Parents' Schooling & Intergenerational Human Capital: Evidence from India

Naveen Sunder*

Economics Department, Cornell University

December 13, 2018

JOB MARKET PAPER

Abstract

This paper examines the impact of a national-level school construction program in India on educational outcomes of direct beneficiaries and their children. Between the years 1993-2004, the District Primary Education Programme (DPEP) served over 50 million children and prioritized districts with below-average female literacy rates. I use a fuzzy regression discontinuity design to estimate the causal impact of the programme by comparing outcomes of school-age children in districts on either side of the average female literacy cutoff. To uncover the difference in timing of programme implementation across districts, I use unique archival information that I collected and digitized. The results show that DPEP increased school access, enrollment, literacy and years of education for both male and female direct beneficiaries. I then provide one of the first evidence of intergenerational effects of a school construction policy. Using test score data spanning the years 2007-2014, I find that children whose mothers were DPEP beneficiaries had higher scores on math (0.18 S.D.), vernacular (0.19 S.D.) and English (0.09 S.D.) tests. Daughters' test scores went up by more than 10 to 15 percentage points higher than that of sons. Father's DPEP exposure had no effect on children's learning. I find evidence that the intergenerational impacts may be mediated through mother's increased bargaining power, higher investments in children's education and better health/health related behaviors.

JEL classification: I21, I25, O15, J16, J24

Keywords: Intergenerational Outcomes, Human Capital, India, Regression Discontinuity

*Address: B16 MVR Hall, Cornell University, Ithaca, New York- 14853 , e-mail: *fn63@cornell.edu*. For their thoughtful discussions and useful comments, I thank Mathew Abraham, Jorge Aguero, Anaka Aiyar, Prateek Bansal, Averi Chakrabarti, Nancy Chau, David Evans, Andrew Goodman-Bacon, George Jakubson, Heidi Kalia, Francesca Marchetta, Prabhu Pingali, David Sahn, Erik Thorbecke, Mallika Thomas and Kira Villa. I also thank seminar participants at Centre for the Study of African Economies (CSAE) 2018, Cornell University, International Association of Applied Econometrics (IAAE) 2018 and the SAU conference 2018. All remaining errors are my own. **Preliminary draft - Please do not cite or distribute without prior permission.**

1 Introduction

Over the past few decades, there have been marked improvements in schooling availability in most parts of the world (Barro and Lee, 2013). Even though this has meant that more children attend school, it has not always been accompanied by improvements in learning levels in schools, especially in developing countries. For example, although net enrolment rate in India is close to 100 percent, only 43 (33) percent of sixth (seventh) graders in India could read a vernacular text at the second grade level, and around one-quarter of fifth grade students were able to solve a math (division) problem (ASER, 2016). Various studies have demonstrated the adverse effects of low childhood learning (Glewwe, 1996, Behrman et al., 2008, Kaila et al., 2018). This has lead to an enhanced focus on policies aimed at improving learning outcomes in schools. Although learning among children is a function of school, household and individual level inputs (Glewwe and Muralidharan, 2016), a bulk of the interventions addressing learning deficits have targetted school inputs. The findings of these evaluations are mixed – test scores have been found to be unresponsive to a number of these interventions (summarized in Kremer et al., 2013 and Muralidharan, 2013).

In this paper, I focus on the household inputs channel[1], and the intergenerational effect that parents can have in shaping children's educational outcomes (Black et al., 2005, Holmlund et al., 2011, Carneiro et al., 2013). In particular, I use data from India to evaluate if enhanced schooling access for parents, when they were of school-going age, not only improves their own educational outcomes, but if it also leads to improvements in their children's learning outcomes.

I contribute to this literature in the following ways. First, I add to the evidence on the important role played by parents, especially mothers, in shaping learning outcomes of children. This outlines the critical role that household inputs can play in complementing school factors to engender better educational outcomes. Second, this is one of the first papers to look at the intergenerational impact of a large scale school construction policy. Most papers have restricted their focus to the impact of such policies on direct beneficiaries (Duflo, 2001, Azam and Saing, 2017). Third, it evaluates the learning impacts of a school construction policy, something that is uncommon in the literature (Burde and Linden, 2013). Fourth, large parts of Central/Western Africa and South Asia have comparable educational indicators to what India had at the time of DPEP implementation. Therefore, results from this analysis could be informative for policy formulation in these contexts.

[1]Studies have looked at different types of interventions in this respect - information provision (Jensen, 2010, Loyalka et al., 2013, Wang et al., 2014), conditional cash transfers (Behrman et al., 2009, Baird et al., 2011, Barrera-Osorio et al., 2011), scholarship programmes (Blimpo, 2014, Li et al., 2014) and other in-kind transfers (Oster and Thornton, 2011, Muralidharan and Prakash, 2017).

DPEP was implemented in 271 districts (in 18 states) between the years 1993 and 2004. This programme expanded school access by constructing primary and upper-primary schools, and was targetted towards districts with female literacy below the national average at the time (39.2 percent). This assignment mechanism creates a discontinuity in the probability of receiving the programme around the threshold of 39.2 percent female literacy. That is, the probability of receiving the treatment is much higher in districts just below this cutoff, as compared to districts just above this threshold. In implementing the Fuzzy RD design I use data-driven tools which estimate the causal impact within an optimal neighborhood around the RD cutoff. I employ two different approaches to constructing these neighborhoods, namely Mean Squared Error (MSE) approach and the Coverage Error Rate (CER) approach (Calonico et al., 2014, 2016) – the results are consistent across both approaches.

One of the innovative aspects of this paper is the use of unique archival data that I collected, which enables me to uncover differences in timing of programme implementation across districts, something that other analyses examining this programme have not done (like Azam and Saing, 2017, Khanna, 2015). These archival documents consist of programme expenditure information, field reports on implementation and other state/federal government documents monitoring DPEP progress. I triangulate information from these documents to uncover when a programme actually took effect in a treatment district. This enables me to accurately infer the start year of the programme in each of the 271 treatment districts, which I use in my empirical strategy[2] [3].

By comparing districts on either side of the RD cutoff, I establish that DPEP regions had higher rates of school construction[4]. This implies that during the programme years children of school-going age in DPEP districts experienced increased schooling infrastructure, as compared to children of the same age group in non-DPEP districts. Since the bulk of the schools constructed under the programme were primary and upper-primary schools (grades 1 to 7), children between the ages of 5 and 14 years are expected to benefit from DPEP. Since DPEP ended in 2004, only children born before 1999 could potentially benefit directly from the programme. The earliest cohort impacted by DPEP would vary from one district to another, depending on when DPEP took effect in the district. For example - If DPEP was implemented in a district in 1995, then people born between 1981 and 1999 would be the direct beneficiaries in this district. I find that both male

[2] I use 2004 as the uniform end year of the programme across the country. This is because the funding for the programme was cut in a phased manner between 2001 and 2004, implying the programme ended around late 2004.

[3] Since the non-DPEP districts did not receive the programme, there is no obvious start year of the programme in these districts. Therefore, I use the within-state average start year of all treatment districts as the start year of the programme in non-DPEP districts. This is needed to define a comparable control group - discussed in detail later

[4] However, there were no changes in the quality of schools in the DPEP districts. I measure quality using indicators on physical infrastructure, teacher quality, grants/incentives and school oversight.

and female direct beneficiaries of DPEP had higher enrolment, literacy and years of education, as compared to non-beneficiaries. This is in line with findings from other comparable studies (Duflo, 2001, Burde and Linden, 2013, Kazianga et al., 2013).

In addition, I use school learning data from the Annual Survey of Education Report (ASER) data to provide one of the first estimates of the intergenerational impacts of a school construction policy. To do so, I use data from the years 2007 to 2014 and focus on a sample of children whose parents were of school going age during the DPEP years, but the children themselves did not directly benefit from the programme. To ensure this, I restrict my sample to children who started school after the programme had ended, that is, children who started school in/after the year 2005. Therefore, the intergenerational sample consists of children who satisfied two conditions - they started school in/after 2005 and they had at least one parent who was between 5 to 14 years of age during the DPEP years in their district.

Using an analogous RD framework (as above), I find that the children whose mothers were the sole beneficiaries of DPEP performed better on vernacular (0.19 S.D.), math (0.18 S.D.) and English (0.09 S.D.) tests, and were more likely to be enrolled in school and achieve smooth progression through grades, as compared to similar children born to comparable women in non-DPEP districts. In contrast to these results, I find no statistically significant positive impacts among the sample of children whose fathers (and not mothers) were exposed to DPEP. Thus, while both genders gained from direct exposure to the school construction programme, the results suggest that only women were able to transmit their benefits to the next generation. Like the evidence from several other studies (Desai and Alva, 1998, Persico et al., 2004, Case et al., 2005, Güneş, 2015), this result demonstrates the critical role played by mothers in their children's human capital development.

I conduct a number of falsification and robustness checks. In a falsification check, I show that people who were too old to directly benefit from the DPEP did not experience any of its gains. In line with expectations, in another falsification check I find that children of these people also do not show any DPEP impacts. Both approaches to RD estimation employed here require the researcher to specify the kernel function and polynomial form to be used in the estimation. I show that the results are robust to changes in the RD approach (MSE & CER), kernel function (triangular and epanechnikov) and polynomial form (linear and quadratic). In another robustness check, I use a stricter cutoff to define the potential direct beneficiary sample - I use the 5-12 years age group to define the direct beneficiary sample, rather than 5-14 years that I use in the main estimations[5].

[5]A lower cutoff of 12 years might be especially valid for girls since they tend to drop out of schools at younger ages

Results remain robust to this change. In the main results, I use approaches to RD estimation that use data within a neighborhood around the RD threshold to estimate DPEP impacts. As a robustness check, I instead use the 2SLS IV technique, which imposes parametric assumptions on the whole data to estimate the impact coefficient. Although the value of the coefficients differ from the main results, the effects retain their sign and significance.

Finally, while the intergenerational effects that I observe can be plausibly attributed to parental educational attainment through DPEP, I conduct additional investigations to understand how individuals were able to use their additional education to impact their children's learning. I find that the women who benefitted directly from DPEP were healthier, had enhanced bargaining power and were investing more in their children's education as compared to similar women in non-DPEP districts.

2 Literature & Background

2.1 Literature Review

Given that I look at how those who directly benefit from enhanced schooling access are able to impact their own children's educational outcomes in the future, my study is closely related to the body of literature investigating the intergenerational links in education outcomes between parents and children, where parent's enhanced education might potentially have a direct positive impact on the educational outcomes of their children, in terms of number of years of education and enrolment (Black et al., 2005, Sacerdote, 2007, Holmlund et al., 2011)[6]. In addition, it is plausible that better educational outcomes for the parent generation might lead to improved health for themselves (Amin et al., 2013, Agüero and Bharadwaj, 2014, Grossman, 2015, Grépin and Bharadwaj, 2015), higher age of marriage and age at first birth (Glick et al., 2015, Grant, 2015, Marchetta and Sahn, 2016), increased contraception usage and better antenatal care practices (Andalón et al., 2014, Johnston et al., 2015, Behrman, 2015), higher bargaining power for women (Lundberg and Pollak, 1993, Duflo, 2012, Samarakoon and Parinduri, 2015) and higher investment in children's education (Yoong, 2012), among other outcomes. These in turn might have a positive impact on their children's outcomes (intergenerational effect). Among these indirect mechanisms, in the cur-

than boys due to a variety of reasons - including onset of menarche and child marriage.

[6]This direct link between the educational outcomes of parents and their children has been studied extensively using many different empirical strategies, which include comparing twins and their children (Behrman and Rosenzweig, 2002, Bingley et al., 2005, 2009, Holmlund et al., 2011), natural experiments related to compulsory schooling laws/tuition fees/location (Black et al., 2005, Oreopoulos et al., 2006, Carneiro et al., 2013, Chevalier et al., 2013) and comparing outcomes between biological children and adopted children of the same parents (Sacerdote, 2002, 2007, Silles, 2017).

rent setting I find evidence for a positive impact on women's bargaining power, investment in children's education and own health/health related behaviors.

This paper also adds to the limited literature that estimates the effects of increasing the supply of schooling infrastructure on educational outcomes in developing countries by estimating intergenerational effects of such programmes. The bulk of this literature (described below) estimates the impacts on direct beneficiaries (Duflo, 2001, 2004, Handa, 2002, Alderman et al., 2003, Burde and Linden, 2013, Kazianga et al., 2013, De Hoop and Rosati, 2014).

There so exist some papers that have evaluated the impacts of the DPEP policy, but only on socioeconomic outcomes for direct beneficiaries. Khanna, 2015 estimates the effect of this school expansion on the rate of return to education, while Azam and Saing, 2017 find that DPEP beneficiaries had higher enrollment, number of years of education and probability of completing primary education. Using data from 42 districts that received the programme in phase one, Jalan and Glinskaya, 2013 find small effects on enrolment, that too mostly for socially disadvantaged groups. While there are some parallels between the current analysis and these previous DPEP papers, there are critical differences. I use detailed archive data to uncover the exact timing of the programme in the 271 treatment districts. This enables me to define the beneficiary sample more accurately and distinguishes this analysis from the aforementioned papers – which do not account for the different start dates of the program in this manner. Also, my results not only confirm that the program had positive impacts on the educational attainment of direct beneficiaries, but also establish that it had intergenerational impacts - which is another key innovation of my analysis.

2.2 DPEP Programme

The District Primary Education Programme (DPEP) was the flagship education programme of the Indian government in the 1990s. Implemented in a phased manner across the country, the programme was oriented towards achieving universal education through an increase in schooling infrastructure. Program rules stipulated that DPEP school construction would be targetted towards districts which had Female Literacy Rates (DFLR) below the national average. According to the 1991 census, the most recently available census data at the time, the national average female literacy was 39.2 percent. This was regarded as the cutoff for determining district eligibility for the programme, which was largely followed. The central government also specified that DPEP would only be introduced in districts that had successfully implemented the Total Literacy Campaign (TLC), a programme that aimed at improving literacy levels across the country (Rao, 1993). Since the TLC had been implemented successfully across all districts in India by 1994, this criterion turned out to not matter for selection into the program (Jalan and Glinskaya, 2013). The program was introduced in 42 districts in 1993, and eventually extended to 271 districts across 18 states.

DPEP was highly effective in boosting India's school infrastructure. By the year 2000, the programme had led to the construction of more than 160,000 schools and the hiring of around about 1,77,000 teachers (Azam and Saing, 2017). According to Jalan and Glinskaya, 2013, the programme potentially impacted close to 51.3 million children across the country. The decentralized nature of DPEP implementation also led to the establishment of local monitoring and implementation agencies at the sub-district level - more than 3,000 block resource centers and nearly 30,000 cluster resource centers. These led to enhanced local monitoring capacities, and these remained in place even after the end of the programme. Pandey, 2000 identifies several drivers that led to the relative success of the DPEP programme. These include a strong focus on student learning, decentralization and local empowerment, along with constant attention on building school capacity.

DPEP was financed by different levels of governments. The central government bore 85 percent of the costs with the support of donors like Official Development Assistance (ODA), the Royal Government of the Netherlands, the U.K. Department for International Development (DFID) and the World Bank. The remaining 15 percent was contributed by the state governments. To ensure that the new programme did not crowd out state funds that were already being spent on existing educational policies, the central government stipulated that states had to continue with their pre-existing non-DPEP expenditures. Since the DPEP funds were committed over and above the existing education budget, the programme represented a massive surge in education expenditure across the country.

3 Data

I use multiple sources of data for this analysis. I describe each of these below.

3.1 Annual Survey of Education Report (ASER)

Pratham, an Indian non-profit organization conducts national annual surveys to measure schooling and learning outcomes across rural India. These Annual Surveys of Education Report (ASER) provide repeated cross sectional data on the educational profile of children aged five to 16 across the entire country. In my analysis, I use eight rounds of ASER data, spanning 21 states (see figure 2) for the years 2007-2014. ASER data contain student test scores for math, vernacular and English. Each test that is fielded includes four questions, with each being more difficult than the previous question. The score assigned to a child indicates the level of difficulty that a child was able to solve/master. For example, on the reading section, a child gets a score of zero is she could not read anything; one if she is able to recognize alphabets, and two, three or four depending on whether she is able to read words, sentences or a paragraph respectively. I use this information to create a score variable which ranges from zero (if a child failed to answer any question correctly) to four (if she demonstrated the highest level of proficiency).

ASER also contains other information on additional educational outcomes for each child that is surveyed. Based on available information, I create a grade-for-age variable that measures whether a child was held back at school or joined school at a later age than he/she should have. Here I use the fact that school starting age in India is typically 5-6 years. This variable takes a value of one if the child is on track in school, that is when age minus grade is at most six, and a value of zero otherwise (akin to Shah and Steinberg, 2017). For example- if a nine year old child is in grade three or four, she get a value of one. But if she is in grade 2, then she gets a value of zero. I create an additional indicator variables for whether a child has ever been enrolled in school.

One of the key advantages of using the ASER dataset is its national coverage. The sample size of each survey is large and it encompasses all rural districts in India[7]. The ASER data is also unique in that it measures educational achievement at home instead of schools. As a result, the sample includes children who have dropped out of school and children who have never enrolled, along children currently attending school. Additionally, the format of the tests, and the way they are administered and scored have remained uniform across different years and regions, thus facilitating spatial and temporal comparisons.

