Large-scale Education Reform in General Equilibrium: Regression Discontinuity Evidence from India

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Abstract

The economic consequences of large-scale government investments in education depend on the general equilibrium (GE) effects in both the labor market and the education sector. I develop a general equilibrium model and derive sufficient statistics that capture the consequences of such massive countrywide schooling initiatives. I provide unbiased estimates of the sufficient statistics using a Regression Discontinuity design generated by Indian government policy. The earnings returns to a year of education are 13.4%, and the general equilibrium labor market effects are substantial: they depress the returns to skill by 6.5 percentage points. These GE effects have distributional consequences across cohorts and skill groups, where as a result of the policy, unskilled workers are better off and skilled workers are worse off. In the education sector, more private schools enter these markets negating concerns of crowd-out.

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Large-scale educational expansions represent substantial investments of public resources and benefit households by increasing productivity in the local economy. However, since they impact both individual behavior and labor market outcomes, convincing causal estimates of their overall economic benefits are hard to generate. While small-scale, carefully controlled, researcher-led experiments provide promising evidence about which educational investments are effective, for a variety of reasons these estimates may not be valid for large scale policies. Importantly, large-scale education programs may have sizable general equilibrium (GE) effects in the education sector and the labor market that may undermine the effectiveness of the intervention. I causally estimate and take into account these GE effects in determining the overall economic consequences and benefits of nationwide education programs.

I build a new framework to analyze the consequences of a large-scale educational expansion program in India with an explicit focus on issues inherent to nationwide government policies: the persistence of effects and the consequences of lost funding, and GE effects in the markets for both education and labor. I model the labor market and education sector and decompose wage changes into the individual returns to education and the GE effects. The allocation rule under which Indian districts receive the funding allows me to estimate the sufficient statistics generated by the model using a Regression Discontinuity (RD) approach. Further, I exploit variation in cohort exposure to the program and skill levels to identify the GE effects, by estimating how the earnings skill-premium changes across local economies. I use the estimated parameters to comprehensively measure the overall benefits of the policy and its distributional consequences across skill levels and age cohorts. Not only do I find substantial GE effects in the labor market, but I am also able to precisely estimate their size—these effects depress the returns to skill by 32% and dampen the increase in labor market benefits by 23%. By expanding the skilled workforce, the policy makes skilled workers worse off and unskilled workers better off, and leads to the adoption of skill-biased capital. At the same time, the GE effects in the education sector suggest a crowd-in of private schools, negating concerns of crowd-out.

From a novel model of households, public schools, private schools, and firms, I derive sufficient statistics that measure the effect of the program on welfare. In the education sector, I model the entry and exit decisions of private schools, the spending decisions of public schools, and household decisions to attend school. On the labor market side, I combine models of education choice (Becker, 1967; Mincer, 1958; Willis, 1986) with frameworks that determine the skill-premium (Card and Lemieux, 2001; Katz and Murphy, 1992) to study how the distribution of earnings affects education choices, and consequently how changes in education choices affect the distribution of earnings.

The returns to education and the change in the returns due to the GE effects are among the model's important sufficient statistics. While a well implemented policy can effectively increase the supply of schooling, equilibrium schooling may not change much if the returns to education are low (Jensen, 2010, 2012). If education levels rise, we expect earnings and therefore the returns to be affected in a few ways. First, a more educated worker is more productive and will earn a higher wage. Second, a more educated worker may reside in a region where there are fewer educated workers, making her relatively more valuable in the labor market. But, if large numbers of people receive additional education, there is also a GE effect in the labor market: an increase in the abundance of high-skill labor puts downward pressure on the earning skill premium. At the same time, as more skilled workers join the labor force, skill-biased capital may be adopted by firms in these regions, raising the premium. Last, as workers switch to more productive skill groups, overall output may increase to the benefit of all workers. I, therefore, estimate all components of the GE effects to better quantify the distributional impacts and the overall increase in labor market benefits.

The policy I study was India's flagship scheme in the 1990s and early 2000s, the District Primary Education Program (DPEP), which expanded public schooling in half the country by targeting low-literacy regions. At that time it was the largest program for primary education in the world, in terms of geography, population and funding, suggesting that its effects would be similarly broad (Jalan and Glinskaya, 2013). The policy primarily built schools, hired teachers and upgraded infrastructure in low-literacy districts. Such schooling expansions reduce the marginal cost of attaining education by improving access to schools (Behrman et al., 1996; Birdsall, 1985). This would induce some students who have potentially high returns to schooling but could not previously afford it to get more education. Duflo (2001) shows that a similar program in Indonesia increased education levels and earnings for eligible cohorts. Similarly, I examine not only the educational outcomes, but also earnings for different cohorts and skill groups long after their exposure to the policy.

Under the allocation rule, districts that had a female literacy rate below the national average were more likely to receive the program. I compare regions on either side of the literacy-rate cutoff to determine the causal impact of the policy. The RD design allows me to tackle biases that may arise when estimating the individual returns to education, and when comparing earnings in two different local economies. I compare students who were induced into getting more education to similarly competent students that were not. Furthermore, some regions may have a large number of skilled workers or industries that require skilled work, and are therefore not comparable to other regions. At the regional level, therefore, the RD tackles biases that arise due to differences in the local economy and labor market.

To support each piece of the general equilibrium model, I create a comprehensive dataset by combining three waves of a household survey, a census of firms, school-level data, test score surveys and the Indian Census. I use the data to estimate the returns to education and the GE effects, exploiting not just the RD, but also the variation in cohort exposure and skill levels. Younger cohorts can change their educational attainment in response to the policy, whereas older cohorts cannot. Both the young and the old are, however, affected by changes in the labor market skill distribution. Using the estimated parameters, I measure the overall impact of the policy on household welfare for the different types of workers and cohorts.[1]

Given evidence from other contexts, it is crucial for researchers to address these labor market GE effects. In the US, Abbott et al. (2013); Heckman et al. (1998a,b); Lee (2005) show how changes in taxes or tuition and financial aid may have large general equilibrium effects. In developing countries as well, an increase in the size of the skilled workforce have been found to depress wages for high-skill workers (Angrist, 1995; Duflo, 2004).[2] I both flexibly model and causally estimate the GE effects on different cohorts and on different skill groups, allowing me to determine distributional consequences across both dimensions, estimate crucial economic parameters, and the returns to schooling both in the presence and the absence of GE effects. I estimate the earnings skill-premium by age group separately on either side of the RD cutoff. The difference in the earnings skill-premium for older workers allows me to measure the GE effects that affect all cohorts. At the same time, since the young and old are not perfect substitutes, there is an often ignored additional impact on younger workers which I estimate by looking at the additional change in the skill-premium for young workers.

There are already a substantial number of micro-interventions in India that can help guide policy-makers in supply-side interventions.[3] These micro-interventions are, however, inherently different from large school expansion programs since they do not have GE effects. While the evidence on smaller changes of inputs within schools is mixed (Muralidharan, 2013), large-scale investments in schooling expansions like the one studied here, have been found to be relatively more successful across the world.[4]

A concern with an expansion in public schooling is that it may crowd out private supply negating the effects of public funds that could have been spent elsewhere. On the other hand, a crowd-in could also have occurred if the program increased the overall size and the demand for

[1] I corroborate my results with a Difference-in-Differences (DID) analysis similar to Duflo (2001, 2004), where I compare treated to untreated districts and the young to older cohorts. Using a DID design, however, it is hard to recover the entire extent of the labor market GE effects as the portion of the GE effects that affect all cohorts are differenced out by the DID estimator. The advantage of the RD is that it allows me to estimate the entire extent of the GE effects, and disentangle them into the portion that affects all cohorts and any additional impact on treated cohorts. The drawback of the RD is in generalizability – the RD estimates the GE effects in districts near the cutoff, which are the most literate of the low-literacy districts. Indeed, as my difference-in-differences results show, the implementation quality was poor and impacts on education small for the least literate districts – i.e. the ones furthest from the cutoff.

[2] There are other types of labor market GE effects in other contexts: Crepon et al. (2013) highlight the possibilities of negative externalities in job-search assistance programs, Albrecht et al. (2009) calibrate a macro model of the change in the labor market equilibrium due to the Swedish Knowledge Lift program, Epple and Ferreyra (2008) study how Michigan's school finance reform affects demographics and house prices, and more recently Bianchi (2016) shows how college major choice in Italy can affect returns.

[3] These studies cover a wide gamut of programs like library programs (Borkum et al., 2010), teacher training (Kingdon and Teal, 2010), teacher incentives (Muralidharan and Sundararaman, 2010), computer-aided programs (Linden, 2008), remedial education (Banerjee et al., 2010, 2007) and class sizes (Banerjee et al., 2007; Jacob et al., 2008; Muralidharan, 2013). Some often cited reasons for low educational outcomes are teacher quality and high levels of teacher absence (Das et al., 2013b; Duflo et al., 2012; Muralidharan, 2013).

[4] Some examples are in Indonesia (Duflo, 2001), Burkina Faso (Kazianga et al., 2013), Zimbabwe (Aguero and Bharadwaj, 2014), Nigeria (Osili and Long, 2008), Sierra Leone (Cannonier and Mocan, 2012), Uganda (Deininger, 2003), Zambia (Ashraf et al., 2015), Kenya (Bold et al., 2013), Tanzania (Sifuna, 2007), West Bank & Gaza (Angrist, 1995), and India (Afridi, 2010; Chin, 2005).

a skilled workforce. I model and estimate this change in private supply. In line with other work (Andrabi et al., 2013), I find an influx of private schools when public schooling grows.

To track long-run outcomes, I assemble a 10 year long panel of districts that allows me to follow local labor and education markets over time. While studies have found that policies that lower the costs of schooling have positive impacts in the short run, the existing evidence on the persistence of impacts is mixed (Angrist et al., 2006; Das et al., 2013a). I find that while there was a net increase in the number of new schools built over this period, only a few of these schooling inputs last in the long run. Once the funding is phased out, the physical infrastructure upgrades remain but the differential effects on more qualified teachers dissipate.[5]

I find that the program increased both education and earnings for students in targeted regions. There are large overall economic benefits to households that are driven by reductions in the household costs of education and an increase in the overall output of the region. However, general equilibrium effects substantially mitigate the rise in labor market earnings for those who acquire more skill. Increases in the supply of educated workers dampened earnings for skilled workers and put upward pressure on the earnings of unskilled workers. The returns to skill are 13.4%, but the estimated labor market GE effects are substantial – for a 17 percentage point increase in the fraction of skilled workers, the GE effects depress the returns by 6.5 percentage points and dampen the increase in benefits to students by 23%. These GE effects have distributional consequences, with a transfer of labor-market benefits from skilled to unskilled workers, particularly among the younger cohorts. High-skill workers who did not change their educational levels under the policy are adversely affected in the labor market, whereas low-skill workers benefit. Importantly, the adoption of skill-biased capital does play a role, however small, in mitigating the GE effects. But consistent with the other literature in this context (Munshi and Rosenzweig, 2015), I find no evidence of labor mobility.[6]

The methodology developed in this paper, and that can be used in other contexts, accounts for the general equilibrium effects of large-scale government spending and finds them to be substantial. In doing so, it improves upon the literature that largely ignores the broader adjustments in the labor market and education sector while estimating the private returns to education by exploiting tuition reductions, changes in compulsory schooling laws, schooling expansions or other large-scale policy reforms. Consistent with the theory I build in this paper, I find in the Indian context, that labor market GE effects dampen private benefits to students that attain more education and have substantial distributional consequences. The results in this paper indicate that these wage responses may undermine some of the effectiveness of micro-

[5]There is also a large literature in the US that studies whether spending on education affects educational outcomes (Card and Payne, 2002; Hanushek, 1997, 2003, 2006; Hoxby, 2001; Krueger, 2003). One crucial result from the US literature is that not all schooling inputs have similar impacts, and so it is necessary to understand which inputs matter (Grogger, 1996; Hanushek, 1986, 2008; Krueger, 1999; Loeb and Bound, 1996). This is why I extensively study the changes in a whole host of schooling inputs, from teachers to physical infrastructure.

[6]In an accompanying paper (Khanna, 2015), I compare this policy to more decentralized policies implemented in the following decade. The decentralized policies targeted sub-districts in a way that allows me to use a Multi-Dimensional Regression Discontinuity (MRD) framework.

interventions when they are scaled up (Acemoglu, 2010; Deaton, 2010).[7] On the other hand, the crowd-in of private schools indicate that large-scale public schooling expansions may have other unintended benefits in the education sector. I also show that once the funding was phased out certain crucial inputs, such as well qualified teachers, no longer remain. These empirically important consequences are vital considerations for both researchers and policymakers who examine or implement large-scale interventions.

1 The District Primary Education Project (DPEP)

I use exogenous variation generated by a large schooling expansion policy (the District Primary Education Project (DPEP)) implemented by the Indian government. The government selected districts based on the prevailing female literacy rate, which allows for a RD design. I compare districts that should have received the policy to those that should not have, on either side of the RD cutoff, to causally estimate the parameters of the model. This was also a time of rapid growth and development in the Indian economy, which is why the RD is necessary to isolate the impact of the policy from other changes. In this section I discuss the program; additional details on the history, funding and secondary objectives can be found in Appendix C.

In 1994, the District Primary Education Project (DPEP) was introduced in seven states and 42 districts, and was over time expanded to 271 of approximately 600 districts in the country. The project spanned four phases, the last of which were implemented in the mid-2000s. While a portion of the funds were released under DPEP through the mid-2000s, the bulk of the funding ended in 2005 when other policies under the newer Sarva Shiksha Abhiyan (SSA) were growing in strength.[8] In 2006, only 2 states received any money, and after 2007 none did.[9]

DPEP grew to consist of seven projects, with funding from international agencies, making it one of the largest donor assisted programs in the world (Jalan and Glinskaya, 2013). States had to maintain the level of expenditure that existed before the program was implemented in an attempt to ensure that there was no crowd-out of state funds.[10] Appendix Figures A.6 and A.7 show how foreign funds and education funds rose steadily under the program – a period of a large increase in externally-financed expenditure on education, most of which was concentrated in less than half the districts of the country, allowing for a valuable policy experiment.

[7] This point has often been made outside the realm of Development Economics as well (Heckman et al., 1999).

[8] SSA was similar to the DPEP, but covered the entire country. There were, however, certain programs under SSA that targeted certain sub-districts.

[9] The phase-out was fairly rapid. In the 2002-3 financial year, the government spent approximately $345 mn on DPEP, whereas in the 2006-7 financial year, they spent only $24 mn on it. Even though taxes were not raised to fund the DPEP, when the shift to the newer SSA program happened, the government levied a 2% education tax to fund an expansion to all districts.

[10] Varghese (1994) claims that states had to maintain their educational expenditures at at least their 1992 values, and World Bank (1997) guidelines claim states had to maintain the same growth rate in educational expenditure. However, states did have the ability to re-allocate funds across districts.

The broader program claims to have covered about 271 low literacy districts, and served approximately 51.3 million children and 1.1 million teachers in about 375,000 schools (Jalan and Glinskaya, 2013). These districts were geographically dispersed all over the country (map in Appendix Figure A.9). It created about 160,000 new schools (Azam and Saing, 2016), and trained about 1 million teachers and 3 million community members. Within states, there was major inter-district variation in planning and management as the districts had the flexibility to allocate funds. In the project states, it increased the average allocation of funds for primary school education by between 17-20% (Jalan and Glinskaya, 2013).[11]

The primary objective of the program was to improve student access to and retention in primary and upper primary education by building schools, hiring teachers, supporting school and community organizations, constructing new classrooms and improving existing school facilities. The "*project would be a reconstruction of primary education as a whole in selected districts instead of piecemeal implementation of schemes*" (GOI, 1994). While most of the funds were directed towards the government schools, some were used towards a training drive for teachers of private and government-aided schools.

There are numerous World Bank and Government of India briefs and media reports that refer to the program's success.[12] One contemporaneous paper uses a difference-in-differences strategy to show that DPEP increased the years of education in treated districts (Azam and Saing, 2016). Another in-depth investigation, is a working paper by Jalan and Glinskaya (2013) that uses a difference-in-differences methodology to compare the enrollment rates for students in the 42 districts in the first of the four phases to other districts. They find that five years after the program started, enrollment and grade progression of minority groups improved only in some specific states. Furthermore, grade progression for boys in certain states was higher, but there were little to no impacts on girls. Over the entire period, districts were not allowed to receive more than \$8 million, which came to approximately \$9.1 per student. Jalan and Glinskaya (2013) estimate that this intervention lowered the private household costs of schooling by between 20 to 40%. Their paper uses two repeated cross sections of enrollment to look at the short-run impacts on the few districts in the first phase of the program. In contrast, I use the RD design and look at the longer run effects fifteen years after the program started, and after all the phases were implemented. Other descriptive studies examine the outcomes for DPEP districts, and by and large consider the program to be a success (Aggarwal, 2000; Menon, 2001; Pandey, 2000). However, they do not compare DPEP districts to others, and hence cannot distinguish between the changes in overall education taking place all across the country driven by robust economic growth, and the changes specifically attributable to the program.

[11] In this period, DPEP was the flagship education program, despite being restricted to less than half the country. For example, in 2001 alone, the Ministry of Human Resource Development, estimates spending \$275 mn on DPEP for the limited number of districts. The second and third largest expenditures were on schemes that covered all districts like the Mid-day Meal Scheme (\$232 mn), and Operation Blackboard (\$130 mn).

[12] See World Bank Report (2003), "World Bank praises India for DPEP" Economic Times, (Sep 2005) and Government of India (2011).

2 The Model, Comparative Statics and Welfare

I set up a model that captures the salient features of the local economy and the market for education, including the general equilibrium effects. The model will identify sufficient statistics that determine the effect of schooling expansion policies on economic benefits.[13] On the labor market side, I combine two sets of canonical models. The first is a returns-to-education model (Becker, 1967; Card, 1999; Mincer, 1958), which determines one individual's optimal level of education for a given distribution of wages. The second is a skill premium model (Card and Lemieux, 2001) that determines the equilibrium distribution of wages for a given distribution of educational skill levels. By combining them, I study how changes in the education (skill) distribution affect the distribution of wages, and vice versa, allowing me to identify the general equilibrium effects in the labor market.

The demand for education (skill) is determined by students' optimization decisions, that also depend on the labor market returns and the general equilibrium effects on earnings. The supply depends on the choices made by both public and private schools.Building new schools and increasing access to schools will reduce the marginal cost of schooling (Behrman et al., 1996; Birdsall, 1985); directly raising household welfare, and inducing more education.[14]

2.1 Economic Production and the Labor Market

Aggregate output Y_d in district d depends on L_d (effective labor) and K_d (capital).[15] Capital is perfectly elastically supplied across districts at rental rate R^*.[16] Effective labor supply L_d depends on the labor aggregate L_{sd} at each skill level s.

$$Y_d = L_d^{\varrho} K_d^{(1-\varrho)} \quad where \quad L_d = \left(\sum_s \theta_{sd} L_{sd}^{\frac{\sigma_E - 1}{\sigma_E}} \right)^{\frac{\sigma_E}{\sigma_E - 1}} \tag{1}$$

$0 < \varrho < 1$ is the share of output accruing to labor, $\theta_{sd} > 0$ is the productivity of workers with education or skill level s, and $\sigma_E > 0$ is the elasticity of substitution across education or skill groups. The productivity parameter θ_{sd} captures the productivity of each skill level, and

[13] As explained in Section 3.1.1, the advantage of using sufficient statistics is that the estimation procedure and measured GE effects do not depend on the specific functional form of the production functions. Indeed, the estimated wage benefits will hold true even if under many alternative formulations, like signaling models. However, couching it in a canonical labor economics model allows us to understand the drivers and parameters under the different effects.

[14] New schools reduce transportation costs, and lower the market price by expanding supply, whereas improvements in quality make it easier for students to complete the grade.

[15] Adding non-tradables like land into the aggregate production function Equation (1) does not directly affect the estimation strategy. The policy will theoretically change the value of non-tradables; however, I will be concentrating on the earnings of workers, and not be examining the returns to owners of capital and land.

[16] The perfectly elastic capital assumption is not essential. The results are unaffected by assuming a fixed capital stock (see Appendix B.II).

increases with an increase in skill-biased capital in the district k_{sd}, such that $\theta'_{sd}(k_{sd}) > 0$.[17] The value of θ_{sd} therefore varies across districts only because of the variation in skill-biased capital k_{sd}. The aggregate supply of workers at skill level s depends on the aggregate effective supply of workers in each skill level ℓ_{asd} in a given age cohort a:

$$L_{sd} = \left(\sum_a \psi_a \ell_{asd}^{\frac{\sigma_A - 1}{\sigma_A}} \right)^{\frac{\sigma_A}{\sigma_A - 1}} \tag{2}$$

Here, σ_A is the elasticity of substitution across age cohorts, and ψ_a is the productivity of a specific cohort. The effective supply ℓ_{asd} may depend on the ability of workers ϵ_i.[18] A worker gets paid their marginal product. The average log earnings are therefore:[19]

$$\log w_{asd} = \log \left(\frac{\partial Y_d}{\partial \ell_{asd}} \right) = \log \widetilde{\varrho} + \log \theta_{sd} + \log \psi_a + \frac{1}{\sigma_E} \log Y_d + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E} \right) \log L_{sd} - \frac{1}{\sigma_A} \log \ell_{asd} \,, \tag{3}$$

where $\log \widetilde{\varrho} \equiv \left[\left(1 - \frac{1}{\sigma_E} \right) \left(\frac{1-\varrho}{\varrho} \right) \log \left(\frac{1-\varrho}{R^*} \right) \right]$ is common across all districts and workers.[20]

There are a few components that drive the differences in average earnings when comparing two different types of people in two different labor markets represented in Equation (4):

$$\begin{aligned} \log \left(\frac{w_{asd}}{w_{a's'd'}} \right) = {} & \underbrace{\log \left(\frac{\theta_{sd}}{\theta_{s'd'}} \right)}_{\text{productivity}} + \underbrace{\log \left(\frac{\psi_a}{\psi_{a'}} \right)}_{\text{cohort}} \\ & + \underbrace{\frac{1}{\sigma_E} \log \left(\frac{Y_d}{Y_{d'}} \right)}_{\text{output}} + \underbrace{\left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E} \right) \log \frac{L_{sd}}{L_{s'd'}}}_{\text{skill distribution}} \underbrace{- \frac{1}{\sigma_A} \log \frac{\ell_{asd}}{\ell_{a's'd'}}}_{\text{skill-cohort distribution}} \end{aligned} \tag{4}$$

This equation is crucial in that it captures why earnings are systematically different across people and across labor markets. The first component – 'productivity' – θ_{sd} is the higher productivity associated with more education. Not only are skilled workers more productive, but variation in the supply of skill-biased capital across districts will affect earnings as well. The second component – 'cohort' – captures the age-specific productivities and returns to experience ψ_a. The third – 'output' – is the difference across labor markets related to differences in the size of the economy. The fourth – 'skill-distribution' – is the difference in earnings due to differences in the supply of more educated workers L_{sd}. This influences the labor market general equilibrium effects that affect all cohorts. Last – 'skill-cohort distribution' – affects the earnings due to differences in the supply of skilled workers within each cohort ℓ_{asd}, and drives an additional GE effect on cohort a. Changes in the skill cohort distribution by age cohort will

[17] For completeness, in Appendix B.III I explicitly model skill-biased capital within the nested CES framework and show how flexible ways of incorporating it do not affect the estimation or results.

[18] For instance, the effective supply $\ell_{asd} = \sum_i \epsilon_i \ell_{asdi}$.

[19] This is at the optimal value of K_d^*, so that $Y_d = \left(\frac{1-\varrho}{R^*} \right)^{\frac{1-\varrho}{\varrho}} L_d$.

[20] For tractability, I have ignored the role played by changes in prices. It is easy to include a $\log P_d$ that will be associated with the $\frac{1}{\sigma_E} \log Y_d$ term, and not affect the returns to skill.

therefore have important GE effects on the earnings of workers.

Furthermore, how much the skill distribution affects the difference in earnings also depends on the elasticities of substitution σ_E and σ_A. For instance, if the young and the old are perfect substitutes, then the skill-cohort distribution should not affect earnings. The increase in earnings for a person who goes from being unskilled u to skilled s will be defined as the returns to education β_{asd}:

$$log\frac{w_{asd}}{w_{aud}} = log\frac{\theta_{sd}}{\theta_{ud}} + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right) log\frac{L_{sd}}{L_{ud}} - \frac{1}{\sigma_A} log\frac{\ell_{asd}}{\ell_{aud}} \equiv \beta_{asd} \tag{5}$$

These returns depend on the difference in the productivity parameters θ_{sd} and θ_{ud}, the skill distribution L_{sd} and L_{ud}, and the cohort specific skill distribution ℓ_{asd} and ℓ_{aud}. For instance, in regions that have relatively more skilled workers, the returns to acquiring skill will be relatively lower. Whereas for regions with more skill-biased capital, the returns to skill are higher.

2.2 Students' Decisions

Students, in my model, choose the optimal level of education given their marginal costs of going to school and the returns to education (Becker, 1967; Mincer, 1958; Willis, 1986). Given how earnings are determined in section 2.1, these choices will also eventually affect earnings, and lifetime utility.[21]

Student i chooses their optimal years of education s_{id} to maximize the present discounted value of their lifetime earnings $w_{aid}(s_{id})$ given the costs of going to school $\kappa(s_{id})$. Since the linear form of $\kappa(s_{id})$ only captures the opportunity costs, Card (1999) suggests a more general formulation of the cost function, to capture credit and other monetary constraints (Becker, 1967):[22]

[21]Individuals i in district d and age a choose their optimal consumption stream, C_{it}, and years of schooling, s_{id}, to maximize utility $u(C_{it})$, where $u'(C_{it}) > 0$ and $u''(C_{it}) < 0$. For a given subjective discount rate δ, an internal rate of interest r_{id} and a constant stream of earnings $w_{aid}(s_{id})$, the optimization problem can be set up as:

$$\max_{\mathbf{C_{it}},\mathbf{s_{id}}} \int_0^\infty u(C_{it})e^{-\delta t}dt \quad s.t. \quad \int_0^\infty w_{aid}(s_{id})e^{-r_{id}(\kappa(s_{id})+t)}dt \geq \int_0^\infty C_{it}e^{-r_{id}t}dt\ , \tag{6}$$

where $\kappa(s_{id})$ captures the costs of schooling. For example, if $\kappa(s_{id}) = s_{id}$, then it only captures the opportunity cost of foregone earnings for each additional year of schooling. This specific opportunity-cost only formulation leads to the familiar form (Mincer, 1958; Willis, 1986):

$$\log\left(\int_0^\infty w_{aid}(s_{id})e^{-r_{id}(s_{id}+t)}dt\right) = \log\ w_{aid}(s_{id}) - (\log\ r_{id} + r_{id}s_{id}) \tag{7}$$

In the absence of incomplete markets and uncertainty, this problem is separable into individuals first choosing s_{id} to maximize their stream of earnings, and then choosing $\mathbf{C_{it}}$ to maximize utility.

[22]Becker (1967) justifies the quadratic costs from the observation that each subsequent year of education is even more expensive than before, because (a) fees are higher for higher levels (and in many cases early education is subsidized), and (b) students first exhaust easy sources of funds (parents, relatives) before using more expensive sources (loans).

$$\max_{s_{id}} \ \log \ w_{aid}(s_{id}) - \left(\log \ r_{id} + r_{id}s_{id} + \frac{1}{2}\Gamma s_{id}^2\right) \ , \tag{8}$$

where Γ is the quadratic cost parameter. Equations (4) and (5) determine the form of the individual earnings function. The benefits from education for individual i can be represented by the following function, where β_{asd} captures the returns to schooling that may differ across districts, cohorts and skill-groups:

$$\log \ w_{aid}(s_{id}) = \gamma_d + \nu_a + \beta_{asd}s_{id} + \log \ \epsilon_i \, , \tag{9}$$

where ϵ_i is the ability of the worker that is not observable to researchers, the distribution of which is the same across districts. This ability will be correlated with the marginal costs of schooling r_{id} and lead to biases in standard OLS regressions ($corr(\epsilon_i, r_{id}) \neq 0$). For instance, high-ability workers earn high wages but also have lower costs of performing in school. Also crucial to notice is that the returns to education β_{asd} differ across districts and skill-groups due to differences in relative skills in the local labor force, and across cohorts due to the cohort-specific differences in the skill distribution.

In Equation (9), average earnings also differ across districts γ_d due to differences in the overall output and capital across regions, and differ across age cohorts ν_a due to the returns to experience or other cohort-specific productivities captured in Equation (4).

Given this setup, from the first order conditions one can obtain the optimal years of education for person i:

$$s_{id}^* = \frac{\beta_{asd} - r_{id}}{\Gamma} \tag{10}$$

The variation in s_{id}^* within a district d is driven entirely by the variation in the marginal cost parameter r_{id}. Notice, however, that the distribution of earnings in district d is driven both by the costs of education r_{id}, and by ϵ_i abilities.[23]

The marginal cost parameter for person i in district d is a function of the district-level costs of going to school, and an individual component η_i that captures individual heterogeneity in the costs of schooling. The district-level costs depend on the access to schooling A_d (like distance to the nearest school) and the monetary price of going to school p_d (like school fees).[24]

$$r_{id} \equiv -\Psi A_d + p_d + \eta_i \ , \tag{11}$$

[23] Smith (1775) highlights the importance of educational capabilities r_{id} when arguing that "The difference between the most dissimilar characters, between a philosopher and a common street porter, for example, seems to arise not so much from nature, as from habit, custom and education." On the other hand, early formal models of variation in earnings (Roy, 1950) discuss the importance of 'abilities' ϵ_i, like "health, strength, skill, and so on."

[24] Restricting the cost parameter to simply depend on either only the monetary costs of going to school (p_d) or only the non-monetary costs (A_d) will not change the qualitative predictions of the model. This is because an expansion in public schooling will lower both types of costs in equilibrium.

where Ψ represents how aggregate access to education affects each i individual. An increase in the number of schools in regions that did not initially have many will directly lower the transportation costs of going to school, but may also lower the competitive equilibrium fees, even for private schools.[25] These education decisions are a nested portion of the problem where individuals maximize their lifetime utility based on their consumption stream.[26]

2.3 Schools

In my model for public schools a district planner makes the decisions for all schools, whereas in the private sector each school decides separately. Furthermore, while public schools are meant to increase the access to schooling to citizens, private schools care about profits. Both types of schools can have heterogeneous costs or efficiency, but they provide the same output. Hence, students merely chose the school that is less costly for them, where the costs not only depend on the school fees p_d, but also transportation costs and non-monetary costs A_d.

2.3.1 District Level Public School Administrator's Decisions

Public school administrators for district d maximize the access to schooling A_d for students by investing in inputs $\mathbf{x_m}$. The total supply of public schooling depends on these inputs like schools, teachers and infrastructure. As access to schooling is increased, this reduces the marginal costs of going to school for students. For instance, one crucial aspect of access to schooling could be the distance to the nearest school; by building more schools, public officials may reduce this distance and increase access to schools. Public schools are not directly concerned with revenues from fees, and many are meant to be free (Kremer and Muralidharan, 2007). They do, however, have a budget constraint that restricts their spending.[27] The district d receives R_d from the government, and spends p_m for each input x_m into the schooling production function. Any funds received under government-backed schemes will increase the value of R_d. This setup, therefore, reduces the district's maximization problem to the following:[28]

$$\max_{\mathbf{x_m}} \; A_d(\mathbf{x_m}) \;\; \text{s.t.} \sum_{\mathbf{m=1}}^{\mathbf{M}} \mathbf{p_m x_m} \leq \mathbf{R_d}\,, \tag{12}$$

where $\frac{\partial A}{\partial x_m} > 0$, $\frac{\partial^2 A}{\partial x_m \partial x_m} < 0$, $\frac{\partial^2 A}{\partial x_m \partial x_n} > 0$. From the first order conditions, it is easy to derive the optimal amount of inputs of type m: $x^*_{md}(R_d, \mathbf{p_m})$, where $\frac{\partial x^*_m}{\partial R_d} \geq 0$ and $\frac{\partial x^*_m}{\partial p_m} \leq 0$. An increase in government funding R_d thus increases the amounts of each input in the schooling-access

[25] Here, students choose the lowest cost school regardless of whether they are privately or publicly owned.

