

# What Did You Do All Day? Maternal Education and Child Outcomes \*

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**Tahir Andrabi**  
Pomona College  
[tandrabi@pomona.edu](mailto:tandrabi@pomona.edu)

**Jishnu Das**  
The World Bank  
[jdas1@worldbank.org](mailto:jdas1@worldbank.org)

**Asim Ijaz Khwaja**  
Harvard University  
[akhwaja@ksg.harvard.edu](mailto:akhwaja@ksg.harvard.edu)

## Abstract

Does maternal education have an impact on children's educational outcomes even at the very low levels found in many developing countries? By combining a nationwide census of schools in Pakistan with household data, we are able to use the availability of girls' schools in the mother's *birth* village as an instrument for maternal schooling to address this issue. Since public schools in Pakistan are segregated by gender, the instrument affects only maternal education rather than the education levels of both mothers and fathers. We find that children of mothers with some education spend 72 minutes more on educational activities at home compared to children whose mothers report no education at all. Mothers with some education also spend more time helping their children with school work; the effect is stronger (an extra 40 minutes per day) in families where the mother is likely the primary care-giver. We also find that members of the household of mothers with some education spend an extra 4.6 hours per week in helping/reading to the child. Finally, test scores for children whose mothers have some education are higher in English, Urdu (the vernacular) and Mathematics by 0.24-0.35 standard deviations. We find no relationship between maternal education and mother's time spent on paid work or housework—a posited channel through which education affects bargaining power within the household. Neither do we find a relationship between maternal education and the mother's role in educational decisions or in the provision of other child-specific goods such as expenditures on pocket money, uniforms and tuition. Our data therefore suggest that at these very low levels of education, maternal education does not substantially affect her bargaining power within the household. Instead, maternal education could directly increase the mother's productivity or affect her preferences towards children's education in a context where her bargaining power is low.

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## 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 inter-generational 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 around the world (in 2000, the average woman aged 25 and over 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 dataset 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).<sup>1</sup> 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. Since 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.

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<sup>1</sup> 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) dataset for females above the age of 25 in Pakistan.

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 and others (1999). In addition, in households with no children above the age of 12 (where the mother is likely the primary care-giver), 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.<sup>2</sup>

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 enrolment. 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 enrolment, although the latter could also be due to a lack of precision in our enrolment estimates.

These results may at first seem puzzling given the small education margins and the poor learning environment in developing countries.<sup>3</sup> The answer to this puzzle, we believe, is that even this small amount of education could have a large effect on cognitive and non-cognitive outcomes *relative to the comparison group of mothers with no education*. For instance, experimental evidence from Afghanistan shows that with just 6 months

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<sup>2</sup> 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.

<sup>3</sup> See the TIMSS Report 2007, Andrabi and others, 2009, for Pakistan and Das and Zajonc, 2009, or the ASER Report, 2008 for India.

of education, there are large gains in test-scores among girls (Burde and Linden 2011). 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 India (Behrman and others 1999).<sup>4</sup>

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 and others (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 (*teshils* in Pakistan) fixed-effects.<sup>5</sup> 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 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

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<sup>4</sup> 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."

<sup>5</sup> A *teshil* in Pakistan is roughly the administrative equivalent to a county in the US.

simultaneous improvements in the education levels of fathers, which would be the case if schooling availability were not gender specific.<sup>6</sup>

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 going age. In our specifications, the effect of having a girls' school in the village of birth is large and significant for mothers who were 7 years 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 onwards. 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 over 400 birth-villages and 1400 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,

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<sup>6</sup> 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.

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 exercise 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 non-labor 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, and Black and others, 2005) or low-income countries (Desai and Alva, 1998) that attempt to address selection into maternal schooling and find little impact.<sup>7</sup> 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 enrolment (Fiszbein and Schady, 2009) while methods of improving learning remain tenuous.

The remainder of the paper is structured as follows. Section 2 presents a brief literature survey, and Section 3 presents the data description. Section 4 presents the econometric strategy. Section 5 presents the results and section 6 discusses robustness and limitations of our analysis. Section 7 concludes.

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<sup>7</sup> Black and others (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.

## Section 2: 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 inter-generational link may be established (Lundberg and others, 1993, 1997).

A second strand highlights the labor force participation channel. More educated mothers are more likely to participate in the labor force. If child-care is not fully 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 US and Colombia. The link between maternal education, employment and child outcomes is unclear. Bianchi (2000) shows that in the US, maternal time with children remained unchanged between 1965 and 1995, a period of dramatic increases in the labor force participation for women. Guryan and others (2008) show that while working women spend less time with children in the US, 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 US 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 10 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 and others (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, or Ravallion and Wodon, 1999). Primary-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 trade-off 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 datasets that allow for this kind of matching to birth-county in high-income countries are much larger. For instance, Black and others (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 inter-generational links, but the insights developed here are possible only through especially collected data sets with smaller sample sizes. An inevitable trade-off is the decline in power and precision in the IV estimates.

### **Section 3: Data Description**

We use a dataset on households and children from 112 villages in 3 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 3 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.<sup>8</sup> 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.

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 1697 mothers with 4331 children between the ages of 5 and 15 in these households.<sup>9</sup> 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.

