Demographic Transition and Structural Transformation*

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Abstract

We explore the effect of demographic transition on structural transformation. When fertility declines, a fixed factor of land in agriculture may induce a larger share of the population to remain in farming as smaller cohorts age into the labor market. We test this hypothesis at the national, subnational, and household-levels. Abortion policy changes around the world in the last 60 years and across U.S. states in the 19th century, and a village-level quasi-experimentally provided family planning program, generate plausibly exogenous variation in fertility. In each of these three empirical analyses, a drop in fertility raises the agricultural employment share. Improving human capital, however, can offset the effect of fertility drops on the agricultural employment share.

Keywords: Economic growth, fertility, human capital, industrialization.

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1 Introduction

Fertility rates are falling in virtually every country on the planet (Delventhal et al., 2021), reducing population growth. Many macroeconomic growth models emphasize the role of population size and structural transformation in the economic growth process (Lewis, 1954; Gollin et al., 2007; Hopenhayn et al., 2022; Jones, 2022). Yet clear evidence on the long-run impact of fertility decline is lacking.

Agricultural production more intensively uses land, which is fixed. Therefore a reduction in the labor force raises the return to agricultural labor until a larger share of the workforce is employed in agriculture. If, however, nonagriculture more intensively uses human capital, then parents investing more per child given smaller family sizes will raise human capital and thereby nonagricultural employment. Hence, the net effect of a fertility reduction on structural transformation, defined here as workers moving out of agriculture, is ambiguous. In Section 2 we formalize this logic in a simple two sector growth model.

We test whether fertility drives subsequent structural transformation using three approaches. In each approach, we find that falling fertility slows down structural transformation. Hence, the population size effect dominate human capital improvements. We leverage a quasi-experimental program providing households access to contraception and early-childhood vaccinations in Bangladesh to show that cohorts receiving vaccines, which boost human capital, do not work more in agriculture relative to comparison households. Our results imply that governments seeking to transform their economy away from agriculture should pair family planning investments with investments in human capital.

There are two key challenges to testing the relationship between fertility and structural transformation. First, one must find an exogenous shifter of fertility. Second, one must be able to observe employment outcomes decades after the exogenous shock, as cohorts take time to enter the labor market. We overcome these challenges by leverage a variety of contexts and data sources.

We begin our empirical analysis by estimating a cross-country panel regression in Section 3. 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 as in Bloom et al. (2009). To the extent that the timing of abortion policy changes are exogenous, the instrument is valid. Reducing a country's total fertility rate by one child increases agricultural employment share by nearly 6 percentage points 30 years later. The elasticity of fertility to agricultural employment share is approximately equal to unity.

Second, we estimate the long-run effect of abortion restrictions passed by U.S. states in the 19th century in Section 4, following Lahey (2014) and Lahey and Wanamaker (2025). Staggered difference-in-differences estimates reveal that abortion restrictions, which increase fertility, reduce agricultural employment share four decades later. A 10 percent increase in fertility reduces agricultural employment share by 27 percent.

The country- and state-level analyses are informative about the aggregate effects of fertility changes in the face of general equilibrium effects. These analyses, however, face two drawbacks. First, the use of aggregate data precludes analysis of mechanisms at the level of decision makers, such as households or individuals. Second, data comparability across countries, and data quality more than a century ago, complicate our first two exercises. We therefore turn to a third analysis using richly detailed household- and individual-level data.

We estimate the long-run impact of a quasi-random intervention that distributed modern contraception and childhood vaccines 50 years ago in Bangladesh in Section 5. 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 to understand the long-run effect on structural transformation and the corresponding mechanisms.

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. The program started in 1977 primarily distributing modern contraception to women of childbearing age, and introduced intensive child health interventions in 1982 including early childhood vaccines. Treatment was assigned by village, with treatment and control villages well balanced across a wide range of pre-intervention characteristics. The program substantially reduced fertility, and net of mortality declines from vaccines, resulting in relatively smaller cohorts born inside the treatment area during the program period (Joshi and Schultz, 2007). The data facilitate tracing individuals back 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 sectoral hours worked in 2014, 35 years after the program started.

We find that the faster demographic transition induced by the program slowed down the movement of workers out of agriculture. However, this effect took several decades to manifest: we detect no economically significant effect of the program on sectoral employment nearly 20 years after program onset. 35 years after the program started, we see large effects. Treated households allocated 19 percent more time to agriculture, but 11 percent less to manufacturing.

We consider the two key channels highlighted by our model: population size and human capital. We find that household size is a crucial mechanism through which the program affects structural transformation. For every boy not born due to the family planning program, the average household's fraction of work time spent in agriculture nearly triples, while the share of work time spent in the manufacturing sector falls substantially.

Second, 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 intensive child health phase of the MCH-FP to those born before it. Vaccines raised affect cohorts human capital (Barham, 2012; Barham et al., 2021b). Treatment area men born during the intensive child health phase of the program worked more in the service sector where human capital returns are likely higher.

Relevant Literature. Our paper contributes to several literatures. We contribute to the expanding literature on the consequences of fertility decline for economic growth (Jones, 2022; Hopenhayn et al., 2022). Relative to previous work, we highlight a different mechanism—a fixed factor of land in agriculture in a neoclassical framework—and provide empirical test of our framework's predictions.

We are the first to empirically establish a causal link between the demographic transition and structural transformation, two central features of economic development (Kuznets, 1957). 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 (2023), 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.

2 Model

In this section we present a simple model of structural transformation. We consider a small open economy in which goods prices are exogenously determined on world markets. There are two sectors, agriculture and manufacturing, and two factors of production: land and labor. We consider more complex models in Appendix B, but focus on the simplest possible formulation here to highlight the key population size and human capital mechanisms and maintain closed-form equations.

2.1 Setup

Consider a small open economy that trades agricultural and manufacturing goods with the world economy.[1] The economy has L households, each inelastically supplying one unit of labor. Each household is endowed with h units of human capital, which is only useful in the manufacturing sector. There are T units of land in total.

