

# Does Demand or Supply Constrain Investments in Education? Evidence from Garment Sector Jobs in Bangladesh<sup>\*</sup>

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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) – also evaluated through another triple difference - does not have as significant an effect on enrollment. Research on education policy has had a stronger 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 has generated important, credible evidence on determinants of educational attainment at the level of households and villages based on either natural or policy experiments, or randomized control trials of programs that build schools, provide inputs, improve school quality or supply parents with cash transfers if children attend school.<sup>1</sup> 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.<sup>2</sup> Even in the United States, education policy is tilted in favor of the supply side.<sup>3</sup> There is comparatively little evidence on the role of 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 schools endogenously emerge (Pritchett, 2001; Easterly, 2002). Understanding the role of demand is important for policy, since it could imply that promoting export-led manufacturing and increasing returns to skill is the

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<sup>1</sup> See, for instance, Burde & Linden (2010) on building schools, Duflo et al. (2011) on decreasing class size and tracking, Duflo et al. (forthcoming) 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 several 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 (2006).

<sup>2</sup> 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).

<sup>3</sup> 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).

most effective path to human capital accumulation (Federman & Levine, 2005; Helper et al., 2006; Le Brun et al., 2011).

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.<sup>4</sup> The geographic and temporal context of this research is 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, thus generating important policy implications for other developing countries interested in emulating Bangladesh's 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

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<sup>4</sup> 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.

throughout the developing world.<sup>5</sup> The dramatic improvement in girls' enrollments in Bangladesh in the past 30 years has frequently but casually been attributed to the FSP,<sup>6</sup> 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

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<sup>5</sup> 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 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; Macours et al., 2012), Colombia (Attanasio et al., 2005), Honduras (Hoddinott, 2010), and Brazil (Morris et al., 2004).

<sup>6</sup> 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." See also Raynor and Wesson (2006) for a review of other potential effects attributed to the FSP.

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 difference estimation strategy. Using retrospective 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 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. We offer additional suggestive

evidence in favor of the demand channel 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. Furthermore, we report a 'within-family' effect throughout, where girls' enrollment decisions are compared to their brothers'. Many sources of wealth effects (e.g. general equilibrium effects such as new factories stimulating local demand, or fathers getting access to new jobs) are likely to benefit all children in the family, but that overall family effect is being differenced out in our estimation strategy. These considerations bolster the case that the enrollment gains that we observe are at least partially a result of 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 another triple difference, since only girls beyond a certain grade level become eligible for the program after 1994. We 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, 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. This is important because 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.<sup>7</sup> 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 (forthcoming) find that supplying cameras to monitor teachers improves the quality of schooling inputs by reducing teacher absenteeism, but Banerjee and

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<sup>7</sup> 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.

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 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 Garment Industry and the School Subsidy Program**

### **2.1. The Demand Side: The Garment Sector in Bangladesh and Effects on Women**

As shown in Figure 2, 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). In fiscal year 2004-05, it accounted for 75% of exports and 11% of Bangladeshi GDP, growing to 79% of exports and 14% of GDP in 2008-09 (Bangladesh Bureau of Statistics, 2010).

Women have benefited disproportionately from this growth: approximately 80% of garment workers are female (Khatun et al., 2007). Furthermore, garment jobs often represent females' only or best option to work outside the home. Women's share of employment in non-export industries was only 7% as of 1993 (Paul-Majumder & Begum, 2000). In our data, women employed in the garment industry earn 13.65 percent more than women of the same education and experience who



work in other industries. The advent of this sector represents a much larger innovation in the labor market for women. About 10% of Bangladeshi women work outside the home, while 88% of men do (data from 2005 Household Income and Expenditure Survey).

Jobs created in this sector are labor intensive in that the garments are mostly sewn by individuals using basic sewing machines. Women have an absolute advantage in most of the tasks. Aside from a few jobs within woven factories that require strength to operate large machines, approximately seventy percent of garment workers do sewing, which requires fine motor skills rather than brawn (Siddiqi, 2000; Absar, 2001). Factories believe women are better at sewing because they tend to be more nimble at doing fine stitching (Miller & Vivian, 2002) and because women are believed to be more patient and compliant (Paul-Majumder & Begum, 2000; Siddiqi, 2000) and therefore require less time and energy to manage.

The availability of garment jobs potentially benefits a large percentage of women nationwide. While there is no reliable nationwide data on garment workers, a back-of-the envelope calculation based on our data and other national surveys<sup>8</sup> suggests that 11.67 percent of women nationwide between the ages of 16 and 30 work in the garment industry. This figure rises to 35 percent in the garment-proximate villages in our sample. Given that many workers only work for relatively short periods (the average garment worker in our sample has been working for 3 years), many more women work at the garment industry at some point in their lives).

## **2.2. Link between Factory Jobs and Schooling**

There are several channels through which the arrival of garment jobs could affect girls'

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<sup>8</sup> Specifically, there are 3 million garment workers nationwide, an estimated 80 percent of whom are female (Khatun et al 2007; BGMEA 2010). We assume that 90 percent of these female workers are between the ages of 16 and 30, which is consistent with the age distribution in our sampled workers. According to the nationally representative 2005 Household Income and Expenditure survey, 25 percent of females nationwide are between ages 16 and 30. So based on the UN's estimate of 148 million total population in 2009, there are 18.5 million women between the ages of 16 and 30.

