What Did You Do All Day?: Maternal Education and Child Outcomes

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A B S T R A C T

Does maternal education have an impact on children's educational outcomes even at the very low levels found in many developing countries? We use instrumental variables analysis to address this issue in Pakistan. We find that children of mothers with some education spend 72 more minutes per day on educational activities at home. Mothers with some education also spend more time helping their children with school work. In the subset that have test scores available, children whose mothers have some education have higher scores by 0.23–0.35 standard deviations. We do not find support for channels through which education affects bargaining power within the household.

I. Introduction

Educating women is often viewed as the single most effective policy lever for improving incomes and impacting a wider set of human development outcomes in low-income countries. In their roles as mothers, women also pass on additional benefits of education to their children. This paper contributes to our understanding of such intergenerational links—an important part of the social returns to education—in three ways. First, we examine the benefits of maternal education for child outcomes at the low levels of education typical of many developing countries

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around the world. In 2000, the average woman aged 25 and older reported 3.2 years of education in India and Kenya, 0.5 years in Niger, 2.6 years in Guatemala, and 1.2 years in Pakistan (Barro and Lee 2000). By employing an instrumental variable analysis to capture potential exogenous variation in maternal education, we add to a literature that establishes the causal impact of maternal education on child outcomes at higher education levels (additional secondary or college education) in the United States and Europe. Second, we broaden our outcome measures to examine the impact of maternal education on child learning for a subset of our sample children that have test scores available. Finally, we report some progress on isolating the numerous direct and indirect channels through which maternal education might influence child outcomes. We demonstrate the importance of maternal and child time use from our especially collected data set in understanding the unique role mothers play in their children's lives.

Using primary data from rural Pakistan—a country characterized by low maternal levels of education—we examine the difference in child outcomes between mothers with no education (75 percent of mothers in our sample) and those with some education (10 percent report higher than primary education and 15 percent primary schooling or less).[1] Gender-segregated schooling allows us to use the availability of girls' schools in the mother's birth village at the time she was of school starting age as an instrument for her education. Because boys cannot attend female schools, this instrument affects only the mother's education levels rather than the joint education levels of mothers and fathers. We control for birth-village population—the main determinant of school placement—to account for village selection into school placement.

Instrumental variable (IV) estimates show significant impacts of some maternal education on time use and test scores. Children of mothers with some education (relative to those with uneducated mothers) spend more time on educational activities outside school hours. The effect is large—an extra 72 minutes per day—and closely aligned with results from Behrman et al. (1999). In addition, in households with no children above the age of 12 (where the mother is likely the primary caregiver), educated mothers spend an extra 40 minutes a day with children on schoolwork. We also find evidence that mothers with some education facilitate learning by employing other members of the household in helping in education and reading to children by an extra 4.64 hours per week. The children of mothers with some education also report test scores between 0.23 and 0.35 standard deviations higher with the caveat that as we have test score data for only a subset of children, this result requires controlling for additional selection issues. The results suggest that child *time use* is an important pathway through which mother's education affects learning, although caution should be applied in interpreting these results as a causal link between time use and test scores.[2]

1. The average years of education in our sample of mothers is 1.34 years, which accords well with the 1.21 years reported in Barro and Lee (2000) data set for females above the age of 25 in Pakistan.

2. Educated mothers could impact child learning through direct inputs (her own time), indirect inputs (for instance, child time), and efficiency changes leading to changes in returns to these inputs. Disentangling and directly measuring the impact of maternal education on "the production function as well as the production process" (Behrman 1997) requires additional exogenous variation.

We present supporting evidence that these effects are unlikely to arise from additional leverage "educated" mothers might have in the household's decision-making process, particularly on decisions that directly affect the child, such as enrollment. We do not find evidence for increased bargaining power due to maternal education. Nor do we find any impact on child outcomes that are more likely to require household-level (joint) decision-making. Maternal education does not increase spending on child-specific goods, and we are unable to detect an effect of maternal education on child enrollment, although the latter could also be due to a lack of precision in our enrollment estimates.

These results may at first seem puzzling given the small education margins and the poor learning environment in developing countries.[3] The answer to this puzzle, we believe, is that even this small amount of education could have a large effect on cognitive and noncognitive outcomes *relative to the comparison group of mothers with no education*. For instance, Burde and Linden (2011) present experimental evidence from Afghanistan that with just six months of education, there are large gains in test scores among girls. Returns to maternal education could be concave in the amount of education so that these gains are particularly high relative to the margin of additional schooling at the secondary level. While we do not test for concavity here, the hypothesis is consistent with previous findings from Behrman et al. (1999) for India.[4]

Our contribution rests, in part, in the attempt to address selection into schooling for mothers. The IV approach follows Currie and Moretti (2003) and Carneiro, Meghir, and Parey (2007), whereby we use the availability of girls' schools in the mother's birth village as an instrument for her education. We obtain the birth village using verbal recall in interviews with mothers, and match this with the census directory of villages and data on schooling availability. The first stage of this IV specification shows that the presence of a girls' school in the birth village of the mother at the time that she was of primary school-going age leads to a 11 point increase in the percentage of mothers with primary education. In years of education, the instrument adds 0.61 years of education for a mother. Given the very low levels of overall female education, this effect is fairly large. As in the previous literature, the correlation is robust to controls for maternal age (using a full set of mother's age indicators) and county of birth (*tehsils* in Pakistan) fixed-effects.[5] In addition, it is also robust to controls for birth village population—the main village characteristic, according to policy documents, that influenced the provision of primary schools in villages.

A prominent characteristic of the Pakistani educational environment leads us to believe that this instrument effectively captures the marginal effects of maternal education. Specifically, public schools in Pakistan are gender-segregated, and at the time that the mothers in our sample were of school age, the only viable schooling

3. See the TIMSS Report 2007; Andrabi et al. 2009 for Pakistan; Das and Zajonc 2009 or the ASER Report 2008 for India.

4. Behrman (1997) summarizes on this point: *"Schooling achievement by women beyond levels that enable literacy, however, are neither associated with higher levels of child study nor with enhanced value in the marriage market."*

5. A *tehsil* in Pakistan is roughly the administrative equivalent to a county in the United States.

options were public schools for girls. In previous applications of similar instruments in other countries, access to education affects the educational attainment of *both* girls and boys; discerning which of these channels affected the outcome in question is then critical. In our case, the presence of a girls' school affects only the educational attainment of girls (the mothers in our sample) thus varying only female education levels. We believe that this could somewhat attenuate the possibility that our results are driven by simultaneous improvements in the education levels of fathers, which would be the case if schooling availability were not gender specific.[6]

Encouragingly, the instrument also passes falsification tests of the type discussed in Currie and Morretti (2003). The presence of a girls' school in the birth village does not affect mother's education if it was built after she had passed primary school age. In our specifications, the effect of having a girls' school in the village of birth is large and significant for mothers who were seven years old or younger when the school was built (relative to no school), but is zero for mothers who got a school between the ages of 8 and 15, or the age of 15 onward. Furthermore, given gender segregation in public schools, we are also able to confirm that the presence of a *boys'* school in the birth village has no effect on mother's education. To the extent that similar village characteristics determine the construction of boys' and girls' schools, this offers further evidence in support of the exclusion restriction.

Nevertheless, this application of an instrument typically used with administrative data in high-income countries, does not fully account for all selection concerns. Specifically, including birth-village fixed effects considerably reduces the power of the instrument in the first stage and therefore the precision of the IV estimates (there are more than 400 birth villages and 1,400 mothers in our sample). Two sets of problems then arise. First, variation across villages used to identify the impact of school construction on maternal education could reflect unobserved village characteristics that simultaneously impact child outcomes today. Perhaps the villages that received schools were also those where the returns to education were highest and these are reflected in educational outcomes today. Second, it could be that the impact of schools operates not only through maternal education but through other channels that affect child outcomes today. Exposure to schooling in itself has an impact on child outcomes, independent of the impacts on maternal education.

We argue for the robustness of our results using five different strategies. First, we examine policy documents on the construction of primary schools and show that village population was the primary criterion for school placement. We demonstrate this empirically, and in all specifications include birth village population as an additional control. Second, we drop from the sample all villages that never received schools, which could be arguably different from those that received schools at some time. Third, we directly address the problem that overall school exposure could affect other aspects of the environment besides maternal schooling by controlling for school exposure (both linearly and in dummies). Fourth, we present estimates with birth-village fixed effects. Finally, we conduct a "regression-discontinuity" style ex-

6. To our knowledge, this is the first application of schooling availability at the birth-village level in a gender-segregated schooling setting as an instrument for parental education in a low-income country context.

ercise by restricting our sample to mothers in nearby age-bands (thereby lessening the need for age-cohort dummies) and a specification with a full set of birth-village fixed effects and one without birth-village fixed effects. The basic finding from these robustness exercises is that in all cases the coefficients of maternal education on child time use, maternal time use and child test scores remains qualitatively similar, although in some cases precision is reduced. We do not find evidence of systematic bias in our estimates.

These results contribute to our understanding of education in low-income countries in a number of different ways. Given low female labor force participation it is difficult to compute the rate of return to education for women in low-income countries. One way to capture these nonlabor market returns, particularly the intergenerational transmission of education, is through examining the application of time use data in household surveys. Furthermore, links between maternal education and child enrollment may be weaker than is commonly assumed. Our results on this front are similar to those from high (Behrman and Rosenzweig 2002; Black, Devereux, and Salvanes 2005) or low-income countries (Desai and Alva 1998) that attempt to address selection into maternal schooling and find little impact.[7] While this could be because our estimates are imprecise, there is a possibility that such a causal link is absent. Instead, we document a strong link between child *learning* and maternal education. This is of direct interest for policy since recent experiments have shown that government policy can increase enrollment (Fiszbein and Schady 2009) while methods of improving learning remain tenuous.

The remainder of the paper is structured as follows. Section II presents a brief literature survey, and Section III presents the data description. Section IV presents the econometric strategy. Section V presents the results and Section VI discusses robustness and limitations of our analysis. Section VII concludes.

II. Literature

In the classic model of human capital accumulation, family characteristics should not be causally linked to child outcomes in the absence of credit constraints (Becker 1985 and Becker and Tomes 1986). The literature departs from this neutrality result in two ways. In one strand, the unitary model of the household is discarded, so that husbands and wives "bargain" with the ultimate outcomes depend on the relative weight given to the preferences of the two parents. Maternal education increases maternal bargaining power and if mothers care more about child outcomes than fathers an intergenerational link may be established (Lundberg and Pollak 1993; Lundberg et al. 1997).