Production of the agricultural output is Cobb-Douglas:

$$Q_g = A_g L_g^{\theta} T_g^{1-\theta} \tag{1}$$

where Q_g is the quantity of agricultural output, A_g is Hicks-neutral agricultural productivity, L_g is the quantity of labor employed in agriculture, and T_g is the quantity of land used in agriculture (equal to T in equilibrium). $\theta \in (0, 1)$ is the labor income share.

Production in manufacturing follows a linear process in labor:

$$Q_m = A_m h L_m \tag{2}$$

where Q_m is the quantity of manufacturing output, A_m is Hicks-neutral manufacturing productivity, L_m is the quantity of labor employed in manufacturing. As in Caselli and Coleman (2001) and Porzio et al. (2022), human capital only yields returns outside of agriculture.[2]

[1] The small open economy assumption obviates the need for modeling demand. We extend the model to include a third non-tradable service sector in Appendix Section B.3 in which we model demand, and show that our main theoretical results 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.

[2] A less restrictive assumption would allow human capital to boost output in both sectors, but moreso in manufacturing. Doing so does not change the main result of the model.

2.2 Comparative Statics

In equilibrium, the agricultural employment share is

$$\frac{L_g}{L} = \left(\frac{\theta p_g A_g T^{1-\theta}}{p_m A_m h}\right)^{\frac{1}{1-\theta}} \frac{1}{L} \tag{3}$$

where the world price of sector x's output is p_x.

We next assess the likely effect of the demographic transition on sectoral employment through the lens of our simple model. Two distinct channels play a role. First, the demographic transition leads to a long-run net reduction in population growth as fertility falls (Delventhal et al., 2021). Second, the stock of human capital may increase 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).

The demographic transition has contrasting effects on agricultural employment. First, a decrease in population L increases agriculture employment share.[3] Second, an increase in human capital decreases agricultural employment share.

The model above is highly stylized. We consider various extensions of the simple model in Appendix Section B. In Appendix Section B.1, we show our results hold when adding an additional factor of production, imported intermediate inputs.[4] Our main results also hold when adding a nontradable service sector and modeling demand, as we show in Appendix Section B.3.

We discuss in Appendix Section B.4 how our results hold when there are trade costs and there is a wedge between wages paid in manufacturing and agriculture. If trade costs are sufficiently high, the economy becomes closed and must rely on local production. Hence, the food problem (Schultz, 1953) becomes salient and reverses our baseline model's prediction: a larger population raises demand for agriculture, thus shifting a greater share of workers into that sector. Hence the relative closedness of the agricultural sector in many developing economies works against our hypothesized population size effect (Matsuyama, 1992; Gollin et al., 2007). If every agricultural sector was perfectly closed, in our model the demographic transition would unambiguously decrease agricultural employment share. Tombe (2015), however, shows a wide range of openness among countries' agricultural sectors, including for developing countries. Moreover, we show in our subsequent empirical analyses that the demographic transition slows down the movement of workers out of agriculture, implying

[3] Even though the population may still be growing, we consider observed population relative to some counterfactual without the demographic transition.

[4] One can instead think of this additional factor as fully adjustable capital, as is the case in the long-run. We further show in Appendix Section B.2 that our main results hold if we allow intermediate inputs and labor to be arbitrarily substitutable.

that agricultural sectors are on average sufficiently open to drive open economy effects.

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.

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:

$$AES_{ct} = \alpha_c + \alpha_t + \beta TFR_{c\tau} + \epsilon_{ct} \tag{4}$$

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 (4) 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.

3.3 Cross-Country Results

We show the results of estimating equation (4) using a variety of samples and specifications in Table 1. Column 1 shows the first-stage: a policy liberalizing abortion policy reduces total fertility by 0.1 children, or 2 percent. This magnitude is quite close to that estimated

Table 1: Effect of Total Fertility Rate on Agricultural Employment Share

	First stage	Dep. var.: Agricultural employment share				
	(1)	(2)	(3)	(4)	(5) Closed	(6) Open
Total fertility rate, t		0.018*** (0.005)				
Total fertility rate, t-30			-0.009 (0.006)	-0.057** (0.027)	-0.048 (0.039)	-0.059* (0.034)
Abortion policy index, t-30	-0.095*** (0.029)					
N	4,192	6,824	4,192	4,192	1,290	2,902
Dep. var. mean	4.724	0.400	0.300	0.300	0.370	0.268
Country FEs	✓	✓	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓	✓	✓
1st-stage F-statistic	10.9			10.9	2.3	9.1

Notes: The table presents regression results at the country-year level. Standard errors clustered at the country-level. Closed defined as absorption in agricultural sector of >95% as of 2005 and measured by Tombe (2015), otherwise considered open. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

by Bloom et al. (2009).

Column 2 shows that a country's agricultural employment share positively correlates with the country's fertility. This positive correlation no longer holds, however, when looking at a country's lagged fertility, as shown in column 3. The effect of lagged fertility on agricultural employment share becomes more negative and statistically significant when instrumenting fertility with the abortion policy index, with the estimated coefficient shown in column 4. A one child reduction in the country's average female lifetime fertility (a reduction of 21% relative to the average TFR of 4.7) raises agricultural employment share by 5.7 percentage points, or about 19 percent.

Matsuyama (1992) notes that the effect of economic fundamentals, such as population, on structural transformation may depend on the how open the country is to trade.[5] In a closed economy, a rise in population means more demand for food from the local agricultural sector, and hence an increase in agricultural employment share. The opposite implication holds if the price of food is set on the international market. We stratify our sample by a country's trade openness within their agricultural sector to test the salience of the prediction of Matsuyama (1992).

