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

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ONLINE APPENDIX

Appendix A

TABLE A1—ITT EFFECTS ON EARNINGS IN THE PAST 12 MONTHS USING DIFFERENT METHODS FOR OUTLIERS, MEN

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

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Column (1) uses a log transformation of income. Column (2) uses the inverse hyperbolic sine transformation. Column (3) reports the results from a median regression. Column (4) presents the effect on the percentile rank of income within each cohort income distribution. Control group means are reported for the untransformed income in 2012 USD.
** p<0.01, * p<0.05, + p<0.1

TABLE A2—ITT EFFECTS ON HOUSEHOLD CONSUMPTION AND ASSETS, MEN AND WOMEN

	Log Consumption per Capita		Assets (USD, Trim 5%)			Land (Decimals = 1/100th of acre)	
	Total	Food	Total	Household	Productive	Agricultural	Non-Agricultural
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Males							
Treat*(Age 24–30)	-0.04	-0.03	43.08	35.57	-5.2	6.59	2.5
	(0.05)	(0.05)	(90.02)	(72.65)	(33.41)	(4.01)	(2.05)
Treat*(Age 31–34)	-0.01	-0.04	-181.32	-65.19	-59.65	-3.59	-3.41
	(0.06)	(0.06)	(100.98)+	(93.83)	(33.08)+	(4.53)	(3.47)
Pr(24–30 = 31–34)	0.726	0.89	0.0998	0.418	0.192	0.0515	0.113
Panel B: Percent Changes							
Treat*(Age 24–30)	-	-	5%	5%	-5%	36%	25%
Treat*(Age 31–34)	-	-	-17%	-8%	-48%	-19%	-24%
Age 24–30 Means	1,059	638	882	715	105	18.2	9.8
Age 31–34 Means	991	595	1,077	835	123	18.7	14.4
Observations	1,094	1,094	1,042	1,042	1,042	1,084	1,079
Panel C: Females							
Treat*(Age 24–30)	0.05	0.03	1.97	-58.68	23.18	13.84	13.24
	(0.04)	(0.04)	(93.62)	(90.04)	(33.19)	(12.78)	(12.20)
Treat*(Age 31–34)	-0.07	-0.04	-29.82	-17.61	-29.73	7.00	7.79
	(0.06)	(0.05)	(168.41)	(156.89)	(31.64)	(6.44)	(7.26)
Pr(24–30 = 31–34)	0.0752	0.279	0.865	0.821	0.232	0.558	0.703
Panel D: Percent Changes							
Treat*(Age 24–30)	-	-	0%	-4%	23%	50%	93%
Treat*(Age 31–34)	-	-	-2%	-1%	-36%	35%	66%
Age 24–30 Means	690	425	1,578	1,376	101	27.7	14.3
Age 31–34 Means	678	403	1,418	1,262	82	20.2	11.8
Observations	1,219	1,218	1,163	1,163	1,163	1,211	1,207

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. For trim 5%, the highest 5 percent of observations based on total assets are set to missing.
** p<0.01, * p<0.05, + p<0.1

TABLE A3—ITT EFFECTS ON MAIN LABOR MARKET OUTCOMES BY MIGRATION STATUS, MEN

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

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

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

Appendix B
Data and Construction of Selected Variables

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

A. *Data Sources*

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

MHSS2 was conducted between 2012 and 2014 and was designed to be a panel to MHSS1 (Rahman et al. 1999). MHSS1 is a seven percent random subsample of household compounds (called baris) from the Matlab area living in both the treatment and comparison areas and was designed to be representative of the study area's 1996 population. In MHSS1, two households were interviewed in each bari: a primary household, selected randomly, and a secondary household, selected purposively. Within a household, individuals aged six and older were randomly selected to be personally interviewed, but basic information, including education, was collected on all household members via proxy.

