The Demographic Transition and Structural Transformation*

Tania Barham — CU-Boulder
Randall Kuhn — UCLA
Brett McCully — Collegio Carlo Alberto
Patrick Turner — Notre Dame

May 23, 2024

Abstract

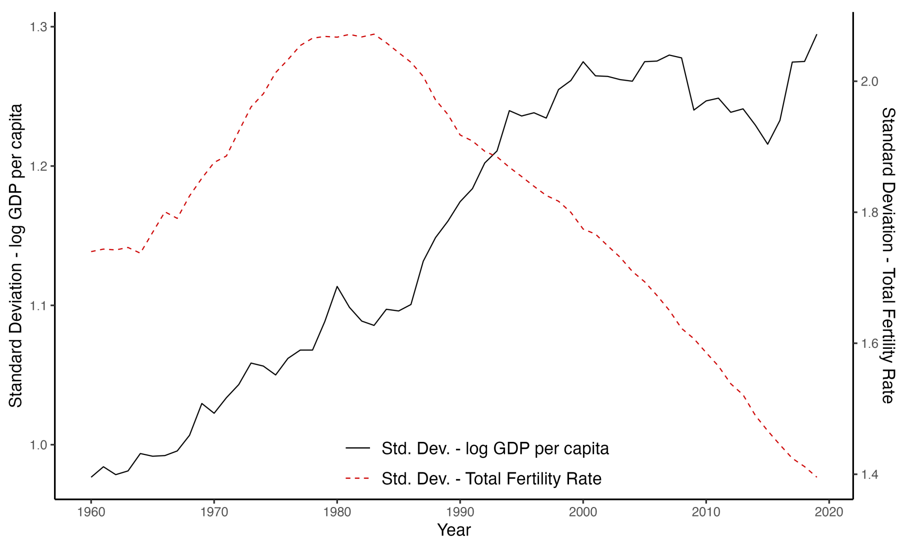
We study the effect of the demographic transition on structural transformation. In a panel of countries, higher fertility in past decades reduces the agricultural employment share today. To better understand the mechanisms driving this cross-country relationship, we leverage the quasi-random placement of a program which reduced fertility and early-life mortality in rural Bangladesh. We use rich data that allows us to follow treated and control households 35 years later. We find that the demographic transition slows down structural transformation. Increased labor market and intrahousehold competition drove control households—which were relatively larger due to the program—to send workers to urban areas in search of employment in manufacturing. By contrast, the program led to a 5 percentage point rise in the share of household working hours spent in agriculture and a 3 percentage point fall in manufacturing. Treated farmers adjusted to smaller household sizes by more intensively using labor-substituting technology, capital, and intermediate inputs.

*We are grateful to Suresh Naidu for helpful discussions. We thank seminar participants at UCLA, Collegio Carlo Alberto, the University of San Francisco, and the Free University of Bozen-Bolzano, as well as conference participants at IPC2021, PAA2023, and the Firms, Labor Markets, and Development workshop at EUI for helpful comments. We are grateful to Antonio Curcio, Ilaria Malisan, Jessica Mancuso, and Elizabeth Sorensen Montoya for helpful research assistance. We acknowledge financial support from CEPR STEG. 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

Neoclassical growth models have anticipated income convergence between historically rich and poor nations (Solow, 1956), yet income per capita has not substantially converged between poor and rich countries since 1960 (Johnson and Papageorgiou, 2020).[1] In contrast, fertility rates are converging as shown in Figure 1, thanks in part to the widespread diffusion of family planning technologies, with virtually every country on earth now experiencing a fall in fertility rates (Delventhal et al., 2021). Given the importance of surplus labor in macroeconomic models of structural transformation and economic growth (Lewis, 1954; Gollin et al., 2002, 2007; Leukhina and Turnovsky, 2016; Jones, 2022), does a faster demographic transition affect the pace of industrialization?

Figure 1: Standard deviation of GDP per capita and total fertility rate



Notes: The graph plots the yearly standard deviation of GDP per capita (solid black line) and the total fertility rate (dashed red line). Birth rates come from database assembled by Delventhal et al. (2021), and GDP per capita data from the Penn World Tables.

This causal relationship is essential to understand as medical technology facilitating falling birth rates has spread around the globe in recent decades, resulting in faster demographic transitions than those experienced by currently industrialized countries when they transitioned (Delventhal et al., 2021). The quicker speed of the demographic transition may outpace structural transformation, 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

[1]Convergence has begun to take place at modest speeds since the 1990s (Kremer et al., 2022; Patel et al., 2021).

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. We do so in two exercises. First, we use a cross-country panel to explore the relationship between agricultural employment share and the total fertility rate 30 years prior. Second, 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. We leverage highly detailed microdata collected across four decades in rural Bangladesh in order to understand long-run effects. We estimate how the intervention affected sectoral employment choice and how agricultural households responded 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. We consider two channels resulting from the demographic transition: (i) reduced population growth as fertility falls, and (ii) higher human capital as children grow up healthier and parents prioritize child quality over quantity. The population size channel results in a larger share of laborers work in agriculture as the supply of arable land is fixed. The effect of human capital depends on the model parameters, but under plausible assumptions the net effect of the demographic transition on industrialization is ambiguous.

We test the model's predictions in two ways. We first estimate a cross-country panel relating lagged fertility rates to the agricultural employment share. In order to obtain causal identification, we instrument for fertility rates using variation in the availability of abortion across countries while controlling for country and year fixed effects. To the extent that the timing of abortion policy changes are exogenous, our instrument is valid. Consistent with our model, we find that higher fertility rates lead to a lower employment share in agriculture decades later.

Our cross-country exercise is suggests that the key mechanisms highlighted by our model hold, and establishes that the relationship may hold across a wide variety in countries in different stages of development. Moreover, the cross-country analysis suggests that general equilibrium effects do not wash out our predicted effects. However, the causal inference in our cross-country panel is lacking, as is the richness of data to assess mechanisms, and hence we turn to a second exercise which ameliorates those drawbacks.

We study 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.[2] 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 several decades to manifest: we detect no economically significant effect of the program on sectoral employment as of 1996. By 2014, we see large effects. Consistent with our theoretical predictions, treated households allocated 23 percent more hours to agriculture, but 17 percent less to manufacturing. We similarly find that entrepreneurship in treated households was stronger in agriculture and weaker in manufacturing relative to control households.

We consider three key channels through which the program effects operated. First, we find that household size is a crucial mechanism through which the program shapes structural transformation. This channel operates through increased intrahousehold and local labor market competition. We find that the more boys born during the program period, the larger the share of household adults in MHSS2 work a non-agricultural job. The gender composition of the household is also important.

Second, we find that rural-to-urban migration is central to the process of structural transformation, as minimal employment growth in the manufacturing or service sector occurs in our rural setting. Instead, workers must migrate to urban centers to leave the agricultural sector. Moreover, we find that some workers were induced to leave the agricultural sector by migrating abroad, where they worked in the service sector.

Third, households allocated workers to sectors based in part on their human capital. We obtain quasi-exogenous variation in human capital by comparing those born during the vaccine arm of the MCH-FP to those born before it, where both groups' parents received the contraception arm of the program. Treated men born during the vaccination phase of the

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

program worked more in the service sector where human capital returns are likely higher. Treated women who received the early childhood vaccination, on the other hand, spent more of their working hours in agriculture.

In the final part of the paper, 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 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 England's industrialization. Another exception is Yin (2021), who leverages 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 the parsing of different mechanisms challenging.

Peters (2022) work on the effect of population size on structural transformation using a semi-endogenous growth model is closer to what we do in this paper. Several notable differences are worth highlighting. First, we document the importance of rural-to-urban migration in mediating our main effects, whereas Peters (2022) shows effects on rural industrialization. Finally, we emphasize the Malthusian mechanism of fixed-factors driving our results rather than the endogenous growth of Peters (2022). Finally, we explore effects at the household- and individual-level, whereas Peters (2022) only has region-level data.

Our evidence on labor-saving input adjustment in the face of thinner labor markets is consistent with research by Hornbeck and Naidu (2014), Clemens et al. (2018), Andersson et al. (2022), and San (2023). In contrast to those studies, we explore the effects of the demographic transition. We also have exceptionally 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 explores patterns in a panel of countries. Section 4 discusses the intervention, data, and context, while Section 5 explains our empirical specifications. Section 6 presents our results and Section 7 concludes.

2 Model

In this section we present a simple model of structural transformation. We consider a small open economy in which goods prices are exogenous. There are two sectors, agriculture and manufacturing, and three factors of production: land, labor, and imported intermediate inputs, which may be durable (i.e., capital) or nondurable.

2.1 Setup

Consider a small open economy that trades agricultural and manufacturing goods with the world economy.[3] The economy has L households, each inelastically supplying one unit of labor. Each households is endowed with h units of human capital, which only is useful in the manufacturing sector.

Production of the manufacturing good follows a Cobb-Douglas production process utilizing imported intermediate inputs Z_m and labor L_m:

$$Q_m = A_m Z_m^{\alpha} (hL_m)^{1-\alpha} \tag{1}$$

for $\alpha \in (0, 1)$, where manufacturing output is denoted by Q_m, Hicks-neutral manufacturing productivity is A_m, and per-capita human capital is denoted by h.