[26] On the consumption side, the inter-temporal consumption stream can be represented by the Euler equation: $\frac{u'(C_{i,t+1})}{u'(C_{i,t})} = \frac{\delta}{r_{id}}$, where overall consumption C_{dt}, must equal overall production by the firms Y_{dt} for the product market to clear.

[27] In the market equilibrium, the fees will be affected by the decisions of public and private schools.

[28] The set-up is agnostic about heterogeneity in public schools – some may be more efficient than others.

production function, increases the overall access to education A_d and reduces the marginal costs of schooling for the students in the district.[29]

2.3.2 Private schools

Building more public schools affects the entry of private schools and determines the extent of crowd-in or crowd-out. If private schools are merely crowded out one-for-one, then the funds may have been better spent elsewhere. Private schools are profit maximizers and have heterogeneous costs.[30] They are price takers in the competitive market and charge a fee p_d. Muralidharan and Sundararaman (2015) are among the first to provide causal evidence that students in private schools have similar test scores as public school students for the subjects that are taught in both schools. They are, however, more cost-effective. Private schools, in my model, therefore, have the same output as public schools, but may do so at a different cost; and there is heterogeneity in private school efficiency (Kremer and Muralidharan, 2007).

The total educational output (in student-years) Q_{jd} by school j is a function of its aggregate inputs X_{jd} in the following way: $Q_{jd} = \overline{\theta_d} X_j$. Here the aggregate output of the private schools depend on the average skill level of the district $\overline{\theta_d}$. This is meant to capture demand externalities. For instance, Birdsall (1982) models the demand for schooling from households, as a function of the aggregate supply of public schools. An expansion of public schooling, will then increase the overall demand for all schools, including privately owned ones. Another alternative comes from peer effects in school participation. If certain students are encouraged to go to school, then the demand from their neighbors may also rise (Bobonis and Finan, 2009).[31] There are, however, quadratic costs associated with using inputs $Z(X_j)$. The school chooses inputs to maximize profits:

$$\max_{X_j} \; p_d \overline{\theta_d} X_j - Z(X_j) \tag{13}$$

The costs $Z(X_j) = z_{1j} X_j + \frac{1}{2} z_{2d} X_j^2$ have a simple quadratic formulation.[32] There is a heterogeneity in costs z_{1j} across schools, where some schools use their inputs more effectively than others, and a heterogeneity in costs z_{2d} across districts, where certain districts have better infrastructure for setting up a school. This is meant to capture the fact that in some districts it is cheaper to hire teachers and some have better physical infrastructure like electricity, drinking water, roads, and resource centers than others. The demand for inputs can, therefore, be found from the first order conditions, and the supply curve and profits are:

[29]See Appendix B.I for a parametric example of this set-up.

[30]Alternatively, they could have been modeled as having heterogeneous productivities, with the same result.

[31]Output in the public schools may also depend on $\overline{\theta_d}$, without a qualitative change in the results.

[32]While it is easy to hire the first few teachers or administrators, it is more costly to hire the next few as the pool of potential candidates dwindles.

$$Q_{jd} = \overline{\theta_d} X_j^* = \overline{\theta_d} \frac{p_d \overline{\theta_d} - z_{1j}}{z_{2d}} \quad and \quad \pi_{jd} = \frac{(p_d \overline{\theta_d} - z_{1j})^2}{2 z_{2d}} \tag{14}$$

Since there is free entry of private schools into these regions, schools will enter until $\pi_{jd} = 0$. The marginal school, therefore, will have a cost parameter $\widetilde{z_{1d}} = \overline{\theta_d} p_d$. If costs are drawn from a distribution $F(z_{1j})$, then the fraction of schools that enter is given by: $F\left(\overline{\theta_d} p_d\right)$.

Notice what guides the entry and exit decision of schools is the average productivity level in the district $\overline{\theta_d}$, the price p_d, and consequently the cost z_{2d} which depends on the infrastructure levels. If we see a fall in the supply of private schools along with a fall in the equilibrium price, then it is clear that the strongest driving force is that an increase in the supply of public schooling drives down the equilibrium price and crowds-out private schools.

Alternatively, if we see a rise in the supply of private schools in the light of an expansion in public schools, there are two possible reasons. The first is that demand externalities and peer effects – captured by θ_d – drive up the equilibrium price and induce private schools to enter. The second is that infrastructure upgrades and the presence of more teachers lowers the operating costs – captured by z_{2d} – and lead to more private school entry and further lower the equilibrium price. The price is, therefore, informative in distinguishing between these channels.

The best evidence for how private schools respond comes from Andrabi et al. (2013), who show that how an expansion in public schooling increased education for girls, and these girls became teachers in Pakistani districts. This increase in the number of teachers allowed private schools to enter the market soon after. Similarly, Jagnani and Khanna (2016) and Pal (2010) find that physical infrastructure upgrades can induce private-school entry in India.[33]

2.4 Definition of an Equilibrium

The exogenous elements of equilibrium are the student utility functions, schooling-cost functions, educational access functions, private firms' production functions, and the amount of exogenous government spending on schooling. What is endogenous is the years of education, the earnings-returns to education, the optimal inputs in the schools, the output of firms, the fraction of private schools that enter, and the equilibrium price and quantity of schooling.[34]

[33] See Appendix B.I for a derivation for the overall supply of private schooling in the region – in the parametric formulation, it is easy to see that $\frac{\partial p_d^*}{\partial z_{2d}} > 0$. The intuition is simple: a fall in the operating costs will increase the supply of private schools and lower the equilibrium price. On the other hand, an increase in the demand externality will increase demand for schooling and raise the equilibrium price. Seeing how prices change will allow us to distinguish between the various potential mechanisms.

[34] In line with the literature, so far I have assumed perfect foresight. When there are general equilibrium effects, students know exactly what the earnings will be *including* the general equilibrium effects. If expectations were adaptive, and students did not take into account the GE effects, they would get "too much" education, causing the skilled wage to fall even further. The subsequent cohort would then need to adjust its expectations as well, and the equilibrium is approached very slowly as each cohort revises its expectations. For a cobweb style model see Freeman (1975).

Appendix B.I characterizes and derives the education-sector equilibrium. For the product market to clear, the amount of consumption C_{td} must equal the amount of output Y_{td}. For the labor market to clear, the demand for workers ℓ_{asd} with education level s (Equation (3)) must equal the supply from the equilibrium amount of schooling.

Proposition 1 (Equilibrium) *Given the following dimensions of the model: A student utility function $U(C)$, returns to education function $\log\ w(s)$ and cost functions $\kappa(s, r, \Gamma)$; access to schooling function $A(\mathbf{x_m})$, and prices of inputs p_m; exogenous revenues from the government R_d; distribution of private school costs $F(z_{1j})$, and cost functions for private schools $Z(X_j)$; firms' production functions Y, different productivities for each education level θ_{sd}, the elasticity of substitution between education groups σ_E, and age groups σ_A; there exists an equilibrium that determines: The returns to an additional year of schooling β_{asd} that varies by district, age-cohort and skill level; the distribution of the optimal years of schooling S^*_d, and the monetary price of going to school p^*_d; the optimal inputs into the access function $x^*_m(R_d, \alpha_{\mathbf{m}}, \mathbf{p_m})$; optimal private school inputs $X^*_j(p_d, z_{1j})$; and equilibrium earnings w_{asd} and quantities of each type of worker ℓ_{asd}.*

3 Identification of Economic Benefits

3.1 Using Policy Changes to Estimate Parameters

The variation in s^*_{id} is driven entirely by the variation in the marginal costs r_{id}. Since the costs of schooling are likely to be correlated with the ability of the worker $Cov(\eta_i, \epsilon_i) \neq 0$, a simple OLS regression of earnings on education will give us biased estimates of the parameters. Moreover, comparisons in the cross-section across different labor markets will provide biased estimates due to underlying baseline differences in the skill distribution and skill-biased capital across these markets (Equation (4)). The equilibrium amount of schooling is affected by the expansion of public schooling:[35]

$$S^*_d = \phi_1\beta_{asd} + \phi_2 R_d - \frac{\eta_d}{\Gamma} \tag{15}$$

There are a few crucial components to this equation – the $\phi_2 R_d$ portion captures how more government spending increases equilibrium schooling by making public schools more accessible, and making (via adjustments in the market price) private schools more affordable (Appendix B.I). The term $\phi_1\beta_{asd}$, captures how changes in the returns to education will affect equilibrium schooling. If, for example, the labor-market general equilibrium effects substantially lower the returns to education β_{asd}, then there may be no increase in the equilibrium amount of schooling.

[35] See Appendix B.I for a parameterization of ϕ_1 and ϕ_2, where $\phi_1 \equiv \left(\frac{\overline{\theta^2_d}}{\Gamma\overline{\theta^2_d}+z_{2d}}\right) > 0$ and $\phi_2 \equiv \left(\frac{\left(z_{2d}+\Psi\overline{\theta^2_d}\right)\left(\prod_m \frac{\alpha_m}{p_m}^{\alpha_m}\right)}{\Gamma\overline{\theta^2_d}+z_{2d}}\right) > 0$, and $\eta_d = \mathbb{E}[\eta_i | i \in d]$..

The final term $\frac{\eta_d}{\Gamma}$ is unaffected by the schooling expansion. We would, however, expect it to be correlated with other unobserved district-level characteristics causing biased estimates in standard estimation frameworks.

Any district that had a female literacy rate below the national average (based on the 1991 Census) was made eligible to receive the policy. Therefore, it is possible to compare districts just above and below this cutoff to determine the causal impact of the policy on equilibrium schooling at the discontinuity using a fuzzy Regression Discontinuity (RD) design. We should expect that η_d is not different for districts that just fall on either side of the cutoff.

Let us define $D_d = 1$ to be districts that just fall on the side of the cutoff that receives the policy, and $D_d = 0$ districts that fall on the other side. In the neighborhood of the cutoff, we should therefore expect:

$$S_d = \phi D_d + \frac{\eta_d}{\Gamma} \quad and \quad \mathbb{E}[\eta_d | D_d = 1] = \mathbb{E}[\eta_d | D_d = 0] \tag{16}$$

If the direct effects of increasing access to schooling outweigh any negative labor market general equilibrium (GE) effects that depress returns, then we should expect $\phi > 0$.

3.1.1 Returns to Education and Disentangling Earnings

The policy changes the distribution of earnings across the RD cutoff. In Equation (3), ψ_a captures the cohort effect. θ_{sd} captures the pure productivity effect and a change in the amount of skill-biased capital in response to the policy will change its value. The term $\frac{1}{\sigma_A} \log \ell_{asd}$ is crucial for the cohort specific labor-market general equilibrium effect, and $\frac{1}{\sigma_E} \log Y_d + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right) \log L_{sd}$ determines the general equilibrium effect that affects all cohorts:[36]

$$\log w_{asd} = \log \widetilde{\varrho} + \log \theta_{sd} + \log \psi_a + \frac{1}{\sigma_E} \log Y_d + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right) \log L_{sd} - \frac{1}{\sigma_A} \log \ell_{asd} \tag{3}$$

I exploit variation along various dimensions to disentangle the components of the change in earnings across the RD cutoff. These dimensions include age cohorts, skill levels and treatment status. By restricting comparisons to be within cohorts, the cohort effect on earnings Ψ_a is differenced out. Cohorts, in treated districts, that were too old to change their years of education when the policy was implemented will be affected by some part of the labor-market general equilibrium effects. The general equilibrium effects that affect all cohorts can thus be isolated by looking at the impact on the skill-premium of older cohorts.

Earnings for younger cohorts, however, will additionally be affected by cohort-specific general equilibrium effects since there are more highly educated people in the younger cohorts.[37]

[36] While this equation is represented in terms of production function parameters, the estimated GE effects will not depend on the specific functional form of the production function as long as workers can be disaggregated into skilled and unskilled; young and old. The functional form is to better understand the role played by underlying economic parameters.

[37] In the estimation exercise, there are a few rules that need to be followed in order to get unbiased estimates.

For ease of exposition I restrict the analysis to two skill levels – skilled s and unskilled u workers. For example, the fraction of each among the young y are represented by ℓ_{sy} and ℓ_{uy} respectively. For any two-skill groups: $\Delta\ell_{sy} \equiv (\ell_{sy,D=1} - \ell_{sy,D=0}) = -\Delta\ell_{uy} \equiv (\ell_{uy,D=1} - \ell_{uy,D=0})$.

Let $D = 0$ represent the local economies that do not receive the program, and $D = 1$ the districts that do. If only a single individual was to acquire skill and change status from unskilled u to skilled s, the GE effects would be infinitesimally small. If the person lives in the untreated region $D = 0$, then that person's earnings would change in the following manner:

$$log\frac{w_{as,D=0}}{w_{au,D=0}} = \underbrace{log\frac{\theta_{s,D=0}}{\theta_{u,D=0}}}_{\text{Productivity}} + \underbrace{\left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right) log\frac{L_{s,D=0}}{L_{u,D=0}}}_{\text{Aggregate skill distribution}} \quad \underbrace{-\frac{1}{\sigma_A} log\frac{\ell_{as,D=0}}{\ell_{au,D=0}}}_{\text{Cohort specific skill distribution}} \quad \equiv \beta_{as,D=0} \ , \tag{17}$$

where $\beta_{as,D=0}$ is defined as the earnings returns to changing ones skill from u to s in a district where $D = 0$. If however, the individual lived in a treated region $D = 1$, where there are a lot more educated people or a lot more skill-biased capital because of the policy, the change in earnings would be:

$$log\frac{w_{as,D=1}}{w_{au,D=1}} = log\frac{\theta_{s,D=1}}{\theta_{u,D=1}} + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right) log\frac{L_{s,D=1}}{L_{u,D=1}} - \frac{1}{\sigma_A} log\frac{\ell_{as,D=1}}{\ell_{au,D=1}} \equiv \beta_{as,D=1} \ , \tag{18}$$

where $\beta_{as,D=1}$ is defined as the earnings returns to changing ones skill from u to s in treated regions $D = 1$. These returns differ because of the differences in the skill distribution of the workforce and the amount of skill-biased capital across regions. The difference in the returns to acquiring skill between these two regions is $\Delta\beta_{as} \equiv \beta_{as,D=1} - \beta_{as,D=0}$. Across the RD cutoff these returns will be different because of a change in the skill composition of the workforce and the adoption of skill biased capital. These are the GE effects on the returns to education:

$$\begin{aligned}\Delta\beta_{as} = & \underbrace{\left(log\frac{\theta_{s,D=1}}{\theta_{u,D=1}} - log\frac{\theta_{s,D=0}}{\theta_{u,D=0}}\right) + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right)\left[log\frac{L_{s,D=1}}{L_{u,D=1}} - log\frac{L_{s,D=0}}{L_{u,D=0}}\right]}_{\text{GE effects on all cohorts}} \\ & \underbrace{-\frac{1}{\sigma_A}\left[log\frac{\ell_{as,D=1}}{\ell_{au,D=1}} - log\frac{\ell_{as,D=0}}{\ell_{au,D=0}}\right]}_{\text{Additional GE on young}}\end{aligned} \tag{19}$$

In order to disentangle the general equilibrium effects on each cohort, one can look at the discontinuity in the skill premium of the younger and older cohorts separately. By restricting the population to a specific skill level (and cohort) one can ensure that the differences in earnings across the RD cutoff are only due to differences in the skill distribution and the amount of skill-biased capital.

The change in returns in Equation (19) can be split up into two components. The first is the GE effect that affects all cohorts. To estimate this effect, I look at the change in the skill

First, wage comparisons must always be made across the RD cutoff. Second, the same cohorts must be compared across the cutoff, and last the same skill group must be compared across the cutoff.

premium for the older cohort o. Empirically, this is the earnings differential between the skilled older population and the unskilled older populations:[38]

$$\underbrace{log\frac{w_{so,D=1}}{w_{so,D=0}} - log\frac{w_{uo,D=1}}{w_{uo,D=0}}}_{\text{GE effects on all cohorts}} = \underbrace{\left(log\frac{\theta_{s,D=1}}{\theta_{u,D=1}} - log\frac{\theta_{s,D=0}}{\theta_{u,D=0}}\right)}_{\text{Skill biased capital}} + \underbrace{\left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right)\left[log\frac{L_{s,D=1}}{L_{u,D=1}} - log\frac{L_{s,D=0}}{L_{u,D=0}}\right]}_{\text{Aggregate skill distribution}} \tag{20}$$

Notice that we would expect that these two portions of the GE effects on all cohorts counteract each other. On the one hand, an increase in the skilled workforce will lead to the adoption of more skill-biased capital and raise the skill premium. On the other hand, increasing the relative supply of skilled workers makes them less valuable in production, lowering the skill premium. If there is differential migration, and skilled workers migrate out of the treated districts in search of work, then it will weaken the strength of the 'Aggregate skill distribution' component of the GE effects by altering size of the skilled workforce in treated districts.

The second component of the GE effects is the additional GE effect on the young y that is driven solely by the change in the age-specific skill distribution. This component can be measured by estimating the earnings differential between the skilled young and unskilled young, and differencing out the earnings differential between the skilled old and unskilled old:[39]

$$\underbrace{\left[log\frac{w_{sy,D=1}}{w_{sy,D=0}} - log\frac{w_{uy,D=1}}{w_{uy,D=0}}\right] - \left[log\frac{w_{so,D=1}}{w_{so,D=0}} - log\frac{w_{uo,D=1}}{w_{uo,D=0}}\right]}_{\text{Additional GE on young}} = \underbrace{-\frac{1}{\sigma_A}\left[log\frac{\ell_{ys,D=1}}{\ell_{yu,D=1}} - log\frac{\ell_{ys,D=0}}{\ell_{yu,D=0}}\right]}_{\text{Age specific skill distribution}} \tag{21}$$

To estimate the two different returns $\beta_{as,D=0}$ and $\beta_{as,D=1}$, I use discontinuities in the average earnings of the young, and the wages of the skilled young, and unskilled young separately:[40]

$$log\frac{w_{y,D=1}}{w_{y,D=0}} = \ell_{sy,D=1}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=1}log\frac{w_{uy,D=1}}{w_{uy,D=0}} + \Delta\ell_{sy}\underbrace{log\frac{w_{sy,D=0}}{w_{uy,D=0}}}_{\beta_{ys,D=0}} \tag{22}$$

$$log\frac{w_{y,D=1}}{w_{y,D=0}} = \ell_{sy,D=0}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=0}log\frac{w_{uy,D=1}}{w_{uy,D=0}} + \Delta\ell_{sy}\underbrace{log\frac{w_{sy,D=1}}{w_{uy,D=1}}}_{\beta_{ys,D=1}} \tag{23}$$

The change in the average earnings for the younger cohorts is a weighted average of how the skilled and unskilled wages change, and the shift from unskilled to skilled work times the returns to skill. These relationships can be used to derive the returns to skill in both the treated and

[38] Regardless of the specific formulation of the production function, the change in the skill premium for older cohorts will be the GE effects on all cohorts, and estimates of the returns and cohort-specific GE effects will empirically rely on the left hand sides of equations (20) and (21). This is true even if wage returns are determined in purely signaling model. The right hand sides merely helps us understand the underlying economic parameters.

[39] Notice that if $\sigma_A < \sigma_E$ then the two components may be of opposite signs.

[40] See Appendix B.IV for detailed derivations of these equations.

untreated districts separately. At the same time, the average years of education in the districts change across the cutoff in the following manner:

$$\begin{aligned}\Delta S &= (\ell_{sy,D=1}s_1 + \ell_{uy,D=1}s_0) - (\ell_{sy,D=0}s_1 + \ell_{sy,D=0}s_0) \\ &= \Delta\ell_{sy}s_1 + \Delta\ell_{uy}s_0 = \Delta\ell_{sy}(s_1 - s_0)\ ,\end{aligned} \tag{24}$$

where s_0 is the years of education for the skilled group, and s_1 are the years for the unskilled group, and $\Delta\ell_{sy}$ is the fraction of students induced into getting more skill.

It is important to remember that the shift in the skill-distribution will change overall output as well. If an individual that has a skill level s were to switch districts from $D = 0$ to $D = 1$, that person's earnings would be different not only because of the skill distribution, but also because of the differences in overall output Y_d and skill-biased capital across the regions:[41]

$$log\frac{w_{s,D=1}}{w_{s,D=0}} = \frac{1}{\sigma_E}log\frac{Y_{D=1}}{Y_{D=0}} + log\frac{\theta_{s,D=1}}{\theta_{s,D=0}} + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right)log\frac{L_{s,D=1}}{L_{s,D=0}} - \frac{1}{\sigma_A}log\frac{\ell_{as,D=1}}{\ell_{as,D=0}} \tag{25}$$

3.2 Outcomes and Economic Benefits

The model predicts that when a district receives more funds for expenditure on public schooling, the following happens: First, public administrators build more schools, increasing the access to schooling (Section 2.3.1). This lowers the marginal cost of schooling for households, and induces certain students to get more education (Section 2.2). At the same time, private schools decide whether to enter or exit the education sector, leading to either a crowd-in or crowd-out of private schools (Section 2.3.2). When the newly skilled workforce joins the labor market they earn the higher skilled wage (Section 2.1). There is, however, a distributional impact on the earnings of skilled and unskilled workers (Section 3.1). If skilled workers are more productive and firms adopt more skill biased capital, then there is an increase in overall output, productivity and consumption (Section 2.1).

The changes in overall benefits will depend on the reduction in the costs of schooling for younger cohorts, the increase in overall output due to skill adoption, and the labor market returns. The increase in total output depends on the productivity parameters and the change is the skill distribution. At the same time, the GE effects will have distributional consequences. The welfare for older cohorts is unaffected by the reduction in the costs of schooling. The skilled old however are adversely affect by the GE effects that affect all skilled workers, whereas the unskilled old benefit from the increase in their earnings.[42]

[41] See Appendix B.II for details on modeling skill biased capital. If aggregate output prices change, then the skill-premium is unaffected since both the skilled and unskilled within a district face the same price change.

[42] Aggregate profits for private schools has a closed form solution and will change due to the policy. The extent of this will depend on the increase in productivity $\bar{\theta}_d$ and the decrease in the equilibrium price of schooling p_d). Furthermore, the returns to capital and land may change as well, depending on the ease of mobility and transaction costs. However, my analysis concentrates on the earnings of workers and costs of schooling.

Let $\beta_{as,D=0}$ be the returns to education in untreated districts, and $\beta_{as,D=1}$ be the returns including the general equilibrium effects. $\Delta\beta_{as}$ is thus the change in the returns due to the GE effects. The welfare for a young high-skill person that would have acquired skill even in the absence of the policy rises by the reduction in the total costs of education, but is dampened by the GE effect that affects all cohorts and the additional GE effect on younger cohorts. Labor market welfare for them is $log\frac{w_{as,D=1}}{w_{as,D=0}}$. Similarly, for those workers who would never acquire more skill even in the presence of the policy, the difference in the unskilled wage at the cutoff captures their labor market welfare: $log\frac{w_{au,D=1}}{w_{au,D=0}}$. For the younger cohorts, who are induced into getting more skill, the labor market welfare change depends on the skilled wage in the treated districts and the unskilled wage in the untreated districts: $log\frac{w_{ys,D=1}}{w_{yu,D=0}}$. To estimate this component of welfare, I use the returns to schooling $\beta_{ys,D=0}$, since:

$$log\frac{w_{ys,D=1}}{w_{yu,D=0}} = log\frac{w_{ys,D=0}}{w_{yu,D=0}} + log\frac{w_{ys,D=1}}{w_{ys,D=0}} = \beta_{ys,D=0} + log\frac{w_{ys,D=1}}{w_{ys,D=0}} \tag{26}$$

The change in labor market benefits for those induced into getting more skill therefore consists of two components – the returns to skill in the untreated districts and the change in economic benefits to the 'always skilled.' In the absence of any GE effects, the change in earnings for a person induced into getting more education would simply be $\beta_{ys,D=0}$. To compare the labor market benefits to the reduction in total costs of schooling to get a measure of aggregate welfare change, I discount the labor market gains by the real interest rate δ, over the time period τ. For a student induced into getting more education, the costs include tuition costs and the opportunity cost of a foregone unskilled wage. The benefits, however, include the present discounted value of a skilled worker's earnings stream.

4 Data

In order to study the policy in a comprehensive manner, I put together a number of large datasets that have not been combined in this manner before. The data are merged at the district level since districts are the relevant local economy and labor market in this context (Duflo and Pande, 2007). I combine data on school-level inputs, household level data on education, migration decisions and schooling expenditures, labor market data on earnings and occupations, and firm-level data on types of manufacturing in the different regions. With this I study how the policy affects the entire district; not just individual households.

It is important to study the dynamic consequences of the policy. For this purpose, I assemble a yearly panel data set that allows me to track schools, firms and other characteristics of the local economy over time. Given the changes in district boundaries over time, this panel is particularly challenging to create. Data details can be found in Data Appendix D.

Data for inputs into schools comes from the District Information System for Education (DISE).

Table 1 summarizes the variables of interest in the year 2005, twelve years after the DPEP started. The top panel classifies schools by ownership (government vs private), and when they were built (before 1993, the first year of the earliest program or after). 27% of all schools existing in 2005 were built post 1993, and while 20% are government schools, the remaining 7% are private schools. I complement the school-level data with test-score data from the Annual Status of Education Report (ASER).

In order to study educational outcomes, household surveys and Census data were used. The Census has detailed tables at all three of the administrative levels - states, districts and sub-districts. A panel of districts can be created using the 1991, 2001 and 2011 Census years, all of which include district-level statistics. The 1991 Census determines the running variable for the RD, since the 1991 female literacy rate was used to determine which districts are eligible for DPEP funds.

I use three different rounds of the National Sample Survey (NSS) to study the impact on education, earnings, expenditures, migration and other labor-market characteristics. It is the largest nationally representative household survey in the country, asks questions on weekly activities for up to five different occupations per person, and records earnings during the week for each individual in the household. Summary statistics for the 2009 NSS round are presented in Table 2. In 2009, only about 60% of the population had finished primary school, and on average people had about 6 years of education and earned about Rs 1466 ($30) a month.

Last, to study the behavior of firms, I use the Annual Survey of Industries (ASI), which is a census of all manufacturing firms in the country that employ more than ten persons. These data are available at the establishment level, and have information on the type of products produced, wages paid, and number of employees. One can then use these data to study whether changes in the skill level of the population can affect production technique and skill biased capital.

5 Estimation

In order to target the DPEP program to districts that were worst off in terms of educational outcomes, a selection criterion was used. Districts that had a female literacy below the national average (based on the previous 1991 Census) were eligible for the program, but not all such districts were selected. Further, in some states that had no low-literacy districts, a few districts were selected at the state's discretion. This imperfect assignment requires a fuzzy regression discontinuity design using the 1991 female literacy rate as a running variable. The fuzzy design allows for imperfect assignment, since not all low-literacy districts were selected, and for states with no low-literacy districts, some high-literacy districts were selected. To my knowledge, there are no other programs that use the district-level 1991 female-literacy rate as cutoff. I empirically test and show that cohorts that were too old to change their schooling decisions by the time the policy was implemented have no discontinuity in educational attainment.

The RD allows me to compare districts just above the literacy cutoff to those just below. Since we should not expect any discontinuity in the distribution of individual labor-market abilities or individual-specific costs of going to school around the cutoff at baseline, the RD estimator is consistent. Furthermore, at the cutoff, we should expect no discontinuity in pre-policy labor market characteristics, skill biased capital and regional outputs that would otherwise bias the estimated parameters. In order to estimate the GE effects, I further exploit variation in cohort exposure and skill levels.

Since more able workers are also more capable students, OLS estimates can suffer from an omitted variable bias. The variation generated by the policy can be used to overcome this bias. The policy induces certain students to go to school, whereas identical students in non-policy regions do not. Following students into the labor market, it is therefore possible to compare wages in the two regions to determine the returns to schooling for the subpopulation that was induced into getting more education. At the same time, local labor markets may differ widely across regions in terms of their skill distributions and skill premiums. This will confound OLS estimates of the GE effects. The RD allows me to compare similar local economies that differ only on the access to the DPEP policy.[43]

The first stage is presented in Figure 1. It is clear that the more literate amongst the eligible districts (i.e. amongst the districts with lower than average female literacy) were selected for the program, leading to a discontinuity at the cutoff. There is also visible imperfect assignment at both ends, with not all eligible districts being selected, and not all selected districts being eligible. Since it is clear that policy makers selected the most literate of the low-literacy districts, there is a high likelihood of political manipulation that is correlated with a whole host of unobserved characteristics in these regions, and regression specifications that do not allow for these differences in unobserved characteristics will be biased. A RD specification can, therefore, provide a causal estimate of the impact of this program. This will be the Local Average Treatment Effect (LATE) for districts near the cutoff (Imbens and Angrist, 1994).

The parameters estimated should be thought of as being representative only for districts near the RD cutoff. Furthermore, as will be discussed below, parameters like the estimated returns to education are for the students who were induced into getting more schooling and lived in the districts near the cutoff. The GE effects as well depend on what type of students get induced into getting more skill, as this may affect the amount of skill biased capital adopted by the change in the effective supply of labor. These general equilibrium effects, however, will also affect sub-populations that were not induced into getting more education.

Estimating causal impacts requires that there is no perfect manipulation of the running variable or the cutoff, which is likely in this case since the cutoff chosen was the national average of the female literacy rate from the previous 1991 Census. Furthermore, McCrary (2008) tests indicate

[43] One additional concern with OLS estimates (but not the RD), especially in developing country data, is the measurement error in the independent variable (Griliches, 1977).

that the density of districts and of households around the cutoff is not discontinuous (Figure 2), since the p-value of the change in density at the cutoff is 0.71. Other falsification tests will be discussed below that solidify the RD assumptions that there were no other discontinuities at the same cutoff.[44]

While RD results will be represented graphically, the coefficients of interest will also be calculated using a two-stage least squares procedure where the optimal bandwidth will be calculated using two different methods. I use the Calonico et al. (2014b) method, and the Imbens and Kalyanaraman (2012) method to select these bandwidths. The Imbens and Kalyanaraman (2012) method uses a data-driven bandwidth selection algorithm to identify the optimal bandwidth for a local linear regression given a squared loss function, whereas the Calonico et al. (2014b) method also performs a bias-correction and develops robust standard errors for such a procedure.[45] Results using both the optimal bandwidth procedures are presented, and are robust to using more parametric approaches like local linear and quadratic control-function approaches as suggested by Hahn et al. (2001) and Imbens and Lemieux (2008).[46]

6 Results and Discussion

The household level analysis can also be split up by the age groups that should and should not have been directly affected by the DPEP program. In Appendix Figure A.8b one can see a sharp drop in schooling enrollment at the age of 19, because by that age students have usually finished schooling, and child-labor laws prevent many workplaces from hiring children below eighteen.[47] Since the household survey was conducted 16 years after the start of the program, anybody above the age of 35 should not be directly affected by the program. Those under the age of 35 in treated districts, however, should be directly affected.

I present RD figures showing the discontinuity along the running variable for the year 2005, which was the last year before the phase-out of funds begin. The 2005 figures can be thought of as capturing the cumulative effects of the last twelve years of funding increases, and alongside the 2SLS coefficient over time will be presented to highlight the dynamics. How the 2SLS coefficient changes over time shows what happens to the outcomes of interest once the funding is cut in 2006, and stopped entirely in 2007.

[44]Cattaneo et al. (2015) offers an alternative test for manipulation at the cutoff that does not rely on the selection of binning parameters. The p-value of a discontinuity in the density using their method is 0.97.

[45]I use the code written by Calonico et al. (2014a) to estimate the parameters.

[46]The results are robust to using various alternative procedures that are as yet unpublished (Appendix Table A.15). The first, described in Bartalotti and Brummet (2017) allows for computing the standard errors at an aggregated level. The second method allows for different-seized optimal bandwidths on either side of the cutoff and for standard errors at an aggregated level (Calonico et al. (2017)).

[47]The Factories Act of 1948 and the Mines Act of 1952.

