ECONOMIC OPPORTUNITIES AND GENDER DIFFERENCES IN HUMAN CAPITAL: EXPERIMENTAL EVIDENCE FOR INDIA[*]

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Abstract: Gender differences in health and education are a concern for a number of developing countries. While standard theory predicts human capital should respond to market returns, social norms (e.g., disapproval of women working outside the home) may weaken or even sever this link for girls. Though many studies have examined the link between women's wages or labor force participation and investment in girls, two important issues are the strong possibility of omitted variables bias and reverse causality, as well as the difficulty in identifying which of several mechanisms (returns, bargaining power, income, etc.) link the two. To overcome these problems, we provided recruiting and career services for the business process outsourcing industry to young women in randomly selected Indian villages over a three year period. Girls in treatment villages were more likely to be in school and had greater measured BMI (but not height). In villages where both boys and girls received recruiting services, girls still gained (and more so than boys). We argue that the design of the experiment (providing opportunities almost exclusively for young, unmarried women rather than current mothers) allows us to rule out that mechanisms other than increases in the returns explain our results.

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I. INTRODUCTION

There has long been considerable concern over the striking gender disparities in human capital outcomes in a number of developing countries, particularly in Asia. For example, in India, the female adult literacy rate is only 48%, compared to 73% for men. And mortality rates in infancy and childhood are 40-50% greater for girls than for boys, primarily due to inadequate provision of nutrition and medical care (Bardhan 1974, Chen, Huq and D'Souza 1981, Sen and Sengupta 1983, Das Gupta 1987 and Behrman 1988; see Strauss and Thomas 1995, Behrman 1997, Haddad, Hoddinott and Alderman 1997, Schultz 2001 and Duflo 2005 for reviews). The problem culminates most visibly in the problem of highly masculine sex ratios (Visaria 1969, Miller 1981) and "missing women" (Sen 1990, 1992), arising from excess mortality of girls and sex selective abortion.

A number of studies have focused on the returns to human capital or the potential economic contributions of women, particularly in agriculture, as an explanation for these gender disparities (Boserup 1970, Bardhan 1974, Miller 1981, Rosenzweig and Schultz 1982, Foster and Rosenzweig 2009). In varying forms, this argument suggests that human capital investments in girls are lower where the returns are lower.[1] However, in many developing countries, the theoretical predictions are more ambiguous than would arise from a simple application of the standard human capital framework, due to cultural practices and norms. For example, Dyson and Moore (1983), Greenhalgh (1985) and Foster and Rosenzweig (2009) note that the practice of patrilocal exogamy (i.e., marriages occurring between individuals from different villages, with girls leaving their birth household to live with their husband's family) means that the returns to investing in girls' human capital do not accrue to parents, leaving them with less incentive to do so.[2] In addition, in many societies, there are strong social prohibitions against women working outside of the home (Boserup 1970, Goldin 1990, 1995, 2006, and Mammen and Paxson 2000;

[1] Lower returns for girls could lead to greater schooling if parents compensate to make sure girls will be sufficiently well-off, or if they care about equality of outcomes among their children (Griliches 1979, Becker and Tomes 1979, Behrman, Pollak and Taubman 1982). However, when few women work, as in India, more education will not help girls earn more (though it may affect marriage market outcomes).

[2] Though they may still invest in girls for other reasons, such as: altruism; the expectation of transfers from daughters even though they leave the household (though typically, under patrilocal exogamy, norms also discourage such transfers, and in practice they are rare); or if the marriage market compensates parents for higher human capital daughters (Behrman et al. 1999, Foster and Rosenzweig 2009).

see Dube and Palriwala 1990 for India).[3] In this setting, the social costs to a household of having a woman work may outweigh the potential income gains.[4] Thus, even if parents invested in girls purely for altruistic reasons (rather than for personal returns in the form of greater support in old age), girls' schooling would still be insensitive to the market returns if women do not work. This is not to say that parents will not invest in their daughters at all;[5] it is simply that if women do not work, the amount of investment in girls (for non-market reasons) should not be affected by changes in the market returns.[6] Overall, in both examples (patrilocal exogamy and prohibitions against women working), cultural norms or practices can weaken and possibly even sever the link between market returns and investments in girls.

Despite the potential theoretical ambiguity, a large literature has documented that greater (relative or absolute) female labor force participation or wages are indeed correlated with improved human capital and survival outcomes for girls (Kishor 1993, 1995, Murthi, Guio and Drèze 1995, Agnihotri, Palmer-Jones and Parikh 2002, Rosenzweig and Shultz 1982 and Qian 2008). However, two important factors make it difficult to conclude that it is the increased future earnings potential of girls that leads parents to invest more in their daughters, which our study hopes to overcome. First, most of these studies are cross-sectional in nature, and concerns about omitted variables bias make it difficult to draw a causal interpretation from the results, as noted by Sen (1990) and Foster and Rosenzweig (2009).[7] Three exceptions are Rosenzweig and Schultz (1982) who use variation in rainfall as a determinant of women's earnings opportunities,

[3] Such attitudes were common until relatively recently in many wealthy countries as well. Erskine (1971) presents several examples for the United States. A 1936 survey found that 48% of men and women agreed that married women should not have full-time jobs outside the home (only 15% disagreed, the rest replied "don't know"). A 1938 Gallup survey found that 78% of people disapproved of a married woman earning money in business or industry if she had a husband capable of supporting her. Data from the General Social Survey show that as recently as the late 1970s, one-third of Americans still disapproved.

[4] The most salient form of this is the practice of *purdah*, followed by some Hindus and Muslims, where women are supposed to avoid contact with men who are not blood relatives.

[5] Human capital investment may be for non-monetary returns; parents may invest in education of their daughters because it will improve their health or that of their children, or for the intrinsic value of learning. Or more educated daughters may attract wealthier or "better" husbands.

[6] Behrman et al. (1999) and Foster and Rosenzweig (20009) argue that even if women don't work, their education can still increase in response to increases in the return to human capital, since there will be a greater demand for more educated wives who are better at producing high human capital sons. However, for the present paper, this issue will not be relevant, since we increase the returns for women only. Thus, the demand for educated wives should not increase, since there is no change in the returns for sons, or for daughters (since they, in turn also will not work).

[7] Foster and Rosenzweig (2009) also point out that under patrilocal exogamy, it is the labor market conditions in the villages girls will marry into, not those they are born into, that should matter.

Qian (2008), who exploits a policy reform in China that differentially affected the value of traditionally male and female crops, and Foster and Rosenzweig (2009) who use land prices and yields as measures of expected future technical change and productivity.

Second, most of these studies cannot specifically identify the mechanism(s) linking current employment rates or wages among adult women to investment in young girls. Beyond expected returns to human capital (i.e., current economic conditions reflect the returns daughters will face in the future), there are at least six other potential channels. First, greater earnings opportunities for adult women may increase women's bargaining power within the household (Thomas 1990, Schultz 1990; see Strauss and Thomas 1995 and Duflo 2005 for surveys). If women prefer to discriminate less against their daughters than their husbands or in-laws do, that greater bargaining power could result in greater investments in girls. Second, increases in women's employment opportunities, all else equal, increase household income. If investment in girls is a normal or luxury good, then increases in income may benefit girls (more than boys), see Schultz (2001).[8] Third, employment of adult women outside the household may increase the value of daughters, as they substitute for the mother in household production activities such as child care, cooking or cleaning. This may increase the incentive for parents to invest in their daughters, both to just ensure they survive and can help in the household (Murthi, Guio and Drèze 1995), and because it may make them more productive at those tasks (ex., higher human capital girls may improve the human capital of the younger siblings they take care of, as is found with adult women (Behrman et al. 1999)).[9] Fourth, if a woman's husband, older children or in-laws take over child care when she enters the labor market, and some of these individuals have lower preferences for discrimination against girls,[10] then women working may lead to gains for girls. Fifth, by raising the opportunity cost of time, women's earnings opportunities may lower fertility, and girls in smaller families may gain more than boys when they compete with fewer siblings for limited family resources.[11] Finally, where women have more employment

[8] Though some studies look at relative male:female earnings or employment, holding income constant.
[9] Of course, the additional time demands of household production could instead lower girls' schooling.
[10] For example, women in India often have weak property rights (even when the law specifies otherwise). A woman may be forced out of her home or lose her land to her husband's family if he dies, unless she has a surviving son (Chen 2000, Agarwal 2001). In this case, a mother may have stronger incentives than others in the household to favor sons and ensure their survival.
[11] Though it is also possible that lower fertility leads to worse outcomes for girls. When the household only wants 2 or 3 children, there is a greater pressure to ensure one or perhaps more are boys, which could lead to greater mortality among girls due to neglect in infancy or childhood, or through infanticide.

opportunities, they in effect have an insurance mechanism to cope with the unemployment, disability or death of their husbands, as well as a source of income to save for old age and possibly even a pension. If one reason sons are favored in the provision of human capital is that they similarly serve a similar insurance role for their parents, greater economic opportunities for women weakens the need to do so.

