No Place Like Home:
Long-Run Impacts of Early Child Health and Family Planning on Economic and Migration Outcomes*

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Abstract: Early childhood health and nutrition programs are believed to improve adult living standards through the effect of improved human capital on labor market opportunities. We take advantage of a quasi-randomly placed Maternal and Child Health and Family Planning program in a Matlab Bangladesh, to examine program effects on economic and migration outcomes 35 years after program start. Program interventions rolled out between 1977–1988 starting with family planning and maternal health, and introducing key child health interventions in 1982. For men born when both family planning and child health interventions were available, a related paper shows treatment group are taller and more educated. This paper shows that treated men work in more skilled jobs and are more entrepreneurial, but surprisingly, average annual earnings are similar to the comparison group. This is in driven in part by lower migration rates to urban areas of Bangladesh, leading to on average lower wages, but also reducing the potentially high non-monetary costs of migrating. Women also benefited. They work more in paid agriculture activities and have more personal savings and credit. However, men born before the child health interventions were available did not fare as well earning significantly less and migrating less to international destinations. The fact that the program led to better job market outcomes and reduced migration rates for the child health cohort is important not only for the debate about the long-term effects of health, but also for migration policy, where there is concern about a general increase in international and rural-urban labor migration.

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I. Introduction

Early childhood health and nutrition programs are believed to improve living standards when children reach adulthood through their effect on improved human capital and subsequent labor market opportunities (Heckman 2006; Strauss and Thomas 2008, Almond and Currie 2011). Understanding if this link is causal is important from a scientific standpoint, but also imperative for policy. Causal evidence on the long-run effects of early childhood on later life economic outcomes is sparse. This is because there are few well-designed programs that took place twenty or more years ago for which rich panel data exists with sufficient sample sizes and low attrition rates that allow for rigorous analysis on adult outcomes.

This paper provides evidence of the effect of the Maternal and Child Health and Family Planning (MCH-FP) program in Matlab, Bangladesh, for children born during the program on their adult economic outcomes, such as labor market participation, earnings, and migration. The inclusion of effects on migration, while not common in the literature, is necessary as labor migration is an increasingly important strategy to improve ones' livelihood and is intricately linked to labor market outcomes. The MCH-FP program is a quasi-randomly placed intervention that started in 1977 encapsulating two of arguably the most important interventions of the last half century – child health vaccination and family planning. Between 1977 and 1981 the program focused on family planning and tetanus toxoid vaccines for pregnant women. Child health interventions were rolled out beginning in 1982 and included vaccinations against debilitating diseases such as measles, tetanus, pertussis, polio, and tuberculosis and vitamin A supplementation. Preventing these diseases not only reduces mortality, but also morbidity, and can substantially improve health, nutrition, and hence cognitive development of children. Interventions were placed in treatment and comparison areas that were geographically contiguous and chosen to be economically and socially similar (Figure 1). Placing the program in geographically contiguous areas was important for reducing the likely positive spillover effects from herd immunity created by vaccination, as well as, informational spillovers about health and family planning. Similar interventions were introduced in the comparison area in the late 1980s, thus defining an approximately 10-year experimental period.

The quasi-experimental design, the roll out of the program over time, and availability of panel data, provides a rare opportunity to rigorously examine the longer-run effect of this important health and family planning program on key indicators of success thirty-five years after program inception. We use single-difference models with birth-year fixed effects and pre-program controls to examine the effect of the program on men and women separately for two cohorts: those born when family planning interventions were available, but before child health vaccinations rolled out (1977–1981), and those born when both child health and family planning interventions were available (1982–1988). The two cohorts of interest are approximately aged 30–34 and 24–29, respectively, at the time of survey. We exploit unusually rich data availability on the study site. Outcome variables come from a socioeconomic survey of the study area, the Matlab Health and Socioeconomic Survey 2 (MHSS2). Completed in 2015 by a team including the authors, the survey was designed specifically to evaluate the MCH-FP program. Attrition rates for MHSS2 are low, less than 10 percent, thirty-five years after the start of the program. The paper also benefits from the ability to link secondary data sources at the individual and household level to MHSS2. This includes pre-program characteristics, demographic surveillance data (pre-program and on-going) from the study area, and data on potentially confounding programs. The resulting dataset is unique due to the length of the panel, depth of available outcome variables, successful tracking rate, availability of pre-program data including pre-program migrant networks, and a quasi-random program design underlying the data.

Our analysis makes three important contributions to the literature that relates early childhood health and nutrition programs to economic and migration outcomes in adulthood. First, much of the early literature focused on the effect of large negative shocks to health and nutrition early in life, such as famines, disease, or natural disasters rather than longer-run effects of interventions designed to *improve* the health and nutrition of children under the age of five, which is key for policy.[1] Second, the study benefits from very low attrition rates, particularly in a setting with a highly mobile population (more than 60 percent of men in the sample are migrants). This reduces attrition bias common in long-term effects studies. In addition, because migrants are often the ones lost to attrition, we have a sufficient sample of both migrants and non-migrants to study work-migration decisions and the role that migration plays as a mechanism for labor market outcomes. Migration is critical to examine because early childhood interventions and family planning (through smaller family sizes) can have a direct effect on whether someone migrates and the length of their migration spell, thereby affecting labor market outcomes. Third, rich data availability enables us to test pre-program balance and examine potential mechanisms, such as exogenous pre-program migration networks, both of which are often missing in the longer-run literature due to lack of data.

Previous research in the study area that showed the MCH-FP program reduced child mortality for measles and fertility (Phillips et al.1984; Koenig et al. 1990, 1991). In the medium-term, Joshi and Schultz (2013) estimate that women in the treated area had 0.78 less children by 1996. Barham (2012) shows that when the 1982–1988 cohort was approximately 8–14 years old, the treated had better human capital as measured by height, cognitive functioning and schooling, but there were no program effects on human capital for the 1977–1981 cohort born prior to the roll out of the child health interventions. Using similar data but a different research design, Joshi and Schultz (2007) find an increase in schooling for boys aged 9–14, but no effect for girls, and Chaudhuri (2005) reports that girls younger than 14 experienced improved weight-for-age and boys were significantly less stunted. Finally, Barham, Kagy, and Hammadani (2016) in a related paper that examines the longer-run effects of the program on human capital using the same sample and data as in this paper, find that the program effects on height and schooling for the 1982–1988 cohort persist into adulthood for men and on height for women.

The findings suggest that an intensive health and family planning program can have important but mixed long-term impacts on labor market and migration outcomes of eligible children at the start of their working life in their 20s. The 24–29 year old men, born when the child health interventions rolled out, are 25 percent more likely to have professional or semi-professional jobs, which we refer to as "better" jobs, and use more skills and less physical strength in their jobs. Additionally, they are 52 percent more likely to start their own business, suggesting they are more entrepreneurial. Surprisingly, there is no difference in average annual earnings for the 24–29 cohort. This appears to be because they are 20 percent more likely to be living and working at home in Matlab, rather than the higher paying urban areas of Bangladesh. This does not mean that the treated earned the same as the comparison group within Matlab and outside of Matlab. Results stratified by endogenous migration status suggest that the treated group have higher annual earnings, especially in Matlab. Work migration is typically solo migration in our sample, so it is important to remember that migration imposes significant utility costs both on the migrants and their families (Imbert and Papp 2016). Given that earnings are

[1] See Strauss and Thomas (2007), Almond and Currie (2011), Currie and Vogl (2013), and Hoynes et al. (2016).

similar between treatment and comparison, but current migration rates lower, this suggests that the treated may be better off. Factoring in migration makes a big difference on earnings and benefits do not seem to be that large.

Examination of key mechanisms shows that having a “better” job is partly driven by the treated group’s higher level of education. There is little heterogeneity with respect to observable baseline characteristics, though, the difference in migration rates between the treated and comparison group is lower for those who have larger pre-treatment migration networks. These findings are consistent with the literature on migrant networks (Munshi 2003), and provide evidence on how the size, and perhaps the quality, of exogenous pre-program migrant networks are important for migration behavior of future generations.

Robustness analysis highlights that effects are not likely driven by the treatment and comparison areas being in separate local labor markets, and that basic food prices are similar between the two areas. They also rule out that these young men have “better” jobs because their fathers had better jobs when they themselves were young men. Finally, results are not an artifact of multiple hypothesis testing or attrition due to mortality or migration (Appendix D).

Men born prior to the child health interventions when only family planning was available have not fared as well. Astoundingly, we find a 23 percent decrease in earnings for men in the 30–34 age cohort. This coincides with an increase in agricultural work and a 37 percent decrease in migration to international destinations, where earnings are notably higher than in Bangladesh. Understanding the mechanisms that lead to the lower earnings and rates of international migration is difficult due to the endogeneity of the likely mechanisms. It is possible that this result is an artifact of how the program was rolled out as some families may choose which child to support to migrate internationally in part based on the child’s human capital. We provide suggestive evidence that treatment group men in the 30–34 cohort migrate less internationally and earn less if they had two or more younger siblings who were born when the child health interventions were available. This negative program effect highlights potential unintended consequences of programs that differentially change human capital within families.

Women born when the child health interventions were available are 23 percent more likely to engage in paid work. Specifically, women are more likely to have paid work in agricultural activities involving raising small animals, have their own savings, and use micro-credit loans. However, there are no differences in average annual income. Migration for work is not as common among women in this area and there are no differences in migration rates. Nor were there any long-term effects on marriage or fertility, two mechanisms that can affect the labor market supply of these young women.

This paper builds on two influential randomized early childhood health and nutrition interventions that examine long-term effects on economic outcomes, but that suffer from attrition or small sample sizes. The Instituto de Nutrición de Centro América y Panamá (INCAP) in Guatamala assigned two of four villages to receive a nutritionally fortified drink, while the control villages received sugar water. Hoddinott et al. (2008) followed 60 percent of the sample and found that men exposed to treatment prior to age three experienced a 46 percent increase in hourly wages but no statistically significant increase in annual incomes. A study in Jamaica randomized 129 stunted 9–24 month olds into 4 arms: nutritional supplement, psychosocial stimulation, receive both, and control group. A long-term follow up found no effect on earnings for the nutrition only group at age 22, but a 25 percent increase for the combined psychosocial stimulation group (Gertler et al. 2014)[2, 3, 4]

Finally, this paper contributes more broadly to the literature on migration. Given the low attrition rates in the MHSS2 data, to our knowledge, this is one of the only papers able to rigorously explore how programs designed to improve health and human capital affect migration behavior in adulthood. As reflected in the migration results, utility costs of migration matter and have implications for labor market outcomes, but also for how researchers think about the migration process.

The remainder of the paper proceeds as follows. Section II provides background on the program, study design, and potential mechanisms through which the program may lead to effects on adult economic and migration outcomes. Section III describes the data. We examine baseline balance and the estimation strategy in Section IV, intent-to-treat effects on men in Section V, analysis on the mechanisms in Section VI, heterogeneity analyses in Section VII, and program effects on women in Section VIII. Section IX concludes.

II. The MCH-FP Program and Mechanisms

A. The Intervention and Program Take-Up

The MCH-FP program was initiated in a rural area of Bangladesh, Matlab, in October 1977 by icddr,b. It started as a demonstration project to help the government design a national family planning program. Treatment and comparison areas were built into the design of the program and covered about 200,000 people in 149 villages, with the population split fairly evenly between the two areas. Randomization was not used to determine who received the program. Instead, the program was quasi-randomly placed in a contiguous geographic area with a set of excluded villages to be used as a comparison group (Figure 1).

The program included integrated family planning and maternal and child health services. Interventions were administered in the home free of charge during monthly visits made by local female health workers hired and trained by the program. In the comparison area, then-standard government health and family planning services were available, but home delivery was limited or non-existent and many of the program interventions, such as childhood vaccinations and the array of family planning options, were not readily available from the government until 1989 or later providing an experimental period, 1978–1988, to evaluate the program.

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[2] In related literature, Baird et al. (2016) examine the effect of a school age health intervention, deworming, on a number of adult outcomes including labor market outcomes. They find that 10 years after deworming treatment men work 17% more hours per week, work more in non-agricultural self-employment, and are more likely to have a manufacturing job. Women from treatment schools work more in cash crops and non-agricultural self-employment.

[3] Non-experimental research on Head Start, a large government early childhood education, health, and nutrition program that targeted to poor families, show no effects on annual earnings.

[4] The Carolina Abecedarian Project and The Perry Preschool are two well-known randomized program in the US focusing on stimulation rather than health or nutrition interventions. The Perry Preschool project randomized 123 low-income African American 3 and 4 year olds into receiving or not receiving preschool in 1962. Almost all participants were followed at age 40, and the treated were more likely to hold a job and have higher earnings (Heckman et al. 2010; Conti et al. 2013). The Carolina Abecedarian Project provided a quality early childhood education, health, and nutrition from 8 weeks to age 5 to 111 children. The control group received nutritional supplements, health care and social services to control for the effect of these interventions. By age 21, the treated were more likely to have a skilled job (Campbell et al. 2002), and by age 30 earn more (García et al. 2016).

Services were rolled out over two main periods: 1977–1981 and 1982–1988. Table A1 provides a summary of the intervention rollout. Before 1982, the focus was family planning and maternal health through the provision of modern contraception[5], 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 also provided counseling on contraceptives, nutrition, hygiene, and breastfeeding, motivated women to continue using contraceptives, and instructed them in how to prepare an oral rehydration solution. A well-developed follow-up and referral system was available to ensure management of side effects and continued use of contraceptives (Phillips et al. 1984; Fauveau et al. 1994).

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. Preventative services were introduced in 1985 to children under five in the entire treatment area and included vitamin A supplementation and vaccines for measles, DPT, polio, and tuberculosis. Curative care, such as nutrition rehabilitation and acute care for respiratory infections, was also introduced in the late 1980s. In addition, tetanus toxoid immunization was expanded to all women of reproductive age, and safe delivery kits were provided to pregnant women.

Program implementation followed the planned timeline, and uptake was rapid. Figure 2 demonstrates that before the program the contraceptive prevalence rate (CPR) for married women 15–49 was low (< 6 percent) in both the treatment and comparison areas. CPR jumped to 30 percent in the treatment area during the first year of the program, 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. There was still a 20 percent difference in the CPR rate between the two areas in 1990.

Figure 2 also shows the measles vaccination rate rose to 60 percent in 1982 after it was introduced in half of the treatment area (treatment area 1), and in 1985 when it was introduced in the other half (treatment area 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 (HDSS 2007). Vaccination rates are not presented in Figure 2 for the comparison area because they were not recorded, however, they are believed to have been near zero. Government services did not regularly provide measles vaccination for children until around 1989, so the comparison area was an unvaccinated population (Koenig et al. 1991). Nationally, measles vaccination for children under the age of five was less than 2% in 1986 (Khan and Yoder 1998) and was below 40% in the comparison area in 1990 (Fauveau 1994). As the national program scaled up after 1988, the differences in access to family planning and vaccinations narrowed substantially.

B. Program Placement

The comparison group was built into the design of MCH-FP (Faveau 1994), but the treatment and comparison villages were not assigned randomly. They were chosen to be geographically contiguous areas and were viewed as socially and economically similar and geographically insulated from outside influences (Phillips et al. 1982). The block design also helped reduce

[5] Family planning options to the beneficiary's home include condoms, oral pills, vaginal foam tablets, and injectables, and in health clinics, intrauterine device insertion, tubectomy, and menstrual regulation.

potential contamination of the comparison area from the family planning interventions (Huber and Khan 1979) and spillovers from positive externalities generated by vaccination.

Past research demonstrates the treatment and comparison areas were balanced on a variety of dimensions. Importantly, mortality and fertility rates were similar (Koenig et al. 1990; Menken and Phillips 1990; Joshi and Schultz 2013), demonstrating that the program was not placed first in areas that had poor child health or high fertility. We explore baseline balance for the specific sample for this paper in Section IV.A, and find the experimental areas are fairly balanced.

C. Mechanisms

The MCH-FP program could affect the adult outcomes of children born during the program evaluation period through several of the program's interventions. We limit the discussion of adult outcomes to those most relevant for this paper: labor market and migration outcomes. We also limit the discussion of specific program interventions to the child health interventions and their impact on human capital, and to the family planning intervention and their impact on family size. Barham (2012) provides a more detailed discussion of how the program interventions affect childhood human capital development under age five, including a discussion of other program interventions (e.g., women's health) and other effects of family planning (e.g., birth spacing).

