upper secondary school.

Table A3: Characteristics of Primary School Switchers Relative to Stayers

	Assets	Mom Educ.	Dad Educ.	High Caste	Female	First Test Score
	(1)	(2)	(3)	(4)	(5)	(6)
Switched School	-0.058	0.012	0.011	0.032	-0.011	-0.018
	(0.109)	(0.025)	(0.028)	(0.025)	(0.023)	(0.095)
Mean Outcome	-0.011	0.239	0.623	0.726	0.451	-0.422
Observations	3087	3061	2784	3093	3093	926
Clusters	1330	1317	1199	1332	1332	750

Notes: This table regresses student characteristics on whether they switched schools. Assets is the first principal component analysis from household assets in 2003. All regressions include a fixed effect for the first school attended. Standard errors are clustered at the household level. Running a regression of switching on the characteristics in columns 1–5 and jointly testing if they are equal to 0 results in an F-statistic (p-value) of 0.900 (p=0.480) with 2,753 observations. If we include first test score, the sample size is 812 observations with an F-statistic = 0.29 (p=0.943).

Table A4: Relationship Between Student Characteristics and Quality of New School

	Dep. Var.: New School's SVA	
	(1) SES	(2) + First Test Scores
Female	-0.080	-0.087
	(0.158)	(0.168)
Assets	0.010	0.012
	(0.026)	(0.026)
High Caste	0.003	-0.005
	(0.140)	(0.137)
Mom Some Educ.	-0.019	-0.025
	(0.135)	(0.137)
Mom Educ. is Missing	0.438	0.375
	(0.320)	(0.294)
Dad Some Educ.	0.021	0.025
	(0.135)	(0.134)
Dad Died	0.201	0.221
	(0.167)	(0.183)
Dad is Living Elsewhere	0.456**	0.415*
	(0.210)	(0.222)
First Test Score		0.152
		(0.151)
First Score is Missing		0.048
		(0.147)
F-statistic	1.228	1.409
P-Value	0.283	0.176
Mean Outcome	-0.021	-0.021
Observations	406	406
Clusters	272	272

Notes: This table reports the association between the switchers' characteristics and the empirical Bayes SVA of the new primary school they attend. For students who switch multiple times, the outcome is the SVA of the first post-switch school. Assets is the first principal component from a principal component analysis of household assets in 2003. Three observations are missing for mother's education, and 11 percent of observations are missing for father's education. Because of the larger set of missings for fathers, we separate the missing indicator variable into indicator variables for missing because the father died and missing because the father is living elsewhere (these categories are exhaustive). Column 2 includes a control for first test score observed in the data, which is set to 0 when this information is missing and a control for missing values (75 percent). All regressions include a fixed effect for the first school attended. Standard errors are clustered at the household level. The reported sample is smaller than the 563 switchers because *reghdfe* reports the number of observations excluding singletons from the first-school fixed effect groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Coefficients on Relationship Between Change in School Quality and Trends in Test Scores and Assets for Switchers

	Mean Test Scores	**Assets**
	(1)	(2)
	Test Score Panel	Long-term Sample
Δ SVA $\times$ Period -3	0.0237	-0.0691
	(0.184)	(0.155)
Δ SVA $\times$ Period -2	0.0567	0.0162
	(0.165)	(0.136)
Δ SVA $\times$ Period -1	0.183	-0.0231
	(0.134)	(0.133)
Δ SVA $\times$ Period 0	1.110***	0.0306
	(0.150)	(0.134)
Δ SVA $\times$ Period 1	1.370***	0.0511
	(0.208)	(0.142)
Δ SVA $\times$ Period 2	1.680***	-0.00852
	(0.279)	(0.160)
P-Value Period 0 = 1	0.466	
Number of Observations	6346	2237

Notes: This table shows the estimates of event studies for school-switching, where the outcomes are mean test scores (column 1) and assets (column 2). The independent variables are the change in mean empirical Bayes SVA between the child's new and old school interacted with indicator variables for event time for school switching (where *Period 0* is the year of the switch). For students who switch multiple times, the SVA is the SVA of the first school into which they switch. *Assets* is an index of household assets using the first component of a principal component analysis and is weighted by the number of members in the household. The regression in column 1 includes age, gender, and cohort-first primary school-year fixed effects. The regression in column 2 includes age, gender, year, and first primary school fixed effects. Standard errors are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: The Effect of School Quality on Attrition

	Is Surveyed		Is Surveyed In-person	
	(1)	(2)	(3)	(4)
	First Sch. FE	+ SES	First Sch. FE	+ SES
Empirical Bayes SVA	-0.027	-0.025	0.009	0.016
	(0.027)	(0.025)	(0.029)	(0.028)
Mean Outcome	0.857	0.857	0.770	0.770
Adjusted R^2	0.308	0.324	0.209	0.218
Observations	3613	3608	3613	3608
Clusters	1533	1532	1533	1532

Notes: The dependent variable is an indicator variable that equals 1 if the individual was surveyed at all (columns 1–2) or surveyed in person (columns 3–4). The sample is all individuals aged 5 to 15 years old in 2003 who have a measure of SVA. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. All columns include age, gender, and first school fixed effects. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: The Effect of School Quality on Years of Education by Switch Type

	Years of Education	
	(1) First Sch. FE	(2) + SES
Empirical Bayes SVA × Good Switch	0.872*	0.752*
	(0.453)	(0.410)
Empirical Bayes SVA × Bad Switch	0.834*	0.857**
	(0.427)	(0.387)
Empirical Bayes SVA × No Switch	1.092***	0.920***
	(0.295)	(0.267)
P.-val. Equality Good vs. Bad Switch	0.933	0.803
Mean Outcome	8.473	8.473
Adjusted R^2	0.194	0.297
Observations	3093	3093
Clusters	1332	1332

Notes: This table estimates the effect of average SVA on years of education, allowing effects to differ by type of switch (or whether a switch occurred). *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. *Good Switch* is an indicator variable that equals 1 for individuals switching to a primary school with a higher SVA than the first school attended. *Bad Switch* is an indicator variable that equals 1 for individuals switching to a primary school with a lower SVA than the first school attended. *No Switch* is an indicator variable for individuals that remain in the same primary school for all the years we observe them. All columns include fixed effects for age, gender, and first school. SES controls in column 2 include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: The Effect of School Quality on Years of Education, Focusing on Most Plausibly Exogenous Switches

	First School Fixed Effect		Sibling Switches to Different Schools		Switchers with Sib. Starting Sch. Level	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES	(5) Baseline	(6) + SES
Empirical Bayes SVA	1.050***	0.904***	0.910*	0.844**	1.186**	0.915**
	(0.298)	(0.268)	(0.478)	(0.418)	(0.494)	(0.460)
Mean Outcome	8.473	8.473	8.445	8.445	8.469	8.469
Adjusted R^2	0.194	0.297	0.193	0.293	0.192	0.299
Observations	3093	3093	2732	2732	2792	2792
Clusters	1332	1332	1260	1260	1269	1269