These mechanisms all yield different implications for understanding both the root causes of gender bias as well as the policy instruments that will most effectively address the problem.[12] Yet most studies cannot identify the underlying mechanism. Qian (2008) provides some arguments that bargaining power of women might explain, at least in part, why increases in the value of women's crops leads to less male-skewed sex ratios in China, but she also notes that it isn't possible to explicitly or directly test this hypothesis against alternatives.

In this paper, we use a randomized intervention in India to ask whether increases in employment opportunities for women lead to greater human capital investments in girls. The intervention provided three years of ongoing career and recruiting services for the Business Process Outsourcing (BPO) industry in India to young women in towns and villages that would not otherwise have received them. The BPO sector in India has grown rapidly over the past decade, creating a significant number of new, high-paying opportunities for women. However, because it was such a new and rapidly growing sector, awareness of these jobs and knowledge of how to access them was fairly limited in general, and extremely limited outside of the urban core or among less educated households. By using experienced BPO recruiters, our intervention was designed to increase awareness of and access to these opportunities.[13]

Our primary focus is to test whether the increased employment opportunities for women lead to human capital investments for girls. The study offers two key advantages in this respect. First, the intervention provided random assignment of potential economic opportunities (or,

[12] For example, if bargaining power rather than the returns explains the correlation between women's employment and investment in girls, then policies aimed at strengthening women's bargaining position, such as reform or enforcement of divorce laws or strengthening women's rights to property, might be favored or prioritized over efforts to promote women's employment. Similarly, if it is an income effect, then the best policies are those that promote growth, rather than specifically stimulating women's employment opportunities. Similarly, efforts to directly address fertility, or access to old age, unemployment or disability insurance, would be favored if those mechanisms are the relevant ones.

[13] In this respect, the paper is similar to Jensen (2010), who finds that (male) students in the Dominican Republic underestimate the returns to schooling, and that providing information on the (higher) measured market returns increases schooling. A key difference is that in the present case, the goal was to increase expected returns by actually increasing access to employment opportunities.

awareness of and access to these opportunities). This allows us to overcome potentially serious problems of omitted variables bias or reverse causality, and results in a weaker identifying assumption than what is required in the cross-sectional and natural experiment studies mentioned above.

Second, the intervention allows us to isolate returns as the mechanism linking employment opportunities and human capital investments in our case, eliminating the alternative mechanisms identified above. Most of the non-returns mechanisms listed above in effect focus on the impact of employment or earnings of current mothers on investments in their daughters, such as a mother having more say over household decisions when she earns a greater share of household income, or changes in the value of the daughter to the household when her mother works. However, our intervention created opportunities primarily for younger, unmarried women, not just because the sessions targeted this group and because employers favor this group, but because most jobs in the BPO sector require familiarity with computers, English language skills and/or a secondary school degree, all of which are uncommon among older women. Further, in our project area very few married women, especially those with young children, work outside the household (about 6 percent), so these opportunities would not apply to them, but instead apply in the future to their daughters. Thus, our experiment enhanced the future returns for currently young children, leaving the employment opportunities for older women (current mothers) unchanged. This design in effect shuts off all links driven by changes in the employment of current mothers.

Using panel data spanning a three year period, we find that the BPO recruiting and placement services increased employment among young women, with no effect for older women or men of any age (as per the design of the experiment). Girls aged 5-15 in villages that received the recruiting services were 3 to 5 percentage points more likely to be in school and experienced an increase in Body Mass Index, reflecting greater nutrition and/or medical care. However, there was no net gain in height. For boys, there was no change for any of these measures. In a second treatment group where the training and recruiting services were available to both young men and women, there were human capital improvements for both girls and boys, with the effects larger for girls than for boys (though we cannot reject equality). Thus, a "gender neutral" increase in economic opportunities did not lead to girls losing out in favor of their brothers.

Three other studies have documented changes similar in nature to those found here. Munshi and Rosenzweig (2006) find that enrollment in English language schools for girls, but not boys, increased in response to increases in the returns to English language education in Bombay driven principally to the expansion of the financial sector and other white collar industries. More closely related, Oster and Millet (2010) use school-level panel data to show that towns in southern India that saw the introduction of a call center experienced increases in school enrolment for both boys and girls. Finally, Shastry (2010) finds that the information technology sector grew more rapidly in areas where English is more widely spoken and that in turn those areas experienced increased school enrolment (particularly English language enrolment). The results we find are broadly consistent with these other studies. As above, they two key advantages of the present study are the weaker identifying assumption due to randomization and an intervention that allows us to isolate returns as the mechanism linking the new opportunities for women to investments in girls.

The results have important policy implications. Many approaches to gender bias have emphasized information and awareness campaigns, and media strategies to promote the status of girls, i.e., efforts to act on any social or cultural component of bias (Croll 2000). Our results suggest that efforts to expand economic opportunities for women can have significant impacts.

The remainder of this paper proceeds as follows. Section II discusses the experimental design and section III discusses the data and empirical strategy. Section IV shows the results of the intervention and Section V concludes.

II. EXPERIMENTAL DESIGN

II. A. The BPO Sector in India

The Business Process Outsourcing (BPO) industry is a broad umbrella capturing a range of activities and "back office" services. The most highly publicized of these jobs are call centers (e.g., customer service, taking reservations), but the industry is much larger, and 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. While the industry has been 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.[14] This in turn lead to dramatic growth in the BPO export sector in many low-wage countries, particularly those like India where English is spoken. In India, the BPO industry experienced 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 Holtgrewe 2007).[15] 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 employers preferred women, including a more pleasant voice and demeanor when interacting with customers, and the belief that women were more trustworthy than men.

Thus, technological change in the BPO sector created a sharp and fairly sudden increase in the demand for female workers, particularly those with computer skills, English language skills and/or secondary school degrees or higher. In order to meet this demand, there was a surge in recruiting efforts, including through specialized private contractors and subcontractors who would seek out and screen potential employees.[16] And because the BPO sector is strongly geographically concentrated in India, with 95% of employment focused around seven major cities, recruiting was fairly geographically concentrated as well, leading to large, localized increases in economic opportunities for women.

In general, BPO jobs are fairly well-paid in relative terms. Starting salaries with no experience often ranged from 5,000−10,000 Rupees (Rs.) per month in 2003 (about $U.S. 110−220, 1 Rs≈0.022 $U.S.), which was about 2-10 times the average starting pay for women outside of the BPO sector. Salaries also often increase rapidly with experience, whereas many other jobs have relatively flat compensation profiles.

II. B. The Intervention

[14] Much of the BPO sector is export-driven. For example, in India, almost three-quarters of call centers service international markets (Holman, Batt and Holtgrewe 2007).
[15] Though women still tend to be underrepresented in the management and senior positions at these firms.
[16] In some instances this recruiting was done directly under contract for a specific employer looking for workers, and in other cases recruiting firms would collect and compile workers' resumes and contact information and supply them to employers upon request for a fee.

Though 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 is likely to be even more pronounced outside the urban centers where these jobs are located. Our experiment was designed to both increase awareness of these opportunities and to make it easier for qualified women to get these jobs.

We hired eight BPO recruiting subcontractors (5 women and 3 men), all with at least two years experience overall, and with at least 6 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 solely to their relative distance from the city and/or their population size. This allowed us to establish a list of where awareness of and access to BPO employment opportunities was likely to be lower, not because there were no qualified women, but simply because the cost per potential recruit was high enough that recruiters chose to visit other areas instead. From this list we chose districts at random (most located approximately 50-150km from Delhi) from the states of Haryana, Punjab, Rajasthan and Uttar Pradesh. This study region includes much of the area where gender bias is concentrated in India today. Haryana and Punjab in particular have the most masculine sex ratios in India, and appear to be the only two states where sex ratios continue to worsen rather than improve (Dasgupta, Chung and Shuzhou 2009). And our sample includes some of the districts with the most extreme incidence of gender bias. For example, if we rank the 593 districts by the sex ratios of children aged 0-6 in the 2001 census, our sample includes the 3rd (Kurukshetra, with a ratio of 770 girls per 100 boys), 10th (Srangur, 784) and 22nd (Karnal, 808) worst districts in all of India.

From these districts, we drew 160 treatment and 80 control villages at random. For the treatment villages, between December 2003 and February 2004, one of the recruiters would first visit local schools and make a small introduction announcing that they would be visiting the village or town at a designated date 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 NGOs to advertise the sessions.

Within a few weeks, the recruiters would visit the village and set up an information and recruiting session. For half the treatment villages, the sessions were open to women only ("women-only" treatment). All women could attend, but it was made clear that the job opportunities were primarily for women with some English language skills and experience with computers, and either a secondary school degree or currently enrolled in school. This in effect ruled out a majority of women over the age of 25; for example, in our data only 5 percent of women aged 26-40 have completed secondary school. In the second set treatment villages, we provided one recruiting session open exclusively to women, and one open exclusively to men ("men+women" treatment).[17] The sessions and the follow-up services offered were identical in every other way in the two treatments.

