NO PLACE LIKE HOME: LONG-RUN IMPACTS OF EARLY CHILD HEALTH AND FAMILY PLANNING ON LABOR AND MIGRATION OUTCOMES

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This paper examines the long-term effects of early childhood health interventions, such as vaccination and family planning, on adult labor market and migration outcomes in Bangladesh. Intent-to-treat effects show men born when intensive child health services and family planning were available worked in more professional/semi-professional and entrepreneurial occupations that required more academic skills but migrated less domestically leaving average annual income unaffected. Similarly aged eligible women also engaged more in entrepreneurial paid work. Spillover effects on an older cohort born when only family planning was available show these men migrated less internationally leading to lower annual earning.

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I. INTRODUCTION

Preventative child health interventions such as vaccination and family planning are lauded as part of the ten great public health achievements of the last century (CDC 1999). Not only have these interventions led to reductions in contagious diseases and smaller families, theory predicts they can improve well-being in adulthood through their effect on human capital and labor market opportunities (Heckman 2007, Strauss & Thomas 2008). However, returns to human capital in the labor market may depend on the role of migration especially in contexts where employment opportunities are contingent on migration (Beagle, Weerdt, Dercon 2011; Clemens 2011; Bryan, Chowdhury, Mobarak, 2014). Moving for work can impose significant monetary and non-monetary costs (e.g. poor working conditions or job quality and time away from family) that affect welfare (Imbert and Papp 2019) and workers may choose lower wages in the place of origin in order to avoid these costs. Given the importance placed on both child health interventions and migration as pathways to development and the scale of migration worldwide, understanding the long-term effects of improved early child circumstances jointly on earnings, job quality, and work-migration is imperative.

A growing number of studies provide causal evidence of the effect of positive health shocks in early childhood on human capital and adult earnings (Currie and Vogl 2013; Almond, Currie and Duque 2018), but few are able to examine migration patterns and job quality. While earnings typically increase in human capital, theoretical models of labor supply, migration, and return migration predict an ambiguous effect of human capital on migration (Wahba 2014; Dustmann and Görlach 2016), leaving the long-run effects on labor market outcomes an empirical question. For example, potential migrants choose where to work to maximize welfare and may opt to stay home or return early, even forgoing higher earnings, if improved human capital leads to better quality or less risky work and reduces the time away from family in the origin relative to the destination area. Currently, there is a lack of long-run causal evidence on the joint effects of earnings, job quality and migration owing to the difficulties in tracking individuals over decades and across countries, small sample sizes, and limited outcome data.

This paper examines the effects of early childhood health investments on adult labor market and migration outcomes. We take advantage of quasi-random variation in eligibility for the Maternal and Child Health and Family Planning Program (MCH-FP) in the Matlab subdistrict of Bangladesh. The MCH-FP interventions were phased-in starting with family planning in 1977 and intensive child health interventions in 1982. Health interventions included vaccinations against debilitating diseases such as measles, tetanus, pertussis, polio, and tuberculosis that can affect child development, nutrition, and health. Treatment and comparison areas were built into the design of the MCH-FP program and were placed in contiguous geographic areas (Figure 1) that were economically and socially similar. The block design was critical for minimizing the potential spillover from vaccination and information sharing. Similar interventions became available in the comparison areas in 1988, providing an approximately 10-year evaluation window.

We exploit the quasi-random program placement to estimate intent-to-treat (ITT) effects using single-difference models with birth-year fixed effects and pre-program controls.[1] We demonstrate that the comparison area provides a good counterfactual: there is pre-program balance in employment, migration trends, human capital, fertility, and individual and household characteristics; and no pre-existing differences in father's labor market outcomes, or labor market outcomes of a similarly-aged cohort using data from an earlier period. Double-difference models using an older pre-program cohort are often used to control for potential baseline differences. Given the treatment and comparison areas are balanced at baseline and the MCH-FP program may have affected the labor market outcomes of individuals born before program rollout, potentially biasing results in an unknown direction, we present the double-difference model as a robustness check. Results are similar. Furthermore, we show results are also robust to the quasi-random block design, including a wild cluster bootstrap to account for the smaller number of clusters and randomization-based inference adjusting inference for the placement of the treatment area across a contiguous group of villages by permuting the assignment of villages to treatment.

Based on the rollout of the MCH-FP program over time, we estimate program effects for two cohorts: individuals born when both intensive child health and family planning interventions were available (1982–1988); and those born when the family planning interventions were available, but prior to the intensive child health interventions (1977–1981). The two cohorts of interest are approximately aged 24–30 and 31–34 at the time of survey, respectively, and are henceforth referred to by these age ranges. The ITT effect for the 24–30 cohort captures the combined effects of the family planning and child health interventions, while the ITT effect for the 31–34 cohort captures the direct effect of the family planning intervention combined with any indirect effects of having younger siblings with potentially higher human capital.

[1] There is no pre-analysis plan for this research as the data collection for this paper was conceived prior to their use in economics. MHSS2 was designed and collected by the authors to study the long-term effects of MCH-FP. Descriptions of the planned research design and analysis are in grant applications for data collection.

Long-term outcomes are drawn from a large socio-economic survey collected by the authors, the Matlab Health and Socioeconomic Survey 2 (MHSS2). MHSS2 links respondents to pre-program census data, over 30 years of monthly demographic surveillance data (e.g., migration, births, deaths, household composition, and location), and data on potentially confounding programs. The rich panel data allow us to develop an exogenous measure of treatment and examine labor market dynamics in more depth including migration location and patterns throughout the life course. Additionally, MHSS2 has less than 9 percent attrition in both the treatment and comparison areas, which is low given that the data were collected 35 years after program start and that more than 60 percent of prime-aged men migrate, a quarter of whom go to international destinations. The low attrition rate reduces concern of attrition bias caused by migration for work that plagues long-term effect studies on labor market outcomes. The panel data also allows us to use weights to address the remaining attrition.

This paper builds on research that demonstrates the MCH-FP had meaningful effects on human capital and family size. During childhood, the 24–30 cohort experienced improved height (0.22 SD), cognition (0.39 SD), and education (0.17 SD) when they were ages 8–14, but there were few effects on human capital for the 31–34 cohort (Barham 2012).[2] A companion paper also using MHSS2 (Barham et al. 2021b) demonstrates that many of these improvements persisted into adulthood for the 24–30 cohort and that the 31–34 cohort remained largely unaffected. Treatment area men and women in the 24–30 cohort experienced about a one-centimeter increase in height and men had better education outcomes (0.82 increase in years of education and 0.2 standard deviation increase in a math test).[3] Finally, the program successfully reduced the completed fertility of older women by between 0.5 and 0.67 births depending on a women's length of exposure to the program (Barham et al. 2021a).

Our findings show that men in the 24–30 cohort experienced improved labor market outcomes and were less likely to migrate domestically. Relative to similarly aged men in the comparison area, they had better quality jobs in that they were more likely to work in professional/semi-professional occupations, used more academic skills on the job, and were more entrepreneurial in that they were more likely to be self-employed

[2] Using a different research designs and treatment variables, Joshi and Schultz (2013) also show the MCH-FP program increased schooling for boys and Driessen et al. (2015) demonstrate that improvements in schooling are associated with measles vaccination take-up, consistent with Barham (2012).
[3] The lack of effect on education for women is expected given a secondary school stipend program for females was available in both the treatment and comparison areas to the 24–30 and 31–34 cohorts during their schooling years.

and take out business loans. We find no differences in annual earnings despite a 23 percent reduction in migration for work to urban areas within Bangladesh where wages are higher on average. There was no difference in international migration, where wages are significantly higher. Given earnings were similar between treatment and comparison individuals, but the disutility from migration lower on average, overall welfare may have improved. Examination of mechanisms suggests that human capital may be one factor that facilitated employment in more professional/semi-professional occupations.

Men in the 31–34 cohort also experienced a reduction in migration, but to international destinations. This was accompanied by a 23 percent reduction in annual earnings. The lower international migration rates reflect both lower rates of ever working abroad and shorter migration spells. The reduction in migration is associated with both human capital and the number of younger brothers. While the point estimates are qualitatively similar across most robustness checks, this cohort is smaller than the younger cohort, and there is a loss of statistical significance in the double-difference model or when standard errors are adjusted for multiple hypothesis testing.

Program effects for women are limited since only 30 percent work for pay or migrate for work. Women in the 24–30 cohort are more likely to be entrepreneurial and engage in paid agricultural work raising small animals, but there is no effect on annual income. Women in the 31–34 cohort were mainly unaffected.

This paper contributes to the literature on the long-term effects on labor and migration outcomes of programs designed to improve health and nutrition in early childhood. There are few experiments that follow participants for more than two decades, with two notable exceptions being Hoddinott et al. (2008) and Gertler et al. (2014). Hoddinott et al. (2008) find a 46 percent increase in hourly wages among the 60 percent of men they followed who were eligible for a protein-enriched beverage prior to age three in rural Guatemala. Gertler et al. (2014) demonstrate that a randomized control trial of 129 stunted 9–24 month-old children in Jamaica had no effect on earnings for a nutrition only group, but a 25 percent increase in earnings for those that additionally received psychosocial stimulation.[4]

We also complement quasi-experimental research that examines the longer-run effects of other health, anti-poverty, and family planning programs in the US. Bleakley (2007, 2010) examines the eradication of

[4] Hamory et al. (2021) examine the longer-term effects of a randomized deworming program in rural Kenya, where health investments were made later in life during school rather than the critical early childhood period of development. They find no effect on annual earnings, though men are more likely to live in an urban area.

malaria and hookworm and Bhalotra and Venkataramani (2015) the introduction of antibiotics in the US in the first half of the 20th century. These papers compare people across states with varying levels of pre-intervention incidence rates and find income proxies or family income increase 1.5–43 percent. Research on Medicaid (Brown, Kowalski, and Lurie 2020), Head Start (Deming 2009, Thompson 2018) and SNAP (Hoynes, Schanzenbach and Almond 2016) show positive earnings effects for some subgroups. Finally, Bailey (2013) shows a mother's access to contraceptives is associated with an increase in her male children's wage earnings.

This paper makes several advances to the literature. To our knowledge this is one of the only papers to rigorously explore how programs designed to improve health and human capital affect migration patterns over the life cycle. Our study highlights the effects on migration, and in turn earnings, may not be as anticipated. However, results for the 24–30 cohort are consistent with research that demonstrates that investments in human capital may substitute with labor migration (McKenzie and Rapoport 2011) since the returns to human capital or welfare may be higher if the beneficiary stays at home, especially once job quality and financial and psychosocial costs of migration are taken into consideration.

Unlike previous studies, we include a cohort born prior to the availability of the child health interventions, but when family planning was available (the 31–34 cohort), to examine spillovers onto siblings. The differing pattern of results across cohorts highlights that programs that improve human capital for some may have unintended consequences on other family members. The reduction in international migration for the 31–34 cohort who did not experience human capital gains is consistent with Jensen and Miller (2017) who find that parents may strategically invest less in the education of children they would like to remain home and provide help.

The study also benefits from low attrition rates and rich data that uniquely enables us to provide a rigorous and more complete understanding of the long-term impact on labor and migration outcomes. While attrition plagues most long-term effect studies, it is particularly problematic when the main cause of attrition is correlated with the outcomes. In this paper and similar research, a major source of attrition is migration for work, but migration also affects earnings and other labor market outcomes, potentially biasing results in an unknown direction. Had we not followed migrants outside of the study area, we would have found positive income effects.

Finally, while it is difficult to separate out the effects of family planning from the child health interventions, these interventions are commonly provided together and interventions that improve child health alone often lead to reduced family sizes (Sah 1991).

II. The MCH-FP Program and Mechanisms

A. The Intervention, Program Placement and Take-Up

The MCH-FP program was initiated in a rural area of Bangladesh, Matlab, in October 1977 by icddr,b (formerly known as International Centre for Diarrhoeal Disease Research, Bangladesh). It started as a demonstration project to help the government design a national family planning program and became an example for other nations. Treatment and comparison areas were built into the design of the program (Fauveau 1994) and covered about 200,000 people in 149 villages, with the population split evenly between the two areas. The program was placed in a block of contiguous villages, with a block of comparison villages on two sides of the treatment area (Figure 1). The block design was intended to reduce potential contamination of the comparison area from information about the family planning interventions (Huber and Khan 1979) and spillovers from positive externalities generated by vaccination. The comparison villages were viewed as socially and economically similar and geographically insulated from outside influences (Phillips et al. 1982). We refer to the placement of this intervention as *quasi-random* and draw further support for our identification strategy from the evidence shown in Section IV-A of pre-program similarities between treatment and comparison areas.

The program included integrated family planning and maternal and child health services. Interventions were administered in the home free of charge during monthly visits by local female health workers. In the comparison area, then-standard government health and family planning services were available, but family planning services were only available at clinics, not in the home, and childhood vaccinations were not readily available until 1989 or later, providing an experimental period, 1978–1988, to evaluate the program.

Services rolled out over two main periods: 1977–1981 and 1982–1988 (Table A1). Before 1982, the focus was family planning and maternal health through the in-home delivery of modern contraception, tetanus toxoid vaccinations for pregnant women, and iron and folic acid tablets for women in the last trimester of pregnancy (Bhatia et al. 1980). Female health workers provided counseling on contraceptives, nutrition, hygiene, and breastfeeding; motivated contraceptive use; and instructed them in how to prepare an oral rehydration solution. Follow-up and referral systems were available to manage side effects and continued use of contraceptives (Fauveau 1994). Tetanus toxoid immunization was expanded to all women of reproductive age, and safe delivery kits were provided after 1985.

Between 1982 and 1988, the program included more health interventions for children under age five, beginning with the provision of the measles vaccine in half the treatment area. In 1985, the measles vaccine was expanded to the entire treatment area and preventive services expanded to include DPT, polio, and tuberculosis vaccines and vitamin A supplementation. Curative care, such as nutrition rehabilitation and acute care for respiratory infections, was introduced in the late 1980s. The increase in vaccination led to a 36 percent reduction in measles mortality among the treated after the introduction of the measles vaccine in the treatment area (Koenig et al. 1991).

Differences between the treatment area and the rest of the country, including the comparison area, narrowed after 1988 as the lessons of the Matlab success were incorporated into the national plan (Cleland et al. 1994). The number of family welfare assistants to deliver in-home contraceptive and immunization services increased throughout the country through government services.

Program implementation followed the planned timeline, and uptake was rapid. The measles vaccination rate rose to 60 percent in 1982 after it was introduced in half of the treatment area, and the other half in 1985 (Figure 2). By 1988, coverage rates for children aged 12–23 months living in the treatment area were 93 percent for the BCG vaccine against tuberculosis, 83 percent for all three doses of DPT and polio, 88 percent for measles, and 77 percent for all three major immunizations (icddr,b 2007). Vaccination rates in the comparison area were not measured but are expected to be zero. Government measles vaccination started around 1989 leaving the comparison area unvaccinated during the evaluation window (Koenig et al. 1991). Nationally, measles vaccination for children under the age of five was less than 2 percent in 1986 (Khan and Yoder 1998) and below 40 percent in the comparison area in 1990 (Fauveau 1994).

Prior to the program, the contraceptive prevalence rate (CPR) for married women 15–49 was low (< 6 percent) in both the treatment and comparison areas (Figure 2). The CPR reached 30 percent in the treatment area in the first year, then rose steadily, reaching almost 50 percent by 1988. Because contraceptives were also provided by the government, the CPR increased in the comparison area, but not as quickly, and remained below 20 percent in 1988. By 1990, there was still a 20 percentage point difference in the CPR rate between the two areas.

B. *Pathways from Human Capital and Sibship Size to Labor and Migration Outcomes*

In this section, we describe some of the potential pathways linking early child health and family planning programs to adult labor and migration outcomes, including increased human capital, thinner migration networks, and reduced sibship size. We build on Barham (2012) who provides a detailed discussion of how the various program interventions affect human capital development. In Section VI, we empirically examine the effect of the program on each of the mechanisms and use descriptive approaches to examine the effect of the mechanisms on labor and migration outcomes.

Models of health over the life cycle describe a positive link between early child health interventions, improved human capital, and earnings (Strauss and Thomas 2007; Heckman 2007; Heckman, Stixurd, Urzula 2006), as educational attainment, cognitive and non-cognitive skills, and adult health are important components of productivity and labor supply. However, the effect of human capital on earnings is also influenced by its effect on migration. Many studies observe migration to be increasing in human capital, but the potential effects of a shock to human capital on migration and return-migration are ambiguous (Wahba 2014; Dustman and Görlach 2016). Indeed, migration rates could be decreasing in human capital if, for example, it allowed employment in a higher-skilled professional/semi-professional job or self-employment in the origin area relative to the destination area. Such a situation might arise if the destination area involved riskier manual labor jobs, such as construction or rickshaw pulling, as is commonly the case of urban areas of Bangladesh. Even if earnings were lower in the origin area, workers may choose to remain home if welfare is higher due to improved job quality or foregone migration costs (both monetary and non-monetary). In addition, earnings trajectories could be higher from working in higher quality local jobs. This type of substitution may be more likely in contexts where the increase in wages in the destination area from human capital gains is relatively small and when migration entails solo migration for lower-skilled/riskier work and the migrant's families is left behind in the origin area.

Increased income from higher human capital in the destination also affects migration through the decision of when or if to return home. If migrants are target savers, increased income in the destination area may allow migrants to return earlier. While an increase in the destination area wage increases the marginal value of staying at the destination (relative wage effect), the marginal utility of consumption at the destination relative to home is lower owning to the fact that there are costs of being away from one's family.

Early childhood interventions may also affect a child's migration networks. The existing literature on placed-based interventions and migration generally points to modest net migration towards areas with greater amenities from interventions due to the value that individuals place on these services (Gelbach 2004; McKinnish 2005). Barham and Kuhn (2014) document modest short-term reductions in out-migration in the treatment area in Matlab immediately following the program rollout between 1979–1991. Reduced out-migration from areas with the intervention could reduce the stock of migration-specific social capital within families and networks, leading to thinner migration networks when the children who benefited from the MCH-FP interventions become adults.

Finally, sibship size may also positively or negatively affect migration. A smaller family could lead to higher migration rates as the family has more per-child resources to support migration. In addition, with smaller family sizes, there is a higher probability that any one child will have to migrate to help the family, perhaps to diversify income across location (Rosenzweig and Stark 1989) or to support other siblings (e.g., education costs). On the other hand, smaller family sizes could lead to lower migration rates if an adult child is required to work in the family business or farm, take care of very young or older family members, or because the need to migrate to support other siblings is lower if there are fewer younger siblings. In addition, which adult child the family chooses to remain in Matlab may depend on their relative human capital, birth order, or social skills. If this is the case, there may be differences in migration rates between the cohorts given the program led to differences in human capital between the two cohorts.

III. Data

This paper draws on the rich data available for the Matlab study area and includes four main data sources: the 2012–2014 Matlab Health and Socioeconomic Survey wave 2 (MHSS2),[5] the 1996 Matlab Health and Socioeconomic Survey wave 1 (MHSS1) (Rahman et al. 1999), periodic censuses conducted by icddr,b in 1974 and 1982, and 1974–2014 Matlab demographic surveillance site (DSS) data on vital events

[5] MHSS2 was designed and collected by the authors, with a team of researchers, and will be made publicly available along with some of the icddr,b data that the authors link to in this paper. Specific icddr,b census and DSS data must be requested from the organization. More information can be found at http://www.icddrb.org/component/content/article/10003-datapolicies/1893-data-policies.

(e.g., births, marriages, deaths, in and out migrations) collected by icddr,b. In addition, we include data from a variety of organizations to control for potentially confounding programs (Appendix C). See Appendix B for additional information on the data sources and the construction of key variables.

MHSS1 and MHSS2 are random samples of the study area, while the census and DSS data cover the entire study area. A key feature of these data is that individuals can be linked across the data sources by a unique individual identifier, allowing the creation of a panel of individuals from the Matlab area over the past thirty-five years. In addition, the census and DSS data are known for their high quality as they were collected bi-weekly or monthly, and allow determination of exact birthdates and treatment status, creation of indicators of migration by specific time periods or ages, and testing of pre-program balance. There are few, if any, other study sites that have similarly rich data availability to allow this type of long-term evaluation. The remainder of this section provides a brief description of MHSS2 including rates of attrition and attrition balance, the analysis sample, and the intent-to-treat indicator.

MHSS2. Outcome variables are from MHSS2 unless otherwise stated. MHSS2 is a panel followup of all individuals in the MHSS1 primary sample, their descendants, plus any individuals born to an MHSS1 household member who migrated out of Matlab between 1972 (5 years prior to the start of the program) and 1989. The MHSS1 primary sample is representative of the study area's 1996 population, and the inclusion of pre-MHSS1 migrants in the MHSS2 sampling frame addresses the concern that the MHSS1 primary sample by itself does not include individuals who migrated between program start and 1996.

MHSS2 was conducted between 2012 and 2014 and has low attrition rates with the loss of less than 10 percent of the target sample. Respondents were tracked throughout Bangladesh and intensive efforts were made to interview international migrants and difficult to track migrants when they returned to the study area to visit family, especially during Eid celebrations. Most data were collected in face-to-face interviews, so are not proxy reports. Fifteen percent of men in our sample, international migrants living abroad, were surveyed using a phone survey. For these men, sample sizes are smaller for some variables, but not key labor market and migration outcomes.

The outcomes for this paper are primarily from individual responses to employment and migration histories for each individual older than fifteen at the time of the survey. Data on earnings, hours worked, and occupation were collected using a matrix organized by type of employment aimed at improving recall of all jobs over a 12-month period, including secondary and unpaid family employment. Data were also collected on current residential location and residence location histories from 2008–2012 to allow the construction of historical migration, though high-quality migration histories are also available in the DSS.