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 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 not 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 of education given standard identification concerns, but parents may respond to this observed correlation in their educational investment choices. Moreover, consistent with reports of factories administering literacy tests to potential workers (Amin et al., 1998), education appears to affect potential workers’ ability to secure a garment job: controlling for age, an additional year of education

is associated with a 1.70 percentage point increase in the probability that a woman works in the garment industry.

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. More indirectly, the arrival of factories could lead to general equilibrium changes (e.g. wage effects) that benefit all households. 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.<sup>9</sup> 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.

### **2.3. 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

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<sup>9</sup> This is especially true before U.S. Senator Tom Harkin proposed the Child Labor Deterrence Act in 1993.

at least 75 percent, achieve 45 percent marks on term and annual exams, and remain unmarried (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 sub-districts of Bangladesh: Savar and Dhamrai in Dhaka District; Gazipur Sadar and Kaliakur in Gazipur district. Further details on the sampling and survey can be found in Appendix A and in Heath (2011). 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. 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 same differential does not exist for an analogous pair of siblings. This strategy of comparing siblings within the same family helps address the concern that our retrospective data only contains a selected sample of households that remained in the sample area and never migrated. We are careful to obtain full enrollment histories for all offspring of the household head, regardless of their migration status, so that estimated coefficients are not affected by selective out-migration. Furthermore, whole-household migration is extremely rare in Bangladesh: In (Pitt et al., 2011)'s long term panel survey in rural Bangladesh, only 1.6% of households had moved away from the village between 1982 and 2002, which suggests that sample selection problems are minimal in this context.

#### 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.<sup>10</sup> 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),

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<sup>10</sup> 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.

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 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 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 having gender-specific effects, 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.

### **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  $t$ :



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

We include household (or sibling) fixed effects ( $\delta_f$ ) and year fixed effects interacted with a dummy for female ( $\lambda_t \times Female_{inft}$ ), 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.

$\gamma_2$  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. There is clear agglomeration in factory locations over time, and initial location choices for the first set of factories are therefore potentially important. The indicators for garment-proximate village and female\*garment village in our estimating equation control for such baseline differences.

Potential omitted variables that threaten identification are variables that both (a) lead 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 it 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**

The 'supply side' school subsidy program was implemented in all villages in our sample in 1994. To identify its effects, we take advantage of the fact that the program targeted girls in grades 6 through 10. Therefore, both boys of the same age and younger girls can act as control groups for girls targeted by the program. We again rely on a triple difference for identification: We estimate whether there was a discrete jump in enrollment of girls eligible for the FSP<sup>11</sup> compared to boys in 1994, after

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<sup>11</sup> Since we do not have data on grade repetition, we use six years of schooling as a proxy for reaching 6<sup>th</sup> grade. Our estimates are thus analogous to intent-to-treat effects, since students must have been in school at least 6 years to be eligible for the program, but may not be eligible after 6 years if they have repeated grades.

differencing out that gender gap in enrollment growth among younger children who were not eligible for the program. We include household fixed effects, which implies that our estimates compare individuals exposed to program relative to their own siblings who were not targeted. This triple difference strategy again allows us to include year fixed effects interacted with gender to capture flexible gender-specific time trends in school enrollment. The equation we estimate:

$$\begin{aligned}
 Enroll_{inft} = & \beta_0 + \delta_f + \lambda_t + \lambda_t \times Female_{inft} + \beta_1 Age_{inft} + \beta_2 Female_{inft} + \beta_3 Female_{inft} \times Age_{inft} \\
 & + \gamma_1 Post1994 + \gamma_2 Post1994 \times Female_{inft} + \gamma_3 In\ School\ 6\ Years_{inft} + \\
 & + \gamma_4 In\ School\ 6\ Years_{inft} \times Post1994 + \gamma_5 In\ School\ 6\ Years \times Post1994 \times Female_{inft} + \varepsilon_{inft} \quad (2)
 \end{aligned}$$

$\hat{\gamma}_5$  captures the triple-difference estimate of the FSP on girls' school enrollment. If boys were also affected by the program (either positively, through income effects or negatively, through substitution effects), then the program effect on girls may be better represented by the overall effect  $\hat{\gamma}_4 + \hat{\gamma}_5$  rather than the effect relative to boys.

## 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  $\log(\text{GarmentJobs}) \times \text{Garment Village} \times \text{Female}$ , 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 3 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. Columns 3 and 4 in Table 4 (and associated Figure 4) show that these results are robust to running the regression with sampling weights to account for the over-sampling of households with certain characteristics (please see Appendix A for details on sampling).

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.

Since a part of the triple-difference effects we estimate is due to a small statistically insignificant *decline* in boys' enrollment, we examine the effects of garment sector growth in the sub-sample of girls (with a double-difference setup) in order to ensure that the main effects we report are not being driven by the perverse effects on boys. The estimated effects are qualitatively similar in this sub-sample, although slightly muted, as expected. Positive enrollment effects are only statistically significant for 5-8 year olds using the double difference estimation strategy.

