DO LABOR MARKET OPPORTUNITIES AFFECT YOUNG WOMEN'S WORK AND FAMILY DECISIONS? EXPERIMENTAL EVIDENCE FROM INDIA*

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Do labor market opportunities for women affect marriage and fertility decisions? We provided three years of recruiting services to help young women in randomly selected rural Indian villages get jobs in the business process outsourcing industry. Because the industry was so new at the time of the study, there was almost no awareness of these jobs, allowing us in effect to exogenously increase women's labor force opportunities from the perspective of rural households. We find that young women in treatment villages were significantly less likely to get married or have children during this period, choosing instead to enter the labor market or obtain more schooling or postschool training. Women also report wanting to have fewer children and to work more steadily throughout their lifetime, consistent with increased aspirations for a career. *JEL* Codes: I21, J12, J13, J16, J22.

I. INTRODUCTION

In many developing countries, women frequently leave school, marry, and start having children at a young age. Such outcomes are often taken as indicators of low social and economic progress for women and may have implications for individual well-being and economic growth. Economic models suggest that the labor market may play a role in influencing these outcomes. Because childrearing is traditionally more intensive in women's time, when there are few well-paying opportunities for women, the opportunity cost of having many children, or getting married and having children at a young age rather than accumulating human capital and/or entering the labor market, is low (e.g., Becker 1960; Mincer 1963; Willis 1973). In this article, we test whether an

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increase in employment opportunities for women can affect these early lifecycle work and family transitions in rural India.

Rural India is a valuable setting for testing this hypothesis. Among rural women aged 30 to 39 in 2005/2006, the median age at marriage was 17, and the median age at first birth was 19; though there have been large declines in the past few decades, women aged 40 to 49 had 4.3 births on average in their lifetime (International Institute for Population Sciences and Macro International 2007). Alongside these outcomes, women's paid labor force participation rates are only around 20%.

To examine the effect of an exogenous change in labor market opportunities for women, we conducted a randomized trial using recruiters for the business process outsourcing (BPO) industry. This field has grown rapidly in India over the past decade, creating a significant number of new, high-paying job opportunities, particularly for women. However, because it was such a new sector, awareness of these jobs and knowledge of how to access them were very limited, especially outside of the urban areas where these jobs were located. Our intervention provided three years of BPO recruiting services to women in randomly selected rural villages. By connecting the villages to experienced recruiters, the intervention was designed to increase awareness of and access to BPO jobs, and thus in effect increase employment opportunities for women.

As in many other developing countries, although women's paid labor force participation rates are low in rural India, they are in fact very economically active. Most women work in household production or on their own family farm or enterprise. For human capital, marriage, and fertility decisions, the kinds of opportunities available to and actually engaged in by women can be important. For example, work around the home or on a family enterprise may not require delays in marriage or fertility to the same extent, nor do they typically require the same investment in human capital. Self-employment profits are generally quite low, implying a low opportunity cost to withdrawing from work to have children.

A number of factors may explain why fewer women work away from home for pay, such as a low capital-to-labor ratio that makes production more intensive in physical than in mental tasks (favoring men's comparative advantage) or a social stigma against women working outside the home (Goldin 1990). Technological change may create jobs that require (and reward)

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greater human capital, favor women's comparative advantage, and do not carry as much of a stigma for women. In this spirit, Goldin (1995) describes a U-shaped pattern in women's labor force participation. At low levels of economic development, women have high employment rates, primarily in agriculture or self-employment. As incomes rise, women leave the labor market, in part because of a preference that women not work outside the home in manual labor. But with greater development, and increases in women's human capital, white-collar opportunities become available and draw women back into the labor force.[1] The BPO sector is an example of this kind of change and represents a particularly relevant case study given the historical role of similar jobs, including those fostered by new office and information technologies, in increasing women's labor force participation (Goldin 1984, 1990, 1995, 2006).

Using panel data spanning a three-year period, we find that women aged 15 to 21 at baseline from villages exposed to the recruiting intervention were 4.6 percentage points more likely to work in a BPO job than women in control villages and 2.4 percentage points more likely to work at all for pay outside the home. In addition, women from treatment villages expressed a greater interest in working throughout their lives, even after marriage and childbirth, indicating shifting aspirations toward work as a career with a longer term attachment.

The higher educational requirements and greater returns to human capital in the BPO sector also led to increased investments for women. The cohort of 15–21-year-old women from treatment villages were significantly more likely enroll in computer or English language courses at private, for-fee training institutes, indicating a willingness to invest in getting a job or building a career when suitable opportunities are available. Significantly, we also find that even younger, school-aged girls had increased school enrollment and greater body mass index (BMI), reflecting better nutrition and/or health investments. These results show that parents are willing to invest in girls' human capital in anticipation of labor market returns far in the future and are particularly important in light of the dramatic gender disparities in health and education in India (see Strauss and Thomas 1995; Behrman 1997; Duflo 2005).

1. See also Boserup (1970), Schultz (1990), Costa (2000), and Mammen and Paxson (2000).

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Finally, the increases in women's employment and schooling/training were accompanied by significant delays in marriage and childbearing. Women from treatment villages aged 15 to 21 at baseline, the peak age range for marriage and the initiation of childbearing in rural India, were 5–6 percentage points less likely to get married or to have given birth over the three-year period of our intervention. Although with a short panel we cannot assess the long-term impact on fertility, women report wanting to have 0.35 fewer children in their lifetime.

Aside from a direct interest in these important measures of women's status, the results are also relevant to the literature on the causes of the fertility transition experienced by many Western countries from the mid-nineteenth to the mid-twentieth centuries and by many developing countries today (Schultz 2002, 2009; Galor 2005a, 2005b; Guinnane 2011). For example, Schultz (1985) shows that increases in the female-to-male relative wage rate driven by world price changes for male-intensive food crops and female-intensive dairy products contributed to Sweden's fertility transition. Galor and Weil (1996) model the demographic transition as arising from technological change that promotes women's labor force participation by shifting production from strength- to mental-intensive tasks where women have a comparative advantage (as in Goldin 1990), as in the BPO sector. Our results show experimentally that improvements in women's labor market opportunities can have a rapid impact on important intermediate variables in this transition.

The experiment offers two advantages for examining the relationship between women's employment opportunities and human capital, marriage, and fertility outcomes. First, randomization allows us to overcome concerns about omitted variables bias and direction of causality. Second, we can isolate the role of increased labor market returns in causing such changes, separate from other mechanisms. For example, suppose we observe in nonexperimental data that in areas with greater women's labor force participation, women also marry later. It may be that young women delay starting a family because there are high returns to being in the labor market. But it could also be that delayed marriage for young women is due to their working mothers having more bargaining power in the household, greater wealth, or the need to keep a daughter around the home to take over household production activities. We exploit the high education requirements in the BPO sector and show that our treatment has an effect

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on young women's outcomes even in households where no other adult member could get a BPO job because they all have too little education. We argue that this result, coupled with some additional tests, allows us to conclude that at least for these households, the gains in human capital, marriage, and fertility were driven just by the greater labor market returns for young woman themselves, rather than these other mechanisms.

Of course, we would not claim that labor market opportunities for women are the only means to simultaneous human capital, marriage. and fertility change. For example, Duflo, Dupas, and Kremer (2011) find that lowering the costs of schooling (via free uniforms) increases education and reduces teen marriage and pregnancy in Kenya. For the United States, Goldin and Katz (2000, 2002) find that by lowering the cost of investing in careers and delaying marriage, the oral contraceptive pill led to increases in women's human capital investments, delayed marriage, and lower desired fertility; Bailey (2006) finds that it also led to increases in women's labor force participation and delays in actual fertility. However, our results imply that economic or labor market factors can have significant effects on marriage, fertility, and human capital investments, and that policies that promote labor market opportunities for women may help improve these outcomes. By contrast, many policies designed to address these outcomes have primarily emphasized the role of social or cultural influences or factors (see Croll 2000 for examples).

The remainder of this article proceeds as follows. Section II discusses the experimental design, and Section III discusses the data and empirical strategy. Section IV shows the results, and Section V presents the tests to determine whether mechanisms other than economic opportunities for younger women can explain the results. Section VI concludes.

