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

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Abstract: Large gender differences in health and education are a concern for a number of developing countries. An important question is whether economic factors underlay these differences, and in particular whether investments in girls respond to changes in their future employment opportunities. This is particularly important because most policy efforts emphasize the role of cultural factors. Though many studies have examined the link between women's wages or labor force participation and investment in girls, there are two major concerns. First, most studies are subject to concerns about omitted variables bias and reverse causality. Second, there are many mechanisms that may underlay this relationship (returns to human capital; bargaining power; income; household time allocation; fertility, etc.), and most studies have been unable to examine which were operative. To overcome these concerns, 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. This allowed us to "in effect" exogenously increase women's labor force opportunities from the perspective of decision-makers. Using panel data spanning three years, we find that girls in treatment villages were more likely to be in school and had greater measured BMI. We also argue that we can test the returns mechanism largely in isolation, by identifying a set of households where the other mechanisms are unlikely to apply. Since these other mechanisms primarily operate through the contemporaneous employment of a mother or other woman in the household, we show that there are gains for girls even in households where no women work and further, where none could get one of these new jobs because they do not meet the education requirements. We argue that these results, coupled with a series of additional tests, suggest that at least for these households, parents responded to the increased future labor market opportunities for their daughters−though we also discuss what remaining possibilities can't be dismissed with our data.

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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. 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 boys, primarily due to inadequate provision of nutrition and medical care (Chen, Huq and D'Souza 1981, Sen and Sengupta 1983, Das Gupta 1987 and Behrman et al. 1988).[1] The excess mortality of girls contributes to the problem of highly masculine sex ratios and "missing women" that has received considerable attention (Visaria 1969, Miller 1981, Sen 1990).

Though such gender differences are well-documented, considerably less is known about the underlying causes or potential remedies. A number of studies have proposed that economic factors, such as the returns to human capital or potential economic contributions of women, particularly in agriculture, may underlay these differences (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 or economic opportunities for women are lower. And indeed, a large literature has documented that greater female labor force participation or wages are correlated with improved human capital and survival outcomes for girls (Agnihotri, Palmer-Jones and Parikh 2002, Kishor 1993, 1995, Miller 1981, Murthi, Guio and Drèze 1995, Qian 2008 and Rosenzweig and Shultz 1982).

However, two important issues make it difficult to conclude that it is increased future employment opportunities or earnings potential that leads parents to invest more in their daughters. First, concerns about omitted variables bias (areas where women work differ in many ways from areas where women do not work) and reverse causality (higher human capital women are more productive and therefore earn higher wages or are more likely to be in the labor force) make it difficult to draw a causal interpretation in many of these studies, as noted by Sen (1990) and Foster and Rosenzweig (2009).[2] Three studies that attempt to address these concerns are Rosenzweig and Schultz (1982), who use variation in rainfall as a determinant of women's earnings opportunities, Qian (2008), who exploits a policy reform in China that differentially

[1] See Strauss and Thomas (1995), Behrman (1997) and Haddad et al. (1997) for reviews.

[2] 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.

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.

A second limitation is that most studies cannot specifically identify which mechanism(s) underlay the link between current employment rates or wages for adult women and investment in contemporaneously young girls. Beyond expected returns to human capital (i.e., current economic conditions for women reflect returns daughters will face in the future), there are many other potential channels. First, greater earnings opportunities for adult women may increase a mother'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 do, that increased 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 will benefit girls (Schultz 2001). 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 or cooking. This may increase the incentive for parents to invest in their daughters, both just to 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.[3] Fourth, 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. These mechanisms all yield very different implications for understanding both the root causes of gender differences in outcomes as well as the policy instruments that will most effectively address the problem.[4] Yet, most studies have not attempted to identify or test for the underlying mechanism. An exception is Qian (2008), who provides some arguments that bargaining power of women might explain, at least in part, why increases in the value of women's crops lead to less male-skewed sex ratios in China. However, she also notes that it isn't possible to explicitly or directly test this hypothesis against alternatives.

[3] Of course, the additional time demands of household production could instead lower girls' schooling.

[4] For example, if bargaining power explains the correlation between women's employment and investment in girls, then policies aimed at strengthening women's bargaining power, such as reform or enforcement of divorce laws or strengthening women's property rights, might be favored over efforts to promote women's employment. If it is instead an income effect, then the best policies are those that promote income growth, rather than specifically promoting women's opportunities. Similarly, efforts to directly address fertility would be favored if that is the relevant mechanism.

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 Business Process Outsourcing (BPO) industry in India has grown rapidly 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 was very limited, especially outside of the urban core where these jobs were located, and among less educated households. 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 these new opportunities, and thus "in effect" increase future employment opportunities for girls from the perspective of decision-makers.[5] Random assignment allows us to overcome omitted variables bias and reverse causality concerns, and results in a weaker identifying assumption and a stronger case for causality than in the cross-sectional and natural experiment studies listed above.

Using panel data spanning a three year period, we find that the BPO recruiting services increased employment among young women, with no effect for older women or men of any age (as per the intent 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 in any of these measures.

We believe that documenting the relationship with a stronger case for causality is an important contribution towards understanding the problem of gender differences in human capital. However, an equally important contribution is the ability to isolate and test, to a greater extent than previous studies, the role of one specific mechanism. We argue that the design of the experiment, as well as evidence from supplemental tests, allows us to show that at least some of the gains we observe were likely to have been driven by greater future returns to human capital investments rather than the other mechanisms listed above. A key part of our strategy is the observation that the non-returns mechanisms entail the impact of employment of mothers (or other adult female household members) on contemporaneous investments in their daughters, such as: a mother gets a job and earns money, and as a result has more say over household

[5] In this respect, the paper is similar to Jensen (2010), who finds that providing information on the returns to schooling increases educational attainment for boys in the Dominican Republic.

decisions; changes in the value of the daughter to the household when her mother works; reduced fertility due to the increased opportunity cost of the mother's time; or increased household income when the mother works. However, our intervention created opportunities primarily for younger, unmarried women, since most employers in this sector strongly favor this group (Ng and Mitter 2005) and because most of the jobs require a secondary school degree, English language skills and familiarity with computers, 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 (much less commute long distances or migrate, which would be required for these urban-based jobs), 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 girls, leaving the employment opportunities for older women (current mothers) largely unchanged. This design helps reduce the likelihood of those links driven by changes in the employment of mothers.