6.1 School Building and Survival

One of the primary objectives of the DPEP program was to build new schools. Figure 3 shows the effect of the program on schools built once the program was underway in 1994. While the top panel shows the fraction of all schools that were built post 1993, the middle panel shows the discontinuity in government schools. Both panels show that DPEP districts had a substantially larger fraction of new schools than non-DPEP districts. The ITT estimates indicate a 4.9 percentage point increase in the fraction of government schools that were new.[48]

Studying how the coefficient in Figure 3 changes over time allows me to trace out the longer term effects of the program. In all of the figures, the first coefficient plotted for the year 2005 shows a large discontinuity in the fraction of new schools, whereas the other coefficients in later years show a smaller difference among the districts on either side of the cutoff. While large amounts of funding were still being received by these districts in 2005, more schools were being built. However, once the funding was whittled down there was no longer any differential impact on the fraction of new schools built. This is because in the absence of funds, regions on the other side of the cutoff catch up by building schools a relatively more rapid rate.

As a falsification test, I can also look for any differential impacts on the fraction of schools that were built in the twenty year period prior to the program (1973 to 1993) out of all schools built before 1993. If schools have a short lifespan, then more funding may allow schools to last longer. However, this was a time when more schools were being opened rather than old schools being shut down. Therefore, we should not expect any discontinuities in the number of older schools, and they may serve as a falsification test in this context. Appendix Figure A.11 shows the lack of a discontinuity in older schools, both for government and private schools.

6.2 Private Schooling Response

How private schools respond to such interventions is crucial for determining the overall benefits. An expansion in public schooling may lower the competitive price that private schools can charge and price out the less efficient private schools. However, it is also possible for them to enter given the likelihood of peer effects, and changes to the local economy and infrastructure with such a large-scale program. In the bottom panel of Figure 3, there is no evidence of crowd-out. If anything, there is mild evidence of crowd-in in 2005, which declines rapidly in the later years as funding is phased out.

[48]The size of the discontinuity and trend over time are robust to the choice of the bandwidth. In Appendix Figure A.10 I show alternative versions where I plot the total schools per capita. The lower panels show the RD coefficients for different types of optimal bandwidths (CCT - Calonico et al. (2014b), I&K - Imbens and Kalyanaraman (2012)), and by also restricting the bandwidths to be the same as in the first year of the data. This was a rapid period of school building, affecting the sample size from year to year. As the sample grows, the bandwidths get smaller, so restricting to the same bandwidth as the first year, shows the results for a balanced panel of districts.

What then drives the crowd in? On the one hand, the demand externality could raise the equilibrium price and pulls in private schools; on the other, the fall in operating costs may induce private school entry and lower the equilibrium price. In Section 2.3.2 I discuss how we can determine which of these mechanisms is stronger by seeing how the price changes. In Section 6.6 – and specifically Table 7 – it is clear that household expenditure on schooling falls suggesting that the cost-reduction mechanism is stronger.

In Section 6.8 I look at what drives this cost reduction. Figure A.17 shows that the initial increase in the supply of female teachers and teachers with a college degree may be an early driver of these effects – but they die out quickly.

The school building results, therefore, indicate that more government schools were built in DPEP areas, and there is no evidence of a crowd-out of private schools. We should expect that this would then increase the access to schooling for households in treated districts, and lower the marginal costs of going to school. A lower marginal cost will then lead to more years of schooling attained by the households.

6.3 Education and Earnings

The DPEP program was specifically targeted towards the primary and upper primary levels, and we may expect the largest impacts at those levels. Any student who was 19 at the time of the program (or 35 at the time of the survey) should be unaffected by the program.

I check for a lack of a discontinuity in schooling attainment at the cutoff for persons above the age of 35, using the same RD methods. The left panel of Figure 4 shows how the older populations do not have any discontinuities in literacy, probability of finishing primary school, or upper primary school. The tables discussed below will also show there is no economically or statistically significant discontinuity in educational outcomes for older populations.

Looking at the younger population in the right panel of Figure 4, one can see discontinuities in different levels of education. Appendix Figure A.12 shows the analogous figures for the full sample, rather than the sub-sample of those who reported earnings. The 2SLS-LATE numbers are shown in Table 5. The young have 0.57 more years of schooling in regions that were just eligible for the program. There is no statistically significant discontinuity in the older population.[49] The policy, therefore, directly affected cohorts that were young enough to change their schooling attainment, and had no impact on the education of older cohorts.

Figure 5 and Table 3 show the RD impacts on education and log earnings for those who reported earnings, across the different bandwidth selection procedures and age groups. After scaling up the ITT estimates by the probability of treatment, the 2SLS increase in the years of education

[49] As a robustness check, I restrict the sample to only those districts that had DISE school-level data. The results are seen in Appendix Table A.21 where the estimates are slightly larger, and more precisely estimated.

is 1.6 years, and younger students in regions eligible for the program had a 0.129 percentage point increase in the likelihood of finishing primary school.

In terms of earnings, there was an increase of about 0.25 log points for the younger cohorts (Table 3). Even though older populations had no discontinuity in education, their earnings are lower by about 0.21 log points due to the GE effects. As we would expect, I find that the general equilibrium effects are largest for close substitutes, like cohorts that were close to the treated cohorts. In Appendix Table A.9, the sample is broken up into more age groups, and even though they are imprecisely estimated, there do seem to be larger effects on the 36 to 45-year-olds which is the closest age group to the treated cohorts.

Comparing Tables 5 and 3, it is clear that the impact on education is higher for the sub-sample that reported earnings.[50] However, as the top panel of Appendix Table A.10 shows, there is no discontinuity in the probability that earnings are reported at the cutoff. This suggests that DPEP did not lead to differential selection into the group of persons reporting earnings.

The difference in the educational impacts between those that reported earnings and the full sample can be tied to the difference in labor market returns by gender. Only one-fourth of the sample reported earnings. One of the strongest predictors of whether earnings are reported is a person's gender, with males having a higher proportion of reported earnings. Persons who are engaged in domestic work, and this is mostly women, are least likely to report earnings. Since men are more likely to be in occupations that report earnings, and gain from education, while women are more likely to be engaged in domestic work, we should expect men to be more responsive to these interventions (Dreze and Sen, 2002; Kingdon, 1998).

Indeed, in Appendix Tables A.11 and A.12, I find that the effects are concentrated among males, which is similar to most of the related literature (Ashraf et al., 2015; Breierova and Duflo, 2003), and other work on this program Jalan and Glinskaya (2013). In the full sample, men increase their years of education by about 0.9 years, whereas women increase theirs only by 0.2 years. For the sub-sample of those who report earnings, however, the impact on education is similar in magnitude, but more precisely estimated for men. There is also little to no change in the earnings of women, even though men's earnings do rise.

Table 4 looks at the fraction of people who have completed at least a given level of education. Since the program was targeted to the primary and upper-primary sections, the biggest increases are seen here. For the sub-sample that reported earnings, literacy rates are higher by 6 percentage points, and the likelihood of finishing primary school is higher by 12 percentage points.[51]

[50] This is similar to Duflo (2001).

[51] As another robustness check I collapse all the household data into district-age cells, and re-run the regressions. The results do not change, as can be seen in Appendix Table A.14. Collapsing the data, however, is not recommended, since we are losing valuable information about the variability in the outcomes that may be different on each side of the cutoff – the optimal bandwidth procedure utilizes this variability.

I also perform a difference-in-differences (DiD) analysis that estimates a different parameter – the average effect on treated districts. In Appendix Table A.13, I compare older cohorts to younger cohorts and DPEP districts to non-DPEP districts. For the full sample, the years of education are higher by 0.39 years, and for the sub-sample of those who reported earnings education is higher by 0.46 years and log earnings increase by 0.065. The education increases being smaller than the RD estimates suggest that districts further away from the cutoff did a relatively poor job of implementing the program.

6.4 Returns to Education: Conventional Instruments

In my sample, a simple OLS regression of log earnings on years of education and a quadratic age profile, yields a Mincerian "return" of 10%. Instrumental variable (IV) estimates, however, will estimate a 2SLS-LATE weighted by the probability of being induced into getting more education by the instrument. Card and Lemieux (2001); Imbens and Angrist (1994); Oreopoulos (2006) discusses why IV estimates of the returns to education are larger than their OLS counterparts. In general, we may expect this to be the case since a reduction in marginal costs that affects all students equally will induce those with higher returns into getting more education.[52] Another possibility is that OLS estimates suffer from measurement error (Griliches, 1977).

One IV method to estimate the returns to education is to simply use the RD cutoff to first estimate the change in the years of eduction for the younger cohort, and then the corresponding change in log earnings for the same cohort. By taking the ratio of the change in log earnings to the change in years of education, one can find an estimate of the returns to schooling. Under the assumption that the policy only induces some younger workers to get more education, this method will identify the change in earnings due to an additional year of schooling, for this marginal group. On the other hand, as the model shows, the policy should simultaneously affect both the skill premium and the overall output in the district. Since the change in the average earnings is not just driven by the switch in the fraction of students from unskilled to skilled groups, but also by the changes in earnings of skilled and unskilled workers, the estimated individual returns would be conflated with the changes in output and the skill premium.[53]

The estimates in Table 3 can be used to calculate the returns taking the ratio of the change in log earnings and the change in years of education. The ratio of 0.25 log earnings and 1.65 years gives us a return of about 15.5%.[54] However, due to the size of the confidence intervals, this number is not statistically indistinguishable from numbers as low as 7% and certainly not from the OLS estimated Mincerian returns of 10% estimated in this sample. This estimate, therefore,

[52]See Carneiro et al. (2011) for a nuanced alternative interpretation based on the generalized Roy model.

[53]From equation (22) we know: $log\frac{w_{y,D=1}}{w_{y,D=0}} = \ell_{sy,D=1}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=1}log\frac{w_{uy,D=1}}{w_{uy,D=0}} + \Delta\ell_{sy}log\frac{w_{sy,D=0}}{w_{uy,D=0}}$. For changes in partial equilibrium, $log\frac{w_{uy,D=1}}{w_{uy,D=0}} = log\frac{w_{sy,D=1}}{w_{sy,D=0}} = 0$, and the change in average earnings across the RD cutoff recover the returns to skill $log\frac{w_{sy,D=0}}{w_{uy,D=0}}$ for the compliers $\Delta\ell_{sy}$.

[54]Appendix Table A.18 shows the 2SLS-LATE version of this exercise, where the first-stage is the change in the years of education rather than the probability of receiving DPEP funds.

lies reasonably within the range of comparable estimates found in the literature (Banerjee and Duflo, 2005; Psacharopoulos and Patrinos, 2004). One of the most recent experimental estimates of returns in a developing country is 13% (Duflo et al., 2017).[55]

Another IV method is to use a difference-in-differences (DiD) strategy. In Appendix Table A.22 and Figure A.14, I compare DPEP districts to non-DPEP districts, and the older cohorts to the younger cohorts.[56] I estimate the difference-in-differences coefficient for three different subsamples. For the full sample, there is an increase in 0.3 years of education, and a 5.5 percentage point increase in the literacy rate. There is also a 3.8 percentage point increase in the likelihood of finishing primary school. The estimates are similar even when restricting the sample to be in the neighborhood of the RD cutoff, and around the cohort-cutoff. For the subsample that reported earnings, there is also a statistically significant increase in earnings. The 2SLS IV-LATE returns can be estimated by taking the ratio of the change in log earnings and the years of education. This 2SLS return is 15.9%, which is statistically and economically indistinguishable from the RD-2SLS return of 15.5%.

The difference-in-differences strategy, however, already accounts for some portion of the GE effect. Portions of the change in average earnings due to an increase in output, and the GE effects that affect older cohorts are differenced out. It is, therefore, impossible to estimate the overall GE effect using the DiD method without additional assumptions. It is, however, possible to measure the 'additional GE on the young' component by looking at how the skill-premium changes differentially for younger rather than older cohorts. This component depresses the returns to being skilled by about 7.9 percentage points (Appendix Table A.22).

6.5 Returns to Education and the Labor Market GE Effects

As pointed out in the model section, the method of taking the ratio of the younger cohort's change in earnings and years of education is confounded by the fact that earnings are affected by the general equilibrium effects in the local economy. Average earnings of all persons in treated districts are affected by changes in overall output. At the same time, the change in the skill distribution and the adoption of skill-biased capital affects the earnings skill premium, as captured by Equation (19). While older cohorts are affected by the change in the aggregate skill distribution and inflow of skill-biased capital, younger cohorts are additionally affected by the change in the cohort-specific skill distribution for the young.

[55] A survey by Psacharopoulos and Patrinos (2004) finds Mincerian returns higher in low-middle income countries. In Asia these are near 10% and the returns to finishing primary schooling are around 20%. Banerjee and Duflo (2005) update this exercise, and document a range of Mincerian returns from 2.7% to 35.3%.

[56] For person i in age cohort a and district d, the following difference-in-differences regression was estimated:

$$y_{ida} = \beta_{DiD} T_{da} + \mu_d + \varpi_a + \epsilon_{ida} \;, \tag{27}$$

where μ_d is a district fixed effect, ϖ_a is a cohort fixed effect, and $T_{da} = 1$ if the individual lives in a DPEP district and is young enough to be affected. Under the usual parallel trends assumption, β_{DiD} is the difference-in-differences parameter.

Given these general equilibrium effects it is necessary to use the method outlined in Section 3.1.1 and specifically, Equations (22) and (23) to derive the returns to education with and without the labor market general equilibrium effects. Table 6 estimates the returns by dividing the population into these skilled and unskilled groups. I define skilled workers as those having finished upper primary school as the policy was targeted at getting students through this level of schooling, and because the largest earnings increase in OLS regressions on untreated districts comes when a student finishes upper primary school.[57] There was a 17 percentage point shift into the skilled category across the cutoff.

The estimated returns to shifting into the skilled group in the absence of GE effects are 19.9%. The returns to being skilled with the GE effects, however, are only 13.4%. This constitutes a 32.5% decrease in the returns attributable to the GE effects. This change in the skill-premium can be split up into the portion that affects all cohorts, and the additional impact only on the young. 91.87% of change in the GE effects are explained by the 'additional impact on the young' term. The GE effect that affects all cohorts may be small because the two components that determine this effect may counteract each other – an increase in the relative supply of skilled workers will tend to lower the skill premium, but adoption of skill biased capital may increase this skill premium. Furthermore, the additional impact on the young term may be high because the young and the old may not be close substitutes in production.

6.6 Total Output, Consumption, and Educational Expenditure

The change in overall output depends on the productivity of the different skill levels and the shift in the labor force from one skill level to another. As workers acquire more skill, and/or if skill-biased capital is adopted by the region, overall productivity and output in the region may increase. In the top panel of Appendix Figure A.16, one can see the impact on total output (the District Domestic Product). These regressions are underpowered, and the standard errors are quite large. The point estimates indicate that between 2000 and 2006, the increase in GDP associated with the policy was between 0.137 and 0.19 log points (Appendix Table A.24).

The change in total output will lead to a change in total consumption. In the top panel of Table 7, one can see that the change in consumption expenditure in the last year of the policy (2004-5) was about 0.17 log points. At the same time, in 2004 the money spent for educational purposes (tuition, fees, books and stationery) falls by about 0.21 log points.[58] In the bottom panel of Appendix Figure A.16 one can see the discontinuity in education expenditure. This fall in educational expenditure is persistent even five years after the program ended in 2009. The decline in total expenditure on education-related items is driven largely by lower expenditures on school tuition and fees. There is, in fact, an increase in expenditure on other education-

[57] In going from literate-below primary to finishing primary school, average earnings increase by 10%, whereas in going from primary to upper primary school average earnings increase by 20%.

[58] Consistently, Jalan and Glinskaya (2013) measure a 20-40% fall in household educational expenditure.

related items, like books and stationery (Table 7), since expenditures on books and stationery can rise when households gain more education, as these are complements in consumption.

Changes in consumption and the costs of education will directly impact overall economic benefits. The increase in output and consumption benefit all cohorts, whereas the fall in the costs of education benefit households with younger cohorts who attend school. The fall in the costs of schooling even benefit those who are *not* induced into getting more education – at an extreme, policies that successfully reduce the costs of schooling can have significant economic benefits for the infra-marginal students even as they do not change the years of education.

6.7 Firm and Worker Productivity & Adopting Capital

Local economies at the district level that received educational funds for at least a decade witnessed a transition in the skill level for younger cohorts in their workforce. For this to have happened, any combination of the following four things may have taken place. First, skilled workers may have migrated out, dampening the portion of the GE effects that depends on the change in the aggregate skill distribution. Second, existing firms may have switched the composition of their workforce by hiring more skilled workers. Third, new firms may enter and hire these skilled workers. Last, workers may have utilized their increase in skill and adopted newer technologies in production. The adoption of skill-biased capital, therefore, will increase the returns to skill in treated districts and be a crucial determinant of the GE effects.[59]

To test the first possibility about worker migration, I assemble the 2007-8 round of the NSS household survey which asked detailed questions on migration. Permanent worker migration is extremely low in the Indian context (DasGupta, 1987; Munshi and Rosenzweig, 2009; Topalova, 2005).[60] It is, therefore, unlikely that those who acquired skills migrated out of these districts. By analyzing the NSS 2007-8 waves, we can see that of all the households that reported having any migrants across districts, only 30% of the migration was work related, whereas more than half were for marriage reasons. Appendix Table A.10 supports the claim that the policy did not impact migration. There are no economically significant changes to the number of out-migrants or the number of households that migrated to that district.[61]

[59]Regions around the RD cutoff are geographically dispersed, so it is less likely that the migration of firms or workers happens among regions near the cutoff.

[60]Many studies on India are explicit about ignoring migration in the main analysis as the numbers are low (Anderson, 2005; Banerjee et al., 2008; Foster and Rosenzweig, 1996). Munshi and Rosenzweig (2009) show that for a sample of rural males aged 20-30, the permanent migration rate outside their village was 8.7%, a lot of which may have taken place within the same district. Deshingkar and Anderson (2004) also show that rates of rural-urban migration are much lower in India than in comparable countries, and Munshi and Rosenzweig (2015) show that male worker migration is extremely low despite the presence of large wage gaps across regions. One possible reason lies in the uncertainty related to getting work at the destination and the fixed cost of migrating (Bryan et al., 2014). Duflo and Pande (2007) argue that the district is the relevant local labor market in the Indian context, and workers of different skills can find employment elsewhere in their own district.

[61]For example, the total number of out-migrants ranges between 4.2 and 10.9 persons per district - this includes migrants for any purpose (like marriage, education, temporary work, etc.). It is not possible to find RD estimates by finer skill groups or age cohorts since almost nobody is migrating in the data.

On the other hand, firm capital is relatively more mobile in India (Ghani et al., 2015). I compile data from the Annual Survey of Industries (ASI), which is a census of all manufacturing firms. The results for these are shown in Appendix Figure A.15, where one can see that even at the manufacturing establishment level, the average wage paid to workers increases as educated workers join the labor market, around the year 2004. Furthermore, I classify firms based on their products as 'high-skill' firms. The figure shows that there is a steady increase in the fraction of firms that produce more mechanized products. This is suggestive of the fact that either existing firms shifted production and employed more high-skill workers, or newer firms entered and hired these skilled workers. Both findings are suggestive evidence in support of the adoption of skill-biased capital into these regions.

One relevant question is whether this capital was previously being utilized in other forms or is flowing from other regions, and in the absence of the policy would it have gone to regions that lie just on the other side of the cutoff. If this is indeed the case, then it would attenuate the GE effects on earnings. It is, however, unlikely that regions *just* above the cutoff receive less capital due to the policy. Policy regions are geographically dispersed all over the country (Figure A.9) rather than being neighbors of districts just on the other side of the cutoff. In Figure A.24, I look at the density of capital-intensive firms in the early period and the late period for the part of the country that should not have received the policy. Regions near the cutoff (normalized to 0), if anything, have an increase in the firms involved in mechanized production and providing higher compensation. On the other hand, regions with high female literacy – often the major cities – show a mild decrease, supporting anecdotal evidence of people residing in major cities investing in villages that they originate from.

In general, there are some clear changes to the labor market for the workers in these regions. The bottom half of Appendix Table A.10 shows that the probability of being paid monthly (as opposed to daily) is higher, and the fraction unemployed is lower in the treated regions. The last possibility, that workers adopted newer technologies given their increased levels of education is, therefore, possible in this context (Foster and Rosenzweig, 1996).

6.8 Teachers, Infrastructure, and Other Funding

While the primary focus of the program was to increase educational attainment by building schools and hiring teachers, there may have been improvements in quality given such a large amount of funding. Such improvements may have increased the returns to schooling in the labor market, attenuating the GE effects on the returns. In Appendix Table A.23 I use a relatively recent dataset – known as the Annual Status of Education Report (ASER) data. This is geographically the most comprehensive test-score dataset in the country. I consider six different test score variables, and only one of them shows a statistically significant increase – being able to identify numbers between 1 and 9 has a 5 percentage point increase at the cutoff. This is, at best, mild evidence of better test scores that may attenuate the estimated

GE effects. On the other hand, better 'quality' in terms of better infrastructure may have made it easier for students to finish a grade and further lower the marginal costs of schooling. In this subsection, I explore how various inputs at the school level were changed around the RD cutoff. Furthermore, I can study what happens when the DPEP funding dissipates over time.

Card and Krueger (1992) show that more qualified teachers and female teachers have important impacts on schooling in the US. In India, female teachers may also encourage female student enrollment and are, therefore, important. In 2005, when program funding was still high, the number of college-educated teachers and the number of female teachers in DPEP districts was higher. However, once the funding is no longer targeted to DPEP districts, this discontinuity dissipated over time (Appendix Figure A.17). A lack of targeted funds may have led to a relative slowdown in the hiring of teachers.[62]

Tangible infrastructure in schools, however, seems to last even when the DPEP funding is reduced (Appendix Figures A.17 and A.18). Drinking water, electricity, and library books are consistently higher in regions that received the DPEP (Appendix Figure A.22). Infrastructure upgrades like girls' toilets may be important in getting girls to school (Adukia, 2016). While there was constant funding, there were substantially more facilities for girls, and there is a slight dissipation as the funding is stopped. Other inputs, such as medical checkups, are also consistently higher for schools in DPEP districts.

The condition of the classrooms also seems to have deteriorated over time once funding was stopped. While, in 2005, schools in DPEP regions had a lower number of classrooms needing some repair, over time more of these classrooms broke down (Appendix Figure A.18). These results indicate that a constant source of funding may be needed to keep the rooms in good condition. In Appendix E I discuss other changes like the crowd out of other funds, the construction of pre-primary sections, the establishment of resource centers, school inspections, and the medium of instruction.

6.9 Overall Economic Benefits

Increases in overall output and reductions in the total cost of schooling will benefit households. The change in labor market earnings depend on the returns to skill and the GE effects on these returns. Table 6 shows the returns by skill group, which helps back out the parameters and the changes in yearly labor market benefits shown in Table 8. For these calculations, the average real interest rate during that period was used (5%). A gap of 10 years is assumed between the time the costs of education are borne and the labor market returns are realized.[63]

[62] The dissipation in the discontinuity does not imply that teachers left DPEP districts – it may be the case that non-DPEP districts hired teachers at a relatively more rapid rate once DPEP funds were gone.

[63] The average real interest rate comes from the World Bank. Changing the interest rate or the gap of 10 years does not affect the percentage change in welfare due to the GE effects, only the levels.

In the top panel of Table 8, I present the results for those in the younger cohort who were induced into getting more skill because of the policy. This is about 17% of the young population. Their welfare increases by 0.121 log points, and the GE effects depress this increase in welfare by 23.3%. At the same time, workers who were always going to acquire skill even in the absence of the policy are worse off by 0.037 log earnings points, whereas workers who were always going to be unskilled are better off by 0.014 log earnings points.[64]

Since unskilled workers are better off and skilled workers are worse off, it is also possible to estimate the transfer in labor-market benefits from the skilled to the unskilled due to the GE effects. Among the older cohorts this transfer is 0.004 log points, and among the young it is 0.05 log points. This indicates that purely when looking at labor-market benefits, those persons who were always going to be skilled even in the absence of the policy, actually lose out, whereas those who were never going to acquire skill even in the presence of the policy benefit.

To measure the change in lifetime welfare for students induced into getting more schooling, I compare the cost of an additional year of schooling to the benefits in the bottom panel of Table 8. These costs include not just the tuition fees but also the opportunity cost of a foregone unskilled wage. The benefits, however, are the present discounted value of the skilled earnings stream. All cohorts and skill groups benefit from increases in the overall output. Furthermore, the young cohorts who acquire skill, benefit from the reductions in the total costs of schooling. Even young students who were always going to attend school even in the absence of the program benefit from the reductions in schooling costs.[65]

7 Conclusion

Large-scale education investments can and do generate substantive general equilibrium effects in the labor market and the education sector. Bringing together a school-level dataset, census data, household surveys, and firm-level data, I perform an intensive analysis of the DPEP program, which measurably increased educational inputs and increased the years of education and earnings for students. With the help of the policy, I estimate the parameters of a general equilibrium model using a RD approach. The estimates imply that the return to acquiring skill is 13.4%, but that it is 6.5 percentage points lower than it would be in the absence of general equilibrium effects. There are also large distributional effects, where labor market benefits are transferred from the skilled to the unskilled, especially among the young. High-skill workers

[64] Note that these results focus on labor-market benefits. A policy such as this should also change the prices of non-tradables, like land, affecting the welfare of non-workers as well. Given the scant number of land transactions in the data, there is no discernible effect on land prices.

[65] These results do not necessarily indicate that the policy was cost effective. I have shown that the direct impacts were concentrated on men that reported earnings, and only for certain cohorts. In other results I find that the impacts were mostly restricted to treated districts that had relatively high literacy rates. The interventions had low persistence as well. Given the large amounts of funds invested, the overall cost effectiveness of this policy is questionable, and is left for future research.

who would have acquired skill even in the absence of the policy lose out in terms of labor market earnings. Overall welfare, however, is higher, driven by decreases in the household's costs of education and increases in output in the local economy.

These findings have two important implications. First, we should take care when extrapolating the benefits demonstrated by small-scale schooling interventions, as scaled up versions of such interventions may have GE effects. Second, using large-scale variation to estimate the returns to education may conflate the individual returns and the general equilibrium effects. This is because an experiment where a single individual receives more education is inherently different from the variation induced by changes in policies like nationwide tuition subsidies, schooling expansions, compulsory schooling laws or other regional variation.

One limitation of my study, however, is that the estimates are not generalizable to regions further away from the RD cutoff in the absence of stronger assumptions. Indeed, as my difference-in-differences results indicate, the policy was poorly implemented at the districts with the lowest literacy rates, suggesting that my RD results should only be thought to apply to the districts near the cutoff.

While there was a larger fraction of new schools built under these policies, over time other regions caught up once the funding was cut. Within schools, there were lasting impacts, however, on physical infrastructure such as electricity and drinking water. As the funding was cut, the gap in the number of more qualified teachers across eligible and ineligible districts dissipated, and the condition of classrooms deteriorated over time. In light of these results, it is reasonable to propose that if policy makers wish to retain teachers, then the regions would require a constant source of funds over a long period.

The debates about the role of the government in health and education investments usually center around the economic benefits of the policy. I show that economic benefits to household depend on a few crucial factors — the costs of education, the labor-market returns to education, and importantly the general equilibrium changes in earnings. While these are sufficient in capturing the direct economic benefits, more education can have other welfare consequences as well. For instance, more education can lead to better health or more informed political participation (Sen, 1999). Exploring these relationships is left for future research.

Identifying who benefits and who does not in the universe surrounding such a policy, and what works and what does not is key to making such large-scale infrastructure investments more targeted and effective. The results in this paper, however, help explain why scaled up government policies may have different impacts than researcher-led micro interventions (Acemoglu, 2010; Deaton, 2010). In light of these results, it is clear that understanding all the consequences of large general equilibrium effects is crucial for both researchers and policy-makers when considering nation-wide interventions in public policy.

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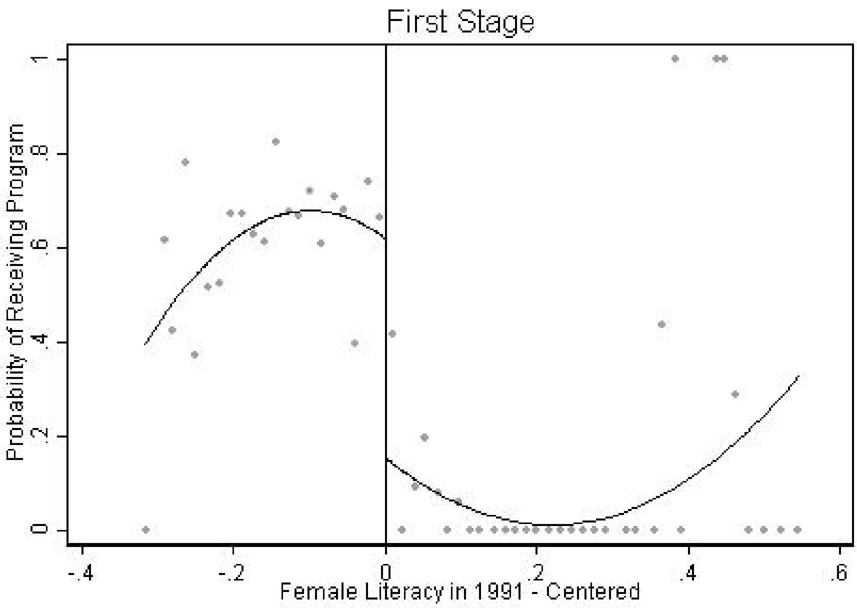
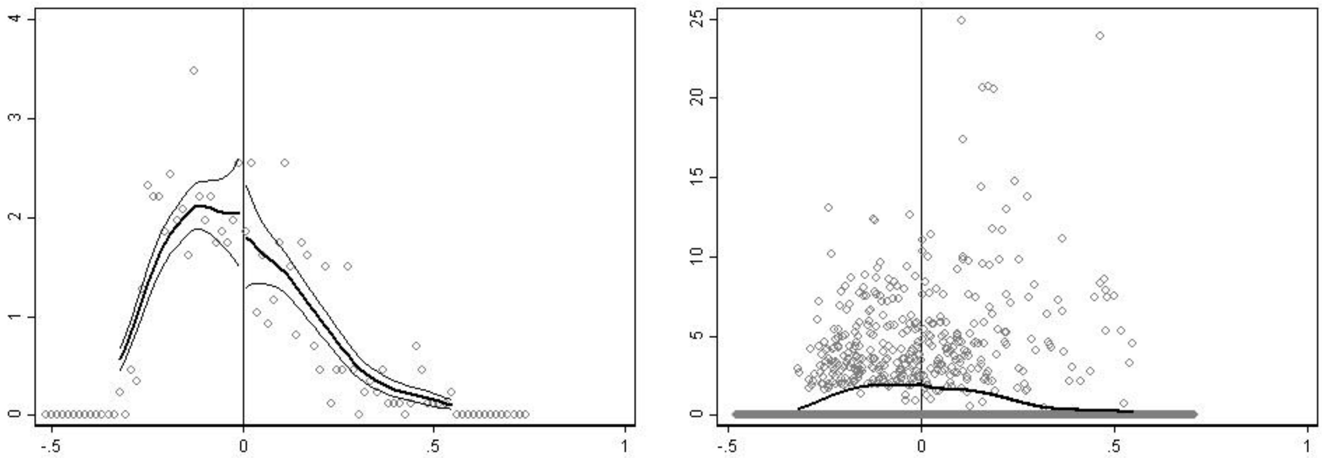
I Figures

Figure 1: First Stage of DPEP



First stage graphs showing probability that a district received DPEP funds. Optimal bin sizes calculated using Calonico et al. (2014b) method.

