Does Demand or Supply Constrain Investments in Education? Evidence from Garment Sector Jobs in Bangladesh[*]

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We study the effects of explosive growth in the ready-made garments industry in Bangladesh (which offers employment opportunities for women) on young girls' school enrollment. A triple difference identification strategy compares girls' enrollment to locations not as exposed to factories, over time as the sector grows, and relative to enrollment decisions of male siblings. We find statistically and quantitatively significant increases in the enrollment of 5-10 year old girls. In contrast, a roughly simultaneous supply-side intervention (a female schooling subsidy) does not have as significant an effect on enrollment. Research on education policy has a larger focus on improving the quantity and quality of educational inputs, but in this context, demand plays a key role in enrollment decisions.

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1. Introduction

Investment in schooling is central to the development process (Lucas, 1988; Galor & Weil, 2000), and human capital accumulation is thought to be a key driver of economic growth (Mankiw et al., 1992; Jones, 2011). A micro-development literature informs macroeconomic analyses of human capital and growth by examining the determinants of educational attainment at the level of households and villages. This literature has generated important, credible evidence on schooling choices based on either natural or policy experiments, or explicit randomized control trials of programs that build schools, provide inputs, improve school quality or supply parents with cash transfers if children attend school.[1] Implicit in this literature's focus on schooling inputs is a belief that improving educational attainment in developing countries requires fixing supply gaps in schooling. Governments and donors who view increasing enrollments as a key development priority have also focused on supply-side strategies.[2] Even in the United States, education policy is tilted in favor of the supply side.[3] There is comparatively little evidence on the role of parental demand for schooling in determining educational investments, even though there exists a strong minority view that variation in demand may be the key factor that ultimately determines when and where good

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[1] See, for instance, Burde & Linden (2010) on building schools, Duflo et al. (2008) on decreasing class size and tracking, Duflo et al. (2009) on rewarding teachers for attendance, Glewwe et al. (2009) on providing textbooks, Banerjee et al. (2007) on remedial education programs, Muralidharan & Sundararaman (2011) on teacher incentive pay, or Glewwe et al. (2004) on flipcharts. Kremer & Holla (2009) reviews the literature and summarizes lessons from randomization evaluations of education sector projects in developing countries. Rawlings & Rubio (2005) provides a summary of the estimated effects of conditional cash transfers. Examples of evaluations based on natural or policy experiments include Duflo (2001) and Urquiola (Urquiola, 2006).

[2] The second U.N. Millennium Development Goal on universal primary schooling places a priority on ensuring that "there are enough teachers and classrooms to meet the demand" (United Nations, 2010). A report on education in India notes 95% of all Indian children has access to a school within half a mile (PROBE Team, 1999).

[3] The 2002 No Child Left Behind Act ties financing to school performance, the U.S. Department of Education 'Blueprint for Reform' focuses on teacher quality (U.S. Department of Education, 2010), and President Barack Obama proposed in his 2012 'State of the Union' address that "…every state require that all students stay in high school until they graduate or turn 18" (Obama, 2012). A large academic literature also focuses on returns to teacher quality and other schooling inputs (Chetty et al., 2012).

schools endogenously emerge (Pritchett, 2001; Easterly, 2002).[4]

Against this backdrop, this paper studies the relative effects on girls' enrollment of a large-scale supply-side schooling intervention, and the coincidental growth of a major demand-side influence on schooling in Bangladesh. The geographic and temporal context of this research is very important for three reasons. First, Bangladesh experienced rapid increase in girls' educational attainment during this period, both in absolute terms and relative to boys (see Figure 1). This allowed the country to surpass the third Millennium Development Goal of gender equity in enrollments, a goal that many other countries in Western Asia and sub-Saharan Africa continue to struggle with. Our research design permits a study of investments in girls relative to boys, which is of considerable policy (Levine et al., 2009; Chaaban & Cunningham, 2011; Gibbs, Nancy. 2011; Girl Up, 2011; World Bank, 2011a; World Bank, 2011b) and also academic interest, given the comparative advantage girls possess in skilled tasks (Pitt et al., 2011). Our results provide one hitherto unexplored explanation for the accelerated gender equity in education in Bangladesh, and this was important to uncover because other developing countries are trying to emulate this success.

Second, the supply-side intervention we study is a large-scale (US$15 million per year) Female Stipend Program (FSP) run by the Bangladesh government with multilateral donor (World Bank, Asian Development Bank) support. The program has paid for 2 million girls to remain in school, and is emblematic of a number conditional cash transfer programs currently in operation throughout the developing world.[5] The dramatic improvement in girls' enrollments in Bangladesh in

[4] For the purposes of this paper, we define the "supply side" as fixing imperfections in schooling access, inputs and quality (including parents lacking funds to send children to school), while the "demand side" are the conditions in the market that determine the returns to investing in education.

[5] Conditional cash transfer (CCT) programs with either health or educational conditionalities have become immensely popular around the world after the documented success of Mexico's *Progresa* program (Gertler, 2004; Schultz, 2004). Over 12 million Brazilian households are enrolled in *Bolsa Família* CCT program, which has been credited with helping Brazil make huge strides in poverty reduction (The Economist. 2010). The idea has now been replicated in the United

the past 30 years has frequently but casually been attributed to the FSP,[6] and a rigorous evaluation of this program with appropriate control groups is therefore important for policy.

Third, the demand factor whose effects on education we study is the rapid expansion of the garment industry which currently employs over 3 million workers in Bangladesh (BGMEA 2010), and which provides employment opportunities to women in a country where women traditionally have not worked outside the home. Since the better jobs within factories require the ability to read English and do basic math (Amin et al., 1998; Zohir, 2001; Paul-Majumder & Begum, 2006), garment jobs reward cognitive skills and therefore increases the returns to education. Younger girls in particular (who are still too young for the factory jobs and do not face the temptation to drop out and begin earning immediately) may respond by investing in education.

The sector was virtually non-existent in 1980 (Mostafa & Klepper, 2009), but grew 17% per year since inception, and now accounts for over 75% of Bangladesh's export earnings (Bangladesh Export Processing Bureau, 2009). Studying the effects of such remarkable growth in an export-oriented sector in a developing economy is valuable in itself, and contributes to a literature on the effects of trade openness on development (Rodriguez & Rodrik, 2000; Verhoogen, 2008; Atkin, 2011; Brambilla et al., 2011). McKinsey and Company has estimated that growth in Bangladesh's garment exports will require an additional 3.5 million workers by 2020 (The Daily Star, 2011).

We identify the effects of the garment sector growth on enrollment decisions using a triple

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States in the form of *Opportunity NYC*, a privately-funded $63 million pilot initiative. A non-exhaustive list of CCT programs that have been evaluated carefully are in Malawi (Baird et al., 2011), Ecuador (Paxson, 2010), Nicaragua (Maluccio & Flores, 2005), Colombia (Attanasio et al., 2005), Honduras (Hoddinott, 2010), and Brazil (Morris et al., 2004).

[6] For instance, the International Development Association (International Development Association, 2009) posted a write-up on its website entitled "Stipends Triple Girls Access to School", in which all of the increase in girls' enrollment between 1991 and 2005 was attributed to the stipend. Since it did not have the data to estimate the counterfactual rise in girls' enrollment absent the program, it was not able to substantiate the claimed effect size. A World Bank internal report writes "There is no systematic evaluation that shows the causal effect of the program on increased enrolment of girls in schools, yet nothing else can explain the exponential increase in gender parity."

difference estimation strategy. Using retrospective panel survey data on school enrollment and factory growth in rural Bangladesh, we analyze girls' enrollment in villages within commuting distance of certain garment factories relative to (a) villages in the same sub-districts that are further away, (b) enrollment in earlier years (taking advantage of the explosive growth of the garment sector over this period), and (c) their male siblings. Garment production is labor-intensive, employs many women who sew, and was a much larger innovation in the labor market for girls than for boys (Amin et al., 1998; Kabeer & Mahmud, 2004), which is why we analyze enrollment of girls relative to boys. The basic strategy is depicted visually in figure 3: girls' enrollment in factory-proximate villages was similar to control villages before the arrival of the garment sector in the early 1980's, but was greater in factory-proximate villages afterwards. In contrast, boys' enrollment remained similar in treatment and control villages throughout the period of industrial growth.