3.2 District Information System for Education (DISE)

For information on schools, I pull information from the District System for Education (DISE), a database of government schools across the country. This dataset contains data on the physical infrastructure and amenities present in each government school, as well as information on teachers, enrolment and other school characteristics. This source compiles data provided by school headmasters on a yearly basis. Information provided by schools is verified at the cluster level and subsequently transferred to the district level. At this stage, the data is verified again before being

[7] See this link for further details on the ASER sampling strategy. In fact, this model has been adopted in some African countries to measure learning levels among children - UWEZO Surveys

aggregated, digitized and published. I use the DISE data for the year 2005, which is the earliest year of data that is publicly available.

I use the DISE data in several different ways. The DISE data allows me to test whether the DPEP led to higher school construction in treatment regions during the years the program was in operation. The DISE data also enables me to examine treatment-control differences in school quality before, during and after the implementation of DPEP. Finally, Using the DISE data, I cross-verify the accuracy of information I gather from government archival documents (described below).

3.3 Archived Government Records

In order to isolate the timing of DPEP program initiation in different parts of the country, I take advantage of archived government records on the implementation of the program. Since the DPEP started in the early to mid 1990's, a large amount of the documentation pertaining to the programme was initially not in a digital format. Although, some of the documents have been recently digitized, a large amount of information still exists solely in paper format in libraries and other institutions. I gather data from these digital and paper files to infer information for programme districts across various states (a total of 271 districts). I discuss this in further detail in the next section.

3.4 District Level Health Survey (DLHS)

I obtain data on the educational attainment of DPEP's direct beneficiaries from the DLHS, a household-level survey conducted by the government of India. This survey collates statistics on a wide range of indicators related to household demographic characteristics, maternal and child health, and family planning. I draw upon two rounds of this data – rounds in 2007-2008 (wave 3) and 2012-2013 (wave 4). In addition, I also use this data to investigate the mechanisms that could potentially be responsible for DPEP's intergenerational effects.

3.5 Indian Human Development Survey (IHDS)

The IHDS is the other dataset that I use to probe the mechanisms that might have mediated the intergenerational impacts of DPEP. The IHDS is a nationally representative panel survey conducted by the University of Maryland in collaboration with the National Council of Applied Economic Research, New Delhi. The first round, IHDS-I, was conducted between November 2004 and October 2005 and covered 41,554 households across 33 states and union territories of India (Desai and Vanneman, 2005). In this analysis, I primarily use the female module that was administered to ever-married women between the ages of 15 and 49.

4 Empirical Strategy

Before discussing the details of the empirical strategy, I describe some important characteristics of the sample creation of the direct and indirect beneficiaries (children of direct beneficiaries) of DPEP. It is to be noted that in some places the direct beneficiaries are referred to as the *parent* generation and the indirect beneficiaries as the *child* generation or intergenerational beneficiaries.

4.1 Sample Definition

In this section, I first describe the sample I use for my analysis. The direct beneficiaries of DPEP are those who were directly exposed to the program since they were of school-going age at the time of programme implementation. But, the focus of the analysis is on estimating the intergenerational effects of DPEP. Hence, my main sample constitutes the children of these direct beneficiaries.

4.1.1 Timing of the programme - DPEP Districts

DPEP was announced in 1993-94. Therefore, one way to identify the direct beneficiaries of DPEP would be to look at everyone who was of primary school-going age at this point in time, an approach used by Khanna, 2015. However, it is worth recognizing that DPEP constructed over 100,000 schools spread across 271 districts. Given the inherent challenges and the wide geographical spread of DPEP, the program began at different times in districts across the country. Using archived government documents[8], I find the exact year of initiation of DPEP in each treatment district and use that date to identify the sample of people who were of primary school-going age at the time that DPEP was implemented in their district.

Given that I manually infer these start dates from these archival documents, I conduct a check to verify if this data holds up to further scrutiny. Using data from DISE, I plot the annual rate of

[8]I manually obtained the start date of the programme by triangulating information on programme expenditures, field reports on programme implementation and progress reports created at the state/central level to infer the time when school construction under DPEP actually began in the treatment districts. For example- I consider DPEP to have begun in a district if money has been received by the district(central documents), it has been spent by the state/district authorities (expenditure reports) and construction of schools has happened (progress reports). Therefore, when multiple pieces of information about DPEP implementation in a district provides a coherent narrative, I use this to infer the time when the programme was implemented there.

growth of schools over time in select districts (figure 3). DPEP was meant to boost school construction in treatment districts. Therefore, one would expect a deviation from the long term pattern in the rate of growth of schools in or around the time that the programme was actually implemented in districts. On the graph, the red line (dashed) represents the year when the DPEP programme was meant to begin in all the treatment districts (1993-94) and the black line (solid) shows the year in which the programme appeared to have been implemented in a particular district according to the archived government records. As the graphs illustrate, the actual spike in the rate of growth of schools in the districts is closer to the start year that I identify from the government documents (solid black line), rather than the uniform start year of 1993-94 (dashed red line). While the data for the underlying graph and the date from government archives (the black line) are from different sources, both pieces of information indicate that DPEP implementation began in different districts well after the central announcement of the program in 1993-94. For my analysis, I thus use the date inferred from the archives for each district (black line) to identify which individuals were exposed to the implementation of DPEP in that district.

4.1.2 Sample Selection - Non-DPEP Districts

I seek to identify the causal impacts of DPEP by comparing the prospects of individuals who were of school-going age in DPEP districts for the duration of the program, with the outcomes of of comparable individuals in non-DPEP districts. Since the non-DPEP districts did not receive the programme, there is no obvious start year of the programme in these districts. If DPEP were assigned to these districts, some people by virtue of their age would have benefitted from it, while others would have missed out. The former is the comparison group in my analysis. Thus I need to understand when the program would have started in these districts should they have received it.

I use two different methods to assign a likely DPEP start year for the control districts. Under the first method, for each state I take the average starting year of DPEP in treatment districts within that state, and consider this year to be the start date for all control districts within the state. I use this in my main analysis. Another way to impute the starting year would be to assign all control districts the nationwide average start date among the treatment districts (instead of using the individual state averages). This method ignores the state (or regional) differences in implementation patterns across different districts. To show the robustness of my results, I replicate the main results using this alternative (national) definition of identifying the control group.

4.1.3 Direct Beneficiary Sample

DPEP was mostly geared towards the construction of primary and upper-primary schools (up to grade 7). Thus, the first step is to identify individuals who were of primary school-going age in the treatment and control districts at the time of DPEP implementation. Primary school children tend to be between the ages of 5 and 10, but studies from India indicate that even children up to the age of 13 years might remain in primary school, mostly due to delayed school entry and/or uneven grade progression (Azam and Saing, 2017). As a result, I define the main sample of direct DPEP beneficiaries to include children who were between 5 and 14 years of age during the DPEP implementation years. I check the robustness of my results to an alternative definition that consider 5 to 12 years to be the relevant group.

4.1.4 Intergenerational Sample (Children of Direct Beneficiaries)

My main focus in this analysis is to identify the intergenerational effects of DPEP. In order to do this, I need to identify children who themselves were not directly impacted by the programme, but who had parents who were exposed to DPEP. Given that the DPEP programme ended around late 2004[9], I identify those who started school only after this time period. Specifically, I restrict the sample of children in my analysis to those who would have began their schooling in 2005 or later.

4.1.5 Identifying district of schooling

Ideally, an individual would likely be considered a direct beneficiary of DPEP if he/she were of school-going age when they resided in a district which received DPEP. The data I use to identify the impacts of DPEP on the direct beneficiaries, were collected in the year 2007 and later. While they include information on past educational attainment of direct beneficiaries, the surveys did not ask individuals to report their district of residence during their school-going years. I thus use individuals' current district of residence to assign treatment/control status to each individual (direct beneficiary) in my sample. The potential issue with this assignment mechanism is that, due to migration, the current district of residence may not be the same as the one that the individual lived in when they went to school.

One of the major sources of migration in India is post-marriage movement of women. In India, which is largely a patriarchal country, it is common for women to move to live with her husband's

[9] A different national program called Sarva Shiksha Abhiyan (SSA) was introduced in India around 2001-2002. While this program also aimed at expanding educational opportunities across the country, it differed from DPEP in that it wasn't targeted based on an allocation rule.

family after marriage. This practice might lead to systematic inter-district migration particularly for women. As a result, the current location of women may not be an appropriate proxy for their past district of residence. There is however evidence that indicates that the majority of marriage-related migration occurs within and not across districts. Bloch et al., 2004 show that on average, a woman moves 21 miles after marriage. In 2001, the average size of an Indian district was close to 2,100 squared miles and thus it is likely that most post-marriage migration occurred within districts. Evidence from multiple nationally representative data sources point to this conclusion. Using National Sample Survey (NSS) data from 1983, 1987 and 1999, Topalova, 2007 finds that although nearly 60 percent of rural women report a change in their location after marriage, a very small proportion (7-8 percent) move across district boundaries[10]. In light of these statistics, I argue that it is reasonable to consider the current district of residence to be the district in which individuals went to school. Hence assigning treatment status based on the current district of residence is unlikely to lead to substantial misclassification errors.

4.1.6 Changes in District Boundaries

India has witnessed substantial administrative decentralization over the past two or three decades - the number of districts in the country has increased from 466 (in 1991) to 640 (in 2011). Given that the DPEP programme was implemented at the district level, it is crucial that I be able to link the residents of current districts to the districts that existed during the program years (1993-2004)[11]. This is important because the parent cohort went to school in old districts (1993-2004). Hence, whether or not they were exposed to DPEP would depend on which district one resided in around program implementation.

Based on the discussion of historical changes in district boundaries in Kumar and Somanathan, 2009, I map districts in 2001 to their parent districts in 1991. While Kumar and Somanathan, 2009 only cover district changes that took place until 2001, I extend their analysis to similarly match districts that were created in subsequent years (until 2011). In doing so, I follow these steps. In some cases, multiple districts were created from a single parent district, and so I assign the treatment status of the parent district to all the new districts. In other cases, several districts were combined to form a new big district. Here, if all the parent districts had the same treatment status, I assign the same status to the new district. However, there are cases in which the treatment status

[10]Using more recent data, Kone et al., 2017 show that overall inter district migration among women in India stands at about 9-10 percent. Almost 70 percent of this migration is due to marriage, but most of it (more than 3/4th) occurs within the same district.

[11]India is administratively split up first into states, which are then split up into districts. These are akin to counties in the US.

of the parent districts differ. If so, if more than 50 percent of the population of the new district comes from parent districts of a certain treatment status, I assign this treatment status to the new district. I use analogous rules in assigning programme start years and district characteristics (such as the 1991 district female literacy rates) to the newly created districts.

4.2 Empirical Methodology

The analysis here estimates the impact of DPEP on two different samples. The parent sample comprises of those who were of school going age (5-14 years) while DPEP was being implemented in their district; these are the direct beneficiaries of the program. The child sample consists of the children of the direct beneficiaries; these were indirect beneficiaries of DPEP, who started their schooling after DPEP had been phased out in 2005. I define the treatment group depending on when a certain district received the scheme. I use government archival records to infer the exact start year of the programme in each of the 271 treatment districts (discussed earlier).

My estimation strategy relies on two important sources of exogenous variation – 1. the district female literacy cut off of 39.2 percent, and 2. the spatial and temporal variation in the implementation of DPEP. With regards to the former, the programme was assigned on the rule that districts with female literacy below the national average rate (39.2 percent) were more likely to receive DPEP. As a result, when moving from the right (above) of the cutoff to the left (below), the probability of treatment receipt experiences a discontinuous increase. This is illustrated in figure 4, where I graph the probability of programme receipt against the female literacy rates of different districts. The figure illustrates that around the RD cutoff (39.2 percent) there is a large discontinuity in the probability of receiving the programme. This setup is a Fuzzy Regression Discontinuity (FRD) design, and allows me to estimate the impact of DPEP around the allocation cutoff.

The intuition here is to identify the effect of DPEP by comparing the outcomes of a sub-set of observations on either side of the RD cutoff ($\bar{x}$). This subset of observations lies within a neighborhood around the cutoff. Recent innovations in the field of RD estimation and inference make it possible to employ the underlying data to estimate the size of the neighborhood[12]. This is in contrast to the previously used methods, like 2SLS-IV, to compute the causal impacts (discussed later).

The neighborhood typically takes the following form: $[\bar{x} - h, \bar{x} + h]$, where h is the optimally determined bandwidth. There are two main data-driven approaches that can be used to calcu-

[12] These observations that are close to the cutoff on either side are similar on most characteristics, except their probability of receiving DPEP, something that I verify in the results section.

late the optimal bandwidth - the Mean Squared Error (MSE) method and the Coverage Error Rate (CER). Although both approaches are semi-parametric in nature, and involve trade-offs between efficiency and robustness, they differ in the optimality criterion used to calculate the bandwidth. In implementing these approaches, I specify several parameters to facilitate the bandwidth estimation. First, I select the kernel function that is to be used to determine the weight assigned to each observation. In my analysis I use a triangular kernel which puts higher weight on observations close to the RD cutoff and less weight on observations that are further away. I show that the results are robust to using an epanechnikov kernel. Second, I select the polynomial function form to used in the model estimation. To allow for more flexibility, I use a quadratic polynomial for the main results, but also use a linear function to show that the results are not sensitive to this change.

In estimating the impacts of DPEP on direct beneficiaries, I incorporate a series of control variables such as age of the individual and categorical variables for religion, caste, state and year of data collection. For the child level specifications, I control for child's age and gender, age of both parents, rain-fall shocks in-utero/birth year of the child (to proxy for environmental circumstances during this crucial period of growth)[13], and dummies for caste, religion, state and year of data collection. In all specifications, I cluster standard errors at the district level.

Imbens and Kalyanaraman, 2012 discuss the MSE approach and devise an asymptotically optimal procedure to estimate the bandwidth. Under this procedure, they assumed a squared error loss function and The formula used to determine the ideal/appropriate bandwidth under this procedure is:

$$h_{MSE} = C_{MSE}.n^{-1/(2p+3)} \tag{1}$$

where n is the sample size, p is the order of the polynomial chosen by the researcher. The constant C_{mse} depends on the kernel function, the polynomial form and the bias/variance of the estimator among other factors[14]. Calonico et al., 2017 discuss *robust-bias* corrections that make inference feasible with the MSE approach[15]. In my analysis, I report these robust-bias corrected

[13]I use the same rainfall definition as used in the main analysis in Björkman-Nyqvist, 2013.

[14]This constant is unknown and needs to be estimated in order to ascertain the bandwidth (h_{mse}). Imbens and Kalyanaraman, 2012 propose a plug-in estimator that is based on a reference model to calculate an estimated value of the constant ($\hat{C}_{mse}$). This estimated value is then used to calculate the value of the bandwidth ($\hat{h}_{mse}$).

[15]Calonico et al., 2014 improved on the initial procedure suggested by Imbens and Kalyanaraman, 2012 by providing a bandwidth selector that has superior finite sample properties. In addition to being completely data driven and providing a mean squared error optimal bandwidth, this improved bandwidth selection procedure also has desirable small and large sample properties (Cattaneo and Vazquez-Bare, 2016). But, it has been shown that the standard errors of the RD estimate obtained from this procedure are not valid for inference. This is because the way the procedure balances

standard errors along with the coefficient estimate.

However, Cattaneo and Vazquez-Bare, 2016 show that when inference is the goal of the estimation, then the MSE estimator and the associated robust-bias corrected confidence intervals may not be the preferred bandwidth selection approach. Their discussion demonstrates that the bandwidth value that reduces the Coverage Error (CE) of the confidence interval would be more appropriate. This is given by:

$$h_{CER} = C_{CER}.n^{-1/(p+3)} \tag{2}$$

where C_{CER} is a constant different from C_{MSE} and is estimated based on the underlying data. The confidence interval of the RD estimate based on this bandwidth (h_{CER}) has been shown to have demonstrably superior properties associated with inference[16][17].

These data-driven approaches are new to the literature and have not been used extensively in empirical applications.. RD analyses usually employ global polynomial approaches which tend to be subjective, not data driven and leads to larger bandwidths, While the global method works best when there is minimal misspecification bias (discussed in Gelman and Imbens, 2017), which is rare to achieve, the approach has appeal since it allow researchers to estimate causal impact with least squares estimation. I thus also estimate the impacts of DPEP with the 2SLS and present these results as a robustness check. I describe the global approach in Appendix A.

4.3 RD Validity

For the RD design to be valid it is critical that individuals not be able to manipulate their treatment status by systematically positioning themselves on either side of the cutoff. If individuals can choose their own value of the running variable, then they can potentially decide whether or not to be a part of the treatment group. This would lead to non-random assignment to treatment, which would complicate the identification of the causal impact of the treatment. Such violations could occur in this case in two potential ways - if sub-national governments (at the state or district

between the bias and the variance makes inference logically inconsistent (for details refer to Calonico et al., 2014). This issue in these bandwidth selection procedures implies that the regular confidence interval that they produce cannot be used for inference. In the limiting case, where we assume a zero bias, the bandwidth size (h_{mse}) tends to infinity (since C_{mse} is inversely proportional to the bias).

[16] In addition, the bandwidth which minimizes the Coverage Error (CE) is also always smaller than the bandwidth which minimizes the Mean Squared Error (MSE)That is, the number of observations used in the estimation using MSE is *larger* than (or equal to) the number of observations used in the estimation using the CE method.

[17] Cattaneo and Vazquez-Bare, 2016 note that owing to the large degree of variability in the point estimates, the RD coefficient from the CER procedure may not always be useful in empirical applications. Even so, I report the point estimates and the associated confidence intervals from these estimations. I primarily focus on the confidence intervals and discuss their relevance in assessing the statistical significance of the estimates.

level) were able to choose their treatment status or if individuals were able to affect their treatment status through systematic migration.