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

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

where Q_g is the quantity of agricultural goods produced, and A_g is Hicks-neutral agricultural productivity. $\epsilon > 0$ 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 accruing to landowners.

The marginal product of labor in agriculture is

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

[3] The small open economy assumption obviates the need for modeling demand. We develop an alternative three-sector model with a non-tradable service sector in Appendix Section B.3 in which we model demand, and show that our main predictions go through. We also show in Table A.1 that the quasi-experimental intervention in Bangladesh that we study did not induce any changes in consumption shares across sector, suggesting that demand-side factors are not driving sectoral reallocations.

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. Given a fixed amount of land T_g, as the number of workers allocated to agriculture L_g increases, the returns to that labor decline.

In the manufacturing sector, the marginal product is

$$MPL_m = A_m(1-\alpha)\left(\frac{Z_m}{L_m}\right)^{\alpha} h^{1-\alpha}$$

where wages serve to pull workers in when human capital rises.

2.2 Equilibrium

Since we are considering a small open economy, prices of goods are exogenous and determined by world markets. Profit maximization implies that the value of marginal products across sectors equal the wage w:

$$p_g MPL_g = w = p_m MPL_m$$

which determines the equilibrium wage,

$$w^* = (1-\alpha)\left(p_m A_m\right)^{\frac{1}{1-\alpha}}\left(\frac{\alpha}{p_z}\right)^{\frac{\alpha}{1-\alpha}} h \tag{3}$$

which is rising in the price of manufacturing goods p_m, manufacturing productivity A_m, and human capital h. In contrast, wages are falling in the price of intermediate inputs p_z. Intuitively, due to the substitutability of workers with imported inputs, firms are able to maintain zero profits only when wages fall as the price of inputs rises.

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

$$\frac{L_g^*}{L} = \left(\Lambda \frac{\left[\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-1}+1\right]^{\frac{\theta\epsilon}{\epsilon-1}-1}}{\left(\frac{\alpha}{1-\alpha}\frac{w^*}{p_z}\right)^{\alpha} h^{1-\alpha}}\right)^{\frac{1}{1-\theta}} \frac{T}{L} \tag{4}$$

where $\Lambda \equiv \frac{(1-\omega)^{\frac{\theta\epsilon}{\epsilon-1}}\theta}{1-\alpha}\frac{p_g}{p_m}\frac{A_g}{A_m}$ is a collection of exogenous parameters.

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

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

$$\frac{Z_g^*}{L} = \left(\frac{\omega}{1-\omega}\frac{w^*}{p_z}\right)^{\epsilon}\frac{L_g^*}{L} \tag{5}$$

For detailed derivations, see Appendix B.1.

2.3 Comparative Statics

We next assess the effect of the demographic transition on sectoral employment. This effect may occur through two distinct channels. First, the demographic transition leads to a long-run net reduction in population growth as fertility falls (Delventhal et al., 2021). Second, it may increase the stock of human capital as early-life mortality and morbidity fall and as parents focus on child quality over child quantity (Becker et al., 1990; Galor and Weil, 2000; Soares, 2005).

Our primary interest is in how these changes wrought by the demographic transition affect sectoral employment. We find contrasting effects of each channel on agricultural employment:

EMPIRICAL PREDICTION 1.

(a) A relatively lower population L will result in an increased share of workers employed in the agricultural sector.

(b) The sign of the effect of a rise in average human capital h on the share of workers employed in the agricultural sector depends on parameter values, as detailed below.

Proof: see Appendix B.2.

In particular, we find that in the model $\frac{\partial L_g/L}{\partial h} < 0$ if and only if the below parameter restriction holds:

$$\frac{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-1}}{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-1}+1} < \frac{1-\epsilon(1-\theta)}{p_z} \tag{6}$$

The term $\left(\frac{\omega}{1-\omega}\right)^{\epsilon}$ captures the productivity of Z relative to L in the agriculture sector and $(w^*/p_z)^{\epsilon-1}$ captures the corresponding relative cost of inputs. This term is equal to 1 with a Cobb-Douglas production function, as $\omega = 0.5$ and $\epsilon = 1$ and as assumed for the manufacturing sector. Hence, the term on the left of inequality (6) indexes the difficulty of substituting between Z and L in agriculture relative to manufacturing and must be between 0 and 1.

On the right-hand side, the term $\epsilon(1-\theta)$ measures the ease of substituting between Z and L in agriculture, weighted by the importance of land, whereas this term equals 1 in manufacturing, where the Cobb-Douglas production function where $\epsilon = 1$ and the land cost share is 0. Hence the numerator $1 - \epsilon(1-\theta)$ measures the difference between the weighted ease of substituting between Z and L between the manufacturing and agricultural sectors. The denominator p_z scales this difference by the cost of input Z.

Inequality (6) is most likely to hold (and hence $\frac{\partial L_g/L}{\partial h} < 0$) when a country is less developed: when manufacturing productivity and human capital are low, so long as the $\epsilon > 1$, as suggested by the estimates of Herrendorf et al. (2015) and Boppart et al. (2023). Hence, the net long-run effect of the demographic transition on industrialization is ambiguous for developing countries, and depends on the parameters which preferences and production, and hence the relative strength of the human capital versus population size effects.

For the most developed countries, on the other hand, the model suggests that both forces shift labor into the agricultural sector. This is because human capital increases essentially free-up labor to move into agriculture one labor is sufficiently productive.[4]

EMPIRICAL PREDICTION 2.

(a) A relatively lower population L results in *more intensive use of intermediate inputs in agriculture per capita.*

(b) The sign of the effect of a rise in average human capital h on the intensity of use of intermediate inputs in agriculture depends on parameter values, as detailed below.

Proof: See Appendix B.2.

We find that

$$\frac{\partial Z_g^*/L}{\partial h} = \epsilon \left(\frac{\omega}{1-\omega} \frac{w^*}{p_z} \right)^{\epsilon - 1} \frac{\partial w^*}{\partial h} L_g^* + \left(\frac{\omega}{1-\omega} \frac{w^*}{p_z} \right)^{\epsilon} \frac{\partial L_g^*}{\partial h}$$

where the first term in the sum represents the shift in inputs Z use due to changes in wages (holding fixed the labor allocation) as a result of the change in human capital, and the second term is driven by changes in labor employed in agriculture (holding the wage fixed) now that efficiency units of labor in manufacturing have increased. Each term is weighted by a coefficient capturing the relative benefits and costs of substituting inputs Z for labor L. Hence, $\frac{\partial Z_g/L}{\partial h} < 0$ when $\frac{\partial L_g/L}{\partial h} < 0$ and $\epsilon \left(\frac{\omega}{1-\omega} \frac{w^*}{p_z} \right)^{\epsilon-1} \frac{\partial w^*}{\partial h} L_g^* > - \left(\frac{\omega}{1-\omega} \frac{w^*}{p_z} \right)^{\epsilon} \frac{\partial L_g^*}{\partial h}$. Note that this is a weaker condition than that on changes in the agricultural employment share: $\frac{\partial Z_g/L}{\partial h}$ can only be negative when $\frac{\partial L_g/L}{\partial h} < 0$, but this is not sufficient.

[4]Because developed countries are on the technological frontier, an endogenous growth model may be more appropriate however, which may instead pull workers into the innovative sector.

In what follows, we first test prediction 1 using cross-country data. Then we test both predictions 1 and 2 using a quasi-experiment in the country of Bangladesh.

3 Cross-Country Analysis

We start to test our theory by looking at variation across countries in fertility rates and the agriculture employment share (AES).

The cross-country analysis has two main advantages. First, we establish that the relationship predicted by our theory holds even when accounting for general equilibrium forces at the country level. Second, we can establish whether this relationship holds for a broad set of countries at different points on the development path and with widely varying cultural norms around fertility.

The drawbacks to the cross-country analysis are also twofold. First, that the causal identification we can obtain using cross-country variation is weak, and second, that we cannot adequately explore which mechanisms drive the aggregate relationship using such data. We address these shortcomings using a quasi-experiment in Bangladesh in the next section.

3.1 Cross-Country Data

To do so we construct a cross-country panel dataset of agricultural employment share and the total fertility rate (TFR). We obtain data on the total fertility rate, which measures the total number of children born to the average woman in a country throughout her lifetime, from the United Nations. The underlying data are computed from population censuses, vital registries, and nationally representative surveys. Interpolation is used to fill in gaps when data is not available otherwise.

To measure agricultural employment share we rely on several datasets. Our primary source for AES data is Wingender (2014), who compiles data and estimates of AES for 169 countries between 1900 and 2010 (although most countries cannot be covered for the entire time period). The dataset of Wingender (2014) is comprised of a variety of underlying sources, including the 10-sector database of Groningen Growth and Development Centre from Timmer et al. (2015); EU-KLEMS; the ILO; imputation based on urbanization rates; and interpolation between observed years. We supplement these data with World Bank data drawn from the ILOSTAT and ETD data from Kruse et al. (2023) in order to update the data through 2021. We assess the robustness of our results below when limiting our sample to non-interpolated values exclusively from the Wingender (2014) data.