II. Experimental Design

II.A. The BPO Sector in India

The BPO industry covers a range of activities and "back-office" services. The most well-known of these jobs are call centers, but the sector also includes data entry and management, claims processing, secretarial services, transcription and online technical support, as well as more skilled activities, such as accounting or software development. Though the industry has been

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around in some form for decades, recent technological changes in telecommunications and networking infrastructure (for example, the development and global deployment of fiber optic cable networks) have made it both possible and relatively inexpensive to provide these services remotely to clients around the world. In recent decades, India has enacted regulatory changes allowing greater foreign investment in the telecommunications sector. The technological and regulatory changes led to large-scale and rapid growth in India's BPO sector, with 30–40% average annual growth rates from 2000 to 2008 (NASSCOM 2009).

Within the BPO sector, particularly call centers, there appears to be a preference for female workers. A study of 2,500 call centers in 17 countries found that on average 69% of frontline call center workers are women (Holman, Batt, and Ursula 2007). Though the rate was closer to 45% in India, this is still high in comparison to the sex ratio of employment in most other industries. The study reported several reasons that employers preferred women, including a more pleasant voice and demeanor when interacting with customers and the belief that women were more trustworthy than men.

BPO jobs on average have high educational requirements, typically a minimum of 10 or 12 years of schooling. Shortly, we show that this exceeds the average attainment levels for both men and women in our rural sample.

Overall, then, growth in the BPO sector in India created a sharp and fairly sudden increase in the demand for educated female workers. To meet this demand, there was a surge in recruiting activities, including through newly formed, specialized private contractors and subcontractors who would seek out and screen potential employees. Because the BPO sector in India is strongly geographically concentrated, with 95% of employment focused around seven major cities, recruiting was fairly geographically concentrated as well, leading to large, localized increases in economic opportunities.

BPO jobs are also well paid in relative terms. In our data, starting salaries with no experience often ranged from 5,000 to 10,000 rupees (about US$110–220) per month in 2003. This was twice the average pay for non-BPO workers with similar levels of education. Salaries also often increase rapidly with experience, whereas many other jobs have relatively flat compensation profiles. Accordingly, as in the present article, Oster and Millett (2010) treat the growth in call centers as a shock to returns to

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education. Shastry (forthcoming) treats the broader information technology sector in India as a similar shock.

II.B. The Intervention

Although the BPO sector created a large number of employment opportunities for women, there remained significant gaps in awareness about those jobs and how to access them, precisely because the industry was so new. This was even more pronounced outside the urban centers where these jobs were located; in fact, in our 2003 baseline survey of rural households (described shortly), no one was employed in this sector, including any members or children of members having temporarily or permanently left the household. The experiment was designed to both increase awareness of these jobs[2] and to make it easier for qualified women to get them.

We hired eight BPO recruiters, all with at least two years of experience overall and at least six months specifically recruiting women (either working directly for recruiting firms or as freelancers). We drew the recruiters from Delhi, one of the most important cities for the BPO sector. Using maps, the recruiters were asked to identify the specific areas within and outside of Delhi they had visited for recruiting and then to define the approximate areas outside of Delhi beyond which they believed BPO recruiters would be unlikely to visit, due to their relative distance from the city and/or their population size. This allowed us to establish a list of rural districts where awareness of and access to BPO jobs was likely to be low, not because there were no qualified women but because the cost per potential recruit was high enough that recruiters chose to visit other areas instead. These districts were all located approximately 50–150 km from Delhi, in the states of Haryana, Punjab, Rajasthan, and Uttar Pradesh (Appendix A and Online Appendix Table A). These villages are, on average, closer to Delhi than the average village in rural India is to the nearest major urban center; more populous (1,900 people on average, compared to around 1,200 for rural India); and have better infrastructure. We would not generalize the analysis to all of rural India. For example, there may be less of a response to this kind of opportunity in more remote areas because of differences

2. In this respect, the experiment is similar to Jensen (2010), who examines the effect of providing information on the returns to schooling on male educational attainment the Dominican Republic.

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in factors such as the openness to women working away from home, poorer quality schools or other infrastructure, or because being closer to Delhi means that women can commute rather than having to migrate.

We compiled a list of all villages in these districts and randomly assigned 80 villages to the treatment group and 80 villages to the control group, with no additional stratification[3] (all randomization in the study was accomplished using Stata's random number generator). We then randomly assigned one of the eight recruiters to each of the treatment villages. Adherence to the randomization design was complete; there was no replacement or substitution of villages in either group, and no replacement or substitution of recruiters across treatment villages.

Between December 2003 and February 2004, recruiters visited the treatment villages and made a small introduction at schools and to local leaders, announcing that they would be visiting the village at a designated date up to a few weeks later to provide information on employment opportunities in the BPO sector. They also contacted and worked with local leaders, government officials, and nongovernmental organizations (NGOs) to advertise the sessions.

Within a few weeks, the recruiters visited the village and set up an information and recruiting session. The recruiters did not have a fixed script but were required to follow a specific organization: an overview of the BPO sector, including the types of jobs and level of compensation; information on the names of specific firms currently or frequently looking for workers; strategies for how to apply for jobs (how to create and submit résumés, plus lists of websites and phone numbers); interview skills lessons and tips; mock interviews; assessment of English language skills; and a question-and-answer session. The recruiters were required to emphasize that the jobs were competitive, so they were not guaranteeing employment.

The sessions were held in a range of facilities including schools and NGO or government offices, and typically lasted four to six hours. The sessions drew a great deal of local interest and attendance was high. Though our survey villages were spread

3. Because we did not stratify when assigning the treatment, there is some imbalance across states and districts. However, we show that there are no baseline differences in women's outcomes or other covariates (including distance to Delhi) between treatment and control groups. The results are also robust to including state or district fixed effects or clustering at the district level.

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out over a fairly large area, we cannot rule out that members of the control group may have attended these sessions or that they learned about the BPO sector from family or friends in treatment villages who attended them. However, any such deviation from our randomization protocol would lead us to underestimate the effect of the treatment. As of round 2, only a handful of people from our control villages were working in a BPO job, suggesting any such contamination was minimal.

The recruiters provided assistance to women only.[4] All women could attend, but it was made clear that the job opportunities were primarily for women with a secondary school degree, and preferably some English language ability and experience with computers. In effect this ruled out a vast majority of women over the age of 25; for example, in our data only 8% of women aged 26–50 have completed secondary school. Furthermore, in our rural sample very few women with young children work for pay away from home. This is likely to be even more binding for our intervention, because the urban BPO jobs require commuting or migration. Additionally, the recruiters also told us that most BPO firms prefer to hire young, unmarried women, which is confirmed by Ng and Mitter (2005) and Oster and Millett (2010). Thus overall, an important aspect of the BPO sector, and a primary reason we chose this sector for our intervention, is that it increases the labor force opportunities of younger, unmarried individuals. Because our recruiters only helped women get jobs, we expect the intervention to affect young women almost exclusively, while leaving the opportunities for others largely unchanged.[5]

One and two years after the initial treatment (i.e., December 2004 to February 2005 and December 2005 to January 2006), we provided "booster shots," with the recruiters again visiting the same treatment villages and providing the same session. After

4. In a second set of 80 treatment villages, we provided recruiting services for both men and women. This second treatment was designed to test a theory of intergenerational transfers and parental investment in children in the face of limited commitment (Jensen and Miller 2011).

5. In principle, the intervention could have led to employment gains for men because information could have been shared, or young women who got jobs could have helped men get jobs. However, we show that men's employment did not change. There may be several reasons for this: men may have already had access to other high-education jobs; BPO employers may have preferred to hire women; or caste-based job networks may have limited men's occupational mobility, as in Munshi and Rosenzweig (2006). It is also possible that without support from the recruiters, the barriers to BPO employment were too high.

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each of the three sessions, the recruiters left their personal contact information so that any woman could follow up for additional information or assistance at no cost. The recruiters were contracted to provide ongoing support for any woman from the designated villages. Thus, the intervention consisted (exclusively) of three in-depth sessions and three years of continuous placement support.

As noted, an important aspect of the intervention is that the employment opportunities are white collar. For women, this distinction may be particularly important. Boserup (1970), Costa (2000), Goldin (1990, 1995, 2006), and Mammen and Paxson (2000) argue that there may be less of a social stigma associated with women working in white-collar jobs. These jobs are considered safer and "cleaner" than manual labor, such as factory work.[6] Another relevant distinction is that women have a comparative advantage in this type of employment, because it does not require physical strength (Goldin 1990). Finally, these jobs may be perceived as more personally satisfying, particularly compared to physical labor. We would therefore not necessarily generalize our findings to opportunities for women in agriculture (as in Foster and Rosenzweig 2009) or blue-collar sectors such as manufacturing (as in Atkin 2009).