However, to provide a more direct and "cleaner" test of the returns mechanism, we minimize the likelihood the alternatives are operative (and the possibility of effects operating through the employment of adults other than the mother, such as an older sister), by showing that the results continue to hold in households where no women work for pay and further, where no household member (including those such as older siblings who may have permanently left home) could get one of the BPO jobs because they all have less than the 10 years minimum required schooling. While there may have been income, bargaining or other effects for those households where a woman did or could get a BPO job, this restriction allows us to identify a set of households where we have reduced or eliminated much (though not all, as discussed below) of the possible effects driven by current working behavior, as well as much of the effects of future or potential employment (e.g., the possibility that even if a woman does not work, her bargaining power might be increased by the threat that she could work).

For this subsample, what remains are possible effects arising from non-household members, such as other relatives or friends having gotten a job and perhaps providing money to the household (either generating an income effect, or, if it is given to the mother, a bargaining power effect). Alternatively, there may be current effects of daughters working in the future (e.g., households may be able to borrow against their future income, generating income effects now, or the mother's bargaining power may be improved now by the possibility that her daughter

will send her money in the future). We provide some direct and indirect tests for such possibilities (and several others, described below), showing that in our restricted subsample, there is no change in: current expenditures (suggesting that borrowing against future income is unlikely to be driving the results); transfers received (e.g, from non-household members who got a BPO job);[6] or direct measures of women's bargaining power such as autonomy or participation in household decision-making. Of course, there is no way to rule out every conceivable alternative, and below we discuss the remaining assumptions required to conclude that this subsample allows us to test the returns mechanism while largely ruling out the alternatives. We believe these results provide more evidence in favor of one particular mechanism and against alternatives than has been possible with previous studies.[7]

We believe our study provides three primary contributions. First, the results provide insight into the underlying economic causes of the dramatic gender differences in human capital observed in countries such as India, and suggest potential policy solutions. This is particularly important because considerable emphasis in both academic studies and policy debates has been placed on the role of cultural factors. For example, Amartya Sen, whose seminal work on missing women stimulated much of the current debate, has recently asked whether "cultural factors" might explain why gender disparities are large in the north of India and largely absent in the south (Sen 2003). Correspondingly, the suggestion has been that gains for girls 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 girls, i.e., efforts to act on any social or cultural component of bias (Croll 2000). While not denying some possible role for such factors,[8] our results demonstrate an economic or labor market underpinning to the causes and potential solutions to these gender differences.

Second, our results also provide insight into the underlying causes of poor education and health more generally in developing countries. Much of the literature and policy on these issues have focused on poverty and credit constraints, or "supply side" factors such as distance or access to schools or health clinics, infrastructure, materials and other inputs, etc. While these

[6] We also rule out effects due to transfers by further restricting to households that did not receive any.
[7] Again, this is not to suggests that these other mechanisms are never operative, or even that they are not operative for other households in our treatment villages, such as those that got a BPO job.
[8] For example, Jensen and Oster (2009) find that the introduction of cable television improves the status of rural women, including girls' education, potentially through exposure to urban lifestyles that show different roles for women.

factors are certainly important, our experiment changed awareness of the returns to human capital but none of these other factors, which suggests that there are important "demand-side" constraints as well (as also suggested by Jensen 2010). In other words, for at least some girls, the problem of low human capital attainment was that parents saw little value to investing in them. When parents were given clear and salient evidence of greater future opportunities for their daughters, they invested more in them even without changes in any of these other factors.

Third, our results contribute to the literature on the historical co-evolution of women's labor force participation and education, including the role of technological change. For example, Goldin (1984, 1990, 2006) has argued that office/information technologies played some role in the increases in women's employment and education in the United States in the early 1900s, particularly in the clerical or white collar sectors. Newer information technologies such as computers and the internet may be playing a similar role in countries like India today to that played by their predecessors such as the telephone and typewriter in currently wealthy countries a century ago.

We also note that our study is closely related to three other studies that point to a role of recent sectoral changes in labor market demand in promoting human capital investments in India. Munshi and Rosenzweig (2006) find that enrollment in English language schools increased for girls (but not boys) in response to increases in the returns to English language education in Bombay, driven principally by 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 large increases in school enrolment for both boys and girls. Finally, Shastry (2010) finds that the information technology sector grew more rapidly in areas of India where English is more widely spoken, and that in turn those areas experienced increased school enrolment. The results we find are broadly consistent with these other studies. As noted, two advantages of the present study are the weaker identifying assumption due to randomization and a greater ability to isolate a particular underlying mechanism.

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 on human capital, and Section V provides a series of tests to understand the role of various potential mechanisms underlying the results. Section VI concludes.

II. EXPERIMENTAL DESIGN

II. A. The BPO Sector in India

The Business Process Outsourcing (BPO) industry is a broad umbrella covering 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.[9] 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. BPO growth in India was also facilitated by regulatory changes allowing greater foreign investment in the telecommunications sector. Overall, India's BPO sector 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). 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.[10]

Thus, technological and regulatory changes in the BPO sector in India created a sharp and fairly sudden increase in the demand for female workers, particularly those with secondary school degrees or higher and English language skills. In order 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. And because the BPO sector is strongly geographically concentrated in India, with 95% of employment focused around

[9] Much of the BPO sector is export-driven. For example, in India, almost three-quarters of call centers serve international markets (Holman, Batt and Holtgrewe 2007).

[10] It is also possible that because women have fewer alternative employment opportunities than men, they can be paid less or treated worse, without risk they will leave the job.

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

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 was even more pronounced outside the urban centers where these jobs were located (in fact, in our 2003 baseline survey of rural households described below, no one was employed in this sector, including any members or children of members having temporarily or permanently left the household). Our experiment was designed to both increase awareness of these jobs and to make it easier for qualified women to get them.

We hired eight BPO recruiters (5 women and 3 men), all with at least two years experience overall and 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 drew 80 treatment and 80 control villages 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 disparities are 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 India's 593 districts by the sex ratio 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.

For the treatment villages, between December 2003 and February 2004, a randomly assigned recruiter would visit and make a small introduction at schools and to local leaders, announcing that they would be visiting the village 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. The sessions were open to women only.[11] 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. This in effect ruled out a vast majority of women over the age of 25; for example, in our data only 8 percent of women aged 26−50 have completed secondary school.

The sessions were held in a range of facilities including schools and NGO or government offices, and typically lasted from 4 to 6 hours. 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 and level of compensation available; 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 resumes, 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 in any way guaranteeing employment.