#### **4.2 Wealth Effect or Increased Demand for Skills?**

The results we report in the first two columns of table 4 (and associated figures 3, 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.<sup>12</sup> Since the results we have reported are based on within-family sibling variation (i.e., controlling for household fixed effects), a wealth effect could explain these results only if added wealth leads to differentially larger enrollment for girls relative to their brothers. Other results in the development literature on gender differences in resource allocation suggest that this is most likely to occur when women gain economic resources. We therefore explore enrollment effects separately for families where mothers took advantage of garment sector work (and whose children are therefore more likely to have benefited from the wealth effect), and families where mothers were not working outside the home. Families where mothers took a garment job may differ in other unobservable ways from families where mothers did not, and these results are therefore only meant to be suggestive.

Columns 5 and 6 in Table 4 add interaction terms with an indicator variable for whether the mother works outside the home. Column 5 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. Columns 7 and 8 show that in the sub-sample of families less likely to have experienced a wealth effect (the sub-sample where mothers never work), there is still a positive effect of garment factory growth on girls' enrollments (relative to their brothers). Figure 6 plots the associated marginal effects. 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. In

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<sup>12</sup> 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.

summary, there appears to be an overall wealth effect from mothers working that increases enrollment of all children in the family, but this does not affect the magnitude or statistical significance of our triple difference estimate comparing girls to their brothers.

### 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:

$$Pr(\text{outcome by age } X) = \beta_1 \text{year of birth}_{ij} + \beta_2 \text{year of birth}_{ij}^2 + \gamma \text{garment exposure to age } X_{ij} + \varepsilon_{ij} \quad (3)$$

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 chose age cutoffs of 16 and 18 following the literature, but these results are entirely robust to any other definition of 'early marriage' or 'early childbirth'. The effects actually get stronger and more statistically significant when we examine marriage or childbirth before ages 19 or 20. The arrival of garment jobs appears to allow rural Bangladeshi women to postpone marriage and childbirth mostly beyond ages 18-20. 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.

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 girls schooling subsidy program on enrollments. This first column shows the results from estimating equation 2, where enrollment is regressed on indicators for gender of child, for years post 1994 (when the stipend program was introduced), and for student who had been in school for at least 6 years (the stipend began in 6<sup>th</sup> grade). The FSP effect is identified through the triple difference: Do FSP-eligible girls become more likely to stay enrolled after 1994 than comparable boys, after the analogous gender gap among younger children is differenced out? Year fixed effects interacted with gender control for differential enrollment trends by gender.

We find that a girl who has been in school six years is more likely to remain in school than a boy who has already been in school 6 years: the double interaction *Female X Reached 6* is positive and significant. However, there is no evidence that this effect became larger after 1994 and in fact, the triple interaction *Post1994 × Female × In school 6 years* interaction is negative and significant. In 1994, eligible girls actually became 9 percentage points less likely to enroll than comparable boys, relative to the *female × year* fixed effect that captures any changes in enrollment *for all girls* in that year. So, there is no evidence that program eligible girls’ enrollment increased relative to a control group of comparably aged boys. If boys can be thought of as an appropriate “control” group to evaluate a

program that was explicitly targeted to girls, then negative coefficient on the triple interaction suggests that the program did not benefit the intended recipients. Even if boys are not the appropriate control group, the overall effect on eligible girls given by the sum of the triple interaction ( $Post1994 \times Female \times reached\ 6$ ) and the double interaction ( $Post1994 \times reached\ 6$ ) is close to zero and insignificant.

The last three columns of table 6 show that (a) we fail to detect any enrollment effect of the FSP in the smaller sample of individuals used for the garment analysis, and (b) the estimated enrollment effect of garment sector growth is robust to the inclusion of the FSP-related variables. We continue to detect a strong enrollment gain among young girls when there is factory growth.

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  $FSP \times Female$ ) 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.

#### 4.5 Comparison to other evaluations of the FSP, and CCT programs in other countries

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 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 majority of other studies purporting to assess the effects of the Female Stipend Program in Bangladesh rely overwhelmingly on descriptive statistics, and do not attempt to control for the effects of any concurrent programs or economic changes (like the growth in the garment sector) that may have also influenced girls' secondary school enrollment rates (Liang, 1996; Arends-Kuenning & Amin, 2004; Raynor & Wesson, 2006; Schurmann, 2009; Talukder, 2011). Only two studies - Fuwa (2001) and Khandker et al. (2003) – attempt to rigorously assess the effects of FSP controlling for the pre-existing growth in enrollment rates. They identify a different parameter than our triple difference estimate using cross sectional household survey data and school-level panel data, in a different geographic area to ours (118 rural thanas in Bangladesh). They find that the stipend program increased girls schooling, but that boys' enrollment was either unaffected or decreased. Schurmann (2009) examines the effects of FSP on downstream outcomes such as childbearing and early marriage, but is unable to find any identifiable causal impacts of FSP..