3.2 Cross-Country Specification

We estimate the following cross-country panel specification:

$$AES_{ct} = \alpha_c + \alpha_t + \beta TFR_{c\tau} + \epsilon_{ct} \tag{7}$$

where α_c are country fixed effects, α_t year fixed effects, AES_{ct} refers to country c's agricultural employment share in year t, and $TFR_{c\tau}$ is c's total fertility rate in year τ, where τ may equal t or may be lagged.

We predict a negative coefficient on β, which would suggest, consistent with our theoretical model, that larger cohorts work increasingly in non-agricultural sectors. However, the choice of τ is key for us to test this story. In particular, we must lag τ relative to t so that we allow enough time for cohorts to grow up and join the labor force.

We aim to estimate the causal effect of fertility rates on the agricultural employment. It may be, however, that other factors, such as secular skill-biased technological change, lead parents to reduce their fertility to focus on quality children over quantity, and simultaneously reduce AES where returns to skill are low as workers flow into nonagricultural sectors where skill returns are higher. While country and year fixed effects take care of time invariant country-specific factors and global shocks, respectively, any country-specific time-varying shocks (such as the skill-biased technical change described above) may bias our estimates.

To address this endogeneity concern, we adopt the instrumental variable strategy of Bloom et al. (2009) by instrumenting for TFR using an abortion policy index. The index sums together indicators for the presence of laws allowing for abortion in various circumstances. The index ranges from 0 to 7, and increments by 1 for each of the following cases in which abortion is permissible in the country: if the pregnancy/birth threatens the mother's life, the mother's physical health, the mother's mental health; if the pregnancy is the result of rape; if there are fetal impairments; for economic reasons; and if, for any reason, the mother requests an abortion.

Bloom et al. (2009) argue that while the level of abortion restrictions in a country are likely endogenous, the timing of their change if plausibly exogenous. We absorb differences in the level of abortion restrictions in equation (7) via the country fixed effects. Moreover, in our preferred specification, we lag the total fertility rate measure, and hence the abortion policy instrument, by 30 years behind the measurement of the agricultural employment share. Hence a violation of the exclusion restriction must be that there is some country-specific time-varying factor which affected both AES today and abortion policy changes 30 years prior.

Table 1: Effect of Total Fertility Rate (30 year lag) on Agricultural Employment Share

	Dependent variable: Agriculture employment share					
	Whole Sample				Wingender (2014) data only, excl. interp-olated values	
	(1)	(2)	(3)	(4)	(5)	(6)
Total Fertility Rate	0.015***	0.0053				
	(0.0048)	(0.0069)				
30 Years lagged Total Fertility Rate		-0.0094*	-0.0077	-0.070**	-0.0058	-0.078*
		(0.0055)	(0.0056)	(0.034)	(0.0060)	(0.042)
N	6,751	2,727	4,674	4,674	1,764	1,724
Dep. var. mean	0.402	0.402	0.300	0.300	0.329	0.329
Country FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
1st-stage F-statistic				8.8		4.2

Notes: The table presents regression results at the country-year level. Standard errors clustered at the country-level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

3.3 Cross-Country Results

We show the results of estimating equation (7) using a variety of samples and specifications in Table 1.

In column 1, we find in an uninstrumented regression that contemporaneous AES and TFR have a positive association. This is consistent with higher fertility raising the demand for food and hence employment in agriculture. In column 2, we include both contemporaneous and 30-year lagged TFR, and find that the contemporaneous correlation loses statistical significance while finding, consistent with our theory, that a decline in TFR 30 years ago raises AES today.

In column 3, we show the OLS estimate when including only the 30-year lagged TFR, and show the 2SLS result in column 4. In both cases we continue to find a negative relationship between AES and lagged TFR, and the relationship becomes significant when instrumenting. The interpretation of the coefficient in column 4 is that a one child reduction in the country's average female lifetime fertility raises agricultural employment share by 7 percentage points. Hence our cross-country results are consistent with the demographic transition slowing down structural transformation.

One might be concerned that we are collating diverse and inconsistent data sources, as well as interpolated values, to measure AES. To address this concern, we re-run the OLS and 2SLS estimation in columns 5 and 6 using only non-interpolated values taken from Wingender (2014). The sign and magnitude on 30-year lagged TFR remains very similar.

There are several drawbacks to our cross-country analysis. First, our instrument is not very strong, with at best a first-stage F-statistic of 8.8, below conventional thresholds. Second, the timing of abortion policy changes may correlate to changes in other policies which affect long-run agricultural employment. Third, we cannot explore mechanisms nor adjustment margins in any detail because our data are highly aggregated. To address these concerns, we turn next to a quasi-experiment in Bangladesh with highly detailed data.

4 Background and Data

4.1 The MCH-FP Program

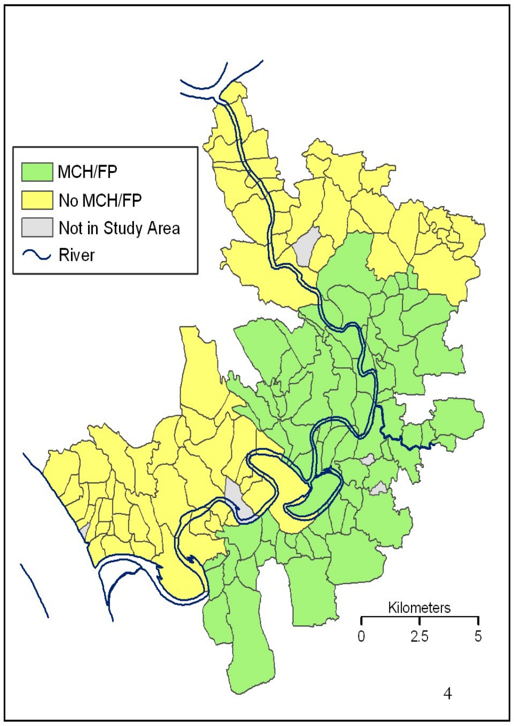
The Maternal and Child Health and Family Planning (MCH-FP) program was introduced in the Matlab subdistrict in Bangladesh in 1977 by icddr,b (formerly known as the International Centre for Diarrhoeal Disease Research, Bangladesh). The program included family planning and maternal and child health services. 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 interventions were rolled out over time starting with access to and advice on using modern contraception for women 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 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 5.1 of pre-program similarities between treatment and comparison areas.

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 2.

Program interventions were phased in, as detailed in Table 2. 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 visits the female health worker 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).

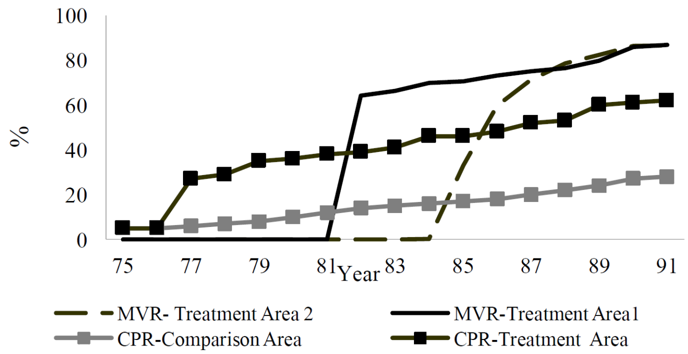
Table 2: MCH-FP Interventions by Cohort

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. (2022)

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 15–49 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,b, 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).

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. (2022)

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. Barham et al. (2021a) 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. (2021b) 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]

[5]The lack of an effect on education for women is not surprising given a secondary school stipend program

4.2 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 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 (see C.1 for more details on our sectoral employment classification).

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,b, 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

for females was available in both the treatment and comparison areas during the schooling years.

[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.

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 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 the household may have moved into the village after the start of the program, and therefore post-1977 location 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.

5 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.

5.1 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. (2022) 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 3 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 3 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 standard deviations demonstrating that the differences that do exist are relatively small. The difference in tubewell access is close to the cut off at 0.20 standard deviations. 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 household's concern about child health or potentially other unobservables.

Tubewell 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 that differences in tubewell water or religion are driving program effects on human capital. In sum, our baseline balance results mimic previous research and show that the two areas are similar across a wide variety of household and household head characteristics.

[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).

Table 3: 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.

5.2 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,

$$Y_h = \omega_0 + \omega_1 T_h + \zeta X_h + \varepsilon_h \tag{8}$$

where T_h is an indicator for whether household h is considered treated (as defined in Section 4.2) and X_h is the vector of demographic and baseline characteristics detailed in Table 3. We cluster standard errors by the village of the household head of h or his antecedents in 1974.

While our baseline specification is at the household level, we also estimate the effect of the MCH-FP on sectoral employment at the individual level. To do so, 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. (2022) 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.

We estimate a single-difference equation at the individual level of the form:

$$\begin{aligned} Y_i = {} & \beta_0 + \beta_1 T_i + \beta_2 Born_i^{77-81} + \beta_3 Born_i^{82-88} + \beta_4 Not\ born_i^{77-88} \\ & + \gamma_1 (T_i \times Born_i^{77-81}) + \gamma_2 (T_i \times Born_i^{82-88}) + \gamma_3 (T_i \times Not\ born_i^{77-88}) + \alpha_{y(i)} + \nu X_i + \epsilon_i \end{aligned} \tag{9}$$

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 4.2; $\alpha_{y(i)}$ 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 3. 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.

6 Results

Our model from Section 2 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. Here we test those theoretical predictions.

