The Long-Term Effects of School Quality in a Low-Income Country: Evidence From 15 Years of Data

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Abstract

Using rich data following children over 15 years in Pakistani villages, we provide the first causal estimates of the effects of attending a better primary school on children's long-term outcomes in a low-income setting. Exploiting panel test score data, which allow us to estimate and validate value-added measures of school quality, in combination with variation generated by moves across schools, we show that on average being exposed to a 1 standard deviation better school increases adult test scores by 0.2 sd, total years of schooling by nearly 1 year (12%), the likelihood of graduating high school by 10 ppt (51%), and college enrollment by 5 ppt (48%). Hence, the effects of attending a better primary school do not fadeout by adulthood. In addition, female labor force participation nearly doubles. Our estimates of the long-run effects of learning on educational attainment in combination with Mincerian estimates of the returns to education – which closely mirror our point estimates for effects on male wages – allow for the quantification of the effects of interventions that increase learning on earnings. Such an exercise suggests that moving only the male children from the worst school to the best school in the average village would generate present discounted wage gains of 182,000 USD (130 times GDP per capita).

1 Introduction

For more than fifty years, dating back to at least Mincer (1974), economists have sought to causally estimate the returns to an additional year of schooling. At the same time, there is growing evidence that school quality is highly heterogeneous, even within extremely small geographic areas (Andrabi et al., 2025). An additional year of education at one school may have very different implications for long-run outcomes than an additional year at another. Understanding the effects of school quality is especially important in low-income settings. On the one hand, foundational learning could have especially high returns in these settings both because curricula are often too advanced for students and do not allow for catch-up

(Muralidharan et al., 2019) and because parents believe there are higher returns to investing in children who are already doing well and therefore re-enforce early differences in cognitive skills (Dizon-Ross, 2019). On the other hand, a growing literature suggests that early gains to cognitive skills fadeout (Bailey et al., 2017), implying that even large differences in school quality early in life could become unimportant by adulthood. Fadeout may be particularly important in low-income contexts where children are often in and out of school to assist households and parents are less able to make complementary investments in skills. As policymakers in low-income countries decide whether to dedicate limited resources to increasing school quality relative to other competing priorities, understanding the returns on these investments is critical. However, estimating these returns in the lowest-income settings is also particularly challenging. There are no detailed administrative data on test scores, allowing for the estimation of school value-added (SVA) – the standard measure of school quality that captures the predicted effect of attending a given school on a student's annual test score gains – much less administrative data that allows for linking SVA to long-run outcomes.

This paper exploits a unique study that followed children in Punjab, Pakistan for 15 years to provide the first causal estimates of the effect of primary school quality on long-run outcomes, including cognitive skills, socioemotional skills, functional literacy/numeracy, educational attainment and employment. Between 2003 and 2007, we collected panel data on primary school students' test scores, allowing for the estimation and validation of SVAs, as described in Andrabi et al. (2025). In this paper, we match these SVA estimates with information on long-run outcomes collected for a subsample of children in subsequent surveys in 2011 and 2018. While this resolves the challenges posed by the lack of data on school quality linked to adult outcomes, a second challenge arises from the fact that children select into schools. While our measures of SVA are unbiased, since as in higher-income countries, richer children typically attend higher SVA schools, simply regressing outcomes on SVA will still lead to biased estimates of the effect of school quality.

We address this selection problem with two distinct identification strategies. For our main strategy, we compare students who are first observed in the same school and exploit variation in the average SVA experienced by primary school students due to school switches. This is a plausible source of exogenous variation because there is substantial school churn in this context and many switches are driven by exit or entry events that are potentially exogenous to students' test score trajectories. This strategy assumes – conditional on first school – that switchers are no better or worse than stayers, that students who switch into better schools are not better in levels or in terms of trajectory, and that there is no differential attrition from the sample. We show that the data are consistent with each of these assumptions.

Experiencing a 1 sd higher average SVA during primary school leads students to complete 1 more year of schooling. This effect remains stable after controlling for potential confounders like socioeconomic status. As adults, in 2018, students who experience 1 sd higher average SVA in primary school have higher cognitive skills (by 0.2 sd) and higher literacy and numeracy (0.3 sd), limiting the scope for fadeout. In contrast, their non-cognitive skills are unchanged. The increases in educational attainment are driven by higher rates of attainment at almost every level. Students who attend better schools are more likely to complete 10th grade (approximately 10 percentage points or 23%), receive a high school diploma (10 percentage points or 50%), and go to college (5 percentage points or 43%).

We complement our main strategy with a lasso instrumental variables strategy exploiting households' high degree of distance sensitivity in this context, as has previously been shown by Bau (2022) and Carneiro et al. (2024). Importantly, Andrabi et al. (2025) show that conditional on distance to the center of the village, a household's location is unrelated to household characteristics. To exploit variation in the quality of nearby schools (and hence a household's effective choice set), we propose as potential instruments the average SVA of schools within varying radii around the household. The lasso regression selects the instruments that are most predictive of the SVA of the school a student actually attends. We then verify that these instruments, like the location instrument in Andrabi et al. (2025), are unrelated to household or student characteristics conditional on distance to the center of the village. This strategy allows us to estimate the effects of school quality using totally different variation than our main strategy. While our main strategy relies on variation in the schools students attend over time due to the panel structure of the data, this strategy exploits cross-sectional variation from students' locations. While less precise, the IV-lasso results are qualitatively similar to those in our main strategy, strengthening the causal interpretation of our results.

We then consider several possible channels for the link between high SVA primary schools and educational attainment. While the most straightforward explanation is that better schools improve cognitive skills, and this in turn leads to greater educational attainment, there are several other potential mechanisms. One possibility is that higher quality schools have better reputations and therefore provide a more positive signal of student quality, which helps with admissions at the secondary/college-level (MacLeod and Urquiola, 2015; Sekhri, 2020). To evaluate whether this is the case, we include an (endogenous) control for peer quality in our regressions. The effects on educational attainment are robust, suggesting that school quality itself rather than peer quality matters. Another possibility is that the effects are driven by attending a private school rather than a higher SVA school per se; this is an important potential confounder since private schools have higher SVA on average in this

context. We directly control for private schooling exposure and show that the effects remain even after accounting for this correlation. Third, it could be that higher SVA schools are more likely to offer higher grades, reducing switching costs, but we again find no evidence that this explains our results. Finally, it could be that schools that produce more cognitive skills also produce more non-cognitive skills, and it is these skills that lead to more educational attainment (Heckman et al., 2013; Jackson et al., 2020). However, we find no evidence that higher SVA schools improve non-cognitive skills more in this context.

In the last part of the paper, we turn to the longer-run impacts of school quality on employment and wages. This is a more challenging question because a large portion of our sample is still in school (their ages range from 17 to 32), and this is causally higher among those who attended higher quality primary schools. As the wage gains from attending higher quality primary schools are likely to be highest among those who ultimately attain the most education, any estimates that condition on employment are likely to be downwardly biased. Moreover, even among those who are currently working, the less-educated are mechanically more experienced, which may lead their wages to look relatively higher in the early career period when we observe participants. Chetty et al. (2014a) observe a similar pattern when measuring the effects of higher quality teachers on their former students, estimating initially negative effects on younger adults that turn strongly positive as they age.

With this in mind, we report suggestive estimates of the effects of attending a better primary school on employment and wages. Reflecting men and women's very different labor force participation rates – employment is close to universal for male non-students, while only 7% for women in our sample – the story varies by gender. For men, there is a strong decline in working, but this is explained by the fact that men are much more likely to still be in school. For women, on the other hand, while enrollment also appears to increase, because baseline labor force participation is so low, this does not lead to a strong decline in labor force participation. In fact, on net, a woman who attends a 1 sd better primary school over the course of her education is 5 ppt (≈70%) more likely to work, perhaps because education gives her access to more socially acceptable teaching jobs. Our results suggest that improved foundational learning can play a role in opening up labor market opportunities to women, even in a highly gender-biased society.

Cautioning that our wage estimates are likely to underestimate the long-run effects of attending a higher quality school for the reasons mentioned above, in our sample of working men, we estimate that attending a 1 sd better school increases wages by 10% (albeit not statistically significantly). This closely matches what we would predict if the full wage effect were through increased education, since attending a 1 sd better school leads to 1 more year of education, and Mincerian regressions suggest that the private return to a year of schooling

globally is 9% (Patrinos and Psacharopoulos, 2020). For completeness, we report results for working women, where the effects are large and positive, but given the extremely small sample size of working women, these estimates should be interpreted with caution.

The fact that our wage increase estimates are so similar to estimates of Mincerian returns supports the implementation of an approach in the style of Athey et al. (2019), where we link the learning gains we observe to their effects on long-run outcomes and then estimate the effects on wages based on the Mincerian returns observed in observational data. This allows us to provide a likely lowerbound – given that we do not expect all the returns of improved learning to manifest via increases in education – quantification of the benefits of moving children across schools. More broadly, our estimates of the link between primary school learning and educational attainment, linked to the Mincerian returns, can be used to assess the cost-effectiveness of learning interventions in low-income countries in terms of long-run wage effects. To our knowledge, without the evidence on the long-run link between learning and educational attainment provided in this paper, such calculations would require researchers to make heroic assumptions about the causal effect of learning on educational attainment or wages.

This paper contributes to two related literatures. First, it contributes to a growing literature documenting the importance of and estimating the effects of school quality. In the context of charter schools in the United States, Hoxby and Murarka (2009) and Angrist et al. (2013) show that school quality is variant and that this has important implications for learning in the short or medium-term, while Andrabi et al. (2025) document that this is also true even in small Pakistani villages with an average of 678 households. A smaller literature mainly in higher-income contexts investigates the longer-run impacts of school quality, but this literature faces many of the same challenges as in low-income settings. To causally estimate the long-run effects of school quality, researchers need both data that links school quality 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 SVA and value-added for longer-run outcomes (Jackson et al., 2020). One exception is Beuermann et al. (2023), which exploits exogenous variation to estimate school value-addeds for test scores, arrests, dropout, teen motherhood, and formal labour market participation and shows that these value-addeds are modestly correlated in the context of Trinidad and Tobago, though this setting is five times richer than the one we study. More broadly, while not directly about school quality, Chetty et al. (2014b) measure the long-run effects of teacher quality on students' long-run outcomes.

Our paper contributes to the literature by (1) providing the first estimates of the long-run effects of school quality in a low-income setting, (2) documenting effects on cognitive skills – for the first time to our knowledge – more than 10 years after students initially attended a school, and (3) estimating school quality effects for the full distribution of schools in a representative sample of villages.

Second, this paper contributes to a still nascent literature on the long-run effects of educational and human capital-oriented interventions in low-income settings. Long-run follow-ups are still rare in development economics, although there are notable exceptions. Araujo and Macours (2021) show that the famous Progresa cash transfer program 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, and Buhl-Wiggers et al. (2023) estimate the impacts of a literacy program in Uganda 8 years later. In contrast, our paper is the first to our knowledge to estimate the long-run effects specifically of increasing learning in a low-income context. Given the relatively small first stage of most educational interventions – Evans and Yuan (2022) estimate that the median intervention increases test scores by 0.1 sd – even in cases where long-run data are available, researchers are likely to be underpowered to estimate long-run effects. In contrast, school quality in Pakistan is highly variant, with the gap in learning between the best and worst school in a village mirroring the test score gap between a high and low-income country (Andrabi et al., 2025). Hence, unusually, we are powered to measure the long-run effects of learning in primary school on later educational attainment.

