Demographic Transition and Structural Transformation*

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Abstract

We explore the effect of demographic transition on structural transformation. When fertility declines, a larger share of the population may remain in farming due to agriculture's reliance on a fixed factor of production, land. We test this hypothesis at the household, state, and country levels. A quasi-experimental family planning program provided to Bangladeshi households, and abortion policy changes across U.S. states in the 19th century and around the world in the last 60 years, generate plausibly exogenous variation in fertility. In each of these three empirical analyses, lower fertility raises the agricultural employment share decades later. Improving human capital can offset the effect of fertility declines on the agricultural employment share, though more human capital is required in less developed countries. Our findings suggest that family planning policies, without complementary investments in human capital, may inadvertently hinder structural transformation and economic development.

Keywords: Fertility, human capital, industrialization, economic growth.

JEL Categories: O1, O41, J1.

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1 Introduction

Economic growth is characterized by two fundamental processes: the demographic transition, marked by declining fertility and mortality, and structural transformation, in which workers shift from agriculture to manufacturing and services. While a large literature explores how economic development and structural transformation influence fertility and mortality patterns (Galor and Weil 1996, 2000; Chatterjee and Vogl 2018; Ager et al. 2020), less is known about how demographic change—particularly fertility decline—affects structural transformation. As fertility declines in virtually every country (Delventhal et al. 2021), and with global population expected to begin falling within the next 60 years (United Nations 2024), understanding the role of declining fertility on structural transformation is increasingly important. We provide novel evidence, exploiting rare policy variation that occurred far enough in the past to observe long-run structural change.

Theoretically, the effect of fertility decline on structural transformation is ambiguous. On the one hand, in Malthusian models where agriculture depends on land—a fixed factor of production—a declining population reduces land congestion, raising returns to labor in farming and slowing the shift of labor out of agriculture (Malthus 1798; Lewis 1954). While endogenous technological progress may offset land congestion effects as fertility falls (Boserup 1965; Galor and Weil 2000), the pace of innovation appears to be slowing (Bloom et al. 2020), and many countries face barriers to adopting frontier technologies (Gancia and Zilibotti 2009; Buera and Oberfield 2020). On the other hand, fertility decline may increase human capital investment through the quantity-quality tradeoff (Barro and Becker 1989), and if nonagricultural sectors rely more heavily on skilled labor, rising human capital may accelerate structural transformation. Whether the Malthusian land congestion effect or human capital channel dominates is therefore an empirical question.

This paper examines the impact of fertility decline on structural transformation both theoretically and empirically. We first present a stylized two-sector, overlapping generations model in Section 2 whereby parents choose both the number of children and the level of human capital investment per child. Fertility is endogenous: parents derive utility from sexual activity, which may lead to costly children. Parents can reduce the number of children they have through the use of family planning technology. As the family planning technology becomes more accessible, fertility declines and human capital investment increases in subsequent generations. When these smaller cohorts enter the labor force, land congestion falls, raising the marginal product of labor in agriculture and drawing some workers into agriculture. At the same time, higher returns to human capital incentivize movement into non-agricultural employment.

Testing the predictions of our model and disentangling the mechanisms presents several challenges. First, fertility may be endogenous to economic development and the sectoral composition of the labor market. Second, the effects of fertility decline on labor supply emerge only gradually, as smaller cohorts age into the workforce, requiring long-run data. Third, identifying underlying mechanisms, particularly the role of human capital, necessitates individual- or household-level data to credibly link intermediate outcomes to sectoral shifts. Fourth, partial equilibrium effects may differ from those in general equilibrium, as fertility changes may induce changes in prices and wages.

We address these challenges in three complementary empirical analyses. First, we exploit exogenous variation in fertility from a landmark family planning and health program in Bangladesh. The Bangladesh context offers rich household and individual data over a long-time horizon, allowing us to examine mechanisms at the level of the decision maker. Second, we examine fertility decline at the country level by using changes in abortion policy in recent decades. Third, we examine state-level abortion policy changes in 19th-century United States. By combining a micro-level setting that identifies household mechanisms with two aggregate settings that capture long-run general equilibrium adjustments, we provide the first evidence on the causal effect of fertility decline on structural transformation.

Findings across all three empirical strategies demonstrate that lower fertility slows structural transformation. These results suggest that the population-size effect dominates the human capital channel. Hence, governments aiming to accelerate structural change through fertility reduction alone may face slower progress unless they simultaneously invest in human capital.

We begin our empirical analysis in Section 3 with a partial equilibrium experiment with rich household-level data including the information on the two key mechanisms. In particular, we leverage the quasi-randomized placement of a family planning intervention in rural Bangladesh: the Maternal and Child Health and Family Planning (MCH-FP) program. Launched in 1977, the MCH-FP first introduced modern contraception and later expanded to include preventive child health services such as immunizations which improved adolescent human capital (Barham 2012). The program accelerated the demographic transition by reducing fertility and improving child survival (Phillips et al. 1982; Joshi and Schultz 2007). Treatment was assigned at the village level, with strong baseline balance between treated and comparison areas. We exploit this design to estimate single difference intent-to-treat (ITT) effects and use detailed longitudinal microdata spanning four decades to examine long-run impacts of the MCH-FP program on structural transformation and the corresponding mechanisms.

The program-accelerated demographic transition slowed structural transformation. Thirtyfive years after implementation, treated households allocated 19 percent higher labor hours share to agriculture and 12 percent less to manufacturing.

We explore two primary mechanisms: land congestion and human capital. On land congestion, we estimate that having one fewer adult male in the average household raises the share of household work time spent in agriculture by 26 percentage points. In levels, however, the one fewer male household member decreases total household hours worked in manufacturing and services, with little effect on agricultural labor, suggesting that agricultural labor is fixed in quantity proportional to the available land.

Second, we estimate the effect of human capital using the rollout of the MCH-FP program over time leading to variation in exposure to the intensive child health phase of the program. Men born in the treatment area during this phase achieved higher levels of schooling and learning compared to similarly aged men in the comparison area (Barham 2012; Barham et al. 2025). We provide suggestive evidence that they were also more likely to work outside agriculture, particularly in the service sector, where returns to skill are higher. These findings indicate that human capital investments can help mitigate Malthusian land congestion.

To determine whether the effects of fertility decline on structural transformation persist in the presence of general equilibrium effects, we extend the analysis to two additional settings in Section 4. First, we use a cross-country event study of changes in abortion access since 1960 to capture the broad macroeconomic response to fertility shocks, including endogenous adjustments in prices, wages, and technology. Second, recognizing the limitations of cross-country analysis—such as harmonizing data across countries (Durlauf et al. 2005) and potential omitted variable bias—we turn to a subnational-level analysis using historical variation in abortion policy across U.S. states in the 19th century to estimate within-country effects. Together, these settings allow us to assess the external validity of the Bangladesh findings in the face of general equilibrium effects.

In the cross-country analysis, we estimate an event study of the effect of abortion access on the agricultural employment share following De Chaisemartin and d'Haultfoeuille (forthcoming) to account for the staggered timing of abortion policy changes across countries. A nearly one standard deviation increase in abortion accessibility is associated with an approximately 7 percentage point increase in the agricultural employment share 2-4 decades later. These results align with the Bangladesh evidence: fertility decline slows the reallocation of labor out of agriculture. They also suggest that in the long run the land congestion mechanism dominates the quantity-quality tradeoff in the aggregate.

The subnational-level analysis examines the staggered adoption of abortion restrictions across U.S. states during the 1800s. We again use an event-study design and estimate staggered dynamic difference-in-differences. Consistent with our other analyses, we find that abortion restrictions accelerate structural transformation in subsequent decades. On average, 19th century abortion restrictions, which increased fertility rates (Lahey 2014), decreased the agricultural employment share by about 3 percentage points three decades later, consistent with our cross-country results.

Finally, we use the model and estimated elasticities from the Bangladesh quasi-experiment to make two back-of-the-envelope calculations. In our Bangladesh context we compute that human capital would need to increase by a factor of three to offset Malthusian land congestion effects. Next, we consider variation in required offsetting human capital investment across the development spectrum, based on the fact that in agriculture the land cost share falls with economic development (Boppart et al. 2023). A low-income country like Bangladesh would need to raise human capital over 3.5 times as much as a high-income country to offset the effect of a given population decrease on agricultural employment share.

This paper makes three key contributions to the growing literature on the consequences of fertility decline on economic growth (Ashraf et al. 2013; Cavalcanti et al. 2021; Jones 2022; Hopenhayn et al. 2022). First, we provide causal evidence on the effect of demographic transition on structural transformation, two central processes of economic development (Kuznets 1957).[1] While a large literature studies how structural transformation and productivity growth affect fertility and the demographic transition (Greenwood and Seshadri 2002; Wanamaker 2012; Ager et al. 2020), fewer studies investigate the reverse relationship: how population growth and fertility decline shape structural transformation. Two notable exceptions in economic history using calibrated growth models are Voigtländer and Voth (2013) and Leukhina and Turnovsky (2016).[2] Gollin and Rogerson (2014) and Herrendorf et al. (2012) quantitatively explore the role of transportation infrastructure facilitating population movements and thereby structural transformation. However, these papers rely on calibrated macroeconomic models, inhibiting causal identification of the effects of fertility on structural transformation and of the corresponding mechanisms at the household level.[3]

Second, theoretically, we develop a model which explicitly considers endogenous fertility featuring the quantity-quality tradeoff and family planning with multiple production sectors, allowing an investigation of the aggregate structural transformation effect of family planning

[1] Li and Zhang (2007) estimate the effect of fertility decline on economic growth in the context of China's one child policy. Their identification strategy relies on regional over-time changes in the ethnic minority population, which itself is likely to be endogenous as workers migrate to faster growing regions.

[2] Voigtländer and Voth (2013) abstracts from human capital, and therefore the quantity-quality tradeoff in their model, which applies to the pre-modern period. Leukhina and Turnovsky (2016) assumes exogenous population. Relative to both papers, our model allows for endogenous population growth, a quantity-quality tradeoff, and a role for family planning technologies to affect fertility.

[3] Fertility and agricultural employment may comove due to confounding factors such as changes in skill-biased technical change, which alter the returns to child quality investments and to employment in agriculture.

interventions. The few growth models jointly featuring endogenous fertility, the quantity-quality tradeoff, and family planning technologies do not include multiple sectors, nor a fixed factor of production.[4] As a result, the Malthusian land congestion mechanism and structural transformation that we emphasize is absent in their models.

Third, we highlight the role of land—a fixed factor in agricultural production—as a countervailing force that limits the growth-enhancing potential of fertility decline.[5] While unified growth theory does feature land and hence land congestion with endogenous fertility, technological progress outstrips the power of land congestion to propel growth (Galor and Weil 2000; Galor 2005). Our theory emphasizes the role of land congestion, and we empirically estimate the strength of the land congestion force in holding back structural transformation and hence growth. We further demonstrate empirically that the land congestion mechanism is stronger than that of the quantity-quality tradeoff.

Finally, we contribute to the literature on the child quantity-quality tradeoff by quantifying the net effect of fertility decline and the accompanying human capital increase on structural transformation. Consistent with Rosenzweig and Zhang (2009), we estimate that the endogenous human capital investment response to declining fertility is modest.[6] A quantitative analysis by Cheung (2023) on the importance of fertility decline and the associated human capital rise does not feature land in agricultural production, and hence abstracts away from the Malthusian land congestion mechanism that we focus on in this paper.

2 Model

In this section we present a simple model of structural transformation. There are two sectors, agriculture and manufacturing, and two factors of production: land and labor. Overlapping generations live together in households in which parents decide the quantity and education of children. Parents enjoy engaging in sex, and can reduce the likelihood of having children by purchasing contraception. We consider the effects of reducing the cost of accessing contraception on human capital investment and agricultural employment share.

[4]These include models developed by Strulik (2017) and Cavalcanti et al. (2021).

[5]We implicitly assume that moving workers out of agriculture is growth-enhancing. This is consistent with extensive empirical evidence on productivity wedges between agriculture and non-agricultural sectors (see, for example, Gollin et al. (2014)). In our model in Section 2 we capture this misallocation with a reduced-form wage wedge, in which workers are paid above their marginal product in agriculture but not in non-agriculture.

[6]The MCH-FP program in Bangladesh provided a package of interventions targeting both family planning and child health. The child health interventions improve child quality along with the family planning induced quantity-quality tradeoff. Given that we find that land congestion is stronger than the child quality channel, this suggests that the quantity-quality tradeoff's effect on human capital is even smaller than the total effect that we estimate, and therefore weaker than the effect of land congestion.

2.1 Setup

2.1.1 Production

Consider a small open economy that trades agricultural and manufacturing goods with the world economy.[7] In total there are T units of land, which are only used in agriculture.

Production of agricultural output is Cobb-Douglas:

$$Q_{at} = A_{at} L_{at}^{\theta} T_{at}^{1-\theta} \tag{1}$$

where Q_{at} is the quantity of agricultural output at time t, A_{at} is Hicks-neutral agricultural productivity, L_{at} is the quantity of labor employed in agriculture, and T_{at} is the quantity of land used in agriculture (equal to T in equilibrium). $\theta \in (0, 1)$ is the labor income share in agriculture. Land rents are paid to absentee landlords.

Production in manufacturing is linear in labor:

$$Q_{mt} = A_{mt} h_t L_{mt} \tag{2}$$

where Q_{mt} is the quantity of manufacturing output, A_{mt} is Hicks-neutral manufacturing productivity, L_{mt} is the quantity of labor employed in manufacturing.[8] As in Caselli and Coleman (2001) and Porzio et al. (2022), per household human capital h_t only yields returns outside of agriculture.[9]

In many developing economies, employment is inefficiently high in agriculture (Gollin et al. 2014). To capture this feature in our model, we assume that labor markets are distorted by a wage wedge, such that agricultural wages are lower than nonagricultural wages:

$$w_{at} = \xi w_{mt}$$

where $\xi \in (0, 1)$. We simplify notation by setting $w_{mt} \equiv w_t$.