The MHSS2 sample includes all individuals selected for personal interview in MHSS1 primary households creating panel data for these individuals.[1] To limit migration selection for key age groups, the MHSS2 sample also includes individuals born between 1972 and 1989 to a MHSS1 primary household that had migrated out of Matlab between 1977 and 1996 (referred to as pre-1996 migrants).[2] To the extent that a whole household and lineage migrated out of Matlab between the start of the program and 1996, leaving no one in that lineage available for selection into the MHSS1 sample, the MHSS2 sample could still suffer from migration selection. It is rare that whole households and lines of descent migrated out of Matlab prior to 1996 and is estimated that the annual whole household migration rate from the entire study area between 1977 and 1996 was small, 0.66 percent, and most exiting households were Hindu. In addition, we test the balance of the treatment groups to check that the treatment and comparison group have similar baseline characteristics on average.

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

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

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

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

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

Demographic Surveillance Site (DSS) Data.—Vital registration data provide prospective tracking of every birth, death, marriage, divorce, and in- and out- migration occurring in the study area (icddr,b 2014). As such, we know when someone enters and leaves the study area. Information on migration destination (rural, urban, international) is also available starting in 1982. Data were collected by icddr,b and are high quality in part because they were collected so frequently: every two weeks until 1997, every month between 1998 and 2006, and every two months between 2007 and MHSS2.These data include pre-program data from 1974 onwards, and are used to construct birth dates and an individual's intent-to-treat status. In addition, we use these data to construct pre-program migration network variables for each individual in the analysis sample, as well as, out-migration variables such as whether someone has ever migrated, and out-migration variables for years not covered in the MHSS2 migration history.

B. *Attrition*

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

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

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

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

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

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

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

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

D. Construction of Selected Outcome and Control Variables

Occupations. Detailed occupation codes are collected for primary and secondary jobs in MHSS2. Occupation codes were collected using a modified version of the Bangladesh Bureau of Statistics 2010 occupation codes[6], which built upon the International Labor Organization's International Standard Classification of Occupations 2008 (ISCO-08)[7]. BBS employed a 5-level hierarchical occupation coding including 1) Major group, 2) Sub-Major Group, 3) Minor Group, 4) Unit Group and 5) Sub-Unit Group. We coded based upon the Minor Group using three-digit codes that we organized under Major groups.

In order to facilitate accurate and verifiable coding and maintain interview flow, enumerators were trained to ask the respondent to describe the work that they performed. Enumerators were asked to summarize the job in four words or less and record those words as open text in the space provided (EMPB01). After finishing the interview, but before they left the household, interviewers were asked to use the descriptive words recorded in EMPB01 to look up the suitable occupation code and record the code (EMPB02). This method built on the Indonesian Family Life Survey and was pretested and piloted in the field. To facilitate rapid and comprehensive coding, enumerators were provided both with a list of the most commonly used codes (based on those most commonly reported in BBS data) and a comprehensive list of all codes arranged in the hierarchical groups. Occupation coding of each survey was reviewed by field supervisors. Common difficulties or inconsistencies were reviewed at regular training and debrief sessions.

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

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

Annual Income. Annual income is constructed from a survey module that captures paid and nonpaid work from a set of eight general employment activities that was designed to cover all possible types of work (e.g., salaried work, piece-rate work, self-employment, etc.). Questions were asked by employment category to reduce the chance that the respondent would forget to report income if they worked multiple jobs. Income for household-related activities (e.g., family business and family farm) is split evenly among workers within the household reporting such activities, though the results are not sensitive to how this income is assigned. Income is deflated to 2012 values using World Bank national accounts data and then converted from Bangladeshi

[6] https://web.archive.org/web/20121113231940/http://www.bbs.gov.bd/WebTestApplication/userfiles/Image/SY2010/Chapter-03.pdf

[7] http://www.ilo.org/public/english/bureau/stat/isco/isco08/index.htm

taka to US dollars using an exchange rate of 78Tk/US$.[8] Income for international migrants interviewed in the phone survey is first converted to taka from the local currency using exchange rates collected at the time of interview, although results are not sensitive to using the average annual exchange rate from 2014 (the year the phone survey was administered). There are some large outliers, so to trim the data, we set to missing the earnings values that are above the 95th percentile, separately by birth cohort and gender. When earnings are not trimmed, the program effect is a 45 percent increase in income. This program effect is driven by a few very large outliers.