It is unlikely that states/districts were able to manipulate their values of the running variable (district female literacy rate) since programme allocation was based on 1991 census data, which was collected at an earlier point of time by a central authority in India which is independent of state/district oversight. Additionally, the census data was collected in or before 1991, whereas the programme was announced in 1993. This meant that there was little chance that the states/districts knew about the programme when the census data was collected. Furthermore, it is highly likely that the states (or districts) had limited knowledge of the exact decision rule regarding the programme prior to DPEP implementation[18], more so because no other government programmes in the past (or since) appear to have been allocated based on the district female literacy rate.

While individuals could potentially have determined their treatment status through systematic migration across districts, I argue that this is unlikely in India and could not have been large enough to bias the estimates that I identify through my analysis. First, migration across districts in India in the 1990's was fairly low (Topalova, 2007). Second, the main reasons for migration in India are marriage and employment. Schooling choice (especially primary school) was not a major reason for migration in India, especially in rural India around the time DPEP was implemented (1993-2004). In terms of migration that is related to seeking enhanced education opportunities, most of it might be expected to be confined to the realms of higher education (high school and beyond). Since the DPEP programme mostly constructed primary, upper primary and secondary schools, the case for systematic migration affecting the composition of the treatment group seems weak.

5 Results

5.1 Discontinuity in Programme Receipt

As a first step, it is critical to show that there is indeed a significant discontinuity in treatment assignment around the programme cutoff (the 1991 national average female literacy rate of 39.2 percent). Figure 4, which plots the probability of a district being part of the treatment group against the 1991 district female literacy rate, clearly illustrates that there is a significant difference

[18]As decisions regarding programme placement were being made by the central government in conjunction with the World Bank and other donors

in the probability of programme receipt around the RD cutoff. This implies that districts just below the cutoff were much more likely to be part of the DPEP treatment group as compared to districts that were just above the RD threshold. Despite their being a significant difference in probability of treatment assignment, it is possible that because of implementation issues this might not translate into differences in the actual number of schools constructed as a result of this programme. This is because it is possible for districts earmarked to receive the programme, due to a variety of reasons, to either not receive DPEP funding or be unable to use the funding properly. Therefore, in addition to showing discontinuity in programme assignment (as announced), it is also vital to establish that there is a significant break in the number of actual schools constructed during the DPEP implementation period in districts around the cutoff. I establish that in the next sub-section.

5.2 School Infrastructure (1993-2004)

I use district level information on number of schools constructed to examine differences in school infrastructure between DPEP and non-DPEP regions at three time periods: in 1993 (Pre-DPEP), between 1993 & 2004 (DPEP years) and in 2005 (end of DPEP). To confirm whether the treatment-control differences observed above are indeed due to the DPEP policy and not due to pre-existing variations, I check whether any such discontinuities existed prior to program initiation in 1993. The results in panel A of Table 1 indicate that while treatment districts had marginally fewer number of schools in 1993 than control areas, the difference is statistically indistinguishable from zero.

Panel B of Table 1 shows the impact of the DPEP programme on school construction during the DPEP years (1993-2004). The results indicate that an average DPEP district received almost 258 more government schools than a comparable non-DPEP district, a difference that is statistically significant. This difference persists when I examine total (public and private) schools and private schools separately, though the latter is not statistically significant. In panel B of Table 1, I also estimate the impact of DPEP on per capita schooling availability - the outcome I examine is the number of schools per 1000 individuals. The results indicate that there was a significant increase in the per capita availability of government schools (0.21 schools per 1000 population) and all schools (0.31 schools per 1000 population) in the treatment districts. I again fail to find an significant differences across the treatment and control districts in the per capita availability of private schools.

To estimate the intergenerational impact of DPEP, we want the parent generation to benefit from enhanced school opportunities, but do not want their children to directly benefit from this programme. This would imply that there should not be significant differences in schooling access when the children start going to school - which is in the year 2005. Therefore, it is critical to establish that there was no difference in schooling access at the end of the DPEP (in 2005). Reassuringly, the results in panel C of Table 1 indicate that there is no statistically significant difference in the total number and per capita (per 1000 population) government/private schools across the RD cutoff in the year 2005. This finding indicates that the results that I identify are likely to emerge solely due to the intergenerational effects of enhanced parental access to schooling.

5.3 School Quality (1993-2004)

Analogous to the analysis above, I test for differences in the quality of school infrastructure at three points in time: in 1993 (Pre-DPEP), between 1993 & 2004 (DPEP years) and in 2005 (end of DPEP). This would help understand if the programme had a significant impact on the underlying quality of schools in DPEP regions. I use DISE school-level census data from 2005 to examine several school quality measures - physical infrastructure (classrooms, toilets, electricity), teacher qualification, school oversight (inspection visits) and grants/incentives received by the school (funding received/spent). These arguably provide a comprehensive overview of the amenities/resources that a school possesses and is a wide enough array of indicators so as to encompass enough aspects of school quality. The results in panel A of Table 2 show that there were no differences in quality of schools in 1993[19], or at the start of the DPEP policy. In addition, for schools built between 1993 & 2004, I find no statistically significant difference on any of the quality indicators[20].

While the DPEP program did not lead to significantly greater numbers of schools in treatment versus control districts at the end of the program (in 2005), it might have led to the construction of superior schools in the former, thus potentially creating a discontinuity in the quality of schooling infrastructure around the RD cutoff once the program was over (in 2005). The children of treatment recipients might thus have begun their schooling in districts with better quality school infrastructure than their control counterparts, thus bringing about higher learning outcomes for the children of DPEP beneficiaries. I verify this and find that in the year 2005 there were no sta-

[19]Ideally, to do this one would have used data on these measures from the year 1993-94. Since such detailed data is unavailable for that time period, I use data from 2005 for schools that were constructed before 1993. This estimation would be valid if there were no systematic differences in upgrades/improvements in schools constructed before 1993 in districts around the programme cutoff. There is no reason to believe that this would be the case.

[20]Ideally, I would want to compare the schools built under the DPEP programme to other schools constructed in this time period to show that the DPEP schools were no different from the other schools. But the dataset does not identify the schools specifically built under this programme. So I compare all schools constructed in this period in districts around the cutoff. Given that DPEP was the flagship government programme for that period, it can be argued that any differences in school quality should be captured in this setup.

tistically significant differences on any of the quality indicators (Table 2). This alleviates concerns about the positive intergenerational learning effects being driven by superior school quality experienced by the children in the treatment group. It also further adds credence to the argument that any positive intergenerational effects of schooling observed in this context are due to enhanced access of parents and not due to improved quality of schooling of parents.

5.4 Effect on Direct Beneficiaries

In this section, I examine the impact that DPEP had on educational outcomes of the cohort of individuals who were of school-going age at the time of programme implementation. As discussed earlier, for the main estimation results I compare people who were of 14 years or below at the time of programme implementation across the RD cutoff. These are the people who would have probably benefitted directly from the DPEP. Table 3 presents estimates for the impact of the DPEP programme on male and female direct beneficiaries separately. I find that the programme had a positive effect on enrolment in school, with both males and females experiencing an 8-10 percentage point increase. Male and female beneficiaries also had more years of education (0.75 - 0.9 years), were more likely to complete primary school (5 - 12 percentage points) and were nearly 9 percentage points more likely to be literate. Thus, in sum, these results indicate that those going to school in the immediate aftermath of DPEP initiation did indeed attain higher schooling through the enhanced schooling access provided by the program. The effects are present for both genders.

5.5 Intergenerational Effects

I now present the main results of this analysis, the impact of the DPEP programme on the learning outcomes of the children of direct DPEP beneficiaries. As discussed in the empirical strategy section, I use estimators based on two different approaches- Mean Squared Error (MSE) and Coverage Error Rate (CER). A child could have indirectly been exposed to the consequences of DPEP programme through either parent (mother or father) or both parents. In my analysis, I consider these cases separately.

5.5.1 Children - Mother was sole DPEP beneficiary

In Table 4, I compare the test scores of children whose mother's had enhanced schooling access due to DPEP with those whose mother's did not have this access. Due to their age, the fathers of neither of these groups could have possibly been impacted by DPEP. Using the CER-optimal bandwidth estimator with a quadratic polynomial. Also, recall that the tests that I look at are scored in a way such that zero means a failure to provide any correct answers and four indicates complete proficiency on the test. Column 1 show that when a woman benefitted from the DPEP programme (but her husband did not), her child's reading test score went up by 0.28, which is around 19 percent of the standard deviation (1.44) - which is also represented in figure 6. Since a one unit increase on the test implies an increase of one skill level, this can also be interpreted as an increase of a little more than one-fourth of a skill. Given that an average child is close to being able to read a word (score = 2), the coefficient implies that DPEP was able to nudge the child of a typical female program beneficiary towards being able to read somewhere between a word and a sentence. The DPEP impact on math scores of 0.21 (column 2 and figure 5) would enable an average child to get closer to recognizing a two digit number (score = 2) rather than a one-digit number (score = 1). This is an improvement of almost 18 percent of the standard deviation (1.14) in math ability. I also find a positive effect of the programme on English reading ability by the children of treatment women - the RD coefficient is 0.10 and it is significant at the five percent level (figure 7). In addition to examining the impact of DPEP on test scores, I also find that the programme has a positive impact on enrollment probability (0.06) and grade-for-age measure (0.04).

No studies have looked at the intergenerational learning impacts of a school construction programme, and hence there are no obvious programmes to compare these results with. To provide some context, I look at other interventions that have aimed to improve learning outcomes in different countries. I find that the effect sizes I find are smaller than, but in line with, other studies from India (and other countries) that have looked at the impact of different school construction programmes (0.4 S.D.(σ) (Kazianga et al., 2013), 0.65 σ (Burde and Linden, 2013)) and other interventions on learning outcomes[21]: around 0.5 σ (Banerjee et al., 2007), 0.2 σ(Glewwe et al., 2009), around 0.2 σ (Duflo et al., 2012) and close to 0.3 σ (Muralidharan et al., 2016).

5.5.2 Children - Father was sole DPEP beneficiary

Akin to the analysis above, I examine outcomes for children whose fathers benefitted from the DPEP, but their mothers did not. The results in Table 5 suggest that there was no statistically significant impact of the programme on this set of children. Although all the estimates (Table 5) are signed in the same way as the estimates for the children whose mothers were DPEP beneficiaries (Table 4), none of the impacts are statistically significant. This cannot be attributed to DPEP not

[21] σ here represents standard deviation

benefiting male beneficiaries - recall, that male beneficiaries exposed to DPEP were found to have improved school attainment (Table 3). Neither can this null result be due to small sample sizes within the bandwidth - the effective number of observations are more than 23,000 in each of the outcomes (except english score). Rather, it seems likely that mothers are able to use their enhanced schooling to improve the outcomes of their children, while fathers are unable to transmit such benefits to their children.

To further probe this result, I divide the children by gender and verify if it is the case that there are gender heterogeneities in the ability to transfer human capital benefits to children. The results in table 6 indicate two patterns - father beneficiaries are not able to benefit children of either gender. Second, daughters gain more from mother beneficiaries across all outcomes. These results are in line with other studies that find the role of mother to be vital in the human capital formation of children (Desai and Alva, 1998, Currie and Hyson, 1999, Currie and Madrian, 1999, Persico et al., 2004, Case et al., 2002, Behrman and Rosenzweig, 2004, Case et al., 2005, King et al., 2007, Güneş, 2015, Vollmer et al., 2016, Alderman and Headey, 2017). In addition, the higher impact on females (daughters) than on males (sons) is similar to the gender difference in the impact of school construction programme in Afghanistan, which was evaluated by Burde and Linden, 2013.

5.5.3 Both Parents Treatment

Table 7 looks at the sample of children with both parents benefitting from the DPEP program. The results are qualitatively similar to the results for the children who only had mothers exposed to DPEP school construction (Table 4). When these results are looked at in conjunction with those for the children with treated fathers, one may conclude that while enhanced maternal schooling through DPEP certainly mattered for child learning, paternal schooling might not have. As pointed out earlier, this is consistent with existing evidence in the development economics literature.

5.5.4 Putting LATE in perspective

The RD estimation procedure estimates the Local Average Treatment Effect (LATE), which in this case is based on observations that are close to the RD cutoff. Put differently, this implies that the RD impact coefficients here would hold for a narrow set of individuals, specifically those around the cutoff. In this context, that would be people living in regions where female literacy rate is close to 39.2 percent. This could mean that the coefficient might not be widely generalizable. Below, I argue from two different perspectives that this criticism might not be relevant in this case.

First, I argue that from a global perspective the results here might be informative about the impact of a similar school construction programme in other developing countries which are in the same stage of educational development as India was at the time DPEP was implemented. There are many countries with large populations, like Pakistan (193 million) and Ethiopia (103 million), that have a female literacy rate around (or below) the RD cutoff (39.2 percent) of this study [22]. Most of these countries are concentrated in Central and West Africa and South Asia. Additionally, there are other countries in Africa and Asia that have higher rates of overall female literacy, but have regions within them where educational indicators are similar to what they were in India at the start of the DPEP. The results here would also be potentially valuable for forming policies in these regions.

Second, I discuss why these results are valuable in the Indian context. If the districts around the cutoff were concentrated in one part of the country, then the results would not be valid for India as a whole. To verify whether this is the case, I plot on a graph the districts that are within a neighborhood of the allocation cutoff. I use the effects on intergenerational reading scores to illustrate my point. The impact of DPEP on the reading scores for the children of program beneficiaries is 0.38, which is based on more than 37,000 observations (out of a total of more than 480,000 observations) within a bandwidth of around 5 percent on either side of the cutoff. While the bandwidth is fairly narrow, the number of observations is sizable, which is uncommon in RD applications of this nature. Additionally, these observations are not localized to a few districts around the cutoff - they come from 46 districts (see figure 8), a non-trivial number, spread across different parts of the country.

5.6 Robustness Checks

Table 4 provides the main set of results for the case when the mother of the child is the sole beneficiary of DPEP. In this section, I discuss the robustness checks that I conduct in which I alter different parts of the empirical strategy[23]. In order to set up an RD analysis, the researcher has to make decisions about several parameters, tweaking any of which might change the results. These include the approach to bandwidth estimation, the polynomial functional form and the kernel function used to assign weights to the observations around the cutoff. I check if the results that I find above are robust to changes in these parameters.

[22] Literacy data from UNICEF (2015) - linkData accessed on 25 July 2018.

[23] Tables for robustness check in the case of the father being the sole beneficiary of the programme show that the results are robust in that case. Tables are available from the author on request.

First, I alter the approach to RD estimation - instead of using the CER approach (Table 4), I use an MSE based approach in table 8. In the MSE approach, the point estimates change - some of them increase and the others decrease. More importantly, they always retain their statistical significance. Next, I check the sensitivity of the results to the type of kernel function chosen. While, I use a triangular kernel for the main results (Tables 4), I re-examine the results for the main outcomes where I pair both the CER and MSE criterion results with an epanechnikov kernel (in Table 9). The point estimates and the bandwidths do change marginally for some outcomes, but they mostly retain their sign and significance (except the enrolled outcome variable). I conduct another check on the same lines where I pair the CER and MSE criterion with a linear polynomial function (power = 1), instead of a quadratic polynomial that is used in the main results (Table 4). Like the change in kernel functional form, the inferences made from the main tables still mostly remain robust.

In a different robustness check, I change the way in which I define programme initiation across treatment and control districts. While I use the archival data to identify the start of the programme in the main analysis, in Panel A of Table 11, I define the sample in the control group using the nationwide average start year of treatment districts. Under this method I assign all control districts the nationwide average start date among the treatment districts (instead of using the individual state averages). This method ignores the state (or regional) differences in implementation patterns across different districts. This change would impact the composition of children in my sample from the control districts. The results from these different assignment mechanisms (Table 11) suggest that the main results are robust to these changes. The impact on all outcomes retain the right sign, while most of them remain statistically significant as well (except English score).

For the next test, I alter the way I define the cohort of individuals who directly benefitted from the DPEP programme. In the main results, I use an age cutoff of 14 years (or below) at the time of programme implementation to identify the people who would likely have benefitted from the DPEP school construction. It is plausible that since the majority of the schools constructed under this policy were primary (and upper-primary) schools, children younger than 14 would have experienced most of the direct impact. This might especially be true for girls since they tend to drop out of schools at younger ages than boys due to a variety of reasons, chief among them being child marriage and onset of menarche. The effect of onset of menses on schooling attainment has been studied in developed (Burrows 1 and Johnson, 2005, Roberts et al., 2002, Joan and Zittel, 1998) and developing countries (Sommer, 2010). Other evidence finds that it may be the case that onset of menarche might lead to higher and earlier dropout from schools amongst girls (Adukia, 2017, Kirk and Sommer, 2006, Burgers, 2000, Fentiman et al., 1999). Therefore, I re-estimate the results with a lower age cutoff of 12 years to define the cohort that might have been impacted by the programme, that is I use a cutoff of 12 years to define the primary (and upper primary) school-going age group in India. This change would lead to the reduction in the size of the treatment group. The results from this exercise are presented in panel D of Table 11 - the RD coefficients fall in magnitude, but retain their statistical significance for most outcomes. This implies that the results mostly remain stable when using a (stricter) definition for the treatment group.

Additionally, I check how the results change when I use the global polynomial approach to RD estimation, instead of a local polynomial approach. The former uses the whole dataset to estimate the RD impact using a 2SLS IV strategy. In Tables 12 and 13, I replicate the analysis from the main results using a global polynomial approach. Since the sample and method used to estimate the coefficients is different, one would expect the point estimates with the global approach to differ from those obtained using the local polynomial approach, but the results do not differ qualitatively. The overall pattern of results does not change in cases when the mother was exposed to the DPEP programme, it had a positive impact on children's reading, math and English test scores while reducing the chances of not being able to answer any questions on these tests.