[7] The small open economy assumption implies prices are exogenous and therefore unaffected by local demand. We discuss the implications of adding trade costs to our model at the end of Section 2.3 and in Appendix Section A.3. We also show in Table D.10 that the quasi-experimental intervention in Bangladesh that we study in Section 3 induced only modest changes in consumption shares across sector, suggesting that demand-side factors do not drive sectoral labor reallocation in our Bangladesh analysis.

[8] We consider alternative manufacturing production functions in Appendices A.1 and A.2.

[9] 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 predictions of the model.

2.1.2 Households

To characterize households, we build on the model of Strulik (2017). Preferences are defined as

$$U = \log c_t^a + \delta \log c_t^m + \alpha \log n_t + \gamma \log w_{t+1} + \sigma \log s_t,$$

where c_t^a is household consumption of the agricultural good, c_t^m is consumption of the manufacturing good, n_t is the number of births per household, w_{t+1} is each child's potential income when they enter the labor force in the following period, s_t is the amount of sex had by the household, and so σ is the desire for sex.[10] We assume $\alpha > \gamma$ to ensure parents have children even if they could be costlessly avoided.

Define the number of births as

$$n_t = \min\{s_t - \mu u_t, \bar{n}\}$$

where u_t represents the quantity of the family planning technology used. Households may use contraception or abortion to limit childbearing. μ is the effectiveness of family planning technologies such that a unit of u_t prevents the birth of μ children. Sex is proportional to births according to some constant that we normalize to 1. $\bar{n}$ is the biological maximum reproduction for a given female; in what follows, we consider only interior solutions.

Human capital is produced according to

$$h_{t+1} = A_{ht} e_{t+1} h_t,$$

where e_{t+1} is the time spent on educating each child and A_{ht} is exogenous human capital production productivity. Households have one unit of time per adult and therefore face the budget constraint

$$w_t[1 - (\phi + e_{t+1})n_t] = p_{ft} u_t + p_{at} c_{at} + p_{mt} c_{mt}$$

given per child rearing time ϕ and the price p_{ft} of a unit of the family planning technology. The price of agricultural goods is p_{at} and of manufacturing goods p_{mt}. Each household works a fraction of their time endowment equal to $\ell_t = 1 - (\phi + e_{t+1})n_t$. Aggregate labor supply is a product of the adult population in time t, n_{t-1}, and the per adult labor supply ℓ_t:

$$L_t = n_{t-1} \ell_t. \tag{3}$$

[10] Note that because we have assumed a small open economy, introducing nonhomotheticity in the demand for agricultural goods would have no effect on our equilibrium results. Strulik (2017) shows in his appendix that Stone-Geary preference for consumption would not change the effect of making family planning more accessible on fertility and education.

2.2 Equilibrium

Labor markets clear so

$$L_t = L_{at} + L_{mt}.$$

The equilibrium wage comes out of the manufacturing firm's marginal product:

$$w_t = p_{mt} A_{mt} h_t.$$

The equilibrium agricultural employment share is therefore,

$$\frac{L_{at}}{L_t} = \left(\frac{\theta p_{at} A_{at}}{\xi p_{mt} A_{mt} h_t} \right)^{\frac{1}{1-\theta}} \frac{T}{L_t}. \tag{4}$$

The land-labor ratio, $\frac{T}{L_t}$, captures the Malthusian land congestion mechanism: the higher the labor force relative to land, the lower the fraction of labor will work in agriculture. Our second key mechanism is that increases in human capital, h_t, will reduce employment share in agriculture as the manufacturing wage rises, as seen with the term in parentheses in equation (4). Finally, the greater are distortions (smaller ξ), the more employment shifts to agriculture.

Each household's optimal choice of fertility and child education are as follows:

$$n_t = \frac{(\alpha - \gamma)\mu w_t}{(1 + \delta + \alpha + \sigma)(\mu w_t \phi - p_{ft})} \tag{5}$$

$$e_{t+1} = \frac{\gamma(\mu w_t \phi - p_{ft})}{(\alpha - \gamma)\mu w_t} \tag{6}$$

2.3 Effects of Changes in the Price of Family Planning Technology

We assess the effect of the fertility transition on sectoral employment through the lens of our model by considering a reduction of the price of the family planning technology p_{ft}.[11] The price includes both monetary and non-monetary costs associated with accessing the family planning technology. As shown in equations (5) and (6), reducing p_{ft} decreases fertility and increases education of the next generation:

$$\frac{\partial n_t}{\partial p_{ft}} > 0, \qquad \frac{\partial e_{t+1}}{\partial p_{ft}} < 0.$$

[11] We do not mean to argue that the fertility transition was caused exclusively by a reduction in price of the family planning technology. However, such a change in price maps best to our empirical applications so we focus on it for that reason.

Hence both current-generation human capital h_t and current-generation adult population n_{t-1} are unchanged as a result of the program in the short-run. The only contemporaneous variable that changes is labor hours, ℓ_t:

$$\frac{\partial \ell_t}{\partial p_{ft}} = -(e_{t+1} + \phi)\frac{\partial n_t}{\partial p_{ft}} - n_t \frac{\partial e_{t+1}}{\partial p_{ft}}. \tag{7}$$

That is, the direction of the change in labor hours depends on the relative strength of the quantity-quality tradeoff. On the one hand, parents have fewer children to raise and therefore less demand on their parenting time, as shown in the first term of equation (7). On the other, parents invest more time educating each child, as shown in the second term. The net effect is theoretically ambiguous. Empirically, Aaronson et al. (2021) estimate that the effect of fertility on women's labor supply is negligible at low levels of development but significantly negative for more developed countries. Lundberg and Rose (2002) finds that men increase their labor supply with fertility in the U.S. Hence, the aggregate net effect is also ambiguous but the small or offsetting estimated effects in the aforementioned literature suggest that the magnitude may not be very large. Indeed, in Section 3 we estimate the effect a statistically insignificant effect of a family planning and vaccine program on household-level labor supply.

In subsequent generations, more accessible family planning technologies has two additional effects. First, human capital (h_t) rises, thereby pulling workers into the manufacturing sector. Second, the adult population (n_{t-1}) falls, reducing land congestion in agriculture. The effect on total labor supply is

$$\frac{\partial L_t}{\partial p_{ft-1}} = n_{t-1}\frac{\partial \ell_t}{\partial p_{ft-1}} + \ell_t \frac{\partial n_{t-1}}{\partial p_{ft-1}}.$$

Relative to the prior period in which only per-adult labor supply ℓ_t may change, the land-labor force ratio rises, increasing the returns to labor in agriculture. The net effect of more accessible family planning technology on agricultural employment share depends on the relative strength of the human capital and land congestion channels.

We show that our predictions are robust to alternative production functions in Appendix A. In Appendix Section A.1, we show our results hold when adding an additional factor of production, imported intermediate inputs.[12] We further show in Appendix Section A.2 that our main results hold if we allow intermediate inputs and labor to be arbitrarily substitutable.

The small open economy assumption is crucial to obtain our model's key predictions. When trade costs are sufficiently high, the economy becomes closed and must rely on local

[12] One can instead think of this additional factor as capital when the economy is open to the global capital market. Introducing capital to the model with closed capital markets makes it intractable, as noted by Galor (2005).

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 (Gollin et al. 2007) works against our hypothesized population size effect. If every country's agricultural sector was perfectly closed, in our model declining fertility would decrease agricultural employment share, so long as the per-household effect on labor supply ℓ_t is sufficiently small.

As we show in the following three empirical tests of our model, the closed economy predictions are inconsistent with our results. This is consistent with the relative insignificance of general equilibrium price effects in the study by Cavalcanti et al. (2021) on the aggregate effects of family planning programs. Tombe (2015), moreover, shows a wide range of openness among countries' agricultural sectors, including for developing countries. A growing literature emphasizes an open-economy perspective on structural change (Uy et al. 2013; Sposi 2019; Fajgelbaum and Redding 2022; Farrokhi and Pellegrina 2023; Gollin et al. 2025). We provide additional details on the case where trade is costly in Appendix Section A.3.

3 Quasi-Experimental Household-Level Evidence

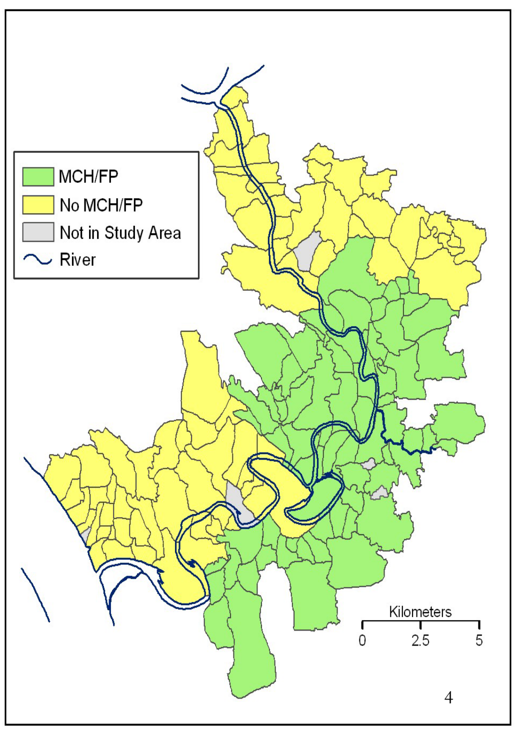
We begin testing the implications of our theoretical model by leveraging a quasi-experiment in Bangladesh. The Maternal and Child Health and Family Planning (MCH-FP) program was introduced in the Matlab subdistrict of Bangladesh in 1977 to a subset of local villages. We estimate the long-run effect of the program, which included family planning and maternal and child health services, on household-level structural transformation. While the study area's small size precludes quantifying general equilibrium effects on prices and wages resulting from changes in fertility, the intervention provides rare causal identification and rich, long-running household- and individual-level data to estimate mechanisms. We return to the issue of general equilibrium forces in subsequent sections.

3.1 The Intervention

The MCH-FP program was implemented by icddr,b (formerly known as the International Centre for Diarrhoeal Disease Research, Bangladesh) in Matlab, Bangladesh, in two distinct phases. During the first phase, which occurred between October 1977 and December 1981, the intervention focused on family planning and maternal health. Trained female community health workers delivered services directly to households for free, including modern contraceptive methods, tetanus toxoid immunizations for pregnant women, and iron and folic acid supplementation during the third trimester (Bhatia et al. 1980). During the second phase, intensive child health interventions were rolled out starting in 1982 including vaccination against measles, tetanus, pertussis, polio, and tuberculosis, and nutrition rehabilitation. The staggered rollout of program components led to differential treatment of children depending on their year of birth.

The MCH-FP program was introduced using a quasi-randomized design, with 70 out of 149 villages assigned to receive the intervention and the remaining villages to an untreated comparison group (see Figure 1). Villages were placed into four contiguous treatment blocks and two comparison blocks that flanked the treatment area. This design facilitated program implementation while minimizing information spillovers related to family planning (Huber and Khan 1979) and reducing potential externalities from increased vaccine coverage. In contrast, households in the comparison area had access only to standard government health and family planning services, which were mainly delivered in government clinics rather than homes. Importantly, several key child health services—such as vaccinations—were not available in government clinics until 1989, creating a period from 1977 to 1988 during which access to health and family planning services differed markedly between treatment and comparison areas.

Figure 1: Map of Matlab Study Area



Notes: The map plots villages in the Matlab subdistrict in Bangladesh. Villages in green are within the treatment area while those in yellow are in the comparison area.

The program was successful in driving rapid take-up of the two key interventions: family planning and the measles vaccine (see Appendix Figure D.1). Prior to the program, the contraceptive prevalence rate for married women 15–49 was low (less than 6 percent) in both the treatment and comparison areas. It rose by over 25 percentage points in the treatment area in the first year and steadily thereafter. Contraceptive use grew more slowly in the comparison area. The measles vaccination rate surged to 60 percent after it was introduced in the second phase of the program; rates of vaccination coverage for diseases targeted by the program increased throughout the program duration. There is no data available on vaccination in the comparison area at this time, but the rate is assumed to be zero as vaccines were not available. We provide additional details about the MCH-FP in Appendix Section B.

3.2 Data and Analysis Sample

Data Sources. We draw on the extraordinarily rich data available for the Matlab study area. Our primary outcome of interest is household- and individual-level sectoral employment measured in both the 1996 Matlab Health and Socioeconomic Survey (MHSS1) (Rahman et al. 1999) and the second wave of the Matlab Health and Socioeconomic Survey (MHSS2), which was collected between 2012 and 2014.[13] MHSS1 and MHSS2 are panel surveys. MHSS1 is a random sample of households in the study site and is representative of Matlab's 1996 population. MHSS2 follows individuals surveyed in MHSS1 and adds a sample of individuals who migrated out of Matlab from sample households prior to MHSS1 (i.e., pre-1996 migrants). MHSS2 also follows children of the MHSS1 respondents.

Questions changed significantly between survey rounds, and the MHSS2 offers a richer set of questions about sectoral employment (see Appendix Section C.1.1 for more details on our sectoral employment classification). In particular, we use as outcome variables the share of months worked by people age 15 and older by sector in MHSS1 and the share of annual hours worked by sector in MHSS2.

We use two supplementary data sources: periodic censuses from 1974 and 1982 (icddr,b 1974, 1982), and 1974–2014 Matlab demographic surveillance site (DSS) data on vital events (e.g., births, marriages, deaths, in and out migrations). These data sources cover all households in the Matlab study site.