### *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 6am, prepared breakfast and then prepared her children for school till 7am. After that, she cleaned from 7am to 9am 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 9½ 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

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<sup>8</sup> See Andrabi and others (2008) for more details on the rise of low cost, for-profit, secular private schools in rural Pakistan.

<sup>9</sup> Birth village information was not available for all these mothers. Summary statistics are provided for the matched sample of 1437 mothers. We return to the matching issue below.

6 hours a day on this activity). The average time spent on paid work is slightly higher for uneducated mothers (44 vs. 29 minutes/day) compared to mothers with some education. Time spent “looking after children’s needs” accounts for 1½ 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 0 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 mothers were spending no more than 90 minutes a day on all types of childcare (Desai and Jain, 1994).

Since 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 sub-categories 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 over 8 hours a day inside the house and 1 hour and 40 minutes outside. If anything, mothers with some education spend less time outside the house (1 versus 2 hours) and more time inside (9½ versus 8 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 2 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. 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 and others 2008). In contrast to the broad similarity on time spent in childcare, Pakistani women allocate a lot more of their time to housework. *Non-employed* women in the US spent just over 3½ hours a day on housework in 1995 and employed women 2½ hours a day—less than a quarter of the time spent by women in our rural Pakistani sample (Bianchi and Robinson 1998). 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.<sup>10</sup>

### *Children*

The survey also covered every child between the ages of 5-15 in the sampled households for a total of 4331 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 10 percentage points less likely to be enrolled than boys; given our list frame and the private school explosion in rural Pakistan (see Andrabi and others, 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

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<sup>10</sup> 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.

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 enrolment.

Variation in time-use by the child’s age and the educational status of the mother shows several noteworthy patterns (Figure 3). 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 and others (2009), we discuss the issue of housework and show that this increase in housework is largely concentrated among 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 i.e. 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 0 and variance 1 in the full sample of tested children.<sup>11</sup> 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.

#### **Section 4: 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.

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

As mentioned earlier, there is large mass (76 percent) of mothers who have never gone to school (0 years of schooling). Therefore, we use a binary measure, "*MotherEducated*" that takes the value 1 if the mother reports non-zero years of schooling and 0 otherwise as our dependent variable. We also show the first stage

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<sup>11</sup> 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.

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 0, does not lead to any significant loss in variation. *SchoolPresent<sub>i</sub>* is an indicator variable that takes the value 1 if the mother had a girl's school in her birth village when she was 7 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 6 as the normal school starting age, but 7 years is more reasonable given the widespread practice of delayed enrolment. 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 9 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 (1437 out of 1697) 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.<sup>12</sup> 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.<sup>13</sup> 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.

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<sup>12</sup> 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.

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

### *School Construction and the Exclusion Restriction*

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 1).

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 enrolment decision. Since other changes in the environment affecting enrollment are also time-varying, the first component of the  $X_i$  vector includes controls 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 US county. The province of Punjab, where our sample is drawn from, has 34 districts and 104 *tehsils* in the latest (1998) census. Since 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 (over 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 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 1000 and that a middle/primary school does not exist within a radius of 1.5 km of the school.”<sup>14</sup>

Figure 2 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.<sup>15</sup> 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 take-away message from these additional checks is that the small sample size reduces efficiency when birth-village dummies are included but the main estimates are likely unbiased. The second stage regressions are then specified as follows.

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

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

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 enrolment 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

<sup>14</sup> The Planning Commission, Government of Pakistan, Manual of Development Projects

<sup>15</sup> The birth-village population could not be obtained for 12 mothers reducing the sample size in regressions to 1425. This has no effect on any regressions or results.

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.

## **Section 5: Results**

### *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.<sup>16</sup> 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.

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 11.42, 12.05 and 10.96 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 and others (2002), with F-statistics of 15.86, 13.35 and 14.60 in the three specifications we use in the second stage.<sup>17</sup>

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<sup>16</sup> 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.

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

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 7 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 7, 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 enrolment profiles for girls in Pakistan—increasing between ages 5 and 10 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 7, relative to one who never received it, is 10 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.

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 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 enrolment, we estimate the same specifications for enrolled children only. The results are stable and in this sub-sample, 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 25<sup>th</sup> percentile to roughly the 75<sup>th</sup> percentile in the time distribution, and is almost identical to the *point* estimates reported in Behrman and others (1999). The result is driven to a large extent by actual time spent studying outside school (Panel B, Table 4).<sup>18</sup>

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 enrolment in the OLS regression. This echoes results found in developed countries, albeit at higher levels of education such as Black and others (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.<sup>19</sup>

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

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<sup>18</sup> 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.

<sup>19</sup> 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 enrolment effects of maternal education.

for whom we have both test-scores and household survey data.<sup>20</sup> 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 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 non-parametric 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 non-linearity 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

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<sup>20</sup> 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.

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 enrolment, 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 Columns (3) present 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 0.25 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 non-parametric 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 and others (2007). The Math score is comparable to the learning incentives experiment in multi-subjects reported in Kremer and others (2009).

#### *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 1 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.

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 identical 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-cut suggested by Behrman and others (1999), we then look at families where there is no child older than 12 in the household (Column 2, Table 4B). 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 and 4 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.

