Structural Transformation and the Demographic Transition: Evidence from Bangladesh*

Tania Barham (CU-Boulder), Randall Kuhn (UCLA),
Brett A. McCully (Collegio Carlo Alberto), Patrick S. Turner (Notre Dame)

July 1, 2022

PRELIMINARY AND INCOMPLETE
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Abstract

We study the effect of the demographic transition on structural transformation. To do so, we leverage the quasi-random placement of a program which reduced fertility and early-life mortality in rural Bangladesh. On net, the program reduced average family size. We use rich data that allows us to follow treated and control households 35 years later. Increased labor market and intra-household competition drove control households–which were relatively larger–to send workers to urban areas in search of employment in manufacturing or office work. By contrast, treated households were 9 percent more likely to farm crops, and 23 percent less likely to have a member working in a factory. Treated farmers adjusted to smaller household sizes by more intensively using labor-substituting technology, capital, and intermediate inputs.

*We thank seminar participants at UCLA and Collegio Carlo Alberto, and conference participants at IPC2021 for helpful comments. McCully acknowledges financial support from CCPR's Population Research Infrastructure Grant P2C from NICHD: P2C-HD041022, CCPR's Population Research Training Grants T32 from NICHD: T32-HD007545, and the Institute on Global Conflict and Cooperation. The data collection for this project was generously funded by the National Institutes of Health, the Population Research Bureau, and the International Initiative for Impact Evaluation. All errors are our own.

1 Introduction

Economic development is characterized by structural transformation—the movement of workers out of agriculture and into manufacturing and services (Lewis, 1954; Kuznets, 1957). Prior research has shown that structural transformation drives another fundamental process associated with economic development—the demographic transition, i.e., the fall in birth and death—among currently-industrialized countries (Galor and Weil, 2000; Greenwood and Seshadri, 2002; Lucas, 2018). However, there is scarcely any empirical evidence on the other direction of causality: how the demographic transition affects structural transformation.

This causal relationship is crucial to understand as medical technology facilitating falling birth rates has spread faster around the globe in recent decades than when currently industrialized countries underwent their demographic transition (Delventhal et al., 2021). The quicker speed of the demographic transition may outpace structural transformation, as in Bangladesh (see Figure 1), making it crucial to understand the implications for economic development. However, the long-run nature of the demographic transition and structural transformation has hindered research, as analysts need both decades of data to chart the effects on households and exogenous variation in birth and death rates.

In this paper, we estimate the causal effect of the demographic transition on structural transformation. To obtain causal identification, we exploit an intervention that distributed modern contraception and childhood vaccines to treatment households. The intervention exogenously accelerated the demographic transition by inducing (i) a fall in birth rates and (ii) a fall in death rates. To understand long-run effects, we leverage highly detailed microdata collected across four decades in rural Bangladesh. We estimate how the intervention affected sectoral employment choice and how agricultural households adjusted to smaller family sizes.

To guide our empirical work, we develop a simple model of structural transformation. We consider a small open economy with two sectors and three factors of production: land, labor, and imported intermediate inputs. Since the intervention we study led to smaller cohort sizes, we consider the effect of a reduction in workforce size. The model predicts that a smaller workforce raises the fraction employed in agriculture. A larger share of workers work in agriculture because the supply of arable land is fixed. Moreover, households substitute intermediate inputs for labor in agricultural production. Thus the model predicts that a fall in fertility will slow down the process of structural transformation and will reduce agriculture's dependence on labor.

We test the empirical predictions of the model by studying the long-run effects of the Maternal and Child Health and Family Planning program (MCH-FP), which was rolled out to treatment villages in the rural subdistrict of Matlab, Bangladesh between 1977 and 1988. The program distributed modern contraception to women of childbearing age as well as vaccines to pregnant women and young children. Treatment was assigned by village, with treatment and control villages well balanced across a range of pre-intervention characteristics. The program substantially reduced fertility, and net of mortality declines from vaccines, resulted in relatively smaller cohorts born inside the treatment area during the program period.

Cohorts affected by the program did not enter the workforce until many years after the program started. This led to a substantial lag between program initiation in 1977 and the manifestation of the program effects on the labor market decades later.[1] We benefit from exceptional data collection efforts in our context. In particular, we can trace back individuals to their pre-intervention villages, thus allowing us to estimate intent-to-treat effects without contamination from endogenous moves after program initiation. Moreover, we see household employment and agriculture outcomes in 1996 and 2014, 19 and 35 years after the program started.

We find that the demographic transition slowed down structural transformation. However, this effect took a while to manifest: we detect no economically significant effect of the program as of 1996. By 2014, we see significant effects. Consistent with our theoretical predictions, treated households were 5 percentage points more likely to have a member working in agriculture, but 5 percentage points less likely to have a member working in a factory. Moreover, treated households were much less likely to have a member working at a large employer or in an office. Our baseline effects are primarily mediated by rural-to-urban migration.

We find that household size and composition is a crucial mechanism through which the program shapes structural transformation. This channel operates through increased intra-household and local labor market competition. We find that the more children born during the program period, the more likely a treatment household is to have a member working in a non-agricultural job. Our baseline results persist even when we aggregate to the village-level, suggesting that general equilibrium effects at the local labor market level do not wash out the sectoral reallocations induced by the demographic transition. The gender composition of the household is also important. The effects we find are concentrated primarily among men, as women are much less likely to participate in the labor force.

Finally, we assess how agriculture adjusted in the face of smaller households and thinner labor markets. For crops that we classify as labor intensive, farmers more intensively used high-yield variety seeds, capital, and market purchased inputs such as pesticides. By contrast, farmers made no change to the mix of inputs used for crops that we classify as non-labor

[1] Bloom et al. (2001) notes that the economic effects of a demographic transition may take many years to play out.

intensive.

Relevant Literature. Our paper contributes to several literatures. We are the first to empirically establish a causal link between the demographic transition, structural transformation, and rural-to-urban migration, three central features of economic development. Most existing studies do not model the way in which the demographic transition shapes structural transformation (Galor and Weil, 1996, 2000). A notable exception is Leukhina and Turnovsky (2016), who link population growth with structural transformation in the context of the England's industrialization. Yin (2021) leverage China's One Child Policy and look at the effect on sectoral employment. However, both studies rely on calibrated macroeconomic models and aggregate time series data, making causal identification and parsing of different mechanisms challenging.

We contribute to the literature on how the demographic transition affects economic growth. At the population level, Bloom et al. (2001) highlight the potential for a "demographic dividend" whereby dramatic increases in economic output may emerge several decades after health and demographic changes as a favorable age structure emerges characterized by relatively large and healthy prime-age cohorts. We provide some of the first direct empirical evidence for these effects.

Our evidence on labor-saving input adjustment in the face of thinner labor markets is consistent with research by Hornbeck and Naidu (2014) and Clemens et al. (2018). In contrast to those studies, we explore the effects of the demographic transition. We also have quite rich crop-level household microdata with which to explore the mechanisms of adjustment in agriculture.

This paper proceeds as follows. The next section lays out our simple theoretical model and predictions. Section 3 discusses the intervention, data, and context, while Section 4 explains our empirical specifications. Section 5 presents our results and Section 6 concludes.

2 Model

In this section we present a simple model of structural transformation. Since our empirical setting is a small subdistrict in Bangladesh, we consider a small open economy. There are two sectors, agriculture and manufacturing, and three factors of production, land, labor, and imported intermediate inputs.

2.1 Setup

Consider a small open economy that trades agricultural and factory goods with the world economy.[2] The economy has L households, each inelastically supplying one unit of labor. Production of the factory good follows a Cobb-Douglas production process utilizing labor and imported intermediate inputs:

$$Q_f = A_f Z_f^{\alpha} L_f^{1-\alpha} \tag{1}$$

for $\alpha \in (0,1)$, where factory output is denoted by Q_f, factory productivity is A_f, intermediate inputs used in the factory sector are Z_f, and labor allocated to factories is L_f.

Production of the agricultural good follows a hybrid Cobb-Douglas/Constant Elasticity of Substitution (CES) production process which requires land, labor, and intermediate inputs:

$$Q_g = A_g \left[\omega Z_g^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) L_g^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\theta\epsilon}{\epsilon-1}} T_g^{1-\theta} \tag{2}$$

where Q_g is the quantity of agricultural goods produced, A_g is agricultural productivity, L_g is labor allocated to agriculture, T_g is land allocated to agriculture, and Z_g is intermediate goods used in agriculture. $\epsilon > 0$ is the elasticity of substitution between intermediate inputs and labor, and the parameters ω and θ are between 0 and 1. ω governs the relative productivity of Z_g relative to L_g, while $1-\theta$ is the revenue share earned by landowners.