We measure openness as the share of domestic agriculture expenditure on domestically

[5] We also discuss the issue of trade openness in our own model in Appendix Section B.4.

produced agricultural output, what we call agricultural absorption. Tombe (2015) computes agricultural absorption for 90 countries in 2005, and finds that while agricultural openness correlates positively with GDP per capita, there is a substantial variation in absorption across the development spectrum. We define a country's agricultural sector as closed if absorption surpasses 95%.

We show our results for closed and open economies in columns 5 and 6 of Table 1. While both coefficients are negative, consistent with Matsuyama (1992), only in open economies does past fertility rises statistically significantly predict a fall in the agricultural employment share. The magnitude is quite close to our baseline coefficient form column 4. In addition, we show that our results are robust to alternative choices of lags, as shown in Appendix Figure A.1. The effect of fertility on agricultural employment share turns statistically significantly negative after 30 years and remains so for any choice of further lags.

In sum, our cross-country results suggest that the demographic transition slows down structural transformation.

4 Regional U.S. Analysis

We next consider a subregional analysis of the long-run effect of abortion policy changes on agricultural employment share. We do so leveraging the tightening of abortion access in the United States during the 19th century.[6]

As surgical abortions became more prevalent in the U.S. in the 1800s, a backlash followed, driving widespread implementation of abortion restrictions across the country. Lahey (2014) finds that the passage of these laws was not correlated to the immigrant population share, literacy rate, pre-law child-to-woman ratio, and, importantly for the present study, urbanization rate. In our baseline estimates, we exclude states which passed abortion restrictions prior to 1840 following Lahey and Wanamaker (2025), as the abortion restrictions were often part of larger bills and not enforced until much later. Lahey (2014) estimates that the abortion restrictions increased fertility by 5 to 15 percent.

To measure agricultural employment share, we use the decadal data compiled by Craig and Weiss (1998) for the period 1800 to 1900. These data are drawn from decennial census tabulations computed by the U.S. Census as well as estimates based on the Census microdata for the 1870 to 1900 waves. Imputations and estimates were necessary, especially so in the

[6] Other U.S. reproductive policy changes may come to mind but are not suitable for our analysis. The liberalization of abortion access in the 1960s and 1970s yields too little across-state variation over time, as most states were treated all at once with the 1973 Roe v. Wade Supreme Court decision. The rollout of oral contraception across the U.S. did not impact fertility, according to Myers (2017), who simultaneously controls for abortion policy changes and carefully codes state laws.

earlier census periods.[7] The dependent variable drawn from these data is the ratio of male agricultural workers ages 10 and older to the total population.[8]

We estimate the causal effect of abortion restrictions on agricultural employment share over time. Specifically, we estimate the staggered dynamic difference-in-differences following De Chaisemartin and d'Haultfoeuille (2020). Each abortion policy's passage is associated to the subsequent decennial census wave.

Figure 1 shows the resulting event study plot of our estimates. There are no differential trends in agricultural employment share prior to the implementation of abortion restrictions. After restrictions are in place, a negative effect on agricultural employment share begins to emerge, becoming statistically significantly negative four decades later. The delayed effect is consistent with the fact that affected cohorts must age into the labor market, and mirrors our findings in the cross-country estimates shown in Section 3. The implication is that increased fertility—a slower demographic transition—speeds up the movement of workers out of agriculture. In terms of the magnitude, agricultural employment share falls by almost 5 percentage points four decades after abortion was restricted, a 27% reduction. If the average abortion policy reduced fertility by 10%, the midpoint of the estimates by Lahey (2014), then the resulting long-run fertility-agricultural employment share elasticity is 2.7, very close to the implied elasticity observed in the cross-country estimates from Section 3.

We conduct two robustness checks of our main results. Appendix Figure A.3 shows the event study plot when including abortion restriction laws passed prior to 1840. With more census waves, we see that the negative effect of abortion restrictions on agricultural employment share persists for several decades. Appendix Figure A.4 plots estimates for our baseline sample but controlling for state-specific linear trends. While the estimates become noisier, the point estimate four decades after abortion was restricted remains negative.

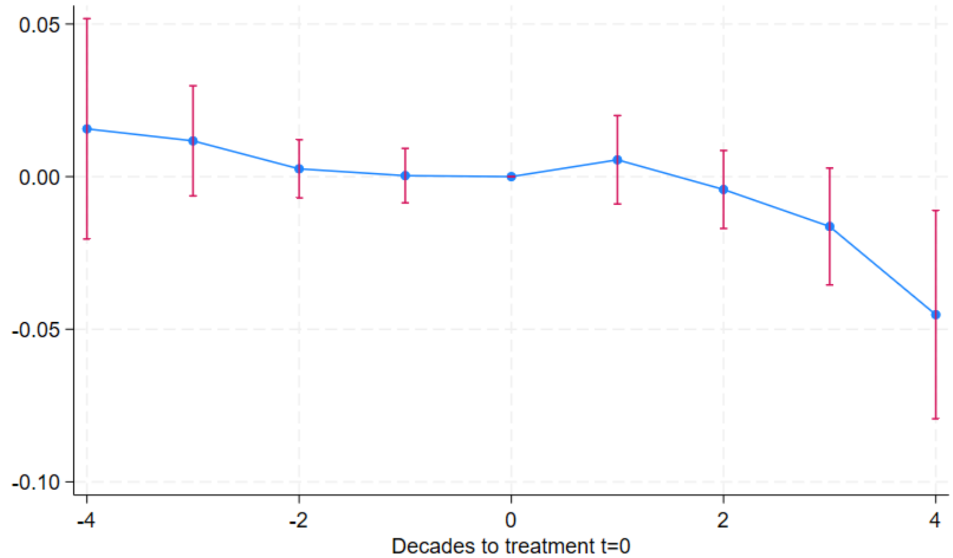
5 Bangladesh Natural Experiment

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.

[7] We redo the estimation using the 1850–1940 full count census waves to construct agricultural employment share and our results do not change; see Appendix Figure A.2.