#### *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 (5 to 9) children.<sup>21</sup> We find that our results are primarily driven by younger children 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, i.e. 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 below 9 years to 64 minutes for children under 12. The pattern is similar for time spent by family members on helping/reading to children (from 8 hours per week to 5 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.

#### *Other Channels*

##### 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 and others (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.

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<sup>21</sup> Note that our child data was asked only for children between the ages of 5 and 15.

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 “non”-results 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 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.<sup>22</sup>

Given that education does not appear to increase mothers’ outside options, we should also 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 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

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<sup>22</sup> There is no category of time apart from 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.

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.

### 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.<sup>23</sup> 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 and others (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

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

likely to locate in villages that are electrified and have fewer children, although the estimate is highly imprecise. We thus note the possible importance of this location and fertility effects as independent channels for our results.<sup>24</sup>

## Section 6: 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 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.<sup>25</sup> 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

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<sup>24</sup> 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.

<sup>25</sup> 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 non-zero correlation at the village, 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 (upwards). 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 8 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.

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. Since 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, 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 0 (just born) to 14 years of age when the school in their birth-village was constructed. The instrument then takes the value 1 for the 0-7 age group and 0 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 (over 70%), 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.<sup>26</sup>

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 datasets 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).

## **Section 7: 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 care-giver. They also facilitate greater contribution from other household members in

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<sup>26</sup> 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.

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 non-existent. 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 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.



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## Tables and Figures

**TABLE 1: SUMMARY STATISTICS**

	(1)	(2)	(3)
	Mean	S.D.	N
<b>All Children</b>			
Age (years)	9.97	2.93	4331
Female (fraction)	0.47	0.50	4331
Enrolled (fraction)	0.76	0.43	4331
Educational Activity (minutes/day)	117.30	88.98	4331
Paid Tutorials (minutes/day)	14.36	39.11	4331
Study (minutes/day)	57.83	60.22	4331
School Preparation (minutes/day)	45.11	34.97	4331
<b>Enrolled Children</b>			
Public Schools (fraction)	0.71	0.45	3305
Mother Responsible for the Choice of School (fraction)	0.07	0.25	3270
Mother Responsible for the Enrollment Decision (fraction)	0.10	0.31	3270
Does anybody help the child or read to him/her? (Y/N)	0.38	0.49	3149
Time anybody in the family spent helping child in education or reading to him/her (hours/week)	3.00	4.81	3149
School Expenditures (Rs./Month) Paid Tutorials	9.40	34.72	3124
School Fees	31.12	55.93	3124
Supplies	35.90	26.07	3124
Uniforms and Shoes	41.40	26.34	3124
<b>Mothers</b>			
Education (years)	1.34	2.75	1437
Educated (Y/N) (fraction)	0.24	0.43	1437
Age	37.87	7.56	1437
Girls School Present in Birth Village by Age 7	0.55	0.50	1437
Time Spent on Children's General Needs (Minutes/Day)	94.47	141.29	1437
Time Spent Helping Child With Schoolwork	5.05	22.77	1437
Was Any Time Spent Helping Child With Schoolwork? (Y/N) (fraction)	0.06	0.23	1437
Paid Work (Minutes/Day)	40.67	131.71	1437
Any Paid Work (Y/N) (fraction)	0.11	0.31	1437
Working Time Outside the House	99.33	157.58	1437
Working Time Inside the House	501.88	225.67	1437

*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)	(2)	(3)
<b>Panel A: Maternal Time Use</b>			
	Mother Not Educated (N=1096)	Mother Educated (N=341)	Difference in Means
Rest	608.69 (5.77)	575.5 (7.4)	33.19 (2.98)
Housework	560.46 (7.49)	596.2 (10.62)	-35.75 (-2.44)
Paid-Work	44.28 (4.23)	29.08 (5.38)	15.20 (1.86)
Children's General Needs	93.16 (4.29)	98.67 (7.54)	-5.50 (-0.63)
Children's Educational Needs	0.3 (0.17)	20.32 (2.29)	-20.02 (-15.29)
Other	137.82 (5.84)	124.37 (6.98)	13.45 (1.20)
Mother's working time outside the house	113.40 (6.25)	54.10 (5.02)	59.29 (6.14)
Mother's working time inside the house	475.71 (7.79)	563.15 (14.05)	-87.44 (-5.47)
<b>Panel B: Child Time Use</b>			
	Child Not Enrolled (N=1026)	Child Enrolled (N=3305)	Difference in Means
Rest	650.92 (4.89)	598.83 (2.10)	52.09 (11.20)
School	0 (0)	356.27 (1.36)	
Housework	197.31 (8.23)	24.14 (1.17)	173.17 (34.37)
Paid Work	62.46 (5.68)	1.57 (0.35)	60.88 (18.87)
Educational Activities	8.74 (1.14)	151 (1.25)	-142.25 (-61.00)
Play	278.48 (8.41)	154.63 (2.05)	123.85 (20.80)
Other	226.30 (7.73)	153.57 (2.60)	72.73 (11.45)
<b>Panel C: Maternal Education and Child Test Scores</b>			
	Mother Not Educated (n=503)	Mother Educated (n=203)	Difference in means
English	-0.17 (0.04)	0.24 (0.07)	-0.41 (-5.10)
Math	-0.12 (0.05)	0.11 (0.07)	-0.23 (-2.64)
Urdu	-0.11 (0.04)	0.21 (0.06)	-0.32 (-4.12)

*Notes:* Mean and Standard errors in parentheses, t-stats 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.