Analysis Sample and Attrition. The main analysis sample includes all individuals born during the experimental period from October 1977 to December 1988 (the 24–30 and 31–34 cohorts) who were randomly selected for individual interviews in an MHSS1 primary household or were a pre-MHSS1 migrant. Including death and any other type of non-response, the attrition rate for household variables is 7 percent and is similar between treatment and comparison areas. Attrition rates are 8.9 percent for men and 7.8 percent for women for employment and migration variables and are 9.6 percent for men and 8.1 percent for women for annual earnings (Table B1). These are low attrition rates compared to other long-term effects studies with shorter follow-up periods,[6] despite a migration rate of approximately 60 percent for men (25 percent international) in this highly mobile population. Most labor and employment outcomes were included in the phone survey, but outcomes such as consumption were not. Without the phone survey, the attrition rate is higher for men at almost 24 percent, but the same for women, because women do not migrate internationally for work. Table B2 shows there is no differential attrition between the treatment areas based on baseline characteristics for the men and women together or men separately, and only one characteristic that differs at the 5 percent level for women.

After attrition, the sample of men and women is 1,299 (588 in treatment) and 1,220 (553 in treatment) respectively. The 24–30 cohort has 838 men and 811 women. Sample sizes area half the size for the 31–34 cohort at 461 men and 409 women.

Intent-to-Treat and Baseline Variables. Access to the MCH-FP program was based on the village of residence. Because our sample was born after the start of the program, even the residence in which they are born into could be endogenous. We exploit the DSS and census data to create an intent-to-treat indicator by tracing the individual's first household head back to their 1974 village of residence to determine eligibility status (Appendix B).[7] The intent-to-treat variable, *Treat*, takes the value of 1 if the 1974 census-linked household head was living in a village in the treatment area in 1974 or migrated into a village in the

[6] For example, attrition rates after 20 years are 17 percent in Gertler et al. (2014) and 16 percent after 10–20 years in Hamory et al. (2021).

[7] The treatment variable is similar if, instead of tracing back the first household, we follow fathers (0.5 percent differences) or mothers (5 percent differences). However, parental information is not available for all observations, whereas initial household heads are observed for everyone.

treatment area from outside Matlab between 1974 and 1977, and 0 otherwise. Baseline characteristics from the 1974 census are linked to individuals through the census-linked household head.

IV. Estimation Strategy

A. Baseline Balance and Trends

Previous research demonstrates the treatment and comparison areas were similar prior to the program across several important dimensions including mortality rates, fertility rates, and preintervention household and household head characteristics (Koenig et al. 1990; Menken and Phillips 1990; Joshi and Schultz 2013; Barham 2012). In addition, Barham and Kuhn (2014) provide evidence that migration stocks and flows were similar between the treatment and comparison area at the start of the program through to 1982, for a cohort of individuals at risk of migration at the start of the program, showing good baseline balance for the migration outcomes. Barham (2012) also shows that cognitive functioning, height, and education were similar across the treatment and comparison areas in 1996 for those who were old enough that their human capital measures, such as height, were not likely to have been affected by the program.

We further explore the baseline balance and early trends of available labor market outcomes for antecedent households of our sample using the 1974 and 1982 censuses. Table 1 presents the likelihood that antecedent household heads had any paid work and worked in one of three main job categories (agriculture, fishing or boating, and business and skilled service). The table reports statistical significance of the differences in means between the treatment and comparison area and normalized differences in means (difference in the means divided by the standard deviation of the mean for the comparison group) because it is not influenced by sample size (Imbens and Wooldridge 2009). Employment and occupation rates were similar prior to the program in 1974, but also in 1982, indicating parallel trends prior to the introduction of intensive child health interventions in 1982.

Additionally, in Table 1, we test the baseline balance for a variety of other socio-economic variables using 1974 data. The areas are indeed similar across the range of variables in Table 1; 4 out of the 29 individual and household characteristics show differences significant below the 5 percent level, however, all but religion and tubewell access have normalized differences of 0.15 or less. The difference in tubewell water is the result of a government and UNICEF program and is exogenous to household behaviors that could be correlated with a person's propensity to migrate or ability to work. Household head and spouse's age are also statistically different. This is likely a program effect because family planning decreased family sizes and increased birth intervals for the treatment cohort in this study. Indeed, if we use the sample born between 1947 and 1972, the difference in means for household head and spouse age are not statistically different and the mean divided by the standard deviation is less than 0.06 (results not reported).

These findings, together with previous results, strongly suggest that the two areas had similar observable characteristics including migration and labor market outcomes. We include the individual and household characteristics listed in Table 1 as controls throughout the analysis to account for any differences at baseline and to reduce the standard errors. To explore the effect of unbalanced variables in the robustness analysis in Appendix D, we also control for a limited set of baseline controls that only include unbalanced variables, exclude Hindus, and control for actual arsenic levels in the tubewell and find results are similar.

B. *Identification Strategy and Empirical Specification*

We take advantage of the variation in program implementation across location and time to estimate ITT effects of the MCH-FP program on labor market and migration outcomes using single-difference models. Double-difference models provide a robustness check and are presented in the appendix. We focus on two main cohorts: those born when the family planning and maternal health interventions were introduced, the 31–34 cohort, and those born when both the family planning and child health interventions were available, the 24–30 cohort.

Identification of causal effects for the single-difference model assumes that the treatment and comparison areas would have had the same mean outcomes in the absence of the program. This is not a testable assumption, but seems reasonable given the similarity between treatment and comparison areas before the program discussed in Section IV-A. ITT single-difference effects for person *i* from village *v* are estimated using the following linear regression:

$$Y_{iv}= \beta_0 + \beta_1 C_i^{24-30} + \beta_2 C_i^{31-34} + \beta_3\left(T_v * C_i^{24-30}\right) + \beta_4\left(T_v * C_i^{31-34}\right) + \alpha_t + X'Z + \varepsilon_{iv} ,$$

where *Y* is a labor market or migration outcome. T_v (referred to as *Treat* in the tables) is a binary variable that takes on the value 1 if person *i*'s first household head resided in the treatment village before the start of the MCH-FP program, and 0 if in the comparison village. C_i^{24-30} and C_i^{31-34} are indicator variables for each age cohort.[8] β_3 and β_4 are the single-difference ITT effects and represent the differences in the conditional mean of the outcome between the treatment and comparison group for the specified cohort. β_3 captures the effects of the family planning and maternal health interventions combined with any spillovers of having younger siblings exposed to the intensive child health interventions, and β_4 is the combined effect of all program interventions.

We also control for birth-year fixed effects, α_t, to account for differences in the outcome due to age, as well as other effects that affect only certain birth cohorts. X is a vector of baseline household and household head characteristics, and religion that are interacted with the two age group dummies (see Table 1 for a list of variables). Standard errors are clustered at the pre-program village level to account for the likely intracluster correlation in the error terms. Results are similar if standard errors are clustered at a higher level that amalgamates villages into 4 treatment blocks and 2 control blocks (Appendix D). Construction of variables is discussed in Appendix B.

All models adjust for attrition between birth and MHSS2 using inverse propensity score weights. Appendix F provides details on the weights and shows results are similar when unweighted or using other weighting schemes, including weighting the sample to be representative of the 1974 baseline.

Finally, several changes took place over the 35-year period since program start in both the treatment and comparison areas that could bias the results. These changes include an irrigation project that involved building an embankment that led to the destruction of some areas forcing people to migrate to nearby areas within Matlab; the expansion of schools and healthcare; the discovery of arsenic in some deep tubewells, which could have affected child development; and the introduction of micro-credit. Details on confounders are in Appendix C and results are similar controlling for these potential confounders (Appendix D).

Despite the similarity between the treatment and comparison areas and the effort to control for potentially confounding changes across the areas, identification hinges on the untestable assumption that average outcomes of treatment area individuals would have been the same as the comparison area in the absence of the MCH-FP program. To overcome this concern, a double-difference model could be used to estimate the ITT effects, controlling for any differences in outcomes using an unexposed cohort. Outcomes examined in this paper change throughout adulthood and the MCH-FP program likely affected the outcomes

[8] Cohort dummies are defined based on program roll dates which do not perfectly overlap with birth year. Results remain the same without the cohort dummies, or if age fixed effects are included.

of any pre-program cohort, thus biasing the double-difference estimates in an unknown direction.[9] Labor and migration decisions of older cohorts and the 31–34 and 24–30 cohorts are also likely linked as labor and migration are family decisions and affected by networks. Nevertheless, we present results from a double-difference model with village fixed-effects in the robustness section for completeness and results are generally similar, but do not use this specification as our main specification given the possible biases.

V. Intent-To-Treat Program Impacts on Men

A. Labor Market Outcomes and Location of Work

We examine effects of the program on labor market outcomes and work location in Tables 2–4 for work in either a primary or secondary job[10] including participation in paid work, occupation, skills used for the job, entrepreneurial activity, earnings, and hours worked. Within each table, we report single-difference point estimates for each cohort in Panel A, increases relative to the comparison area mean for that cohort in Panel B, and the mean of the outcome variables for the comparison group for each cohort at the bottom of each table. Robust standard errors clustered at the treatment village level are reported in parentheses. To account for the possibility of multiple hypothesis testing across the primary outcomes explored in Tables 2–4, we report adjusted p-values to control for the false discovery rate (FDR) in square brackets (Anderson 2008). We discuss results based on the naive p-values and note implications of the FDR adjustment.

Labor market attachment is strong in this context and most men in our sample migrate for work. More than 90 percent of men work for pay and work long hours, close to 60 hours a week. Men in either cohort work mainly in professional/semi-professional or manual jobs. On average, earnings are lower for the 24–30 cohort than the 31–34 cohort, as they are at an earlier point in their earnings trajectory, but neither cohort has reached the age of peak earnings which typically occurs in one's 40s (Figure A1). Migration for work is prevalent with more than 65 percent of both cohorts from the comparison area working primarily outside of Matlab. Among our sample, about 40 percent work in urban areas of Bangladesh, 20 percent work internationally, and less than 5 percent work in other rural areas. Repeat migration is common in this setting,

[9] Barham (2012) examines the effect of the MCH-FP program on human capital by estimating a double-difference model that uses an older cohort born prior to the program to control for pre-program differences. Human capital outcomes, such as height, years of education, and cognition, are generally set by adulthood making a pre-program cohort a good counterfactual of the pre-program treatment and comparison area differences.

[10] Primary and secondary jobs are defined by level of earnings in the past 12 months. Findings for primary jobs only are reported in Table A2 and are qualitatively the same.

with migration rates starting to recede around age 40 (Figure A2).

Labor Market Participation (Table 2, columns 1–2). There is no program effect on work in any paid activities in the past 12 months for either cohort. This is not surprising given high labor market participation rates. However, approximately 15 percent of the sample work in more than one job and there is an 8 percentage point (63 percent) increase in participation in a secondary job for the 31–34 cohort, though the effect is not robust to multiple hypothesis testing.

Occupation (Table 2, columns 3–5). Occupation includes three categories: semi-professional and professional (e.g., professional, clerical, or sales work), agricultural, and manual (e.g., trade jobs such as construction, factory work, driving, handicrafts, and elementary jobs such as rickshaw driving and day labor). See Table B4 for occupations within each category. The 24–30 cohort experienced a 10 percentage point (31 percent) increase in work in professional/semi-professional. These jobs tend to be higher quality jobs, as hourly wages are approximately 30 percent higher for professional/semi-professional jobs than for manual jobs (Table B3). In contrast, the 31–34 cohort were 9 percentage points (80 percent) more likely to work in agriculture, though this effect is not robust to multiple hypothesis testing.

Skills (Table 2, columns 6 and 7). In line with working in more professional jobs, the 24–30 cohort used more academic skills in their primary job and were 8 percentage points (31 percent) more likely to need to read, write, and use math in their work.

Entrepreneurial Activity (Table 3). Information on type of employment demonstrates the 24–30 cohort is more entrepreneurial as self-employment increased by 38 percent. This increase is not driven by joint businesses with friends or family members, rather the 24–30 cohort were 48 percent more likely to start their own business and increased business loans by 119 percent. In contrast, the 31–34 cohort was 7 percentage points (60 percent) more likely to work on a family farm or in a family business, though this result is not robust to multiple hypothesis testing.

Earnings, Hours Worked, Consumption (Table 4 columns 1–3). Despite the improvements in labor market outcomes for the 24–30 cohort, earnings in the past 12 months were the same between the treatment and comparison areas.[11] While the point estimate on untrimmed earnings is large, positive and marginally

[11] Earnings are deflated for purchasing power parity across international and various domestic locations. Profits from the family farm or business are spread evenly among all household members working in that business, though results are robust to other ways of distributing (or not distributing) this income.

significant (column 1), it is driven by a few outliers. When earnings are trimmed at the 5 percent level the ITT effect is close to zero, 0.56 2012 USD. Results using other methods to address outliers, such as logs, are similar (Table A3).[12] Findings indicate the 31–34 cohort earned on average 23 percent less based on trimmed annual earnings (-460.87 2012 USD) and that this reduction is not driven by working fewer hours. The lack of a positive effect on earnings for both cohorts may be driven by work migration which we examine further in Section VI. We also examine household consumption and find no program effects for either cohort (Table A4).[13]

Primary Job Location (Table 4 columns 4–6). ITT effects for primary job location indicate the MCH-FP program lowered migration for work out of the Matlab by approximately 11 percentage points (16 percent) for both cohorts though effects differ by destination. For the 24–30 cohort, the difference in migration to international destinations is close to zero, but migration to domestic urban destinations[14] was 23 percent lower. For the 31–34 cohort, migration to international destinations was 34 percent lower, and the difference in migration to urban areas close to zero. While the FDR test suggests the reduction in international migration for the 31–34 cohorts is not robust, Section VI highlights similar results with two different data sets strongly suggesting the program effect on international migration is not likely an artifact of multiple hypotheses testing.

Labor Market Outcomes Conditional on Migration (Table 4 columns 7 and 8). We examine if the lack of program effects on earnings is correlated to reduced migration by estimating program effects by whether the primary job location was in or outside of the Matlab district for earnings in Table 4 and for other outcomes in Appendix Table A5. For the 24–30 cohort, the program impact on annual earnings, professional/semi-professional work, and hours are positive both in and outside of Matlab, and there is a reduction in manual work outside of Matlab, indicating that migration may be driving the lack of program effect on earnings. Specifically, annual earnings are 226 USD (31 percent) higher in Matlab and significant at the 5 percent level. Even if the treated earned more in Matlab, the average earnings across the entire treatment sample were still the same as the comparison group as more treated individuals worked in Matlab

[12] Different methods are used to account for outliers include log transformation, inverse hyperbolic sine transformation, median regression, as well as using the percentile rank of earnings.
[13] There is no household consumption data for the approximately 15 percent of men in the sample whose survey responses came from the phone survey instrument. Results are similar if we use the sending household.
[14] Urban destinations include Dhaka and surrounding districts or the Chittagong district.

where wages are still on average lower than in urban areas of Bangladesh. For the 31–34 cohort, the effect on income remains negative both inside and outside of Matlab, though statistically insignificant and smaller (-143 2012 USD) in Matlab. Splitting the sample by endogenous job location status is problematic because of selection into migration, however, any selection bias that occurs must lead to higher earnings in both locations for the 24–30 cohort. We explore other mechanisms that may affect earnings and migration in Section VI.

Discussion: These findings demonstrate that the 24–30 cohort, who were born when both family planning and vaccinations were available, had improved labor market outcomes. They were more likely to work in professional/semi-professional and entrepreneurial jobs that required them to use more academic skills but did not earn more. They also migrated less to urban areas of Bangladesh. The human capital improvements experienced by this cohort may have allowed treatment area individuals to better support their families at home and forego migration to urban areas of Bangladesh where they may have worked more manual jobs, increasing their welfare in the absence of overall income gains.

In contrast, the 31–34 cohort experienced a reduction in annual earnings and migrated less to international locations where wages are substantially higher. Migration and destination are often family decisions that consider the need for a male child to be close by to help with the family farm or business, and relative human capital within the family. Consistent with theory, the results show the 31–34 cohort experienced an increase in agricultural work, work on the family farm, and work in a secondary job. However, these results are not robust to multiple hypothesis testing leaving these results suggestive.

B. Timing of Migration

We examine the timing of migration including rates of ever migration, age at first migration, and duration of migration in Table 5. We use demographic surveillance data to measure migration over an individual's life. Because MHSS2 data on duration is restricted to the previous five years and does not include specific dates of migration episodes, to determine migration durations to international destinations we also use a follow-up survey to MHSS2 implemented by the authors that included MHSS2 respondents who had ever migrated internationally.

We first explore program effects on current residence at the time of survey in Table 5 (column 1), because outcomes that consider timing of migration are derived from measures of residence rather than primary job location. Results for current residence are similar to primary job location in Table 4 for the 24–30 cohort, but lower for the 31–34 cohorts because some of the cohort returned to Matlab in the 12 months prior to the survey.

Program impacts on ever migrated indicate both cohorts are nearly 10 percent less likely to have ever migrated from Matlab, though the effect is only significant at the 10 percent level for the 31–34 cohort. Similar to results for job location, the 24–30 cohort is 17 percent less likely to have ever migrated to an urban destination, and the 31–34 cohort is 27 percent less likely to have ever migrated internationally.

The reduction in migration is not because the age of first migration differs between the areas. Using DSS data to determine the age of first migration, we find that conditional on having ever migrated, there is no difference in age at first migration (Table 5, column 5).

Finally, we examine migration duration. Conditional on having lived outside of Matlab in the last five years, MHSS2 data show that those in the treatment area lived away for approximately 0.3 fewer years, in both the older and younger cohorts. Consistent with the findings on job location, the 24–30 cohort has shorter durations to urban destinations and 31–34 cohort to international destinations, though results by destination are not statistically significant. Data on duration of international migration over one's lifetime are consistent with MHSS2 migration patterns (Table 5 column 9). There is no effect on the 24–30 cohort, but the 31–34 cohort migrated internationally for 28 fewer months (31 percent).

Taken together, and across multiple data sources, these results indicate that the lower current migration rates are a combination of a reduction in ever migrating and of migrating for shorter durations, particularly among international migrants from the 31–34 cohort.

C. Robustness, Weighting, Multiple Hypothesis Testing and Inference

A detailed discussion of robustness checks, including tests of inference, and different weighting schemes are in Appendices D, E, and F and support the validity of the research design on the 11 main outcomes. This section presents a summary of a subset of the robustness checks including estimates using a double-difference model, which are presented in Tables A6–A9. It is important to note that given treatment and comparison areas are contiguous, of particular concern for identification is anything outside of MCH-FP that affected individuals in one area but not the other. This concern is limited because the study area is relatively small and homogeneous (almost all people in the comparison area are located within 5 km of the treatment area), the programs/services available in both areas tend to be similar because they are in the same district, and the labor markets not distinct.

First, as a check on identification, we examine if there are pre-existing differences between the treatment and comparison group by examining program effects on similarly aged men in 1996 using MHSS1. These men are born more than five years before program rollout, and there are no significant differences in earnings or being in a professional/semi-professional occupation for these men (Table D1). This suggests that there were no pre-existing differences in earnings or occupations, or life cycle effects on labor market outcomes between the areas for men in 1996, when the two cohorts of interest were still in school.

Second, as an additional check for pre-existing differences between the treatment and comparison group we estimate a double-difference model with village fixed-effects using a pre-program cohort to attempt to control for non-time varying differences between the treatment and comparison areas. Identifying a pre-program cohort whose labor and migration outcomes are not affected by the program is difficult because they need to still be of working age and an age where they migrate, but their outcomes not linked to those of the sample cohorts by being a father, sibling or in the migration or labor network of our sample cohort. We identify an age group to minimize these effects for each gender, but estimates are still likely to be biased in an unknown direction.[15] Results are reported in Tables A6–A9 for men and are similar for the 24–30 cohort. While point estimates are also similar for the 31–34 cohort, there is a reduction in the earnings disadvantage from 28 to 13 percent and a loss of statistical significance for most outcomes, except the negative migration effect.

Third, to compare more proximate areas, we restrict the sample to men whose pre-program village is within 3 kilometers, or an approximately 45-minute walk, of the treatment and comparison border using GIS data of village centroids and borders. Table D2 (panel B) shows results remain qualitatively similar.

Fourth, we take advantage of the two distinct geographic areas that form the comparison area, one north

[15] The double difference cohort for men are born between 1955 and 1972 (aged 40–57 in MHSS2). They are young enough to limit number of fathers of men in our sample, but born at least five years before the program to limit siblings and sibling competition. Still, of the men in the 40–57 comparison cohort half have a child in 24–30 or 31–34 cohorts and 17 percent a sibling born after program implementation. Women's migration and work decisions are less affected by other family members as women in our sample do not migrate for work and mainly work at home in agriculture. However, outcomes could be affected by fertility. Differences in access to family planning between the treatment and comparison area narrowed substantially after 1990, so we use a cohort born in the five years prior to the program, 1972–1976 (aged 35–40 in MHSS2) who largely started childbearing after 1990.

and one west (see Figure 1), to show that it is unlikely there was a confounding shock in the comparison area, that geography is driving the results, or that there are distinct labor markets in the study area. We repeat the analysis using each comparison block separately and results remain largely unchanged, though there is some loss of significance due to a smaller sample size (Table D2 panels C and D).

Fifth, we show results are not driven by the location of the main town in the treatment area. Results are similar excluding those whose pre-program village was in or around the main town (Table D2 panel E).

Sixth, results are robust to the inclusion of extended controls for prominent changes across the study site that include the introduction of microcredit, a flood mitigation program, education and healthcare supply construction, and differential exposure to arsenic (Table D2 panel F).