The marginal product of labor in agriculture is

$$MPL_a = A_g (1-\omega)\theta L_g^{-\frac{1}{\epsilon}} [\cdot]^{\frac{\theta\epsilon}{\epsilon-1}-1} T_g^{1-\theta}$$

where $[\cdot]$ is the CES portion of equation 2. A key determinant of the wage is the quantity of the fixed factor, T_g, available. As land increases, so does the marginal returns to labor in agriculture, and hence more workers will remain in agriculture.

2.2 Equilibrium

Since we are considering a small open economy, the local price ratio P_g/P_f equals the world price ratio, $(P_g/P_f)^*$. Profit maximization implies that the value of marginal products across sectors equal the wage w:

[2]The small open economy assumption obviates the need for modeling demand. We show in Table A.1 that the experimental program did not induce any changes in consumption shares across sector, suggesting that demand-side factors are not driving sectoral reallocations.

$$P_g MPL_g = w = P_f MPL_f$$

which determines the equilibrium wage,[3]

$$w = P_f A_f (1-\alpha) \left(\frac{P_z}{A_f \alpha} \right)^{\frac{\alpha}{1-\alpha}} \tag{3}$$

Equation B.5 plus land market clearing ($T_g = T$, where T is the aggregate endowment of land) determine the equilibrium share of labor working in agriculture:

$$\frac{L_g^*}{L} = \frac{1}{L} \left[\frac{A_g}{A_f} \frac{(1-\omega)\theta}{(1-\alpha)} \frac{\left[\omega \left(\frac{1-\omega}{\omega} \frac{\alpha}{1-\alpha} \right)^{\epsilon-1} \left(\frac{P_z}{A_f \alpha} \right)^{\frac{\epsilon-1}{1-\alpha}} + (1-\omega) \right]^{\frac{\theta\epsilon}{\epsilon-1}-1}}{P_f \left(\frac{P_z}{A_f \alpha} \right)^{\frac{\alpha}{1-\alpha}}} T^{1-\theta} \right]^{\frac{1}{\frac{1}{\epsilon}+1-\theta}} \tag{4}$$

The fraction of workers employed in the factory sector can be obtained using the labor market clearing constraint, $L = L_g + L_f$.

Furthermore, the equilibrium per-household use of intermediate inputs in agriculture is

$$\frac{Z_g^*}{L} = \left(\frac{1-\omega}{\omega} \frac{\alpha}{1-\alpha} \right)^{\epsilon} \left(\frac{P_z}{A_f \alpha} \right)^{\frac{\epsilon}{1-\alpha}} \frac{L_g^*}{L}$$

Finally, the value of output per acre is

$$\frac{P_g^* Q_g^*}{T_g^*} = P_g A_g \left[\omega \left(\frac{1-\omega}{\omega} \frac{\alpha}{1-\alpha} \right)^{\epsilon-1} \left(\frac{P_z}{A_f \alpha} \right)^{\frac{\epsilon-1}{1-\alpha}} + (1-\omega) \right]^{\frac{\theta\epsilon}{\epsilon-1}} \left(L_g^* \right)^{\theta} T^{-\theta}$$

For detailed derivations, see Appendix B.

2.3 Comparative Statics

We next assess the effect of the MCH-FP on sectoral employment. The MCH-FP on net reduced the size of cohorts, as the reduction in fertility swamped the reduction in early-life mortality in terms of overall population growth (Joshi and Schultz, 2013; Barham et al., 2021b). Therefore we look at how labor allocations change in response to a change in the population, L.

We analyze data aggregated at the household level, and thus consider per-capita or per-acre outcomes. Our simple model therefore generates the following testable predictions:

[3]See Appendix B for all model derivations.

EMPIRICAL PREDICTIONS: A relatively lower population will result in:

1. *an increased share of workers employed in the agricultural sector,*
2. *a decreased share of workers employed in manufacturing,*
3. *more intensive use of intermediate inputs in agriculture,*
4. *no change to the value of output per acre,*
5. *no change to wages.*

Proof: See Appendix B.

In what follows, we test these predictions in the context of Bangladesh.[4]

3 Background and Data

3.1 The MCH-FP Program

The Maternal and Child Health and Family Planning (MCH-FP) program was introduced in the Matlab subdistrict in Bangladesh in 1977 by icddr,b (formerly known as International Centre for Diarrhoeal Disease Research, Bangladesh). The program included integrated family planning and maternal and child health services. A key feature of the program was that interventions were administered in the home free of charge during monthly visits by local female health workers. Program intervention were rolled out over time starting with a focus on interventions for women including family planning program and tetanus toxoid vaccines for pregnant women. Intensive child health interventions started in 1982 with the measles vaccine and other child health interventions were introduced in 1985 including vaccination against measles, tetanus, pertussis, polio, and tuberculosis were distributed for children starting in 1985.

In the comparison area, then-standard government health and family planning services were available, but family planning services were only available at clinics, not in the home, and some of the childhood services, such as vaccinations, were not readily available in clinics until 1989 or later, providing an experimental period, 1978–1988, to evaluate the program.

The MCH-FP program was introduced to half of Matlab, with the remaining half serving as an untreated comparison. We depict treatment and comparison villages in Figure 2. The program covered about 200,000 people in 149 villages, with the population split evenly

[4]Barham et al. (2021b) already confirmed the Prediction 5, finding no significant effect of the MCH-FP on wages.

between the two areas. The program was placed in a single block of contiguous villages, with a block of comparison villages on two sides. The block design was intended to reduce potential contamination of the comparison area with information about the family planning interventions (Huber and Khan, 1979) and spillovers from positive externalities generated by vaccination. The comparison villages were socially and economically similar to the treatment villages and geographically insulated from outside influences (Phillips et al., 1982). Treatment and comparison blocks were chosen in order to balance the average distance to transport and health infrastructure between the blocks. We thus refer to the placement of this intervention as quasi-random and draw further support for our identification strategy from the evidence shown in Section 4.1 of pre-program similarities between treatment and comparison areas.

Program interventions were phased in, as detailed in Table 1. Between 1977 and 1981, program services focused on family planning and maternal health through the provision of modern contraception, tetanus toxoid vaccinations for pregnant women, and iron folic acid tables for women in the last trimester of pregnancy (Bhatia et al., 1980). Take up of tetanus toxid was low during this period at less than 30 percent of eligible women (Chen et al., 1983). Health workers provided a variety of family planning methods in the homes of the beneficiaries including condoms, oral pills, vaginal foam tablets, and injectables. In addition, beneficiaries were informed about fertility control services provided by the project in health clinics such as intrauterine device insertion, tubectomy, and menstrual regulation. During these visit the female health works also provided counseling on contraception, nutrition, hygiene, and breastfeeding, and motivated women to continue using contraceptives. These services were supported by followup and referral systems to manage side effects and continued use of contraceptives (Phillips et al., 1982; Fauveau, 1994).

Program implementation followed the planned timeline, and uptake was rapid as evidenced by the takeup of two key interventions family planning and the measles vaccine 3. Prior to the program, the contraceptive prevalence rate (CPR) for married women 15â49 was low (< 6 percent) in both the treatment and comparison areas (Figure 3). The CPR reached 30 percent in the treatment area in the first year, then rose steadily, reaching almost 50 percent by 1988. Because contraceptives were also provided by the government, the CPR increased in the comparison area, but not as quickly, and remained below 20 percent in 1988. By 1990, there was still a 20 percentage point difference in the CPR rate between the two areas. The measles vaccination rate rose to 60 percent in 1982 after it was introduced in half of the treatment area, and in 1985 when it was introduced in the other half as shown in Figure 3. By 1988, coverage rates for children aged 12–23 months living in the treatment area were 93 percent for the vaccine against tuberculosis, 83 percent for all three doses of the vaccines against diptheria, pertussis, tetanus, and polio, 88 percent for measles, and 77 percent across all three major immunizations (icddr, 2007). Government services did not regularly provide measles vaccination for children until around 1989, so the comparison area was an almost entirely unvaccinated population (Koenig et al., 1991). Nationally, measles vaccination for children under the age of five was less than 2 percent in 1986 (Khan, 1998) and was below 40 percent in the comparison area in 1990 (Fauveau, 1994).

The staggered rollout of program components led to differential treatment of children depending on their year of birth. However, children of all ages may have experienced some effects as parents shift child-specific investments in response to the program. Moreover, the program affected all participants in the labor market, as the intervention significantly affected cohort size.