**TABLE 3: FIRST STAGE REGRESSIONS AND FALSIFICATION TESTS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mother Education (years)							
	Mother Educated (Y/N)					Falsification	
	Mother level						
	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*** (0.17)	0.12*** (0.03)	0.11*** (0.03)	0.12*** (0.03)	0.13*** (0.03)		0.10*** (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	1425	1425	4294	3275	1204	1412	1425
R-squared	0.16	0.17	0.17	0.18	0.19	0.17	0.17
F Stat	12.73	17.29	15.53	14.13	14.22		

*Notes:* Robust standard errors in parentheses, clustered at the birth village level (a): significant at  $p < 0.01$ ; (b): significant at  $p < 0.05$ ; (c): 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 regression 1, 2, 3 and 4 is girls' school present after age 7 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 7. The omitted variable in Equation 6 is boys' school present after age 7 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).

**TABLE 4: MATERNAL EDUCATION AND CHILD OUTCOMES**

	(1)	(2)	(3)	(4)
<b>Panel A: Child Time On All Educational Activity Outside School (Mins/Day)</b>				
	<b>All Children</b>		<b>Enrolled Children</b>	
	OLS	IV	OLS	IV
Mother Educated (Y/N)	43.07*** (4.36)	71.92* (40.26)	19.79*** (4.56)	73.60* (37.87)
Observations	4294	4294	3275	3275
R-squared	0.19	0.18	0.18	0.08
F test	.	15.31	.	14.05
<b>Panel B: Child Time On Studying Outside School (Mins/Day)</b>				
	<b>All Children</b>		<b>Enrolled Children</b>	
	OLS	IV	OLS	IV
Mother Educated (Y/N)	29.05*** (3.48)	49.56* (28.72)	18.25*** (3.71)	58.94** (29.88)
Observations	4294	4294	3275	3275
R-squared	0.14	0.12	0.15	0.07
F test	.	15.31	.	14.05
<b>Panel C: Child Enrolled (Y/N)</b>				
	OLS	IV	Biprobit	
Mother Educated (Y/N)	0.17*** (0.02)	-0.00 (0.18)	0.09 (0.07)	
Observations	4294	4294	4294	
R-squared	0.201	0.175		
<b>Panel D: Child Test Scores (English, Math and Urdu)</b>				
	<b>English</b>			
	OLS	Control Function	Heckman	
Mother Educated (Y/N)	0.35*** (0.10)	0.34*** (0.11)	0.34*** (0.08)	
	<b>Math</b>			
Mother Educated (Y/N)	0.28*** (0.10)	0.23** (0.11)	0.23** (0.09)	
	<b>Urdu</b>			
Mother Educated (Y/N)	0.33*** (0.10)	0.33*** (0.10)	0.33*** (0.08)	
Observations	727	711	4266	
<i>Notes:</i> 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 <i>tehsil</i> . 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.				



**TABLE 5: MATERNAL EDUCATION AND MOTHER AND FAMILY TIME USE**

	(1)	(2)	(3)	(4)	(5)	(6)
	All Mothers			Mothers with no child over 12		
<b>Panel A: Mother's Time Spent on Children's Educational Needs (Mins/Day)</b>						
	OLS	IV		OLS	IV	
Mother Educated (Y/N)	19.10*** (2.24)	13.96 (12.76)		25.30** * (3.61)	40.34** (20.13)	
Observations	1425	1425		577	577	
R-squared	0.24	0.23		0.35	0.31	
F test	.	16.87		.	8.17	
<b>Panel B: Did The Mother Spend Any Time On Children's Educational Needs? (Daily) (Y/N)</b>						
	OLS	IV	Biprobit	OLS	IV	Biprobit
Mother Educated (Y/N)	0.21*** (0.02)	0.11 (0.13)	0.25*** (0.04)	0.29*** (0.04)	0.23 (0.19)	0.27*** (0.04)
Observations	1425	1425	1425	577	577	577
R-squared	0.24	0.21		0.32	0.32	
F test	.	16.87		.	8.17	
<b>Panel C: Time Spent Helping in Education/Reading To Child By Any Family Member</b>						
	Time Spent (hrs/week)			Any Time Spent (Y/N)		
	OLS	IV		OLS	IV	Biprobit
Mother Educated (Y/N)	2.11*** (0.33)	4.62* (2.65)		0.23*** (0.03)	0.23 (0.25)	0.25*** (0.09)
Observations	3120	3120		3120	3120	3120
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.