A key feature of all these data is that individuals can be linked across different data sources over time by a unique individual identifier. There are few, if any, other study sites that have similarly rich data availability to allow for this type of long-term evaluation.

[13]See Appendix C.1 for additional details on the surveys.

Intent-to-Treat Assignment. Access to the MCH-FP program was based on the village of residence of the 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's location decision may have been affected by the program (Barham and Kuhn 2014).

We create an individual-level intent-to-treat (ITT) indicator by tracing each individual back to their 1974 household, and use that village of residence at the time of the 1974 census to determine eligibility status. If the person was not alive or present in the 1974 census, we use the residency of their first DSS household head (or that person's first observed DSS household head) present in the 1974 census. For an individual, the ITT variable takes the value of 1 if the 1974 census-linked individual or traced-back household head was living in a village in the treatment area in the 1974 census or first migrated into a village in the treatment area from outside Matlab between 1974 and 1977 (using the DSS), and 0 otherwise. For our primary household-level analysis, the treatment indicator for the MHSS1 household is derived from the individual treatment indicator of the household head.

Analysis Sample and Attrition. We focus on the MHSS1 household as our baseline unit of analysis. We follow all members of the household and the descendants to the MHSS2 survey, and aggregate the MHSS2 individuals to the MHSS1 household they link back to. Appendix Section C.1.2 discusses in detail how we aggregate outcomes measured across multiple households and individuals in MHSS2 to the MHSS1 household level.

Because the MCH-FP program could have drawn households into the treatment area (Barham and Kuhn 2014), we use the pre-program village of residence described above to restrict our sample to households in which the household head from the MHSS1 survey was present in Matlab prior to the start of the program (i.e., October 1977). Our sample restrictions result in a sample of 2,534 MHSS1 households. Due to the low attrition in MHSS2, fewer than 2 percent of MHSS1 households have no members who were tracked to the MHSS2 survey round, and our household-level analysis using MHSS2 data include the 2,484 households tracked to this second round.

To assess the role of human capital as a mechanism, in Section 3.5.2 we analyze employment outcomes at the individual level. This analysis relies on the same set of individuals that underlie our household-level analysis. Because we are focused on employment outcomes, we additionally restrict the individual-level analysis to individuals born in 1947 or later, and were thus 65 or younger at the start of MHSS2 surveying, and to those who were born in 1988 or earlier, to focus on the set of individuals born before or during the MCH-FP evaluation period. Including death and other types of non-response, the attrition rate is 16 percent among all men in our sample, and 11 percent among men born during the program interventions. This is a low attrition rate despite 60 percent of men in our sample migrating out of the Matlab study area, 25 percent of whom migrated internationally.

3.3 Empirical Strategy

3.3.1 Pre-Program Balance and Trends

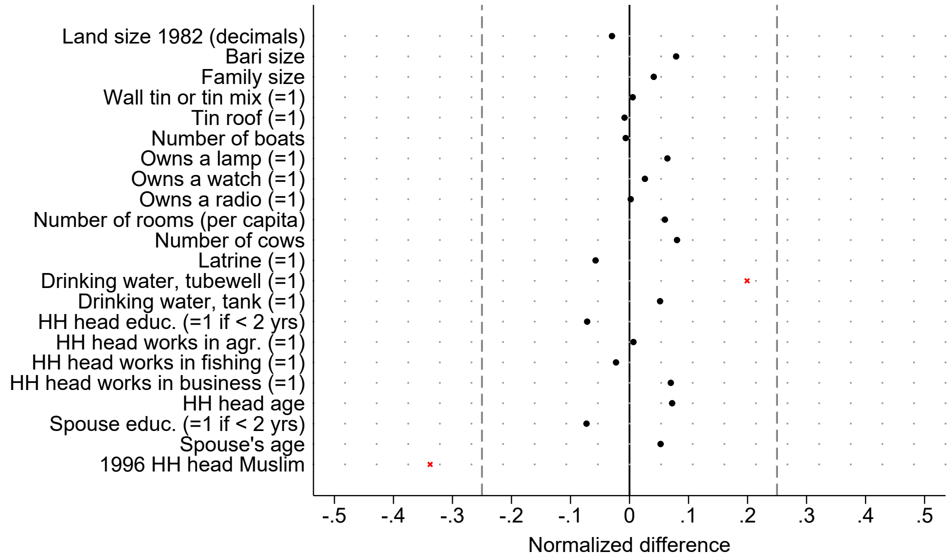
The MCH-FP analysis takes advantage of the treatment and comparison areas that were built into the program and designed to be socially and economically similar and geographically insulated from outside influences (Phillips et al. 1982). Prior studies extensively document balance between treatment and control villages across a range of variables including mortality rates, fertility rates, and pre-intervention household and household head characteristics (Koenig et al. 1990; Menken and Phillips 1990; Barham 2012; Joshi and Schultz 2013). Additionally, migration stocks and flows were similar between the treatment and comparison area at the start of the program and through to 1982, for the cohort of individuals most likely to migrate at the start of the program (Barham and Kuhn 2014). Barham et al. (2023) further show that men born between 1977 and 1988 come from households experiencing similar labor market outcomes in 1974, 1982, and 1996. Finally, Barham (2012) shows that cognitive functioning, height, and years of 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.

We demonstrate that this balance persists in our analysis sample of MHSS1 households, complementing previous work examining individual differences among the panel of MHSS1 respondents. Figure 2 depicts the normalized differences in means (difference in the means divided by the standard deviation of the comparison area) of pre-intervention household characteristics measured in the 1974 census.[14] These normalized differences provide an indication of the economic significance of the differences that do not depend on sample sizes. Normalized differences bigger than 0.25 standard deviations are generally considered to be economically meaningful (Imbens and Wooldridge 2009). In Figure 2, any difference which is statistically significant at the 5% level is indicated with a red X.

Differences in means are statistically insignificant at the five percent level for all variables except whether the household head is Muslim and a dummy for the household using tubewell water for drinking. Because we test balance across 22 variables, it is unsurprising that two are statistically different. With the exception of religion and the use of tubewell drinking

[14] Appendix Table D.1 presents the means for the treatment and comparison group separately and the level differences in means between the two groups.

Figure 2: Baseline Balance in Normalized Differences



Notes: The chart plots normalized differences in pre-intervention variables. Each variable, unless otherwise specified, is measured using the 1974 census. The normalized difference is the difference in means divided by the comparison area's standard deviation. Any difference between treatment and comparison average which is statistically significant at the 5% level is indicated with a red X.

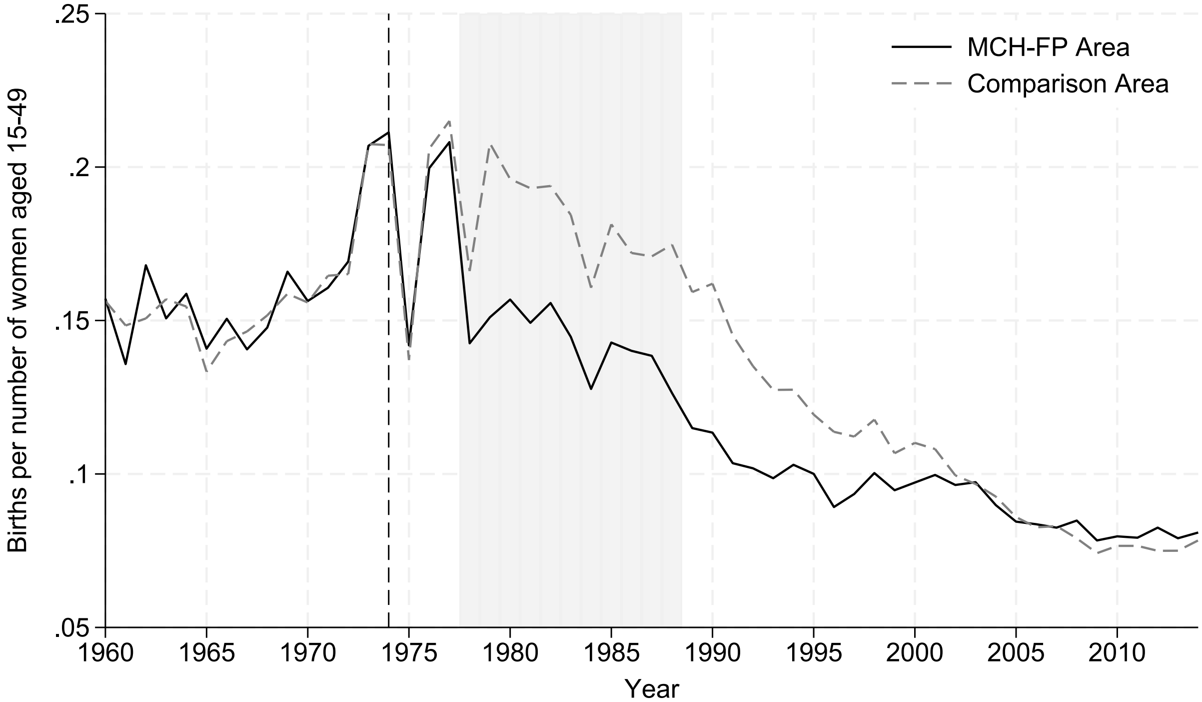
water, the normalized differences are less than 0.12 standard deviations suggesting that the differences that do exist are relatively small. In our main specification, we control for all these baseline characteristics.

The difference in tubewell water access at 0.20 standard deviations is close to the cut off of 0.25. This difference is not a result of household decisions, but rather the rollout of a government program. Although groundwater may be considered cleaner than surface water, there is widespread groundwater arsenic contamination in 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). Barham (2012) finds no evidence that tubewell access biases estimates of the program's effect on human capital.

Finally, because this paper focuses on the demographic transition, we document the similarity in birth rate trends during the nearly two decades before program rollout. Figure 3 plots the number of births relative to the population of adult women of childbearing age (15–49) for both the treatment and comparison area using data from the DSS.[15] Prior

[15]The DSS began tracking demographic events in April 30, 1974. In years prior to 1974, we construct birth rates using the population of women aged 15–49 present in the DSS at the time of the initial census on April 30, 1974 divided by the number of individuals born in the given year still residing in the DSS on that same date. In counting the number of births in 1974, we count the number of children born before April

Figure 3: Trends in Birthrates 1960–2014, MCH-FP Treatment Area and Comparison Area



Notes: The figure reports birth rate estimates across the Matlab Demographic Surveillance Site (DSS) from 1960 through 2014. DSS records begin April 30, 1974 (denoted by the vertical dashed line). In 1974 and earlier, we construct birth rates by counting the number of individuals who resided in the DSS area on April 30, 1974 who were born in a given year and dividing by the number of women aged 15–49 residing in the DSS on April 30, 1974. In later years, birth rates are constructed by counting the number of individuals residing in the DSS on the day of their birth in a given year and dividing by the number of women aged 15–49 residing in the DSS area on January 1 of that year. The shaded gray area marks the period (1977–1988) when the MCH-FP intervention was available in the treatment area, but not the comparison area.

to the implementation of the program in 1977, the levels and trend of birth rates were nearly identical, including the severe drop in the birth rate in 1975 around the time of the Bangladesh famine.[16] During the experimental period, which is shaded in gray, there is a substantial divergence in birth rates. The comparison area experienced nearly 30% higher birth rates than the treatment area.

3.3.2 Empirical Specification

To examine the effect of the program on sectoral employment and agricultural outcomes, we take advantage of well-balanced treatment and comparison areas and use a single-difference

30, 1974 and residing in the DSS, as well as any new births recorded later that year. For later years, the denominator includes the population of women 15–49 present in the DSS on January 1 of that year.

[16]Note that birth rates prior to 1974 undercount the actual number of live births as the estimates are based on individuals who survived to 1974.

intent-to-treat (ITT) model. We estimate the household-level specification,

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

where T_h is an indicator for whether household h is considered treated (as defined in Section 3.2) and X_h is the vector of demographic and baseline characteristics listed in Figure 2. The coefficient of interest, ω_1, measures the difference in average outcomes between treatment and comparison area households conditional on the set of 1974 household characteristics. To adjust our inference for the village-level treatment assignment, we cluster standard errors by the village of the household head of h or his antecedents, traced back following our ITT assignment.

For the difference in average outcomes to identify the causal effect of the MCH-FP program, we assume that average post-program outcomes among treatment area households, conditional on pre-intervention controls, would have been the same as the average outcomes in the comparison area, had the treated not been treated. While this is not a testable assumption, we demonstrated in Section 3.3.1 that comparison group exhibit very similar behavior relative to treated households during the preperiod. Treatment and comparison areas had similar birth rate trends in the nearly two decades leading up to program rollout and there was excellent baseline balance across a wide-range of pre-intervention covariates.

3.4 Effects on Sectoral Employment

We show the estimated ITT effects of the MCH-FP program on household-level sectoral labor allocation in Table 1. We distinguish between medium-run effects, measured 19 years after program initiation using the 1996 MHSS1 survey, and long-run effects, captured 35 years later in the 2012–2014 MHSS2 survey. The outcome variables differ slightly across surveys. In MHSS1, the outcomes capture the share of months per year that household members worked in agriculture or non-agriculture (columns 1 and 2).[17] In MHSS2, the dependent variables are the share of total annual hours worked in each sector (columns 3–5),[18] as well as the average total number of annual hours worked by household members (column 6).[19]

In the medium-run, the effects of the program on the agriculture and non-agricultural share are close to zero and not statistically significant.

[17]Because time is measured at the monthly level, individuals working in both sectors within a single month may cause shares to sum to more than one.

[18]Employment shares may not sum to one for two reasons: (i) results for the construction sector are not reported, and (ii) households with no reported work are coded as allocating 0 percent of time to each sector.