[8] We focus on male employment since female farm employment was likely substantially under-measured in official Census tabulations (Ngai et al., 2024).

Figure 1: Effect of Abortion Restriction on Agricultural Employment Share, U.S. States



Notes: Data on state-level agricultural employment shares 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restrictions come from Lahey (2014) and Lahey (2014). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

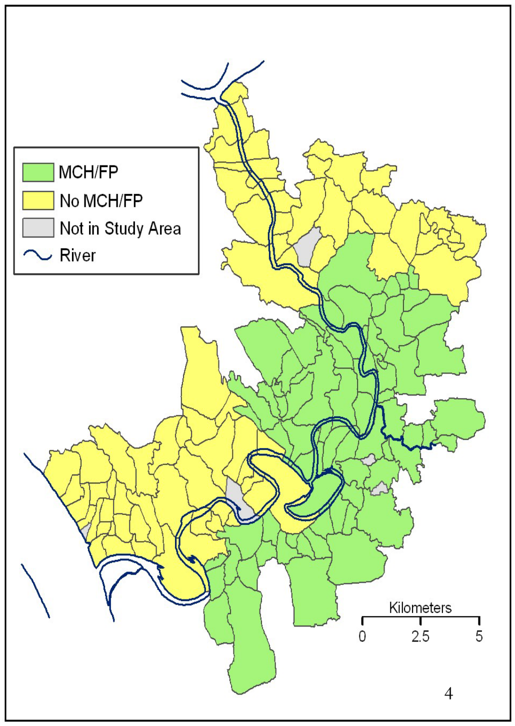
Program interventions were rolled out over time starting with access to and advice on using modern contraception for women and tetanus toxoid vaccines for pregnant women. Intensive child health interventions started in 1982 with the measles vaccine and other child health interventions were introduced 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 5.2.1 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).

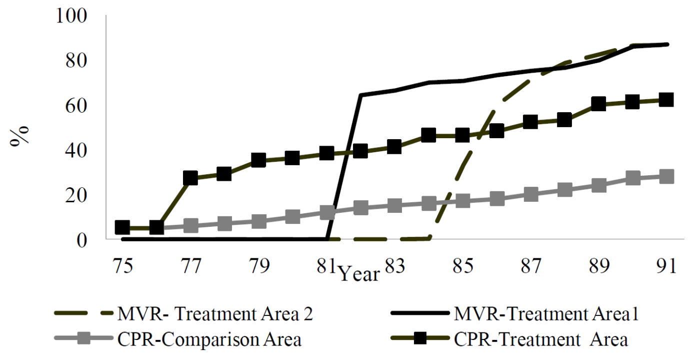
Table 2: MCH-FP Interventions by Cohort

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

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)

was an almost entirely unvaccinated population (Koenig et al., 1991). Nationally, measles vaccination for children under the age of five was less than 2 percent in 1986 (Khan, 1998) and was below 40 percent in the comparison area in 1990 (Fauveau, 1994).

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

Previous research demonstrates that the MCH-FP program had significant effects on fertility and human capital. 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).[9]

5.1 Data and Treatment Assignment

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.[10] 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, al-

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

[10]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.

lowing the the linkage of individuals and households from the Matlab area across time and with their parents over the past thirty-five years. In addition, the 1974 census allows one to test pre-intervention balance. The DSS data are collected bi-weekly or monthly and allow determination of exact birth dates and birth place, key inputs to our assignment of treatment status as we detail below. There are few, if any, other study sites that have similarly rich data availability to allow for this type of long-term evaluation.

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

5.2 Empirical 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.

5.2.1 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,[11] 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

[11]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.

and show that the two areas are similar across a wide variety of household and household head characteristics.

5.2.2 Empirical Specification

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

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

where T_h is an indicator for whether household h is considered treated (as defined in Section 5.1) 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:

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

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 **??**; $\alpha_{y(i)}$ 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.

5.3 Main 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.

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.[12]

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 0 percentage points (SE=2.2). The effect of the program on non-agricultural employment is similarly small, with an estimated effect of 0.7 p.p. (SE=2.2).

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 3.9 p.p. (SE=1.4 p.p), representing a 19 percent increase over the comparison area (column 1). The share of household members in manufacturing fell by 2.2 p.p. (SE=1.4), an 11 percent fall relative to comparison households (column 2). In services, we find a very small effect of -1 p.p. (SE=1.8), a 2 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

[12]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	
Treatment	-0.000	0.007	
	(0.022)	(0.022)	
% chg. rel. to mean	-0.0	2.0	
Mean	0.67	0.36	
Baseline controls	Y	Y	
Embankment control	Y	Y	
Observations	2534	2534	
PANEL B: MHSS2 (2012-2015)			
	(1)	(2)	(3)
	Agriculture	Manufacturing	Services
Treatment	0.039***	-0.022	-0.010
	(0.014)	(0.014)	(0.018)
% chg. rel. to mean	18.8	-10.9	-2.0
Mean	0.21	0.20	0.48
Baseline controls	Y	Y	Y
Embankment control	Y	Y	Y
Observations	2488	2488	2488

Notes: The table presents estimates of equation 5 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.

and the early-childhood years. Indeed, one must observe outcomes well after the affected cohorts have entered the labor market.

5.4 Mechanisms

We take advantage of the richness of the household data from Matlab to examine the mechanisms driving the main effects. The model outlined in Section 2 posits two key mechanisms which may work in opposite directions: population size and human capital.

5.4.1 Role of Family Size

We start by 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.[13,14]

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

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

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 5. 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.

5.4.2 Human Capital

Following the examination of household-level effects, we report individual-level differences in employment outcomes, estimated using equation 6. 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

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

[14] 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 5: 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.

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.

Table 6 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).

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.

5.4.3 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 5 by sector, but further split the dependent variable of work hours share by rural and urban location of employment.