**TABLE 6: MATERNAL EDUCATION: EFFECTS BY AGE AND GENDER, IV ESTIMATES**

	(1)	(2)	(3)	(4)	(5)
<b>AGE</b>	Age>=10	Age<=9	Age<=10	Age<=11	Age<=12
<b>Panel A: Child Time On All Educational Activity Outside School (Mins/Day)</b>					
Mother Educated (Y/N)	19.47 (44.54)	141.56*** (52.87)	90.49* (46.84)	80.57* (44.09)	63.54* (36.58)
Observations	1762	1513	1995	2298	2689
F Statistic	11.55	10.04	10.80	11.15	14.30
<b>Panel B: Time Spent Helping in Education/Reading To Child By Any Family Member (Hours/week)</b>					
Mother Educated (Y/N)	1.86 (2.92)	7.57** (3.34)	5.98* (3.13)	5.46* (3.06)	4.82* (2.61)
Observations	1659	1461	1918	2211	2581
F Statistic	14.56	9.536	10.51	11.00	14.20
<b>Panel C: Child Enrolled</b>					
Mother Educated (Y/N)	-0.13 (0.25)	0.20 (0.20)	0.11 (0.18)	0.01 (0.18)	0.07 (0.15)
Observations	2931	1903	2454	2807	3288
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.

**TABLE 7: MATERNAL EDUCATION AND ALTERNATE CHANNELS: PREFERENCES AND BARGAINING**

	(1)	(2)	(3)	(4)	(5)
	OLS	IV	F-Stat	Bi-probit	n*
Time Spent by Mother on Paid Work (minutes/day)	-10.75 (7.65)	41.10 (76.39)	16.87		1425
Time Spent by Mother on Paid Work (Y/N)	-0.01 (0.02)	0.01 (0.18)	16.87	0.01 (0.09)	1425
Time Spent by Mother Outside the House (minutes/day)	-57.43*** (9.48)	-94.41 (90.70)	16.87		1425
Was the Mother Responsible for the School Enrollment Decision (Enrolled children only) (Y/N)	0.02 (0.02)	-0.03 (0.17)	14.75	0.01 (0.07)	3277
Was the Mother Responsible for the choice of School (Enrolled children only) (Y/N)	0.02 (0.02)	0.05 (0.13)	14.75	0.05 (0.03)	3277
Is the Child enrolled in a Public School (Y/N)	-0.19*** (0.03)	0.04 (0.29)	14.05	0.02 (0.10)	3275
Total Education Expenditure per Child (Rs./month)	59.06*** (7.17)	29.95 (52.90)	15.40		3096
Books and Supplies per Child (Rs./month)	11.67*** (1.44)	15.36 (10.54)	15.40		3096
Uniforms and Shoes per Child (Rs./month)	7.10*** (1.56)	-16.52 (13.93)	15.40		3096
Time Spent by Mother on Children's General Needs (minutes/day)	0.31 (8.74)	-11.99 (80.58)	16.87		1425

*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-stat is presented for the IV regression. For discrete variables, we report both the IV coefficient and the Average Treatment on Treated using a bivariate probit 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.

\*The number of observations in column 5 is the same for all regressions in the given rows.

**TABLE 8: MATERNAL EDUCATION AND SPOUSAL CHARACTERISTICS**

	(1)	(2)	(3)	(4)	(5)
	OLS	IV	F-Stat	Bi-probit	n*
Father Educated	0.30*** (0.03)	-0.13 (0.29)	15.92	0.06 (0.12)	1222
Time Spent by Father on Child's Educational Needs	7.15*** (2.62)	-5.26 (14.60)	16.50		1199
Concrete Housing	0.14*** (0.03)	0.03 (0.25)	16.87	0.05 (0.12)	1425
Electricity	0.11*** (0.02)	0.15 (0.24)	16.54	0.08 (0.08)	1422
(Log) Expenditure	0.36*** (0.05)	-0.05 (0.41)	16.87		1425
Number of children	-0.10 (0.08)	-0.35 (0.65)	16.87		1425

*Notes:* Robust standard errors, clustered at birth village level (a): significant at  $p < 0.01$ , (b): significant at  $p < 0.05$ , (c): 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

**TABLE 9: ROBUSTNESS**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Base Specification	Ever Present School	Same Village	Birth Village School Exposure (Categorical)	Birth Village School Exposure (Linear)	Birth Village Fixed Effects	School Exposure Mother Age 0-14 w/Birth Village Fixed Effects	School Exposure Mother Age 0-14
<b>Panel A: Child Educational Activity (min/day)</b>								
Mother educated?	73.60* (37.87)	83.00** (33.52)	137.95 (92.07)	107.02** (45.11)	109.45** (42.97)	88.15 (99.88)	94.85 (62.55)	93.80** (41.20)
Observations	3275	2775	1540	3275	3275	3275	949	949
F test	14.05	21.90	3.41	12.80	14.71	2.56	6.48	16.46
<b>Panel B: Time Helping/Reading Child (hrs/wk)</b>								
Mother educated?	4.62* (2.65)	4.49* (2.47)	8.90 (6.27)	5.68* (3.03)	3.59 (2.69)	7.40 (6.56)	6.27* (3.33)	3.93 (2.87)
Observations	3120	2640	1472	3120	3120	3120	890	890
F test	15.21	22.23	4.01	13.17	15.49	3.90	8.15	16.70
<b>Panel C: Mother Time in Child Education (min/day)</b>								
Mother educated?	13.96 (12.76)	20.45 (0.00)	5.76 (24.09)	28.94** (14.76)	21.37 (14.55)	39.93 (46.19)	49.34** (22.33)	24.69* (12.95)
Observations	1425	1204	706	1425	1425	1425	417	417
F test	16.87	19.62	4.47	12.70	12.60	1.81	4.20	15.92
<b>Panel D: English Test Scores—Heckman Selection</b>								
Mother educated?	0.34*** (0.084)	0.35*** (0.088)	0.47*** (0.128)	0.36*** (0.082)	0.36*** (0.082)	0.39*** (0.093)	0.62** (0.279)	0.28* (0.15)
Observations	4266	3578	2043	4266	4266	4275	1245	1245
<b>Panel E: Math Test Scores—Heckman Selection</b>								
Mother educated?	0.23** (0.093)	0.18* (0.099)	0.15 (0.140)	0.21** (0.091)	0.21** (0.091)	0.28*** (0.105)	0.03 (0.200)	0.01 (0.172)
Observations	4266	3578	2043	4266	4266	4275	1245	1245
<b>Panel F: Urdu Test Scores—Heckman Selection</b>								
Mother educated?	0.33*** (0.084)	0.33*** (0.090)	0.34** (0.133)	0.27*** (0.084)	0.27*** (0.085)	0.28*** (0.096)	0.18 (0.222)	0.15 (0.161)
Observations	4266	3578	2043	4266	4266	4275	1245	1245