Notes: This table estimates the effect of mean SVA on years of education, focusing on the most plausibly exogenous switches. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. Columns 1–2 show the results from the main empirical strategy (see columns 1–2 of Table 2). Columns 3–4 restrict the sample to non-switchers and switchers whose switches involve a school closure or a sibling entering, dropping, or switching school, excluding switchers when their siblings switch to the same school. Columns 5–6 restrict the sample to non-switchers and switchers whose switches involve a school closure or a sibling entering, or switching school in kachi (pre-primary), grade 1, middle school, or lower or upper secondary school. All columns include fixed effects for age, gender, and first primary school. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: The Effect of Type-Specific School Quality on Years of Education

	First Sch. FE	Gender SVA			First Sch. FE	SES SVA		
	(1)	(2) Own	(3) Other	(4) Both	(5)	(6) Own	(7) Other	(8) Both
Empirical Bayes SVA	1.216***				1.081***			
	(0.422)				(0.331)			
Own Gender Emp. Bayes SVA		1.087***		1.327***				
		(0.392)		(0.402)				
Other Gender Emp. Bayes SVA			0.118	0.683				
			(0.508)	(0.485)				
Own SES Emp. Bayes SVA						0.755***		0.862***
						(0.287)		(0.275)
Other SES Emp. Bayes SVA							0.553*	0.684**
							(0.284)	(0.267)
Mean Outcome	9.096	9.096	9.096	9.096	8.483	8.483	8.483	8.483
Adjusted R^2	0.252	0.252	0.245	0.253	0.196	0.195	0.194	0.197
Observations	1327	1327	1327	1327	3005	3005	3005	3005
Clusters	669	669	669	669	1302	1302	1302	1302

Notes: This table estimates the effects of type-specific mean SVA on years of education. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. For *Own Gender Emp. Bayes SVA*, the empirical Bayes SVA is calculated separately for women and men, and a student is assigned the SVA for his/her gender. *Other Gender Emp. Bayes SVA* is the SVA of the opposite gender. For *Own SES Emp. Bayes SVA*, we calculate SVA separately for students with above and below median baseline asset indices. *Own SES Emp. Bayes SVA* is then the SVA for a student of the same socioeconomic background, and *Other SES Emp. Bayes SVA* is the SVA for a student of the opposite background. All empirical Bayes SVA measures are standardized with respect to mean empirical Bayes SVA to allow for a consistent interpretation of the coefficients. All columns control for age, gender, and first school fixed effects. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Movers' Estimates of Effect of School Quality on Years of Education

	Baseline		× Exposure		Age of Switch × δ Assets		Sibling Switches to Different Schools		Switchers with Sib. Starting Sch. Level	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES	(5) Baseline	(6) + SES	(7) Baseline	(8) + SES	(9) Baseline	(10) + SES
Post-switch Emp. Bayes SVA	0.718***	0.614***					0.878**	0.949**	0.953**	0.760**
	(0.240)	(0.219)					(0.419)	(0.398)	(0.397)	(0.373)
Post-switch Emp. Bayes SVA × One Year			0.611**	0.537**	0.568**	0.494*				
			(0.282)	(0.255)	(0.281)	(0.255)				
Post-switch Emp. Bayes SVA × Two Years			0.783***	0.673**	0.816***	0.697**				
			(0.287)	(0.278)	(0.297)	(0.287)				
Post-switch Emp. Bayes SVA × Three Years			1.048**	0.879**	1.023**	0.808**				
			(0.434)	(0.401)	(0.425)	(0.387)				
Mean Outcome	8.571	8.571	8.571	8.571	8.571	8.571	8.624	8.624	8.673	8.673
Adjusted R^2	0.140	0.252	0.136	0.247	0.135	0.255	0.106	0.227	0.256	0.361
Observations	556	556	556	556	556	556	186	186	243	243
Clusters	395	395	395	395	395	395	153	153	161	161

Notes: The dependent variable is years of education. *Post-switch Emp. Bayes SVA* is the average empirical Bayes SVA of the primary school(s) attended after a switch. SVA is measured in standard deviations of the VA distribution. Baseline controls include the empirical Bayes SVA of the student's first school and fixed effects for gender, age, age at time of switch, and village. SES controls in even columns include indicator variables for whether the mother and the father have some education, whether the household head is from a high caste, and an index of baseline household assets (measured in 2003). Missing values are coded to 0, and controls for missing values are included. Columns 3–6 interact *Post-switch Emp. Bayes SVA* with indicator variables for the number of years the individual enrolled in a primary school after the switch and include number of years observed in primary school post-switch fixed effects. Columns 5–6 also include controls for age at the time of the switch fixed effects interacted with a measure of the change in the household's assets the year of the switch. Columns 7–10 are similar to 1–2, but restrict the sample to switchers whose switch involved a school closure or a sibling entering, dropping, or switching school, excluding switchers if their sibling switched to the same school. Columns 9–10 restrict the sample to switchers whose switches involved a school closure or a sibling starting or switching school in kachi (pre-primary), grade 1, middle school, lower or upper secondary school. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Effect of Report Card on Likelihood Observed in 2011 & 2018

	(1) Member Found	(2) Male	(3) Low Caste	(4) Child Has Mother With Some Education	(5) Child Has Father With Some Education	(6) Age	(7) Asset Index	(8) Child Highest Grade Completed
Panel A: 2011								
Report Card	0.031	0.059*	0.046	0.021	0.015	0.004	0.036	0.032
	(0.022)	(0.030)	(0.029)	(0.025)	(0.038)	(0.108)	(0.022)	(0.038)
Attribute		0.059**	0.010	-0.035	0.025	-0.005	0.012*	0.002
		(0.027)	(0.041)	(0.034)	(0.036)	(0.008)	(0.007)	(0.011)
Report Card × Attribute		-0.048	-0.051	0.020	0.007	0.003	-0.015	-0.000
		(0.033)	(0.055)	(0.047)	(0.047)	(0.011)	(0.011)	(0.014)
Control Mean	0.750	0.750	0.750	0.759	0.782	0.750	0.749	0.750
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.003	0.005	0.004	0.002	0.004	0.002	0.004	0.002
Observations	2547	2547	2544	2516	2240	2547	2542	2532
Clusters	112	112	112	112	112	112	112	112
Panel B: 2018								
Report Card	-0.010	0.001	-0.020	-0.021	-0.052	0.021	-0.015	-0.005
	(0.033)	(0.039)	(0.040)	(0.037)	(0.045)	(0.091)	(0.033)	(0.039)
Attribute		0.035	-0.037	-0.024	-0.022	-0.001	-0.007	0.012*
		(0.023)	(0.033)	(0.031)	(0.029)	(0.006)	(0.011)	(0.006)
Report Card × Attribute		-0.020	0.032	0.042	0.057	-0.004	0.017	-0.003
		(0.029)	(0.048)	(0.045)	(0.041)	(0.010)	(0.013)	(0.011)
Control Mean	0.851	0.851	0.851	0.853	0.868	0.851	0.851	0.851
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.018	0.018	0.018	0.019	0.016	0.017	0.019	0.018
Observations	2547	2547	2544	2516	2240	2547	2542	2532
Clusters	112	112	112	112	112	112	112	112