Mechanisms Linking the MCH-FP Program and Human Capital Development. — The MCH-FP interventions could affect a child's human capital by improving their nutrition, health, and cognition while under the age of five. The fact that the interventions targeted children under the age of five is important. These initial years are a key period for child development, and later skill development often builds on the base developed at young ages (Thompson and Nelson 2001; Knudsen et al. 2006). Two important channels for human capital development are the reduction in the incidence of vaccine preventable diseases and the quantity-quality trade off resulting from smaller family sizes (Becker and Lewis 1973; Becker and Tomes 1976).

The diseases targeted by the vaccinations can cause mortality and severe morbidity, thereby affecting the cognitive and physical development of children directly through complications, such as encephalitis, and indirectly through undernutrition (Grantham-McGregor, Fernald, Sethuraman 1999a, b; Walker et al. 2007). For example, measles complications include pneumonia and diarrhea and can leave a child weakened and at increased risk of illness for a year (Greenberg, von Konia, and Heininger 2005). Undernutrition was a major problem in Bangladesh during program roll out. Children with lower levels of nutrition prior to infection tend to have weaker immune systems, which increases the severity and duration of the sickness and makes catch-up development and growth harder. In addition, sickness and undernutrition cause malaise and apathy, resulting in less stimulation due to decreased physical activity, play, and perhaps parental attention, again hindering cognitive development (Walker et al. 2007).

While the program may improve childhood human capital directly, the program could also indirectly affect human capital through changes in the pattern of parental investments among their children. However, the sign of this potential effect is unknown ex ante. Parents may compensate the less-healthy children not exposed to the child health interventions or reinforce the program effect by investing more in the healthier, more able child.

Child Human Capital Development and Adult Labor Market Outcomes.— A key channel that links the MCH-FP child health interventions to adult labor market outcomes is the direct effect of improved child human capital on adult human capital (health and cognition), as well as through their indirect effects on schooling. Educational attainment and improved adult health and cognition are important components of the earnings function as they can increase productivity and labor supply. Whether the hypothesized link between early human capital development and adult labor market outcomes materializes depends on other factors such as: (1) fade-out of human capital differences over time, perhaps due to negative shocks or lack of investments later in a child's life to harvest the earlier investments (Heckman 2007); (2) poorly functioning local and migrant labor markets, and; (3) gender, as women often do not have access to the same job market opportunities as men.

Child Human Capital Development, and Adult Migration.— Economic evidence linking improvements in early childhood human capital or changes in family size to adult migration is limited (Bodvarsson, Simpson, and Sparber 2015; Bratti, Riore, and Mendola 2016). While we do not examine all the pathways, the effects of the program interventions on migration and return-migration depend on a number of factors and are ambiguously signed. We consider both permanent and return migration given that more than 60% of males in our sample migrate out of the study site during early adulthood, and evidence from older cohorts suggests that migrants return to their village home while still of working age.

The decision to migrate depends on a number of factors. The potential migrant compares expected lifetime income to potential costs, as well as the utility received from consumption at home and the destination. Thus, changes in human capital resulting from the MCH-FP program could positively or negatively affect the decision to migrate and when to return, leaving the effect of the program an empirical question. For example, out-migration could increase because a migrant with higher human capital (e.g., education) may receive a higher destination wage. However, changes in out-migration depend on the relative returns to human capital in the destination and origin areas. Additionally, increased income in the destination area has a theoretically ambiguous effect on return migration (Wahba 2014). While an increase in the destination area wage increases the marginal value of staying in the destination area (relative wage effect), additional consumption abroad decreases the marginal utility of consumption at the destination relative to home. Alternatively, if migrants are target savers, increased income in the destination area due to higher human capital may allow the migrant to return earlier.

In addition, uncertainty plays an important role in the calculation of expected net income or lifetime utility including estimation of the non-monetary costs (Wahba 2014; Borjas and Bratsberg 1996). Returns to higher human capital in the destination area may be more uncertain, reducing expected income and out-migration for the treated. Dustman (1997) examines the return migration decision comparing decisions under certainty and uncertainty and shows that uncertain future income has an ambiguous effect on migration decisions.

Migrant networks, both of past migrants and migrants currently in destination areas, can also affect migration by reducing migration costs and uncertainty (Bodvarsson, Simpson, and Sparber 2015). Earlier work suggests that the program reduced historical migration from the treatment area (Barham and Kuhn 2014). By affecting the size of migrant networks, the program could potentially reduce migration. Additionally, the available migration network may not be able to link treated children to the higher skilled jobs they now seek (i.e., migration network quality is low), further adding to the cost of migration and increasing uncertainty about future income.

Finally, migration can be a household-level decision where members strategically migrate to diversify sources of family earnings (Stark and Bloom 1985) or to support the consumption, education, or migration of other family members. Which adult child the family chooses to keep in Matlab may depend on the relative human capital of the adult children. If this is the case, there may be differences in migration rates between the cohorts.

Sibship Size and Adult Migration.— Sibship size may also be positively or negatively correlated with migration out of Matlab. A smaller family could lead to higher migration rates as the family has more per-child resources to support migration. In addition, with smaller sibship sizes, there is a higher probability any one child will have to migrate to help the family, perhaps to diversify family member jobs across location (Rosenzweig and Stark 1989) or to support other siblings (e.g., education cost of sibling). On the other hand, smaller family sizes could lead to lower migration rates out of Matlab if an adult child is required to stay in Matlab 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 (e.g., the education cost of younger siblings) is lower if there are fewer younger siblings.

III. Data

This paper draws on the unusually 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), the 1996 Matlab Health and Socioeconomic Survey wave 1 (MHSS1) (icddr,b 1996), periodic censuses conducted by icddr,b in 1974 and 1982 (icddr,b 1974, 1982), and 1974-2012 Matlab demographic surveillance site (DSS) data on vital events (e.g., births, marriages, deaths, in and out migrations) collected by icddr,b.[6] MHSS1/2 are 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 and allow us to determine exact birthdates, treatment statuses, pre-program migrant networks, and test the pre-program balance. There are few, if any, other study sites that have similar rich data availability to allow this type of long-term evaluation. This 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. For more detailed information on these topics as well as a description of how key outcome and control variables were constructed see Appendix B.

MHSS2. — The main outcomes variables used in this paper are from MHSS2. MHSS2 was conducted between 2012 and 2014 and has very low attrition rates at less than 10 percent. It was designed to be a panel to MHSS1 primary households (icddr,b 1996). MHSS1 is an eight percent random subsample of baris[7] from the Matlab area living in both the treatment and comparison areas and was designed to be representative of the study area's 1993 population. To limit migration selection for key age groups, the MHSS2 sample also includes individuals born between 1972 and 1989 to a MHSS1 household that had migrated out of Matlab between 1977 and 1996 (referred to as pre-1996 migrants). To the extent that a whole household migrated out

[6] MHSS2 data is not currently publically available. It was collected by the authors together with a team of researchers from the University of Colorado Boulder and icddr,b. 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.
[7] Baris are household compounds where a number of related households live together and often eat together.

of Matlab between the start of the program and 1996, leaving the household unavailable for selection into the MHSS1 sample, the MHSS2 sample could still suffer from migration selection. It is rare that whole households migrated out of Matlab prior to 1996 and is estimated to be minimal at less than 1 percent of the target sample. and were collected in an individual instrument so are not proxy reports.

MHSS2 collected extensive information on employment history for each individual older than fifteen at the time of survey, as well as, current location of residence and residence location histories from 2008-2012 to allow the construction of migration status. A respondent is defined as an out-migrant if their current residence, given by their village code in the survey, is outside the Matlab's district, Chadpur. An out-migration episode is defined as being urban if the location is in Dhaka and surrounding districts, or the Chittagong district.

Most data were collected during face-to-face interviews, so are not proxy reports. A subset was collected in a short phone survey instrument of international migrants who did not return to Bangladesh during the data collection period. As a result, there are smaller sample sizes for some variables, but not for the key labor market and migration outcomes.

Analysis Sample and Attrition. — The main sample for this paper includes all individuals born during the experimental period from October 1977 and December 1988 (the 24–29 and 30–34 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 for both age cohorts together is 8.8 percent for men and 7.8 percent for women for migration information, and 9.5 percent for men and 8.1 percent for women for income information (Table A2). Rates are similar for each age cohort separately and not statistically different between the treatment and comparison areas for any age cohort or sex. These are low attrition rates compared to other long-term effects surveys with shorter follow-up periods, [8] despite a migration rate of approximately 60 percent for men in this highly mobile population. Appendix B shows there is no differential attrition between the treatment areas based on individual and baseline characteristics.

Intent-to-Treat and 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. 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. We 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. 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.

[8] For example, Gertler et al. (2014) report an attrition rate of 17 percent after 20 years, Araujo et al. (2016) 19 percent after 10 years, Barham, Macours, and Maluccio (2016) 10 percent after 10 years, Baird et al. (2016) 16.1 percent after approximately 10 years.

Baseline characteristics from the 1974 census are linked to individuals in the same manner used to construct treatment status. We fill in baseline characteristics for the few individuals that could not be linked to the 1974 census by assigning means based on treatment status. Finally, the bari of the person traced to the 1974 census and the DSS migration data are combined to construct pre-program bari migrant networks.

IV. Estimation Strategy

A. *Baseline Balance*

Previous research shows 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 (2012) shows that cognitive functioning, height, and education was similar across the treatment and comparison areas in 1996 for those who were old enough that their human capital height was not likely to have been affected by the program. Two variables that are not balanced between treatment areas are religion and access to tube well water in 1974. However, these differences were found not to affect program effects on human capital using MHSS1 (Barham 2012) or migration during program roll out (Barham and Kuhn 2014).

We use the 1974 census to test whether the baseline characteristics are similar across the treatment and control areas for the observations in our sample with non-missing income data. This allows us to examine whether the sample is balanced after attrition. We present the baseline balance by pooling both men and women and the 24–29 and 30–34 cohorts in Table A3. The balance is similar for the sexes and age groups separately (results not included). As well as reporting the statistical significance of the differences in means between the treatment and comparison areas, we examine the normalized differences in means (difference in the means divided by the standard deviation of the mean for the sample). The normalized difference indicates the size of the differences in means, because small differences in means can be statistically significant in large samples (Imbens and Wooldridge 2009). Normalized differences bigger than 0.25 standard deviations are generally thought to be substantial.

Table A3 shows that the areas are similar even with attrition: 4 out of the 24 household characteristics show differences significant below the 5 percent level, and all the normalized differences are 0.15 or less, with the exception of Muslim. Similar to previous studies, religion and access to tube well water are statistically different at the 5 percent level. Household head and spouse's age are also statistically different. This is likely due to the effect of the family planning program, which both decreased family sizes and increased birth intervals for the cohort of interest in this study. Indeed, if we use a sample born between 1947 and 1976, 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.

The treatment area has 10 percentage points fewer Muslims than the comparison area, the remainder being Hindu, and 14 percentage points more tubewells used for drinking water than the comparison area. The difference in tubewell water in 1974 is the result of a government and UNICEF program and does not reflect household income, propensity to drill a tubewell, a household's concern about child health, or other unobservable factors that could be correlated with a person's propensity to migrate or ability to work. Unfortunately, there is widespread arsenic contamination of water in the tubewells in Bangladesh and arsenic is a serious health concern affecting the cognitive development of children (Wasserman et al. 2006), so could lead to a downward bias on human capital measures. Barham (2012) shows that program effects on human capital when the individuals in the sample were children did not vary by access to tubewell water. Regardless, to help control for these differences, access to tubewell water is included in the baseline controls and we control for arsenic exposure in the robustness checks.

These findings, together with previous results on fertility and mortality, strongly suggest that the two areas had similar observable characteristics except for religion and access to tubewell water. To account for the differences in baseline characteristics we include the observable characteristics, interacted with the main time period dummies in the regressions, to help control for any biases, and we include birth-year fixed effects. In addition, as a robustness check we exclude Hindus from the analysis.

B. *Identification Strategy and Empirical Specification*

We estimate the intent-to-treat (ITT) effects of the MCH-FP program on labor market and migration outcomes. We take advantage of the variation in program implementation across location and time to estimate single difference models on two main cohorts: those born when the family planning and maternal health interventions were introduced, the 30–34 cohort, and those born when both the family planning and child health interventions were available, the 24–29 cohort. 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.

We estimate ITT single difference effects for person *i* from village *v* using the following linear regression:

$$O_{iv}= \beta_1 AG^{24-29} + \beta_2 AG^{30-34} + \beta_3(T_v * AG^{24-29}) + \beta_4(T_v * AG^{30-34}) + \alpha_{by} + X'Z + \varepsilon_{iv} ,$$

where O 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*, or person *i*'s household, resided in the treatment village before the start of the MCH-FP program, and 0 if in the comparison village. *AG* are fixed effects for the two age cohorts and control for common difference in the outcome for each age group. 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. We also control for birth-year fixed effects, α_{by}, to account for differences in the outcome due to age, as well as other effects that affect only certain birth years. X is a vector of baseline household and household head characteristics and religion that are interacted with the two age group dummies (Table A3 lists the variables). Standard errors are clustered at the treatment village level to account for the likely intracluster correlation in the error terms. Details on construction of key outcome and control variables are in Appendix B.

There were some important changes that took place in the study after the program started that could bias the results. These include an irrigation project that involved building an embankment that left some villages more protected than others from frequent flooding; the expansion of education supply, particularly for girls; the discovery of arsenic in some deep tube-wells in the area, which could have affected child development; the expansion of health workers across the study site; and the introduction of micro-credit. These potential confounders changed over time in both the treatment and comparison areas. More details about are provided in Appendix C, and they are included as a robustness check in Appendix D.

In addition, we show results representative of the 1974 baseline population using a weighting methodology developed by Foster and Milusheva (2017) are similar (Appendix E).

V. Intent-To-Treat Program Impacts on Men

We examine the ITT single difference effects for males in the 24–29 and 30–34 cohorts. Tables present the single-difference coefficients in Panel A, the percent change that the coefficient represents over the comparison group mean for that age cohort in Panel B, and the mean of the outcome variable for the comparison group for each age cohort at the bottom.

We perform a number of robustness analyses to test the validity of the results. A limited number of robustness analysis are described in the paper, but the majority are described in Appendix D. These include restricting the sample to those who are Muslim, including an extended set of controls to account for changes over time from numerous programs and for exposure to arsenic in the tubewells, multiple hypothesis testing, mortality and migration attrition, and spatially correlated errors. The results are robust to the various checks. In addition, we test for spillover effects but don't find any evidence of them.

A. *Labor Market Outcomes*

We first examine the effects of the program on labor market outcomes, including participation in work activities by type of work and skills used for the job (Table 1), and income and hours worked (Table 2). A person's primary job, the job for which a person earned the most in the past 12 months, is used for analysis on labor market participation and skills.[9] Jobs are broken into four categories: semiprofessional and professional work (e.g., professional, clerical, or sales work), agricultural work, and manual (e.g., trade jobs such as construction, factory work, driving, handicrafts), or elementary jobs (e.g. rickshaw driving, day labor). Descriptively, the semi-professional or professional jobs seem to be "better" jobs in terms of remuneration. In our sample, the semi-professional or professional hourly wage was 12–35 percent higher, depending on location, than those working in the next highest paying job type (i.e. manual work).

Labor Market Participation and Type of Jobs. — Table 1 highlights that participation in paid work activities is high, with participation rates of 90% and 96% for the 24–29 and 30–34 age groups respectively (column 1). The participation rate is lower for the younger age group because some of these young men are still enrolled in school. the primary occupations for men are semiprofessional or professional work (32–39%) and manual jobs (56–57%).

As expected, there is no program effect on work in any paid activities (column 1) for either cohort, because labor market participation rates are high. However, there are differences in type of work. The 24–29 age cohort in the treatment group is 25 percent more likely to have a semiprofessional or professional job than the untreated group (column 2) and 13 percent less likely to have a manual job (column 4).

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[9] Including secondary job occupations, which allows someone to belong to more than one job type, does not change the results.

For the 30–34 cohort, there are few program effects by job type. Column 3 shows that the treatment group is more likely to work in agriculture. However, this result needs to be interpreted with care because few men work in this sector as a primary job.