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

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

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

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

TABLE B1—ANALYSIS SAMPLE ATTRITION RATES FOR MHSS2 DATA

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

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

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

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

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

TABLE B3—COMMON OCCUPATIONS BY CATEGORY

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

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

TABLE B4—DIFFERENCES IN AVERAGE HOURLY WAGE RELATIVE TO MANUAL WORK, MEN AGED 24–34

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

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

Appendix C
Potential Confounders

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

One government program that is pertinent for MCH-FP effects on education and difficult to control for is the Bangladesh Female Secondary Education Stipend Program. This was a national program that became available in Matlab in 1984 for girls attending grades 6-10 who were unmarried, had 75 percent attendance and scored 45 percent on school exams. The program targeted individual girls, not schools, and provided a stipend and covered many school costs. This program was available to all the females in the sample in this paper for the entirety of their secondary schooling in both the treatment and comparison areas, providing no variation to test for heterogeneity in treatment effects of MCH-FP based on differential access to this stipend program. In addition, we do not have data on who received the stipend program, nor sufficient data to determine who may have been eligible. Shamsuddin (2015) estimates that five years of exposure to the program led to one year gain in education. As a result, any program effects on educational outcomes of the MCH-FP program on girls needs to outweigh the already large effects on education from the female stipend program.

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

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

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

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

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

Panel F in Tables D5 and D10 repeat our earlier main analysis but include these extended controls. As with our baseline characteristics, these controls are fully interacted with our age group dummies. Our main results remain unchanged with the inclusion of these controls.

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Appendix D
Robustness Analysis

We perform a number of robustness checks to examine the validity and inference of the results for men (Tables D1–D7) and for women (Table D8–D11) for key outcome variables, including double difference results for men in Tables D2–D4 and for women in Tables D8–D9. For the men, we focus on the main outcomes from Tables 2, 3, and 4 including type of work, required skills, earnings, hours worked, and migration. For the women, we consider the main outcomes from Tables 9 and 10. We recreate the main results in panel A of Table D5 and Table D10 for a base comparison. Results remain similar unless noted. In addition, in Appendix F, we test the robustness to a number of different weights. In sum, the findings reported in the paper are similar across a variety of robustness checks, methods for inference, and types of weights.

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

Double-Difference. Double-difference results with villages fixed-effects to control for pre-program and non-time varying village differences are presented in Tables D2–D4 for males and Tables D8–D9 for females. We do not use the double-difference model as our main model because the MCH-FP program likely affected the labor and migration outcomes of the pre-program cohort. For example, labor and migration decisions of older cohorts and the 31–34 and 24–30 cohorts are likely linked because labor supply and migration are family decisions. It could be that improvements in human capital substitute for the labor supply of the older cohort, as younger family members are able to contribute more to the household. Conversely, the older cohort may choose to work more to support additional investments in treated children. Finally, smaller family sizes would further affect the household calculus of who works and where. Thus, double-difference estimates are biased in an unknown direction. The double difference cohort for men are born between 1955 and 1972 (aged 40–57 in MHSS2). They are young enough to limit number of fathers of men in our sample but born at least five years before the program to limit siblings and sibling competition. Still, of the men in the 40–57 comparison cohort half have a child in 24–30 or 31–34 cohorts and 17 percent a sibling born after program implementation. Women's migration and work decisions are less affected by other family members as women in our sample do not migrate for work and mainly work at home in agriculture. However, outcomes could be affected by fertility. Differences in access to family planning between the treatment and comparison area narrowed substantially after 1990, so we use a cohort born in the five years prior to the program, 1972–1976 (aged 35–40 in MHSS2) who largely started childbearing after 1990. Results are generally similar for the 24–30 cohort, though there is an increase in the earning advantage, but it is still not statistically significant. While point estimates are also similar for the 31–34 cohort, there is a reduction in the earnings disadvantage from 28 to 13 percent and a loss of statistical significance for most outcomes, except the negative migration effect. The stability of the point estimates between the single- and double-difference models underscores the similarity in outcomes of the pre-program cohort between the treatment and comparison areas, indicating that this cohort was largely unaffected by the program, though the difference in earnings for the pre-program cohort is negative between the treatment and comparison group.