Notes: This table tests for differential attrition by treatment status (column 1), and then for differential attrition by treatment status interacted with observable baseline characteristics in columns 2–8 in 2011 (Panel A) and 2018 (Panel B). The dependent variable is an indicator variable equal to 1 if survey data were collected on an individual. Column 1 reports the coefficient on treatment. Columns 2–8 additionally include the baseline characteristic ("Attribute") and its interaction with treatment ("Report Card × Attribute"), where the attribute used appears in the column name. All specifications include district fixed effects and cluster standard errors at the village level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A12: Balance in the RCT Analysis Sample

Panel A: Balance in 2003

	(1) N	(2) Control Mean	(3) Control SD	(4) N	(5) Treatment Mean	(6) Treatment SD	(7) Regression Difference	(8) Difference SE	(9) Difference P-Value
Age in 2003 (Years)	1,271	9.037	1.341	1,276	9.011	1.341	-0.030	0.044	0.493
Male (%)	1,271	0.573	0.495	1,276	0.544	0.498	-0.029	0.020	0.151
Share Low Caste (%)	1,271	0.275	0.446	1,273	0.247	0.431	-0.027	0.037	0.477
Asset Index	1,269	0.253	2.039	1,273	0.292	2.092	0.050	0.156	0.750
Mother has Some Education (%)	1,253	0.254	0.435	1,263	0.271	0.445	0.018	0.031	0.556
Father has Some Education (%)	1,120	0.628	0.484	1,120	0.636	0.481	0.007	0.032	0.826
Child Highest Grade Completed in 2003	1,264	1.858	1.369	1,268	1.870	1.366	0.003	0.086	0.974

Panel B: Balance in 2011

	(1) N	(2) Control Mean	(3) Control SD	(4) N	(5) Treatment Mean	(6) Treatment SD	(7) Regression Difference	(8) Difference SE	(9) Difference P-Value
Age in 2003 (Years)	953	9.028	1.329	998	9.010	1.332	-0.022	0.048	0.657
Male (%)	953	0.592	0.492	998	0.547	0.498	-0.044	0.024	0.068
Share Low Caste (%)	953	0.276	0.447	998	0.236	0.425	-0.038	0.040	0.342
Asset Index	951	0.316	2.016	996	0.268	2.021	-0.038	0.146	0.794
Mother has Some Education (%)	951	0.245	0.430	992	0.266	0.442	0.021	0.031	0.504
Father has Some Education (%)	876	0.635	0.482	899	0.645	0.479	0.010	0.035	0.780
Child Highest Grade Completed in 2003	948	1.870	1.346	992	1.878	1.334	0.000	0.086	0.997

Panel C: Balance in 2018

	(1) N	(2) Control Mean	(3) Control SD	(4) N	(5) Treatment Mean	(6) Treatment SD	(7) Regression Difference	(8) Difference SE	(9) Difference P-Value
Age in 2003 (Years)	1,082	9.031	1.341	1,070	8.996	1.333	-0.035	0.046	0.443
Male (%)	1,082	0.582	0.493	1,070	0.549	0.498	-0.032	0.022	0.143
Share Low Caste (%)	1,082	0.268	0.443	1,067	0.246	0.431	-0.022	0.039	0.582
Asset Index	1,080	0.229	1.928	1,068	0.362	2.118	0.131	0.160	0.413
Mother has Some Education (%)	1,069	0.249	0.433	1,062	0.278	0.448	0.027	0.032	0.388
Father has Some Education (%)	972	0.622	0.485	952	0.645	0.479	0.021	0.032	0.512
Child Highest Grade Completed in 2003	1,076	1.871	1.369	1,063	1.881	1.389	0.006	0.090	0.948

Notes: This table reports the baseline covariate balance between treatment and control groups for the report card study sample, shown separately at baseline (2003) and for the subsets of respondents observed in 2011 (Panel B) and 2018 (Panel C). For each panel, columns 1–3 report sample size, mean, and standard deviation of the variable in the control group. Columns 4–6 report the sample size, mean, and standard deviation of the variable in the treated group. Column 7 reports the difference in treatment and control from a regression of each baseline covariate on treatment and district fixed effects, which is the stratifying variable. Column 8 reports the standard error for the regression, and column 9 reports the p-value of the difference. Standard errors are clustered at the village level. Panels A and B include 112 clusters, while Panel C includes 108 clusters.

Table A13: Effect of Report Card on Test Scores, Educational Attainment, and Labor Market Outcomes for the Placebo Group (13–15 in 2003)

	Test Scores	Educational Attainment				Labor Market		
	(1) Cognitive Skills 2018	(2) High School Diploma	(3) Went to College	(4) Is Enrolled	(5) Years of Schooling	(6) Works	(7) Earnings Levels	(8) Log Earnings
Report Card	0.011	-0.034	-0.027	-0.006	-0.163	-0.003	1.810	-0.089
	(0.073)	(0.038)	(0.036)	(0.012)	(0.386)	(0.021)	(8.494)	(0.097)
Control Mean	0.271	0.278	0.187	0.031	9.810	0.593	104.840	4.927
Control SD	0.770							
Treatment Effect wrt Control SD	0.014							
Adjusted R^2	0.011	0.022	0.019	0.006	0.034	0.606	0.316	0.070
Observations	545	650	650	650	650	650	649	378
Clusters	108	108	108	108	108	108	108	107

Notes: This table reports placebo estimates of the effect of the report card intervention on test scores, educational attainment, and labor market outcomes for individuals aged 13–15 in 2003. Cognitive skills are measured as the average score on tests in math, Urdu, and English, administered in 2018–19. Cognitive tests were only administered to individuals who completed in-person surveys. Earnings levels are monthly earnings (in 2018 USD). Works is defined as being engaged in a paid activity in the past month at the time of the survey. The "Treatment Effect wrt Control SD" row reports the treatment effect divided by the control-group standard deviation for the test-score outcome. All specifications include district, age, and gender fixed effects. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Randomization Inference for Report Card

	Full Sample					Older Cohort	
	(1) Cognitive Skills 2018	(2) High School Diploma	(3) Went to College	(4) Is Enrolled	(5) Years of Schooling	(6) Earnings Levels	(7) Log Earnings
Report Card	0.094*	0.049**	0.044**	0.043**	0.539*	10.733**	0.118**
	(0.055)	(0.024)	(0.021)	(0.017)	(0.270)	(5.184)	(0.059)
Control Mean	0.188	0.226	0.138	0.102	8.723	74.450	4.642
Randomization Inference p-value	0.097	0.050	0.042	0.013	0.050	0.040	0.049
Observations	1908	2152	2152	2152	2152	1320	682
Clusters	108	108	108	108	108	108	108

Notes: This table reports estimates of the report card intervention on test scores, educational attainment, and labor market outcomes in 2018. Column 1 reports the treatment effect on the average score on tests in math, Urdu, and English, measured in 2018–19. Cognitive tests were only administered to individuals who completed in-person surveys. Earnings levels are monthly earnings (in 2018 USD). The older cohort are those between ages 9 and 11 in 2003. All specifications include district, age, and gender fixed effects. Standard errors are clustered at the village level. The table reports randomization-inference p-values. Randomization-inference p-values are calculated by re-randomizing treatment assignment across villages within district strata and re-estimating the same specification using 10,000 re-randomizations. Stars are based on randomization-inference p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Effect of School Quality and Report Card on Job Type