Previous research demonstrates that the MCH-FP program had significant effects on fertility and human capital. **?** show that completed family size was between 0.52 and 0.67 smaller in the treatment than the comparison area depending on the number of reproductive years a woman was exposed to the MCH-FP Program. In terms of human capital, using data collected in 1996, Barham (2012) finds that children born between 1982 and 1988 (approximately age 8–14 at the time) in the treatment area, experienced significant improvements in height (0.22 SD), cognitive functioning (0.39 SD), and schooling (0.17 SD). There was no effect on those born prior to the introduction of intensive child health interventions for those born between 1977-1981. Joshi and Schultz (2007) use a different research design and also find schooling increased for boys. In a follow-up paper, Barham et al. (2021a) show results on height and education, but not cognition, persisted into adulthood for those born between 1982-88 and results differed by gender. There are still no effects for children born when the focus of the program was on family planning between 1977-1981, Men and women born between 1982-1988 experience about a one-centimeter increase in height, though it is only statistically significant for women, and only men experienced improved education outcomes (0.82 increase in years of education and 0.2 standard deviation increase in a math test).[5]

3.2 Data, Sample, and Treatment Indicators

Data Sources This paper draws on the extraordinarily rich data available for the Matlab study area. The outcomes for this paper are primarily from household-level data on agriculture production, as well as individual employment responses. To measure these outcomes, we use both the 1996 Matlab Health and Socioeconomic Survey wave 1 (MHSS1) (Rahman et al., 1999) and the 2012–2014 Matlab Health and Socioeconomic Survey wave 2 (MHSS2). These

[5]The lack of an effect on education for women is not surprising given a secondary school stipend program for females was available in both the treatment and comparison areas during the schooling years.

data contain a rich set of household agricultural variables, including crop-level inputs (e.g., acres, use of high-variety seeds, spending on other inputs) and output (quantity harvested) for 11 types of crops. MHSS2 also asked about the use of high-yield variety seeds as well as a rich set of outcomes for employment, including about the firms founded by respondents. We use questions about factory employment, agricultural employment, and office employment to understand individual's sector of employment. Questions changed significantly between survey rounds, and the MHSS2 offers a richer set of questions about sectoral employment.

MHSS2 was conducted between 2012 and 2014 and has low attrition rates with the loss of less than 10 percent of the target sample.[6] Respondents were tracked throughout Bangladesh and intensive efforts were made to interview international migrants and difficult-to-track migrants when they returned to the study area to visit family, especially during Eid celebrations. Most data were collected in face-to-face interviews, so are not proxy reports. Fifteen percent of men in our sample, international migrants living abroad, were contacted using a phone survey.

We also use two supplementary data sources: periodic censuses in 1974 and 1982 (icddr, 1974, 1982), and 1974–2014 Matlab demographic surveillance site (DSS) data on the universe of vital events (e.g., births, marriages, deaths, in and out migrations) collected by the International Center for Diarrhoeal Disease Research, Bangladesh (icddr,b). The MHSS1 and MHSS2 are a panel of a random sample of households from the study area, while the census and DSS data cover the entire study area. A key feature of all these data is that individuals can be linked across different data sources by a unique individual identifier, allowing the the linkage of individuals and households from the Matlab area across time and with their parents over the past thirty-five years. In addition, the 1974 census allows one to test pre-intervention balance. The DSS data are collected bi-weekly or monthly and allow determination of exact birth dates and birth place, key inputs to our assignment of treatment status as we detail below. There are few, if any, other study sites that have similarly rich data availability to allow for this type of long-term evaluation.

Analysis Sample and Attrition. In this paper, we consider two primary units of analysis. In our baseline estimation, we look at households, the unit at which decisions about the family farm are typically made in Bangladesh. Moreover, households often jointly make migration decisions for individual members. Because household composition may change

[6]The MHSS2 is a panel followup of all individuals in the MHSS1 primary sample and their descendants. The MHSS1 primary sample is representative of the study area's 1996 population, but does not include individuals who migrated between program start and 1996. To address this unrepresentativeness, MHSS2 also includes individuals born to an MHSS1 household member between 1972 and 1989 who had migrated out of Matlab between 1977 and 1996, which we refer to as pre-1996 migrants.

over time in response to the MCH-FP, we consider 1996 MHSS1 households as our unit of household analysis. That is, we aggregate MHSS2 households into the household in which survey respondents resided in 1996. Household composition at this early stage is unlikely to be shaped by the program since the children born during the program were not yet of age to form their own households. For individual outcomes, such as sector of employment, we consider that outcome to have occurred if at least 1 member of the 1996 household experienced the outcome. Only 0.5 percent of MHSS1 households cannot be tracked to the MHSS2 survey round.

In supplementary analysis, we also analyze employment outcomes at the individual level. The sample of individuals includes those who were randomly selected for individual interviews in an MHSS1 primary sample household or were a pre-1996 migrant into Matlab. Including death and any other type of non-response, the attrition rate is 7 percent. This is a low attrition rates compared to other long-term effects studies with shorter follow-up periods despite a migration rate of approximately 60 percent for men (25 percent international) in this highly-mobile population.

Intent-to-Treat and Baseline Variables. Access to the MCH-FP program was based on the village of residence of the individual/household during the program period. We cannot use the area where the household or individual lived at the time of survey or even when some of the individuals in our individual sample were born because may have been after the start of the program and therefore might be endogenous (Barham and Kuhn, 2014). We determine treatment at the household and individual level by exploiting the Demographic Surveillance System and census data, tracing back an individual in the MHSS2 2012–2014 survey back through their family tree to find where the household head lived prior to the program.

Specifically, we create an individual-level intent-to-treat (ITT) indicator by tracing each individual back to their 1974 village of residence to determine eligibility status. If the person was not alive then, we trace back the residency of their earliest known household head to 1974. The ITT variable takes the value of 1 if the 1974 census-linked household head was living in a village in the treatment area in the 1974 census or migrated into a village in the treatment area from outside Matlab between 1974 and 1977 (using the DSS), and 0 otherwise. At the household level, a household is considered treated if the household head in the 1996 MHSS1 survey is considered treated based on the individual-level trace back described above.

Baseline characteristics from the 1974 census are linked to individuals through the census-linked household head. In our individual-level models, we further isolate the hypothesized effects on children born during the intervention period by interacting the ITT variable with the timing of birth as between 1978–1981, 1982–1988, and a dummy for being born outside of the program period.

4 Estimation Strategy

We now discuss how we leverage the quasi-experimental variation induced by the MCH-FP program to estimate the causal effect of the program on structural transformation and agricultural outcomes. The placement of the program was balanced across a wide-range of pre-intervention covariates, providing support for an identification strategy that relies on estimating single-difference equations.

4.1 Baseline Balance and Trends

Because our identification strategy uses variation between treatment and comparison villages, we now show that pre-intervention characteristics were balanced between these two areas with the exception of access to tube well water and religion. Prior studies have shown that the treatment and control villages are extremely well-balanced across a range of variables. Importantly, balance holds across several important dimensions including mortality rates, fertility rates, and pre-intervention household and household head characteristics (Koenig et al., 1990; Menken and Phillips, 1990; Joshi and Schultz, 2013; Barham, 2012). In addition, migration stocks and flows were similar between the treatment and comparison area at the start of the program and through to 1982, for a cohort of individuals most likely to migrate at the start of the program, showing good baseline balance (Barham and Kuhn, 2014). Barham et al. (2021b), further show that for men born between 1977 and 1988, the labor market outcomes for their antecedent households were similar in 1974 and the trends were similar in the early years of the program between 1974 and 1982. Finally, Barham (2012) also shows that cognitive functioning, height, and education were similar across the treatment and comparison areas in 1996 for those who were old enough that their human capital and height were not likely to have been affected by the program.

Much of the previous literature examined baseline balance at the individual level. Because our baseline estimation is at the household level, we further explore the baseline balance between the treatment and comparison area at the household level in Table 2 using 1974 census data. Table 2 presents means for the treatment and comparison group separately and the differences in means between the two group. As well as reporting the statistical significance of the differences in means between the treatment and comparison areas, we examine the normalized differences in means (difference in the means divided by the standard deviation of the mean for the sample). The normalized difference provides an indication of the size of the differences in means, since small differences in means can be statistically significant with large sample sizes (Imbens and Wooldridge, 2009). Normalized differences bigger than 0.25 standard deviations are generally thought to be substantial.

Table 2 highlights that the differences in means are insignificant at the five percent level for all variables except household head years of education, household head is Muslim, and using tubewell water for drinking. Since we test balance across 22 variables it is not surprising that a few are statistically different. In our baseline specification, we control for all baseline variables.

With the exception of religion and tubewell water for drinking water, the normalized differences are less than 0.12 demonstrating that the differences that do exist are relatively small. The difference in tubewell access is close to the cut off at 0.20SD. It is important to note that the difference in tubewell access is a result of a government program,[7] so do not reflect household income, propensity to drill a tubewell, or a householdâs concern about child health or potentially other unobservables.

Tube-well water is often thought to be the cleanest source of drinking water and could potentially affect human capital development. Unfortunately, there is widespread groundwater arsenic contamination in the tubewells in Bangladesh (Chowdhury et al., 2000) and arsenic is a health concern and has been shown to reduce IQ among school aged Bangladeshi children (Wasserman et al., 2006) making any bias on human capital unclear. Barham (2012) explores this concern and does not find differences in tubewell water, or religion, are driving program effects on human capital. In sum, the baseline balance results mimic previous research and show that the two areas are similar across a wide variety of household and household head characteristics.