Evaluations of conditional cash transfer (CCT) programs in other countries have documented much larger positive impacts. Schultz (2004) and Behrman et al. (2005) document positive enrollment effects and lower dropout of Mexico's Progresa (or Oportunidades) program. The larger gains relative to the Bangladesh FSP may be due to the size of the cash transfers involved: Schultz (2004) estimates that the average grant in Mexico was equivalent to 44% of the typical male day-

labourer's wage. Similar programs in Colombia (Barrera-Osorio et al., 2008) and Honduras (Glewwe & Olinto, 2004) also show positive, but smaller effects on enrollment and promotion rates, in line with the smaller sizes of the cash transfers involved (US\$5 per month per child in Honduras, \$15 per month per child in Colombia, compared to \$25/month/child in Mexico, and only \$0.64-\$1.50 per month per child in the Bangladesh FSP).

## **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 percentage point increase in girl's enrollment rate. The garment sector more than quadrupled in size between 1983 and 2000, and the cumulative effect of that explosive growth is therefore large when we apply our regression estimates to this change. By contrast, even when we make the most liberal assumptions about the effects of the FSP on girls' schooling, the estimated effects are much lower than the impact of job growth in the garment sector. Even data in its simplest form (the time series graphs of boys' and girl' enrollment) suggest that the acceleration in girls' enrollment in Bangladesh 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.

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. In comparison, Mexico's *Progres*a program (which provided three years of monthly cash grants equivalent to one-fourth of average family income conditional on child school attendance) increased enrollment by 3.4-3.6 percentage points for all students (Kremer & Holla, 2009). In Kenya, providing free school uniforms to sixth grade pupils increased the enrollment rate by 2 percentage points for boys, and by 2.5 percentage points for girls.

The garment sector's contribution to the *national* increase in girls' enrollment in Bangladesh 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.

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

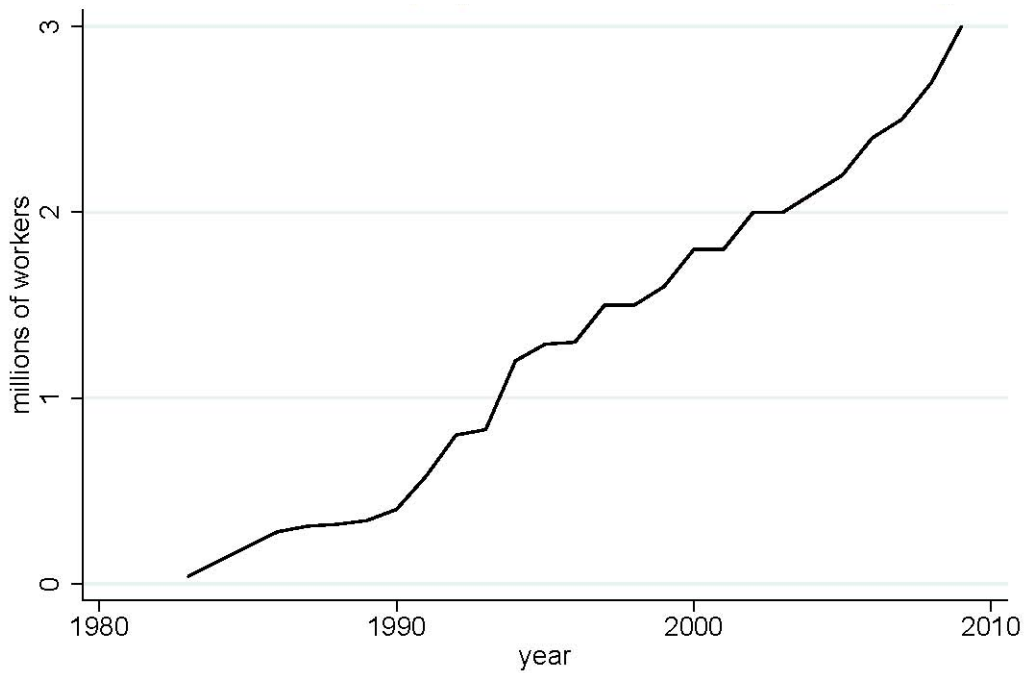


Figure 3: Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment in School, by Age (with 95% confidence interval)