We report results in Table 7, 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: Individual-Level

PANEL A: Men	Share hours by sector			
	(1)	(2)	(3)	(4)
	Agriculture	Manufacturing	Services	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)	(2)	(3)	(4)
	Agriculture	Manufacturing	Services	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. The dependent variable for all regressions is the fraction of total hours worked by sector. See Appendix C.1 for more details on how we classify workers into sectors. Industry employment shares do not sum to 1 due to a small set of respondents not providing sufficient information to classify them into sectors, as well as a fourth, very small, mining sector. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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

	(1) Urban Agriculture	(2) Urban Manufacturing	(3) Urban Services	(4) Rural Agriculture	(5) Rural Manufacturing	(6) Rural Services
Treatment	0.008	-0.028***	-0.008	0.031**	0.006	-0.002
	(0.005)	(0.010)	(0.020)	(0.014)	(0.009)	(0.017)
% chg. rel. to mean	205.6	-18.4	-3.3	15.4	12.7	-0.7
Mean	0.00	0.15	0.24	0.20	0.05	0.24
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y	Y
Observations	2488	2488	2488	2488	2488	2488

Notes: The table presents estimates of equation 5 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. The dependent variable is the share of hours worked within the household in different sectors and in different locations. 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.

5.4.4 Entrepreneurship

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 Appendix 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 Appendix 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.[15]

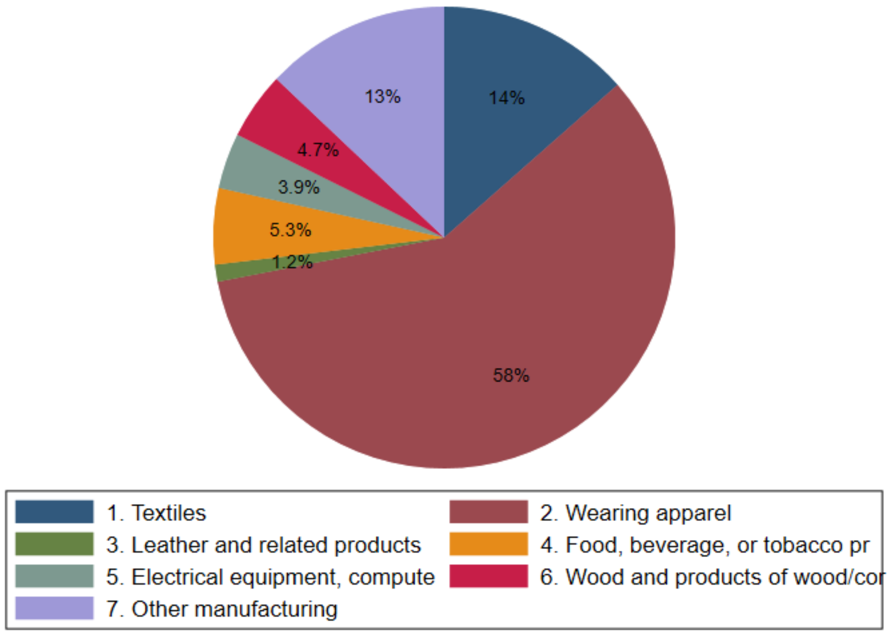
5.4.5 Agricultural Adjustment

We next examine household-level effects of the program on agriculture in Tables 8 and **??**.

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

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

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



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

Table 8: ITT Effects of MCH-FP on Farming and Land Ownership

	MHSS1 (1996)		MHSS2 (2012-2014)	
	(1)	(2)	(3)	(4)
	=1 if household farms	Acres owned per cap.	=1 if household farms	Acres owned per cap.
Treatment	0.010	-0.059	0.032*	-0.000
	(0.027)	(0.044)	(0.017)	(0.006)
% chg. rel. to mean	1.5	-18.4	4.0	-0.3
Mean	0.69	0.32	0.80	0.10
Baseline controls	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y
Observations	2525	2518	2488	2486

Notes: The table presents estimates of equation 5 for outcomes aggregated to the MHSS1 household-level and measured in 1996 (columns 1 and 2) and 2014 (columns 3 and 4). Variable means refer to the comparison area. Standard errors are clustered by the 1996 household head's pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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 3.2 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 how farmers' crop mix changes in response to the program. We show the results in Appendix Table A.4. We find that the program induced a shift towards crops which yields larger output per laborer. That is, farmers shifted away from rice and into potatoes. This is consistent with the reduced labor supply in treatment households and villages compelling farmers away from labor intensive crops.

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

We show our results in Table A.5, estimated on the subset of households which grow crops. In columns 1 and 2 we look at the effect on potential revenue per acre, while we estimate the effect on profits per acre in columns 3 and 4. Across all outcomes, we can not statistically rule out a null effect. If anything, the effects are negative. This result is consistent with our individual-level estimates in Table 6 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.6, 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.

5.5 Robustness

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.7. 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.8. 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.9, 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.10. 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.11, we show that our results are largely unchanged when we remove households who resided in Pourashava prior the intervention.

6 Conclusion

This paper provides the first direct empirical evidence on the effect of the demographic transition on structural transformation. We assess this empirical relationship in varying contexts, using distinct sources of exogenous variation and levels of aggregation. The key takeaway is that fertility reductions slow down structural transformation, but sufficient human capital investments can offset this effect. Policymakers should therefore take care to pair family planning programs with education and public health investments that raise human capital.