A second strand highlights the labor force participation channel. More educated mothers are more likely to participate in the labor force. If childcare is not fully

7. Black, Devereux, and Salvanes (2005) use data on compulsory education laws from Norway, and Behrman and Rosenzweig (2002) use twin studies from the United States. Behrman and Rosenzweig (2002) hypothesize that more educated mothers work more, and this has a negative effect on children. Mothers induced to complete more education under compulsory education laws are those who would have dropped out, and for these mothers, more education may not have an effect on children's outcomes.

contractible, then the effect of maternal education depends on the relative importance of higher income versus direct maternal presence, and the impact of maternal education on maternal presence. See, for instance, Cawley and Liu (2007) or Miller and Urdinola (2007) for evidence on the maternal *employment*-child outcomes link in the United States and Colombia. The link between maternal education, employment and child outcomes is unclear. Bianchi (2000) shows that in the United States, maternal time with children remained unchanged between 1965 and 1995, a period of dramatic increases in the labor force participation for women. Guryan, Hurst and Kearney (2008) show that while working women spend less time with children in the United States, more educated women spend *more* time with their children *and* work more—because they cut down on leisure activities and housework. They confirm similar patterns in 14 other countries for which these data exist. In contrast, Behrman and Rosenzweig (2002) argue that the child educational outcomes are *causally* worse for educated mothers in the United States probably because of lower maternal presence, but they do not present empirical evidence on the channels in their data.

In the Pakistani context, and, for that matter, in most low-income country contexts, both the bargaining and the labor force participation channels are likely absent. Women in our sample spend the bulk of their time (just under ten hours a day) on housework, and we will demonstrate that there is no causal link between female education and labor force participation; indeed there is no link at all between female education and time on work *outside* the house. If increases in bargaining power do arise solely from higher female earnings, the absence of significant levels of labor force participation among women in our sample means the bargaining channel is effectively shut off. Commensurate with this line of thought, we find no differences in self-reported decision-making regarding children's schooling between mothers with some education and mothers with none. Furthermore, the lack of work outside the house also closes the (potentially detrimental) channel of less maternal presence in children's lives.

Mothers with some education in our sample act within the domestic space they control by ensuring that their children study more, by spending more time with them, and by creating a nurturing learning environment which has a positive effect on their children's learning. As Behrman et al. (1999) postulate, when both channels of bargaining and of maternal presence in the household are closed, it is likely that the effects of maternal education arise from the direct productivity benefits of higher female education, although these findings are also consistent with increased maternal preference for education *or* more information about how much effort is required for children to learn. The direct productivity channel suggests that mothers spend more time with their children because their marginal product in the production of education is higher; children spend more time because maternal and child effort are complements in the production function.

One question is whether the increase in time that children spend studying is at the expense of "child labor." In particular, if the extent of child labor *also* represents the bargaining position of the parents, we should similarly expect to see little difference in the activities reported by children. Key to our understanding of these results is the concept of "idle" children—children who are neither in school, nor at work (see for instance, Bacolod and Rajan 2008; Ravallion and Wodon 1999). Pri-

mary-school age children in our sample do not spend most of their waking time outside school in housework or paid work; they spend the time playing: An average child in school spends 2.5 hours a day "playing"; an average child *not* in school spends just under 3.5 hours "playing." Given these large numbers, if a mother makes sure that her child spends an extra hour a day studying it does not necessarily imply a tradeoff with other "productive" tasks that the child is responsible for, and thus probably avoids any conflict with the husband. In practice, the compensation for the extra time studying is spread over all other activities thus countering fears that it might reduce child "play" time, an activity which itself could be healthy for the child.

Research on high-income countries differs both in the margin studied and the availability of data. As stated previously, the typical margin examined in the high-income literature is at a higher level of education usually secondary schooling. Thus the channels through which education affects child outcomes may be different in our case, where the margin is one of basic literacy.

They also differ in the availability of data. On the positive side, administrative data sets that allow for this kind of matching to birth county in high-income countries are much larger. For instance, Black, Devereux, and Salvanes (2005) use variation in the implementation of compulsory schooling laws across Norwegian municipalities in a sample of more than 300,000 observations; Currie and Moretti (2003) examine the availability of colleges in a mother' county of birth using more than 600,000 observations. In low-income countries, applications of a similar strategy with limited availability of administrative data necessarily constrain the set of outcomes that can be examined. Our study brings time use and test scores centrally into the debate on intergenerational links, but the insights developed here are possible only through especially collected data sets with smaller sample sizes. An inevitable tradeoff is the decline in power and precision in the IV estimates.

III. Data Description

We use a data set on households and children from 112 villages in three districts of Punjab—home to 56 percent of Pakistan's population. The Learning and Educational Achievements in Punjab Schools (LEAPS) study follows a panel of households from 2003–2005 in three districts of the province—Attock, Faisalabad, and Rahim Yar Khan. These districts represent an accepted stratification of the province into North (Attock), Central (Faisalabad), and South (Rahim Yar Khan). The villages were chosen randomly from the list of all villages with an existing private school and are therefore bigger and richer than the average village in these districts.[8] However, almost half of all children in rural Punjab now live in a village with at least one private school, like those in our sampling list-frame. We focus on the cross-section data from 2003.

8. See Andrabi, Das, and Khwaja (2008) for more details on the rise of low-cost, for-profit, secular private schools in rural Pakistan.

In addition to demographic and educational data, the survey collected detailed parental and child time-use data, which forms the basis for a series of estimations in the paper. These data cover 1,697 mothers with 4,331 children between the ages of five and 15 in these households.[9] Table 1 provides summary statistics for the variables used in the regression analysis; given the general paucity of time-use data in low-income countries, we describe the basic patterns in some detail below.

A. Mothers

A majority of mothers in our sample—76 percent—report not having gone to school at all, and less than 10 percent report any education beyond the primary grade-level. Consequently, the average number of years of education for mothers is 1.34 years, a number comparable to many other low-income countries (Barro and Lee 2000). Time use is computed from a question that reconstructs an "average day in the last week" and allows the respondent to flexibly specify activities and time-slots. For instance, a respondent may say that she woke up at 6 a.m., prepared breakfast, and then prepared her children for school till 7 a.m. After that, she cleaned from 7 a.m. to 9 a.m. and so on for the remainder of the day, till she went to sleep. To code the activities reported we used 11 different codes. Table 2, Panel A shows time allotted to different activities for mothers with no education and for those with some education across the five main codes, aggregating the remaining (entertainment, prayer, shopping, sickness, religious activity, and other) into the residual category of "other activities."

Immediately striking is the remarkable similarity in time use for most categories across mothers with some education and those with none. Apart from "rest," "housework" is the single largest category with both educated and uneducated mothers reporting approximately 9.5 hours a day. In contrast to the considerable allocation of time to housework, mothers work for pay an average of 40 minutes per day and only 11 percent of mothers report any paid work at all (conditional on reporting any paid work at all, mothers report just over six hours a day on this activity). The average time spent on paid work is slightly higher for uneducated mothers (44 versus 29 minutes/day) compared to mothers with some education. Time spent "looking after children's needs" accounts for 1.5 hours a day and is, again, very similar across education levels—93 minutes and 99 minutes for uneducated and educated mothers, respectively. Where we do see some difference across maternal educational levels is in the time spent on children's educational needs, distinct from "looking after children's needs," which in the questionnaire was explicitly defined as distinct from "looking after children's educational needs." The educational needs category was also defined broadly so that it includes not only time helping with school-work but also time spent ensuring the child sits down each night to finish their work or to help the child get ready for school. The time spent in this category is virtually zero for uneducated mothers and 20 minutes a day for mothers with some education. These numbers from Pakistan accord well with previous work in rural India, where

9. Birth village information was not available for all these mothers. Summary statistics are provided for the matched sample of 1,437 mothers. We return to the matching issue below.

Table 1
Summary Statistics

	(1) Mean	(2) Standard Deviation	(3) N
All children			
Age (years)	9.97	2.93	4,331
Female (fraction)	0.47	0.50	4,331
Enrolled (fraction)	0.76	0.43	4,331
Educational activity (minutes/day)	117.30	88.98	4,331
Paid tutorials (minutes/day)	14.36	39.11	4,331
Study (minutes/day)	57.83	60.22	4,331
School preparation (minutes/day)	45.11	34.97	4,331
Enrolled children			
Public schools (fraction)	0.71	0.45	3,305
Mother responsible for the choice of school (fraction)	0.07	0.25	3,270
Mother responsible for the enrollment decision (fraction)	0.10	0.31	3,270
Does anybody help the child or read to him/her? (Y/N)	0.38	0.49	3,149
Time anybody in the family spent helping child in education or reading to him/her (hours/week)	3.00	4.81	3,149
School expenditures (rupees/month) paid tutorials	9.40	34.72	3,124
School fees	31.12	55.93	3,124
Supplies	35.90	26.07	3,124
Uniforms and shoes	41.40	26.34	3,124
Mothers			
Education (years)	1.34	2.75	1,437
Educated (Y/N) (fraction)	0.24	0.43	1,437
Age	37.87	7.56	1,437
Girls school present in birth village by age 7	0.55	0.50	1,437
Time spent on children's general needs (minutes/day)	94.47	141.29	1,437
Time spent helping child with schoolwork	5.05	22.77	1,437
Was any time spent helping child with schoolwork? (Y/N) (fraction)	0.06	0.23	1,437
Paid work (minutes/day)	40.67	131.71	1,437
Any paid work (Y/N) (fraction)	0.11	0.31	1,437
Working time outside the house	99.33	157.58	1,437
Working time inside the house	501.88	225.67	1,437

Notes: Data from the LEAPS survey of households in 2004. "Paid Tutorials" are expenditures on private tuitions outside school. All expenditures are reported in Pakistani Rupees (Rs.) (at the time of the survey, $1 was approximately Rs.65).

Table 2
Time Use, Maternal Education And Learning (Minutes/Day)

	(1) Mother Not Educated (N = 1,096)	(2) Mother Educated (N = 341)	(3) Difference in Means
Panel A: Maternal Time Use			
Rest	608.69	575.5	33.19
	(5.77)	(7.4)	(2.98)
Housework	560.46	596.2	−35.75
	(7.49)	(10.62)	(−2.44)
Paid work	44.28	29.08	15.20
	(4.23)	(5.38)	(1.86)
Children's general needs	93.16	98.67	−5.50
	(4.29)	(7.54)	(−0.63)
Children's educational needs	0.3	20.32	−20.02
	(0.17)	(2.29)	(−15.29)
Other	137.82	124.37	13.45
	(5.84)	(6.98)	(1.20)
Mother's working time outside the house	113.40	54.10	59.29
	(6.25)	(5.02)	(6.14)
Mother's working time inside the house	475.71	563.15	−87.44
	(7.79)	(14.05)	(−5.47)
	Child Not Enrolled (N = 1,026)	Child Enrolled (N = 3,305)	Difference in Means
Panel B: Child Time Use			
Rest	650.92	598.83	52.09
	(4.89)	(2.10)	(11.20)
School	0	356.27	
	(0)	(1.36)	
Housework	197.31	24.14	173.17
	(8.23)	(1.17)	(34.37)
Paid work	62.46	1.57	60.88
	(5.68)	(0.35)	(18.87)
Educational activities	8.74	151	−142.25
	(1.14)	(1.25)	(−61.00)
Play	278.48	154.63	123.85
	(8.41)	(2.05)	(20.80)
Other	226.30	153.57	72.73
	(7.73)	(2.60)	(11.45)

(continued)

Table 2 *(continued)*

	Mother Not Educated (n = 503)	Mother Educated (n = 203)	Difference in means
Panel C: Maternal Education and Child Test Scores			
English	−0.17	0.24	−0.41
	(0.04)	(0.07)	(−5.10)
Math	−0.12	0.11	−0.23
	(0.05)	(0.07)	(−2.64)
Urdu	−0.11	0.21	−0.32
	(0.04)	(0.06)	(−4.12)

Notes: Mean and Standard errors in parentheses, $t-$statistics in parentheses for difference in means. Time allocations are based on a flexible time use surveys where respondents tell the surveyors what they did in a typical day during the last week. Test scores are from a school-based test administered to all enrolled children in Grade 3 in the village. The results reported are Item-Response scaled scores where the distribution was standardized with respect to the universe of all test takers. Numbers reflect standard deviations from the mean.

mothers were spending no more than 90 minutes a day on all types of childcare (Desai and Jain 1994).