The sessions were held in a range of facilities including schools and NGO or government offices, and typically lasted from 4 to 6 hours (depending on how many people attended). The sessions drew a great deal of local interest and attendance was generally high. The recruiters did not have a fixed script, but were required to follow a specific organization. In particular, after introducing themselves, the recruiters: provided an overview of the BPO sector and the specific types of jobs available, as well as compensation; information on the names of specific firms currently or frequently looking for workers; how to apply for jobs (how to create and submit resumes, plus lists of websites and phone numbers); interview skills lessons and tips; mock interviews; assessment of English language skills; and an open ended question and answer session. The recruiters were required to emphasize that the jobs were competitive, so they were not in any way guaranteeing employment.

One and two years after the initial experiment (i.e., December 2004 to February 2005 and December 2005 to January 2006), we provided a "booster shot," with the recruiters again visiting the same treatment villages and providing the same session. After these sessions, the recruiters left their personal contact information so that anyone could follow up for additional information or assistance (at no cost). The recruiters were contracted to provide ongoing support for anyone from the designated villages. Thus, the intervention consisted (exclusively) of three in-depth sessions and continuous placement support.

[17] We did not provide any mixed-sex sessions, since for example most schools are sex segregated as well. However, we do not feel this impacts the interpretation of our results.

We wish to note that our goal is not to test whether recruiting services as a policy instrument can help address gender disparities in human capital. While it is certainly worthwhile to make sure that information on economic opportunities is widely available, our intervention does not actually create any new jobs for women. The women in our sample may simply get jobs at the expense of other women, with no net effect on women's employment overall.[18] The goal of the experiment is to supply exogenous variation in employment opportunities to test whether the returns influence household investment in girls.

One important aspect of the intervention worth highlighting is that the employment opportunities were white-collar. While the distinction is commonly overlooked when discussing employment opportunities or the returns to schooling, for women the 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 blue collar jobs in manufacturing, like working in factories, or for example domestic work.[19] This may have particular relevance for India, since an important aspect of the caste system is the designation that certain jobs, which are supposed to be set aside for "untouchables" outside the caste system, are considered "impure" or "polluting." Another relevant distinction is that women would appear to have a comparative advantage in this type of employment, since it does not require physical strength. We would therefore not necessarily generalize our findings to increases in the returns in agriculture (as in Foster and Rosenzweig 2009) or in the blue-collar sector. 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 towards the service sector, where white collar employment

[18] Though if growth of the sector and competition with other BPO 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 the worker-job match or increases overall productivity in the sector, net employment could potentially increase.

[19] Though there are some respects in which BPO jobs are considered less appropriate for women, particularly in rural areas. For example, women may have to commute for these jobs, and parents or husbands might perceive risks in women traveling alone. Alternatively, for more distant areas, taking one of these jobs might require migrating to a city and possibly living on their own, which parents may not want. Finally, some BPO jobs, particularly those in call centers servicing daytime hours in the United States, require working and commuting and night, which might be perceived as even less safe for women. Thus, some households would not want any female members to hold one of these jobs, and we expect they would therefore not be affected by the treatment (either in terms of employment or human capital).

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 IT sector alone comprising 8% of GDP (up from 1% just a decade ago). Second, throughout the world, much of the history of women's increasing paid labor force participation (particularly for married women) is the history of the development of white collar, service or clerical sector (Boserup 1970, Goldin 1990, 1995, 2006, Costa 2000, Mammen and Paxson 2000). It was only in these sectors that jobs were considered to be acceptable or respectable for women. In fact, our experiment relating to information technology may be particularly apt in light of the historical evidence; for example, Goldin (2006) notes that the rise in female labor force participation in the United States in the early 20th century was due in part to growth in office and clerical jobs arising in particular from innovations in information technology.

III. DATA AND EMPIRICAL STRATEGY

A. Survey Information

We conducted a baseline household survey during September and October of 2003 for each of the 240 treatment and control villages. The survey was conducted by students at the Management Development Institute, a business school based just outside of Delhi. In each of the villages, we worked with a local official to draw up a list of households, and randomly selected 20 households per village.

The survey consisted of both a household questionnaire and an adult questionnaire. The household questionnaire was to be answered by the person with most knowledge of the household, and included basic questions on household demographic and socioeconomic characteristics (age, sex, and education of all members, as well as expenditure). We also asked for demographic and socioeconomic characteristics of all children of household members who have either temporarily or permanently left the household. The adult questionnaire was asked of all individuals aged 18 and older. This brief questionnaire focused on questions related to norms and expectations, described in more detail below.

In addition, enumerators weighed and measured the height of all household members aged 5 and older.[20] If the household survey was complete but someone was not home to be

[20] Enumerators used consumer-grade bathroom scales and marked board or measuring tape, and were trained to approximate as closely as possible the protocols in the 1988 National Health and Nutrition

weighed or measured, enumerators were to schedule up to three return visits to take these measurements. As a result, we were able to get weight and height measurements for 98% of youths aged 5-15 (who will be the focus of our analysis) in round 1 and 99% in round 2. These data are particularly valuable because they are objective and thus more likely to accurately reflect changes in nutrition and health care provided to children.

A second round, follow-up survey with the same households was conducted in September and October of 2006. With a few exceptions noted below, the survey instrument for the second round was the same as the first round, and enumerators again weighed and measured all household members aged 5 and older. We also tracked, and where possible measured, individuals who left home between the rounds (such as for school, work or marriage). Finally, on the same days at the round 2 main survey, we also conducted a supplemental survey measuring employment and expectations in more detail. This survey was much shorter than the main survey, so that it could be asked of the 5 households closest to the sample household.

Thus, overall, the project time line was as follows. The baseline survey was conducted in August and September of 2003. The first recruiting sessions were carried out from December 2003 to February 2004. The follow-up, booster-shot recruiting sessions were carried out from December 2004 to February 2005 and December 2005 to February 2006. The second round survey was conducted in September and October of 2006. Recruiting assistance was available continuously from the time of the first session through the second round survey (and one year beyond that as well), spanning nearly 3 years.

Before turning to the data, we discuss attrition. Our analysis will focus on children aged 5-15 (in round 2), since most of these children will still live at home and therefore be measured. It is therefore important to examine attrition (or other reasons for non-measurement) for this group, and whether it is correlated with the treatment. One specific concern is the possibility that the intervention affected whether children left home due to work, school, marriage or death (all of which we gathered data on). The expected bias is difficult to predict. For example, if the intervention lead to delayed marriage for girls with the greatest pre-treatment human capital (since they would benefit the most from staying in school longer to get a BPO job), the sample of non-attriting treatment girls will have greater human capital on average than the non-attriting

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Examination Survey (NHANES) III manual. While the absence of a fixed setting and trained professionals likely introduced imprecision, the errors should be uncorrelated with the treatment and therefore should not bias our regression results.

control girls even if the treatment had no effect (though marriage is very uncommon for girls under age 15 (thus age 12 at round 1)). Alternatively, the higher human capital children might be sent away to better schools because of the new economic opportunities, leaving the non-attriting treatment children with lower human capital on average than the non-attriting control children, again just due to selection. As noted above, we did ask information about children of household members who had left home, and where possible, we tracked and weighed and measured them.

In our sample, the overall fraction of 5-15 year olds living away from home at baseline is less than 1 percent. Additionally, slightly less than 1 percent of children living at home could not be measured (for various reasons, including infirmity or temporary absence from the home). Total attrition for children aged 5-12 at baseline (and thus 8-15 at round 2) is just under 5 percent. About 60 percent reflects attrition of entire households (i.e., no one from the round 1 household was present at the same residence in round 2).[21] The remainder, approximately 2 percent drop out of the sample between rounds 1 and 2. For non-death causes of attrition, we were able to locate and weigh and measure 62% of children. Total attrition among girls 5-12 in the women-only treatment villages was 4.7 percent, compared to 4.5 percent for the control and men+women treatment villages. Though the difference in attrition for the treatment and control groups is small, it is certainly possible that there were large but offsetting effects of the treatment on attrition, and it is difficult to determine the possible bias this may introduce. Below, we will assess the sensitivity of our results to assumptions on the outcomes of attriters.

B. Descriptive Statistics

Table I reports summary statistics for the full sample and separately by treatment status, as well as tests of treatment-control balance in baseline characteristics. The variables overall appear balanced between the controls and the two treatment groups. Formal tests suggest that randomization was successful: the *p*-value for the *F*–test that baseline characteristics jointly predict treatment is 0.71 and variable-by-variable individual tests cannot reject that the means are the same for treatment and control groups.