6.1 Employment

We first estimate the effects of the MCH-FP on the share of work time spent in each sector at the household level. Results are shown in Table 4. We separate the estimates into medium-run effects (Panel A) measured as of the 1996 MHSS1 survey, and long-run effects (Panel B) measured as of the 2012-2015 MHSS2 survey. The dependent variable in panel A is the share of annual work days spent in each sector; in panel B, the dependent variable is the share of annual work hours spent in each sector.[8]

[8] Note that the difference in measurement between MHSS survey rounds means that the coefficient estimates are not directly comparable between panels A and B.

Table 4: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level

PANEL A: MHSS1 (1996)		
	(1)	(2)
	Agriculture	Non-agricultural
Treated	0.014	0.003
	(0.026)	(0.009)
% chg. rel. to mean	5.5	11.4
Mean	0.26	0.03
Baseline controls	Y	Y
Embankment control	Y	Y
Observations	2580	2580

PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treated	0.052***	-0.031**	-0.011
	(0.016)	(0.015)	(0.017)
% chg. rel. to mean	23.18	-17.18	-2.52
Mean	0.22	0.18	0.42
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2580	2580	2580

Notes: The table presents estimates of equation 8 for outcomes 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. Panel A refers to the 1996 MHSS1, while Panel B refers to the 2012-2015 MHSS2. The dependent variable in panel A is the share of working months in the year in which household members could work allocated to each sector. The dependent variable in panel B is the share of hours worked by sector within the household. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

As of 19 years after the MCH-FP program started, we find no significant effect of the program on sectoral employment, as shown in Panel A. We find a negligible effect on agricultural employment by 1996. The coefficient estimate of the treatment effect is 1.4 percentage points (SE=2.6). The effect of the program on non-agricultural employment is similarly small, with an estimated effect of 0.3 p.p. (SE=0.9).

Next, we turn to the long-run effects of the MCH-FP, 35 years after it started. Panel B of Table 4 reports our results at the time of the 2012-2015 MHSS2 survey. Consistent with our theoretical predictions, we find that the MCH-FP raised the share of household adults working in agriculture by 5.2 p.p. (SE=1.6 p.p), representing a 23 percent increase over the comparison area (column 1). The share of household members in manufacturing fell by 3.1 p.p. (SE=1.5), an 18 percent fall relative to comparison households (column 2). In services, we find a near-zero effect of -1.1 p.p. (SE=1.7), a 2.5 percent reduction relative to comparison households (column 3).

The results from Table 4 have two key takeaways. First, consistent with our theoretical model, we find that the MCH-FP program reduced the speed of structural transformation. Second, a 19 year look-back window—considered very "long-term" for nearly any randomized control trial—is insufficiently long to observe the effects of an intervention targeting fertility and the early-childhood years. Indeed, one must observe outcomes well after the affected cohorts have entered the labor market.

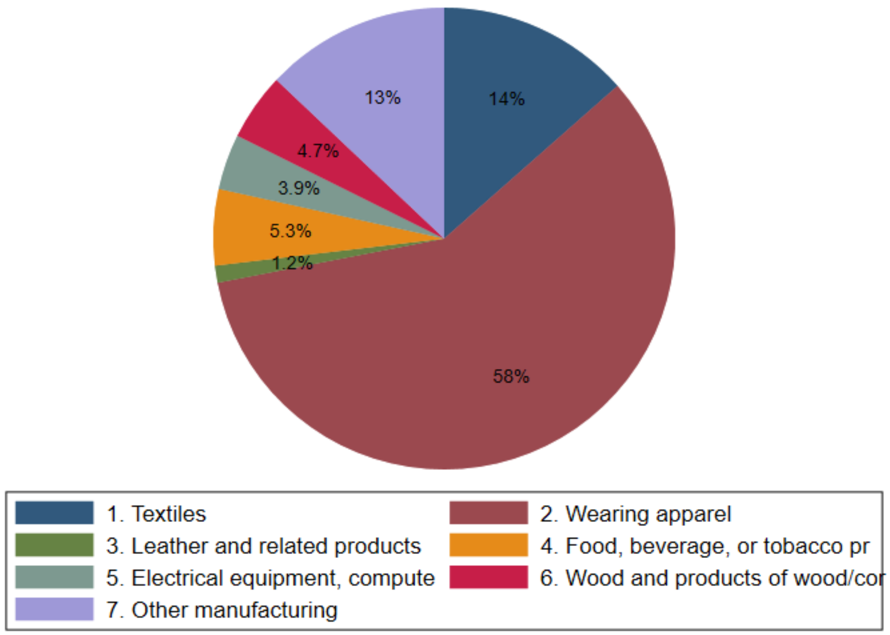
We have shown that treated households send workers more to agriculture and less to manufacturing. Does this pattern carry over to entrepreneurship? We find that it does, with results reported in Table A.2. In columns 1 through 3, we estimate the effect of the MCH-FP on the share of household members who own an enterprise by sector. Consistent with our employment results, we find that entrepreneurship in agriculture is almost 5 p.p. (SE=1.4) higher in treatment household, 23 percent higher than comparison area households (column 1). Moreover, the program reduced manufacturing entrepreneurship by over 37 percent (column 2). We find no effect of the program on the ownership of service sector enterprises (column 3).

Because the manufacturing sector includes both factories and small handicraft enterprises, such as blacksmiths, one may worry whether our results are driven by one or the other part of the industry. We explore this question in column 4 through 6 of Table A.2.

We show that relative employment losses for treated households come from work in factories and large employers. In column 4, we show that the program reduced the share of household members who had ever worked in a factory by 3.3 p.p. (SE=0.9), a nearly 24 percent reduction relative to comparison households. We find a similar effect on the share of household members currently working in a factory (column 5). Finally, we also find that a 2 p.p. smaller share (SE=0.7) of household members work for a larger employer (column 6). Our results suggest that the program induced a reduced share of employment in the part of the manufacturing sector considered to have the highest productivity, i.e., factories and large employers.

We can further observe the kinds of goods produced in factories at which workers were employed. In Figure 4, we 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.[9]

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.

Individual-level estimates. Following the examination of household-level effects, we report individual-level differences in employment outcomes, estimated using equation 9. We allow for heterogeneous program effects by cohort given the differential program exposure children had depending on their year of birth (see Table 2). In the individual-level results, we report single-difference estimates for three intervention cohorts: those born between 1977 and 1981 (during the family planning phase of the program), those born 1982-88 (family planning plus childhood vaccinations), and those born during other years (effects of MCH-FP only through household or labor market spillovers). 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.

[9]The share working in factories producing wearing apparel falls to 46% when we condition on men, and rises to 92% for women.

Table 5 reports results at the individual level among men in panel A and women in panel B. We find that, consistent with our household-level estimates, treated individuals increase the share of hours worked in agriculture (column 1) and reduce it in manufacturing (column 2).

Table 5: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector: Individual-Level

PANEL A: Men

	Share hours by sector			
	(1) Agriculture	(2) Manufacturing	(3) Services	(4) Hours worked
Treatment × Born 1982-88	-0.0048	-0.079**	0.069*	-7.68
	(0.022)	(0.031)	(0.041)	(106.7)
Treatment × Born 1977-81	0.058*	-0.045	-0.039	10.1
	(0.030)	(0.034)	(0.046)	(122.2)
Treatment × Not born 1977-88	0.052*	0.016	-0.035	-222.5**
	(0.027)	(0.015)	(0.030)	(103.5)
% Chg., Treat×(Born 1982–88)	-5.68	-35.26	13.23	-0.25
% Chg., Treat×(Born 1977–81)	59.37	-24.32	-6.97	0.31
% Chg., Treat×(Born Pre-1977 or Post-1988)	18.68	16.90	-10.05	-9.78
Mean if born 1982-88	0.09	0.22	0.52	3073
Mean if born 1977-81	0.10	0.18	0.57	3290
Mean if born pre-1977 or post-1988	0.28	0.09	0.35	2276
Observations	2819	2819	2819	2819

PANEL B: Women

	Share hours by sector			
	(1) Agriculture	(2) Manufacturing	(3) Services	(4) Hours worked
Treatment × Born 1982-88	0.060***	0.0075	-0.021	76.1
	(0.022)	(0.026)	(0.019)	(78.6)
Treatment × Born 1977-81	-0.019	-0.0096	0.025	-52.5
	(0.037)	(0.029)	(0.027)	(89.7)
Treatment × Not born 1977-88	0.012	-0.0084	-0.0091	-42.8
	(0.028)	(0.012)	(0.011)	(44.3)
% Chg., Treat×(Born 1982–88)	41.13	6.13	-28.90	16.75
% Chg., Treat×(Born 1977–81)	-10.61	-8.68	41.44	-11.22
% Chg., Treat×(Born Pre-1977 or Post-1988)	5.02	-22.00	-18.98	-12.53
Mean if born 1982-88	0.14	0.12	0.07	454
Mean if born 1977-81	0.18	0.11	0.06	468
Mean if born pre-1977 or post-1988	0.25	0.04	0.05	341
Observations	3322	3322	3322	3322

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes for men (panel A) and women (panel B) at the individual-level. Means by age group refer to the non-treated. Standard errors are clustered by pre-program village. Regressions are weighted to adjust for attrition between birth and the MHSS2 survey. All variables control for the baseline controls listed in Table 3 as well as erosion exposure.