The arrival of garment factory jobs could increase educational attainment either through girls enrolling in school with hopes of obtaining well-paying garment jobs which require numeracy and literacy, or through increasing the wealth of parents (especially, mothers) working in the sector. Conversely, older girls may be more likely to drop out of school to access the factory jobs. To separate this latter mechanism from the increased enrollment effects, we analyze heterogeneity in the treatment response by age groups. We find that the arrival of garment jobs increases schooling for younger girls only (and is statistically significant for ages 5-10). A ten percent increase in garment jobs leads to a 1.4 percentage point increase in the probability that a 5-year-old girl is in school. There is a roughly zero average effect for older girls, with a negative point estimate for 17 and 18 year olds, some of whom likely drop out of school to take the jobs right away.

This pattern of heterogeneity in effects by age also makes it less likely that the increased enrollment is derived entirely through an income or wealth channel. But we isolate the demand-for-education effect more directly by studying children's school enrollment separately for families in which the mother took advantage of the garment factory work, and for families where the mother did not work outside the home. The wealth effect would be more prominent for the former set of families, and we do see evidence of stronger enrollment responses in that subset. However, we show that proximity to garment factories leads to greater schooling among young girls even in the sub-sample of families where mothers did not work. This suggests that enrollment rises due to a greater demand for skills when factory jobs arrive, in addition to any positive wealth effects derived from those jobs.

We estimate effects of the Female Stipend Program (FSP) with a regression discontinuity at the time of the program inception and conclude that once we take into account the general upward trend in girls' education, the program had negligible effect on the households in our survey. Figure 1 anticipated this finding – the increasing trend in girls' schooling pre-dates the introduction of the FSP, and the post-FSP period looks like a continuation of the pre-trend, with no obvious differential change. Overall, while yearly enrollment data we have make it difficult to precisely estimate the effects of the FSP, our results still suggest that in villages within commuting distance to garment factories, the garment sector had a larger effect on girls' enrollment than did the FSP.

Next we examine downstream outcomes associated with greater school enrollment and access to factory jobs, and find that girls exposed to garment factory openings (within a commutable distance to their village) earlier in life are less likely to get married at an early age (e.g. 15 or 16). They are also less likely to bear children at an early age. Other research has documented large negative welfare implications of early marriage and early childbirth (Geronimus & Korenman, 1992; Ribar, 1994; Jensen & Thornton, 2003; Hotz et al., 2005; Ashcraft & Lang, 2006; Fletcher & Wolfe, 2009).

Our analysis and results make four contributions to the literature. First, much of the recent literature on education demand in developing countries studies the effects of changing the *perceptions of* the returns to education through informational interventions.[7] We analyze enrollment decisions in a setting where actual returns to education were improved. Second, other closely related studies have examined schooling decisions after the returns to specific types of skills improved in India, such as farmer comprehension of new agricultural technologies (Foster & Rosenzweig, 1996; Badiani, 2009), or English language skills that improve access to IT service jobs (Munshi & Rosenzweig, 2006; Oster & Millett, 2010; Shastry, 2011). We complement this literature by providing estimates in a different country where the returns to education improved because manufacturing growth led to a greater demand for basic, generalist skills like literacy and numeracy.

Third, studying the enrollment effects of a roughly simultaneous supply-side initiative and a demand-side shock on the same population in the same context allows us to gauge the *relative* importance of demand and supply constraints in preventing investments in education. The previous literature has considered either demand or supply factors in isolation. Fourth, studying the demand for education can help us interpret the economic mechanisms underlying results from the large and influential program evaluation literature on the supply of schooling inputs (Kremer & Holla, 2009). As a simple example, Duflo, Hanna and Ryan (2009) find that supplying cameras to monitor teachers improves the quality of schooling inputs by reducing teacher absenteeism, but Banerjee and Duflo (2011) report that the same intervention is an ineffective cure for nurse absenteeism. The stark difference in findings across the two contexts may be related to the fact that the supply-side intervention is only effective when there is already a latent demand for the product (i.e. higher quality schooling rather than better health services) being generated through the input. Developing

[7] Jensen (2010a), Jensen (2010b), Nguyen (2008), and Dinkelman and Martinez (2011) conduct randomized controlled trials aimed at changing parents' and children's perceptions of the returns to schooling.

a theory of human behavior based on such contradictory empirical findings might benefit from an understanding of the way demand and supply sides of that market interact.

The rest of the paper proceeds as follows. Section 2 provides background on the FSP and the garment industry's relationship with girls schooling. In section 3 we describe the empirical strategy we use to estimate the effects of the FSP and garment industry. Section 4 gives results, and section 5 concludes.

2. Background on the Female Subsidy Program and Garment Industry

2.1. The Demand Side: Growth in the Garment Sector in Bangladesh

As shown in Figure 3, the Bangladeshi garment industry has experienced explosive growth in the past 30 years. In 1983 there were 40,000 people employed in the industry; since then an average yearly growth rate of 17 percent has resulted in a current employment of over 3 million (BGMEA 2010). Approximately eighty percent of garment workers are female, and garment jobs often represent females' only option to work outside the home. The jobs are labor intensive in that the garments are mostly sewn by individuals using basic sewing machines.

There are several channels through which the arrival of garment jobs could affect girls' schooling. The first is that better jobs within factories require education. Supervisors must be able to keep written records, and educated workers on assembly lines can more easily learn new work from a pattern than from than watching it be done, which allows them to fill in for absent workers. Indeed, some factories administer English or arithmetic tests to job applicants (Amin et al., 1998). Education is a requirement for almost all factories in the Export Processing Zone (Zohir, 2001), which tend to be highly desirable garment sector jobs with good working conditions and some benefits such as health care. In other factories, illiterate workers are hired, but cannot advance beyond entry level positions if they do no have education (Paul-Majumder & Begum, 2006). In all positions, production takes place in teams (Heath, 2011), and therefore requires effective communication and coordination across individuals.

Since education is rewarded in garment factories, when a new job arrives, if parents assume it will persist, they may choose to keep their pre-working age daughters in school with the hopes that their daughters will later be able to secure a better garment factory job. Afsar (1998) argues that parents respond to the returns to education in the garment industry: "both urban and rural poor educate their girl children with an intention to engage them in the garment industry." (cited in Paul-Majumder & Begum, 2006, p. 7).

At a descriptive level, our data on garment workers does show a positive correlation between education and wages: In a simple Mincer wage regression controlling for age and experience, wages are 3.67% higher for each extra year of education. This does not necessarily imply a causal effect if education given standard identification concerns, but parents may respond to this observed correlation in their educational investment choices. There is also a positive correlation between proximity to garment factories and access to garment jobs: 31.7% of women ages 16 to 50 in garment-proximate villages work in garment factories, versus 1.8 percent of women of the same age in our sample of control villages. The modal garment worker in our sample is a 26-year old married female without a child who has 6 years of education and 3 years of work experience (see table 1). She has 2 extra years of education relative to other workers in her village (p-value < 0.001) and three extra years (p-value < .001) relative to workers in villages in her sub-district that are not in close proximity to garment factories.

Garment jobs could also increase girls' schooling through income effects if their parents get jobs in the industry. Furthermore, the arrival of new labor force opportunities for females could also impact the bargaining power of women, even those who are not working in garment factories by improving their outside option. However, the garment industry also has the potential to decrease girls' schooling if girls drop out to take jobs in factories. Even though officially the minimum age to work in the factories is 16, anecdotal evidence suggests that this has not always been enforced.[8] The direction of the effect of garment jobs on girls' schooling therefore likely varies by age, as older girls face greater temptation to drop out in order to take advantage of a factory job.

The garment sector has been an increasingly important part of the Bangladesh economy. In fiscal year 2004-05, it accounted for 75% of exports and 11% of total GDP, growing to 79% of exports and 14% of GDP in 2008-09 (Bangladesh Bureau of Statistics, 2010). Employment statistics have reflected this growing importance: while agricultural employment fell from 62.1% of the employed population to 48.1% from 2000 to 2005, employment in industry grew from 10.3% to 14.5% over the same time period (UNData, 2011). These gains have been particularly large for women. Of the approximately 2 million people employed in the garments sector (as of 2007), 80% are women (Khatun et al., 2007). In contrast, women's share of employment in non-export industries was only 7% as of 1993 (Paul-Majumder & Begum, 2000). Women's labor force participation overall is quite low, at only 12%, as compared to 82% for men; yet 60% of job creation for women from 2000 to 2005 occurred in urban areas, most typically in the textile and apparel industries (World Bank, 2008).