[19]We exclude analyzing the effect on months worked in MHSS1 because of a lack of variation at this extensive margin with most adults work either 0 or 12 months per year in our data. There is no data intensive margin of hours worked in MHSS1.

Table 1: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level

	MHSS1 (1996)		MHSS2 (2012–2014)			
	(1) Agriculture Share	(2) Non-Agriculture Share	(3) Agriculture Share	(4) Manufacturing Share	(5) Services Share	(6) Annual Hours Per Person
Treatment	0.007	0.004	0.041***	-0.032**	-0.013	-27.083
	(0.021)	(0.021)	(0.014)	(0.014)	(0.018)	(35.457)
% chg. rel. to mean	1.1	1.2	19.9	-15.8	-2.8	-1.9
Mean	0.68	0.36	0.21	0.20	0.48	1445.47
Baseline controls	Y	Y	Y	Y	Y	Y
Observations	2534	2534	2484	2484	2484	2484

Notes: The table presents estimates of equation (8) for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Columns (1) and (2) measure outcomes in the 1996 MHSS1, while columns (3) through (6) measure outcomes in the 2012–2014 MHSS2. The MHSS1 dependent variable is the share of working months in the year allocated to each sector. MHSS2 dependent variables are the share of hours worked by sector within the household (columns 3–5) and the average annual hours worked per person aged 15 or older (column 6). Employment hours shares do not sum to 1 for two reasons. First, we do not report results for the construction sector. Second, we code the small set of households who do not work as spending 0 percent of their time working in each sector. MHSS2 regressions are weighted to account for household-level attrition between the MHSS1 and MHSS2 surveys; see Appendix C.1.3 for more details. See Appendix C.1.1 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.

In the long run, once treated cohorts entered the labor force, the effects are substantial and consistent with the Malthusian land congestion mechanism. The MCH-FP increased adultsâ agricultural work share by 4.1 percentage points (20 percent of comparison households' mean; column 3) and reduced manufacturing work share by 3.2 percentage points (16 percent of comparison households' mean; column 4). Both effects are statistically significant at the 5 percent level. In contrast, the effect on share of workers in services is small (-3 percent; column 5) and not statistically significant. Similarly, the effect on annual hours worked per person is negligible (-2 percent; column 6) and not statistically significant, suggesting that the changes in sectoral time allocations are not driven by shifts in total hours worked.

Next, we look at the effect of the MCH-FP on several related structural transformation outcomes.

Given the centrality of rural-to-urban migration in the development process (Lagakos 2020; Lagakos et al. 2023), we disaggregate the results in Appendix Table D.2 to reflect urban (columns 1–3) and rural (columns 4–6) work locations. Specifically, we re-estimate equation (8) by sector, but split the dependent variable, share of work hours by sector, by rural or urban location of employment as of 2014. The results reflect that sectoral-employment effects are driven by treated households engaging more in rural agriculture (+16 percent; significant at the 5 percent level) and less in urban manufacturing (-24 percent; significant at the 1 percent level) relative to comparison households, underlining the importance of rural-to-urban migration in structural transformation in Bangladesh.

Given the importance of entrepreneurship for development (McMillan and Woodruff 2003; Buera et al. 2011, 2021), we further investigate in Appendix Table D.4 whether the patterns observed in employment are matched by sector-specific entrepreneurship, defined as someone in the family owning a business. Columns 1 through 3 show a similar pattern as Table 1: increased agricultural entrepreneurship by 20 percent (significant at the 1 percent level), with no change in manufacturing or services entrepreneurship.

Since large firms, especially factories, drive structural change and growth (Buera and Kaboski 2012), we further examine how the MCH-FP affected employment across firm types in Appendix Table D.4. Employment at factories among treated households lagged behind comparison area households by 14-23 percent (significant at the 5 percent level or below; columns 4 and 5), as did employment at large firms by 21 percent (significant at the 5 percent level; column 6).

Taken together, the MCH-FP program slowed the pace of structural transformation across an array of outcome measures. Interpreted through the lens of the model from Section 2, this pattern is consistent with the Malthusian land congestion mechanism dominating both the quantity-quality tradeoff and any direct human capital gains from the child health interventions. We next assess the robustness of the baseline results and then examine the relative strength of each mechanism.

Robustness. We now consider the robustness of our main results to the block design and baseline imbalance in religion.

It is possible that areas closer to treatment and control borders differ due to spillovers (informational or from vaccines) or because the areas could be less similar since they are farther apart. To explore this possibility we examine effects for the sample living in a village prior to the intervention with a centroid within 3km of the treatment-control border. Findings in panel B of Appendix Table D.5 demonstrates the magnitudes are similar to our baseline results of Table 1.

Given our finding in Section 3.3.1 that Muslims are disproportionately represented in control villages, we re-estimate equation 8 using only Muslim households in Panel C of Appendix Table D.5. Again, results are virtually unchanged. Since Matlab is about 85% Muslim, we do not have sufficient statistical power to estimate program effects for the Hindu population on its own.

Additionally, we address two other asymmetries between treatment and control areas: (i) the erosion of some study villages and construction of a flood embankment, and (2) the only urban center in the study area exists in the treatment area. In Panels D and E of Appendix Table D.5, we show that our results are largely unchanged when we control for the villages affected by the river erosion or remove households who resided in the urban center prior to the intervention.

Finally, we present additional tests of inference in Appendix Table D.6. The first row displays our baseline p-values for the effect of the MCH-FP allowing for village-level clustering of standard errors. To account for the limited number of village clusters over which treatment was assigned (there were four blocks of villages within the treatment area and two in the comparison area), we alternatively compute standard errors using a wild cluster bootstrap in the second row. In the third row, we use a randomization-based inference method to address the concern that the treatment area constitutes one contiguous region. To do so we generate 10,000 placebo village-level treatment assignments maintaining the geographic contiguity of the treatment area to construct a distribution of the underlying test statistic. In the last row, we adjust for multiple hypothesis testing. In each case, we continue to find that the MCH-FP increases treated households' agricultural work share, and a decrease in share in manufacturing (with the exception of random inference).

3.5 Mechanisms

We leverage the richness of the household-level data to examine the underlying mechanisms. Consistent with the model in Section 2, we focus on two channels that may work in opposing directions: population size and human capital.

3.5.1 Family Size

We first test the model's prediction that a reduction in the labor force increases the share of employment in agriculture. Specifically, we estimate how household size affects the agricultural employment share. In equation (4), the land-to-labor ratio T/L_t, captures the Malthusian land congestion mechanism central to this prediction.

A necessary condition for this mechanism to operate is that land is fixed. This assumption holds at the local level, but at the household level families may adjust to changes in household size by buying or selling land. In Appendix Table D.3, we find no evidence that the program affected the number of acres owned by the average households.[20,21] Moreover, land transactions outside the family are relatively rare—fewer than 6% of households engaged in

[20]Our measure of land ownership includes both plots used for agricultural and non-agricultural purposes.

[21]There are statistically insignificant changes of -0.04 acres (2.7 percent) in the medium-run and +0.02 acres (1.3 percent) in the long-run.

them annually in MHSS1—and such transactions are typically modest in size.[22]

The program resulted in a substantial decline in population by reducing the birth rate. Fauveau (1994), Joshi and Schultz (2013), and Barham et al. (2023) all find that the MCH-FP significantly reduced 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 D.7. Consistent with earlier research and Figure 3, we find the program reduced household size. Specifically, the program reduced the number of males per household aged 24 to 34 as of 2014 by 16 percent, and decreased the number of females per household in the same age range by 11 percent.[23]

Next, to understand how population pressure contributed to structural transformation within households, we estimate how the number of male children per household born during the experimental period affected the household's subsequent sectoral employment choices. We focus on males because of their stronger labor market attachment than females in this context. In particular, we estimate an equation of the form,

$$Y_h = \alpha_0 + \alpha_1 Num.\ males\ age\ 24\ to\ 34_h + \gamma X_h + \epsilon_h \tag{9}$$

where Y_h is either the share of household work hours by sector or the number of hours by sector. Because the program directly affects other factors shaping sectoral allocation, such as human capital, we instrument for $Num.\ males\ age\ 24\ to\ 34_h$ using the treatment dummy T_h.

Results are presented in Table 2. Panel A reports estimates for the share of total hours worked by sector, while Panel B presents results for the total number of hours worked by sector.

Panel A shows that larger households allocate a smaller share of labor to agriculture (column 1). Specifically, the birth of one additional male child during the program period leads to a reduction of nearly 26 percentage points in the household's share of time worked in agriculture. In contrast, larger households allocate a greater share of labor to manufacturing (column 2) and, to a lesser extent, services (column 3), though the estimate for services is less precise.

Panel B examines the effect of household size on the total number of hours worked

[22] Land transaction data in MHSS1 do not specify whether the plots were agricultural or non-agricultural plots.

[23] The difference in number of 24-34 year olds by gender is statistically indistinguishable. The effect size on fertility is slightly smaller than what is reported by Joshi and Schultz (2013) and Barham et al. (2023). 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 2: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector and Household-Size: Household-Level

	(1) Agriculture	(2) Manufacturing	(3) Services
Panel A: Work Time Shares by Sector			
No. males aged 24–34	-0.260**	0.201*	0.085
	(0.101)	(0.105)	(0.110)
% chg. rel. to mean	-125.8	100.2	17.5
Mean	0.21	0.20	0.48
First-stage F-stat.	14.2	14.2	14.2
Baseline controls	Y	Y	Y
Observations	2484	2484	2484
Panel B: Total Work Time by Sector			
No. males aged 24–34	-426	2,507***	2,001*
	(574)	(955)	(1,108)
% chg. rel. to mean	-31.1	141.8	49.9
Mean	1368	1769	4007
First-stage F-stat.	14.2	14.2	14.2
Baseline controls	Y	Y	Y
Observations	2484	2484	2484

Notes: The table presents 2SLS estimates for outcomes measured in the 2012–2014 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. The dependent variable in Panel A is the share of hours worked within the household by sector. Employment hours shares do not sum to 1 for two reasons. First, we do not report results for the construction sector. Second, we code the small set of households who do not work as spending 0 percent of their time working in each sector. The dependent variable in Panel B is the total hours worked within the household by sector. See Appendix C.1.1 for more details on how we classify workers into sectors. Regressions are weighted to account for household-level attrition between the MHSS1 and MHSS2 surveys. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

by sector, allowing us to assess how reduced availability of son labor affects the level of agricultural versus non-agricultural labor. While the estimated effect on agricultural hours is negative, it is imprecisely estimated (column 1). In contrast, the number of hours worked in both manufacturing and services rises significantly with household size. These results are consistent with the marginal son being employed outside of agriculture. Overall, the findings indicate that fertility changes induce a change in the composition of sectoral employment rather than in the level of agricultural labor.

3.5.2 Human Capital

The second key mechanism we study is human capital. The model predicts that when returns to human capital are higher in non-agricultural sectors, increases in human capital will shift labor out of agriculture. We test this prediction by leveraging the staggered rollout of the child health component of the MCH-FP program which boosted human capital and cross-cohort variation in exposure.

First, we confirm that returns to human capital are in fact higher outside of agriculture in this context consistent with findings by Caselli and Coleman (2001) and Porzio et al. (2022). We estimate a sector-specific Mincer equation relating log wages to years of education and potential experience. Results are presented in Appendix Table D.10. The average earnings return to an additional year of education in Matlab are 2.5% in agriculture, 4.7% in manufacturing, and 6.2% in services.

Previous research on the MCH-FP program finds substantial human capital gains for cohorts born between 1982 and 1988, and minimal effects for those born between 1977 and 1981 (Barham 2012; Barham et al. 2025). Effects on education for the 1982–1988 cohort are strongest among men, who experienced an increase of nearly one year of schooling by the time of the MHSS2. In Appendix Table D.12, we extend these results to examine the effect of the MCH-FP on years of education at the household-level using MHSS2. Consistent with the program's emphasis on early childhood health, we find negligible effects on education for the full sample of adults. Similar to earlier literature, effects are driven by men born during the intensive child health period (1982–1988) who attained 0.7 more years of schooling (significant at the 5 percent level) than their counterparts in the comparison area. In what follows, we therefore treat the 1982–1988 birth cohort—exposed to the intensive child health component of the MCH-FP—as experiencing a significant human capital gain relative to other cohorts.

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

$$Y_i = \alpha_{y(i)} + \gamma_1(T_i \times Born_i^{77-81}) + \gamma_2(T_i \times Born_i^{82-88}) + \gamma_3(T_i \times Born_i^{Pre-77}) + \nu X_i + \epsilon_i \quad (10)$$

where T_i is an indicator for whether i is eligible for the program as defined in Section 3.2; $\alpha_{y(i)}$ is a set of indicator variables for i's birth year; and X_i is the vector of pre-intervention demographic and baseline characteristics detailed in Figure 2.[24] The outcomes Y_i we consider

[24] We additionally control for dummy variables indicating whether i was born (i) prior to the intervention starting in October 1977, (ii) during the first phase of the intervention October 1977 to February 1982, and during the second phase of the intervention March 1982 to December 1988. Because we define our cohort dummies $Born_i^{77-81}$, $Born_i^{82-88}$, and $Born_i^{Pre-77}$ using these year-month cutoffs, they are not collinear with the vector of birth year cohort dummies $\alpha_{y(i)}$.

are the sectoral hours share and total hours worked. 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 capture the difference in conditional means for the outcome for each 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 health interventions. γ_3 captures any indirect spillover effects of the program on older generations.