	SVA		Report Card	
	(1) Public Sector Job	(2) Contract-based Job	(3) Public Sector Job	(4) Contract-based Job
Panel A: Full Sample				
Empirical Bayes SVA	0.016	0.014		
	(0.021)	(0.018)		
Report Card			0.012	0.015
			(0.015)	(0.016)
Mean Outcome	0.041	0.069		
Control Mean			0.058	0.093
Adjusted R^2	0.039	0.084	0.026	0.065
Observations	3090	3090	1326	1326
Clusters	1332	1332	108	108
Panel B: Men Only				
Empirical Bayes SVA	-0.003	0.013		
	(0.032)	(0.032)		
Report Card			0.023	0.029
			(0.024)	(0.026)
Mean Outcome	0.066	0.118		
Control Mean			0.086	0.140
Adjusted R^2	0.014	0.062	-0.000	0.043
Observations	1632	1632	773	773
Clusters	1028	1028	108	108

Notes: This table reports estimates of the effects of school quality and the report card intervention on job type. The dependent variables are indicators equal to 1 if the individual works in a public-sector job (columns 1 and 3) or in a contract-based job (columns 2 and 4). Panel A reports estimates for the full sample, and Panel B restricts the sample to men. Columns 1–2 report estimates for *Empirical Bayes SVA*, where SVA is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007; SVA is measured in standard deviations of the VA distribution. Columns 3–4 report effects of the report card intervention for the older cohort, defined as individuals aged 9–11 in 2003. All SVA regressions include age, first school, and (when relevant) gender fixed effects. Standard errors for SVA regressions are clustered at the household level. All report card regressions include district, age, and (when relevant) gender fixed effects. Standard errors for report card regressions are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Effect of School Quality and Report Card on Logged Earnings with Occupational, Task-Content, and Migration Controls

	SVA				Report Card			
	(1) Base	(2) Base + Occupation FE	(3) Base + Task–Content Controls	(4) Base + Migration FE	(5) Base	(6) Base + Occupation FE	(7) Base + Task–Content Controls	(8) Base + Migration FE
Panel A: Full Sample								
Empirical Bayes SVA	0.224*	0.213*	0.317**	0.226**				
	(0.118)	(0.119)	(0.126)	(0.106)				
Report Card					0.118**	0.152***	0.167**	0.098*
					(0.059)	(0.056)	(0.068)	(0.053)
Mean Outcome	4.553	4.552	4.624	4.553				
Control Mean					4.642	4.636	4.667	4.642
Adjusted R^2	0.282	0.362	0.167	0.418	0.193	0.308	0.128	0.359
Observations	1364	1348	1095	1364	682	669	550	682
Clusters	899	891	771	899	108	108	107	108
Panel B: Men Only								
Empirical Bayes SVA	0.250**	0.231*	0.323**	0.249**				
	(0.118)	(0.121)	(0.130)	(0.107)				
Report Card					0.115*	0.161***	0.160**	0.101*
					(0.061)	(0.061)	(0.072)	(0.057)
Mean Outcome	4.724	4.726	4.674	4.724				
Control Mean					4.784	4.779	4.707	4.784
Adjusted R^2	0.041	0.117	0.101	0.245	0.015	0.103	0.097	0.250
Observations	1193	1176	1024	1193	605	593	508	605
Clusters	822	813	736	822	108	108	107	108

Notes: This table reports estimates of the effects of school quality and the report card on log monthly earnings, including alternative controls for potential mediating channels. The dependent variable is the log of monthly earnings among individuals with positive earnings. Panel A reports estimates for the full sample, and Panel B restricts the sample to men. Columns 1–4 report estimates for *Empirical Bayes SVA*, where SVA is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. Columns 5–8 report effects of the report card intervention for the older cohort, defined as those between ages 9–11 in 2003. Columns 1 and 5 are the baseline specifications; columns 2 and 6 add occupation fixed effects; columns 3 and 7 add task-content controls; and columns 4 and 8 add migration fixed effects. Questions on task-content controls were administered only to those who completed a direct interview, in person or by phone, and had engaged in a paid activity in the past month. All SVA regressions include age, first school, and (when relevant) gender fixed effects. Standard errors for SVA regressions are clustered at the household level. All report card regressions include district, age, and (when relevant) gender fixed effects. Standard errors for report card regressions are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Tests of Alternative Channels

	Years of Education				Log Earnings			
	(1) First Sch. FE	(2) Peer Quality	(3) Share Private	(4) Last Grade	(5) First Sch. FE	(6) Peer Quality	(7) Share Private	(8) Last Grade
Empirical Bayes SVA	1.050***	1.124***	0.700**	1.054***	0.224*	0.231*	0.170	0.221*
	(0.298)	(0.309)	(0.327)	(0.307)	(0.118)	(0.121)	(0.160)	(0.122)
Peers' Mom Educ.		-1.489				-0.842*		
		(0.977)				(0.463)		
Peers' Dad Educ.		2.042**				0.836*		
		(1.030)				(0.476)		
Peers' Assets		-0.365				-0.046		
		(0.254)				(0.081)		
Time Share in Priv.			1.464**				0.242	
			(0.642)				(0.330)	
Offers at least some Middle School				0.050				-0.187
				(0.343)				(0.118)
Offers at least some Matric				0.339				-0.435***
				(0.380)				(0.154)
Offers at least some Higher Secondary				-0.002				-0.038
				(0.655)				(0.284)
Mean Outcome	8.473	8.473	8.473	8.476	4.553	4.553	4.553	4.554
Adjusted R^2	0.194	0.196	0.195	0.194	0.282	0.285	0.282	0.288
Observations	3093	3093	3093	3086	1364	1364	1364	1359
Clusters	1332	1332	1332	1329	899	899	899	896

Notes: The dependent variable is years of education in columns 1 to 4 and log earnings in columns 5 to 8. *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. *Peer quality* includes controls for average fathers' and mothers' education and average assets of the peers in the school. *Share private school* is the share of time spent in a private primary school. *Last grade offered* is a control for the highest grade offered by the last primary school the individual attended. We group the last grade offered into four categories: the school offers at least some primary levels (grades 1 to 5, the omitted category), middle school (grades 6 to 8), Matric (i.e., grades 9 and 10), and higher secondary (grades 11 and 12). All columns include age, gender, and first school fixed effects. Standard errors are clustered at the household level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Benefit-Cost Ratio, IRR, and MVPF (Males Only)