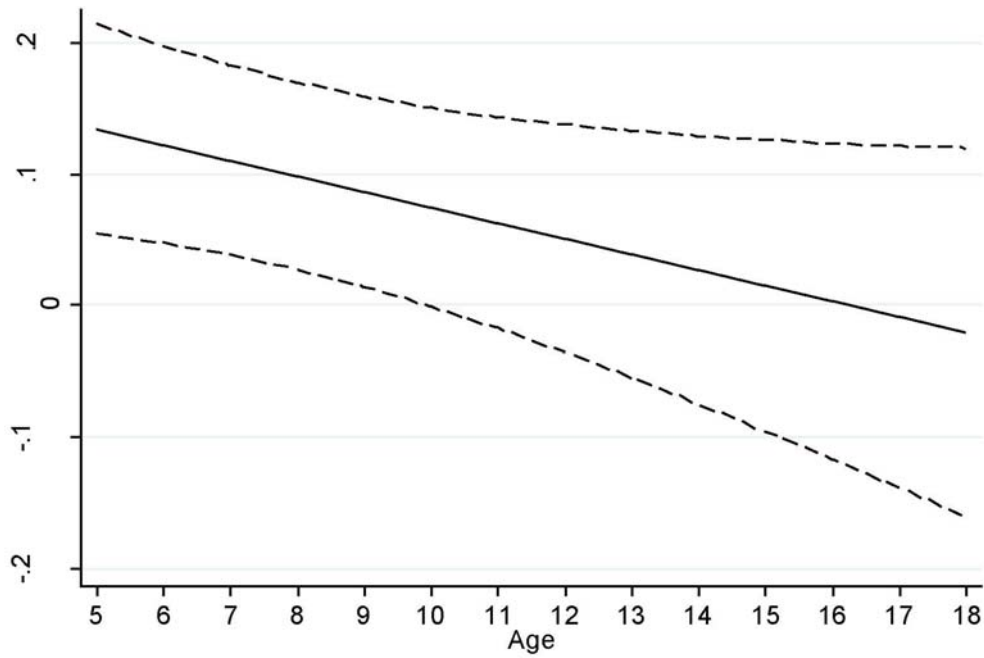


Figure 4: Marginal Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment, from regression estimated with Sampling Weights

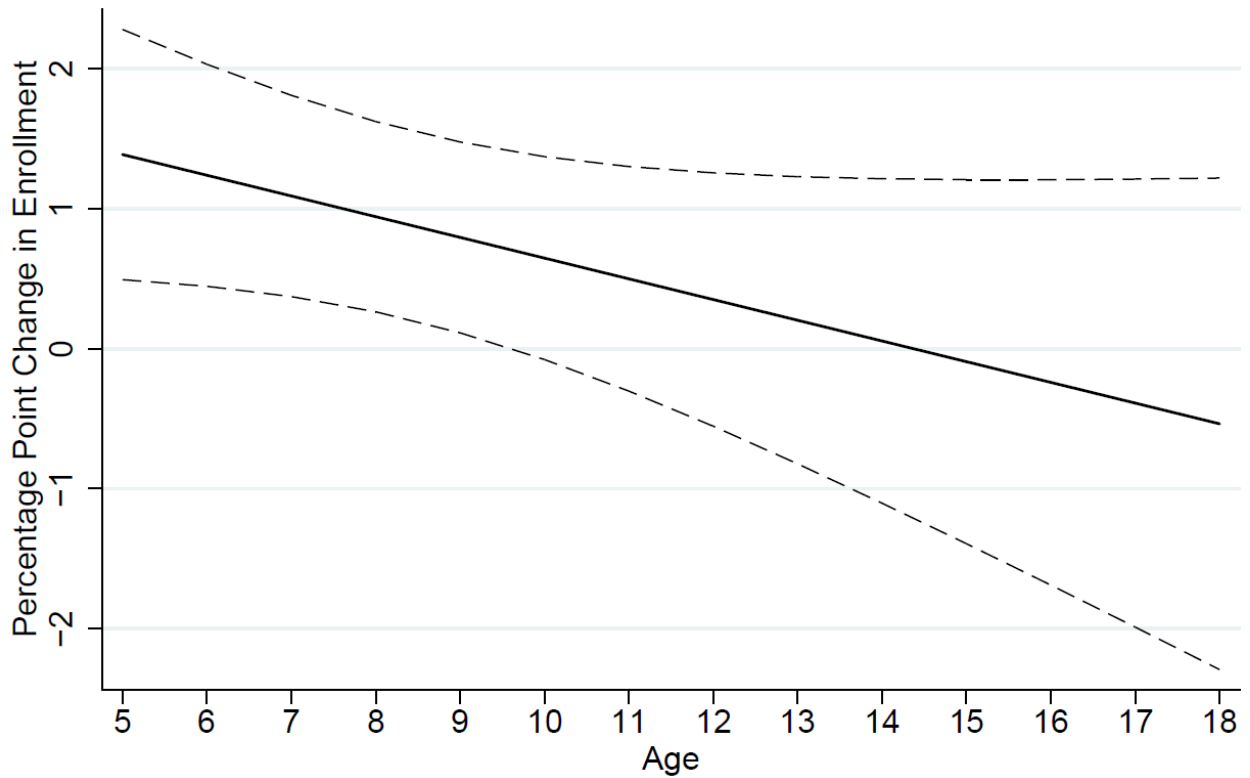


Figure 5: Marginal Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment, Controlling for Pre-Program Trends (with 95% confidence interval)