Labor Market Robustness. It is possible the results are affected by how we define occupations. Our broad definition of the professional/semi-professional occupation includes small shop workers and small shop owners, such as owners of tea stands, which may not be viewed as better quality jobs. Table D6 demonstrates that results are robust to excluding small shop workers and owners from the occupation, and only including professional occupations.

In addition, it is possible that treatment children inherited their jobs from their fathers who were more likely to stay in Matlab, rather than migrate for work, when the program rolled out (Barham and Kuhn 2014). We examine whether fathers themselves were more likely to work in a professional/semiprofessional occupation in 1996 using MHSS1 (Table D6 column 4). These young men do not appear to have inherited their better job from their fathers as there are no positive effects.

Extended Controls, changes over time in Matlab. Changes over time in the study area include the introduction of an embankment as part of the Meghna Dhonnagoda irrigation project in the 1980s, and the introduction of a BRAC microcredit program in the early 1990s (see Appendix C for more details on these programs and the construction of the control variables). BRAC microcredit was introduced in a crossover design with the MCH-FP program in the 1990s and then became available in other villages, limiting potential biases. There was also an expansion of education during this time, including construction of primary and secondary schools as well as scholarships for girls. Finally, differential exposure to arsenic and healthcare throughout one's lifetime could potentially confound our results. Indeed, there was some imbalance in access to tubewell water at baseline, and tubewells allow arsenic to leach into the water. As our identification strategy relies on the assumption that the comparison area provides a good counterfactual for the treatment area over time, these changes could potentially bias the results. We include household level controls for arsenic exposure in 2003, and village-level controls from the 1990s and from MHSS2 for each of the other potential confounders. The controls from the 1990s adjust for access to these other programs when the sample were children, and the MHSS2 controls, when they are adults. Results are reported in Tables D5 and D10 (panel F). Again, the results are qualitatively the same.

Limited Set of Controls. Our main specification controls for a large number of baseline household characteristics with the dual purpose of controlling for potential imbalances across treatment and comparison households and improving statistical precision by controlling for characteristics related to the outcome variable. However, it is not clear *ex ante* which variables should be included, if any. Belloni, Chernozhukov, and Hansen (2014) propose a data driven approach to select a sparse set of controls when the goal is estimating causal parameters. Tables D5 and D10 (panel G) report results from a post-double selection LASSO procedure that remain robust to using a sparser set of controls.[1] Results are similar for a model that only includes baseline controls that were not balanced between experimental areas (results not reported).

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[1] We implement the post-double selection procedure using the pdslasso command in Stata. For the 24–30 men, the procedure selects indicators for religion, tin roof, tank drinking water, and latrine, bari size, family size, the number of rooms per capita, age of the household head, age of the household head's spouse, education of the household head's spouse, and the share of the bari that migrated prior to the program for all outcomes. For some outcomes, indicators

Micro-credit. Micro-credit for women has been pervasive in both the treatment and comparison areas for some time and there is some concern that this could impact the results for women. In addition to including controls for access to micro-credit in the 1990s, we examine interaction effects with access to micro-credit when it was first rolled out between 1993 and 1996 and do not find that results vary by a village's early access to micro-credit (results not reported).

Muslim Only. The baseline balance table revealed imbalance in the treatment and comparison area by religion. To determine if this imbalance affects the results, we restrict the sample to only those who report their religion as Muslim. There is insufficient sample size on those who reported Hindu as their religion to run results separately for this religious group. Results are reported in Tables D5 and D10 (panel H) and show the results remain the same.

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

Clustering Standard Errors. In our main specification, we conduct inference by clustering standard errors at the pre-treatment village level. This choice allows for potential correlation between individuals from the same village. The implementation of the program occurred within six blocks of villages across the study site (four comparison blocks and two treatment blocks). To allow for potential correlation in errors across these broader geographic areas, we further conduct inference by clustering standard errors at this level. We use the wild cluster bootstrap sampling method because of the small number of blocks (Cameron and Miller 2015). P-values are reported in Table D7 and D11 and statistical significance is almost identical.