2.2. The Supply-side Intervention: A Stipend Program for Girls' Schooling

The Female Stipend Program (FSP) was piloted in a sample of rural villages in 1991 and became nationwide in rural areas in 1994. The program gives a monthly stipend (ranging from $0.64 in Grade 6 to $1.50 in Grade 10) to female students in rural areas who maintain attendance rates of at least 75 percent, achieve 45 percent marks on term and annual exams, and remain unmarried

[8] This is particularly true before U.S. Senator Tom Harkin proposed the Child Labor Deterrence Act in 1993.

(Liang, 1996). The stipend money is directly deposited in an account in the girl's name in the nearest Agrani Bank, a nationwide system of rural banks. A girl successfully completing all five years leading a Secondary School Certificate (SSC) will have received \$107. In addition, the girl's school is directly paid all of her tuition by the project. The stipend that the girl receives is expected to cover about 30-54 percent of all non-tuition direct educational expenses (textbooks, uniforms, stationary, exam fees, transportation to/from school).

The program is most similar to interventions in other countries that target girls' enrollment, such as the female fellowship program in Pakistan (Kim et al., 1999) and a school voucher program for girls in Colombia (King et al., 1999). The ratio of stipend levels to average income in the FSP is low relative to other Conditional Cash Transfer programs: FSP amounts to 0.8 percent of the income of beneficiaries; whereas the well-known Oportunidades Program in Mexico represents 21.8 percent of the income of beneficiaries (Fiszbein & Schady, 2009). It would therefore be surprising if the FSP had such large effects on household behavior as has been informally claimed in policy reports (e.g. World Bank, 1993; International Development Association, 2009). However, other projects have documented large effects on welfare of similarly small transfers in rural Bangladesh (Bandiera et al., 2011; Bryan et al., 2011), so this is worth empirically investigating. The FSP program is costly to administer (despite the relatively low payments to beneficiaries), and represents up to 13 percent of the total national education budget during certain years with much foreign aid funding allocation (BANBEIS, 2008). An assessment of the true effects of the FSP is therefore important for policymakers who are assessing the most efficient use of government funding.

3. Empirical Strategy

3.1 Data

The data in the survey come from a survey of 1395 households conducted by the authors in sixty villages in four subdistricts of Bangladesh: Savar and Dhamrai in Dhaka District; Gazipur Sadar and Kaliakur in Gazipur district.[9] For each surveyed household, we gathered information about the schooling history of all offspring of the household head and spouse: age that the child began schooling, timing and length of any interruptions in schooling, and eventual years of completed education. These data allow us to construct a binary variable for whether a child was enrolled in school in a given year, from ages 5 to 18, and this will serve as our primary dependent variable of interest. We also know the entire history of each child's location (i.e. in- and out-migration history) for this sample of about 1400 individuals (including 713 girls), which allows us to construct child-specific measures of exposure to garment sector jobs. This leads to a sample of 10,433 child-year observations useful for analysis. For a small part of the analysis reported in section 4.3 (on marriage and childbearing), we take advantage of the entire sample of 3,030 females in these 1395 households whose marriage and childbearing status (but not the entire enrollment history) is known.

3.2 Identifying the Effects of Garment Jobs

We identify the effect of access to garment sector jobs on schooling using a triple difference strategy (by a village's proximity to garment factories; over time as more factories open; and by gender as the factories represent new opportunities for girls more so than for boys). We add household fixed effects, so that comparisons are based only on siblings within the same household. In other words, we compare how a girl's school enrollment propensity changes relative to her brother when garment jobs arrive in a village over time, and difference out that same diff-in-diff comparison in a nearby non-garment village. We take advantage of the fact that some individuals have greater exposure to garment sector jobs compared to their siblings on the basis of whether they are school-aged when the factory growth occurs, but that in other villages not exposed at all, that

[9] For more details on the survey, see Heath (2011)

same differential does not exist for an analogous pair of siblings.

First Difference:

The first of the three components of this identification strategy exploits the fact that 44 of our villages are within commuting distance of a garment factory and 16 were not.[10] Since garment factories are not placed randomly, it is important to acknowledge the pre-treatment differences between garment-proximate villages and non-garment villages. Table 2 provides summary statistics of some differences between garment and non-garment villages before the takeoff of the garment industry in the early 1980's. The garment villages are on average 1.8 km away from Dhaka, versus an average distance of 6.8 km for non-garment villages. There are also differences in educational attainment of adults over 50 (who would have finished school before the garment industry began), though they are stronger for males. Specifically, males over 50 in garment villages have an average of 3.48 years of schooling (vs. 1.94 in non-garment villages), while females in garment villages have an average of 0.82 years of schooling (vs. 0.54 in non-garment villages).

Difference in Differences:

However, if these baseline differences in are captured by a dummy variables for garment village (and an interaction of that dummy with an indicator for female) then we can still recover estimates of the effects of the growth in the garment industry on enrollment. Identification would only be threatened by differential enrollment trends in garment vs. non-garment villages. Section 4.1 provides some evidence against such trends.

To minimize problems associated with the endogenous selection of specific villages where

[10] This distinction was made by a knowledgeable industry affiliate based on the location of factories in the year 2009. As pointed out in section 2, a check that the classification does actually reflect the villages in which workers can live at home to work in garment jobs comes from comparing the percent of women ages 16 to 50 working in garment factories in garment versus control villages: 1.8 percent in non-garment villages versus 31.6 in the garment villages. Of course, to the extent that parents in non-garment villages also responded to the arrival of garment jobs, our estimates represent an underestimate of the effects of the arrival of garment jobs.

garment factories might locate, we measure each village's change in exposure to garment jobs using the Bangladeshi national growth rate in garment sector employment. In other words, we assume that garment jobs in our sample villages grew at the nationwide rate. This allows us to circumvent concerns about the reasons why specific villages within the garment-proximate areas may have experienced more rapid growth in factory openings. This leaves us with a less-precise measure of village-specific factory growth (which makes it more difficult to detect statistically significant effects of factory growth on enrollment), but we avoid endogeneity concerns associated with new factory openings near some specific villages.

Triple Difference:

The double difference strategy using changes in exposure to garment jobs over time would allow us to identify the effect only under the assumption that garment-proximate and other (control) villages did not experience differential growth in other variables over time that could also affect school enrollment. If the existence of factories close to garment-proximate villages subsequently led to greater road and other infrastructure investments that in turn facilitated schooling in those areas, then this assumption would be invalidated. We therefore introduce a third difference in our estimation strategy that exploits the fact that the garment industry represented a larger, more fundamental change in the economic environment for females. Historically, boys have had many more opportunities to work outside the home compared to girls in the rural Bangladesh context. Restrictions on women's mobility have meant that women have been confined to home labor, and women's labor force participation has been quite low, at 11% in 2000 as compared to 82% for men (Kabeer & Mahmud, 2004; World Bank, 2008). The growth of the garment sector therefore represents a much larger labor market innovation for girls. This insight leads us to adopt a triple difference identification strategy that compares changes in girls' enrollment to changes in boys at the time of the arrival of garment sector jobs. This is useful for identification because the investments in infrastructure in garment-proximate villages that might threaten our interpretation of the factory jobs on enrollment would be equally likely to affect boys' and girls' enrollment patterns. The girl-boy comparison would therefore difference out such factors. The remaining objects of concern would be investments that are gender-specific. Not only is it difficult to think of infrastructure as being gender-specific, but if one gender happens to have greater use for infrastructure like roads in a traditional Muslim society, then it would be boys (who can travel more freely). And that would make it less likely that we find stronger female enrollment response to garment sector growth, and imply that the effect we document is an under-estimate.

3.3 Estimating Equation for the Effects of Garment Jobs on Enrollment

To summarize, we estimate the following equation for child *i* in family *f* living in village *v* at year *t*:

$$\begin{aligned} Enroll_{ivft} = {} & \beta_0 + \delta_f + \lambda_t + \lambda_t \times Female_{ivft} + \beta_1 Age_{ivft} + \beta_2 Female_{ivft} \\ & + \beta_3 Female_{ivft} \times Age_{ivft} + \beta_4 Garment\ Village_{ivft} \times Female_{ivft} \\ & + \gamma_1 \log(Garment\ Jobs)_t \times Garment\ Village_{ivft} \\ & + \gamma_2 \log(Garment\ Jobs)_t \times Garment\ Village_{ivft} \times Female_{ivft} + \varepsilon_{ivft} \end{aligned} \tag{1}$$

We include household (or sibling) fixed effects (δ_f) and year fixed effects interacted with a dummy for female ($\lambda_t \times Female_{ivft}$), which allows for flexible gender-specific time trends in enrollment. We also control for different baseline enrollments for females in garment villages by including an interaction between a female dummy and an indicator for garment village.