There is, however, considerable heterogeneity in program effects across cohorts. To interpret these differences across cohorts, 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., 2021b).

We find that men born during the human-capital building phase of the program, between 1982 and 1988, worked more in the service sector and less in manufacturing (first row of coefficients, panel A). However, this increase in service sector employment was offset by reductions in the share of hours worked by all other cohorts of men (column 3). These other cohorts of men (born before 1982 or after 1988) increased their agricultural employment. Our results can be understood to the extent that the returns to human capital are higher in the service sector than in agriculture or manufacturing, and that families optimally allocated sons to sectors based on their human capital.

We see some evidence of an increase in time spent working for women who experienced the greatest human capital gains from the program (column 4 of panel B), although the effect is not precisely estimated. These women (born between 1982 and 1988 and therefore vaccinated in early childhood) are 6 percentage points more likely to work in agriculture than women born 1982-88 in the comparison area. Therefore households who sent away their highest human capital son to the service sector appear to have made up for this loss by bringing in their highest human capital daughter to work the family farm.

Rural-to-urban migration. Next, to better understand how the MCH-FP drives sectoral employment allocations, we explore the importance of rural-to-urban migration in shaping our baseline estimates. We re-estimate equation 8 by sector, but further split the dependent variable of work hours share by rural and urban location of employment.

We report results in Table 6, with the effect on hours worked share in urban areas reported in panel A, and in rural areas in panel B. We find that rural-to-urban migration can explain much of the impact of the MCH-FP on sectoral employment outcomes. We find that only manufacturing employment in urban areas (column 2 of panel A) was affected by the program, with no effect on rural manufacturing employment (column 2 of panel B). Similarly, agricultural employment was only raised in rural areas as a result of the program (column 1 of panel B), with no effect on urban agricultural employment (column 1 of panel A). We find no precisely estimated effect of the program on service sector employment in either urban or rural settings. These results suggest that structural transformation is not happening within rural Matlab, as manufacturing employment growth is concentrated in

Bangladesh's urban centers.

Table 6: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector and Urbanicity: Household-Level

PANEL A: Urban Employment by Sector			
	(1) Agriculture	(2) Manufacturing	(3) Services
Treated	0.010	-0.033***	0.014
	(0.006)	(0.012)	(0.018)
% chg. rel. to mean	191.6	-24.1	6.9
Mean	0.00	0.14	0.20
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2580	2580	2580
PANEL B: Rural Employment by Sector			
	(1) Agriculture	(2) Manufacturing	(3) Services
Treated	0.042***	0.001	-0.025
	(0.015)	(0.007)	(0.015)
% chg. rel. to mean	19.34	3.10	-11.45
Mean	0.22	0.05	0.21
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2580	2580	2580

Notes: The table presents estimates of equation 8 for outcomes measured in 2014 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. The dependent variable in panel A is the share of household members working in each sector. The dependent variable in panel B is the fraction of total hours worked with the MHSS1 household allocated to each sector. See Appendix C.1 for more details on how we classify workers into sectors. Industry employment shares do not sum to 1 for two reasons. First, we do not report results for two small sectors, construction and mining. Second, a small set of respondents not providing sufficient information to classify them into sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

6.2 Role of Family Size and Child Gender

We next turn to testing how household size shapes our results, a key mechanism highlighted by our theoretical model. Fauveau (1994), Joshi and Schultz (2013), and Barham et al. (2022) 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.3. 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.[10,11]

Next, to understand how population pressures within the household contributed to structural transformation, we estimate how the number of male children per household born during the experimental period affected those children's later-life sectoral employment choices. we focus on males because of their stronger labor market attachment. In particular, we estimate an equation of the form

$$Y_h = \alpha_0 + \alpha_1 Num.\ males\ age\ 24\ to\ 34_h + \gamma X_h + \epsilon_h \tag{10}$$

Because the number of males born during the experimental period is an outcome of the program, we instrument for $Num.\ males\ age\ 24\ to\ 34_h$ using the treatment dummy. We expect that households with more children will be more likely to send a child to work in a non-agricultural sector.

We present our results in Table 7. Consistent with our proposed mechanism of household size, larger households have a smaller share of their adults working in agriculture (column 1). Conversely, larger households are more likely to have a member working in manufacturing (column 2) or services (column 3), though the effect is less precisely estimated for services.

6.3 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.

[10] The difference in number of 24-34 year olds by gender is statistically indistinguishable.

[11] Note that these effect sizes are smaller than those reported in Joshi and Schultz (2013) and Barham et al. (2022). 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.

Table 7: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector and Household-Size: Household-Level

	(1) Agriculture	(2) Manufacturing	(3) Services
Num. males age 24-34	-0.389**	0.235*	0.0787
	(0.155)	(0.140)	(0.118)
% chg. rel. to mean	-173.4	128.5	18.9
Mean	0.22	0.18	0.42
First-stage F-stat.	10.4	10.4	10.4
Baseline controls	Y	Y	Y
Embankment controls	Y	Y	Y
Observations	2580	2580	2580

Notes: The table presents 2SLS estimates for outcomes measured in 2014 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. The dependent variable in panel A is the share of household members working in each sector. The dependent variable in panel B is the fraction of total hours worked with the MHSS1 household allocated to each sector. See Appendix C.1 for more details on how we classify workers into sectors. Industry employment shares do not sum to 1 for two reasons. First, we do not report results for two small sectors, construction and mining. Second, a small set of respondents not providing sufficient information to classify them into sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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

	MHSS1 (1996)		MHSS2 (2012-2014)	
	(1) =1 if household farms	(2) Acres owned per cap.	(3) =1 if household farms	(4) Acres owned per cap.
Treated	0.02	-0.03	0.07***	0.01
	(0.03)	(0.03)	(0.02)	(0.02)
Observations	2580	2580	2580	2580
Adjusted R^2	0.067	0.007	0.030	0.024
Mean	0.65	0.20	0.76	0.25
% chg. rel. to mean	3.7	-17.4	9.0	5.9
Embankment dummies	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y

Notes: The table presents estimates of equation 8 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	0.146***	0.004	0.026	0.010	29.689**	-0.539
	(0.041)	(0.045)	(0.017)	(0.007)	(13.088)	(35.280)
Observations	785	1346	785	1346	785	1346
Adjusted R^2	0.041	0.006	0.003	0.022	-0.003	0.035
% chg. rel. to mean	32.3	0.8	2.7	1.1	32.0	-0.2
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 8 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.

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.[12]

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.[13] Our results are consistent

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

[13]Unfortunately, the MHSS2 survey does not allow us to separate market-purchased labor from non-labor inputs.

with households switching away from household labor and into labor-saving technology and inputs as a result of a reduction in household size.

We test our final 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.4, 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 is consistent with our individual-level estimates in Table 5 which shows that the men whose human capital was improved most by the program (i.e., were born during the vaccine arm of the MCH-FP) left agriculture to work in services.

A plausible outcome of the program is that treated households expand their land holdings due to their greater participation in agriculture. In Table A.5, we show that this is not the case. In particular, we find no effect of the MCH-FP on household land holdings, either as of the MHSS1 (in 1996) or the MHSS2 (in 2012-2014). We also find no statistically significant effect on the change in land owned.

6.4 Robustness of Results

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

We estimate the effect of the program on the extensive margin of employment, specifically the share of household members who work in each sector. We show our results in Table A.6. Consistent with our result on the share of work hours allocated to each sector, we find no significant effect of the program as of 1996 (panel A), but a large a positive effect on agricultural employment by the 2012-2015 MHSS2 survey (column 1 of panel B) and a large and negative effect on manufacturing employment (column 2 of panel B). We find no effect on employment in the service sector (column 3 of panel B).

Second, we 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.

Next, we assess the concern that information spillovers along the border of the treatment and control zones may reduce our estimated effect. To do so, we restrict our sample to those living in a village prior to the intervention which has a centroid within 3000 meters of the border. In Table A.8, we show that our results are very similar in magnitude to our baseline estimates when applying this restriction.

Given our finding in Table 3 that Muslims are disproportionately represented in control villages, we re-estimate our main results using only Muslim households. We find that results are virtually unchanged with this sample restriction, as shown in Table A.9. Since Matlab is about 85% Muslim, we do not have sufficient statistical power to estimate program effects for the Hindu population on its own.

Finally, we address one other asymmetry between treatment and control areas: the only urban center in the study area, Pourashava, exists in the treatment area. In Table A.10, we show that our results are largely unchanged when we remove households who resided in Pourashava prior the intervention.

7 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 faced the same migration options as the control households, and yet chose to stay in rural Matlab predominantly working in agriculture. Indeed, urban and employment disamenities are a substantial cost to rural-to-urban migrants in many developing country settings (Imbert and Papp, 2020).

References

David Andersson, Mounir Karadja, and Erik Prawitz. Mass migration and technological change. *Journal of the European Economic Association*, 20(5):1859–1896, 2022.

Tania Barham. Enhancing cognitive functioning: Medium-term effects of a health and family planning program in Matlab. *American Economic Journal: Applied Economics*, 4(1):245–73, 2012.

Tania Barham and Randall Kuhn. Staying for benefits: The effect of a health and family planning program on out-migration patterns in Bangladesh. *Journal of Human Resources*, 49(4):982–1013, 2014.