[11] 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 imperfect commitment. The latter requires a greater theoretical and empirical treatment of the parental decision-making process, as well as stronger data requirements, and is left to a companion paper (Jensen and Miller 2010). The purpose of the present paper is to test specifically for gender effects, which is particularly important in light of the large and well-documented gender disparities in human capital in many countries. Here we simply note that the human capital gains for girls in this second set of treatment villages is very similar to what is found here.

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 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 anyone from the designated villages. Thus, the intervention consisted (exclusively) of three in-depth sessions and three years of continuous placement support.

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 study may simply get jobs at the expense of other women, with no net effect on women's employment overall.[12] The goal of the experiment is to supply exogenous variation in employment opportunities to test whether the change in potential returns influences household investment in girls.

One important aspect of the intervention worth highlighting is that the employment opportunities were 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.[13] 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 have a comparative advantage in this type of employment, since it does not require physical

[12] 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.

[13] 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. For more remote 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).

strength. We would therefore not necessarily generalize our findings to increases in the returns in agriculture (as in Foster and Rosenzweig 2009) or the blue-collar sector 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 towards 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 IT sector alone comprising 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) is driven by the white collar, service or clerical sectors (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 (1984, 1990, 2006) argues 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 part 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 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 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, and education of all members, expenditures, etc.). Importantly for our analysis below, we also asked for demographic and socioeconomic characteristics of all household members or children of members who have either temporarily or permanently left the household. The adult questionnaire was asked of all individuals aged 18 and older.

Enumerators also weighed and measured the height of all household members aged 5 and older.[14] If someone was not home at the time of the survey, enumerators scheduled up to three return visits to take these measurements, and would also visit other locations if the person lived away from home. 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, and enumerators again weighed and measured all household members aged 5 and older. We also tracked, and where possible measured, all individuals who left home between the rounds (such as for work or marriage).

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 non-measurement in general) 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).[15] As noted, we gathered information about children of household

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

[15] The expected bias is difficult to predict. 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 control girls even if the treatment had no effect (though marriage is very uncommon

members who had left home, and where possible tracked and weighed and measured them.[16] 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 reasons including infirmity or temporary absence from the home). Total attrition for children aged 2−12 at baseline (and thus 5−15 at round 2) was 4.6 percent, about 3 percent of which was attrition of entire households (i.e., no one from the round 1 household was present at the same residence in round 2).[17] Total attrition among girls 5−12 in the treatment villages was 4.7 percent, compared to 4.5 percent for the control villages. Though the difference in attrition for the treatment and control groups is small, it is possible 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 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 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 0.71 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).

The table also provides evidence of gender differences in education. The average adult woman's years of schooling is 1.8, compared to 3.8 for men. Similarly, only 74% of girls aged 7−15 are currently enrolled in school, compared to 87% of boys (all enrollment data were verified by contacting schools that students said they attended). These enrollment figures are broadly comparable to the rest of India.[18]

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for girls under age 15). 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.

[16] We were able to locate and weigh and measure 62% of children 5−15 who left home between rounds.

[17] 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.

[18] A direct comparison with other data sources is not possible because we look over the 7-15 age range, which includes primary and some secondary years, whereas official data are only available by level. However in 2002 (the comparable academic year for our survey), enrollment rates for primary school, which covers much of the 7-15 range, were 72% for girls and 84% for boys (WDI 2003).

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 developed by the World Health Organization (de Onis et al. 2007). These measures are considered the appropriate indicators of growth and development for children aged 5−19 and reflect the provision of nutrition and health care.[19] The table shows that children in the sample are undernourished, with very low weight (BMI) and height relative to international norms. On average, children aged 5−15 are 1.2 to 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 however indicate any differential between boys and girls. This somewhat surprising result is also found in other data sets; 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),[20] 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. There are several reasons why anthropometric measures may not detect gender differences, and it is beyond the scope of the present paper to resolve this issue.[21] 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.

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 child, *i*, lives in a village that was exposed to the recruiting intervention. We limit the sample to children under 15 to minimize selective attrition, given the increasing

[19] The WHO also computes weight-for-age standards, but only up to age 10 because it is not considered an informative measure for older children (de Onis et al. 2007), so we do not use it here.

[20] 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).

[21] 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 the least healthy children. Deaton (2007) discusses the potential offsetting impacts of these "scarring" vs. "selection" effects.

likelihood of leaving the household for work or marriage after this age. Limiting the sample in this way also means that we are examining changes in human capital (e.g., BMI) not for those individuals who got placed in one of the BPO jobs, but younger children for whom the recruiting sessions represent future economic opportunities, which their parents might respond to now.

The second specification adds controls that are baseline predictors of human capital, $Y_i = \beta_0 + \beta_1 Treatment_i + \sum \gamma_i Z_i + \varepsilon_i$ (parent's education, log of family expenditure per capita, family size and child age). Our third specification uses the change in outcomes between rounds 1 and 2, 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. For example, the samples they cover will be slightly different, since the changes specification cannot include children 5−7 in round 2 (because they will have been 2−4 in round 1, and thus not weighed or measured) or any children not measured in round 1 for reasons such as 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, and the regressions with controls or using changes will capture some of these differences.

For all specifications, we estimate linear regressions regardless of whether 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. We also note again that all of our human capital measures are objective, rather than self-reports (since school attendance was verified with schools).

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[22] 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. To assess the impact of the treatment on women's employment with our survey data, we regress an indicator for whether an individual works for pay in a non-family enterprise[23] in the Round 2 on an indicator for living in a treatment village. As noted above, employment data were gathered for all household members and children of members, including those not living at home (either temporarily or permanently), so we include these individuals in the sample as well. Since the treatment was focused only on younger women, we split the sample into age and sex categories.

Table II shows the results. In villages that received the recruiting treatment, paid employment was 2.4 percentage points higher for women aged 18−24. The effect is perhaps small in absolute terms, but large when compared to the Round 2 control group mean for women of this age of about 21 percent.[24] As expected given the experimental design, there was no change in paid employment for women aged 25−44 or 45 and older. The coefficients for these two groups are 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

[22] Though some of this includes helping some women get a job more than once over the three years.