2 Context and Data

2.1 Context

Education Markets Our study takes place in rural Punjab, Pakistan's largest province. As of 2023, the province had a population of 128 million. This is a context with substantial school choice. During the period when the children we study were in primary school, the average village in our sample had 680 households but 7.4 schools (4.4 public and 2.2 private) in our baseline survey round in 2003. Students choose between public schools, which are nominally single-sex, and private schools, which are typically co-educational. Almost all private schools are for-profit and non-religious (Andrabi et al., 2006). There is substantial heterogeneity in school quality among primary schools, even within the same village (Andrabi et al., 2025). Moving a student from the worst to the best school in her village would increase

her annual test score gains by 0.43 test score standard deviations, which is roughly equivalent to average annual test score gains.

Villages also experience substantial churn in both the public and private sectors, which contributes to potentially exogenous school-switching. In our data, 13% of private schools that were open at baseline in 2003 close by 2007 (4 years later) and 12% of schools open in 2007 did not exist in 2003. This churn generates much of the switching across schools we observe in the data.

Educational Outcomes The period we study is also a transformative one for educational outcomes, though such a transformation is not atypical for low-income settings in recent decades. Our study focuses on children who were enrolled in primary school between 2003–2007. Among these children, about a quarter have mothers with *any schooling*. While roughly two-thirds have fathers with some primary schooling, very few have parents of either gender that have education beyond primary school. In contrast, 15 years after our baseline round, the average child in our analysis 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 transformations in human capital attainment, this is not a boom period where we would expect skills to have unusually high returns relative to other low-income settings. Growth fluctuated greatly, but real GDP growth averaged around 4% from 2000-2020. For comparison, India averaged real GDP growth of 6% over the same period and Sub-Saharan Africa as a region averaged growth of 4% (International Monetary Fund, 2001). Hence, the Pakistani experience is likely externally valid to other low-income settings.

Gender & Labor Force Participation While Pakistan is generally considered a gender-biased society – in the 2021-2022 UNDP Human Development Report, Pakistan ranked 161st out of 190 countries on the Gender Inequality Index (United Nations Development Programme, 2022) – gaps in educational attainment in our setting are small. In our sample, the average woman completes 8.3 years of education, while the average man completes 8.0 years. At the college-level, 14% of women have attended college, while only 9% of men have. However, men and women have vastly divergent outcomes when we consider labor force participation. In our sample, only 7% of women report working, while 76% of men work. This under-estimates the true difference 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, the employment rate converges to around 90%. In contrast, for women over the age of 25, employment rates remain below 10% (see Appendix

Figure A1). Low female labor force participation rates may reflect the fact that there are few socially acceptable jobs available to women (Heath and Jayachandran, 2018). Indeed, one socially acceptable occupation – teaching – accounts for 60% of employed women in our data.

2.2 Data and Descriptive Statistics

The Learning and Educational Achievements in Punjab Schools (LEAPS) project is a longitudinal dataset on households and schools during which data were collected from 112 villages in rural Punjab, Pakistan. These villages are a stratified random sample of 3 districts of Punjab (Attock, Faisalabad, and Rahim Yar Khan). To be included in the sample, a village had to have a private school at baseline. Hence, these villages are wealthier than the average village but representative of the villages in which approximately 60% of the population lives.

Appendix Figure A2 presents a timeline of data collection. From 2003–2007, 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 from the 112 original villages. In 2018-19, we tracked and surveyed some individuals from the 1,807 LEAPS households. This section describes the school and household data collection exercises, focusing on the variables of interest used in this paper.

2.2.1 School Surveys (2003–2007)

Three types of data were collected at schools: test score data from students, socioeconomic information from students, and school characteristics from owners and head teachers. We first describe the test score data and then briefly describe the information on students and schools. The latter type of information is mainly used to construct controls in our analysis and to construct the instrumental variables for our second empirical strategy.

Test Scores In 2003-04, we tested a first cohort of children enrolled in grade 3 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 when most students in this second cohort were enrolled in grade 4. Students were tested in math, Urdu (the vernacular), and English. The tests were administered in-school by enumerators. Tests were independent, low-stakes, and designed by the principal investigators. The test scores were constructed over this four year period using Item Response Theory, which results in a distribution of test scores with a mean of 0 and a standard deviation of 1.

We use the test score data from the period between 2003 to 2007 to construct empirical

Bayes school value-added (SVA) estimates, our measure of school quality. In Section 3.1, we describe the procedure to construct SVA estimates, which exactly replicates Andrabi et al. (2025). When we construct the SVA measures, we exclude the data of all children who appeared in the 2018-19 long-term follow-up LEAPS data (1,147 individuals or 3,594 child-year observations from the sample of 31,361 tested students or 71,467 student-year observations). Excluding these individuals avoids cases where estimation error leads the estimated SVA to be mechanically related to students' long-run outcomes. The reason this restriction only excludes 1,147 children, even though the 2018-19 survey is considerably larger, is because inclusion in the SVA regression, which controls for lagged test scores, requires test scores from at least two adjacent years. The majority of our long-run sample, as described below, do not satisfy this restriction.

School Characteristics We surveyed all primary schools in the sample villages and collected socioeconomic information for a random sample of 10 students enrolled in each school. This allows us to calculate measures of peers' characteristics for the students in our study sample, which consist of the average of peers' asset indices, as well as the percentage of peers whose mother and father have at least some primary schooling. During the school data collection, enumerators also collected information about the school type (public, private, NGO, madrassa), GPS coordinates, and grades offered by the schools.

2.2.2 Household Surveys

In our baseline round in 2003, we randomly selected 1,807 households (approximately 16 households in each village), such that in each village, three-quarters of these households had a child enrolled in grade 3, and one-quarter had a child not enrolled in school, but was at an age eligible to be enrolled in grade 3. This stratification is consistent with the share of eligible children who are actually enrolled in grade 3 at this time. All resident members of these households were surveyed annually until 2007 and then surveyed again in 2011. In 2018-19, household members who were between 5 and 15 years old in 2003 were tracked for a long-term follow-up survey with the aim of surveying them regardless of whether they had remained in the same household. While this survey was technically not linked to the household, for ease of exposition, we refer to it as a "Household Survey" since the initial sample tracked was identified in the earlier rounds of the household survey. We separately discuss the initial panel between 2003-2007 and the subsequent waves below.

2003-07 Household Data All six waves of household surveys collected information about socioeconomic status, educational attainment and expenditures, and importantly, between

2003 and 2007, the school attended by all children aged 5 to 15 years old. Combined with information the current grade, we observe which schools children attended for primary school and can match the school information to the SVA estimates from the panel test score data. For most of the analyses in this paper, the final sample consists of children whose enrollment information was observed in the household data between 2003-2007, who attended schools were SVA is available during this period, and for whom adult outcomes were collected in 2018.[1]

In addition to the enrollment histories in the 2003-2007 household panel, we observe socioeconomic information. This allows us to construct control variables for assets (measured in the baseline round of 2003),[2] the caste of the household head,[3] and the education of their parents.

2011-12 Household Survey Data These data are mainly used to track the evolution of cognitive skills 4 years after the initial data collection ended. Children who were still in the household in these data were tested in math, Urdu, and English in the household. Hence, even non-enrolled children are included in these data.

2018-19 Long-term Follow-up Data Based on our sampling frame of individuals in the household survey who were 5–15 in 2003, we identified 5,865 potential respondents. We tracked these individuals over the course of two years between 2016 and 2018. When possible, we surveyed them in-person and administered cognitive and non-cognitive tests (75% of the sample). When impossible, e.g., because they had left the village and did not return, we surveyed them over the phone. Ultimately, in 2018-19, we surveyed 4,956 individuals for an attrition rate of 15.5 percent, or roughly one percent per year, including individuals who died or could not be surveyed due to security reasons.[4] Appendix Figure A3 shows the age distribution for the final sample.

In all surveys, we collected information on individuals' educational attainment, wages, and labor market outcomes. In addition, in the in-person surveys, we tested students to collect information on their cognitive and non-cognitive skills, as well as functional literacy and numeracy. Our main outcome of interest is the number of years of education completed,

[1] While we attempted to test children in the universe of schools, SVA information is missing for a small number of schools. This occurs both because a small number (7) schools refused to ever take part in the data collection, and if a school existed in 2003 but exited the village in round 2, we are unable to calculate SVA. This is because calculating SVA requires us to observe students in the school with lagged test scores.

[2] Assets is the first component of a principal component analysis of 25 indicator variables for households' assets ownership, normalized for the household size.

[3] We follow Karachiwalla (2019) to determine whether the household head is from a high or low caste.

[4] Four villages in Attock were excluded for security reasons.

but we also observe whether they completed important milestones in their schooling trajectories: 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 tests that were used in the earlier rounds of the LEAPS project. Scores in each subject are constructed following Item Response Theory, as in Das and Zajonc (2010), and we average the subject-specific scores to create a single measure of cognitive skills. Danon et al. (2024) show that there are no ceiling effects from using the same tests seven to fifteen years later. Our measure of non-cognitive skills follows Danon et al. (2024) and 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.[5] Finally, we measure functional literacy and numeracy with several questions related to everyday tasks such as reading an electricity bill and understanding greeting text messages. The inclusion of these measures is inspired 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 the literacy and numeracy problem-solving questions and again use the first component of a PCA. We refer the reader to Danon et al. (2024) for the list of questions used for each of these measures.

We also collected information on respondents' employment status, whether they still report being in school, and earnings conditional on working. Panel (a) of Appendix Figure A1 reports the percent of men and women in our sample who engaged in an activity for which they have received a payment in the past month. While less than 10 percent of women work, we observe that starting at 24 years old, close to 90 percent of young men work. Panel (b) of Appendix Figure A1 shows the percent of young men and young women who are currently enrolled in school by age. As opposed to employment, both young men and young women display a similar enrollment pattern, where enrollment rates gradually drop from 35 to 10 percent for those aged 18 to 23 years old, and remain at less than 10 percent for individuals aged 24 years old and older.

2.2.3 Summary Statistics

Our final sample consists of 3,165 individuals in the 2018-19 follow-up survey who were enrolled in a primary school between 2003-07 and whose schools can be matched with an

[5]The Big Five and Grit are self-reported measures constructed following Laajaj and Macours (2021). This is discussed in detail in Danon et al. (2024) who also use data from the LEAPS project. The risk-aversion measure is determined 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 its screen when it changed color (i.e., the "GoNoGo" task).

SVA measure.[6] Appendix Table A1 presents summary statistics for key outcome and control variables for the final sample separately by gender.

3 Empirical Strategy

Our empirical strategy consists of three parts. In the first subsection, we describe how we calculate test score SVA. The second subsection describes our main empirical strategy and provides evidence in support of the key assumptions. The final section details the lasso 2SLS strategy, which provides a robustness test for the main strategy.