Tania Barham, Brachel Champion, Andrew D Foster, Jena D Hamadani, Warren C Jochem, Gisella Kagy, Randall Kuhn, Jane Menken, Abdur Razzaque, Elisabeth Dowling Root, et al. Thirty-five years later: Long-term effects of the matlab maternal and child health/family planning program on older women's well-being. *Proceedings of the National Academy of Sciences*, 118(28):e2101160118, 2021a.

Tania Barham, Gisella Kagy, Brachel Champion, and Jena Hamadani. Early childhood health and family planning: Long-term and intergenerational effects on human capital. April 2021b.

Tania Barham, Randall Kuhn, and Patrick Turner. No place like home: Long-run impacts of early child health and family planning on labor and migration outcomes. June 2022.

Gary S Becker, Kevin M Murphy, and Robert Tamura. Human capital, fertility, and economic growth. *Journal of political economy*, 98(5, Part 2):S12–S37, 1990.

Shushum Bhatia, W Henry Mosley, Abu SG Faruque, and Jotsnamoy Chakraborty. The matlab family planning-health services project. *Studies in family planning*, 11(6):202–212, 1980.

David E Bloom, David Canning, and JP Sevilla. Economic growth and the demographic transition. 2001.

David E Bloom, David Canning, Günther Fink, and Jocelyn E Finlay. Fertility, female labor force participation, and the demographic dividend. *Journal of Economic Growth*, 14:79–101, 2009.

Timo Boppart, Patrick Kiernan, Per Krusell, and Hannes Malmberg. The macroeconomics of intensive agriculture. Technical report, National Bureau of Economic Research, 2023.

Paula Bustos, Bruno Caprettini, and Jacopo Ponticelli. Agricultural productivity and structural transformation: Evidence from Brazil. *American Economic Review*, 106(6):1320–65, 2016.

Lincoln C Chen, Makhlisur Rahman, Stan D'Souza, J Chakraborty, and AM Sardar. Mortality impact of an mch-fp program in Matlab, Bangladesh. *Studies in family planning*, pages 199–209, 1983.

Uttam K Chowdhury, Bhajan K Biswas, T Roy Chowdhury, Gautam Samanta, Badal K Mandal, Gautam C Basu, Chitta R Chanda, Dilip Lodh, Khitish C Saha, Subhas K Mukherjee, et al. Groundwater arsenic contamination in bangladesh and west bengal, india. *Environmental health perspectives*, 108(5):393–397, 2000.

Michael A Clemens, Ethan G Lewis, and Hannah M Postel. Immigration restrictions as active labor market policy: Evidence from the mexican bracero exclusion. *American Economic Review*, 108(6):1468–87, 2018.

Matthew J Delventhal, Jesús Fernández-Villaverde, and Nezih Guner. Demographic transitions across time and space. Technical report, National Bureau of Economic Research, 2021.

Vincent Fauveau. *Matlab: Women, Children and Health*. Dhaka; ICDDR, B, 1994.

Oded Galor and David N Weil. The gender gap, fertility, and growth. *American Economic Review*, 86(3):374, 1996.

Oded Galor and David N Weil. Population, technology, and growth: From malthusian stagnation to the demographic transition and beyond. *American Economic Review*, 90(4): 806–828, 2000.

Douglas Gollin, Stephen Parente, and Richard Rogerson. The role of agriculture in development. *American Economic Review*, 92(2):160–164, 2002.

Douglas Gollin, Stephen L Parente, and Richard Rogerson. The food problem and the evolution of international income levels. *Journal of Monetary Economics*, 54(4):1230–1255, 2007.

Berthold Herrendorf, Christopher Herrington, and Akos Valentinyi. Sectoral technology and structural transformation. *American Economic Journal: Macroeconomics*, 7(4):104–133, 2015.

Richard Hornbeck and Suresh Naidu. When the levee breaks: black migration and economic development in the american south. *American Economic Review*, 104(3):963–90, 2014.

Douglas H Huber and Atiqur Rahman Khan. Contraceptive distribution in bangladesh villages: The initial impact. *Studies in Family Planning*, 10(8/9):246–253, 1979.

icddr,b. Matlab 1974 census, 1974.

icddr,b. Health and demographic surveillance system - matlab, 1982.

icddr,b. Health and demographic surveillance system - matlab, 2007.

Guido W Imbens and Jeffrey M Wooldridge. Recent developments in the econometrics of program evaluation. *Journal of economic literature*, 47(1):5–86, 2009.

Clément Imbert and John Papp. Costs and benefits of rural-urban migration: Evidence from India. *Journal of Development Economics*, 146:102473, 2020.

Paul Johnson and Chris Papageorgiou. What remains of cross-country convergence? *Journal of Economic Literature*, 58(1):129–75, 2020.

Charles I Jones. The end of economic growth? unintended consequences of a declining population. *American Economic Review*, 112(11):3489–3527, 2022.

Shareen Joshi and T Paul Schultz. Family planning as an investment in development: Evaluation of a program's consequences in Matlab, Bangladesh. *Yale University Economic Growth Center Discussion Paper*, (951), 2007.

Shareen Joshi and T. Paul Schultz. Family planning and women's and children's health: Long-term consequences of an outreach program in Matlab, Bangladesh. *Demography*, 50 (1):149–180, 2013.

M Mahmud Khan. *Expanded Program on Immunization in Bangladesh: Cost, cost-effectiveness, and financing estimates*. Number 24. Partnerships for Health Reform, 1998. URL http://www.path.org/vaccineresources/files/Abt-PNACH278.pdf.

Michael A. Koenig, Mehrab Ali Khan, Bogdan Wojtyniak, John D. Clemens, Jyotsnamoy Chakraborty, Vincent Fauveau, James F. Phillips, Jalaluddin Akbar, and Uday S. Barua. Impact of measles vaccination on childhood mortality in rural Bangladesh. *Bulletin of the World Health Organization*, 68(4):441, 1990.

Michael A Koenig, Vincent Fauveau, and Bogdan Wojtyniak. Mortality reductions from health interventions: The case of immunization in Bangladesh. *Population and Development Review*, pages 87–104, 1991.

Michael Kremer, Jack Willis, and Yang You. Converging to convergence. *NBER Macroeconomics Annual*, 36(1):337–412, 2022.

Hagen Kruse, Emmanuel Mensah, Kunal Sen, and Gaaitzen de Vries. A manufacturing (re)naissance? industrialization in the developing world. *IMF Economic Review*, 71(2): 439–473, 2023.

Oksana M Leukhina and Stephen J Turnovsky. Population size effects in the structural development of england. *American Economic Journal: Macroeconomics*, 8(3):195–229, 2016.

W. Arthur Lewis. Economic development with unlimited supplies of labour. *The Manchester School*, 22(2):139–191, 1954.

Jane Menken and James F. Phillips. Population change in a rural area of Bangladesh, 1967-87. *The Annals of the American Academy of Political and Social Science*, 510(1):87–101, 1990.

Dev Patel, Justin Sandefur, and Arvind Subramanian. The new era of unconditional convergence. *Journal of Development Economics*, 152:102687, 2021.

Michael Peters. Market size and spatial growthâevidence from germany's post-war population expulsions. *Econometrica*, 90(5):2357–2396, 2022.

James F. Phillips, Wayne S. Stinson, Shushum Bhatia, Makhlisur Rahman, and Jyotsnamoy Chakraborty. The demographic impact of the family planning–health services project in Matlab, Bangladesh. *Studies in Family Planning*, pages 131–140, 1982.

O Rahman, J Menken, A Foster, C Peterson, MN Khan, R Kuhn, and P Gertler. The matlab health and socio-economic survey: Overview and user's guide, 1999, 1999.

Shmuel San. Labor supply and directed technical change: Evidence from the termination of the bracero program in 1964. *American Economic Journal: Applied Economics*, 15(1): 136–63, 2023.

Rodrigo R Soares. Mortality reductions, educational attainment, and fertility choice. *American Economic Review*, 95(3):580–601, 2005.

Robert M Solow. A contribution to the theory of economic growth. *Quarterly Journal of Economics*, 70(1):65–94, 1956.

Marcel Timmer, Gaaitzen J de Vries, and Klaas De Vries. Patterns of structural change in developing countries. In *Routledge handbook of industry and development*, pages 65–83. Routledge, 2015.

United Nations. Classification of individual consumption according to purpose (coicop). *Department of Economic and Social Affairs, Statistical Papers Series M No. 99*, 2018.

Gail A Wasserman, Xinhua Liu, Faruque Parvez, Habibul Ahsan, Diane Levy, Pam Factor-Litvak, Jennie Kline, Alexander van Geen, Vesna Slavkovich, Nancy J LoIacono, et al. Water manganese exposure and children's intellectual function in araihazar, bangladesh. *Environmental health perspectives*, 114(1):124–129, 2006.

Asger Moll Wingender. Structural transformation in the 20th century: A new database on agricultural employment around the world. *Available at SSRN 2533360*, 2014.

Yongkun Yin. China's demographic transition: A quantitative analysis. *Available at SSRN 3988073*, 2021.