4.2 Empirical Specification

To examine the effect of the program on sectoral employment and agricultural outcomes we take advantage of the well-balanced treatment and comparison areas and use a single-difference intent-to-treat (ITT) models. We estimate the household-level specification,

$$Y_h = \omega_0 + \omega_1 T_h + \zeta X_h + \varepsilon_h \tag{5}$$

[7]In 1968 the government of Bangladesh (then East Pakistan) set out a goal of installing one tubewell for every 200 people. With the support of the United Nations Children Fund, by 1978 over 300,000 tubewells had been sunk, about one for every 250 rural inhabitants (Black, 1986).

where T_h is an indicator for whether household h is considered treated (as defined in Section 3.2) and X_h is the vector of demographic and baseline characteristics detailed in Table 2. We cluster standard errors by the village of the household head of h or his antecedents in 1974.

To estimate the effect of the MCH-FP on sectoral employment at the individual level, we use variation in 1974 location (treatment versus comparison villages) as well as the timing of the rollout of program components over time to examine the ITT effects on two cohorts (1977-81 and 1982-88). Past research on the effects of the MCH-FP by Barham (2012) and Barham et al. (2021b) have found pronounced effects for the cohorts born between 1982 and 1988 and negligible effects for those born between 1977 and 1981 We also separately estimate our individual regressions by gender.

While our baseline specification is at the household level, we also estimate some outcomes at the individual level. We estimate a single-difference equation at the individual level of the form:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 T_i + \beta_2 Born_i^{77-81} + \beta_3 Born_i^{82-88} + \beta_4 Not\ born_i^{77-88} \\ \gamma_1(T_i \times Born_i^{77-81}) + \gamma_2(T_i \times Born_i^{82-88}) + \gamma_3(T_i \times Not\ born_i^{77-88}) + \alpha_{y(i)} + \nu X_i + \epsilon_i \quad (6) \end{aligned}$$

where $Born_i^{y_1-y_2}$ is an indicator variable for whether individual i was born between years y_1 and y_2. T_i is an indicator for whether i is treated as defined in Section 3.2; $\alpha_{y(i)}$ is a set of indicator variables for i's birth year; and X_i is the vector of pre-intervention demographic and baseline characteristics detailed in Table 2. We cluster standard errors by the 1974 village of i (or i's antecedents if i was not born by 1974).

The coefficients γ_1, γ_2, and γ_3 represent the intent-to-treat single-difference coefficients of interest. In particular, they represent the difference in conditional means for the outcome for the relevant age group. γ_1 captures the effects of the family planning and maternal health interventions combined with any spillovers of having younger siblings exposed to the intensive child health interventions, and γ_2 is the combined effect of all program interventions, including the childhood vaccination programs. γ_3 captures any indirect spillover effects of the program on older or younger generations.

5 Results

Here we test our theoretical predictions from Section 2. Our model implied that a relatively lower population should induce (i) a relatively higher fraction of workers to be employed in the agricultural sector, (ii) a lower fraction of workers in the manufacturing sector, (iii) an increase in non-labor agricultural inputs, and (iv) no change in agricultural output per acre.

5.1 Employment

We first estimate the effects of the MCH-FP on employment by sector at the household level. In the medium term (i.e., in 1996, 19 years after program initiation), we find no precisely estimate impact of the program, as shown in Table 3. The magnitude of the impacts tends to be small: the program increased the likelihood of a household having a member currently working in agriculture by 6.7 percent (column 1) and ever having worked in agriculture by 1.3 percent (column 2). We do, however, see a non-negligible increase in total earnings from agriculture (16 percent, column 3), but we cannot rule out that the effect is equal to 0.

Table 4 reports effects at the time of the MHSS2 survey, taken between 2012 and 2014 (35 years after program initiation). Consistent with our theoretical predictions, we find that the MCH-FP raised the likelihood of the household having someone employed in agriculture by 5 percentage points (column 1). An individual working in agriculture may do so as a sharecropper, on the family farm, or as a hired laborer on others' farms. Treated households are also 6 percentage points less likely to have a member who works for an employer with at least 100 workers, consistent with the theory of Buera and Kaboski (2012) that scale economies are a key driver of structural transformation (column 2).

We also find that non-agricultural employment rose significantly. Treated households are 5 percentage points less likely to have a members working in an office (column 3), 5 percentage points less likely to have a members currently working in a factory (column 4), and 10 percentage points less likely to have a member who has ever worked in a factory (column 5). Finally, treated households are 3 percentage points less likely to have a member who owns a manufacturing sector enterprise.One advantage of the MHSS2 survey instrument over the MHSS1 questionnaire is that we can observe greater detail in the types of firms respondents worked for. Unfortunately, this means we cannot directly compare all outcomes across surveys.

To understand what drives the differential effects in sectoral employment, we perform a mediation analysis by whether the household sent a member to migrate to an urban area. We report our results in Table 5. We find that rural-to-urban migration strongly mediates the impact of the MCH-FP on sectoral employment outcomes. Treated households without members in an urban area are 8 percentage points more likely than control households to engage in agricultural employment (column 1). Rural treated households are 28 percentage points less likely to work for a large employer relative to all control households, while treated households with some members working in an urban area are 9 percentage points more likely than control households to have a member working for a large employer (column 2). The pattern is similar for office work (column 3), and factory work (columns 4, 5, and 6). These results are consistent with rural-to-urban migration driving structural transformation, as the rural Matlab region remains a primarily agrarian economy.

Following the examination of household-level effects, we report individual-level differences in employment outcomes, estimated using equation 6. We separate out effects by cohort given the differential program exposure children had depending on their year of birth (see Table 1. In the individual-level results, we report single-difference estimates for three intervention cohorts. For each cohort, we also report the cohort's mean outcome in the comparison area, and the percent change relative to the cohort comparison mean.

Table 6 reports results at the individual level among men. We find that reductions in urban area work are concentrated among the 1982–1988 cohort, rather than older men (column 1). Unsurprisingly then, reductions in factory, manufacturing, and large-employer work also are concentrated among the same cohort (columns 2 through 5). We do not detect any precise effects across cohorts in terms of office work (column 6).

Finally, we look at the individual-level effects of the program on agricultural employment in column 7. Agricultural employment rises for all cohorts, but is only statistically significant for the cohorts born between 1977 and 1981. While the 1982–1988 cohort is 21 percent more likely to work in agriculture, the effect is not precisely estimated. By contrast, men born between 1977 and 1981 are 61 percent more likely to work in agriculture, and the effect is statistically significant.

To interpret these findings, recall that the 1977–81 cohort in the treatment area only directly experienced the effects of smaller family sizes via the contraception arm of the MCH-FP. By contrast, the cohorts born between 1982 and 1988 experienced both smaller family sizes and improved early-life health from vaccinations, which translated into higher later-life human capital (Barham, 2012; Barham et al., 2021a). If the 1982–88 cohort was less likely to work in manufacturing or offices, but no more likely to work in agriculture, what were they doing? In complementary results, Barham et al. (2021b) find that this cohort of men was more likely to work in professional or semi-professional occupations, though their exact industry of employment is not measured.

Similar to Table 6, we explore the effects of the MCH-FP on individual women in Table A.2. Overall we find more muted effects for women, who are less likely to participate in the labor force. However, we estimate that women born during the family planning-only part of the program (1977–81) are less likely in the treatment area to have ever worked in a factory (column 3). We also find that women born during the family planning and vaccination phase of the program (1982–88) were more likely to work in agriculture than control women of the same cohort (column 7). These results are broadly similar as what we find for men, albeit with smaller magnitudes and less precision.

The effects that we see at the household and individual levels may be washed out by general equilibrium effects at the labor market level. To address this concern, we also estimate the effect of the program aggregated at the village-level. In particular, we trace each individual back to their own or their antecedent's village in 1974. We then compute the fraction of each worker engaging in each activity (e.g., working in a factory) and estimate the effect of the MCH-FP. We show in Table A.3 and find results that are quite similar to our baseline estimates, although the absolute value of the magnitudes falls across the board.

We also see that respondents are not simply working at factories processing food, and thus remaining close to the agricultural sector. Instead, the vast majority work in factories that produce goods such as apparel and textiles, as we show in Figure 4.