We would like to know whether the respondents themselves created these jobs, or whether they were more likely to be hired in the available skilled jobs. This is a difficult question to answer, but we can examine the effect on self-employment in general (column 5) and self-employment in professional and semi-professional jobs (column 6), and whether the respondent started their own business (column 7). For the 24–29 cohort, the program effects show a 9 percentage point (44 percent) increase in self-employment, a 5 percentage point (39 percent) increase in work in professional and semi-professional jobs among those who are self-employed, and a 10 percentage point (52 percent) increase in starting a business. These results suggest that the treatment group is more entrepreneurial and perhaps create their own job.

Skills. — Fitting with having "better" jobs, the 24–29 cohort are more likely to use academic skills in their jobs rather than physical strength. Table 1 presents skills used for a person's primary job. The 24–29 year olds in the treatment area are 34 percent more likely to need to be able to read, write, add, and subtract than their comparison age peers (column 8), and 5 percent less likely to need to use physical strength at work (column 9). However, there are no significant differences in the need for any level of education at their job (column 10). For the 30–34 cohort, there are no statistically significance differences in the skills they use in their primary job.

Earnings and Hours Worked. — Table 2 presents ITT program effects on annual income and hours worked, where income is denominated in 2012 US dollars. Due to some large outliers, we trim income at the five percent level. Non-trimmed effects are reported in Table 2, but we only discuss the trimmed results. For the 24–29 age cohort, the program effect on earnings in the past 12 months is -36.73 USD and is not significantly significant, suggesting there is no effect of the program on earnings. This lack of effect on income is not a result of working shorter hours (column 3). There are no program effects on hours worked for this age group, and the difference between the treatment and comparison group is small, 23 fewer hours over a 12-month period.

In contrast, the 30–34 cohort earns on average 23 percent less (462 USD) in the treated group than the comparison group (column 2). This cohort works about 70 fewer hours in a year, though this effect is not significant.

The program effects on income are surprising for both age groups, especially given that the age 24–29 cohort in the treatment area was more likely to have "better" jobs. In Bangladesh, there are very high levels of migration for work. To understand how the program effects labor market outcomes, we need to examine the program effects on migration. It is possible that the treatment effect on earnings is driven by differential migration rates because migrants in general make higher incomes than non-migrants, with international migrants earning the most.

Robustness of Labor Market Outcomes. — We defined "better job" broadly to encompasses small shop workers and small shop owners, including workers at tea and betel leaf stands. However, it is debatable if owning a small shop is a "better" job. In addition, it is possible that children inherited the better jobs from their father because their fathers were more likely to stay in Matlab for the program (Barham and Kuhn, 2014). To test the robustness of our results, in Table A4, we exclude small shop workers or owners (column 2), focus only on professional jobs (column 3), and examine whether if their fathers were more likely to have professional or semiprofessional job when they were in their 20s and 30s in 1996 using MHSS1(column 4). We find that the results are robust to the different definitions of "better" job and these young men do not appear to have inherited their better job from their father.

It is also possible that we are picking up local labor market effects because the main rural town is in the treatment area, though many smaller markets exist in both treatment and comparison areas. To check that the results are not driven by the location of the main town, we present results dropping anyone whose 1974 treatment village was in the main town and the surrounding villages (referred to in tables only as main town). Table D1 Panel B shows that results are similar when these villages are dropped.

It is also possible that labor market opportunities differ in the treatment and comparison areas and that some men cannot commute to the other experimental area. Matlab is a fairly small area, and almost all people in the comparison area are located within 5 km of the treatment area, so labor markets are close. To compare more geographically similar areas, we use GIS data on village borders and village centroids to restrict the analysis to people whose 1974 village is within 3 kilometers, or approximately a 45-minute walk, of the treatment and comparison border. Table D2 Panel C shows results remain qualitatively similar, though we lose significance with the loss of sample size. One might also be concerned that the comparison area consists of two distinct areas, to the north and to the west (see Figure 1). We repeat the analysis using each comparison block separately. Results remain largely unchanged, showing that there doesn't seem to be a specific area with a specific labor market driving the results (results not reported).

Additionally, separate labor markets might exist if prices varied geographically within the region. Using market price data from the study, we find evidence that food prices do not vary between the treatment and comparison areas (see Appendix F).

B. Migration

Current Migration. — Current migration out of Matlab by destination is presented in Table 2. The means for the comparison group underscore the prevalence of migration, at 65 and 58 percent for the 24–29 and 30–34 age cohorts, respectively. While more than half of migration is to urban areas (Dhaka and Chittagong), approximately 25 percent is international. The ITT effects demonstrate that the MCH-FP program lowered current rates of migration out of the study area by 20 percent for the 24–29 age cohort (significant at the 5 percent level) and by 17 percent for the 30–34 age cohort (significant at the 10 percent level). Splitting by destination shows migration differs by destination for the two cohorts. For the 24–29 year olds, the ITT effect on migration to domestic urban destinations was 28 percent lower, but there is no significant difference in migration to international destinations. For the 30–34 year olds, it is the opposite, migration to international destinations was 37 percent lower for the treated group and there was no significant difference in migration to urban areas. These differences in migration patterns between the 24–29 and 30–34 cohorts may help explain the differing patterns in income.

Duration of Migration, Ever Migration, and Age of Migration.— The differences in current migration could be due to a number of factors including different rates in duration of migration, ever migrating, or age of migration. Information on migration duration by destination was collected only for the past five years in MHSS2, fortunately these are critical years for migration for both groups. We report migration duration conditional on having migrated in the last five years, because results including those who have never migrated are driven by the extensive margin of if they are a current migrant, and we would like to know if, conditional on migrating, these young men return earlier or later. Table 3 shows that, conditional on having migrated in the last five years, both cohorts have shorter migration durations, of 0.2 fewer years (column 2).

Using DSS data to measure if someone ever migrated after the age of 12, we find that the treated group was also about 10 percent less likely to have ever migrated (Table 3, column 6).[10] Together these results show that the treated area was less likely to migrate in the first place, and for those who migrated they were more likely to return earlier.

Finally, there is no difference in age of first migration (Table 3, column 7). On average, the 30–34 cohort starts migrating at age 21 and the 24–29 cohort at age 19. Indeed, using DSS data, Figure A1 plots the estimated ITT effect of migration at ages 12–25 for the 24–29 cohort and shows the program impacts on migrations rates starts around age 19 and remains through age 25.

Labor Market Outcomes Conditional on Current Migration. — Differences in current migration could be driving the lack of positive program effects on income. To explore this possibility, we examine the effects of the program on labor market outcomes separately by migration status in Table A5. For the 24–29 cohort, the treated are more likely to participate in professional jobs and have higher average earnings than their comparison area peers regardless of migrant status, though the differences are for non-migrants. Results by migration status are similar to the combined results for the 30–34, so do not provide an explanation for what might be driving the negative effect on migrant earnings for this age group. We look further into the earnings effects in Section VI on mechanisms.

Splitting the sample by endogenous migration status is problematic because of selection into migration. However, any selection bias that occurs has to be such that it would lead to higher incomes and better jobs for both types for the 24–29 age groups. We do not find any differential selection into migration on baseline characteristics in Section VII.

Migration Robustness. — It is possible that there is some bias in our measure of migration due to the survey being administered across multiple years or because migrants were interviewed when they returned to Matlab during EID holidays potentially leading to mistakes in recording migration status. To test for any bias, in Table 3 column 8 we present results on current migration status using migration data from the DSS instead of MHSS2 and find similar results.

It is possible that some of the differences between the two cohorts of interest are due to life cycle effects. We remove lifecycle effects across cohorts by considering a person's migration status on December 31 of the year he is aged 24. Column 9 presents similar results for the oldest and youngest cohorts. The 30–34 year olds are still less likely to have migrated, but this effect is now smaller and no longer statistically significant, this maybe because they started to migrate later in life than the 25-29 year olds.

Finally, differential migration is not driven by differential migration costs. Costs to reach the main urban areas (Chittagong and Dhaka) are not likely to differ substantially within Matlab. The bus is the primary mode of transportation to Dhaka and travels along the main road near the river embankment that cuts through the middle of Matlab and both the treatment and comparison areas (see Figure 1).

[10] Ever migration, is coded as if migrated since age 12 to isolate migration episodes for work.

C. Consumption, Assets, and Loans

For populations whose consumption is constrained by their budget, savings are usually low, so one can estimate income by examining household consumption. For temporary migrants who travel to work in order to save or send back remittances, their own consumption may be quite low. However, the consumption of the sending household, which is likely to receive remittances, may increase. The effect of the MCH-FP program on log annual consumption per capita in 2012 US dollars is presented in Table 4 for one's own household (column 1) and sending or Matlab household (column 2). There is less attrition for sending households so the sample size is slightly larger. There are no statistically significant effects for either cohort of interest, and the program effects are small.[11] These results are small and insignificant, and they mimic the income results.

To measure the effect of the program on investment or wealth instead of income, we examine the effect of the program on assets. In Table 4, we consider four categories of assets: household assets, productive assets, livestock, and land. We create a measure of total asset values that includes nonland assets. Land sizes are presented for agricultural and nonagricultural plots owned by the household, and are in decimals (1/100 acre).[12] Similar to consumption, we report effects for the Matlab or sending household. Column 3 shows that treatment individuals in the 24–29 cohort have 14 percent lower assets values, and the 30–34 cohort 18 percent. These results are consistent with a log specification where outliers are not dropped as an alternative way to deal with outliers (results not reported). The differences in total asset values are largest for household assets (column 4) and livestock (column 6), with the differences being biggest for livestock values at 20–37 percent. There is no program effect on land (columns 7 and 8). Reductions in household assets is driven by lamps and televisions among the 24–29 cohort, and among most types of household assets among the 30–34 cohort (Table A6), and for livestock by a reduction in cows but more ducks (Table A7)

Finally, the effect of the program on the number of loans by type shows that both age cohorts are more likely to have a loan (Table A8). The 24-29 cohort is more likely to have business loans and the 30-34 cohort personal loans, though results are only significant at the 10 percent level. These results are consistent with the 24-29 being more likely to start a business.

VI. Mechanisms and Heterogeneity

Two key mechanisms through which the MCH-FP interventions may affect the outcomes are reduced family sizes and improved human capital. In addition, Barham and Kuhn (2014) demonstrate that adults in the treatment area during program roll out were less likely to migrate out of the study areas, creating thinner migrant networks for their children (of whom many are in the sample for this paper) and creating another potential mechanism through which the program may have affected outcomes. To understand the mechanisms, we first examine the direct effect of the program on each mechanism in Table 5. The endogeneity of the potential mechanisms makes it difficult to determine the causal effect of these mechanisms on labor market and migration outcomes directly. However, to provide some evidence we include each of the endogenous variables as controls in the main regressions for labor market and migration outcomes in Table 6. If including the endogenous mechanism reduces the point estimate of the

[11] Results are similar for household consumption instead of per capita or including monetary transfers received.
[12] Given that land is often inherited rather than sold, land values can be reported with error, we focus on estimates using land size which is better known by the respondents.

treatment effect, the mechanism may be a potential pathway through which the interventions affected the outcome. Finally, to further explore why the program had a negative effect on earnings and international migration for the 30-34 cohort, we examine heterogeneity by the number of siblings born when the child health interventions were available in Table A9.

We explore the program effect on three potential types of mechanisms: human capital, family size, and migration networks. Barham (2012) and Barham, Kagy, and Hamadani (2016) have already demonstrated the positive direct effect the program has on height and education for the 20–24 cohort and the statistically insignificant negative effect for the 30–34 cohort.[13] So, Table 5 focuses on two measures of family size (number of younger male siblings, number of older male siblings), 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). We restrict our attention to male siblings because it is men who predominately migrate for work. The 24–29 cohort is 19 percent less likely to have older siblings, and the 30–34 cohort 33 percent less likely to have younger siblings, consistent with the roll out of the family planning program. Both treated cohorts have fewer people who are migrants in their bari migrant network than the comparison group.

To examine if these mechanisms are possible pathways through which the program affected the main labor market and migration outcomes, the mechanisms are added as controls to the main regression specifications in Table 6 for earning in past 12 months (Panel A), professional or semi-professional jobs (Panel B), current migration (Panel C), and international migration (panel D). We include current migration to any destination as a potential mechanism because it is possible that the differential migration patterns between the treatment and comparison area account for the lack of effects on income. We include each mechanism separately, as well as, together as the variables may be correlated. To be able to compare across columns, we restrict the sample size to be the same, only keeping observations with all mechanisms, and present the main results with reduced sample size in the first column. Earning is only affect by including the control for endogenous migration. The program effect for the 24–29 year olds is now positive at US$ 159.52, but for the 30–34 year olds, remains negative and significant at US$ -334.19.

For semi-professional or professional jobs (Panel B), there is a reduction in the treatment effect for the 24–29 cohort when controls for the number of male siblings and education are included. Controlling for education reduces the program effect by 25 percent, and the program effect is not significant controlling for all mechanisms together. This represents a 40% reduction in the point estimate. These results indicate that education may be one of the pathways through which the program led to the treatment group obtaining "better" jobs.

For current migration, including the endogenous outcomes does not explain differential migration among the 24-29 cohort. However, for the 30–34 cohort, the program effect on migration to any destination is reduced by 25 percent when education is introduced. While there are no significant effects of the program on male education for the 30–34 year olds, if more educated children in a family migrate, this may explain why including education attenuates the program effect on migration. There is a similar effect of education on international migration, but in addition, the program effect is also attenuated by 25 percent with the introduction of controls for the number of younger siblings.

[13] Barham (2012) shows the 24–29 cohort was taller, had higher levels of cognition, and received more schooling between the ages of 8 and 14. By age 24–29, treated boys have 0.5 more years of education, and are 0.15 standard deviations taller (Barham, Kagy, and Hamadani, 2016). There are significant effects for the 30–34 cohort.

Effect of Younger Siblings on the 30–34 Cohort— The previous analysis on mechanisms is suggestive that the number of siblings may play a role in the negative program effect on international migration, and through migration, income for the 30–34 cohort. The program led the 30–34 cohort to have fewer siblings, and for those with younger siblings born after the child health program rolled out, their younger siblings have on average higher human capital. If the family needs at least one male child to stay in Bangladesh to help the family, and/or prefer to support children with more education or human capital to migrate, then those in the 30-34 cohort with more younger siblings born after the child health interventions maybe less likely to migrate. To test this hypothesis, we would like to know how the number of siblings the treated group would have had, in the absence of the program. Instead, we use endogenous sibship size, and compare the program effects for those who have one or fewer male sibling born after 1981, to those who have two or more male siblings born after 1981 (referred to as 2 plus sibs born after 81 in the tables). We focus on number of male siblings because it is almost exclusively men who migrate internationally for work in the sample. None of the point estimates on the interaction effects are statistically significant, highlighting the fact that the sample size is small. However, the point estimates in Table A9 panel A suggest that those in the 30–34 cohort with 2 or more younger siblings born after 1981 are approximately 40 percent (10 percentage points on a base of 0.25) less likely to be a current international migrant and have earnings that are about 16 percent (311 USD on a base of 1991 USD) lower.

Who migrates within the family may be related to birth order. Birth order is endogenous due to the family planning program, however, the first-born male is likely to be the least endogenous of the birth orders, because families in the study area desire a male child. Sample sizes are small with 114 observations or less on first-born children in the 30-34 cohort. Results in Table A9 panel B indicate that first-born children with two more siblings born after 1981 are less likely to migrate internationally and earn substantially less, though results are not significant.

Heterogeneity.— It is important to understand what types of people or households the program most affected. We examine the heterogeneity of program effects on whether the person had a semiprofessional or professional job, income and current migration. We include the baseline controls and their interactions with age group treatment effects for the 24–29 and 30–34 year olds, but only report the interactions. We use principal components to construct four composite measures of baseline assets: housing characteristics, improved water, household assets, and household amenities. Among the baseline variables, we include an exogenous measure of an individual's network prior to the program, we focus on bari networks and define the network as the number of people in an individual's treatment bari in 1974 who migrated out of Matlab before the MCH-FP program started.[14] All variables are in z-scores.

Results, reported in Table A10, show there are few heterogeneous effects and significant variables are not consistent across outcomes or cohorts. Network size seems to be important for the age 24–29 cohort. An increase of one standard deviation in the preprogram network size results in a 7 percentage point increase in the differential program effect on current migration to urban destinations and a 4 percentage point increase to international destinations, though the effect on international migration is not statistically significant. These results indicate that, for 24–29 year olds, once their network is large enough, there is no longer a statistically significant difference in urban out-migration between treatment and comparison areas.