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 8 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 United Nations (2018). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively

Table A.2: ITT Effects of MCH-FP on Long-term Entrepreneurship and Employer Characteristics: Household-Level

	Entrepreneurship by sector					
	(1)	(2)	(3)	(4)	(5)	(6)
	Agriculture	Manufacturing	Services	Ever worked factory	Work in factory	Employer has >100 employees
Treated	0.049***	-0.008*	0.002	-0.033***	-0.022***	-0.020***
	(0.014)	(0.004)	(0.008)	(0.009)	(0.007)	(0.007)
% chg. rel. to mean	23.2	-37.4	1.7	-23.6	-29.0	-26.2
Mean	0.21	0.02	0.13	0.14	0.08	0.08
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment controls	Y	Y	Y	Y	Y	Y
Observations	2580	2580	2580	2580	2580	2580

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Each dependent variable is the share of household members exhibiting the described behavior. The dependent variable of column 4 refers to the share of household members who ever worked in a factory with more than 30 employees. Standard errors are clustered by pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.3: 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	-0.13***	-0.06*
	(0.04)	(0.04)
Observations	2580	2580
Adjusted R^2	0.007	-0.001
Mean	0.8	0.7
% chg. rel. to mean	-16.05	-8.99
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.4: 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	-0.591	-24.74	-10.63	-34.27
	(39.52)	(143.0)	(52.18)	(144.3)
% chg. rel. to mean	-0.1	16.0	-1.6	-41.4
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.5: ITT Effects of MCH-FP on Land Ownership

	MHSS1 (1996)	MHSS2 (2012-2014)	
	(1) Acres owned	(2) Acres owned	(3) Change land owned
Treated	-0.10	0.05	0.15
	(0.11)	(0.09)	(0.13)
Observations	2580	2580	2580
Adjusted R^2	0.014	0.021	0.001
Mean	1.0	1.2	0.1
% chg. rel. to mean	-9.742	4.517	117.479
Embankment dummies	Y	Y	Y
Baseline controls	Y	Y	Y

Notes: The table presents estimates of the effect of the MCH-FP on land ownership at the MHSS1 household-level from 1996 (column 1), 2014 (column 2), on the change in household (column 3) land ownership. 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.*, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively

Table A.6: ITT Effects of MCH-FP on Long-term Share of Household Members Employed by Sector

PANEL A: MHSS1 (1996)			
	(1)	(2)	
	Agriculture	Non-agricultural	
Treated	-0.003	0.026	
	(0.023)	(0.027)	
% chg. rel. to mean	-1.0	5.2	
Mean	0.28	0.51	
Baseline controls	Y	Y	
Embankment control	Y	Y	
Observations	2580	2580	
PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treated	0.048***	-0.021**	-0.005
	(0.014)	(0.008)	(0.010)
% chg. rel. to mean	21.42	-18.99	-1.92
Mean	0.22	0.11	0.26
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2580	2580	2580

Notes: The table presents estimates of equation 8 for outcomes 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. Panel A refers to the 1996 MHSS1, while Panel B refers to the 2012-2015 MHSS2. The dependent variable in both panels is the share of household members working in each sector. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.7: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector: Fraction of Household Treated

PANEL A: Share of household members employed by sector			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
% HH treated	0.051***	-0.021***	-0.007
	(0.014)	(0.008)	(0.010)
% chg. rel. to mean	23.0	-19.5	-2.5
Mean	0.22	0.11	0.26
Baseline controls	Y	Y	Y
Embankment control			
Observations	2580	2580	2580
PANEL B: Fraction of household hours worked by sector			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
% HH treated	0.053***	-0.033**	-0.006
	(0.016)	(0.015)	(0.016)
% chg. rel. to mean	23.81	-18.13	-1.56
Mean	0.22	0.18	0.41
Baseline controls	Y	Y	Y
Embankment control			
Observations	2580	2580	2580

Notes: The table presents estimates for outcomes measured in 2014 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. The dependent variable in panel A is the share of household members working in each sector. The dependent variable in panel B is the fraction of total hours worked with the MHSS1 household allocated to each sector. See Appendix C.1 for more details on how we classify workers into sectors. Industry employment shares do not sum to 1 for two reasons. First, we do not report results for two small sectors, construction and mining. Second, a small set of respondents not providing sufficient information to classify them into sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.8: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level, Close to Treatment/Control Border

PANEL A: MHSS1 (1996)			
	(1)	(2)	
	Agriculture	Non-agricultural	
Treated	-0.018	-0.012	
	(0.024)	(0.010)	
% chg. rel. to mean	-6.5	-38.4	
Mean	0.28	0.03	
Baseline controls	Y	Y	
Embankment control	Y	Y	
Observations	1738	1738	
PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treated	0.040**	-0.035*	-0.010
	(0.018)	(0.019)	(0.019)
% chg. rel. to mean	16.77	-19.95	-2.39
Mean	0.24	0.18	0.41
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	1738	1738	1738

Notes: The table presents estimates of equation 8 for outcomes at the MHSS1 household-level, restricting the sample to individuals whose pre-program village is less than 3km away from the treatment border. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Panel A refers to the 1996 MHSS1, while Panel B refers to the 2012-2015 MHSS2. The dependent variable in panel A is the share of working months in the year in which household members could work allocated to each sector. The dependent variable in panel B is the share of hours worked by sector within the household. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.9: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level, Muslims Only

PANEL A: MHSS1 (1996)			
	(1)	(2)	
	Agriculture	Non-agricultural	
Treated	0.006	0.013	
	(0.027)	(0.009)	
% chg. rel. to mean	2.2	59.4	
Mean	0.26	0.02	
Baseline controls	Y	Y	
Embankment control	Y	Y	
Observations	2325	2325	
PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treated	0.050***	-0.034**	-0.008
	(0.018)	(0.016)	(0.018)
% chg. rel. to mean	22.20	-18.38	-1.96
Mean	0.22	0.18	0.41
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2325	2325	2325

Notes: The table presents estimates of equation 8 for outcomes at the MHSS1 household-level, for Muslim households only. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Panel A refers to the 1996 MHSS1, while Panel B refers to the 2012-2015 MHSS2. The dependent variable in panel A is the share of working months in the year in which household members could work allocated to each sector. The dependent variable in panel B is the share of hours worked by sector within the household. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.10: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level, Excluding Main City

PANEL A: MHSS1 (1996)			
	(1)	(2)	
	Agriculture	Non-agricultural	
Treated	0.039	0.005	
	(0.030)	(0.011)	
% chg. rel. to mean	15.1	24.6	
Mean	0.26	0.02	
Baseline controls	Y	Y	
Embankment control	Y	Y	
Observations	1970	1970	
PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treated	0.075***	-0.041**	-0.013
	(0.017)	(0.017)	(0.019)
% chg. rel. to mean	32.44	-21.80	-3.12
Mean	0.23	0.19	0.40
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	1970	1970	1970

Notes: The table presents estimates of equation 8 for outcomes at the MHSS1 household-level, excluding individuals whose pre-program village is Matlab town. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Panel A refers to the 1996 MHSS1, while Panel B refers to the 2012-2015 MHSS2. The dependent variable in panel A is the share of working months in the year in which household members could work allocated to each sector. The dependent variable in panel B is the share of hours worked by sector within the household. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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:

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

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

$$MPL_m = A_m(1-\alpha)Z_m^{\alpha}(hL_m)^{-\alpha}h$$

So then to get the wage, given exogenous p_z and the fact that the manufacturing firm's two first-order conditions imply that $\frac{w}{p_z} = \frac{1-\alpha}{\alpha}\frac{Z_m}{L_m}$, we can pin down the wage, equation (3). Taking ratios between marginal products in agriculture, we obtain

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

and plugging this into the CES part of the agriculture production function to get

$$\left[\omega Z_g^{\frac{\epsilon-1}{\epsilon}} + (1-\omega)L_g^{\frac{\epsilon-1}{\epsilon}}\right]^{\frac{\theta\epsilon}{\epsilon-1}} = (1-\omega)^{\frac{\theta\epsilon}{\epsilon-1}} \left[\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w}{p_z}\right)^{\epsilon-1} + 1\right]^{\frac{\theta\epsilon}{\epsilon-1}} L_g^{\theta}$$

Hence,

$$w = A_g(1-\omega)^{\frac{\theta\epsilon}{\epsilon-1}-1}\theta \left[\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w}{p_z}\right)^{\epsilon-1} + 1\right]^{\frac{\theta\epsilon}{\epsilon-1}-1} \left(\frac{T}{L_g}\right)^{1-\theta}$$

Now, given equality of wages between sectors, we have

$$p_gA_g(1-\omega)^{\frac{\theta\epsilon}{\epsilon-1}-1}\theta \left[\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w}{p_z}\right)^{\epsilon-1} + 1\right]^{\frac{\theta\epsilon}{\epsilon-1}-1} L_g^{\theta-1}T^{1-\theta} = p_mA_m(1-\alpha)\left(\frac{\alpha}{1-\alpha}\frac{w}{p_z}\right)^{\alpha} h^{1-\alpha}$$

which yields equation (4).

We get Z_g^* by plugging equilibrium labor supplied to agriculture L_g^* described by equation (4) into equation (B.2). Agricultural output is simply equation (2) with L_g^* and Z_g^* plugged in. To solve for L_m^*, simply plug L_g^* into the labor market clearing condition, $L = L_g + L_m$.

B.2 Theoretical Predictions

We next derive the theoretical predictions from Section 2.3.