[23] We prefer this measure over BPO-only employment for several reasons. First, though we gathered detailed job data (via open-ended responses on occupation, industry and employer), defining the exact boundaries of the sector is difficult (just matching which women in our sample got a job from one of our recruiters would not capture the employment gain since some control and treatment women could have gotten these jobs without recruiters). Further, to the extent that the recruiters provided job search skills that helped women get jobs in other sectors (ex., bank teller), we would want to count these gains as well. Finally, if we focus only on BPO jobs, we could overestimate the impact of the treatment since some women may just shift to this sector from, say, other white collar jobs, with no net gain in employment.

[24] It is unlikely that the jobs gained in treatment villages came at the expense of women in the 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; and since few women in rural areas get these jobs, any losses are likely to be found in urban areas, which are outside our sample. Though the interpretation would largely be the same in the face of such effects; observing human capital reductions for girls where job opportunities for women decline would still show that returns affect investments (unless we believe there are asymmetric effects where girls are harmed when opportunities decline but do not gain when they increase).

targeted towards, and only those women.[25] It is these gains that we predict may have effects on investments in girls.

B. Human Capital

Table III shows the effects of the treatment on education and anthropometric measures. The first set of columns shows the specification where the dependent variable is Round 2 outcomes with no additional controls, the middle columns add baseline controls, and the last columns examine changes in outcomes between the two rounds. Girls (top panel) were about 5 percentage points more likely to be enrolled in school in the treatment villages. The coefficient is robust across the three specifications, and statistically significant at the 1 percent level in all cases. The effects are large, representing about 6−7 percent gains for the treatment group relative to the control group (control group means are presented at the bottom of each column), and close about 40 percent of the baseline boy-girl gap in enrollment (Table I).

Columns 2, 5 and 8 show that the treatment also resulted in an average increase in BMI-for-age z-score of about 0.20−0.30 for girls, 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; the treatment closed about 15−20 percent of the BMI gap between our sample and the well-nourished WHO reference population. Finally, columns 3, 6 and 9 show that point estimates of the effect of the treatment on height-for-age are positive, but very small and not statistically significant in any of the specifications.[26]

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 panel 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

[25] The intervention could have lead to employment gains for men or older women if those 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. There are several reasons why men's employment may not have changed: men may have already had access to other high-education jobs; BPO employers may also have preferred to hire women; and caste-based job networks may have limited men's occupational mobility, as found in Munshi and Rosenzweig (2006).

[26] The medical literature suggests that nutrition appears to have little effect on height after the age of 2, 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.

of human capital measures for girls at the one percent level or better for each of the three empirical specifications.

The bottom panel of the table shows the effect of the 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 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 anthropometric results focus on children aged 5 to 15, and the effects of nutrition or health care may be greater for infants (Russell and Rhoads 2008). Alternatively, it is possible we overstate the long-run effects; improved nutrition for younger girls may simply lead to an earlier onset of puberty, rather than long-term gains in weight.[27] 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 ultimate adult weight gain, this would still serve as an indicator that there had been greater nutrition and/or health care investments in girls. Our interest in the anthropometric measures is as an indicator of those unobserved investments; it seems unlikely there is another mechanism through which the intervention 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. Finally, we should also mention that it is possible that the long-run effects of our intervention may be smaller than what is observed here if parents "overreact" to the treatment and overestimate the future returns to girls' human capital, and later adjust expectations downward (because, say, they observe that not every woman gets a high paying BPO job). However, again, our goal is not to test the impact of this recruiting intervention as a policy instrument, but whether some signal of increased returns leads to greater investment in girls.

[27] Though in this case we would expect to see differences in height.

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 still living at home. Though at baseline there should be no difference between treatment and control villages along this dimension, if our intervention caused some girls to stay home longer, such as by delaying marriage for work or more schooling, we may systematically skew the treatment sample and conclude there is an effect of the treatment even if there is none (we noted above that the treatment and control groups have similar attrition rates, but there could be offsetting effects). Attrition has less of an impact for our schooling measure, since parents are asked to provide information on schooling for children not at home (which we verify by contacting schools); thus, we primarily lose data for children whose entire household has attrited.

Rather than modeling attrition, Table IV shows that our results continue to hold even under a fairly extreme assumption about the outcomes of children not living at home in Round 2. In particular, we assign all non-measured treatment children as not being in school, and give them a BMI and height (or changes in BMI and height) equal to the 25th percentile of the corresponding distribution for other children of their age and sex, while all non-measured control children are assigned as being in school and given the 75th percentile of their age*sex BMI 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 strongly against our finding an effect, the coefficient for the treatment indicator is still statistically significant for girls at the 10 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.[28]

V. MECHANISMS

Though the gains for girls are consistent with parents responding to the increased future returns to human capital for their daughters, there are many alternative mechanisms though which economic opportunities for women might lead to greater contemporaneous investments in girls. Of course, our intervention would still show a causal link between opportunities for women and investments in girls, which is valuable in itself. However, it is important to understand the

[28] For boys, some of the negative effects are statistically significant under these assumptions.

mechanism underlying this relationship, which most previous studies have not been able to examine. Our goal is not to reject that these other mechanisms are ever relevant, or even that they don't apply to some households in our sample (such as those who got BPO jobs), but simply to specifically test the returns mechanism as cleanly and in as much isolation as possible.

As noted, a key aspect of our approach is the observation that most of the non-returns mechanisms (bargaining power, income, fertility, household allocation of time, etc.) result from changes in whether the mother (or possibly other adult women, including older children) works, whereas the returns mechanism is based solely on the potential future opportunities of currently young girls, and thus would take place even with no change in mother's employment. We will use one common approach that can help reduce the likelihood that any of these alternative mechanisms are operative, namely looking at the subset of households where either no adult female member works or where no one in the household could get one of the jobs because they have too little education. However, this test will not completely rule out all other mechanisms, or some specific refinements of these mechanisms. Therefore, we will also need to present some additional tests, so we discuss each of the potential mechanisms individually.

A. Bargaining Power

Changes in mother's bargaining power is one of the most commonly proposed mechanisms linking women's labor force participation or wages and investments in girls.[29] We specifically chose the BPO sector for our intervention in order to target the employment opportunities of younger women almost exclusively, in that older women are less likely to have finished high school, speak English or have computer experience. The recruiters also told us that most BPO firms prefer to hire young, unmarried women, which is confirmed by Oster and Millet (2010) and Ng and Mitter (2005). More generally, in our rural sample very few women with young children work for pay (less than 5 percent). This is likely to be even more binding for our intervention, since the BPO jobs are not based in the rural areas of our sample, and thus require commuting or migration. Turning to the data, Table II shows there was no increase in paid employment for women over age 25, and few women 24 or younger are likely to have children aged 5 or older. In addition, for women with children 5−15, if we regress Round 2 paid

[29] Alternatively, it need not be bargaining power per se, but that women who work are able to set aside money from their earnings, either with or without their husband's knowledge, and use that money to provide children food, medical care or schooling.

employment on an indicator for the treatment, we get a coefficient of 0.00321 with a standard error of 0.029. Thus, there appear to have been no employment gains for mothers of the children whose human capital we analyzed in the previous section, as per the intent of the experiment.