3.1 SVA Estimation

In order to compute SVA, for math, Urdu, and English, we use the first four years of panel data from 2003–2007 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. Equation (1) is the same estimating equation used by Andrabi et al. (2025) and is similar to specifications used by Angrist et al. (2017) to estimate school value-added and Chetty et al. (2014b) and Bau and Das (2020) to estimate teacher value-added.

Intuitively, a school's SVA estimate is the predicted mean annual test score gain for a student who is assigned to a school s. 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 al. (2025), where we demonstrate that the SVA estimates are forecast unbiased (they predict the test score gains of a student who switches into a school with coefficient 1) and pass an overidentification test for unbiasedness proposed by Angrist et al. (2017).

While our SVA estimates are unbiased, SVA is still estimated with error. If we include our SVA estimate as an explanatory variable in a regression, estimation error will attenuate its coefficient. To account for this, we use our fixed effect estimates to construct empirical

[6] We lose 1,781 individuals from the original 4,956 person sample. This is because 660 respondents had already completed their primary education by 2003-04 and 498 never enrolled in school. Further, for 395 individuals, we do not observe them enrolled in 2003-2007 and we cannot calculate SVA for 228 individuals.

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). We always use the empirical Bayes estimates when SVA is the explanatory variable in a regression.

3.2 School Switcher Identification Strategy

Even if our estimates of SVA are valid and unbiased, students are still likely to sort into schools based on socioeconomic status (SES) or ability. To estimate the causal effect of SVA on long-run outcomes, our main identification strategy exploits variation in the average SVA that students are exposed to during primary schooling due to school switches. This strategy exploits the fact that there is substantial churn in educational markets in Pakistan, with 47 schools entering and 52 schools exiting over the 4 year period between 2003 and 2007, leading to a substantial degree of school switching that is not necessarily related to students' test score levels/trajectory or changes in socioeconomic status. Appendix Table A2 provides descriptive information on the 563 switches we observe in the data. Notably, students do not typically switch to better schools (the average old and new school SVAs are similar at 0.099 and 0.077 sd), though – as seen in Appendix Figure A4 – there is substantial variation in changes in SVA due to switches. The majority of switches are within the public sector (possibly related to school consolidation efforts over this period or improved respect for gender segregation as children age) or from the private to public sector (likely related to private school closures), although a minority of switches do occur between private schools or from the public to private sector.

Our main estimating equation is

$$y_{is^1T} = \gamma_{s^1} + \beta \frac{1}{n^t} \sum_{t \in [2003,2007]} \widehat{SVA}_{st} + \Lambda \mathbf{X_i} + \upsilon_{is^1T}, \tag{2}$$

where T denotes the long-run round of interest (2011 or 2018), s^1 denotes the first school a child is observed attending such that γ_{s^1} is a fixed effect for the first school the child attends, and $\frac{1}{n^t} \sum_{t \in [2003,2007]} \widehat{SVA}$ is the average of the school-specific empirical Bayes SVA estimates a child is exposed to over the course of primary school. The vector $\mathbf{X_i}$ of controls always includes age and gender fixed effects and variously also includes socioeconomic controls (indicator variables for mother and father have some education, whether the household head is high caste, and an index of household assets calculated in the first round the household is observed). In regressions to test for alternative stories and mechanisms, $\mathbf{X_i}$ may also include endogenous controls to account for different mechanisms (measures of average peer quality, share of years observed in a private school, and measures of whether last school attended

offers higher levels of education).

Our key estimate is β, the effect of attending a higher value-added primary school on long-run outcomes. Our identification relies on the idea that controlling for the first school a child attends accounts for the sorting of children across schools due to differences in socioeconomic status or ability. Conditional on this control, any switching between schools is for idiosyncratic reasons that are unrelated to students' learning trajectories or long-term outcomes. That is, the first school control plays a similar role to the control for lagged test scores in the conventional value-added estimation equation (see equation (1)). The alternative would be to control for baseline test scores, following the standard logic of value-added estimation, but this is not possible because, as described in Section 2.2, most of the children we observe in our long-run sample were not in the cohorts that were tested in 2003–2007 and hence, we do not have test score data for these individuals.

Our identification assumption could be violated for several reasons. First, better students (conditional on first school attended) in levels could switch into better schools. Second, students who are improving faster (conditional on first school attended) could switch into better schools. Finally, students who switch into better schools could be differentially likely to attrit from the sample. We test for each of these potential threats in turn.

Differential Switching & Student Characteristics In Appendix Table A3, we regress student characteristics on an indicator variable for whether they were observed switching primary schools controlling for first school fixed effects. Conditional on first school fixed effects, there is no directionally systematic or statistically significant relationship between switching and socioeconomic status or academic achievement. An F-test that jointly tests whether all of the covariates are related to switching cannot reject a null relationship. Switchers do not appear to differ systematically from non-switchers.

In Appendix Table A4, we restrict the sample to switchers and evaluate whether wealthier or higher achieving students switch into higher SVA schools conditional on first school fixed effects. There is again no evidence of a relationship between child characteristics and the quality of the school into which they switch. With the exception of indicator variables for missing data on father or mother education, which affect a very small number of observations (1% for mothers and 10% for fathers), there are no statistically significant relationships between the measures of socioeconomic status and ability and the measure of new school value-added. F-tests testing whether the covariates jointly predict the value-added of the new school again cannot reject that there is no relationship.

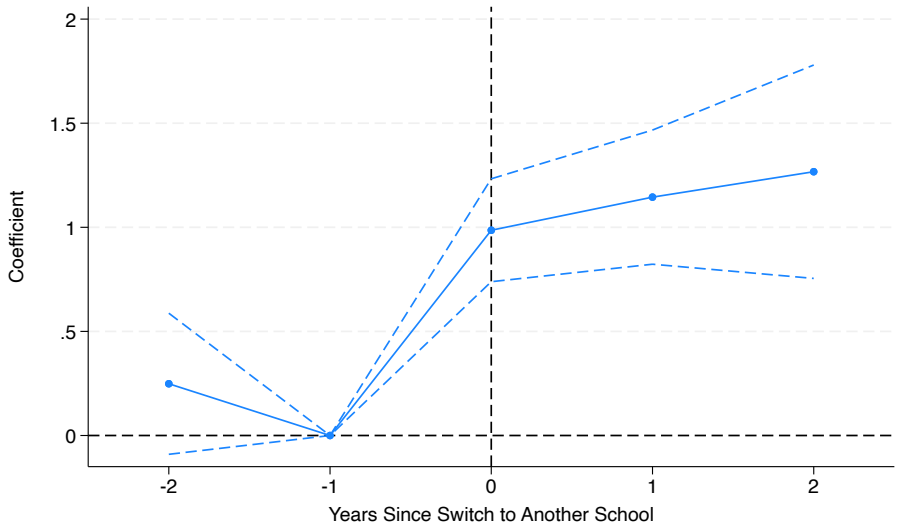
Differential Switching & Student Trajectories Even if students' time-invariant characteristics do not predict switches, students who are on different trajectories or who experience time-varying shocks may switch into different schools. This could occur if parents observe that a child is performing well and decide to invest more in her education (e.g., by switching her to a 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 estimate the following event study regression

$$y_{igst} = \sum_{k \in \{-2,..,2\}} \tau_k event^k_{ist} \widehat{SVA}_{st} + \mathbf{\Gamma X_{igst}} + \alpha^G_g + \alpha^T_t + \epsilon_{igst},$$

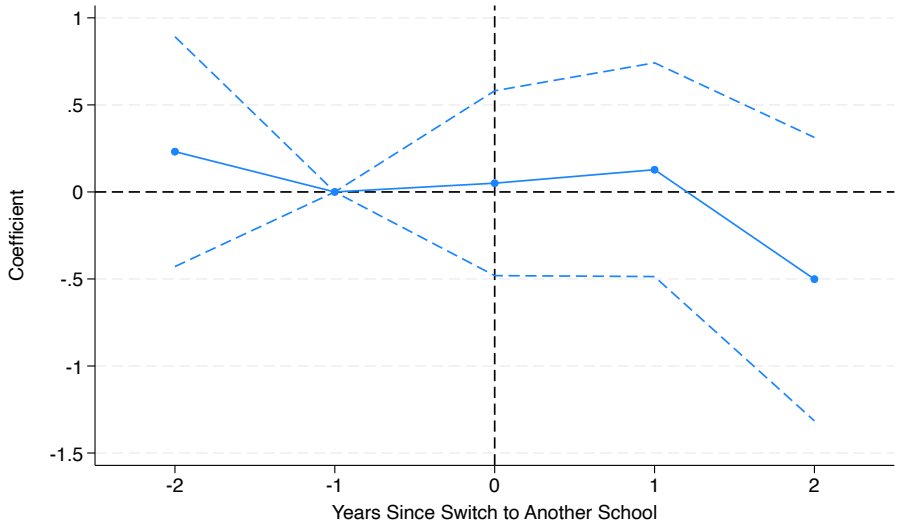
where y_{igst} is either a student i's mean test scores in year t or the household asset index and k denotes event time (the years around a school switch). The indicator variable $event^k_{ist}$ is equal to 1 if a student is observed k years after an event. We note that this event study regression is nearly identical to the event study regressions typically used to assess the validity of school or teacher value-added estimates except that we control for first school fixed effects instead of a rich function of lagged test scores.

Figure 1 plots τ_k for the two outcomes of interest. To improve power, we include all test-takers in Panel (a) rather than just students for whom we have long-run outcomes. Panel (a) shows that there is no pre-trend in test scores prior to a school switch. After the switch, the school's value-added predicts test score gains with a coefficient of 1 in period 0, exactly as we would expect if the first school fixed effect fully accounts for selection. Panel (b) shows that there is no pre-trend in household income prior to a switch – students who switch into better schools are not in households that are growing richer. In addition, students who switch into better schools do not receive a more positive time-varying income shock at the time of the switch. Altogether, switches into better schools are not driven by students who are learning faster or households that are on a better income trajectory (or experiencing positive transitory shocks). Appendix Table A5 reports the point estimates for the event study graphs, as well as the (very similar) point estimates when we restrict to the subsample of test-takers for whom we observe long-run outcomes.

Figure 1: Event Study Graphs for Primary School Switchers



(a) Mean Test Scores



(b) Assets

Notes: Panel (a): Event study graph of mean test scores on years prior to and after a school switch interacted with mean empirical Bayes school value-added (SVA) of the child's new school. Panel (b): Event study graph of household assets on years prior to and after a school switch. *Assets* is the first component of a principal component analysis of 25 indicator variables for households' assets ownership, normalized by the household size. In both panels, controls include year, year of switch, and first primary school fixed effects. Standard errors are clustered at the individual level. Point estimates are displayed in columns 2 and 3 of Appendix Table A5, respectively.

Differential Attrition Finally, we evaluate whether our results could be confounded by differential attrition from the 2018, long-run follow-up survey. In Table 1, we include all participants in the sample frame and regress an indicator variable for being surveyed at all (either in-person or over the phone) and an indicator for being surveyed in-person on the measure of average school quality. Given that individuals are surveyed 15 years after the first round of data collection, the response rate is high (86% with 77% in-person). Furthermore, primary school quality has no effect on the likelihood an individual is surveyed (or surveyed in-person), with or without the inclusion of school fixed effects.