Seventh, we find no evidence of spatially correlated errors across villages (results not reported) or have knowledge of a disease outbreak that affected just one area from the decades of demographic surveillance data on mortality and disease even in the early years (Fauveau 1994).

Eighth, one would be worried about separate labor markets if prices varied across the region. Using market price data collected in MHSS2 from different markets in the treatment and comparison area, we find no evidence that food prices vary between the two areas (Appendix E).

Ninth, the sample has low attrition rates, and in Appendix F we show that the findings are unlikely due to differences in mortality or migration attrition over time between the treatment and comparison areas. Results are robust to various weighting schemes including an unweighted specification, only correcting for attrition between MHSS1 and MHSS2, and weighting the data to be representative of households in 1974.

Additional tests of inference and robustness presented in Appendix D include a wild cluster bootstrap to account for the smaller number of clusters, randomization-based inference including adjusting inference for the placement of the treatment area across a contiguous group of villages by permuting the assignment of villages to treatment, restricting the definition of professional/semi-professional occupation to those that are clearly more professional, showing no occupational differences among fathers in 1996, restricting the sample to those who are Muslim due to the pre-program imbalance in religion, and using a limited set of controls selected using a data-driven approach (Beloni et al. 2014).

In sum, results are stable across many robustness checks demonstrating the strength of the research design. While the point estimates are qualitatively the same for the 30–34 cohort, there is a loss of significance in key outcomes in the double-difference model or when adjusting standard errors for multiple hypothesis testing. This is not surprising given the smaller sample sizes and more limited significance in the main regression, but leaves results for 30–34 cohort to be more suggestive.

VI. Mechanisms

Three key mechanisms through which the MCH-FP interventions could affect labor and migration outcomes are improved human capital, reduced family size, and thinner migration networks. Additionally, the program potentially changed the pattern of sibling competition through an imbalance of human capital among siblings treated and not treated by the child health interventions or due to smaller family sizes. We first examine the direct effect of the program on each mechanism. Due to the endogeneity of the mechanisms, it is difficult to causally determine how each of the mechanisms affect the labor market and migration outcomes but use descriptive approaches to provide suggestive evidence.

Barham et al. (2021b) use MHSS2 to demonstrate the program had positive contemporaneous effects on human capital (height for both sexes and education for males) of the 24–30 cohort, but not 31–34 cohort. In Table 6, we examine the direct effect of the MCH-FP program five other mechanisms including reduced family size (total number of siblings, number of younger male siblings, number of older male siblings, first born male, and mother's age), and two measures of migrant networks (if the child's father ever migrated and the number of ever migrants in a child's bari network as of Jan. 1, 2015). Consistent with the roll out of the family planning program, the 24–30 cohort had 0.67 fewer siblings overall, both fewer younger brothers (0.14) and fewer older brothers (0.33) and are 32 percent more likely to be first born. The 31–34 cohort, had 0.2 less siblings overall (not statistically significant) but 0.4 fewer younger brothers. They are also less likely to be first born indicating that some mothers may have delayed childbearing at the start of the program. There are no statistically significant differences in mother's age as represented by year of birth, or whether the father ever migrated since 1974. However, both treated cohorts have a reduction of 0.12–0.19 standard deviations in the number of migrants from their bari (household compound) relative to the comparison group demonstrating thinner migrant networks. These finding indicate that any of these potential mechanisms—improved human capital, reduced sibship size, and thinner migration networks — could be driving the results.

Controlling for Program Mechanisms (Table 7). We further control for each of the mechanisms. The mechanisms are endogenous so results are correlational, but if the ITT effects are reduced this indicates that the mechanism may be a potential pathway for the main outcomes of interest. We follow Gelbach (2016) and compare the point estimates from our baseline specification (column 1) with a model that includes controls from five groups of potential mechanisms—migration, education, height, family structure, and migrant networks (column 2).[16] Column 3 reports the differences in point estimates between the two models and columns 4 through 8 provide the conditional decomposition of this change across the mechanisms.[17] To compare across columns, we restrict the sample to observations with non-missing values for all mechanisms.

The effect on earnings (panel A) for the 24–30 cohort increases slightly when controlling for all potential mechanisms. For the 31–34 cohort, inclusion of the job location controls reduced the negative earnings effect by almost half to -212.32 USD (column 2). The decomposition shows that nearly all of this reduction is related to changes in migration (column 4).

For professional/semi-professional occupations (panel B), the treatment effect is reduced by nearly 20 percent for the 24–30 cohort when all controls are included. The decomposition of the change suggests that education (column 5) may be one of the pathways through which the program led to better jobs, which accounted for more than all of the difference.

The effect on job locations in any destination outside of Matlab (panel C) or to international destinations (panel D) is reduced for the 31–34 cohort by more than 30 percent. The results suggest that human capital—both education (column 5) and height (column 6)—is an important pathway for either type of migration, and family structure is a potential pathway through which the program affected international migration (column 7), where the family structure effect is dominated by presence of younger brothers (results not reported).

Effect of Younger Brothers and Firstborn Status on the 31–34 Cohort (Table 8). To further investigate the negative program effect on earnings and migration destination for the 31–34 cohort, we examine

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[16] Migration controls include any migration outside of Matlab, migration to an urban destination, and migration duration. Education controls include grades completed and current enrollment status. Family structure controls include a set of indicators for the number of older and younger brothers to allow for non-linearities, as well as an indicator for being the firstborn male. We restrict our attention to number to male brothers because it is men who predominately migrate for work. Number of younger and older siblings are included as fixed effects to control for non-linearities. All controls are additionally interacted with an indicator for program cohort.

[17] The decomposition method in Gelbach (2016) is similar to stepping in each of the endogenous control variables one at a time to determine how their inclusion reduced the treatment effect, except the decomposition accounts for the fact that the order in which you step in the endogenous controls can matter.

heterogeneity on number of younger brothers and firstborn status through treatment effect interactions. Both of these variables are endogenous so results only provide descriptive evidence. Reduced international migration may exist if the decision of which son migrates internationally is a family decision and depends on relative human capital within the family. The fact that both cohorts had reduced migration, but the cohort with lower human capital, the 31–34 cohort, was less likely to migrate internationally and have shorter migration durations is consistent with this theory.[18] While we examine heterogeneity by firstborn male status, the Muslim inheritance system divides assets evenly among the males, so there is no inherent preference for first born males to work in agriculture, as there are in other cultures.

Table 8 first presents program heterogeneity by comparing those with one or fewer younger male siblings to those who have two or more younger male siblings.[19] The second model includes heterogeneity based on firstborn status. Since family size can be a determinant of migration, a third model controls for family size using number of brothers fixed effects. The point estimates on the interaction of treatment with younger brothers show that for the 31–34 cohort with two or more younger brothers the negative program effect on earnings is almost double and the reduction in international migration is more than double. The point estimates do not change controlling for first born status or number of brother fixed-effects. None of the point estimates on the heterogeneity interaction are statistically significant due to the relatively the sample size is small for this analysis. However, these results provide suggestive evidence that the reduction in migration to international destinations among the 31–34 cohort could be due to the presence of younger siblings with potentially higher human capital (results restricting the sample of younger brother to those born after the intensive child health interventions were available are similar).

VII. ITT Program Impacts for Women

Women's labor and migration opportunities differ substantially from men's in Bangladesh. For example, based on the comparison group means less than 30 percent of women work for wages and almost none have a secondary job. Of those who work, the majority work in agriculture or manual jobs, and on

[18] Qualitative interviews taken during MHSS2 fieldwork corroborate the importance of a son remaining in Matlab, and that the son who is more likely to be successful (perhaps due to higher human capital) is preferred for international migration.
[19] About 30 percent of the sample have 2 or more younger male siblings.

average earnings are low, less than 200 2012 USD annually. While 40 percent of women migrate, there is almost no international migration, and most women migrate for marriage or to be with their husband.

Labor Market Outcomes. Similar to men, women experienced a positive effect on labor market outcomes for the 24–30 cohort (Table 9). The likelihood of engaging in paid work increased by 7 percentage point (23 percent), driven by a 7 percentage point (48 percent) increase in work in agriculture, though work in agriculture is not robust to multiple hypothesis testing. Disaggregating the agriculture results by type of activity shows that the increased work is in rearing small animals (e.g., chickens, ducks) rather than in crops or larger animals. There are no statistically significant effects by skills (results not reported). For the 31–34 cohort, there are no significant effects on labor market participation or occupation.

The program did not lead to statistically significant differences in annual earnings (trimmed or untrimmed) or hours worked between the treatment and comparison areas for either age group (Table 10 columns 1-3).[20] There are no program effects on household consumption (Table A4).

Primary Job Location/Migration. Program effects for primary job location (or residence if not working) by destination for the 24–30 cohort, are small and insignificant (Table 10 columns 4–6). Similarly to men, the 31–34 cohort are 11 percentage points (27 percent) less likely to have a primary job location outside of Matlab, with most of the reduction coming from reduced migration to urban areas. Because most women migrate for marriage or to be with their husband, we examine the program effect on migration for the spouses of the 31–34 cohort. We find that spouses of these women are also about 12 percentage points less likely to migrate to urban areas (results not reported) suggesting that the migration effects for these women are linked to the program effect of their husbands.

Personal Assets, Savings and Loans. We take advantage of a module on women's status to determine if the women themselves own any productive assets, have cash savings, or have ever taken out a micro-credit loan (Table 10 columns 7-9). These women are not more likely to own their own productive assets, but consistent with the 24–30 cohort being more likely to raise small animals, these women are 30 percent more likely to both have some cash savings and to have taken out a micro-credit loan at some point, though

[20] There are fewer clear outliers for women than men, and less than a third of women have positive incomes. Rather than trimming the largest 5% of incomes, we trim female incomes by setting the trim threshold as the smallest income that is more than two SD larger than the next closest income. This results in dropping 3 observations from the sample.

neither are robust to multiple hypothesis testing across all outcomes. While the point estimates are positive for the 31–34 cohort, they are small and statistically insignificant.

Mechanisms (Marriage and Fertility). To understand the effects of the program on labor market and migration outcomes, we examine the effect of the program on two important mechanisms for women: marriage and fertility. Child rearing is typically the woman's responsibility and affects her employment and migration opportunities and choices. While access to family planning was similar between the treatment and comparison areas for these young women, their parents experienced differential access to family planning during their fertile years as a result of the program, and this may have influenced the young women's own pattern of marriage and fertility. Table 11 demonstrates that there are no meaningful program effects on marriage, age of marriage, age at menarche, age at birth of first child, or number of children. Most of these young women have not completed their fertility so differences could appear later in life.

Robustness. Similar robustness checks and weighting schemes as men are discussed in Appendix D (Table D5 and Table D6) and Appendix F, and double-difference results presented in Tables A9-A10. Results are qualitatively the same. There is a reduction in the point estimate and loss of statistical significance on working for pay for several models, though the point estimate remains positive.

VIII. Conclusion

This paper uses the quasi-random placement of the MCH-FP program in rural Bangladesh during the 1970s and 1980s to provide new evidence of the longer-run effects of improved early childhood health on adult labor market outcomes and migration patterns. Estimates are the combined effects of family planning and child health interventions, which continue to form the backbone of preventative health policy. Bangladesh has been a leader among low- and middle-income countries in decreasing fertility rates and improving human capital.

We find that men in the 24–30 cohort, who were eligible for child health interventions at birth, did not earn more, but were more likely to work professional/semi-professional jobs, were more entrepreneurial, and more likely to remain in the study area. The lack of effect on earnings is linked to a reduction in having ever migrated and shorter migration durations to urban areas in Bangladesh. Examination of mechanisms suggests human capital may have facilitated the shift of employment to more professional/semi-professional occupations.

Men in the 31–34 cohort, born when only the family planning interventions were available, did not fare as well in the labor market. They had substantially lower annual earnings and were less likely to work in high-wage international destinations and had shorter migration durations. The statistical significance of these results is sensitive in some robustness checks due to the smaller sample size of this cohort leaving results suggestive.

These finding are important and differ from other studies that find increased earnings resulting for early childhood interventions. Indeed, the results highlight that in some contexts it is critical to understand the role migration plays when determining the welfare effects of these programs, as migration could be decreasing in human capital or sibling size in some contexts. The reduction in migration in both cohorts and the increase in entrepreneurial activity for the 24–30 cohort are informative to policy makers given the present policy debates on both rural-urban migration and international migration.

The difference in migration rates between the treatment and comparison areas may not persist, as historically migrants return. However, the difference in the migration impacts by destination for cohorts who were differentially exposed to the child health intervention highlight unintended effects and the importance of potential intrahousehold dynamics from programs that lead to changes in family sizes and differential human capital among siblings.

Finally, the study took place in the context of a highly gender-segregated labor market. Women in the 24–30 cohort also experienced improved labor market outcomes and were more entrepreneurial. They worked more in paid agricultural work, but there were no income gains or changes in hours worked. The lack of substantial improvements in labor market outcomes is consistent with women in the study area not traditionally working outside the home.

The results speak to the benefits of early child health and family planning programs. Despite earnings not increasing, reduction in migration for the 24–30 cohort may well be welfare improving since monetary and non-monetary costs of migration can be large. The lack of an effect on earnings may also be temporary as the 24–30 cohort are early in their careers. If working in a more professional or entrepreneurial job earlier in one's career changes the earnings trajectory, our estimated program effects could understate the program's effect on lifetime earnings. It will be important to examine the trajectory for these young men in the future, as well as how they invest in the local community, when earnings are higher and the life cycle process of return migration and the accumulation of capital from migration more complete.

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Tables and Figures

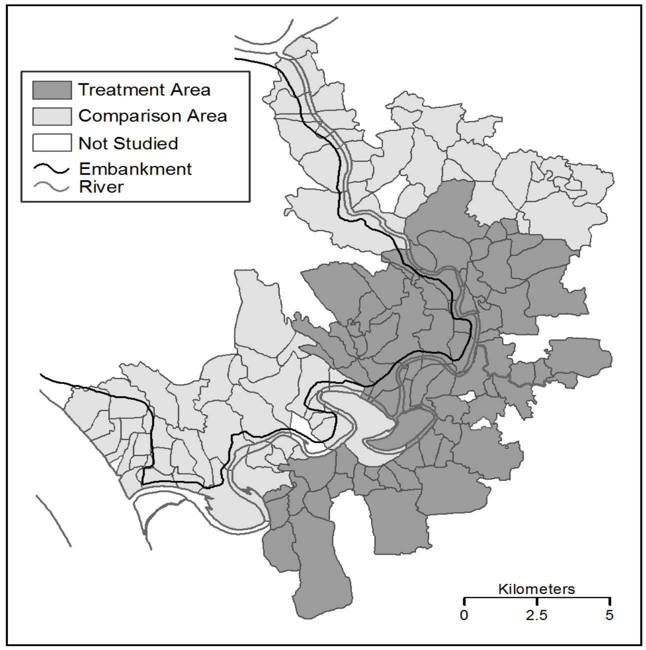

FIGURE 1. MAP OF MATLAB STUDY AREA

Notes: This figure is adapted from Barham (2012).

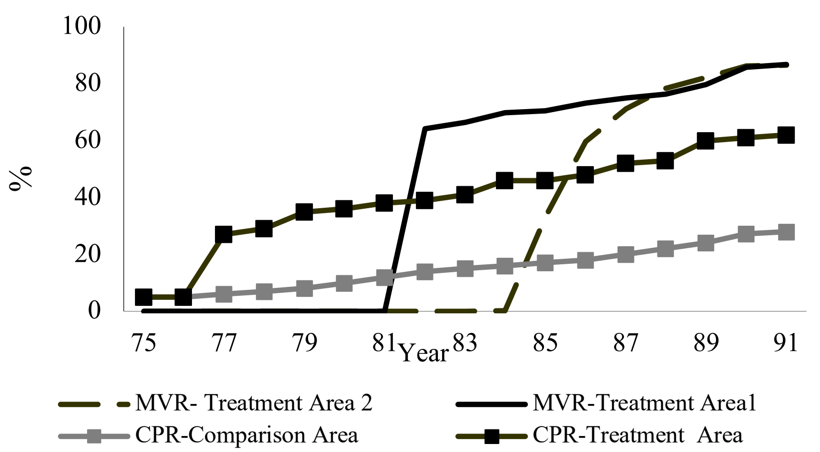

FIGURE 2. TRENDS IN CONTRACEPTIVE PREVALENCE RATE (CPR)
AND MEASLES VACCINATION RATES (MVR)
FOR CHILDREN 12–59 MONTHS BY CALENDAR YEAR

Notes: This figure is adapted from Barham (2012).

TABLE 1—BALANCE ON PRE-PROGRAM INDIVIDUAL AND HOUSEHOLD CHARACTERISTICS, MEN AND WOMEN AGED 24–34

	Treatment Area		Comparison Area		Difference in Means		
	Mean	SD	Mean	SD	Mean	T-stat	Mean/SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household Head Occupation in 1974 Census							
Any paid work	0.91	(0.55)	0.90	(0.57)	0.01	0.57	0.02
Agriculture	0.60	(0.79)	0.57	(0.81)	0.02	0.73	0.03
Fishing or boating	0.07	(0.50)	0.07	(0.40)	0.00	-0.20	-0.01
Business or skilled service	0.11	(0.44)	0.09	(0.47)	0.02	1.06	0.04
Household Head Occupation in 1982 Census							
Any paid work	0.89	(0.49)	0.89	(0.39)	0.00	-0.08	0.00
Agriculture	0.60	(0.80)	0.60	(0.83)	0.00	0.12	0.00
Fishing or boating	0.06	(0.47)	0.06	(0.43)	0.00	-0.24	-0.01
Business or skilled service	0.15	(0.66)	0.15	(0.56)	0.00	-0.06	0.00
Individual Characteristics							
Male (=1)	0.51	(0.48)	0.51	(0.50)	0.00	0.05	0.00
Birth year	1983	(2.78)	1983	(3.06)	0.16	1.36	0.05
Muslim (=1)	0.84	(0.92)	0.95	(0.42)	-0.11	-3.74	-0.26
Household Characteristics							
Land size in 1982 (decimals)	10.26	(20.22)	10.67	(18.54)	-0.41	-0.52	-0.02
Bari size	8.71	(11.91)	7.99	(11.10)	0.72	1.56	0.07
Family size	6.75	(4.32)	6.45	(3.59)	0.30	1.89	0.08
Owns a lamp (=1)	0.62	(0.80)	0.57	(0.87)	0.05	1.49	0.06
Owns a watch (=1)	0.15	(0.53)	0.15	(0.54)	0.00	0.13	0.01
Owns a radio (=1)	0.08	(0.37)	0.07	(0.33)	0.01	0.51	0.02
Wall tin or tinmix (=1)	0.28	(0.69)	0.29	(0.69)	-0.01	-0.31	-0.01
Tin roof (=1)	0.82	(0.61)	0.82	(0.65)	0.00	0.03	0.00
Number of rooms per capita	0.23	(0.13)	0.22	(0.16)	0.00	0.27	0.01
Number of cows	1.37	(2.67)	1.29	(2.27)	0.09	0.89	0.04
Number of boats	0.64	(1.18)	0.63	(1.17)	0.01	0.16	0.01
Drinking water, tubewell (=1)	0.29	(1.07)	0.15	(0.91)	0.14	3.49	0.15
Drinking water, tank (=1)	0.41	(1.38)	0.34	(1.53)	0.08	1.32	0.05
Latrine (=1)	0.81	(0.79)	0.87	(0.86)	-0.06	-1.76	-0.07
HH age	47.81	(21.37)	45.43	(20.22)	2.37	2.84	0.12
HH years of education	2.23	(4.56)	1.93	(4.84)	0.30	1.62	0.06
HH spouse's age	37.29	(18.15)	35.09	(17.66)	2.20	3.07	0.12
HH spouse's years of education	0.69	(2.06)	0.60	(2.31)	0.09	1.04	0.04

Notes: The sample includes male and female respondents in the 24-30 and 31-34 age cohorts who have employment data in MHSS2. Unless otherwise noted, household characteristics come from the 1974 census. Standard deviations (SD) are clustered at the treatment village level. There are 1,141 treatment area observations and 1,378 comparison area observations. Standard deviations in column 7 are based on the comparison group. Household head and spouse age are reported, but these variables are likely affected by the family planning program increasing birth intervals and decreasing family size in the treatment area. Observations are weighted to correct for attrition between birth and the MHSS2 survey.