5.7 Validity Checks

In this section, I conduct several checks to demonstrate that the results that I obtain are due to the DPEP school construction policy and are not due to any other factors. First, I verify whether the setup I use detects any impacts for groups that should have been un-affected by the DPEP programme. People who were 14 years or older at the time of programme implementation would have been too old to benefit from the school expansion under the DPEP. The results in table 14 that these people did not experience any positive effects of DPEP. Further, the children of these people should also not exhibit any effects of the programme. I verify this in Table 15 - I conduct the estimation separately for children whose mothers just missed benefiting from the program and for children whose fathers just missed being exposed. I find that in both cases, there are no impacts of DPEP. The results indicate that the school construction programme had no statistically significant impact on the outcomes for the children of women who were likely to have left school or aged out of the school-going age range by the time DPEP was implemented in their districts. This further strengthens the main results.

I also use the same RD setup to estimate the impact on pre-determined or unrelated outcomes.

These are outcomes that have been determined independently of the DPEP programme, and hence the programme should have no implications for them. These include age of the mother, gender and age of the child and birth/current year rainfall shock[24]. The results from this exercise are shown in Figures 9 & 10. I plot the point estimates and their 90 percent confidence intervals that are estimated using different bandwidths and kernel functions (Triangle and Epanechnikov). The confidence interval of the point estimate of the impact of DPEP on these outcomes always consists of the zero value, showing that DPEP, did not shape outcomes unrelated to the programme.

6 Mechanisms

There are potentially multiple pathways through which a school building programme (like DPEP) could shape intergenerational learning outcomes in a developing country like India. While results that I present earlier (Table 3) show that individuals who directly benefitted from DPEP were able to increase their school attainment, we don't know what it was about this schooling that enabled them to positively impact their children's learning outcomes. I now examine several potential pathways that could have been responsible for the observed intergenerational effects. Given that female DPEP beneficiaries appear to be most able to use their education to shape their children's lives, here I focus on the women beneficiaries and the sample of children who had mothers, but not fathers, who benefitted from the program.

6.1 Educational Investments

It is possible that highly educated parents might invest more in their children's education than less educated parents. I use IHDS data in this section to examine whether DPEP's intergenerational effects in education could have been transmitted through such a channel. One way that parents can do this is to enroll their children in potentially higher quality schools, which in the Indian context could mean private schools. Evidence from India shows that even though private schools pay teachers lesser and spend less per pupil than government schools, learning outcomes in private schools are better (Desai et al., 2009, Kingdon, 2007, Muralidharan and Kremer, 2006). Singh, 2015 shows that private school attendance in India leads to large improvements on English test scores and a moderate impact on mathematics and vernacular test scores. I estimate whether DPEP programme exposure had an impact on parents' choice between private and government

[24] The data on rainfall comes from the University of Delaware dataset on precipitation and air temperature (Matsuura and Willmott, 2015). Any differences in current year rainfall around the RD cutoff can potentially be a confounder, but ideally this should not be the case. Therefore, I show this empirically to assuage any such concerns.

schools. Results in Table 17 indicate that there was no statistically significant impact on private school enrolment. Higher investment in children could also take the form of greater schooling related expenditures (example - on books, tuition etc.). DPEP beneficiaries do appear to allocate more resources towards the payment of school fees, and the purchase of books and uniforms for children. I also find that children of programme beneficiaries spent around two more hours doing homework than the children of non-beneficiaries (Table 17), which might be due to the increased supervision by their mothers. This is similar to the results found by Andrabi et al., 2012 in a similar context (Pakistan). Therefore, there is some evidence that DPEP's intergenerational impacts might have been mediated through higher parental investments in children's education.

6.2 Health of Direct Beneficiaries

Extensive research shows that health in infancy (especially birthweight) has a significant impact on later life outcomes for children (Black et al., 2007, Oreopoulos et al., 2008, Royer, 2009, Bharadwaj et al., 2010). Additionally, it is well established that mother's health (and health behaviors) are key determinants of the health and well-being of children (Ahlburg, 1998, Coneus and Spiess, 2012, Bhalotra and Rawlings, 2013, Yan, 2015). Therefore, it is plausible that DPEP's positive effect on female education had a knock-on effect on their health, and the latter led to higher well-being of children. Using data from the Annual Health Survey (2012-13) I test this hypothesis. I find that DPEP female beneficiaries indeed had better health as adults (as measured by BMI and hemoglobin - more details in Sunder, 2018b). This in line with findings from other studies that provide evidence on the positive impact of women's education on their own health (Grossman, 2015, Grépin and Bharadwaj, 2015, Agüero and Bharadwaj, 2014, Lundborg, 2013, Amin et al., 2013, Silles, 2009, Currie and Moretti, 2003). In addition, I find that DPEP had a beneficial impact on female contraceptive usage - which might reduce unwanted fertility in the high fertility context of rural India (table 16). This in turn might be an important promoter of child human capital (Kugler and Kumar, 2017), which resonates with those from other studies (Johnston et al., 2015, Andalón et al., 2014).

6.3 Child Care Investments

A child's human capital is significantly impacted by in-utero and early life conditions, or what is known as the first 1000 days of life (Almond and Currie, 2011 and Currie and Vogl, 2013 provide good reviews of this literature). Did DPEP exposure lead to higher usage of Ante Natal Care

(ANC) and Post Natal Care (PNC) by beneficiaries when pregnant? Such services would have lead to better outcomes for children as well as for the women themselves (Paudel et al., 2014, Onasoga et al., 2012, Simkhada et al., 2008, Kerber et al., 2007). In Table 16, I find that the women DPEP beneficiaries were more likely to make at least one anc visit (7-11 percent), make more ANC visits in total (0.2-0.3 visits), obtain IFA[25] (3-10 percent), deliver in a facility (6-8 percent) and make at least one PNC visit (6 percent). Based on these findings, it is clear that the women who were impacted by DPEP are more likely to receive care during and after their pregnancy, which arguably could have led to the enhanced child level human capital effects later in their lives.

6.4 Marriage Outcomes & Bargaining Power

Research shows that when women who stayed enrolled longer in schools, tended to marry at a higher age, and consequently experienced improved outcomes in adulthood such as enhanced bargaining power (Lundberg and Pollak, 1993, Field and Ambrus, 2008, Duflo, 2012, Samarakoon and Parinduri, 2015, Crandall et al., 2016, Sunder, 2018a, Yount et al., 2018). As the next set of potential mechanisms, I test whether DPEP impacted such outcomes (using IHDS data). The results in Table 16 show that women beneficiaries married about half a year later and had their first birth 0.25 years later than women in the control group. I also examine the social status of female beneficiaries in the households they married into, where I find that the DPEP women report a higher likelihood of participating in decisions related to their children and household meals (Table 17). These women are also less likely to say that physical violence (by husbands) against wives is justified. It thus seems like the women who benefitted from DPEP have higher bargaining power within the households and might be able to shape their children's outcomes more effectively than non-beneficiaries (Yoong, 2012, Bono et al., 2016).

7 Conclusion

In this paper, I use the geographic and temporal variation in implementation of a national level school building programme (DPEP) to conduct a Regression Discontinuity analysis to estimate the program's impact on intergenerational learning outcomes. The programme started in 1993, and ended in 2004. The timing of implementation varied across the 271 treatment districts, which I account for using government archival data. I first demonstrate that the programme engendered

[25]This is an especially important outcome which addresses Iron Deficiency Anemia among pregnant women - a major health concern in the context of India (see Rai et al., 2018 for a recent discussion on this)

increased access to schooling in treatment districts during the period that DPEP was in operation. I find that individuals exposed to the program (of both genders) were more likely to be literate and complete more years of education than comparable individuals in districts that did not receive the program. Further, I find that children of female DPEP beneficiaries experienced positive effects on vernacular reading, math and English test scores. In contrast, male beneficiaries were unable to transfer any benefits to their children.

I conduct a battery of robustness checks to establish the internal validity of the results of my analysis. In falsification tests I validate the results through a placebo test - I show that individuals too old to benefit from DPEP and their children show no effects of the programme. In demonstrating the potential generalizability of these findings, I note that although the estimates are based on comparing individuals in districts close to the program cut-off, the sample consists of individuals from different parts of the country. This makes the results nationally relevant. Second, there are many countries in Africa (like Ethiopia and Ivory Coast) and South Asia (Pakistan and Afghanistan) that have female literacy levels close to or lower than the RD cutoff (39.2 percent) in this study. Therefore, I argue that even though I am able to identify the Local Average Treatment Effects of DPEP, I am able to do so at a point in the female literacy distribution which approximates those prevailing in many developing countries. Therefore, the results from this analysis can possibly inform the education policies in these parts of the world.

In this analysis, I also explore the potential mechanisms through which the intergenerational impacts of the school construction could have been mediated. I find that women who were able to enhance their educational attainment through DPEP had better health (BMI) and superior health behavior in terms of contraceptive usage, pre-natal care and post-natal care as compared to non-beneficiary women. I also find DPEP's female beneficiaries married later, and had higher bargaining power in their marital households. All these factors could have enabled women to allocate greater resources towards their children's welfare. In fact, I do find that the children of these women benefitted from higher spending on school fees and uniforms/books.

Cognitive development and learning in childhood has an important bearing on later life outcomes and policy needs to focus on ways to enhance these outcomes. The bulk of the literature has focused on school based reforms to improve learning outcomes (Kremer et al., 2013, Muralidharan, 2013). Through this analysis I demonstrate that parents, particularly mothers, play an important role in shaping their children's ability to learn. Some interventions have been found to increase parental investment in children include providing parents with accurate info on returns to schooling (Bettinger and Slonim, 2007, Jensen, 2010, Levitt et al., 2011). Additionally, as the results of this analysis (and Andrabi et al., 2012, Banerji et al., 2017) show, improving the skill set of mothers could go a long way in boosting child performance on cognitive tests. Therefore, there is a need for policy action that targets parents to potentially increase educational investment in their children (Houtenville and Conway, 2008, Andrabi et al., 2015, Bergman, 2015). These reforms should complement, and not substitute, the school-based reforms aimed at improving child learning.

References

Anjali Adukia. Sanitation and education. *American Economic Journal: Applied Economics*, 9(2):23–59, 2017.

Jorge M Agüero and Prashant Bharadwaj. Do the more educated know more about health? evidence from schooling and hiv knowledge in zimbabwe. *Economic Development and Cultural Change*, 62(3):489–517, 2014.

Dennis Ahlburg. Intergenerational transmission of health. *The American Economic Review*, 88(2): 265–270, 1998.

Harold Alderman and Derek D Headey. How important is parental education for child nutrition? *World development*, 94:448–464, 2017.

Harold Alderman, Jooseop Kim, and Peter F Orazem. Design, evaluation, and sustainability of private schools for the poor: the pakistan urban and rural fellowship school experiments. *Economics of Education review*, 22(3):265–274, 2003.

Douglas Almond and Janet Currie. Killing me softly: The fetal origins hypothesis. *The Journal of Economic Perspectives*, 25(3):153–172, 2011.

Vikesh Amin, Jere R Behrman, and Tim D Spector. Does more schooling improve health outcomes and health related behaviors? evidence from uk twins. *Economics of education review*, 35:134–148, 2013.

Mabel Andalón, Jenny Williams, and Michael Grossman. Empowering women: The effect of schooling on young women's knowledge and use of contraception. Technical report, National Bureau of Economic Research, 2014.

Tahir Andrabi, Jishnu Das, and Asim Ijaz Khwaja. What did you do all day? maternal education and child outcomes. *Journal of Human Resources*, 47(4):873–912, 2012.

Tahir Andrabi, Jishnu Das, and Asim Ijaz Khwaja. *Report cards: The impact of providing school and child test scores on educational markets*. The World Bank, 2015.

ASER. Enrolment and learning report card. 2016.

Mehtabul Azam and Chan Hang Saing. Assessing the impact of district primary education program in india. *Review of Development Economics*, 21(4):1113–1131, 2017.

Sarah Baird, Craig McIntosh, and Berk Özler. Cash or condition? evidence from a cash transfer experiment. *The Quarterly journal of economics*, 126(4):1709–1753, 2011.

Abhijit V Banerjee, Shawn Cole, Esther Duflo, and Leigh Linden. Remedying education: Evidence from two randomized experiments in india. *The Quarterly Journal of Economics*, 122(3):1235–1264, 2007.

Rukmini Banerji, James Berry, and Marc Shotland. The impact of maternal literacy and participation programs: Evidence from a randomized evaluation in india. *American Economic Journal: Applied Economics*, 9(4):303–37, 2017.

Felipe Barrera-Osorio, Marianne Bertrand, Leigh L Linden, and Francisco Perez-Calle. Improving the design of conditional transfer programs: Evidence from a randomized education experiment in colombia. *American Economic Journal: Applied Economics*, 3(2):167–95, 2011.

Robert J Barro and Jong Wha Lee. A new data set of educational attainment in the world, 1950–2010. *Journal of development economics*, 104:184–198, 2013.

Jere R Behrman and Mark R Rosenzweig. Does increasing women's schooling raise the schooling of the next generation? *American Economic Review*, 92(1):323–334, 2002.

Jere R Behrman and Mark R Rosenzweig. Returns to birthweight. *Review of Economics and statistics*, 86(2):586–601, 2004.

Jere R Behrman, David Ross, and Richard Sabot. Improving quality versus increasing the quantity of schooling: Estimates of rates of return from rural pakistan. *Journal of Development Economics*, 85(1-2):94–104, 2008.

Jere R Behrman, Susan W Parker, and Petra E Todd. Schooling impacts of conditional cash transfers on young children: Evidence from mexico. *Economic development and cultural change*, 57(3): 439–477, 2009.

Julia Andrea Behrman. Does schooling affect womenâĂŹs desired fertility? evidence from malawi, uganda, and ethiopia. *Demography*, 52(3):787–809, 2015.

Peter Bergman. Parent-child information frictions and human capital investment: Evidence from a field experiment. 2015.

Eric Bettinger and Robert Slonim. Patience among children. *Journal of Public Economics*, 91(1-2): 343–363, 2007.

Sonia Bhalotra and Samantha Rawlings. Gradients of the intergenerational transmission of health in developing countries. *Review of Economics and Statistics*, 95(02):660–672, 2013.

Prashant Bharadwaj, Juan Eberhard, and Christopher Neilson. Do initial endowments matter only initially? the persistent effect of birth weight on school achievement. 2010.

Paul Bingley, Kaare Christensen, and Ian Walker. Twin-based estimates of the returns to education: evidence from the population of danish twins. *unpublished paper*, 2005.

Paul Bingley, Kaare Christensen, and Vibeke Myrup Jensen. Parental schooling and child development: Learning from twin parents. 2009.

Martina Björkman-Nyqvist. Income shocks and gender gaps in education: Evidence from uganda. *Journal of Development Economics*, 105:237–253, 2013.

Sandra E Black, Paul J Devereux, and Kjell G Salvanes. Why the apple doesn't fall far: Understanding intergenerational transmission of human capital. *American economic review*, 95(1):437–449, 2005.

Sandra E Black, Paul J Devereux, and Kjell G Salvanes. From the cradle to the labor market? the effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, 122(1):409–439, 2007.

Moussa P Blimpo. Team incentives for education in developing countries: A randomized field experiment in benin. *American Economic Journal: Applied Economics*, 6(4):90–109, 2014.

Francis Bloch, Vijayendra Rao, and Sonalde Desai. Wedding celebrations as conspicuous consumption signaling social status in rural india. *Journal of Human Resources*, 39(3):675–695, 2004.

Emilia Del Bono, Marco Francesconi, Yvonne Kelly, and Amanda Sacker. Early maternal time investment and early child outcomes. *The Economic Journal*, 126(596):F96–F135, 2016.

Dana Burde and Leigh L Linden. Bringing education to afghan girls: A randomized controlled trial of village-based schools. *American Economic Journal: Applied Economics*, 5(3):27–40, 2013.

Lizette Burgers. Background and rationale for school sanitation and hygiene education. *New York: UNICEF*, 2000.

Anne Burrows 1 and Sally Johnson. Girls' experiences of menarche and menstruation. *Journal of Reproductive and Infant Psychology*, 23(3):235–249, 2005.

Sebastian Calonico, Matias D Cattaneo, and Rocio Titiunik. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326, 2014.

Sebastian Calonico, Matias D Cattaneo, and Max H Farrell. Coverage error optimal confidence intervals for regression discontinuity designs. *working paper*, 2016.

Sebastian Calonico, Matias D Cattaneo, and Max H Farrell. On the effect of bias estimation on coverage accuracy in nonparametric inference. *Journal of the American Statistical Association*, (just-accepted), 2017.

Pedro Carneiro, Costas Meghir, and Matthias Parey. Maternal education, home environments, and the development of children and adolescents. *Journal of the European Economic Association*, 11(suppl_1):123–160, 2013.

Anne Case, Darren Lubotsky, and Christina Paxson. Economic status and health in childhood: The origins of the gradient. *American Economic Review*, 92(5):1308–1334, 2002.

Anne Case, Angela Fertig, and Christina Paxson. The lasting impact of childhood health and circumstance. *Journal of health economics*, 24(2):365–389, 2005.

Matias D Cattaneo and Gonzalo Vazquez-Bare. The choice of neighborhood in regression discontinuity designs. *Observational Studies*, 2:134Ā146, 2016.