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 7. 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.

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 test score equations (1) through (6) control for mother and child age variables and child gender. Test score equation (7) and (8) controls for child age and gender and not mother's age. School present in birth village by age 7 is the excluded variable. The Heckman selection estimates use the distance to school as an additional excluded variable in the determination of testing results. The test score results are presented as standard-deviation changes.



Figure 1

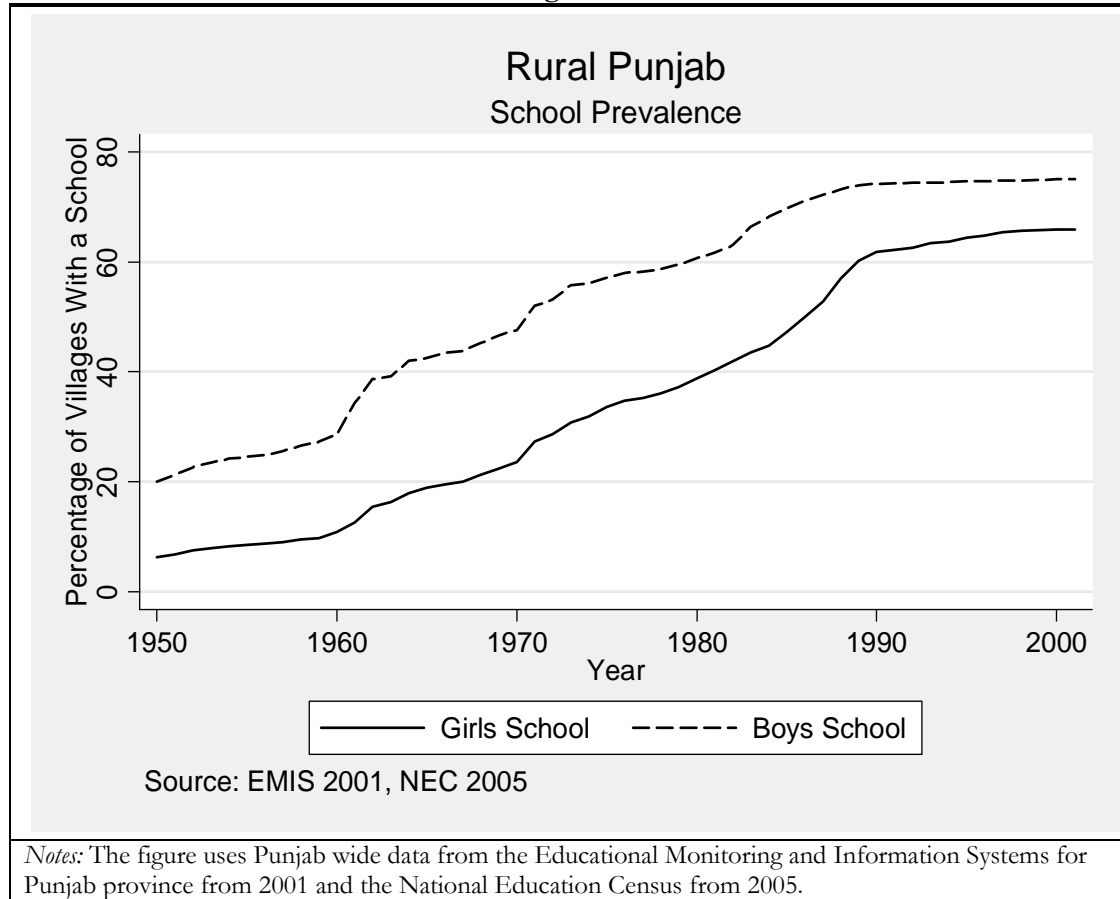
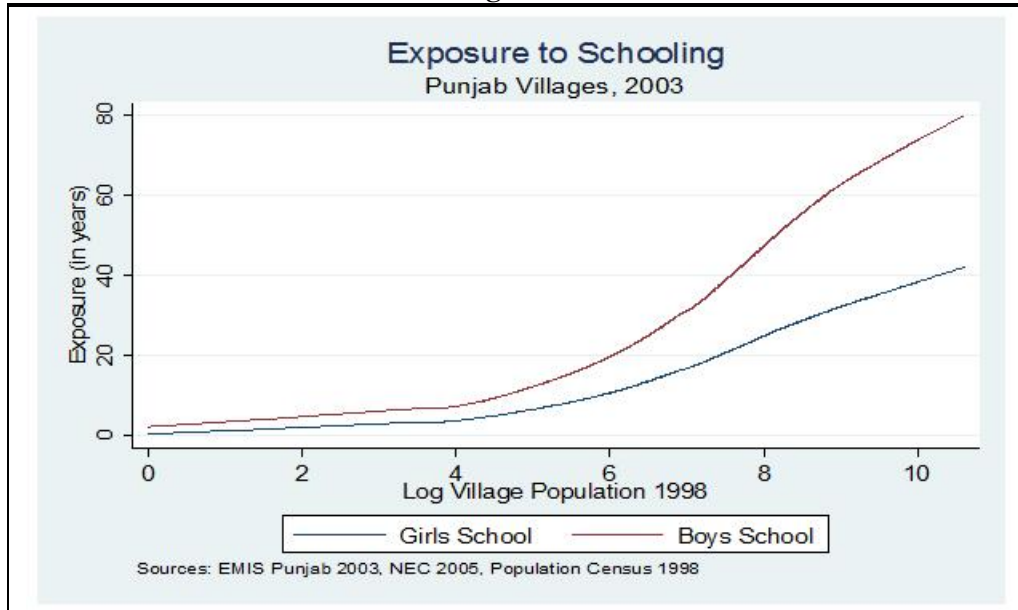
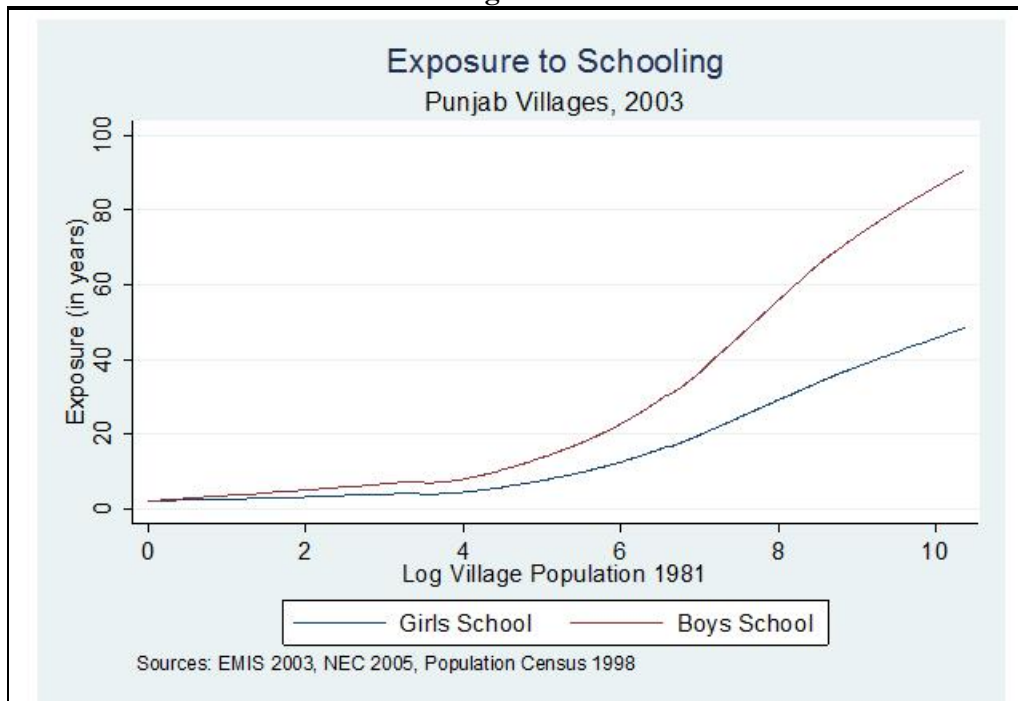


Figure 2a



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.