TABLE 2—ITT EFFECTS ON LABOR MARKET PARTICIPATION, OCCUPATION, AND JOB SKILLS IN THE PAST 12 MONTHS, MEN

	Any Paid Work Past 12 Months (=1)	Had Second Job Past 12 Months (=1)	Occupation (=1)			Skills Used in Primary Job (=1)	
			Prof. & Semi-Prof.	Agriculture	Manual	Reading, Writing, Math	Physical
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Single Differences							
Treat*(Age 24–30)	0.01	0.03	0.10**	0.01	-0.06+	0.08*	-0.04
	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)	(0.04)	(0.03)
	[0.588]	[0.237]	[0.048]	[0.491]	[0.133]	[0.089]	[0.148]
Treat*(Age 31–34)	0.01	0.08*	-0.01	0.09*	-0.01	-0.05	0.02
	(0.02)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)
	[0.725]	[0.225]	[0.846]	[0.222]	[0.725]	[0.575]	[0.725]
Pr(24–30 = 31–34)	0.98	0.31	0.09	0.07	0.40	0.03	0.14
Panel B: Percent Changes							
Treat*(Age 24–30)	1%	27%	31%	13%	-11%	31%	-5%
Treat*(Age 31–34)	1%	63%	-2%	80%	-2%	-19%	3%
Age 24–30 Means	0.92	0.13	0.33	0.11	0.57	0.26	0.85
Age 31–34 Means	0.96	0.16	0.39	0.13	0.57	0.31	0.85
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,299

Notes: Standard errors are clustered at the pre-program village level and reported in parentheses. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables 2–4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE 3—ITT EFFECTS ON SOURCES OF EMPLOYMENT IN THE PAST 12 MONTHS, MEN

	Source of Employment (=1)				Start Own Business (=1)	No. of Business Loans in Past 12 Months
	Salaried	Self-Employed	Family Farm or Biz	Daily Labor or Piece Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Single Differences						
Treat*(Age 24–30)	-0.05	0.09*	0.02	-0.00	0.09**	0.09*
	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)	(0.04)
	[0.237]	[0.073]	[0.269]	[0.614]	[0.048]	[0.085]
Treat*(Age 31–34)	-0.08	0.03	0.07+	0.04	0.02	-0.02
	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)	(0.06)
	[0.359]	[0.725]	[0.242]	[0.359]	[0.725]	[0.725]
Pr(24–30 = 31–34)	0.61	0.34	0.27	0.25	0.25	0.10
Panel B: Percent Changes						
Treat*(Age 24–30)	-8%	38%	20%	-3%	48%	119%
Treat*(Age 31–34)	-14%	11%	60%	31%	12%	-32%
Age 24–30 Means	0.55	0.23	0.12	0.14	0.20	0.08
Age 31–34 Means	0.54	0.32	0.14	0.12	0.30	0.20
Observations	1,299	1,299	1,299	1,299	1,299	1,094

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables 2–4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Loan information (6) is not available for phone survey respondents.

** $p<0.01$, * $p<0.05$, + $p<0.1$

TABLE 4—ITT EFFECTS ON INCOME, HOURS, AND LOCATION OF WORK IN THE PAST 12 MONTHS, MEN

	Earnings Past 12 Months (2012 USD)		Hours Worked Past 12 months	Primary Job Location (=1)			Earnings Past 12 Months (2012 USD) Trim 5%	
	Full Sample	Trim 5%	Full Sample	Outside Matlab	Destination		Outside Matlab	Matlab
					International	Urban		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences								
Treat*(Age 24–30)	1,390.49+	0.56	-8.99	-0.11**	-0.02	-0.09*	96.93	226.18
	(784.92)	(108.85)	(93.21)	(0.04)	(0.03)	(0.04)	(161.35)	(113.47)*
		[0.633]	[0.614]	[0.048]	[0.491]	[0.089]		
Treat*(Age 31–34)	-1,040.69**	-460.87**	-143.16	-0.10*	-0.09+	-0.02	-385.15	-143.49
	(375.36)	(151.81)	(113.56)	-0.05	(0.04)	(0.06)	(201.82)+	(142.82)
		[0.058]	[0.389]	[0.222]	[0.225]	[0.725]		
Pr(24–30 = 31–34)	0.01	0.02	0.34	0.92	0.21	0.32	0.08	0.05
Panel B: Percent Changes								
Treat*(Age 24–30)	60%	0%	-0%	-16%	-6%	-23%	5%	31%
Treat*(Age 31–34)	-34%	-28%	-5%	-15%	-34%	-6%	-15%	-15%
Age 24–30 Means	2,305	1,639	3,028	0.68	0.25	0.39	2,081	720
Age 31–34 Means	3,091	2,029	3,282	0.66	0.27	0.36	2,628	975
Observations	1,287	1,181	1,287	1,299	1,299	1,299	732	449

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables 2–4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. All incomes are reported in 2012 USD. For trim 5%, the highest 5 percent of male incomes in the MHSS2 survey are set to missing. Urban locations are Dhaka, Chittagong, and their surrounding metro areas.
** p<0.01, * p<0.05, + p<0.1

TABLE 5—ITT EFFECTS ON TIMING OF MIGRATION, MEN

	Current Residence Out of Matlab (=1)	Ever Migrated Out of Matlab	Ever Migrated to Urban Destination	Ever Migrated to International Destination	Age at First Migration Out of Matlab	Migration Duration Over Last 5 Years Conditional on Migration by Destination (Years)			Intl. Migration Duration (Months) Conditional on Ever Migrated Internationally
						All	International	Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences									
Treat*(Age 24–30)	-0.12	-0.08	-0.09	-0.03	0.11	-0.25	-0.03	-0.19	5.99
	(0.04)**	(0.03)**	(0.04)*	(0.04)	(0.35)	(0.12)*	(0.22)	(0.15)	(6.95)
Treat*(Age 31–34)	-0.07	-0.07	-0.05	-0.09	-0.16	-0.31	-0.32	-0.03	-28.33
	(0.05)	(0.04)+	(0.06)	(0.05)+	(0.58)	(0.13)*	(0.27)	(0.23)	(13.64)*
Pr(24–30 = 31–34)	0.39	0.80	0.44	0.29	0.66	0.74	0.41	0.53	0.03
Panel B: Percent Changes									
Treat*(Age 24–30)	-19%	-9%	-17%	-11%	1%	-6%	-1%	-5%	10%
Treat*(Age 31–34)	-12%	-8%	-9%	-27%	-1%	-7%	-8%	-1%	-31%
Age 24–30 Means	0.63	0.85	0.54	0.28	19.36	4.36	3.64	4.05	58.23
Age 31–34 Means	0.57	0.86	0.55	0.33	21.30	4.56	4.13	4.36	90.79
Observations	1,299	1,299	1,299	1,299	1,044	903	357	552	365

Notes: Standard errors are clustered at the pre-program village level. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Current migration out of Matlab is determined by an individual's MHSS2 village ID. Ever migrated out of Matlab and age at migration are measured using DSS data. International migration duration in column (9) is measured using an extended survey of international migrants. Yearly migration duration comes from MHSS2. Columns (6) through (9) only includes individuals who have ever migrated to the destination of interest.

** $p<0.01$, * $p<0.05$, + $p<0.1$

TABLE 6—ITT EFFECTS ON POTENTIAL MECHANISMS, MEN

	Number of Siblings	Number of Younger Male Siblings	Number of Older Male Siblings	First-born Male (=1)	Mother Birth Year	Father Migrated Since 1974 (=1)	No. of Migrants in Bari Network (z-score)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Single Differences							
Treat*(Age 24–30)	-0.67	-0.14	-0.33	0.09	0.24	-0.06	-0.12
	(0.20)**	(0.08)+	(0.13)*	(0.04)*	(0.53)	(0.04)+	(0.04)**
Treat*(Age 31–34)	-0.20	-0.40	0.04	-0.08	-0.44	-0.06	-0.19
	(0.27)	(0.12)**	(0.14)	(0.04)+	(0.64)	(0.05)	(0.04)**
Pr(24–30 = 31–34)	0.09	0.06	0.04	0.00	0.39	0.98	0.17
Panel B: Percent Changes							
Treat*(Age 24–30)	-14%	-16%	-20%	32%	-	-15%	-
Treat*(Age 31–34)	-4%	-31%	3%	-19%	-	-16%	-
Age 24–30 Means	4.886	0.897	1.652	0.282	1958	0.412	-
Age 31–34 Means	5.094	1.306	1.277	0.419	1954	0.364	-
Observations	1,277	1,278	1,278	1,276	1,293	1,273	1,299

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Migration network outcomes in columns (6) and (7) are created using DSS data.
** p<0.01, * p<0.05, + p<0.1

TABLE 7—ITT EFFECTS CONTROLLING FOR MECHANISMS, MEN

	Base Model	Controlling for All Mechanisms	Difference between Models (1) – (2)	Decomposition of Difference				
				Migration and Duration Outside Matlab	Grades Completed/ Enrollment	Height	Family Composition/ Firstborn	No. Migrants in Network
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Earnings Past 12 Months (USD) 5% Trim								
Treat*(Age 24–30)	-12.97	50.01	-62.98	-107.32	-0.03	9.92	25.72	8.72
	(109.83)	(84.88)	(78.38)	(70.13)	(14.83)	(8.34)	(16.38)	(11.11)
Treat*(Age 31–34)	-464.32	-251.99	-212.32	-190.04	-36.55	-6.32	17.12	3.48
	(153.57)	(128.85)	(105.88)	(99.07)	(27.45)	(7.55)	(21.49)	(25.11)
Observations	1,139	1,139	1,139	1,139	1,139	1,139	1,139	1,139
Panel B: Type of Job Professional or Semi Professional (=1)								
Treat*(Age 24–30)	0.091	0.075	0.016	-0.010	0.018	0.001	0.006	0.001
	(0.038)	(0.035)	(0.016)	(0.007)	(0.011)	(0.002)	(0.007)	(0.005)
Treat*(Age 31–34)	-0.008	0.012	-0.020	0.013	-0.016	-0.001	-0.000	-0.015
	(0.048)	(0.050)	(0.028)	(0.015)	(0.013)	(0.004)	(0.010)	(0.012)
Observations	1,241	1,241	1,241	1,241	1,241	1,241	1,241	1,241
Panel C: Job Location Out of Matlab to any Destination (=1)								
Treat*(Age 24–30)	-0.113	-0.135	0.022		0.011	0.003	0.011	-0.003
	(0.038)	(0.039)	(0.014)		(0.012)	(0.003)	(0.007)	(0.004)
Treat*(Age 31–34)	-0.109	-0.077	-0.032		-0.023	-0.008	0.005	-0.006
	(0.049)	(0.052)	(0.024)		(0.014)	(0.007)	(0.012)	(0.009)
Observations	1,241	1,241	1,241		1,241	1,241	1,241	1,241
Panel D: Job Location Out of Matlab to International Destination (=1)								
Treat*(Age 24–30)	-0.012	-0.007	-0.005		0.001	0.005	-0.008	-0.004
	(0.035)	(0.037)	(0.011)		(0.007)	(0.004)	(0.006)	(0.003)
Treat*(Age 31–34)	-0.082	-0.051	-0.031		-0.009	-0.007	-0.016	0.001
	(0.043)	(0.043)	(0.015)		(0.006)	(0.005)	(0.010)	(0.010)
Observations	1,241	1,241	1,241		1,241	1,241	1,241	1,241

Notes: Columns (1) and (2) come from regressions of the outcome variables in the panel title. Column (1) follows the same specification as in the main results, but restricta the sample to individuals who have all endogenous mechanism variables. Column (2) includes the endogenous variables as mediating controls. Column (3) reports the difference in point estimates between column (1) and column (2). This difference is decomposed into the contribution across the groups of endogenous variables in columns (4) through (8) following Gelbach (2016). The migration variables in column (4) include: migration out of Matlab, migration to an urban destination, and migration duration. The family composition variables in column (7) include: number of younger brothers, number of older brothers, and an indicator for being the first-born male. Standard errors are clustered at the pre-program village level.

TABLE 8—ITT EFFECTS BY NUMBER OF YOUNGER BROTHERS AND FIRSTBORN STATUS, MALES IN 31–34 COHORT

	Earnings Past 12 Months (USD)			Primary Job Location Out of Matlab					
				International			Urban		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat*(Age 31–34)	-303.95	-344.65	-396.98	-0.02	-0.01	-0.02	-0.06	-0.08	-0.07
	(181.30)+	(196.85)+	(211.20)+	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)
Treat*(Age 31–34)*2+ Younger Brothers	-444.66	-484.64	-489.32	-0.14	-0.15	-0.14	0.04	0.04	0.04
	(299.91)	(308.30)	(320.03)	(0.08)+	(0.08)+	(0.08)+	(0.11)	(0.11)	(0.11)
Treat*(Age 31–34)*Firstborn Male		144.86	198.00		0.00	0.02		0.05	0.03
		(284.29)	(306.29)		(0.07)	(0.07)		(0.09)	(0.09)
2+ Younger Brothers	198.02	197.30	323.33	0.12	0.11	0.08	-0.10	-0.09	0.00
	(214.19)	(215.07)	(254.55)	(0.05)*	(0.05)*	(0.06)	(0.06)	(0.07)	(0.07)
Firstborn Male		14.69	-135.11		0.05	0.08		-0.11	-0.18
		(178.31)	(235.68)		(0.05)	(0.07)		(0.06)+	(0.08)*
Number of Brothers FE	N	N	Y	N	N	Y	N	N	Y
Age 31–34 Mean	2,000	2,000	2,000	0.25	0.25	0.25	0.37	0.37	0.37
Observations	411	411	411	453	453	453	453	453	453

Notes: Standard errors are clustered at the pre-program village level. All regressions include individual characteristics and pre-intervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects and controls for religion. Pre-intervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE 9—ITT EFFECTS ON LABOR MARKET PARTICIPATION AND OCCUPATION IN THE PAST 12 MONTHS, WOMEN

	Any Paid Work (=1)	Has a Second Job (=1)	Occupation (=1)				Raise Animals (=1)	Raise Animals (=1)	
			Prof. & Semi-Prof.	Agriculture	Manual	Unpaid Household Work		Cows, Goats, or Sheep	Ducks or Hens
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences									
Treat*(Age 24–30)	0.07*	0.00	0.01	0.07**	-0.01	-0.06+	0.07**	0.01	0.07**
	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
	[0.127]	[0.766]	[0.766]	[0.017]	[0.766]	[0.176]	[0.017]	[0.766]	[0.001]
Treat*(Age 31–34)	0.04	0.00	-0.01	0.03	0.03	-0.04	0.03	0.02	0.05+
	(0.05)	(0.02)	(0.02)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.905]
Pr(24–30 = 31–34)	0.58	0.94	0.46	0.47	0.37	0.82	0.47	0.79	0.59
Panel B: Percent Changes									
Treat*(Age 24–30)	23%	25%	13%	48%	-6%	-9%	48%	8%	100%
Treat*(Age 31–34)	13%	17%	-30%	23%	20%	-7%	23%	20%	73%
Age 24–30 Means	0.29	0.01	0.04	0.14	0.17	0.64	0.14	0.09	0.07
Age 31–34 Means	0.32	0.01	0.04	0.19	0.14	0.63	0.19	0.12	0.08
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables 10–11. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE 10—ITT EFFECTS ON EARNINGS, HOURS, LOCATION OF WORK, ASSETS, AND MICRO-CREDIT, WOMEN

	Earnings Past 12 Months (USD)		Hours Worked Past 12 Months	Primary Job Location or Current Residence			Owns a Productive Asset (=1)	Any Cash Savings (=1)	Ever had a Micro Credit Loan (=1)
	Full Sample	Trim 3 Largest	Full Sample	Outside Matlab	Urban	Rural			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences									
Treat*(Age 24–30)	41.44	8.31	75.09	-0.04	-0.03	-0.02	0.02	0.06*	0.06+
	(40.23)	(24.66)	(73.52)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)	(0.03)
		[0.766]	[0.523]	[0.507]	[0.670]	[0.670]	[0.766]	[0.109]	[0.244]
Treat*(Age 31–34)	-119.70	-37.56	-90.56	-0.11*	-0.10*	-0.02	0.00	0.05	0.05
	(92.70)	(36.40)	(111.06)	(0.05)	(0.04)	(0.03)	(0.04)	(0.05)	(0.05)
		[1.000]	[1.000]	[0.516]	[0.516]	[1.000]	[1.000]	[1.000]	[1.000]
Pr(24–30 = 31–34)	0.09	0.24	0.20	0.30	0.24	0.98	0.84	0.82	0.79
Panel B: Percent Changes									
Treat*(Age 24–30)	31%	7%	18%	-11%	-9%	-19%	11%	30%	28%
Treat*(Age 31–34)	-67%	-31%	-22%	-28%	-32%	-18%	3%	24%	22%
Age 24–30 Means	133	119	421	0.41	0.31	0.10	0.14	0.21	0.21
Age 31–34 Means	177	134	491	0.41	0.32	0.09	0.18	0.22	0.31
Observations	1,216	1,211	1,216	1,220	1,220	1,220	1,214	1,209	1,214

Notes: Standard errors are clustered at the pre-program village level and reported in parenthesis. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables 10–11. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. All incomes are reported in 2012 USD. For trim, incomes are sorted, the first value that is more than 2 SD larger than the next closest value is chosen as the cutoff threshold. The observations with that income or larger are dropped from the sample. Urban locations are Dhaka, Chittagong, and their surrounding metro areas.
** p<0.01, * p<0.05, + p<0.1

TABLE 11—ITT EFFECTS ON POTENTIAL MECHANISMS - MARRIAGE AND FERTILITY, WOMEN

	Ever Married (=1)	Age at First Marriage	Age at Menarche	Age at First Child	Number of Children
	(1)	(2)	(3)	(4)	(5)
Panel A: Single Differences					
Treat*(Age 24–30)	0.00	-0.47+	-0.06	-0.08	-0.04
	(0.02)	(0.27)	(0.07)	(0.25)	(0.07)
Treat*(Age 31–34)	0.01	0.08	-0.01	-0.28	0.17
	(0.01)	(0.39)	(0.12)	(0.38)	(0.12)
Pr(24–30 = 31–34)	0.81	0.19	0.75	0.67	0.11
Panel B: Percent Changes					
Treat*(Age 24–30)	0%	-2%	-0%	-0%	-3%
Treat*(Age 31–34)	1%	0%	-0%	-1%	11%
Age 24–30 Means	0.94	20.11	13.46	21.58	1.54
Age 31–34 Means	1.00	19.93	13.45	22.08	2.32
Observations	1,231	1,153	1,211	1,069	1,220

Notes: Standard errors are clustered at the pre-program village level. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** $p<0.01$, * $p<0.05$, + $p<0.1$

ALL APPENDICES ARE FOR ONLINE PUBLICATION

Appendix A

Appendix Figures and Tables

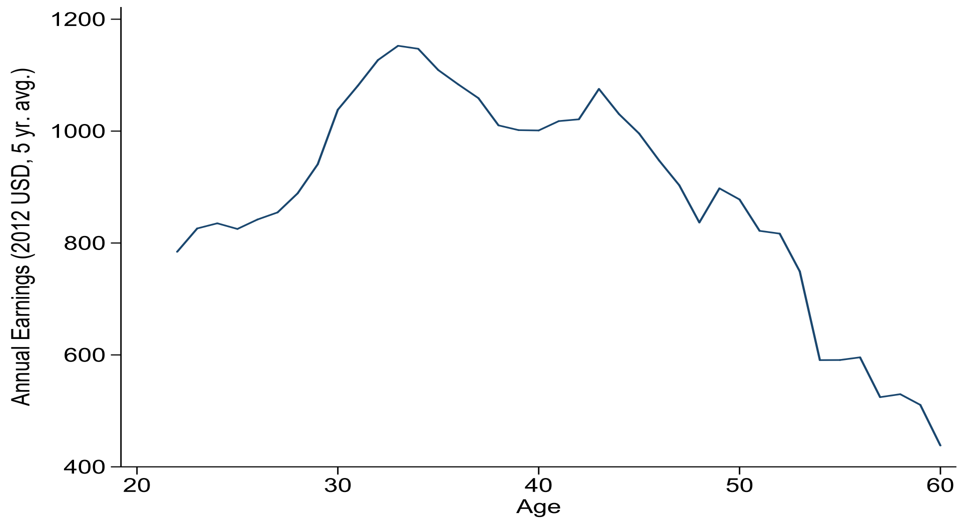

FIGURE A1. AGE-EARNINGS PROFILE, MEN LIVING IN MATLAB

Notes: Based on author's calculations using data from MHSS2. Earnings are smoothed by taking 5-year averages across age groups.

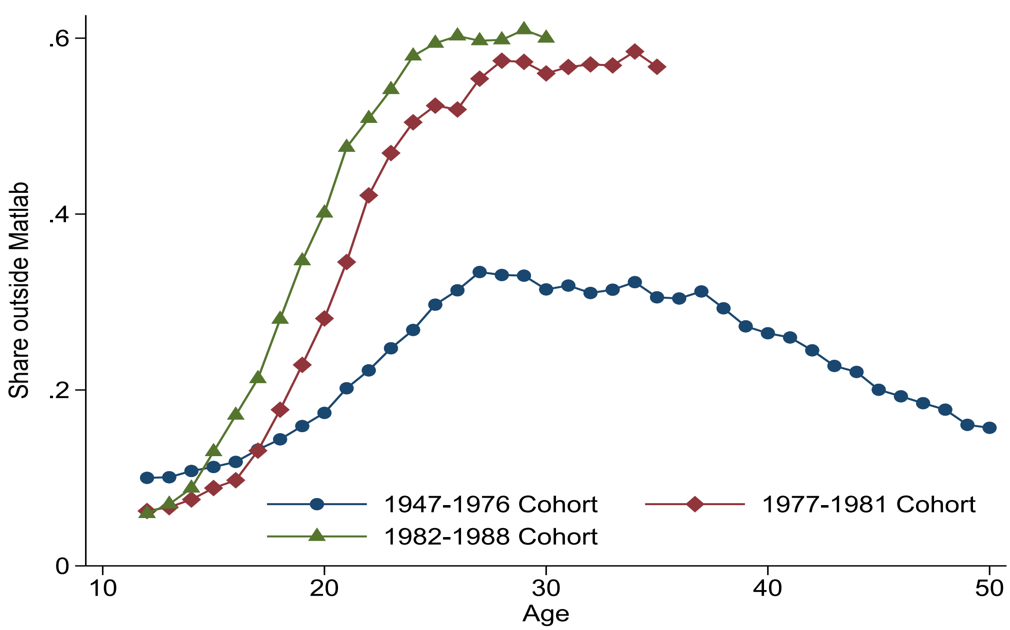

FIGURE A2. AGE-SPECIFIC MIGRATION RATES IN THE COMPARISON AREA, BY COHORT

Notes: Based on author's calculations using migration information from the Matlab demographic surveillance survey.