Table 3 reports results at the individual level for men on hours worked by sector.[25] Consistent with our household-level estimates, on average cohorts experience an increase in the share of hours worked in agriculture (column 1) and reduction in manufacturing (column 2).[26] However, there is heterogeneity in program effects across cohorts. Men born between 1982 and 1988—who were exposed to intensive child health interventions and experienced greater gains in human capital—were 5 percentage points (10 percent) more likely to work in the service sector and 8 percentage points (30 percent) less likely to work in manufacturing, although the effect on service-sector employment is not statistically significant. In contrast, men born before 1982—prior to the rollout of intensive child health interventions—exhibited patterns consistent with the land congestion mechanism. Specifically, for the 1977–1981 cohort, the share of hours worked in agriculture increased by 5 percentage points (56 percent), and the share in manufacturing declined by 6 percentage points (31 percent). On net, the land congestion mechanism dominates the human capital mechanism.

To the extent that the MCH-FP improved human capital, it may have improved agricultural productivity. We do not find evidence that the program improved in agricultural productivity. We proxy per-acre productivity using revenue and profit per acre.[27] Appendix Table D.9 shows our estimates of the effect of the MCH-FP on farm productivity for the subsample of households that grew crops. Columns 1 and 2 report estimates for revenue per acre, while columns 3 and 4 present results for profits per acre. Across all specifications, we cannot statistically reject a null effect.[28]

[25]We show results for women in Appendix Table D.8. The program did not affect hours worked for women. The program caused vaccine-treated women to work more in agriculture and less in non-market activities. Given women's much lower labor supply, we focus on men in our analysis.

[26]coefficients across each row (including the construction sector excluded from the table) must sum to 0.

[27]Estimating the value of output requires crop price data, which are not available at the household level in the MHSS2. To address this, we use crop price information from the Bangladesh Statistical Yearbooks for 2012–2014. These yearbooks report prices at the variety level (e.g., coarse paddy boro or fine paddy boro), rather than at the aggregate crop level (e.g., paddy boro). To construct crop-level prices, we take two approaches: using either the minimum or the maximum variety-specific price within each crop category.

[28]The one area in which farming was affected by the program is crop choice. As eligible households became

Table 3: ITT Effects of MCH-FP on Long-term Work Hours by Sector: Individual-Level

	Share hours by sector			
	(1)	(2)	(3)	(4)
	Agriculture	Manufacturing	Services	Hours worked
Treatment × Born 1982–1988	0.010	-0.076**	0.053	-24.200
	(0.018)	(0.030)	(0.035)	(87.489)
Treatment × Born 1977–1981	0.046*	-0.065**	-0.012	-64.323
	(0.024)	(0.031)	(0.036)	(85.682)
Treatment × Born Pre-1977	0.038*	-0.001	-0.036	-145.842**
	(0.022)	(0.015)	(0.022)	(61.918)
% chg. (1982–88)	12.6	-30.4	10.3	-0.8
% chg. (1977–81)	56.5	-30.5	-2.0	-2.0
% chg. (Pre-1977)	13.2	-0.7	-6.9	-5.1
Comparison mean (1982–88)	0.08	0.25	0.51	3040.01
Comparison mean (1977–81)	0.08	0.21	0.59	3184.98
Comparison mean (Pre-1977)	0.29	0.10	0.52	2857.40
Observations	4744	4744	4744	4744

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes for men at the individual level. Means by age group refer to the comparison area. Standard errors are clustered by pre-program village. Regressions are weighted to adjust for attrition between the MHSS1 and MHSS2 surveys. All variables control for the baseline controls listed in Table D.1.The dependent variable in columns (1) through (3) is the fraction of total hours worked by sector. See Appendix C.1.1 for more details on how we classify workers into sectors. Employment shares do not sum to 1 for two reasons. First, we do not report results for the construction sector. Second, a small set of respondents do not work and are coded as spending 0 percent of their time working in each of the given sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

While the MCH-FP quasi-experiment in Matlab yields causal partial equilibrium effects of improved access to family planning and child health interventions, it does not capture general equilibrium effects. In particular, large-scale changes in fertility and human capital may induce shifts in wages, prices, and technology (Acemoglu 2010). To assess the broader, general equilibrium effects of fertility change, we leverage cross-country and sub-national cross-state variation in the next section.

smaller, reducing the availability of family labor, farmers chose to cultivate less labor-intensive crops. Figure D.2 shows that the program induced a shift toward crops that generate higher revenue per unit of labor.

4 Cross-Country and State-Level Evidence

We next explore whether the relationship between fertility and structural transformation estimated in Section 3 holds when general equilibrium forces are salient. We do so with two exercises using aggregate data. First, we leverage cross-country data and changes in abortion policies between 1960-2006. Second, we use cross-U.S. state data and changes in abortion restrictions in the 19th century.

4.1 Cross-Country Evidence from Abortion Policy Changes

We leverage variation across countries in abortion policies to assess how fertility affects the agricultural employment share. The cross-country analysis has two main advantages. First, we assess whether the relationship estimated in partial equilibrium in Section 3 holds even when accounting for general equilibrium forces at the country level, such as changing prices. 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.

4.1.1 Cross-Country Data

We construct a cross-country panel dataset of agricultural employment shares and abortion policy changes. To measure the agricultural employment share we rely on Wingender (2014b), who compiles and harmonizes data for an unbalanced panel of 169 countries between 1900 and 2010. Additional data details are provided in Wingender (2014a).

We use abortion policy changes across countries between 1960 and 2006 collected by the United Nations Population Division following Bloom et al. (2001).[29] We collapse specific policy changes into an index that varies between 1 and 4 in the spirit of Elías et al. (2017). A value of 1 indicates that abortion is permitted only to save the mother's life; 2 permits abortion to protect the mother's physical or mental health; 3 extends permission to cases of rape or incest; and 4 encompasses all of these grounds as well as economic or social reasons, or abortion upon request.[30] Hence, a higher value of the index indicates that abortion is more accessible. The median value of the index is 2.4, indicating that the typical country only allows abortion to protect the mother's health. The standard deviation of the index is

[29] The UN discontinued updating their abortion policy database in 2007.

[30] The description of abortion laws is not comprehensive, as it does not account for restrictions based on gestational age, procedural requirements such as the number of doctors who must authorize the procedure or the need for a husband's consent, and potential discrepancies between the law on paper and its implementation in practice (Bloom et al. 2009).

1.3. Over the sample period, 84 countries experienced one policy change, 7 experienced two, and 2 experienced three changes.

The combined dataset is an unbalanced panel of 166 countries spanning 1960 to 2006, each with at least one year of non-missing data on abortion policy and agricultural employment share.

4.1.2 Cross-Country Empirical Specification

Fertility rates and agricultural employment share are likely endogenously determined. For example, an improvement in nonagricultural productivity may pull workers away from the farm and raise the returns to human capital, inducing parents to switch away from child quantity and into child quality (Galor 2005). We therefore need an exogenous shifter of fertility rates which is uncorrelated with other factors shaping the agricultural employment share, conditional on controls.

We leverage panel data on countries and variation in country-level policy changes to abortion access, conditional on country and year fixed effects. Specifically, we estimate the following dynamic difference-in-differences specification:

$$AES_{ct} = \alpha_c + \alpha_t + \sum_{\tau=T_0}^{T} \beta_\tau Abortion_{ct} + \epsilon_{ct} \tag{11}$$

where AES_{ct} is country c's agricultural employment share in year t. $Abortion_{ct}$ is equal to the abortion policy index in country c in year t. β_τ then traces out the dynamic effect of abortion policy changes on the birth rate and agricultural employment share. α_c is a vector of country fixed effects to control for non-time varying country factors and α_t a vector of year fixed effects which control for global shocks that uniformly affect each country's agricultural employment share. Standard errors are clustered at the country level.

Given the continuous nature of the treatment, that abortion access may become more or less restrictive, and to address the possibility of negative weights with the two-way fixed effects estimator, we estimate the staggered dynamic difference-in-differences equation (11) following De Chaisemartin and d'Haultfoeuille (forthcoming). Our identifying assumption is that countries with the same initial abortion index (the comparison group) would have experienced similar outcome trends in the absence of changes to the initial abortion index. While this assumption is not directly testable, we examine pre-trends to assess whether outcome trends were similar prior to policy changes to provide suggestive evidence that they may have been similar in the post-period when abortion policies may have changed.

As robustness, we also use a binary abortion policy variable to compare countries where there are few restrictions on abortion policy (a value of 4 in the index) to those with more restrictions (values less than 4 in the index). The results, shown in Appendix Figure D.4, are similar to our baseline estimates.

4.1.3 Cross-Country Results

The estimated β_τ coefficients from equation (11) are plotted in Figure 4, where the horizontal axis counts the years since the first change in abortion policy in a country. The β_τ estimates for the years prior to an abortion policy change are close to zero and statistically indistinguishable from zero, indicating there are no pre-policy trends. This indicates that abortion policy changes do not correlate to pre-policy trends in the agricultural employment share and provides suggestive evidence that the identifying assumption of the model holds.

The effect of abortion policy changes on the agricultural employment share takes a number of years to manifest, suggesting that the immediate effect of fertility reduction on labor force participation is modest. The average effect of a policy making abortion more accessible, approximately a one standard deviation increase in the abortion policy index, is a 6.4 percentage point increase in agricultural employment share 15 to 40 years later. Relative to a mean agriculture employment share of 37 percent, this represents a 17 percent increase.[31]

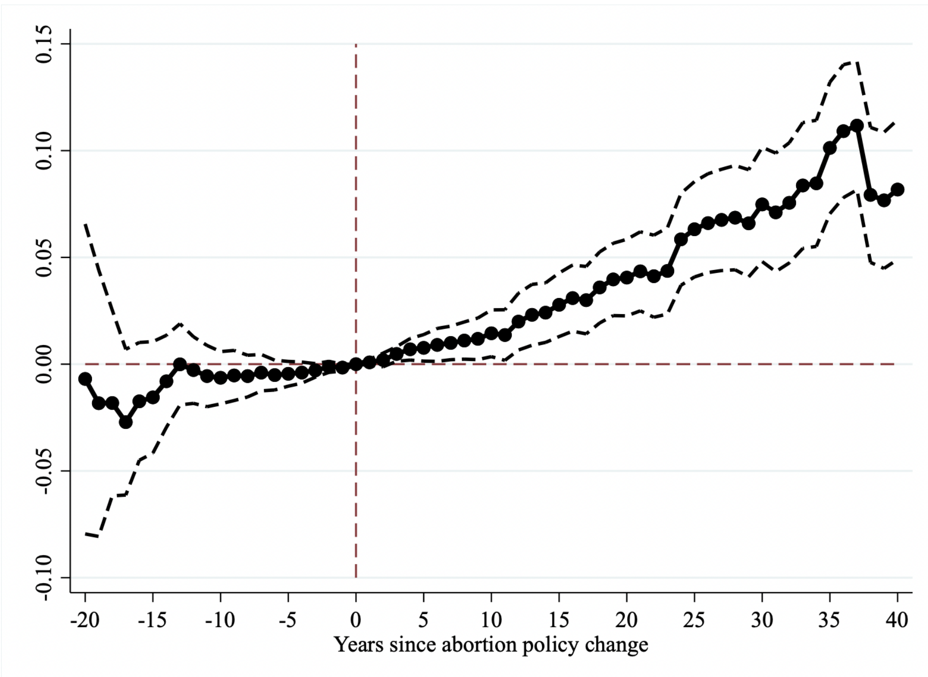
We also estimate the effect of abortion policy on the birth rate using equation (11). Appendix Figure D.3 shows the result. We do not find strong evidence of pre-trends, with the pre-trends being close to zero, flat and statistically insignificantly different from 0. A relaxation of abortion restrictions reduces the birth rate immediately and persistently. The average cumulative effect of a one point increase in the policy index (corresponding to abortion becoming more accessible) reduces the birth rate by 0.29 children per 1,000 population on average over the 10 year period since the policy change. Relative to a mean birth rate of 30 over the sample period, this implies a 1% reduction. This magnitude is very close to the 1.1 percent decline estimated by Bloom et al. (2009), whose sample and estimation approach differs from ours.

Our cross-country results therefore suggest that the demographic transition slows down structural transformation. This is consistent with the modest human capital effects driven by the quantity-quality tradeoff found by Rosenzweig and Zhang (2009) and Bhalotra and Clarke (2020). Hence, the Malthusian land congestion effect dominates, as with the Bangladesh evidence presented in Section 3.

A drawback in the cross-country analysis is that the data may not be directly comparable across countries over time (see, for example, Behrman and Rosenzweig 1994 and Durlauf et al. 2005). To address this concern, we turn next to a within-country analysis. Of course,

[31] Appendix Figure D.4 depicts similar results using the binary abortion indicator for free abortion.

Figure 4: Effect of Abortion Policy Changes on Agricultural Employment Share



Notes: The figure shows event study coefficient estimates for the effect of abortion policy changes on the agricultural employment share. Dashed lines depict 95% confidence intervals with standard errors clustered at the country level. Data on country-level agricultural employment shares 1960–2006 comes from Wingender (2014b). Abortion policy change database compiled by Bloom et al. (2009). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

disaggregating is not without drawbacks of its own as smaller regions are less likely to influence prices and hence we may miss out on some general equilibrium effects that we captured in the cross-country analysis. We therefore view these separate analyses as complementary.

4.2 U.S. State-level Analysis From Abortion Policy Changes

We next consider a subnational 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.[32]

4.2.1 Cross-State Data

To measure agricultural employment share, we use the decennial data compiled by Craig and Weiss (1998) for the period 1800 to 1890. 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 1890 waves. Imputations were necessary, especially in earlier census periods.[33] We provide additional details on the data and their construction in Appendix Section C.2. The dependent variable drawn from these data is the ratio of male agricultural workers ages 10 and older to the total population.[34]

To measure abortion policy changes, we use the database collected by Lahey (2014). 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. Each abortion policy's passage is associated to the subsequent decennial census wave, and the event study variable is a dummy equal to 1 if abortion became more restricted in the given year.