However, we feel the experiment is relevant for understanding the consequences of changes in women's employment for several reasons. First, the Indian economy, along with that of most other countries, is shifting toward the service sector, where white-collar employment predominates. Services are the most rapidly growing sector in India, currently accounting for over 60% of GDP (up from 26–28% in the 1980s), with the information technology sector alone representing 8% of GDP (up from 1% just a decade ago). Second, throughout the world, much of the modern history of women's increasing paid labor force participation, particularly for married women, was driven by the white-collar, service, or clerical sectors (Goldin 1990, 1995, 2006; Costa 2000; Mammen and Paxson 2000). In particular, Goldin (1984, 1990, 2006) argues that the rise in female labor force participation in the United States in the early twentieth century was due in part to growth in clerical jobs, in turn fostered at least partly by innovations in office or information technologies. Newer information technologies such

6. Though there are some respects in which BPO jobs are considered less appropriate or more risky for rural women (for example, if they must commute, or migrate to the city and live on their own).

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as computers and the Internet may be playing the same role in countries like India today as that played by their predecessors (such as the telephone and typewriter) in currently wealthy countries a century ago.

Finally, although we argued that the greater opportunities for women caused by our intervention could lead to delays or declines in marriage or fertility through changes in opportunity costs, there may be offsetting factors, such as through the marriage market. For example, if women can work after marriage, BPO jobs might increase the demand for younger brides, as the families of potential grooms are more willing to take on a new member who can bring greater income to the household (though parents may be more be reluctant to part early with their daughters for the very same reason). Therefore, the net effect on age at marriage is perhaps more ambiguous.[7] In rural India, many women do work after marriage, though working for pay away from home is much less common. Furthermore, because women tend on average to marry at an early age, and the education requirements for the BPO sector are high, for the youngest women we largely expect a delay in marriage or fertility, because schooling and marriage are rarely combined.

III. Data and Empirical Strategy

III.A. Survey Information

We conducted a baseline household survey in September and October 2003 for each of the 160 treatment and control villages. The survey was conducted by students at the Management Development Institute, a business school outside of Delhi. In each of the villages, we worked with a local official to draw up a list of all households and randomly selected 20 households per village. The sampling was conducted independently of the intervention, and thus the sample contains some individuals that attended the recruiting sessions and some that did not.

The survey consisted of a household questionnaire and an adult questionnaire. The household questionnaire included questions on demographic and socioeconomic characteristics (age, sex,

7. The net effects may even change over time. For example, Goldin (1997) finds dramatic shifts in the timing of work versus family, including the likelihood of childlessness or never marrying, across five sequential cohorts of twentieth-century American college-educated women.

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and education of all members, expenditures, etc.). The adult questionnaire included, among other things, a module for marital, fertility, and employment histories and expectations.

Follow-up surveys with the same households were conducted in September to October 2006. We also tracked and where possible surveyed all individuals who left home between the rounds, such as for work or marriage. We discuss attrition in more detail shortly.

Importantly for our analysis, we asked for information (demographic, socioeconomic, and marital and fertility histories) of all household members or children of members who have either temporarily or permanently left the household, such as for work or marriage. In addition, in both rounds we also directly administered the adult questionnaire to such individuals through phone surveys. These interviews were conducted over a longer period of time (September to February) because it often took longer to contact these absent individuals. Overall, we were able to interview 91% of such individuals in round 1 and 94% in round 2.

Table I reports baseline summary statistics for the full sample and separately by treatment status, as well as tests of treatment-control balance. The variables overall are well balanced between the control and the treatment groups. Formal tests suggest that randomization was successful: the p-value for the F-test that baseline characteristics jointly predict treatment is .77 and variable-by-variable individual tests cannot reject that the means are the same for treatment and control groups for almost all variables (column (4)).

III.B. Empirical Strategy

Because the intervention was randomly assigned, for our primary empirical specification we simply regress round 2 outcomes on an indicator for being from a treatment village, $Y_i = \beta_0 + \beta_1 Treatment_i + \varepsilon_i$. In a second specification, we add controls that are baseline predictors of the outcome variables, $Y_i = \beta_0 + \beta_1 Treatment_i + \sum_{\gamma i} Z_i + \varepsilon_i$, where Z includes parents' education, log of family expenditure per capita, family size, and a full set of age dummies. Finally, a third specification uses the change in outcomes between rounds 1 and 2 as the dependent variable, $\Delta Y_i = \beta_0 + \beta_1 Treatment_i + \varepsilon_i$. Although randomization should result in treatment and control groups being similar across all variables in expectation, in any particular sample there can be small baseline differences, and these additional specifications will capture

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those differences. However, because the three specifications yield very similar conclusions, for brevity the results from the second two specifications are presented in Appendix B. For all specifications, we estimate linear regressions regardless of whether

TABLE I

MEANS, STANDARD DEVIATIONS, AND TESTS OF TREATMENT-CONTROL COVARIATE BALANCE AT BASELINE

	(1) All	(2) Control	(3) Treatment	(4) Diff. (3) – (2)
Village-level variable				
Distance to Delhi (km)	129	130	128	−1.5
	[57]	[56]	[59]	(9.1)
Household-level variables				
Log (expenditure per capita)	6.38	6.37	6.38	0.01
	[0.66]	[0.65]	[0.67]	(0.023)
Head's years of schooling	3.76	3.67	3.86	0.19
	[3.77]	[3.72]	[3.81]	(0.13)
Spouse's years of schooling	1.80	1.79	1.80	0.022
	[2.67]	[2.62]	[2.72]	(0.094)
Family size	5.57	5.53	5.63	0.094
	[2.44]	[2.47]	[2.41]	(0.092)
Individual-level variables (by age at round 1)				
Age (female): 5–24	14.0	14.0	14.1	0.14
	[5.62]	[5.63]	[5.62]	(0.19)
Age (Male): 5–24	14.0	13.8	14.1	0.35*
	[5.52]	[5.46]	[5.56]	(0.19)
% women 15–21 married	0.36	0.36	0.36	−0.003
	[0.48]	[0.48]	[0.48]	(0.027)
% women 15–21 have given birth	0.27	0.28	0.27	−0.011
	[0.45]	[0.45]	[0.44]	(0.025)
% men 15–21 married	0.20	0.20	0.20	0.003
	[0.40]	[0.40]	[0.40]	(0.022)
% men 15–21 whose wife has had a child	0.045	0.050	0.041	−0.008
	[0.21]	[0.22]	[0.20]	(0.011)
% women 18–24 work for pay	0.20	0.19	0.20	0.006
	[0.40]	[0.40]	[0.40]	(0.024)
In school: girls 6–17	0.73	0.73	0.73	−0.006
	[0.44]	[0.44]	[0.44]	(0.018)
In school: boys 6–17	0.81	0.81	0.80	−0.009
	[0.40]	[0.40]	[0.40]	(0.017)
BMI-for-age (z-score): girls 5–15	−1.25	−1.25	−1.26	−0.015
	[1.35]	[1.38]	[1.33]	(0.057)

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TABLE I
(CONTINUED)

	(1) All	(2) Control	(3) Treatment	(4) Diff. (3) – (2)
BMI-for-age (z-score): boys 5–15	−1.31	−1.29	−1.34	−0.056
	[1.54]	[1.51]	[1.55]	(0.065)
Height-for-age (z-score): girls 5–15	−2.03	−2.02	−2.04	−0.014
	[1.34]	[1.32]	[1.37]	(0.057)
Height-for-age (z-score): boys 5–15	−2.01	−1.99	−2.03	−0.034
	[1.36]	[1.36]	[1.33]	(0.056)

Notes: Values for variables collected in the round 1 survey (September–October 2003). Standard deviations in brackets in columns (1)–(3). Number of observations: village level: 160; household-level: 3,211; female 5–24: 3,922; male 5–24: 4,342; women 15–21: 1,361; men 15–21: 1,547; girls 6–17: 2,464; boys 6–17: 2,819; girls 5–15: 2,233; boys 5–15: 2,508. Distance to Delhi is the shortest distance by road, calculated using Google Maps. The last column contains t-tests of the difference in means between the control and the treatment samples; heteroskedasticity-consistent standard errors accounting for clustering in parentheses. *Significant at 10% level.

the outcomes are continuous or discrete, but limited dependent variable models yield nearly identical results. Standard errors are adjusted for clustering at the village level.

For our analysis of the work versus marriage and childbearing decision, we focus on women aged 15 to 21 at baseline (18 to 24 at follow-up). This is the primary "exposed" cohort in our treatment, in the sense that they were old enough to potentially be placed in BPO jobs during the three-year period of our intervention and might therefore choose[8] to forgo marriage or childbearing in favor of working. This age range also represents the peak period during which women typically make these work and family transitions; less than 1% are married at 14 or younger,[9] but about 80% are married by 24. Separately, we also consider the effects of the treatment on human capital outcomes for younger girls (5 or 6 to 17 at follow-up).