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Appendix

A Additional Tables and Figures

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

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

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

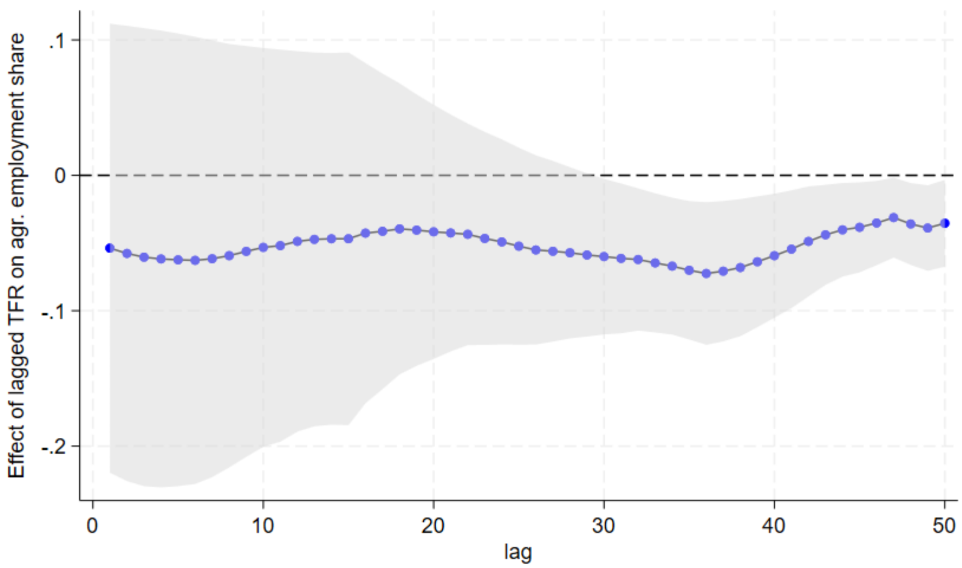
Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Each dependent variable is the share of household members exhibiting the described behavior. The dependent variable of column 4 refers to the share of household members who ever worked in a 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.

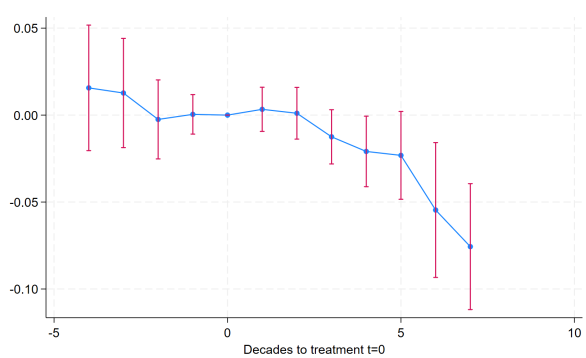
Figure A.1: Effect of Fertility on Agricultural Employment, Various Lags



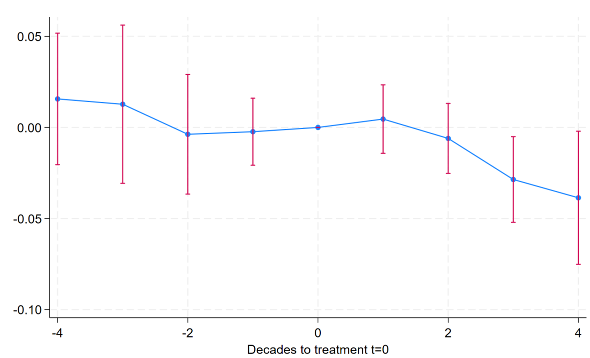
Notes: Each point on the chart depicts the point estimate from a different regression of lagged total fertility rate (TFR) on agricultural employment share, in which TFR is lagged between 1 and 50 years. TFR is instrumented for the abortion policy index described in Section 3.2 in every regression.

Figure A.2: Effect of Abortion Restrictions on Agricultural Employment Share, U.S. States, Full Count Census 1850–1940

(a) all abortion policies



(b) post-1840 abortion policies



Notes: Data on state-level agricultural employment shares 1800-1840 comes from Craig and Weiss (1998). Agricultural employment shares for 1850–1940 computed from Ruggles et al. (2024). Timing of abortion restriction laws come from Lahey (2014) and Lahey (2014). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

Table A.4: ITT Effects of MCH-FP on Crop Choice

	Avg. Yield (Kg/Labor)	Grew Crop	Conditional on growing crop		
			Used HYV Seeds	Used Capital	Cost of Market Inputs
	(1)	(2)	(3)	(4)	(5)
Dal		-0.004	-0.247	0.106*	-5.83
		(0.011)	(0.175)	(0.062)	(4.25)
		[0.038]	[0.479]	[0.960]	[17.89]
Jute	14.5	0.024	0.043	-0.013	-18.28*
		(0.019)	(0.069)	(0.021)	(9.79)
		[0.102]	[0.448]	[0.985]	[64.35]
Maize	82.3	0.092***	0.142	0.062	17.48
		(0.030)	(0.043)	(0.022)	(11.86)
		[0.117]	[0.621]	[0.934]	[72.17]
Mustard	28.2	-0.004	0.093	-0.012	-8.73
		(0.019)	(0.062)	(0.008)	(7.50)
		[0.150]	[0.311]	[1.000]	[43.45]
Onion	100.5	0.013*	0.004	0.076	-0.33
		(0.007)	(0.253)	(0.142)	(10.63)
		[0.017]	[0.523]	[0.870]	[12.10]
Paddy Aman	18.8	-0.040	-0.063	-0.021	18.46
		(0.030)	(0.047)	(0.014)	(12.94)
		[0.196]	[0.298]	[0.992]	[84.45]
Paddy Aus	16.5	-0.014	-0.050	0.002	15.47
		(0.027)	(0.054)	(0.021)	(11.24)
		[0.114]	[0.214]	[0.980]	[79.32]
Paddy Boro	29.8	0.015	0.070	0.014	-6.18
		(0.026)	(0.044)	(0.010)	(21.62)
		[0.486]	[0.461]	[0.978]	[178.56]
Potato	83.2	0.094***	0.020	0.017*	-1.49
		(0.032)	(0.065)	(0.009)	(67.75)
		[0.125]	[0.425]	[0.982]	[369.24]
Vegetable		0.016*	0.087	0.078	5.52
		(0.008)	(0.114)	(0.065)	(26.87)
		[0.036]	[0.490]	[0.914]	[61.15]
Wheat	69.4	0.014*	-0.430	0.031	-20.24
		(0.007)	(0.419)	(0.048)	(15.52)
		[0.008]	[0.500]	[1.000]	[31.16]
Other		0.001	-0.025	0.026	0.20
		(0.011)	(0.096)	(0.033)	(18.36)
		[0.062]	[0.293]	[0.939]	[54.20]