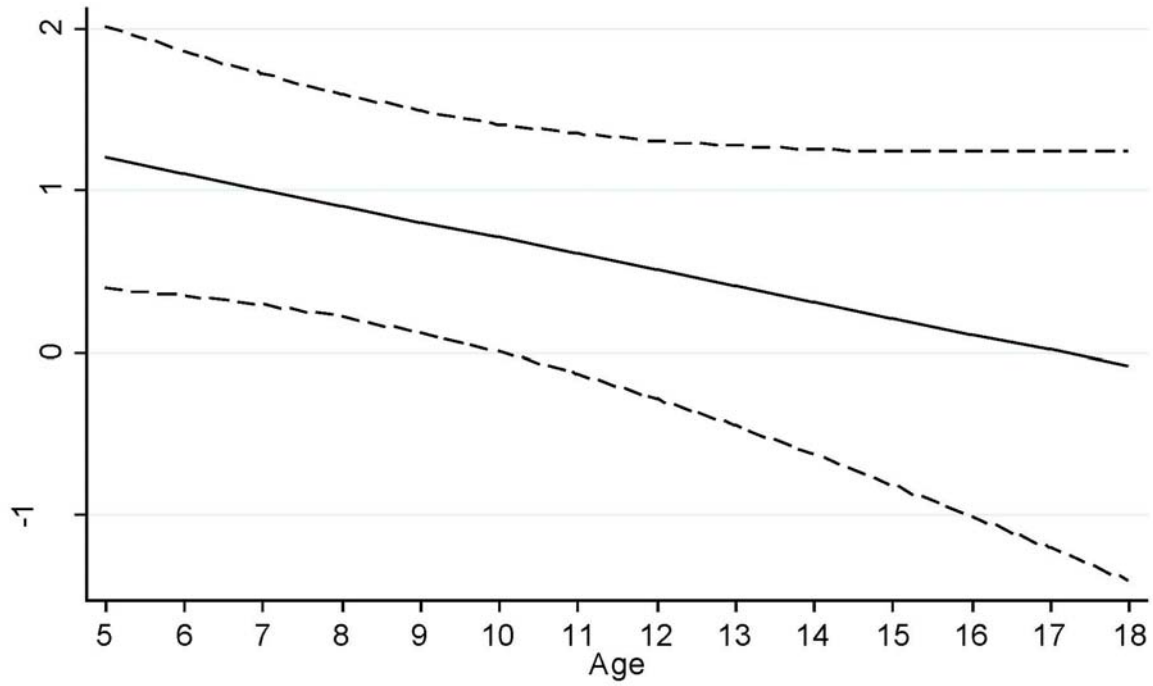


Figure 6: Effects of a 10 percent Increase in Garment Jobs on Girls' Enrollment, Children Whose Mothers Have Never Worked

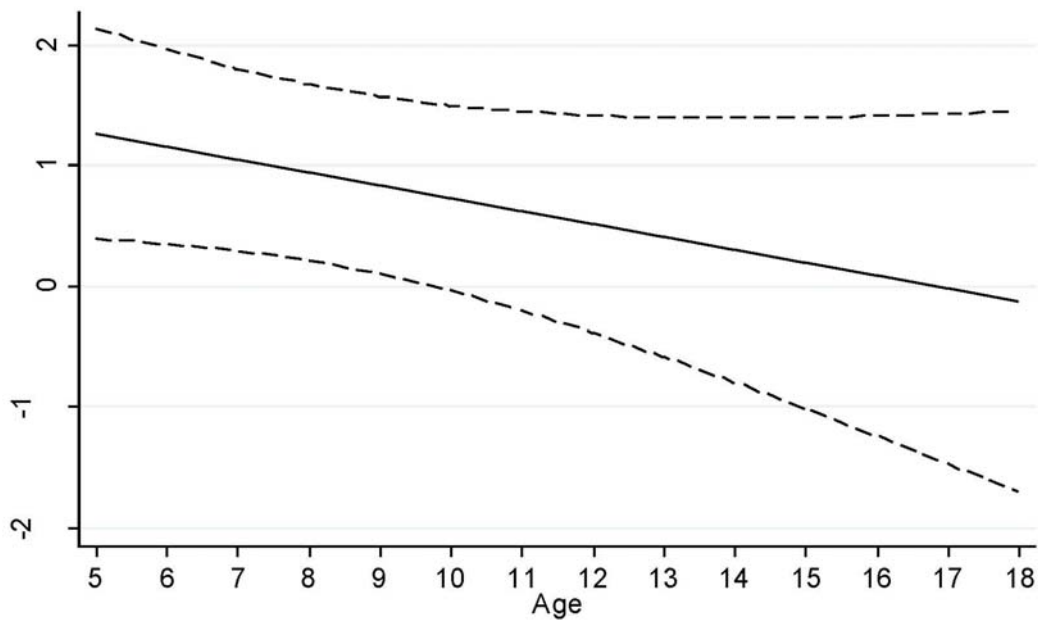
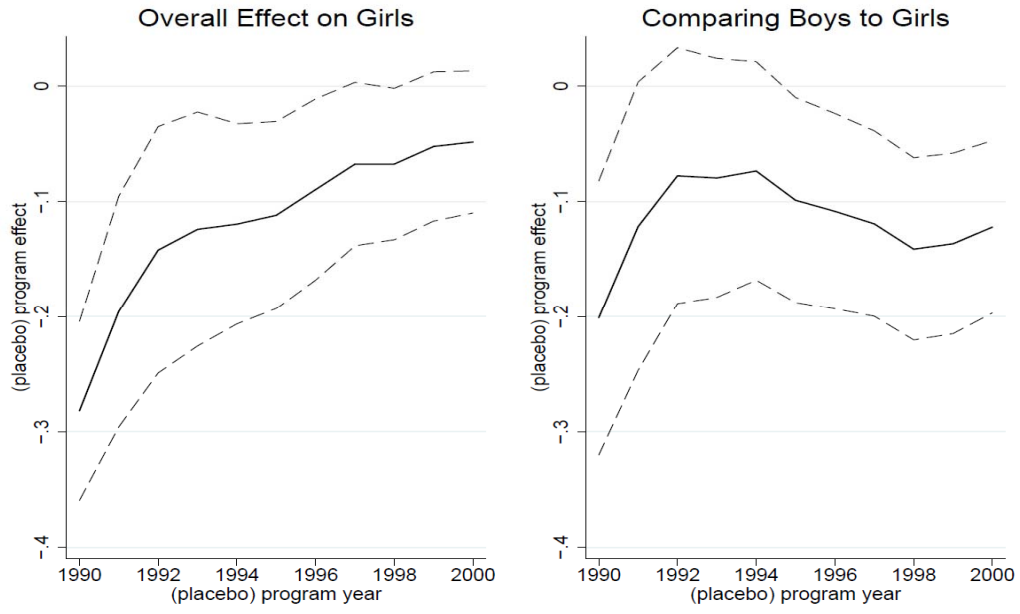