Table 1: Attrition and SVA

	Is Surveyed				Is Surveyed In-person			
	(1) Vill. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) Vill. FE	(6) + SES	(7) First Sch. FE	(8) + SES
Empirical Bayes SVA	-0.004	-0.003	-0.028	-0.027	0.012	0.015	0.004	0.011
	(0.007)	(0.007)	(0.028)	(0.025)	(0.010)	(0.010)	(0.026)	(0.025)
Mean Outcome	0.857	0.857	0.857	0.857	0.769	0.769	0.770	0.770
Adjusted R^2	0.272	0.286	0.329	0.344	0.198	0.205	0.237	0.245
Observations	3695	3695	3612	3612	3695	3695	3612	3612
Clusters	561	561	478	478	561	561	478	478

Notes: The dependent variable is a dummy variable that equals 1 if the individual was surveyed (columns 1 to 4) or surveyed in person (columns 5 to 8). The sample is all individuals aged 5 to 15 years old in 2003 who have a measure school value-added (SVA). *Empirical Bayes SVA* is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a control for gender. Columns 1, 2, 5, and 6 include village fixed effects, and columns 3, 4, 7, and 8 a fixed effect of the first primary school the individual attended. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.3 Lasso 2SLS

We complement the switcher identification strategy with a second 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 the socioeconomic variation in household locations can be controlled for by conditioning on distance to the center of the village. 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 radii, we also forceably 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 results, we use the same instruments chosen by the IV-lasso 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.

4 Long-Run Effects on Educational Attainment

Years of Education We begin by reporting the long-run effects of attending a higher quality school on years of education. As this is our primary outcome of interest, we report estimates with and without controls for the first school fixed effect to better evaluate the sensitivity of the results. In addition, we report the coefficients for our control variables as well as our main explanatory variable of interest. Table 2 reports the results. Our preferred specification with first school fixed effects, as well as age and gender fixed effects, appears in column 3, with its binscatter corollary appearing in Figure 2. We estimate that attending a 1 standard deviation better school increases years of education by 1 year. In line with the tests in the previous section, there is little evidence of bias conditional on the inclusion of the first school fixed effect. Including controls for socioeconomic status and parental education has little effect on the coefficient of interest but increases the adjusted R^2 by 0.1, suggesting that the controls are meaningful predictors of student outcomes. In contrast, columns 1 and

2 show that the estimates are more sensitive to the inclusion of controls when we do not condition on a first school fixed effect. Finally, Appendix Figure A5 reports estimates of the heterogeneous effects of school quality on years of education. The effects are generally stable across different measures of socioeconomic status, suggesting that school quality neither exacerbates or closes existing inequalities.

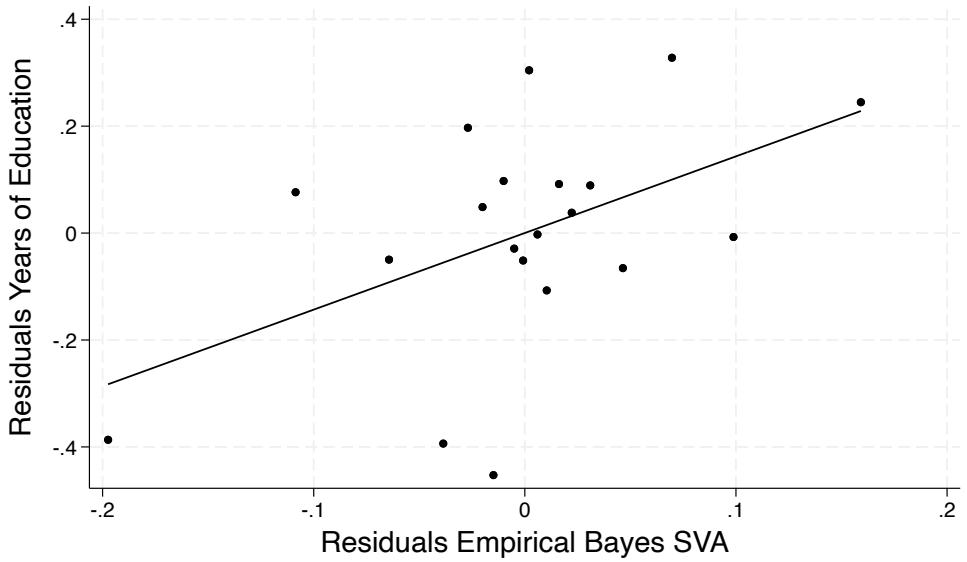
Table 2: Years of Education and SVA

	Village FE		First School FE	
	(1) Baseline	(2) + SES	(3) Baseline	(4) + SES
Empirical Bayes SVA	0.830***	0.576***	1.008***	0.882***
	(0.083)	(0.076)	(0.284)	(0.258)
Female	-0.041	-0.139	0.375	0.394
	(0.134)	(0.129)	(0.254)	(0.255)
Assets in 2003		0.342***		0.337***
		(0.038)		(0.044)
High Caste		0.544***		0.580***
		(0.179)		(0.202)
Mom Educ.		1.395***		1.373***
		(0.169)		(0.187)
Dad Educ.		1.433***		1.302***
		(0.166)		(0.190)
Mean Outcome	8.282	8.282	8.299	8.299
Adjusted R^2	0.138	0.259	0.200	0.296
Observations	3165	3165	3092	3092
Clusters	529	529	456	456

Notes: The dependent variable is the number of years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a control for gender. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Columns 1–2 include village fixed effects and columns 3–4 include a fixed effect of the first primary school the individual attended. Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Effects by Level of Schooling To understand when in the life cycle the gains in educational attainment occur, in Table 3, we estimate the effects of attending a better school on four high stakes outcomes: completing 8th grade (middle school), 10th grade (which requires passing a matriculation exam), receiving a high school diploma, and attending college. For

Figure 2: Binscatter of Years of Education and Mean Empirical Bayes SVA



Notes: Binscatter of residualized years of education and residualized mean empirical Bayes SVA after controlling for age fixed effects, gender, and a fixed effect for the first primary school they attended. *Empirical Bayes SVA* is our measure of school quality and is the average mean empirical Bayes SVA of the primary school(s) the individual attended between Round 1 and Round 4. Point estimates are displayed in column 2 of Table 2. We trim the fifth lowest and highest percentiles of empirical Bayes SVA.

every level of schooling, the effects are positive, though they become larger and more statistically significant for outcomes later in a student's educational trajectory. Attending a 1 sd better school increases completing 8th grade by a statistically insignificant 7 ppt (10%), completing 10th grade by a significant 14 ppt (34%), completing a high school diploma by a significant 10 ppt (51%), and attending college by a significant 6 ppt (48%). The point estimates are largely insensitive to the inclusion of the socioeconomic controls.

Table 3: Educational Attainment and SVA

	Completed Grade 8		Completed Grade 10		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
Empirical Bayes SVA	0.071	0.061	0.144***	0.130***	0.098***	0.089***	0.055***	0.049**
	(0.049)	(0.046)	(0.039)	(0.036)	(0.028)	(0.027)	(0.021)	(0.021)
Mean Outcome	0.660	0.660	0.428	0.428	0.191	0.191	0.115	0.115
Adjusted R^2	0.159	0.223	0.185	0.257	0.117	0.173	0.109	0.150
Observations	3092	3092	3092	3092	3092	3092	3092	3092
Clusters	456	456	456	456	456	456	456	456

Notes: The dependent variables are an indicator variable that equals 1 if the individual completed grade 8 (columns 1–2), grade 10 (columns 3–4), high school (columns 5–6), and if they attended any postsecondary institution (columns 7–8). *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects, a control for gender, and a fixed effect of the first primary school the individual attended. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Cognitive and Non-Cognitive Skills Motivated by a literature that suggests that the effects of educational interventions on cognitive skills quickly fadeout but may re-emerge through non-cognitive skills later on, we report SVA's effects on cognitive and non-cognitive skills in Table 4. Columns 1–2 report effects on mean test scores (across math, Urdu, and English) in 2011 (8 years after the first round of LEAPS), while columns 3 and 4 report effects on mean test scores in 2018 (15 years later). We note that the sample for whom we have test scores in 2011 is markedly smaller because the household tracking in the 2011 round missed children who were no longer in their original household. Columns 5–6 report the results using life skills measures, which test literacy and numeracy with questions that mimic how those skills are used in day-to-day life. Columns 7–8 report effects on a summary measure of non-cognitive skills, which is the first principal component from questions related to "Big Five" personality measures, a grit measure, and two tablet tasks that measure risk-taking and self control. The point estimates for the effects on cognitive skills are similarly-sized in 2011 and 2018-19, though less precise in 2011, with no evidence of fadeout over the subsequent 7 years. Being exposed to 1 sd higher SVA increases cognitive skills measures by 0.13-0.21 sd. For comparison, a 1 sd better school increases test scores contemporaneously by 0.27 sd.

We observe similar effects on the life skills measures of numeracy and literacy. In contrast to Banerjee et al. (2025), who suggest there may be limited transferability between math and reading skills in the classroom and everyday life, we find a large and significant increase in life skills measures of 0.3 sd. However, we find no evidence that attending a school that increases test scores more has any impact on non-cognitive skills.

Table 4: Skills and SVA

	Cognitive Skills (2011-12)		Cognitive Skills (2018-19)		Life Skills Index		Non-cognitive 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
Empirical Bayes SVA	0.211	0.125	0.187**	0.158*	0.295***	0.275***	0.006	0.005
	(0.178)	(0.177)	(0.093)	(0.089)	(0.085)	(0.085)	(0.081)	(0.081)
Mean Outcome	1.194	1.194	0.170	0.170	0.176	0.176	0.027	0.027
Adjusted R^2	0.188	0.230	0.205	0.279	0.131	0.194	0.180	0.178
Observations	1995	1995	2763	2763	2765	2765	2765	2765
Clusters	407	407	446	446	447	447	447	447

Notes: The dependent variable in columns 1 to 4 is the average score from a test in math, Urdu, and English, measured in 2011-12 (columns 1–2) and in 2018-19 (columns 3–4). In columns 5 and 6, the outcome is a score measuring literacy and numeracy. In columns 7 and 8, the outcome is an index from Danon et al. (2024) measuring socioemotional skills. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects, a control for gender, and a fixed effect of the first primary school the individual attended. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5 Channels & Robustness

5.1 Alternative Channels

While we have focused on the fact that high SVA schools directly increase cognitive skills as the main explanation for our findings, there are other possibilities. In this subsection, we explore the role of other potential explanations.

Reputation & Peer Effects It could be that high SVA schools also have better reputations and that this ultimately improves students' ability to gain admission to and attend better secondary schools and colleges (MacLeod and Urquiola, 2015; Sekhri, 2020). Alternatively, it could be that schools that have high SVA increase students' outcomes because they provide students with better peers and better peer effects rather than that cognitive skills improve long-run outcomes. To rule out both these possibilities, in Table 5, we compare our baseline estimates (column 1) to the estimated effects when we include rich controls for peer quality, which capture both direct peer effects and the reputational component of school quality. While one of the peer quality measures – share of fathers with at least some education – does positively predict years of schooling, the inclusion of the peer controls has almost no effect on our coefficient of interest. Appendix Figures A6 and A7 report the results with the additional peer quality controls for other outcomes and show that the results are largely insensitive to their inclusion.