The table provides evidence of gender differences in education. The average adult (18 and older) woman's years of schooling is 2.0, compared to 3.1 for men. Similarly, only 76% of

[21] For attriting households, we chose replacement households at random from the original population lists. Below we will use only the data from the panel, but the results are robust to including the replacements.

girls aged 7-15 are currently enrolled in school, compared to 86% of boys (enrollment data were verified by contacting schools students said they attended).

Since weight and height vary considerably over the 5-15 age range, we computed z-scores for height-for-age (HFA) and Body Mass Index(BMI)-for-age (BFA) using the age- and sex-specific standards for school-aged children and adolescents recently developed by the World Health Organization (de Onis et al. 2007).[22] The table shows that children in the sample are undernourished, with very low weight and height relative to international norms.[23] On average, children aged 5-15 are 1.2-1.3 standard deviations below their age- and sex-specific reference median BMI, and 2 standard deviations below the reference median height. The BMI and height data taken on their own do not indicate any differential between boys and girls. This somewhat surprising result is also found in other samples and for other age groups. For example, in the 3 rounds of the National Family Health Survey, there is almost no difference in anthropometric measures for boys and girls under the age of 5 (IIPS 1995, IIPS and ORC Macro 2000, IIPS and Macro International 2007), despite the fact that mortality rates for girls are significantly higher in this age range and a large literature documents differential treatment of boys and girls.[24] There are several reasons why anthropometric data compared to international standards may not detect gender differences, and it is beyond the scope of the present paper to resolve this issue.[25] However, for our analysis, we will examine changes in BFA and HFA, which should reflect changes in the provision of nutrition and/or medical care (the latter because some diseases, if left untreated, may harm nutrition or stunt growth).

The table also provides evidence about baseline norms regarding expectations of sons and daughters. In particular, all adults aged 18 and older were asked whether they thought it was acceptable for women to work for pay away from home at various stages in their lives: before

[22] While anthropometric measures such as weight-for-height or height-for-age were previously only used for children under 5 and the Body Mass Index (BMI) was considered appropriate for adults only, these new standards are considered appropriate indicators of growth and development for children aged 5-19. Note also, while the WHO also computes weight-for-age standards, these are only computed up to age 10 because it is not considered an informative measure of growth for older children (de Onis et al. 2007).

[23] India has among the highest rates of child malnutrition in the world (Gragnolati et al. 2005).

[24] For example, in the 2005/6 survey measuring children 5 and under, the mean height-for-age, weight-for-height and weight-for-age z-scores are identical for boys and girls (-1.9, -1.0 and -1.8, respectively; IIPS and Macro International 2007).

[25] For example, while lower nutrition should lead to girls being lighter and shorter for a given age, this may be offset by lower nutrition also causing death for less healthy children. Deaton (2007) discusses the potential offsetting impacts of these "scarring" vs. "selection" effects.

they are married; after they are married but before they have children; after they have children, but before those children have gone to school; after all their children are in school; and after their children have left the household. Overall, most people do not support women working for pay away from home. Only about 17 percent of adults believe it is appropriate for a woman to work for pay before marriage. This declines to 7 percent after she is married but before she has children, 4 percent when her children are young and not yet in school, then increases just slightly to 5 percent after all children are in school and 12 percent after they have left the household.

Adults were also asked whether they think it is appropriate for adults to accept financial support from their sons and daughters in old age and whether they expect to live in the same dwelling as their son/daughter when they are old.[26] The differences are striking. While almost all adults (97 percent) say it is appropriate to accept support from sons, only 4 percent say it is appropriate to accept support from daughters.

Thus, under prevailing expectations or norms, the incentive to educate girls may be quite low, especially compared to boys. While parents may still invest in their daughters for non-financial reasons, these investments may not respond to increases in market returns. Of course, these are just subjective, self-reports, and responses may deviate from true beliefs in either direction. We view these data as simply suggestive, and consistent with the large anthropological and ethnographic literature.

C. Empirical Strategy

For each human capital measure, we present three specifications. First, we regress round 2 outcomes for children (aged 5-15 for weight-for-age and BMI-for-age, and 7-15 for schooling, since most children do not enroll until age 6 or 7) on an indicator for residing in a treatment village, separately for boys and girls: $Y_i = \beta_0 + \beta_1 Treatment_i + \varepsilon_i$, where *Treatment* is a dummy equal to one if the individual, *i*, lives in a village that was exposed to the recruiting intervention (for ease of presentation, we estimate the effect of the two treatments separately, rather than in one pooled regression). We limit the sample to children less than 15 to minimize selective attrition, given the increasing likelihood of leaving the household for work, school or marriage after this age. The second specification adds controls that are baseline predictors of human capital, $Y_i = \beta_0 + \beta_1 Treatment_i + \sum \gamma_i Z_i + \varepsilon_i$ including parent's education and log of family

[26] Parents may also expect non-financial support from children, such as care in old age.

expenditure per capita. Our third specification uses the changes in outcomes between rounds rather than round 2 levels, $\Delta Y_i = \beta_0 + \beta_1 Treatment_i + \varepsilon_i$, for children aged 5-12 in round 1 (and thus 8-15 in round 2). While the three specifications should yield similar estimates of the effect of the treatment, they will in general not be identical. First, the samples they cover will be slightly different, with the changes specification only including children aged 8-15 in round 2 (since children under 8 in round 2 will have been under 5 in round 1, and thus will not have been weighed or measured) and excluding children of this age who could not be measured in round 1 for some other reason like absence from the household. Further, although randomization should result in treatment and control groups being similar across all variables, in any particular sample there can be small baseline differences, even if they are not statistically significant. So, for example, the changes specification will capture any pre-existing differences across the groups. Finally, the changes specification will feature more measurement error in the dependent variable, leading to less precise estimates. For all specifications, we estimate linear regressions regardless of whether the outcomes are continuous or discrete, but limited dependent variable models yield nearly identical conclusions in terms of magnitudes and significance. Standard errors are adjusted for clustering at the village level.

Since we are considering the impact of the treatment on a number of outcomes, in addition to presenting the results for each individual outcome, we present two other statistics. The first is the mean effect of the treatment across outcomes, computed using the methodology described in Kling, Liebman, and Katz (2007). This approach standardizes all variables to mean zero and unit standard deviation and redefines them where necessary so that a higher value constitutes an improvement. The average effect is computed as the unweighted average of the coefficient on the treatment variable for each of the standardized outcomes. We also provide *F*-tests of the null hypothesis that the effect of the treatment is jointly zero for all outcomes.

IV. RESULTS

A. Employment

The recruiters reported placing a significant number of women in jobs in the treatment villages (about 900 women over the three year period). Of course, some of these women might have gotten jobs in this sector or other sectors even without the recruiters, which we can net out through comparison to the control group. Table II shows the impact of the women-only treatment

on paid employment outside the home; as mentioned above, these data were asked of all children of the household, including those not living at home (either temporarily, such as due to work or school, or permanently, such as due to marriage). We regress the employment indicator on an indicator for being in the treatment group; since the treatment was focused only on younger women, we split the sample into age and sex categories to examine the employment effects. In villages that received the recruiting treatment, paid employment outside the home in 2006 was 2.4 percentage points higher for women aged 18−24. The coefficient is statistically significant at the 5 percent level.[27]

As expected given the experimental design, there was no change in paid employment for women aged 25−45 or 45 and older. The coefficients for these two groups are both extremely small and not statistically significant. Similarly, there is no impact on employment for men of any age, again consistent with the experimental design. Thus overall, the recruiting intervention increased employment specifically for the set of younger women it was targeted towards, and only those women.[28] It is these gains that we predict could have effects on investments in girls.

B. Effect on Human Capital

Table III shows the effects of the treatment on education and anthropometric measures. We focus first in this section on the women-only treatment. The first set of columns shows the specification where the dependent variable is Round 2 outcomes with no additional controls, the middle columns adds baseline controls, and the last columns examines changes in outcomes between the two rounds.[29] For girls (top panel), the treatment had a positive effect on the human capital measures. Girls were about 4-5 percentage points more likely to be enrolled in school.

[27] We believe it is unlikely that the jobs gained in treatment villages came at the expense of employment for women in the control villages. The pool of women competing for these jobs is large, so the loss of jobs by women within our set of control villages is likely to be very small; and since few women in rural areas get these jobs, any declines in women's employment is likely to be focused in urban areas, which are outside our sample. Though the interpretation would still largely be the same in the face of such effects. Observing losses for women in areas where job opportunities declined would still support the theory that returns affect investments. However, it is possible there could be asymmetric effects, where women are harmed when opportunities decline but do not gain when they increase.

[28] The intervention could have lead to employment gains for men or older women if young women who attended the sessions shared the information or helped recruit others once they got a job. However, most older women are married and do not work, have too little education or do not speak English. For men, the lack of gains may be due to the effect of networks (Munshi and Rosenzweig 2006), discussed below.