Randomization Inference. With any assignment of village-level treatment status, significant treatment effects could occur simply by chance. We following Athey and Imbens (2017), but adjust the process to impose a contiguity restriction. Using a map of Matlab, we identify all neighboring villages for each village.[2] We construct 10,000 new treatment assignments using the following procedure. First, randomly select a village from which to grow the treatment area. Then, identify

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for tin or tinmix walls and tubewell water and the household head's education are also included. For the 31–34 men, the procedure selects indicators for religion, tin roof, and latrine, the number of rooms per capita, the age of the household head, the age of the household head's spouse for all outcomes. For some outcomes, an indicator for HH occupation in fishing, the number of cows, and the years of education of the household head's spouse are also selected.

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

all neighboring villages of that seed village and randomly select one of them to be assigned to the treatment area. Finally, continue identifying the neighbors and of the growing treatment area and randomly add one to the treatment area until the number of selected villages equals the actual number of treatment villages. Inference from this approach is reported in Tables D7 and D11 under "Rand Inf. – Contiguous Area". To test the sharp null hypothesis of no treatment effect for any person, we estimate single-difference treatment effects over the two sets of simulated treatment assignments. Tables D7 and D11 report the p-values for men and women, respectively. They are calculated as the share of trials where the simulated t-statistic is larger in absolute value than the actual t-statistic. For most outcomes, the level of significance remains the same between p-values constructed from clustered errors and randomization-based p-values. The p-values for skills used and urban migration are larger when simulating treatment using a contiguous area.

Bounding Attrition Bias. Tables D5 and D10 report results that aim to bound potential bias from survey attrition. Panels I and J report results from the worse-case scenario. The lower bound is constructed by assigning treatment (comparison) attritors the minimum (maximum) value of the outcome in the sample. For binary outcomes, this approach assigns each treatment (comparison) individual that value of 0 (1). The upper bound is constructed analogously, assigning treatment (comparison) attritors the outcome maximum (minimum). Following Kling and Liebman (2004), we also construct bounds by assigning attritors the sample mean of the outcome, +/- one standard deviation (Panel K and L). Significant results in most cases are bounded away from zero, or at least close to zero.

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

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

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

TABLE D2—DOUBLE-DIFFERENCE ITT EFFECTS ON LABOR MARKET PARTICIPATION, OCCUPATION, AND JOB SKILLS IN THE PAST 12 MONTHS, MEN

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

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

TABLE D3—DOUBLE-DIFFERENCE ITT EFFECTS ON SOURCES OF EMPLOYMENT IN THE PAST 12 MONTHS, MEN

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

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

TABLE D4— DOUBLE-DIFFERENCE ITT EFFECTS ON INCOME, HOURS, AND LOCATION OF WORK IN THE PAST 12 MONTHS, MEN

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

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

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

TABLE D5—ITT EFFECTS, MEN, ROBUSTNESS CHECKS

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

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

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

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

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

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

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

TABLE D6—ITT EFFECTS ON TYPE OF WORK, ROBUSTNESS CHECKS, MEN

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

Notes: Standard errors are clustered at the pre-program village level. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. Column (1) is the main result from Table 1. Column (2) removes those who work in a small shop. Column (3) indicates whether the respondent works in a professional occupation. Column (4) indicates whether the respondent's father was in a professional, clerical, or sales occupation in 1996 from MHSS1.
** p<0.01, * p<0.05, + p<0.1