Private Premium Another possibility is that SVA is correlated with private schooling (as shown by Andrabi et al. (2025)), and our estimates capture some kind of private school premium for educational attainment rather than the direct effects of attending a school with a larger effect on cognitive skills. To evaluate whether this is the case, in column 3 of Table 5, we also control for the share of time a student spends in private school. The coefficient on this control is positive and imprecise. However, the coefficient for SVA remains similarly large and statistically significant. Appendix Figures A6 and A7 report the results with the additional private schooling controls for other outcomes. The results remain stable.

Combination Primary/Secondary Schools A third possibility is that high value-added 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. This is a plausible alternative explanation, at least for increases in education at lower levels. About 50% of children attend schools that offer at least some middle school, though very few of the primary schools offer levels of education beyond middle school. Only 20% of students are

Table 5: Years of Education and SVA - Tests for Alternative Channels

	(1) Baseline	(2) Peer Quality	(3) Share Private	(4) Last Grade
Empirical Bayes SVA	1.008***	0.941***	0.798***	0.896***
	(0.284)	(0.266)	(0.283)	(0.269)
Assets in 2003		0.334***	0.336***	0.336***
		(0.044)	(0.044)	(0.043)
High Caste		0.564***	0.580***	0.562***
		(0.202)	(0.202)	(0.202)
Mom Educ.		1.371***	1.366***	1.364***
		(0.187)	(0.187)	(0.188)
Dad Educ.		1.315***	1.301***	1.310***
		(0.190)	(0.190)	(0.190)
Peers' Mom Educ.		-1.498		
		(0.958)		
Peers' Dad Educ.		2.584***		
		(0.969)		
Peers' Assets		-0.378*		
		(0.222)		
Time Share in Priv.			0.376	
			(0.485)	
Offers at least some Middle School				0.045
				(0.321)
Offers at least some Matric				0.335
				(0.360)
Offers at least some Higher Secondary				-0.213
				(0.486)
Baseline Controls	Yes	Yes	Yes	Yes
Parental Controls	No	Yes	Yes	Yes
Peers Quality	No	Yes	No	No
Private School	No	No	Yes	No
Last Grade Offered	No	No	No	Yes
Mean Outcome	8.299	8.299	8.299	8.302
Adjusted R^2	0.200	0.299	0.296	0.295
Observations	3092	3092	3092	3085
Clusters	456	456	456	456

Notes: The dependent variable is the number of years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. Baseline controls include age fixed effects and a control for gender. Parental controls in columns 2 to 4 include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). *Peer quality* in column 2 includes three controls for the average fathers' and mothers' education and average assets of the peers in the school. Missing dummies are included. *Share private school* in column 3 is the share of time spent in a private primary school. *Last grade offered* in column 4 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 a fixed effect for the first primary school the individual attended. Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

in schools that continue through grade 10, and only 2% offer through grade 12. In the last column of Table 5, we include controls for the highest grade offered by the *last* primary school a student attended. The inclusion of these controls has little effect on our main coefficient of interest. In addition, while the coefficients on the these controls are imprecise, there is no systematic evidence that students receive more education if they attend a primary school that offers higher grades. Altogether, we conclude the effects of attending a higher SVA school appear to be driven by the direct impact on cognitive skills.

5.2 Robustness: IV Strategy

We next evaluate whether an alternative identification strategy that leverages cross-sectional variation in students' locations rather than longitudinal variation in the schools they attend produces similar results. As described in Section 3, we use lasso to select the instruments that are most predictive of the average SVA of the schools a child attends from a pool of measures of the average SVA of schools within different radii of the child, conditional on distance to the center of the village. The first column of Table 6 reports the selected instrument and the F-statistic of the first stage. Appendix Table A6 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 a 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 6 reports the estimated effect. While underpowered relative to the switcher identification strategy, the results are qualitatively similar. Here, attending a 1 sd deviation 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. Consistent with the larger first stage sample, the F-statistic for the first stage is larger, and the effect of SVA is more precisely estimated. Otherwise, the results are broadly similar. Reassuringly, a secondary strategy using orthogonal variation to our main strategy arrives at similar results.

Table 6: IV Estimates of the Effect of SVA 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	0.159**		0.110*	
	(0.064)		(0.062)	
Avg. SVA within 1.5 km	0.010		0.079	
	(0.138)		(0.133)	
Avg. SVA within 1.7 km	0.252		0.038	
	(0.173)		(0.153)	
Avg. SVA within 1.9 km	0.190		0.303*	
	(0.138)		(0.153)	
Empirical Bayes SVA		1.721*		2.175***
		(0.940)		(0.551)
F-statistic	7.557		8.554	
Number of Observations	3165	3165	4580	
Number of Obs. 1st Stage				4580
Number of Obs. 2nd Stage				3143

Notes: Columns 1 and 2 show the first and second stages of our instrumental variable (IV) strategy, and columns 3 and 4 using the two-sample IV strategy following Inoue and Solon (2010). The dependent variable is number of years of education completed. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. The potential instruments included are the average SVA of schools in different radii ranging between 500 meters and 2 kilometers in increments of 100 meters. The instrument is selected using the lasso IV strategy. Missing dummies are included for all potential instruments. All regressions include controls for village fixed effects, age fixed effects, age interacted with gender fixed effect, controls for gender, distance to the village center, and distance to the village center interacted with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Employment and Wages

Our last set of results concern the effects of attending a better school on employment and wages. Unlike education, where men and women's attainment are largely similar, female and male outcomes strongly diverge when we consider employment and wages. As shown in Appendix Table A1, 7% of women report working while 77% of men do. So, when we analyze effects on labor market outcomes, we always separately report results for men and women, along with results for the pooled sample.

While these outcomes are important, we view our results here as less definitive than the educational results, which capture most of individuals' completed education. Our sampling frame consisted of those who were 5–15 in 2003-04. Given that our long-run outcomes were measured in 2018-19, this results in a sample that is roughly 17–32 when they were surveyed

about labor force outcomes.[7] Appendix Figure A1 plots the percent of individuals working (Panel (a)) and enrolled in school (Panel (b)) by age in the long-term follow-up. We observe that until age 25, a fairly sizable fraction of both men and women are still enrolled in school. This appears to be important for male labor force participation, which tracks up with age as enrollment falls but has negligible effects on female labor force participation, which is always low. Thus, among men, a substantial part of our sample that is likely to enter the labor force in the future (after finishing their education) and may see the highest returns to attending a high quality primary school (as they are completing the most education) is not yet working.

Table 7 more directly illustrates this effect. Panel (a) shows that attending a higher quality primary school has no effect on whether an individual is engaged in a productive activity (defined as working or being enrolled in school). But this null effect masks the fact that students who attended higher SVA schools are more likely to be currently enrolled (columns 3–4) and less likely to work, albeit insignificantly (columns 5–6). These pooled effects in turn mask important heterogeneity by gender. For men (Panel (b)), where employment rates are close to universal if a man is out of school, there is a clear positive effect on enrollment and a negative and significant effect on employment, which produces an insignificant negative net effect on productive activities. Intriguingly, for women, who have a very low labor force participation rate, the results are more imprecise but positive for both working and enrollment. There is no contradiction in these two positive effects: because the female labor force participation rate is so low, enrollment does little to crowd out employment. While the estimate is only significant at a 10% level, attending a 1 sd better school over the course of the observation period increases the likelihood a woman is employed by 5 ppt (72%), suggesting that attending a better primary school increases female labor force participation. We speculate that this is because education gives women access to teaching, one of the few socially acceptable jobs for women in this context.

In Table 8, we estimate the effects of attending a 1 sd better school on logged incomes among the sample that works. These regressions are likely to be underestimates of the long-run effects of higher quality primary schooling for at least two reasons. First, as mentioned above, attending a higher quality school causes a man to be more likely to be enrolled in 2018, and this in turn means he is not observed working. If a large part of the benefits of attending a better school occur through increased education and/or if the students who experience increased enrollment at high levels (upper secondary school and college) are higher ability and have higher latent wages than those who have dropped out, we will underestimate the effects of attending a higher quality school. In addition, we observe students early in

[7] There are some deviations because ages are not always fully consistently reported over time.

Table 7: Employment and SVA

	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: Young Individuals						
Empirical Bayes SVA	0.024	0.026	0.056**	0.051**	-0.034	-0.027
	(0.034)	(0.035)	(0.025)	(0.024)	(0.031)	(0.030)
Mean	0.560	0.560	0.129	0.129	0.451	0.451
Observations	3092	3092	3092	3092	3092	3092
Clusters	456	456	456	456	456	456
Panel B: Young Men						
Empirical Bayes SVA	-0.042	-0.035	0.062**	0.058**	-0.101**	-0.090**
	(0.040)	(0.040)	(0.029)	(0.027)	(0.046)	(0.043)
Mean	0.864	0.864	0.108	0.108	0.775	0.775
Observations	1635	1635	1635	1635	1635	1635
Clusters	274	274	274	274	274	274
Panel C: Young Women						
Empirical Bayes SVA	0.138*	0.121	0.091	0.070	0.048*	0.049*
	(0.077)	(0.078)	(0.076)	(0.076)	(0.026)	(0.028)
Mean	0.198	0.198	0.152	0.152	0.067	0.067
Observations	1333	1333	1333	1333	1333	1333
Clusters	240	240	240	240	240	240

Notes: The dependent variables are indicator variables that equal 1 if the individual reports working or being currently enrolled in school (columns 1–2), being enrolled in school (columns 3–4), or working (columns 5–6). Panel A includes all individuals, Panel B is the sample of men, and Panel C is the sample of women. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a fixed effect of the first primary school the individual attended. Panel A includes a control for gender. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

their careers, when those who got less education mechanically have more experience and when benefits from differential income trajectories are yet to be realized. This is consistent with Chetty et al. (2014b) in the U.S., who show that, in the case of teacher value-added, students who had a higher value-added teacher are on a steeper income trajectory, and income effects are initially negative when students are young (in their early 20s) but become positive and statistically significant at age 24. With these caveats in mind, we estimate that

among working men, attending a 1 sd better school increases earnings by 11%, although this effect is imprecise and not statistically significant. For completeness, we also report results for working women. While the estimates are very large and statistically significant, we do not view them as reliable given the very small sample size of women who work combined with the demanding data requirements of the switchers identification strategy.[8] While the earnings results for men are imprecise, recalling that attending a 1 sd better primary school leads to 1 additional year of education, the point estimates are strikingly close to both Mincerian estimates of the effect of an additional year of education – Patrinos and Psacharopoulos (2020) estimate the global return to a year of education is 9% – and causal estimates from Duflo (2001) and more recently, Khanna (2023). Given the close mapping between (our likely downwardly biased) estimates of the long-run returns of foundational learning on wages and the Mincerian return, in the next section, when we quantify the gains to attending better primary schools, we use estimates of the Mincerian return of a year of education as likely conservative estimates of the effect of attending a 1 sd better school.