More directly, we can identify a subset of households where it is unlikely that the gains for girls were due to the increased bargaining power of newly working mothers (or other adult women in the household), by considering only those households that do not have any women (either living at home or temporarily or permanently away from home (such as older daughters who have left the household)) who work for pay in Round 2.[30] The top panel of Table V shows that there are still gains for girls in this sample, and in fact the results are very similar to the full sample results in Table III. Again, we note that this test also rules out that the gains for girls were driven by contemporaneous employment gains of women in the household besides the mother, such as an older sibling[31,32] (though this would not rule out effects driven by contemporaneous employment gains for relatives or others not living in the household, or future employment gains for the mother or other woman in the household, all of which we consider below).

Of course, it is possible that a woman's bargaining power may increase even if she does not work, simply by the possibility or threat that she could. Though a full test of this hypothesis is not possible, we can examine whether the treatment had an effect for households where there was essentially no possibility of women getting a BPO job, because none have a high school degree.[33,34] For this test, we apply the restriction not just to the mother herself, but to all female household members, whether living at home or away from home, which again would include any daughters who have left the household even on a permanent basis. In the middle panel of Table V, we see that the effects are again very similar to the full sample results in Table III, and remain statistically significant at the 1 percent level.

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[30] One potential problem with this test is that we stratify on a choice variable affected by our intervention. Next, we look instead at subsamples where not having a woman work is a function of factors exogenous to the treatment, i.e., where no adult female members have a high school education.

[31] As noted above, the household survey collected demographic and economic information on all children of members of the household, even if they have temporarily or permanently left the household.

[32] The results are very similar if we restrict the sample to households where no woman works and all children ever given birth to are under 16 (the latter to rule out effects driven by older sisters).

[33] Of course, there are many employment opportunities for women without a high school degree. However, our treatment is unlikely to have caused a change in access to those opportunities.

[34] Though this is a select sample (but still a vast majority of women), we might perhaps otherwise expect the treatment to have a smaller effect for this group, since their lower education may signal that they have lower preferences for education, perhaps even specifically with regards to girls.

So far, we can largely rule out that the gains for girls in this subsample are driven by bargaining power changes directly through a mother or any other female household member having gotten a job or having the possibility of getting a job in the future. However, there are a few remaining possibilities. The first is that friends or relatives who are not members of the household could get a BPO job and give money to the mother, thereby increasing her bargaining power. We of course could not collect information on all possible friends and relatives. However, we can directly test for such transfers. The survey asked each person in the household about all transfers or gifts received by individuals outside the household, including cash, goods or payments made by others on behalf of the household.[35] The first two columns of Table VI regress the probability of having received any such transfers and the amount received on the treatment indicator (we continue to focus on households where no member has enough education for one of the jobs).[36] The results show that there was no change in either the incidence or amount of transfers received by these households. Overall, this suggests it was unlikely women in our subsample gained bargaining power via cash earned and sent by others (though we cannot rule out that such transfers are underreported). We can also estimate our human capital regressions restricting the sample further to additionally exclude those that received any transfers in Round 2. The bottom panel of Table V shows that the results continue to hold. The results are slightly less precise due to a reduction in sample size. However, the education and BFA coefficients continue to be significant at the 5 percent level or better in all three specifications. Thus, we have now found gains for girls in households where no currently adult women got a job or could get a job, and where no one outside the household who may have gotten a job sent money to the household.

We consider a few remaining possibilities. First, it is possible that a mother's bargaining power may be increased now by the fact that her daughter may work and send her money in the future. In this case, a mother's current bargaining power might be influenced even if she has no daughters who are old enough yet to work or to have enough education to get one of the jobs. Second, the recruitment of women itself may send a signal about the importance placed on women or the role of women among people who live outside the village, which might increase

[35] About 14 percent of households report having received any such transfers over the past year, and conditional on receipt, the median transfer was about 100 Rupees per month.

[36] Households where a member got a BPO job and migrated to the city, for example, are likely to have increased reported transfers. Our goal is to test for transfers to households that did not get a BPO job.

women's status or bargaining power within the treatment villages. Alternatively, the fact that some women from the village now work in the city may improve other women's bargaining power, even those who do not work and cannot hope to work in one of these jobs in the future.[37] These possibilities are not testable with our data. However, we can examine some simple, direct measures of women's bargaining power. These data can serve as a crude test of these remaining possibilities, as well an overall test for all the bargaining power explanations above: i.e., regardless of how women's bargaining power changed (though own work, the threat of future work, other women sending her money, etc.), we can examine whether we detect changes in direct (though imperfect) measures of women's bargaining power.

The survey asked women whether they participated in household decision-making for a range of actions: 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, which included "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)." We note that women's participation in many of these decisions is quite limited overall;[38] for example, in the control group in Round 2, 53 percent of women report they do not participate in decisions about children's schooling or health care at all (39 percent report making the decision jointly with others in the household).[39] Additional questions were asked about women's "autonomy": whether they can visit the market without permission; visit family or friends without permission; and whether they were permitted to keep money set aside to spend as they wish. This second set of questions does not directly measure bargaining power, but they are actions we might expect to change if women's bargaining power has changed. Both sets of questions are of course crude and limited, and do not capture the full possible expression of women's bargaining power. However, similar questions

[37] For example, this might include a close female relative having gotten an urban BPO job. Though we ruled out that the effects for our subsample were due to transfers from other working women, it may be the fact that she works, rather than any money she sends, that affects a woman's bargaining power.

[38] Similar conclusions arise from other surveys asking similar questions. For example, in the 1998/9 National Family Health Survey, 50 to 55 percent of rural women report they do not participate in decisions on their own health care, purchasing jewelry and other items or staying with family or friends.