Figure 2: McCrary Density Tests

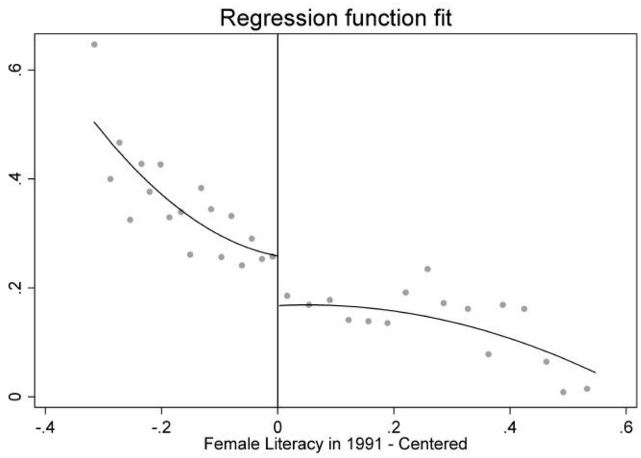


District Level McCrary Test

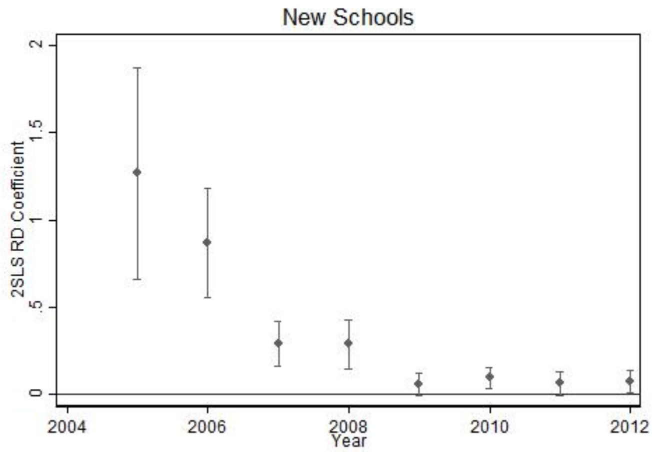
Household Level McCrary Test

McCrary (2008) tests for discontinuity in density at the cutoff. These tests look for evidence of one-sided manipulation of the running variable by testing the discontinuity in the density at the RD cutoff.

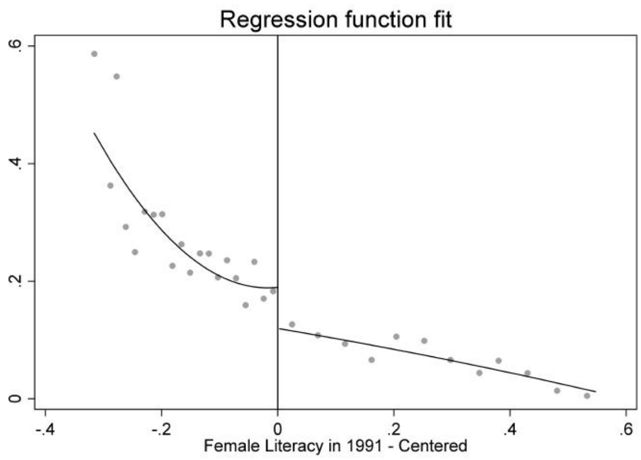
Figure 3: Fraction of Schools Built Post 1993



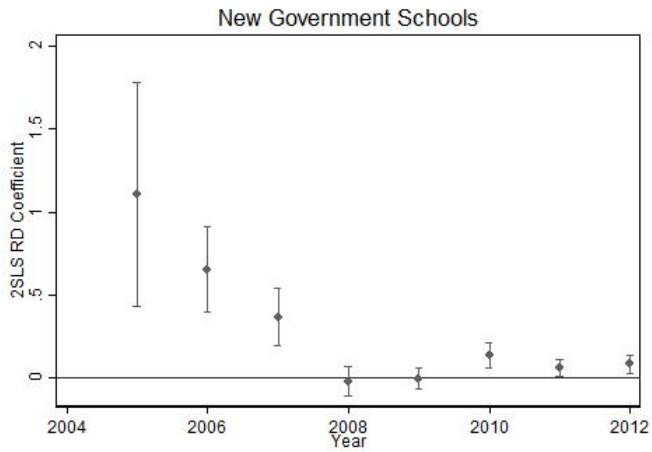
Fraction of All Schools Built Post 1993



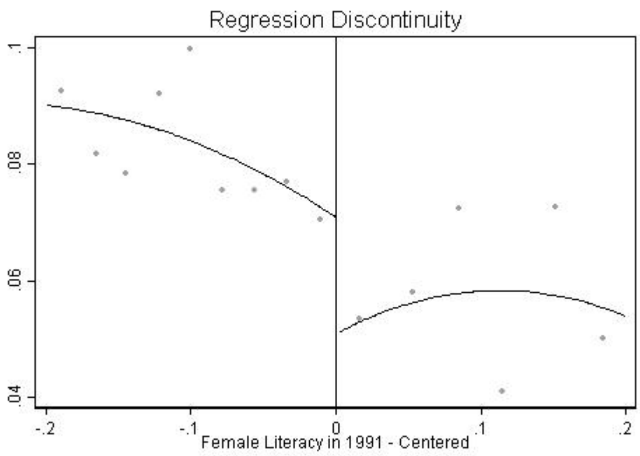
All Schools - RD Coefficient Over Time



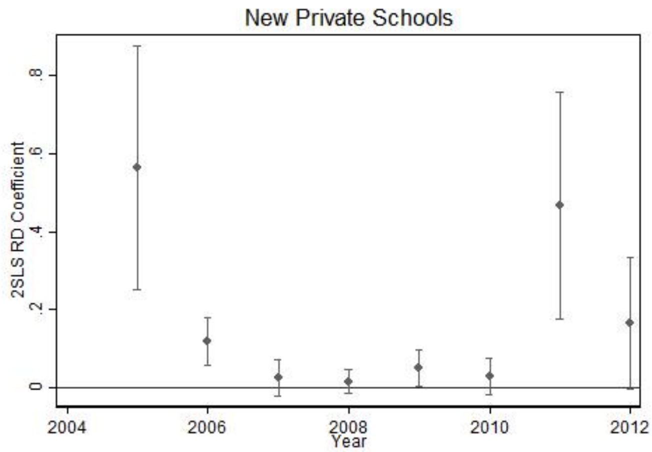
Fraction of Government schools Built Post 1993



Government Schools - RD Coefficient Over Time



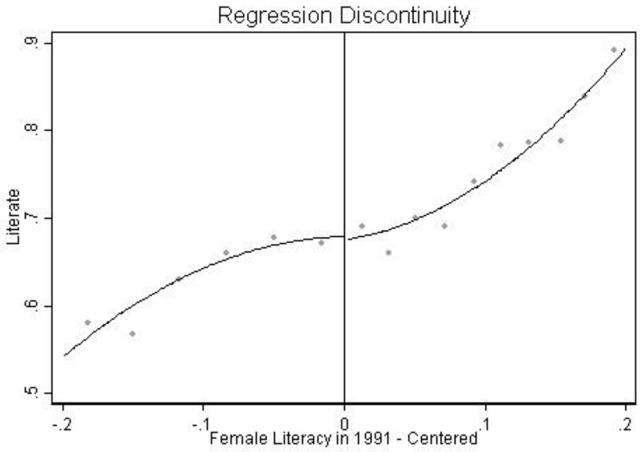
Fraction of Private schools built post 1993



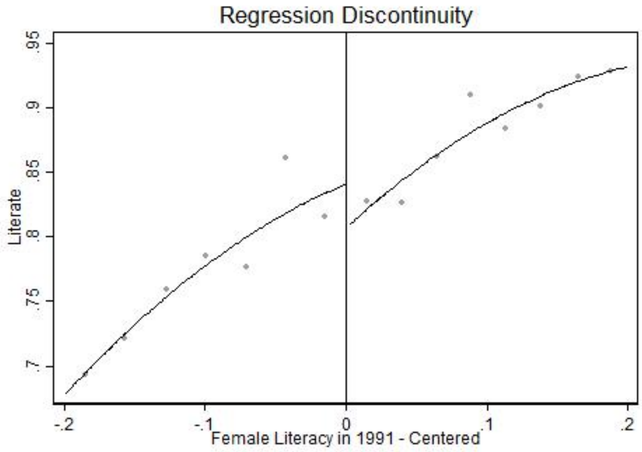
Private schools built post 1993

Source: DISE (District Information System for Education) data. Top panels show RD graphs using the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

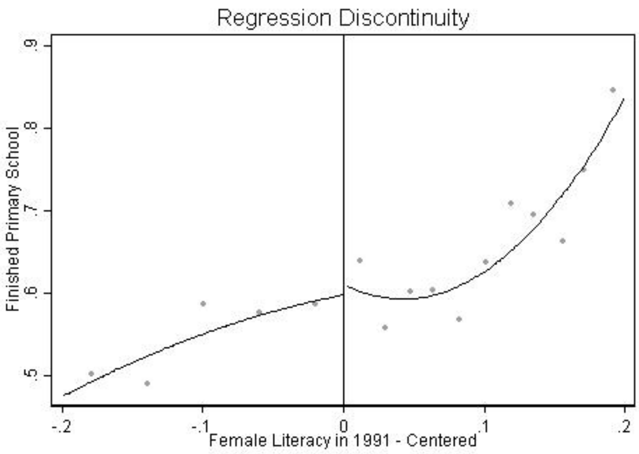
Figure 4: RD figures - Levels of Education



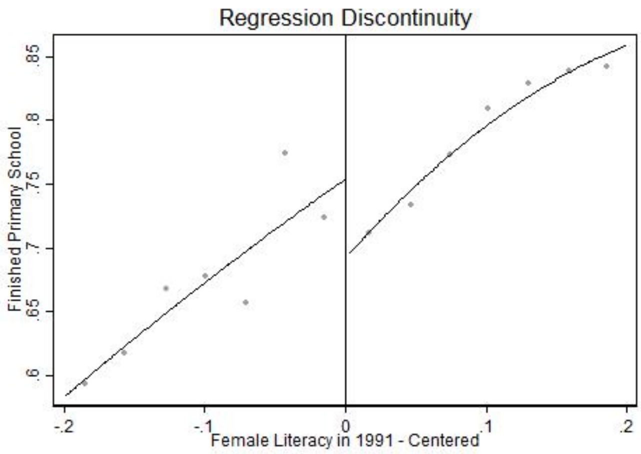
Literate - Older (Placebo)



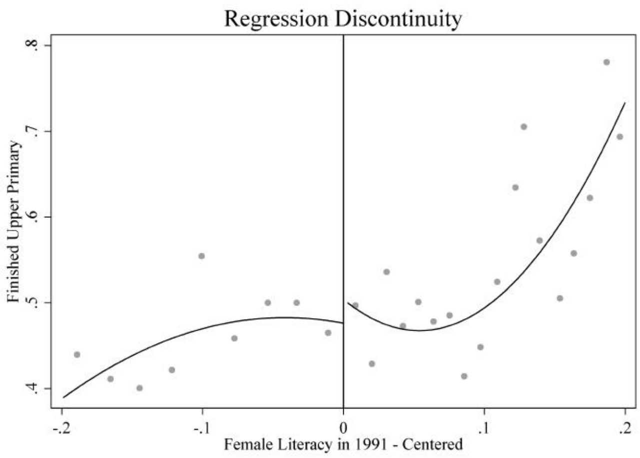
Literate - Younger



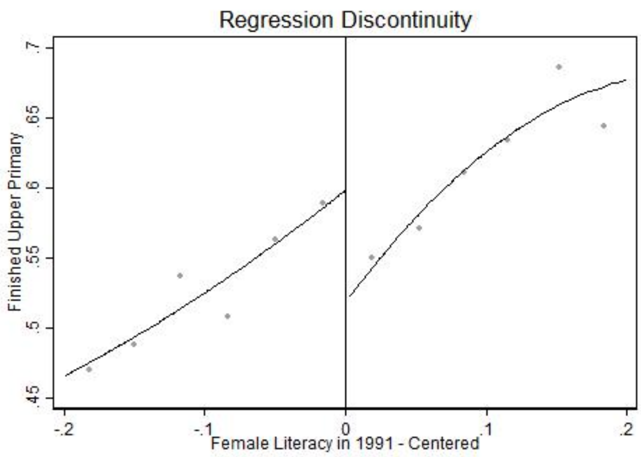
Finished Primary - Older (Placebo)



Finished Primary - Younger



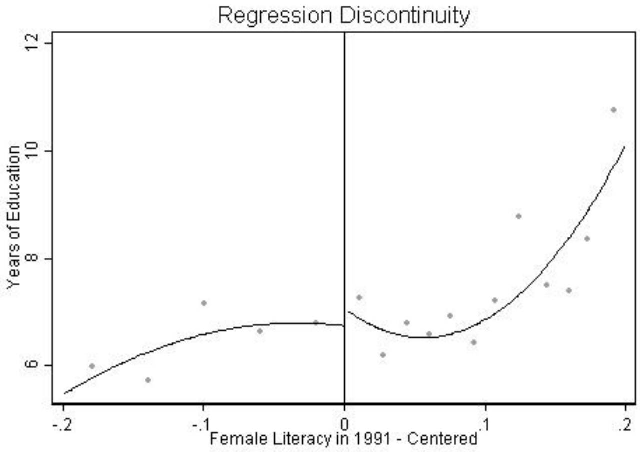
Upper Primary - Older (Placebo)



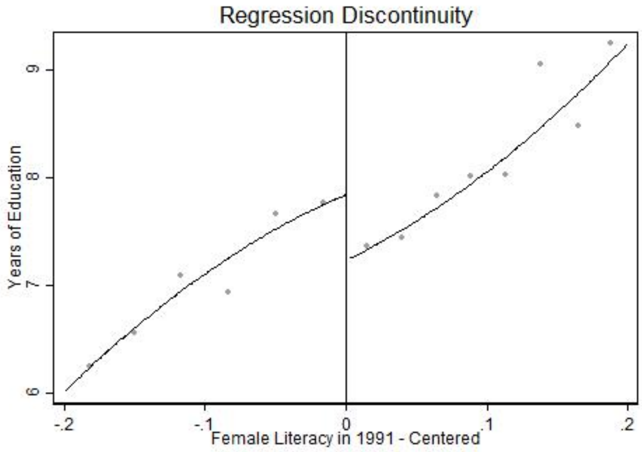
Upper Primary - Younger

National Sample Survey 2009 for persons who report earnings in primary occupation. Appendix Figure A.12 shows the analogous graphs for the full sample of persons. Figures made using Calonico et al. (2014b) method of using regression curves to approximate the conditional means on either side of the cutoffs and the equally spaced sample means, and optimally spaced bins.

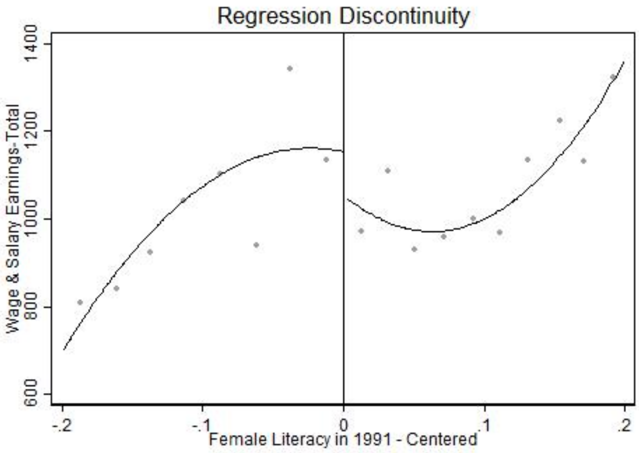
Figure 5: RD Years of Education and Earnings - Young Sample



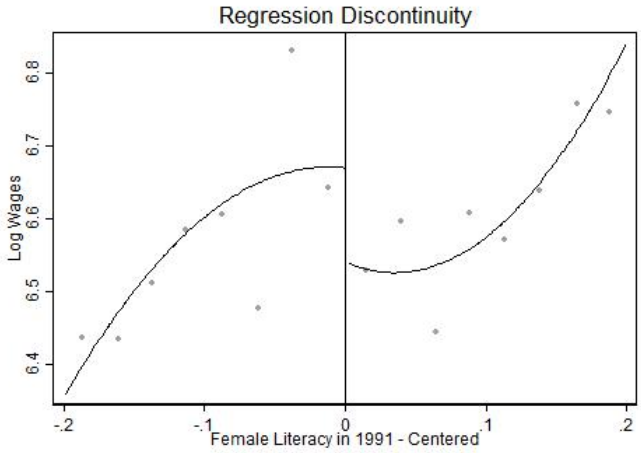
Years of Education - Older (Placebo)



Years of Education - Younger



Earnings (Rupees)



Log(Earnings)

National Sample Survey 2009 for those who reported earnings. Figures made using Calonico et al. (2014b) method of using regression curves to approximate the conditional means on either side of the cutoffs and the equally spaced sample means in optimally spaced bins. Average exchange rate in 2009 is Rs. 40 = $1.

II Tables

Table 1: Summary Statistics: School Level (2005)

	Mean	SD
Fraction of Schools:		
Built post 1993	0.277	0.447
Gov schools built post 1993	0.200	0.400
Pvt school built post 1993	0.075	0.263
Built between 1973-93	0.227	0.419
Gov schools built 1973-93	0.170	0.376
Pvt Schools built 1973-93	0.055	0.228
Fraction of Schools Having:		
A Girl's Toilet	0.400	0.490
Electricity	0.312	0.463
Playground	0.549	0.498
Medical Checkups	0.541	0.498
Ramps	0.182	0.386
A Boundary Wall	0.506	0.500
Drinking Water	0.846	0.361
A Pre-primary section	0.213	0.410
Block and Cluster Resource Centers:		
Visits by BRC Official	1.485	2.543
Distance to BRC (km.)	13.462	15.936
Visits by CRC Official	4.496	5.612
Distance to CRC (km.)	4.438	8.689
Teacher Learning Materials Grant:		
Amount Received (Rs.)	1517.100	8010.138
Amount Spent (Rs.)	1332.604	7611.869

Source: DISE (2005). Fraction of schools are for schools that still exist in 2005. BRC is Block Resource Center, and CRC is Cluster Resource Center. All schools, regardless of DPEP status, are eligible for Teacher Learning Material Grants (TLM).

Table 2: Summary Statistics: Household Level

	Non DPEP Mean	Non DPEP SD	DPEP Mean	DPEP SD	All Mean	All SD
Finished Primary School	0.71	0.45	0.60	0.49	0.67	0.47
Finished Upper Primary	0.59	0.49	0.48	0.50	0.55	0.50
Years of Education	7.40	5.26	6.14	5.38	6.95	5.34
Male	0.50	0.50	0.50	0.50	0.50	0.50
Age	37.75	14.63	37.39	14.59	37.59	14.62
Weekly Earnings	42.17	51.29	31.55	38.50	38.92	47.43

Source: National Sample Survey (2009). Age in years. Earnings in 2005 USD, where Rs. 40 = $1.

Table 3: Education and Earnings for those with Reported Earnings

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.654	-0.381	1.569	-0.199
	(0.491)***	(0.590)	(0.417)***	(0.553)
Observations	10,175	7,997	14,277	8,630
Fuzzy Conventional p-value	0.000753	0.519	0.000168	0.719
Fuzzy CCT Corrected p-value	0.00142	0.469	0	0.217
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.129	-0.0403	0.171	-0.0536
	(0.0353)***	(0.0454)	(0.0484)***	(0.0497)
Observations	14,277	8,630	10,175	7,997
Fuzzy Conventional p-value	0.000249	0.375	0.000419	0.280
Fuzzy CCT Corrected p-value	0.00374	0.0291	0.000358	0.241
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Earnings in Rupees	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	217.6	-401.5	306.4	-327.4
	(113.9)*	(187.8)**	(99.61)***	(176.2)*
Observations	10,175	7,997	14,277	8,630
Fuzzy Conventional p-value	0.0561	0.0325	0.00210	0.0632
Fuzzy CCT Corrected p-value	0.580	0.0138	0.706	0.000419
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Log Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.256	-0.217	0.326	-0.151
	(0.0829)***	(0.105)**	(0.0703)***	(0.0988)
Observations	10,175	7,994	14,277	8,627
Fuzzy Conventional p-value	0.00197	0.0389	0	0.126
Fuzzy CCT Corrected p-value	0.0806	0.00227	0.311	0
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10, for all districts, and all persons between the ages of 16 and 75 that reported earnings. The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table 4: Fraction of People that Have Finished At Least a Given Level of Education

Literate	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0623	-0.0643	0.0655	-0.0300
	(0.0368)*	(0.0513)	(0.0289)**	(0.0383)
Observations	9,003	7,413	14,277	11,088
Fuzzy Conventional p-value	0.0906	0.210	0.0236	0.434
Fuzzy CCT Corrected p-value	0.0827	0.104	0.0417	0.00229
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Some pre-primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0622	-0.0617	0.0657	-0.0181
	(0.0373)*	(0.0513)	(0.0293)**	(0.0363)
Observations	9,003	7,413	14,277	12,625
Fuzzy Conventional p-value	0.0956	0.229	0.0250	0.617
Fuzzy CCT Corrected p-value	0.0927	0.121	0.0220	9.12e-05
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.121	-0.0616	0.139	-0.0288
	(0.0445)***	(0.0491)	(0.0390)***	(0.0419)
Observations	9,273	7,869	11,972	9,920
Fuzzy Conventional p-value	0.00663	0.209	0.000354	0.493
Fuzzy CCT Corrected p-value	0.00747	0.117	0.000443	0.0815
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Upper-primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.167	-0.0557	0.170	-0.0291
	(0.0518)***	(0.0509)	(0.0485)***	(0.0430)
Observations	9,045	7,729	10,175	9,920
Fuzzy Conventional p-value	0.00129	0.274	0.000443	0.499
Fuzzy CCT Corrected p-value	0.000798	0.230	0.000240	0.0250
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for persons between 16 and 75 years of age that reported earnings.
The sample of ‘Below 35 years’ are of school going age during the policy, whereas those ‘Above 35’ are too old to change their schooling in response to the policy.
Bandwidths: ‘CCT’ is the Calonico et al. (2014b) method. ‘I and K’ is the Imbens and Kalyanaraman (2012) method.
‘CCT corrected p-value’ is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table 5: Education Changes - Full Sample

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.573	0.279	0.571	0.303
	(0.190)***	(0.242)	(0.185)***	(0.224)
Observations	61,787	34,119	65,650	41,893
Fuzzy Conventional p-value	0.00253	0.249	0.00205	0.175
Fuzzy CCT Corrected p-value	0.00175	0.168	0.0246	0.103
Bandwidth selection procedure	CCT	CCT	I and K	I and K
Finished Primary School	**Below 35 years**	**Above 35 years**	**Below 35 years**	**Above 35 years**
RD Estimate	0.0365	0.0252	0.0574	0.0263
	(0.0191)*	(0.0226)	(0.0154)***	(0.0218)
Observations	39,326	36,584	68,050	40,068
Fuzzy Conventional p-value	0.0562	0.264	0.000185	0.227
Fuzzy CCT Corrected p-value	0.130	0.419	0.0840	0.404
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10, for all districts, and all persons between the ages of 16 and 75 (including those who did not report earnings). The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table 6: Returns, and Wage Parameters

	Fraction Switched	Change in Returns $\Delta\beta$	
Estimate	0.173	-0.065	
SE	(0.059)	(0.030)	
	Returns without GE $\beta_{y,D=0}$	**Returns with GE $\beta_{y,D=1}$**	**% Change in returns**
Estimate	0.199	0.134	-32.5%
Bootstrapped p-val	[0.055]	[0.098]	
	Change for older cohorts	**Additional on Young**	**% Change on young**
$\Delta\beta$	-0.0053	-0.0594	91.87%

National Sample Survey 2009-10. The estimation follows the procedures described in the Model section 2, and detailed in Appendix B.IV, specifically Equations (19), (22) and (23).
Younger cohorts are those between 17 and 35, whereas older cohorts are between 36 and 50.
P-values for returns with GE $\beta_{y,D=1}$ and returns without GE $\beta_{y,D=0}$ were bootstrapped using 1000 draws of sampling with repetition. The null was created by jointly permutating the RD running variable, treatment status and probability of treatment.
The results in this table further suggest that the elasticity of substitution across age-cohorts is approximately $\sigma_A = 5$, and in the absence of adoption of additional skill-biased capital the elasticity of substitution across skill groups would be $\sigma_E = 4.24$.

Table 7: Household Expenditures

Log(Consumption Expenditure)	2004-5		2009-10	
RD Estimate	0.179	0.172	0.405	0.112
	(0.0372)***	(0.0334)***	(0.0682)***	(0.0322)***
Observations	27,372	33,758	12,563	26,420
Fuzzy Conventional p-value	0	0	0	0
Fuzzy CCT Corrected p-value	0	0	0	0
Bandwidth selection procedure	CCT	I and K	CCT	I and K

Log(Total Educational Expenditure)	2004-5		2009-10	
RD Estimate	-0.217	-0.510	-0.191	-0.232
	(0.154)	(0.127)***	(0.135)	(0.118)**
Observations	8,922	11,388	8,205	9,937
Fuzzy Conventional p-value	0.159	0	0.157	0.0489
Fuzzy CCT Corrected p-value	0.0535	0	0.0668	0.0171
Bandwidth selection procedure	CCT	I and K	CCT	I and K

Log(School Fees and Tuition)	2004-5		2009-10	
RD Estimate	-0.504	-0.977	-0.578	-0.616
	(0.204)**	(0.165)***	(0.186)***	(0.150)***
Observations	8,308	12,034	7,608	10,219
Fuzzy Conventional p-value	0.0136	0	0.0018	0
Fuzzy CCT Corrected p-value	0.0029	0	0.0005	0
Bandwidth selection procedure	CCT	I and K	CCT	I and K

Log(Expenditure on newspapers, books, internet, libraries, stationery)	2004-5		2009-10	
RD Estimate	0.140	-0.0572	0.120	0.0189
	(0.121)	(0.101)	(0.0996)	(0.0914)
Observations	8,783	14,068	12,614	14,207
Fuzzy Conventional p-value	0.247	0.573	0.230	0.836
Fuzzy CCT Corrected p-value	0.0591	0.256	0.449	0.885
Bandwidth selection procedure	CCT	I and K	CCT	I and K

Household Expenditure Sources: National Sample Survey 2009-10. Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table 8: Labor Market Benefits

Change in Yearly Labor Market Benefits for

(1) Young, Induced into getting more Skill

With GE	Without GE	% Change	Fraction of cohort
0.121	0.157	-23.3%	0.17

(2) Always Skilled (Young)

With GE	Without GE	% Change	Fraction of cohort
-0.037	0	-	0.39

(3) Always Unskilled (Young)

With GE	Without GE	% Change	Fraction of cohort
0.014	0	-	0.44

Transfer in Yearly Benefits from Skilled to Unskilled

Among Old with GE	Among Old without GE	Among Young with GE	Among Young without GE
0.004	0	0.051	0

Change in Lifetime Welfare for Induced Students

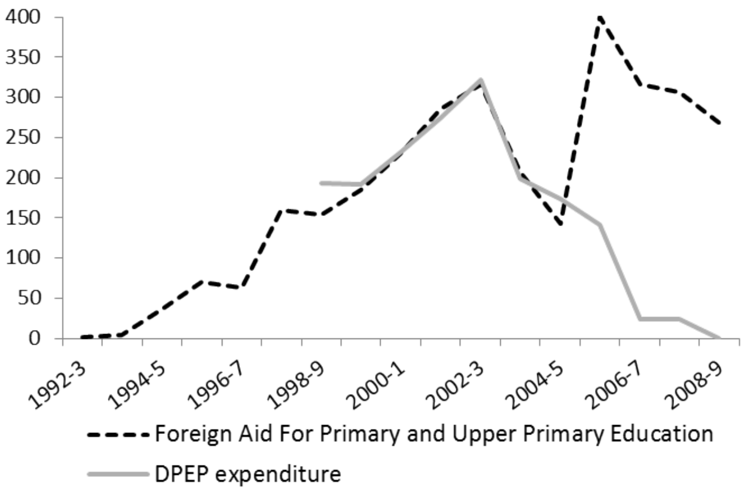
Costs	Benefits	Net	% Change (due to GE)
5.153	6.596	1.443	-23.3%

Welfare numbers are in monetary log-points. GE - indicates general equilibrium effects.
'Change in Benefits' shown for the sub-population that was young and changed their years of education to acquire skill. This is split up by 'With GE' effects, and a possible counterfactual of what would happen to their welfare in the absence of GE effects ('Without GE'). '% Change' is defined as change in welfare with the 'Without GE' as the base.
'Induced into getting more Skill' indicate the population that switched from unskilled to skilled only because of the policy. 'Always Skilled' indicate the population that would have acquired skill even in the absence of the policy. 'Always Unskilled' indicate the fraction of the population who would not have acquired skill even in the presence of the policy. 'Fraction switchers' is estimated (across RD cutoff) difference in sub-populations that acquired a higher level of education.
Yearly welfare calculations assume an interest rate of 2.37% and a gap of ten years between the costs of education and the labor market returns. Real Interest Rates from the World Bank. The World Bank uses the lending rate and adjusts it for inflation using the GDP deflator. For the period 2010-13, the average real interest rate was 2.37%.
'Change in Lifetime Welfare for Induced Students' : Costs include (a) opportunity cost of foregone earnings for unskilled work, and (b) tuition costs for students in DPEP districts near the cutoff. Costs are calculated in 2004 (NSS 61st round).
'Change in Lifetime Welfare for Induced Students' : Benefits include present discounted value of lifetime earnings stream assuming a real interest rate of 2.37%.

ONLINE APPENDIX

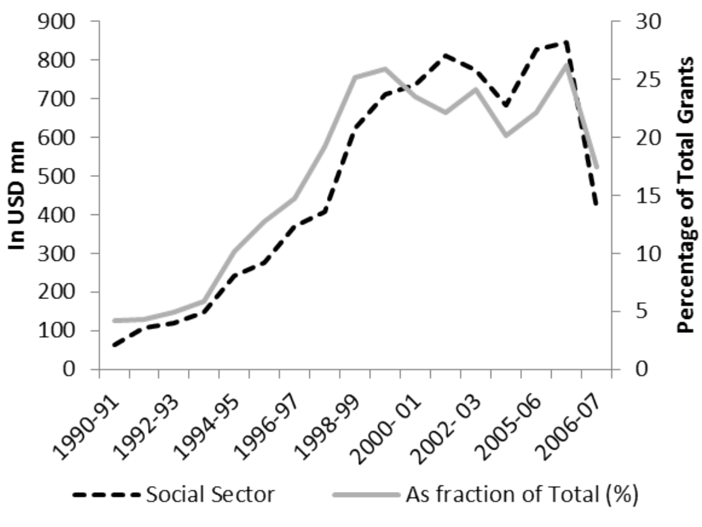
A Additional Tables and Figures

Figure A.6: Foreign Aid and DPEP Expenditure (in USD mn)



Foreign aid for expenditure on primary and upper primary education, and funds disbursed for DPEP. 1999 Indian rupees converted to USD using the 1999 exchange rate of Rs. 40 to $1. Sources: Foreign Aid from Colclough and De (2010). DPEP expenditures data compiled by author from Ministry of Human Resources and Development Reports, National Institute of Educational Planning and Administration, Lok Sabha Unstarred Question Numbers: 1807- 07.03.2006; 552- 24.02.2009; 55 - 26.02.2008; 267- 22.03.2005; 1320- 10.12.2003; 2018- 4.3.2003, and Rajya Sabha Unstarred Question No. 2855- 19.04.2002.

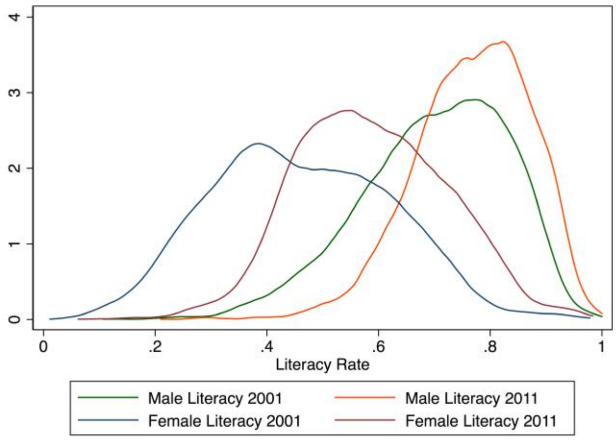
Figure A.7: Social Sector (Health and Education) Grants/Loans from Central to State Governments



Central government grants and loans to State governments for spending in the social sector (health and education), and as a proportion of total grants/loans. 1999 Indian rupees converted to USD using the 1999 exchange rate of Rs. 40 to $1. Source: External Assistance Brochure of CAA&A, Department of Economic Affairs, Ministry of Finance, Government of India.