Figure 7: Placebo Tests of Effects of FSP (post 1994)





**Table 1: Summary Statistics 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) <sup>(1)</sup>  | 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                 |

<sup>(1)</sup> conditional on working

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

|  | Garment Villages | Non-Garment Villages | P-value for diff | N <sub>garment</sub> | N <sub>non-garment</sub> |
|--|------------------|----------------------|------------------|----------------------|--------------------------|
| <b>INDIVIDUAL LEVEL</b>                    |                  |                      |                  |                      |                          |
| 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                       |
| <b>VILLAGE LEVEL</b>                       |                  |                      |                  |                      |                          |
| 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 statistics 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**

|  | Whole Sample       |                      |                     |                      |                     |                     | Mother Never Worked |                      |
|--|--------------------|----------------------|---------------------|----------------------|---------------------|---------------------|---------------------|----------------------|
|  | (1)                | (2)                  | (3)                 | (4)                  | (5)                 | (6)                 | (7)                 | (8)                  |
| log (Garment Jobs) x Garment Village                               | 0.0115<br>[0.0588] | 0.006<br>[0.0625]    | -0.0483<br>[0.0790] | -0.0257<br>[0.0827]  | 0.0111<br>[0.0595]  | 0.0197<br>[0.0650]  | -0.0556<br>[0.0808] | -0.0157<br>[0.0853]  |
| log (Garment Jobs) x Garment Village x Female                      | 0.0714<br>[0.0532] | 0.1286**<br>[0.0602] | 0.1194*<br>[0.0702] | 0.1643**<br>[0.0809] | 0.0649<br>[0.0539]  | 0.1036<br>[0.0642]  | 0.1288*<br>[0.0723] | 0.1697**<br>[0.0859] |
| log (Garment Jobs) x Garment Village x Age                         |                    | 0.0017<br>[0.0058]   |                     | -0.0029<br>[0.0052]  |                     | -0.0009<br>[0.0065] |                     | -0.0056<br>[0.0056]  |
| log (Garment Jobs) x Garment Village x Female x Age                |                    | -0.0136*<br>[0.0081] |                     | -0.0119<br>[0.0110]  |                     | -0.0096<br>[0.0093] |                     | -0.011<br>[0.0126]   |
| log (Garment Jobs) x Garment Village x Mother Works                |                    |                      |                     |                      | 0.1863*<br>[0.1098] | 0.1089<br>[0.1238]  |                     |                      |
| log (Garment Jobs) x Garment Village x Mother Works x Female       |                    |                      |                     |                      | -0.1263<br>[0.0886] | -0.0487<br>[0.1078] |                     |                      |
| log (Garment Jobs) x Garment Village x Mother Works x Age          |                    |                      |                     |                      |                     | 0.0086<br>[0.0137]  |                     |                      |
| log (Garment Jobs) x Garment Village x Mother Works x Female x Age |                    |                      |                     |                      |                     | -0.0147<br>[0.0090] |                     |                      |
| Sampling Weights   | N                  |                      | Y                   |                      | N                   |                     | Y                   |                      |
| Observations   | 10,433             | 10,433               | 10,433              | 10,433               | 10,373              | 10,373              | 7,456               | 7,456                |
| R-squared  | 0.119              | 0.121                | 0.15                | 0.153                | 0.123               | 0.125               | 0.154               | 0.159                |

*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<br>before Age<br>16      | Married<br>before Age<br>18 | Not Married<br>by Age 26 | Had First<br>Birth before<br>Age 16 | Had First<br>Birth before<br>Age 18 | Married<br>before Age<br>16 | Married<br>before Age<br>18 | Not Married<br>by Age 26 | Had First<br>Birth before<br>Age 16 | Had First<br>Birth before<br>Age 18 |
| Total exposure to garment<br>jobs from birth to age 16 | -0.0019<br>[0.0018]              |                             |                          | -0.001<br>[0.0012]                  |                                     | -0.0020***<br>[0.0007]      |                             |                          | -0.0020***<br>[0.0004]              |                                     |
| Total exposure to garment<br>jobs from birth to age 18 |                                  | -0.0048**<br>[0.0022]       |                          |                                     | -0.0029<br>[0.0018]                 |                             | -0.0022***<br>[0.0008]      |                          |                                     | -0.0035***<br>[0.0006]              |
| Total exposure to garment<br>jobs from birth to age 26 |                                  |                             | 0.0048***<br>[0.0017]    |                                     |                                     |                             |                             | -0.0027***<br>[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: Effects of Female Stipend Program on School Enrollment, Including Cohort Comparison (In village sample)**