5.2 Role of Family Size and Child Gender

A key mechanism that we argue drove program effects is household size. Fauveau (1994), Joshi and Schultz (2013), and Barham et al. (2021b) have all found significant effects of the MCH-FP in reducing fertility. We also estimate the effect of the program on the number of men and women born during the experimental period, with results shown in Table A.4. Consistent with the earlier research, we find the program reduced household size. In particular, we find the program reduced the number of males per household aged 24 to 34 by 16 percent, and decreased the number of females per household in the same age range by 9 percent.[8]

Next, to understand how population pressures within the household contributed to structural transformation, we estimate how the number of children per household born during the experimental period affected those children's later-life sectoral employment choices. In particular, we estimate an equation of the form

$$Y_h = \alpha_0 + \alpha_1 T_h + \alpha_2 Num.\ males\ age\ 24\ to\ 34_h + \alpha_3 Num.\ females\ age\ 24\ to\ 34_h + \gamma X_h + \epsilon_h \tag{7}$$

where, relative to our baseline specification, we add terms for the number of children in the household born during the experimental period, differentiated by gender. We expect that households with more children will be more likely to send a child to work in an urban area

[8]Note that these effect sizes are smaller than those reported in Joshi and Schultz (2013) and Barham et al. (2021b). This is because for the present estimation at the household level, we are not subsetting to families most likely to have children, i.e., by the age of the household head. Therefore, we have some households, for example, with exclusively older individuals in the MHSS1 who had no children, and this drives down the average effect we estimate.

in a non-agricultural sector. Since men are more likely to participate in the labor market, we expect larger effects for households with more males.

We stress that the coefficients α_2 and α_3 in equation 7 cannot be interpreted causally, since the number of children born during the experimental period was directly effected by the treatment. Nevertheless, we estimate equation 7 to provide suggestive evidence regarding the mechanism driving structural transformation.

We present our results in Table 7. We find that the treatment coefficient falls modestly when controlling for the number of children in the household who were born during the experimental period, relative to the results in Table 4. Consistent with intrahousehold competition driving migration decisions, we find that having more children makes it more likely that the household will have a member working in agriculture (column 1), working for a large employer (column 2), working in an office (column 3), or working in manufacturing (columns 4–6). For each of the aforementioned outcomes, with the exception of working in an office, the effect of the program conditional on not having children during the experimental period remains statistically and economically significant.

5.3 Agricultural Adjustment

We next examine household-level effects of the program on agriculture in Tables 8 and 9. These results assess Predictions 3 and 4 from our theoretical model in Section 2.

Table 8 reports estimates of the effect of the MCH-FP program on the extensive margin of farming and the number of acres owned in 1996 from the MHSS1 survey in 2012–2014 from the MHSS2 survey. The program had negligible effects on farming in 1996 (columns 1–2 of Table 8). In particular, treated households were no more likely to farm than comparison households in 1996 (column 1). We also do not detect any statistically significant medium-term effect of the program on the number of acres owned per capita (column 2).

By contrast, the program induced treated households to remain in farming relative to control households. By 2014, treatment area households were 6 percentage points more likely to farm relative to comparison area households (column 3), consistent with our theoretical predictions. Households in both areas owned a similar number of acres per member (column 4).

We interpret the differing effects between medium- and long-run as being driven by the age and life stage of the treated children and their role in family farming practices. For example, children affected by the MCH-FP were likely not contributing substantially to the household's farm by the time of the 1996 survey.

To understand how agriculture is affected by smaller household sizes, we explore differences in the use of labor-substituting crop inputs. To do so, we first categorize crops into labor intensive and non-labor intensive crops using our detailed crop-level data on inputs. We hypothesize that treatment households will have a greater need to adopt labor-saving technology and intermediate goods due to their smaller family size.

To categorize each of our 11 observed crops by labor intensity, we compute the ratio of land cultivated to the number of hours worked by the family on the household's cropland. The six most labor intensive crops (jute, vegetables, paddy aus, maize, wheat, and crops listed as "other") we consider in columns 1, 3, and 5; and the least labor intensive crops (dal, mustard, paddy boro, paddy aman, and potatoes) we consider in columns 2, 4, and 6.[9]

We present our estimates of input use by crop labor intensity in Table 9. We find that treated households are 15 percentage points more likely to use high-yield seeds for labor intensive crops (column 1), while they are no more likely to use high-yield variety seeds for non-labor intensive crops (column 2). We also find that treated households are more likely to use capital for their labor-intensive crops, although the difference is not statistically significant (column 3). Finally, treated households spend about $30 more (a 32 percent increase) on crop inputs purchased in the market relative to control households. These inputs include seeds, fertilizer, pesticides, irrigation, tilling, and labor.Unfortunately, the MHSS2 survey does not allow us to separate market-purchased labor from non-labor inputs. Our results are consistent with households switching away from household labor and into labor-saving technology and inputs as a result of a reduction in household size.

Finally, we test our theoretical prediction that the value of output per acre should not change as a result of the program. To compute the value of output, we first need data on crop prices. Lacking farmgate prices for each household in the MHSS2 data, we instead draw upon the Bangladesh statistical yearbooks for 2012 through 2014. These yearbooks, however, list prices at the variety level (e.g., coarse or fine paddy boro), not the crop level (e.g., paddy boro). Hence we take prices in two ways: either the minimum price within crop across varieties, or the maximum.

We show our results in Table A.5, estimated on the subset of households which grow crops. In columns 1 and 2 we look at the effect on potential revenue per acre, while we estimate the effect on profits per acre in columns 3 and 4. Across all outcomes, we can not statistically rule out a null effect. If anything, the effects are negative. This result obtains despite the positive impact of the MCH-FP on human capital accumulation (Barham, 2012; Barham et al., 2021a), but is consistent with our discussions with farmers in the region who report little role for "smarts" in raising agricultural output per acre.

[9]Paddy aman, paddy boro, and paddy aus are all varieties of rice.

5.4 Robustness of Results

We finally explore the robustness of our main results above to variations in sampling, specification, and variable construction.

In our baseline sectoral employment results, shown in Table 4, we construct our dependent variables as equal to one if at least one member of the household engages in the activity (e.g., works in a factory). However, such an approach may hide significant heterogeneity across households in terms of how many household members engage in the activity. To address this concern, we re-estimate equation 5, but construct dependent variables as the fraction of household members engaging in the activity. We show our results in Table A.6. The conclusions we draw remain the same: the MCH-FP program induced recipient households to be less likely to have members working in factories, offices, or with big employers (columns 1–5), but more likely to farm and work in agriculture (columns 6 and 7).

We next address potential concerns about our household-level treatment assignment. In our baseline treatment assignment, we consider a household treated if the household head could be traced back to a treatment village in 1974. However, households may have mixed treatment status, with some treated and some control members. To gauge the sensitivity of our results to the way we assign household treatment status, we alternatively compute the fraction of household members treated. We show our results in Table A.7. Our results are nearly the same as in our baseline specification.

6 Conclusion

This paper provides the first direct empirical evidence on the effects that the demographic transition has on structural transformation. We causally identify these effects by studying the impact of a consequential contraception and vaccination program in rural Bangladesh. The program exogenously accelerated the demographic transition for treatment villages, and led to less out-migration to urban centers. By contrast, in the control area, in which labor market competition was fiercer due to the higher population, individuals left rural Bangladesh to find factory or office work in urban areas.

Our findings are broadly consistent with recent research on open economy models of structural transformation, in which certain kinds of technological change may inhibit structural transformation (Bustos et al., 2016).

Our results imply that the demographic transition slows down structural transformation. We stress, however, that treated households reveal preferred to stay in agriculture, as they faced the same migration options as the control households. Indeed, urban and employment disamenties are a substantial cost to rural-to-urban migrants in many developing country settings (Imbert and Papp, 2020).

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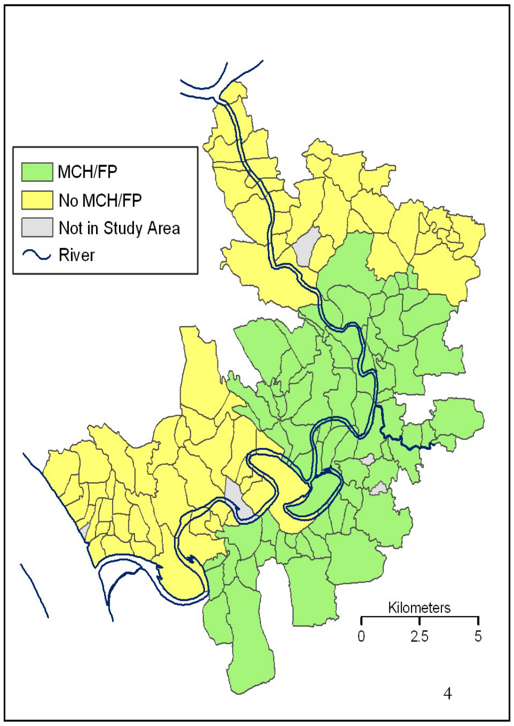
Figure 1: Bangladesh's Crude Birth Rate and Agricultural Employment Share

Figure 2: Map of Matlab Study Area



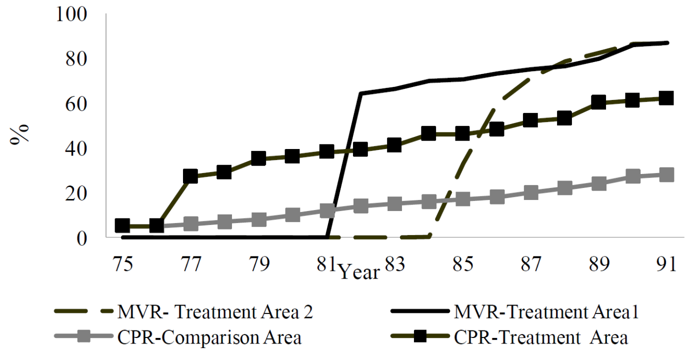
Notes: Villages in green are within the treatment area while those in yellow are in the comparison area. For more details on the program rollout, see Table 1.