Prediction 1(a): *A relatively lower population L will result in an increased share of workers employed in the agricultural sector.*

Proof:

$$\frac{\partial L_a/L}{\partial L} = -\frac{L_g^*}{L^2} < 0$$

Prediction 1(b): *The sign of the effect of a rise in average human capital h on the share of workers employed in the agricultural sector depends on parameter values, as detailed below.*

$$\frac{\partial L_a/L}{\partial h} = \frac{1}{1-\theta}\frac{L_a^*}{L}\left[(\theta\epsilon - \epsilon + 1)\frac{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-2}}{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-1}+1}\frac{\partial w^*}{\partial h} - \frac{\partial w^*/\partial h}{w^*}\right] \tag{B.3}$$

where $\frac{\partial w^*}{\partial h} = (p_m A_m)^{\frac{1}{1-\alpha}}(1-\alpha)\left(\frac{\alpha}{p_z}\right)^{\frac{\alpha}{1-\alpha}} h$. So $\frac{\partial L_g/L}{\partial h} < 0$ if and only if $(\theta\epsilon - \epsilon + 1)\frac{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-2}}{\left(\frac{\omega}{1-\omega}\right)^{\epsilon}\left(\frac{w^*}{p_z}\right)^{\epsilon-1}+1}\frac{\partial w^*}{\partial h} < \frac{\partial w^*/\partial h}{w^*}$ which can be rearranged to be inequality (6).

Prediction 2(a): *A relatively lower population L results in more intensive use of intermediate inputs in agriculture per capita.* We consider intermediate use at the household level, i.e., $\frac{Z_g}{L}$. Hence,

Proof:

$$\frac{\partial Z_g^*/L}{\partial L} = -\frac{1}{L^2}\left(\frac{\omega}{1-\omega}\frac{w^*}{p_z}\right)^{\epsilon} L_g^* < 0$$

Prediction 2(b): *The sign of the effect of a rise in average human capital h on the intensity of use of intermediate inputs in agriculture depends on parameter values, as detailed below.*

$$\frac{\partial Z_g^*/L}{\partial h} = \epsilon\left(\frac{\omega}{1-\omega}\frac{w^*}{p_z}\right)^{\epsilon-1}\frac{\partial w^*}{\partial h}L_g^* + \left(\frac{\omega}{1-\omega}\frac{w^*}{p_z}\right)^{\epsilon}\frac{\partial L_g^*}{\partial h}$$

where $\frac{\partial w^*}{\partial h} > 0$ always and the sign of $\frac{\partial L_g^*}{\partial h}$ depends on parameter values, as shown for Prediction 1(b). Hence, the overall effect $\frac{\partial Z_g^*/L}{\partial h} < 0$ if

$$\epsilon\frac{1-\omega}{\omega}\frac{p_z}{w^*}\frac{\partial w^*}{\partial h}L_g^* > -\frac{\partial L_g^*}{\partial h}$$

which can be rewritten as

$$\varepsilon_{w,h} > -\Psi(\omega, \epsilon, p_z)\varepsilon_{L_g,h} \tag{B.4}$$

where $\varepsilon_{x,y}$ is the elasticity of x with respect to y and $\Psi(\omega, \epsilon, p_z) \equiv \frac{\omega}{1-\omega}\frac{1}{\epsilon p_z}$ indexes the 'ease' of substituting Z for L in the agricultural sector. In words, inequality (B.4) states that intermediate input intensity in agriculture will fall with human capital if and only if either (i) agricultural employment increases in human capital, or (ii) when agricultural employment decreases in human capital, wages are more responsive than employment in agriculture is to shifts in human capital, up to some multiplicative constant Ψ.

B.3 Three-Sector Model with Service Sector

We extend our model to allow for a third sector producing nontradable output. We do so to understand how our predictions may change with such an addition, and as we explore the effects of a faster demographic transition on services employment in the context of our Bangladesh natural experiment discussed in Section 4.

We start with the same production functions for agriculture and manufacturing as described in Section 2.1. Similar to manufacturing, the service sector has a Cobb-Douglas production function with identical cost-share parameter α:

$$Q_s = A_s Z_s^{\alpha} (hL_s)^{1-\alpha}$$

Because service sector output is not traded, we must model demand. Following Bustos et al. (2016), we assume a Cobb-Douglas utility function:

$$U(c_{a,L}, c_{m,L}, c_{s,L}) = c_{a,L}^{\eta_a} c_{m,L}^{\eta_m} c_{s,L}^{\eta_s}$$

where $c_{x,L}$ refers to the quantity consumed of goods from sector x by laborers. Also following Bustos et al. (2016), we assume that a fraction ξ of landowners live and consume locally. Hence, the market clearing condition for services implies

$$Q_s = c_{s,L} L + c_{s,T} \xi T$$

where $c_{s,T}$ is the quantity of services consumed by landowners.

In equilibrium, we obtain the same analytic results on agricultural employment (equation 4) and intermediate input intensity (equation 5). For equilibrium services and manufacturing employment share, we obtain

$$\frac{L_s^*}{L} = \eta_s \left(\frac{w^* + \xi r^* \frac{T}{L}}{A_m p_m} \right) \frac{1}{h^{1-\alpha}} \left(\frac{\alpha}{1-\alpha} \frac{w^*}{p_z} \right)^{-\alpha}$$
$$\frac{L_m^*}{L} = 1 - \frac{L_a^*}{L} - \frac{L_s^*}{L}$$

Hence we find that

$$\frac{\partial L_s^*/L}{\partial L} = -\eta_s \frac{1}{h^{1-\alpha}} \left(\frac{\alpha}{1-\alpha} \frac{w^*}{p_z} \right)^{-\alpha} \frac{T}{L^2} < 0$$

That is, as the population shrinks, so does demand for nontradables.

$$\frac{\partial L_m^*/L}{\partial L} = -\frac{\partial L_g^*/L}{\partial L} - \frac{\partial L_s^*/L}{\partial L} > 0$$

Meanwhile with agricultural employment share shrinking due to the fixed factor of production (land) leading to diminishing marginal returns to labor in the sector, a greater share of labor is employed in manufacturing, which can flexibly expand output with great labor input as inputs Z are not fixed.

Turning to the effects of human capital on sectoral employment allocations, we again find an ambiguous where the sign of the effect depends on the various exogenous parameters. In the service sector, we obtain

$$\frac{\partial L_s^*/L}{\partial h} = \xi \frac{T}{L} \frac{1}{A_m p_m} \left(\frac{1-\alpha}{\alpha} p_z \right)^{\alpha} \frac{r^*}{(w^*)^{\alpha} h^{1-\alpha}} \left(\varepsilon_{r,h} - \varepsilon_{w,h} \right)$$

whose sign depends on whether the elasticity of rents with respect to human capita $\varepsilon_{r,h}$, is greater than the elasticity of wages with respect to human capital, $\varepsilon_{w,h}$. Whether this inequality holds depends in turn depends on the value of the exogenous parameters.

Likewise in manufacturing, sectoral employment changes

$$\frac{\partial L_m^*/L}{\partial h} = -\frac{\partial L_g^*/L}{\partial h} - \frac{\partial L_s^*/L}{\partial h}$$

also depends on parameter values. Hence, the net effect of the demographic transition on sectoral employment is ambiguous, as in the two-sector model presented in the main text.

C Data Appendix

C.1 Industry Classification

In neither the MHSS1 nor the MHSS2 surveys, respondents were not asked directly about their non-agricultural industry of employment. Therefore, we must classify industry using indirect measures. Moreover, because the survey questions differed between waves, we take slightly different approaches to industry classification for each survey round.

MHSS1. We consider a job to be in the agriculture sector if the job was on a farm or in fishing. In particular, the agricultural occupations are, "agriculturalist," "agricultural laborer," "fisherman," "husking/boiling/drying paddy," "goat rearing," "duck/hen rearing," and "produce vegetables/fruits." All other occupations are non-agricultural.

Unfortunately, occupation codes alone do not provide sufficient information about sector of employment. For example, we are unable to allocate most white-collar professions (e.g., accountant) or generic "laborers" to a sector.

MHSS2. As in the MHSS1, a job is in the agriculture sector if the job was on a farm or in fishing.

An individual is considered to work in manufacturing if they work in a factory (in answer to a question about the respondent's place of work), their occupation code matches to factory work, or their work in a craftmaking occupation. Craftmaking occupations are: sheet and structural metal supervisor, moulders and welders, blacksmith or tool maker, handicraft worker (e.g. jewelry, fabrics, pottery, printing, hand embroidery), food processing (e.g. baker, butcher, dried fish maker), woodworking (e.g. treaters, cabinet makers, furniture maker), or garment and related trade workers (e.g. tailor, seamstress, machine embroidery, upholstery, tanning).

We consider a job in the service sector if the occupation corresponds to a purely service occupation, such as healthcare (nurses, doctors, traditional healer), teaching, transportation (rickshaw or van drivers, bus drivers), retail (e.g., shopkeepers), personal service providers (e.g., hair cutters or cobblers), maintenance workers (e.g., plumbers, electricians, appliance repair), social work, or hospitality (e.g., restaurant or hotel workers). In addition, we consider all other occupations to be in the service sector as long as the respondent did not report that the work occurred on a farm or in a factory.