Because some paid work is done inside the house and some housework could be outside the house, it is further useful to classify total work done—both paid and unpaid—as inside versus outside the house. We do so using subcategories such as cooking, cleaning, livestock, and unpaid farm work for every main category in our survey. Women's lives are centered largely inside the house. A typical mother's working day involves spending more than eight hours a day inside the house and one hour and 40 minutes outside. If anything, mothers with some education spend less time outside the house (one versus two hours) and more time inside (9.5 versus eight hours) than uneducated mothers. This paucity of paid work and, more generally, the fact that the bulk of work is inside the house has implications for bargaining models of household decision-making that rely, quite literally, on "outside" options.

A comparison with the United States is of interest. The Americans' Time Use survey data between 2003 and 2006 show that the average mother spends 13.96 hours a week, or just under two hours a day on all types of childcare—a number roughly comparable to our categories of "looking after children's needs" and "children's educational needs." Of this, the bulk of time is spent on "basic child care"—such as feeding, medical care, and putting a child to sleep. And 2.1 hours a week, or just under 20 minutes a day is spent on "educational child care." The gradient of time spent on childcare with respect to maternal education is positive and significant—women with 16 + years of education spend 9.7 hours more every week on childcare relative to high school dropouts (Guryan, Hurst, and Kearney 2008). In contrast to the broad similarity on time spent in childcare, Pakistani women allocate a lot more of their time to housework. *Nonemployed* women in the United States spent just over 3.5 hours a day on housework in 1995 and employed women 2.5 hours a day—less than a quarter of the time spent by women in our rural Pakistani

sample (Bianchi and Robinson 1998–99). The difference in time allocated to childcare is then perhaps not as large as we may have imagined, given the dominance of housework in the Pakistani context.[10]

B. Children

The survey also covered every child between the ages of 5–15 in the sampled households for a total of 4,331 children. The mean age for a child in our sample is 10 years and 47 percent of the sample is female. Overall child enrollment is 66 percent with girls ten percentage points less likely to be enrolled than boys; given our list frame and the private school explosion in rural Pakistan (see Andrabi, Das, and Khwaja 2008), 30 percent of the children in our sample are enrolled in private schools.

Child time use was reported by parents for a typical school-day in the previous week. The main categories are rest, play (unstructured, unsupervised, leisure time), time in school, time spent on educational activities (school preparation, homework, formal tutorial sessions), paid work, housework and the composite category, "other," which is a sum of time spent on religious activities, media entertainment, and any other time. Apart from "Rest," "Play" is the largest component of the out-of-school day for the children, averaging 183 minutes, followed by educational activities at around two hours per day. Less time is spent by children on housework and paid work, at 65 and 16 minutes respectively. Given the concern in the literature on issues of child labor, both inside the house and in paid work, the comparison of these categories for enrolled and out-of-school children is of interest. Table 2, Panel B shows that children who are out-of-school do spend more time on housework—197 minutes for out-of-school children compared to 24 minutes for enrolled in housework. The extent of paid work is fairly low with enrolled children reporting virtually no paid work and out-of-school children reporting 62 minutes a day. Of particular interest is that "play time" for both enrolled and out-of-school children is the single largest time-use category of these three in the children's daily lives. Out-of-school children report 278 minutes per day, but even enrolled children report more than 150 minutes per day of play time. This is very much in line with Bacolod and Ranjan's (2008) emphasis on idle time as a third category to be taken into account when discussing the tradeoffs between child labor and enrollment.

Variation in time use by the child's age and the educational status of the mother shows several noteworthy patterns (Figure 1). First, consistent with other studies of time use (deTray 1983), children spend more time on housework and on paid work as they age; they also spend less time on play so that, by the time they are 15 years old, play time has dwindled to less than 60 minutes, as compared to 300 minutes when they were 5. For children in their teenage years, the burden of housework is quite high. In our companion work, Andrabi et al. (2009), we discuss the issue of housework and show that this increase in housework is largely concentrated among

10. The Americans' Use of Time survey classifies childcare as a primary or secondary activity, where the former is child spent exclusively with children while the latter includes time spent on multiple tasks, one of which is with children (such as cooking while supervising homework). The usual caveats of comparability across surveys with different questions thus apply in force.

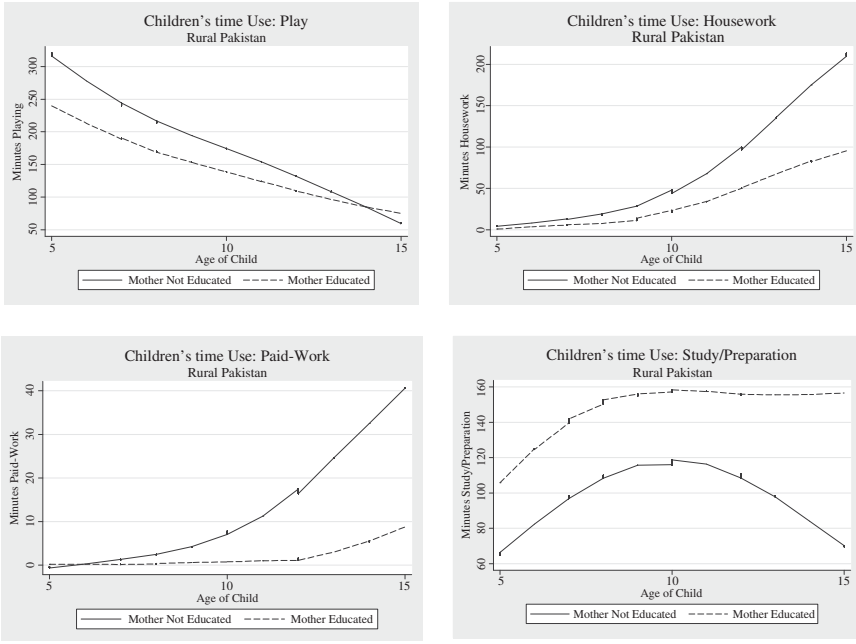

Figure 1

Children's Time use and Age, and Maternal education

Notes: The figures show nonparameteric graphs relating child time use with age for the sample of mothers with some education relative to those with no education. To focus on the relative differences between these groups, the vertical axis are different across the graphs. Child time use is based on a time allocation module completed for every child through the LEAPS survey in 2003.

teenage girls who are out-of-school. This issue demands a separate, more focused explanation. Second, children of educated mothers spend less time on housework, paid work and play time, largely because the gradient of time spent in housework and paid work with age is lower for them. Third, these children spend significantly more time across all ages on educational work outside the school. The difference is consistent across ages, averaging about 40 more minutes a day up to age 10 and more so after that—for children with uneducated mothers, homework time declines quite sharply after age 10, while it remains constant for children with educated mothers.

We also have information from a direct set of questions that ask whether any family member has spent time helping in educational activities or reading to the child. This is perhaps the most narrow question in that it captures more direct educational inputs (rather than broader educational needs) but we expand it to include all family members since we believe that such direct inputs are more likely provided by older (more literate) siblings. However, to the extent that these inputs also increase with maternal education, we feel this is also an effect induced through the

mother, that is, the (educated) mother helps her child's educational outcome (also) by encouraging her older children/more literate family members to directly help the child.

These data on the time that children spend in study at home combined with the relatively small direct involvement of mothers' time on their children's education suggest that mothers with some level of education, even when they are not directly involved in a child's home study, are creating a space for these children to focus on their schoolwork.

To assess the link between maternal education and child outcomes, in the absence of any national tests, we used school-based testing to assess all enrolled children in Grade 3 in the village. These children were tested in the subjects of English, Urdu (the vernacular) and Mathematics. We then matched the children who were tested in the school to children in our household survey, eventually yielding a sample of 676 children for whom we have both test scores and household survey data. We use item-response-scaled scores as our measure of learning achievement—the measure is standardized to have a mean of zero and variance of one in the full sample of tested children.[11] Table 2, Panel C shows a strong correlation between maternal education and child test scores for this smaller sample. The difference of 0.43 standard deviations in English, 0.25 in mathematics, and 0.35 in Urdu corresponds to roughly one additional year of learning in these villages.

IV. Econometric Specification and Identification strategy

To identify the variation in maternal education exogenous to her ability, we follow an established literature first proposed by Card (1999) that uses maternal access to a school in her *birth* village at the time of her enrollment decision as an instrument for educational attainment. The exclusion restriction requires that presence of a school affects the outcome variables only through mother's education and not through other mechanisms such as changing social norms.

Our econometric specification for the first stage is as follows.

$$(1) \quad MotherEducated_i = \beta_0 + \beta_1 SchoolPresent_i + \beta_2 X_i + \varepsilon_i$$

As mentioned earlier, a large mass (76 percent) of mothers have never gone to school (zero years of schooling). Therefore, we use a binary measure, "*MotherEducated*," that takes the value one if the mother reports nonzero years of schooling and zero otherwise as our dependent variable. We also show the first stage for the continuous measure of mother's years of education but prefer the dichotomous measure, since it is less noisy than years of education and, given the large mass at zero, does not lead to any significant loss in variation. $SchoolPresent_i$ is an indicator variable that takes the value one if the mother had a girl's school in her birth village

11. IRT scores ensure that change in one part of the distribution is equal to a change in another, in terms of the latent trait captured by the test. All items were modeled using the three parametric logistic (3PL) item response function and estimated using BILOG-MG.

when she was seven or younger, and X_i captures additional conditioning variables that could affect both maternal education and child outcomes. The Government of Pakistan's guidelines use the age of six as the normal school starting age, but seven years is more reasonable given the widespread practice of delayed enrollment. A cutoff age higher than that is probably inaccurate since the enrollment window for girls in rural Pakistan is quite small. Nevertheless, our results are robust to small variations in the specific cutoff and increasing this cutoff to nine years does not change the results significantly.

We obtain the birth village using verbal recall in interviews with mothers, and then match this with the census directory of villages, the national census of schooling conducted by the Government of Pakistan and the Educational Management Information System data collected by the Government of Punjab and the National Education Census 2005. This allows us to obtain the year of establishment of schools in all villages in Pakistan.