Table 1: MCH-FP Interventions by Cohort

Birth year	Age in 2012	Program Eligibility
Oct. 1977–Feb. 1982	31–34	Family planning and maternal health interventions: mothers eligible for family planning, tetanus toxoid vaccine, and folic acid and iron in last trimester of pregnancy.
March 1982–Dec. 1988	24–30	Child health interventions added
March 1982–Oct. 1985	27–30	Interventions added in half the treatment area: children under age five eligible for measles vaccination
Nov. 1985–Dec. 1988	24–26	Interventions extended to entire treatment area: Children under age five eligible for all vaccines (measles, DPT, polio, tuberculosis), vitamin A supplementation, and nutrition rehabilitation for children at risk starting in 1987.
Any other birth year	≤ 24 or ≥ 35	No effect except indirectly, e.g., through sibling competition.

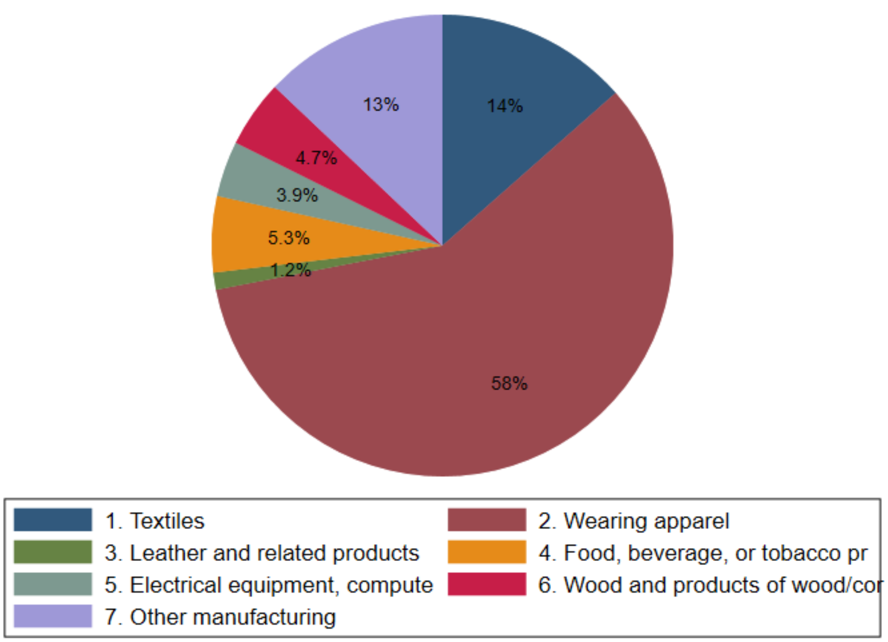
Notes: This table is based on Table 1 of Barham (2012) and Table A1 of Barham et al. (2021b)

Figure 3: Trends in contraceptive prevalence rate (CPR) and measles vaccination rates (MVR) for children 12-59 months by calendar year



Source: Replicated from Figure 2 in Barham et al. (2021b)

Figure 4: Products Produced at Factories by Matlab Workers (if ever worked in factory)



Notes: The figure shows the shares of each product produced at factories that respondents had ever worked in. The question on factory products was limited to workers under 60 years of age who worked at least 20 days in a factory employing at least 30 people.

Table 2: Baseline Balance (MHSS1 Household-level)

	Treatment Area		Comparison Area		Difference in Means		
	Mean	SD	Mean	SD	Diff.	T-stat	Diff./SD
Land size 1982 (decimals)	11.06	20.22	11.50	21.53	-0.43	-0.49	-0.02
Bari size	8.82	9.60	8.04	10.22	0.79	1.65	0.08
Family size	7.00	3.58	6.85	3.82	0.15	1.09	0.04
Wall tin or tin mix (=1)	0.32	0.57	0.32	0.61	0.00	0.04	0.00
Tin roof (=1)	0.83	0.52	0.83	0.56	-0.00	-0.02	-0.00
Number of boats	0.66	1.06	0.67	1.12	-0.01	-0.28	-0.01
Owns a lamp (=1)	0.65	0.57	0.61	0.61	0.05	1.18	0.07
Owns a watch (=1)	0.16	0.39	0.15	0.41	0.02	0.69	0.04
Owns a radio (=1)	0.08	0.29	0.08	0.31	0.00	0.22	0.01
Number of rooms	0.21	0.11	0.21	0.12	0.01	1.19	0.05
Number of cows	1.44	1.92	1.29	2.05	0.15	1.64	0.07
Latrine (=1)	0.82	0.72	0.86	0.77	-0.04	-1.43	-0.05
Drinking water, tubewell (=1)	0.33	0.77	0.16	0.82	0.17	4.16	0.20
Drinking water, tank (=1)	0.39	1.37	0.32	1.45	0.07	1.32	0.05
HH head years of education	2.46	3.28	2.04	3.49	0.43	2.35	0.12
HH head works in agriculture (=1)	0.59	0.67	0.59	0.72	0.00	0.08	0.00
HH head works in fishing (=1)	0.05	0.34	0.07	0.36	-0.01	-0.73	-0.03
HH head age	47.17	12.74	46.34	13.56	0.83	1.55	0.06
HH head spouse's years of education	0.85	2.13	0.67	2.27	0.18	1.65	0.08
HH head spouse's age	36.76	12.43	36.11	13.23	0.65	1.16	0.05
HH head works in business (=1)	0.13	0.42	0.10	0.45	0.03	1.24	0.07
1996 HH Head Muslim	0.84	0.35	0.96	0.38	-0.12	-3.51	-0.32

Notes: The sample includes MHSS1 households which had at least 1 member appear in the MHSS2 survey. Unless otherwise noted, household characteristics come from the 1974 census. MHSS1 household baseline (1974) characteristics are traced back from the MHSS1 household head. Standard deviations (SD) are clustered at the treatment village level. There are 1,209 treatment area households and 1,371 comparison area households. Standard deviations in column 7 are based on the comparison group.

Table 3: ITT Effects of MCH-FP on Medium-term Employment Sector

	(1)	(2)	(3)
	Work in agr.	Ever worked in agr.	Tot. earnings agr.
Treated	0.04	0.01	386.57
	(0.03)	(0.03)	(403.71)
% chg. rel. to mean	6.7	1.3	16.0
Mean	0.54	0.63	2411.95
Embankment controls	Y	Y	Y
Baseline controls	Y	Y	Y
Observations	2580	2580	2580

Notes: The table presents estimates of the effect of the MCH-FP on 1996 outcomes at the MHSS1 household-level. Standard errors are clustered by pre-program village. Agricultural employment is defined based on occupation codes (=1 if agriculturist, agricultural laborer, or fisherman). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: ITT Effects of MCH-FP on Long-term Employment Sector: Household-Level

	(1)	(2)	(3)	(4)	(5)	(6)
	Work in agr.	Employer size $\geq$ 100	Work in office	Work in factory	Ever work in factory	Own manufacturing enterprise
Treated	0.05*	-0.06***	-0.05**	-0.05***	-0.10***	-0.03**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	2580	2580	2580	2580	2580	2580
Adjusted R^2	0.018	0.008	0.006	0.006	0.012	0.007
% chg. rel. to mean	7.9	-22.3	-16.0	-23.1	-22.3	-30.6
Mean	0.59	0.29	0.31	0.22	0.45	0.09
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y

Table 5: ITT Effects of MCH-FP on Long-term Employment Mediated by Urbanicity: Household-Level

	(1)	(2)	(3)	(4)	(5)	(6)
	Work in agr.	Employer size $\geq$ 100	Work in office	Work in factory	Ever work in factory	Own manufacturing enterprise
Treated=1 $\times$ Work in urban area=0	0.08***	-0.28***	-0.24***	-0.19***	-0.30***	-0.04**
	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Treated=1 $\times$ Work in urban area=1	0.02	0.09***	0.09***	0.06**	0.05	-0.02
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Observations	2580	2580	2580	2580	2580	2580
Adjusted R^2	0.019	0.089	0.067	0.053	0.069	0.007
Comparison mean	0.59	0.29	0.31	0.22	0.45	0.09
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y