TABLE A1—MCH-FP PROGRAM INTERVENTIONS BY BIRTH YEAR

Year of Birth	Age in 2012	Program Eligibility
Jan. 1947–Sept. 1977	35–65	*Preintervention Period:* could be indirectly affected by the program through children or siblings.
Oct. 1977–Feb. 1982	31–34	*Intensive Family Planning and Maternal Health Interventions* Mother eligible for family planning, tetanus toxoid vaccine, and folic acid and iron in last trimester of pregnancy.
Mar. 1982–Dec. 1988	24–30	*Intensive Child Health Interventions Added* Children under age five eligible for measles vaccination in half the treatment area. In Nov. 1985 Children under age five eligible in entire treatment area for vaccination (measles, DPT, polio, tuberculosis), vitamin A supplementation, nutrition rehabilitation for children at risk (starting 1987).

Notes: The 2012 age groupings are based on age in years rounded to approximate age in December 2012. The exact year and month cutoffs are used to create groups for the analysis. Services were added over time, so those in later cohorts had access to the earlier interventions.

TABLE A2—ITT EFFECTS ON OCCUPATION AND SOURCE OF EMPLOYMENT FOR PRIMARY JOB IN THE PAST 12 MONTHS, MEN

	Occupation (=1)			Source of Employment (=1)			
	Prof. & Semi-Prof.	Agriculture	Manual	Salaried	Self-Employed	Family Farm or Biz	Daily Labor or Piece Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Single Differences							
Treat*(Age 24–30)	0.09	-0.03	-0.07	-0.06	0.09	-0.02	-0.02
	(0.03)**	(0.02)	(0.04)*	(0.03)+	(0.03)*	(0.02)	(0.02)
Treat*(Age 31–34)	-0.02	0.06	-0.03	-0.08	0.02	0.04	0.03
	(0.05)	(0.03)*	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
Panel B: Percent Changes							
Treat*(Age 24–30)	28%	-48%	-13%	-11%	42%	-31%	-16%
Treat*(Age 31–34)	-5%	168%	-5%	-15%	7%	74%	29%
Age 24–30 Means	0.32	0.06	0.56	0.54	0.21	0.06	0.13
Age 31–34 Means	0.38	0.04	0.56	0.53	0.30	0.05	0.10
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,299

Notes: Primary job is the job at which the respondent earned the most in the past 12 months. Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE A3—ITT EFFECTS ON EARNINGS IN THE PAST 12 MONTHS USING DIFFERENT METHODS FOR OUTLIERS, MEN

	Earnings Past 12 Months (2012 USD)			
	Log	IHS	Median	Percentile Rank
	(1)	(2)	(3)	(4)
Panel A: Single Differences				
Treat*(Age 24–30)	0.06	-0.01	-47.43	-0.03
	(0.10)	(0.18)	(91.09)	(2.11)
Treat*(Age 31–34)	-0.32	-0.32	-467.70	-9.26
	(0.11)**	(0.19)+	(137.99)**	(3.17)**
Panel B: Percent Changes				
Treat*(Age 24–30)	0%	0%	-2%	0%
Treat*(Age 31–34)	0%	0%	-15%	0%
Age 24–30 Means	2,476	2,305	2,305	2,305
Age 31–34 Means	3,181	3,091	3,091	3,091
Observations	1,207	1,287	1,287	1,287

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Column (1) uses a log transformation of income. Column (2) uses the inverse hyperbolic sine transformation. Column (3) reports the results from a median regression. Column (4) presents the effect on the percentile rank of income within each cohort income distribution. Control group means are reported for the untransformed income in 2012 USD.

** $p<0.01$, * $p<0.05$, + $p<0.1$

TABLE A4—ITT EFFECTS ON HOUSEHOLD CONSUMPTION, MEN AND WOMEN

	Log Household Consumption Per Capita	Share of Total Expenditure, by Category					
		Food	Durables	Housing	Health	Education	Other Non-Food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Single Differences (Men)							
Treat*(Age 24–30)	-0.04	0.59	-0.38	-0.25	0.59	0.84	-1.29
	(0.05)	(1.10)	(0.41)	(0.57)	(0.43)	(0.60)	(0.81)
Treat*(Age 31–34)	-0.01	-1.47	-0.18	0.05	1.50	0.49	0.03
	(0.06)	(1.58)	(0.45)	(0.93)	(0.73)*	(0.41)	(1.11)
Age 24–30 Means	$1,059	64.63	1.53	7.49	3.71	2.25	20.39
Age 31–34 Means	$991	66.50	1.37	8.29	3.82	1.90	18.28
Observations	1,094	1,094	1,090	1,090	1,090	1,090	1,090
Panel B: Single Differences (Women)							
Treat*(Age 24–30)	0.05	-1.25	0.12	1.27	0.54	-0.21	-0.61
	(0.04)	(1.13)	(0.25)	(0.82)	(0.50)	(0.30)	(0.66)
Treat*(Age 31–34)	-0.07	1.63	0.21	-1.93	0.62	-1.06	0.35
	(0.06)	(1.51)	(0.31)	(1.04)+	(0.59)	(0.64)	(0.90)
Age 24–30 Means	$690	65.93	1.71	7.06	5.15	2.98	17.35
Age 31–34 Means	$678	63.78	1.44	7.91	4.18	6.06	17.11
Observations	1,219	1,219	1,212	1,212	1,212	1,212	1,212

Notes: These results are for the respondent's household, though are not available for phone survey respondents. Results are similar if we instead use the sending household for respondents who migrated from Matlab. Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.

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

TABLE A5—ITT EFFECTS ON MAIN LABOR MARKET OUTCOMES BY MIGRATION STATUS, MEN

	Second Job	Occupation			Start Own Business	Skills Used	Earnings (USD) Trim 5%	Hours Worked
		Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences - Inside Matlab								
Treat*(Age 24–30)	0.02	0.09	-0.01	0.05	0.17	0.15	226.18	142.66
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)*	(0.06)**	(113.47)*	(174.57)
Treat*(Age 31–34)	0.09	-0.07	0.15	0.03	-0.01	0.03	-143.49	-206.35
	(0.08)	(0.08)	(0.08)+	(0.07)	(0.07)	(0.07)	(142.82)	(203.97)
Age 24–30 Means	0.28	0.28	0.29	0.48	0.30	0.20	720	2,477
Age 31–34 Means	0.38	0.38	0.37	0.50	0.42	0.20	975	3,046
Observations	472	472	472	472	472	472	449	461
Panel B: Single Differences - Outside Matlab								
Treat*(Age 24–30)	-0.01	0.11	-0.02	-0.11	0.02	0.02	96.93	79.66
	(0.02)	(0.05)*	(0.01)	(0.04)*	(0.04)	(0.05)	(161.35)	(108.05)
Treat*(Age 31–34)	0.03	0.02	0.01	-0.01	0.00	-0.07	-385.15	-18.70
	(0.03)	(0.06)	(0.01)	(0.06)	(0.05)	(0.06)	(201.82)+	(125.02)
Age 24–30 Means	0.06	0.36	0.03	0.61	0.15	0.29	2,081	3,273
Age 31–34 Means	0.05	0.40	0.00	0.61	0.23	0.37	2,628	3,400
Observations	827	827	827	827	827	827	732	826

Notes: Standard errors are clustered at the treatment village level. Means by age are for the comparison group. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Panel A restricts the sample to individuals whose primary job is in Matlab. Panel B restricts the sample to individuals whose primary job is outside Matlab.

** $p<0.01$, * $p<0.05$, + $p<0.1$

TABLE A6—DOUBLE-DIFFERENCE ITT EFFECTS ON LABOR MARKET PARTICIPATION, OCCUPATION, AND JOB SKILLS IN THE PAST 12 MONTHS, MEN

	Any Paid Work Past 12 Months (=1)	Had Second Job Past 12 Months (=1)	Occupation (=1)			Skills Used in Primary Job (=1)	
			Prof. & Semi-Prof.	Agriculture	Manual	Reading, Writing, Math	Physical
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Double Differences							
Treat*(Age 24–30)	0.01	0.10*	0.10*	0.03	-0.03	0.06	-0.04
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
	[0.618]	[0.093]	[0.093]	[0.449]	[0.554]	[0.270]	[0.270]
Treat*(Age 31–34)	0.00	0.13*	-0.00	0.09	0.00	-0.05	0.03
	(0.02)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)	(0.04)
	[1.000]	[0.451]	[1.000]	[0.451]	[1.000]	[0.529]	[0.753]
Pr(24–30 = 31–34)	0.89	0.66	0.11	0.18	0.62	0.07	0.12
Panel B: Percent Changes							
Treat*(Age 24–30)	1%	80%	31%	29%	-5%	22%	-5%
Treat*(Age 31–34)	0%	97%	-0%	82%	1%	-21%	3%
Age 24–30 Means	0.92	0.13	0.33	0.11	0.57	0.26	0.85
Age 31–34 Means	0.96	0.16	0.39	0.13	0.57	0.31	0.85
Age 40–57 Means	0.95	0.47	0.38	0.46	0.43	0.20	0.87
Observations	2,250	2,250	2,250	2,250	2,250	2,250	2,250

Notes: Standard errors are clustered at the pre-program village level and reported in parentheses. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables A2–A4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.

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

TABLE A7—DOUBLE-DIFFERENCE ITT EFFECTS ON SOURCES OF EMPLOYMENT IN THE PAST 12 MONTHS, MEN

	Source of Employment (=1)				Start Own Business (=1)	No. of Business Loans in Past 12 Months
	Salaried	Self-Employed	Family Farm or Biz	Daily Labor or Piece Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Double Differences						
Treat*(Age 24–30)	-0.11*	0.18**	0.04	0.02	0.18**	0.15**
	(0.05)	(0.06)	(0.04)	(0.04)	(0.06)	(0.06)
	[0.093]	[0.030]	[0.321]	[0.580]	[0.039]	[0.046]
Treat*(Age 31–34)	-0.13+	0.11+	0.06	0.05	0.10	0.01
	(0.07)	(0.06)	(0.06)	(0.04)	(0.06)	(0.08)
	[0.451]	[0.451]	[0.481]	[0.470]	[0.451]	[1.000]
Pr(24–30 = 31–34)	0.72	0.30	0.60	0.40	0.23	0.04
Panel B: Percent Changes						
Treat*(Age 24–30)	-20%	78%	32%	12%	92%	196%
Treat*(Age 31–34)	-24%	47%	51%	38%	51%	10%
Age 24–30 Means	0.55	0.23	0.12	0.14	0.20	0.08
Age 31–34 Means	0.54	0.32	0.14	0.12	0.30	0.20
Age 40–57 Means	0.20	0.43	0.46	0.25	0.45	0.18
Observations	2,250	2,250	2,250	2,250	2,250	1,994

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables A2–A4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Loan information (6) is not available for phone survey respondents.
** p<0.01, * p<0.05, + p<0.1

TABLE A8— DOUBLE-DIFFERENCE ITT EFFECTS ON INCOME, HOURS, AND LOCATION OF WORK IN THE PAST 12 MONTHS, MEN

	Earnings Past 12 Months (2012 USD)		Hours Worked Past 12 months	Primary Job Location (=1)		
	Full Sample	Trim 5%	Full Sample	Outside Matlab	Destination International	Destination Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Double Differences						
Treat*(Age 24–30)	1720.37*	234.47	177.07	-0.12*	-0.01	-0.10+
	(793.99)	(148.09)	(143.05)	(0.05)	(0.04)	(0.05)
		[0.148]	[0.270]	[0.046]	[0.618]	[0.097]
Treat*(Age 31–34)	-671.15	-221.17	3.38	-0.11*	-0.08	-0.01
	(458.34)	(200.30)	(162.64)	(0.05)	(0.05)	(0.06)
		[0.481]	[1.000]	[0.451]	[0.451]	[1.000]
Pr(24–30 = 31–34)	0.01	0.03	0.23	0.83	0.19	0.20
Panel B: Percent Changes						
Treat*(Age 24–30)	75%	14%	6%	-18%	-4%	-25%
Treat*(Age 31–34)	-22%	-13%	0%	-16%	-33%	-4%
Age 24–30 Means	2,305	1,639	3,028	0.68	0.25	0.39
Age 31–34 Means	3,091	2,029	3,282	0.66	0.27	0.36
Age 40–57 Means	1,553	1,282	2,874	0.27	0.09	0.16
Observations	2,231	2,097	2,231	2,250	2,250	2,250

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables A2–A4. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. All incomes are reported in 2012 USD. For trim 5%, the highest 5 percent of male incomes in the MHSS2 survey are set to missing. Urban locations are Dhaka, Chittagong, and their surrounding metro areas.
** p<0.01, * p<0.05, + p<0.1

TABLE A9— DOUBLE-DIFFERENCE ITT EFFECTS ON LABOR MARKET PARTICIPATION IN THE PAST 12 MONTHS, WOMEN

	Any Paid Work (=1)	Has a Second Job (=1)	Occupation (=1)				Raise Animals (=1)	Raise Animals (=1)	
			Prof. & Semi-Prof.	Agriculture	Manual	Unpaid Household Work		Cows, Goats, or Sheep	Ducks or Hens
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Double Differences									
Treat*(Age 24–30)	0.14+	0.02	0.04	0.07	-0.01	-0.08	0.07	-0.02	0.11*
	(0.08)	(0.02)	(0.03)	(0.07)	(0.05)	(0.08)	(0.07)	(0.06)	(0.05)
	[0.653]	[0.811]	[0.811]	[0.811]	[1.000]	[0.811]	[0.811]	[1.000]	[0.244]
Treat*(Age 31–34)	0.09	0.03	0.01	0.01	0.05	-0.04	0.01	-0.02	0.07
	(0.09)	(0.02)	(0.04)	(0.09)	(0.06)	(0.09)	(0.09)	(0.07)	(0.05)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Pr(24–30 = 31–34)	0.40	0.62	0.34	0.29	0.29	0.50	0.29	0.98	0.27
Panel B: Percent Changes									
Treat*(Age 24–30)	48%	123%	85%	54%	-7%	-13%	54%	-24%	162%
Treat*(Age 31–34)	31%	178%	27%	11%	28%	-6%	11%	-25%	98%
Age 24–30 Means	0.29	0.01	0.04	0.13	0.17	0.64	0.13	0.09	0.07
Age 31–34 Means	0.32	0.01	0.04	0.19	0.14	0.63	0.19	0.12	0.08
Age 35–40 Means	0.41	0.03	0.06	0.31	0.12	0.53	0.31	0.18	0.18
Observations	1,595	1,595	1,595	1,595	1,595	1,595	1,595	1,595	1,595

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables A10–A11. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE A10—DOUBLE-DIFFERENCE ITT EFFECTS ON EARNINGS, HOURS, LOCATION OF WORK, ASSETS, AND MICRO-CREDIT, WOMEN

	Earnings Past 12 Months (USD)		Hours Worked Past 12 Months	Primary Job Location or Current Residence			Owns a Productive Asset (=1)	Any Cash Savings (=1)	Ever had a Micro Credit Loan (=1)
	Full Sample	Trim 3 Largest	Full Sample	Outside Matlab	Urban	Rural			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Double Differences									
Treat*(Age 24–30)	99.90	62.02	196.40	-0.04	-0.00	-0.03	0.05	0.14*	0.08
	(81.31)	(46.48)	(144.71)	(0.07)	(0.06)	(0.03)	(0.06)	(0.06)	(0.07)
		[0.811]	[0.811]	[1.000]	[1.00]	[0.811]	[0.811]	[0.244]	[0.811]
Treat*(Age 31–34)	-98.21	7.91	11.38	-0.12	-0.08	-0.04	0.02	0.10	0.03
	(150.25)	(60.83)	(191.18)	(0.07)	(0.06)	(0.04)	(0.07)	(0.08)	(0.08)
		[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Pr(24–30 = 31–34)	0.09	0.20	0.21	0.24	0.24	0.77	0.57	0.57	0.33
Panel B: Percent Changes									
Treat*(Age 24–30)	75%	52%	47%	-9%	-0%	-41%	35%	64%	38%
Treat*(Age 31–34)	-55%	7%	3%	-30%	-25%	-31%	11%	49%	13%
Age 24–30 Means	133.1	119.28	420.97	0.41	0.31	0.10	0.14	0.21	0.21
Age 31–34 Means	177.4	133.69	491.08	0.41	0.31	0.09	0.18	0.22	0.31
Age 35–40 Means	170.1	145.41	553.92	0.20	0.17	0.03	0.20	0.23	0.36
Observations	1,590	1,583	1,590	1,595	1,595	1,595	1,587	1,579	1,588

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables f–A11. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. All incomes are reported in 2012 USD. For trim, incomes are sorted, the first value that is more than 2 SD larger than the next closest value is chosen as the cutoff threshold. The observations with that income or larger are dropped from the sample. Urban locations are Dhaka, Chittagong, and their surrounding metro areas.

** $p<0.01$, * $p<0.05$, + $p<0.1$

Appendix B
Data and Construction of Selected Variables

This appendix describes the data sources, attrition, and the creation of the intent-to-treat and main outcome variables.

A. *Data Sources*

MHSS1/2.—The main outcomes variables used in this paper are from MHSS2. It is a large socioeconomic survey comprised of several instruments including an individual survey, a household survey, village survey, facility surveys, and market price survey of major markets areas throughout Bangladesh where MHSS2 respondents lived. The key labor market and migration outcomes were collected in the individual instrument and are not proxy reports as is the case in many surveys. Most of the data were collected during face to face interviews, though a subset of data was collected in a phone survey of international migrants who did not return to Bangladesh during the data collection period (about 15 percent of our male sample). The MHSS2 phone survey instrument was shorter than the in-person survey instrument, as a result, there are smaller sample sizes for some variables such as consumption, but not for the key labor market and migration outcomes.

MHSS2 was conducted between 2012 and 2014 and was designed to be a panel to MHSS1 (icddr,b 1996). MHSS1 is a seven percent random subsample of household compounds (called baris) from the Matlab area living in both the treatment and comparison areas and was designed to be representative of the study area's 1996 population. In MHSS1, two households were interviewed in each bari: a primary household, selected randomly, and a secondary household, selected purposively. Within a household, individuals age six and older were randomly selected to be personally interviewed, but basic information, including education, was collected on all household members via proxy.

The MHSS2 sample includes all individuals selected for personal interview in MHSS1 primary households creating panel data for these individuals.[1] To limit migration selection for key age groups, the MHSS2 sample also includes individuals born between 1972 and 1989 to a MHSS1 primary household that had migrated out of Matlab between 1977 and 1996 (referred to as pre-1996 migrants).[2] To the extent that a whole household and lineage migrated out of Matlab between the start of the program and 1996, leaving no one in that lineage available for selection into the MHSS1 sample, the MHSS2 sample could still suffer from migration selection. It is rare that whole households and lines of descent migrated out of Matlab prior to 1996 and is estimated to be minimal at 2.6 percent of the study site. In addition, we test the balance of the treatment groups to check that the treatment and comparison group have similar baseline characteristics on average.

MHSS2 collected extensive information on employment history for each individual older than fifteen at the time of survey. Income and labor supply measures (hours worked in a typical week and weeks worked) were collected by source of employment for the previous 12 months, and

[1] MHSS2 sample also included all panel member descendants, and their co-resident spouses. Among those panel members who had migrated out of Matlab, spouses were interviewed if they lived in the same house as the panel member or in Matlab. However, non-co-resident spouses of migrants were not tracked for interview.

[2] The pre-1996 migrants were identified by using the detailed DSS data.

include all earned income, including income from family businesses or farms. Additionally, occupation, job characteristics, and some employer information were collected for an individual's primary and secondary jobs, where primary job is defined as the job in which the respondent earned the most income in the past 12 months.

Census Data.—Periodic censuses were collected for all individuals in the study area (treatment and comparison areas) by iccdr,b. These data typically include household location, household characteristics and composition, employment, education, and assets. We obtain pre-program individual and household data on the analysis sample from the 1974 census (icddr,b 1974) and use these data to test for differences in baseline characteristics between the treatment and comparison areas. We also use the 1974 and 1982 census (icddr,b 1982) to link individuals to the study area (which is the demographic surveillance site) before 1977 to construct an individual's intent-to-treat status (see section C below).

Demographic Surveillance Site (DSS) Data.—Vital registration data provide prospective tracking of every birth, death, marriage, divorce, and in- and out- migration occurring in the study area. As such, we know when someone enters and leaves the study area. Information on migration destination (rural, urban, international) is also available starting in 1982. Data were collected by icddr,b and are high quality in part because they were collected so frequently: every two weeks until 1997, every month between 1998 and 2006, and every two months between 2007 and MHSS2.These data include pre-program data from 1974 onwards, and are used to construct birth dates and an individual's intent-to-treat status. In addition, we use these data to construct pre-program migration network variables for each individual in the analysis sample, as well as, out-migration variables such as whether someone has ever migrated, and out-migration variables for years not covered in the MHSS2 migration history.

B. *Attrition*

The main sample for this paper includes all individuals born during the experimental period from October 1977 and December 1988 (the 24–30 and 31–34 cohorts) who were a member of a MHSS1 primary household or a pre-1996 migrant. Including death and any other type of non-response, the attrition rate at the household level is 7 percent. Attrition rates are slightly higher for variables from the individual survey at 9.6 percent for men and 8.1 percent for women for income information (Table B1). The low attrition rate is a result of a carefully designed tracking protocol. Migrants were tracked all over Bangladesh, and a rapid response system was developed that allowed trackers in Matlab to connect enumerators placed in different parts of the country with respondents who had left Matlab. Intensive interviewing took place during all the Eid holidays from 2012–2014. Survey teams targeted international migrants, far away domestic migrants, and hard-to-track migrants returning to Matlab for the holiday. Finally, a phone survey was employed to collect information on a subset of questions from the main survey from predominately international migrants who did not return to Bangladesh during the survey period. While there is a limited set of variables available for this group, most employment and migration outcomes used in this study were collected during the phone survey. Without the phone survey, the attrition rate is higher for men at almost 24 percent, but the same for women, because women do not migrate internationally for work.