γ_2 is the parameter of interest, which measures the effects of garments jobs on girls' enrollment (relative to boys) in response to the number of garment jobs available. This parameter is an unbiased estimator of the effect of garment jobs on girls school enrollment if there are no other factors influencing girls enrollment, relative to boys, that occur in garment villages at the same time as increases in the number of garment jobs.

Two potential threats to this condition are reverse causality and an omitted variable correlated with both girls' school enrollment and the arrival of garment jobs. Reverse causality would be an issue if factories expanded their labor force into specific areas in response to increases in girls' schooling there. To minimize this issue, we use the national-level data on factory expansions rather than village-specific job growth data. In any case, qualitative interviews we conducted with factory owners suggest that this concern is likely second order anyway. They reported that the two most common reasons for choosing a location are proximity to roads and other infrastructure and the convenience of using buildings already owned by the factory owner or his family members. Imperfections in land and property markets in Bangladesh due to a weak institutional environment make the availability of convenient land or building a primary input into factories' location decisions.

Potential omitted variables that threaten identification are variables that both (a) leads to greater growth in the garments sector in the factory-proximate villages, and (b) differentially increases girls' schooling relative to boys'. One can easily imagine some unmeasured factors that lead to growth in garment areas (such as more new roads built in the areas closer to Dhaka where garment factories are located), but it is more difficult to argue that those factors would have a gender-differentiated effect on enrollments in the same direction as the ones we observe. One could argue, for example, that is easier for boys to take advantage of the new roads to travel farther and access better jobs in nearby urban areas. But that would lead to greater investments in young boys relative to young girls, which is the opposite of what we find. Nevertheless, to allay these concerns, we allow for baseline trends in both overall enrollment and *specifically in girls' enrollment* to be different in garment versus non-garment villages.

3.4 Identifying the Effects of the Girls' School Subsidy Program

Identifying the effect of the 'supply side' schooling subsidy program is more challenging given the structure of that program. Since all of the villages in our sample received the program in 1994, we cannot include year fixed effects. Instead, we implement a simple regression discontinuity design to estimate any discrete jump in girls' enrollment in 1994. Here again we add household fixed effects to compare individuals exposed to program relative to (say) an older sibling who was not exposed. And again we examine the gender difference in enrollment, taking advantage of the fact that the program provided a cash transfer conditional on girls' enrollment, but not boys.

$$Enroll_{ivft}=\beta_0+\delta_f+\beta_1 Age_{ivft}+\beta_2 Female_{ivft}+\beta_3 Female_{ivft}\times Age_{ivft}+\lambda_1 t+\lambda_2 t^2+\lambda_3 t\times Female_{ivft} \\ +\lambda_4 t^2\times Female_{ivft}+\gamma_1 Post1994+\gamma_2 Post1994\times Female_{ivft}+\varepsilon_{ivft} \tag{2}$$

The estimated $\hat{\gamma}_2$ captures the effects on enrollment if the program did not affect boys. Then the regression discontinuity is valid if the overall and female-specific quadratic time trends accurately model the time trend in schooling for both genders.

However, boys' enrollment may have been affected by the program, either positively through income effects or negatively through substitution effects. Given that possibility, we conduct a separate analysis where we restrict the sample to girls only, and study the effects of the *Post* 1994 dummy variable on enrollment.

4. Results

4.1 The Effects of Garment Jobs on School Enrollment

Table 4 shows the results from estimating equation (1) to assess the effects of the arrival of garment jobs on girls' enrollment using the triple difference strategy. This specification includes household fixed effects, so the inferences are drawn based on comparisons of siblings. The coefficient of interest is on the variable log(GarmentJobs) x Garment Village x Female, which examines the effect of the national growth in garment jobs on the enrollment of girls *relative to their brothers* in garment-proximate villages. The first column indicates that 10% national growth in garment industry employment leads to a 0.71 percentage point increase in girls' enrollment in garment-proximate villages (relative to boys in the same family). This is a 2% increase in the sample average enrollment rate, which implies an elasticity of enrollment with respect to new garment jobs of 0.2. This enrollment effect has a p-value of 0.18, and is therefore not statistically significant at conventional levels. This is the overall effect on girls across all age groups, and therefore combines all possible channels of influence, such as the drop-out associated with increased job opportunities for older girls, and the wealth or demand-for-skills effects that increase a family's demand for education.

The next column attempts to separate these mechanisms by examining the heterogeneity in this effect of garment jobs on enrollment across different age groups. The drop-out channel is only pertinent for older girls who can access these jobs, and younger girls are therefore expected to benefit more from the improving perceptions of the future returns to schooling. The coefficient estimates in the second column show that for the youngest girls (5-year-olds) in the sample, 10% growth in garment sector employment leads to a 1.3 percentage point increase in the probability of enrollment. This estimate becomes smaller for older girls, and is only positive up to the age of 16. In contrast, the enrollment effect for boys is comparatively very small, statistically insignificant, and flat across different age groups.

Figure 4 plots these varying marginal effects for girls by age and the 95% confidence interval around the effects. Growth in garment sector employment increases school enrollment for girls aged 5-16 and decreases it for girls aged 17 and 18. The increased enrollment effect is significantly different from zero (with 95% confidence) for ages 5-10. In this age group, there is a 1 percentage point (or 2.9% at mean) increase in the enrollment probability under a 10% growth in garment sector employment. This implies an elasticity of employment with respect to job arrivals of 0.3 for this age group. These results are consistent with the hypothesis that the arrival of garment sector jobs induce some older girls drop out to take advantage of the employment opportunities right away, while the younger girls remain in school to increase their potential to access the better jobs in the future.

Next we re-do this analysis while controlling for differential pre-program (i.e. pre- garment growth) trends in enrollment for boys and girls. Figure 5 shows that the results remain largely unchanged even after we explicitly control for differential trends that may pre-date the period of growth in the garment sector. We allow for differential pre-1983 trends in garment villages which vary by gender and by age (to account for pre-existing differentials in any of the main sources of variation that draw an inference on). Even with these controls, 5-10 year old girls are significantly more likely to enroll relative to their male siblings in factory-proximate villages after the growth in the garment sector occurs.

4.2 Wealth Effect or Increased Demand for Skills?

The results we report in the first two columns of table 4 (and associated figures 4 and 5) may be driven by either an increased demand for schooling due to an increase in the returns to skill in villages that have better access to factory jobs, or by a wealth effect in which mothers gained access to better employment opportunities at higher wages in garment factories, which in turn allowed them to send children to school because they could afford it.[11] Two types of families in garment proximate villages allow us to separately identify the importance of each of these two channels of

[11] Since we show triple-difference results by gender, a wealth effect could explain these results only if the wealth effect is larger for girls than for boys – e.g. boys are sent to school anyway, whereas girls are sent only when the family can afford it.

influence. We have families where mothers took advantage of garment sector work, and whose daughters therefore benefited from the wealth effect and the demand-for-skills effect. We also have families where mothers were not working outside the home, where the wealth effect should therefore be absent.

Columns 3 and 4 in Table 4 explore these two channels by re-examining the first two columns where we add interaction terms for an indicator variable for whether the mother works outside the home. Column 3 shows that there is a wealth effect that increases boys' enrollment and a weaker, positive (but statistically insignificant) wealth effect on girls' schooling. Importantly, in both specifications, the coefficient on girls' schooling for families where mothers do not work outside the home remains essentially unchanged. There is still a positive effect of garment factory growth on girls' enrollments (relative to their brothers), even in the sub-sample of families that did not benefit from any wealth effect. Figure 6 demonstrates these gains clearly. Limiting our attention to families where mothers did not work outside the home, we still see a statistically significant increase in 5 – 10 year old girls' enrollment associated with growth in garment sector employment. Our main conclusions are virtually unchanged – both qualitatively and quantitatively – in this sub-sample.