Table 6: ITT Effects of MCH-FP on Urbanicity, Sector, and Employer Size: Individual-Level (men)

	(1) Work in urban area	(2) Work in factory	(3) Ever work in factory	(4) Own manufacturing enterprise	(5) Employer size $\geq$ 100	(6) Work in office	(7) Work in agr.
Treatment*1982-1988	-0.132***	-0.0863***	-0.114***	-0.00360	-0.0901***	-0.0245	0.0233
	(0.0368)	(0.0205)	(0.0344)	(0.0109)	(0.0275)	(0.0302)	(0.0254)
Treatment*1977-1981	-0.0539	0.00294	-0.0250	-0.0236	-0.0249	-0.0510	0.0992***
	(0.0539)	(0.0252)	(0.0398)	(0.0157)	(0.0328)	(0.0393)	(0.0379)
Treatment*Not born 1977-88	0.00498	0.0125	-0.0399*	0.000105	0.0105	-0.00566	0.0447
	(0.0337)	(0.0105)	(0.0234)	(0.00930)	(0.0149)	(0.0221)	(0.0288)
% Chg., Treat×(Born 1982–88)	-28.12	-81.25	-44.24	-16.74	-58.08	-12.93	21.44
% Chg., Treat×(Born 1977–81)	-10.85	4.11	-9.67	-77.77	-17.97	-27.33	60.98
% Chg., Treat×(Born Pre-1977 or Post-1988)	2.00	29.68	-29.80	0.53	18.07	-7.09	12.85
Mean if born 1982-88	0.47	0.11	0.26	0.02	0.16	0.19	0.11
Mean if born 1977-81	0.50	0.07	0.26	0.03	0.14	0.19	0.16
Mean if born pre-1977 or post-1988	0.25	0.04	0.13	0.02	0.06	0.08	0.35
Baseline controls	Y	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y	Y
Observations	2819	2819	2819	2819	2819	2819	2819

Table 7: ITT Effects of MCH-FP on Long-term Employment by Family Size and Gender Composition: Household-Level

	(1) Work in agr.	(2) Employer size $\geq$ 100	(3) Work in office	(4) Work in factory	(5) Ever work in factory	(6) Own manufacturing enterprise
Treated	0.06**	-0.05**	-0.03	-0.04**	-0.08***	-0.02*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Num. males age 24-34	0.07***	0.09***	0.11***	0.05***	0.12***	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Num. females age 24-34	0.07***	0.05***	0.02*	0.04***	0.05***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2580	2580	2580	2580	2580	2580
Adjusted R^2	0.050	0.049	0.052	0.026	0.062	0.015
% chg. rel. to mean	10.5	-16.7	-10.4	-18.5	-17.8	-26.4
Mean	0.59	0.29	0.31	0.22	0.45	0.09
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y

Table 8: ITT Effects of MCH-FP on Agriculture in MHSS1 and MHSS2

	MHSS1 (1996)		MHSS2 (2012-2014)	
	(1)	(2)	(3)	(4)
	=1 if household farms	Acres owned per cap.	=1 if household farms	Acres owned per cap.
Treated	0.01	-0.04	0.06***	0.01
	(0.03)	(0.03)	(0.02)	(0.02)
Observations	2580	2580	2580	2580
Adjusted R^2	0.062	0.006	0.018	0.020
Mean	0.65	0.20	0.76	0.25
% chg. rel. to mean	2.1	-21.0	7.3	2.9
Embankment dummies	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y

Notes: The table presents estimates of equation 5 at the MHSS1 household-level from 1996 (columns 1 and 2) and 2014 (columns 3 and 4). Variable means refer to the comparison group. Embankment control assigned based on the MHSS1 household head's village location. Standard errors are clustered by pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: ITT Effects of MCH-FP on Crop Input Use by Crop Labor Intensity

	Use of High-Yield Seeds		Use of Capital for Crop		Cost of market inputs	
	(1) HH grew labor intensive crops	(2) HH grew non-labor intensive crops	(3) HH grew labor intensive crops	(4) HH grew non-labor intensive crops	(5) HH grew labor intensive crops	(6) HH grew non-labor intensive crops
Treated	0.151***	0.006	0.026	0.011	30.074**	-7.269
	(0.042)	(0.046)	(0.017)	(0.008)	(13.351)	(36.019)
Observations	785	1346	785	1346	785	1346
Adjusted R^2	0.038	0.005	0.005	0.014	-0.001	0.030
% chg. rel. to mean	33.6	1.3	2.7	1.1	32.4	-2.7
Mean	0.45	0.46	0.96	0.99	92.87	267.61
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y

Notes: The table presents estimates of equation 5 at the MHSS1 household-level for outcomes measured in 2014. Variable means refer to the comparison group. Standard errors are clustered by pre-program village. Regressions are conditional on the household growing either a labor-intensive crop (columns 1, 3, and 5) or a non-labor-intensive crop (columns 2, 4, and 6). Labor intensive crops are jute, vegetables, paddy aus, other crops, maize, and wheat, while non-labor intensive crops are dal, mustard, paddy boro, paddy aman, and potatoes. Labor intensity is computed as the ratio of acres cultivated for a given crop (including both owned and sharecropped land) to hours worked by family members on the family farm (number of weeks × average weekly hours) for households that grew only 1 crop. Market inputs are crop inputs purchased by the household. They are seeds, fertilizer, pesticides, irrigation, tilling, and labor. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix

A Additional Tables

Table A.1: ITT Effects of Consumption Shares by Sector

	(1) Agriculture	(2) Manufacturing	(3) Services
Treated	0.01	0.00	-0.01
	(0.01)	(0.00)	(0.02)
Observations	2575	2575	2575
Adjusted R^2	-0.001	0.002	-0.001
% chg. rel. to mean	1.4	0.3	-2.3
Mean	0.49	0.19	0.35
Embankment dummies	Y	Y	Y
Baseline controls	Y	Y	Y

Notes: The table presents estimates of equation 5 for consumption shares measured in the MHSS2 aggregated at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Baseline and embankment control variables assigned based on the MHSS1 household head's traceback household. Consumption goods classified into sectors based on (?). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively

Table A.2: ITT Effects of MCH-FP on Long-term Employment: Individual-Level (women)

	(1) Work in urban area	(2) Work in factory	(3) Ever work in factory	(4) Own manufacturing enterprise	(5) Employer size $\geq$ 100	(6) Work in office	(7) Work in agr.
Treatment*1982-1988	0.01	0.00	-0.03	0.01	-0.01	-0.01	0.06***
	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
Treatment*1977-1981	-0.02	-0.01	-0.09**	0.01	-0.03	-0.01	-0.01
	(0.03)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)	(0.04)
% Chg., Treat×(Born 1982–88)	10.26	1.14	-25.31	22.06	-17.64	-21.86	39.62
% Chg., Treat×(Born 1977–81)	-19.23	-23.20	-67.96	38.32	-39.07	-22.10	-7.18
% Chg., Treat×(Born Pre-1977 or Post-1988)	-2.28	11.38	0.89	6.77	-3.86	-12.69	4.53
Mean if born 1982-88	0.12	0.07	0.13	0.03	0.08	0.04	0.15
Mean if born 1977-81	0.11	0.06	0.13	0.03	0.07	0.04	0.19
Mean if born pre-1977 or post-1988	0.25	0.04	0.13	0.02	0.06	0.08	0.35
Baseline controls	Y	Y	Y	Y	Y	Y	Y
Embankment control							
Observations	3322	3322	3322	3322	3322	3322	3322

Table A.3: ITT Effects of MCH-FP on Long-term Employment: Village-level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Work in urban area	Work in factory	Ever work in factory	Own manufacturing enterprise	Employer size $\geq$ 100	Work in office	Work in agr.
Treated village	-0.01*	-0.01***	-0.02***	-0.00	-0.01**	-0.01	0.03***
	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Observations	148	148	148	148	148	148	148
Mean	0.08	0.03	0.08	0.01	0.04	0.04	0.13
% chg. rel. to mean	-14.93	-29.57	-23.35	-8.89	-22.05	-13.18	26.11
Embankment dummy	Y	Y	Y	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y	Y	Y	Y

Table A.4: ITT Effects of MCH-FP on Household Size and Composition

	(1) Number of Men Age 24-34	(2) Number of Women Age 24-34
Treated	-0.14***	-0.07*
	(0.04)	(0.04)
Observations	2580	2580
Adjusted R^2	0.006	-0.001
Mean	0.8	0.7
% chg. rel. to mean	-17.37	-9.78
Baseline controls	Y	Y
Controlling for embankment	Y	Y

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.5: ITT Effects of MCH-FP on Revenue and Profits per Acre