8. These decisions may be made by women, their parents, or both (or, for fertility, a husband). We cannot determine who makes these decisions with our data, so a change in outcomes could reflect a change in desires of any or all of these parties. Greater opportunities for women may even affect relative influence.

9. Children or young adults may be married but live apart in their natal households for as long as a few years. Cohabitation and conjugal marriage begin later, at which time a second ceremony (*gauna*) is typically performed. We include marriages "without *gauna* performed," but they are very uncommon for those over age 15, so our conclusions are unchanged if we exclude them.

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IV. Results

IV.A. Employment

The recruiters reported placing about 900 women from treatment villages in jobs over the three-year period (though this includes helping some women get a job more than once). Some of these women might have gotten BPO jobs even without our intervention, which we can net out using the control group.

In addition to looking at BPO employment, we also examine any work for pay in a nonfamily enterprise. We use this additional outcome for several reasons. First, although we gathered detailed data on occupation, industry, and employer, the exact boundaries of the BPO sector are not always clear-cut. Second, to the extent that the recruiters provided job search skills that helped women get jobs in other sectors (e.g., bank teller), we should count these gains as well. Finally, if we focus only on BPO jobs, we might overestimate the impact of the treatment on women's net employment, because some women may have just shifted to this sector from another.

Table II shows the results. We split the sample by age and sex, because the intervention was targeted toward younger women. Round 2 control group means are presented in the bottom row in this and all other tables. The first column of the top panel shows that in treatment villages in round 2, women age 18 to 24 were 4.6 percentage points more likely to be employed in the BPO sector than similarly aged women in control villages (for whom BPO employment was close to 0).[10] This effect is fairly large in light of the fact that only about 28% of women in this age group had enough schooling to qualify for these jobs as of round 2.

Paid employment outside the home in any industry was 2.4 percentage points higher for women in this age group. This effect is small in absolute terms, though somewhat larger when compared to the control group mean of 21%. The fact that this is smaller than the increase in BPO employment suggests that some women who would have worked even without the treatment substituted from other jobs into the BPO sector. Columns (1) and (2) in Appendix B show that both employment effects are robust

10. It is unlikely that the jobs gained in treatment villages came at the expense of women in control villages. The pool of women competing for these jobs is large, so the loss of jobs within our set of control villages is likely to be very small; because few women in rural areas get these jobs, any losses are likely to be found in urban areas, which are outside our sample.

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TABLE II
EFFECT OF THE INTERVENTION ON EMPLOYMENT, BY AGE AT ROUND 2

	BPO employment			Works for pay away from home		
	(1) 18–24	(2) 25–44	(3) 45–60	(4) 18–24	(5) 25–44	(6) 45–60
Panel A: Women						
Treatment	0.046***	0.003	∼	0.024**	0.0029	−0.006
	(0.008)	(0.003)		(0.011)	(0.0089)	(0.014)
Observations	1,278	2,233	1,029	1,278	2,233	1,029
Control group mean	0.004	0.002	0.00	0.21	0.24	0.22
R^2	0.022	0.000	∼	0.054	0.001	0.000
Panel B: Men						
Treatment	−0.007	0.002	∼	0.003	0.007	−0.004
	(0.005)	(0.004)		(0.011)	(0.024)	(0.035)
Observations	1,442	2,469	1,104	1,442	2,469	1,104
Control group mean	0.008	0.003	0.00	0.47	0.56	0.52
R^2	0.001	0.000	∼	0.000	0.001	0.000

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. Age ranges are for age at round 2. The dependent variable is an indicator for whether an individual in round 2 had a job in the BPO sector in columns (1)–(3), and whether they worked for pay away from home in round 2 in columns (4)–(6). ∼ indicates that the coefficient could not be estimated because no one in the age*sex category had a BPO job. *Significant at 10% level; **significant at 5% level; *** significant at 1% level.

to the specifications adding additional baseline controls or using changes in employment as the dependent variable.

Table II also shows that as expected given the experimental design, there was no change in BPO employment or any work for pay away from home for older women or for men of any age. The coefficients in these cases are small and not statistically significant. Thus, BPO and net employment increased specifically for the set of younger women the intervention was targeted toward, and only those women.

The survey also asked women whether they expected to work for pay in a nonfamily enterprise in various future life stages: before marriage, after marriage but before they have children, after they have children but when their children are still young, and after those children are all adults. Table III shows round 2 means for the treatment and control groups for women aged 18 to 24, as well as the coefficient from a regression of each outcome on an indicator for being from a treatment village. Women's (paid) work expectations in general are very low. Only 30% of women in the control group hope to work for pay before they marry, and

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TABLE III
INTENTIONS TO WORK THROUGHOUT THE LIFE COURSE, WOMEN 18–24 IN ROUND 2

	(1) Control	(2) Treatment	(3) Difference (2) – (1)
Do you expect to work for pay (nonfamily) away from home . . .			
before you get married?	0.30	0.43	0.13**
	[0.46]	[0.49]	(0.051)
after marriage, but before you have children?	0.19	0.30	0.10**
	[0.40]	[0.46]	(0.048)
when your children are still young?	0.045	0.074	0.029
	[0.21]	[0.27]	(0.026)
after all children have left school and are married?	0.23	0.34	0.11**
	[0.42]	[0.47]	(0.049)

Notes: Data are from round 2. Standard deviations in brackets in columns (1)–(2)2; heteroskedasticity-consistent standard errors accounting for clustering in parentheses in column (3). The last column contains the coefficient from a regression of the outcome on an indicator for being from a treatment village at baseline, as well as an indicator for whether the woman was living at home (versus the interview being conducted by phone). Number of observations: 344 in row 1; 360 in row 2; 397 in row 2; and 432 in row 4. **Significant at 5% level.

this declines substantially to around 19% after marriage. Only 5% expect to work when they have young children, with an increase back up to 23% when their children are older.

However, the treatment resulted in large changes in expectations. The desire to work before marriage is around 13 percentage points higher for women from treatment villages. The share who want to work after marriage but before they have children and the share who want to work after their children are older increase by 10–11 percentage points. However, there is no significant effect on whether women expect to work when they have young children; thus, there remains the expectation that women will leave the labor market to raise children, even if they hope to return afterward (women with children participate in household production and on family farms or enterprises at a much higher rate). Although these questions only measure intentions or desires, the results are consistent with a change in women's aspirations toward work as a career in the sense of a life-long attachment to the workforce (albeit still with a departure for raising children).[11]

11. We also asked adults whether they thought it was acceptable for women to work away from home after marriage. We find that the treatment increased the

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IV.B. Human Capital

Most women in the 18–24 years age cohort will be too old to still be enrolled in school at round 2. However, the survey asked about enrollment in vocational or training institutes, academies, or "colleges" that offer courses, programs, or certification in a range of subjects, including computers and English. Though these entities (which are primarily private and fee-charging) are of varying quality and the payoff to such investments are unknown, women may choose to invest in such training if they believe it can improve their chances of getting and keeping a BPO job.

Column (1) of Table IV shows that women 18–24 in treatment villages were 2.8 percentage points more likely to be enrolled in such programs. This is a particularly large effect, given that only about 0.5% of similarly aged women in control villages were enrolled in such programs, and it indicates a willingness to invest in human capital to get a job or build a career when suitable opportunities are available. For men, there is no evidence of increased enrollment in these programs.

Most girls 17 and younger at round 2 are too young to have completed sufficient schooling to get placed in a BPO job during the period of our intervention. However, if parents believed their daughters might still be able to get BPO jobs even after the intervention ends,[12] they may still keep them in school longer. Their beliefs about future BPO opportunities would also more generally increase the returns to other forms of human capital, such as health (the returns may also include nonfinancial returns or returns in the marriage market).

We consider two human capital outcomes for younger persons. First, parents were asked about the current enrollment of each child, which we then verified by contacting the school the child attended.[13] We consider enrollment for children aged 6 to 17 at round 2. Because not every child is enrolled at age 6 and

reported acceptability by 4.3 percentage points, relative to a control group mean of 7.1.

12. For example, parents may have mistakenly believed that the recruiters would continue assistance beyond the three-year period. Or they may have believed that the recruiters were not essential to getting a BPO job, or that women who got BPO jobs during the intervention period would become a network of contacts, making it easier for their daughters to get these jobs (e.g., Munshi 2011).