By 1890, all states had passed an abortion restriction, so our estimation sample period is 1800–1880. Additionally, Census coverage expanded with the Western frontier, leading some states to be added later in the sample period. We observe 46 states, the District of Columbia, and the combined Dakota Territories.[35] The combined sample is an unbalanced panel of 306 state-years.

[32]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. Regarding the 'power of the pill,' Myers (2017) argues that the rollout of oral contraception across the U.S. had little impact on fertility.

[33]We redo the estimation using the 1850–1890 full count census waves to construct agricultural employment share and our results do not change; see Appendix Figure D.5.

[34]We focus on male employment since female farm employment, primarily unpaid, was substantially undermeasured in official Census tabulations which focused on paid work (Ngai et al. 2024).

[35]We combine North and South Dakota because the census did so in the first years of covering the Dakotas.

4.2.2 Cross-State Empirical Specification

We estimate the causal effect of abortion restrictions on agricultural employment share over time. Similar to the cross-country analysis, we estimate a staggered dynamic difference-in-differences specification following De Chaisemartin and d'Haultfoeuille (2020) with state and decade fixed effects.

As in the cross-country analysis, our identifying assumption relies on the exogeneity of the timing of abortion restriction law passage within a state. Specifically, the assumption of the estimator is that the states with the same abortion restrictions in 1800 would have similar trends in agricultural employment had abortion policies not changed over time. Given this is not a testable assumption, we again analyze pretrends in agricultural employment share ahead of abortion policy changes. This assumption is also supported by Lahey (2014) finding that the passage of abortion restriction laws in 19th-century America was not correlated with the immigrant population share, literacy rate, pre-law child-to-woman ratio, and, importantly for the present study, the urbanization rate.

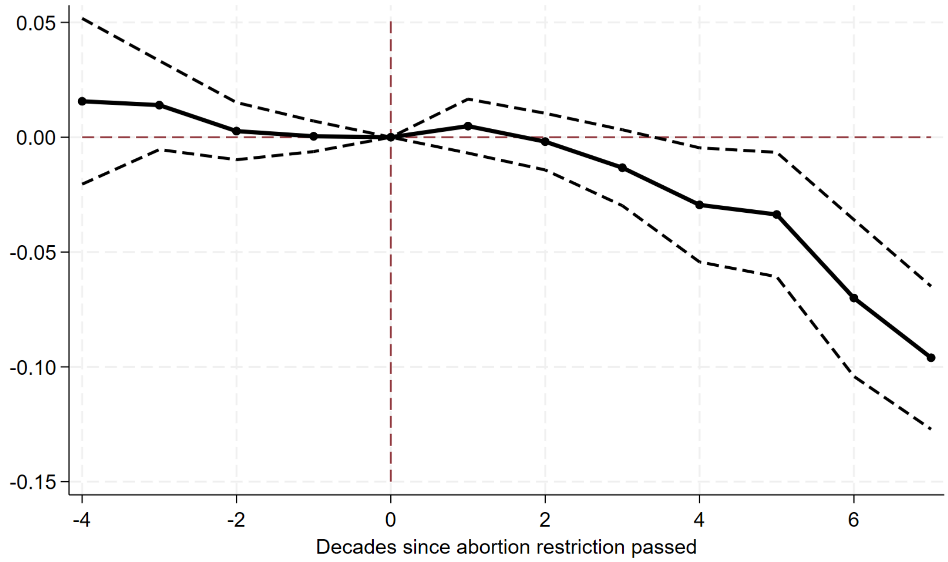
Given the plausibly exogenous timing of the abortion laws, Lahey (2014) leverages the staggered rollout of these laws across U.S. states to show that abortion restrictions had a direct effect on fertility of 10 percent.[36]

4.2.3 Cross-State Results

Figure 5 plots the resulting event study 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 Bangladesh from Section 3 and the cross-country estimates shown in Section 4.1. 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 about 3 percentage points four decades after abortion was restricted, a 16% reduction.

[36]Measures of abortion use across states and over time do not exist for 19th century America, unfortunately. Still, the responsiveness of fertility that Lahey (2014) finds is similar to magnitudes in other studies. For example, Fischer et al. (2018) estimate that policies reducing funding to family planning clinics and imposing burdensome regulations on abortion providers in Texas reduced abortions by 16.7% and increased fertility by 1.3%. Myers (2021) estimates that mandatory waiting periods for abortion reduced abortion takeup by almost 9% and raised fertility by 1.5%. Given the lack of alternative modern contraceptive options available to women in the 19th century, it is unsurprising that the fertility elasticity estimated by Lahey (2014) is larger than those estimated in modern contexts.

Figure 5: Effect of Abortion Restriction on Agricultural Employment Share, U.S. States



Notes: Data on state-level agricultural employment shares 1800-1890 comes from Craig and Weiss (1998). Timing of abortion restrictions come from Lahey (2014) and Lahey and Wanamaker (2025). Dashed lines depict 95% confidence intervals with standard errors clustered at the state level. Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

We conduct two robustness checks of our main state-level results. First, Appendix Figure D.6 shows the event study plot excluding states that passed abortion restriction laws prior to 1840. Laws passed prior to 1840 were often part of larger bills and not enforced until later years (Lahey and Wanamaker 2025). While fewer states and years are included, we still see a statistically and economically significantly negative effect of abortion restrictions on agricultural employment share four decades later.

Second, given the westward expansion of the U.S. throughout the 19th century, we cannot observe the agricultural employment shares for all states for all Census waves. In our baseline, we included an unbalanced panel of states. To gauge the degree to which our sampling composition of states drives our results, we show the event study plot when including only states that we observe in 1800 in Appendix Figure D.7. Because we only observe 20 states as of 1800, we bootstrap the clustered standard errors. As in our baseline analysis, we observe no pretrend and a significantly negative effect of abortion restrictions three plus decades after passage.

4.3 Discussion

Our partial equilibrium quasi-experimental results from Bangladesh line up well with our general equilibrium cross-state and cross-country results: the demographic transition slows down structural transformation. The key potential general equilibrium force which might reverse our partial equilibrium result is the role of demand for food, and hence changes in the price of agricultural output, in a closed economy. Our results in this section are consistent with economies being sufficiently open, on average, such that local agricultural prices are influenced by the world price. They are also consistent with Cavalcanti et al. (2021), who find only modest general equilibrium price effects due to family planning in Kenya. Next, we return to our model from Section 2 to compute several policy-relevant back-of-the-envelope calculations.

5 Back-of-the-Envelope Quantification

How much of a human capital increase would be needed to offset a fertility reduction's effect on agricultural employment share? And how does the effect of fertility reduction vary by country income? To answer these questions, we use the stylized framework to conduct a series of back-of-the-envelope calculations. Specifically, we combine the empirical estimates from the Matlab MCH-FP experiment program of Section 3 with our stylized model from Section 2.

We express the model variables in proportional changes between a baseline and counterfactual scenario. Define $\hat{x}$ as the proportional change in a variable x due to a change in parameters, such as the accessibility of family planning technologies. Then, substituting equation (3) into the right-hand side of equation (4), the change in the agricultural employment share can be written as follows:

$$\widehat{L_a/L} = -\left(\frac{\hat{h}}{1-\theta} + \widehat{n_{-1}} + \hat{\ell}\right). \tag{12}$$

where we removed t subscripts for convenience. Equation (12) indicates that a proportional change in the agricultural employment share is equal to the negative sum of the proportional changes in (i) human capital normalized by the land cost share in agriculture, (ii) the adult population, and (iii) labor supply per adult. We consider three scenarios.

First, we compute the model's predicted agricultural employment share change in Matlab shutting down the human capital channel. This sets an upper bound on the effect one would expect from the first phase of the MCH-FP, which primarily distributed contraception.[37] The predicted change in the agricultural employment share with no human capital effects is thus $\widehat{L_a/L} = -(\widehat{n_{-1}} + \hat{\ell})$.

We set the change in fertility $\widehat{n_{-1}}$ to be -15% (column 1 of Appendix Table D.7) and the change in per household labor supply $\hat{\ell}$ to be -1.9% (column 6 of Table 1). The resulting model-predicted rise in agricultural employment share is 18.1%, modestly smaller than the percent change that we estimated in column 3 of Table 1.

Second, we allow population average human capital to change. As shown by Barham (2012), Barham et al. (2025), and our own household-level estimates in Appendix Table D.12, the child health component of the program increased treated cohorts' years of education. We therefore set $\hat{h}$ equal to 0.057, the percent change in years of education induced by the program for cohorts receiving both the contraceptive and child health interventions (column 4 of Appendix Table D.12). We also must now calibrate the land cost share in agricultural production, $1 - \theta$. Using data from ICRISAT's Village Dynamics in South Asia project, Boppart et al. (2023) compute the land share to be 0.35 in Bangladesh. Adding in human capital substantially reduces the model's predicted impact of the program on the agricultural employment share to a 1.8% increase, as wages in nonagriculture rise with higher human capital. Hence our results suggest that much of the structural transformation slowdown induced by the family planning phase of the MCH-FP can be undone with sufficient human capital investments.

Third, we explore how our quantitative results would change for more developed countries. Boppart et al. (2023) show that the land cost share in agriculture decreases as income per capita rises, and that the value added share of land in agriculture for high-income countries is about 0.1. A high-income country would therefore only need a human capital increase of about 1.8% to offset a 16% reduction in population size. By contrast, a less-developed country like Bangladesh with a 0.35 agricultural land cost share requires 6.3% higher human capital to offset an equivalent fertility drop. Hence, a low-income country would have to raise human capital by 3.5 times more than a high-income country in order to offset an equal fertility reduction's effect on the agricultural employment share.

[37] Human capital increased disproportionately for cohorts born in the latter part of the experimental period, as only those cohorts received the human capital-enhancing child health interventions, as we showed in Section 3.5.2.

6 Conclusion

This paper examines the effects of fertility decline on structural transformation, a central feature of economic development. In the model, we show that a decline in fertility affects the share of employment in agriculture through two opposing channels: reduced land congestion increases the returns to agricultural labor, while increased human capital—arising from the quantity-quality tradeoff—raises the returns to nonagricultural work. Across three empirical settings, we find that the Malthusian land congestion effect dominates, leading to a relative increase in agricultural employment as a result of fertility decline.

Fertility decline is widely viewed as a key mechanism by which countries escape the "Malthusian trap" of high population growth and persistent poverty (Galor 2012). However, population decline may also dampen the pace of technological progress (Jones 2022). We contribute to the debate on the economic growth consequences of falling fertility by highlighting the underexplored mechanism of Malthusian land congestion and, for the first time, empirically assessing its impact on structural transformation.

These findings do not necessarily imply that countries should reduce investments in family planning policies. Rather, they underscore the importance of complementing fertility-reduction efforts with policies that promote human capital accumulation. To ensure that fertility decline contributes to long-run structural transformation, it is critical that family planning programs be accompanied by human capital-improving investments.

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Online Appendix

A Theoretical Appendix

In this section, we provide several extensions to the production side of our simple baseline model from Section 2. We remove time subscripts for convenience.

A.1 Adding Intermediate Inputs

To our baseline framework in Section 2, we have both sectors rely on intermediate inputs for production. 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

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

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

$$w^* = (1-\alpha)\,(p_m A_m)^{\frac{1}{1-\alpha}} \left(\frac{\alpha}{p_z}\right)^{\frac{\alpha}{1-\alpha}} h \tag{A.2}$$

and the agricultural employment share is

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

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

A.2 Adding Intermediate Inputs with CES Functional Form

In Section A.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 as in Equation (A.1). Production of the agricultural good follows a hybrid Cobb-Douglas/Constant Elasticity of Substitution (CES) production process which requires land T_a, labor L_a, and imported intermediate inputs Z_a:

$$Q_a = A_a \left[\omega Z_a^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) L_a^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\theta\epsilon}{\epsilon-1}} T_a^{1-\theta} \tag{A.3}$$

where Q_a is the quantity of agricultural goods produced, and A_a 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_a relative to L_a, while $1-\theta$ is the revenue share accruing to landowners.

The marginal product of labor in agriculture is

$$MPL_a = A_a(1-\omega)\theta L_a^{-\frac{1}{\epsilon}} \left[\cdot\right]^{\frac{\theta\epsilon}{\epsilon-1}-1} T_a^{1-\theta},$$

where $[\cdot]$ is the CES portion of equation (A.3). A key determinant of the wage is the quantity of the fixed factor, T_a, available. Given a fixed amount of land T_a, as the number of workers allocated to agriculture L_a increases, the returns to that labor decline.

In the manufacturing sector, the marginal product is

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

which rises with human capital.

A.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:

$$p_a MPL_a = w = p_m MPL_m$$

which determines the equilibrium wage to be the same as in equation (A.2).

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

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

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

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

A.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. The model generates two key empirical predictions:

(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, $\frac{\partial L_a/L}{\partial h}$ is negative if and only if the below parameter restriction holds:

$$\frac{\left(\frac{\omega}{1-\omega}\right)^{\epsilon} \left(\frac{w^*}{p_z}\right)^{\epsilon-1}}{\left(\frac{\omega}{1-\omega}\right)^{\epsilon} \left(\frac{w^*}{p_z}\right)^{\epsilon-1} + 1} < \frac{1-\epsilon(1-\theta)}{p_z} \tag{A.5}$$

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, both subject to the ease of substituting labor for intermediates in agriculture. 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 (A.5) 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 (A.5) is most likely to hold (and hence $\frac{\partial L_a/L}{\partial h} < 0$) 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 land congestion effects, as in our baseline model from Section 2.

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.[38]

A.3 Partially Closed Economy

The effect of population size on structural transformation necessarily depends on whether the economy is open or closed. 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, we consider the implications of nesting both closed and open economy cases by introducing trade costs.