7 Quantifying the Gains From Better Primary Schooling

A policymaker in a low-income country deciding how much to invest in education and at what point in the life cycle may worry that any benefits to increasing investment in foundational learning will fadeout by the time a student reaches the workforce 10–15 years later. In this section, we provide a back-of-the-envelope quantification of the benefits of better primary schooling. While any such exercise requires strong assumptions, we aim to be conservative. We assume all the wage benefits of improved learning in primary school operate through the causal effects on years of schooling, even though improved learning may have additional effects over and beyond these "Mincerian returns." Since the vast majority of women do not work, we focus only on benefits to male wages, even though attending a 1 sd better school almost doubles female labor force participation. Furthermore, we ignore all potential non-pecuniary benefits to better schooling, although there is a growing literature suggesting that education has other non-wage benefits (for example, Andrabi et al. (2012) on maternal

[8]Measuring earnings for women is difficult because some women report not being employed but then have small, part-time earnings from tutoring or handicrafts. We did collect data on earnings in these cases for women, but did not ask questions about earnings for men who reported being unemployed. To maintain consistency in our regressions, we do not include these women in the logged income regression. However, unsurprisingly, given the very small sample size, including all women with positive earnings leads to different point estimates ($\beta = 0.201$, se=0.918 for the baseline specification and $\beta = -0.113$, se $= 1.029$ for the specification with SES controls).

Table 8: Log Income and SVA

	All		Men		Women	
	(1) First Sch. FE	(2) + SES	(3) First Sch. FE	(4) + SES	(5) First Sch. FE	(6) + SES
Empirical Bayes SVA	0.090	0.093	0.108	0.110	2.501**	1.749**
	(0.083)	(0.079)	(0.082)	(0.078)	(0.992)	(0.802)
Mean Outcome	4.865	4.865	4.912	4.912	3.822	3.822
Adjusted R^2	0.141	0.150	0.054	0.060	0.551	0.723
Observations	1191	1191	1122	1122	32	32
Clusters	254	254	232	232	15	15

Notes: The dependent variable is log monthly earnings for individuals who report working. *Empirical Bayes SVA* is the average mean empirical Bayes school value-added of the primary school(s) the individual attended between 2003 and 2007. All columns include age fixed effects and a fixed effect of the first primary school the individual attended. Socioeconomic status (SES) controls in even columns include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index of households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). Standard errors are clustered at the first school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

education and children's educational outcomes in our context or Cutler and Lleras-Muney (2006) on the potential link between education and health).

To quantify the effects of better primary schooling, we estimate the present discounted wage benefits of moving all the *boys* in the worst school in the average village to the best school. Note that this will result in much lower gains than moving *all* boys to the best school in their village, as doing so would affect many more children. Combining the average gap between the worst and best school in a village estimated by Andrabi et al. (2025) with our estimates in Table 2, we estimate that moving a boy from the worst to best school in the average village would increase years of schooling by 1.2. In our data, average monthly male wages for males are 186 USD, and we assume a Mincerian return of 9%. If we assume the switch occurs when a student is in 3rd grade (in line with the fact that we typically observe students for 3 primary school years in our data) and that the student begins working at age 18, after 30 years of working, applying a discount rate of 0.95, this leads to a gain of approximately 2,500 USD for each male moved from the worst to best school in the village. As the average worst primary school has 74 boys in our first round of data collection, this generates gains *for just the average village* of 182,000 USD. Even though we have moved only a small fraction of children in the village, these are large effects; Pakistan's GDP per capita in 2023 was 1,400 USD.

This suggests that there are very large benefits to students attending better primary schools. However, a policy that moves all students to the best school may not be feasible (e.g., because of capacity constraints), and a policymaker may wonder if there are also potential benefits from improving the quality of existing schools as opposed to reshuffling students. Our estimates suggest that the benefits to "gold standard" interventions could indeed be great, but most interventions do not generate very large gains in learning for low costs. An alternative quantification considers the effects of the median educational intervention, which Evans and Yuan (2022) estimate is 0.1 sd. This effect is often thought of as small or moderate in the literature, given that it is only a small fraction of the learning gap between poor and rich countries, but our estimates of the long-run effects of increased learning in primary school allow us to actually quantify the gains from such an intervention. Assuming the intervention increases value-added by this much for three years of primary school, this implies an increase in years of education of 0.1 year, implying additional earnings of 20 USD per year based purely on the Mincerian returns. In present discounted terms, this would increase a student's income by 200 USD. So, if an intervention expected to have the median effects costs less than 200 USD per male student to implement, it would pay for itself purely from pecuniary benefits.

8 Conclusion

A large literature in development and education economics has focused on how to improve foundational learning in low-income countries. Yet, high rates of fadeout of cognitive gains, even in high-income contexts where rates of school attendance are higher and students are less casually attached to school, call into question the returns on these investments. This paper is the first to our knowledge to causally link gains in foundational learning – via attending higher quality primary schools – to longer-run educational and even early-stage labor market outcomes. To do so, in a low-income context with no administrative data, we take advantage of a unique fifteen year data collection effort that includes four years of panel test score data collection in primary school – facilitating the estimation of valid school value-addeds – and tracks individuals from childhood in 2003–2007 to adulthood in 2018. These unusually rich data allow us to both link school quality with long-run outcomes and to exploit potentially exogenous variation in the schools students attend to estimate causal effects.

In contrast to concerns about fadeout, children who attend higher SVA schools at the primary-level have greater cognitive skills as adults. This may at least in part be due to the fact that children who attend higher quality primary schools receive more schooling.

Attending a 1 sd better school increases educational attainment by 1 year, with gains that emerge in 10th grade (when students need to pass a high-stakes matriculation exam) and persist through secondary school completion and college enrollment. In contrast, these gains are *not* due to improved non-cognitive skills. Attending a better primary school – and in turn, receiving more education – has no effect on a composite measure of non-cognitive skills.

Turning to the labor market, we observe little effect on male labor force participation, which is already very high, though this may in part reflect the fact that attending a better primary school increases secondary schooling and college, which reduces observed employment due to an "incarceration effect" whereby men are more likely to still be enrolled. For women, whose baseline labor force participation is extremely low, this incarceration effect is limited, and attending a 1 sd better primary school almost doubles labor force participation. Foundational learning improves educational attainment, which in turn helps women access socially acceptable jobs as teachers in this highly gender biased setting. Our results suggest that even in settings where few jobs are available to women, education can be an important gateway for labor force participation.

While the fact that we observe young adults (and few women working) complicates estimating wage effects, our point estimates – though imprecise – are in line with Mincerian returns to education in this and other settings. This provides us with confidence in quantifying the gains to improved foundational learning under the strongly conservative assumption that all gains operate through male wages. Because our estimates allow us to link improved foundational learning with educational attainment and then earnings, we are able to more broadly characterize the earnings effects of interventions to improve learning in low-income settings. This allows for the calculation of rates of return on investment for learning interventions, which would have previously required heroic assumptions about the link between foundational learning and wages.

Altogether, our findings suggest that despite evidence of fadeout in other work, improving foundational learning has large and lasting effects, pointing to the value of investment in improving primary educational systems in low-income countries. Given that children in our context and many other low-income contexts live in settings with a high degree of choice, our results also point to the value of interventions that improve the allocation of students to higher quality schools.

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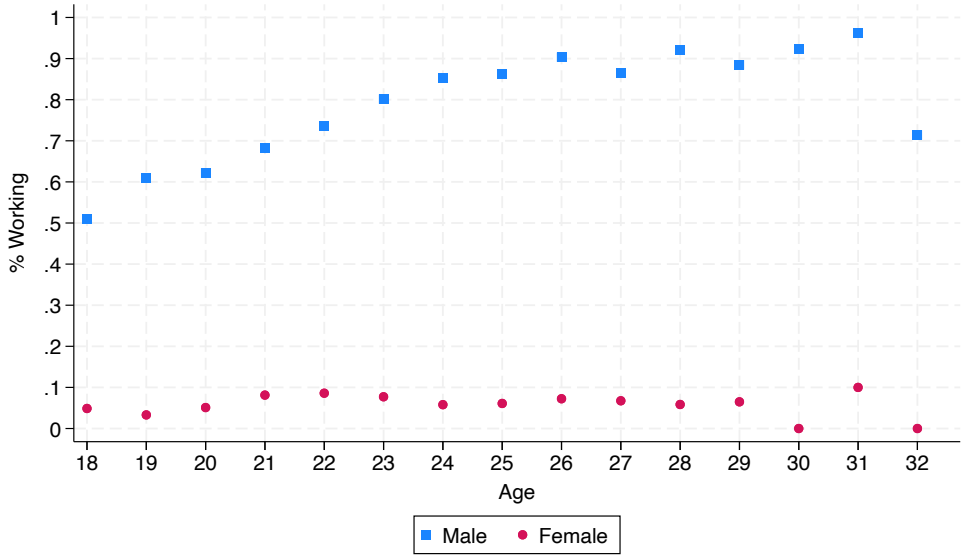
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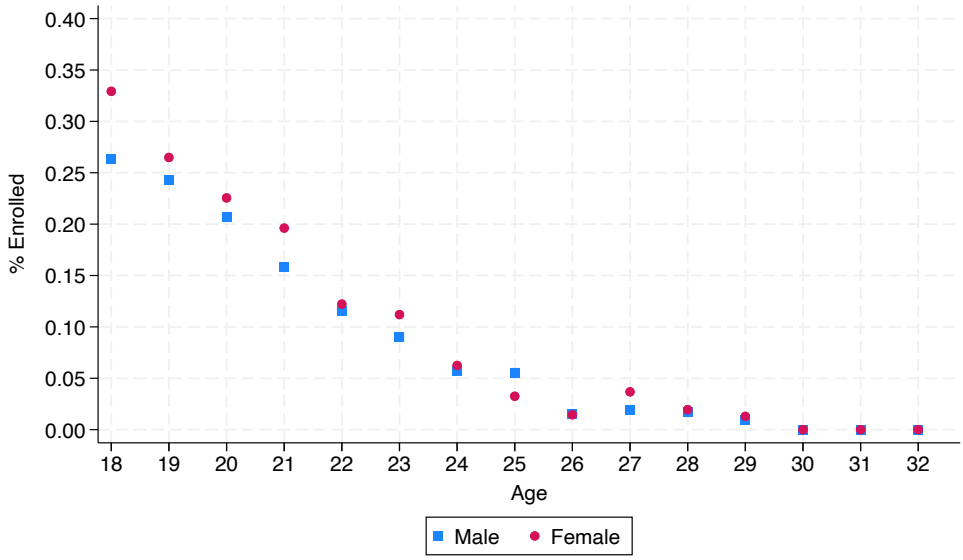
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A Appendix Figures