[29] As noted above, the number of observations using the changes specification differs from the other two because the changes specification excludes children measured in round 2 but not round 1.

The results are robust across the two specifications, and statistically significant at the 1 percent level in both cases. The effects are large, similar in magnitude to the effects of call centers found by Oster and Millett (2010). They represent about 5-6 percent gains for the treatment group relative to the control group (control group means for all dependent variables are presented in the bottom row), and close about half the boy-girl gender gap in enrollments.

Columns 2, 5 and 8 show that the treatment also resulted in an average increase in BFA z-score of 0.15-0.20, again significant at the 1 percent level in all specifications. The effect is fairly large, particularly relative to the control group mean deficit of 1.3. So the treatment closed a moderate fraction of the BMI gap between our sample and the well-nourished population.

Finally, columns 3, 6 and 8 show that point estimates of the effect of the treatment on HFA are positive, but extremely small and not statistically significant in any of the specifications. The point estimates also vary a bit more across the three specifications, though we would again not reject equality.

Overall, we find positive point effects of the treatment on human capital outcomes for all three measures, but only two of the three are statistically significant. The bottom rows of the table provide a summary assessment of the effects. With both the Kling-Liebman-Katz average effect and the *F*-test, we can reject the hypothesis that the treatment had no effect across the set of human capital measures for girls at the one percent level or better.

The bottom panel of the table shows the effect of the women-only treatment for boys. The coefficients are small across all three human capital measures in all specifications, and none are statistically significant. The absence of an effect for boys is consistent with the women-only intervention having increased the expected returns for girls only. However, it is perhaps surprising that parents were able to increase investments in girls without decreasing investment in boys. This suggests that poverty and credit constraints may not be as important as "demand constraints" in limiting investment in girls, i.e., parents may be providing little to girls not because they can't afford to, but because they don't find it optimal to, due to low expected returns.

Of course, we are only able to measure short-run effects, and the longer run effects may be greater or smaller. For example, if a girl is provided better nutrition or health care for more than three years, there may be a greater cumulative effect on weight or height. Also, our results based on round 2 outcomes were for children aged 5 to 15, and the effect of nutrition on height

might be greater in infancy or the later stages of puberty (the latter of which occurs at later ages on average in India than in wealthier countries due to poor nutrition). Alternatively, it is possible we overstate the long-run effects; improved nutrition for younger girls may simply lead to an earlier onset of puberty and the growth spurt, rather than long-term gains in weight or height. The effect of nutrition and medical care on the onset of puberty and long-term outcomes is complex, and beyond the scope of the present paper to determine. However, for our conclusions, even if the intervention only lead to an earlier onset of puberty with no effect on final adult weight or height, this would still serve as an indicator that there had been greater nutrition and/or health care investments in girls. Our interest in the anthropometrics measures is as an indicator of those unobserved investments; it seems very unlikely there is any other mechanism through which the experiment might have influenced the onset of puberty, so we would still conclude that nutrition and/or health care for girls had improved in response to the increased potential returns.

C. Attrition

As noted, because we are only able to weigh and measure children living at home, we may be selectively missing some girls who differ in human capital from those who still live at home. Though at baseline there should be no difference between treatment and control villages along this dimension, if our intervention kept girls at home longer, such as by delaying marriage for work or more schooling, we may systematically skew the treatment or control samples and conclude there is an effect of the treatment even if there is none (though we noted above there is little differential attrition for the treatment and control groups). We should note however, that attrition affects the weight and height variables more than schooling, since when attrition is only for the child and not the entire households, parents are asked to provide information on schooling for children not at home (which we verify by contacting schools).

Table IV shows that our results continue to hold even under an extreme assumption about the outcomes of children not living at home in round 2 (this includes both attrition or leaving the home permanently as well children temporarily away from home that we were just unable to measure). In particular, in this table we assign all non-measured treatment children as not being in school a weight and height (or changes in weight and height) equal to the 25th percentile of the corresponding distribution for other children of their age and sex, and all non-measured control children as being in school and the 75th percentile of their age*sex weight and height

distributions. Though we have no reason to believe children from the treatment group that are not measured are any worse off than children from the control group that are not measured, even under this assumption that works against our finding an effect, the effect of the treatment, though smaller in magnitude, is still statistically significant for girls at the 5 percent level or better for schooling and BFA. Using the Kling- Liebman-Katz and *F*-tests we still reject at the 5 percent level or better that the treatment had no effect on human capital outcomes.

For boys, the negative effects of the treatment under these assumptions is now statistically significant for schooling and BFA in the changes specification. And we can reject at the 10 percent level or better that the treatment had no effect on boys' human capital outcomes.

D. Distinguishing Returns From Other Mechanisms

While the increase in human capital investment in girls is consistent with parents responding to the increased awareness of the returns to human capital for women, as noted above, there are many alternative reasons why economic opportunities for women might lead to greater human capital investments in girls. Of course, even in these cases, the intervention would still show that increases in women's employment opportunities leads to increases in human capital investment in girls, which is a valuable result in itself. However, as noted, it is important to understand the mechanism through which this relationship occurs, which most studies have not been able to identify. Our goal is not to reject that these other mechanisms are ever relevant, which they likely are, but simply to demonstrate that in our case, they are not operative and thus that changes in returns can have an effect on the human capital of girls. As noted, the key factor is that most of the non-returns mechanisms (bargaining power, change in the household allocation of time, etc.) result from changes in the mother's employment, whereas the returns argument is based solely on the potential future economic opportunities of daughters (and thus would take place even with no change in mother's employment).

First, the results are unlikely to be explained by the fact that greater economic opportunities increase women's bargaining power (which allows them to have their (assumed) preference for less bias against girls to be reflected in the allocation of household resources). The intervention was designed to specifically target the employment opportunities of younger women, in that older women are less likely to have finished high school, to speak English or to have experience with computers. In addition, very few married women with young children work

away from home. In our sample, of the women we can identify that got jobs through our recruiters, none had children. And Table II shows there was no increase in employment for women over 25 (this includes women not living at home), and few women 24 or younger are likely to have children at least 5 years old. In addition, if we simply regress whether there was any increase in employment for women with children on an indicator for treatment, we get a coefficient of 0.0024 with a standard error of 0.023. Thus, there were no gains for current mothers of young children. Finally, we can also explore this hypothesis more directly by considering households that do not have any women (either living at home or away from home) who work for pay in round 2. The top panel of Table V shows that the effects continue to hold for this sample; this is perhaps not surprising, since few women work for pay, so this restriction does not change the sample very much.

It is possible that women's bargaining power may be increased even if they do not work simply by the possibility or threat that they could work. However, this seems less likely. The recruiters told us that most of the firms in their industry prefer to hire young, unmarried women, which is confirmed by Oster and Millet (2010) and Ng and Mitter (2005). In addition, most of the jobs that we increased access to require a secondary school degree, which few older women have. While this hypothesis can't be fully tested, we can examine whether the treatment had an effect for households with women for which this threat was much lower, i.e., the woman does not have a high school degree, does not speak English and has never used a computer, and thus have little chance of getting one of the BPO jobs that our recruiters provided information and services for.[30] While this is a select sample (though it still an overwhelming majority of women), we would probably otherwise expect the impact of the treatment to be smaller for this group, since their lower education might signal a household with lower preferences for education, perhaps even specifically with regards to girls. In the bottom panel of Table V, we see that the effects are very similar in magnitude to the results for the full sample of women in Table III, and remain statistically significant at the 1 percent level.[31] Thus, bargaining power is unlikely to explain our effects.[32]

[30] Of course, there are employment opportunities for women without a high school degree, English language skills or experience with computers. However, our treatment is unlikely to have caused a change in access to those opportunities.

[31] Finally, we can also look for direct (though self-reported) evidence of changes in women's bargaining power. In particular, the survey asked women whether they participated in decision-making in the

Similar arguments to those above (that the BPO opportunities are current or future opportunities for daughters, not women old enough to already be mothers themselves, and that the effects hold even in households where no women work), can rule out several other mechanisms: that parents choose to invest more in girls because the daughter becomes more valuable to the household when the mother works; that a working mother means others taking care of her children provide more equal treatment; that couples no longer need sons to provide unemployment, disability, or old age insurance; and changes in the couple's intended fertility (via a change in the value of the mother's time).[33] The fact that we still find an effect of the treatment even in households where no women work also rules out that the effect is driven by young mothers having gotten one of the jobs (a woman who is, say, 24 and finished high school at 18 could in principle have children old enough to be in our sample), or that a woman other than the mother got one of the jobs (eg., if an older daughter got a job, it might change the value of her younger sister to the household, or she might send money to her younger sisters to buy food or health care to compensate for the poor treatment by their parents[34]).[35]

Finally, we can explore whether our experiment affected human capital investments in girls via changes in income; either current income, via remittances sent home by women who got

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household for a range of activities (obtaining health care, purchasing major household items, and visiting friends or family) as well as their "autonomy" (whether they are able to visit the market, or visit family/friends (separate questions) without permission, and whether they were permitted to keep money set aside to spend as they wish)). We create an index for both female autonomy and decision-making as the sum of these responses, as in Jensen and Oster (2010). We find that the treatment does not have a statistically significant impact on either measure (results available upon request). While these are only self-reports and thus may have reporting errors, provided those errors are uncorrelated with the treatment, the results are at least consistent with the conclusion that women's bargaining power did not change.