	(1) No Wage Growth	(2) Informal Sector Wage Growth	(3) Average of Formal and Informal Sector	(4) Formal Sector Wage Growth
Panel A: Costs				
Direct Cost of Report Card	3.00	3.00	3.00	3.00
Additional Private Schooling Costs	11.69	11.69	11.69	11.69
Additional Public Schooling Costs	82.62	82.62	82.62	82.62
Additional Household Education Expenditure	115.04	115.04	115.04	115.04
Lost Earnings from 2003 to Age 25 (Baseline Ages 7-11)	369.22	369.22	369.22	369.22
Lost Taxes from 2003 to Age 25 (Baseline Ages 7-11)	27.69	27.69	27.69	27.69
Panel B: Benefits				
Additional Earnings between Ages 26–60	1567.00	1727.11	1865.63	2004.15
Gained Taxes between Ages 26–60	117.53	129.53	139.92	150.31
Panel C: Cost-Effectiveness Metrics				
Net Cost to Government	-4.22	-16.23	-26.61	-37.00
Total Benefits	1567.00	1727.11	1865.63	2004.15
Total Costs	581.57	581.57	581.57	581.57
Benefit-Cost Ratio	2.69	2.97	3.21	3.45
Internal Rate of Return (%)	12.09	12.68	13.08	13.44
MVPF	∞	∞	∞	∞

Notes: This table reports benefit-cost calculations for the male sample under alternative age-earnings profile assumptions. Column 1 applies no age-earnings profile scaling and assumes a constant earnings effect. Columns 2 and 4 scale the treatment effect using male age-earnings profiles for the informal and formal sectors, respectively. Column 3 averages the formal- and informal-sector profile-scaled earnings gains. Monetary values are expressed in 2018 USD discounted to 2003 using a standard discount factor of 0.95. Panel A reports costs; Panel B reports benefits and fiscal gains; and Panel C reports the key cost-effectiveness metrics. Except for the direct report card cost and fiscal tax rows, cost and benefit components are constructed as individual-level present discounted values and then estimated as the coefficient on the report card treatment in regressions controlling for district and age fixed effects, with standard errors clustered at the village level (108 clusters). Lost Taxes and Gained Taxes are fiscal externalities used in the net-government-cost and MVPF calculations; they are not included in Total Costs or Total Benefits. The tax calculations in Panels A and B assume a 15 percent GST rate and 50 percent compliance. Net Cost to Government equals the direct report card cost plus lost taxes plus additional public schooling costs minus gained taxes. Total Benefits equal the additional earnings gains for treated men between ages 26 and 60. Total Costs sum the direct report card cost, additional private and public schooling costs, additional household education expenditure, and lost earnings from 2003 through age 25. The Benefit-Cost ratio is the ratio of Total Benefits to Total Costs. The MVPF numerator, willingness to pay, equals additional after-tax earnings net of additional private schooling costs, additional household education expenditures, and after-tax earnings foregone while participants remain in school. The MVPF equals willingness to pay divided by the net cost to the government; because the net government cost is negative in all columns, the MVPF is reported as infinity. The Internal Rate of Return is computed from the annual undiscounted net benefit stream. For further details on the calculations, refer to Appendix C

Table A19: Benefit-Cost Ratio, IRR, and MVPF (Full Sample)

	(1) No Wage Growth	(2) Informal Sector Wage Growth	(3) Average of Formal And Informal Sector	(4) Formal Sector Wage Growth
Panel A: Costs				
Direct Cost of Report Card	3.00	3.00	3.00	3.00
Additional Private Schooling Costs	12.69	12.69	12.69	12.69
Additional Public Schooling Costs	73.06	73.06	73.06	73.06
Additional Household Education Expenditure	94.76	94.76	94.76	94.76
Lost Earnings from 2003 to Age 25 (Baseline Ages 7-11)	220.82	220.82	220.82	220.82
Lost Taxes from 2003 to Age 25 (Baseline Ages 7-11)	16.56	16.56	16.56	16.56
Panel B: Benefits				
Additional Earnings (Ages 26–60 for Men; Ages 26-30 for Women)	868.51	955.61	1030.52	1105.42
Gained Taxes (Ages 26–60 for Men; Ages 26-30 for Women)	65.14	71.67	77.29	82.91
Panel C: Cost-Effectiveness Metrics				
Net Cost to Government	27.49	20.95	15.34	9.72
Total Benefits	868.51	955.61	1030.52	1105.42
Total Costs	404.33	404.33	404.33	404.33
Benefit-Cost Ratio	2.15	2.36	2.55	2.73
Internal Rate of Return (%)	10.28	10.85	11.23	11.59
MVPF	17.89	27.31	41.83	73.14

Notes: This table reports benefit-cost calculations for the full sample under alternative age-earnings profile assumptions. Column 1 applies no age-earnings profile scaling and assumes a constant earnings effect. Columns 2 and 4 scale the treatment effect using gender-specific age-earnings profiles for the informal and formal sectors, respectively. Column 3 averages the formal- and informal-sector profile-scaled earnings gains. Monetary values are expressed in 2018 USD discounted to 2003 using a standard discount factor of 0.95. Panel A reports costs; Panel B reports benefits and fiscal gains; and Panel C reports the key cost-effectiveness metrics. Except for the direct report card cost and fiscal tax rows, cost and benefit components are constructed as individual-level present discounted values and then estimated as the coefficient on the report card treatment in regressions controlling for district, age, and gender fixed effects, with standard errors clustered at the village level (108 clusters). Lost Taxes and Gained Taxes are fiscal externalities used in the net-government-cost and MVPF calculations; they are not included in Total Costs or Total Benefits. The tax calculations in Panels A and B assume a 15 percent GST rate and 50 percent compliance. Net Cost to Government equals the direct report card cost plus lost taxes plus additional public schooling costs minus gained taxes. Total Benefits equal the additional earnings gains for treated men between ages 26 and 60 and treated women between ages 26 and 30. Total Costs sum the direct report card cost, additional private and public schooling costs, additional household education expenditure, and lost earnings from 2003 through age 25. The Benefit-Cost ratio is the ratio of Total Benefits to Total Costs. The MVPF numerator, willingness to pay, equals additional after-tax earnings net of additional private schooling costs, additional household education expenditures, and after-tax earnings foregone while participants remain in school. The MVPF equals willingness to pay divided by net cost to the government. The Internal Rate of Return is computed from the annual undiscounted net benefit stream. For further details on the calculations, refer to Appendix C

Table A20: Benefit-Cost Ratio, IRR, and MVPF (Low SES Men)

	(1) No Wage Growth	(2) Informal Sector Wage Growth	(3) Average of Formal And Informal Sector	(4) Formal Sector Wage Growth
Panel A: Costs				
Direct Cost of Report Card	3.00	3.00	3.00	3.00
Additional Private Schooling Costs	14.16	14.16	14.16	14.16
Additional Public Schooling Costs	4.22	4.22	4.22	4.22
Additional Household Education Expenditure	26.84	26.84	26.84	26.84
Lost Earnings from 2003 to Age 25 (Baseline Ages 7-11)	181.74	181.74	181.74	181.74
Lost Taxes from 2003 to Age 25 (Baseline Ages 7-11)	13.63	13.63	13.63	13.63
Panel B: Benefits				
Additional Earnings between Ages 26–60	1947.77	2146.78	2318.96	2491.14
Gained Taxes between Ages 26–60	146.08	161.01	173.92	186.84
Panel C: Cost-Effectiveness Metrics				
Net Cost to Government	-125.23	-140.15	-153.07	-165.98
Total Benefits	1947.77	2146.78	2318.96	2491.14
Total Costs	229.97	229.97	229.97	229.97
Benefit-Cost Ratio	8.47	9.33	10.08	10.83
Internal Rate of Return (%)	22.84	23.41	23.76	24.09
MVPF	∞	∞	∞	∞