[39] An exception is that 91 percent of women participated in, or decided on their own, what to cook.

have been used in a number of studies, and have for example been shown to be responsive to factors that can affect women's status or bargaining power (e.g., Jensen and Oster 2009).[40]

Column 3 of Table VI shows the 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 4 and 5 shows 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); we again restrict the sample to cases where no women have enough education for a BPO job and where no one in the household receives transfers. 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, and in fact of mixed signs. While these are only self-reports and may have reporting errors, the results are at least consistent with the possibility that the bargaining power of women who did not get a BPO job, could not get one and did not receive any money from others who did, did not change as a result of the treatment through these other bargaining power channels. Though it is beyond the scope of the paper to test why, this may be due to the fact that bargaining power is a durable feature of household relationships that cannot be changed without a major current-period change such as working and earning income, or just by the fact that a few other women in the village now work[41] or that sometime in the future a daughter might work and then might send money only to her mother. Though again, our measures of bargaining power are crude, and of course we cannot necessarily conclude that bargaining power did not change at all; for example, our measures do not capture the strength of women's participation in decision-making, and it may be that a woman and her husband have always jointly decided on children's schooling and health care, but that her preferences now matter more in the final decision than they did before.

Thus to summarize, there are several pieces of evidence against a role for bargaining power being operative for our restricted set of households. First, the opportunities were not generally available for older women, and in fact we see no employment changes at all among

[40] And in the current data, these measures are statistically significantly correlated with variables such as women's education and labor force participation (though we would not draw a causal inference from this).
[41] Because education levels were so low among adult women at baseline, each village typically had only 2-3 women more working per year as a result of the intervention, which is small relative to the typical village size of about 2,000 people.

mothers in our sample. Second, the effects still hold even in households where no woman (home or away) works for pay, including older siblings. Third, the effects still hold even where no female household member, adult or child, home or away, could have gotten one of the jobs because none have enough education. Fourth, the results still hold where households do not get any transfers, possibly from other female friends or relatives who got one of the jobs. What we can't rule out is that all women's bargaining power was increased by other women in the village working, or that the mother's bargaining power is increased now by the fact that her daughters may work in the future. Though the traditional indicators of women's bargaining power (decision-making and autonomy) do not show gains for women, we cannot rule out that there were gains that our measures are too crude to capture. And again, we are not ruling out that there were gains for girls driven by changes in bargaining power for other households in the treatment villages, such as where, say, an older sibling did get a job; the point of focusing on our restricted sample is to identify a set of households where changes it is unlikely to have played much of a role, and thus make a stronger case for the role of returns.

B. Income or Wealth

Next, we explore whether we can minimize the potential role of changes in income or wealth in explaining the human capital gains for girls. Again, we focus on our restricted sample, since our goal is not to show that income effects are not at play at all in the treatment villages, but simply to find a set of households where they are unlikely to apply. Many of the same tests as above will also be relevant here.

First, we consider the possibility of changes in current income. The results from the top panel of Table V would appear to largely rule out changes in current income earned by the household itself, since these are households where no women (including those who have temporarily or permanently left home) work for pay. And the results showing that there was no change in transfers, and that the human capital gains continue to hold even for those households in our restricted sample that also did not receive any transfers, make it unlikely that the effects in this sample are driven by changes in income through gifts from others who did get a BPO job.

However, this still leaves open the possibility that these households gained more current income because other women who got one of the BPO jobs spent more in the village (or sent home money that others spent in the village), increasing the incomes of others who did not get

BPO jobs. However, there are several pieces of evidence against this possibility. First, we note that any income gains or wage changes were likely to be quite modest relative to the size of the villages. The average village experienced an increase in employment of about 2 to 3 women per year over this period relative to an average village population of about 2,000, so any effects on total village income are likely to be fairly small. And any corresponding increase in money spent in the village would likely have only a very small effect on the income of any given household that did not get a BPO job. Second, Table III showed that the intervention had no effect for boys. If the gains for girls are driven by increases in household income, we would probably expect at least some human capital gains for boys as well. In fact, the literature often finds greater effects of wealth on boys than girls; Table II provides some evidence of that in the present setting. While girls' education may be considered a luxury, so we might expect greater increases for girls than boys in response to wealth increases once the household is beyond a certain threshold, given the diversity of wealth in our sample it seems unlikely that all households just happened to be at the exact wealth level where boys were receiving what the household considered to be the optimal amount of schooling and health, so that all marginal increases in wealth go only to girls' human capital, with no effect at all for boys (especially since boys are still very malnourished by international standards, as seen in Table I, and again, since Table II shows that if anything, marginal changes in wealth in our sample have a bigger effect on investments in boys than girls). Third, we can examine changes in consumption. If there were an increase in household income driven by increased local spending (or underreported transfers) by those who did get jobs, we would expect to see gains in average household expenditures. The final column of Table VI shows regressions for total household expenditure on the treatment indicator, again restricting the sample to those households where no one has a secondary school degree. The coefficient on the treatment is small, and not statistically significant from zero. Though we note there are significant difficulties in measuring expenditures, so we can't definitively rule out these effects.[42] We note that the latter two tests, finding no effects for boys or changes in expenditures, in

[42] One additional possibility these arguments also suggest is unlikely is that women leaving the village for BPO jobs could increase the local wage rate for women who stay in the village. Again, we observe no change in expenditures for the households in our subsample, or any gains for boys. In addition, as noted, the number of women getting a BPO job is small relative to the size of the village, so their departure is unlikely to affect the local wage rate significantly. For a more direct test, we can regress the wages earned by women who work on the treatment indicator. The coefficient is small, negative and not statistically significant (using village-level data on wages for adult women also does not yield evidence of wage gains, though the smaller sample size reduces the power to detect any possible changes).

general serve not just as a test of the possible income effects of spending by others in the village, but as crude, overall tests of any effects of general increases in household income regardless of the source (earned by the household or through others).

It is also possible that households who did not experience an increase in employment or earned income now could borrow against higher lifetime income from children's future employment, and use that to finance investment in girls now. The rural households in our sample are quite poor by international standards,[43] but we have no explicit indicators or tests for the extent to which they are credit constrained. However, we note that many of the same arguments above ruling out current income gains would also make it unlikely the results for our subsample are driven by increases in future or permanent/lifetime income or wealth. First, again, income tends to be spent as least as much on boys as on girls (if not more), yet there are no gains for boys in Table III. Second, if households could borrow against the future income of their children to finance current consumption, we would expect to see changes in current expenditures, which was not observed in Table VI; though again, measuring expenditures is difficult. Though this is far from a direct test for whether households are able to borrow against this future income, these results are at least consistent with households being highly credit constrained, perhaps particularly in terms of borrowing against income gains that in most cases are both uncertain and will only be realized many years in the future.