We match 85 percent of all mothers (1,437 out of 1,697) with their birth-village information. The remaining mothers could not be matched either because the transliteration of the village name into English differed from that in the census directory or because a residential location rather than the official census village designation was provided by the respondent.[12] There is a possibility that more "able" mothers provide better verbal recall information, but we do not find any correlation between the probability of a match and village or maternal characteristics. Table A1 in the Appendix provides the means of several characteristics of the matched and unmatched samples; for all the variables, the differences are negligible.[13] In the matched sample used for all estimations, mothers are split evenly between those living in the same village in which they were born and those born outside the current village.

The main source of identifying variation, like in previous studies, is the exposure to schooling for a mother in her childhood age window. The institutional environment and the policy details of school construction help guard against potential violations of the exclusion restriction, suggesting specific conditioning variables for inclusion in the X_i vector. As a matter of policy, the Government of Punjab's public schooling system is segregated by gender at all educational levels, so that mother's education is sensitive to the availability of girls schooling in the village. Girls' school construction was ramped up during the Sixth five-year plan in the early 1980s as a part of the Social Action Programs. Nevertheless, they are less prevalent and of a later vintage than boys' schooling, allowing us to exploit variation over time in schooling opportunities (Figure 2).

One immediate issue with the expansion in school construction over the last three decades is that younger mothers will have greater exposure to schools at the time of their enrollment decision. Because other changes in the environment affecting enrollment are also time-varying, the first component of the X_i vector includes con-

12. The Surveyor General of Pakistan mapping information on localities does not follow the census village designation and has many localities marked that are not in the census list. A digital village area map for Pakistan does not exist.

13. Differences in age and family time in helping the child are significant at the 10 percent level, but both are qualitatively small.

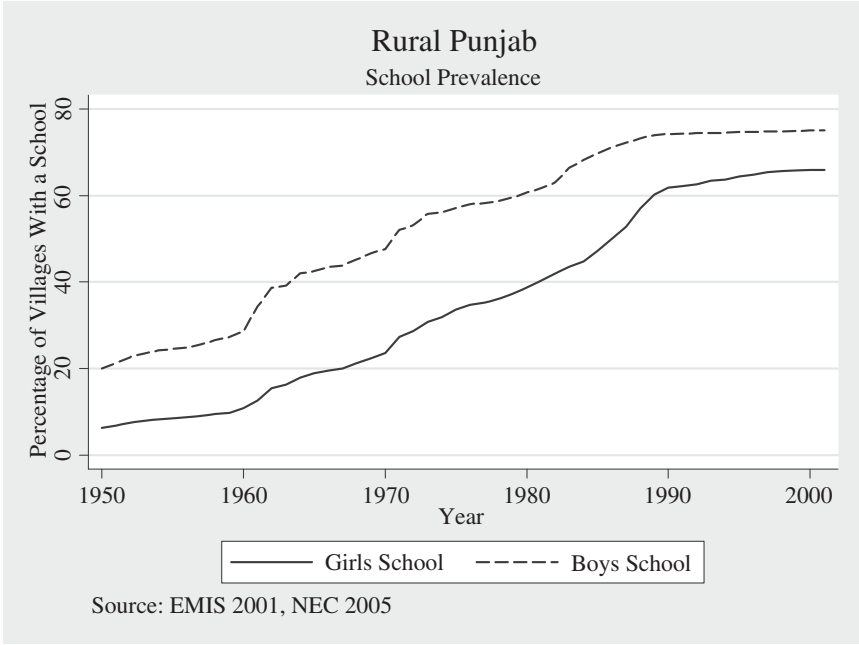

Figure 2

Notes: The figure uses Punjab wide data from the Educational Monitoring and Information Systems for Punjab province from 2001 and the National Education Census from 2005.

trols for maternal age with a full set of age dummies—one for each year—for all mothers in the sample. Second, schools may have been constructed in selected villages and unobserved characteristics of these villages could be correlated both with maternal education and current child outcomes. To partially account for this selection, the second component of our X_i vector is a full set of *tehsil* dummies, where a *tehsil* is the administrative unit one level below the district, roughly equivalent in size to a U.S. county. The province of Punjab, where our sample is drawn from, has 34 districts and 104 *tehsils* in the latest (1998) census. Because roughly 50 percent of our mothers were born in a different village, there are 72 different *tehsil* dummies in our specifications. Ideally (instead of the *tehsil*) we would have included birth-village dummies so that identification is based only on variation arising from maternal age at the time of school construction. However, including birth-village dummies (more than 400) in our first stage significantly reduces the power of the instrument. This raises the concern that unobserved characteristics of villages that received schools were correlated both to maternal education and to child outcomes today or that school exposure in and of itself has a direct impact on child outcomes independent of maternal education.

In our main specifications we attempt to account for this unobserved variation by taking cognizance of the official Government policy outlined in various program

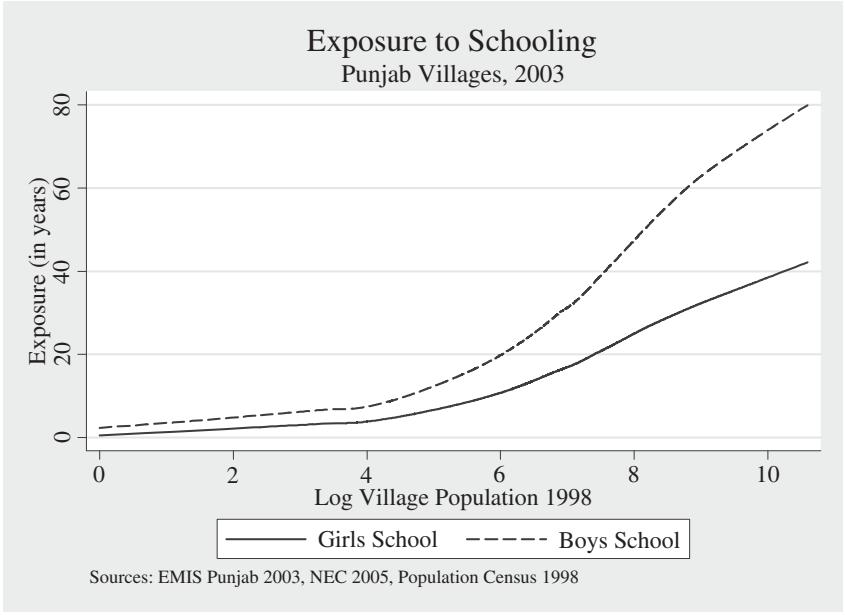

Figure 3

Notes: The figure uses Punjab wide data from the Educational Monitoring and Information Systems for Punjab province from 2001 and the National Education Census from 2005. The curve is a locally weighted smoothed estimate using the STATA LOWESS command.

documents. In these documents, village population was used as the main criterion for school construction: *"Primary schools will be established in those areas where population of school age (boys and girls) is at least 80, the total population catchment area is at least 1,000 and that a middle/primary school does not exist within a radius of 1.5 km of the school."*[14]

Figure 3 shows that exposure to both girls and boys school increases with village population suggesting that the rule was largely followed in practice as well. Therefore, the third component of the X_i vector is the (log of) birth-village population.[15] To the extent that this picks up salient dimensions of the unobserved heterogeneity in village characteristics, it should strengthen the case for the validity of the exclusion restriction. If even after controlling for village population, school construction was correlated with unobserved birth-village characteristics that were then transmitted to the mothers and children, our estimates will be biased. In the robustness section, we present a number of different strategies to address this issue; the takeaway message from these additional checks is that the small sample size reduces

14. The Planning Commission, Government of Pakistan, Manual of Development Projects
15. The birth-village population could not be obtained for 12 mothers reducing the sample size in regressions to 1,425. This has no effect on any regressions or results.

efficiency when birth-village dummies are included but the main estimates are likely unbiased. The second stage regressions are then specified as follows.

$$(2a)\quad MotherOutcomes_i = \gamma_0 + \gamma_1 MotherEducated_i + \gamma_2 MotherAge_i + \gamma_3 BirthTehsil_i + \gamma_4 BirthVillagePop_i + \xi_i$$

$$(2b)\quad ChildOutcomes_{ij} = \alpha_0 + \alpha_1 MotherEducated_i + \alpha_2 Age_i + \alpha_3 BirthTehsili_i + \alpha_4 BirthVillagePop_i + \alpha_5 ChildAge_{ij} + \alpha_6 ChildGender_{ij} + \varepsilon_{ij}$$

Here, the subscript i refers to the mother and j to the child. Equation 2a is a mother-level equation. The variable *MotherEducated* is instrumented using the first-stage Regression 1 and estimated using 2SLS. The variable *SchoolPresent*, which captures the presence of a school in the birth village at the time of the enrollment decision, is the excluded variable from Equation 2a. *MotherAge*, *BirthTehsil* and *BirthVillagePop* are the same variables as in the first stage and are in the second-stage regression because of their potential direct effect on child-level outcomes. All specifications cluster for standard errors at the village-level. Equation 2b is run at the child level and adds indicator variables for child age and gender to the set of explanatory variables. In the 2SLS estimation, all child-related controls are also included in the first-stage regression.

Finally, we also present bivariate probit results with the same specifications as above for all the discrete variables used in the analysis. Although the linear IV specifications are unbiased in terms of the Local Average Treatment Effects (LATE), the efficiency of the IV estimator is low at the sample sizes that we are working with; the bivariate probit estimates buy us greater precision, but at the cost of assuming a standard bivariate normal distribution over the error terms in Equation 1 and the outcome equations.

V. Results

A. First Stage

Table 3 presents the first-stage regression and the results of two falsification tests to check the validity of our instrument. The first column uses mother's years of education as a dependent variable. The next three columns use an indicator variable for whether a mother is educated as the dependent variable; our IV results are presented using this indicator variable rather than the continuous version.[16] Column 2 runs the regression for all mothers in the sample, Column 3 for all enrolled children in the sample as some of our time-use variables are applicable only for enrolled children and Column 4 for all children in the sample to match the IV regressions specifications further below. Columns 5 and 6 present falsification tests.

16. Using the continuous version of maternal education gives similar results. Since 75 percent of mothers in our sample report no education, the difference between mothers with some and mothers with no education is the main source of variation in the data.