Even though the attrition rates are low and not statically different between treatment and comparison area, there could still be differential attrition between the treatment and comparison area, potentially biasing the results. To check for this possibility, Table B2 presents results of a regression of the treatment variable, individual and baseline characteristics, and the interaction of the treatment variable with the characteristics on an indicator of if a target sample respondent had missing income information in MHSS2. Results are reported for the analysis sample (men and women for both the 24–30 and 31–34 year old cohorts), as well as for men and women separately. Regression results are reported over two columns, the first column reports the coefficients on the main effects of the individual and baseline characteristics and the second column the coefficients on the interaction between the main effect and treatment. In addition, we test that all the interaction variables together equal zero using an *F*-test. Taking the interaction variables together, we find that there is no differential attrition between the treatment and comparison area based on individual characteristics and baseline variables. There are no significant differences on the interaction variables for both genders together and only 1 that is significant at the 5 percent level when examining males and females separately.

C. *Intent-to-Treat and Linking Baseline Variables*

Access to the MCH-FP program was based on the village of residence of the individual during the program period. Because a person's residence after program start is potentially endogenous, we use DSS and census data to create an intent-to-treat indicator based on the village of residence for an individual's first household head prior to 1977.[3] We take advantage of the fact that each individual has a unique ID that allows us to link the MHSS1/2 data with the DSS and census data, and use the following sequence of linkages. First, we link our respondents to the 1974 census through the household head of their first residence in the DSS area. If their household head was not present in the 1974 census, we then identify that person's first household head in the DSS area and link that new person to the 1974 census. Finally, remaining unlinked individuals are assigned a treatment status using the location of their household head in the DSS area after the 1974 census, but before the inception of MCH-FP in 1977.[4] The intent-to-treat variable, *Treat*, takes the value of 1 if the 1974 census-linked household head was living in a village in the treatment area in 1974 or migrated into a village in the treatment area between 1974 and 1977.

Baseline characteristics from the 1974 census are linked to individuals in the same manner used to construct treatment status. For the few individuals that could not be linked to the 1974 census, missing baseline characteristics are assigned means based on treatment status, sex, and cohort.[5] Finally, the village from the 1974 census link is used to cluster standard errors in our analysis.

[3] The treatment indicator would be nearly identical if individuals were linked to 1974 through their fathers and grandfathers. Less than 0.5% of the sample would have been assigned a different treatment status. We use household head because this sequence of linkages results in more direct links to the 1974 census, and therefore fewer missing baseline characteristics.

[4] We link over about 96% of individuals in our sample to the 1974 census through their first household head. An additional 3 percent link to the 1974 census through that person's first household head. The remaining less than 1 percent link through their household head's location in the DSS after the 1974 census, but before program inception in October 1977.

[5] Only 12 male and 13 female respondents have missing baseline data.

D. Construction of Selected Outcome and Control Variables

Occupations. Detailed occupation codes are collected for primary and secondary jobs in MHSS2. We aggregate these occupation codes into three main categories: professional and semi-professional, manual, and agriculture. Table B3 reports the differences in average hourly wages between the three main categories, separately by location. Table B4 provides a list of the common occupations that men report by category. On average, professional and semi-professional occupations have higher hourly wages than manual and agricultural work.

Skills. Unfortunately, information on skills was collected only for salaried and piece-rate workers. We impute this measure for self-employed and family business workers by comparing the responses for salaried workers in the same occupation code. If the majority of workers in an occupation report needing a skill, we recode the missing for that skill to 1 and 0 otherwise.

Annual Income. Annual income is constructed from a survey module that captures paid and nonpaid work from a set of eight general employment activities that was designed to cover all possible types of work (e.g., salaried work, piece-rate work, self-employment, etc.). Questions were asked by employment category to reduce the chance that the respondent would forget to report income if they worked multiple jobs. Income for household-related activities (e.g., family business and family farm) is split evenly among workers within the household reporting such activities, though the results are not sensitive to how this income is assigned. Income is deflated to 2012 values using World Bank national accounts data and then converted from Bangladeshi taka to US dollars using an exchange rate of 78Tk/US\$.[6] Income for international migrants interviewed in the phone survey is first converted to taka from the local currency using exchange rates collected at the time of interview, although results are not sensitive to using the average annual exchange rate from 2014 (the year the phone survey was administered). There are some large outliers, so to trim the data, we set to missing the earnings values that are above the 95^{th} percentile, separately by birth cohort and gender. When earnings are not trimmed, the program effect is a 45 percent increase in income. This program effect is driven by a few very large outliers.

Primary Job Location. For each source of employment, respondents report where they spent most of their time for work relative to their current residence. We construct primary job location using the main source of employment, based on annual earnings. A job is considered to be outside Matlab if the work location, relative to their village code in the survey, is outside the Chandpur district. A location is defined as being urban if the location is in Dhaka and surrounding districts (Munshiganj, Narayanganj, Narsingdi, Gazipur, and Joydevpur), or the Chittagong district.

Migration. Current location of residence, as well as residence location histories from 2008-2012, are collected in MHSS2 to allow the construction of migration status. A respondent is defined as a current out-migrant if their current residence, given by their village code in the survey, is outside the Chandpur district. An out-migrant is defined as being urban if the location is in Dhaka and surrounding districts (Munshiganj, Narayanganj, Narsingdi, Gazipur, and Joydevpur), or the Chittagong district.

[6] The exchange rate did not fluctuate much of the period of the survey.

Consumption. Consumption data come from the household head's reports of consumption of various items over 7-day, 30-day, and 12-month recall periods, as is typical in the World Bank Living Standard and Measurement surveys. 7-day recall includes 118 food, drink or tobacco related items that were purchased, produced, and transferred to the household. The 30-day recall records expenditure of basic household items (such as items for basic hygiene), services, and utility expenses, and the 12-month recall includes personal and household items such as clothing, kitchen items, appliances and furnishings, and vehicle repair. For food items, when available we use the value and quantity of purchased food to assign a value to the quantity of food produced or transferred. For households without purchased food, we use average prices determined from households in nearby areas. Additionally, we remove outlier values by item, defining the outlier cutoff as the smallest value that falls more than two standard deviations above the nearest value. We construct annual aggregate consumption measures at per capita levels because treated households are on average larger than non-treated because the treated are less likely to migrate.

TABLE B1—ANALYSIS SAMPLE ATTRITION RATES FOR MHSS2 DATA

	Men			Women		
	%	Difference in Rates Treatment - Comparison		%	Difference in Rates Treatment - Comparison	
		Mean	SE		Mean	SE
Not found or refused	5.2	-0.009	(0.012)	5.4	-0.021	(0.013)
Not found, refused, or dead	7.0	-0.014	(0.013)	7.0	-0.019	(0.014)
Non-missing employment/migration information	8.9	-0.013	(0.013)	7.8	-0.013	(0.016)
Non-missing annual income information	9.6	-0.021	(0.015)	8.1	-0.012	(0.016)
Non-missing annual income information, no phone survey	24.0	-0.040	(0.022)	8.2	-0.014	(0.016)

Notes: Sample includes 24–30 and 31–34 cohorts combined. The standard error on the difference in attrition rates between treatment and control is clustered at the pre-program village level. There are 1,423 men and 1,321 women across the two cohorts in the sample frame.

TABLE B2—ATTRITION BALANCE, MEN AND WOMEN AGED 24–34

	Men and Women		Men		Women	
	Main Effect	Interaction	Main Effect	Interaction	Main Effect	Interaction
	(1)	(2)	(3)	(4)	(5)	(6)
Male (=1)	0.013	-0.001				
	(0.015)	(0.019)				
Birth year	0.002	0.001	-0.001	0.006	0.005	-0.004
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)+	(0.004)
Muslim (=1)	-0.052	0.082	0.014	0.021	-0.109	0.131
	(0.045)	(0.049)+	(0.062)	(0.073)	(0.071)	(0.076)+
Land size 1982 (decimals)	0.001	-0.001	0.001	-0.002	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)+	(0.001)	(0.001)
Bari size	0.001	0.002	0.000	0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Family size	0.004	0.001	0.005	0.004	0.004	-0.003
	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.007)
Owns a lamp (=1)	-0.014	0.019	-0.012	0.033	-0.014	0.004
	(0.017)	(0.023)	(0.029)	(0.040)	(0.023)	(0.030)
Owns a watch (=1)	0.009	-0.018	-0.006	-0.003	0.023	-0.037
	(0.023)	(0.031)	(0.027)	(0.043)	(0.040)	(0.051)
Owns a radio (=1)	-0.058	0.054	-0.027	0.024	-0.094	0.091
	(0.021)**	(0.035)	(0.033)	(0.055)	(0.027)**	(0.045)*
Wall tin or tinmix (=1)	-0.004	0.033	-0.020	0.041	0.013	0.031
	(0.015)	(0.022)	(0.023)	(0.034)	(0.028)	(0.037)
Tin roof (=1)	-0.021	-0.006	0.004	-0.029	-0.052	0.021
	(0.023)	(0.033)	(0.032)	(0.045)	(0.034)	(0.044)
Number of rooms per capita	0.013	-0.028	0.003	0.110	0.022	-0.186
	(0.080)	(0.122)	(0.100)	(0.159)	(0.113)	(0.186)
Number of cows	-0.005	0.002	-0.011	0.014	0.000	-0.009
	(0.005)	(0.007)	(0.006)*	(0.009)	(0.008)	(0.010)
Number of boats	-0.016	0.005	-0.017	-0.009	-0.013	0.022
	(0.016)	(0.021)	(0.020)	(0.030)	(0.028)	(0.034)
Drinking water, tubewell (=1)	-0.003	-0.003	0.005	-0.026	-0.017	0.034
	(0.022)	(0.033)	(0.033)	(0.048)	(0.026)	(0.037)
Drinking water, tank (=1)	-0.001	0.000	0.001	-0.024	-0.002	0.029
	(0.015)	(0.027)	(0.022)	(0.038)	(0.022)	(0.033)
Latrine (=1)	-0.024	0.002	-0.035	0.009	-0.012	-0.005
	(0.026)	(0.033)	(0.039)	(0.052)	(0.032)	(0.040)
HH age	-0.001	0.001	-0.002	0.000	-0.001	0.002
	(0.001)	(0.001)	(0.001)+	(0.001)	(0.001)	(0.002)
HH years of eductation	-0.000	-0.001	0.000	0.000	-0.000	-0.003
	(0.003)	(0.004)	(0.005)	(0.007)	(0.004)	(0.006)
HH works in agriculture (=1)	0.013	-0.011	0.000	0.024	0.028	-0.045
	(0.015)	(0.021)	(0.019)	(0.030)	(0.025)	(0.037)
HH spouse's age	0.002	-0.002	0.001	0.000	0.002	-0.004
	(0.001)+	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)+
HH spouse's years of education	0.000	0.002	-0.002	0.004	0.001	0.001
	(0.005)	(0.008)	(0.009)	(0.012)	(0.007)	(0.010)
F-statistic that all interactions = 0		0.76		1.04		1.55
P-value		0.77		0.42		0.07
N		2,744		1,423		1,321

Notes: Each set of two columns report output from one regression where the outcome is an indicator variable that takes on the value 1 if the respondent is missing MHSS2 employment information. Each regression includes the treatment variable, variables listed in the table and the interaction of the treatment and variables listed in the table. For each group, the main effects are reported in the first column and the interaction with the treatment group in the second column (named interaction). The coefficient on the treatment variable is not reported because of the set of interaction terms. Unadjusted differences in attrition between the treatment and comparison area are reported in Table B1. The regressions include both the 24–30 and 31–34 year old cohorts used in the analysis. Standard errors clustered at the village level.

TABLE B3—DIFFERENCES IN AVERAGE HOURLY WAGE RELATIVE TO MANUAL WORK, MEN AGED 24–34

	Matlab	Urban	International
	(1)	(2)	(3)
Panel A: Differences Relative to Manual Work			
=1 if Professional & Semi-Professional	0.15	0.58	0.69
	(0.12)	(0.25)	(0.29)
=1 if Agriculture	-0.19		
	(0.06)		
Panel B: Percent Changes			
Professional & Semi-Professional	29%	97%	46%
Agriculture	-37%		
Mean Hourly Manual Wage (2012 USD)	0.51	0.60	1.51
R-Squared	0.05	0.07	0.08
Observations	387	422	317

Notes: Standard errors are clustered at the pre-program village level. All regressions include birth year fixed effects. The sample includes males from the main analysis sample who have a nonzero wage. The dependent variable is the average hourly wage of an individual's primary activity in 2012 USD. One observation with a wage above $50/hour was trimmed as an outlier.

TABLE B4—COMMON OCCUPATIONS BY CATEGORY

Manual	*Professional & Semi-Professional*
Carpenter, skilled house builder, supervisor, house contractor, mason	Owner of small business, shop, or moneylending business
Garment factory worker	Shop worker
Skilled home finish or repair	Business and administration associate
Driver of baby taxi / CNG / autorickshaw / tempo / tractor	Science, engineering, and technology associate professional or technician
Garment and related trade workers	Other technician or associate professional
Other factory machine operator	Restaurant worker
Other daily laborer or elementary worker	Other skilled professional
Woodworking	Management professional in business, non-profit, or government
Agricultural laborer	Director, chief executive, or senior manager in large business or NGO
Handicraft worker	Hair cutter or other personal service provider
Rickshaw / bicycle van driver	Owner of large/medium business, shop, or moneylending business
Driver of heavy equipment	Hotel or tourism worker
Electrical and electronic appliance repair, maintenance, installation	Other clerk
Driver of car, van or motorcycle, motor boat	Religious Professional
Tutor	
Food processing factory worker	*Agriculture*
Sheet and structural metal supervisor, molders and welders	Farmer (own farm)
Caretaker, gardener, messenger, or doorman in home or office	Fishing in river or sea
Construction or earth-work laborer (non-food for work)	Farmer (sharecropper)
Machinery mechanics and repair	Raising cows, goats, sheep
Food processing	Other agriculture or forestry production
Domestic worker in home or office	Raising ducks or hens
Bearers and peons	Fish farm or fish hatchery

Notes: The table only reports occupations that account for more than 1% of the occupation group among sample men.

Appendix C
Potential Confounders

There are always potential confounders for any evaluation, but they may be especially salient for a long-term evaluation. This paper benefits from the rich availability of data, as well as, the long-term presence of icddr,b in the field to be able to control for confounders that could potentially be correlated with the placement of the MCH-FP intervention and affect individuals' health, human capital attainment, and labor market opportunities. These include an irrigation project, access to primary and secondary school, access to health facilities and practitioners, exposure to a BRAC microfinance experiment, and difference in arsenic exposure.

One potential confounder is the Meghna Dhonnogoda Irrigation Project. In 1987 the government of Bangladesh completed this project, which involved constructing a river embankment along the northern bank of the major Meghna River where it meets the west bank of the smaller Dhonnogoda River, which runs through Matlab (see Figure 1). The villages near this project were all located in the comparison areas, and the embankment had two important consequences for these villages. First, seven villages in this area lining the river were partially or fully inundated as part of the embankment project between 1984 and 1986. All households in these villages were displaced, with most initially relocating to adjoining villages within the comparison area. Second, owing to the size and strength of the Meghna River, the embankment was relocated mid project to a more stable position farther from the river, so there are a number of villages in the Meghna area between the river and the embankment that are more likely to suffer from flooding. Indeed, there were major floods on this river in 1987 and 1988. Migration rates were slightly higher in general in these two areas before the embankment project because of more frequent flooding. To control for potential differences in the Meghna area in general, we include two variables indicating whether a person's treatment village was submerged as a result of the project or was not submerged but was between the Meghna River and the embankment.

To control for differences in access to schooling and healthcare, we use the school facility data from MHSS2 to create a variety of controls. Every school in the study site was surveyed, and we observe the school's type and establishment date. Similar information was collected on schools that had closed prior to the survey. We take advantage of the timing of school placement to allow for the schooling control to vary at the individual level. We construct indicators for whether an individual's treatment village had a primary (secondary) school in the year they turned age six (eleven).

Data on access to healthcare come from the MHSS1 Village Survey and MHSS2 Community and Facility Survey. MHSS1 surveyed village leaders about health facilities used by people from their village, and MHSS2 surveyed prominent women in each village about the location of different types of health facilities used by people in their village in 2013. We construct indicators variables for the presence of different types of clinics.

Another potential confounder is the rollout of a microfinance program in the study site. During the 1990s, BRAC introduced microfinance loans in a subset of the study site. The rollout was designed to be orthogonal to the placement of MCH-FP, so it is unlikely that the presence of the program would bias the result but important to check. We include indicators for whether the treatment village participated in an experimental period of BRAC from 1991 to 1999 or whether BRAC was present at the individual's age 11 (secondary school age).

Finally, we control for differences in arsenic exposure. This control is created using 2003 measures of arsenic in tube well water. These data were collected by icddr,b. Wells are linked to MHSS1 households using the ID of the person who takes care of the well. For household who don't take care of a well, we take the average arsenic level in the 3 closest wells. For households that reported not using a tubewell in MHSS1 (which was prior to knowing about arsenic in the well), the value of arsenic is set to zero. Arsenic is measured in parts billion (micrograms per liter). Results are similar across various methods of including the control (i.e. as a continuous variable, binary based on cut off of 100, 150 or 200). For households that reported not using a tubewell in MHSS1 (which was prior to knowing about arsenic in the well), the value of arsenic is set to zero, as arsenic is found in tubewell water in Bangladesh. We use the 2003 measure or arsenic rather than the one collected in 2010 because it was measured prior to knowledge or arsenic in the well, so before families engaged in well switching which could be correlated with treatment status, and since it was measured at a time closer to when the sample of interest were young children. Note a majority of the children in the sample were born after the wells were established, so the birth-year fixed-effects control for the length of time exposed to the well water.

Tables D2 and D5 panel F repeat our earlier main analysis but include these extended controls. As with our baseline characteristics, these controls are fully interacted with our age group dummies. Our main results remain unchanged with the inclusion of these controls.

Appendix D
Robustness Analysis

We perform a number of robustness checks to examine the validity and inference of the results for men (Tables D1–D4) and for women (Table D5–D6) for key outcome variables. For the men, we focus on the main outcomes variables from Tables 2, 3, and 4 including type of work, required skills, earnings, hours worked, and migration. For the women, we consider the main outcomes from Tables 10 and 11. We recreate the main results in panel A of Table D2 and Table D5. Results remain similar unless noted. In addition, in Appendix F, we test the robustness to a number of different weights. In sum, the findings reported in the paper are similar across a variety of robustness checks, methods for inference, and types of weights.

In section IV.A we showed that it is unlikely that the findings are a result of pre-program imbalance between the treatment and comparison areas. In section V.C we provide other tests to show the two experimental areas are similar, further document that the results are not likely to be due to differences between the treatment and comparison areas that happened after the start of the program, or that the treatment and comparison areas are in distinct labor market. In this appendix, we expand the discussion for some of the robustness checks in section V.C, include additional robustness checks, and provide details on the tests for statistical inference.

Labor Market Robustness. It is possible the results are affected by how we define occupations. Our broad definition of the professional/semi-professional occupation includes small shop workers and small shop owners, such as owners of tea stands, which may not be viewed as better quality jobs. Table D3 demonstrates that results are robust to excluding small shop workers and owners from the occupation, and only including professional occupations.

In addition, it is possible that treatment children inherited their jobs from their fathers who were more likely to stay in Matlab, rather than migrate for work, when the program rolled out (Barham and Kuhn 2014). We examine whether fathers themselves were more likely to work in a professional/semiprofessional occupation in 1996 using MHSS1 (Table D3). These young men do not appear to have inherited their better job from their fathers as there are no positive effects.

Extended Controls, changes over time in Matlab. Changes over time in the study area include the introduction of an embankment as part of the Meghna Dhonnagoda irrigation project in the 1980s, and the introduction of a BRAC microcredit program in the early 1990s (see Appendix C for more details on these programs and the construction of the control variables). BRAC microcredit was introduced in a crossover design with the MCH-FP program in the 1990s and then became available in other villages, limiting potential biases. There was also an expansion of education during this time, including construction of primary and secondary schools as well as scholarships for girls. Finally, differential exposure to arsenic and healthcare throughout one's lifetime could potentially confound our results. Indeed, there was some imbalance in access to tubewell water at baseline, and tubewells allow arsenic to leach into the water. As our identification strategy relies on the assumption that the comparison area provides a good counterfactual for the treatment area over time, these changes could potentially bias the results. We include household level controls for arsenic exposure in 2003, and village-level controls from the 1990s and from MHSS2 for each of the other potential confounders. The controls from the 1990s adjust for access to these other programs when the sample were children, and the MHSS2 controls, when they are adults. Results are reported in Tables D2 and D5 (panel F). Again, the results are qualitatively the same.

Limited Set of Controls. Our main specification controls for a large number of baseline household characteristics with the dual purpose of controlling for potential imbalances across treatment and comparison households and improving statistical precision by controlling for characteristics related to the outcome variable. However, it is not clear *ex ante* which variables should be included, if any. Belloni, Chernozhukov, and Hansen (2014) propose a data driven approach to select a sparse set of controls when the goal is estimating causal parameters. Tables D2 and D5 (panel G) report results from a post-double selection LASSO procedure that remain robust to using a sparser set of controls.[1] Results are similar for a model that only includes baseline controls that were not balanced between experimental areas (results not reported).