4.3 Effects of Garment Jobs on Marriage and Childbearing

Table 5 examines the effects of access to garment sector jobs on some 'downstream' outcomes that are important indicators of women's welfare: Are girls with greater exposure to factory jobs (a) less likely to be married off at an early age?, (b) less likely to bear children at an early age?, and (c) less likely to remain unmarried at later ages? Specifically, we estimate regressions where the dependent variables track whether a girl is married or gives birth before age 16 or 18, or whether the girl remains unmarried past age 26. These outcomes are of interest because early marriage and early child-birth have negative development effects and adverse welfare consequences for women and children. We additionally analyze effects on "remaining unmarried past age 26" in order to determine whether access to employment allows women to target their marriage age better (as opposed to displacing them from the marriage market entirely).

We employ a difference-in-differences estimation strategy based on growth in the garment sector that differentially affected girls resident in villages in close proximity to garment factories. The independent variable of interest is a girl's cumulative exposure to garment factory jobs up to the age at which the dependent variable is measured (e.g. the hazard of being married at age 16 is regressed on the factory jobs that arrived until the girl turned 16). We control for a quadratic in year of birth to capture changing national trends in age of marriage and childbearing. Our estimating equation is therefore:

$$
\begin{aligned}
Pr(\textit{outcome by age } X) &= \beta_1 \textit{year of birth}_{ij} + \beta_2 \textit{year of birth}_{ij}^2 \\
&\quad + \gamma_1 \textit{garment exposure to age } X_{ij} + \varepsilon_{ij} \qquad (3)
\end{aligned}
$$

We do not use the triple difference strategy (differencing out the outcomes for boys) because these dependent variables on marriage and childbearing have fundamentally different interpretations for boys: e.g. boys almost never get married by age 16 or 18 and they do not bear children. Moreover, since girls have to marry boys, there would be some spillover effects on boys of girls delaying marriage and childbirth. Nevertheless, we are able to show using our data that an appropriately re-defined "early marriage" variable for boys to make it sensible in this context (i.e. marriage before age 20 or 22) are not as strongly affected by garment sector exposure as the analogous outcomes for girls. The difference-in-differences results we show in table 5 using the sample of girls would therefore also hold in a "quasi" triple difference setup that examines girls' marriage and childbearing relative to boys.

In Table 5, we test whether the cumulative exposure to garment jobs from birth to age 16 or 18 has an impact on the probability that a girl is married by age 16 or 18 or has her first birth before age 16 or 18. Lifetime exposure to the garment industry had a negative and statistically significant impact on the probability that a girl is married by age 18. Early marriage seems to respond strongly to garment opportunities; the estimated effect implies an elasticity of 0.726. In other words, for a 10% increase in garment jobs, a girl's propensity to be married before age 18 decreases by 7.3%. This effect results from a combination of the facts these girls were more likely to be enrolled in school earlier in life, and that they have better current labor market opportunities. Exposure to garment jobs also tends to decrease marriage by age 16 and first birth by age 16 or 18, but these effects are not statistically significant. It does appear that some of these girls are either postponing marriage for considerable time or never marrying: exposure to garment jobs also leads to statistically significant increases in the number of girls remaining unmarried by age 26.

We investigate several alternative specifications to explore the robustness of these results. First, even though our preferred sample - used throughout this paper - is limited to the offspring of the household head (whose entire migration history is available, allowing for a precise match to the garment exposure data), this results in a relatively small sample for the marriage and childbearing regressions where we have only one lifetime outcome per child. We therefore re-estimate the marriage and childbearing equations for the entire sample of females, implicitly assuming that each person's current location provides a good indicator for their history of exposure to garment jobs. In this larger sample, the coefficients in the marriage and childbearing regressions are more precisely estimated. We find that greater exposure to garment jobs decreases women's propensity to marry by age 16 or 18, and decreases their propensity to give birth by age 16 or 18, and these results are highly statistically significant. The coefficients are qualitatively and quantitatively very similar to our main specifications with the limited sample, with one exception. In this expanded sample, garment jobs lead to statistically significant *decreases* in the probability that a girls remains unmarried by age 26, which suggests that girls are not leaving the marriage market entirely, even though they are avoiding early marriage.

We also estimate a hazard model of the probability that an unmarried (or childless) girl gets married (or has a child) in that year as a function of her exposure to garment jobs in that year. Since we are focused on early marriage and childbearing which have clear adverse welfare consequences, we only use data up to age 16 and 18. Reassuringly, the results from this specification are qualitatively similar to the results of estimating equation 3 (results available on request).

Finally, we study appropriately re-defined outcomes for boys ("early" marriage or propensity to become a father before age 22 or 24, since boys marry later), and we find that the results are smaller in magnitude. Boys delay marriage (elasticity of 0.12, compared to 0.73 for girls) and fatherhood slightly in garment-proximate villages, and this may simply be a marriage-market-spillover effect from the fact that girls are delaying marriage, and that there is a somewhat inelastic social norm regarding the appropriate spousal age gap.

4.4 The Effects of the Female Stipend Program (FSP) on School Enrollment

Table 6 examines the effect of the introduction of the girls schooling subsidy program on enrollments. This first column shows the results from estimating equation 2, where enrollment is regressed on an indicator for years post 1994 (when the stipend program was introduced), and we allow this effect to vary across the two sexes. We control for quadratic gender-specific time trends and age effects by gender. We find that overall schooling levels increased in 1994: the coefficient on the *Post*1994 dummy shows a statistically significant increase of 8.80 percentage points in enrollment, above the prevailing quadratic time trend, in 1994. However, the *Post*1994 × *Female* interaction is insignificant and very close to zero. So while overall schooling does appear to have jumped in 1994, there is no evidence that girls' enrollment increased relative to boys.[12] If boys can be thought of as an appropriate "control" group to evaluate a program that was explicitly targeted to girls, then this small, statistically insignificant coefficient on *Post*1994 × *Female* suggests that the program did not change the behavior of the intended recipients.

Figure 7 examines the magnitude of the *FSP* (*post 1994*) effect relative to enrollment fluctuations in the years immediately prior and immediately after the introduction of the program. The strategy here is to conduct a series of placebo tests where equation 2 (or the first column of table 6) is estimated repeatedly with the program indicator (FSP or post 1994 dummy) is replaced with dummies for 'placebo' years (1993, 1995, 1992, 1996, etc). In other words, we compare the actual effect of the FSP in 1994 to the estimated effect of placebo "programs" beginning in the years 1990 to 2000. The panel on the right tracks the overall effect on girls enrollment (the sum of *FSP* and *FSP* × *Female*) of this series of tests, and shows that while the estimated effect is largest when the *FSP* indicator is set to the *actual* program inception year of 1994, we can also detect similar "program effects" during the placebo years. The magnitude of enrollment effects for the 1994 actual FSP program is very similar to (and not statistically distinguishable from) hypothetical programs that are assumed to begin in the year immediately before and after. This casts further doubt that the allocation of the schooling subsidy changed the behavior of FSP beneficiaries.

The results in Figure 7 are entirely consistent with the descriptive finding shown in Figure 1: that the increasing trend in girls enrollment in Bangladesh - both in absolute terms and relative to

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[12] Fuwa (2001) and Khandker et al. (2003) identify a different parameter relating to the FSP and examine a different geographic area to ours (118 rural thanas in Bangladesh). They estimate program effects separately in both cross sectional household survey data and school-level panel data. Both datasets indicate that the stipend program increased girls schooling. The cross-sectional data suggests that boys schooling was unaffected by the stipend program, while the school level panel data suggests that boys schooling may have decreased as a result of the program.

boys – pre-dates the introduction of the FSP. That figure, also constructed by Pitt et al. (2011) using different data sources, suggests that prior casual evaluations of the FSP may have conflated the program with some pre-existing trends that had a different underlying source.

The second column of Table 6 looks for interaction effects between garment job growth and the introduction of the stipend program in 1994. While the coefficients are noisy, the general result is that the positive effects on the garment industry on girls' education come almost entirely after 1994. There are two possible reasons for this result. The first is that there is complementarity between the demand side impetus of garment jobs and the supply-side CCT: maybe households want to take advantage of future garment jobs but they are credit-constrained and only enroll when new jobs come if the stipend is paid. A second possible explanation is that the arrival of garment jobs led to a stronger enrollment effect after U. S. Senator Tom Harkin proposed the Child Labor Deterrence Act, which sought to prohibit the importation of manufactured and mined goods into the United States produced by children under the age of 15. Even though the Act did not pass, the threat of bad publicity led many garment factories in Bangladesh to stop the use of child workers (Dhaka Courier, 1998; Rahman et al., 1999). After 1993, children's propensity to drop-out of school to immediately access new factory jobs may have decreased, which allowed the positive enrollment effects associated with demand-for-skill to dominate. While Harkin proposed the Act in 1993, factories responded at different times over the course of the next several years, so there is no easy way to differentiate the effects of this Act from that of the introduction of the stipend program.