Table 3
First-Stage Regressions and Falsification Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mother Education (years)	Mother Educated (Y/N)					
					Mother level	Falsification	
	Mother Level	Mother Level	Child Level	Enrolled Child Level	Only Birth Villages That Got a Girls Schools At Some Point	Mother Level	Mother level
Girls school present by age 7	0.62***	0.12***	0.11***	0.12***	0.13***		0.10***
	(0.17)	(0.03)	(0.03)	(0.03)	(0.03)		(0.04)
Boys school present by age 7						−0.05	
						(0.04)	
Girls school present ages 8–15							−0.04
							(0.04)
Girls school present after age 15							0.00
							(0.04)
Observations	1,425	1,425	4,294	3,275	1,204	1,412	1,425
$R-$squared	0.16	0.17	0.17	0.18	0.19	0.17	0.17
F-statistic	12.73	17.29	15.53	14.13	14.22		

Notes: Robust standard errors in parentheses, clustered at the birth village level *, significant at $p < 0.01$; **, significant at $p < 0.05$; ***, significant at $p < 0.1$. Regression 1 uses mother's education in years of education as a dependent variable. All other variables use mother's education as a binary (y/n) variable. Regressions 1, 2, 5, 6, and 7 are at the mother level. Regression 3 is for all children and Regression 4 is at the enrolled children's level. The omitted variable in Regressions 1, 2, 3, and 4 is girls' school present after age seven or not present at all. Regression 5 drops villages where a girls' school was never present and the omitted variable is girls' school present after age seven. The omitted variable in Equation 6 is boys' school present after age seven or not present at all. The omitted variable in Equation 7 is girls' school not present at all. All regressions control for village (log) population, a full set of mother's age indicator variables and fixed effects for mother's birth *tehsil* (county).

Column 1 shows that a girls' school in the birth village increases a mother's years of education by 0.61 years. Given that the average years of education are 1.34, this is a large increase. Columns 2, 3, and 4 show that a girls' school in the birth village increases the likelihood of a mother reporting some education by 12, 11, and 12 percentage points respectively. The increased probability is both statistically significant and of a large magnitude since only 25 percent of all mothers reports any education at all. The instrument satisfies the criteria for detecting weak instruments discussed in Stock, Wright, and Yogo (2002), with *F*-statistics of 17.29, 15.53, and 14.13 in the three specifications we use in the second stage. [17]

To address the possibility that villages that received a school at some point are systematically different from those that never received schools, Column 5 drops all mothers in the sample whose birth villages never received schools. The results in this specification are quite similar to the base specification as the presence of a school in this reduced set of villages increases a woman's likelihood of going to school by 13 percentage points with an *F*-statistic of 14.22.

Columns 6 and 7 present two falsification tests. The first test reflects the sex-segregated nature of school provision in the province of Punjab. Unobservable village-level political variables or other factors that resulted in the construction of a girls' school could affect maternal education through other means than access to schooling. If the process of setting up boys' schools follows a similar but independent process, one might expect to see these direct effects to show up, at least to some extent in villages with boys schools. Column 6 presents the effects of the presence of a *boys'* school in the village by age seven on the mother's education. The coefficient is small (−0.05) and not significantly different from zero.

The second falsification test divides mothers into four categories—those that received a girls' school by age seven, those that received a girls' school between the ages of 8–15, those that received a girls' school after age 15 and those that never received one. Given the enrollment profiles for girls in Pakistan—increasing between ages five and ten and dropping quite steeply after that—a valid instrument should imply that receiving a school after the relevant age should not have any effect on enrollment. Column 7 shows that the mother born in a village that received the school by age seven, relative to one who never received it, is ten percentage points more likely to report some education. For those who received a school between the ages of 8–15 or after 15 years of age (relative to having never received it), the effect is small, of the wrong sign and insignificant at all conventional levels of confidence. The difference in coefficients between the first age group and the latter two age-groups is statistically significant at the 1 percent level. The difference between mothers born in villages that received a school after age 15 and between the ages of 8–15 is statistically insignificant.

B. Maternal Education and Child Outcomes

Our first set of results examines differences in child time use across maternal education levels. Our first outcome variable is the composite time spent by children on

17. Columns 3 and 4 present the results at child level and enrolled child level to correspond to second stage regressions at these levels.

studying outside of school, preparation for school and extra paid tutorials; we also present separate results for child time spent studying outside of school. We present results for all children and for enrolled children only.

Across the entire sample in the OLS specification, a child of a mother with some education spends 43 more minutes on educational activities outside the school (Panel A, Table 4). In the IV specification, the estimates increase to 72 minutes, with some loss in precision. To check that these results are not driven by an increase in enrollment, we estimate the same specifications for enrolled children only. The results are stable and in this subsample, the preferred IV estimate increases to 74 minutes. To calibrate this increase in terms of the overall distribution, the 72 minute increase in study time moves a child from the 25th percentile to roughly the 75th percentile in the time distribution, and is almost identical to the *point* estimates reported in Behrman et al. (1999). The result is driven to a large extent by actual time spent studying outside school (Panel B, Table 4). [18]

Panel C, Table 4 presents the result on whether the child was enrolled in school or not. We are unable to detect an effect of mother's education on enrollment or school choice even though there is a positive effect of maternal education on enrollment in the OLS regression. This echoes results found in developed countries, albeit at higher levels of education such as Black, Devereux, and Salvanes (2005), Behrman and Rosenzweig (2002) and a detailed survey by Strauss and Thomas (1995) on developing countries that points out the inconclusive nature of many studies relating child outcomes to maternal education.[19]

The final panel presents results on learning outcomes as measured by test scores. To assess whether maternal education has an impact on learning outcomes, we matched children in our household survey to those who were tested in school through our study in Grade 3, eventually yielding a sample of 738 children for whom we have both test scores and household survey data.[20] The key econometric issue that this poses, in addition to that arising from the selection into maternal education, is that the children for whom we observe test score data may systematically differ from children for whom test score data are not available. This arises both because some children are not enrolled, but also because children may be absent on the day of the test (10 percent of all children in the relevant grade were not administered the test due to absenteeism). Therefore, IV specifications followed for other outcome data may be biased if such selection is not accounted for. Following Angrist (1995), the test score equation is determined through a linear equation conditional on the existence of a test score observation and a censoring equation indicating whether the test score is missing. Thus, although presence of a school is a valid instrument for

18. We note that none of the other child time categories show any significant change in response to maternal education. The increased time we find for extra study is spread over all other activities and does not necessarily just come from play.

19. Given the relatively small sample, the precision of our estimates does *not* allow us to rule out that the OLS and IV coefficients are statistically the same, and therefore rule out the positive enrollment effects of maternal education.

20. We managed to match test scores for 64 children for whom the time use data was incomplete and were not included in the earlier regression sample. They are included in the test score regressions. Their test scores and other characteristics are statistically indistinguishable from the rest of the sample.

Table 4
Maternal Education And Child Outcomes

	(1)	(2)	(3)	(4)
Panel A: Child Time On All Educational Activity Outside School (Minutes/Day)				
		All Children		Enrolled Children
	OLS	IV	OLS	IV
Mother educated (Y/N)	43.07***	71.92*	19.79***	73.60*
	(4.36)	(40.26)	(4.56)	(37.87)
Observations	4,294	4,294	3,275	3,275
$R-$squared	0.19	0.18	0.18	0.08
$F-$test	.	15.31	.	14.05
Panel B: Child Time On Studying Outside School (Minutes/Day)				
		All Children		Enrolled Children
	OLS	IV	OLS	IV
Mother educated (Y/N)	29.05***	49.56*	18.25***	58.94**
	(3.48)	(28.72)	(3.71)	(29.88)
Observations	4,294	4,294	3,275	3,275
$R-$squared	0.14	0.12	0.15	0.07
$F-$test	.	15.31	.	14.05
Panel C: Child Enrolled (Y/N)				
	OLS	IV	Biprobit	
Mother educated (Y/N)	0.17***	−0.00	0.09	
	(0.02)	(0.18)	(0.07)	
Observations	4,294	4,294	4,294	
$R-$squared	0.201	0.175		
Panel D: Child Test Scores (English, Math and Urdu)				
		English		
	OLS	Control Function	Heckman	
Mother educated (Y/N)	0.35***	0.34***	0.34***	
	(0.10)	(0.11)	(0.08)	
		Math		
Mother educated (Y/N)	0.28***	0.23**	0.23**	
	(0.10)	(0.11)	(0.09)	

(continued)

Table 4 *(continued)*

	(1)	(2)	(3)	(4)
		Urdu		
Mother educated (Y/N)	0.33***	0.33***	0.33***	
	(0.10)	(0.10)	(0.08)	
Observations	727	711	4,266	

Notes: Robust standard errors, clustered at birth village level; *** significant at $p < 0.01$, **significant at $p < 0.05$, *** significant at $p < 0.1$. For discrete variables, we report both the IV coefficient and the Average Treatment on Treated using a biprobit specification. Girl's school present in mother's birth village is the excluded variable in the IV and the biprobit. All regressions control for birth village (log) population and full set of indication variables for mother's age, mother's birth village *tehsil*. In addition, Child-level regressions control for a full set of indicator variables for child's age and child gender. Test scores are from a school-based test administered to all enrolled children in Grade 3 in the village. The results reported are item-response scaled scores where the distribution was standardized with respect to the universe of all test-takers. The test score results are presented as standard deviation changes. The Heckman selection and control function estimates use the distance to school as an additional excluded variable in the determination of testing results.

maternal education, it is not a valid instrument in equation for selection into the test scores.

There are two potential solutions. One approach proposed by Heckman and Robb (1986) and developed by Ahn and Powell (1993), uses the "control-function" approach, where we condition on the predicted probability in the test score equation. In essence, this method proposes to estimate the effect of maternal education by using pair-wise differences across children for whom the nonparametric probability of participation is very close. The approach is implemented by first estimating the censoring equation directly, and then including the predicted probability of participation and its polynomials as additional controls in the test score equation. The other is to follow Heckman (1978). If we assume that errors are jointly normally distributed, homoskedastic and independent of the instrument, we obtain the familiar "Mills ratio" as the relevant expectation function conditional on participation. This Mills ratio is then directly included in test score equation as the appropriate selection-correction. An alternative approach,

Specifications using Heckman's selection model and the "control function" approach base identification on the nonlinearity of the selection equation (see Duflo 2001, as an example). Augmenting the instrument set with potential candidates that are correlated to the probability of being tested in school but uncorrelated to the test score can help in identification and the efficiency of the estimator. Following literature on the distance to school as a determinant of enrollment and absenteeism in Pakistan (see for instance Holmes 2003), we propose using the distance to the closest eligible school as an additional instrument in the selection equation.

To construct this distance variable, we collected geographical coordinates of all households in the household survey as well as the coordinates of all schools in the village. We then computed straight-line distances for every household-school pair

and computed the minimum distance to an eligible school, incorporating both the level of the school and its gender status (boys only, girls only, or coeducational) as well as the gender of the child. The distance to the closest eligible school is a strong predictor of enrollment, and, of concern for us, larger distances also make it more likely that the child was not tested in the school as part of the testing exercise.

The results from this exercise are presented in the third panel. Column 1 presents the OLS specification; Column 2 presents the results based on specifications based on the control function approach while Column 3 presents results using the Heckman correction. As before, all specifications include a full set of dummies for mother's age as well as the birth *tehsil* (county); in addition, we include additional controls for the age and gender of the child and (log of) the village population. We find a positive effect of maternal education on child test scores, with children of mothers with some education reporting test scores at the end of Grade III that are greater than 0.3 standard deviations higher than those of children whose mothers report no education at all in English and Urdu. This impact is significant at the 1 percent level of confidence. The effects are slightly smaller for mathematics, and suggest close to a 0.23 standard deviation boost for children with "educated" mothers, significant at the 5 percent level of confidence. These results appear to be robust to the methods used to control for selection with similar qualitative and quantitative findings, from specifications that account for selection, by specifically controlling for the probability of selection (Columns 2–3). The nonparametric approach yields similar coefficients, with the control-function estimates close to those obtained in the OLS regressions and the Heckman estimates only slightly lower for all three subjects.