C. Other Mechanisms

We note that the arguments above also provide evidence against other mechanisms that operate via mother's or other women's employment being operative in our restricted subsample. For example, in households where the mother has not and could not get a job in the BPO sector, it would appear unlikely that the gains for girls are driven by reductions in their mother's fertility.[44] Similarly, in households where no female member works for pay, it is unlikely the results are driven by the fact that when the mother, an older sister or any other female household

[43] At baseline, about 19 percent of our sample falls below the Government of India's state-specific rural poverty lines, which is slightly below the Planning Commission's 2004/5 all-India rural poverty estimate of 22 percent. However, our survey contains a different level of consumption detail and recall periods than the data used for estimating the official poverty rates, so we make this comparison with caution.

[44] Our survey also asked about fertility intentions for all women in the household. We find that the treatment did not reduce either the intended timing or desired number of additional children for mothers of the children aged 7-15 in our sample.

member works, the value of younger girls to the household is increased because they take over more household activities.

Thus, overall, while there is no test that can rule out every conceivable alternative, most of the primary alternative explanations that have been put forward in the literature (bargaining power, income, fertility or household time allocation) appear less likely to apply to our restricted sample, subject to the caveats and limitations of our tests discussed above. Though certainly not conclusive, we believe our results allow us to better exclude the likelihood of these other mechanisms, and provide a better test of the returns mechanism, than has been possible in previous studies.

VI. 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 we are able to provide more evidence of one particular underlying mechanism, returns, than previous studies have been able to. While not ruling out that mechanisms other than returns may matter, testing the role of returns as both an underlying cause of the large gender differences in human capital and as a potential area for policy interventions, is valuable in itself. Additionally, showing this mechanism plays a role is important because if the lesson of the large literature linking women's employment and investment in girls is that it is all driven by bargaining power, fertility, etc., there may be other policy instruments available (e.g., reform or enforcement of divorce laws, strengthening women's inheritance or property rights, family planning services, etc.) that also address these factors, and that may perhaps be more direct or effective. Another way to view this is that our results show that it is possible to have gains for girls without first needing to change bargaining power, income or fertility.

Our intervention did not create any new jobs for women, and thus we do not view recruiting as a solution to the problem of gender disparities in human capital. Rather, the value of our results is in demonstrating that human capital investments in girls respond to their future economic opportunities. Many approaches to the dramatic gender differences in outcomes by governments, NGO's, rights groups and international organizations have emphasized the social or cultural component of bias (Croll 2001). While there is certainly some role for such efforts,

our results suggest that expanding economic opportunities for women, which should involve a focus on the private sector, can play an important role in reducing gender disparities in human capital. Public policy instruments include enforcement of anti-discrimination laws in hiring and promotion, and ensuring equality in public sector employment and promotions. Other instruments include 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 programs, so women can engage in more entrepreneurial activities (though Field, Jayachandran and Pande 2010 discuss possible barriers to this kind of activity for women).

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 white-collar service sector more generally, has been 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 female labor and a corresponding increase in female labor force participation, as has been observed in other countries (Goldin 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.

Finally, the results also show that there is a demand side limitation to investment in girls for at least some households. For these households, the limiting factor in investment in girls is not related to poverty, or access, costs, distance or quality of schooling, nutrition or health care. Though these other factors are certainly important, at least some of the problem was that parents saw little value to investing in girls. But clear and salient evidence of greater opportunities for women can lead to gains for girls even without changes in any of these other factors.

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

	(1) All	(2) Control	(3) Treatment	(4) Difference (3) − (2)
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	5.46	5.42	5.49	0.072
	[2.37]	[2.41]	[2.32]	(0.084)
In school: girls 7-15	0.74	0.75	0.74	-0.011
	[0.44]	[0.43]	[0.44]	(0.020)
In school: boys 7-15	0.87	0.87	0.86	-0.010
	[0.34]	[0.34]	[0.35]	(0.015)
BMI-for-age (z-score): girls 5-15	-1.25	-1.25	-1.26	-0.015
	[1.35]	[1.38]	[1.33]	(0.057)
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: Baseline values for key variables, collected in the Round 1 survey (September-October, 2003). Standard deviations in brackets in columns 1-3; heteroskedasticity-consistent standard errors accounting for clustering in parentheses in column 4. The last column contains *t*-tests of the difference in means between the control and the treatment samples. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

TABLE II. EFFECT OF THE INTERVENTION ON PAID EMPLOYMENT

	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

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the village-level in parentheses. All variables measured in round 2 survey. Dependent variable is an indicator for whether an individual was in paid employment in the round 2, post-treatment 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

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	0.052***	0.24***	0.063	0.053***	0.23***	0.060	0.051***	0.27***	0.051
	(0.019)	(0.070)	(0.066)	(0.019)	(0.069)	(0.063)	(0.21)	(0.051)	(0.051)
log (expend per cap)				0.025	0.11*	0.147**			
				(0.018)	(0.058)	(0.057)			
Head's Education				0.005*	0.017	0.008			
				(0.003)	(0.012)	(0.009)			
Spouse's Education				0.008**	-0.033*	0.040			
				(0.004)	(0.018)	(0.014)			
Family Size				0.002	-0.014	0.030**			
				(0.004)	(0.014)	(0.013)			
Child's Age				-0.002	-0.038***	0.265***			
				(0.004)	(0.011)	(0.092)			
R^2	0.004	0.007	0.001	0.016	0.017	0.06	0.010	0.012	0.005
Observations	1,830	2,031	2,031	1,830	2,031	2,031	1,265	1,556	1,556
Control Group Mean	0.75	-1.25	-2.02	0.75	-1.25	-2.02	0.76	-1.27	-2.05
Mean Effect		0.11***			0.11***			0.12***	
		(0.027)			(0.025)			(0.024)	
F-stat		7.40			7.62			9.81	
(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	-0.011	-0.020	0.005	-0.013	-0.030	-0.007	-0.007	0.083	-0.018
	(0.013)	(0.076)	(0.052)	(0.013)	(0.074)	(0.049)	(0.017)	(0.068)	(0.046)
log (expend per cap)				0.033***	0.064	0.206***			
				(0.013)	(0.061)	(0.051)			
Head's Education				0.003	0.008	0.008			
				(0.002)	(0.011)	(0.009)			
Spouse's Education				0.004	0.013	0.018			
				(0.003)	(0.014)	(0.014)			
Family Size				0.000	0.001	0.017			
				(0.003)	(0.012)	(0.012)			
Child's Age				-0.011***	-0.102***	-0.087***			
				(0.003)	(0.011)	(0.008)			
R^2	0.00	0.00	0.00	0.017	0.041	0.065	0.001	0.001	0.00
Observations	2,013	2,295	2,295	2,013	2,295	2,295	1,385	1,735	1,735
Control Group Mean	0.87	-1.29	-1.99	0.87	-1.29	-1.99	0.87	-1.32	-1.96
Mean Effect		-0.012			-0.019			0.007	
		(0.023)			(0.021)			(0.021)	
F-stat		0.28			0.44			0.53	
(p-value)		0.84			0.72			0.67	