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



(a) Percentage of Individuals Employed



(b) Percentage of Individuals Enrolled

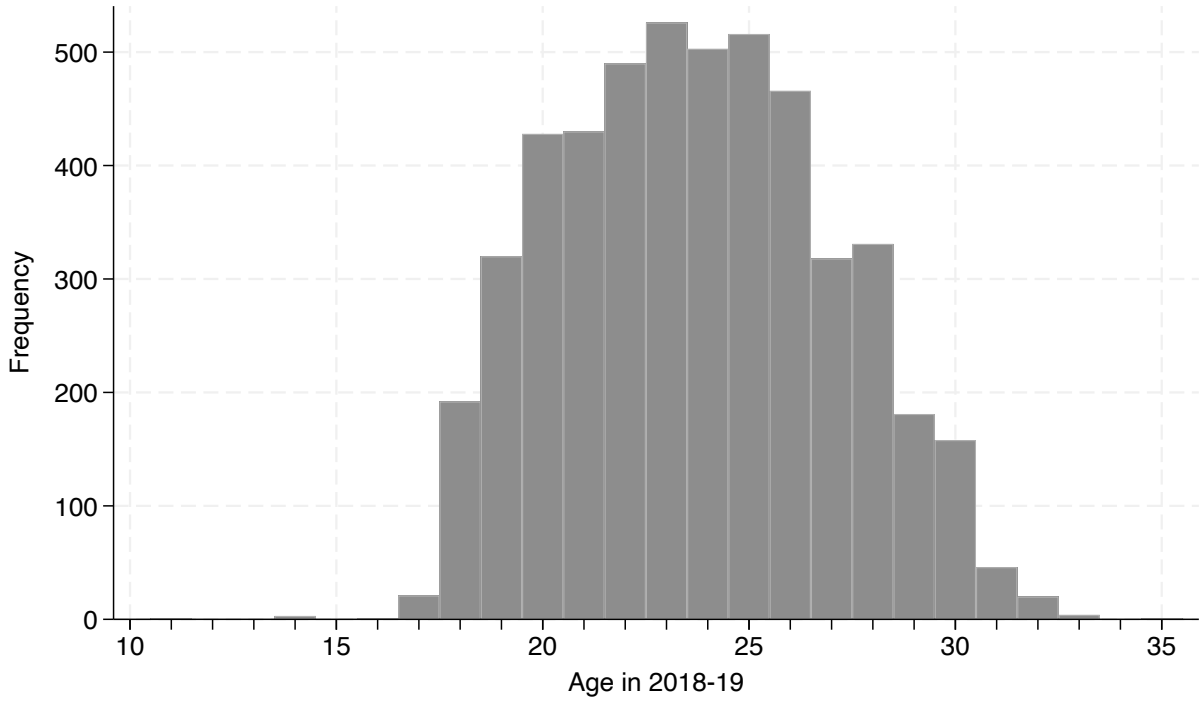
Notes: Panel (a): Percentage of men and women at a given age who have engaged in an activity in the past month for which they have received a payment. Panel (b): Percentage of men and women at a given age who are currently enrolled in school. The majority of individuals enrolled in school is in grade 12 (18%), or pursuing a Bachelor's (30%), or Master's degree (22%).

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	–	–

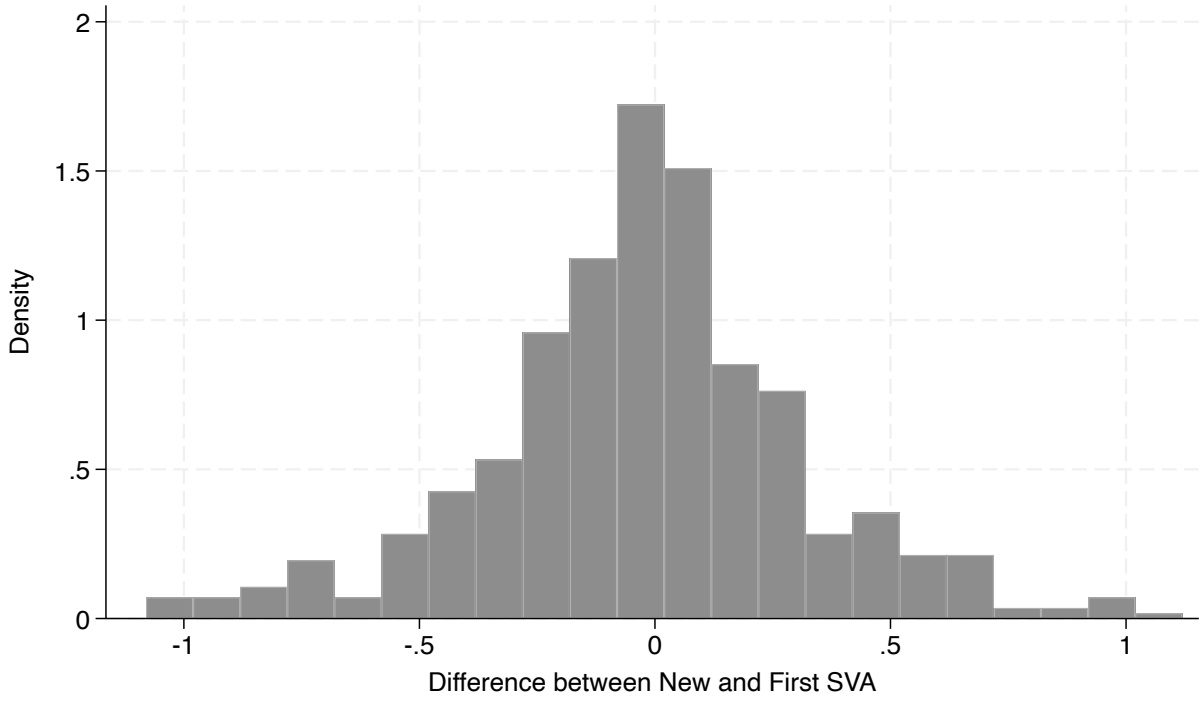
Notes: Timeline of LEAPS data collection for household and school surveys between the first round in 2003-04 and the last and sixth round of data in 2018-19. Check marks (✓) mean data was collected, and dash (−) means data were not collected. Data from the two tested cohorts is used to construct the empirical Bayes school value-added (SVA) estimates.

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



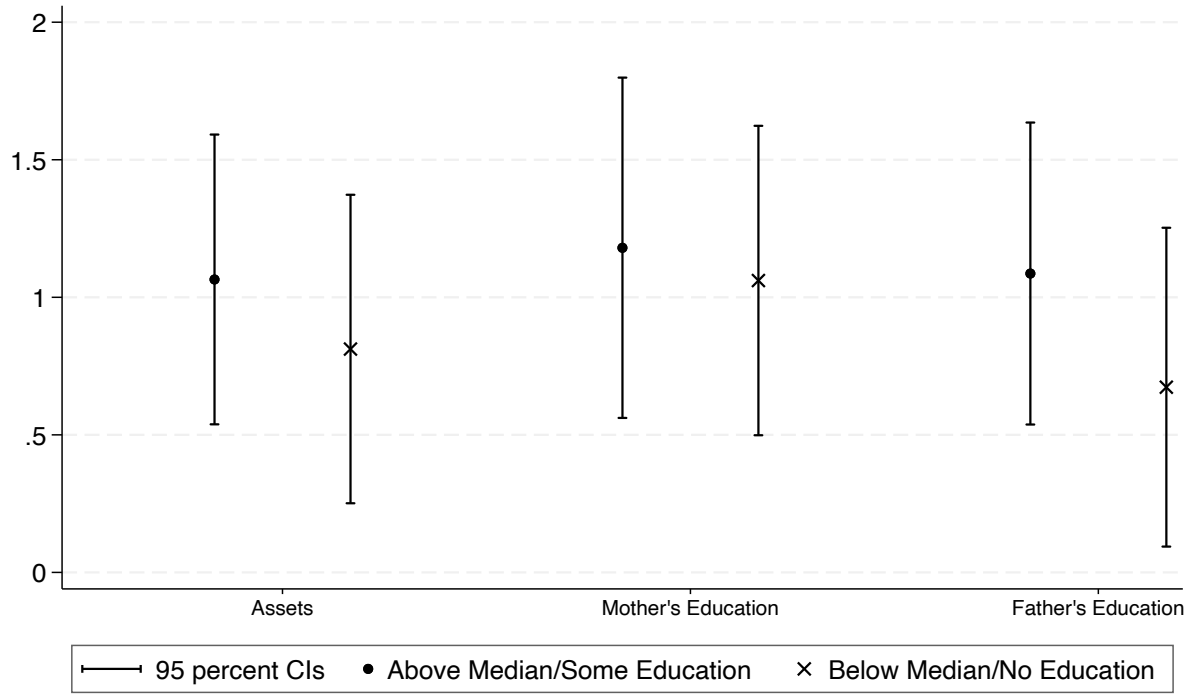
Notes: Histogram showing the frequency of individuals who completed the 2018-19 long-term follow-up survey by age. The sampling frame consisted of individuals who were between 5 and 15 years old in 2003-04.

Figure A4: Histogram of SVA Differences for Switchers



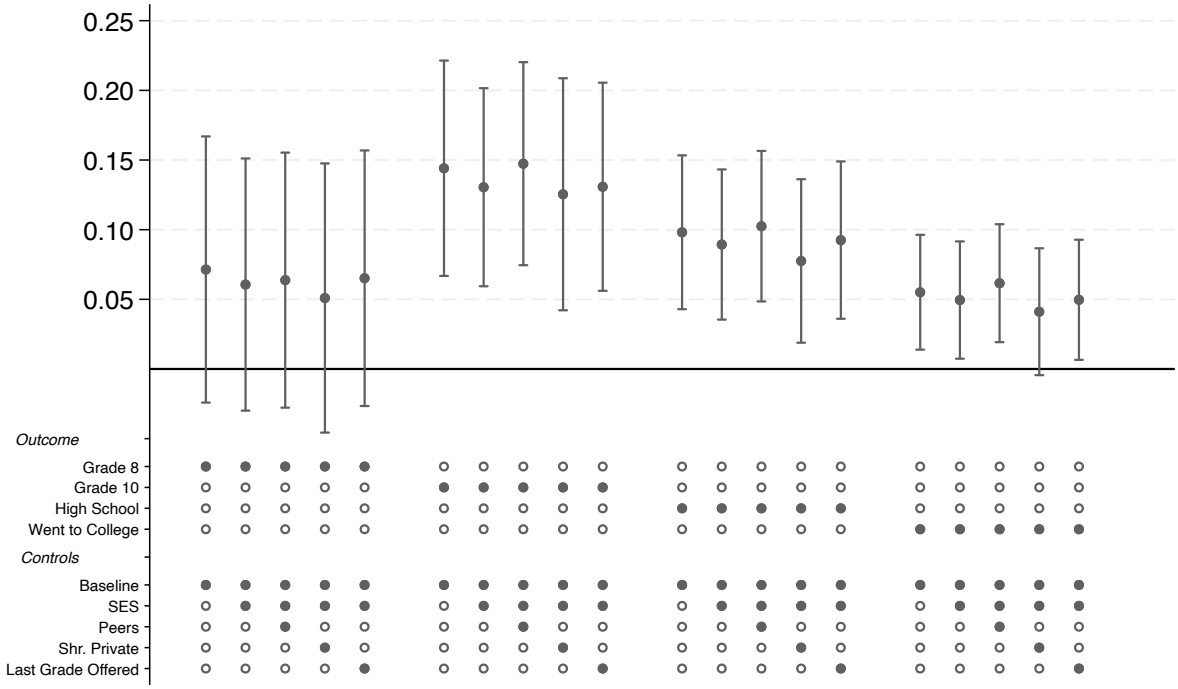
Notes: Histogram of the difference between the empirical Bayes school value-added (SVA) estimate of the second primary school switchers attend and the SVA estimate of their first primary school. The sample includes 563 switchers.