Although fraught with comparability issues, the impact of mother's education on learning is similar to, and indeed in many cases, greater than the impacts of widely reported experimental interventions. The language effects in our sample are greater than those associated with an extra teacher or computer-aided learning program reported in Banerjee et al. (2007). The Math score is comparable to the learning incentives experiment in multi-subjects reported in Kremer, Miguel, and Thornton (2009).

C. Maternal Education: Potential Channels

Mothers directly control their time inside the house. Thus, one might expect to see some effect of maternal education on the time spent on children's educational needs. In our survey, a specific time-use category was used to record the time spent by the mother on children's educational needs. For every enrolled child we also enquired how much time was spent in helping or reading to the child *by any family member* during the preceding week. Given a large number of mothers and households who report "0" time spent with children on their educational needs, we also present specifications that examine the link between *any* time spent and maternal education. These results are presented in Tables 5A–C. Columns 1 and 2 in Table 5A examine *maternal* time use in helping children with schoolwork. We use both the continuous version of the maternal time variable as well as a discrete version reported in 5B, where the outcome variable takes the value one if the mother spent any time at all on children's educational needs—informative because only 6 percent of mothers fall in this category, with the remainder reporting zero time spent.

Table 5
Maternal Education And Mother And Family Time Use

	(1)	(2)	(3)	(4)	(5)	(6)
		All Mothers		Mothers with no child older than 12		
Panel A: Mother's Time Spent on Children's Educational Needs (Minutes/Day)						
	OLS	IV		OLS	IV	
Mother educated (Y/N)	19.10***	13.96		25.30***	40.34**	
	(2.24)	(12.76)		(3.61)	(20.13)	
Observations	1,425	1,425		577	577	
$R-$squared	0.24	0.23		0.35	0.31	
$F-$test	.	16.87		.	8.17	
Panel B: Did The Mother Spend Any Time On Children's Educational Needs? (Daily) (Y/N)						
	OLS	IV	Biprobit	OLS	IV	Biprobit
Mother educated (Y/N)	0.21***	0.11	0.25***	0.29***	0.23	0.27***
	(0.02)	(0.13)	(0.04)	(0.04)	(0.19)	(0.04)
Observations	1,425	1,425	1,425	577	577	577
$R-$squared	0.24	0.21		0.32	0.32	
$F-$test	—	16.87		—	8.17	
Panel C: Time Spent Helping in Education/Reading To Child By Any Family Member						
	Time Spent (hrs/week)			Any Time Spent (Y/N)		
	OLS	IV		OLS	IV	Biprobit
Mother educated (Y/N)	2.11***	4.62*		0.23***	0.23	0.25***
	(0.33)	(2.65)		(0.03)	(0.25)	(0.09)
Observations	3,120	3,120		3,120	3,120	3,120
$R-$squared	0.18	0.13		0.16	0.16	
$F-$test	—	15.21		—	15.21	

Notes: Robust standard errors, clustered at birth village level; *** significant at $p < 0.01$, **significant at $p < 0.05$, *** significant at $p < 0.1$. For discrete variables, we report both the IV coefficient and the Average Treatment on Treated using a biprobit specification. Girl's school present in mother's birth village is the excluded variable in the IV and the biprobit. All regressions control for birth village (log) population and full set of indication variables for mother's age, mother's birth village *tehsil*. Panel C reports results for enrolled children. In addition, Child-level regressions control for a full set of indicator variables for child's age and child gender.

For the sample of all mothers, the IV specifications suggest no relationship between maternal education and maternal time spent on children's educational needs. Although OLS results are significant, the IV coefficient is smaller with large standard errors. This could, in part, reflect the low precision of the IV estimator. Therefore, we also present the Average Treatment Effect on the Treated (ATT) from the bivariate probit specification for the discrete outcome variable. Here, the results are iden-

tical to the OLS specification and highly precise. The increase in the probability of a mother spending any time at all is 25 percentage points for mothers with some education, which is large given the low overall numbers in the data.

Following a sample suggested by Behrman et al. (1999), we then look at families where there is no child older than 12 in the household. Although the distinction is endogenous, behavior in these households sheds more light on the mother's role for a couple of reasons. First, mothers with the educational levels in our sample may not be directly able to help older children in their more complex schoolwork. Second, older siblings themselves could potentially help the younger ones, thus decreasing the necessity for direct maternal supervision (see, for instance, Tiefenthaler 1997, on the role of older daughters as "mother-substitutes" and Bianchi 2000). Finally, younger children may need more direct supervision and help in schoolwork so that the time allocation of mothers' and of other household members to these activities could well increase.

Columns 3, 4, 5 and 6 in panels A and B present these results. The data show a large difference by maternal education in the time use for these households. All three specifications using the discrete variable indicating whether mothers spent any time on children's educational activities (OLS, IV and biprobit) are similar in magnitude and suggest an increase between 29 (OLS) and 23 (IV) percentage points. The biprobit estimate at 27 percentage points lies between the OLS and IV results and is larger than the result obtained for all households. Additionally, in these families, educated mothers spend more time—40 more minutes—on children's educational needs; the effect is more precisely estimated in the IV specification than for all families.

Finally, Table 5C shows that maternal education also has an effect on the contribution of other household members to child learning. Using the hours spent in the last week by any family member on helping the child in educational activities or reading to children as the dependent variable, we find that children living in households with "educated" mothers are more likely to be helped/read to by 23 (OLS) to 25 (biprobit) percentage points and that this increase is associated with an additional 4.6 hours per week (IV) spent on this activity by all household members.

These results on maternal and child time allocation paint a picture of the learning environment that is very different in households with mothers who have some education. Of interest is that, while time spent directly on children's educational needs does increase for mothers with some education, child study-time increases even more dramatically. Mothers with some education create a space whereby their children are able to spend an extra hour and fifteen minutes each day studying and preparing for school.

D. Heterogeneity: Age

Heterogeneous effects in families with and without older children suggest that family characteristics mediate the impact of maternal education on time use. Table 6 presents further evidence of such heterogeneity by child-age. Columns 1 and 2 (Panel A) split the sample roughly equally between older (10 to 15) and younger (five to nine) children.[21] We find that our results are primarily driven by younger children

21. Note that our child data was asked only for children between the ages of five and 15.

Table 6
Maternal Education: Effects By Age And Gender, Instrumental Variable Estimates

Age	(1) Age ≥ 10	(2) Age ≤ 9	(3) Age ≤ 10	(4) Age ≤ 11	(5) Age ≤ 12
Panel A: Child Time On All Educational Activity Outside School (Minutes/Day)					
Mother educated (Y/N)	19.47	141.56***	90.49*	80.57*	63.54*
	(44.54)	(52.87)	(46.84)	(44.09)	(36.58)
Observations	1,762	1,513	1,995	2,298	2,689
F-statistic	11.55	10.04	10.80	11.15	14.30
Panel B: Time Spent Helping in Education/Reading To Child By Any Family Member (Hours/week)					
Mother educated (Y/N)	1.86	7.57**	5.98*	5.46*	4.82*
	(2.92)	(3.34)	(3.13)	(3.06)	(2.61)
Observations	1,659	1,461	1,918	2,211	2,581
F-statistic	14.56	9.536	10.51	11.00	14.20
Panel C: Child Enrolled					
Mother educated (Y/N)	−0.13	0.20	0.11	0.01	0.07
	(0.25)	(0.20)	(0.18)	(0.18)	(0.15)
Observations	2,931	1,903	2,454	2,807	3,288
F-statistic	12.69	11.68	12.04	12.57	15.42

Notes*:* Robust standard errors, clustered at birth village level; *** significant at $p < 0.01$, **significant at $p < 0.05$, *** significant at $p < 0.1$. Only IV results reported. Results are for enrolled children only except for the enrolled question which is for all children. Girl's school present in mother's birth village is the excluded variable in the IV. All regressions control for birth village (log) population and full set of indication variables for mother's age, mother's birth village *tehsil*. In addition, Child-level regressions control for a full set of indicator variables for child's age and child gender.

where the coefficient on child time is large (an additional 142 minutes) and significant. Columns 3 to 5 show that in fact there is a gradually declining trend of the impact with age, that is, our coefficients of interest get smaller as we start adding older children to our sample. Younger children respond more to maternal education in their time spent on educational activity, with IV estimates showing a steady decline from an additional 142 minutes for children younger than nine to 64 minutes for children younger than 12. The pattern is similar for time spent by family members on helping/reading to children (from eight hours per week to five hours per week) and for enrollment—although in this case, the estimates are highly imprecise. These results are very much in line with Behrman (1997) who points out that we should expect to see effects of maternal educational at such low levels of education in younger children. In our context, the educated mother is directly able to help younger children whose their academic needs are less complex and who may be

more receptive to maternal attention. While we also examined heterogeneity along gender lines, the estimates lack statistical precision to be able to draw robust conclusions. For child time spent studying and time spent by family members, the point estimates are larger for boys than girls, but the standard errors are not small enough to be able to reject equality of coefficients. For enrollment, girls may benefit more, but the estimates are again too imprecise for inference.

E. Other Channels

1. Household Bargaining

In the literature on middle- and high-income countries, maternal education is typically associated with greater bargaining power within the household; this is the classic link between education and female empowerment. If mothers give greater weight to child outcomes, an increase in women's bargaining power will favor children. Lundberg, Pollak, and Wales (1997) classic study shows that money given to mothers leads to greater expenditures on children relative to money given to fathers. One channel through which this link has been posited to work is through greater labor force participation and income generating potential for educated women.

There is little direct evidence for these empowerment/bargaining effects in our data. We test whether maternal education affects time spent on paid work time and time spent outside the house. We follow the same estimation and reporting strategy as in time-use channels. Since paid work by mothers is very low in the data, we present both the discrete variable measuring the presence of any paid work and the continuous version using daily minutes of paid work. As before, we present the Average Treatment Effect on the Treated (ATT) from a bivariate probit specification for the discrete variables.

There are several noteworthy "nonresults" reported in Table 7. First, the effect of maternal education on the time spent by the mother working outside the house is negative in both the OLS and IV (57 and 94 fewer minutes). It is significant in the OLS but with lower precision for the IV. Second, there is a small effect of maternal education on whether the mother does any paid work at all (-1, 1, and 1 percentage points in the OLS, IV, and biprobit specifications), but in all cases the effect is not significant. Third, in time use, we do find and an increase in time allocated to paid work in the IV specification (41 minutes more), but the estimate is imprecise and is not consistent with the negative correlation in the OLS regression. Taken across the three measures it is likely that this channel, where education affects paid work and time outside the house, which is so prominent in the discussion on developed countries, is missing in this low income country environment.[22]

Given that education does not appear to increase mothers' outside options, we also should not expect to see any changes in the decision-making role for the mother who has some years of schooling. Our survey asks two questions about the mother's decision-making in their children's education: whether the mother was principally

22. There is no category of time apart form time spent on child's educational needs that changes significantly in response to mother's education in the IV specification. The overall picture suggests that the increased time spent by the mother on her children's education is coming from all the other various activities in a diffused manner.