Notes: This table reports benefit-cost calculations for the subsample of men who come from households below the median asset index at baseline under alternative age-earnings profile assumptions. Column 1 applies no age-earnings profile scaling and assumes a constant earnings effect. Columns 2 and 4 scale the treatment effect using male age-earnings profiles for the informal and formal sectors, respectively. Column 3 averages the formal- and informal-sector profile-scaled earnings gains. Monetary values are expressed in 2018 USD discounted to 2003 using a standard discount factor of 0.95. Panel A reports costs; Panel B reports benefits and fiscal gains; and Panel C reports the key cost-effectiveness metrics. Except for the direct report card cost and fiscal tax rows, cost and benefit components are constructed as individual-level present discounted values and then estimated as the coefficient on the report card treatment in regressions controlling for district and age fixed effects, with standard errors clustered at the village level (108 clusters). Lost Taxes and Gained Taxes are fiscal externalities used in the net-government-cost and MVPF calculations; they are not included in Total Costs or Total Benefits. The tax calculations in Panels A and B assume a 15 percent GST rate and 50 percent compliance. Net Cost to Government equals the direct report card cost plus lost taxes plus additional public schooling costs minus gained taxes. Total Benefits equal the additional earnings gains for treated men between ages 26 and 60. Total Costs sum the direct report card cost, additional private and public schooling costs, additional household education expenditure, and lost earnings from 2003 through age 25. The Benefit-Cost ratio is the ratio of Total Benefits to Total Costs. The MVPF numerator, willingness to pay, equals additional after-tax earnings net of additional private schooling costs, additional household education expenditures, and after-tax earnings foregone while participants remain in school. The MVPF equals willingness to pay divided by the net cost to the government; because the net government cost is negative in all columns, the MVPF is reported as infinity. The Internal Rate of Return is computed from the annual undiscounted net benefit stream. For further details on the calculations, refer to Appendix C

B IV-Lasso Identification Strategy

This section describes an additional IV-lasso identification strategy to measure the effects of school quality on years of education. The strategy delivers similar results.

B.1 Lasso 2SLS Empirical Strategy

We complement the switcher identification strategy with a strategy that exploits cross-sectional variation in access to higher quality schools as opposed to longitudinal variation in the schools children attended. We exploit the fact that school choice is extremely distance sensitive (Bau, 2022; Carneiro et al., 2024). Hence, children who are closer to good schools will be more likely to attend good schools all things equal. While school locations are not exogenous — in particular, we would expect private schools (which are higher quality on average) to locate closer to richer households — Andrabi et al. (2025) argue that controlling for distance to the center of the village accounts for the socioeconomic variation in household locations. This argument relies on the fact that the villages we study were originally developed as planned communities by British colonialists, with richer landowners placed in the middle of the village and poorer workers living on the outskirts. The identification therefore relies on comparing two households that are equally distant from the center but where one happens (for example) to be on the same side of the village as a low quality school, while the other is relatively farther from that school.

To maximize power and choose distance instruments in a principled way, we follow the lasso IV strategy of Belloni et al. (2012) and use a lasso regression to select the instruments that are most predictive of $\frac{1}{n^t}\sum_{t\in[2003,2007]}\widehat{SVA}_{st}$. For our pool of potential instruments, we calculate the average SVA of schools within 500 meters of a student, 600 meters, 700 meters, and so on up to 2 km. Since some students do not have any schools within a given radius, we also forcibly include controls for missing values for the potential instruments and code missings as zeros. The new second stage is identical to equation (2) except that it includes controls for village instead of first school fixed effects, age by gender fixed effects, and distance to the village center and its interaction with gender. This choice of controls adheres to the controls in the related distance IV specifications used to estimate the contemporaneous private school premium for test scores in Andrabi et al. (2025).

In addition to reporting the lasso IV results, we use the same instruments chosen by the lasso regression and report results from a two-sample IV strategy (Inoue and Solon, 2010). The two-sample IV allows us to use a larger sample (all children for whom we have distance data and school choice data regardless of whether they appear in the long-term data) to estimate the first stage, increasing power.

B.2 Results

The first column of Table B2 reports the selected instrument and the F-statistic of the first stage. Appendix Table B1 reports the associations between the instruments and different measures of household socioeconomic status, conditional on distance to the center of the village. Each column reports an F-statistic which tests if the instruments are jointly related to a given covariate. The joint tests show that there is no relationship between the selected instruments and the measures of household socioeconomic status, supporting the exclusion restriction. Column 2 of Table B2 reports the estimated effect. While underpowered relative to the switcher identification strategy, the results are qualitatively similar. Here, attending a 1 sd better school increases years of education by 1.7 years. While we note that the local average treatment effect for this strategy could differ from the average treatment effect estimated with the switchers strategy, we do observe that our switcher estimate is within the confidence interval of the distance IV strategy. In columns 3 and 4, we re-estimate the IV strategy using a two-sample IV strategy (Inoue and Solon, 2010). The two-sample IV strategy allows us to use all children for whom we have distance and school quality information in the first stage, even if we do not observe their long-run outcomes. The effect of average school quality is now more precisely estimated. Otherwise, the results are broadly similar (with an estimated effect of 1.8 years of education). Reassuringly, an IV strategy using orthogonal variation to our main strategy arrives at similar results.

Table B1: Correlations Between Selected Instruments and Student Characteristics

	(1) Assets	(2) High Caste	(3) Mom Educ.	(4) Dad Educ.	(5) Printed Media	(6) Land Area	(7) High Ability	(8) Household Size
Avg. SVA within 1.2km	0.147	-0.036	0.013	-0.036	-0.008	0.501	0.039	0.279
	(0.299)	(0.066)	(0.043)	(0.075)	(0.033)	(1.175)	(0.035)	(0.513)
Avg. SVA within 1.7km	-0.034	0.043	0.002	0.058	0.041	-1.111	0.027	-0.988
	(0.289)	(0.059)	(0.042)	(0.082)	(0.038)	(1.194)	(0.052)	(0.741)
Mean Outcome	-0.019	0.726	0.237	0.563	0.091	2.305	0.272	8.774
F-Statistic	0.236	0.315	0.047	0.259	0.614	1.291	1.514	0.900
Observations	3166	3166	3166	3166	3166	3163	3133	3166
Clusters	108	108	108	108	108	108	108	108

Notes: This table reports the correlations between the selected instruments from the lasso instrumental variables strategy and individuals' characteristics. The outcomes are constructed using data from the 2003–04 household survey. All regressions include controls for whether the average empirical Bayes SVA within the two different radii is missing, and village, age, and gender fixed effects, as well as distance to the village center and its interaction with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: IV Estimates of the Effect of School Quality on Years of Education

	Lasso IV		TS2SLS Lasso IV	
	(1)	(2)	(3)	(4)
	First Stage	Years of Education	First Stage	Years of Education
Avg. SVA within 1.2 km	0.172***		0.186**	
	(0.061)		(0.073)	
Avg. SVA within 1.7 km	0.403***		0.345***	
	(0.108)		(0.116)	
Empirical Bayes SVA		1.701*		1.847***
		(0.904)		(0.629)
F-statistic	13.990		13.367	
Observations	3166	3166	3364	
Observations 1st Stage				3365
Observations 2nd Stage				3152