Figure A5: Heterogeneous Effects of SVA on Years of Education



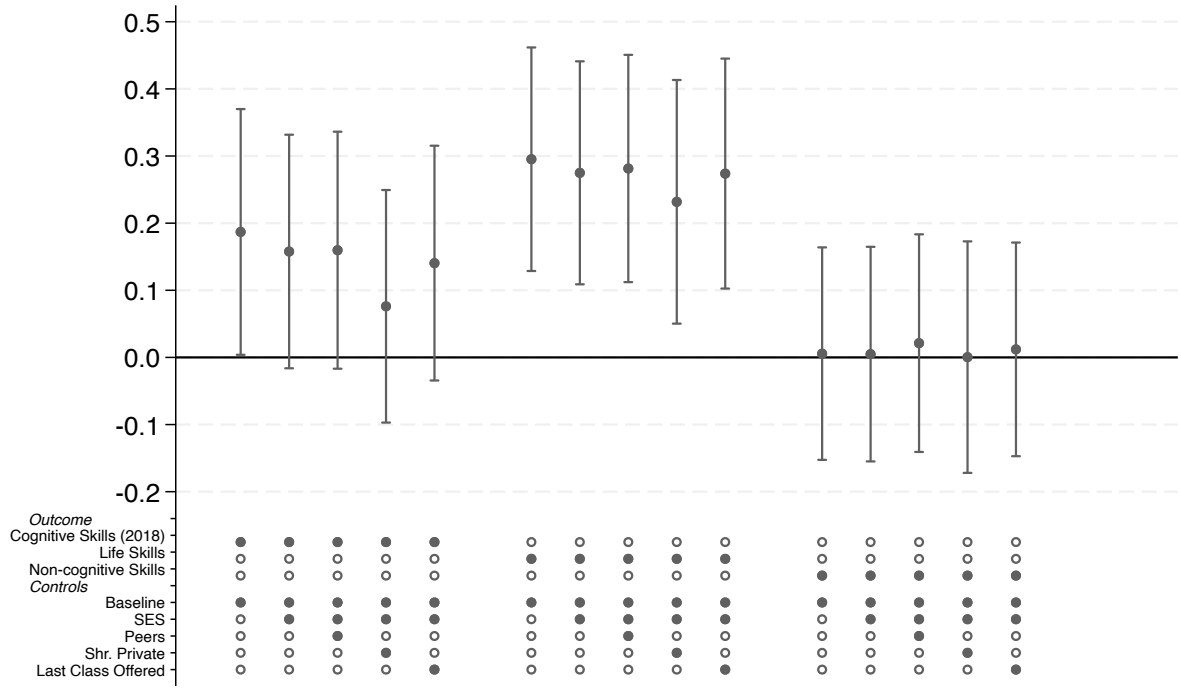
Notes: The dependent variable is the number of years of education completed. The graph plots the point estimates of the interaction between empirical Bayes school value-added (SVA) and a measure of socioeconomic status. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. The three measures for socioeconomic status are: assets (first two bars), mother's education (middle two bars), and father's education (last two bars). Assets is the first principal component analysis from household assets in 2003 weighted by the number of household members split by above and below median. Mother's and father's education are split by whether the parent has some education or not. All regressions include age fixed effects, a control for gender, and a fixed effect of the first primary school the individual attended. Standard errors are clustered at the first school level, and 95 percent level confidence intervals are displayed.

Figure A6: Educational Attainment and SVA – Tests for Alternative Channels



Notes: The graph plots the point estimates of the impact of empirical Bayes school value-added (SVA) on educational attainment, controlling for alternative channels. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. The first four rows of bullets displayed at the bottom of the graph indicate the outcome: dummies 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 and a control for gender. Socioeconomic status (SES) controls include dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). *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). All regressions include a fixed effect of the first primary school the individual attended. Standard errors are clustered at the first school level, and 95 percent level confidence intervals are displayed.

Figure A7: Skills and SVA – Tests for Alternative Channels



Notes: The graph plots the point estimates of the impact of empirical Bayes school value-added (SVA) on skills, controlling for alternative channels. SVA is the average mean empirical Bayes SVA of the primary school(s) the individual attended between 2003 and 2007. 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 *Non-cognitive 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 dummies for whether the father has some education, the mother has some education, whether the household head is from a high caste, and an index on households assets (measured in 2003). Dummies for missing values are also included (1 percent of the data, except for father's education which corresponds to 10 percent of the data). *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 first school level, and 95 percent level confidence intervals are displayed.

B Appendix Tables

Table A1: Summary Statistics

	Women		Men	
	(1) Mean	(2) SD	(3) Mean	(4) SD
Age	22.498	2.729	22.790	2.765
High Caste	0.749	0.434	0.709	0.454
Assets	0.344	2.016	0.080	1.937
Mom Has Some Education	0.258	0.437	0.224	0.417
Dad Has Some Education	0.657	0.475	0.596	0.491
Years of Education	8.284	3.704	8.040	3.342
Enrolled	0.154	0.361	0.109	0.312
Went to College	0.141	0.348	0.094	0.292
Works	0.067	0.251	0.764	0.425
Ever Migrated	0.432	0.496	0.449	0.498
Is Married	0.474	0.499	0.256	0.437
Has Children	0.342	0.475	0.158	0.365
Observations	1425		1740	

Notes: This table shows the average characteristics of women and men in our analysis sample. Standard deviations are reported in even columns. High caste, assets, and parental education are measured in 2003-2007. All other variables are from the 2018-19 long-term follow-up survey.

Table A2: Descriptive Statistics for School Switches

Empirical Bayes School Value-Added (SVA)	Mean
First SVA	0.099
New SVA	0.077
Share of Switchers by School Types	
Public to Public	0.483
Public to Private	0.133
Private to Public	0.238
Private to Private	0.146
Observations	563

Notes: The top panel reports the mean empirical Bayes school value-added (SVA) of the first primary school switchers attended and of their second primary school. The bottom panel shows the proportion of different types of switches in the data.

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	-0.059	0.012	0.010	0.031	-0.011	-0.044
	(0.094)	(0.023)	(0.026)	(0.021)	(0.022)	(0.122)
Mean Outcome	-0.010	0.239	0.623	0.727	0.451	-0.542
Observations	3086	3060	2783	3087	3092	926

Notes: This table shows the characteristics of primary school switchers relative to stayers. The sample is students enrolled in grades 1 to 5. Assets is the first principal component analysis from household assets in 2003 normalized by the number of household members. All regressions include a fixed effect for the first school they attended. Standard errors are robust. The F-statistic (p-value) of a test that the characteristics jointly equal to 0 excluding first test score in column 6 is 1.16 (p=0.328) with 2,761 observations. If we include first test score, the sample size is 817 observations with a F-statistic = 0.30 (p=0.938).

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

	Outcome: New School's SVA	
	(1) SES	(2) + First Test Scores
Female	-0.013	-0.016
	(0.037)	(0.040)
Assets	0.003	0.003
	(0.007)	(0.007)
High Caste	-0.006	-0.008
	(0.034)	(0.034)
Mom some educ.	-0.001	-0.005
	(0.037)	(0.039)
Mom educ. miss. dummy	0.112*	0.098
	(0.068)	(0.064)
Dad some educ.	0.006	0.009
	(0.035)	(0.034)
Dad educ. miss. dummy	0.086**	0.081*
	(0.041)	(0.043)
First test score		0.035
		(0.028)
First score miss. dummy		0.013
		(0.035)
F-statistic	1.359	1.591
P-Value	0.223	0.118
Mean Outcome	0.099	0.099
Observations	406	406

Notes: This table reports the association between the switchers' characteristics and the empirical Bayes school value-added (SVA) of the new primary school they attend. For students who switch multiple times, the outcome is the empirical Bayes SVA of the first school into which they switch. Assets is the first principal component from a pca analysis of household assets in 2003 normalized by the number of household members. Three observations are missing for mother's education, and 11 percent of observations are missing for father's education. 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 missing dummy for missing values (75 percent). All regressions include a fixed effect for the first school attended. Standard errors are robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Event Study Coefficients for Primary School Switchers

	Mean Test Scores		**Assets**
	(1)	(2)	(3)
	Long-term Sample	Full Sample	Long-term Sample
SVA × Period - 2	-0.104	0.249	0.232
	(0.639)	(0.173)	(0.336)
SVA × Period - 1	–	–	–
SVA × Period 0	0.960**	0.986***	0.0502
	(0.447)	(0.126)	(0.270)
SVA × Period 1	1.881***	1.145***	0.128
	(0.586)	(0.164)	(0.313)
SVA × Period 2	0.973	1.267***	-0.501
	(0.780)	(0.261)	(0.414)
P-Value Period 0 = 1	0.929	0.911	
Number of Observations	321	3331	2237

Notes: This table shows the estimates of event studies for school-switching, where the outcomes are mean test scores (columns 1–2) and assets (column 3). The independent variables are mean empirical Bayes school value-added (SVA) of the new school the child switches to 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. The outcome in columns 1 and 2 is the mean test score. The assets measure is the first component of a principal component analysis of 25 indicator variables for households' assets ownership, normalized for the household size. All regressions include year, year of switch, and first primary school fixed effects. Standard errors are clustered at the individual level. Period - 3 is not displayed as the sample size is limited for the test score regressions (5 and 34 observations in columns 1 and 2 respectively). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: 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	0.061	-0.022	0.025	-0.042	-0.004	-0.402	0.045	-0.032
	(0.244)	(0.058)	(0.037)	(0.076)	(0.034)	(0.810)	(0.036)	(0.309)
Avg. SVA within 1.5km	0.195	-0.032	-0.073	0.111	-0.007	3.309	-0.042	0.800
	(0.476)	(0.130)	(0.056)	(0.105)	(0.066)	(2.083)	(0.079)	(1.091)
Avg. SVA within 1.7km	-0.410	-0.029	0.044	0.068	0.065	-2.822	0.005	-2.772*
	(0.541)	(0.151)	(0.082)	(0.151)	(0.057)	(2.267)	(0.077)	(1.474)
Avg. SVA within 1.9km	0.364	0.110	0.003	-0.094	-0.034	-0.648	0.072	1.919*
	(0.446)	(0.110)	(0.086)	(0.159)	(0.056)	(1.071)	(0.070)	(1.004)
Mean Outcome	-0.019	0.727	0.239	0.623	0.091	2.306	0.272	8.773
F-Statistic	0.347	0.486	0.473	0.495	0.488	1.011	1.194	1.256
Number of Observations	3159	3159	3132	2860	3165	3162	3132	3165
Number of Clusters	108	108	108	108	108	108	108	108

Notes: This table shows the correlations between the selected instruments from the lasso instrumental variable strategy and individuals' characteristics. The outcomes are constructed using data from 2003-04 household surveys. All regressions include controls for dummies for missing values of the average empirical Bayes school value-added (SVA) within different radii, village fixed effects, age fixed effects, age interacted with a gender fixed effect, controls for gender, distance to the village center, and distance to the village center interacted with gender. Standard errors are clustered at the village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.