[32] Though we can't rule out an alternate form of bargaining power, namely that adults believe daughters would prefer to support their parents in old age, but typically cannot because they have little power in their marital household. Increased investment in girls may reflect parental beliefs that higher earning daughters will have more control over their personal or joint marital resources, and provide support to their parents. Though we note that the norm about support from girls in old age is a social stigma against accepting money from a daughter, not whether she can send it.

[33] Though we can't rule out an alternate fertility story, namely that parents believe greater education will lead their daughters to have fewer children, leaving more resources to send to their parents in old age. Though again, if the norm is that parents don't expect support from their daughters, this seems unlikely.

[34] Again, we can rule out that they gave more to their brothers and sisters, or the household in general, since we would then expect to see gains among boys as well.

[35] Similar conclusions are arrived at if we focus on younger couples who could not yet have children old enough to get one of the jobs (say, 35 and under), or directly on households where no child is older than 18. Stratifying in this way is less likely to be endogenous with respect to girl's outcomes than whether a woman works. Regressions using this subsample (available upon request) reveal human capital gains for girls as large as in the main sample.

a job, or lifetime income (if parents expect changes in whether daughters support them in old age). Changes in current income can be ruled out as in Table V, where there were gains for girls even in households where no woman works for pay (which would include not just mothers, but older sisters sending back money for their younger sisters).[36] For permanent or lifetime income, Table III showed that the intervention had no effect on education, weight or height for boys. If lifetime wealth of parents had increased, we would expect at least some increase in schooling for boys as well. While it certainly may be the case that girls' education is considered a luxury by parents, so we expect it to respond more to wealth once the household is beyond a certain threshold wealth level, given the diversity in incomes in the sample, it seems highly unlikely that the sample households all just happened to be at the exact wealth level where boys were receiving what the household considered to be optimal investment in schooling, so that all marginal increases in wealth go only to girls' schooling, with no effect at all for boys.

Thus, overall, while we cannot rule out every alternative, most of the primary explanations put forward (changes in income, fertility, household time allocation or women's bargaining power) are not applicable in the present case because the opportunities were not for current mothers. It therefore seems likely that it was the increase in returns that lead to increased investment in girls.

E. The Men+Women Intervention

The women-only recruiting intervention allows us to answer our primary theoretical question of interest, namely whether increasing the returns and employment opportunities for women leads to investment in girls' human capital, despite the pervasive social norms that would seem to weaken this link. However, a separate question is whether an expansion of economic opportunities or the returns to human capital will promote investments in girls if those opportunities are available to both men and women, or whether parents will instead invest more heavily or exclusively in boys (i.e., if the returns are equal for men and women, and the parents

[36] We also note that this hypothesis could not explain all of the observed human capital increases unless we assume the effects of the income are very large, since the 4-5 percentage point increase in schooling enrollment was greater than the 2-3 percentage point increase in employment. It would then have to be that every woman who got a job had 1.6 sisters aged 5-11 who were not in school before, but now all of whom were in school, which seems very unlikely.

are resource constrained, they may choose to educate their son instead of their daughter for reasons other than the returns).

Table VI presents the results for human capital investments using the set of villages where the treatment was made separately available to men and women (and excluding villages with the women-only treatment). Overall, we continue to find a statistically significant effect for girls. Girls' enrolments again increase by about 4-5 percentage points. The effects on BFA are smaller, but still statistically significant (and we could not reject they are equal in the two experiments). And as above, there is no effect on height. And again, with both the Kling-Liebman-Katz and *F*-tests, we would not reject an improvement in girls' human capital at the 5 percent level or better.

By contrast, the effects for boys, while positive, are smaller and not statistically significantly different from zero in most cases. We again cannot reject that the experiment had no effect for boys, though the point estimates are consistently greater for boys than in the women-only experiment. There are several possible interpretations for why there may be a smaller or no effect for boys. The first is that awareness of and access to these opportunities, or other high-education employment opportunities, was already very high among boys, so the intervention changed little. Second, employers may have preferred to hire women only (though, as noted, employment at call centers for example tends to be close to parity for men and women in India (Holman, Batt and Holtgrewe 2007)). Alternatively, there may be effects similar to those found in Munshi and Rosenzweig (2006). They find that increases in the returns to English language education due to a shift from manufacturing towards white-collar jobs in Bombay lead to large increases in English-language school enrolment and a closing of the gap in such enrolment by caste for girls, but not boys. They argue the difference by gender is due to caste-based network externalities; lower-caste boys continued to use networks to find blue collar jobs, whereas women, having historically participated less in the labor market, did not have such networks, and thus found an advantage to being in English schools that could them get white collar jobs. It is also possible that the networks would impose sanctions on men looking for work in occupations outside the network. Unfortunately, we cannot test these theories directly with our data. All of the effects above would also mean that our intervention was not really a "gender neutral" expansion in the returns to education, since boys did not really gain in access to the same extent as girls. However, the results of our experiment would still reflect what would be

expected under an expansion of economic opportunities that are in principle open to all, not specifically tilted towards women.

However, we should also note that although we cannot reject that the experiment had no effect for boys, we also cannot not reject that the effect for boys and girls is the same. Thus, while we should perhaps not necessarily conclude the expansion of economic opportunities in this sector has a greater effect on girls than boys, we can reject that in the presence of such a neutral expansion, girls do not gain at all, which is the policy question of interest.

V. DISCUSSION AND CONCLUSION

We find that an intervention making employment opportunities for women more salient and accessible increased human capital investments in girls. Two key advantages of this study are that we supply exogenous variation in opportunities for women and that we can exclude other explanations for the link between employment opportunities and investment in girls, such as bargaining power or the allocation of time within the household.

Our intervention did not create any new employment for women, and thus we do not view recruiting as a solution to the problem of gender bias. Rather, the value of our results is in demonstrating that human capital investments in girls respond to economic opportunities.[37] Many approaches to gender bias by government, NGO's, rights groups and international organizations have emphasized efforts to promote "girls' rights," and focus on information and awareness campaigns, media outputs and campaigns to promote the status of girls, i.e., efforts to act on the social or cultural component of bias (Croll 2001). While there is certainly some role for such efforts, the results here suggest that efforts to expand economic opportunities for women, including a focus on the private sector, are likely to be an important part of the strategy for reducing gender disparities in human capital. Public policy instruments include enforcement of anti-discrimination laws in hiring and promotion, ensuring equality in public sector employment and promotions and/or possibly hiring quotas.[38] Other instruments include focusing

[37] The results also suggest another potential interpretation of the results found in Jensen and Oster (2009), namely that the introduction of cable television to rural villages is met with increases in girls' schooling and lower reported son preference. Cable television may have directly influenced awareness of economic opportunities for women, for example through news stories on the rise of the BPO sector. In addition, many of the most widely watched television programs, especially soap operas, feature women working in non-traditional jobs, including the BPO sector.

[38] For example, the most recent data available (2005) show that government employment in Haryana included 230,517 men but only 83,258 women. http://haryana.gov.in/haryana state/ataglance.asp

on reducing barriers to women entering the labor market, such as workplace laws strengthening part-time work and maternity leave or expanding women's access to credit, either via commercial banks or microfinance schemes, so women can engage in more entrepreneurial activities.