Table 7
Maternal Education and Alternate Channels: Preferences and Bargaining

	(1) OLS	(2) IV	(3) $F-$Statistic	(4) Biprobit	(5) n
Time spent by mother on paid work (minutes/day)	−10.75	41.10	16.87		1,425
	(7.65)	(76.39)			
Time spent by mother on paid work (Y/N)	−0.01	0.01	16.87	0.01	1,425
	(0.02)	(0.18)		(0.09)	
Time spent by mother outside the house (minutes/day)	−57.43***	−94.41	16.87		1,425
	(9.48)	(90.70)			
Was the mother responsible for the school enrollment decision (enrolled children only) (Y/N)	0.02	−0.03	14.75	0.01	3,277
	(0.02)	(0.17)		(0.07)	
Was the mother responsible for the choice of school (enrolled children only) (Y/N)	0.02	0.05	14.75	0.05	3,277
	(0.02)	(0.13)		(0.03)	
Is the child enrolled in a public school (Y/N)	−0.19***	0.04	14.05	0.02	3,275
	(0.03)	(0.29)		(0.10)	
Total education expenditure per child (Rs./month)	59.06***	29.95	15.40		3,096
	(7.17)	(52.90)			
Books and supplies per child (Rs./month)	11.67***	15.36	15.40		3,096
	(1.44)	(10.54)			
Uniforms and shoes per child (Rs./month)	7.10***	−16.52	15.40		3,096
	(1.56)	(13.93)			
Time spent by mother on children's general needs (minutes/day)	0.31	−11.99	16.87		1,425
	(8.74)	(80.58)			

Notes: Robust standard errors, clustered at birth village level. *** significant at $p < 0.01$, ** significant at $p < 0.05$, * significant at $p < 0.1$ the F-statistic is presented for the IV regression. For discrete variables, we report both the IV coefficient and the Average Treatment on Treated using a biprobit specification. Girls' school present in mother's birth village is the excluded variable in the IV and the bivariate probit specifications. All expenditure regressions are presented in Pakistani Rupees (Rs.) per month. At the time of the survey, $1 was approximately (Pakistani) Rs.65. All regressions control for birth village (log) population and a full set of indicator variables for mother's age and mother's birth village *tehsil*. In addition, child level regressions control for full set of indicator variables for child's age and child gender.

responsible for the child's enrollment decision and whether the mother was principally responsible for the choice of school. The effect of maternal education is uniformly small and insignificant in all three specifications (OLS, biprobit and IV) for both these questions.

Results on schooling expenditures also confirm that on the *intensive* margin (allocations conditional on school enrollment) maternal education has little impact on child inputs other than the time allocations discussed previously. We estimate the impact of maternal education on disaggregated schooling expenditures (books and supplies and uniforms and shoes) and total educational expenditures (Table 7). For all three outcome variables we are unable to detect a causal impact of maternal education on child allocations. Finally, we examined whether the causal impact of maternal education on time allocations for educational needs extended to children's *general* needs. We find no correlation or causal link between maternal time on children's general needs and maternal education. Taken together these results strongly suggest that improvements in education at low starting levels do not alter bargaining power within the household. However, education still impacts the way in which both mothers and children allocate their time in a household learning environment created by "educated" mothers.

2. Maternal Sorting

Perhaps critical for our interpretation of the channels through which maternal education affects child outcomes, is that our reduced form specifications do not account for sorting in the marriage market. If education allows mothers to choose "better" husbands (there is certainly a strong correlation with spousal education), we will attribute too much to the direct effects of maternal education on child effort.[23] Accounting for assortative mating requires a second instrument (which we do not have) that determines the quality of the match in the marriage market. Black, Devereux, and Salvanes (2005) suggest that the potential importance of this channel can at least be assessed by examining the causal link between maternal education and observable spousal/household characteristics and this check is presented in Table 8. Clearly, in the OLS specifications, mothers with some education also have more educated spouses, spouses who spend more time with their children, live in richer households (as measured by log per-capita expenditures), have smaller families, are more likely to have electricity (which has been independently linked to child outcomes in other studies in low-income countries) and are more likely to live in concrete housing. In both the IV and biprobit specifications none of these remain significant, and in the IV specifications the coefficient estimate for spousal education, father's time with children and household expenditures is of the wrong sign. The coefficient on the type of housing drops to zero. Nevertheless, there is limited evidence that mothers with some education *are* more likely to locate in villages that are electrified and have fewer children, although the estimate is highly imprecise.

23. One problem in the previous literature—school construction affects both paternal and maternal schooling—does not arise here due to gender-segregated schooling.

Table 8
Maternal Education and Spousal Characteristics

	(1) OLS	(2) IV	(3) *F*-Statistic	(4) Biprobit	(5) n
Father educated	0.30***	−0.13	15.92	0.06	1,222
	(0.03)	(0.29)		(0.12)	
Time spent by father on child's educational needs	7.15***	−5.26	16.50		1,199
	(2.62)	(14.60)			
Concrete housing	0.14***	0.03	16.87	0.05	1,425
	(0.03)	(0.25)		(0.12)	
Electricity	0.11***	0.15	16.54	0.08	1,422
	(0.02)	(0.24)		(0.08)	
(Log) expenditure	0.36***	−0.05	16.87		1,425
	(0.05)	(0.41)			
Number of children	−0.10	−0.35	16.87		1,425
	(0.08)	(0.65)			

Notes: Robust standard errors, clustered at birth village level *, significant at $p < 0.01$, **, significant at $p < 0.05$, ***, significant at $p < 0.1$. Electricity is an indicator variable for whether the household has electricity; concrete housing is a standard measure of wealth and has been shown to be directly associated with better school performance. All regressions control for birth village (log) population and full set of indicator variables for mother's age and mother's birth village *tehsil.* Girls' school present in mother's birth village is the excluded variable in the IV and the bivariate probit. For all variables, the regressions suggest no causal relationship between spousal attributes, spousal time allocation and household-level characteristics with maternal education.
*The number of observations in Column 5 is the same for all regressions in the given rows

We thus note the possible importance of this location and fertility effects as independent channels for our results.[24]

VI. Robustness and Limitations

An overarching theme from the analysis thus far is that even low levels of maternal education impact mother and child educational time use and child test scores. Yet, there are legitimate concerns that variation across villages in the availability of schools could be directly correlated with current child outcomes, both due to possible nonrandom placement of schools but perhaps also because of the long-term presence of the school itself. Girls' schools could have been placed in

24. It is tempting, but likely incorrect, to correct for this by using current village fixed-effects. By introducing within-village comparisons, we are comparing uneducated mothers who were selected to live in "better" villages with similarly selected educated mothers. It is very likely that these uneducated mothers differ in some other unobserved dimension—perhaps along the same characteristics that education provided for other mothers in the sample.

villages that are systematically different, for instance, if villages with greater returns to women's education were more likely to receive schools. Second, and distinct from the previous case, the provision of a school itself may alter village norms or attitudes that then directly affect relevant outcomes in the future. In this case, the reduced form impact (what does the provision of a school do) is still of policy interest and is identified, but its use as an instrument for maternal education is invalid.[25] For brevity, we refer to the first as the "selection effect" and the second as the "direct schooling effect."

Under the assumption that direct schooling effects are the same for all ages (that is, it is age-independent), our tightest comparison is between two mothers of similar age who experienced different exposure to schools. This includes both women born in the same village who straddle (in age eligibility) the school construction year, and women of exactly the same age born in different villages with different school exposure. In such a "regression-discontinuity (RD) like" comparison, the exclusion restriction is compelling since most alternative channels, unrelated to maternal schooling, likely have a similar impact on (future) mothers who are close in age, regardless of whether they were under 8 at school construction time or not.

To approximate this comparison we follow two approaches; since we have separate age dummies for each year, our primary concern is controlling for relevant differences across birth villages. First, we control for direct effects of schooling, by including the length of exposure to schooling. Second, we construct tighter counterfactuals in terms of the village environment. Table 9 presents these robustness checks with Column 1 reporting estimates from our original specification.

Column 2 addresses the "selection effect" by dropping 221 mothers whose birth villages never received schools and base estimates on this reduced sample; the instrument then captures only the variation in age-specific enrollment possibilities for the mother in villages that received a school at some point. Because the largest difference in environment is likely between villages that never received a girl's schools versus those that did, it is reassuring that our results do not change with this restriction on the sample.

A second way to assess the degree of this correlation is by restricting the sample to mothers who were born in the village that they currently reside in. Approximately 50 percent of mothers were born in a village other than the one they are living in currently; 86 percent of movements were marriage related. There may be a concern that the mothers who moved grew up in families whose unobserved characteristics,

25. Note that the validity of the instrument is retained in within-village comparisons if the direct effect of a school is the same for mothers all ages, so that we worry primarily about the nonzero correlation at the village level rather than the individual level. While one could construct examples where these unobserved factors vary at the individual level, these are more contrived since they have to argue that the environment changes only for some people and not others in a way that biased our estimates (upward). Our instrument is not whether a mother had a school in her birth village, but rather whether she received the school at a time when she could actually attend it. Specifically, mothers who received a primary school in their village when they were eight years or older are not considered to have been exposed to the school even though they likely experienced the same social/environmental changes that one is concerned about. One may indeed argue that these environmental changes affect younger women more but to the extent that we have age-dummies for every single age year in all our specifications, our comparisons already adjust for aggregate age-specific differences.