13. We asked parents in advance for permission to visit their children's school to verify enrollment, so parents had little incentive to misreport enrollment. Only 0.9% of cases had a discrepancy between what the parent and school reported. The rate is slightly higher in treatment villages, but the difference is small (1.1%

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TABLE IV
EFFECT OF THE INTERVENTION ON HUMAN CAPITAL

	(1) Enrolled in training (18–24)	(2) Enrolled in school (6–17)	(3) BMI for age (5–15)	(4) Height for age (5–15)
Panel A: Women				
Treatment	0.028***	0.050***	0.24***	0.063
	(0.008)	(0.015)	(0.070)	(0.066)
R^2	0.010	0.004	0.007	0.001
Observations	1,278	2,264	2,031	2,031
Control group bean	0.005	0.76	−1.25	−2.02
Panel B: Men				
Treatment	0.003	0.010	−0.020	0.005
	(0.004)	(0.011)	(0.076)	(0.052)
R^2	0.000	0.001	0.000	0.000
Observations	1,442	2,511	2,295	2,295
Control group mean	0.004	0.81	−1.29	−1.99

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. All dependent variables measured in round 2; the number ranges in parentheses indicate the (round 2) age range over which the regression is estimated. The regressions contain only an indicator for coming from a treatment village, with no additional covariates. BMI for age and Height for age are *z*-scores.
*** Significant at 1% level.

because some repeat grades, some students could still be enrolled at age 18 or 19, but the results are robust to using these later cut-offs.

Second, as part of our survey, enumerators took physical measurements of weight and height for all household members aged 5 and older.[14] To capture the joint effects of nutrition and health care, we computed height-for-age and BMI-for-age *z*-scores, using the age- and sex-specific standards for school-aged children and adolescents developed by the World Health Organization (de Onis et al. 2007). We focus on children aged 5 to 15; we exclude older children because we can only physically measure those individuals still living at home, and after 15 the likelihood of leaving home due to marriage increases. We could then have selectively missing data, possibly even correlated with treatment status if the treatment affects marriage (this concern does not affect data on

versus 0.7%). The regression results are similar using either parental reports or the verified data.

14. By scheduling up to three return visits, we were able to get measurements for 98% of youths aged 5–15 in round 1 and 99% in round 2 (excluding those that left the sample between rounds).

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schooling or marriage and fertility outcomes, because remaining household members were asked about these outcomes for absent members, and, as noted, the absent members themselves were separately visited or contacted by phone).

Table I provides baseline means for these variables. Enrollment rates are 73% for girls aged 6–17, compared to 81% for boys. The sample of children is fairly undernourished, with a very low average BMI and height relative to international norms. On average, children 5–15 are 1.2 to 1.3 standard deviations below their age- and sex-specific reference median BMI and 2.0 standard deviations below the reference median height. However, unlike schooling, there are no evident gender differences in these measures (as is commonly found in other anthropometric data for India; see Deaton 2007 for possible explanations).

Column (2) of Table IV shows that in round 2, girls 6–17 were 5.0 percentage points more likely to be enrolled in school in treatment villages. This is a large gain in absolute terms, and closes about 60% of the baseline boy–girl gap in enrollment at these ages. Overall, these gains are consistent with Oster and Millett (2010), who find that call centers caused similarly large schooling increases for both boys and girls in India (because those jobs were available to both sexes) and Shastry (forthcoming), who finds education gains associated with growth in the information technology sector in India. Consistent with these gains, and the high costs of schooling in India (Das et al. 2010), in Online Appendix Table B we show that the treatment led to an increase in the budget share devoted to schooling in households with school-aged girls.[15]

Column (3) shows that the treatment also resulted in an average increase in BMI-for-age z-score of 0.24 for girls at Round 2. The effect is fairly large, particularly relative to the control group mean deficit of 1.25; the treatment closed about 15–20% of the BMI gap between our sample and the well-nourished WHO reference population, or about 30–40% of the gap with the wealthiest residents of Delhi (as measured by an index of asset ownership), for whom the mean BMI-for-age z-score is –0.64 (International Institute for Population Sciences and Macro International 2007).

15. The treatment coefficient is negative for several other expenditure categories, but none are statistically significant. With total expenditures unchanged (see later discussion), increased schooling expenditures appear to be financed through smaller reductions across several categories.

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Point estimates of the effect of the treatment on height for age are positive but very small and not statistically significant. The medical literature suggests that nutrition appears to have little effect on height after the age of two, including any possible "catch-up" effects (Russell and Rhoads 2008). Thus, though we were not aware of it when designing the study, it is perhaps not surprising that we find no effect on height.

Overall, these results show that clear and salient evidence of greater economic opportunities for women is met with increases in human capital investments for girls (Appendix B again shows that these results are robust to the alternative specifications).[16] These results have important implications for understanding the potential role of labor market returns in addressing the dramatic gender differences in human capital observed in countries such as India, as suggested by Rosenzweig and Shultz (1982) and Foster and Rosenzweig (2009).[17]

The bottom panel of Table IV shows the effect of the treatment for working-aged men and school-aged boys. Across all human capital measures, the coefficients are small, and none are statistically significant. This conclusion holds even if we restrict the sample to households with at least one boy and at least one girl (results available on request). The absence of an effect for boys is consistent with the intervention having increased the opportunities for girls only.

IV.C. Marriage and Fertility

Table V presents the effects of the treatment on marriage and fertility. Women aged 15 to 21 at baseline from treatment villages were 5.1 percentage points less likely to get married during the three-year period of the study. This is a fairly large effect in absolute terms and relative to the 29% of women in the control group who remain unmarried at round 2. Column (4) of

16. Other papers finding that human capital investments responds to changes in returns include Foster and Rosenzweig (1996), Jensen (2010), and Abramitzky and Lavy (2011); see also Rosenzweig (2010).

17. We also estimated separate regressions by age. Girls 6–10 (primary school age) were 3.1 percentage points more likely to be enrolled in treatment villages, but the effect is not statistically significant (p-value of .23). For girls 11–17, the treatment resulted in a 5.9 percentage point gain (significant at the 5% level). Thus, the schooling gains were greater for older girls. This is perhaps not surprising, because enrollment rates are 90% for girls 6–10, but only 51% for girls 11–17. For anthropometrics, the effects are fairly similar for girls ages 5–10 and 11–15. None of the effects are significant for boys of either age.

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TABLE V

EFFECT OF THE INTERVENTION ON MARRIAGE AND FERTILITY, AGES 18–24 IN ROUND 2

	(1) Married	(2) Had child	(3) Desired fertility
Panel A: Women			
Treatment	−0.051**	−0.057**	−0.35***
	(0.024)	(0.026)	(0.078)
R^2	0.003	0.003	0.018
Observations	1,278	1,278	1,226
Control group mean	0.71	0.43	3.0
Panel B: Men			
Treatment	−0.002	−0.009	0.027
	(0.025)	(0.018)	(0.066)
R^2	0.000	0.000	0.000
Observations	1,442	1,442	1,437
Control group mean	0.44	0.15	3.3

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. All dependent variables measured in round 2. The regressions contain only an indicator for coming from a treatment village, with no additional covariates. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

Appendix B shows that these conclusions are robust to adding baseline covariates or using changes in outcomes as the dependent variable.[18]

Column (2) of Table V shows that there were also large reductions in childbearing in response to the treatment. Women 15 to 21 were 5.7 percentage points less likely to have given birth by round 2. Again, this is a fairly large effect. This is also larger than the effect of the treatment on marital status, indicating that some women who married despite the treatment (or who were already married at baseline) still chose to delay having a child (so they could continue working or get additional training). Not all women who want to work will delay marriage (because marriage and work are compatible for some women), but almost all women who want to work will delay childbearing, since far fewer women work for pay away from home in the years immediately after giving birth.

18. Though we cannot observe outcomes beyond our 3 year period, parents were asked at what age they expect each of their children to get married (a decision over which they typically exert considerable control). Expected age at marriage for women increases by 0.73 years in treatment villages (significant at the one percent level). For some women, the changes are dramatic; the expected age at marriage increased by 5 or more years between rounds for 7 percent of women.

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Overall, the effects on marriage and fertility for women are large. For comparison, Duflo, Dupas, and Kremer (2011) find in Kenya that subsidized school uniforms reduced the likelihood of ever being married or pregnant by 2.5 to 4.5 percentage points 3 and 5 years after the intervention began, for girls who would have been mostly 16 to 19 years old at follow-up. For the United States, Goldin and Katz (2002) find that access to the oral contraceptive pill at age 17 resulted in a 3 percentage point decrease in the likelihood of marriage by age 23 for women (as in the present case, due to an increase in women's employment in professional occupations requiring greater human capital). Bailey (2006) finds the pill also led to a 7 percentage point reduction in the likelihood of having a child by age 22 (but no effect at 19).