[14] Results are similar if the share of migrants is used instead of the total number of migrants.

VIII. ITT Program Impacts for Women

Women's labor market and migration opportunities are different than for men in Matlab, Bangladesh. For example, less than 30 percent of women work for wages. The majority work in agriculture or manual jobs, few work in professional or semi-professional jobs, and approximately 70 percent report unpaid household work as their primary activity. Generally, women in our sample have on average low incomes, less than $200 annually. Migration is also less common than for men at around 40 percent and there is almost no international migration. Unlike men who predominately migrate for work, almost all the women in our sample migrate for marriage or to be with their husband. While in other areas of Bangladesh women may migrate to work in the garment industry, less than 5 women in our entire sample migrated for garment industry work, and there is no garment work in Matlab.

We present a number of the same robustness checks for women in in Table D2, that were performed on the results for the men. Results accounting for a number of different weight schemes are presented in Appendix E. Results are similar for the various robustness checks and weight schemes. There is some loss of statistical significant on working for pay for some runs, though this was only statistically significant at the 10 percent level in the main tables.

Labor Market Outcomes. —To understand the impact of the program on women's labor market outcomes, we examine program effects on labor market participation and type of work in Table 7, and earnings and hours worked in the past 12 months in Table 8. For the 24–29 cohort, there is a 23 percent increase in the likelihood of working in paid work that is marginally significant at the 10 percent level. The increase in paid work is driven by a 49 percent increase in paid agriculture work. 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 30–34 cohort, there are no significant program effects on labor market participation or type of job.

The increase in labor market participation did not lead to significant differences in earnings or hours worked between the treatment and comparison areas for either age group. Given the relatively small percent of women who work, we also examine difference in wages conditional on working, though these program effects need to be interpreted with care because selection into paid work is endogenous. Again, there are no program effects on earnings among those who work for either age group.

Micro-credit programs often encourage investment in animals, possibly confounding ITT effects if access to micro-credit programs differs across treatment and comparison areas in the past or present. Micro-credit is now pervasive in both the treatment and comparison areas, and when it was first introduced between 1993-1996, a cross over design with the MCH-FP program was used so availability of credit would be balanced in both areas. In the robustness checks we include an extended set of controls, that includes controls for micro-credit and the results remain the same (see Table D2, Panel D and E). 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).

In order to understand the effects of the program on labor market and migration outcomes, we examine the effect of the program on 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 is 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 A11 show that, there is no program effect on marriage, age of marriage, age of first child or number of children at this point in time. Most of these young women have not completed their fertility so differences may appear later in life.

Migration. —Table 8 presents program effects for current migration by destination. For the 20–24 cohort, program effects on current migration are small and insignificant. However, among the 30–34 cohort current migration rates are 12 percentage points or 30 percent lower among treated than the comparison group, 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 just for the spouses of the 30–34 cohort. We find that the spouses of these women are also about 12 percentage points less likely to migrate to urban areas (results not reported). These results need to be interpreted with care because we were only able to match about 60 percent of women to their spouse, but they are suggestive that the migration results for these women are linked to the program effect of their husbands.

Consumption, Assets, Savings and Loans. — Similar to men, there are no statistically significant program effects on consumption (Table 9 columns 1 and 2). However, the point estimates suggest a modest 6 percent increase in both total household and per capita consumption. Similarly, there are no statically significant program effects on household assets (Table 9 column 6) or whether the women reports owning any productive assets herself (Table 9 column 7). We take advantage of a module on women's status to determine if the women themselves have any cash savings or have ever taken out a micro-credit loan. Unfortunately, individual or household savings was not collected in MHSS2. Consistent with the 24–29 cohort being more likely to raise small animals, these women are also 32 percent more likely to have some cash savings and 29 percent more likely to have taken out a micro-credit loan at some point (Table 9, column 8 and 9). While the point estimates are positive for the 30–34 cohort, they are small and statistically insignificant. These results are encouraging because they indicate that, similar to the men, the women who benefited from the child health interventions appear to be more entrepreneurial in their work, take advantage of micro-credit for their business, and have some autonomy over the money they make by having their own savings.

IX. DISCUSSION

This paper provides new evidence that early child health and family planning interventions can lead to improvements in labor market outcomes and reduce migration. We take advantage of program rollout and the quasi-random placement of a well-run health and family planning program in a rural area of Bangladesh, Matlab, to examine the effect of the program thirty-five years after program start. Program effects represent the combined effects of all MCH-FP interventions. While it is difficult to separate out the effect of the individual interventions, having healthier children often leads to smaller family sizes regardless of family planning interventions (Angeles, 2010; Sah, 1991). Even so, two of the main program interventions, family planning and childhood vaccinations, are arguably two of the most effective and widespread health programs in the past century, and understanding their combined effect is important.

We find that both men and women in the 24–29 cohort who had access to the child health interventions at birth had better labor market outcomes and current migration rates were lower for men. Men had more professional or semi-professional jobs as evidenced by using more skill and less physical labor in their work and were more entrepreneurial. Women were more likely to engage in paid agricultural work, have their own savings and take out micro-credit loans. Despite these improvements, earnings in the last 12 months were on average similar between the treated and comparison areas. For men, this is linked to a reduction in migration and shorter migration durations mainly to urban areas in Bangladesh where wages are generally higher.

Examination of mechanisms suggests that improvements in education account for approximately a quarter of the program's impact on "better" jobs, and the negative program effect on migration is attenuated for those who had larger migrant networks at baseline. These results provide new evidence on the importance of human capital accumulation and migrant networks for labor market outcome in low-income countries. It is possible that the program effect on earnings was not larger because past migrant networks were unable to help these young men find "better" jobs in migrant destinations, because migrants in the network did not themselves have "better" jobs, pointing to the importance of investigating the quality of the migrant networks in the future. In contrast, males in the 30–34 cohort born prior to the child health interventions did not fare as well, and there were no significant effects for women in this cohort. The men experienced a significant reduction in annual earnings and were less likely to be a current migrant to international destinations where wages are substantially higher. Heterogeneity analysis is suggestive that those with more younger siblings born when the child health program was available are more negatively affected. This suggests that, in areas where location and occupation decisions are often made at the household level, there can be unintended consequences of policies that lead to differential human capital among siblings and/or changes in family sizes.

In general, the results speak to the potential benefits of early access to health and family planning interventions. The fact that the treated men were less likely to migrate in both cohorts was surprising, but consistent with the treated having smaller migrant networks as a result of their parents being more likely to stay in Matlab for the program (Barham and Kuhn 2014), and models of return migration. These findings demonstrate that place-based programs designed to improve human capital may reduce labor migration and its associated costs.

Lastly, the lack of effect on earnings may be temporary, as the 24–29 year olds are early in their careers and about five percent of men are still in school. If working a "better" or more entrepreneurial job earlier in one's career increases one's future earnings potential, our estimated program effects could understate the program's effect on lifetime earnings. The results also provide some evidence that improved human capital may be one factor that facilitates a shift of employment to more productive jobs in low-income countries. It will be important to track these young men in the future when earnings are higher and when the process of return migration and the accumulation of capital from migration is more complete.

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Tables and Figures

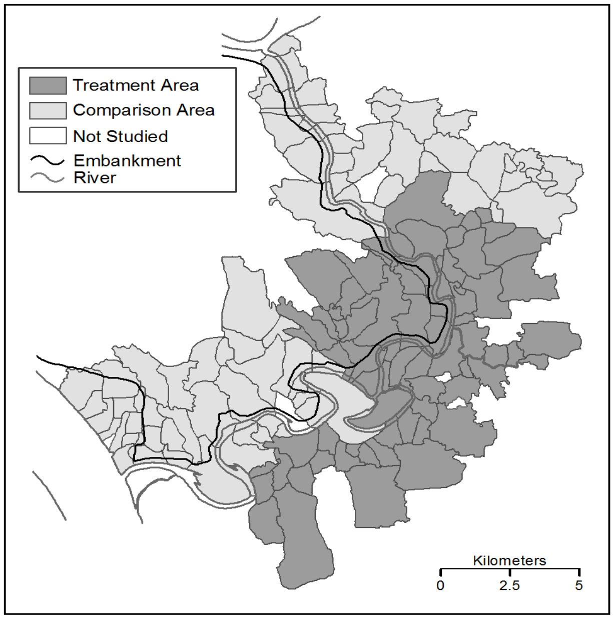

FIGURE 1. MAP OF MATLAB STUDY AREA

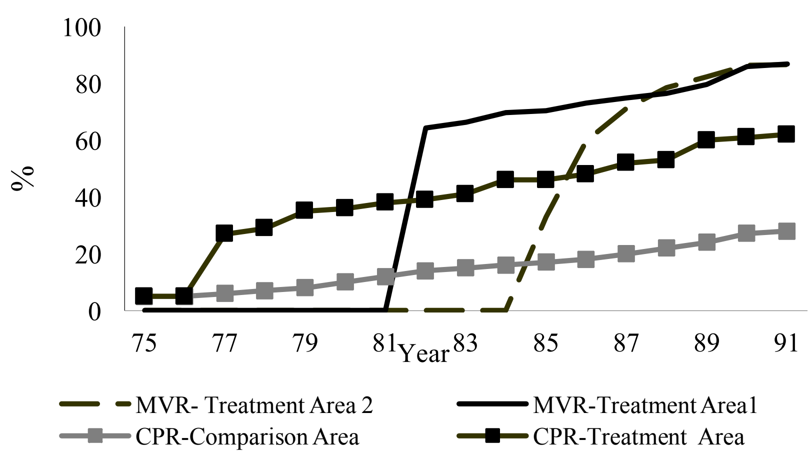

FIGURE 2. TRENDS IN CONTRACEPTIVE PREVALENCE RATE (CPR) AND MEASLES VACCINATION RATES (MVR) FOR CHILDREN 12–59 MONTHS BY CALENDAR YEAR

Source: Contraceptive use data from van Ginniken et al. 1998; measles vaccination data from iccdr,b Record Keeping System.

TABLE 1—ITT EFFECTS ON PARTICIPATION IN PAID WORK AND SKILL REQUIREMENTS, MEN

	Any Paid Work (=1)	Type of Work (=1)			Self-Employed (=1)		Start Own Business (=1)	Required Skills (=1)		
		Prof. & Semi-Prof.	Agriculture	Manual	Any	Prof. & Semi-Prof.		Reading, Writing, Math	Physical	Any Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Single Differences										
Treat*(Age 24–29)	-0.01	0.08	-0.02	-0.07	0.09	0.05	0.10	0.09	-0.04	0.01
	(0.02)	(0.03)*	(0.02)	(0.04)+	(0.04)*	(0.03)+	(0.04)**	(0.04)*	(0.03)	(0.03)
Treat*(Age 30–34)	-0.04	-0.01	0.06	-0.05	0.02	0.01	0.02	-0.06	0.02	-0.05
	(0.02)	(0.05)	(0.02)*	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
Panel B: Percent Changes										
Treat*(Age 24–29)	-1%	25%	-34%	-13%	44%	39%	52%	34%	-5%	6%
Treat*(Age 30–34)	-4%	-3%	168%	-9%	7%	5%	7%	-19%	2%	-23%
Age 24–29 Means	0.90	0.32	0.06	0.56	0.21	0.13	0.19	0.26	0.85	0.17
Age 30–34 Means	0.96	0.39	0.04	0.57	0.29	0.19	0.29	0.31	0.85	0.21
R-Squared	0.07	0.06	0.06	0.07	0.06	0.05	0.07	0.07	0.07	0.09
Observations	1,302	1,302	1,302	1,302	1,302	1,302	1,302	1,302	1,302	1,302

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE 2—ITT EFFECTS ON INCOME, HOURS AND CURRENT MIGRATION, MEN

	Earnings Past 12 Months (USD)		Hours Worked Past 12 months	Current Migration Out of Matlab			
	Full Sample	Trim 5%	Full Sample	All	International	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Single Differences							
Treat*(Age 24–29)	1,074.64	-36.73	-23.39	-0.13	-0.02	-0.10	-0.01
	(679.42)	(108.57)	(97.40)	(0.04)**	(0.03)	(0.04)*	(0.01)
Treat*(Age 30–34)	-1,157.73	-462.00	-72.40	-0.10	-0.10	-0.02	0.02
	(409.87)**	(155.39)**	(114.47)	(0.05)+	(0.04)*	(0.04)	(0.02)
Panel B: Percent Changes							
Treat*(Age 24–29)	45%	-2%	-1%	-20%	-8%	-28%	-24%
Treat*(Age 30–34)	-36%	-23%	-2%	-17%	-37%	-7%	168%
Age 24–29 Comp. Means	2,363	1,638	3,004	0.65	0.25	0.35	0.04
Age 30–34 Comp. Means	3,232	2,024	3,252	0.58	0.27	0.30	0.01
R-Squared	0.04	0.09	0.08	0.06	0.07	0.06	0.04
Observations	1,290	1,182	1,290	1,302	1,302	1,302	1,302

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3. All incomes are reported in 2012 USD. For trim 5% the highest 5 percent of male incomes are set to missing. Urban migration is defined as living in Dhaka, Chittagong, or their surrounding metro areas.

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

TABLE 3—ITT EFFECTS ON MIGRATION ROBUSTNESS, MEN

	Current Migration Out of Matlab	Migration Duration Past 5 Years Conditional on Migration by Destination (in Years)				Ever Migrated	Age at First Migration	DSS Current Migration	Migration at Age 24
		All	International	Urban	Rural				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences									
Treat*(Age 24–29)	-0.13	-0.21	0.04	-0.13	0.05	-0.09	0.02	-0.08	-0.14
	(0.04)**	(0.12)+	(0.22)	(0.16)	(0.54)	(0.03)**	(0.35)	(0.03)*	(0.04)**
Treat*(Age 30–34)	-0.10	-0.29	-0.33	-0.05	0.80	-0.07	-0.38	-0.11	-0.02
	(0.05)+	(0.14)*	(0.26)	(0.26)	(0.77)	(0.04)+	(0.58)	(0.05)*	(0.05)
Panel B: Percent Changes									
Treat*(Age 24–29)	-20%	-5%	1%	-3%	2%	-10%	0%	-13%	-23%
Treat*(Age 30–34)	-17%	-6%	-8%	-1%	29%	-8%	-2%	-18%	-4%
Age 24–29 Means	0.65	4.364	3.638	4.035	2.960	0.86	19.36	0.63	0.62
Age 30–34 Means	0.58	4.563	4.118	4.358	2.786	0.86	21.25	0.60	0.51
R-Squared	0.06	0.07	0.19	0.09	0.52	0.09	0.18	0.06	0.07
Observations	1,302	907	358	554	113	1,302	1,048	1,302	1,302

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3. Age at first migration and total months spent as a migrant are calculated using the DSS data.

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

TABLE 4—ITT EFFECTS ON CONSUMPTION AND ASSETS, MEN

	Log PC Consumption (USD)		Total Assets Trim 5%	Asset Value (USD)			Land (Decimals)	
	Own House	Matlab House		Household	Productive	Live stock	Agricultural	Non-Agric.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences								
Treat*(Age 24–29)	-0.05	-0.00	-246.37	-173.83	-0.44	-72.10	-8.94	0.25
	(0.05)	(0.05)	(122.17)*	(98.77)+	(52.16)	(27.28)**	(6.29)	(2.57)
Treat*(Age 30–34)	-0.05	-0.03	-299.97	-239.62	-17.20	-43.15	-10.67	-9.45
	(0.06)	(0.06)	(142.05)*	(147.61)	(41.78)	(46.96)	(8.47)	(5.89)
Panel B: Percent Changes								
Treat*(Age 24–29)			-14%	-13%	0%	-37%	-19%	1%
Treat*(Age 30–34)			-18%	-18%	-13%	-20%	-25%	-37%
Age 24–29 Comp. Means	1,069	837	1,710	1,348	165	197	47.01	21.66
Age 30–34 Comp. Means	1,016	738	1,668	1,312	138	219	43.18	25.85
R-Squared	0.12	0.13	0.19	0.17	0.07	0.12	0.22	0.10
Observations	1,110	1,208	1,151	1,151	1,151	1,151	1,207	1,204

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3. All dependent variables are in USD except columns 7 and 8 , which are in land decimals (1/100 acre). In columns 2-5, 5% of the largest total assets values are set to missing. Column 1 residents are linked to their current own household's asset module. Column 2, migrants out of Matlab are linked to the household in Matlab that listed them as a migrant in the migrant tracking module. Standard errors are clustered at the treatment village level.