Notes: Columns 1–2 report the first and second stages of the baseline IV strategy, and columns 3–4 use a two-sample IV strategy following Inoue and Solon (2010). *Empirical Bayes SVA* is the average empirical Bayes SVA of the primary schools attended between 2003 and 2007. SVA is measured in standard deviations of the VA distribution. The potential instruments are the average SVA of schools in different radii ranging between 500 meters and 2 kilometers in increments of 100 meters. The instruments are selected using the lasso IV strategy. Missing indicator variables are included for all potential instruments. All regressions include controls for village fixed effects, age-by-gender fixed effects, distance to the village center, and its interaction with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Details of Cost-Effectiveness Calculations

We focus on two measures of cost-effectiveness: the social benefit-cost ratio and the MVPF. The social benefit-cost ratio is given by $\frac{Social\ Benefit}{Social\ Cost}$, while the MVPF is given by $\frac{WTP}{Net\ Govt\ Cost}$, where WTP is the willingness to pay of beneficiaries and $Net\ Govt\ Cost$ is the PDV of government costs net of the increase in tax revenues. If the net cost to the government is negative, the MVPF is said to be infinity.

All benefits and costs are calculated in 2018 USD using World Bank CPI estimates and discounted to 2003 (the year we began testing the intervention) using a standard discount factor of 0.95. Children are assumed to potentially be in school up to age 25 and to always be out of school from age 26 onward.

C.1 Social Costs

We calculate five types of social costs: (i) the direct cost of the report card intervention per student, (ii) the additional cost of expenditures on private school (potentially both due to school switching and spending more time in school overall), (iii) the additional cost of public school (to the government), (iv) additional household educational expenditures besides school fees, and (v) the opportunity cost of staying in school. To estimate the effects of the report card intervention on social costs, we calculate the total present discounted value (PDV) of each cost type, excluding the direct cost of the intervention for each individual in our data. We then regress this object on treatment status, conditioning on the same set of controls (age and district fixed effects for the men-only sample and additional gender fixed effects in the full sample) as in our main regression specifications. The treatment coefficient is then the effect of the intervention on the PDV of costs in that category. The direct cost of the intervention is straightforwardly calculated by dividing the total cost of testing and distributing the report card by the number of beneficiaries. We obtain a direct cost of the intervention of USD 3 per child.

Calculating the individual-level PDVs of costs requires first generating a dataset of costs for each year from when children are first observed in 2003 through age 25 and taking the discounted sum of these annual costs. This is relatively straightforward for the years in which we collected survey data. For years we did not collect data, we impute whether a child was enrolled in school and what type of school they attended. To construct the enrollment variable, we first identify the latest pre-2018 survey wave in which the child is observed to be enrolled and has non-missing information on completed schooling and current grade-equivalent schooling, prioritizing 2011, then 2006, 2005, 2004, and 2003. We compare the child's grade-equivalent schooling in this wave with schooling observed in 2018 to infer

the number of additional years of schooling completed between the waves, setting negative implied increments to zero. For children no longer enrolled in 2018, we combine baseline completed schooling with this additional increment to construct a continuous enrollment spell. This assumes that schooling progresses without repeated grades during the imputed spell and that dropout is absorbing. For children still enrolled in 2018, we assume they were enrolled for the entire period from 2003 through age 25. This assumption is conservative since it increases costs without increasing benefits. To test the validity of the imputation, we compare the imputed enrollment variable with observed enrollment in survey years. Across 11,781 observed child-year records, the imputation correctly predicts enrollment status in 91.47 percent of cases. The remaining cases are unevenly split between false negatives (6.77 percent) and false positives (1.76 percent) in the full sample. However, the prediction error is not differential by treatment status; the error rate is 8.47 percent in the control group and 8.59 percent in the treatment group.

We next assign the school type for each year the child is enrolled. We classify government schools as public and group the small number of NGO schools and madrassas with private schools. We use the child's observed school type in the LEAPS survey years (2003–06 and 2011). When the school type is missing in one of these survey years for a child who has been imputed to be enrolled, we fill the missing survey-year school type using the child's own observed school type in other LEAPS survey waves. We infer that a child's school type does not change from 2011 onward (e.g., if they were in public school in middle school, they did not switch to private school in high school). For cases between 2007 and 2010, if a child is in the same type of school in both 2006 and 2011, we infer that they were in that type in the intermediate years. If the school type is available for only one of the two years, we use the available one. When the type differs, we infer that the student is in public school. This is to be conservative since public schools have much higher per-pupil costs (which increase the social costs of the intervention) and are funded by the government (which reduces net revenue for the MVPF calculation).

We next discuss the calculation of specific cost types in greater detail.

C.1.1 Costs of Providing Schooling

If a student is not enrolled in school, the cost of schooling is set to 0. Below, we discuss how we infer the social costs of providing public and private schooling for different years.

Private Schooling For the years in which the school survey was conducted (2003–2006 and 2011), we collected data directly on schools' costs and enrollment, so it is straightforward to calculate real per-pupil costs for children in grades 1–8 in these years. For non-survey

years, since private real per-pupil costs are relatively stable for the entire period over which we collected school data (8 years), we assign the average of observed real per-pupil costs across the survey rounds. We focus on costs rather than private fees because any difference between total costs and total fees is a transfer to private schools, not a social cost of the program. This strategy is conservative if there are economies of scale, since in that case, the marginal cost of providing education to an additional student will be less than the average total cost.

For private schooling, in grades 9–12 (secondary schooling) and for post-secondary school, survey costs are less informative, as these surveys primarily focus on lower schooling levels. For secondary schooling and higher, we use household-reported school fees as a proxy for the per-pupil cost of private schooling. In a perfectly competitive market, this would exactly equal the marginal cost. If markets are not perfectly competitive, this measure will be conservative as it will include transfers to the schools.

We use observed household fees to construct fee schedules. We first convert fees across survey years to 2018 USD and winsorize them at the 2nd and 98th percentiles within grade-school type-survey year cells. We assign each enrolled private school child-year the mean fee for its cell. For non-survey years, between 2007 and 2010, we interpolate the cell means using the annual real growth rate in private secondary-school fees between 2006 and 2011. For private secondary schooling costs incurred after 2011, we use the 2011 fee schedule.

Costs of Public Schooling We use the school survey to calculate real per-pupil costs for public schooling in grades 1–8 for 2003–2006 and 2011. Real per-pupil costs are much more stable from 2003–2006, but increase by around 40 percent between 2006 and 2011. This is in line with a large school funding policy during this period. So, for 2007–2010, we infer public-school costs using the annual growth rate that rationalizes the per-pupil costs we observe in 2006 and 2011. Since the policy had been rolled out by 2011, we assume per-pupil real costs remained fixed at the higher 2011 rate thereafter.