5. Conclusion

This paper studied the effects of a demand-side market innovation (the growth of factories) that changed the demand for skills and affected girls' propensity to enroll in school, and compared it to the effects of a roughly coincident supply-side intervention which decreased the direct cost of schooling for girls. We find that the growth of the garment industry in Bangladesh had sizeable effects on enrollment. To approximately infer the magnitude of this impact, we multiply the actual growth in garment jobs between 1983 and 2000 in by the marginal effect of garment jobs on girls' enrollment estimated in our regression model, and find that in villages within commuting distance to garment factories, exposure to these jobs led to a 27% increase in girl's enrollment rate. Since the garment sector experienced explosive growth and more than quadrupled in size between 1983 and 2000, the cumulative effect of the growth is large when we apply our regression estimates to this change. By contrast, when we estimate the effects of the FSP using the boys as a comparison group to girls, we find little effect on girls' schooling: both boys and girls schooling increased by approximately the same amount in the year the FSP was introduced to the study area. Even if we instead liberally assume that the total increase in girls' enrollment in 1994 was the FSP effect (i.e. not the change relative to boys), then the overall effect of 7.71 percentage points is still lower than the estimate of the effects of job growth in the garment sector.

Another interesting question for policy is whether the remarkable growth in garment sector exports and employment in Bangladesh in its recent history was a big or small contributor to the overall impressive gains in girls' educational attainment in the country during this period. In our sample villages, 5-18 year old girls' enrollment rate increased by 27 percentage points, from 0.22 in 1983 to 0.49 in 2000. According to our estimates, garment sector job growth can account for the entirety of the gain in enrollment over this period, mostly due to the fact that the job growth was explosive during this period, with the sector more than quadrupling in size. The sector's contribution to the national increase in girls' enrollment is of course more modest, since most of the country was not as exposed to the garment industry as the residents of our sample villages. 8% of women across the country work in the garment sector, compared to 37% in our sample of garment-proximate villages, which suggests that roughly 20-25% of the gain in girls' enrollment across the country could be attributed to the remarkable growth in this export industry.

In contrast, we don't find much of an effect of the supply side program, which is the FSP stipend allotment for girls. It is difficult to evaluate the effects of FSP precisely given the structure of the program, but even data in its simplest form (the time series graphs of boys' and girl' enrollment) suggests that the acceleration in girls' enrollment started before the FSP program was instituted, and that differential trend simply continued after FSP. This could be related to the very small size of the transfer offered by the FSP, relative to other conditional cash transfer programs around the world that are deemed to be more successful.

Taken together, our results suggest that education policy in developing countries is closely tied to trade policy or industrial policy, and enrollments strongly respond to the arrival of jobs that require education. Shifting academic and policy focus to studying the determinants of households' decisions to invest in education may be an important complement to the impressive and large literature that has focused more on improvements in the quantity and quality of educational inputs.

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Figure 1: School Enrollment in Bangladesh, Ages 5 to 18