Arnaud Chevalier, Colm Harmon, Vincent OâĂŹSullivan, and Ian Walker. The impact of parental income and education on the schooling of their children. *IZA Journal of Labor Economics*, 2(1):8, 2013.

Katja Coneus and C Katharina Spiess. The intergenerational transmission of health in early childhoodâĂŤevidence from the german socio-economic panel study. *Economics & Human Biology*, 10 (1):89–97, 2012.

AliceAnn Crandall, Kristin VanderEnde, Yuk Fai Cheong, Sylvie Dodell, and Kathryn M Yount. Women's age at first marriage and postmarital agency in egypt. *Social science research*, 57:148–160, 2016.

Janet Currie and Rosemary Hyson. Is the impact of health shocks cushioned by socioeconomic status? the case of low birthweight. *American Economic Review*, 89(2):245–250, 1999.

Janet Currie and Brigitte C Madrian. Health, health insurance and the labor market. *Handbook of labor economics*, 3:3309–3416, 1999.

Janet Currie and Enrico Moretti. Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *The Quarterly Journal of Economics*, 118(4):1495–1532, 2003.

Janet Currie and Tom Vogl. Early-life health and adult circumstance in developing countries. *Annu. Rev. Econ.*, 5(1):1–36, 2013.

Jacobus De Hoop and Furio C Rosati. Does promoting school attendance reduce child labor? evidence from burkina faso's bright project. *Economics of Education Review*, 39:78–96, 2014.

Sonalde Desai and Soumya Alva. Maternal education and child health: Is there a strong causal relationship? *Demography*, 35(1):71–81, 1998.

Sonalde Desai and Reeve Vanneman. National council of applied economic research, new delhi. 2005. *India human development survey (IHDS)*, 2005.

Sonalde Desai, Amaresh Dubey, Reeve Vanneman, Rukmini Banerji, et al. Private schooling in india: A new educational landscape. In *India Policy Forum*, volume 5, pages 1–38. National Council of Applied Economic Research, 2009.

Esther Duflo. Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment. *American economic review*, 91(4):795–813, 2001.

Esther Duflo. The medium run effects of educational expansion: Evidence from a large school construction program in indonesia. *Journal of Development Economics*, 74(1):163–197, 2004.

Esther Duflo. Women empowerment and economic development. *Journal of Economic Literature*, 50(4):1051–79, 2012.

Esther Duflo, Rema Hanna, and Stephen P Ryan. Incentives work: Getting teachers to come to school. *American Economic Review*, 102(4):1241–78, 2012.

Alicia Fentiman, Andrew Hall, and Donald Bundy. School enrolment patterns in rural ghana: a comparative study of the impact of location, gender, age and health on children's access to basic schooling. *Comparative education*, 35(3):331–349, 1999.

Erica Field and Attila Ambrus. Early marriage, age of menarche, and female schooling attainment in bangladesh. *Journal of political Economy*, 116(5):881–930, 2008.

Andrew Gelman and Guido Imbens. Why high-order polynomials should not be used in regression discontinuity designs. *Journal of Business & Economic Statistics*, (just-accepted), 2017.

Paul Glewwe. The relevance of standard estimates of rates of return to schooling for education policy: A critical assessment. *Journal of Development economics*, 51(2):267–290, 1996.

Paul Glewwe and Karthik Muralidharan. Improving education outcomes in developing countries: Evidence, knowledge gaps, and policy implications. In *Handbook of the Economics of Education*, volume 5, pages 653–743. Elsevier, 2016.

Paul Glewwe, Michael Kremer, and Sylvie Moulin. Many children left behind? textbooks and test scores in kenya. *American Economic Journal: Applied Economics*, 1(1):112–35, 2009.

Peter Glick, Christopher Handy, and David E Sahn. Schooling, marriage, and age at first birth in madagascar. *Population studies*, 69(2):219–236, 2015.

Monica J Grant. The demographic promise of expanded female education: trends in the age at first birth in malawi. *Population and development review*, 41(3):409–438, 2015.

Karen A Grépin and Prashant Bharadwaj. Maternal education and child mortality in zimbabwe. *Journal of health economics*, 44:97–117, 2015.

Michael Grossman. The relationship between health and schooling: WhatâĂŹs new? Technical report, National Bureau of Economic Research, 2015.

Pınar Mine Güneş. The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, 47:1–16, 2015.

Sudhanshu Handa. Raising primary school enrolment in developing countries: The relative importance of supply and demand. *Journal of development Economics*, 69(1):103–128, 2002.

Helena Holmlund, Mikael Lindahl, and Erik Plug. The causal effect of parents' schooling on children's schooling: A comparison of estimation methods. *Journal of Economic Literature*, 49(3): 615–51, 2011.

Andrew J Houtenville and Karen Smith Conway. Parental effort, school resources, and student achievement. *Journal of Human resources*, 43(2):437–453, 2008.

Guido Imbens and Karthik Kalyanaraman. Optimal bandwidth choice for the regression discontinuity estimator. *The Review of economic studies*, 79(3):933–959, 2012.

J Jalan and E Glinskaya. Small bang for big bucks. *An Evaluation of a Primary School Intervention in India. Centre for Training and Research in Public Finance And Policy, Calcutta*, 2013.

Robert Jensen. The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics*, 125(2):515–548, 2010.

C Chrisler Joan and PhD Carolyn B Zittel. Menarche stories: Reminiscences of college students from lithuania, malaysia, sudan, and the united states. *Health Care for Women International*, 19 (4):303–312, 1998.

David W Johnston, Grace Lordan, Michael A Shields, and Agne Suziedelyte. Education and health knowledge: Evidence from uk compulsory schooling reform. *Social Science & Medicine*, 127:92–100, 2015.

Heidi Kaila, David Sahn, and Naveen Sunder. Early life determinants of cognitive ability: A comparative study on madagascar and senegal. *Working Paper*, 2018.

Harounan Kazianga, Dan Levy, Leigh L Linden, et al. The effects of. *American Economic Journal: Applied Economics*, 5(3):41–62, 2013.

Kate J Kerber, Joseph E de Graft-Johnson, Zulfiqar A Bhutta, Pius Okong, Ann Starrs, and Joy E Lawn. Continuum of care for maternal, newborn, and child health: from slogan to service delivery. *The Lancet*, 370(9595):1358–1369, 2007.

Gaurav Khanna. Large-scale education reform in general equilibrium: Regression discontinuity evidence from india. *unpublished paper, University of Michigan*, 2015.

Elizabeth M King, Stephan Klasen, and Maria Porter. *Women and development*. Copenhagen Consensus Center, 2007.

Geeta Gandhi Kingdon. The progress of school education in india. *Oxford Review of Economic Policy*, 23(2):168–195, 2007.

Jackie Kirk and Marni Sommer. Menstruation and body awareness: linking girlsâĂŹ health with girlsâĂŹ education. *Royal Tropical Institute (KIT), Special on Gender and Health*, pages 1–22, 2006.

Zovanga Louis Kone, Maggie Y Liu, Aaditya Mattoo, Caglar Ozden, and Siddharth Sharma. Internal borders and migration in india. 2017.

Michael Kremer, Conner Brannen, and Rachel Glennerster. The challenge of education and learning in the developing world. *Science*, 340(6130):297–300, 2013.

Adriana D Kugler and Santosh Kumar. Preference for boys, family size, and educational attainment in india. *Demography*, 54(3):835–859, 2017.

Hemanshu Kumar and Rohini Somanathan. Mapping indian districts across census years, 1971-2001. *Economic and Political Weekly*, pages 69–73, 2009.

Steven D Levitt, John A List, Susanne Neckermann, and Sally Sadoff. The impact of short-term incentives on student performance. *Unpublished mimeo, University of Chicago*, 2011.

Tao Li, Li Han, Linxiu Zhang, and Scott Rozelle. Encouraging classroom peer interactions: Evidence from chinese migrant schools. *Journal of Public Economics*, 111:29–45, 2014.

Prashant Loyalka, Chengfang Liu, Yingquan Song, Hongmei Yi, Xiaoting Huang, Jianguo Wei, Linxiu Zhang, Yaojiang Shi, James Chu, and Scott Rozelle. Can information and counseling help students from poor rural areas go to high school? evidence from china. *Journal of Comparative Economics*, 41(4):1012–1025, 2013.

Shelly Lundberg and Robert A Pollak. Separate spheres bargaining and the marriage market. *Journal of political Economy*, 101(6):988–1010, 1993.

Petter Lundborg. The health returns to schoolingâĂŤwhat can we learn from twins? *Journal of population economics*, 26(2):673–701, 2013.

Francesca Marchetta and David E Sahn. The role of education and family background in marriage, childbearing, and labor market participation in senegal. *Economic Development and Cultural Change*, 64(2):369–403, 2016.

Kenji Matsuura and Cort Willmott. Terrestrial air temperature and precipitation: 1900-2014 gridded monthly time series, version 4.01. *University of Delaware.*, 2015.

Karthik Muralidharan. Priorities for primary education policy in indiaâĂŹs 12th five-year plan. In *India Policy Forum*, volume 9, pages 1–61. National Council of Applied Economic Research, 2013.

Karthik Muralidharan and Michael Kremer. Public and private schools in rural india. *Harvard University, Department of Economics, Cambridge, MA*, 2006.

Karthik Muralidharan and Nishith Prakash. Cycling to school: increasing secondary school enrollment for girls in india. *American Economic Journal: Applied Economics*, 9(3):321–50, 2017.

Karthik Muralidharan, Abhijeet Singh, and Alejandro J Ganimian. Disrupting education? experimental evidence on technology-aided instruction in india. Technical report, National Bureau of Economic Research, 2016.

Olayinka A Onasoga, Joel A Afolayan, and Bukola D Oladimeij. Factors influencing utilization of antenatal care services among pregnant women in ife central lga, osun state, nigeria. *Advances in Applied Science Research*, 3(3):1309–1315, 2012.

Philip Oreopoulos, Marianne E Page, and Ann Huff Stevens. The intergenerational effects of compulsory schooling. *Journal of Labor Economics*, 24(4):729–760, 2006.

Philip Oreopoulos, Mark Stabile, Randy Walld, and Leslie L Roos. Short-, medium-, and long-term consequences of poor infant health an analysis using siblings and twins. *Journal of human Resources*, 43(1):88–138, 2008.

Emily Oster and Rebecca Thornton. Menstruation, sanitary products, and school attendance: Evidence from a randomized evaluation. *American Economic Journal: Applied Economics*, 3(1):91–100, 2011.

Raghaw Sharan Pandey. Going to scale with education reform: India's district primary education programme 1995-1999. Technical report, Education Reform and Management Publication Series, 2000.

Dillee Prasad Paudel, Baburao Nilgar, and Manisha Bhandankar. Determinants of postnatal maternity care service utilization in rural belgaum of karnataka, india: A community based cross-sectional study. *International Journal of Medicine and Public Health*, 4(1), 2014.

Nicola Persico, Andrew Postlewaite, and Dan Silverman. The effect of adolescent experience on labor market outcomes: The case of height. *Journal of Political Economy*, 112(5):1019–1053, 2004.

Rajesh Kumar Rai, Wafaie W Fawzi, Anamitra Barik, Abhijit Chowdhury, et al. The burden of iron-deficiency anaemia among women in india: how have iron and folic acid interventions fared? *WHO South-East Asia journal of public health*, 7(1):18, 2018.

Nitya Rao. Total literacy campaigns: a field report. *Economic and Political Weekly*, pages 914–918, 1993.

Tomi-Ann Roberts, Jamie L Goldenberg, Cathleen Power, and Tom Pyszczynski. âĂIJfeminine protectionâĂİ: The effects of menstruation on attitudes towards women. *Psychology of Women Quarterly*, 26(2):131–139, 2002.

Heather Royer. Separated at girth: Us twin estimates of the effects of birth weight. *American Economic Journal: Applied Economics*, 1(1):49–85, 2009.

Bruce Sacerdote. The nature and nurture of economic outcomes. *American Economic Review*, 92(2): 344–348, 2002.

Bruce Sacerdote. How large are the effects from changes in family environment? a study of korean american adoptees. *The Quarterly Journal of Economics*, 122(1):119–157, 2007.

Shanika Samarakoon and Rasyad A Parinduri. Does education empower women? evidence from indonesia. *World Development*, 66:428–442, 2015.

Manisha Shah and Bryce Millett Steinberg. Drought of opportunities: Contemporaneous and long-term impacts of rainfall shocks on human capital. *Journal of Political Economy*, 125(2):527–561, 2017.

Mary A Silles. The causal effect of education on health: Evidence from the united kingdom. *Economics of Education review*, 28(1):122–128, 2009.

Mary A Silles. The intergenerational transmission of education: New evidence from adoptions in the usa. *Economica*, 84(336):748–778, 2017.

Bibha Simkhada, Edwin R van Teijlingen, Maureen Porter, and Padam Simkhada. Factors affecting the utilization of antenatal care in developing countries: systematic review of the literature. *Journal of advanced nursing*, 61(3):244–260, 2008.

Abhijeet Singh. Private school effects in urban and rural india: Panel estimates at primary and secondary school ages. *Journal of Development Economics*, 113:16–32, 2015.

Marni Sommer. Where the education system and women's bodies collide: The social and health impact of girls' experiences of menstruation and schooling in tanzania. *Journal of adolescence*, 33 (4):521–529, 2010.

Naveen Sunder. Marriage age, social status and intergenerational effects in uganda. Technical report, Working Paper, 2018a.

Naveen Sunder. Schooling & health : Causal estimates from a school construction programme in india. *Working Paper*, 2018b.

Petia Topalova. Trade liberalization, poverty and inequality: Evidence from indian districts. In *Globalization and poverty*, pages 291–336. University of Chicago Press, 2007.

Sebastian Vollmer, Christian Bommer, Aditi Krishna, Kenneth Harttgen, and SV Subramanian. The association of parental education with childhood undernutrition in low-and middle-income countries: comparing the role of paternal and maternal education. *International journal of epidemiology*, 46(1):312–323, 2016.

Huan Wang, James Chu, Prashant Loyalka, Xin Tao, Yaojiang Shi, Qinghe Qu, Chu Yang, and Scott Rozelle. Can school counseling reduce school dropout in developing countries? *Rural Education Action Program Working Paper No*, 275, 2014.

Ji Yan. Maternal pre-pregnancy bmi, gestational weight gain, and infant birth weight: A within-family analysis in the united states. *Economics & Human Biology*, 18:1–12, 2015.

Joanne Yoong. The impact of economic resource transfers to women versus men: a systematic review. 2012.

Kathryn M Yount, AliceAnn Crandall, and Yuk Fai Cheong. WomenâĂŹs age at first marriage and long-term economic empowerment in egypt. *World development*, 102:124–134, 2018.

APPENDIX A

Under the global approach, an RD setup can be estimated using an Instrumental Variable (IV) strategy, where the allocation rule on either side of the cutoff provides the IV. The programme assignment rule used by DPEP, that provide treatment to districts depending on whether their female literacy levels were more or less than the national average, allows me to construct this IV. In this setup, whether a district's female literacy level (in 1991) was above or below this cutoff (39.2 percent) is the instrument. This instrument is is highly predictive of whether or not a district receives the DPEP programme ($DPEP_d$). I create a categorical variable ($BelowAvg_d$) that takes a value of one if the district to which the individual belongs lies below the literacy cutoff (39.2 percent), and takes a value of zero otherwise. The first and second stage equations of this Two Stage Least Squares (2SLS) approach can be written as:

$$\begin{aligned}\text{First Stage}: \quad DPEP_d =& \alpha_1 * BelowAvg_d + \alpha_2 * BelowAvg_d * (DFLR - 39.2) \\ &+ \alpha_3 * BelowAvg_d * (DFLR - 39.2)^2 + \gamma * X_{idt} + \nu_{idt}\end{aligned}$$

$$\begin{aligned}\text{Second Stage}: \quad Y_{idt} =& \beta_1 * DPEP_d + \beta_2 * BelowAvg_d * (DFLR - 39.2) \\ &+ \beta_3 * BelowAvg_d * (DFLR - 39.2)^2 + \delta * X_{idt} + \epsilon_{idt}\end{aligned}$$

To be a valid instrument for programme participation, the categorical variable ($BelowAvg_d$) needs to satisfy two conditions. The inclusion restriction requires that the potentially endogenous independent variable of interest ($DPEP_d$) be correlated with the instrument ($BelowAvg_d$). In other words, the instrument should be a strong predictor of programme participation. This can be directly tested and I present these results later in the paper. The second condition is the exclusion restriction under which the instrument ($BelowAvg_d$) should impact the outcome only through the instrumented variable ($DPEP_d$), and not through other variables. The exclusion restriction is not directly testable, but I argue that it is likely to be satisfied in this setting. In my knowledge, there were no other government programme at that time (or since) that were allocated based on the allocation rule of DPEP. Given that there were no discontinuities in the provision of other government schemes before DPEP, it is apriori unlikely that there would be any discontinuities in outcomes around the female literacy cutoff chosen for DPEP. In addition, through some falsification tests I show that there were no discontinuities in variables that should be unaffected by the programme. Hence this instrument ($BelowAvg_d$) is unlikely to be correlated with any other covariates around this cutoff. I provide a more detailed discussion in the results section.