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

TABLE 5—ITT EFFECTS ON POTENTIAL MECHANISMS, MEN

	No. Younger Male Siblings	No. of Older Male Siblings	Father Ever Migrated (=1)	No. of Migrants in Bari Network
	(1)	(2)	(3)	(4)
Panel A: Single Differences				
Treat*(Age 24–29)	-0.14	-0.31	-0.06	-7.60
	(0.09)	(0.12)*	(0.04)	(2.38)**
Treat*(Age 30–34)	-0.43	0.10	-0.08	-9.52
	(0.11)**	(0.14)	(0.05)+	(2.31)**
Panel B: Percent Changes				
Treat*(Age 24–29)	-16%	-19%	-14%	-9%
Treat*(Age 30–34)	-33%	8%	-22%	-12%
Age 24–29 Comp. Means	0.90	1.64	0.41	81.30
Age 30–34 Comp. Means	1.31	1.26	0.37	81.56
R-Squared	0.10	0.10	0.08	0.77
Observations	1,299	1,299	1,389	1,425

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 variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE 6—ITT EFFECTS CONTROLLING FOR MECHANISMS, MEN

	Base Model	Endogenous Control Variables for Regressions on Outcome in Each Panel						
		Migrant (=1)	No. Older Male Sibs.	No. Younger Male Sibs.	Years of Education	Height Z-score	No. Migrants in Network Z-score	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Earnings Past 12 Months (USD) 5 % Trim								
Treat*(Age 24-29)	-32.58	159.52	-52.41	-38.91	-58.12	-49.54	-37.14	127.28
	(109.33)	(108.80)	(106.31)	(109.00)	(109.65)	(108.94)	(109.36)	(105.23)
Treat*(Age 30-34)	-460.88	-354.49	-461.07	-477.73	-405.39	-434.31	-475.11	-334.19
	(161.91)**	(141.32)*	(162.05)**	(163.28)**	(150.29)**	(157.63)**	(161.34)**	(133.17)*
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 24-29)	0.08	0.09	0.07	0.07	0.06	0.08	0.08	0.05
	(0.04)*	(0.03)**	(0.03)*	(0.04)*	(0.03)+	(0.03)*	(0.04)*	(0.03)
Treat*(Age 30-34)	-0.01	-0.01	-0.01	-0.02	0.01	-0.00	-0.02	-0.01
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Observations	1,246	1,246	1,246	1,246	1,246	1,246	1,246	1,246
Panel C: Current Migration Out of Matlab to Any Destination (=1)								
Treat*(Age 24-29)	-0.13		-0.14	-0.13	-0.15	-0.13	-0.13	-0.16
	(0.04)**		(0.04)**	(0.04)**	(0.04)**	(0.04)**	(0.04)**	(0.04)**
Treat*(Age 30-34)	-0.08		-0.08	-0.08	-0.06	-0.08	-0.09	-0.05
	(0.05)+		(0.05)+	(0.05)	(0.05)	(0.05)	(0.05)+	(0.05)
Observations	1,246		1,246	1,246	1,246	1,246	1,246	1,246
Panel D: Current Migration Out of Matlab to International Destination (=1)								
Treat*(Age 24-29)	-0.01		-0.01	-0.01	-0.02	-0.02	-0.01	-0.02
	(0.03)		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Treat*(Age 30-34)	-0.09		-0.09	-0.08	-0.08	-0.09	-0.09	-0.06
	(0.04)*		(0.04)*	(0.04)+	(0.04)*	(0.04)*	(0.04)*	(0.04)
Observations	1,246		1,246	1,246	1,246	1,246	1,246	1,246

Notes: Each column is a separate regression of the outcomes variables in the panel title on the endogenous variable in the column headings. 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 variables in Table A3. ** p<0.01, * p<0.05, + p<0.1

TABLE 7—ITT EFFECTS ON PARTICIPATION IN PAID WORK BY TYPE OF WORK, WOMEN

	Any Paid Work (=1)	Type of Work (=1)				Animals (=1)	Type of Animals (=1)	
		Professional, Clerical, or Sales	Agriculture	Manual	Unpaid Household Work		Cows, Goats, or Sheep	Ducks Or Hens
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences								
Treat*(Age 24–29)	0.06	0.01	0.06	-0.01	-0.05	0.06	-0.01	0.07
	(0.03)+	(0.01)	(0.02)**	(0.03)	(0.03)	(0.02)**	(0.02)	(0.02)**
Treat*(Age 30–34)	0.08	-0.01	0.04	0.04	-0.06	0.04	-0.00	0.04
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)	(0.04)	(0.03)	(0.03)
Panel B: Percent Changes								
Treat*(Age 24–29)	23%	23%	49%	-6%	-8%	49%	-15%	129%
Treat*(Age 30–34)	29%	-28%	21%	30%	-9%	21%	0%	47%
Age 24–29 Means	0.27	0.04	0.12	0.16	0.64	0.12	0.07	0.05
Age 30–34 Means	0.28	0.04	0.19	0.13	0.63	0.19	0.10	0.08
R-Squared	0.04	0.05	0.08	0.05	0.04	0.08	0.06	0.07
Observations	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. Means by age cohort are for the comparison group. 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 variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE 8—ITT EFFECTS ON INCOME, HOURS AND CURRENT MIGRATION, WOMEN

	Earnings Past 12 Months (USD)		Earning Past 12 Months Conditional on Working (USD) Trim 5%	Hours Worked Past 12 months Full Sample	Current Migration Out of Matlab			
	Full Sample	Trim 5%			All	International	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences								
Treat*(Age 24–29)	49.29	-1.19	14.32	79.37	-0.03	0.00	-0.02	-0.02
	(41.19)	(12.30)	(40.81)	(71.31)	(0.04)	(0.01)	(0.04)	(0.02)
Treat*(Age 30–34)	-125.89	17.80	-40.79	-39.77	-0.12	0.00	-0.11	-0.01
	(112.98)	(11.18)	(37.60)	(111.86)	(0.06)*	(0.01)	(0.05)*	(0.03)
Panel B: Percent Changes								
Treat*(Age 24–29)	37%	-3%	9%	19%	-8%	0%	-7%	-22%
Treat*(Age 30–34)	-67%	60%	-35%	-8%	-30%	0%	-35%	-11%
Age 24–29 Comp. Means	134	41	163	420	0.38	0.00	0.29	0.09
Age 30–34 Comp. Means	189	30	117	480	0.41	0.00	0.31	0.09
R-Squared	0.06	0.04	0.20	0.05	0.06	0.05	0.05	0.07
Observations	1,216	1,121	254	1,216	1,220	1,220	1,220	1,220

Notes: All incomes are reported in USD and are calculated by summing earnings across all employment activities where earnings from household businesses are split evenly between all workers in a household that report working for the family business. For trim 5% the highest 5 percent of female incomes are set to missing. Income is expressed in US Dollars. 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 variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE 9—ITT EFFECTS ON PERSONAL ASSETS, SAVINGS AND LOANS, WOMEN

	Consumption		Assets Values (USD)		Women's Report of Own Resources		
	Log	Log Per Capita	Full Sample	Trim 5%	Owns a Productive Asset (=1)	Any Cash Savings (=1)	Ever had Micro Credit Loan (=1)
	(1)	(2)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences							
Treat*Age 24-29	0.06	0.06	-248.92	-56.21	0.02	0.07	0.06
	(0.04)	(0.04)	(468.79)	(106.48)	(0.03)	(0.03)*	(0.03)+
Treat*Age 30-34	-0.00	-0.07	-553.18	-163.19	0.01	0.03	0.04
	(0.06)	(0.06)	(616.45)	(162.23)	(0.04)	(0.05)	(0.05)
Panel B: Percent Changes							
Treat*(Age 24–29)			-9%	-3%	15%	32%	29%
Treat*(Age 30–34)			-26%	-11%	5%	13%	13%
Age 24–29 Comp. Means	2860	690.7	2719	1652	0.130	0.216	0.207
Age 30–34 Comp. Means	2860	682.3	2109	1483	0.184	0.234	0.314
R-squared	0.11	0.10	0.12	0.14	0.05	0.07	0.08
Observations	1,225	1,225	1,129	1,079	1,214	1,209	1,214

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

Appendix A

Appendix Tables and Figures

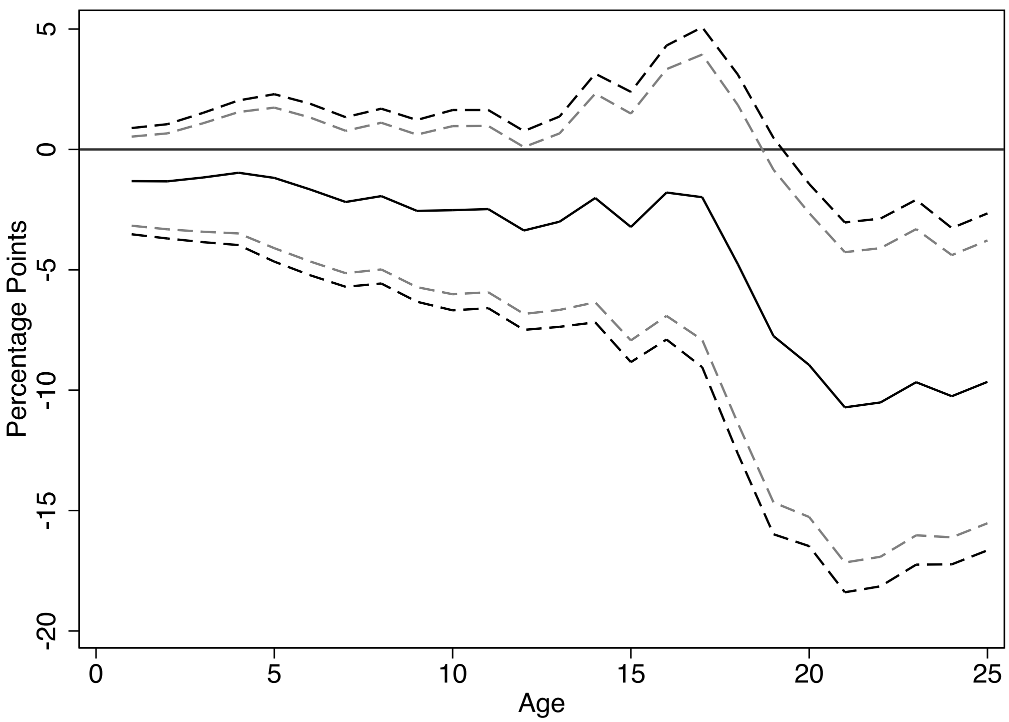

FIGURE A1. ITT EFFECTS ON MIGRATION BY AGE, MALES AGED 24–29

Notes: The data are the estimated coefficients of the ITT effect for the 24–29 age cohort from regressions where the dependent variable is migration at age *a*. Migration at each age is determined using the DSS. The figure plots 5% and 10% confidence bands for each estimated coefficient.

TABLE A1—MCH-FP PROGRAM INTERVENTIONS BY BIRTH YEAR

Year of Birth	Age 2012	Program Eligibility
Jan. 1947–Sept. 1977	35–65	*Preintervention Period*
Oct. 1977–Feb. 1982	30–34	*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	24–29	*Child Health Interventions Added*
Mar. 1982–Oct. 1985	27–29	*Interventions Added in Half the Treatment Area* Children under age five eligible for measles vaccination in half the treatment area
Nov. 1985–Dec. 1988	24–26	*Intervention Extended to Entire Treatment Area* 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—ANALYSIS SAMPLE ATTRITION RATES FOR MHSS2 DATA

	Men			Women		
	%	Difference in Rates Treatment - Comparison		%	Difference in Rates Treatment - Comparison	
		Mean	SE		Mean	SE
Panel A: Analysis sample ages 24-34 with non-missing MHSS2 annual income information						
Not found or refused	5.2	-0.009	(0.012)	5.4	-0.021	(0.013)
Not found, refused, or dead	7.0	-0.013	(0.013)	7.0	-0.020	(0.014)
Non-missing annual income information	9.5	-0.016	(0.015)	8.1	-0.013	(0.016)
Age 24-29	9.6	-0.002	(0.020)	8.5	-0.022	(0.019)
Age 30-34	9.5	-0.042	(0.022)	7.3	-0.005	(0.026)

Notes: The standard error on the difference in attrition rate between treatment and control is clustered at the village level.

TABLE A3—BALANCE ON 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)
Individual Characteristics							
Male (=1)	0.51	(0.47)	0.52	0.52	0.00	-0.18	-0.01
Birth year	1983	(2.54)	1983	2.79	0.05	0.43	0.02
Muslim (=1)	0.86	(0.33)	0.96	0.36	-0.10	-3.84	-0.29
Household Characteristics							
Land size 1982 (decimals)	10.46	(18.29)	11.17	20.11	-0.71	-0.84	-0.04
Bari size	8.66	(9.21)	7.92	10.12	0.74	1.64	0.07
Family size	6.79	(3.49)	6.55	3.83	0.24	1.40	0.06
Owns a lamp (=1)	0.63	(0.77)	0.59	0.85	0.04	1.27	0.05
Owns a watch (=1)	0.15	(0.49)	0.16	0.54	0.00	-0.22	-0.01
Owns a radio (=1)	0.08	(0.34)	0.08	0.37	0.00	0.25	0.01
Wall tin or tinmix (=1)	0.29	(0.62)	0.30	0.68	-0.01	-0.35	-0.01
Tin roof (=1)	0.83	(0.57)	0.84	0.62	0.00	-0.11	0.00
Latrine (=1)	0.82	(0.70)	0.88	0.77	-0.06	-1.98	-0.08
Number of rooms per capita	0.23	(0.15)	0.22	0.16	0.00	0.49	0.02
Number of cows	1.43	(2.20)	1.37	2.42	0.06	0.59	0.03
Number of boats	0.65	(1.09)	0.66	1.20	-0.02	-0.35	-0.01
Drinking water, tubewell (=1)	0.30	(0.87)	0.16	0.95	0.14	3.46	0.15
Drinking water, tank (=1)	0.42	(1.40)	0.34	1.54	0.08	1.31	0.05
HH age	47.88	(17.93)	45.47	19.71	2.41	2.93	0.12
HH years of education	2.43	(3.84)	2.27	4.22	0.16	0.89	0.04
HH works in agriculture (=1)	0.60	(0.73)	0.58	0.80	0.02	0.76	0.03
HH works in business or service (=1)	0.11	(0.39)	0.08	0.43	0.03	1.47	0.06
HH works in fishing (=1)	0.06	(0.33)	0.06	0.36	-0.01	-0.32	-0.01
HH spouse's age	37.29	(16.21)	35.25	17.81	2.04	2.80	0.11
HH spouse's years of education	1.04	2.15	1.21	2.37	-0.16	-1.72	-0.07

Notes: Sample includes respondents who have annual income data in MHSS2. Standard deviations (SD) are clustered at the treatment village level. There are 1,135 treatment area observations and 1,371 comparison area observations. Standard deviations in column 7 are based on the comparison group.