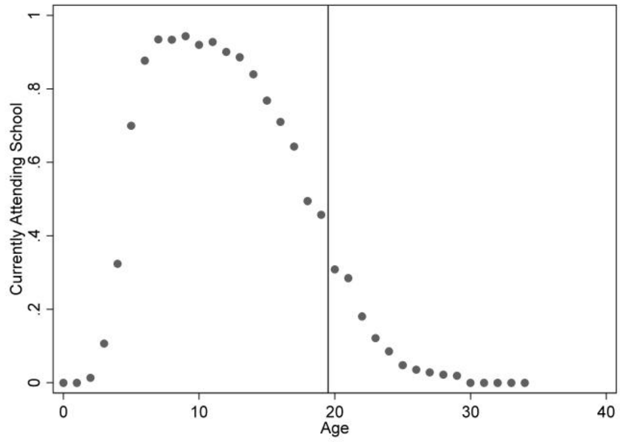
Figure A.8: Literacy and Enrollment

(a) Literacy Rates Across Census Years



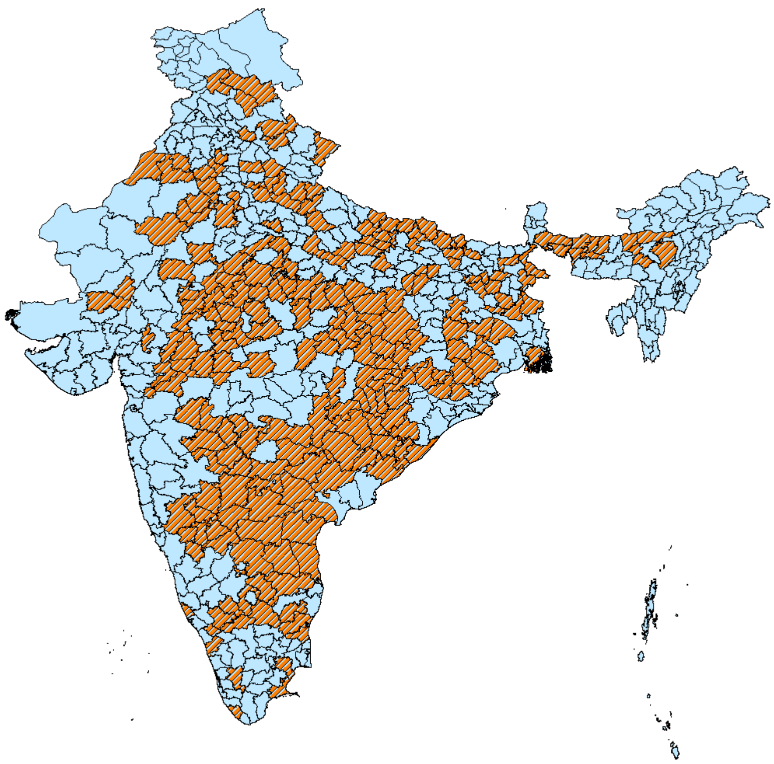
Source: Census of India 2001 and 2011. Distributions calculated over sub-districts.

(b) Enrollment Rates by Age



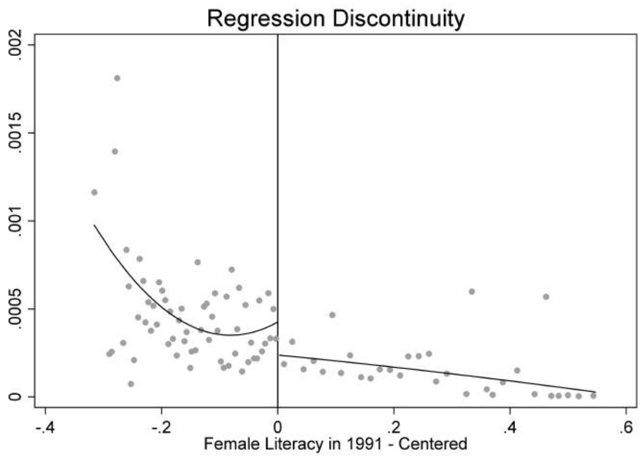
National Sample Survey 2009. The largest drop in school enrollment occurs between the ages of 19 and 20 - representing a 15 percentage point fall.

Figure A.9: Map of DPEP Districts

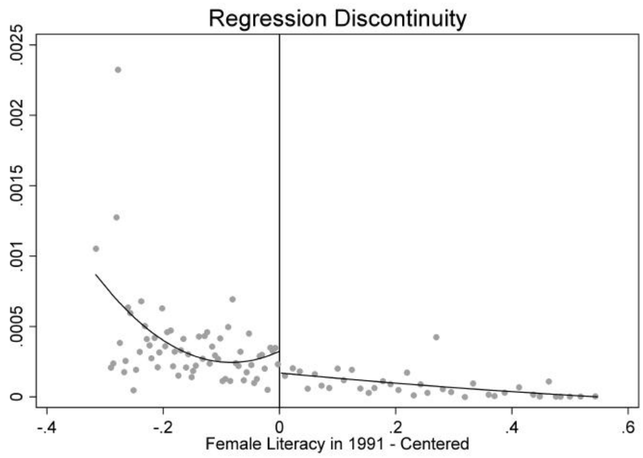


Orange and shaded districts received DPEP, whereas blue-unshaded districts did not.

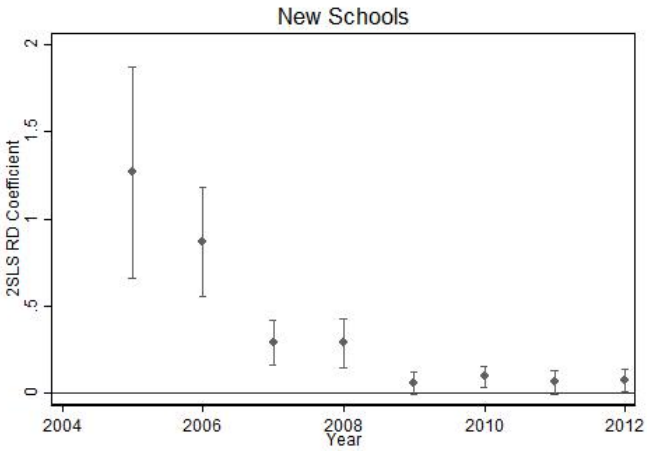
Figure A.10: Schools Built Post 1993 - Bandwidth Selection



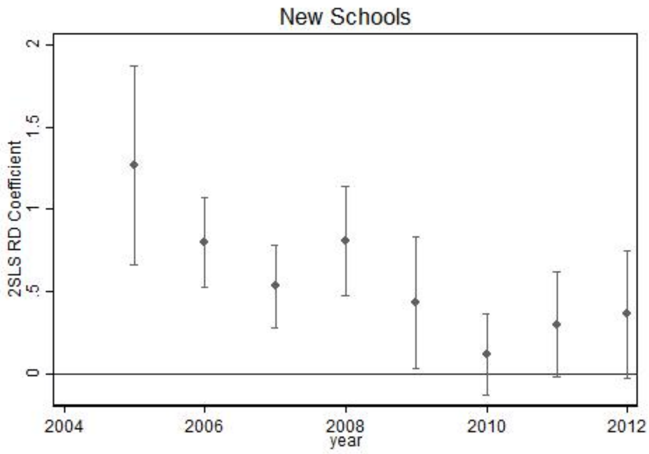
Total Schools (per cap) Built Post 1993



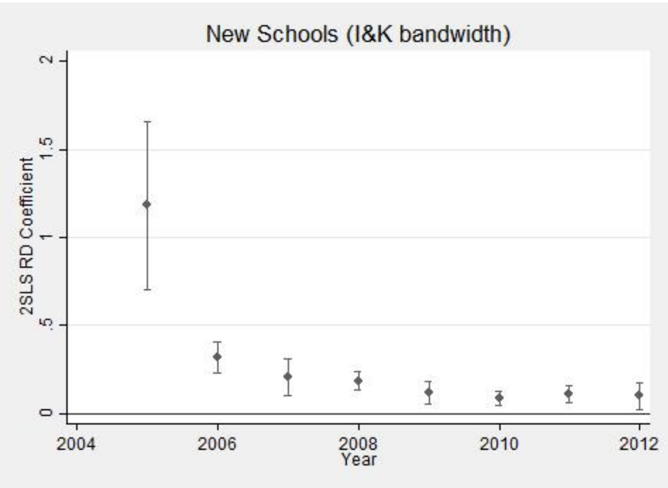
Total Government Schools (per cap) post 1993



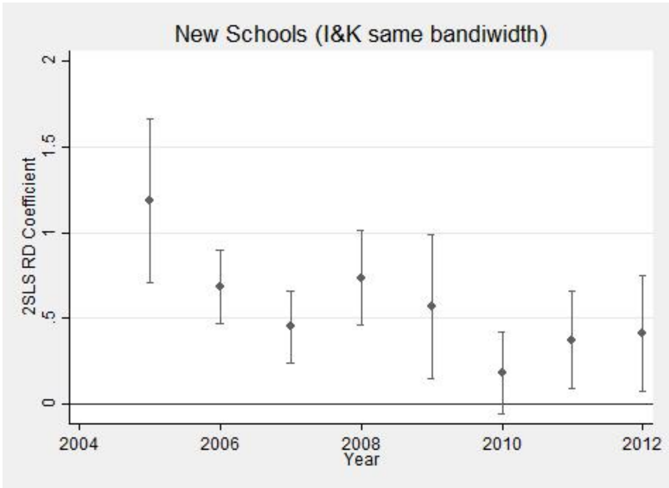
Fraction New - CCT



CCT Same Bandwidth for All Years



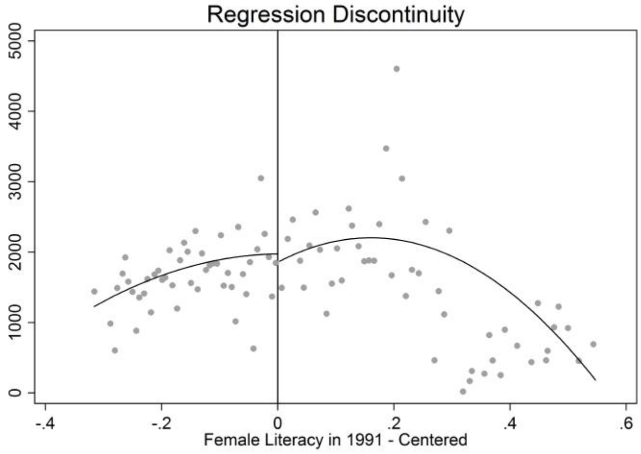
Fraction New - I&K Bandwidth



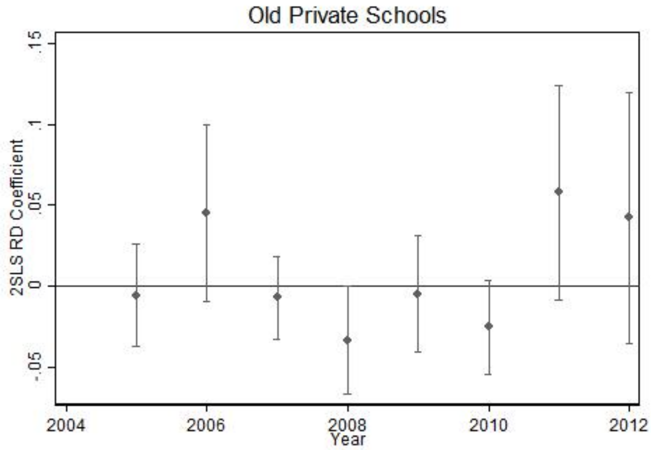
I&K Same Bandwidth for All Years

Source: DISE (District Information System for Education) data. CCT is Calonico et al. (2014b), whereas I&K is Imbens and Kalyanaraman (2012). 'per cap' figures normalized by total population in district. 'Same Bandwidth for All Years' is where the estimation is restricted to have the same bandwidth as it is in the first year of the data.

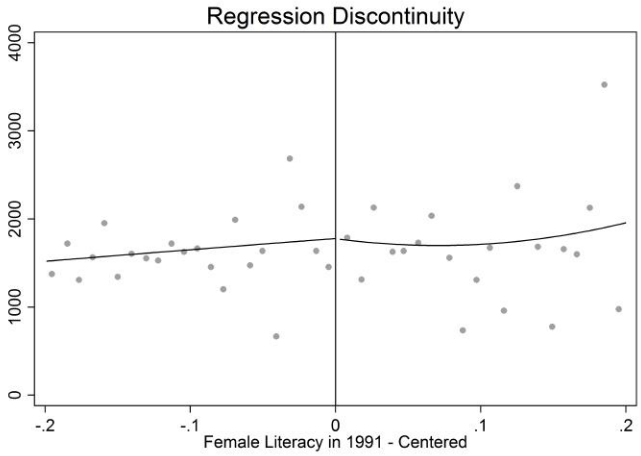
Figure A.11: No Discontinuity in Number and Fraction of Old Schools



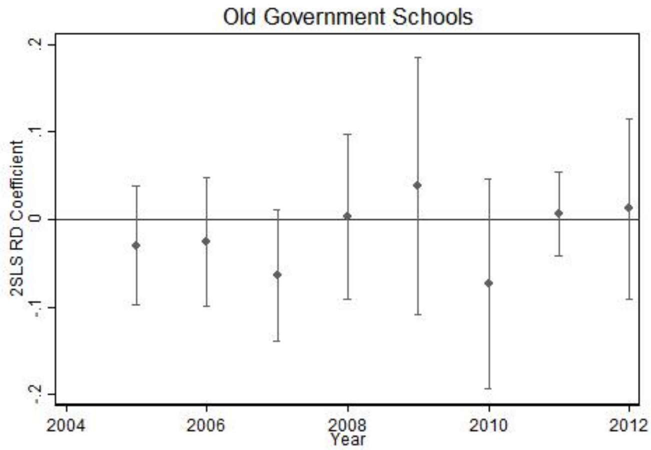
Total Number of Old Schools (built pre-1993)



Over Time: Private Schools (1973-93) as a Fraction of all Old Pvt Schools (pre-1993)



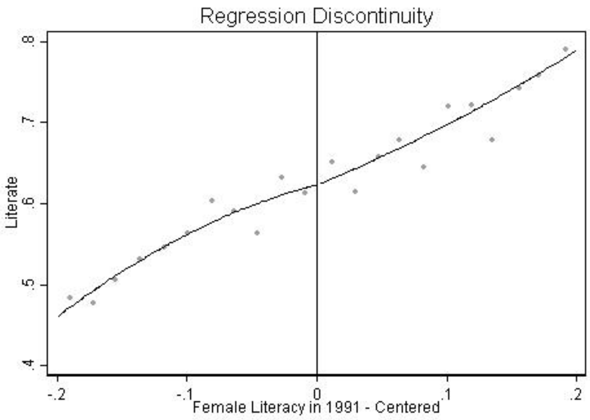
Total Number of Old Gov Schools (1993)



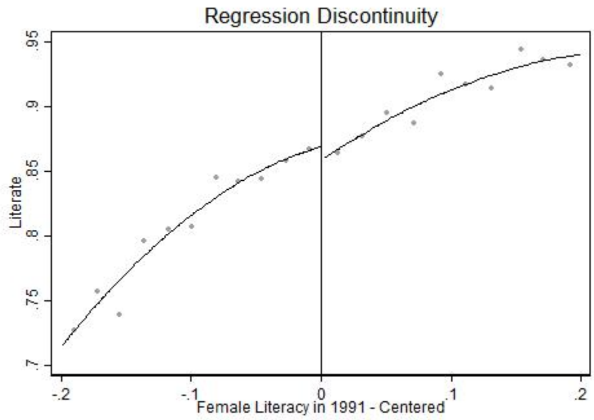
Over Time: Government Schools (1973-93) as a Fraction of all Old Gov Schools (pre-1993)

Source: DISE (District Information System for Education) data. RD graphs (Regression Function Fit) use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

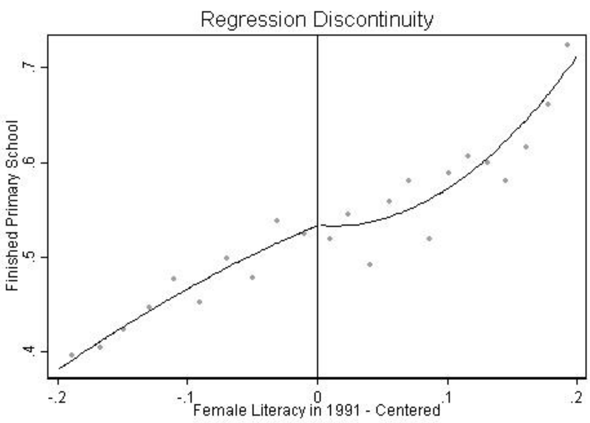
Figure A.12: RD Figures - Levels of Education - Full Sample



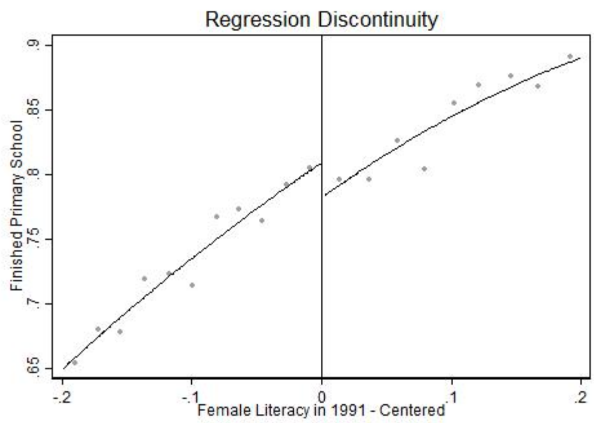
Literate - Old



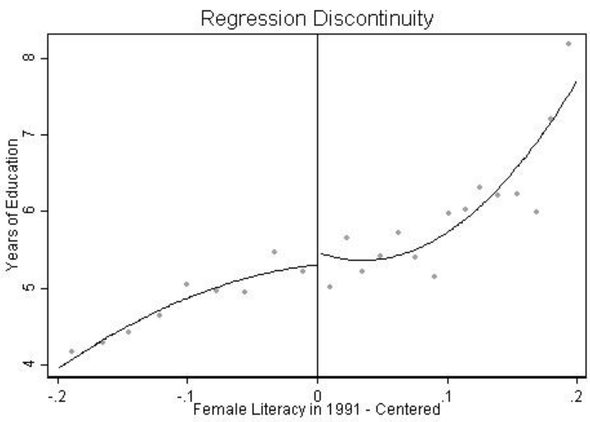
Literate - Young



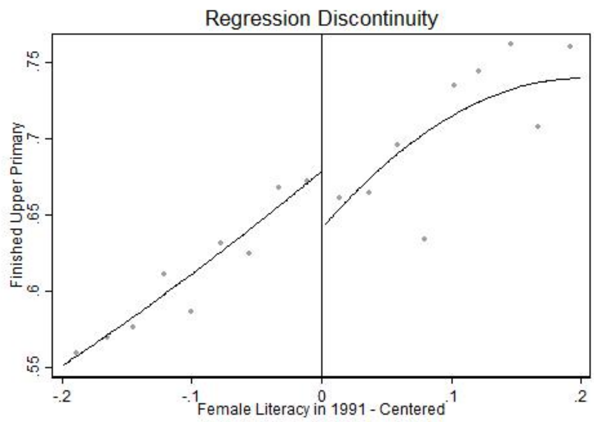
Finished Primary School - Old



Finished Primary - Young



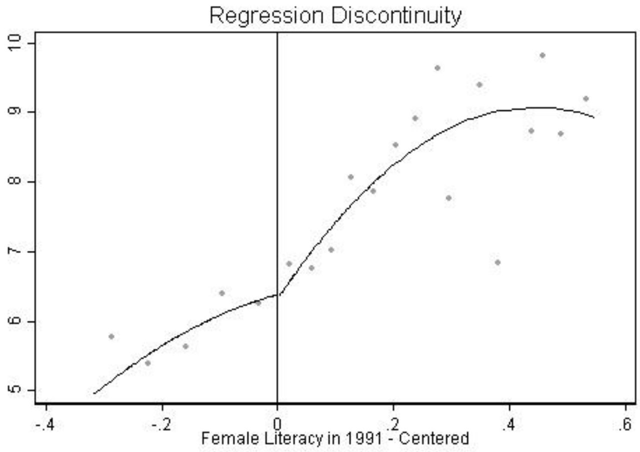
Years of Education - Old



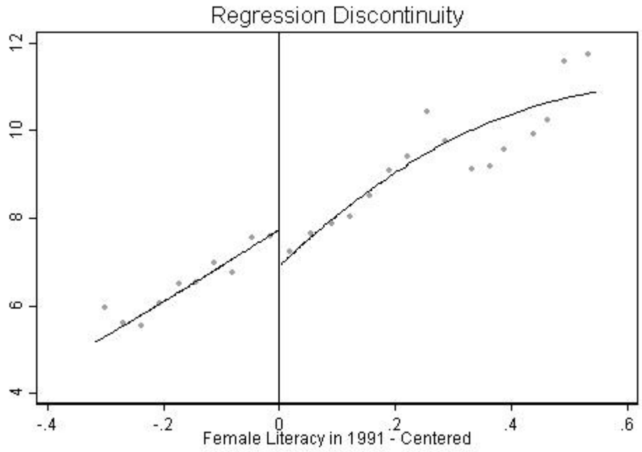
Finished Upper Primary - Young

National Sample Survey 2009 for all persons (whether or not earnings reported). Figures made using Calonico et al. (2014b) method of using regression curves to approximate the conditional means on either side of the cutoffs and the equally spaced sample means, and optimal number of bins.

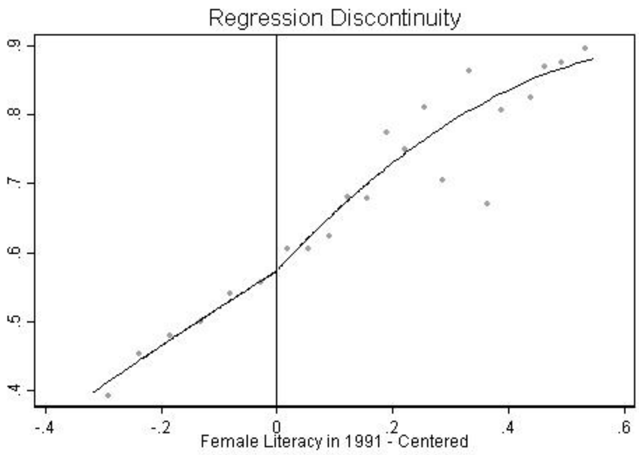
Figure A.13: RD figures for DISE - data districts



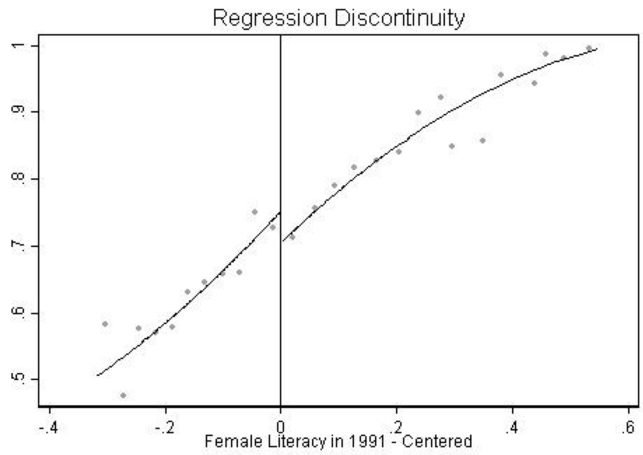
Years of Education - Older



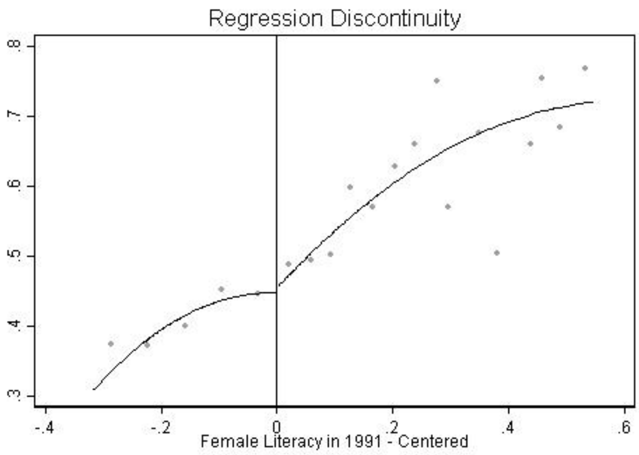
Years of Education - Younger



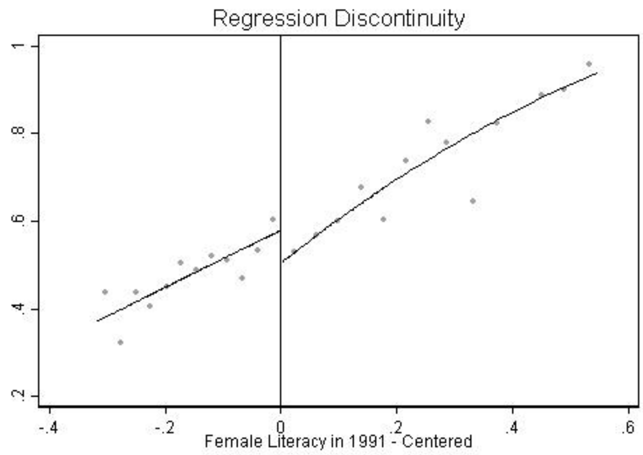
Finished Primary - Older



Finished Primary - Younger



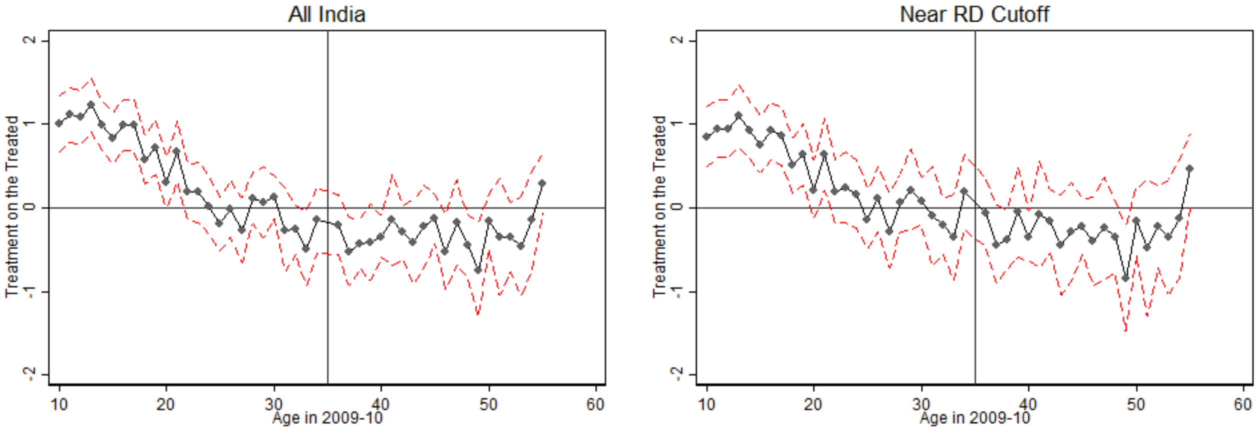
Finished Upper Primary - Older



Finished Upper Primary - Younger

National Sample Survey 2009. DISE districts include districts that have school-level DISE data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

Figure A.14: Difference-in-Differences: Years of Education

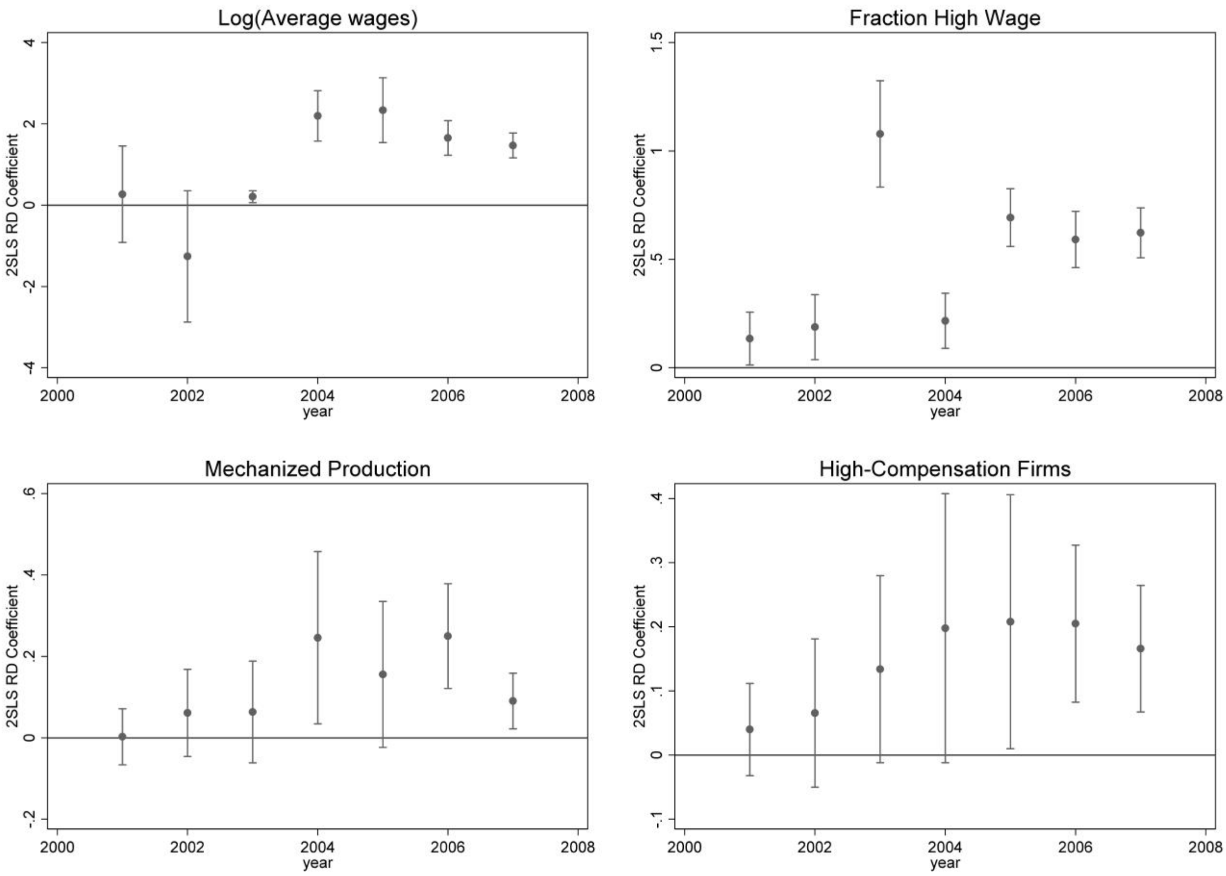


Full Sample

Short Bandwidth

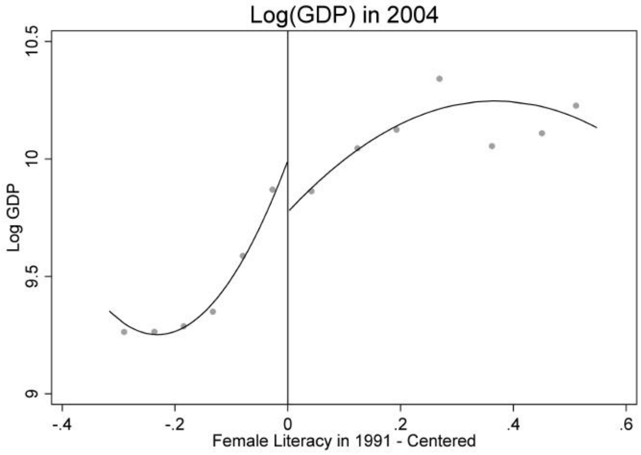
Coefficients of regression that includes age fixed effects and district fixed effects. Difference-in-Differences coefficient based on age and DPEP status. 'Short Bandwidth' restricts to sample near RD cutoff.

Figure A.15: Adoption of Skill Biased Capital: Firm-Level Data

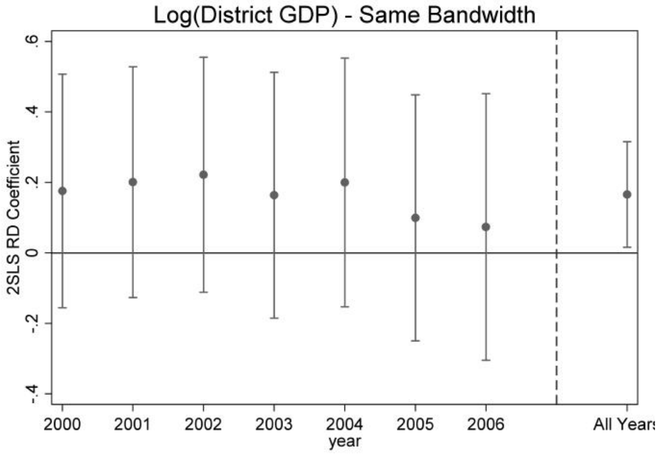


Source: Annual Survey of Industries (2001 to 2007). Firm level data. Wages and compensation calculated at the firm-level. 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure. 'High-wage' or 'high-compensation' defined as being above median wages for the entire country.