For men, there is no effect on marriage or fertility. This is consistent with the lack of any schooling, training, or work changes for men induced by the treatment. Because marriage is commonly between individuals from different villages, there is no reason marriage rates for men must match the rates for women (unless a large share of women in our treatment villages marry men from our sample of control villages; even then, because women tend to marry older men on average, we still might not expect changes for men of this age range).

With a short panel, we cannot distinguish whether these changes will translate into a decline in completed fertility or simply a delay.[19] However, the survey did gather data on fertility intentions. Adults were asked how many children they would like to have in their lifetime (again, this includes those not residing at home and interviewed by phone). The last column of Table V shows that the treatment resulted in women 18 to 24 reporting they wanted approximately 0.35 fewer children on average (compared to a control group mean of 3.0). These responses are, of course, just reported intentions; we have no way of knowing whether they will result in actual changes to completed fertility. However, these expectations at least reflect a desire for fewer children and perhaps a willingness to trade off between career and fertility (e.g., Goldin and Katz 2002).

19. Historically, delayed marriage and fertility are often early indicators of fertility decline. Furthermore, they can have a direct effect on fertility (through both a shorter period of exposure to the risk of unintended pregnancies, and the increased difficulty in conceiving with age) and on their own can slow the rate of population growth by increasing the length of time between generations.

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IV.D. Summary of Effects

Overall, the treatment led to employment gains and increased enrollment in postschool training courses for working-aged women 18–24 and, correspondingly, delays in marriage and fertility. For school-aged girls, there were increases in both enrollment and BMI. For working-aged men and younger boys, there is no evidence of any changes in response to the treatment.

Because we are considering the impact of the treatment on a number of outcomes, we present the mean effect of the treatment across outcomes computed using the method developed by Kling, Liebman, and Katz (2007). This approach standardizes the outcome variables to mean zero and unit standard deviation and redefines them where necessary so that a higher value always constitutes an improvement. The mean effect is then computed as the unweighted average of the coefficients on the treatment variable for each of the standardized outcomes. For women and men 18–24, we use six outcomes (enrolled in training; work for pay; work in a BPO job; married; has had a child; desired number of children). For school-aged boys and girls, we use three (enrolled in school; BMI for age; height for age).

The test statistics in columns (1) to (3) of Table VI show that the treatment had statistically significant effects on the aggregate set of work–family transition outcomes for working-aged women across all three specifications. The same holds for younger girls. By contrast, the treatment had no effect on working-aged men or younger boys, consistent with the fact that the treatment was designed to improve employment opportunities for women only.

IV.E. Attrition

As noted, we gathered information about household members and children of members who left home either temporarily or permanently and also visited and/or conduced phone interviews with most such individuals. Thus, in cases where an individual left home, we still have reports for most of the relevant outcomes (the exception being anthropometric measures). It is primarily in cases where the entire household left that we do not have these data. Online Appendix Table C presents the attrition rates by treatment status.

Household attrition was fairly similar for treatment and control groups (3.3% and 3.0%, respectively). The same holds for the individual-level samples we analyze, where we also include

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TABLE VI

SUMMARY EFFECTS OF THE INTERVENTION

	(1) Full sample (no covariates)	(2) Full Sample (w/ baseline covariates)	(3) Full sample (changes specification)	(4) Households w/ no qualified members (no covariates)
Working-aged women	0.15***	0.15***	0.085**	0.10***
	(0.040)	(0.038)	(0.039)	(0.042)
School-aged girls	0.11***	0.10***	0.10***	0.12***
	(0.031)	(0.030)	(0.026)	(0.032)
Working-aged men	0.003	0.003	−0.032	0.006
	(0.026)	(0.025)	(0.024)	(0.031)
School-aged boys	0.006	0.004	0.021	0.011
	(0.030)	(0.028)	(0.017)	(0.032)

Notes. The table presents the mean effect of the treatment across outcomes for the specified age*sex cohort, computed using the methodology described in Kling, Liebman, and Katz (2007). For working-age women and men, the outcomes are: working for pay; working in a BPO job; taking a training course; marriage; having had a child; and desired fertility. For school-aged girls and boys, the outcomes are: going to school; BMI for age; height for age. Column (1) uses round 2 outcomes as the dependent variable and includes only a treatment dummy in the regressions. Column (2) adds baseline log of household expenditure per capita, mother's and father's education, family size, and age to the regressions (and indicators for missing values of these variables, which are replaced with sample medians). Column (3) uses round 2 – round 1 changes as the dependent variable. Column (4) restricts the sample to households where all adult members, excluding the individual analyzed in the regression, has less than 10 years of schooling. Column (1) tests for the mean effects across the regressions in Tables II, IV, and V; columns (2) and (3) test for the mean effects for the regressions in Appendix Table I; column (4) tests for the regressions in Table VII. **Significant at 5% level; ***significant at 1% level.

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attrition due to death or otherwise having no data in round 2. Attrition was 6.2% for women 15–21 in treatment villages and 5.7% in control villages; for girls 6–17 it was 5.0% in both treatment and villages control villages, and for girls 5–15 it was 4.9% in treatment villages and 4.7% in control villages. Online Appendix Table D provides summary statistics for attriting and nonattriting households, and Online Appendix Table E presents regressions for baseline predictors of attrition for households and individuals in the individual subsamples. In general, attrition is greater for those from households that are poorer, do not own land, and have younger heads, as might be expected given that attrition is driven largely by household migration. Women's baseline outcomes (marriage, fertility, schooling) are much worse in attriting households. However, there is no correlation between attrition and treatment.

We also estimated the baseline determinants of attrition as in Online Appendix Table E separately for the treatment and control groups (results available on request). Though there are some differences in the coefficients, overall we are unable to reject equality of the sets of coefficients and conclude that the same model can predict attrition in both the treatment and control groups (of course, this does not rule out differential attrition between the two groups based on unobservable characteristics). Finally, following Fitzgerald, Gottschalk, and Moffitt (1998), we reestimated the regressions in Tables II through V using the inverse probability of attrition predicted from the regressions above as weights. Online Appendix Table F shows that the results are similar to the original results. Some coefficients are larger and some smaller, but we arrive at the same conclusions that the treatment was associated with statistically significant gains in employment and training and delays in marriage and fertility for women. Although there is no perfect test or correction for attrition bias, the results in Online Appendix Tables C to F suggest that attrition is unlikely to account for our results.

V. Discussion of Alternative Mechanisms

We argued that the changes in marriage, fertility, and human capital were driven by increases in women's own employment or training (for the 18–24 working-aged cohort) or the greater future economic opportunities for currently young women and girls (for the school-aged cohorts). However, we need to consider other

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channels through which the intervention may have influenced these outcomes. In particular, adults other than the individual themselves (e.g., parents or older siblings) might have gotten a BPO job, which could affect the outcomes of younger women in the household. For example, if the mother of a woman 18–24 or a girl 6–17 got a BPO job, she may have more control over household decisions and choose to keep her daughter in school longer or to delay her marriage. Employment of other adults in the household could also influence women's and girls' outcomes via income effects (e.g., higher income leads to greater education), changes in the household allocation of time (e.g., the mother gets a job, so they delay the daughter's marriage so that she can take over her mother's household activities), or changes in the parents' fertility (e.g., the mother has fewer children because she herself is working, so younger girls are fed more because they compete for resources with fewer siblings, as in Garg and Morduch 1998). Even under these alternatives, our intervention would still show a causal link between opportunities for women and these outcomes. However, these mechanisms yield very different implications for understanding both the root causes of the outcomes analyzed and the policy instruments that might address them most effectively.

Our goal is not to reject that these other mechanisms ever occur, possibly even in our sample (though again, we chose the BPO sector precisely because the opportunities are targeted almost exclusively toward younger, unmarried women) but simply to test as cleanly as possible the effect of employment of the young woman herself or the potential for future employment (for girls). Though Table II showed no employment gains for men or older women, we can reduce or eliminate these channels by looking at households where no member (other than the individual themselves), male or female, including adult children or other members temporarily or permanently living away from home, could get one of the jobs, now or in the future, because they have too little education (less than 10 years). This restriction eliminates only about 20% of our sample, so the conclusions still apply quite widely (but we would not generalize these results to the more educated sample, who might already know about opportunities for women, and thus respond less, or have more progressive attitudes toward women working, and thus respond more). Online Appendix Table G shows that these households are poorer and have slightly worse baseline outcomes for both women and girls than the full sample. But the treatment and control groups within this subsample are still well

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balanced with respect to baseline covariates. However, although we consider this to be a useful test against these alternative mechanisms, once we stratify our sample in this way we are deviating from the original experimental design, so the results must be interpreted with more caution.