TABLE A4—ITT EFFECTS ON TYPE OF WORK, ROBUSTNESS CHECKS, MEN

	Professional, Clerical, or Sales			Father Had Prof. or Semi Prof. Job in 1996 (MHSS1)
	Main	Remove Small Shops	Profession Only	
	(1)	(2)	(3)	(4)
Panel A: Single Differences				
Treat*(Age 24–29)	0.08	0.06	0.06	-0.01
	(0.03)*	(0.02)*	(0.03)*	(0.04)
Treat*(Age 30–34)	-0.01	-0.03	-0.00	-0.08
	(0.05)	(0.04)	(0.04)	(0.06)
Panel B: Percent Changes				
Treat*(Age 24–29)	25%	43%	65%	-3%
Treat*(Age 30–34)	-3%	-21%	0%	-27%
Age 24–29 Comp. Means	0.32	0.14	0.09	0.30
Age 30–34 Comp. Means	0.39	0.14	0.12	0.30
R-Squared	0.06	0.08	0.08	0.15
Observations	1,302	1,302	1,302	925

Notes: 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 only in a professional occupation. Column (4) indicates whether the respondent's father was in a professional, clerical, or sales occupation in 1996 from MHSS1.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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

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

	Type of Work (=1)						Earnings Past 12 Months (USD) 5% Trim		Hours Worked Past 12 Months 5% Trim	
	Professional and Semi-Professional		Agriculture		Manual					
	Migrants	Non-Migrants	Migrants	Non-Migrants	Migrants	Non-Migrants	Migrants	Non-Migrants	Migrants	Non-Migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Single Differences										
Treat*(Age 24–29)	0.10	0.07	-0.01	-0.06	-0.11	0.02	90.68	217.60	33.74	245.56
	(0.05)+	(0.06)	(0.01)	(0.04)	(0.05)*	(0.06)	(175.78)	(101.45)*	(113.26)	(158.91)
Treat*(Age 30–34)	0.05	-0.09	0.01	0.10	-0.07	-0.02	-472.13	-323.39	53.07	-103.60
	(0.06)	(0.08)	(0.01)	(0.05)*	(0.06)	(0.07)	(186.62)*	(179.46)+	(103.86)	(148.91)
Panel B: Percent Changes										
Treat*(Age 24–29)	28%	26%	-75%	-43%	-18%	4%	4%	29%	1%	11%
Treat*(Age 30–34)	14%	-21%	-	117%	-11%	-4%	-17%	-28%	2%	-4%
Age 24–29 Means	0.353	0.267	0.01	0.14	0.60	0.49	2,159	749	3,152	2,328
Age 30–34 Means	0.361	0.419	0.00	0.09	0.63	0.48	2,727	1,144	3,288	2,875
R-Squared	0.09	0.15	0.08	0.13	0.10	0.15	0.16	0.11	0.13	0.14
Observations	745	557	745	557	745	557	652	530	694	522

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE A6—ITT EFFECTS ON HOUSEHOLD ASSET OWNERSHIP, MEN

	No. of Assets	Asset Type (=1)							
		Lamp	Television	Radio	Fan	Stove	Refrigerator	Camera	Computer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Single Differences									
Treat*(Age 24–29)	-0.35	-0.07	-0.09	-0.03	-0.04	0.04	-0.01	-0.03	-0.01
	(0.16)*	(0.03)+	(0.04)*	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.01)
Treat*(Age 30–34)	-0.83	-0.07	-0.19	-0.08	-0.23	-0.10	-0.12	-0.03	-0.04
	(0.19)**	(0.03)*	(0.05)**	(0.04)+	(0.06)**	(0.03)**	(0.05)*	(0.02)	(0.02)+
Panel B: Percent Changes									
Treat*(Age 24–29)	-4%	-8%	-21%	-17%	-7%	75%	-6%	-52%	-24%
Treat*(Age 30–34)	-10%	-8%	-45%	-39%	-37%	-88%	-60%	-76%	-70%
Age 24–29 Means	7.81	0.84	0.43	0.17	0.59	0.05	0.17	0.06	0.04
Age 30–34 Means	7.92	0.87	0.42	0.21	0.63	0.11	0.20	0.04	0.06
R-Squared	0.16	0.07	0.1	0.06	0.09	0.12	0.13	0.12	0.11
Observations	1,208	1,208	1,208	1,208	1,208	1,208	1,208	1,208	1,208

Notes: Residents of Matlab are linked to their own household's asset module. Migrants out of Matlab are linked to the household in Matlab that listed them as a migrant in the migrant tracking module. 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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** $p<0.01$, * $p<0.05$, + $p<0.1$

TABLE A7—ITT EFFECTS ON LIVESTOCK OWNERSHIP, MEN

	Number of Animals				Value of Animals			
	Cows	Goats	Chickens	Ducks	Cows	Goats	Chickens	Ducks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Single Differences								
Treat*(Age 24–29)	-0.39	-0.04	-0.34	0.38	-91.64	-1.55	-1.49	1.15
	(0.12)**	(0.05)	(0.40)	(0.23)	(28.94)**	(1.43)	(1.12)	(0.69)+
Treat*(Age 30–34)	-0.24	0.13	-0.13	0.74	-63.79	4.27	-0.22	2.20
	(0.13)+	(0.08)	(0.50)	(0.29)*	(44.32)	(2.07)*	(1.32)	(0.89)*
Panel B: Percent Changes								
Treat*(Age 24–29)	-47%	-21%	-8%	27%	-47%	-29%	-14%	26%
Treat*(Age 30–34)	-27%	71%	-3%	49%	-30%	100%	-2%	47%
Age 24–29 Means	0.83	0.19	4.13	1.43	193.30	5.31	10.63	4.49
Age 30–34 Means	0.89	0.18	3.82	1.51	211.10	4.28	9.98	4.70
R-Squared	0.10	0.07	0.09	0.07	0.12	0.08	0.08	0.08
Observations	1,208	1,207	1,208	1,208	1,208	1,208	1,208	1,206

Notes: The dependent variables in columns (1)-(4) are the number of the listed animals the household owns. The dependent variables in columns (5)-(8) are the values of the stock of listed animals in USD. Residents of Matlab are linked to their own household's asset module. Migrants out of Matlab are linked to the household in Matlab that listed them as a migrant in the migrant tracking module. 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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE A8—ITT EFFECTS ON LOANS, MEN

	Any Loan	Type of Loan				
		Farm	Business	Housing	Personal	Repayment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Single Differences						
Treatment*(Age 24-29)	0.10	-0.01	0.07	0.02	0.03	-0.01
	(0.06)+	(0.01)+	(0.04)+	(0.02)	(0.05)	(0.01)
Treatment*(Age 30-34)	0.15	0.01	-0.03	0.03	0.12	0.02
	(0.09)+	(0.03)	(0.05)	(0.02)	(0.06)+	(0.01)
Panel B: Percent Changes						
Treatment*(Age 24-29)	38%	-58%	102%	67%	23%	-52%
Treatment*(Age 30-34)	32%	21%	-18%	69%	61%	169%
Age 24-29 Comp. Means	0.27	0.02	0.07	0.03	0.13	0.02
Age 30-34 Comp. Means	0.47	0.05	0.17	0.04	0.20	0.01
R-Squared	0.08	0.05	0.04	0.06	0.05	0.04
Observations	1,306	1,306	1,306	1,306	1,306	1,306

Notes: The dependent variable is the number of outstanding loans of the given type. 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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

TABLE A9—ITT EFFECTS BY NUMBER OF SIBLINGS BORN AFTER 1981, MEN

	Earnings	Current Migration Out of Matlab			
	Past 12 Months (USD)	All	International	Urban	Rural
	(1)	(2)	(3)	(4)	(5)
Panel A: All Birthorders					
Treat*(Age 30–34)	-380.44	-0.08	-0.06	-0.03	0.01
	(177.04)*	(0.06)	(0.04)	(0.05)	(0.02)
Treat*(Age 30–34)*2 plus sibs born after 81 (=1)	-311.31	-0.01	-0.10	0.03	0.06
	(374.01)	(0.13)	(0.10)	(0.12)	(0.04)
2 plus sibs born after 81 (=1)	-7.55	0.04	0.07	-0.02	-0.02
	(300.37)	(0.09)	(0.07)	(0.08)	(0.01)*
Observations	412	454	454	454	454
Age 30–34 Comp. Means	1991	0.57	0.25	0.31	0.01
Panel B: First Born Male					
Treat*(Age 30–34)	257.44	0.00	0.03	-0.03	0.00
	(433.47)	(0.13)	(0.11)	(0.10)	(0.02)
Treat*(Age 30–34)*2 plus sibs born after 81 (=1)	-1,143.83	-0.15	-0.08	-0.18	0.11
	(818.98)	(0.28)	(0.21)	(0.20)	(0.06)+
2 plus sibs born after 81 (=1)	80.16	0.16	0.19	-0.02	-0.00
	(566.05)	(0.16)	(0.13)	(0.13)	(0.02)
Observations	99	114	114	114	114
Age 30–34 Comp. Means	1796	0.53	0.25	0.28	0.00

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. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.

TABLE A10—HETEROGENEITY OF TREATMENT EFFECTS BY BASELINE CHARACTERISTICS, MEN

	Males Age 30-34				Males age 24-29			
	Type of Work: Prof./Clerical /Sales	Earning Past 12 months (5% Trim USD)	Current Migrant		Type of Work: Prof./Clerical/ Sales	Earning Past 12 months (5% Trim USD)	Current Migrant	
			Urban	Int'l			Urban	Int'l
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat (=1)	0.03	-223.42	-0.01	-0.11	0.07	41.22	-0.09	-0.01
	(0.07)	(209.30)	(0.07)	(0.06)*	(0.05)	(166.16)	(0.06)	(0.05)
T*1974 Migrant Network	0.03	85.08	-0.02	0.02	0.02	115.24	0.07	0.04
	(0.04)	(130.62)	(0.04)	(0.04)	(0.04)	(94.36)	(0.03)*	(0.03)
T* Family size (Z)	-0.06	67.93	-0.05	0.02	0.01	-119.33	0.06	-0.06
	(0.06)	(170.48)	(0.06)	(0.05)	(0.05)	(131.20)	(0.04)	(0.04)+
T* 1982 Land	0.01	28.14	-0.03	0.07	-0.00	-0.35	0.01	0.03
	(0.06)	(185.85)	(0.05)	(0.05)	(0.03)	(125.18)	(0.04)	(0.04)
T*WI - Housing Characteristics (Z)	0.14	-5.73	0.11	-0.08	-0.01	-10.40	0.02	-0.03
	(0.07)*	(198.66)	(0.07)	(0.06)	(0.04)	(167.12)	(0.04)	(0.05)
T*WI - Improved water (Z)	0.05	136.18	0.11	-0.03	-0.01	89.44	0.03	-0.02
	(0.05)	(143.64)	(0.05)*	(0.03)	(0.03)	(103.95)	(0.04)	(0.03)
T*WI- household assets (Z)	-0.02	150.99	0.00	0.08	-0.02	-240.30	0.02	-0.09
	(0.05)	(162.78)	(0.04)	(0.04)+	(0.04)	(116.17)*	(0.03)	(0.04)*
T* WI- house amenities (Z)	-0.06	132.65	-0.03	0.02	-0.04	-30.00	0.00	-0.01
	(0.04)	(122.45)	(0.04)	(0.04)	(0.03)	(111.79)	(0.03)	(0.03)
T*Household head education (Z)	-0.08	-86.46	-0.06	0.01	0.05	164.72	-0.05	0.03
	(0.05)	(186.31)	(0.05)	(0.05)	(0.04)	(126.51)	(0.04)	(0.03)
T* Head works in agriculture (Z)	-0.03	-274.13	-0.01	0.06	0.01	-169.74	-0.00	-0.02
	(0.10)	(270.53)	(0.09)	(0.08)	(0.09)	(228.69)	(0.08)	(0.07)
Observations	459	416	459	459	828	754	828	828
R-squared	0.07	0.06	0.07	0.10	0.05	0.07	0.05	0.05

Notes: Standard errors are clustered at the treatment village level. Reported estimates are the coefficients on the listed variable interacted with treatment. All regressions include the main effects of the variables, as well as the treatment status indicator variable.
** p<0.01, * p<0.05, + p<0.1

TABLE A11—ITT EFFECTS ON MARRIAGE AND FERTILITY, WOMEN

	Ever Married	Age At First Marriage	Number of Children	Age At First Child
	(1)	(2)	(3)	(4)
Panel A: Single Differences				
Treat*Age 24-29	0.00	-0.42	-0.06	-0.02
	(0.02)	(0.27)	(0.06)	(0.24)
Treat*Age 30-34	0.00	0.18	0.16	-0.25
	(0.01)	(0.39)	(0.12)	(0.38)
Treat*Age 35-65	0.00	0.42	-0.57	0.06
	(0.00)	(0.31)	(0.09)**	(0.16)
Panel B: Percent Changes				
Treat*(Age 24–29)	0.00	-0.02	-0.04	0.00
Treat*(Age 30–34)	0.00	0.01	0.07	-0.01
Treat*(Age 35–65)	0.00	0.02	-0.12	0.00
Age 24–29 Comp. Means	0.932	20.06	1.519	21.54
Age 30–34 Comp. Means	0.996	19.9	2.304	22.06
Age 35-65 Comp. Means	1	17.02	4.895	19.94
R-Squared	0.38	0.15	0.57	0.19
Observations	3,388	3,088	3,316	3,127

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 variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

Appendix B
Data and Construction of Selected Variables

This appendix describes the data sources, attrition of the panel from MHSS2, 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, facilities survey, 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. The MHSS2 phone survey instrument was shorter than the in-person survey instrument, as a result, there are smaller sample sizes for some variables, 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 an eight percent random subsample of baris[16] from the Matlab area living in both the treatment and comparison areas and was designed to be representative of the study area's 1993 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 interview in MHSS1 primary households creating panel data for these individuals.[17] 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).[18] To the extent that a whole household migrated out of Matlab between the start of the program and 1996, leaving the household unavailable for selection into the MHSS1 sample, the MHSS2 sample could still suffer from migration selection. It is rare that whole households migrated out of Matlab prior to 1996 and is estimated to be minimal at less than 1 percent of the target sample. 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 and weeks worked) were collected by type of employment for the previous 12 months, and 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.

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[16] Baris are household compounds where a number of related households live together and often eat together.
[17] MHSS2 sample also included all panel member descendants, and their co-resident spouses. However, among those members who had migrated out of Matlab, spouses were tracked for interview.
[18] The pre-1996 migrants were identified by using the detailed DSS data.

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 an out-migrant if their current residence, given by their village code in the survey, is outside the Chadpur district. An out-migration episode is defined as being urban if the location is in Dhaka and surrounding districts (Munshiganj, Narayanganj, Narsingdi, and Gazipur/Joydevpur), or the Chittagong district.

Census Data.— Periodic censuses were collected for all individuals in the study area (treatment and comparison areas) by iccd,r. 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 DSS area before 1977 to construct an individual's intent-to-treat status (Section III.C).

DSS Data.—Vital registration data provide prospective tracking of every birth, death, marriage, divorce, and in- and out- migration occurring in the study area. Information on migration destination (rural, urban, international) is also available starting in 1982. Data were collected by icddr,b monthly for many years and then quarterly, contain pre-program data from 1974 onwards, and are believed to be high quality data. These data are used to construct birth dates and an individual's intent-to-treat status, because we can observe when and where an individual in our sample first entered the DSS area. In addition, we use these data to construct pre-program migration network variables for each individual in the analysis sample, as well as additional 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 24–29 and 30–34 year old 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 for both age cohorts together is 8.8 percent for men and 7.8 percent for women for migration information, and 9.5 percent for men and 8.1 percent for women for income information (Table A1). Rates are similar for each age cohort separately and not statistically different between the treatment and comparison areas for any age cohort or sex. These are low attrition rate compared to other long-term effects surveys with shorter follow-up periods, [19] despite a migration rate of approximately 60 percent for men in this highly mobile population.

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

[19] For example, Gertler et al. (2014) report an attrition rate of 17 percent after 20 years, Araujo et al. (2016) 19 percent after 10 years, Barham, Macours, and Maluccio (2016) 10 percent after 10 years, Baird et al. (2016) 16.1 percent after approximately 10 years.

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, all 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 B1 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 24-29 and 30-34 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. However, there is differential attrition by religion for women. This variable is also not balanced at baseline (Section IV.A), so it will be important to examine the effects of the program only on Muslims, who represent more than 90 percent of the study population, as a robustness check.

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. 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.[20] 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. We fill in baseline characteristics for the few individuals that

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

could not be linked to the 1974 census by assigning means based on treatment status.[21] Finally, the village from the 1974 census link is used to cluster standard errors in our analysis, and the bari of the person traced to the 1974 census is used to construct a network.

D. Construction of Selected Outcome and Control Variables

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$. 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 outcomes, so to trim the data, we set to missing the earnings values that are above the 95th percentile, separately by birth cohort and gender. When earning is not trimmed the program effect is a 45 percent increase in income. This program effect is driven by a few very large outliers.

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.

Current Migrant.— A respondent is defined as an current migrant if their place of residence in MHSS2 is outside the Chadpur district (Matlab). Specifically, we use the village ID of the individual's household to determine if current village of residence is inside Matlab, in a district other than Matlab, or outside of Bangladesh. An out-migration episode is defined as being urban if the location is in Dhaka and surrounding districts,[22] or the Chittagong district.

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. 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

[21] Only 11 male and 14 female respondents have missing baseline data.
[22] These include Dhaka, Munshiganj, Narayanganj, Narsingdi, and Gazipur/Joydevpur.

values by item, defining the outlier cutoff as the smallest value that falls more than two standard deviations above the nearest value.