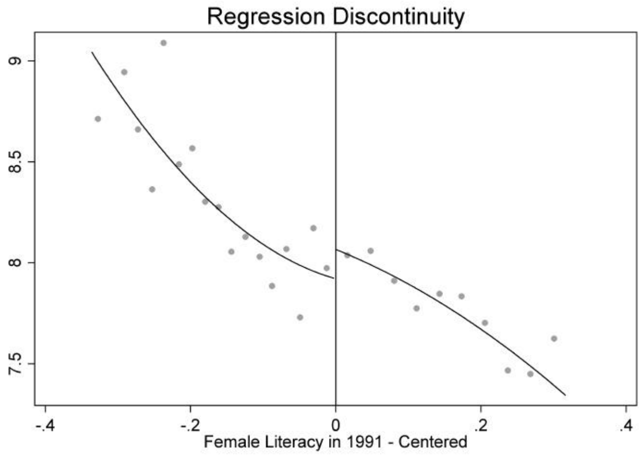
Figure A.16: Change in Overall Output and Household Expenditure on Education



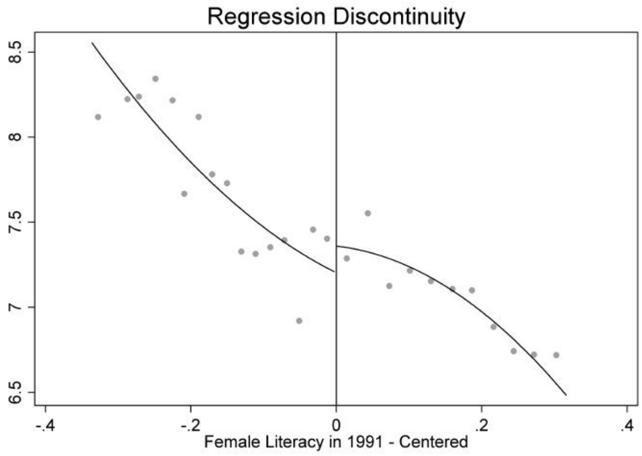
Log District GDP in 2004



RD 2SLS Coefficients - Same Bandwidth



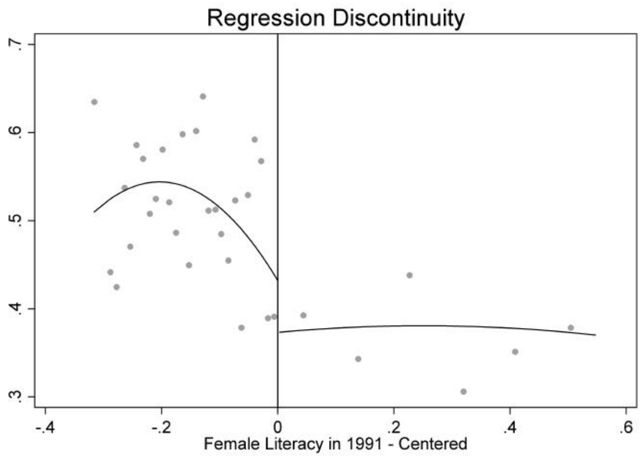
Log(Educational Expenditure)



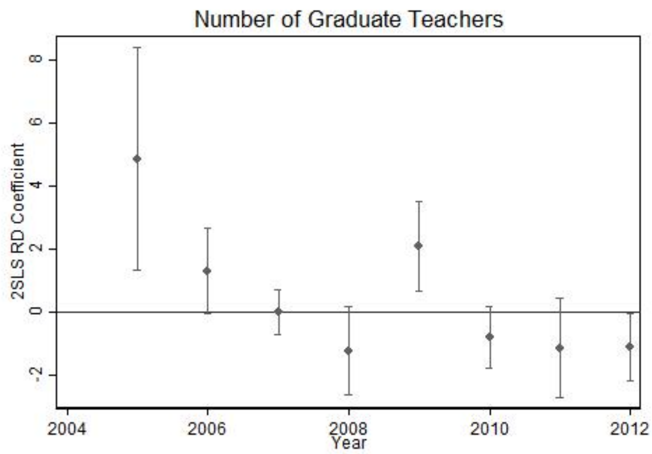
Log(Expenditure on Tuition/Fees)

RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure. 'Same bandwidth' restricts bandwidth to be the same as the first year's optimal bandwidth.
Educational Expenditure Source: National Sample Survey 66th Round.
District Domestic Product Sources: Department of Statistics and Programme Implementation, Government of West Bengal; Planning Commission; Directorate of Economics and Statistics Government of Uttar Pradesh; Department of Economics and Statistics Government of Tamil Nadu; Directorate of Economics and Statistics Government of Rajasthan; Department of Planning Government of Punjab; Planning and Coordination Government of Odisha; Directorate of Economics and Statistics Government of Maharashtra; Directorate of Economics and Statistics Government of Kerala; Planning Programme Monitoring and Statistics Department Government of Karnataka; Directorate of Economics and Statistics Government of Bihar; Directorate of Economics and Statistics Government of Assam; Andhra Pradesh State Portal.

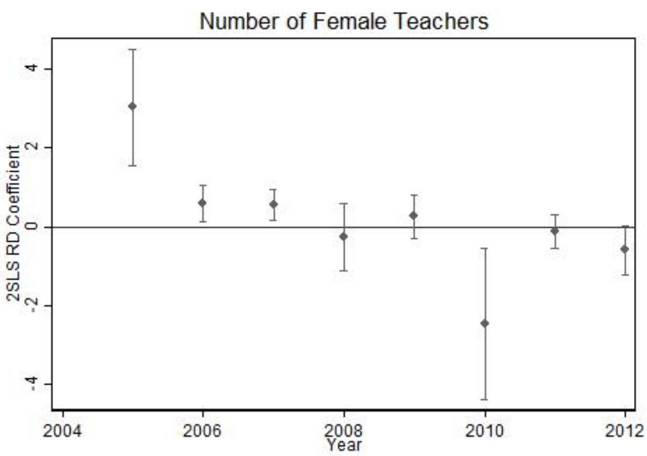
Figure A.17: Teachers, Drinking Water, Restrooms and Electricity



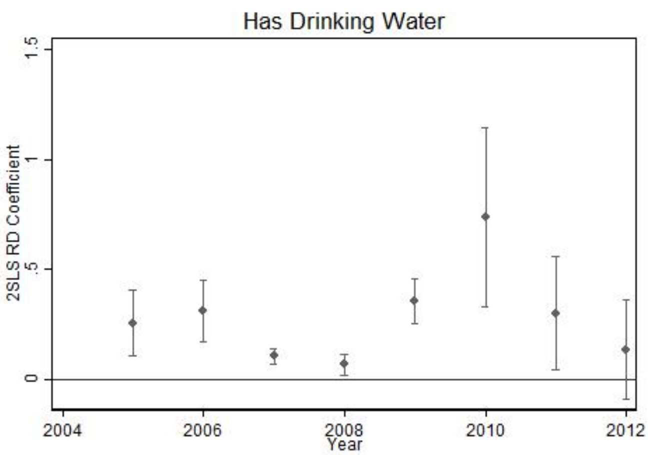
Teachers (per school) with College Degrees



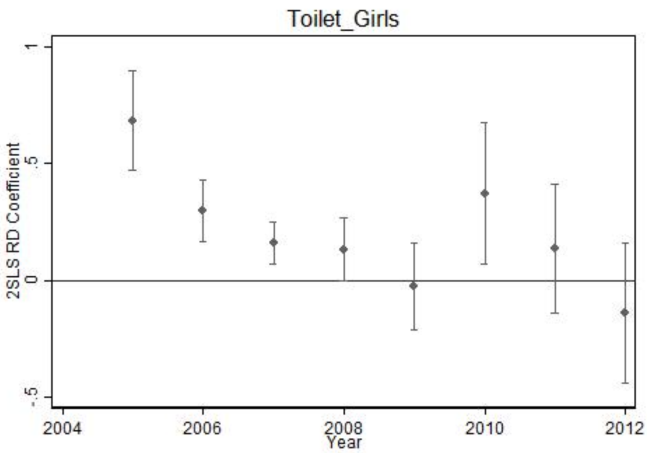
Teachers (per school) with College Degrees



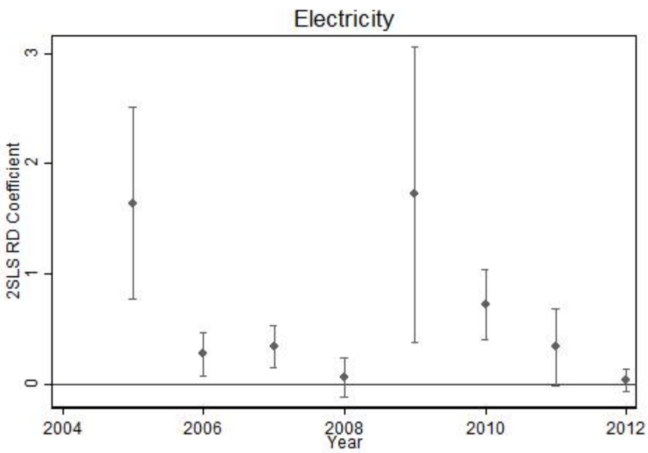
Female Teachers (per school)



Drinking Water



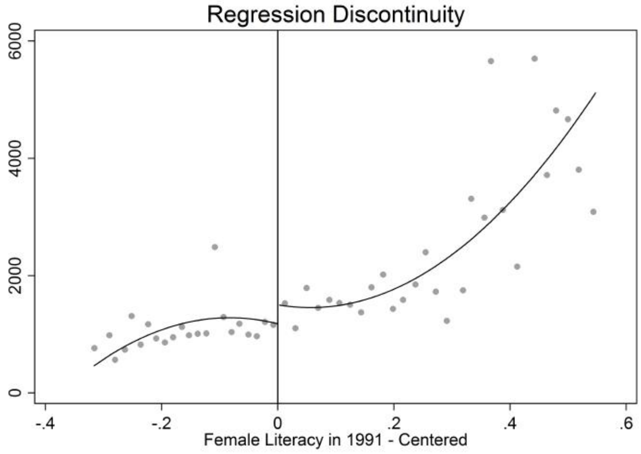
Girls' Restrooms



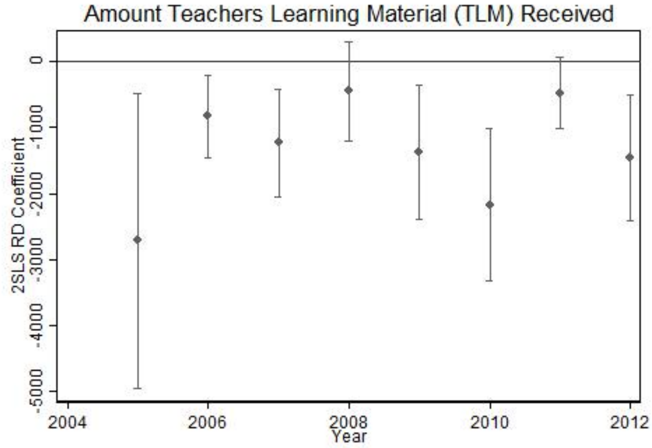
Coefficient Over Time: Electricity

Source: DISE data. RD graphs (Regression Function Fit) use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

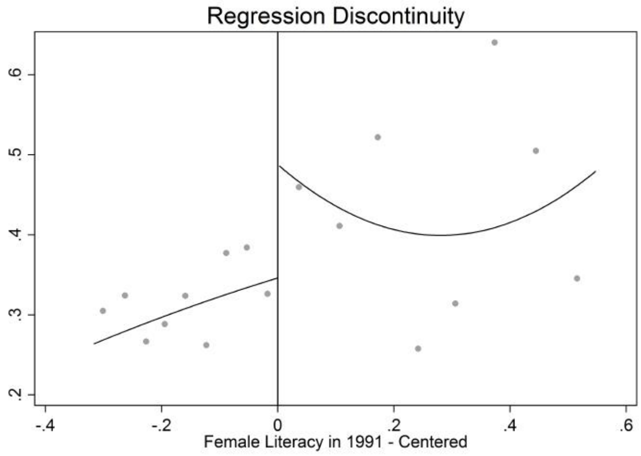
Figure A.18: Crowd Out of Other Funds, Conditions of Classrooms, Distance to Resource Centers and Pre-Primary Sections



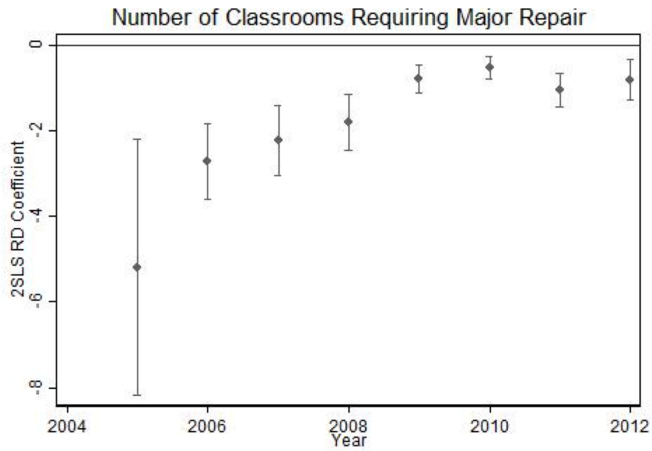
TLM grants Spent (2005)



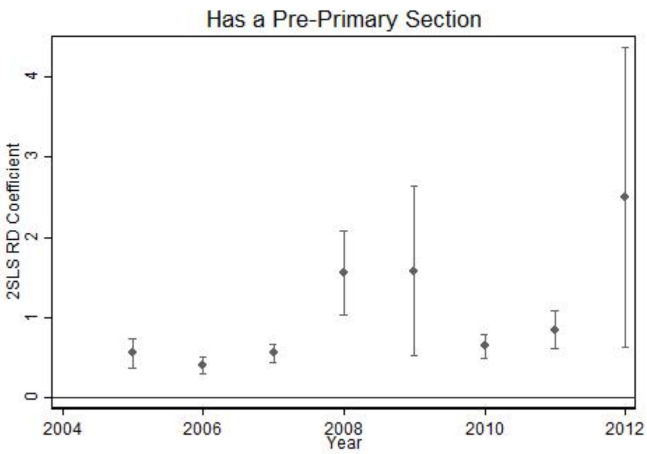
RD Coefficient Over Time: TLM grants Received



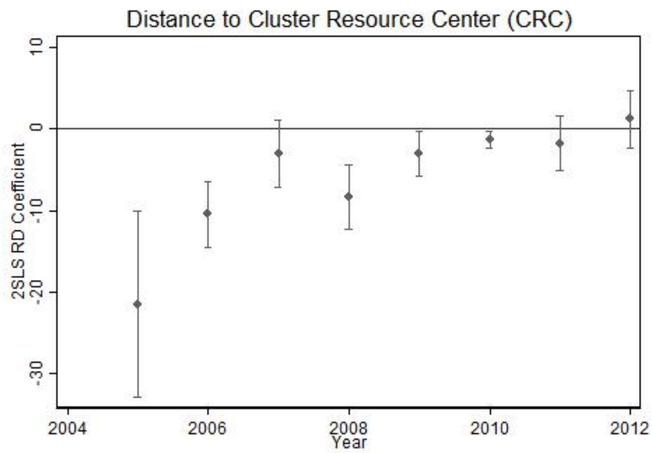
Classrooms Needing Major Repair (2005)



RD Coefficients: Classrooms Need Major Repair



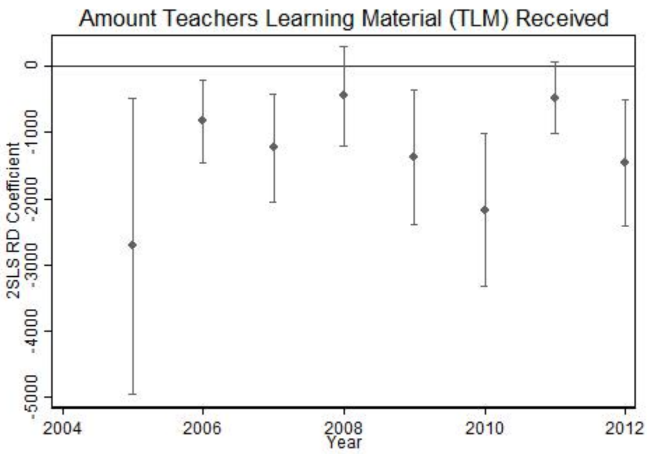
Coefficient Over Time: Pre-Primary Schools



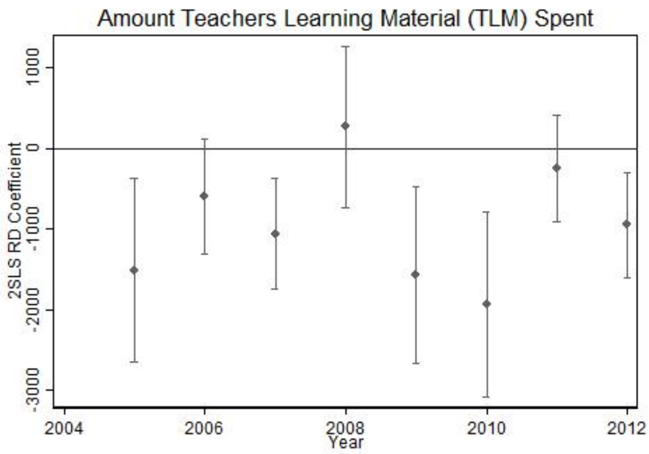
Coefficient Over Time: Distance to CRC

Source: DISE data. All schools, regardless of their which district they are in, are eligible to receive the Teacher Learning Materials (TLM) grant. RD graphs (Regression Function Fit) uses the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure. Cluster Resource Centers (CRCs) provide facilities and training to teachers. RD graphs for TLM grants received (rather than spent) can be found in Figure A.19. Classrooms needing minor repair and in good condition can be found in Figure A.19. Other results related to pre-primary sections can be found in Figure A.20. Other results related to Block and Cluster resource centers can be found in figure A.21. Other infrastructure related figures can be found in Figure A.22

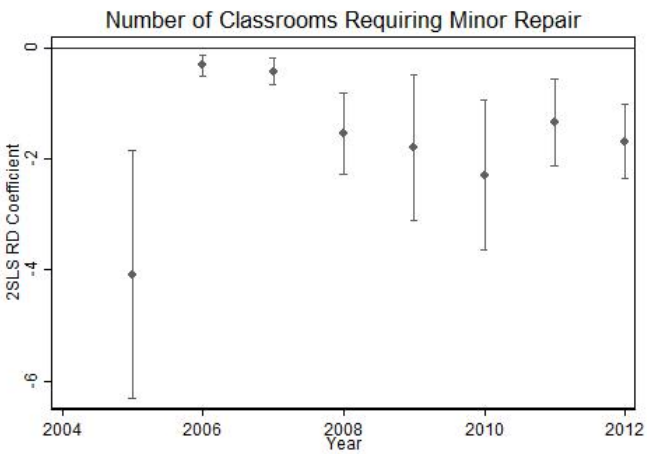
Figure A.19: Other Funds Spent, and Condition of Rooms



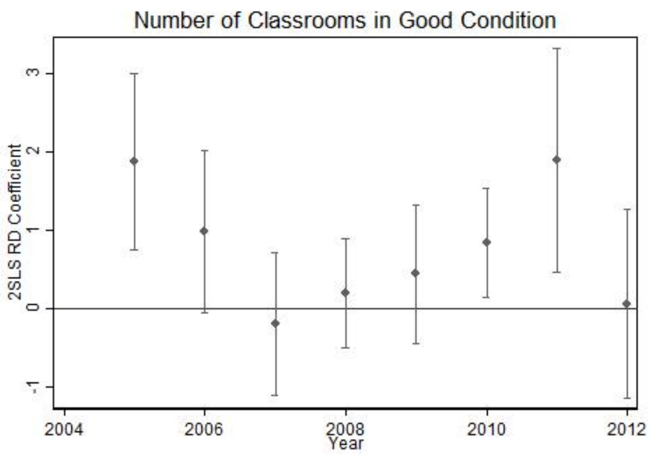
RD Coefficient Over Time: TLM grants Received



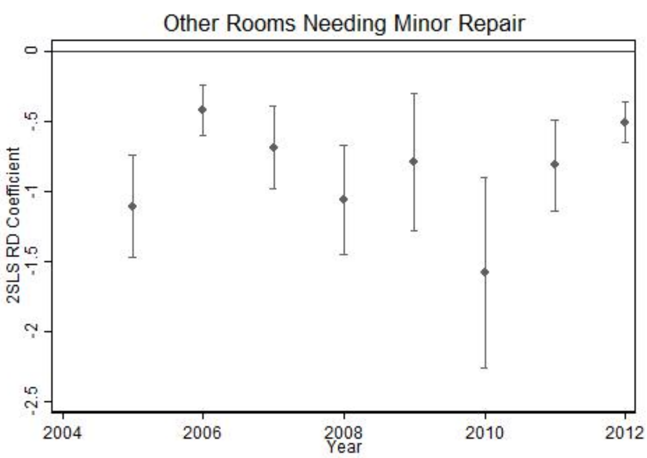
RD Coefficient Over Time: TLM grants Spent



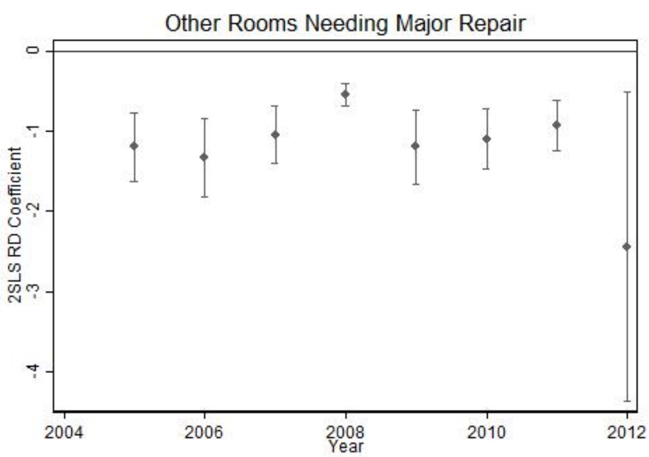
RD Coefficients: Classrooms Need Minor Repair



RD Coefficients: Classrooms in Good Condition



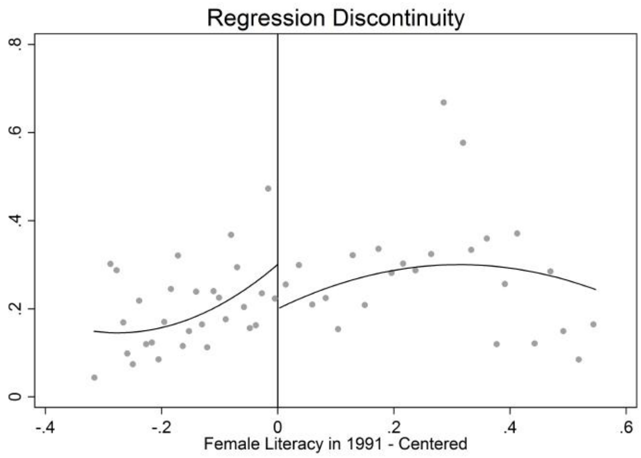
RD Coefficients: Other Rooms Need Minor Repair



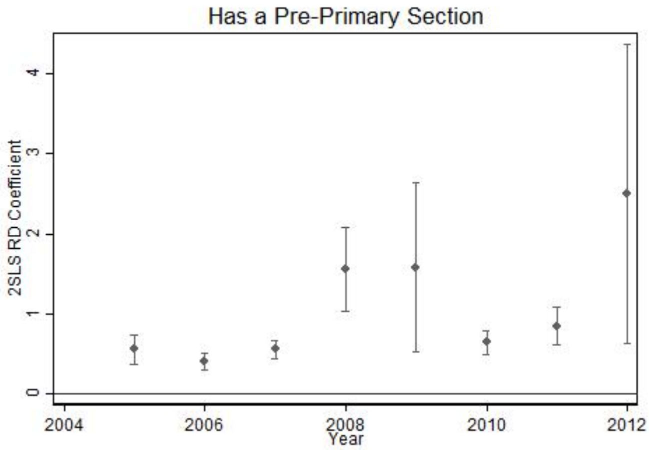
RD Coefficients: Other Rooms Need Major Repair

Source: DISE (District Information System for Education) data. RD graphs (Regression Function Fit) uses the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure. All schools, regardless of their which district they are in, are eligible to receive the Teacher Learning Materials (TLM) grant. Discontinuity for TLM grants spent, and for 'Classrooms need Major Repair' can be found in Figure A.18.

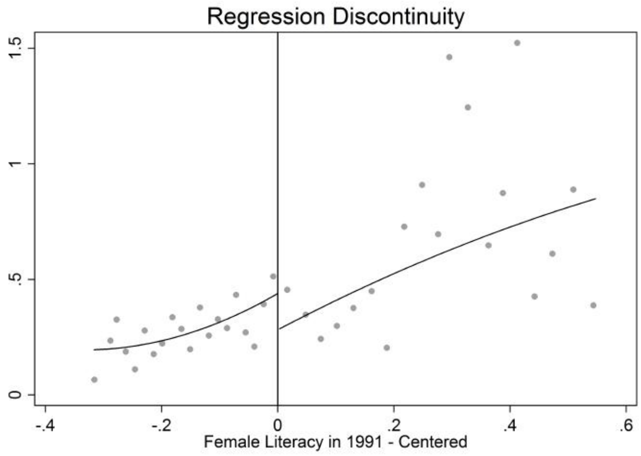
Figure A.20: Pre-Primary Sections



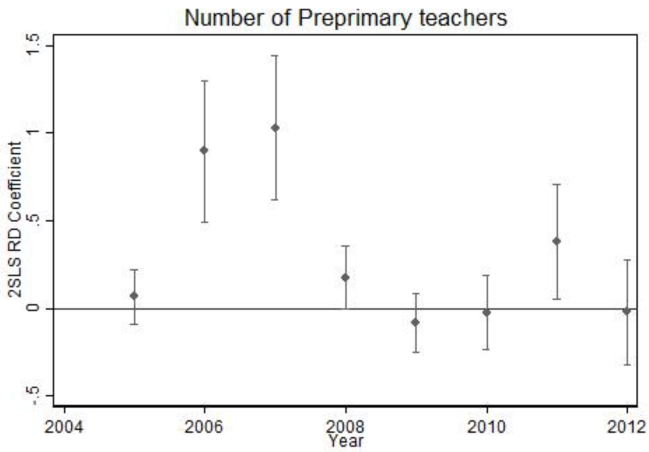
Schools with Pre Primary Sections



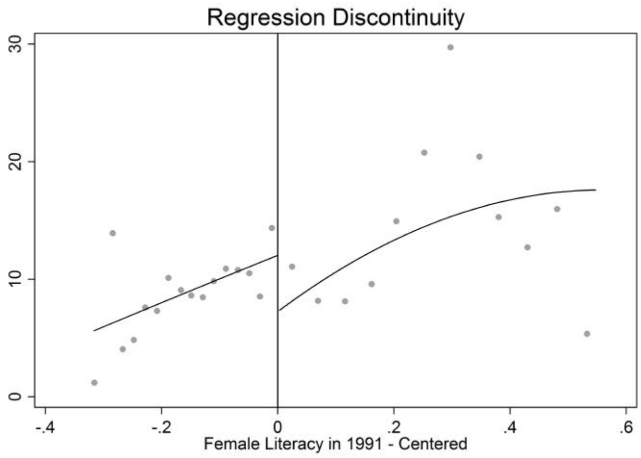
Coefficient Over Time: Pre-Primary Schools



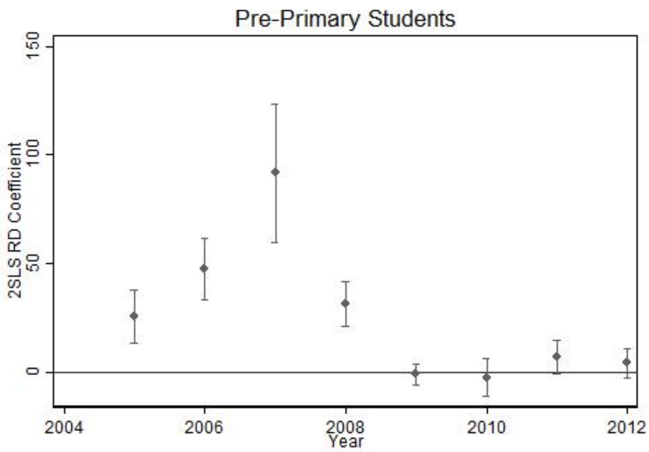
Number of Pre Primary Teachers



Coefficient Over Time: Pre-Primary Teachers



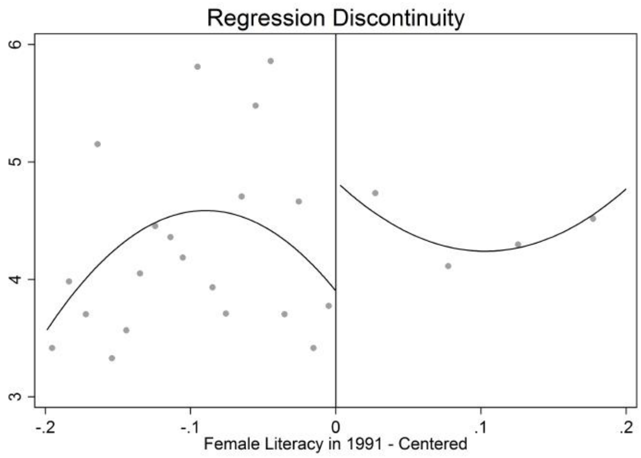
Number of Pre Primary Students



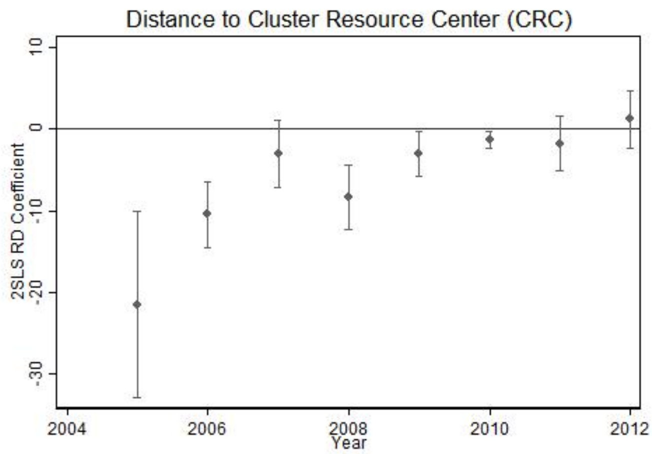
Coefficient Over Time: Pre-Primary Students

Source: DISE (District Information System for Education) data. RD graphs in the left-panel use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

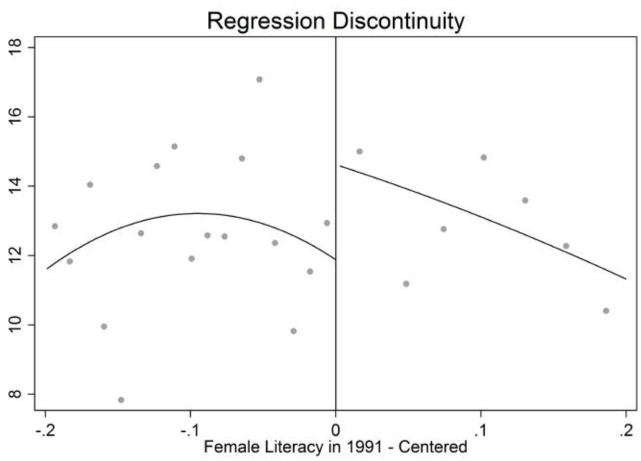
Figure A.21: Academic Inspections and Regional Resource Centers