Table 1: **Impact of DPEP on School Construction**

	Panel A: All Schools in 1993 (DPEP Start Year)					
	Total Number of Schools			Schools per 1000 Population		
	All Schools	Government	Private	All Schools	Government	Private
RD Estimate	-143.7	-104.2	-50.77	-0.05	-0.04	-0.01
S.E. (coef)	343.9	301.6	83.2	0.28	0.49	0.03
Total Obs.	495	495	495	488	488	488

	Panel B: Schools Constructed Between 1993 & 2005 (During DPEP Years)					
	Total Number of Schools			Schools per 1000 Population		
	All Schools	Government	Private	All Schools	Government	Private
RD Estimate	413.8**	258.1**	148.5	0.31**	0.21**	0.08
S.E. (coef)	173.3	125.6	103	0.14	0.11	0.07
Total Obs.	495	495	495	488	488	488

	Panel C: All Schools in 2005 (DPEP End Year)					
	Total Number of Schools			Schools per 1000 Population		
	All Schools	Government	Private	All Schools	Government	Private
RD Estimate	270.1	153.9	107.7	0.28	0.19	0.09
S.E. (coef)	335.9	359.4	128.4	0.40	0.37	0.06
Total Obs.	495	495	495	488	488	488

Based on author's calculations using the District Information on System of Education (DISE) district level data for the year 2005. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. The standard errors are robust-bias corrected and are clustered at the district level.

LINK TO RESULTS SECTION

Table 2: **Impact of DPEP on School Quality**

	School Infrastructure											
	# Classrooms			Any Common Toilet			Any Girls Toilet			Any Electricity		
	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005
RD Estimate	5.06	3.6	6.5	0.68	-0.35	0.46	-0.90	-0.6	0.03	0.78	-0.08	0.58
S.E. (coef)	62.9	3.1	16.1	3.9	0.4	2.26	4.3	0.6	2.5	6.6	0.4	2.3
Total Obs.	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894
	Teacher Characteristics											
	# Male Teachers			# Female Teachers			# Graduate Teachers			Professional Qual.		
	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005
RD Estimate	-2.2	-0.79	-3.22	6.12	3.05	5.33	4.79	1.24	3.44	4.99	2.06	6.74
S.E. (coef)	15.1	1.22	9.2	18.8	3.1	10.1	19.2	1.9	7.5	21.1	1.84	11.2
Total Obs.	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894
	School Oversight											
	Distance - Block (kms)			Distance - Cluster (kms)			# Visits - Block			# Visits - Cluster		
	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005
RD Estimate	-20.8	-7.54	-9.22	7.62	0.15	8.04	9.09	2.62	6.15	-10.76	-1.41	-8.49
S.E. (coef)	49.5	9.4	7.9	12.2	3.08	36.6	58.6	2.37	12	22.4	3.1	27.6
Total Obs.	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894
	Grants & Incentives											
	Devt. Grant - Received ('000 Rs.)			Devt. Grant - Spent ('000 Rs.)			TLM Grant - Received ('000 Rs.)			TLM Grant - Spent ('000 Rs.)		
	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005	In 1993	1993-2004	In 2005
RD Estimate	-0.68	-0.11	0.34	-0.96	-0.11	0.23	-0.05	0.16	0.34	0.11	0.08	0.26
S.E. (coef)	8.9	0.2	0.9	8.6	0.16	0.8	1.68	0.12	0.5	1.46	0.05	0.3
Total Obs.	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894	726,494	291,280	1,017,894

Based on author's calculations using the District Information on System of Education (DISE) district level data for the year 2005. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. The standard errors are robust-bias corrected and are clustered at the district level. *TLM Grant* refers to grants received under the Total Literacy Mission. The **triangular kernel** with **local polynomial of order 2** is used to construct the point estimates. Estimates are based on author's calculations using the individual school level data from DISE (2005).

LINK TO RESULTS SECTION

Table 3: **Direct Beneficiary Impacts**

District Level Household & Facility Survey (DLHS) Round 3 (2007-08)								
	Panel A: Female Sample				Panel B: Male Sample			
	Ever School	Highest Grade	Completed Primary	Literate	Ever School	Highest Grade	Completed Primary	Literate
RD Estimate	0.10***	0.84***	0.12***	0.09***	0.09***	0.90***	0.05***	0.08***
S.E. (coef)	0.02	0.15	0.02	0.02	0.02	0.24	0.02	0.02
Total Obs.	110,543	110,517	110,517	110,212	92,098	90,759	90,759	90,756

District Level Household & Facility Survey (DLHS) Round 4 (2011-12)								
	Panel A: Female Sample				**Panel B: Male Sample**			
	Ever School	Highest Grade	Completed Primary	Literate	Ever School	Highest Grade	Completed Primary	Literate
RD Estimate	0.08***	0.75***	0.11***	-	0.08***	0.78***	0.05***	-
S.E. (coef)	0.01	0.17	0.03	-	0.02	0.21	0.01	-
Total Obs.	101,513	101,233	101,233	-	90,976	90,116	90,116	-

Note: The sample for this table consists of people who were below the age of 14 years at the time of programme implementation (from government archives data) in the treatment districts (DLHS data Rounds 3 & 4). The starting year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD impact point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector bandwidth selector. All the specifications control for the age of the individual and categorical variables for caste, religion, state and year of data collection. Standard errors are robust and clustered at the district level.

LINK TO RESULTS SECTION

Table 4: **Impact on Children when mother is sole DPEP beneficiary**- CER Optimal

	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.28***	0.21**	0.10**	0.04*	0.06***
S.E. (coef)	0.11	0.10	0.05	0.024	0.018
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	2.4	1.5	1.5	0.9	3.9
Effective Obs.	37,203	28,240	13,159	25,604	51,154
Mean (Y)	1.93	1.70	1.5	0.85	0.90
S.E. (Y)	1.44	1.14	1.1	0.20	0.15

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 5: **Impact on Children when father is sole DPEP beneficiary**- CER Optimal

	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.28	0.28	0.22	0.07	0.002
S.E. (coef)	0.22	0.27	0.16	0.07	0.04
Total Obs.	142,217	141,821	73,713	129,773	131,605
Bandwidth	6.1	4.7	6.1	4.9	5
Effective Obs.	32,672	26,172	16,680	23,662	23,936
Mean (Y)	2.39	2.16	2.08	0.84	0.92
S.E. (Y)	1.44	1.27	1.45	0.29	0.09

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their father was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The standard errors are robust and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 6: **Impact of DPEP on child outcomes - Gender Heterogeneity**

	Mother to Daughter			**Mother to Son**		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	0.29***	0.27**	0.13***	0.26***	0.23*	0.11**
S.E. (coef)	0.10	0.13	0.05	0.10	0.12	0.04
Total Obs.	230,101	229,266	119,616	253,913	252,977	133,556
	Father to Daughter			**Father to Son**		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	0.29	0.39	0.30	0.26	0.37	0.24
S.E. (coef)	0.23	0.32	0.24	0.23	0.34	0.16
Total Obs.	68,264	68,074	34,645	73,953	73,747	39,068

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their father was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 7: **Impact on Children when both parents are DPEP beneficiaries**- CER Optimal

	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.22***	0.22**	0.12**	0.03**	0.05*
S.E. (coef)	0.08	0.11	0.06	0.015	0.027
Total Obs.	137,980	137,611	71,325	125,660	127,408
Bandwidth	3.6	3.6	3.9	2.1	3.7
Effective Obs.	11,959	11,742	6,575	7,349	12,021
Mean (Y)	1.98	1.78	1.55	0.88	0.91
S.E. (Y)	1.46	1.16	1.12	0.21	0.14

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that both their mother and father were below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The standard errors are robust and clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 8: **Impact on Children when mother is sole DPEP Beneficiary** - MSE Optimal

	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.22***	0.19**	0.11**	0.05**	0.08***
S.E. (coef)	0.08	0.10	0.05	0.025	0.022
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	5.4	3.3	2.8	8.9	7.5
Effective Obs.	73,180	46,664	21,406	102,179	80,078
Mean (Y)	1.97	1.70	1.53	0.84	0.9
S.E. (Y)	1.44	1.16	1.12	0.22	0.15

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common MSE-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The standard errors are robust-bias corrected and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 9: **Robustness Check - Epanechnikov Kernel**

CER Optimal	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.29***	0.22**	0.10	0.04*	0.04
S.E. (coef)	0.11	0.10	0.08	0.023	0.04
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	1.5	2.5	2.6	1.1	4.6
Effective Obs.	13,791	25,660	32,370	28,203	26,469
MSE Optimal	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.23***	0.24*	0.10**	0.05*	0.07
S.E. (coef)	0.09	0.14	0.05	0.028	0.08
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	1.6	3.6	3.6	8.4	6.1
Effective Obs.	26,981	56,560	56,381	94,230	139,999

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the epanechnikov kernel, local polynomial of order 2 and with one common CER-optimal & MSE-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The standard errors are robust-bias corrected and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 10: **Robustness Check - Linear Polynomial**

CER Optimal	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.28***	0.27**	0.08*	0.06**	0.09
S.E. (coef)	0.12	0.13	0.102	0.04	0.06
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	0.92	0.96	3.1	0.92	0.98
Effective Obs.	7,140	19,780	25,603	27,203	14,756
MSE Optimal	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.33***	0.29**	0.09*	0.04***	0.06
S.E. (coef)	0.15	0.14	0.05	0.01	0.05
Total Obs.	488,862	487,037	253,172	472,338	526,087
Bandwidth	1.5	1.8	2.7	8.2	1.84
Effective Obs.	14,642	32,451	37,669	91,661	30,274

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 1 and with one common CER-optimal & MSE-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The standard errors are robust-bias corrected and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 11: **Robustness Checks - Different Start Years - CER RD**

	Panel A: National Avg. Start			Panel B: Control = Age Cutoff = 12 yrs		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	0.26***	0.20***	0.12	0.25***	0.26***	0.11***
S.E. (coef)	0.09	0.07	0.23	0.09	0.09	0.04
Total Obs.	486,264	484,428	247,489	309,775	310,074	164,351

Note: The score variables run from 0-4, whereas the other outcomes are categorical variables. The sample for this table consists of children born in or after the year 2000 to parents who were both below the age of 14 years at the time of programme implementation (from government archives data) in the treatment districts (ASER data, 2007-2014). The starting year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD impact point estimates are constructed with **triangular kernel**, **local polynomial of order 2** and with **one common CER-optimal bandwidth selector** bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The bandwidth is expressed in terms of percentage of the running variable (district female literacy rate in 1991). The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The mean and standard deviation of the dependent variable within the bandwidth of that particular outcome are included in the table.

LINK TO RESULTS SECTION

Table 12: **Robustness Check - Full Sample Regression - Exact Timing**

	Read Score	Math Score	English Score	GFA	Enrolled
RD Estimate	0.19***	0.10***	0.09***	0.024***	0.002
S.E. (coef)	0.06	0.035	0.03	0.004	0.002
Total Obs.	488,862	487,037	253,172	472,338	526,087

Note: The sample consists of children born in or after 2000 to mothers who were below 14 years of age at the time of implementation of the DPEP programme in their district. The score variables run from 0-4, whereas the other outcomes are categorical variables. GFA refers to a Grade-for-Age measure. This programme implementation timing is derived from detailed government archives that describe the exact process of programme implementation. The corresponding population in the control districts is identified on the basis of the average start date in treatment districts within the same state. All specifications control for a quadratic polynomial of the running variable, the child's age, ages of both parent, rainfall shocks in-utero/birth year of the child, and dummies for caste, religion, state and year of data collection. The standard errors are robust and are clustered at the district level.

LINK TO RESULTS SECTION

Table 13: **Robustness Check - Full Sample Regression - Different Start Years**

	Panel A: Control = National Avg. Start			Panel B: Age Cutoff = 12 yrs		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	0.26***	0.20***	0.14**	0.23***	0.16***	0.14***
S.E. (coef)	0.07	0.06	0.06	0.09	0.06	0.04
Total Obs.	486,264	484,428	247,489	309,775	310,074	164,351

Note: The score variables run from 0-4. The sample consists of children born in or after 2000 to mothers who were below 14 years of age in a particular year. This year is the estimated start date of the programme using the DISE dataset - the year with the maximum year on year rate of growth of schools in a particular district after the implementation of the DPEP programme. All specifications control for a quadratic polynomial of the running variable, the child's age, ages of both parent, rainfall shocks in-utero/birth year of the child, and dummies for caste, religion, state and year of data collection. Standard errors are robust and clustered at district level.

LINK TO RESULTS SECTION

Table 14: **Falsification - Direct Beneficiary Impacts**

	District Level Household & Facility Survey (DLHS) Round 3 (2007-08)							
	Panel A: Female Sample				Panel B: Male Sample			
	Ever School	Highest Grade	Completed Primary	Literate	Ever School	Highest Grade	Completed Primary	Literate
RD Estimate	-0.08	-0.54	-0.08	-0.02	-0.07	-0.34	-0.06	-0.01
S.E. (coef)	0.06	0.48	0.05	0.02	0.06	0.26	0.04	0.02
Total Obs.	271,978	271,940	271,940	269,320	189,231	188,764	188,764	188,223

	District Level Household & Facility Survey (DLHS) Round 4 (2011-12)							
	Panel A: Female Sample				**Panel B: Male Sample**			
	Ever School	Highest Grade	Completed Primary	Literate	Ever School	Highest Grade	Completed Primary	Literate
RD Estimate	0.15	0.35	0.11	-	0.11	0.38	0.09	-
S.E. (coef)	0.12	0.22	0.08	-	0.09	0.27	0.06	-
Total Obs.	116,593	113,760	113,760	-	101,982	101,124	101,124	-

Note: The score variables run from 0-4. The sample for this table consists of people who were below the age of 14 years at the time of programme implementation (from government archives data) in the treatment districts (DLHS data Rounds 3 & 4). The starting year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD impact point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector bandwidth selector. All the specifications control for the age of the individual and categorical variables for caste, religion, state and year of data collection. Standard errors are robust and clustered at the district level.

LINK TO RESULTS SECTION

Table 15: **Falsification Check - DPEP impact on children of non-beneficiaries**

	Panel A: Non-Beneficiary Mother- CER			Panel B: Non-Beneficiary Mother - MSE		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	-0.14	-0.07	-0.13	-0.09	-0.04	-0.24
S.E. (coef)	0.47	0.46	0.95	0.37	0.49	0.88
Total Obs.	737,551	733,786	382,316	737,551	733,786	382,316
Bandwidth	7.5	6.1	7.6	10.2	8.9	10.3
Effective Obs.	161,474	131,288	81,412	266,237	183,369	131,179

	Panel C: Non-Beneficiary Father - CER			Panel D: Non-Beneficiary Father - MSE		
	Read Score	Math Score	English Score	Read Score	Math Score	English Score
RD Estimate	-0.11	-0.16	-0.29	-0.13	-0.29	-0.22
S.E. (coef)	0.47	1.75	0.32	0.12	1.17	2.2
Total Obs.	584,862	582,528	266,994	584,862	582,528	266,994
Bandwidth	7.8	5.3	5.1	7.7	7.5	7.2
Effective Obs.	154,024	99.949	45,407	148,117	147,485	65,504

Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother (or father) was above the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal & MSE-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The score variables run from 0-4, whereas the other outcomes are categorical variables. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated.

LINK TO RESULTS SECTION

Table 16: **Potential Mechanisms - Woman (Parent) Level**

	Marriage Age		Age at First Birth		Contraceptive Use		Any ANC	
	DLHS-3	DLHS-4	DLHS-3	DLHS-4	DLHS-3	DLHS-4	DLHS-3	DLHS-4
RD Estimate	7.53***	6.62***	3.72***	3.01***	0.06***	0.07***	0.07***	0.11***
S.E. (coef)	0.52	0.68	0.42	0.6	0.02	0.03	0.02	0.03
Total Obs.	110,564	89,773	73,628	72,712	72,775	55,033	64,276	47,993

	# ANC Visits		IFA Taken		Delivery - Formal		Any PNC	
	DLHS-3	DLHS-4	DLHS-3	DLHS-4	DLHS-3	DLHS-4	DLHS-3	DLHS-4
RD Estimate	0.22**	0.31	0.032**	0.098**	0.063***	0.083***	0.058**	-
S.E. (coef)	0.12	0.03	0.014	0.038	0.021	0.027	0.024	-
Total Obs.	48,482	39,497	41,228	43,041	41,212	47,560	64,274	-

Source: Based on authors calculations using the District Level Household and Facility Survey (DLHS) data from Rounds 3 (2007-08) and Round 4 (2011-12). The sample consists of women who were below 14 years of age at the time of implementation of the DPEP programme in their district. This programme implementation timing is derived from detailed government archives that describe the exact process of programme implementation. The corresponding population in the control districts is identified on the basis of the average start date in treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the same set of variables as the main specifications. The standard errors are robust-bias corrected and are clustered at the district level. Marriage age refers to the age at marriage (in months), Age at first birth refers to age when the woman had her first child (In months), Contraceptive use is a dummy that takes a value of one if the women reported using contraceptives, *Any ANC* is a categorical variable that takes a value of one if the woman accessed any ANC facilities during the last pregnancy, # ANC visits refers to the number of ANC visits made during the last pregnancy, *IFA Taken* is a categorical variable that takes a value of one if the woman reported taking IFA tablets during the last pregnancy, *Delivery-Formal* is a categorical that takes a value of one if the woman reported giving birth in a formal health facility and *Any PNC* refers to a dummy that takes a value of one if the woman reported using any Post Natal Care (PNC) facilities.