The results also suggest there are likely to be improvements in women's human capital even in the absence of policy interventions. The rise of the BPO sector, along with rapid growth in the service sector more generally, has been shifting the economy away from agriculture and manufacturing. Though the growth in employment in these sectors has been slower than the growth in the GDP share, this shift is likely to continue to generate a greater demand for female labor and a corresponding increase in female labor force participation, as has been observed in countries such as the U.S. (Goldin 1984, 1990, 1995, 2006). And historical evidence suggests such changes can be rapid. As recently as the 1960s, paid labor force participation rates were only around 30 percent in both the United States and Great Britain, increasing to 58 and 71 percent respectively in less than three decades (Costa 2000). Our results indicate that any coming gains in employment opportunities for women will likely result in human capital gains as well.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Control	Women-Only Treatment	Men+ Women Treatment	Difference (3) − (2)	Difference (4) − (2)
Log (expenditure per capita)	6.36	6.36	6.35	6.38	-0.012	0.02
	[0.66]	[0.65]	[0.67]	[0.68]	(0.02)	(0.02)
Head's years of schooling	2.97	3.02	2.92	2.96	-0.10	-0.06
	[3.71]	[3.74]	[3.67]	[3.74]	(0.13)	(0.13)
Spouse's years of schooling	1.59	1.52	1.63	1.61	0.10	0.08
	[2.67]	[2.56]	[2.75]	[2.70]	(0.09)	(0.09)
Family Size	5.55	5.45	5.53	5.68	0.08	0.23*
	[3.10]	[2.37]	[2.39]	[4.20]	(0.08)	(0.12)
Expect Money From Son When Old	0.97	0.97	0.97	0.97	0.01	-0.00
	[0.17]	[0.17]	[0.18]	[0.17]	(0.01)	(0.01)
Expect Money From Daughter When Old	0.04	0.04	0.04	0.03	-0.00	-0.01
	[0.18]	[0.19]	[0.19]	[0.17]	(0.01)	(0.01)
Appropriate for women to work for pay:						
Before marriage	0.17	0.17	0.16	0.17	-0.00	-0.00
	[0.50]	[0.50]	[0.49]	[0.49]	(0.01)	(0.01)
After married, before children	0.067	0.069	0.067	0.063	0.00	-0.00
	[0.25]	[0.25]	[0.25]	[0.24]	(0.01)	(0.01)
In school: girls 7-15	0.76	0.75	0.76	0.76	0.01	0.01
	[0.43]	[0.43]	[0.43]	[0.43]	(0.02)	(0.02)
In school: boys 7-15	0.86	0.87	0.85	0.86	-0.02	-0.01
	[0.35]	[0.34]	[0.36]	[0.35]	(0.02)	(0.01)
BMI-for-age (z-score): girls 5-15	-1.24	-1.26	-1.23	-1.22	-0.03	-0.04
	[1.34]	[1.36]	[1.34]	[1.31]	(0.06)	(0.06)
BMI-for-age (z-score): boys 5-15	-1.30	-1.32	-1.29	-1.30	-0.03	-0.02
	[1.54]	[1.51]	[1.55]	[1.56]	(0.06)	(0.06)
Height-for-age (z-score): girls 5-15	-2.04	-2.03	-2.06	-2.03	-0.03	-0.00
	[1.34]	[1.36]	[1.31]	[1.36]	(0.06)	(0.06)
Height-for-age (z-score): boys 5-15	-1.95	-1.96	-1.89	-2.00	0.07	-0.04
	[1.36]	[1.36]	[1.33]	[1.40]	(0.05)	(0.05)

Notes: Baseline values for variables, collected in the Round 1 survey (September-October, 2003). Standard deviations in brackets in columns 1-4; heteroskedasticity-consistent standard errors accounting for clustering in parentheses in columns 5 and 6. The last two columns are *t*-tests of the difference in means between the control and the women-only treatment sample (column 5) and the women+men treatment sample (column 6). *Significant at 10%; **Significant at 5%; ***Significant at 1%.

TABLE II. EFFECT OF THE INTERVENTION ON ROUND 2 EMPLOYMENT

WOMEN-ONLY TREATMENT

	WOMEN			MEN		
	(1)	(2)	(3)	(4)	(5)	(6)
	18-24	25-44	45-60	18-24	25-44	45-60
Treatment	0.024**	0.0031	-0.006	0.002	0.006	-0.004
	(0.010)	(0.0088)	(0.013)	(0.011)	(0.023)	(0.033)
R^2	0.054	0.001	0.000	0.000	0.001	0.000

MEN+WOMEN TREATMENT

	WOMEN			MEN		
	(1)	(2)	(3)	(4)	(5)	(6)
	18-24	25-44	45-60	18-24	25-44	45-60
Treatment	0.019**	0.005	0.002	0.008	0.002	-0.001
	(0.009)	(0.0018)	(0.019)	(0.008)	(0.011)	(0.027)
R^2	0.043	0.001	0.000	0.001	0.001	0.000

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. All variables measured in round 2 survey. All regressions also include an indicator for whether expenditure data was unavailable (these household are assigned the median sample expenditure). *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

TABLE III. EFFECT OF THE INTERVENTION ON HUMAN CAPITAL: WOMEN-ONLY TREATMENT

PANEL A. GIRLS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age
Treatment	0.046***	0.18***	0.031	0.043***	0.19***	0.043	0.051***	0.22***	0.048
	(0.017)	(0.069)	(0.068)	(0.016)	(0.069)	(0.067)	(0.20)	(0.071)	(0.054)
Log (inc per capita)				0.063***	0.14**	0.165***			
				(0.014)	(0.058)	(0.059)			
Head's Education				0.001	-0.00	0.021			
				(0.003)	(0.012)	(0.021)			
Spouse's Education				0.006***	0.003	0.031			
				(0.002)	(0.016)	(0.014)			
Family Size				-0.001	0.022*	0.031**			
				(0.003)	(0.012)	(0.013)			
Child's Age				0.004	-0.062***	-0.072***			
				(0.004)	(0.011)	(0.009)			
R^2	0.005	0.008	0.00	0.014	0.028	0.05	0.010	0.012	0.00
Observations	1,873	2,106	2,106	1,873	2,106	2,106	1,699	1,631	1,631
Control Group Mean	0.76	-1.27	-2.05	0.76	-1.27	-2.05	0.76	-1.27	-2.05
Mean Effect		0.11***			0.11***			0.14***	
		(0.027)			(0.026)			(0.027)	
F-stat		8.11			8.75			9.76	
(p-value)		0.000			0.000			0.000	

PANEL B. BOYS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age
Treatment	-0.009	0.027	0.018	-0.012	0.015	0.001	-0.012	-0.016	0.013
	(0.016)	(0.075)	(0.058)	(0.016)	(0.072)	(0.058)	(0.020)	(0.068)	(0.047)
Log (inc per capita)				0.016	-0.001	0.117**			
				(0.014)	(0.072)	(0.052)			
Head's Education				0.005**	-0.026**	0.006			
				(0.002)	(0.011)	(0.008)			
Spouse's Education				0.00	0.029*	0.032**			
				(0.003)	(0.016)	(0.015)			
Family Size				-0.003	0.012	0.02			
				(0.002)	(0.017)	(0.014)			
Child's Age				-0.01***	-0.12***	-0.08***			
				(0.003)	(0.011)	(0.008)			
R^2	0.00	0.00	0.00	0.012	0.06	0.05	0.001	0.00	0.00
Observations	2,141	2,442	2,442	2,141	2,442	2,442	1,896	1,828	1,828
Control Group Mean	0.87	-1.32	-1.96	0.87	-1.32	-1.96	0.87	-1.32	-1.96
Mean Effect		0.006			-0.003			0.022	
		(0.025)			(0.024)			(0.027)	
F-stat		0.17			0.14			0.40	
(p-value)		0.92			0.94			0.75	

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. Columns 1-6 use Round 2 outcomes as the dependent variable, and Columns 7-9 use the change in outcome from Round 1 to Round 2. For the first 6 columns, the sample is children aged 7-15 for schooling and 5-15 for BMI and height. For the first columns, the sample is children aged 5-12 at baseline (8-15 at round 2). BMI- and Height-for Age are z-scores. All control variables in columns 1-6 are measured in Round 2. All regressions also include indicators for whether expenditure or mother's or father's education data was unavailable (these household are assigned median values for these variables). "Mean effect" is the mean effect of the treatment across the three outcomes for a given specification, computed using the methodology described in Kling, Liebman, and Katz (2007). F-stat is from a joint test that the treatment variable is zero in the three specifications. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

TABLE IV. EFFECT OF THE INTERVENTION ON HUMAN CAPITAL: SENSITIVITY TO ATTRITION

PANEL A. GIRLS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age		BMI-for-Age	Height-for-Age
Treatment	0.034*	0.136**	-0.034	0.033*	0.144**	-0.024	0.041*	0.163**	0.001
	(0.019)	(0.066)	(0.066)	(0.018)	(0.067)	(0.065)	(0.022)	(0.066)	(0.052)
Log (inc per capita)				0.015	0.120**	0.149**			
				(0.019)	(0.056)	(0.058)			
Head's Education				0.00	0.004	0.003			
				(0.003)	(0.012)	(0.009)			
Spouse's Education				0.012***	-0.001	0.030**			
				(0.004)	(0.016)	(0.014)			
Family Size				0.001	0.023**	0.032***			
				(0.003)	(0.011)	(0.012)			
Child's Age				0.004	-0.06***	-0.07***			
				(0.004)	(0.010)	(0.009)			
R^2	0.001	0.003	0.00	0.012	0.022	0.048	0.003	0.005	0.00
Observations	1,907	2,191	2,191	1,907	2,191	2,191	1,753	1,734	1,734
Mean Effect		0.060**			0.064**			0.065***	
		(0.027)			(0.026)			(0.032)	
F-stat		4.20			4.40			4.33	
(p-value)		0.007			0.005			0.013	