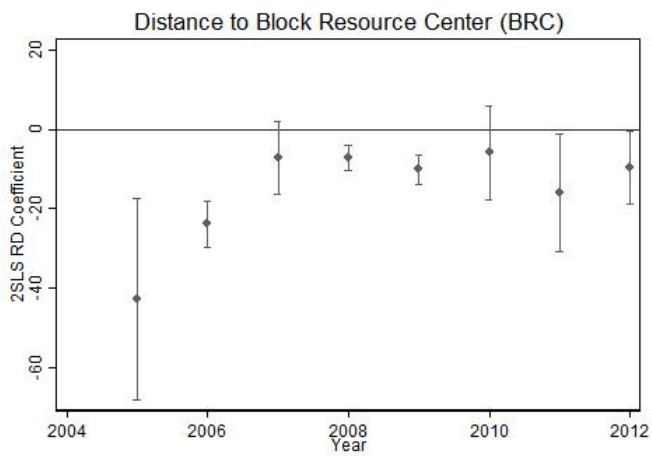
Distance to CRC (2005)



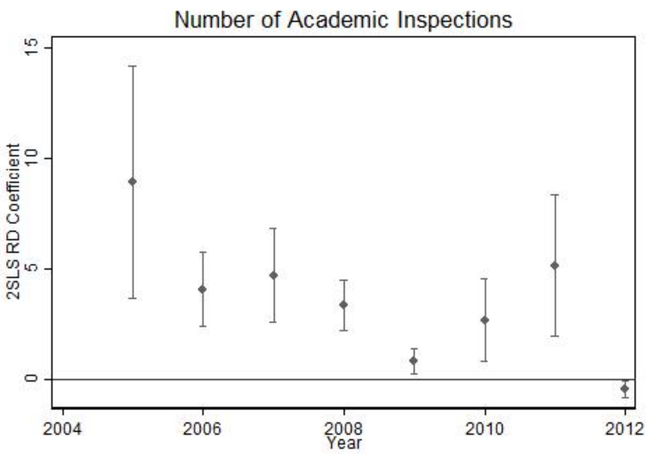
Coefficient Over Time: Distance to CRC



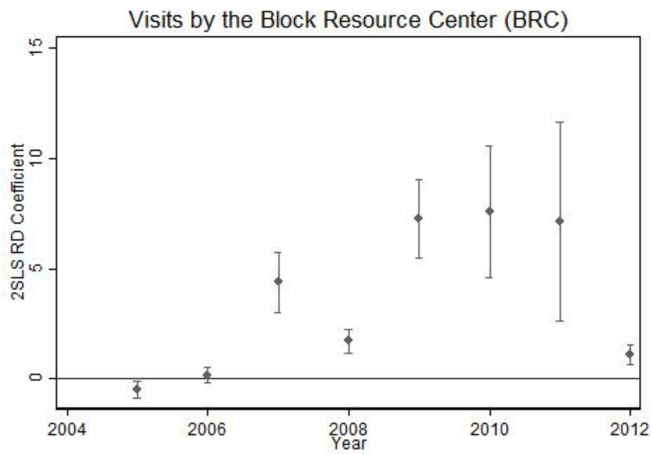
Distance to BRC (2005)



Coefficient Over Time: Distance to BRC



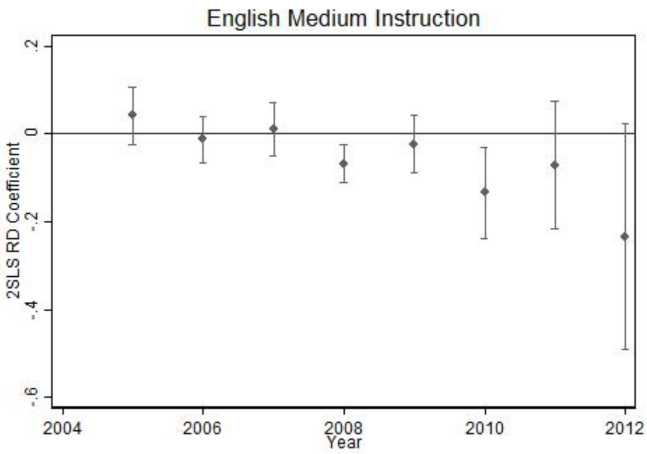
Number of Academic Inspections



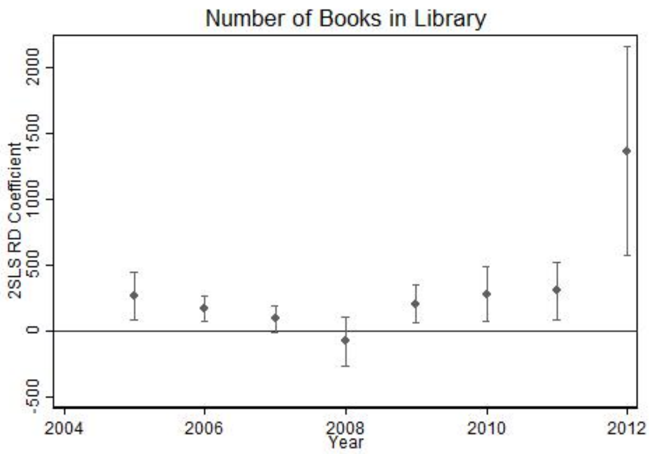
Visits by BRC Official

Source: DISE (District Information System for Education) data. Cluster Resource Centers (CRCs) and Block Resource Centers (BRCs) provide facilities and training to teachers. RD graphs in the left-panel use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

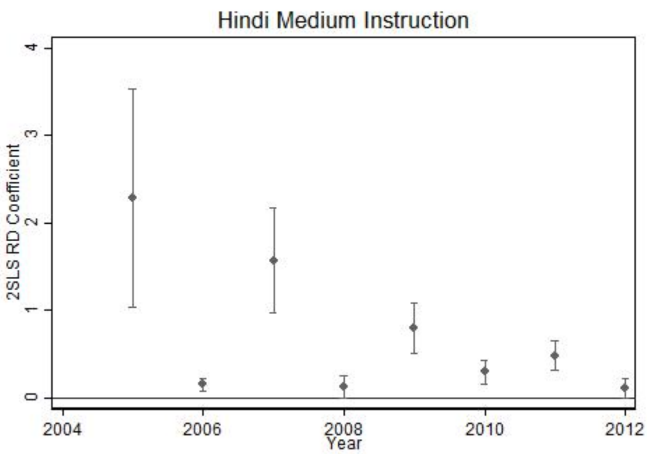
Figure A.22: Medium of Instruction and Other Infrastructure



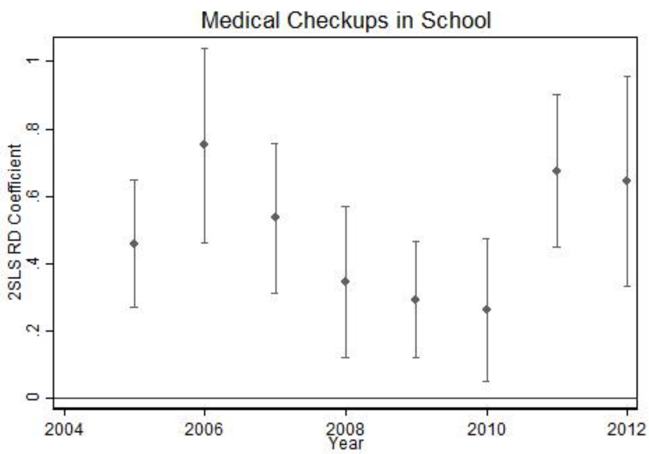
Coefficient Over Time: English Medium



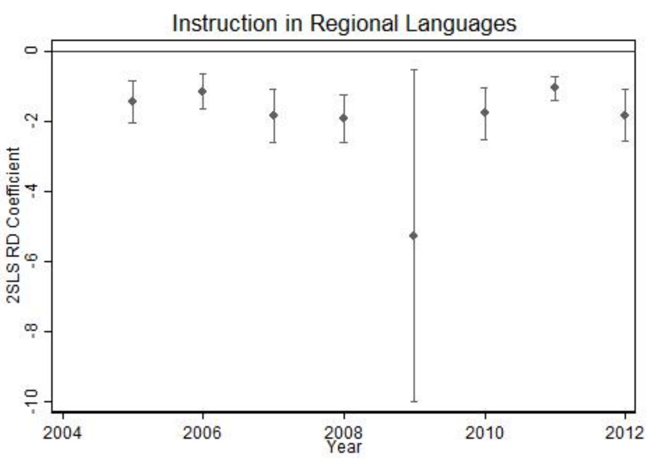
Coefficient Over Time: Library Books



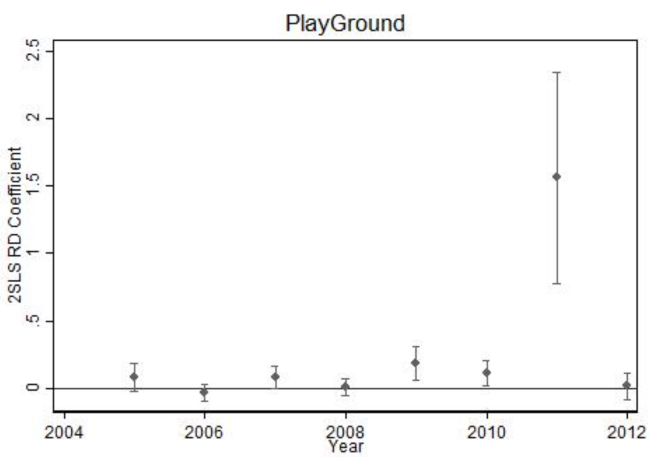
Coefficient Over Time: Hindi Medium



Medical Checkups



Coefficient Over Time: Regional Language

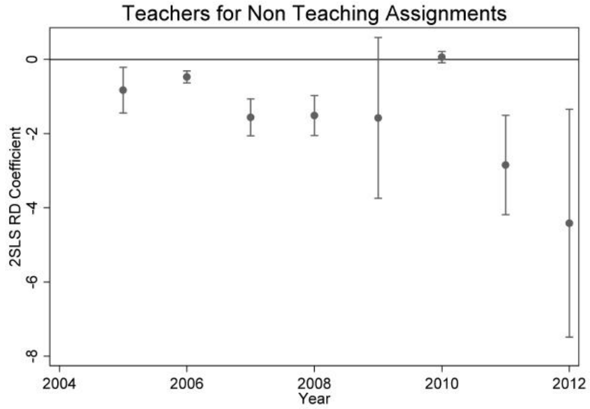


Coefficient Over Time: Playground

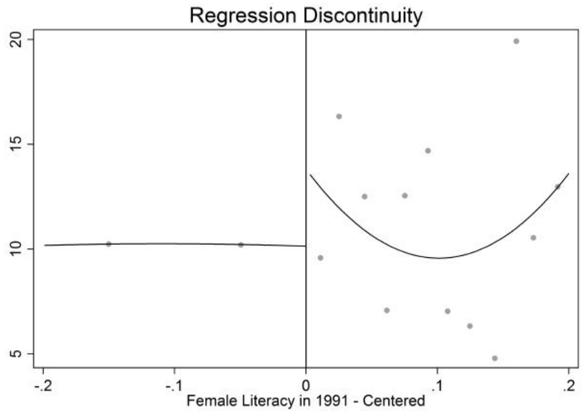
Source: DISE (District Information System for Education) data. RD graphs in the left-panel use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure. Other infrastructure related graphs can be found in Figures A.17, A.20, A.21 and A.19.

Figure A.23: Involvement in Non Teaching Assignments

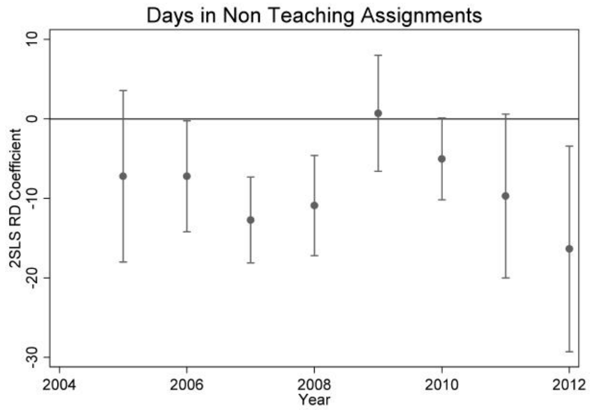
Teachers (per school) in Non-Teaching Assignments



Teachers (per school) in Non-Teaching Assignments



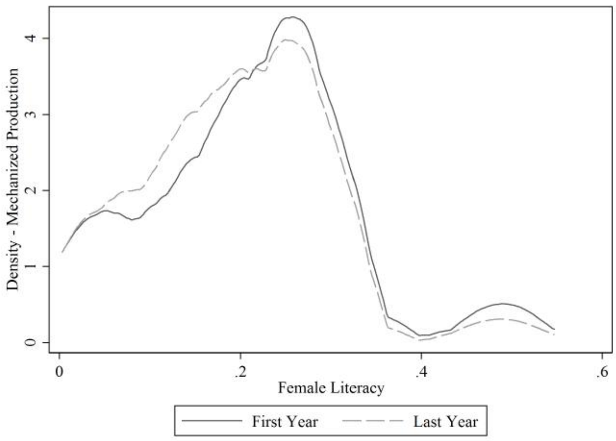
Days Involved in Non-Teaching Assignments



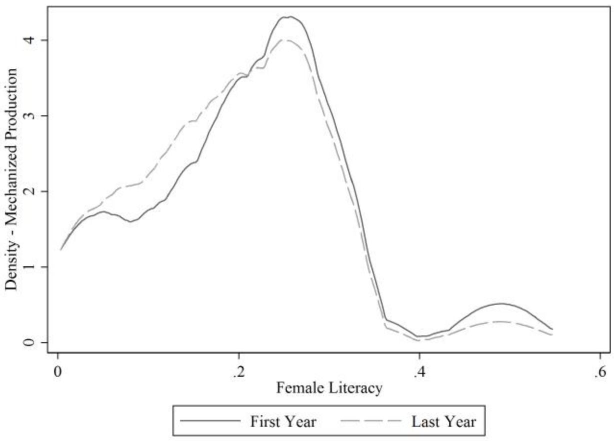
Days Involved in Non-Teaching Assignments

Source: DISE (District Information System for Education) data. RD graphs in the left-panel use the 2005 data. RD graph optimal binning and 2SLS RD coefficients calculated using Calonico et al. (2014b) procedure.

Figure A.24: Density of Capital Intensive Firms Above Cutoff



Firms with Mechanized Production



High Compensation Firms

Source: Annual Survey of Industries (ASI) panel from 2001 (first year of data) and 2007 (last year of data).

Table A.9: Education, Earnings and Returns By Age Groups

Years of Education - Younger	16 to 25	26 to 35	16 to 25	26 to 35
RD Estimate	2.751	1.161	2.379	1.179
	(0.768)***	(0.672)*	(0.559)***	(0.535)**
Observations	4,071	5,747	7,301	8,874
Fuzzy Conventional p-value	0.000340	0.0839	0	0.0277
Fuzzy CCT Corrected p-value	0.000170	0.108	0	0.0213
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Years of Education - Older	36 to 45	46 to 55	36 to 45	46 to 55
RD Estimate	-0.821	0.856	-0.450	0.649
	(0.787)	(1.008)	(0.684)	(0.850)
Observations	4,502	3,158	5,508	4,285
Fuzzy Conventional p-value	0.297	0.396	0.510	0.445
Fuzzy CCT Corrected p-value	0.180	0.257	0.171	0.198
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Log(Earnings) - Younger	16 to 25	26 to 35	16 to 25	26 to 35
RD Estimate	0.403	0.136	0.481	0.265
	(0.134)***	(0.111)	(0.0973)***	(0.0884)***
Observations	4,072	5,747	7,302	8,874
Fuzzy Conventional p-value	0.00257	0.219	0	0.00270
Fuzzy CCT Corrected p-value	0.0109	0.844	0.00259	0.749
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Log(Earnings) - Older	36 to 45	46 to 55	36 to 45	46 to 55
RD Estimate	-0.184	0.0350	-0.0585	0.192
	(0.134)	(0.182)	(0.117)	(0.157)
Observations	4,501	3,157	5,507	4,284
Fuzzy Conventional p-value	0.172	0.848	0.617	0.223
Fuzzy CCT Corrected p-value	0.0409	0.978	0.0697	0.432
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10, for all districts, and for persons that reported earnings. Bandwidths: Calonico et al. (2014b) method. Bias corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b). Earnings regressions are restricted to the same bandwidth as the years of education regressions.

Table A.10: Earnings Reported, Migration, Paid Monthly, and Unemployment

P(Earnings Being Reported)	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	-0.0147	-0.0208	-0.0135	-0.0201
	(0.0209)	(0.0185)	(0.0223)	(0.0190)
Observations	37,201	42,316	32,742	39,823
Fuzzy Conventional p-value	0.481	0.261	0.546	0.289
Fuzzy CCT Corrected p-value	0.376	0.299	0.566	0.749
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Number of Migrants in District	Total Migrants		Households Migrated	
RD Estimate	10.93	4.230	-7.671	-1.863
	(38.95)	(36.95)	(4.590)*	(3.474)
Observations	153	277	175	523
Fuzzy Conventional p-value	0.779	0.909	0.0947	0.592
Fuzzy CCT Corrected p-value	0.786	0.853	0.0493	0
Bandwidth selection procedure	CCT	I and K	CCT	I and K

Paid monthly (non-daily)	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.244	0.0450	0.239	0.0616
	(0.0581)***	(0.0526)	(0.0491)***	(0.0437)
Observations	7,962	7,680	10,395	9,869
Fuzzy Conventional p-value	0	0.393	0	0.159
Fuzzy CCT Corrected p-value	0	0.375	0	0.403
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Fraction Unemployed	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	-0.0291	-0.00857	-0.0354	-0.00839
	(0.00527)***	(0.00379)**	(0.00616)***	(0.00343)**
Observations	82,936	38,060	62,393	50,887
Fuzzy Conventional p-value	0	0.0237	0	0.0143
Fuzzy CCT Corrected p-value	0	0.0105	0	0.00256
Bandwidth selection procedure	CCT	CCT	I and K	I and K

'Number of Migrants in the District' uses the small-sample National Sample Survey 2007-8 (64th Round) that asks questions on migration. 'Household Migrated' is indicator for whether the household every migrated for any reason. 'Total Migrants' counts the number of people who may have ever left the village for any reason - the most common reasons are marriage (54%). Less than 30% of migration is for work-related reasons.

The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.

The other panels use National Sample Survey 2009-10. 'P(Earnings Reported)' is probability that earnings are reported - regresses indicator of whether earnings data is non-missing. 'Paid-monthly' is an indicator for whether the person receives earnings at a monthly (as opposed to daily) frequency. 'Unemployed' includes those who 'sought-work', those who 'did not seek but were available for work', did not work due to 'sickness' or 'other reasons.'

Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table A.11: Education and Earnings - Men

Full sample Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.911 (0.345)***	0.400 (0.285)	0.685 (0.245)***	0.399 (0.285)
Observations	16,197	29,622	34,248	29,622
Fuzzy Conventional p-value	0.00827	0.161	0.00521	0.161
Fuzzy CCT Corrected p-value	0.00285	0.183	0.000255	0.0711
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.641 (0.546)***	0.121 (0.615)	1.623 (0.501)***	0.454 (0.495)
Observations	8,047	6,767	9,638	12,517
Fuzzy Conventional p-value	0.00265	0.845	0.00119	0.359
Fuzzy CCT Corrected p-value	0.00485	0.992	0.00230	0.554
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Finished Upper-Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.166 (0.0615)***	-0.00540 (0.0533)	0.171 (0.0509)***	0.0465 (0.0412)
Observations	6,947	6,589	9,841	13,236
Fuzzy Conventional p-value	0.00697	0.919	0.000788	0.259
Fuzzy CCT Corrected p-value	0.00419	0.758	0.00520	0.661
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Log Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.356 (0.0921)***	-0.0691 (0.104)	0.366 (0.0836)***	0.139 (0.0825)*
Observations	8,047	6,766	9,638	12,516
Fuzzy Conventional p-value	0.000110	0.506	1.19e-05	0.0927
Fuzzy CCT Corrected p-value	0.00201	0.172	0.000377	6.60e-06
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for people between 16 and 75 years of age. Sample of males.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table A.12: Education and Earnings - Women

Full sample Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.204	-0.0556	0.146	-0.0477
	(0.344)	(0.300)	(0.352)	(0.283)
Observations	17,244	16,834	16,486	19,809
Fuzzy Conventional p-value	0.553	0.853	0.678	0.866
Fuzzy CCT Corrected p-value	0.864	0.840	0.953	0.676
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.616	-0.131	1.489	-0.159
	(1.099)	(0.967)	(0.904)*	(1.011)
Observations	2,213	2,128	2,945	2,026
Fuzzy Conventional p-value	0.141	0.892	0.0994	0.875
Fuzzy CCT Corrected p-value	0.127	0.736	0.0634	0.868
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Finished Upper-Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.161	-0.0593	0.156	-0.0605
	(0.0818)**	(0.0757)	(0.0894)*	(0.0821)
Observations	2,620	2,157	2,250	1,998
Fuzzy Conventional p-value	0.0493	0.434	0.0801	0.461
Fuzzy CCT Corrected p-value	0.0486	0.365	0.0246	0.433
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Reported Earnings Log Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	-0.0910	-0.119	0.0684	-0.140
	(0.162)	(0.180)	(0.136)	(0.188)
Observations	2,213	2,126	2,945	2,024
Fuzzy Conventional p-value	0.575	0.509	0.615	0.457
Fuzzy CCT Corrected p-value	0.0761	0.187	0.0651	0.181
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for people between 16 and 75 years of age. Sample of females.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table A.13: Difference-in-Differences Table

Full Sample Years of Education	Non DPEP	DPEP	Difference
Young	8.742	7.634	-1.108
	0.098	0.105	0.143
Old	6.255	4.758	-1.497
	0.118	0.096	0.152
Difference	2.487	2.876	0.389***
	0.071	0.074	0.102

Reported Earnings Years of Education	Non DPEP	DPEP	Difference
Young	8.57	7.20	-1.37
	0.14	0.15	0.20
Old	7.91	6.08	-1.83
	0.15	0.15	0.21
Difference	0.66	1.12	0.458**
	0.13	0.13	0.18

Log Earnings	Non DPEP	DPEP	Difference
Young	6.759	6.521	-0.238
	0.031	0.026	0.041
Old	7.102	6.800	-0.303
	0.031	0.026	0.040
Difference	-0.344	-0.279	0.065**
	0.023	0.021	0.031

National Sample Survey 2009-10 for people between 16 and 75 years of age.
The two dimensions for the Difference-in-Differences are district (received policy vs did not receive policy) and age (young enough to change schooling).
Table reports means for each sub-group and standard errors calculated at the district level below the means.

Table A.14: District-Age Cells

Literate	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0833	-0.0132	0.0808	-0.0139
	(0.0381)**	(0.0526)	(0.0414)*	(0.0567)
Observations	3,983	3,064	2,839	2,736
Fuzzy Conventional p-value	0.0289	0.802	0.0510	0.806
Fuzzy CCT Corrected p-value	0.0157	0.568	0.0944	0.718
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.103	-0.0238	0.104	-0.0275
	(0.0453)**	(0.0557)	(0.0464)**	(0.0571)
Observations	3,892	3,064	3,432	2,899
Fuzzy Conventional p-value	0.0224	0.669	0.0249	0.630
Fuzzy CCT Corrected p-value	0.0224	0.464	0.922	0.798
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Upper-Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.142	-0.0358	0.158	-0.0363
	(0.0557)**	(0.0562)	(0.0527)***	(0.0566)
Observations	3,908	3,080	4,798	3,057
Fuzzy Conventional p-value	0.0109	0.524	0.00278	0.522
Fuzzy CCT Corrected p-value	0.0146	0.464	0.677	0.663
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.975	-0.415	1.153	-0.430
	(0.598)	(0.673)	(0.461)**	(0.670)
Observations	3,526	3,182	6,470	3,296
Fuzzy Conventional p-value	0.103	0.538	0.0123	0.521
Fuzzy CCT Corrected p-value	0.178	0.537	0	0.831
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Log Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0922	-0.0842	0.289	-0.0738
	(0.110)	(0.138)	(0.0860)***	(0.137)
Observations	3,526	3,182	6,470	3,296
Fuzzy Conventional p-value	0.400	0.540	0.000768	0.590
Fuzzy CCT Corrected p-value	0.822	0.257	0	0.344
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10. Data collapsed to the district-age cell level. Sample of persons that reported earnings, ages between 16 and 75 years.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b).

Table A.15: Robustness: In-Progress RD Methods for Bandwidths and Standard Errors

Panel A: Bartalotti and Brummet (2017) cluster-robust variance estimation				
Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.654	-0.337	1.569	-0.0985
	(0.742)**	(0.877)	(0.650)**	(0.836)
Bandwidth	CCT	CCT	I and K	I and K
Finished Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.121	-0.0580	0.139	-0.0266
	(0.0615)**	(0.0793)	(0.0568)**	(0.0669)
Bandwidth	CCT	CCT	I and K	I and K
Finished Upper-Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.167	-0.0507	0.170	-0.0291
	(0.0766)**	(0.0659)	(0.0730)**	(0.0610)
Bandwidth	CCT	CCT	I and K	I and K

Panel B: Calonico et al. (2017) Two-sided bandwidth; district cluster-robust nearest neighbor variances				
Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.300	-0.0793	1.520	-0.790
	(0.523)**	(0.767)	(0.589)***	(0.954)
Bandwidth	MSE-2	MSE-2	CER-2	CER-2
Finished Upper Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.190	-0.0545	0.116	-0.0741
	(0.0452)***	(0.0538)	(0.0561)**	(0.0792)
Bandwidth	MSE-2	MSE-2	CER-2	CER-2

National Sample Survey 2009-10. Sample of persons that reported earnings, ages between 16 and 75 years. The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy. Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method.
Panel A: Uses the Bartalotti and Brummet (2017) method to compute standard errors at the district-age group level. The optimal bandwidths are chosen using the Calonico et al. (2014b) and Imbens and Kalyanaraman (2012) methods. I thank the authors for sharing their code.
Panel B: Uses an in-progress method developed by Calonico et al. (2017) that allows for a separate optimal bandwidth on either side of the cutoff and cluster-robust standard errors at the district level. MSE-2 is mean squared error optimal two-sided bandwidth, and CER-2 is the coverage error rate two sided bandwidth.

Table A.16: Parametric RD - Short Bandwidth

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.999***	0.769	0.911**	0.782
	(0.387)	(0.487)	(0.383)	(0.488)
Observations	128,799	124,077	128,799	124,077
R-squared	0.039	0.022	0.030	0.024
Control Function	Linear	Linear	Quadratic	Quadratic

Reported Earnings Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.406***	1.194	1.416***	1.206
	(0.498)	(0.892)	(0.493)	(0.892)
Observations	26,898	29,343	26,898	29,343
R-squared	0.026	0	0.028	0
Control Function	Linear	Linear	Quadratic	Quadratic

Reported Earnings Finished Upper Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.107**	0.0912	0.108**	0.0916
	(0.0458)	(0.0633)	(0.0455)	(0.0632)
Observations	26,899	29,346	26,899	29,346
R-squared	0.022	0	0.023	-0.006
Control Function	Linear	Linear	Quadratic	Quadratic

Log Wage & Salary Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.470***	0.358**	0.544***	0.413**
	(0.138)	(0.174)	(0.126)	(0.170)
Observations	26,894	29,342	26,894	29,342
R-squared	0	0	0	0
Control Function	Linear	Linear	Quadratic	Quadratic

National Sample Survey 2009-10. Parametric RDs using local linear and quadratic functions. Bandwidth restricted to 0.3 on either side of the cutoff. Sample of persons between 16 and 75 years.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.

Table A.17: Parametric RD - Longer Bandwidth

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.665*	0.632	0.781**	0.662
	(0.345)	(0.401)	(0.352)	(0.427)
Observations	133,669	129,192	133,669	129,192
R-squared	0.043	0.035	0.035	0.030
Control Function	Linear	Linear	Quadratic	Quadratic

Reported Earnings Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.035**	0.913	1.097**	0.992
	(0.416)	(0.716)	(0.449)	(0.789)
Observations	28,290	30,836	28,290	30,836
R-squared	0.044	0.011	0.032	-0.004
Control Function	Linear	Linear	Quadratic	Quadratic

Reported Earnings Finished Upper Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0832**	0.0711	0.0865**	0.0766
	(0.0398)	(0.0511)	(0.0418)	(0.0561)
Observations	28,291	30,839	28,291	30,839
R-squared	0.036	0.011	0.028	-0.002
Control Function	Linear	Linear	Quadratic	Quadratic

Log Wage & Salary Earnings	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.427***	0.332**	0.399***	0.305**
	(0.117)	(0.145)	(0.113)	(0.147)
Observations	28,285	30,835	28,285	30,835
R-squared	0	0	0	0
Control Function	Linear	Linear	Quadratic	Quadratic

National Sample Survey 2009-10. Parametric RDs using local linear and quadratic functions. Bandwidth restricted to 0.4 on either side of the cutoff. Sample of persons between 16 and 75 years.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.