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	Enrolled	BMI-for-Age	Height-for-Age
Treatment	0.037*	0.15**	-0.015	0.039**	0.14**	-0.013	0.028	0.160**	0.011
	(0.019)	(0.068)	(0.062)	(0.019)	(0.068)	(0.060)	(0.023)	(0.064)	(0.049)
log (expend per cap)				0.023	0.101*	0.140**			
				(0.018)	(0.057)	(0.055)			
Head's Education				0.006**	0.015	0.006			
				(0.003)	(0.011)	(0.009)			
Spouse's Education				0.009**	-0.032*	0.037***			
				(0.004)	(0.017)	(0.014)			
Family Size				0.004	-0.011	0.032**			
				(0.004)	(0.013)	(0.012)			
Child's Age				-0.003	-0.041***	-0.083***			
				(0.004)	(0.010)	(0.009)			
R^2	0.002	0.003	0.00	0.016	0.013	0.058	0.001	0.004	0.00
Observations	1,854	2,124	2,191	1,854	2,124	2,191	1,282	1,674	1,649
Mean Effect		0.059**			0.060**			0.070**	
		(0.026)			(0.025)			(0.027)	
F-stat		2.88			3.01			3.07	
(p-value)		0.038			0.032			0.030	

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	-0.029**	-0.09	-0.061	-0.030**	-0.097	-0.066	-0.021	-0.029	-0.064
	(0.013)	(0.073)	(0.050)	(0.013)	(0.072)	(0.047)	(.017)	(0.064)	(0.044)
log (expend per cap)				0.028**	0.063	0.196***			
				(0.013)	(0.059)	(0.049)			
Head's Education				0.002	0.008	0.008			
				(0.002)	(0.010)	(0.008)			
Spouse's Education				0.005	0.013	0.018			
				(0.004)	(0.014)	(0.013)			
Family Size				0.002	0.002	0.018			
				(0.003)	(0.012)	(0.012)			
Child's Age				-0.01***	-0.10***	-0.09***			
				(0.003)	(0.011)	(0.008)			
R^2	0.002	0.001	0.001	0.017	0.042	0.066	0.001	0.001	0.001
Observations	2,047	2,388	2,388	2,047	2,388	2,388	1,403	1,851	1,828
Mean Effect		-0.061***			-0.066***			-0.049**	
		(0.023)			(0.021)			(0.023)	
F-stat		3.10			3.95			1.62	
(p-value)		0.028			0.009			0.19	

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: EXCLUDING ALTERNATE MECHANISMS

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	0.056***	0.19**	0.027	0.060***	0.18**	0.032	0.059**	0.23***	0.060
	(0.021)	(0.075)	(0.072)	(0.021)	(0.074)	(0.070)	(0.26)	(0.079)	(0.059)
R^2	0.005	0.005	0.001	0.014	0.017	0.053	0.005	0.008	0.001
Observations	1,353	1,501	1,501	1,353	1,501	1,501	935	1,156	1,156
Mean Effect		0.094***			0.098***			0.13***	
		(0.031)			(0.029)			(0.030)	
F-stat		4.37			4.94			5.96	
(p-value)		0.006			0.002			0.001	

NO WOMEN W/ 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	0.051***	0.23***	0.07	0.051***	0.22***	0.065	0.050***	0.24***	0.068
	(0.020)	(0.072)	(0.067)	(0.019)	(0.071)	(0.065)	(0.22)	(0.070)	(0.052)
R^2	0.004	0.007	0.001	0.014	0.015	0.054	0.004	0.009	0.001
Observations	1,676	1,851	1,851	1,676	1,851	1,851	1,192	1,426	1,426
Mean Effect		0.11***			0.11***			0.12***	
		(0.027)			(0.026)			(0.025)	
F-stat		6.90			7.04			9.03	
(p-value)		0.000			0.000			0.000	

NO TRANSFERS	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	0.050***	0.21***	0.040	0.051***	0.20***	0.086	0.048**	0.22***	0.088
	(0.021)	(0.075)	(0.071)	(0.022)	(0.077)	(0.068)	(0.024)	(0.077)	(0.055)
R^2	0.004	0.003	0.000	0.016	0.012	0.048	0.002	0.008	0.002
Observations	1,444	1,603	1,603	1,444	1,603	1,603	1,026	1,228	1,228
Mean Effect		0.12***			0.12***			0.11***	
		(0.028)			(0.022)			(0.027)	
F-stat		7.06			6.94			7.64	
(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 log expenditure per capita, head's and spouse's education, family size, child age, and 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. TESTING FOR ALTERNATIVE MECHANISMS

	(1)	(2)	(3)	(4)	(5)	(6)
	Receives Transfers	Amount Received	Schooling/ Health Decisions? (0-2)	Decision-Making (women>18, 0-10)	Autonomy (women>18, 0-3)	Expenditure per capita
Treatment	0.009	5.86	-0.010	-0.071	0.112	-21.2
	(0.012)	(9.94)	(0.023)	(0.184)	(0.347)	(32.1)
R^2	0.000	0.001	0.001	0.001	0.005	0.000
Observations	2,985	421	3,345	3,334	3,415	2,554
Control Group Mean	0.14	93.1	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 Column 1 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 Column 2 it is the amount of the transfer. Column 3 is whether the woman participates in household decisions on children's schooling and health care, and Column 4 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 5 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; and is allowed to keep money to spend on her own as she chooses. The dependent variable in Columns 2 and 6 are in year 2006 Rupees. *Significant at 10 percent level. **Significant at 5 percent level. ***Significant at 1 percent level.