For secondary public schooling, the school surveys are less informative. We therefore use the private secondary-school fee schedule described above and scale it by the public-to-private cost ratio observed in the school survey ($\sim$2.6). For public post-secondary per-pupil costs, we use the annual per-pupil costs incurred by the government for Cadet Colleges as reported by Khan (2022). The cost estimate is PKR 24,269.48, averaged over three financial years (2018/19 to 2020/21), and does not specify a real price base. We therefore deflate this nominal estimate to 2018 PKR using the average CPI over those fiscal years and then convert it to 2018 USD using the 2018 exchange rate.

C.1.2 Household Expenditures on Education

The LEAPS household surveys collect detailed data on educational expenditures besides school fees, such as textbooks, transportation costs, and tutoring. For survey years, we use these data to calculate child-level educational expenditures. We do not include school fees in this measure because we already count the increased costs of providing private education in the school costs above; any difference between schools' costs and fees is a transfer to private schools.

We begin with observed non-fee household education expenditures in the survey years. For years in which the child is enrolled/imputed to be enrolled and has reported non-fee education expenditures, we use the child's reported expenditure. As with other costs, household education expenditures are all converted to 2018 US dollars and winsorized at the 2nd and 98th percentiles within the cells used to construct the expenditure schedule. For grades 5 and below (primary) and grades 6 to 8 (middle school), these cells are at the survey year-district-schooling level (primary vs middle school)-school type level. For secondary school (grades 9–12), where the cells are thinner, we pool across districts and define cells at the survey year-school type level. For post-secondary schooling, where there are very few observations overall, we pool across both districts and years and define cells only by school type.

For enrolled child-years where expenditures are missing, including non-survey years, we assign the corresponding cell mean from this expenditure schedule. To do so, we construct each child's current schooling level in every year of the cost panel. We use observed current grade where available, and then impute forward or backward as needed. For non-survey years between 2007 and 2010, and for years after 2011, we use the 2011 expenditure schedule to impute costs. Household education expenditures are set to zero in all years in which the child is not enrolled.

C.1.3 Opportunity Cost of Staying in School

The final social cost is the foregone earnings due to increased education and the subsequent postponement of entry into the labor market. Using the panel of data we constructed for each child up to age 25, we set earnings to 0 if the child is enrolled. In addition, out-of-school children under age 10 are assumed not to work, and earnings are set to 0, consistent with the fact that less than 1% of these children work in the LEAPS data. To infer earnings for out-of-school children aged 10 to 25, we use two data sources.

For children aged 10–19, we use the time-use data among non-enrolled children in the 2011 household survey. We estimate mean paid-work intensity (defined as total paid-work hours in the time-use data divided by an eight-hour workday and capped at one) by age and

gender, and multiply it by the conditional monthly earnings of children with paid work. To calculate conditional earnings, we average earnings among children by age category (10–14 and 15–18) and gender. As with other costs, we also winsorize earnings at the 2nd and 98th percentiles, separately by gender. Finally, because the time-use data do not include information on 19-year-old respondents, we assign them paid-work intensity and earnings levels equivalent to those of 18-year-old respondents, separately by gender.

For participants aged 20–25, we use the 2018–19 survey, which focused on individuals who were 5–15 in 2003. For these ages, we group years of schooling into the following categories: zero, primary (1–5), middle (6–8), secondary (9–12), and post-secondary (>12). We also group individuals into two-year age groups (20–21, 22–23, and 24–25). We then compute the likelihood of working if not in school (defined as being engaged in a paid activity in the past month) for each age group–education level–gender cell. We also compute average monthly earnings within these cells among those with non-zero earnings. In instances where earnings data are missing in these cells, we assign earnings for the corresponding education level, separately by gender. In instances where the likelihood of working is missing, we assign the gender-specific average work likelihood.

C.2 Social Benefits

We conservatively assume that all social benefits are given by the increase in the present discounted value of earnings from the time an individual enters the labor force to the time they retire. For the full-sample MVPF (Table A19), we estimate the experimental effect of the intervention on logged earnings in the pooled older cohort (Table 8, Panel A, Column 6), defined at baseline as children aged 9–11 and enrolled in school in 2003. We convert the estimated log-earnings effects into monthly level effects by multiplying the pooled control-group mean monthly earnings by $\exp(\beta) - 1$. This gives an estimated monthly earnings gain of USD 19.8, which we apply to both men and women. For the male-only MVPF (Table A18), we re-estimate the same specification using only men in the older cohort (Table 8, Panel B, Column 6), yielding a monthly earnings gain of USD 20.7.

We calculate the expected present discounted value of these earnings using gender-specific work shares and benefit horizons. For men, we assume that 90% work (the share for men aged 25 and above in our data), and that benefits accrue from ages 26 through 60 (in line with the very high rates of employment for older men in the Pakistani Labor Force Survey). For women, we assume that 12% work, and that benefits accrue from ages 26 through 30. In the most conservative specification, we assume that the monthly treatment effect remains constant in real terms over the benefit horizon. This is conservative because it ignores

earnings increases with seniority or learning by doing.

We also report results from three less conservative variations. In these specifications, we scale the estimated earnings gain using gender-specific lifecycle wage profiles (obtained from ILO data on the Pakistani labor market) for the informal and formal sectors, as well as their average. These profiles are normalized to one at age 26 and therefore capture lifecycle wage growth but not aggregate economic growth.

C.3 Additional Details for the MVPF Calculations

The WTP for the MVPF calculation is the post-tax PDV of the increase in participants' earnings, net of the increase in household educational expenditures, the post-tax earnings loss from the crowd-out of labor by education, and the private provision of education costs. Details on the calculation of all these costs and benefits are provided in Sections C.1 and C.2. To calculate the post-tax PDV, we assume a GST rate of 15% (in line with the rate in Pakistan for much of this period) and a compliance rate of 50%.

Net government costs are given by PDV of Implementation Cost of the Report Card Program + Increased Public Education Costs + Loss in Tax Revenue due to Opportunity costs – Increase in Tax Revenue due to Increased Future Earnings. As shown in the paper, under various assumptions, this value is always negative for men. That is, the increase in tax revenue due to increased earnings is always greater than the total costs to the government.

C.4 Welfare Calculations for the Poor

In Appendix Table A20, we present the welfare analysis for the subsample of men who come from households below the median baseline household wealth index. For this group, schooling histories (enrollment, school type, and grade progression) are constructed within the subsample of poor individuals. To avoid working with thin cells, we use the same cost schedules as the ones described above for private and public schooling costs, household expenditures on education, and the opportunity cost of staying in school for those between ages 10 and 19. The age 20–25 opportunity cost schedule and the earnings gains are estimated within the subsample of poor individuals. All other assumptions (regarding discounting, exchange rates, likelihood of working, retirement ages, etc) and econometric specifications remain the same.[34]

[34]For this sample, the treatment effects on undiscounted annual net benefits are positive in 2003–05 due to slightly lower educational expenses reported by low SES men in the treatment group. However, this is before the welfare calculation assigns any earnings gains. We interpret these early positive cash flows as estimation error in the cost components, not real treatment benefits. The estimates are statistically insignificant in 2003 and 2005 and significant in 2004. For the IRR calculation only, we set the 2003–05 annual net benefit cash flow to 0 before solving for the discount rate. This adjustment leaves the reported benefit-cost ratio and MVPF unchanged.