Let τ denote iceberg trade costs, i.e., a firm must export τ units of a good in order for one unit to arrive at the destination. Then the equilibrium price of sector x's output is

$$P_x^* = \begin{cases} P_x^{cl} & \text{if } \tau P_x^W \geq P_x^{cl} \geq P_x^W/\tau \text{ (closed)} \\ \tau P_x^W & \text{if } \tau P_x^W < P_x^{cl} \text{ (importing)} \\ P_x^W/\tau & \text{if } P_x^W/\tau > P_x^{cl} \text{ (exporting)} \end{cases} \tag{A.6}$$

where P_x^W is the world price and P_x^{cl} is the prevailing local price given a closed economy. Hence whether the economy is fully closed depends on two factors: the magnitude of τ and the difference between the world price and the closed economy price.

If the agricultural sector is closed, 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. As we discuss in Section 2.3, we

[38]Because developed countries are on the technological frontier, an endogenous growth model may be more appropriate, whereby endogenous technical change pulls workers into the innovative sector.

consider it unlikely that many countries' agricultural sector is completely closed. We also refer to Cavalcanti et al. (2021) who estimate only a modest effect of family planning policies on general equilibrium price fluctuations. Finally, our empirical results from Sections 3 and 4 are incompatible with the closed economy assumption.

B Maternal and Child Health and Family Planning Program Details

In this appendix, we describe in greater detail the Matlab Maternal and Child Health and Family Planning program, or MCH-FP. Program interventions were phased in over time. 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 tablets for women in the last trimester of pregnancy (Bhatia et al. 1980). Take up of tetanus toxoid 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).

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 (see Figure D.1). 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. 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 D.1. By 1988, coverage rates for children aged 12–23 months living in the treatment area were 93 percent for the vaccine against tuberculosis, 83 percent for all three doses of the vaccines against diptheria, pertussis, tetanus, and polio, 88 percent for measles, and 77 percent across all three major immunizations (icddr,b 2007). Government services did not regularly provide measles vaccination for children until around 1989, so the comparison area was an almost entirely unvaccinated population (Koenig et al. 1991). Nationally, measles vaccination for children under the age of five was less than 2 percent in 1986 (Khan 1998) and was below 40 percent in the comparison area in 1990 (Fauveau 1994).

C Data Appendix

C.1 Matlab Health and Socioeconomic Survey

Our study relies on household-level and individual-level data collected through two waves of the Matlab Health and Socioeconomic Survey (MHSS1 and MHSS2). The first wave of the survey (MHSS1) collected in 1996 provides the sampling frame for our analysis. MHSS1 was a seven percent random sample survey of household compounds (i.e., baris) in the Matlab area. In each bari, two households were randomly selected for interview: a primary household selected randomly, and a secondary household selected purposively. Within a household, individuals (aged 6 or older) were randomly sampled for in depth interviews.

We begin by building a sample of households using the primary households that were randomly selected for interview in MHSS1. We select households where the household head was a respondent to Book 3 ("Adult Information") of the MHSS1 Household survey. From this set, we remove households where the household head could not be linked back to a treatment status or who could not be linked back to the Matlab area (i.e., the DSS) prior to the start of the MCH-FP in 1977. These criteria result in a set of 2,534 households. When measuring individual-level outcomes in MHSS1, we consider outcomes from Book 3 respondents from these households.

MHSS2 is a panel follow-up survey to the original MHSS1 Household survey that was collected between 2012 and 2014. Fieldwork occurred across multiple years with increasing effort in order to maximize response rates among difficult-to-track migrants. Migrants were identified as a part of the survey and tracked throughout the country. Beginning in October 2013, rapid-response teams were put in place in major city centers in Bangladesh so that interviews could take place once a migrant was found via family members in Matlab. In-person surveys were collected during the two Eid festivals in July and October 2014 when migrants returned to their villages in Matlab. Finally, some international and distant domestic migrants were interviewed via a phone survey in late 2014.

The sample for MHSS2 includes all individuals who were from primary households in

MHSS1 and were selected for personal interviews. The MHSS2 sample further includes the spouses of MHSS1 primary respondents, their descendants, and an additional sample of "pre-MHSS1" migrants who were individuals who had migrated out of the DSS from primary MHSS1 households area prior to the collection of the survey.

MHSS2 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. Migrants were intensively interviewed around Eid celebrations if they were visiting family in Matlab. 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 link outcomes measured in MHSS2 back to our sample of MHSS1 households either through the individuals from the MHSS1 households and their descendants, or based on the household-level outcomes in MHSS2 households where our sample members (and their descendants) reside. For that reason, an MHSS1 household may have sample members living in multiple MHSS2 households. Because of attrition, it is also the case that an MHSS1 household may not have had any respondents in the MHSS2 survey. Indeed, we are able to track outcomes in MHSS2 for 2,484 of the 2,534 MHSS1 household, just over 98 percent. For each outcome, we describe below how we aggregate both household-level and individual-level outcomes to the MHSS1 household-level.

C.1.1 Classifying Industry of Employment

Neither the MHSS1 nor the MHSS2 surveys asked respondents 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. Employment information for MHSS1 come from three modules in the survey: (i) Book 2 Agricultural Employment (AE); (ii) Book 2 Non-Agricultural Employment (NAE); and (iii) Book 3 Employment (EMP). The household head was the respondent to the two modules from Book 2 and they provided information about household members' farm and off-farm employment. We consider all work reported in the AE module to be agricultural employment. We further classify agricultural and fishing occupations reported in NAE and EMP as agricultural employment. These occupations and their corresponding codes include: (1, 2) agriculturalist; (3) agricultural laborer; (24) fisherman; (65) husking, boiling, and drying paddy; (66) goat rearing; (67) duck or hen rearing; and (70) produce vegetables or fruits. We classify all other occupations as non-agricultural.

In each module (AE, NAE, and EMP) we observe the number of months that an individual spent working in their given occupation. To measure the amount of time an individual spends working in each sector, we sum the number of months an individual reports working in each sector across their different occupations. If that summation exceeds 12 months, we top code the amount of work in that sector at 12 months.

MHSS2. Employment information from MHSS2 come from two of the Book 3 Employment modules, Parts A and B. Part A of the Employment module (EMPA), collects information by activity type (e.g., salaried work, piece-rate work, work on the family farm) for work over the previous 12 months. From this module, we observe number of weeks worked by activity over the previous 12 months and the typical number of hours worked in that activity in a week, the product of which gives us our measure of annual hours worked in the activity.

Activities do not directly have an occupation attached to them. We assign these hours to a sector by merging the occupation code collected in Part B of the Employment module (EMPB) to the respective activity. EMPB collects information on each individual's primary and secondary occupation and each are linked to an activity type. Because only two jobs are present in EMPB, some activities in EMPA are not assigned an occupation (i.e., they worked in an activity but it was not their primary/secondary occupation). Beyond occupation code, we use additional information from EMPB to help classify work.

We assign work into one of four sectors: manufacturing, agriculture, service, and construction.

We classify work into manufacturing using the following rules. First, we include all factory work. We determine factory work based on whether an individual works at a government or private factory mill (empb04). We further classify factory work based on occupation codes: garment factory worker (712); jute mill worker (713); food processing factory worker (714); and other factory machine operator (715). Finally, we rely on translated job titles and select occupation titles that include the words "factory" or "mill." In addition to factory work, we also classify crafts-making occupations as manufacturing, including the following occupations: sheet and structural metal supervisor, moulders, and welders (621); blacksmith or tool maker (622); handicraft worker (e.g., jewelry, fabrics, pottery, printing, hand embroidery) (630); food processing (e.g., baker, butcher, dried fish maker) (650); woodworking (e.g., treaters, cabinet makers, furniture maker) (651); garment and related trade workers (e.g., tailor, seamstress, machine embroidery, upholstery, tanning) (652); other craft workers (680); and mine worker or mineral processing (711).

To classify agricultural work, we rely on the activity types that work is reported by in EMPA, as well as occupation codes from EMPB. Two activities from EMPA are explicitly related to agriculture—work as an agricultural day laborer and work on a family farm. We further include work reported in other activities in EMPA if the corresponding occupation code from EMPB is related to agricultural work: farmer, own farm (511); farmer, sharecropper (512); raising cows, goats, or sheep (513); raising ducks or hens (514); fish farm or fish hatchery (515); fishing in river or sea (516); other agriculture or forestry production (517); and agricultural laborer (820). From these, we exclude any work that was classified as manufacturing because the occupation title included the words "mill" or "factory."

We classify a job in the service sector if the occupation corresponds to a purely service occupation, as well as other occupations not classified into the agriculture or manufacturing sector. We include occupation codes 100–442, which broadly represent work as managers (100s), professionals (200s), technicians and associate professionals (300s), and clerical support, sales workers and security (400s). Beyond these broad categories, we also include: skilled home finish or repair (612); machinery mechanics and repair (623); electrical and electronic appliance repair, maintenance and installation (640); traditional healer (660); traditional birth attendant (661); entry-level or non-degree healthcare worker (662); social worker (663); tutor (670); driver of car, van or motorcycle (730); driver of heavy equipment (731); driver of taxi, CNG, autorickshaw (732); domestic worker in home or office (811); caretaker, gardener, messenger, or doorman of home or office (812); rickshaw driver (813); boatman (814); street vendor or hawker (815); bearer or peon (816); food preparation assistant or kitchen helper (840); sweeper (860); refuse worker, sorter recycler, forager (870); and other daily laborer or elementary worker (890). We also include some records with occupation code 830 if the given occupation title was translated to be bus conductor, transport labor, tire business, transport worker, truck helper, and truck labor. Finally, any work that could not be classified with an occupation code (i.e., work reported in EMPA that did not have a corresponding job reported in EMPB) and was not in an agricultural activity was included in service.

The final sector we classify is the construction sector. Here we include work as: carpenter, skilled house builder, supervisor, house contractor, mason (611); construction or earth-work laborer, non-food for work (821); construction or earth-work laborer, food for work (822), and any remaining unclassified work as laborer in factory, mine, or transport (830). A relatively small share of work is in construction (about 7 percent among comparison households) so we do not report results for this sector.

C.1.2 Aggregating MHSS2 Outcomes to MHSS1 Households

Our MHSS2 sample includes individuals who resided in or descended from an MHSS1 household. Consequently, every MHSS2 respondent in our sample links back to a single MHSS1 household, making it relatively straightforward to aggregate individual-level outcomes to the MHSS1 household level. When constructing measures of share of work time by sector, we first sum total hours worked and hours by sector to the MHSS1 household level. We then construct our sectoral share measures by dividing time spent working in a given sector by the total time spent working. When measuring binary outcomes at the individual level (e.g., whether an individual ever worked in a factory), we aggregate to the MHSS1 household level by averaging across respondents in the household.

MHSS2 respondents from a given MHSS1 household, however, reside in (potentially) multiple MHSS2 households, making it less straightforward to aggregate MHSS2 household-level outcomes to a single MHSS1 household. When constructing binary outcomes at the household level (e.g., does the household farm?), we aggregate to the MHSS1 household by asking whether any MHSS2 respondents from the household live in a household with that outcome (i.e., taking the maximum value across MHSS2 households with a sample member). When constructing continuous measures (e.g., acres of land owned by the household), we sum the amounts across the MHSS2 households, using each household's outcome only once regardless of the number of sample respondents residing in a given household.[39]

C.1.3 Accounting for Household-Level Attrition in MHSS2

The main results are weighted to account for household-level attrition between MHSS1 and MHSS2. Our analysis sample includes 2,534 households selected from the primary MHSS1 sample. We are able to track outcomes into MHSS2 for more than 98 percent of them. To account for this small amount of attrition, we construct inverse propensity weights that predict household-level attrition using the set of baseline characteristics reported in Table D.1 as well as their interaction with treatment assignment. Similarly, in our individual-level analysis we construct weights to account for attrition in MHSS2 among our individual panel sample following Barham et al. (2023).

C.2 U.S. State-level Data Construction

This section summarizes the data construction decisions taken by Craig and Weiss (1998) to generate agricultural employment to population ratios for each U.S. state between 1800 and 1900.

States appear in the data over time as the U.S. expanded westward and the Census Bureau began covering them. Our interest is in computing the agricultural employment to

[39]In principle, individuals from two separate MHSS1 households could reside in the same MHSS2 household. In those cases, the household's outcome is used in constructing the outcome for each MHSS1 household. In practice, this was very rare, which makes sense given the low sampling rate in MHSS1.

population ratio over time. The denominator, the total population, is readily available from the U.S. Census.[40]

The numerator, the agricultural workforce, is trickier to compute and requires some assumptions and imputations. Craig and Weiss (1998) focus on rural agricultural employment; we further restrict our focus to male workers, since unpaid work, which was disproportionately done by women, was substantially undermeasured by the Census (Goldin 1990; Ngai et al. 2024). Agricultural employment is measured for those age 10 and up.

The approach to imputing male agricultural employment differs between the antebellum and post-civil war periods. For censuses conducted between 1870 and 1900, agricultural work was imputed based on each respondent's occupation. For occupations with an ambiguous sector, specifically "laborers not otherwise specified," Craig and Weiss (1998) used the 1910 census's proportion of such workers by industry among workers living in rural areas. 1910 was the first census wave in which industry was asked of respondents. This approach contrasts with the IPUMS's construction of a consistent industry variable (`ind1950`) across census waves, in which they do not impute an industry for "non classifiable" workers.[41] As a robustness check, we show very similar results to our baseline in Figure D.5 when using the 1850 to 1890 full count censuses from IPUMS (Ruggles et al. 2024). We stick with the data of Craig and Weiss (1998) as our baseline to maximize comparability and consistency in data construction across census waves.