Table A.18: Returns to Education using Two-Staged Least Squares

First-Stage Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.654	-0.381	1.569	-0.199
	(0.491)***	(0.590)	(0.417)***	(0.553)
Observations	10,175	7,997	14,277	8,630
Fuzzy Conventional p-value	0.000753	0.519	0.000168	0.719
Fuzzy CCT Corrected p-value	0.00142	0.469	0	0.217
Bandwidth selection procedure	CCT	CCT	I and K	I and K
2 SLS Log(Earnings)	**Below 35 years**	**Above 35 years**	**Below 35 years**	**Above 35 years**
Years of Education	0.155	0.567	0.208	0.744
	(0.0465)***	(0.699)	(0.0494)***	(1.706)
Observations	10,175	7,994	14,277	8,627
Fuzzy Conventional p-value	0.000856	0.417	0	0.663
Fuzzy CCT Corrected p-value	0.0394	0.269	0.569	0.813
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for persons between 16 and 75 years of age. '2SLS' regressions treats the first stage as 'change in years of education' as opposed to probability of receiving DPEP funds.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table A.19: Robustness: Widening Age Restrictions - Full Sample

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.605	0.209	0.600	0.287
	(0.166)***	(0.237)	(0.176)***	(0.225)
Observations	74,342	35,064	63,388	39,456
Fuzzy Conventional p-value	0.000266	0.378	0.000641	0.202
Fuzzy CCT Corrected p-value	0	0.262	0	0.165
Bandwidth selection procedure	CCT	CCT	I and K	I and K
Finished Primary School	**Below 35 years**	**Above 35 years**	**Below 35 years**	**Above 35 years**
RD Estimate	0.0288	0.0218	0.0429	0.0243
	(0.0176)	(0.0222)	(0.0147)***	(0.0216)
Observations	42,713	37,199	66,472	39,839
Fuzzy Conventional p-value	0.102	0.327	0.00346	0.259
Fuzzy CCT Corrected p-value	0.219	0.457	0.0599	0.456
Bandwidth selection procedure	CCT	CCT	I and K	I and K
Finished Upper Primary School	**Below 35 years**	**Above 35 years**	**Below 35 years**	**Above 35 years**
RD Estimate	0.0729	0.0276	0.0765	0.0299
	(0.0216)***	(0.0216)	(0.0178)***	(0.0185)
Observations	42,713	36,145	70,270	57,738
Fuzzy Conventional p-value	0.000754	0.201	1.81e-05	0.106
Fuzzy CCT Corrected p-value	0.00188	0.277	8.64e-09	0.344
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for sample of persons aged 15 to 100 years of age.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table A.20: Robustness: Widening Age Restrictions - For Reported Earnings

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	1.733	-0.374	1.674	-0.113
	(0.487)***	(0.589)	(0.438)***	(0.534)
Observations	10,559	8,002	12,866	9,057
Fuzzy Conventional p-value	0.000372	0.525	0.000132	0.832
Fuzzy CCT Corrected p-value	0.000668	0.477	0.000359	0.186
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Primary School	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.136	-0.0607	0.149	-0.0272
	(0.0443)***	(0.0488)	(0.0422)***	(0.0403)
Observations	9,822	8,002	10,560	11,033
Fuzzy Conventional p-value	0.00210	0.214	0.000412	0.500
Fuzzy CCT Corrected p-value	0.00178	0.121	0.000143	0.0543
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Upper Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.173	-0.0549	0.172	-0.0334
	(0.0514)***	(0.0506)	(0.0503)***	(0.0381)
Observations	9,662	7,734	10,117	13,441
Fuzzy Conventional p-value	0.000773	0.278	0.000637	0.380
Fuzzy CCT Corrected p-value	0.000549	0.236	0.000545	0.00170
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Log(Earnings)	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.272	-0.218	0.298	-0.112
	(0.0834)***	(0.105)**	(0.0747)***	(0.0956)
Observations	10,560	7,999	12,867	9,054
Fuzzy Conventional p-value	0.00109	0.0380	6.57e-05	0.242
Fuzzy CCT Corrected p-value	0.0508	0.00217	0.839	4.39e-06
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10 for sample of persons aged 15 to 100 years of age that reported earnings.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method.
'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table A.21: Robustness: Restricting to DISE districts

Years of Education	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.978	-0.0310	0.770	-0.105
	(0.269)***	(0.202)	(0.216)***	(0.177)
Observations	21,099	34,331	31,727	46,462
Fuzzy Conventional p-value	0.000275	0.878	0.000356	0.552
Fuzzy CCT Correct p-value	0	0.888	0	0.308
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.0801	-0.00677	0.0647	-0.00571
	(0.0221)***	(0.0172)	(0.0168)***	(0.0186)
Observations	21,258	46,012	35,465	37,713
Fuzzy Conventional p-value	0.000280	0.694	0.000111	0.759
Fuzzy CCT Correct p-value	0	0.387	0	0.707
Bandwidth selection procedure	CCT	CCT	I and K	I and K

Finished Upper Primary	Below 35 years	Above 35 years	Below 35 years	Above 35 years
RD Estimate	0.144	0.00521	0.126	0.00343
	(0.0314)***	(0.0180)	(0.0250)***	(0.0175)
Observations	18,169	37,713	22,612	41,715
Fuzzy Conventional p-value	0	0.772	0	0.845
Fuzzy CCT Correct p-value	0	0.867	0	0.450
Bandwidth selection procedure	CCT	CCT	I and K	I and K

National Sample Survey 2009-10.
The sample of 'Below 35 years' are of school going age during the policy, whereas those 'Above 35' are too old to change their schooling in response to the policy.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method.
'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

Table A.22: Difference-in-Differences (Full Model)

Full Sample	Years of Education	Literate	Finished Primary	Finished Upper Primary
Estimate	0.332***	0.0551***	0.0386***	0.0196***
	(0.0388)	(0.00311)	(0.00338)	(0.00363)
Observations	279,452	279,483	279,483	279,483
R-squared	0.176	0.189	0.193	0.170

Small Bandwidth	Years of Education	Literate	Finished Primary	Finished Upper Primary
Estimate	0.311***	0.0426***	0.0302***	0.0209**
	(0.106)	(0.00764)	(0.00834)	(0.00959)
Observations	144,248	144,261	144,261	144,261
R-squared	0.108	0.118	0.117	0.103

Reported Earnings	Years of Education	Literate	Finished Primary	Finished Upper Primary
Estimate	0.377**	0.0558***	0.0410***	0.0299**
	(0.155)	(0.0111)	(0.0119)	(0.0150)
Observations	66,093	66,098	66,098	66,098
R-squared	0.157	0.166	0.164	0.139

	Log(Earnings)		2SLS Returns	
Estimate	0.0596**		0.159***	
	(0.0251)		(0.0473)	
Observations	66,086		66,081	
R-squared	0.241		0.393	

	Log (Earnings) Skilled	Log (Earnings) Unskilled	Additional GE on young
Estimate	-0.0611**	0.0183	-0.0794**
	(0.0283)	(0.0213)	(0.0354)
Observations	37,748	28,338	
R-squared	0.311	0.225	

National Sample Survey 2009-10 – 17 to 75 year olds. Regressions include district and cohort fixed effects. Diff-in-diff coefficient on interaction between being below 35 and in DPEP district. Robust standard errors at the district level. 'Small Bandwidth' restricts the sample in two ways: (1) restricts ages to be +/− 15 years of the 35 year cutoff, (2) restricts districts to have female literacy $\in (-0.2, 0.2)$. '2SLS Returns' estimates two-staged least squares returns where the first stage dependent variable is the years of education, and the second stage dependent variable is log-earnings. 'Additional GE on young' estimates the GE effect that only affects the skill-premium of the young (note: this excludes the average change in wages due to changes in output, and the portion of the change in the skill premium experienced by all-cohorts).

Table A.23: Test Scores

Panel A: Reading Scores 2008	Read Letter	Read Word	Reading Level 1
RD Estimate	0.00411	-0.0158	-0.0147
	(0.0107)	(0.0118)	(0.0120)
Bandwidth	CCT	CCT	CCT
Panel B: Math Scores 2008	**Numbers 1-9**	**Numbers 10-99**	**Subtraction**
RD Estimate	0.0531	0.0197	0.0196
	(0.0116)***	(0.0136)	(0.0137)
Bandwidth	CCT	CCT	CCT
Panel C: Reading Scores 2012	**Read Letter**	**Read Word**	**Reading Level 1**
RD Estimate	-0.0143	0.0164	0.0216
	(0.0148)	(0.0141)	(0.0145)
Bandwidth	CCT	CCT	CCT
Panel D: Math Scores 2012	**Numbers 1-9**	**Numbers 10-99**	**Subtraction**
RD Estimate	0.0514	-0.0277	0.0351
	(0.0156)***	(0.0184)	(0.0183)*
Bandwidth	CCT	CCT	CCT

Source: Annual Status of Education Report (ASER) Data – years 2008 and 2012 – for children (aged 3 through 15) still in school.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method.
Variables: 'Read Letter' is if the child can recognize the letter. 'Read Word' is if the child can read the word. 'Read Level 1' if the child has achieved reading level 1. 'Numbers 1-9' if the child can identify the digits between 1 and 9. 'Numbers 10-99' can identify 10 through 99. 'Subtraction' can perform simple subtractions.

Table A.24: District GDP

Log(District GDP) 2000-06		
RD Estimate	0.137	0.190
	(0.132)	(0.126)
Observations	664	838
Fuzzy Conventional p-value	0.303	0.132
Fuzzy CCT Corrected p-value	0.316	0.141
Bandwidth selection procedure	CCT	I and K

District GDP (Rupees) 2000-6		
RD Estimate	5,346	3,711
	(2,874)*	(3,142)
Observations	1,109	650
Fuzzy Conventional p-value	0.0629	0.237
Fuzzy CCT Corrected p-value	0.0181	0.236
Mean dependent variable	17471.8	17471.8
Bandwidth selection procedure	CCT	I and K

District Domestic Product Sources: Department of Statistics and Programme Implementation, Government of West Bengal; Planning Commission; Directorate of Economics and Statistics Government of Uttar Pradesh; Department of Economics and Statistics Government of Tamil Nadu; Directorate of Economics and Statistics Government of Rajasthan; Department of Planning Government of Punjab; Planning and Coordination Government of Odisha; Directorate of Economics and Statistics Government of Maharashtra; Directorate of Economics and Statistics Government of Kerala; Planning Programme Monitoring and Statistics Department Government of Karnataka; Directorate of Economics and Statistics Government of Bihar; Directorate of Economics and Statistics Government of Assam; Andhra Pradesh State Portal.
Bandwidths: 'CCT' is the Calonico et al. (2014b) method. 'I and K' is the Imbens and Kalyanaraman (2012) method. 'CCT corrected p-value' is the bias-corrected p-values using the method in Calonico et al. (2014b)

B Derivations in the Model

B.I Education Sector

B.I.1 Supply of Public and Private Schools

Public schools want to maximize the overall access to education A_d for the students in the entire district d. The district d receives R_d from the government, and spends p_m for each input x_m into the schooling production functions. The vector of inputs at the district level $\mathbf{x_m}$ can consist of new schools, better qualified teachers, better infrastructure, more resource-centers, etc.

$$\max_{\mathbf{x_m}} \; A_d(\mathbf{x_m}) \tag{28}$$

$$s.t. \; \sum_{m=1}^{M} p_m x_m \leq R_d \,, \tag{29}$$

where $\frac{\partial A}{\partial x_m} > 0, \frac{\partial^2 A}{\partial x_m \partial x_m} < 0, \frac{\partial^2 A}{\partial x_m \partial x_n} > 0$. From the first order conditions, it is easy to derive the optimal amount of inputs of type m: $x^*_{md}(R_d, \mathbf{p_m})$, where $\frac{\partial x^*_m}{\partial R_d} \geq 0$ and $\frac{\partial x^*_m}{\partial p_m} \leq 0$. An increase in government funding R_d, thus increases the amounts of inputs into the schooling-access production function, and increases the overall access to education for the students in the district A_d.

For example, one functional form that is consistent with the setup is a simple Cobb-Douglas function:

$$A(\mathbf{x_m}) = \prod_{\mathbf{m}} \mathbf{x_m^{\alpha_m}} \,, \tag{30}$$

where $0 < \alpha_m < 1$ and $\sum_m \alpha_m = 1$.

The optimal amount of inputs of type m are therefore $x^*_m = R_d \frac{\alpha_m}{p_m}$, and the overall access to education is given by:

$$A_d(R_d, \mathbf{p_m}) = \mathbf{R_d} \prod_{\mathbf{m}} \left(\frac{\alpha_\mathbf{m}}{\mathbf{p_m}} \right)^{\alpha_\mathbf{m}} \tag{31}$$

An increase in government funding increases the overall access to education in a proportional manner under the Cobb-Douglas form.

Private schools, however, are profit maximizers with heterogeneous costs:

$$\max_{X_j} \;\; p_d \bar{\theta}_d X_j - Z(X_j) \,, \tag{32}$$

where the costs are $Z(X_j) = z_{1j}X_j + \frac{1}{2}z_{2d}X_j^2$. The supply-curve of schooling for school j is therefore:

$$Q_{jd} = \bar{\theta}_d X_j^* = \bar{\theta}_d \frac{p_d\bar{\theta}_d - z_{1j}}{z_{2d}}\,, \tag{33}$$

Since there is free entry of private schools into these regions, schools will enter until $\pi_{jd} = 0$. The marginal school, therefore will have a cost-parameter $\tilde{z_{1d}} = \bar{\theta}_d p_d$. If costs are drawn from a distribution $F(z_{1j})$, then the fraction of schools that enter the region is given by: $F\left(\bar{\theta}_d p_d\right)$.

The overall supply of private schooling is therefore:

$$S^{sy}_{pvt,d} = \int_0^{p_d\bar{\theta}_d} \bar{\theta}_d \frac{p_d\bar{\theta}_d - z_{1j}}{z_{2d}} f(\tilde{z_1}) dz_{1j} = \frac{\bar{\theta}_d}{z_{2d}}\left[p_d\bar{\theta}_d - \mathbb{E}_d(z_{1j}|z_{1j} < p_d\bar{\theta}_d)\right], \tag{34}$$

where $f(\tilde{z_1})$ is the conditional distribution of private school costs of entrants.

The aggregate profits of private schools, Π, will also be affected by changes in prices and average productivity, where the aggregate profits are:

$$\Pi = \int_0^{\bar{\theta}_d p_d} \frac{(p_d\bar{\theta}_d - z_{1j})^2}{z} dF(z_{1j}) \tag{35}$$

B.I.2 Education Market Equilibrium and Changes in Policy

The demand for schooling is determined by the household decisions, where $s^*_{id} = \frac{\beta_d - \bar{r_d} - \eta_i}{\Gamma}$. Given a distribution for $\eta_i \sim H(\eta)$, the overall demand for schooling in district d comes from households:

$$S^{Dd}_d = \int \frac{\beta_d + \Psi A_d - p_d - \eta_i}{\Gamma} dH(\eta) = \frac{\beta_d + \Psi A_d - p_d - \bar{\eta_d}}{\Gamma}, \tag{36}$$

where $\bar{\eta_d} = \mathbb{E}[\eta_i | i \in d]$. The overall supply of schooling comes from both public and private schools:[66]

$$S^{Sy}_d = \frac{\bar{\theta}_d}{z_{2d}}\left[p_d\bar{\theta}_d - \mathbb{E}_d(z_{1j}|z_{1j} < p_d\bar{\theta}_d)\right] + A_d \tag{37}$$

Here, it is clear that the supply of public-schools doesn't depend on the fees, since many do not charge fees, and profit-maximization is not the motive of public school provisioning. Together, equations (36) and (37) determine the equilibrium price and quantities of schooling in the district. Depending on the distribution of z_{1j}, a closed-form solution may be found. For example, if the conditional distribution of private school costs is uniform $f(\tilde{a}) \sim U[0, p_d\bar{\theta}_d]$,

[66] Alternatively, the public-school "supply" can be separated from the notion of access A_d. For example, the supply of public schools, specifically, could be $x^*_{school} = R_d \frac{\alpha_{school}}{p_{school}}$. Doing this, would not change the model's predictions.

then the equilibrium price and quantity is:[67]

$$p_d^* = \frac{\beta_d + (\Psi - \Gamma)A_d - \bar{\eta_d}}{\Gamma\left(\frac{\bar{\theta_d^2}}{z_{2d}}\right) + 1} \quad and \quad S_d^* = \frac{\bar{\theta_d^2}\left(\beta_d + \Psi A_d\right) + z_{2d}A_d}{\Gamma\bar{\theta_d^2} + z_{2d}} - \frac{\bar{\eta_d}}{\Gamma} \tag{38}$$

Improving access to schooling, by building newer schools or upgrading its infrastructure will reduce the marginal costs of schooling (Behrman et al., 1996; Birdsall, 1985). For example, under the Cobb-Douglas public-schooling production function, one can see that the fall in the marginal costs of schooling are directly in proportion to the increase in revenues from the government.

$$r_{id} = -R_d\Psi \prod_m \frac{\alpha_m}{p_m}^{\alpha_m} + p_d^*(R_d) + \eta_i \tag{39}$$

One can define $D = 1$ for districts that received government funds. Then the optimal years of schooling becomes:

$$S_d^* = \phi_1\beta_d + \phi_2 R_d - \frac{\eta_d}{\Gamma}\ , \tag{40}$$

where $\phi_1 \equiv \left(\frac{\bar{\theta_d^2}}{\Gamma\bar{\theta_d^2} + z_{2d}}\right)$ and $\phi_2 \equiv \left(\frac{\left(z_{2d} + \Psi\bar{\theta_d^2}\right)\left(\prod_m \frac{\alpha_m}{p_m}^{\alpha_m}\right)}{\Gamma\bar{\theta_d^2} + z_{2d}}\right)$. In equation (40) the equilibrium amount of schooling is affected by the expansion of public schooling.

B.II Elasticity of Capital

So far the model assumes (a) that capital is perfectly supplied at the rate R^*, and (b) is not skill-biased. If however, capital was fixed at a value $\bar{K_d}$ in a district, it would not change the qualitative predictions of the model, nor the parameters estimated. The average earnings for a worker with age a and skill s in district d would be:

$$\log w_{asd} = \log\left(\frac{\partial Y_d}{\partial \ell_{asd}}\right) = \log\theta_{sd} + \log\psi_a + \left(\left(\frac{1}{\sigma_E} - 1\right)\left(\frac{1}{\varrho}\right)\log Y_d - \left(\frac{1}{\sigma_E} - 1\right)\left(\frac{1-\varrho}{\varrho}\right)\log\bar{K_d}\right) + \tag{41}$$
$$\left(\frac{1}{\sigma_A} - \frac{1}{\sigma_E}\right)\log L_{sd} - \frac{1}{\sigma_A}\log\ell_{asd}\ ,$$

Here the term $\left(\left(\frac{1}{\sigma_E} - 1\right)\left(\frac{1}{\varrho}\right)\log Y_d - \left(\frac{1}{\sigma_E} - 1\right)\left(\frac{1-\varrho}{\varrho}\right)\log\bar{K_d}\right)$ is common across cohorts and skill levels. Along with Y_d, it gets differenced out in the derivation.

[67]If the supply of public schools was instead modeled as x^*_{school}, then the equilibrium quantity would be $S_d^* = \frac{\bar{\theta_d^2}(\beta_d + \Psi A_d) + z_{2d}\left(R_d\frac{\alpha_{school}}{p_{school}}\right)}{\Gamma\bar{\theta_d^2} + z_{2d}} - \frac{\bar{\eta_d}}{\Gamma}$. This would produce the same qualitative results going forward.

B.III Skill Biased Capital

In Model subsection 2.1 I introduce skill biased capital as affecting the productivity parameter θ_{sd}. Below, I explicitly model skill biased capital to show how flexible forms of introducing it do not influence the estimation strategy or results. In the following set up, the noticeable changes are where Equation (2) has been modified into Equation (44), which includes an elasticity of substitution between labor ℓ_{sd} and skill biased capital k_{sd} represented by σ_s:

$$Y_d = L_d^{\varrho} K_d^{(1-\varrho)} \tag{42}$$

$$L_d = \left(\sum_s \theta_{sd} L_{sd}^{\frac{\sigma_E - 1}{\sigma_E}} \right)^{\frac{\sigma_E}{\sigma_E - 1}} \tag{43}$$

$$L_{sd} = \left(\Lambda_s k_{sd}^{\frac{\sigma_s - 1}{\sigma_s}} + (1-\Lambda_s) \ell_{sd}^{\frac{\sigma_s - 1}{\sigma_s}} \right)^{\frac{\sigma_s}{\sigma_s - 1}} \tag{44}$$

$$\ell_{sd} = \left(\sum_a \psi_a \ell_{asd}^{\frac{\sigma_A - 1}{\sigma_A}} \right)^{\frac{\sigma_A}{\sigma_A - 1}} \tag{45}$$

Given this new set up, earnings can be represented by Equation (46), instead of Equation (3):

$$\log w_{asd} = \log \widetilde{\varrho} + \log \psi_a + \frac{1}{\sigma_E} \log Y_d + \left(\frac{1}{\sigma_s} - \frac{1}{\sigma_E} \right) \log L_{sd} + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_s} \right) \log \ell_{sd} - \frac{1}{\sigma_A} \log \ell_{asd} \tag{46}$$

This new set up does not change the estimation or the interpretation of the estimates. In the following equation, that replaces Equation (20) to estimate the GE effects on all workers, the skill-biased capital term is captured by the term L_{sd}:

$$log \frac{w_{so,D=1}}{w_{so,D=0}} - log \frac{w_{uo,D=1}}{w_{uo,D=0}} = \left(\frac{1}{\sigma_s} - \frac{1}{\sigma_E} \right) \left[log \frac{L_{s,D=1}}{L_{u,D=1}} - log \frac{L_{s,D=0}}{L_{u,D=0}} \right] + \left(\frac{1}{\sigma_A} - \frac{1}{\sigma_s} \right) \left[log \frac{\ell_{s,D=1}}{\ell_{u,D=1}} - log \frac{\ell_{s,D=0}}{\ell_{u,D=0}} \right] \tag{47}$$

B.IV Deriving Equations (22) and (23)

In Equations (22) and (23) I derive how to estimate the two different returns to education $\beta_{as,D=1}$ and $\beta_{as,D=0}$, in terms of earnings for the younger cohorts. First to derive $\beta_{as,D=0}$, we use the fact that the average earnings is a weighted average of skilled and unskilled workers:

$$log\frac{w_{y,D=1}}{w_{y,D=0}} = (\ell_{sy,D=1}\log w_{sy,D=1} + \ell_{uy,D=1}\log w_{uy,D=1}) - (\ell_{sy,D=0}\log w_{sy,D=0} + \ell_{uy,D=0}\log w_{uy,D=0})$$

$$= \ell_{sy,D=1}(\log w_{sy,D=1} - \log w_{sy,D=0}) + (\ell_{sy,D=1} - \ell_{sy,D=0})\log w_{sy,D=0} +$$
$$\ell_{uy,D=1}(\log w_{uy,D=1} - \log w_{uy,D=0}) + (\ell_{uy,D=1} - \ell_{uy,D=0})\log w_{uy,D=0}$$

$$= \ell_{sy,D=1}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=1}log\frac{w_{uy,D=1}}{w_{uy,D=0}} +$$
$$(\ell_{uy,D=1} - \ell_{uy,D=0})\log w_{uy,D=0} + (\ell_{sy,D=1} - \ell_{sy,D=0})\log w_{sy,D=0}$$

$$= \ell_{sy,D=1}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=1}log\frac{w_{uy,D=1}}{w_{uy,D=0}} + \Delta\ell_{sy}\underbrace{log\frac{w_{sy,D=0}}{w_{uy,D=0}}}_{\beta_{as,D=0}} \tag{48}$$

Similarly, I derive $\beta_{as,D=1}$ in terms of observable wage discontinuities that I can estimate:

$$log\frac{w_{y,D=1}}{w_{y,D=0}} = (\ell_{sy,D=1}\log w_{sy,D=1} + \ell_{uy,D=1}\log w_{uy,D=1}) - (\ell_{sy,D=0}\log w_{sy,D=0} + \ell_{uy,D=0}\log w_{uy,D=0})$$

$$= \ell_{sy,D=0}(\log w_{sy,D=1} - \log w_{sy,D=0}) + (\ell_{sy,D=1} - \ell_{sy,D=0})\log w_{sy,D=1} +$$
$$\ell_{uy,D=0}(\log w_{uy,D=1} - \log w_{uy,D=0}) + (\ell_{uy,D=1} - \ell_{uy,D=0})\log w_{uy,D=1}$$

$$= \ell_{sy,D=0}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=0}log\frac{w_{uy,D=1}}{w_{uy,D=0}} +$$
$$(\ell_{uy,D=1} - \ell_{uy,D=0})\log w_{uy,D=1} + (\ell_{sy,D=1} - \ell_{sy,D=0})\log w_{sy,D=1}$$

$$= \ell_{sy,D=0}log\frac{w_{sy,D=1}}{w_{sy,D=0}} + \ell_{uy,D=0}log\frac{w_{uy,D=1}}{w_{uy,D=0}} + \Delta\ell_{sy}\underbrace{log\frac{w_{sy,D=1}}{w_{uy,D=1}}}_{\beta_{as,D=1}} \tag{49}$$

C Details about DPEP Guidelines and Funding

In 1992, the Indian Parliament updated their National Policy on Education with a renewed focus on primary and upper primary education. Based on recommendations from the Central Advisory Board of Education, the Parliament amended the constitution and transferred education-related decisions to local bodies, and stressed the decentralization of decision making by helping districts plan and manage both primary and upper primary education.[68]

In 1994, the District Primary Education Project (DPEP) was introduced in seven states and 42

[68] Primary is usually grades 1 through 4 or 5, and upper primary is grades 5 or 6 through 8.

districts, and was over time expanded to 271 of approximately 600 districts in the country. The project spanned four phases, the last of which were implemented in the mid-2000s. While a portion of the funds were released under DPEP through the mid-2000s, the bulk of the funding ended in 2005 when other policies under the newer Sarva Shiksha Abhiyan (SSA) were growing in strength.[69]

The funding largely came from international donors like the World Bank, the European Commission (EC), the U.K. Department for International Development (DFID) and Official Development Assistance (ODA), the Royal Government of the Netherlands, and UNICEF. In general, India has received aid on various social and infrastructure programs, and in 2005-6 alone it received $4 bn (Colclough and De, 2010). By 2002 the World Bank alone had committed about $1.62 bn on DPEP, whereas the other donors concentrated on certain states. For example, in the first few years of the program, the EC spent ECU 150mn in Madhya Pradesh, the Netherlands spent $25.8 mn in Gujarat, DFID spend 80 mn pounds in Andhra Pradesh and West Bengal, whereas UNICEF spent $ 153 mn in Bihar (GOI, 2000). World Bank (1997) claims that in 1993, the EC provided a grant of ECU 150 mn, whereas the World Bank approved credits of $265 mn in 1994 and $425 mn in 1996. At the time of the transfer to the wider SSA program in 2004, the World Bank's contribution consisted of less than half of the external aid funds, with DFID and the EC being the other major donors. Between 2004 and 2007 alone, about $7.8 bn was spent on the expanded SSA program, including the Government's contributions (Ayyar, 2008).

Other than building schools and hiring teachers, an additional objective was to improve the access to primary and upper primary education by establishing district institutions to decentralize planning. Specifically, this was to be done by managing the delivery of education, including teacher support and materials development through Block Resource Centers (BRC) and Cluster Resource Centers (CRC), and strengthening the District Institutes of Education and Training (DIET). This also included targeted interventions for girls and minority groups, and the expansion of Early Childhood Education (ECE). The program established a DPEP Bureau in the Ministry of Human Resource Development that served as a financial and technical intermediary. They appraised, monitored and supervised the district programs. The programs were developed by each participating district and appraised by the Bureau that also provided implementation support. The programs were evaluated and the poorly performing subprojects are dropped.

Of the approximately 160,000 new schools, more than 84,000 were 'alternative' or 'community schools.' Alternative or community schools are part of the non-formal schooling system. They provide the basic schooling infrastructure to remote areas and disadvantaged groups with the help of the local community. The guidelines of the policy also discussed the local community initiatives in promoting enrollment and retention. For example, Village Education Committees

[69] SSA was similar to the DPEP, but covered the entire country. There were, however, certain programs under SSA that targeted certain sub-districts.

and local bodies like the Mother-Teacher Associations were tasked with creating local awareness campaigns and getting more children into schools and preventing them from dropping out of schools.

D Data Appendix

DISE: Data for inputs into schools comes from the District Information System for Education (DISE), which was established to collect data at the school level in order to inform policy makers in the Indian government about the bottlenecks in the education sector. While a limited number of their variables are available freely at an aggregated level, the bulk of their interesting data is obtainable only at a school-by-school basis on their website. I therefore collected 10% of the data, stratified by year, on a school-by-school basis and compiled it for each school separately. DISE claims to cover all the schools in the country (about 1.45 million schools in 2014) each year between 2005 and 2014, and consists of detailed information on number of schools, when they were built, whether they are public or privately owned, number of teachers by levels of education, and various infrastructural features. The DISE data was initially meant to cover only in DPEP districts, but was expanded to cover the rest of the country in the early 2000s. The data is collected by head teachers, and verified by cluster resource coordinators and block educational officers. Cross verification is done by head teachers of one school for another, and by Department of Education officials. See table 1 for summary statistics for the year 2005.

Census data has a limited number of outcome variables, including literacy by gender and rural-urban status. The Census has detailed tables at all three of the administrative levels - states, districts and sub-district. A panel of sub-districts can be created using the 1991, 2001 and 2011 Census years, all of which include sub-district-level statistics. The panel is particularly challenging because of splits and merges in various districts, so I used detailed information on administrative areas to compile the panel. The 1991 Census determines the running variable for the RD, since the 1991 female literacy rate was used to determine which districts are eligible for DPEP funds. I calculate this female literacy rate in 1991 for females above 6 years old, and exactly replicate the numbers highlighted in the DPEP reports.

National Sample Survey (NSS): I use household surveys to study the impact on education, earnings, expenditures, migration and other labor market characteristics. The National Sample Survey (NSS) is a nationally representative survey used by many researchers studying India. It is the largest household survey in the country, and asks questions on weekly activities for up to five different occupations per person, and earnings during the week for each individual in the household. The NSS asks detailed questions about thirteen different levels of education, which I convert into years for some of the analysis. There is also a consumption module which asks detailed questions on expenditures on various goods, including education-related expenditures, with a 30 day recall period. The probability-weighted sample is constructed using a two-staged stratified sampling procedure with the first stage comprising of villages and block, and the second stage consisting of households. Households are selected systematically with equal probability, with a random start.

I use three different rounds of the NSS data. The 2004-5 "thick" round is the last large-sample round while the policy was still in place. This allows me to get at costs of education from the household side. The 2007-8 small-sample "thin" round asks detailed questions on migration, which I use to test the effect of this policy on migration decisions as well. The main dataset, however, is the 2009 round, which was used to study the longer term impacts of the DPEP policy. The 2009 round is the first large-sample round after the end of the DPEP program, and has the added advantage of allowing enough time for students affected by the policy to become a part of the labor market. Summary statistics for the 2009 NSS round are presented in Table 2. In my analysis, I restrict individuals to be between 17 and 75 years of age, and the results are robust to relaxing these constraints.

Annual Survey of Industries (ASI): To study the behavior of firms, I use the Annual Survey of Industries (ASI), which is a census of all manufacturing firms in the country that employ more than ten persons. This data is available at the establishment level, and has information on the type of products produced, wages paid, and number of employees among other things. One can then use this data to study whether changes in the skill level of the population can affect firm mobility and production decisions.

Annual Status of Education Report (ASER): To study the impact on test scores, I use a geographically comprehensive data set that consists of a household survey done by an NGO (Pratham). The survey focuses on children in the age group 3-16. It surveys children at home – whether they went to government school, private school, religious schools and even dropouts. The focus of the testing is on the ability to read simple texts and do basic arithmetic.

District Domestic Product (DDP) Data: DDP data is compiled from each state's statistical office and made into a panel. The series is for gross (rather than net) domestic product, and the base year is the year 2000. The various statistical offices are: Department of Statistics and Programme Implementation, Government of West Bengal; Planning Commission; Directorate of Economics and Statistics Government of Uttar Pradesh; Department of Economics and Statistics Government of Tamil Nadu; Directorate of Economics and Statistics Government of Rajasthan; Department of Planning Government of Punjab; Planning and Coordination Government of Odisha; Directorate of Economics and Statistics Government of Maharashtra; Directorate of Economics and Statistics Government of Kerala; Planning Programme Monitoring and Statistics Department Government of Karnataka; Directorate of Economics and Statistics Government of Bihar; Directorate of Economics and Statistics Government of Assam; Andhra Pradesh State Portal.

Creating the Panel: Due to splits and merges, and other changes in district boundaries, creating a consistent dataset is a non-trivial task. Only 41% of districts were unaffected by changes in district boundaries between 1991 and 2009. Of the 607 districts in the 2009 NSS household survey data, 571 were successfully merged with the 1991 Census (to obtain the running variable) and the list of DPEP districts. This merging was done based on administrative Census reports and shapefiles using Arc-GIS. Of these, 551 were merged with the manufacturing industries ASI data (the other twenty districts had no manufacturing firms). The school-level DISE dataset only covers 408 of these districts since the schools were surveyed only in the larger states. The household-level results will therefore be shown for both the entire dataset and the sub-sample of DISE districts only as well.

E Other Impacts on the Education Sector

Since, under DPEP, funding was stepped up to districts below the cutoff, there may have been a crowd-out of other funds that schools were supposed to receive. The Teachers Learning Material (TLM) grant is funding that is available to schools regardless of whether they lie in DPEP districts or not. In the top panel of Appendix Figure A.18, one can see that regions that were eligible for DPEP systematically spent less TLM funds, showing the possibility that other funds were actually crowded out when DPEP funds were allocated. Appendix Figure A.19 shows that DPEP regions both received less and spent less TLM grant money. One significant change in the DPEP regions is the introduction of pre-primary sections, which was thought to be a good way to get children into schools at a young age. Many more schools in DPEP regions have such pre-primary sections after the policy, and there are more pre-primary teachers and students in these schools (Appendix Figures A.18 and A.20).

Under the DPEP regional educational centers called Block Resource Centers (BRCs) and Cluster Resource Centers (CRCs) were built, with facilities for training teachers, and other learning materials that teachers could access. There were also government officials at these centers who would visit the schools, and could assist with teacher training at these schools. In Appendix Figures A.18 and A.21, it is clear that the distance to the closest center was lower for DPEP regions, since many more centers were built under DPEP. Over time, however, once the funding was reduced, centers continued to be built in non-DPEP regions, and the differential effect dissipated. In the lowermost panel, however, one can see that the number of academic inspections and visits by center officials was, over time, consistently higher in treated areas.

English-medium education may have greater potential returns in urban labor markets but higher costs for the students who are unfamiliar with the language. At the same time, Hindi-medium education may be valued elsewhere in the country, whereas regional languages are only valued in certain localized areas. In the left panel of Figure A.22, one can see that the schools in DPEP regions are more likely to be Hindi-medium and less likely to be in regional languages. While the discontinuity is slight, there is sharp evidence of a kink indicating that the relationship between the medium of instruction and literacy changes across the DPEP cutoff.