| Sampling Weights<br>Dependent variables             | Whole Sample           |                        | In Village Only        |                       |                        |
|---|------------------------|------------------------|------------------------|-----------------------|------------------------|
|   | N<br>Enrollment        | Y<br>Enrollment        | N<br>Enrollment        | Y<br>Enrollment       | Y<br>Enrollment        |
| At least 6 years of school                          | 0.6202***<br>[0.0236]  | 0.6070***<br>[0.0431]  | 0.6678***<br>[0.0361]  | 0.7118***<br>[0.0396] | 0.7210***<br>[0.0431]  |
| Post 1994 x At least 6 years of school              | -0.0008<br>[0.0239]    | 0.0206<br>[0.0437]     | -0.0556<br>[0.0353]    | -0.0967**<br>[0.0404] | -0.1088***<br>[0.0412] |
| Post 1994 x At least 6 years of school x Female     | -0.0913***<br>[0.0261] | -0.1373***<br>[0.0294] | -0.0831<br>[0.0526]    | -0.0296<br>[0.0508]   | 0.0379<br>[0.0576]     |
| At least 6 years of school x Female                 | 0.0758***<br>[0.0255]  | 0.1193***<br>[0.0288]  | 0.0834<br>[0.0522]     | 0.0601<br>[0.0496]    | 0.0035<br>[0.0594]     |
| log (Garment Jobs) x Garment Village                |                        |                        |                        |                       | -0.0123<br>[0.0663]    |
| log (Garment Jobs) x Garment Village x Female       |                        |                        |                        |                       | 0.1389**<br>[0.0653]   |
| log (Garment Jobs) x Garment Village x Age          |                        |                        |                        |                       | 0.0005<br>[0.0047]     |
| log (Garment Jobs) x Garment Village x Female x Age |                        |                        |                        |                       | -0.0167*<br>[0.0093]   |
| Constant  | 0.3760***<br>[0.0907]  | 0.4627***<br>[0.0774]  | -0.2295***<br>[0.0821] | -0.2485**<br>[0.1168] | -0.2594**<br>[0.1141]  |
| Observations  | 36,668                 | 36,668                 | 10,433                 | 10,433                | 10,433                 |
| R-Squared   | 0.233                  | 0.254                  | 0.24                   | 0.275                 | 0.28                   |

*Dependent variable is enrollment in year t. Regressions include Year x Female Fixed Effects, not shown*

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

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

## Appendix A: Construction of sample and variables used in the analysis

We began by randomly sampling 60 villages in 4 subdistricts in Dhaka and Gazipur districts in Bangladesh (Savar and Dhamrai in Dhaka District; Gazipur Sadar and Kaliakur in Gazipur district). We stratified by whether the villages were considered by a BGMEA official to be within commuting distance of a garment factory (at the time of the survey, summer 2009) and then chose 44 garment-proximate villages and 16 control villages not within commuting distance to garment factories.

For each selected village, we conducted a census that provided a sampling frame of all households in the villages and also identified whether the households contained a garment worker. We then selected a random sample of households from each selected village, sampling more households from the larger villages (exact stratification available from authors upon request). While also we oversampled households with current garment workers (and also, for use in other projects, households with a consanguineous marriage or a female born between 1975 and 1980), we use sampling weights to ensure that the regression results are representative of the population of households in the villages.

The retrospective schooling information is taken from the module administered to the female household head. Information is collected about all of her offspring, whether or not they still reside in the home. Details on the construction of specific variables in the enrollment regressions:

- **Yearly enrollment data:** this variable is defined for each child in the sample from the ages of 5 to 18, provided that they were living in the village of current residence at that age (based on whether the household head reported having arrived in the village in that year). We use questions on the age the offspring began school, whether he/she left school for a period of time (and if so, at what age he/she left and then reentered) and the eventual years completion to pinpoint the exact ages the child was in school.
- **Garment Employment Growth:** the national trend in garment employment from 1983 to 2009 was taken from the Bangladesh Garment Manufacturers' Exporters Association (BGMEA) 2010 yearbook
- **Garment Village:** this is a binary variable defined to be equal to 1 if the village was considered to be within commuting distance from a garment factory when the villages were sampled for the survey, and 0 if not. After the survey data was received, we checked whether any residents of the "non-garment" villages worked in garment factories in order to confirm the BGMEA's official's calculation: only 2.1 percent of adults ages 16 to 30 work in garment factories in non-garment villages, versus 40.0 percent of adults of these ages in garment villages
- **Mother Ever Worked:** This variable is binary and equal to 1 if the mother reported ever having worked outside the home.

For the marriage/childbearing regressions

- **Married by age X/First birth by age X:** all children were asked if they had ever been married or had children, and if so, by what age. To look specifically at early marriage/childbearing, we convert these responses to binary variables that indicate whether the child was married or had her first birth by age 16 (and other ages, to demonstrate the robustness of the result to different definitions of early marriage and childbearing)
- **Cumulative garment exposure to age X:** we construct a cumulative lifetime measure of exposure to garment jobs (up to the age defined by the dependent variable in each regression), using the yearly employment data from the BGMEA used in the enrollment regression. Cumulative exposure is zero for children in non-garment villages.