It includes food and nonfood items measured over different recall periods, as is typical in the World Bank Living Standard and Measurement surveys. We construct annual aggregate consumption measures at both household and per capita levels for two main reasons. First, treated households are on average larger than non-treated because the treated are less likely to migrate. Second, the composition of households over a year can change, making per capita measures potentially noisy, especially in high-migration areas. All consumption dependent variables in Table 4 are in logs, and comparison means report the mean value in 2012 US dollars.

Pre-Program Migrant Bari Network.— This variable measures the number of people who ever migrated out of the study area from the bari prior to the program. We use the same trace back method used to determine intent-to-treat to determine the preprogram bari. So, we place individuals in the same 1974 bari network if their household heads who linked to the 1974 census lived in the same bari. DSS migration data is used to determine the number of migrants from a bari prior to the program.

Arsenic Exposure.— The control for arsenic 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 measures 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. 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 measures 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 age fixed-effects control for the length of time exposed to the well water.

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TABLE B1—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)	0.019	-0.004				
	(0.015)	(0.022)				
Birth year	0.002	-0.000	-0.000	0.003	0.005	-0.004
	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.005)
Muslim (=1)	-0.039	0.078	0.025	0.002	-0.095	0.137
	(0.038)	(0.046)+	(0.057)	(0.069)	(0.051)+	(0.061)*
Land size 1982 (decimals)	0.001	-0.001	0.001	-0.002	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Bari size	0.001	0.002	0.000	0.002	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Family size	0.002	0.004	0.001	0.008	0.002	-0.001
	(0.004)	(0.005)	(0.005)	(0.008)	(0.005)	(0.007)
Owns a lamp (=1)	-0.023	0.030	-0.020	0.046	-0.021	0.007
	(0.017)	(0.027)	(0.025)	(0.039)	(0.025)	(0.037)
Owns a watch (=1)	0.009	-0.021	0.010	-0.020	0.013	-0.025
	(0.024)	(0.036)	(0.035)	(0.052)	(0.033)	(0.051)
Owns a radio (=1)	-0.048	0.044	-0.034	0.025	-0.066	0.066
	(0.031)	(0.046)	(0.045)	(0.066)	(0.044)	(0.065)
Wall tin or tinmix (=1)	0.005	0.018	-0.006	0.021	0.019	0.022
	(0.019)	(0.029)	(0.027)	(0.041)	(0.027)	(0.041)
Tin roof (=1)	-0.017	0.000	0.008	-0.032	-0.049	0.038
	(0.021)	(0.032)	(0.031)	(0.048)	(0.030)	(0.043)
Number of rooms per capita	-0.043	0.035	-0.099	0.197	0.024	-0.158
	(0.081)	(0.125)	(0.118)	(0.176)	(0.114)	(0.180)
Number of cows	-0.001	-0.001	-0.001	0.001	-0.001	-0.001
	(0.005)	(0.008)	(0.008)	(0.012)	(0.007)	(0.011)
Number of boats	-0.024	0.011	-0.039	0.026	-0.009	-0.010
	(0.015)	(0.022)	(0.021)+	(0.032)	(0.021)	(0.031)
Latrine (=1)	-0.031	0.002	-0.037	-0.019	-0.022	0.014
	(0.023)	(0.032)	(0.035)	(0.048)	(0.031)	(0.043)
Drinking water, tubewell (=1)	0.009	-0.011	0.016	-0.050	-0.007	0.046
	(0.022)	(0.031)	(0.031)	(0.045)	(0.031)	(0.044)
Drinking water, tank (=1)	-0.002	-0.005	-0.009	-0.034	0.002	0.032
	(0.017)	(0.027)	(0.025)	(0.039)	(0.023)	(0.037)

HH age	-0.002	0.002	-0.002	0.001	-0.001	0.002
	(0.001)+	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
HH years of education	0.001	-0.001	0.000	0.002	0.001	-0.004
	(0.003)	(0.005)	(0.005)	(0.007)	(0.004)	(0.007)
HH works in agriculture (=1)	0.027	-0.016	0.031	-0.014	0.021	-0.016
	(0.018)	(0.028)	(0.026)	(0.042)	(0.025)	(0.038)
HH works in business or service (=1)	0.011	-0.002	0.038	-0.036	-0.021	0.036
	(0.030)	(0.044)	(0.042)	(0.062)	(0.045)	(0.063)
HH works in fishing (=1)	0.085	-0.053	0.142	-0.254	0.027	0.138
	(0.033)*	(0.054)	(0.047)**	(0.080)**	(0.047)	(0.074)+
HH spouse's age	0.002	-0.003	0.001	-0.000	0.003	-0.005
	(0.001)+	(0.002)	(0.002)	(0.003)	(0.002)+	(0.002)*
HH spouse's years of education	-0.005	0.004	-0.002	-0.002	-0.010	0.014
	(0.006)	(0.009)	(0.009)	(0.013)	(0.009)	(0.013)
F-statistic that all interactions = 0		0.577		0.984		1.069
N		2,746		1,425		1,321

Notes: 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). Regression includes both the 24-29 and 30-34 year old cohorts in the analysis. Standard errors clustered at the village level.

Appendix C

Potential Confounders

To supplement our main analysis, we include additional controls to rule out the presence of potential confounders. In conducting a long-run follow-up, other factors at the village level could be affecting individuals' health, human capital attainment, and labor market opportunities. We control for various village level differences that could potentially be correlated with the placement of the MCH-FP intervention: 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 Meghan 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 (referred to as "erosion villages") 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 midproject to a more stable position farther from the river, so there are a number of villages in the Meghna area (referred to as "Meghna villages") 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 ("erosion village") or was not submerged but was between the Meghna River and the embankment.

Additionally, we include a set of characteristics to control for differences in access to schooling and healthcare. School facility data come from the MHSS2 School Survey. Every school study site was surveyed, and we observe the school's type (e.g., primary, secondary, maktab, madrasha) and establishment date. 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, which surveyed village leaders about health facilities used by people from their village. We construct indicators for the presence of a Family Welfare Center (a government clinic, FWC), a Family Welfare Assistant (a government health worker that travels to villages, FWA), a non-MBSS allopathic doctor, and a Trained Traditional Birth Attendant (a midwife).[23]

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, but the presence of the program could still bias our results. We include indicators for whether the treatment village participated in

[23] MBSS is the degree earned in a medical college for medicine and surgery.

an experimental period of BRAC from 1991 to 1999 or whether BRAC was present at the individual's age 11 (secondary school age).

The controls for the Meghna Project, schools, health facilities in the 1990s, and BRAC comprise our set of "intermediate controls" that control for potential confounders between baseline and endline.

We further control for any remaining differences in access to healthcare during the 2010s and differences in arsenic exposure. We observe village access to health facilities and health practitioners in the MHSS2 Community and Facility Survey. In 2013, prominent women in each village were surveyed and asked about locations of different types of health facilities used by people in their village. We construct indicators for the presence of that same types of health facilities and workers listed in the previous paragraph. We also control for the distance from the nearest FWC and community clinic.

Tables D1 and D2 repeat our earlier main analysis but include these "intermediate controls" (Panel D) and "endline controls" (Panel E). As with our baseline characteristics, these controls are fully interacted with our age group dummies. Our main results on migration, type of work, and income remain unchanged with the inclusion of these controls.

Appendix D
Robustness Analysis

We perform a number of robustness checks to examine the validity of the results for the men and women. We report these checks in Tables D1 (men) and D2 (women) for key outcome variables. For the men, we focus on variables from Tables 1 and 2 including migration, type of work, migration, required skills, earnings, and hours worked. For the women, we consider the main outcomes from Tables 7, 8, and 9. In addition, in Appendix E, we test the robustness to a number of different weights including inverse propensity weights designed to account for attrition. In sum, the findings reported in the paper are similar across a variety of robustness checks and types of weights.

Muslim Only Results.— The baseline balance table revealed imbalance in the treatment and cmparison 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 D1 and D2, Panel A and show the main results remain the same.

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). This program was introduced in a crossover design with the MCH-FP program between 1991 and 1995 and then became available in other villages. There was also a large 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 a household level controls for arsenic exposure in 2003, and village-level controls from the 1990s for each of the other changes (referred to as the "intermediate controls") and and from MHSS2 for each of the village controls (referred to as the "endline controls"). The intermediate controls adjust for access to other programs when the sample were children, and the endline controls, when they are adults. See Appendix C for more details on the controls. Results are reported in Panels D and E of Tables D1 and D2. Again, the results are qualitatively the same.

Multiple Hypothesis Testing.— We adjusted the standard errors for multiple hypothesis testing using the false discover rate following Anderson (2012). In Table D1 and D2 panel F we use all variables in Tables 1 and 2 and in Panel G all variables in the tables in the paper except Table A5 (which splits results by migration status). All results remain significant at the 10 percent level or less when standard errors are adjusted.

Mortality and Migration Attrition.— Two prominent causes of attrition in this context are mortality and migration. Even if the MCH-FP program were truly randomized, the program itself is likely to cause mortality and migration to differ between treatment and comparison areas over time, potentially biasing the results. Mortality selection is likely to bias the results downwards, since frailer individuals (or those less healthy) are more likely to survive in the treatment area. To test the program balance if those in the sample who died prior to being surveyed are included, we use the DSS to identify any children from MHSS1 households in our sample who died between program start and MHSS1. We first examine if the baseline variables are statistically significant between those who lived and those who died prior to MHSS1. We find no substantial differences with the mean divided by the standard deviation being less the 0.1 for all baseline variables. In addition, we examine if baseline characteristics differ between those who died in the treatment and comparison area. The differences in baseline variables mimics the main results, with the exception of radio and lamp, where the treatment area is less likely to own a lamp but more likely to own a radio. However, only source of drinking water has a mean divided by the standard deviation is greater than 0.25.

The most important way to reduce migration attrition is to have a low attrition rate. With a less than 10 percent attrition rate for our sample, migration attrition is not likely to be a large problem. To explore the effect of attrition from the sample, we follow the typical strategy of using baseline variables to predict attrition and compute inverse-probability-of-attrition weights for each observation. See Appendix Section E for details on the construction of the weights. Results are presented in Table E1 panel A and remain unchanged.

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 the 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 the treatment and comparison areas for the sample. We find no evidence of spatial correlation in the error terms (results not reported).

Spillover Effects.— The ITT effects may be biased by the program's indirect effects on nonparticipants: informational spillovers or positive externalities from interventions such as vaccinations. In both of these cases, spillovers are more likely to occur in areas that border or are relatively close to the treated villages, since knowledge about the programs is likely to be spread by word-of-mouth, and the positive externalities of vaccination are largely local. We explore this possibility using the following linear regression:

$$O_{iv} = \beta_1 AG^{24-29} + \beta_2 AG^{30-34} + \beta_3(C_v * AG^{24-29}) + \beta_4(C_v * AG^{30-34}) \\ +\beta_5(B_v * AG^{24-29}) + \beta_6(B_v * AG^{30-34}) + \alpha_{by} + X'Z + \varepsilon_{iv},$$

where C_v is an indicator for the comparison group ($1\text{-}T_v$) and B_v equals 1 if the individual's 1974 treatment village is in the comparison area and borders a treatment village and 0 otherwise. All other variables are defined as earlier. This equation examines spillover effects by testing whether the estimated ITT effects differ between two areas in the comparison area: villages close to the treatment area and villages farther away. β_3 and β_4 estimate the difference in mean outcomes by age group between nonbordering comparison villages and the treatment area. In the absence of spillover effects, these estimates will be similar in magnitude to earlier ITT effects, but with the opposite sign. β_5 and β_6 are the double-difference estimators for each age group that estimate any additional difference in effect between comparison villages that border the treatment area and ones that do not.

In this analysis, we consider spillover effects on migration and type of work. Point estimates in Table D3, column 1 show that the point estimates of the treatment border interactions are small and statistically insignificant for all age groups, indicating that there are no spillovers. In column 2, we repeat the same analysis but use the distance from the village centroid to the treatment border to define our border variable. Specifically, the border variable equals 1 if the village falls within the closest quartile of distances within the comparison area and 0 otherwise. The estimated spillover effect remains close to zero for the youngest age group and increases in magnitude somewhat for the middle age group, but remains insignificant. Columns 3 and 4 present similar results for whether an individual has a semiprofessional or professional job. Regardless of how we define the border, the difference between the border and other areas is small and negative, and statistically insignificant.

TABLE D1—ITT EFFECTS, MEN, ROBUSTNESS CHECKS

	Type of Work (=1)				Start Own Business (=1)	Required Skill	Earnings (USD) Trim 5%	Hours Worked Trim 5%	Migration out of Matlab		
	Any Paid	Prof. & Semi-Prof.	Ag	Manual		Reading, Writing, Math (=1)			All	Intl.	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Only Muslims											
Treat*(Age 24-29)	-0.02	0.08	-0.03	-0.07	0.10	0.08	-23.95	3.33	-0.13	-0.01	-0.12
	(0.02)	(0.03)*	(0.02)	(0.04)+	(0.04)*	(0.04)*	(109.69)	(97.71)	(0.04)**	(0.03)	(0.04)**
Treat*(Age 30-34)	-0.03	-0.01	0.05	-0.04	0.03	-0.07	-517.17	-38.96	-0.09	-0.09	-0.03
	(0.02)	(0.05)	(0.03)+	(0.06)	(0.05)	(0.05)	(167.68)**	(114.49)	(0.05)+	(0.04)*	(0.05)
Observations	1,197	1,197	1,197	1,197	1,197	1,197	1,081	1,109	1,197	1,197	1,197
Panel B: Exclude Matlab Town											
Treat*(Age 24-29)	-0.02	0.08	-0.02	-0.08	0.09	0.10	-42.22	-108.97	-0.14	-0.00	-0.12
	(0.03)	(0.04)+	(0.02)	(0.04)+	(0.04)*	(0.04)*	(123.56)	(105.00)	(0.04)**	(0.04)	(0.05)*
Treat*(Age 30-34)	-0.05	0.00	0.06	-0.04	0.02	-0.02	-318.06	-7.87	-0.11	-0.08	-0.04
	(0.03)	(0.06)	(0.03)*	(0.06)	(0.05)	(0.05)	(167.78)+	(108.99)	(0.05)*	(0.04)+	(0.05)
Observations	1,051	1,051	1,051	1,051	1,051	1,051	946	974	1,051	1,051	1,051
Panel C: <3km of border											
Treat*(Age 24-29)	-0.02	0.11	-0.02	-0.09	0.11	0.08	-147.14	-0.08	-0.10	-0.04	-0.07
	(0.03)	(0.04)*	(0.03)	(0.04)*	(0.04)*	(0.04)+	(137.18)	(112.07)	(0.04)*	(0.04)	(0.05)
Treat*(Age 30-34)	-0.05	-0.02	0.07	-0.07	0.03	-0.06	-559.00	-20.50	-0.10	-0.11	-0.02
	(0.03)+	(0.06)	(0.03)*	(0.07)	(0.05)	(0.06)	(192.00)**	(138.24)	(0.06)+	(0.05)*	(0.05)
Observations	885	885	885	885	885	885	801	821	885	885	885
Panel D: Intermediate Controls											
Treat*(Age 24-29)	-0.01	0.10	-0.01	-0.10	0.10	0.06	-103.05	17.28	-0.16	-0.04	-0.11
	(0.03)	(0.04)**	(0.02)	(0.04)*	(0.04)*	(0.04)+	(117.59)	(99.75)	(0.04)**	(0.03)	(0.05)*
Treat*(Age 30-34)	-0.03	-0.01	0.07	-0.07	0.01	-0.05	-427.92	-42.44	-0.11	-0.08	-0.05
	(0.02)	(0.05)	(0.02)**	(0.06)	(0.05)	(0.05)	(157.65)**	(120.37)	(0.05)*	(0.04)+	(0.05)
Observations	1,302	1,302	1,302	1,302	1,302	1,302	1,182	1,206	1,302	1,302	1,302