Micro-credit. Micro-credit has been pervasive in both the treatment and comparison areas for some time and there is some concern that this could impact the results for women. In the robustness checks, we include an extended set of controls, that includes controls for access to micro-credit in the 1990s and currently and the results remain the same (Table D5, Panel F). In addition, we examine interaction effects with access to micro-credit when it was first rolled out between 1993 and 1996 and do not find that results vary by a village's early access to micro-credit (results not reported).

Muslim Only. The baseline balance table revealed imbalance in the treatment and comparison area by religion. To determine if this imbalance affects the results, we restrict the sample to only those who report their religion as Muslim. There is insufficient sample size on those who reported Hindu as their religion to run results separately for this religious group. Results are reported in Tables D2 and D5 (panel H) and show the results remain the same.

Spatially Correlated Errors. Because the treatment and comparison areas are contiguous, it is possible that errors are spatially correlated in either the treatment or the comparison area. This could arise, for example, if there was a health shock such as a disease outbreak or a flood that led to migration in a given year in one of the experimental areas but not the other. Clustering at the village level is not sufficient to correct for the resulting lack of independence. To examine this possibility, we test whether the error terms from the regressions on migration, type of job, and income are spatially correlated, using Moran's I test with the Euclidean distance between village centroids as a weight. We perform the test at the village level, and create village level error terms by predicting the errors from our main model, and averaging the errors at the village level separately for each cohort. We find no evidence of spatial correlation in the error terms (results not reported).

[1] We implement the post-double selection procedure using the pdslasso command in Stata. For the 24–30 men, the procedure selects indicators for religion, tin roof, tank drinking water, and latrine, bari size, family size, the number of rooms per capita, age of the household head, age of the household head's spouse, education of the household head's spouse, and the share of the bari that migrated prior to the program for all outcomes. For some outcomes, indicators for tin or tinmix walls and tubewell water and the household head's education are also included. For the 31–34 men, the procedure selects indicators for religion, tin roof, and latrine, the number of rooms per capita, the age of the household head, the age of the household head's spouse for all outcomes. For some outcomes, an indicator for HH occupation in fishing, the number of cows, and the years of education of the household head's spouse are also selected.

Clustering Standard Errors. In our main specification, we conduct inference by clustering standard errors at the pre-treatment village level. This choice allows for potential correlation between individuals from the same village. The implementation of the program occurred within six blocks of villages across the study site (four comparison blocks and two treatment blocks). To allow for potential correlation in errors across these broader geographic areas, we further conduct inference by clustering standard errors at this level. We use the wild cluster bootstrap sampling method because of the small number of blocks (Cameron and Miller 2015). P-values are reported in Table D4 and statistical significance is almost identical.

Randomization Inference. With any assignment of village-level treatment status, significant treatment effects could occur simply by chance. Following Athey and Imbens (2017), we simulate the distribution of treatment effects that would occur from randomly assigning a fixed number of villages to treatment. We take two approaches in re-assigning treatment status. In the first approach, we randomly select a fixed number of villages. Inference from this approach is reported in Tables D4 and D6 under "Rand Inf. – Any Village". The second approach takes into account the fact that the treatment area was a contiguous block of villages. Using a map of Matlab, we identify all neighboring villages for each village.[2] We construct 10,000 new treatment assignments using the following procedure. First, randomly select a village from which to grow the treatment area. Then, identify all neighboring villages of that seed village and randomly select one of them to be assigned to the treatment area. Finally, continue identifying the neighbors and of the growing treatment area and randomly add one to the treatment area until the number of selected villages equals the actual number of treatment villages. Inference from this approach is reported in Tables D4 and D6 under "Rand Inf. – Contiguous Area". To test the sharp null hypothesis of no treatment effect for any person, we estimate single-difference treatment effects over the two sets of simulated treatment assignments. Tables D4 and D6 report the p-values for men and women, respectively. They are calculated as the share of trials where the simulated t-statistic is larger in absolute value than the actual t-statistic. For most outcomes, the level of significance remains the same between p-values constructed from clustered errors and randomization-based p-values. The p-values for skills used and urban migration are larger when simulating treatment using a contiguous area.

Multiple Hypothesis Testing. We adjusted p-values for multiple hypothesis testing using an approach that controls for the false discovery rate following Anderson (2008), and report and discuss these when the results are presented.

Bounding Attrition Bias. Tables D2 and D5 report results that aim to bound potential bias from survey attrition. Panels I and J report results from the worse-case scenario. The lower bound is constructed by assigning treatment (comparison) attritors the minimum (maximum) value of the outcome in the sample. For binary outcomes, this approach assigns each treatment (comparison) individual that value of 0 (1). The upper bound is constructed analogously, assigning treatment (comparison) attritors the outcome maximum (minimum). Following Kling and Liebman (2004), we also construct bounds by assigning attritors the sample mean of the outcome, +/- one standard deviation. Significant results in most cases are bounded away from zero, or at least close to zero.

[2] The map of Matlab we used does not include seven villages that were inundated with flooding as a result of the embankment project (see Appendix C). Thus, we simulate treatment over the remaining 142 villages, assigning 70 to the treatment area. For the few individuals in our data from the flooded villages, we assign simulated treatment based on their relocation village, which was typically an adjoining village.

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TABLE D1—ITT EFFECTS ON MHSS1 OCCUPATION AND EARNINGS, ROBUSTNESS CHECKS, MEN

	Same-Aged Respondents in MHSS1 (1996)		
	Prof. & Semi-Prof. Occupation	Agriculture Occupation	Earnings Past 12 Months (2012 USD)
	(1)	(2)	(3)
Single Differences			
Treat*(Age 24–30)	0.06	-0.09	8.95
	(0.07)	(0.07)	(17.16)
Treat*(Age 31–34)	0.00	0.01	5.73
	(0.07)	(0.09)	(24.88)
Age 24–30 Means	0.23	0.38	114
Age 31–34 Means	0.32	0.30	138
Observations	502	502	555

Notes: The sample includes male respondents to MHSS1 who were aged 24–34 at the time of the 1996 survey. Standard errors are clustered at the pre-program village level. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1.
** p<0.01, * p<0.05, + p<0.1

TABLE D2—ITT EFFECTS, MEN, ROBUSTNESS CHECKS

	Second Job	Occupation			Start Own Business	Skills Used	Earnings (USD) Trim 5%	Hours Worked	Work Location		
		Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math			Outside Matlab	Intl.	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Base Results											
Treat*(Age 24-30)	0.03	0.10	0.01	-0.06	0.09	0.08	0.56	-8.99	-0.11	-0.02	-0.09
	(0.03)	(0.04)**	(0.02)	(0.04)+	(0.04)**	(0.04)*	(108.85)	(93.21)	(0.04)**	(0.03)	(0.04)*
Treat*(Age 31-34)	0.08	-0.01	0.09	-0.01	0.02	-0.05	-460.87	-143.16	-0.10	-0.09	-0.02
	(0.04)*	(0.05)	(0.04)*	(0.05)	(0.04)	(0.05)	(151.81)**	(113.56)	(0.05)*	(0.04)+	(0.06)
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,181	1,287	1,299	1,299	1,299
Panel B: <3km of border											
Treat*(Age 24-30)	0.02	0.12	-0.00	-0.07	0.10	0.08	-109.29	-29.40	-0.09	-0.04	-0.05
	(0.03)	(0.05)*	(0.03)	(0.04)+	(0.04)*	(0.05)	(135.63)	(107.96)	(0.04)*	(0.04)	(0.05)
Treat*(Age 31-34)	0.08	-0.02	0.12	-0.01	0.02	-0.01	-558.92	25.90	-0.11	-0.11	-0.03
	(0.04)+	(0.06)	(0.04)**	(0.06)	(0.05)	(0.06)	(192.17)**	(146.71)	(0.06)*	(0.06)*	(0.06)
Observations	886	886	886	886	886	886	805	879	886	886	886
Panel C: Treatment vs. Northern Comparison Area											
Treat*(Age 24-30)	0.02	0.09	-0.01	-0.03	0.11	0.07	167.33	2.79	-0.09	-0.01	-0.08
	(0.03)	(0.04)*	(0.03)	(0.05)	(0.04)*	(0.05)	(131.77)	(109.17)	(0.04)*	(0.04)	(0.05)
Treat*(Age 31-34)	0.11	0.08	0.10	-0.09	0.08	-0.04	-363.69	-232.56	-0.10	-0.10	0.02
	(0.05)*	(0.06)	(0.05)+	(0.06)	(0.06)	(0.06)	(164.00)*	(131.40)+	(0.06)	(0.05)*	(0.08)
Observations	937	937	937	937	937	937	860	930	937	937	937
Panel D: Treatment vs. Western Comparison Area											
Treat*(Age 24-30)	0.06	0.10	0.05	-0.08	0.09	0.10	-160.21	-63.88	-0.14	-0.02	-0.11
	(0.03)+	(0.04)*	(0.02)*	(0.04)+	(0.04)*	(0.04)*	(126.99)	(110.56)	(0.05)**	(0.04)	(0.05)*
Treat*(Age 31-34)	0.06	-0.07	0.11	0.03	-0.04	-0.06	-529.97	-86.55	-0.10	-0.05	-0.06
	(0.05)	(0.06)	(0.04)*	(0.05)	(0.05)	(0.06)	(192.40)**	(149.72)	(0.06)+	(0.05)	(0.06)
Observations	950	950	950	950	950	950	867	943	950	950	950

TABLE D2—ITT EFFECTS, MEN, ROBUSTNESS CHECKS (CONT.)

	Second Job	Occupation: Prof. & Semi-Prof.	Occupation: Ag	Occupation: Manual	Start Own Business	Skills Used: Reading, Writing, Math	Earnings (USD) Trim 5%	Hours Worked	Work Location: Outside Matlab	Work Location: Intl.	Work Location: Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel E: Exclude Matlab Town											
Treat*(Age 24-30)	0.02	0.09	0.02	-0.08	0.09	0.10	-27.93	-110.91	-0.12	-0.00	-0.10
	(0.03)	(0.04)*	(0.03)	(0.04)+	(0.04)*	(0.04)*	(124.68)	(109.20)	(0.04)**	(0.04)	(0.05)*
Treat*(Age 31-34)	0.11	0.00	0.11	-0.02	0.02	-0.02	-324.06	-124.06	-0.13	-0.05	-0.07
	(0.05)*	(0.06)	(0.05)*	(0.05)	(0.05)	(0.05)	(165.05)+	(121.63)	(0.05)*	(0.05)	(0.06)
Observations	1,047	1,047	1,047	1,047	1,047	1,047	943	1,036	1,047	1,047	1,047
Panel F: Extended Controls											
Treat*(Age 24-30)	0.07	0.11	0.04	-0.08	0.11	0.04	-117.86	-22.25	-0.14	-0.03	-0.11
	(0.03)*	(0.04)**	(0.02)+	(0.04)+	(0.04)**	(0.04)	(134.43)	(105.05)	(0.04)**	(0.04)	(0.05)*
Treat*(Age 31-34)	0.06	-0.03	0.10	-0.01	-0.02	-0.03	-421.65	-148.63	-0.12	-0.10	-0.04
	(0.04)	(0.05)	(0.04)*	(0.05)	(0.04)	(0.05)	(155.37)**	(135.28)	(0.06)*	(0.04)*	(0.05)
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,181	1,287	1,299	1,299	1,299
Panel G: Limiting baseline controls using Post-Double Selection LASSO											
Treat*(Age 24-30)	0.05	0.12	0.04	-0.09	0.11	0.08	-83.30	-17.02	-0.13	-0.01	-0.11
	(0.03)+	(0.04)**	(0.02)+	(0.04)*	(0.03)**	(0.04)*	(126.58)	(96.86)	(0.05)**	(0.03)	(0.04)**
Treat*(Age 31-34)	0.07	0.05	0.08	-0.07	0.02	-0.06	-378.08	-65.62	-0.12	-0.09	-0.05
	(0.04)+	(0.05)	(0.04)*	(0.05)	(0.05)	(0.05)	(144.76)**	(110.86)	(0.05)*	(0.04)*	(0.05)
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,181	1,287	1,299	1,299	1,299
Panel H: Only Muslims											
Treat*(Age 24-30)	0.03	0.10	0.01	-0.05	0.09	0.07	9.31	-24.34	-0.12	-0.01	-0.11
	(0.03)	(0.03)**	(0.02)	(0.04)	(0.04)*	(0.04)+	(109.36)	(99.37)	(0.04)**	(0.03)	(0.05)*
Treat*(Age 31-34)	0.10	-0.01	0.11	0.00	0.03	-0.06	-526.79	-87.66	-0.11	-0.08	-0.03
	(0.04)*	(0.05)	(0.04)**	(0.05)	(0.05)	(0.05)	(163.91)**	(118.10)	(0.05)*	(0.05)+	(0.06)
Observations	1,194	1,194	1,194	1,194	1,194	1,194	1,080	1,182	1,194	1,194	1,194

TABLE D2—ITT EFFECTS, MEN, ROBUSTNESS CHECKS (CONT.)

	Second Job	Occupation			Start Own Business	Skills Used	Earnings (USD) Trim 5%	Hours Worked	Work Location		
		Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math			Outside Matlab	Intl.	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel I: Worse Case Bounds - Lower Bound											
Treat*(Age 24-30)	-0.05	0.01	-0.07	-0.14	0.01	-0.00	-917.74	-605.44	-0.18	-0.10	-0.16
	(0.03)+	(0.03)	(0.03)*	(0.03)**	(0.03)	(0.03)	(115.53)**	(116.05)**	(0.04)**	(0.03)**	(0.04)**
Treat*(Age 31-34)	-0.03	-0.09	-0.02	-0.11	-0.08	-0.15	-1,457.36	-1,050.86	-0.17	-0.19	-0.12
	(0.04)	(0.04)*	(0.04)	(0.04)*	(0.04)*	(0.05)**	(158.75)**	(165.01)**	(0.05)**	(0.04)**	(0.05)*
Observations	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423
Panel J: Worse Case Bounds - Upper Bound											
Treat*(Age 24-30)	0.11	0.17	0.09	0.02	0.16	0.15	866.65	553.87	-0.03	0.06	-0.00
	(0.03)**	(0.03)**	(0.02)**	(0.04)	(0.04)**	(0.03)**	(125.34)**	(99.43)**	(0.04)	(0.03)+	(0.04)
Treat*(Age 31-34)	0.15	0.09	0.16	0.07	0.10	0.03	501.33	497.78	0.00	-0.01	0.06
	(0.04)**	(0.05)*	(0.04)**	(0.04)+	(0.04)*	(0.04)	(167.51)**	(122.22)**	(0.04)	(0.04)	(0.05)
Observations	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423
Panel K: Kling-Liebman Bounds - Lower Bound											
Treat*(Age 24-30)	-0.02	0.02	-0.03	-0.14	0.03	0.01	-451.05	-237.09	-0.19	-0.09	-0.16
	(0.02)	(0.03)	(0.02)	(0.03)**	(0.03)	(0.03)	(95.74)**	(92.36)*	(0.04)**	(0.03)**	(0.04)**
Treat*(Age 31-34)	0.01	-0.08	0.03	-0.11	-0.06	-0.13	-910.59	-343.83	-0.19	-0.17	-0.11
	(0.04)	(0.04)+	(0.04)	(0.04)**	(0.04)	(0.05)**	(129.71)**	(107.99)**	(0.05)**	(0.04)**	(0.05)*
Observations	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423
Panel L: Kling-Liebman Bounds - Upper Bound											
Treat*(Age 24-30)	0.09	0.17	0.06	0.02	0.16	0.16	421.98	218.45	-0.04	0.05	-0.01
	(0.03)**	(0.03)**	(0.02)**	(0.04)	(0.03)**	(0.03)**	(100.59)**	(86.19)*	(0.03)	(0.03)	(0.04)
Treat*(Age 31-34)	0.15	0.10	0.16	0.06	0.11	0.03	68.79	114.67	-0.01	-0.01	0.06
	(0.04)**	(0.05)*	(0.04)**	(0.04)	(0.04)*	(0.04)	(135.31)	(103.92)	(0.04)	(0.04)	(0.05)
Observations	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423	1,423

Notes: Standard errors are clustered at the pre-program village level. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition. Individual characteristics include year of birth fixed effects, age cohort fixed effects and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Panel A reports the baseline specification. Panel B restricts the sample individuals whose pre-program village is within 3km of the treatment border. Panels C and D restrict the set of comparison individuals to those from the northern and western comparison blocks, respectively. Panel E excludes individuals whose pre-program village is Matlab Town. Panel F controls for changes in study site over time, interacted by birth cohort. See Appendix C for details. Panel G selects controls using a post-double selection LASSO procedure. Panel H restricts the sample to Muslims. Panels I and J report estimates based on worse-case Scenario bounds, assigning minimum and maximum values of outcomes to attritors, differently by treatment status. Panels K and L report Kling-Liebman Bounds, assigning attritors the mean value of the outcome +/- one standard deviation, differently by treatment status.

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

TABLE D3—ITT EFFECTS ON TYPE OF WORK, ROBUSTNESS CHECKS, MEN

	Professional & Semi-Professional (=1)			Father Had Prof. or Semi Prof. Job in 1996 (MHSS1) (=1)
	Main	Remove Small Shops	Professional Only	
	(1)	(2)	(3)	(4)
Panel A: Single Differences				
Treat*(Age 24–30)	0.10	0.07	0.07	-0.01
	(0.04)**	(0.02)**	(0.03)**	(0.04)
Treat*(Age 31–34)	-0.01	-0.01	0.01	-0.03
	(0.05)	(0.04)	(0.04)	(0.06)
Panel B: Percent Changes				
Treat*(Age 24–30)	31%	45%	78%	-2%
Treat*(Age 31–34)	-2%	-7%	9%	-9%
Age 24–30 Comp. Means	0.33	0.16	0.09	0.36
Age 31–34 Comp. Means	0.39	0.15	0.12	0.33
Observations	1,299	1,299	1,299	1,197

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Column (1) is the main result from Table 1. Column (2) removes those who work in a small shop. Column (3) indicates whether the respondent works in a professional occupation. Column (4) indicates whether the respondent's father was in a professional, clerical, or sales occupation in 1996 from MHSS1.

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

TABLE D4—ITT EFFECTS, MEN, INFERENCE ROBUSTNESS CHECKS

	Have a Second Job (=1)	Occupation (=1)			Start Own Business (=1)	Skills Used	Earnings (USD) Trim 5%	Hours Worked	Primary Job Location (=1)		
		Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math (=1)			Outside Matlab	Intl.	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: P-Values for Age 24-30											
Naïve P-value	0.201	0.005	0.550	0.085	0.008	0.033	0.996	0.923	0.003	0.642	0.038
Block-Level Wild Cluster Bootstrap	0.180	0.006	0.723	0.013	0.014	0.034	0.997	0.934	0.039	0.302	0.038
Rand Inf. – Any Village	0.187	0.006	0.567	0.101	0.010	0.035	0.996	0.924	0.003	0.642	0.037
Rand Inf. – Contiguous Area	0.124	0.001	0.800	0.067	0.017	0.162	0.998	0.977	0.008	0.767	0.199
FDR Correction	0.237	0.048	0.491	0.133	0.048	0.089	0.633	0.614	0.048	0.491	0.089
Panel B: P-Values for Age 31-34											
Naïve P-value	0.046	0.916	0.029	0.773	0.598	0.304	0.003	0.210	0.032	0.054	0.657
Block-Level Wild Cluster Bootstrap	0.010	0.935	0.015	0.810	0.817	0.366	0.059	0.278	0.085	0.046	0.686
Rand Inf. – Any Village	0.063	0.930	0.052	0.792	0.593	0.332	0.004	0.226	0.054	0.058	0.697
Rand Inf. – Contiguous Area	0.228	0.954	0.034	0.853	0.631	0.195	0.004	0.353	0.031	0.073	0.759
FDR Correction	0.225	0.846	0.222	0.725	0.725	0.575	0.058	0.389	0.222	0.225	0.725

Notes: All robustness checks are based on the main single-difference estimation equation. FDR is the false discovery rate. Panel A and Panel B report p-values for age 24–30 men and age 31–34 men, respectively, using the main regression specification. Each panel reports p-values from (i) standard errors clustered by pre-treatment village, (ii) standard errors from a wild cluster bootstrap where clusters are pre-treatment village blocks, (iii) randomization inference where a distribution of test statistics is constructed by reassigning treatment status to villages over 10,000 permutations, (iv) a similar randomization inference procedure, but permuting treatment assignment in order to maintain a geographically contiguous treatment area, and (v) adjusted p-values that control for the false discovery rate (Anderson 2008) from multiple hypothesis testing across outcomes reported in Tables 2–4 and in square brackets in the main tables.