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

	(1) Revenue per acre (min. price)	(2) Revenue per acre (max. price)	(3) Profit per acre (min. price)	(4) Profits per acre (max. price)
Treated	-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.6: 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.7: 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 5 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.8: 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	Y	Y	Y
Observations	2580	2580	2580

Table A.9: 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 5 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.10: 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

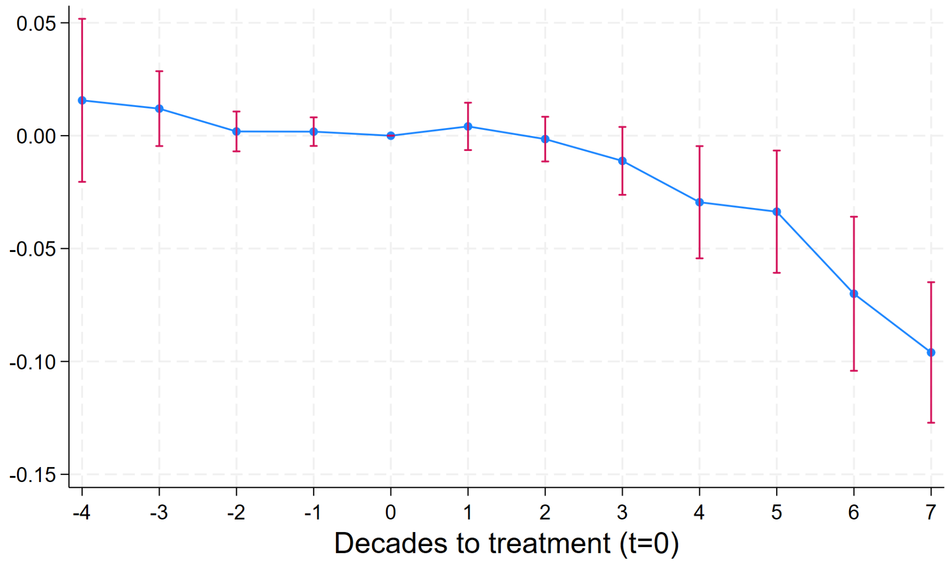
Notes: The table presents estimates of equation 5 for outcomes at the MHSS1 household-level, for Muslim households only. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Panel A refers to the 1996 MHSS1, while Panel B refers 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.11: 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

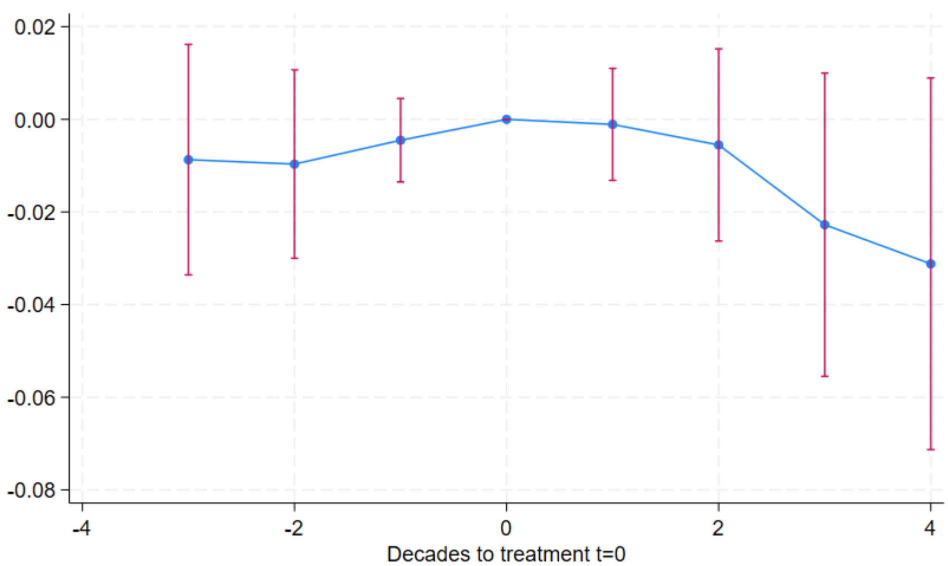
Notes: The table presents estimates of equation 5 for outcomes at the MHSS1 household-level, excluding individuals whose pre-program village is Matlab town. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Panel A refers to 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.

Figure A.3: Effect of Abortion Restrictions (including those passed before 1840) on Agricultural Employment Share, U.S. States



Notes: Data on state-level agricultural employment shares 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey (2014). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

Figure A.4: Effect of Abortion Restrictions (including those passed before 1840) on Agricultural Employment Share, U.S. States, Controlling for State-Trends



Notes: Data on state-level agricultural employment shares 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey (2014). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

B Theoretical Appendix

In this section, we provide several extensions to our simple baseline model from Section 2.

B.1 Adding Intermediate Inputs

Assume the production function in agriculture is

$$Q_a = A_a Z_a^{\theta_z} L_a^{\theta_\ell} T_a^{1-\theta_z-\theta_\ell},$$

and in manufacturing, it is

$$Q_m = A_m Z_m^{\alpha} (L_m h)^{1-\alpha}, \tag{B.1}$$

where Z_a and Z_m are imported intermediate inputs used in each sector. The exogenous price of this input is p_z. One can think of the intermediate inputs as imported capital in the long-run (in which capital is fully adjustable) or as materials used in production.