Table 9
Robustness

	(1) Base Specification	(2) Ever Present School	(3) Same Village	(4) Birth Village School Exposure (Categorical)	(5) Birth Village School Exposure (Linear)	(6) Birth Village Fixed Effects	(7) School Exposure Mother Age 0–14 w/Birth Village Fixed Effects	(8) School Exposure Mother Age 0–14
Panel A: Child Educational Activity (min/day)								
Mother educated?	73.60*	83.00**	137.95	107.02**	109.45**	88.15	94.85	93.80**
	(37.87)	(33.52)	(92.07)	(45.11)	(42.97)	(99.88)	(62.55)	(41.20)
Observations	3,275	2,775	1,40	3,75	3,275	3,275	949	949
F-test	14.05	21.90	3.41	12.80	14.71	2.56	6.48	16.46
Panel B: Time Helping/Reading Child (hours/week)								
Mother educated?	4.62*	4.49*	8.90	5.68*	3.59	7.40	6.27*	3.93
	(2.65)	(2.47)	(6.27)	(3.03)	(2.69)	(6.56)	(3.33)	(2.87)
Observations	3,120	2,640	1,472	3,120	3,120	3,120	890	890
F-test	15.21	22.23	4.01	13.17	15.49	3.90	8.15	16.70
Panel C: Mother Time in Child Education (minutes/day)								
Mother educated?	13.96	20.45	5.76	28.94**	21.37	39.93	49.34**	24.69*
	(12.76)	(0.00)	(24.09)	(14.76)	(14.55)	(46.19)	(22.33)	(12.95)
Observations	1,425	1,204	706	1,425	1,425	1,425	417	417
F-test	16.87	19.62	4.47	12.70	12.60	1.81	4.20	15.92

(continued)

Table 9 *(continued)*

	(1) Base Specification	(2) Ever Present School	(3) Same Village	(4) Birth Village School Exposure (Categorical)	(5) Birth Village School Exposure (Linear)	(6) Birth Village Fixed Effects	(7) School Exposure Mother Age 0–14 w/Birth Village Fixed Effects	(8) School Exposure Mother Age 0–14
Panel D: English Test Scores—Heckman Selection								
Mother educated?	0.34***	0.35***	0.47***	0.36***	0.36***	0.39***	0.62**	0.28*
	(0.084)	(0.088)	(0.128)	(0.082)	(0.082)	(0.093)	(0.279)	(0.15)
Observations	4,266	3,578	2,043	4,266	4,266	4,275	1,245	1,245
Panel E: Math Test Scores—Heckman Selection								
Mother educated?	0.23**	0.18*	0.15	0.21**	0.21**	0.28***	0.03	0.01
	(0.093)	(0.099)	(0.140)	(0.091)	(0.091)	(0.105)	(0.200)	(0.172)
Observations	4,266	3,578	2,043	4,266	4,266	4,275	1,245	1,245
Panel F: Urdu Test Scores—Heckman Selection								
Mother educated?	0.33***	0.33***	0.34**	0.27***	0.27***	0.28***	0.18	0.15
	(0.084)	(0.090)	(0.133)	(0.084)	(0.085)	(0.096)	(0.222)	(0.161)
Observations	4,266	3,578	2,043	4,266	4,266	4,275	1,245	1,245

Notes: All specifications except (7) include an indicator variable for mother's age. Child level second stage regressions control for indicator variables for child age and child gender and are run for enrolled children. Column 1 reproduces results from the base specification. Column 2 restricts the sample to mothers born in villages that that ever got a school and in addition controls for birth village log population in the first stage. Column 3 controls for (log) birth village population and includes only mother's currently living in the same village as they were born in. Column 4 controls for an indicator variable for exposure to girls school in the birth village at 10 year intervals, whether the mother was born in an urban area, log birth locality population and current village fixed effects. Column 5 controls for years of exposure to girls school in the birth village linearly, whether the mother was born in an urban area, log birth locality population and current village fixed effects. Column 6 puts in birth village fixed effects. Column 7 restricts to mothers that got schools in their birth villages in ages 0–14.

The reported coefficients in panels A, B, and C are 2SLS estimates on mother's education instrumented by whether school is present in the birth village by age seven. The *F*-statistic is the value that the instrument in the corresponding first stage is equal to zero. Standard errors clustered at the birth village level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

such as attitudes, were systematically different and that these characteristics affect child outcomes directly today. Column 3 reports estimates limiting our sample to women who are still living in the village they were born in. While the statistical significance of the results drops, the magnitudes are mostly unchanged.

Columns 4 and 5 address the "direct schooling effect" concern by controlling for the length of exposure to schooling thus comparing across villages that received schools at similar times. Column 4 includes categorical variables for a village's exposure to primary school (a separate dummy for each ten-year interval) and Column 5 controls linearly for the number of years of exposure to a school in the mother's birth village. In both specifications, the magnitudes of estimates are very similar, though in some cases there is a drop in statistical precision.

Column 6 addresses both the "selection effect" and the "direct schooling effect" by including (400 plus) birth-village fixed effects, forcing comparisons only across women born in the same village. Together with age-year dummies this is demanding on the data because sufficient variation can only be obtained in samples with many women per birth village *and* substantial variation in the timing of school construction (if all the women in our sample came from the same village, age-year fixed-effects would fully absorb variation in our instrument). It is reassuring that even with this specification, test score results are similar and the point estimates on time use remain unchanged, although precision drops.

Finally, Column 7 and Column 8 addresses both the effects in an "RD style" approach, restricting the data to women who were zero (just born) to 14 years of age when the school in their birth village was constructed. The instrument then takes the value one for the 0–7 age group and zero for the 8–14 age-groups. Thus we compare women who are close in age (seven years apart on average) but were either just in time to attend a school or narrowly missed receiving it. We include birth-village fixed effects in Column 7 to eliminate the correlations in the error terms, but given the age proximity of our sample group we do not include age cohort dummies. Here, both the size and statistical significance of our baseline specification are maintained. Narrowing the age-bands this way further shows more clearly the power issues (as one expects in the usual RD designs)—the point estimates remain similar but precision worsens.

Across all specifications, these results illustrate two basic patterns. First, the effect-size in the first stage remains roughly the same as in the main specifications, ranging from 0.07 and 0.08 (in Columns 3 and 6) to 0.12 (Column 2) and 0.15 (Columns 7 and 8), although the power of the instrument drops when the sample size is smaller (Column 3) or when birth-village fixed effects (Column 6 and 7) are included. For both these specifications, the F-test is below the threshold for detecting weak instruments.

Second, estimates are fairly stable across all the specifications, although when the first stage is weaker, the precision drops considerably. For instance, the effect of maternal education on the time that the child spends studying ranges from 83 minutes (Column 2, Table 9) to 138 minutes (Column 3, Table 9); on the time that household members spend in helping or reading to the child from 3.59 hours per week (Column 5, Table 9) to 8.90 hours per week (Column 3, Table 9). The results are similar for maternal time (ranging from 20 minutes per day in Column 2 to 49 minutes per day in Column 7), with the exception of Column 3, where half of the mothers (those who

moved) are excluded. With the exception of Column 7 and Column 8 where the drop in the sample is the largest (more than 70 percent), the results on test scores are somewhat more stable suggesting smaller effects on Mathematics and larger effects on English and Urdu (the results with birth-village fixed effects should be interpreted with caution given the large number of fixed-effects). These estimates suggest a significant loss of precision when the sample size decreases or birth-village fixed effects are included, but encouragingly stable coefficients across the specifications.[26]

The literature on IV estimation of intergenerational effects such as by Currie and Morretti (2003) is based on administrative data from rich countries with close to half a million observations. In most low-income countries, where detailed census data at the household are not available and national level household surveys do not include outcome variables like time use and child test scores, smaller data sets like ours afford an alternate way to address identification concerns common the literature on intergenerational transmission of human capital. The robustness table presents some grounds for optimism in that the loss from doing so may be restricted to imprecise estimates rather than biased coefficients. Finally, we also note that like with all IV estimates, we estimate the Local Average Treatment Effect, or LATE. It is likely that the compliers—mothers who shifted their education levels as a result of school presence—behave differently from the sample of all mothers (Card 1999).

VII. Conclusions

We are able to demonstrate a causal link between maternal education and time spent by their children on educational activities outside school. Our IV estimates suggest that this is as much as 75 minutes more for every child. In addition, mothers with some education also spend more time with their children on schoolwork, an effect that is particularly large and significant (40 minutes) for families where the mother is likely to be the primary caregiver. They also facilitate greater contribution from other household members in reading and helping their children with schoolwork. This extra effort put in by the mothers, children, and households pays off. We find that test scores are significantly higher for children whose mothers have some education, though this result required controlling for additional selection issues for the subset of children in our sample who took the test.

We believe that these results most likely reflect a direct productivity effect of maternal education or an effect on maternal preferences in a context where bargaining power is low or nonexistent. The low educational achievement of mothers in our sample (as measured by years of education) does not lead to greater enrollment and does not affect the school choice decision. In response to questions on mother's role in decision-making, "educated" mothers were no more responsible for these

26. Another concern is that birth *tehsil* fixed-effects could lead to more bias if there is more room for villages to lobby for girls' schools within a *tehsil*. Estimates without birth tehsil fixed-effects are similar; child time spent studying is large and significant (83 minutes); maternal time spent on children is still 30 minutes extra per day and significant in households with no older children. Estimates of the effect of maternal education on time spent helping children are of similar magnitude and significance. The test score results are as in the base specification and are all significant.

decisions than their unschooled counterparts. This is, perhaps, not surprising at such low levels of education and paid work that are typical of developing countries. Ironically, these very same mothers with low levels of education do enhance their children's learning, an outcome that is ostensibly much harder to achieve. Mothers do not need to be at an advanced cognitive level to make their children study. Perhaps, by spending some years in school, mothers learnt that learning requires considerable effort. Consequently, they are clearer on the steps (and effort) that children need to take to improve their cognitive achievement. The findings in this paper thus emphasize the role of parental-child interaction and child effort in studying as an important channel for improving learning.

Table A1
Matching Statistics

	Mothers matched (n = 1,437)	Mothers Unmatched (n = 260)	Mean Difference
Mother			
Age	37.9	37.3	0.5
	(0.2)	(0.52)	(0.5)
Education (years)	1.3	1.4	0.0
	(0.1)	(0.2)	(0.2)
Educated (Y/N?)	0.2	0.2	0.0
	(0.011)	(0.026)	(0.02)
Time spent on child needs (minutes/day)	94.5	88.5	6.0
	(3.73)	(8.41)	(9.5)
Time spent on child school work (minutes/day)	5.0	3.5	1.6
	(0.6)	(1.1)	(1.5)
Child			
Age (years)	10.0	10.0	0.2
	(0.04)	(0.1)	(0.1)
Female	0.5	0.5	−0.0
	(0.01)	(0.02)	(0.02)
Enrolled	0.8	0.8	−0.00
	(0.01)	(0.02)	(0.02)
Child educational activity outside school (minutes/day)	121.3	117.3	4.0
	(3.3)	(1.4)	(3.4)
Child study time outside school (minutes/day)	60.4	57.8	2.6
	(2.3)	(0.9)	(2.3)
TimeHelpRead (hours/week)	2.6	2.3	0.3
	(0.2)	(0.1)	(0.2)

(continued)

Table A1 *(continued)*

	Mothers matched (n = 1,437)	Mothers Unmatched (n = 260)	Mean Difference
ChildPlay (minutes/day)	179.9	184.0	−4.1
	(6.0)	(2.7)	(6.7)
ChildPaidWork (minutes/day)	14.1	16.0	−1.9
	(3.0)	(1.4)	(3.6)
ChildHouse work (minutes/day)	66.8	65.2	1.6
	(5.6)	(2.4)	(6.1)
ChildSchool Time (minutes/day)	273.7	271.9	1.9
	(2.5)	(5.8)	(6.4)
ChildRest time (minutes/day)	617.4	611.2	6.2
	(4.3)	(2.0)	(5.0)
Public	0.71	0.69	0.02
	(0.01)	(0.02)	(0.02)

Notes: Means and standard error of the mean in parentheses. The table compares attributes of children and mothers for the sample of mothers whose reported birth village could be matched to a village in the Pakistani census with the mothers whose villages we were unable to find. The reasons for the "missing mothers" are detailed in the text.

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