LINK TO MECHANISMS SECTION

Table 17: **Potential Mechanisms - Woman (Parent) & Child level**

	Decision-Child	Decision-Cook	Decision-Purchases	Beat-Bad Cook	Beat-Neglect House
RD Estimate	0.08***	0.14***	0.08***	-0.09***	-0.05***
S.E. (coef)	0.02	0.053	0.03	0.02	0.01
Total Obs.	13,159	13,159	13,159	13,114	13,114
	School Fees	**Uniform/Books**	**Tuition Fees**	**Private School**	**Homework Hours**
RD Estimate	781.8*	706.8**	-15.6	0.05	2.06*
S.E. (coef)	426.3	328.6	312.8	0.05	1.16
Total Obs.	4,545	4,545	4,545	4,545	4,545

Source: Based on authors calculations using the Indian Human Development Survey (IHDS) 2005 round. The sample consists of women who were below 14 years of age at the time of implementation of the DPEP programme in their district. This programme implementation timing is derived from detailed government archives that describe the exact process of programme implementation. The corresponding population in the control districts is identified on the basis of the average start date in treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the same set of variables as the main specifications. The standard errors are robust-bias corrected and are clustered at the district level. Decision-Cook and Decision-Purchases are dummy variables that take a value of one if the woman reported being involved in decisions related to cooking and purchases made in the household. *Beat-Bad Cook* and *Beat-Neglect House* are categorical variables that take a value of one if woman reported that it was common for women in their community to be beaten up in cases when she was a bad cook or neglected household work respectively. School Fees, Uniform/Books and Tuition fees refer to variables that measure the expenditure on these categories made on the woman's children. Private School is a dummy that takes a value of one if the child goes to private school, and homework hours refers to the amount of time the child spent doing homework.

LINK TO MECHANISMS SECTION

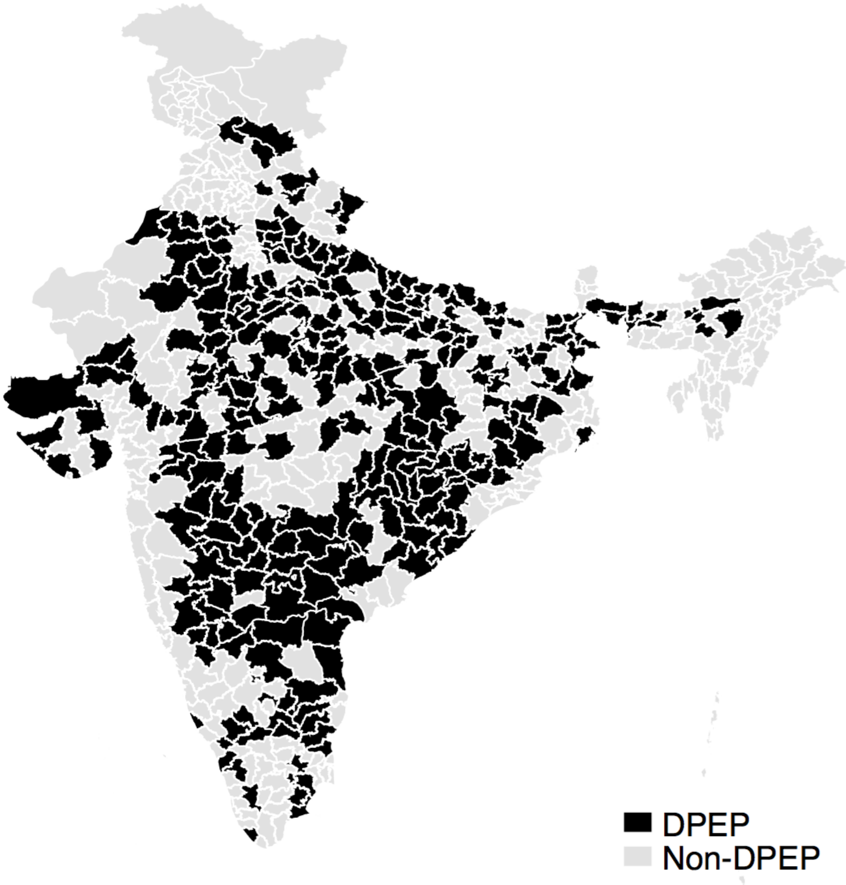

Figure 1: Sample of districts that received the DPEP programme.
LINK TO BACKGROUND SECTION

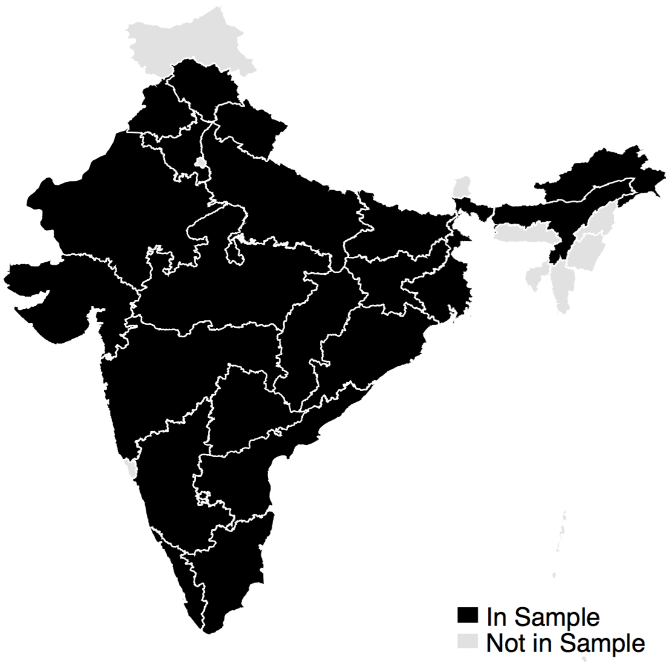

Figure 2: States included in my sample (ASER DATA).
LINK TO DATA SECTION

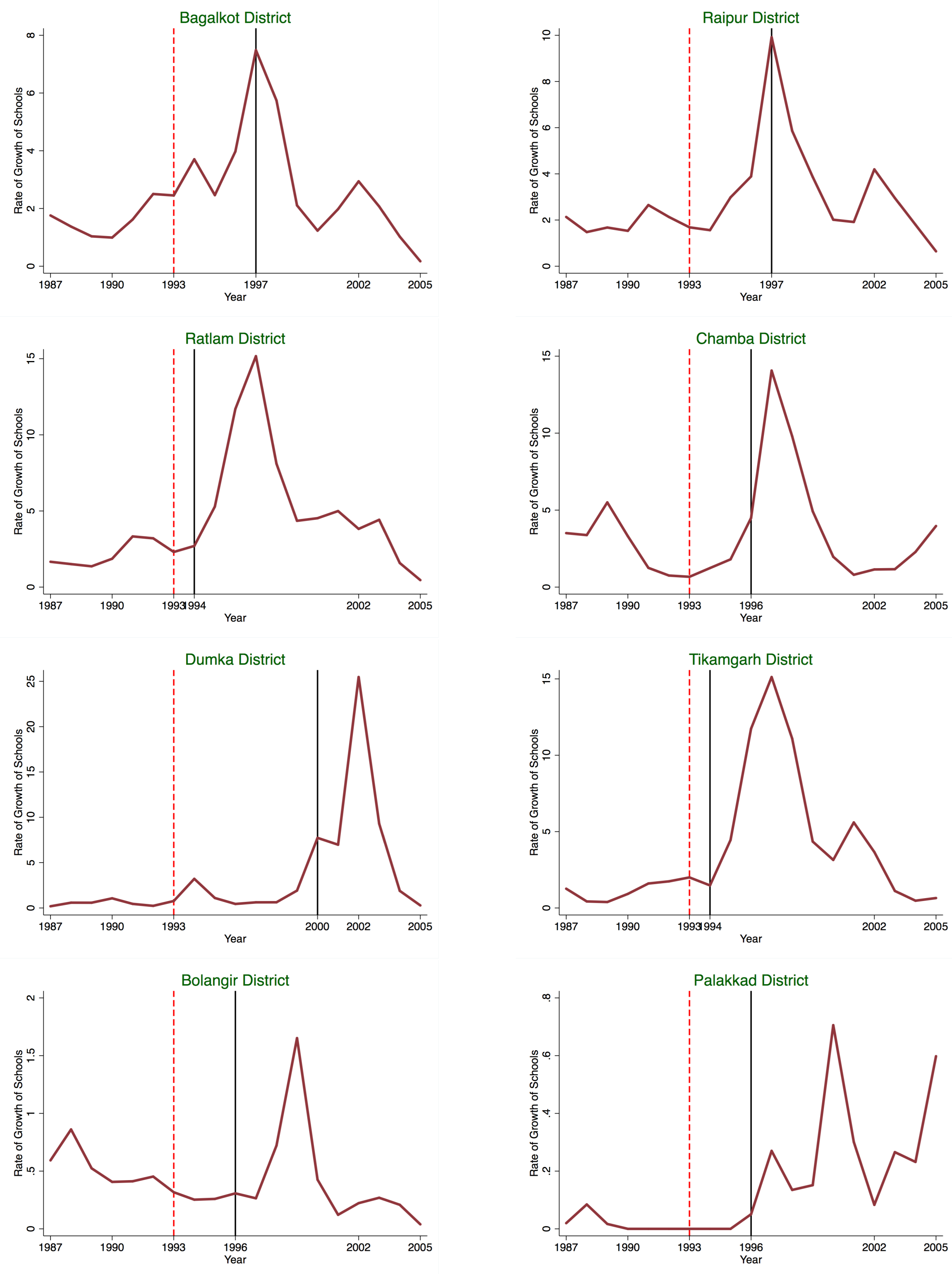

Figure 3: **Yearly rate of growth of school construction plotted against time - Treatment Districts** The graphs in this figure illustrate that the peak in school construction growth in treatment districts is better *predicted* by the year of programme implementation that I infer using the government archival data, rather than the uniform start year of 1993-94. Data : DISE 2005.

LINK TO RESULTS SECTION
LINK TO EMPIRICAL STRATEGY SECTION

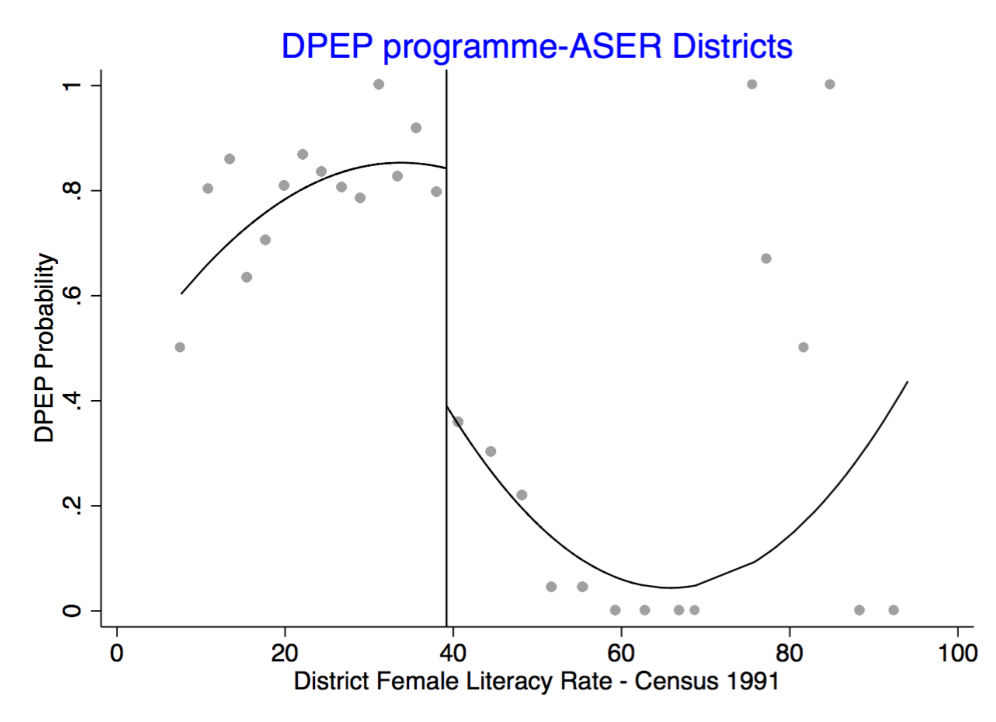

Figure 4: Probability of Receiving DPEP Programme. The graph shows the discontinuity of treatment assignment at the cutoff of 39.2 percent in terms of District Female Literacy Rate. Data Source : ASER data combined with information in government archives. LINK TO RESULTS SECTION LINK TO EMPIRICAL STRATEGY SECTION

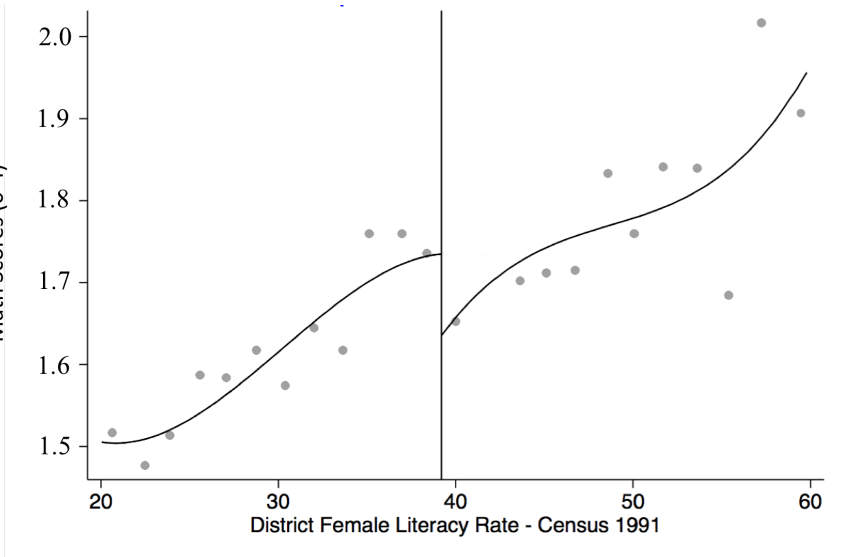

Figure 5: Impact on Mathematics test scores. This is a graphical representation of the estimate presented in table 4. Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. LINK TO RESULTS SECTION

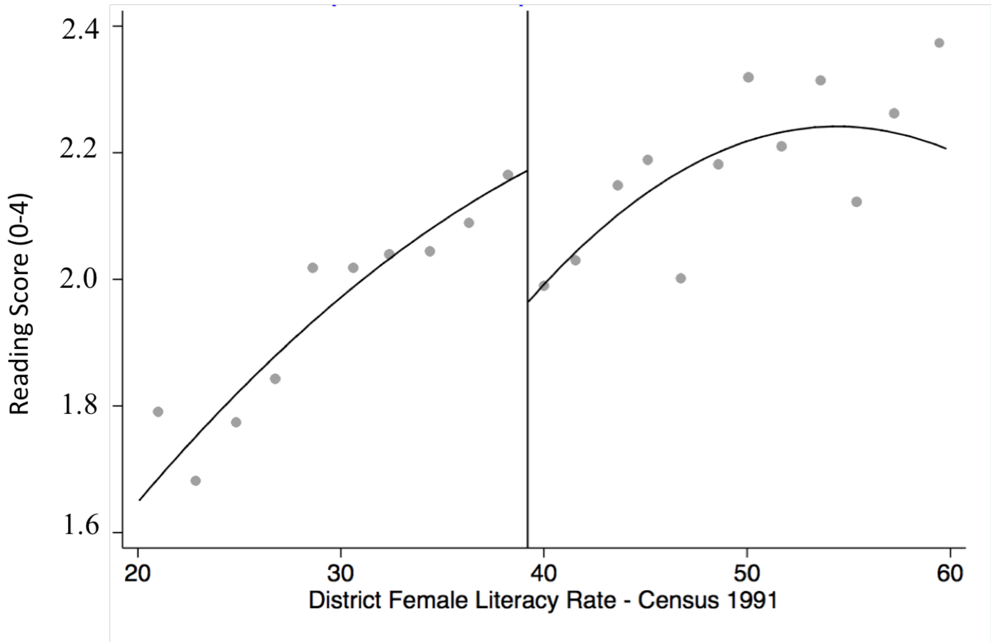

Figure 6: Impact on Reading test scores. This is a graphical representation of the estimate presented in table 4. Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. LINK TO RESULTS SECTION

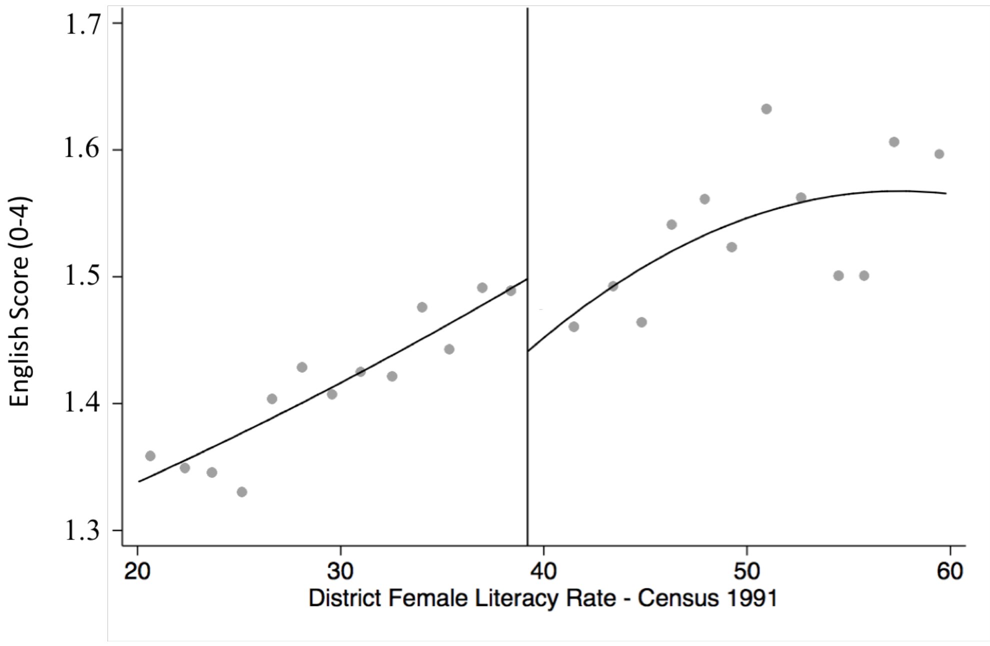

Figure 7: Impact on English test scores. This is a graphical representation of the estimate presented in table 4. Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the age of the child, ages of both parents, rainfall shocks in-utero/birth year of the child and dummy variables for state and year of data collection. The standard errors are robust and are clustered at the district level. LINK TO RESULTS SECTION