Figure 2b

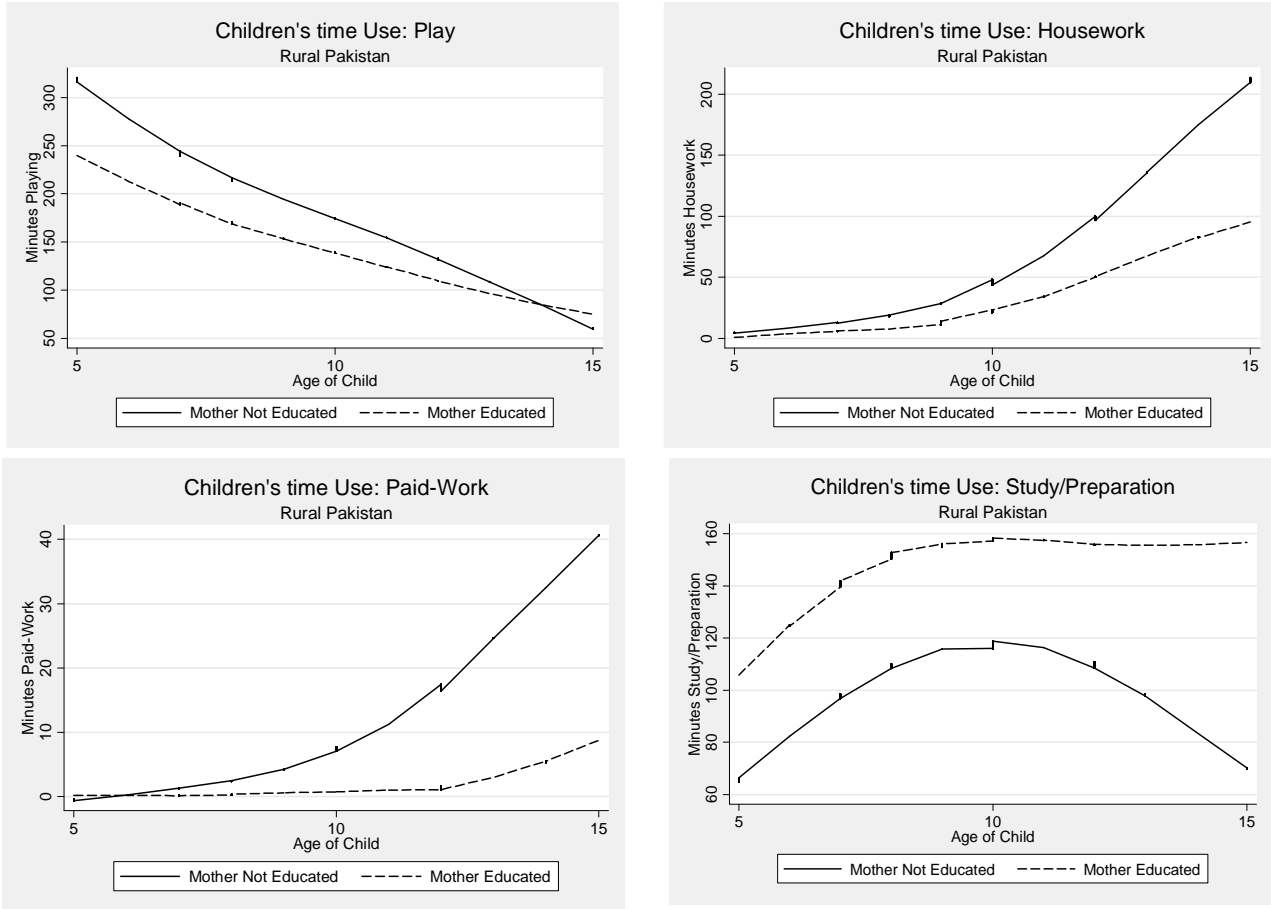


Notes: The figure uses Punjab wide data from the Educational Monitoring and Information Systems for Punjab province from 2001 and the National Education Census from 2005. The curve is a locally weighted smoothed estimate using the STATA LOWESS command.





**Figure 3: Children's Time-Use and Age, and Maternal education**



*Notes:* The figures show non-parametric 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.

**Appendix: Table A1**  
Matching Statistics

	Mothers matched (n=1437)	Mothers Unmatched (n=260)	Mean Diff
<b>Mother</b>			
Age	37.9 (0.2)	37.3 (0.52)	0.5 (0.5)
Education (years)	1.3 (0.1)	1.4 (0.2)	0.0 (0.2)
Educated (Y/N?)	0.2 (0.011)	0.2 (0.026)	0.0 (0.02)
Time Spent on Child Needs (minutes/day)	94.5 (3.73)	88.5 (8.41)	6.0 (9.5)
Time Spent on Child School Work (minutes/day)	5.0 (0.6)	3.5 (1.1)	1.6 (1.5)
<b>Child</b>			
Age (Years)	10.0 (0.04)	10.0 (0.1)	0.2 (0.1)
Female	0.5 (0.01)	0.5 (0.02)	-0.0 (0.02)
Enrolled	0.8 (0.01)	0.8 (0.02)	-0.00 (0.02)
Child Educational Activity Outside School (Min/Day)	121.3 (3.3)	117.3 (1.4)	4.0 (3.4)
Child Study Time Outside School (Min/Day)	60.4 (2.3)	57.8 (0.9)	2.6 (2.3)
TimeHelpRead (Hours/Week)	2.6 (0.2)	2.3 (0.1)	0.3 (0.2)
ChildPlay (Min/Day)	179.9 (6.0)	184.0 (2.7)	-4.1 (6.7)
ChildPaidWork(Min/Day)	14.1 (3.0)	16.0 (1.4)	-1.9 (3.6)
ChildHouse Work (Min/Day)	66.8 (5.6)	65.2 (2.4)	1.6 (6.1)
ChildSchool Time(Min/Day)	273.7 (2.5)	271.9 (5.8)	1.9 (6.4)
ChildRest Time(Min/Day)	617.4 (4.3)	611.2 (2.0)	6.2 (5.0)
Public	0.71 (0.01)	0.69 (0.02)	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.