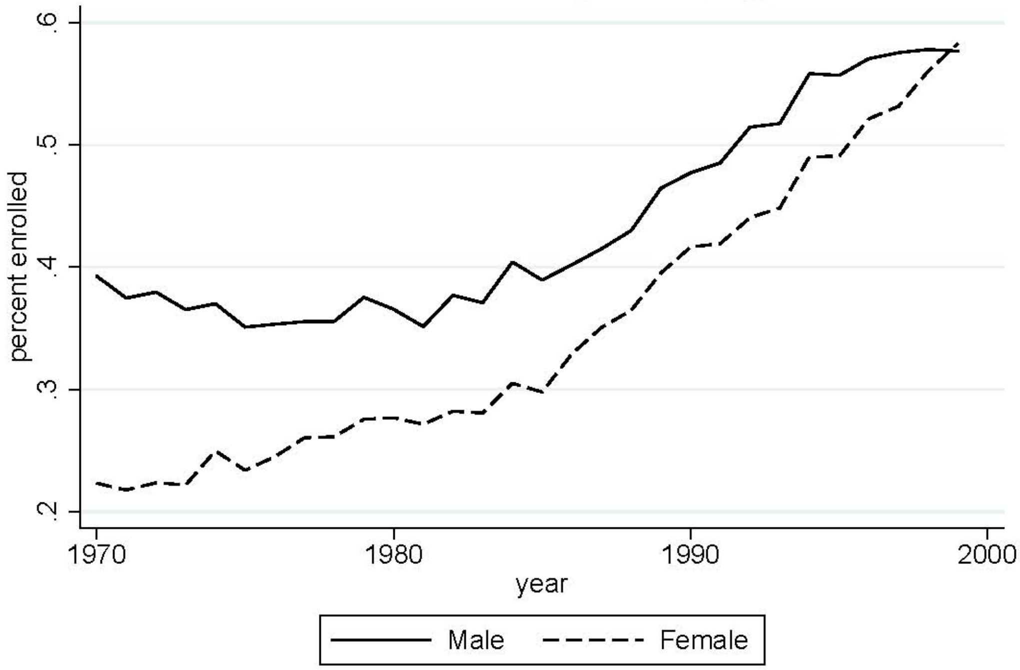

Figure 2: Nation-wide Employment in the Garment Industry

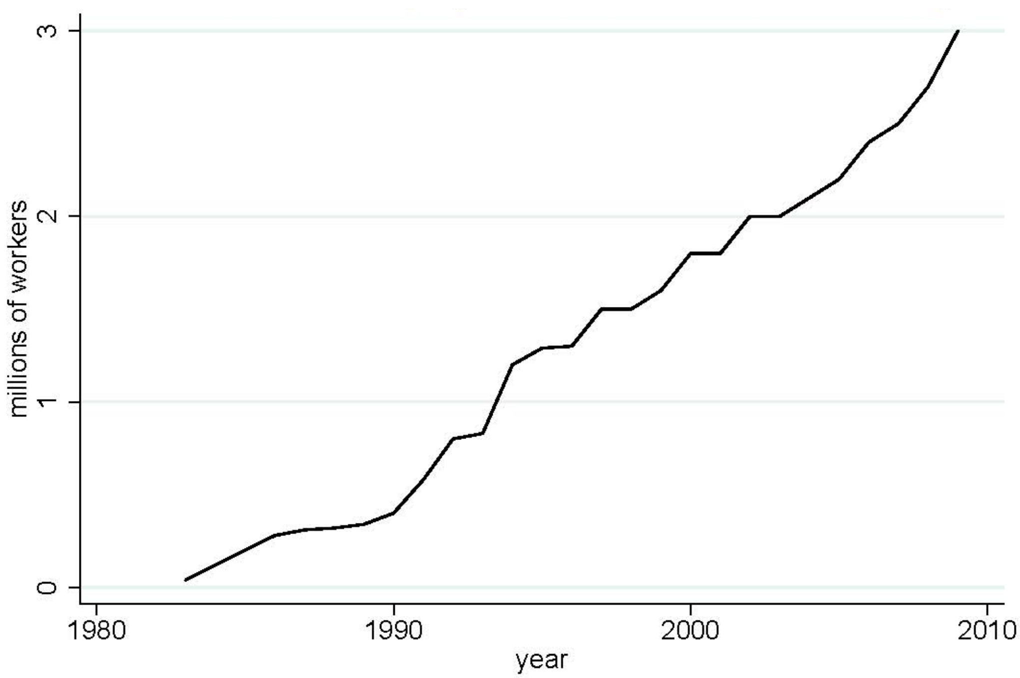

Figure 3: School Enrollment, ages 5 to 18, Garment vs. Non-Garment Villages

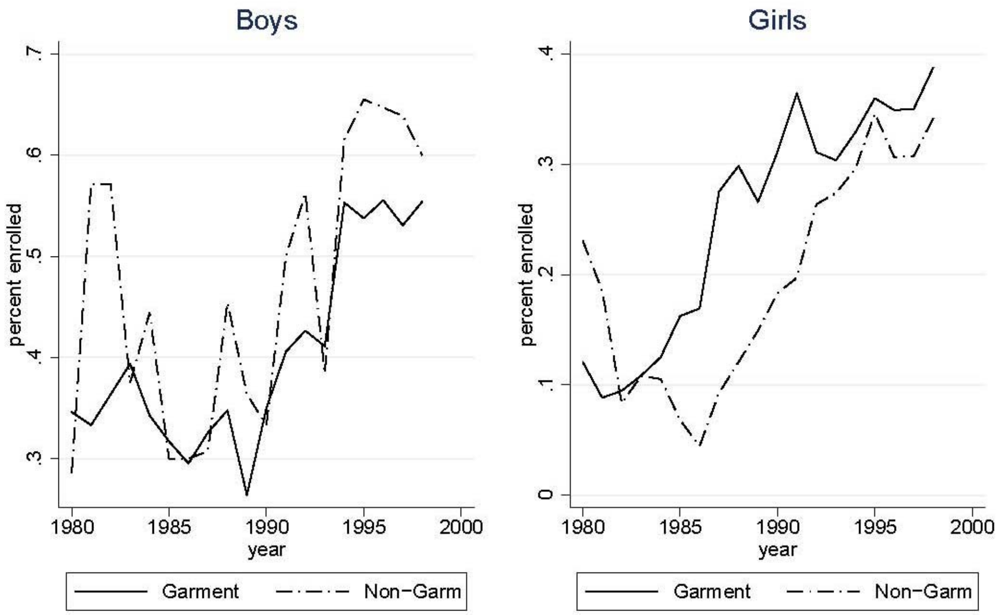

Figure 4: Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment in School, by Age (with 95% confidence interval)

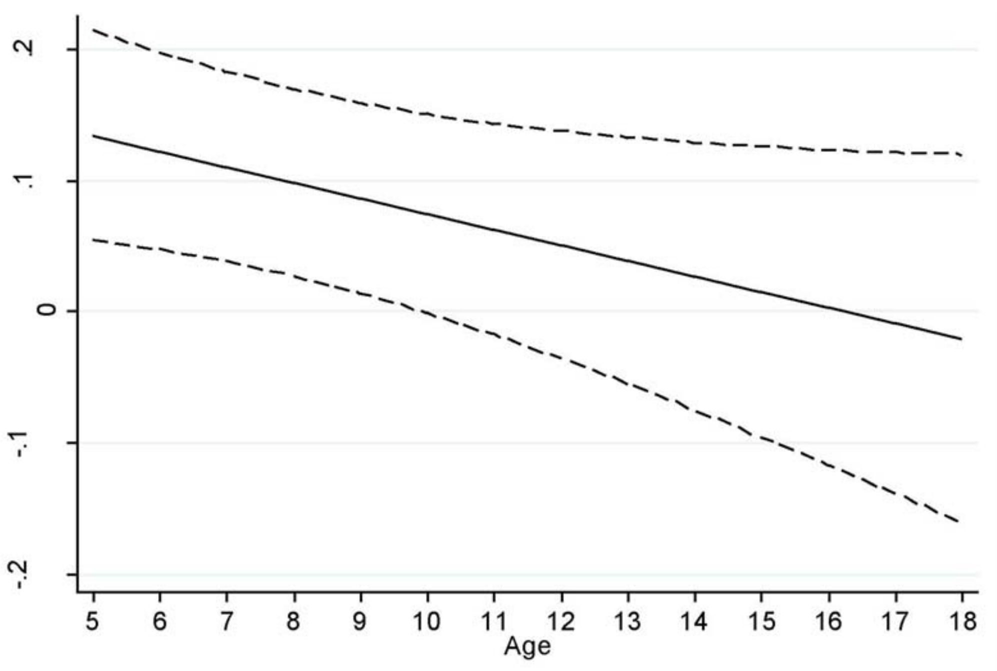

Figure 5: Marginal Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment, Controlling for Pre-Program Trends (with 95% confidence interval)

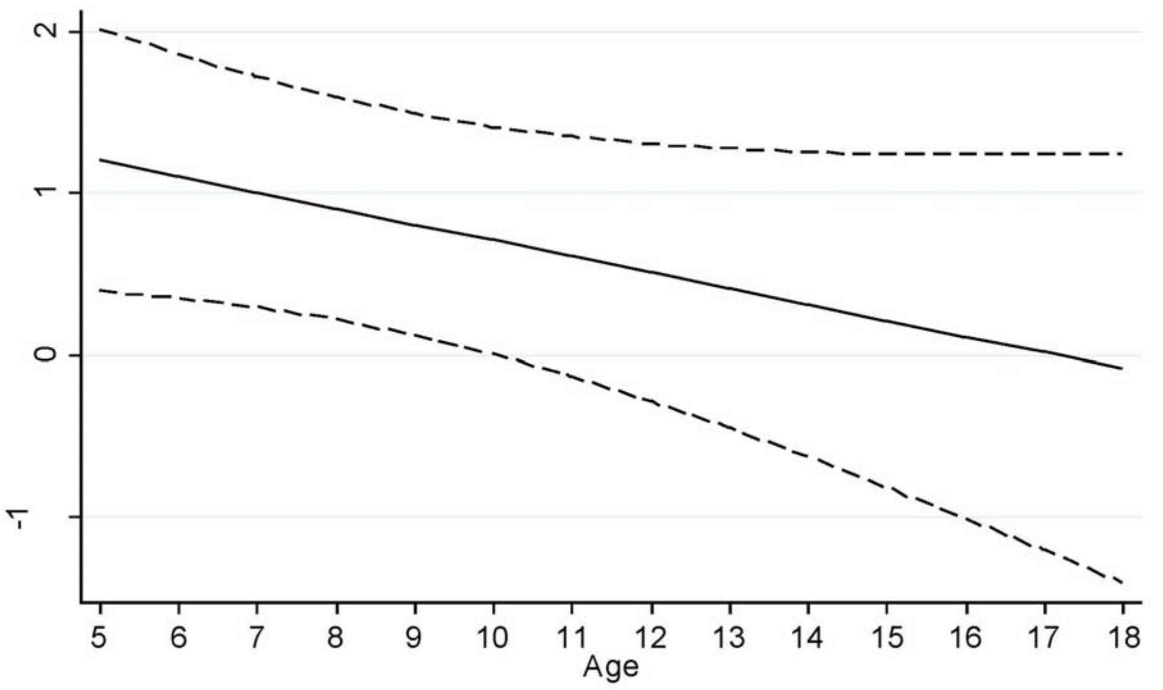

Figure 6: Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment, Children Whose Mothers Have Never Worked

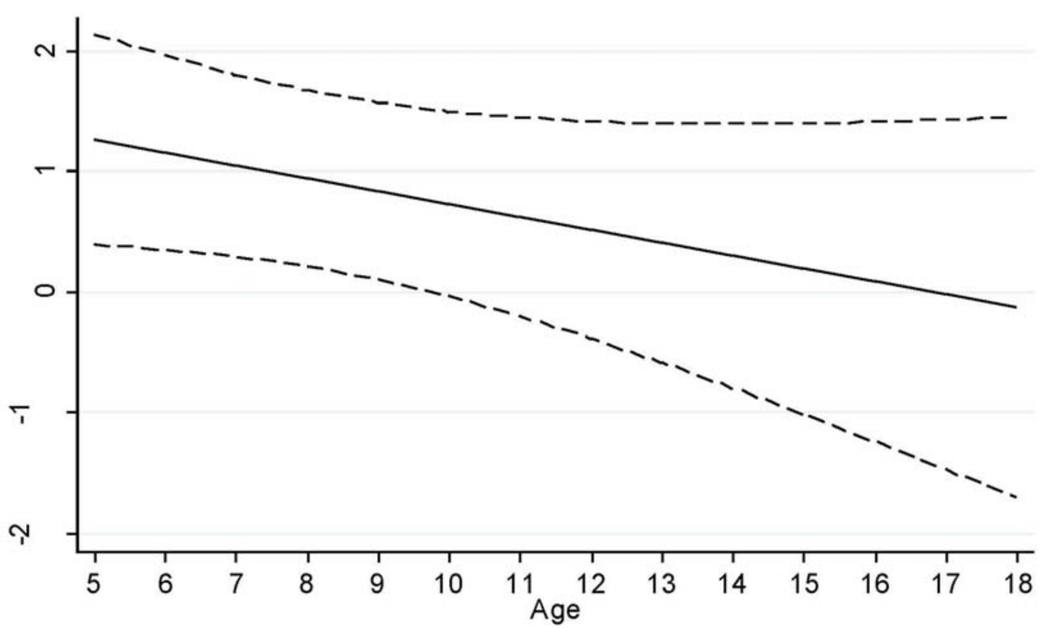

Figure 7: Placebo Tests of Effects of FSP (post 1994)