Table VII shows that there are still gains for women and girls in this sample. The results are similar to the full sample results, because we still use 80% of the full sample. Human capital, marriage, and childbearing are still positively affected by the treatment. The point estimates differ slightly from the previous tables, but the mean effects of the treatment on the set of outcomes is still significant (column (4) of Table VI). Thus, even in households that could not have experienced changes in income, bargaining power, or time allocation through employment of one of their other members, there are still gains for women.

However, this does not rule out similar effects that do not come through changes in adult employment within the household. For example, income or bargaining power effects could arise through money given to the household by friends or other relatives who got a BPO job. The survey asked each person in the household about all transfers or gifts received from individuals outside the household, including cash, goods, or payments made by others on behalf of the household. The first four columns of Table VIII regress the probability of having received any such transfers and the amount received on the treatment indicator, both for the household as a whole (which could create income effects) and for female members in particular (which could alter bargaining power). We continue to focus on households where no member has enough education for one of the jobs; households where a member got a BPO job and migrated to the city, for example, are likely to have increased transfers, and our goal is to test for transfers to households that did not get a BPO job. The results show that there was no change in either the incidence or amount of transfers received for households as a whole or by women in particular.

We can also address some possible refinements of these mechanisms that would not be excluded by these tests. First, we consider bargaining power. One possibility is that a mother's bargaining power may be increased now by the fact that her currently young daughter may work and send her money in the future. Alternatively, the recruitment of women in itself may serve as a signal about the status of women among people who live outside the village. The fact that some women from the village now work

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TABLE VII

TESTING FOR ALTERNATIVE MECHANISMS: HOUSEHOLDS WITH NO QUALIFIED MEMBERS

	Working age (18–24)						School age (as indicated)		
	(1) Works BPO	(2) Works for pay	(3) Training	(4) Married	(5) Gave birth	(6) Desired fertility	(7) In school (6–17)	(8) BMI for Age (5–15)	(9) Height for age (5–15)
Panel A: Women									
Treatment	0.042***	0.022*	0.024***	−0.058**	−0.053*	−0.38***	0.055***	0.23***	0.089
	(0.009)	(0.013)	(0.007)	(0.028)	(0.030)	(0.093)	(0.016)	(0.077)	(0.069)
R^2	0.017	0.001	0.008	0.004	0.003	0.020	0.005	0.007	0.001
Observations	1,020	1,020	1,020	1,020	1,020	983	1,944	1,688	1,688
Panel B: Men									
Treatment	−0.007	0.006	0.003	−0.009	−0.011	0.054	0.000	0.041	−0.008
	(0.005)	(0.019)	(0.004)	(0.025)	(0.020)	(0.075)	(0.017)	(0.083)	(0.054)
R^2	0.001	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	1,245	1,245	1,245	1,245	1,245	1,242	2,162	1,878	1,878

Notes. Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. The sample includes only those individuals from households where all members, excluding the individual themselves, has less than 10 years of schooling. All dependent variables measured in round 2; the number ranges in parentheses indicate the (round 2) ages over which we are estimating the regression. The regressions contain only an indicator for residing in a treatment village, with no additional covariates. BMI for age and Height for age are *z*-scores. *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

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TABLE VIII

TESTING FOR ALTERNATIVE MECHANISMS

	(1) Receives transfers	(2) Amount received	(3) Receives transfers, women only	(4) Amount received, women only	(5) Schooling/health decisions? (0–2)	(6) Decision-making (women > 18, 0–10)	(7) Autonomy (women > 18, 0–3)	(8) Monthly expenditure per capita
Treatment	0.009	5.86	0.010	−2.3	−0.010	−0.071	0.112	−21.2
	(0.012)	(9.94)	(0.015)	(6.8)	(0.023)	(0.184)	(0.347)	(32.1)
R^2	0.000	0.001	0.000	0.000	0.001	0.001	0.005	0.000
Observations	2,985	421	2,985	120	3,345	3,334	3,415	2,554
Control group mean	0.14	93	0.04	78	0.55	3.93	1.08	726

Notes. Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. All variables measured in the round 2 survey. The dependent variable in columns (1) and (3) is whether any member of the household received a transfer (in cash, goods, or payments made by others) in the past 12 months, and in columns (2) and (4) it is the amount of the transfer. Column (5) is whether the woman participates in household decisions on children's schooling and health care, and column (6) is the sum of responses to questions on whether the woman participates in household decisions on children's schooling and health care; health care for herself; what foods to cook; purchasing major household items; visiting family or friends. The possible responses for these questions ranged from 0 to 2: "2. respondent makes decision alone; 1. respondent makes decision jointly with others in the household; 0. respondent does not participate in the decision (husband or other household members decide)". The dependent variable in column (7) is the sum of the responses to questions about whether the woman: can visit the market without permission from her husband; visit family or friends without permission from her husband; is allowed to keep money to spend on her own as she chooses. The dependent variable in columns (2), (4), and (8) are in 2006 rupees.

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in the city may improve the bargaining power of all women, even those who cannot hope to work in one of these jobs in the future. These possibilities are not testable with our data. However, we can examine some simple, direct measures of women's bargaining power as a crude, overall test for any of these alternatives.

The survey asked all ever-married women whether they participated in household decision making on children's schooling and health care, obtaining health care for themselves, what items to cook, purchasing major household items, and visiting friends or family. The possible responses were on a scale of 0 to 2: "2. Respondent makes decision alone; 1. Respondent makes decision jointly with others in the household; 0. Respondent does not participate in the decision at all (husband or others decide)."[20] Additional questions were asked about women's "autonomy": whether they can visit the market without permission, can visit family or friends without permission, and are permitted to keep money set aside to spend as they wish. This second set of questions does not directly measure bargaining power, but we might expect responses to change along with women's bargaining power. Both sets of questions are limited, of course, and do not capture the full possible expression of women's bargaining power.

Column (5) of Table VIII shows results where an indicator for whether women participate in the schooling and health decisions of their children in round 2 is regressed on the treatment indicator, and columns (6) and (7) show results using two indexes, created as the sum of the round 2 responses to all of the individual questions on decision making and autonomy (with higher values of both indexes reflecting more bargaining power); again we restrict the sample to cases where no one has enough education for a BPO job. The treatment does not have a statistically significant impact on women's participation in decision making for schooling and health specifically, or for overall decision making and women's autonomy; the coefficients are all small and not statistically significant. Although these are only self-reports and may have reporting errors, the results are at least consistent with the possibility that the bargaining power

20. Women's participation in many of these decisions is quite limited overall; for example, in the control group in round 2, 53% of women report they do not participate in decisions about children's schooling or health care at all (39% report making the decision jointly with others in the household. An exception is that 91% of women participated in or decided on their own what to cook.

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of women who did not get a BPO job and could not get one did not change as a result of the treatment.

Next we address wealth. The results from Table VII largely rule out changes in current income earned by the household itself for our restricted sample, because no one in these households could have gotten a BPO job. We already noted that there was no change in transfer probabilities or amount received. However, this still leaves open several possibilities. First, households may gain income if other women who get a BPO job spend more in the village (or send home money that others spend). Alternatively, women leaving the village for BPO jobs may increase the local wage rate for women who stay. Finally, households may borrow against higher lifetime income from children's future BPO employment; however, households are likely to be highly credit constrained, particularly in terms of borrowing against income gains that are both uncertain and will only be realized many years in the future.

However, we note that the average village experienced an increase in employment of just a few women per year over this period relative to an average village population of about 1,900. Thus, any changes via other women working, sending back money, or through changes in local wages are likely to be quite small.[21] Additionally, any effects on the outcomes driven by changes in current income, regardless of the source, should be reflected in gains in average household expenditures. The final column of Table VIII shows regressions of total household expenditure on the treatment indicator, again restricting to households where no one has enough schooling for a BPO job. The coefficient on the treatment indicator is small and not statistically significant. However, there are significant difficulties in measuring expenditures, so this test cannot definitively rule out any income effects.