TABLE D7—ITT EFFECTS, MEN, INFERENCE ROBUSTNESS CHECKS

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

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

TABLE D8— DOUBLE-DIFFERENCE ITT EFFECTS ON LABOR MARKET PARTICIPATION IN THE PAST 12 MONTHS, WOMEN

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

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

TABLE D9—DOUBLE-DIFFERENCE ITT EFFECTS ON EARNINGS, HOURS, LOCATION OF WORK, ASSETS, AND MICRO-CREDIT, WOMEN

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

Notes: Standard errors are clustered at the pre-program village level. Adjusted p-values are reported in brackets and control for the false discovery rate (Anderson, 2008) across outcomes in Tables f–A11. Pr(24–30 = 31–34) is the p-value from the test of the null hypothesis that the two cohort ITT effects are equal. Means by age cohort are for the comparison group. Regressions include individual characteristics and preintervention characteristics interacted with birth cohort and are weighted to correct for attrition between birth and the MHSS2 survey. Individual characteristics include year of birth fixed effects, age cohort fixed effects, and controls for religion. Preintervention characteristics include all individual and household characteristics in Table 1. All incomes are reported in 2012 USD. For trim, incomes are sorted, the first value that is more than 2 SD larger than the next closest value is chosen as the cutoff threshold. The observations with that income or larger are dropped from the sample. Urban locations are Dhaka, Chittagong, and their surrounding metro areas.
** p<0.01, * p<0.05, + p<0.1

TABLE D10—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS

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

TABLE D10—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS, (CONT.)

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

TABLE D10—ITT EFFECTS, WOMEN, ROBUSTNESS CHECKS, (CONT.)

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

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

TABLE D11—ITT EFFECTS, WOMEN, INFERENCE ROBUSTNESS CHECKS

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

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

Appendix E
Local Variation in Food Prices within Matlab

In this section, we show that food prices were similar between treatment and comparison areas. We use market prices from the MHSS2 Community Survey, collected longitudinally in markets throughout Bangladesh.[1] We have market price data for six separate markets within the study site. Prices were collected at nine points in time, semi-regularly between January 2013 and September 2014. We determine the treatment status of the market by considering the share of treatment area households that are closest to the market.[2] For each household in our data, we identify the closest market within the market price survey using the distance between the household centroid and the market centroid. This 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.

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

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

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

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

TABLE E1—WITHIN-ITEM PERCENT DIFFERENCES IN LOG PRICES

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

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

[1] 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.

[2] 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.

Appendix F
Weights

The main results are weighted for attrition between birth and MHSS2 using inverse propensity weights. The main reasons for non-response are migration in early adulthood and death primarily during infancy. Weights are constructed in two steps. First, we estimate weights to account for selection into the MHSS1 sample frame between birth and MHSS1, which is mainly a result of mortality. Second, we estimate weights to account for attrition of MHSS1 respondents in the MHSS2 survey. We estimate these two probabilities separately and then multiple them to obtain a weight to account for attrition between birth and MHSS2.

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

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

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

In Panel C, we further weight observations so that the analysis is representative of the pre-program population in 1974. We choose 1974 as the pre-program year because census data of the entire study site is available for that year so we are able to construct weights for that year. Because the sample was selected in 1996, it is possible that it is not representative of the pre-program population if the program altered household formation and re-formation between the baseline in 1974 and the time at which the population was sampled in 1996. Foster and Milusheva (2017) develop a weighting methodology to derive 1974 household weights. The weight incorporates both the probability of a household being sampled in 1996 and the probability that a 1974 linked-household was sampled in 1996. We follow their procedure but adjust it to account for the fact that we link individuals back to 1974 households based on where their household head lived in 1974. The 1974 evaluation weight is then the ratio of the 1974 sampling probability[3] to the product of the 1996 sampling probability and the total number of 1974 household descendants in 1996 in the individual's cohort. We multiple this weight by the birth to MHSS2 attrition weight used in the paper, so that we also account for sample attrition. Again, results are similar using this weighting scheme, with a few exceptions. For men, the negative effect on work in a manual occupation in the 24–30 cohort is larger and now statistically significant, and for the 31–34 cohort migration for

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

work to any destination is smaller. For women, there is no longer an effect on working more in paid work and in agriculture for the 24–30 cohort, and an increase in the point estimate and significance for ever having a micro-credit loan for 31–34 cohort.

REFERENCES

Foster, Andrew, and Sveta Milusheva. 2017. "Household Recombination, Retrospective Evaluation, and Educational Mobility over 40 Years," Unpublished manuscript.

TABLE F1— ITT EFFECTS, MEN, WEIGHTS

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

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

TABLE F2—ITT EFFECTS, WOMEN, WEIGHTS

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

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