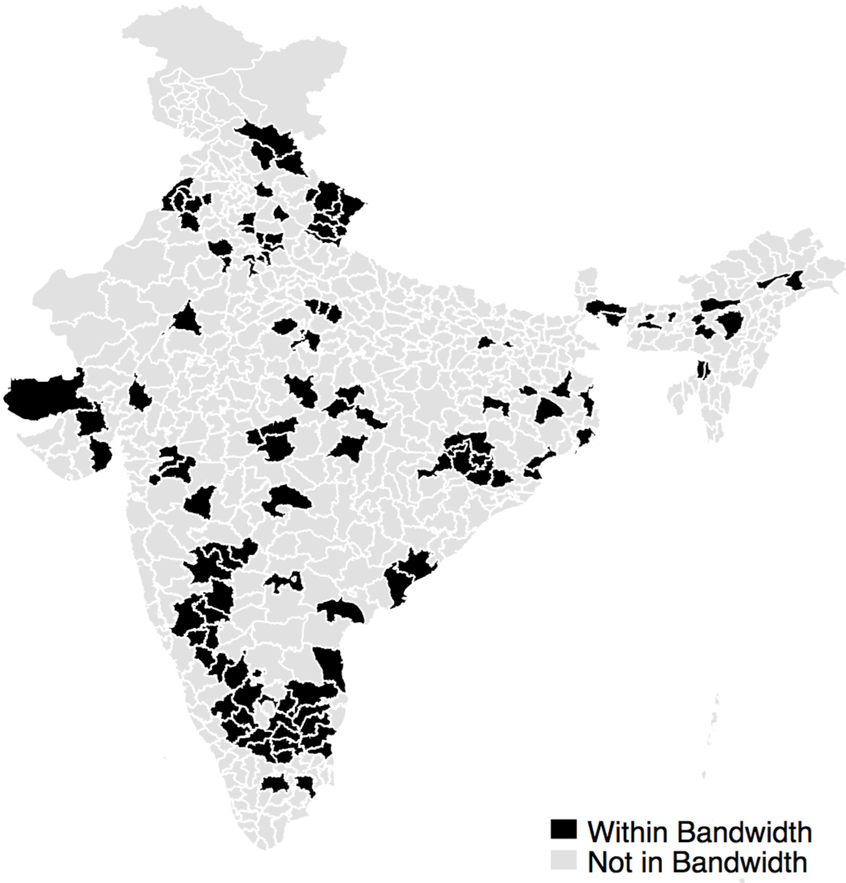

Figure 8: Figure shows the districts that are within the bandwidth for the estimation of the RD impact on reading scores using the following empirical setup: Based on author's calculations using the Annual Survey of Education Report (ASER) individual level data for the years 2007-2014. The sample consists of children satisfying two criterion - likely started school after the year 2005 (DPEP end year) and that their mother was below the age of 14 years at the time of DPEP implementation (start year from government archives data). The start year of the programme for control districts is the state average starting year of treatment districts within the same state. The RD point estimates are constructed using the triangular kernel, local polynomial of order 2 and with one common CER-optimal bandwidth selector. All specifications control for the child's age, age of both parents, rainfall shocks in-utero/birth year of the child, state dummies and year dummies. The standard errors are robust-bias corrected and are clustered at the district level. The bandwidth is expressed in terms of the running variable - district female literacy rate in 1991. The effective number of observations indicates the number of observations that lie within the bandwidths indicated in the table - these are different from the the full sample sizes which are also indicated in the table. The score variables run from 0-4, whereas the other outcomes are categorical variables. The mean and standard deviation of the dependent variable within the bandwidth for each outcome is indicated. LINK TO RESULTS SECTION

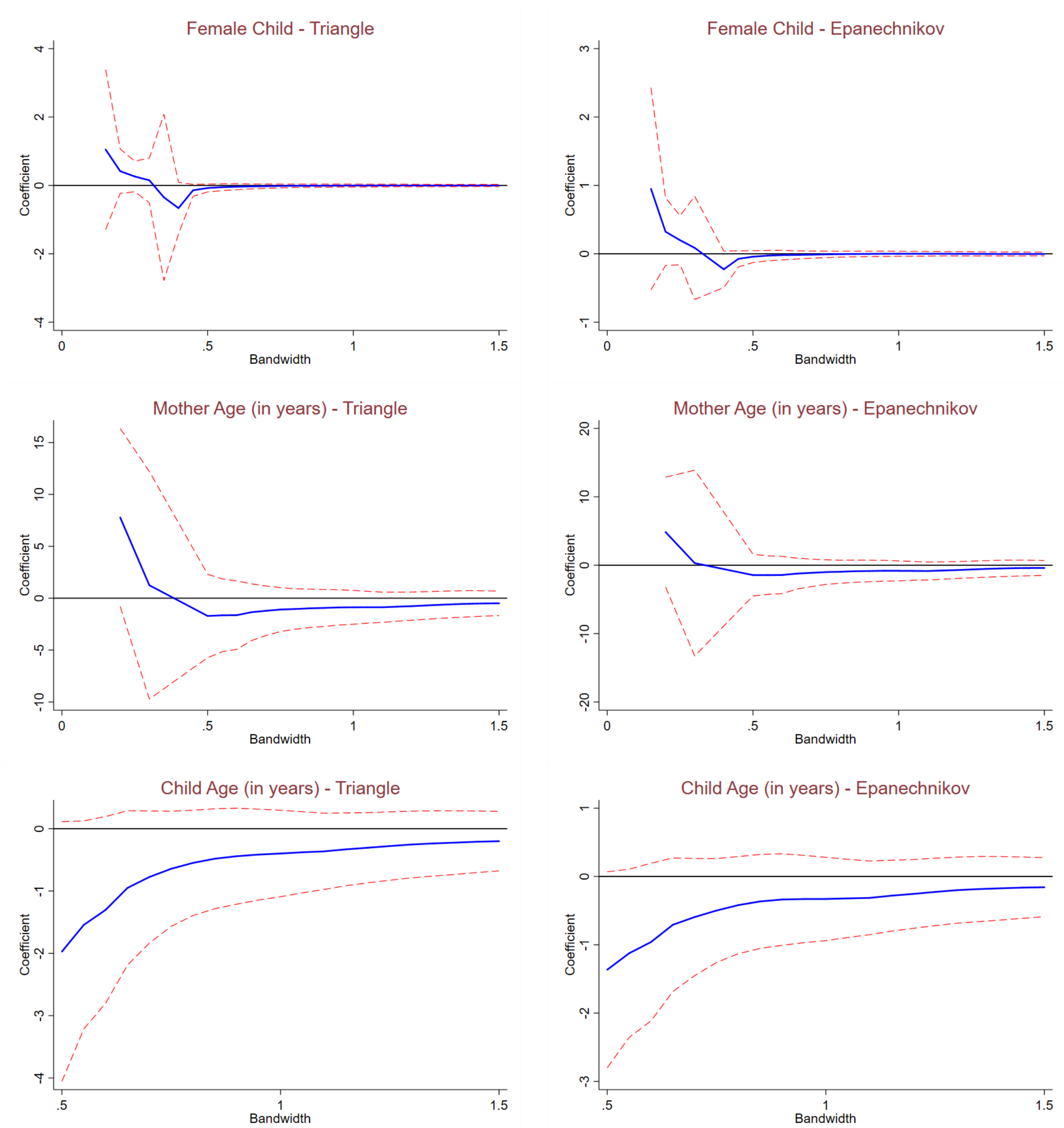

Figure 9: **Discontinuity in Pre-Determined Outcomes** These graphs show that the DPEP programme had a statistically insignificant (i.e. indistinguishable from zero) impact on these outcomes. The staring year for the treatment districts comes from the government archives, while that for the control districts comes from the average starting year of treatment districts within the same state. The left panel shows graphs of the RD impact estimate for an outcome using **triangular** kernel with a **polynomial of degree 2**. The right panel does the same with an epanechnikov kernel. In both graphs the coefficients are estimated at different bandwidths, where bandwidths are increased in steps of 0.05. All estimates use **robust-bias standard errors** with **clustering at the district level**. LINK TO RESULTS SECTION

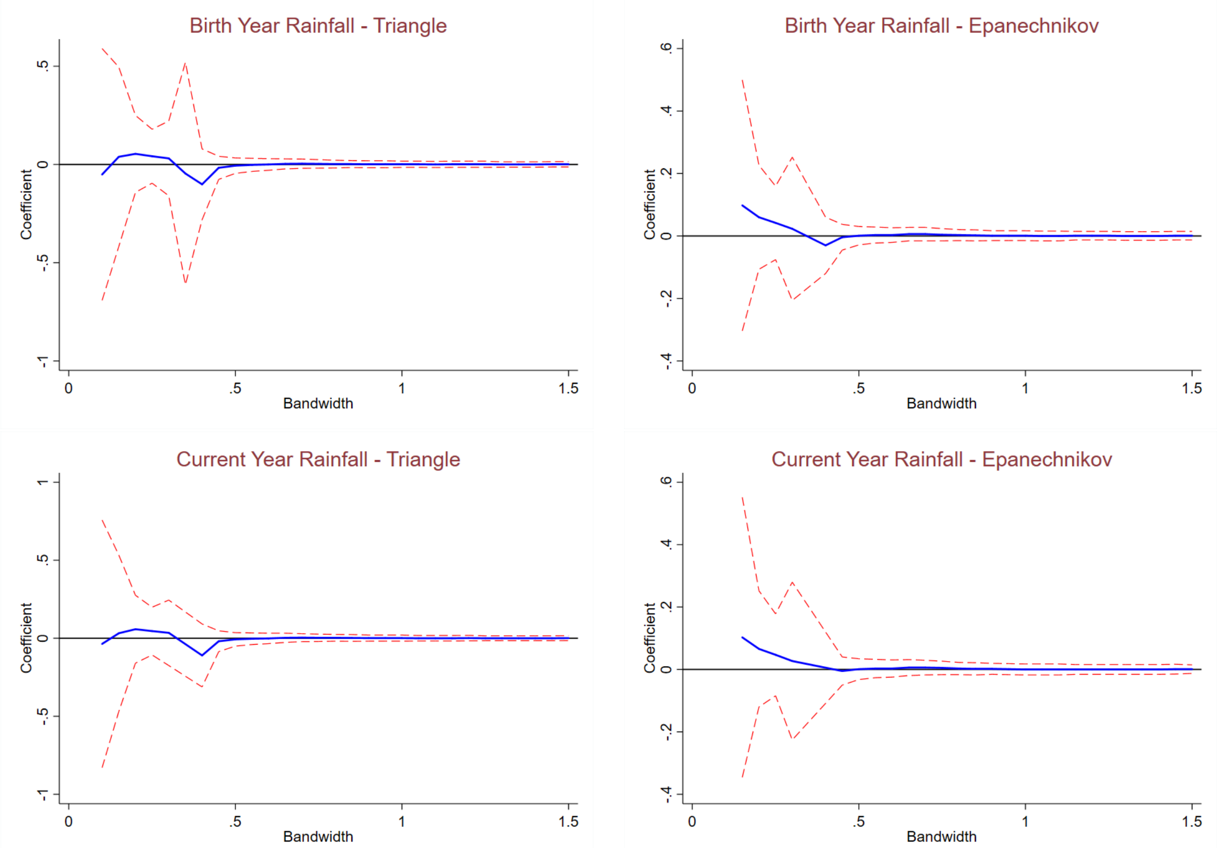

Figure 10: **Discontinuity in Pre-Determined Outcomes** These graphs show that the DPEP programme had a statistically insignificant (i.e. indistinguishable from zero) impact on these outcomes. The staring year for the treatment districts comes from the government archives, while that for the control districts comes from the average starting year of treatment districts within the same state. The left panel shows graphs of the RD impact estimate for an outcome using **triangular** kernel with a **polynomial of degree 2**. The right panel does the same with an epanechnikov kernel. In both graphs the coefficients are estimated at different bandwidths, where bandwidths are increased in steps of 0.05. All estimates use **robust-bias standard errors** with **clustering at the district level**. LINK TO RESULTS SECTION

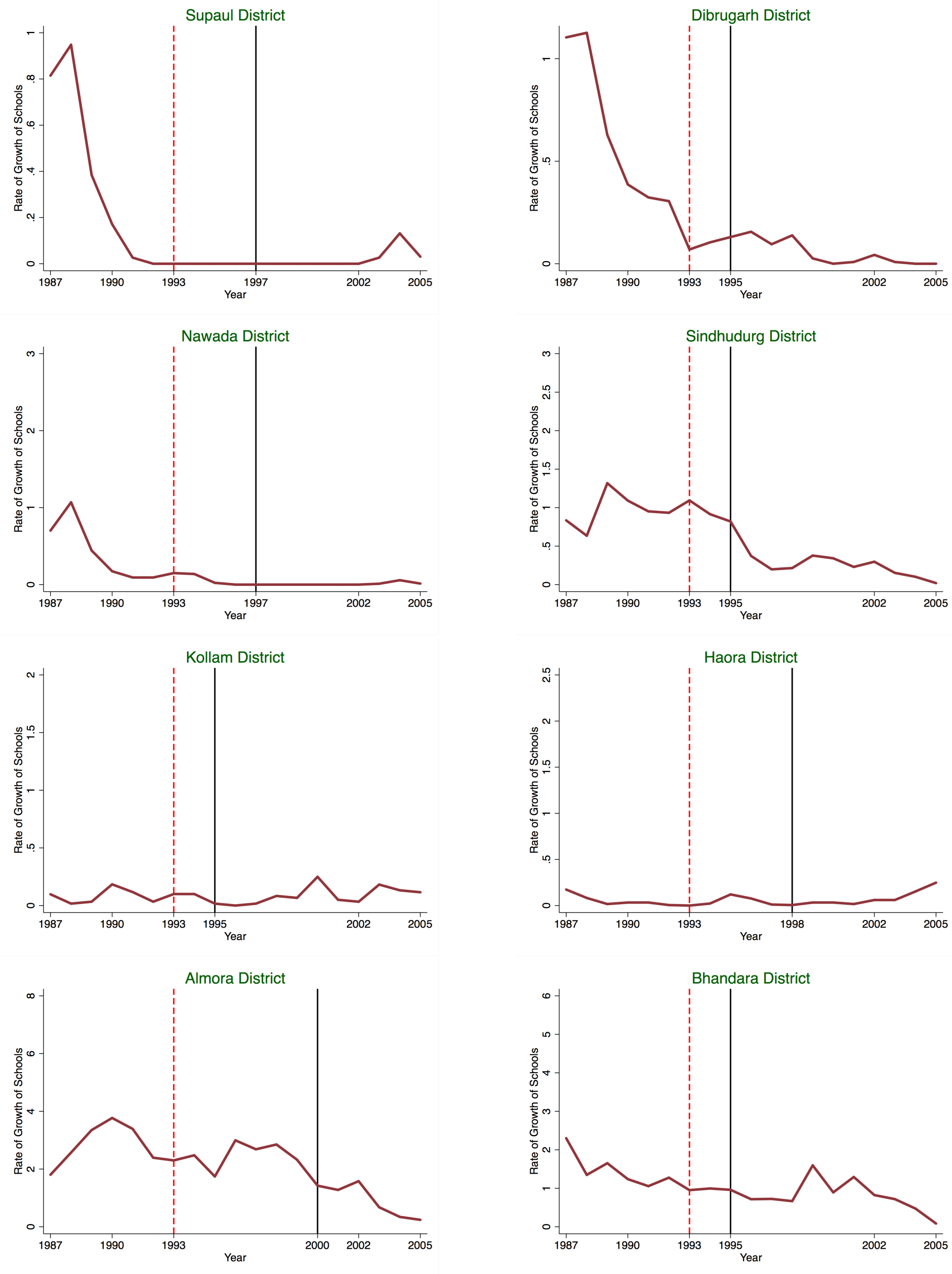

Figure 11: **Yearly rate of growth of school construction plotted against time - Control Districts** The graphs in this figure illustrate that there was no upward trend in school construction in the control districts around the time the DPEP programme was implemented. Data : DISE 2005. LINK TO RESULTS SECTION

Table 18: **Intergenerational Effects**

	Mother Beneficiary		
	Read Score	Math Score	English Score
RD Estimate	0.28***	0.21***	0.10***
Bandwidth	2.4	1.5	1.5
Effective Obs.	37,203	28,240	13,159
Mean (Y)	1.93	1.70	1.5
	Father Beneficiary		
	Read Score	Math Score	English Score
RD Estimate	0.28	0.28	0.22
Bandwidth	6.1	4.7	6.1
Effective Obs.	32,672	26,172	16,680
Mean (Y)	2.39	2.16	2.08