Though there is little evidence that our results were driven by these other factors, we cannot rule out that the experiment worked not by directly stimulating the demand for schooling or work among girls or their parents but by stimulating teachers to encourage girls. There is no evidence available on the responsiveness of schooling to teacher effort or encouragement. However, given that secondary schooling is fairly expensive in India, parents would perhaps be unlikely to incur the costs unless they perceived a value to doing so. Thus, it seems likely that any such teacher effects would be relatively small compared to

21. For a more direct test, we regress the wages earned by women who work on the treatment indicator. The coefficient is small and not statistically significant.

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the effects of the intervention on parents' desire to educate their daughters or delay their marriage. From a policy perspective, any intervention that stimulates a demand for educated female workers in any salient way is also likely to influence teachers to encourage students just as much as in the present case.

VI. Discussion and Conclusion

An intervention making employment opportunities for women more salient and accessible led to increased human capital investments for girls and delayed marriage and childbearing for women. Women also report wanting to work more and have fewer children in their lifetimes, consistent with increasing aspirations for careers.

The goal of the experiment was to test whether increased employment opportunities for women can affect lifecycle work and family transitions, rather than whether recruiting services as a policy instrument can help address these outcomes. Our particular intervention, and recruiting services more generally, does not actually create any new jobs. The women in our study may simply have gotten jobs at the expense of others, with no net effect on women's employment.[22] From both an efficiency and an equity perspective it is certainly worthwhile to make sure that information on economic opportunities is widely available, and studies such as Jensen (2010) show that students may not always be well informed of labor market returns. However, there are likely to be more cost-effective means of doing so, such as through the use of mass media.[23] For example, Jensen and Oster (2009)

22. Though if growth of the sector and competition with other firms internationally was constrained by a shortage of skilled labor, or if providing information to a broader pool of potential applicants improves the quality of worker–job match or increases productivity in the sector, net employment may increase.

23. Though our intervention compares favorably to others. For example, (Baird, McIntosh, and Ozler) (2011) provided girls in Malawi with cash transfers, either unconditional or conditional on school attendance. At the end of two years, the unconditional grant reduced marriage and fertility rates by 7.9 and 6.7 percentage points, respectively, but had only a modest (and not statistically significant) effect on schooling. These effects held even for the lowest transfer amounts, $100 per household. We find a slightly lower decline in marriage and fertility (5.1 and 5.7 percentage points) but a larger gain in schooling (5.0 percentage points). Each recruiter was paid $15,000 in salary and expenses and provided service to 10 villages, each with approximately 125 girls/women of the relevant ages, for a cost of $12 per individual. The conditional grant ($100/household, plus $140/household in administrative costs) increased enrolment by 11 percentage points but had no effect on marriage or fertility.

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show that the introduction of cable television in rural Indian villages also led to gains in women's schooling and reductions in fertility, potentially by providing new information on roles women might play outside of the home more generally and in the labor market in particular. Our recruiting intervention was a way of providing more targeted information, but media campaigns may perhaps be a more cost-effective way to provide the same information, particularly in a large, rural, and geographically dispersed populations.

The effects we observe are fairly large. Our intervention was focused and targeted, and highly successful in placing women in jobs if they wanted them and were qualified. We would therefore not necessarily generalize our results to other interventions aimed at helping women get jobs or smaller increases in labor force opportunities for women. Even if our intervention created a degree of overoptimism regarding opportunities for women, what is relevant—both for understanding the underlying decision-making processes for these outcomes and the possible impact of real, sustained gains in opportunities for women—is that the results reveal that if parents believe there are opportunities for their daughters, they will increase their human capital investments in them or delay their marriage.

We do not suggest that all historical changes in fertility, marriage, and women's human capital are driven by economic opportunities for women, or that this is the only arena in which policy efforts might be successful. However, the results are valuable because they show that these outcomes can respond to women's economic opportunities. Many governments, NGOs, rights groups, and international organizations have emphasized social or cultural determinants of poor outcomes for women (see Croll 2000). Correspondingly, the suggestion has been that gains may be difficult without deeper social or cultural change, and thus most policy efforts have emphasized awareness raising, and information and media strategies to promote the status of women, that is, efforts to act on the social or cultural factors. Although not denying some possible role for such efforts (again, as possibly shown by the effects of cable television in rural India found by Jensen and Oster 2009), our results demonstrate an economic or labor market underpinning to the causes of and potential solutions to these outcomes.

The results also suggest there may be improvements in these outcomes even in the absence of policy interventions. The rise

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of the BPO sector, along with rapid growth in the white-collar service sector more generally, is shifting the Indian economy away from agriculture and manufacturing. Though employment growth in these new sectors has been slower than the growth in their GDP share, this shift is likely to continue to generate a greater demand for educated female labor and a corresponding increase in female labor force participation, as has been observed in other countries (Goldin 1990, 1995, 2006). Historical evidence suggests such changes can be rapid. As recently as the 1960s, paid labor force participation rates were only around 30% in both the United States and Britain, increasing to 58% and 71%, respectively, in less than three decades (Costa 2000). Our results indicate that any coming gains in opportunities for women may result in changes in human capital, marriage, and fertility outcomes as well.

APPENDIX

APPENDIX A

Map of the survey area

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APPENDIX B

EFFECTS OF THE INTERVENTION ON WOMEN: ADDING BASELINE CONTROLS AND USING CHANGES

	Working age (18–24)						School age (as indicated)		
	(1) Works BPO	(2) Works for pay	(3) Training	(4) Married	(5) Gave birth	(6) Desired fertility	(7) In school (6–17)	(8) BMI for age (5–15)	(9) Height for age (5–15)
Panel A: Baseline Controls									
Treatment	0.048***	0.023**	0.028***	−0.055**	−0.061**	−0.37***	0.050***	0.20***	0.058
	(0.009)	(0.011)	(0.008)	(0.024)	(0.026)	(0.076)	(0.015)	(0.070)	(0.064)
log(expend pc)	0.023***	0.018	0.023***	−0.069***	−0.064***	0.083	0.038***	0.11*	0.17***
	(0.007)	(0.019)	(0.008)	(0.019)	(0.021)	(0.062)	(0.015)	(0.058)	(0.057)
Head's educ	−0.003	0.004	0.002*	−0.006	−0.011**	−0.024**	0.004*	0.018	0.008
	(0.002)	(0.004)	(0.001)	(0.004)	(0.005)	(0.012)	(0.002)	(0.012)	(0.010)
Spouse's educ	0.004	0.005	−0.002*	−0.003	−0.004	−0.011	0.006	−0.034*	0.035**
	(0.003)	(0.006)	(0.001)	(0.005)	(0.005)	(0.015)	(0.004)	(0.017)	(0.014)
Family size	0.002	0.004	0.005*	−0.006	−0.007	−0.011	0.002	−0.011	0.025**
	(0.001)	(0.005)	(0.002)	(0.005)	(0.005)	(0.015)	(0.003)	(0.013)	(0.012)
R^2	0.036	0.023	0.031	0.11	0.078	0.029	0.070	0.017	0.06
Observations	1,278	1,278	1,278	1,278	1,278	1,226	2,264	2,031	2,031

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APPENDIX B
(CONTINUED)

	Working age (18–24)						School age (as indicated)		
	(1) Works BPO	(2) Works for pay	(3) Training	(4) Married	(5) Gave birth	(6) Desired fertility	(7) In school (6–17)	(8) BMI for age (5–15)	(9) Height for age (5–15)
Panel B: Changes									
Treatment	0.049***	0.023**	0.025***	−0.052*	−0.047**	−0.35***	0.051***	0.27***	0.051
	(0.009)	(0.011)	(0.008)	(0.029)	(0.020)	(0.041)	(0.21)	(0.051)	(0.051)
R^2	0.024	0.004	0.008	0.003	0.004	0.049	0.010	0.012	0.005
Observations	1,278	1,278	1,278	1,278	1,278	1,201	1,265	1,556	1,556

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village level in parentheses. All regressions are for women only. All dependent variables in Panel A measured in round 2, all independent variables are from round 1. Regressions in Panel A also include a full set of age dummies and indicators for whether log(expenditure per capita), head's education, and spouse's education are missing (all missing values replaced with sample medians). All dependent variables in Panel B are round 2 – round 1 changes. The number ranges in parentheses indicate the age range over which we are estimating the regression. BMI for age and Height for age are *z*-scores. In columns (7)–(9), there are fewer observations when using changes as the dependent variable than when using round 2 levels because children aged 5–7 in round 2 are dropped; these children would not have been measured or asked about school in round 1 because they were too young (2–4). *Significant at 10% level; **significant at 5% level; ***significant at 1% level.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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