	(1) Revenue per acre (min. price)	(2) Revenue per acre (max. price)	(3) Profit per acre (min. price)	(4) Profits per acre (max. price)
Treated	-3.807	-22.39	-14.57	-32.77
	(43.12)	(139.5)	(53.39)	(141.2)
% chg. rel. to mean	-0.9	14.5	-2.1	-39.6
Mean	446.13	-154.24	683.45	82.84
Embankment controls	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y
Estimation method	OLS	OLS	OLS	OLS
Observations	1411	1411	1411	1411

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Standard errors are clustered by pre-program village. Prices derived from the national Bangladeshi statistical yearbooks 2012-2014. Minimum prices are the minimum price listed in the yearbook for a given year within a crop type (e.g., Paddy Aman) amongst all varieties of that crop type (e.g., coarse or fine). Profits net of imputed family farm labor costs. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.6: ITT Effects of MCH-FP on Long-term Employment: Dependent Variables Averaged within Household

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Work in factory	Ever work in factory	Own manufacturing enterprise	Employer size $\geq$ 100	Work in office	Work in agr.	HH farms
Treated	-0.0144***	-0.0328***	-0.00838*	-0.0198***	-0.0156**	0.0456***	0.106***
	(0.00547)	(0.00922)	(0.00430)	(0.00673)	(0.00684)	(0.0142)	(0.0161)
Adjusted R^2	0.008	0.020	0.018	0.012	0.007	0.020	0.056
Mean	0.1	0.1	0.0	0.1	0.1	0.2	0.4
% chg. rel. to mean	-26.33	-23.55	-40.65	-26.19	-19.03	21.09	24.05
Baseline controls	Y	Y	Y	Y	Y	Y	Y
Observations	2580	2580	2580	2580	2580	2580	2580

Table A.7: ITT Effects of MCH-FP on Long-term Employment by Fraction of Household Treated

	(1) Work in urban area	(2) Work in factory	(3) Ever work in factory	(4) Own manufacturing enterprise	(5) Employer size $\geq$ 100	(6) Work in office	(7) Work in agr.
% HH treated	-0.04	-0.04**	-0.09***	-0.03**	-0.06***	-0.05**	0.06**
	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
Observations	2580	2580	2580	2580	2580	2580	2580
Adjusted R^2	0.023	0.009	0.028	0.014	0.016	0.006	0.021
% chg. rel. to mean	-6.7	-20.8	-19.6	-42.8	-20.6	-17.2	10.4
Mean	0.63	0.21	0.45	0.08	0.28	0.31	0.59
Embankment dummies	Y	Y	Y	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y	Y	Y	Y

B Theoretical Appendix

In this section, we provide the derivations necessary to solve the model and generate the predictions presented in Section 2.

B.1 Solving the Model

First, we solve for the marginal products:

$$MPL_a = A_g(1-\omega)\theta L_g^{-\frac{1}{\epsilon}} \left[\cdot\right]^{\frac{\theta\epsilon}{\epsilon-1}-1} T_g^{1-\theta} \tag{B.1}$$

$$MPZ_a = A_g\omega\theta Z_g^{-\frac{1}{\epsilon}} \left[\cdot\right]^{\frac{\theta\epsilon}{\epsilon-1}-1} T_g^{1-\theta}$$

$$MPL_f = A_f(1-\alpha)\left(\frac{Z_f}{L_f}\right)^{\alpha}$$

$$MPZ_f = A_f\alpha\left(\frac{L_f}{Z_f}\right)^{1-\alpha}$$

Next, we consider the ratios of marginal products, where

$$\frac{MPL_a}{MPZ_a} = \frac{MPL_f}{MPZ_f}$$

which yields

$$\frac{1-\omega}{\omega}\left(\frac{Z_g}{L_g}\right)^{\frac{1}{\epsilon}} = \frac{1-\alpha}{\alpha}\frac{Z_f}{L_f} \tag{B.2}$$

Next, use the fact that $P_z = P_f MPZ_f$ to get

$$\frac{L_f}{Z_f} = \left(\frac{P_z}{A_f\alpha}\right)^{\frac{1}{1-\alpha}} \tag{B.3}$$

Now, plug equation B.3 into equation B.2 to get

$$\frac{Z_g}{L_g} = \left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon}{1-\alpha}} \tag{B.4}$$

Now we can pin down the wage from manufacturing:

$$\begin{aligned} w &= P_f MPL_f \\ w &= P_f A_f (1-\alpha)\left(\frac{P_z}{A_f \alpha}\right)^{\frac{\alpha}{1-\alpha}} \end{aligned} \tag{B.5}$$

Plug equation B.4 into the CES portion of the agriculture production function:

$$\left[\omega Z_g^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) L_g^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\theta\epsilon}{\epsilon-1}} = L_g^{\theta}\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}} + 1-\omega\right]^{\frac{\theta\epsilon}{\epsilon-1}} \tag{B.6}$$

Finally, solve for L_g using equations B.1, B.5, and B.6:

$$L_g^* = \left[\frac{A_g}{A_f}\frac{(1-\omega)\theta}{(1-\alpha)}\frac{\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}} + (1-\omega)\right]^{\frac{\theta\epsilon}{\epsilon-1}-1}}{P_f\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\alpha}{1-\alpha}}} T^{1-\theta}\right]^{\frac{1}{\frac{1}{\epsilon}+1-\theta}} \tag{B.7}$$

Plugging equation B.7 into equation B.4, we obtain:

$$Z_g^* = \left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon}{1-\alpha}} L_g^*$$

Hence, agricultural output is:

$$\begin{aligned} Q_g^* &= A_g\left[\omega\left\{\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon}{1-\alpha}} L_g^*\right\}^{\frac{\epsilon-1}{\epsilon}} + (1-\omega)\left(L_g^*\right)^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\theta\epsilon}{\epsilon-1}} T^{1-\theta} \\ &= A_g\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}}\left(L_g^*\right)^{\frac{\epsilon-1}{\epsilon}} + (1-\omega)\left(L_g^*\right)^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\theta\epsilon}{\epsilon-1}} T^{1-\theta} \\ &= A_g\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}} + (1-\omega)\right]^{\frac{\theta\epsilon}{\epsilon-1}}\left(L_g^*\right)^{\theta} T^{1-\theta} \end{aligned}$$

To solve for L_f^*, simply plug equation B.7 into the labor market clearing condition, $L = L_g + L_f$.

B.2 Theoretical Predictions

We next derive the theoretical predictions from Section 2.3.

Prediction 1: *An increase in L increases the share of workers employed in the agricultural sector*

Proof:

$$\frac{\partial L_g^*/L}{\partial L} = -\frac{1}{L^2}\left[\frac{A_g}{A_f}\frac{(1-\omega)\theta}{(1-\alpha)}\frac{\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}}+(1-\omega)\right]^{\frac{\theta\epsilon}{\epsilon-1}-1}}{P_f\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\alpha}{1-\alpha}}}T^{1-\theta}\right]^{\frac{1}{\frac{1}{\epsilon}+1-\theta}} < 0$$

Prediction 2: *An increase in L decreases the share of workers employed in manufacturing*

Proof:

$$\frac{\partial L_f^*/L}{\partial L} = \frac{1}{L^2}\left[\frac{A_g}{A_f}\frac{(1-\omega)\theta}{(1-\alpha)}\frac{\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}}+(1-\omega)\right]^{\frac{\theta\epsilon}{\epsilon-1}-1}}{P_f\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\alpha}{1-\alpha}}}T^{1-\theta}\right]^{\frac{1}{\frac{1}{\epsilon}+1-\theta}} > 0$$

Prediction 3: *An increase in L leads to more intensive use of intermediate inputs in agriculture*

Proof:

We consider intermediate use at the household level, i.e., $\frac{Z_g}{L}$. Hence,

$$\frac{\partial\frac{Z_g^*}{L}}{\partial L} = -\frac{1}{L^2}\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon}{1-\alpha}}L_g^* < 0$$

Prediction 4: *An increase in L leads to no change to the value of output per acre*

Proof:

Output per acre is

$$\frac{P_g^*Q_g^*}{T_g^*} = P_g^*A_g\left[\omega\left(\frac{1-\omega}{\omega}\frac{\alpha}{1-\alpha}\right)^{\epsilon-1}\left(\frac{P_z}{A_f\alpha}\right)^{\frac{\epsilon-1}{1-\alpha}}+(1-\omega)\right]^{\frac{\theta\epsilon}{\epsilon-1}}\left(L_g^*\right)^{\theta}T^{-\theta}$$

which does not rely on L, so

$$\frac{\partial\frac{P_g^*Q_g^*}{T_g^*}}{\partial L} = 0$$

Prediction 5: *An increase in L leads to no change in wages*

Proof:

Wages, defined by equation B.5, are not dependent on the population L, so

$$\frac{\partial w}{\partial L} = 0$$