Panel E: Endline Controls											
Treat*(Age 24-29)	-0.02	0.10	-0.01	-0.12	0.10	0.07	-137.20	-14.99	-0.15	-0.04	-0.11
	(0.03)	(0.04)*	(0.02)	(0.04)**	(0.04)**	(0.04)+	(127.34)	(104.20)	(0.04)**	(0.04)	(0.05)*
Trea*(Age 30-34)	-0.04	-0.02	0.07	-0.06	-0.01	-0.05	-417.62	-95.91	-0.11	-0.10	-0.03
	(0.02)+	(0.05)	(0.03)**	(0.06)	(0.04)	(0.05)	(157.36)**	(121.89)	(0.05)*	(0.04)*	(0.05)
Observations	1,302	1,302	1,302	1,302	1,302	1,302	1,182	1,206	1,302	1,302	1,302
Panel F: Multiple Hypothesis Testing: Using All Variables in Tables 1 and 2											
Age 24-29 Naive P-value	0.725	0.018	0.231	0.051	0.007	0.020	0.736	0.985	0.610	0.725	0.010
Age 24-29 FDR P-value	0.583	0.052	0.264	0.097	0.049	0.052	0.583	0.621	0.583	0.583	0.049
Age 30-34 Naive P-value	0.131	0.830	0.021	0.277	0.585	0.181	0.003	0.502	0.015	0.656	0.174
Age 30-34 FDR P-value	0.397	0.955	0.092	0.563	0.688	0.417	0.052	0.688	0.088	0.695	0.417
Panel G: Multiple Hypothesis Testing: Using All Variables in Main Tables Except Table A5											
Age 24-29 Naive P-value	0.725	0.018	0.231	0.051	0.007	0.020	0.736	0.985	0.001	0.610	0.010
Age 24-29 FDR P-value	0.756	0.081	0.373	0.159	0.070	0.081	0.756	0.814	0.019	0.756	0.070
Age 30-34 Naive P-value	0.131	0.830	0.021	0.277	0.585	0.181	0.003	0.502	0.052	0.015	0.656
Age 30-34 FDR P-value	0.306	0.876	0.094	0.442	0.626	0.395	0.028	0.626	0.184	0.079	0.650

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 variables in Table A3. Panel A restricts the sample to Muslims. Panel B excludes individuals from Matlab Town. Panel C restricts the sample to treatment vilages within 3km of the treatment border. Panel D and Panel E add additional sets of controls, interacted by birth cohort. Panel F and G adjust the p-values for multiple hypothesis testing using the false discover rate (FDR) following Anderson (2012). See Appendix C for details.
** p<0.01, * p<0.05, + p<0.1

TABLE D2—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS

	Type of Work (=1)					Migration Out of Matlab (=1)			Any Cash	Ever Had
	Any Paid	Prof. or Semi. Prof.	Ag	Manual	Unpaid Household	All	Int'l.	Urban	Savings (=1)	Mircrocredit Loan (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Only Muslims										
Treat*(Age 24-29)	0.05	0.01	0.05	-0.02	-0.03	-0.02	0.01	-0.01	0.08	0.06
	(0.03)	(0.01)	(0.02)*	(0.03)	(0.03)	(0.04)	(0.01)	(0.04)	(0.03)*	(0.03)+
Treat*(Age 30-34)	0.08	-0.02	0.03	0.04	-0.05	-0.13	0.00	-0.13	0.01	0.05
	(0.06)	(0.02)	(0.04)	(0.04)	(0.06)	(0.06)*	(0.01)	(0.05)**	(0.05)	(0.06)
Observations	1,106	1,106	1,106	1,106	1,106	1,106	1,106	1,106	1,095	1,100
Panel B: Exclude Matlab Town										
Treat*(Age 24-29)	0.04	-0.00	0.06	-0.02	-0.05	-0.04	0.00	-0.04	0.06	0.07
	(0.03)	(0.02)	(0.03)*	(0.03)	(0.04)	(0.05)	(0.01)	(0.04)	(0.03)+	(0.03)+
Treat*(Age 30-34)	0.03	-0.01	0.02	0.00	-0.02	-0.12	0.00	-0.14	-0.01	0.06
	(0.06)	(0.02)	(0.05)	(0.04)	(0.06)	(0.06)+	(0.01)	(0.05)**	(0.05)	(0.06)
Observations	1,001	1,001	1,001	1,001	1,001	1,001	1,001	1,001	990	995
Panel C: <3km of border										
Treat*(Age 24-29)	0.07	0.01	0.06	-0.01	-0.05	-0.03	0.01	-0.01	0.08	0.05
	(0.04)+	(0.02)	(0.03)*	(0.03)	(0.04)	(0.05)	(0.00)+	(0.04)	(0.04)*	(0.04)
Treat*(Age 30-34)	0.07	0.00	0.02	0.01	-0.05	-0.14	0.00	-0.12	0.01	0.04
	(0.05)	(0.03)	(0.05)	(0.05)	(0.06)	(0.07)*	(0.01)	(0.06)*	(0.06)	(0.07)
Observations	833	833	833	833	833	833	833	833	827	829
Panel D: Intermediate Controls										
Treat*(Age 24-29)	0.04	0.01	0.05	-0.03	-0.03	-0.04	0.01	-0.02	0.07	0.06
	(0.03)	(0.02)	(0.02)*	(0.03)	(0.04)	(0.04)	(0.00)+	(0.04)	(0.03)*	(0.04)+
Treat*(Age 30-34)	0.03	-0.01	0.04	0.01	-0.04	-0.14	0.00	-0.14	0.01	0.07
	(0.05)	(0.02)	(0.04)	(0.04)	(0.05)	(0.05)**	(0.01)	(0.05)**	(0.06)	(0.06)
Observations	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,220	1,209	1,214

Panel E: Endline Controls										
Treatment*(Age 24-29)	0.04	0.00	0.05	-0.03	-0.03	-0.01	0.01	-0.01	0.08	0.06
	(0.03)	(0.01)	(0.02)*	(0.03)	(0.04)	(0.04)	(0.00)*	(0.04)	(0.03)*	(0.04)+
Treatment*(Age 30-34)	0.00	-0.00	0.04	-0.02	-0.02	-0.15	0.00	-0.14	-0.01	0.06
	(0.06)	(0.03)	(0.04)	(0.04)	(0.06)	(0.06)*	(0.00)	(0.06)**	(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 F: Multiple Hypothesis Testing: Using All Variables in Tables 7, 8, 9										
24-29 Naive P-value	0.08	0.69	0.01	0.57	0.16	0.45	0.38	0.64	0.01	0.07
24-29 FDR P-value	0.33	0.92	0.07	0.92	0.48	0.92	0.92	0.92	0.09	0.33

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 variables in Table A3.See Appendix C for details on controls. Panel F adjust the p-values for multiple hypothesis testing using the false discover rate (FDR) following Anderson (2012).

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

TABLE D3—SPILLOVER EFFECTS ON MIGRATION AND TYPE OF WORK, MEN

	Current Migration Out of Matlab		Professional, or Semi-Professional	
	(1)	(2)	(3)	(4)
Comparison area*(Age 24–29)	0.13	0.14	-0.08	-0.09
	(0.04)**	(0.04)**	(0.04)*	(0.04)*
Comparison area*(Age 30–34)	0.10	0.11	0.02	0.02
	(0.05)+	(0.05)*	(0.05)	(0.05)
Comparison area*(Age 24–29)*Border treatment village	0.00		-0.04	
	(0.05)		(0.05)	
Comparison area*(Age 30–34)*Border treatment village	-0.04		0.00	
	(0.07)		(0.07)	
Comparison area*(Age 24–29)*Border treatment village-closest quartile		-0.02		0.01
		(0.06)		(0.06)
Comparison area*(Age 30–34)*Border treatment village-closest quartile		-0.09		-0.00
		(0.08)		(0.07)
R-Squared	0.06	0.06	0.06	0.06
Observations	1,302	1,302	1,302	1,302

Notes: Standard errors are clustered at the treatment village level. All regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are run on the sample of all men. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all variables in Table A3.
** p<0.01, * p<0.05, + p<0.1

Appendix E
Weights

We check the robustness of our results to a number of weighting schemes designed to correct for potential differences in attrition, to make our sample representative of the 1974 Matlab population, and to correct for sampling bias in MHSS1. We consider our main outcomes of interest – migration, type of work, required skills, income and hours worked – and results are presented in Table E1 for men and Table E2 for women.

Panels A address potential attrition bias by weighting our specification by the inverse-probability weight estimator. We estimate the probability of attrition using probit model where the outcome is an indicator of being surveyed in MHSS2, and include the controls from our main specification as predictors. The weight for Panel A is the inverse of the predicted probability of being surveyed. Results are robust to these weights.

We would like our analysis to be representative of the 1974 baseline population. Since the sample was selected in 1996, it is possible that it is not representative of 1974 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 the 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, as opposed to following back through anyone they have ever lived with. The 1974 evaluation weight is then the ratio of the 1974 sampling probability[24] to the product of the 1996 sampling probability and the total number of 1974 household descendants in 1996 in the individual's cohort. Panels B shows that the results are similar when using this weighting scheme, with the exception of a decrease in the size of the point estimates and a loss of statistical significance on women working more in paid work and in agriculture.

Finally, Panel C addresses potential sampling bias in the survey design from MHSS1. MHSS1 had a two-step sampling procedure of households that first randomly selected a set of baris and then chose one household within a bari to be sampled. Thus, households from smaller baris had a higher probability of being sampled then households from larger baris. Additionally, individuals were randomly selected within households. MHSS1 therefore has weights based on the inverse probability of an individual being sampled in 1996. Results from Panel A show that our main results are unchanged for a representative population from 1996, with the exception of 30-34 year old women where the treatment effect on every having a micro-credit loan is statistically significant.

References

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

[24] To construct the 1974 probability, we resample the set of 1996 households 1,000 times following the MHSS1 sampling procedure and count the number of times a 1996 household that was picked has an individual that is linked to the 1974 household.

TABLE E1— ITT EFFECTS, MEN, WEIGHTS

	Type of Work				Start Own Business	Required Skills	Earnings	Hours	Migration out of Matlab		
	Any Paid	Prof. & Semi-Prof	Agriculture	Manual	Business (=1)	Reading, Writing, Math	(USD) Trim 5%	Worked Trim 5%	All	Intl.	Urban
	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(1)	(2)	(3)
Panel A: Inverse Probability Weight for Attrition											
Treat*(Age 24-29)	-0.01	0.09	-0.02	-0.07	0.10	0.09	-42.22	-10.12	-0.13	-0.02	-0.10
	(0.02)	(0.03)*	(0.02)	(0.04)*	(0.04)**	(0.04)*	(108.97)	(90.82)	(0.04)**	(0.03)	(0.04)*
Treat*(Age 30-34)	-0.04	-0.01	0.06	-0.04	0.02	-0.07	-469.10	-70.29	-0.10	-0.10	-0.02
	(0.02)	(0.05)	(0.02)*	(0.05)	(0.04)	(0.05)	(155.69)**	(104.59)	(0.05)*	(0.04)*	(0.04)
Observations	1,302	1,302	1,302	1,302	1,302	1,302	1,182	1,206	1,302	1,302	1,302
Panel B: 1974 Evaluation Weight											
Treat*(Age 24-29)	-0.00	0.10	-0.02	-0.09	0.09	0.14	17.08	-17.28	-0.14	-0.02	-0.12
	(0.03)	(0.04)*	(0.02)	(0.04)*	(0.04)*	(0.05)**	(152.91)	(108.73)	(0.04)**	(0.04)	(0.04)**
Treat*(Age 30-34)	-0.01	-0.02	0.09	-0.04	-0.01	-0.08	-455.64	-42.61	-0.08	-0.13	0.02
	(0.04)	(0.06)	(0.03)**	(0.06)	(0.05)	(0.05)+	(164.13)**	(130.00)	(0.06)	(0.04)**	(0.05)
Observations	1,291	1,291	1,291	1,291	1,291	1,291	1,174	1,196	1,291	1,291	1,291
Panel C: MHSS1 Individual Weight											
Treat*(Age 24-29)	0.00	0.09	-0.02	-0.05	0.07	0.12	-76.48	27.54	-0.11	-0.02	-0.08
	(0.03)	(0.04)*	(0.02)	(0.04)	(0.05)	(0.04)**	(136.20)	(99.94)	(0.04)**	(0.04)	(0.04)+
Treat*(Age 30-34)	-0.04	0.10	0.06	-0.18	0.06	-0.06	-511.01	-151.52	-0.17	-0.16	-0.01
	(0.03)	(0.05)+	(0.03)*	(0.06)**	(0.05)	(0.05)	(188.20)**	(129.26)	(0.06)**	(0.05)**	(0.05)
Observations	1,269	1,269	1,269	1,269	1,269	1,269	1,150	1,175	1,269	1,269	1,269

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 variables in Table A3.See Appendix C for details on controls. Panel F adjust the p-values for multiple hypothesis testing using the false discover rate (FDR) following Anderson (2012).
See Appendix D for details on weight construction.
** p<0.01, * p<0.05, + p<0.01

TABLE E2—ITT EFFECTS, WOMEN, WEIGHTS

	Type of Work (=1)					Migration Out of Matlab (=1)			Any Cash Savings (=1)	Ever Had Microcredit Loan (=1)
	Any Paid	Prof. & Semi-Prof	Ag	Manual	Unpaid Household	All	Intl.	Urban		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Inverse Probability Weight for Attrition										
Treat*(Age 24-29)	0.05	0.01	0.06	-0.02	-0.05	-0.03	0.00	-0.02	0.07	0.06
	(0.03)+	(0.01)	(0.02)**	(0.03)	(0.03)	(0.04)	(0.01)	(0.04)	(0.03)*	(0.03)+
Treat*(Age 30-34)	0.09	0.01	0.04	0.02	-0.06	-0.13	0.00	-0.15	0.01	0.04
	(0.06)	(0.02)	(0.04)	(0.04)	(0.06)	(0.06)*	(0.01)	(0.05)**	(0.05)	(0.05)
Observations	1,131	1,131	1,131	1,131	1,131	1,131	1,131	1,131	1,120	1,125
Panel B: 1974 Evaluation Weight										
Treat*(Age 24-29)	0.02	0.00	0.01	-0.03	0.02	-0.05	0.00	-0.03	0.06	0.09
	(0.04)	(0.01)	(0.02)	(0.03)	(0.03)	(0.04)	(0.00)	(0.04)	(0.03)+	(0.04)*
Treat*(Age 30-34)	0.06	0.01	0.04	0.01	-0.06	-0.15	-0.00	-0.15	0.04	0.12
	(0.06)	(0.02)	(0.05)	(0.05)	(0.06)	(0.06)*	(0.01)	(0.06)**	(0.06)	(0.06)*
Observations	1,207	1,207	1,207	1,207	1,207	1,207	1,207	1,207	1,196	1,201
Panel C: MHSS1 Individual Weight										
Treat*(Age 24-29)	0.07	0.01	0.06	-0.01	-0.08	-0.07	0.00	-0.04	0.06	0.07
	(0.04)+	(0.02)	(0.03)*	(0.04)	(0.04)*	(0.05)	(0.00)	(0.05)	(0.03)+	(0.04)+
Treat*(Age 30-34)	0.06	0.00	0.04	-0.00	-0.04	-0.19	0.00	-0.17	0.02	0.16
	(0.06)	(0.03)	(0.06)	(0.05)	(0.07)	(0.07)**	(0.00)	(0.06)**	(0.07)	(0.06)**
Observations	1,161	1,161	1,161	1,161	1,161	1,161	1,161	1,161	1,151	1,156

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 variables in Table A3.See Appendix C for details on controls. Panel F adjust the p-values for multiple hypothesis testing using the false discover rate (FDR) following Anderson (2012).
See Appendix D for details on weights.
** p<0.01, * p<0.05, + p<0.1

Appendix F
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 across nine phases of survey work. 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. We determine the treatment status of the market by considering the share of treatment area households that are closest to the market.[25] 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 during survey phase 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 F1 presents results from this analysis. Column 1 presents estimates with item fixed effects, and Column 2 nonparametrically controls for time trends in prices by including phase fixed effects. In both cases the point estimates are small and statistically insignificant.

[25] 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 F1—WITHIN-ITEM PERCENT DIFFERENCES IN PRICES, TREATMENT VS. COMPARISON AREAS

	Log Price	Log Price
	(1)	(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	X	X
Phase FE	-	X
R-squared	0.789	0.794
Observations	4,783	4,783

Notes: The dependent variable is the log price of the consumption item. Column 1 includes Item fixed effects. Column (2) adds survey phase fixed effects. Estimates represent the within-item percent difference in prices between the stated market and the comparison area.
** p<0.01, * p<0.05, + p<0.1