PANEL B. BOYS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age		BMI-for-Age	Height-for-Age
Treatment	-0.034**	-0.048	-0.05	-0.035**	-0.062	-0.068	-0.01	-0.109*	-0.035
	(0.017)	(0.072)	(0.057)	(0.017)	(0.070)	(0.056)	(.021)	(0.064)	(0.045)
Log (inc per capita)				0.011	-0.004	0.107**			
				(0.016)	(0.069)	(0.049)			
Head's Education				0.006**	-0.023**	0.008			
				(0.002)	(0.011)	(0.008)			
Spouse's Education				0.00	0.026*	0.029**			
				(0.004)	(0.015)	(0.014)			
Family Size				-0.001	0.015	0.02			
				(0.002)	(0.016)	(0.013)			
Child's Age				-0.01***	-0.12***	-0.08***			
				(0.003)	(0.011)	(0.008)			
R^2	0.00	0.00	0.00	0.04	0.03	0.01	0.00	0.00	0.00
Observations	2,141	2,442	2442	2,141	2,442	2,442	1,833	1,828	1,828
Mean Effect		-0.049*			-0.058**			-0.058**	
		(0.026)			(0.025)			(0.026)	
F-stat		1.67			2.17			2.63	
(p-value)		0.18			0.09			0.08	

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. Columns 1-6 use Round 2 outcomes as the dependent variable, and Columns 7-9 use the change in outcome from Round 1 to Round 2. For the first 6 columns, the sample is children aged 7-15 for schooling and 5-15 for BMI and height. For the first columns, the sample is children aged 5-12 at baseline (8-15 at round 2). BMI- and Height-for Age are z-scores. All control variables in columns 1-6 are measured in Round 2. All regressions also include indicators for whether expenditure or mother's or father's education data was unavailable (these household are assigned median values for these variables). "Mean effect" is the mean effect of the treatment across the three outcomes for a given specification, computed using the methodology described in Kling, Liebman, and Katz (2007). F-stat is from a joint test that the treatment variable is zero in the three specifications. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

TABLE V. EFFECT OF THE INTERVENTION: TESTING ALTERNATE HYPOTHESES

NO WOMAN WORKS FOR PAY	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age
Treatment	0.047***	0.165**	0.047	0.044***	0.175**	0.058	0.053**	0.209***	0.08
	(0.020)	(0.071)	(0.073)	(0.019)	(0.079)	(0.072)	(0.22)	(0.074)	(0.059)
Log (inc per capita)				0.004	0.201***	0.190***			
				(0.022)	(0.067)	(0.066)			
Head's Education				-0.001	-0.005	-0.004			
				(0.003)	(0.014)	(0.011)			
Spouse's Education				0.014***	-0.005	0.034**			
				(0.004)	(0.018)	(0.016)			
Family Size				-0.003	0.005	0.018			
				(0.004)	(0.016)	(0.015)			
Child's Age				0.01*	-0.06***	-0.07***			
				(0.004)	(0.012)	(0.011)			
R^2	0.00	0.00	0.00	0.04	0.03	0.01	0.00	0.00	0.00
Observations	1,704	1,909	1,909	1,704	1,909	1,909	1,564	1,482	1,482
Mean Effect		0.12***			0.13***			0.15***	
		(0.030)			(0.029)			(0.036)	
F-stat		7.58			8.15			10.16	
(p-value)		0.000			0.000			0.000	

MOTHERS W/ NO HIGH SCHOOL	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age	Enrolled	BMI-for-Age	Height-for-Age
Treatment	0.048***	0.178***	0.044	0.046***	0.188***	0.056	0.049***	0.217***	0.052
	(0.020)	(0.069)	(0.070)	(0.019)	(0.070)	(0.067)	(0.23)	(0.071)	(0.054)
Log (inc per capita)				0.009	0.152**	0.148**			
				(0.020)	(0.060)	(0.060)			
Head's Education				-0.001	0.001	0			
				(0.003)	(0.012)	(0.010)			
Spouse's Education				0.013***	-0.002	0.024			
				(0.005)	(0.017)	(0.016)			
Family Size				-0.001	0.022*	0.034***			
				(0.003)	(0.012)	(0.013)			
Child's Age				0.00	-0.06***	-0.07***			
				(0.004)	(0.011)	(0.009)			
R^2	0.001	0.003	0.00	0.012	0.026	0.045	0.010	0.01	0.001
Observations	1,728	1,934	1,934	1,728	1,934	1,934	1,569	1,506	1,506
Mean Effect		0.10***			0.11***			0.12***	
		(0.028)			(0.027)			(0.033)	
F-stat		6.58			7.28			8.15	
(p-value)		0.000			0.000			0.000	

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. Columns 1-6 use Round 2 outcomes as the dependent variable, and Columns 7-9 use the change in outcome from Round 1 to Round 2. For the first 6 columns, the sample is children aged 7-15 for schooling and 5-15 for BMI and height. For the first columns, the sample is children aged 5-12 at baseline (8-15 at round 2). BMI- and Height-for Age are z-scores. All control variables in columns 1-6 are measured in Round 2. All regressions also include indicators for whether expenditure or mother's or father's education data was unavailable (these household are assigned median values for these variables). "Mean effect" is the mean effect of the treatment across the three outcomes for a given specification, computed using the methodology described in Kling, Liebman, and Katz (2007). F-stat is from a joint test that the treatment variable is zero in the three specifications. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.

TABLE VI. EFFECT OF THE INTERVENTION ON HUMAN CAPITAL: MEN+WOMEN TREATMENT

PANEL A. GIRLS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Enrolled	Weight	Height	Enrolled	Weight	Height	Enrolled	Weight	Height
Treatment	0.044***	0.135*	0.013	0.041***	0.145**	0.034	0.045***	0.150**	0.041
	(0.020)	(0.069)	(0.067)	(0.019)	(0.071)	(0.064)	(0.21)	(0.057)	(0.056)
Log (inc per capita)				0.014	0.173***	0.218***			
				(0.020)	(0.064)	(0.055)			
Head's Education				0.002	0.011	0			
				(0.003)	(0.013)	(0.009)			
Spouse's Education				0.010**	-0.042**	0.025*			
				(0.005)	(0.018)	(0.014)			
Family Size				0.002	0.018	0.027**			
				(0.004)	(0.013)	(0.013)			
Child's Age				-0.00	-0.06***	-0.07***			
				(0.004)	(0.010)	(0.009)			
R^2	0.02	0.01	0.00	0.06	0.04	0.01	0.00	0.02	0.00
Observations	1873	2102	2102	1873	2102	2102	1684	1631	1631
Mean Effect		0.079**			0.089***			0.072**	
		(0.028)			(0.027)			(0.033)	
F-stat		4.72			5.26			2.81	
(p-value)		0.004			0.002			0.063	

PANEL B. BOYS	Round 2 Outcomes			Round 2 Outcomes			Δ (Round 2-Round 1)		
	Enrolled	Weight	Height	Enrolled	Weight	Height	Enrolled	Weight	Height
Treatment	0.016	0.147*	-0.024	0.015	0.132*	-0.039	0.008	0.07	0.057
	(0.014)	(0.074)	(0.059)	(0.014)	(0.071)	(0.057)	(0.21)	(0.072)	(0.048)
Log (inc per capita)				0.015	0.132**	0.179***			
				(0.013)	(0.065)	(0.055)			
Head's Education				0.007***	0.002	-0.002			
				(0.002)	(0.012)	(0.009)			
Spouse's Education				-0.004	-0.003	0.030**			
				(0.003)	(0.017)	(0.015)			
Family Size				0.002	0.025*	0.003			
				(0.002)	(0.014)	(0.015)			
Child's Age				-0.01***	-0.11***	-0.08***			
				(0.003)	(0.011)	(0.008)			
R^2	0.02	0.01	0.00	0.06	0.04	0.01	0.00	0.02	0.00
Observations	2141	2442	2442	2141	2442	2442	1904	1828	1828
Mean Effect		0.038			0.030			0.054**	
		(0.028)			(0.026)			(0.025)	
F-stat		1.80			1.71			2.34	
(p-value)		0.15			0.17			0.10	

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. Columns 1-6 use Round 2 outcomes as the dependent variable, and Columns 7-9 use the change in outcome from Round 1 to Round 2. For the first 6 columns, the sample is children aged 7-15 for schooling and 5-15 for weight and height. For the first columns, the sample is children aged 5-12 at baseline (8-15 at round 2). BMI- and Height-for Age are z-scores. All control variables in columns 1-6 are measured in Round 2. All regressions also include indicators for whether expenditure or mother's or father's education data was unavailable (these household are assigned median values for these variables). "Mean effect" is the mean effect of the treatment across the three outcomes for a given specification, computed using the methodology described in Kling, Liebman, and Katz (2007). F-stat is from a joint test that the treatment variable is zero in the three specifications. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.