TABLE D5—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS

	Any Paid (=1)	Occupation (=1)				Primary Location (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
		Prof. & Semi-Prof	Ag	Manual	Unpaid HH Work	Outside Matlab	Urban	Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Base Results										
Treat*(Age 24-30)	0.07	0.01	0.07	-0.01	-0.06	-0.04	-0.03	-0.02	0.06	0.06
	(0.03)*	(0.01)	(0.02)**	(0.03)	(0.03)+	(0.04)	(0.04)	(0.02)	(0.03)*	(0.03)+
Treat*(Age 31-34)	0.04	-0.01	0.03	0.03	-0.04	-0.11	-0.10	-0.02	0.05	0.05
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)*	(0.04)*	(0.03)	(0.05)	(0.05)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214
Panel B: <3km of border										
Treat*(Age 24-30)	0.07	0.00	0.06	-0.00	-0.05	-0.03	-0.02	-0.03	0.07	0.06
	(0.04)+	(0.02)	(0.03)*	(0.03)	(0.04)	(0.05)	(0.04)	(0.03)	(0.04)+	(0.04)
Treat*(Age 31-34)	0.03	-0.00	-0.01	0.03	-0.03	-0.12	-0.08	-0.04	0.03	0.04
	(0.06)	(0.03)	(0.05)	(0.05)	(0.06)	(0.07)+	(0.05)	(0.04)	(0.06)	(0.07)
Observations	832	832	832	832	832	832	832	832	826	828
Panel C: Treatment vs. Northern Comparison Area										
Treat*(Age 24-30)	0.08	-0.00	0.08	0.00	-0.08	-0.09	-0.03	-0.06	0.07	0.04
	(0.04)*	(0.02)	(0.03)**	(0.03)	(0.04)+	(0.05)+	(0.04)	(0.03)+	(0.03)*	(0.04)
Treat*(Age 31-34)	0.09	-0.02	0.07	0.05	-0.11	-0.17	-0.11	-0.06	0.08	0.09
	(0.06)	(0.03)	(0.05)	(0.05)	(0.06)+	(0.07)*	(0.05)*	(0.04)	(0.06)	(0.06)
Observations	892	892	892	892	892	892	892	892	885	887
Panel D: Treatment vs. Western Comparison Area										
Treat*(Age 24-30)	0.03	0.01	0.04	-0.03	-0.01	0.02	-0.03	0.04	0.06	0.08
	(0.04)	(0.02)	(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.02)*	(0.04)	(0.04)+
Treat*(Age 31-34)	0.00	0.01	-0.01	0.03	0.01	-0.06	-0.09	0.03	0.04	0.02
	(0.06)	(0.03)	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.04)	(0.06)	(0.06)
Observations	881	881	881	881	881	881	881	881	873	877

TABLE D5—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS, (CONT.)

	Any Paid (=1)	Occupation (=1)				Primary Location (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
		Prof. & Semi-Prof	Ag	Manual	Unpaid HH Work	Outside Matlab	Urban	Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel E: Exclude Matlab Town										
Treat*(Age 24-30)	0.06	0.00	0.07	-0.02	-0.06	-0.05	-0.04	-0.01	0.05	0.06
	(0.03)+	(0.01)	(0.03)**	(0.03)	(0.04)+	(0.05)	(0.04)	(0.03)	(0.03)	(0.04)+
Treat*(Age 31-34)	-0.01	-0.01	0.02	0.00	-0.00	-0.12	-0.12	0.00	0.02	0.06
	(0.05)	(0.02)	(0.05)	(0.04)	(0.06)	(0.06)+	(0.05)*	(0.03)	(0.05)	(0.06)
Observations	1,002	1,002	1,002	1,002	1,002	1,002	1,002	1,002	991	996
Panel F: Extended Controls										
Treat*(Age 24-30)	0.04	0.00	0.06	-0.02	-0.04	-0.03	-0.03	-0.01	0.08	0.07
	(0.04)	(0.02)	(0.02)*	(0.03)	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)*	(0.04)+
Treat*(Age 31-34)	-0.02	-0.01	0.03	-0.01	0.00	-0.14	-0.13	-0.02	0.03	0.05
	(0.06)	(0.03)	(0.04)	(0.04)	(0.06)	(0.05)*	(0.05)*	(0.03)	(0.06)	(0.07)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214
Panel G: Post-Double Selection LASSO										
Treat*(Age 24-30)	0.06	0.00	0.06	0.00	-0.06	-0.03	-0.01	-0.02	0.08	0.07
	(0.03)+	(0.02)	(0.02)*	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)*	(0.04)+
Treat*(Age 31-34)	-0.02	-0.02	-0.01	0.01	0.02	-0.08	-0.09	-0.01	0.02	0.05
Panel F+B41	(0.05)	(0.02)	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)+	(0.03)	(0.06)	(0.05)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214
Panel H: Only Muslims										
Treat*(Age 24-30)	0.05	0.01	0.06	-0.02	-0.04	-0.03	-0.02	-0.01	0.06	0.07
	(0.03)	(0.01)	(0.02)**	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.03)*	(0.03)*
Treat*(Age 31-34)	0.04	-0.02	0.02	0.03	-0.04	-0.14	-0.13	-0.01	0.04	0.06
	(0.05)	(0.02)	(0.04)	(0.04)	(0.06)	(0.06)*	(0.05)**	(0.03)	(0.05)	(0.05)
Observations	1,106	1,106	1,106	1,106	1,106	1,106	1,106	1,106	1,095	1,100

TABLE D5—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS, (CONT.)

	Any Paid (=1)	Occupation (=1)				Primary Location (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
		Prof. & Semi-Prof	Ag	Manual	Unpaid HH Work	Outside Matlab	Urban	Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel I: Worse Case Bounds - Lower Bound										
Treat*(Age 24-30)	-0.03	-0.09	-0.04	-0.10	-0.13	-0.12	-0.12	-0.11	-0.04	-0.03
	(0.03)	(0.02)**	(0.02)	(0.03)**	(0.03)**	(0.04)**	(0.04)**	(0.03)**	(0.03)	(0.03)
Treat*(Age 31-34)	-0.03	-0.06	-0.03	-0.02	-0.11	-0.16	-0.15	-0.06	-0.03	-0.03
	(0.05)	(0.03)*	(0.04)	(0.04)	(0.05)*	(0.05)**	(0.04)**	(0.03)*	(0.05)	(0.05)
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Panel J: Worse Case Bounds - Upper Bound										
Treat*(Age 24-30)	0.13	0.07	0.13	0.06	0.04	0.04	0.05	0.05	0.13	0.13
	(0.03)**	(0.02)**	(0.02)**	(0.03)*	(0.03)	(0.04)	(0.03)	(0.02)*	(0.03)**	(0.03)**
Treat*(Age 31-34)	0.10	0.06	0.09	0.10	0.02	-0.04	-0.03	0.06	0.11	0.10
	(0.05)*	(0.03)*	(0.04)*	(0.04)**	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)*	(0.05)*
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Panel K: Kling-Liebman Bounds - Lower Bound										
Treat*(Age 24-30)	-0.01	-0.03	0.00	-0.07	-0.13	-0.12	-0.10	-0.06	-0.02	-0.01
	(0.03)	(0.01)*	(0.02)	(0.03)**	(0.03)**	(0.04)**	(0.03)**	(0.02)**	(0.03)	(0.03)
Treat*(Age 31-34)	-0.02	-0.03	-0.03	-0.01	-0.10	-0.17	-0.16	-0.04	-0.02	-0.03
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)*	(0.05)**	(0.04)**	(0.03)	(0.05)	(0.05)
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Panel L: Kling-Liebman Bounds - Upper Bound										
Treat*(Age 24-30)	0.14	0.04	0.12	0.05	0.03	0.04	0.04	0.03	0.14	0.14
	(0.03)**	(0.01)**	(0.02)**	(0.03)*	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)**	(0.03)**
Treat*(Age 31-34)	0.09	0.01	0.07	0.08	0.02	-0.05	-0.05	0.03	0.10	0.10
	(0.05)+	(0.02)	(0.04)+	(0.04)*	(0.06)	(0.06)	(0.04)	(0.03)	(0.05)*	(0.05)+
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321

Notes: Standard errors are clustered at the pre-program village level. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition. Individual characteristics include year of birth fixed effects, age cohort fixed effects and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Panel A reports the baseline specification. Panel B restricts the sample individuals whose pre-program village is within 3km of the treatment border. Panels C and D restrict the set of comparison individuals to those from the northern and western comparison blocks, respectively. Panel E excludes individuals whose pre-program village is Matlab Town. Panel F controls for changes in study site over time, interacted by birth cohort. See Appendix C for details. Panel G selects controls using a post-double selection LASSO procedure. Panel H restricts the sample to Muslims. Panels I and J report estimates based on worse-case Scenario bounds, assigning minimum and maximum values of outcomes to attritors, differently by treatment status. Panels K and L report Kling-Liebman Bounds, assigning attritors the mean value of the outcome +/- one standard deviation, differently by treatment status.
** p<0.01, * p<0.05, + p<0.1

TABLE D6—ITT EFFECTS, WOMEN, INFERENCE ROBUSTNESS CHECKS

	Any Paid (=1)	Occupation (=1)				Primary Location (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
		Prof. & Semi-Prof	Ag	Manual	Unpaid HH Work	Outside Matlab	Urban	Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: P-Values for Age 24-30										
Naïve P-value	0.040	0.705	0.003	0.709	0.087	0.269	0.420	0.441	0.028	0.074
Block-Level Wild Cluster Bootstrap	0.165	0.738	0.050	0.682	0.340	0.490	0.154	0.717	0.162	0.012
Rand Inf. – Any Village	0.046	0.700	0.004	0.712	0.098	0.322	0.442	0.423	0.034	0.069
Rand Inf. – Contiguous Area	0.087	0.733	0.013	0.644	0.223	0.424	0.480	0.836	0.023	0.072
FDR Correction	0.127	0.766	0.017	0.766	0.176	0.507	0.670	0.670	0.109	0.174
Panel B: P-Values for Age 31-34										
Naïve P-value	0.477	0.575	0.472	0.402	0.428	0.040	0.030	0.576	0.303	0.359
Block-Level Wild Cluster Bootstrap	0.521	0.527	0.519	0.225	0.513	0.181	0.049	0.636	0.510	0.292
Rand Inf. – Any Village	0.502	0.595	0.496	0.382	0.464	0.049	0.029	0.585	0.338	0.366
Rand Inf. – Contiguous Area	0.633	0.554	0.534	0.524	0.582	0.124	0.047	0.806	0.360	0.461
FDR Correction	1.000	1.000	1.000	1.000	1.000	0.516	0.516	1.000	1.000	1.000

Notes: All robustness checks are based on the main single-difference estimation equation FDR is the false discovery rate. Panel A and Panel B report p-values for age 24–30 women and age 31–34 women, respectively, using the main regression specification. Each panel reports p-values from (i) standard errors clustered by pre-treatment village, is constructed by reassigning treatment status to villages over 10,000 permutations, (iv) a similar randomization inference procedure, but permuting treatment assignment in order to maintain a geographically contiguous treatment area, and (v) adjusted p-values that control for the false discovery rate (Anderson 2008) from multiple hypothesis testing across outcomes reported in Tables 2–4.

Appendix E
Local Variation in Food Prices within Matlab

In this section, we explore the possibility that food prices vary between the treatment and comparison areas. We use market prices of various food items (the same ones for which we have consumption data) from the MHSS2 Community Survey, collected longitudinally in markets throughout Bangladesh during the administration of the Household Survey.

We have market price data for six separate markets within the study site. Prices were collected at nine points in time, semi-regularly between January 2013 and September 2014. We determine the treatment status of the market by considering the share of treatment area households that are closest to the market.[1] For each household in our data, we identify the closest market within the market price survey using the distance between the household centroid and the market centroid. This delineation results in three markets in the comparison area, two markets in the treatment area, and one market that serves equal shares of treatment and comparison households.

For each good, we construct prices using a common unit of measurement (e.g., kilogram, liter, one unit). Prices are collected only if the item was in stock at the shop. For many items, prices were collected both for a given size (kilogram/liter) and for one piece. In the latter case, the piece was measured and the weight/volume of that piece was recorded. To construct prices for a common unit size, first the price is recorded for the given size (if available), then fill in with the collected piece price, converted to the common size.

We test for a difference in prices between the three types of market areas—treatment, comparison, and shared—by estimating the following linear regression:

$$\ln p_{ist} = \beta_0 + \beta_1 L_s^{treat} + \beta_2 L_s^{shared} + \delta_i + \tau_t + \epsilon_{ist} \,,$$

where p_{it} is the price of item i in shop s collected during month t. L_s^{treat} and L_s^{shared} indicate whether shop s is located in the treatment area or in an area that serves both treatment and comparison area households, respectively. δ_i and τ_t are item and phase fixed effects. β_1 and β_2 are our coefficients of interest and represent the within-item percent difference in prices between the treatment/shared areas and the comparison areas.

Table E1 column 1 presents estimates with item fixed effects, and column 2 nonparametrically controls for time trends in prices by including month fixed effects. In both cases the point estimates are small and statistically insignificant, indicating that food prices are similar between the treatment and comparison areas.

[1] Unlike our main analysis, which uses an individual's 1974 treatment status, this analysis considers a household's treatment status given his or her current village from MHSS2.

TABLE E1—WITHIN-ITEM PERCENT DIFFERENCES IN PRICES, TREATMENT VS. COMPARISON AREAS

	Log Price (1)	Log Price (2)
=1 if Treatment Area	0.017	0.026
	(0.017)	(0.017)
=1 Shared Market	-0.005	0.005
	(0.022)	(0.022)
Item FE	Y	Y
Survey Period FE	-	Y
R-squared	0.789	0.794
Observations	4,783	4,783

Notes: An observation is a consumption item observed across markets and time periods. The dependent variable is the log price of the consumption item. Column (1) includes item fixed effects. Column (2) adds survey month fixed effects. Estimates represent the within-item percent difference in prices between the stated market and the comparison area.

Appendix F
Weights

The main results are weighted for attrition between birth and MHSS2 using inverse propensity weights. As described in Section III on data, the analysis sample includes respondents from MHSS1 and individuals from MHSS1 households that had migrated out of the DSS area prior to the survey conducted in 1996. The main reasons for non-response are migration in early adulthood and death primarily during infancy. Weights are constructed in two steps. First, we estimate weights to account for selection into the MHSS1 sample frame between birth and MHSS1, which is mainly a result of mortality. Second, we estimate weights to account for attrition of MHSS1 respondents in the MHSS2 survey. We estimate these two probabilities separately and then multiple them to obtain a weight to account for attrition between birth and MHSS2.

To account for attrition between birth and MHSS1, we construct an estimate of the conditional probability that an individual born in the study site was present to be surveyed in MHSS1 using demographic surveillance data. To estimate this probability, we assign treatment status to the universe of individuals born in the study site between 1977 and 1988. Separately by cohort and sex, we use a probit model to predict the probability an individual is present in the study site on January 1, 1996 using the set of baseline household and household head characteristics (which includes pre-program migration networks for the household compound), their interactions with the treatment variable, month of birth and year of birth fixed-effects, and indicators for whether an individual was from a village that experienced erosion or was exposed to the Meghna Dhonnogoda Irrigation Project.

To account for attrition between MHSS1 and MHSS2, we proceed in a similar manner. We estimate the probability of non-attrition between the two survey waves for each cohort-sex group using a probit model and the same set of covariates. Our resulting attrition weight is the inverse of the product of the two estimated probabilities.

To show how the results vary with different weighting schemes, Tables F1 and F2 report results under different weighting schemes for men and women, respectively. Panel A presents the unweighted results. Panel B presents results using the weight that corrects for attrition between MHSS1 and MHSS2 only in Panel B. Results are similar to the main findings in Panel A and B.

In Panel C, we further weight observations so that the analysis is representative of the pre-program population in 1974. We choose 1974 as the pre-program year because census data of the entire study site is available for that year so we are able to construct weights for that year. Because the sample was selected in 1996, it is possible that it is not representative of the pre-program population if the program altered household formation and re-formation between the baseline in 1974 and the time at which the population was sampled in 1996. Foster and Milusheva (2017) develop a weighting methodology to derive 1974 household weights. The weight incorporates both the probability of a household being sampled in 1996 and the probability that a 1974 linked-household was sampled in 1996. We follow their procedure but adjust it to account for the fact that we link individuals back to 1974 households based on where their household head lived in 1974. The 1974 evaluation weight is then the ratio of the 1974 sampling probability[2] to the product of the 1996 sampling probability and the total number of 1974 household descendants in 1996 in the individual's cohort. We multiple this weight by the birth to MHSS2 attrition weight used in the

[2] To construct the 1974 probability, we resa mple the set of 1996 households 100,000 times following the MHSS1 sampling procedure and count the number of times a sampled 1996 household has an individual that is linked to the 1974 household.

paper, so that we also account for sample attrition. Again, results are similar using this weighting scheme, with a few exceptions. For men, the negative effect on work in a manual occupation in the 24–30 cohort is larger and now statistically significant, and for the 31–34 cohort migration for work to any destination is smaller. For women, there is no longer an effect on working more in paid work and in agriculture for the 24–30 cohort, and an increase in the point estimate and significance for ever having a micro-credit loan for 31–34 cohort.

REFERENCES

Foster, A. and S. Milusheva, "Household Recombination, Retrospective Evaluation, and Educational Mobility over 40 Years," Working Paper, 2017.

TABLE F1— ITT EFFECTS, MEN, WEIGHTS

	Second Job	Occupation			Start Own Business	Skills Used	Earnings (USD) Trim 5%	Hours Worked	Work Location		
		Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math			Outside Matlab	Intl.	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Unweighted											
Treat*(Age 24-30)	0.03	0.10	0.02	-0.06	0.09	0.08	-7.28	-6.48	-0.11	-0.02	-0.09
	(0.03)	(0.04)**	(0.02)	(0.04)+	(0.04)*	(0.04)*	(107.64)	(93.11)	(0.04)**	(0.03)	(0.04)*
Treat*(Age 31-34)	0.08	0.00	0.09	-0.02	0.02	-0.05	-452.34	-115.34	-0.10	-0.09	-0.02
	(0.04)*	(0.05)	(0.04)*	(0.04)	(0.04)	(0.05)	(151.36)**	(113.22)	(0.05)*	(0.04)*	(0.05)
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,181	1,287	1,299	1,299	1,299
Panel B: IPW Attrition Weight MHSS1 - MHSS2											
Treat*(Age 24-30)	0.03	0.10	0.02	-0.06	0.09	0.08	-8.04	-14.45	-0.11	-0.02	-0.09
	(0.03)	(0.04)**	(0.02)	(0.04)+	(0.04)**	(0.04)*	(107.95)	(93.28)	(0.04)**	(0.03)	(0.04)*
Treat*(Age 31-34)	0.08	-0.01	0.09	-0.01	0.02	-0.05	-466.03	-109.38	-0.11	-0.09	-0.03
	(0.04)*	(0.05)	(0.04)*	(0.05)	(0.04)	(0.05)	(153.53)**	(114.15)	(0.05)*	(0.04)+	(0.05)
Observations	1,299	1,299	1,299	1,299	1,299	1,299	1,181	1,287	1,299	1,299	1,299
Panel C: 1974 Evaluation x Attrition Weight Birth - MHSS2											
Treat*(Age 24-30)	0.03	0.12	0.01	-0.09	0.09	0.13	54.72	-27.07	-0.12	-0.01	-0.10
	(0.03)	(0.04)**	(0.02)	(0.04)*	(0.04)*	(0.05)**	(149.44)	(116.91)	(0.05)*	(0.04)	(0.04)*
Treat*(Age 31-34)	0.08	0.00	0.12	-0.04	-0.02	-0.08	-445.60	-112.09	-0.07	-0.10	0.03
	(0.05)+	(0.06)	(0.05)*	(0.06)	(0.05)	(0.05)	(143.15)**	(141.71)	(0.06)	(0.05)*	(0.06)
Observations	1,288	1,288	1,288	1,288	1,288	1,288	1,173	1,276	1,288	1,288	1,288

Notes: Standard errors are clustered at the pre-program village level. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort. Individual characteristics include year of birth fixed effects, age cohort fixed effects and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Panel A removes weights. Panels B weights the regressions by the inverse propensity weights created to correct for 1996-2014 attrition. Panel C weights the regressions by the interaction of the main mortality/attrition weight and the 1974 evaluation weight. See the Data section for details on weight construction.
** p<0.01, * p<0.05, + p<0.1

TABLE F2—ITT EFFECTS, WOMEN, WEIGHTS

	Any Paid (=1)	Occupation (=1)				Primary Location (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
		Prof. & Semi-Prof	Ag	Manual	Unpaid HH Work	Outside Matlab	Urban	Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Unweighted										
Treat*(Age 24-30)	0.06	0.00	0.06	-0.01	-0.05	-0.04	-0.03	-0.02	0.06	0.06
	(0.03)+	(0.01)	(0.02)**	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.03)*	(0.03)+
Treat*(Age 31-34)	0.03	-0.01	0.03	0.03	-0.04	-0.11	-0.10	-0.01	0.04	0.03
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)*	(0.04)*	(0.03)	(0.05)	(0.05)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214
Panel B: IPW Attrition Weight MHSS1 - MHSS2										
Treat*(Age 24-30)	0.06	0.00	0.06	-0.01	-0.05	-0.05	-0.03	-0.02	0.06	0.06
	(0.03)+	(0.01)	(0.02)**	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.03)*	(0.03)+
Treat*(Age 31-34)	0.03	-0.01	0.02	0.03	-0.04	-0.11	-0.10	-0.01	0.04	0.03
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)+	(0.04)*	(0.03)	(0.05)	(0.05)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214
Panel C: 1974 Evaluation x Attrition Weight Birth - MHSS2										
Treat*(Age 24-30)	0.01	0.00	0.03	-0.04	0.02	-0.06	-0.05	-0.02	0.04	0.08
	(0.04)	(0.01)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)	(0.03)	(0.04)+
Treat*(Age 31-34)	0.01	-0.00	0.03	0.02	-0.04	-0.11	-0.11	-0.00	0.06	0.12
	(0.06)	(0.02)	(0.05)	(0.05)	(0.07)	(0.06)+	(0.06)*	(0.02)	(0.05)	(0.06)*
Observations	1,207	1,207	1,207	1,207	1,207	1,207	1,207	1,207	1,196	1,201

Notes: Standard errors are clustered at the treatment village level. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort. Individual characteristics include year of birth fixed effects, age cohort fixed effects and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Panel A removes weights. Panels B weights the regressions by the inverse propensity weights created to correct for 1996-2014 attrition. Panel C weights the regressions by the interaction of the main mortality/attrition weight and the 1974 evaluation weight. See the Data section for details on weight construction.
** p<0.01, * p<0.05, + p<0.1