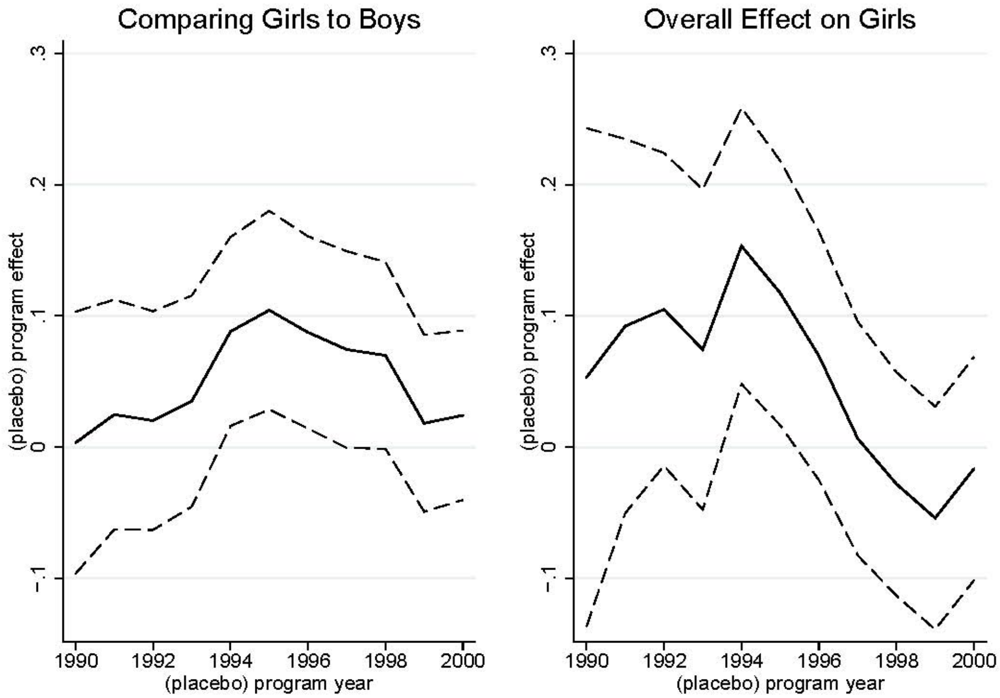

Table 1: Summary Stastistics of Workers in Sample

	Garment Villages		Non-Garment Villages
	Garment Workers	Non-Garment Workers	
Age	26.004	24.687	26.646
Female	0.546	0.494	0.499
Wage (Taka)[(1)]	3800.759	4929.739	4188.98
Years of Experience	3.237	6.991	8.634
Years of Education	5.977	4.086	3.188
Mother's Years of Education	1.491	2.322	1.622
House has a Cement Floor	0.79	0.64	0.252
Married	0.742	0.503	0.499
Has a Child	0.389	0.426	0.458
N	965	3306	1225

[(1)] conditional on working

Table 2: Differences in Garment vs. Non-Garment villages

	Garment Villages	Non-Garment Villages	P-value for diff	$N_{garment}$	$N_{non\text{-}garment}$
INDIVIDUAL LEVEL					
Completed Education, Females 50+	0.824	0.537	0.232	176	80
Completed Education, Males 50+	3.486	1.943	0.002***	222	88
Age At Marriage, Females 50+	14.788	14.462	0.604	85	39
Age At First Birth, Females 50+	19.286	21.162	0.090*	84	37
VILLAGE LEVEL					
Distance to Dhaka (Km)	1.795	6.813	< 0.001***	44	16
Distance to a Girls' Secondary School (Km)	5.659	6.375	0.662	44	16
Distance to a Boys' Secondary School (Km)	6.932	10	0.16	44	16
Male Agr. Wage (Peak Season, In Taka)	27.559	27.997	0.802	44	16
Female Agr. Wage (Peak Season, In Taka)	22.563	22.701	0.945	44	16

**** p<0.01, ** p<0.05, * p<0.1*

Only individuals born in village included

Table 3: Summary stastistics of key variables in regressions

	Females	Males
School Enrollment	0.404	0.436
Age	10.670	10.411
Year	1999.750	2000.199
Lives in a garment-proximate village	0.760	0.800
School enrollment in the pre-treatment (pre-1983) period	0.338	0.361
Married before age 16 (dummy)	0.039	0.020
Married before age 18 (dummy)	0.058	0.034
Had First Birth before Age 16 (dummy)	0.018	0.008
Had First Birth before Age 18 (dummy)	0.040	0.023
Total exposure to garment jobs from birth to age 16	8.023	7.663
Total exposure to garment jobs from birth to age 18	8.471	8.008

Table 4: Effects of Garment Jobs on Girls' School Enrollment

	(1)	(2)	(3)	(4)
log (Garment Jobs) x Garment Village	0.0115	0.006	0.0111	0.0197
	[0.0588]	[0.0625]	[0.0595]	[0.0650]
log (Garment Jobs) x Garment Village x Female	0.0714	0.1286**	0.0649	0.1036
	[0.0532]	[0.0602]	[0.0539]	[0.0642]
log (Garment Jobs) x Garment Village x Age		0.0017		-0.0009
		[0.0058]		[0.0065]
log (Garment Jobs) x Garment Village x Female x Age		-0.0136*		-0.0096
		[0.0081]		[0.0093]
log (Garment Jobs) x Garment Village x Mother Works			0.1863*	0.1089
			[0.1098]	[0.1238]
log (Garment Jobs) x Garment Village x Mother Works x Female			-0.1263	-0.0487
			[0.0886]	[0.1078]
log (Garment Jobs) x Garment Village x Mother Works x Age				0.0086
				[0.0137]
log (Garment Jobs) x Garment Village x Mother Works x Female x Age				-0.0147
				[0.0090]
Observations	10,433	10,433	10,373	10,373
R-squared	0.119	0.121	0.123	0.125

Regressions include family fixed effects and controls for female, age, and female x age. Standard errors in brackets, clustered at the level of the family

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of Exposure to Garment Sector Jobs on Girls' Probability of Early Marriage and Childbirth

	Just daughters of household head					Entire sample of females				
	Married before Age 16	Married before Age 18	Not Married by Age 26	Had First Birth before Age 16	Had First Birth before Age 18	Married before Age 16	Married before Age 18	Not Married by Age 26	Had First Birth before Age 16	Had First Birth before Age 18
Total exposure to garment jobs from birth to age 16	-0.0019			-0.001		-0.0020***			-0.0020***	
	[0.0018]			[0.0012]		[0.0007]			[0.0004]	
Total exposure to garment jobs from birth to age 18		-0.0048**			-0.0029		-0.0022***			-0.0035***
		[0.0022]			[0.0018]		[0.0008]			[0.0006]
Total exposure to garment jobs from birth to age 26			0.0048***					-0.0027***		
			[0.0017]					[0.0006]		
Observations	713	713	713	713	713	3,030	3,030	3,030	3,030	3,030
R-squared	0.151	0.209	0.196	0.118	0.207	0.077	0.121	0.188	0.043	0.096

Robust standard errors in brackets. Regressions also include a dummy for garment village and a quadratic in year of birth

**** p<0.01, ** p<0.05, * p<0.1*

Table 6: Effect of Female Stipend Program on School Enrollment

	(1)	(2)
Post 1994	0.0880***	0.1248**
	[0.023]	[0.0568]
Post 1994 x Female	-0.0109	-0.0718
	[0.030]	[0.0542]
log (Garment Jobs) x Garment Village		0.0143
		[0.0513]
log (Garment Jobs) x Garment Village x Female		0.0147
		[0.0545]
log (Garment Jobs) x Garment Village x Post 1994		0.0308
		[0.1312]
log (Garment Jobs) x Garment Village x Female x Post 1994		0.1582
		[0.1281]

Dependent variable is enrollment in year t. Regressions include family fixed effects and controls for female, age, and female x age.

Standard errors in brackets, clustered at the level of the family;

**** p<0.01, ** p<0.05, * p<0.1*