The first order conditions imply that

$$\frac{w}{p_z} = \frac{\theta_\ell}{\theta_z}\frac{Z_a}{L_a} = \frac{1-\alpha}{\alpha}\frac{Z_m}{L_m}.$$

The wage is then

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

and the agricultural employment share is

$$\frac{L_a^*}{L} = \left[\frac{(p_a A_a)^{\frac{1}{1-\theta_z}} \theta_\ell \theta_z^{\frac{\theta_z}{1-\theta_z}} T^{\frac{1-\theta_z-\theta_\ell}{1-\theta_z}}}{p_z^{\frac{\theta_z}{1-\theta_z}} (p_m A_m)^{\frac{1}{1-\alpha}} (1-\alpha) \left(\frac{\alpha}{p_z}\right)^{\frac{\alpha}{1-\alpha}} h}\right]^{\frac{1-\theta_z}{1-\theta_\ell-\theta_z}} \frac{1}{L}.$$

As in the baseline model, $\frac{\partial L_a/L}{\partial L} < 0$ and $\frac{\partial L_a/L}{\partial h} < 0$.

B.2 Adding Intermediate Inputs and CES Functional Form

In Section B.1 we assumed that the elasticity of substitution between labor and intermediate inputs is equal to one. It may be more realistic, however, to allow for a substitution elasticity different than one, as suggested by Herrendorf et al. (2015) and Boppart et al. (2023).

Production of the manufacturing good is the same in Equation (B.1). 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:

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

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

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

where $[\cdot]$ is the CES portion of equation (B.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.

B.2.1 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,

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

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:

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

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

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

B.2.2 Comparative Statics

We next assess the effect of the demographic transition on sectoral employment. As with our baseline model, we find contrasting effects of each channel on agricultural employment:

EMPIRICAL PREDICTION.

(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.

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:

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

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. The product of these two terms, $\left(\frac{\omega}{1-\omega}\right)^{\epsilon}(w^*/p_z)^{\epsilon-1}$, is equal to 1 when agriculture is produced using a Cobb-Douglas production function. That is, when $\omega = 0.5$ and $\epsilon = 1$, as we assume for the manufacturing sector. Hence, the term on the left of inequality (B.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 $1-\theta$. This term equals 1 in manufacturing, in which $\epsilon = 1$ 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 (B.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.[16]

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 whether our baseline model predictions change with an addition that requires modeling demand.

Agricultural production is defined by equation (1). Service sector production is linear in human capital-augmented labor:

$$Q_s = A_s h L_s$$

We modify the manufacturing production function to allow for differential returns to human capital in manufacturing relative to services:

$$Q_m = A_m h^{\alpha} L_m$$

where $\alpha > 0$ determines the return to human capital in manufacturing relative to services. If $\alpha < 1$, human capital has a higher return in services.

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

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

[16]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.

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 3) and therefore the same effect of changes in population size and human capital as in our baseline. For equilibrium services and manufacturing employment share, we obtain

$$\frac{L_s^*}{L} = \eta_s + \frac{r^*}{w^*}\xi\frac{T}{L}$$
$$\frac{L_m^*}{L} = 1 - \frac{L_g^*}{L} - \frac{L_s^*}{L}$$

where $r^* = (1-\theta)\theta^{\frac{\theta}{1-\theta}}\frac{(p_g A_g)^{\frac{1}{1-\theta}}}{(p_m A_m h^\alpha)^{\frac{\theta}{1-\theta}}}$ is the equilibrium rental rate of land paid to landowners, and $w^* = p_m A_m h^\alpha$ is the equilibrium wage.

Hence

$$\frac{\partial L_s^*/L}{\partial L} = -\frac{r^*}{w^*}\xi\frac{T}{L^2} < 0$$

As the population shrinks, so does demand for nontradables, and hence for nontradable employment. For manufacturing, an increase in the population reduces its employment share:

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

This is because land being fixed implies a diminishing marginal returns to labor in agriculture, a greater share of labor is employed in manufacturing, which can more flexibly expand output with more labor input.

Turning to the effects of human capital on sectoral employment allocations, in the service sector the effect of an increase in human capital is unambiguously positive. Contrarily, in manufacturing, sectoral employment changes depend on the strength of changes in services relative to agriculture, and hence on parameters:

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

B.4 Partially Closed Economy with Wage Wedge

The effect of population on structural transformation necessarily depends on whether the economy is open or closed (Matsuyama, 1992). Our baseline model assumes a fully open economy, but the predicted effect of population size on agricultural employment share would be reversed if the economy were fully closed, as the food problem dominates. In this section, consider the implications of nesting both closed and open economy cases by introducing trade costs and of introducing a wedge between the wage paid in agriculture relative to nonagriculture.

No arbitrage implies that if sector x is exporting, then $P_x^W = P_x\tau$ otherwise, if sector x is importing, then $P_x^W = P_x/\tau$.

The price P_x is knowable with the following steps: (i) solve for the price P_x^{closed} when the economy is closed. (ii) compare P_x^{closed} to P_x^W to determine if m is exported or imported. (iii) set $P_x = P_x\tau$ if x is exported or $P_x = P_x/\tau$ if x is imported.

Hence, the equilibrium price of sector x's output is

$$P_x^* = \begin{cases} P_x^{cl} & \text{if } \tau P_x^W \geq P_x^{cl} \geq P_x^W/\tau \text{ (closed)} \\ \tau P_x^W & \text{if } \tau P_x^W < P_x^{cl} \text{ (importing)} \\ P_x^W/\tau & \text{if } P_x^W/\tau > P_x^{cl} \text{ (exporting)} \end{cases} \tag{B.7}$$

where P_x^W is the world price, P_x^{cl} is the prevailing local price given a closed economy, and τ is the iceberg trade cost.

If the agricultural sector is closed, consistent with Matsuyama (1992), the predicted effect of population size reverses. A larger population induces a higher agricultural employment share in order to feed the population. If the agricultural sector imports or exports, then consistent with our baseline model a greater population induces a lower agricultural employment share.

The wedge between agricultural and nonagricultural wages simply adds a multiplicative term to the equilibrium equations, and therefore does not impact the predictions derived from the comparative statics.

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.