For censuses conducted between 1800 and 1860, we sum free and enslaved farm workforces. The Census reports state-level male agricultural employment for those 16 and older in 1850 and 1860. Craig and Weiss (1998) then impute free male agricultural employment among those age 10–15 using both the fraction residing in rural areas as of 1860 and the fraction of rural residents employed in agriculture within the 10–15 age group. For enslaved people within the same age group, Craig and Weiss (1998) allocate a fraction of rural enslaved people age 10 and older to agriculture according to patterns observed in the 1820 and 1840 censuses, following Weiss (1992). Again, we emphasize that results are little changed when using the complete count census waves from 1850 onwards by Ruggles et al. (2024).

For the 1820 and 1840 waves, Weiss (1992) notes in his appendix several shortcomings in census tabulations. These include nonexhaustive industry coverage, the exclusion of some enslaved people, and seemingly arbitrary variation in demographic and industry coverage across states related to local census supervisors' discretion. This leads to the presence of many outliers. Weiss corrects these outliers by identifying counties within the same census

[40] See, for example, https://www2.census.gov/library/publications/decennial/1850/1850a/1850a-02.pdf for the state population between 1800 and 1850.

[41] See https://usa.ipums.org/usa-action/variables/IND1950#comparability_section.

year that exhibited reliable coverage, or looks to other census years when coverage was more reliable. In many cases, Weiss uses observations from these reliable counties/years to impute values for unreliable counties.

For census years 1800, 1810, and 1830, additional imputations were done by Weiss (1992). These relied primarily on the 1820 and 1840 waves, but, in some cases, also the 1860 census.

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D Additional Tables and Figures

Table D.1: Baseline Balance

	Treatment Area		Comparison Area		Difference in Means		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Mean	SD	Mean	SD	Diff.	T-stat	Diff./SD
Land size 1982 (decimals)	11.68	(15.96)	11.06	(16.19)	-0.62	-0.72	-0.03
Bari size	8.06	(5.50)	8.87	(5.99)	0.81	1.75	0.08
Family size	6.87	(2.95)	7.01	(2.94)	0.15	1.14	0.04
Wall tin or tin mix (=1)	0.314	(0.460)	0.317	(0.462)	0.003	0.13	0.01
Tin roof (=1)	0.833	(0.370)	0.828	(0.375)	-0.005	-0.26	-0.01
Number of boats	0.672	(0.623)	0.667	(0.630)	-0.006	-0.13	-0.01
Owns a lamp (=1)	0.613	(0.484)	0.652	(0.473)	0.040	1.08	0.07
Owns a watch (=1)	0.149	(0.354)	0.160	(0.364)	0.011	0.58	0.03
Owns a radio (=1)	0.080	(0.269)	0.081	(0.271)	0.001	0.10	0.00
Number of rooms (per capita)	0.206	(0.097)	0.212	(0.102)	0.007	1.49	0.06
Number of cows	1.29	(1.73)	1.45	(1.70)	0.16	1.81	0.08
Latrine (=1)	0.864	(0.341)	0.821	(0.381)	-0.043	-1.62	-0.06
Drinking water, tubewell (=1)	0.163	(0.367)	0.322	(0.464)	0.159	4.14	0.20
Drinking water, tank (=1)	0.321	(0.464)	0.394	(0.485)	0.073	1.40	0.05
HH head < 2 years education	0.610	(0.485)	0.564	(0.493)	-0.046	-1.84	-0.07
HH head works in agriculture (=1)	0.592	(0.489)	0.596	(0.487)	0.004	0.15	0.01
HH head works in fishing (=1)	0.063	(0.241)	0.055	(0.227)	-0.008	-0.49	-0.02
HH head works in business (=1)	0.096	(0.293)	0.125	(0.329)	0.029	1.40	0.07
HH head age	46.24	(13.38)	47.17	(13.72)	0.93	1.76	0.07
HH head spouse < 2 years education	0.844	(0.334)	0.806	(0.366)	-0.038	-2.02	-0.09
HH head spouse's age	36.04	(10.29)	36.65	(10.81)	0.62	1.32	0.06
1996 HH head Muslim	0.959	(0.199)	0.839	(0.367)	-0.119	-3.47	-0.34

Notes: The sample includes MHSS1 households where the household head could be traced back to the DSS area before 1977 and that had at least one household member or descendant who appeared in the MHSS2 survey. Unless otherwise noted, household characteristics come from the 1974 census. MHSS1 household baseline characteristics are traced back from the MHSS1 head. Standard deviations (SD) are clustered at the treatment village level. There are 1,176 treatment area households and 1,308 comparison area households. Standard deviations used in Column (7) come from comparison area households.

Table D.2: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector and Urbanicity: Household-Level

	Urban			Rural		
	(1) Agriculture	(2) Manufacturing	(3) Services	(4) Agriculture	(5) Manufacturing	(6) Services
Treatment	0.008	-0.037***	-0.015	0.033**	0.005	0.003
	(0.005)	(0.010)	(0.019)	(0.014)	(0.009)	(0.017)
% chg. rel. to mean	207.5	-24.0	-6.2	16.3	10.1	1.3
Mean	0.00	0.15	0.25	0.20	0.05	0.24
Baseline controls	Y	Y	Y	Y	Y	Y
Observations	2484	2484	2484	2484	2484	2484

Notes: The table presents estimates of equation (8) for outcomes measured in 2014 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.1.1 for more details on how we classify workers into sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.3: ITT Effects of MCH-FP on Land Ownership

	Acres owned	
	(1) MHSS1 (1996)	(2) MHSS2 (2012-2014)
Treatment	-0.044	0.017
	(0.108)	(0.097)
% chg. rel. to mean	-2.7	1.3
Mean	1.61	1.33
Baseline controls	Y	Y
Observations	2525	2482

Notes: The table presents estimates of equation (8) where the outcome is total acres owned by the MHSS1 household. Results for 1996 are shown in column 2, and for 2014 in column 2. 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.

Table D.4: 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 in factory	Currently works in factory	Works at employer with > 100 employees
Treatment	0.04***	0.00	0.01	-0.02**	-0.02***	-0.02**
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
% chg. rel. to mean	19.9	5.4	3.6	-14.4	-22.6	-20.5
Mean	0.22	0.02	0.14	0.15	0.08	0.08
Baseline controls	Y	Y	Y	Y	Y	Y
Observations	2484	2484	2484	2484	2484	2484

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes aggregated to the MHSS1 household level. Each dependent variable is the share of household members exhibiting the described behavior. Entrepreneurship is defined as owning a business. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.5: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level, Robustness

	MHSS1 (1996)		MHSS2 (2012–2014)			
	(1) Agriculture Share	(2) Non-Agriculture Share	(3) Agriculture Share	(4) Manufacturing Share	(5) Services Share	(6) Annual Hours Per Person
Panel A: Full Sample						
Treatment	0.007	0.004	0.041***	-0.032**	-0.013	-27.083
	(0.021)	(0.021)	(0.014)	(0.014)	(0.018)	(35.457)
% chg. rel. to mean	1.1	1.2	19.9	-15.8	-2.8	-1.9
Mean	0.68	0.36	0.21	0.20	0.48	1445.47
Observations	2534	2534	2484	2484	2484	2484
Panel B: Within 3km of Treatment Border						
Treatment	-0.009	0.010	0.029*	-0.007	-0.013	-5.758
	(0.027)	(0.027)	(0.017)	(0.017)	(0.023)	(40.947)
% chg. rel. to mean	-1.2	3.0	13.4	-3.8	-2.8	-0.4
Mean	0.71	0.34	0.22	0.18	0.48	1425.30
Observations	1718	1718	1686	1686	1686	1686
Panel C: Only Muslim Households						
Treatment	0.003	0.009	0.035**	-0.032**	-0.007	-27.852
	(0.022)	(0.022)	(0.015)	(0.015)	(0.018)	(36.094)
% chg. rel. to mean	0.5	2.7	16.7	-16.1	-1.4	-1.9
Mean	0.68	0.35	0.21	0.20	0.48	1440.58
Observations	2286	2286	2241	2241	2241	2241
Panel D: Control for Flood Embankment						
Treatment	-0.003	0.011	0.039***	-0.024*	-0.010	-28.699
	(0.023)	(0.022)	(0.014)	(0.014)	(0.018)	(37.510)
% chg. rel. to mean	-0.5	3.0	18.8	-12.1	-2.1	-2.0
Mean	0.68	0.36	0.21	0.20	0.48	1445.47
Observations	2534	2534	2484	2484	2484	2484
Panel E: Exclude Main City						
Treatment	0.018	-0.003	0.057***	-0.041***	-0.014	-60.979
	(0.023)	(0.023)	(0.014)	(0.015)	(0.020)	(39.354)
% chg. rel. to mean	2.6	-0.9	27.1	-19.7	-3.1	-4.2
Mean	0.68	0.35	0.21	0.21	0.47	1446.75
Observations	2064	2064	2020	2020	2020	2020

Notes: The table presents estimates of equation (8) for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Columns (1) and (2) measure outcomes in the 1996 MHSS1, while Columns (3) through (6) measure outcomes in the 2012–2014 MHSS2. MHSS1 outcomes are the share of working months in the year in which household members could work allocated to each sector. MHSS2 outcomes are the share of hours worked by sector within the household. Panel A uses the full sample of households. Panels B and C restrict the sample to households from villages within 3km of the treatment border and Muslim households, respectively. Panel D adds a control indicating whether the household's traceback village was affected by a flood protection embankment constructed in 1987. Panel E excludes households whose pre-program village is within the Matlab town boundary. See Appendix C.1.1 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 D.6: P-values of Effect of MCH-FP on Work Time Shares by Sector: Household-Level, Inference Robustness Checks

	MHSS1 (1996)		MHSS2 (2012–2014)			
	(1)	(2)	(3)	(4)	(5)	(6)
	Agriculture Share	Non-Agriculture Share	Agriculture Share	Manufacturing Share	Services Share	Annual Hours Per Person
Naive P-value	0.740	0.833	0.003	0.027	0.453	0.446
Block-Level W.C. Bootstrap	0.612	0.282	0.074	0.121	0.282	0.484
Rand Inf. with Contiguous Area	0.791	0.798	0.059	0.202	0.304	0.583
FDR Correction	1.000	1.000	0.019	0.072	0.829	0.829

Notes: All robustness checks are based on the estimates reported in Table 1. The table reports p-values from (i) standard errors clustered by pre-treatment village, (ii) a wild cluster bootstrap where clusters are pre-treatment village blocks, (iii) randomization based inference where a distribution of test statistics is constructed by reassigning treatment status to villages over 10,000 permutations while maintaining a geographically contiguous treatment area, and (iv) adjusted p-values that control for the false discovery rate (FDR) (Anderson 2008) from multiple hypothesis testing across the outcomes reported in the table.

Table D.7: ITT Effects of MCH-FP on Household Size and Composition

	(1) Number of Men Age 24–34	(2) Number of Women Age 24–34
Treatment	-0.16***	-0.10***
	(0.04)	(0.04)
% chg. rel. to mean	-16.2	-11.4
Mean	0.98	0.90
Baseline controls	Y	Y
Observations	2484	2484

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

Table D.8: ITT Effects of MCH-FP on Long-term Work Hours by Sector: Individual-Level, Women

	Share hours by sector				
	(1)	(2)	(3)	(4)	(5)
	Agriculture	Manufacturing	Services	Non-Market	Hours worked
Treatment × Born 1982–1988	0.051***	0.008	-0.007	-0.052*	46.216
	(0.019)	(0.022)	(0.017)	(0.028)	(58.654)
Treatment × Born 1977–1981	-0.026	-0.019	0.030	0.014	-90.233
	(0.032)	(0.023)	(0.023)	(0.043)	(83.717)
Treatment × Born Pre-1977	-0.002	-0.003	0.011	-0.004	-4.312
	(0.021)	(0.009)	(0.009)	(0.019)	(29.078)
% chg. (1982–88)	40.8	6.7	-8.0	-7.8	10.9
% chg. (1977–81)	-12.5	-16.4	44.7	2.3	-17.5
% chg. (Pre-1977)	-0.6	-8.8	23.2	-0.7	-1.1
Comparison mean (1982–88)	0.12	0.12	0.08	0.67	423.77
Comparison mean (1977–81)	0.21	0.11	0.07	0.61	514.93
Comparison mean (Pre-1977)	0.36	0.03	0.05	0.56	392.90
Observations	4628	4628	4628	4628	4628

Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes for women at the individual level. Means by age group refer to the comparison area. Standard errors are clustered by pre-program village. Regressions are weighted to adjust for attrition between the MHSS1 and MHSS2 surveys as discussed in Section C.1.3. All variables control for the baseline controls listed in Figure 2. The dependent variable in columns (1) through (3) is the fraction of total hours worked by sector. See Appendix C.1.1 for more details on how we classify workers into sectors. Employment shares do not sum to 1 for two reasons. First, we do not report results for the construction sector. Second, a small set of respondents do not work and are coded as spending 0 percent of their time working in each of the given sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.9: ITT Effects of MCH-FP on Revenue and Profits per Acre

	Revenue per acre		Profit per acre	
	(1)	(2)	(3)	(4)
	Min. Price	Max. Price	Min. Price	Max. Price
Treatment	34.604	28.261	18.641	12.298
	(37.612)	(47.956)	(29.487)	(40.644)
% chg. rel. to mean	10.1	5.4	11.8	3.7
Mean	341.02	519.65	157.64	336.28
Baseline controls	Y	Y	Y	Y
Observations	2003	2003	2003	2003

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. Revenues are constructed for each crop and are equal to the total amount of the crop harvested (in kilograms) multiplied by the prevailing national price (per kilogram). Prices are derived from the national Bangladeshi statistical yearbooks 2012–2014. Minimum (maximum) prices are the minimum (maximum) price listed in the yearbook for a given year within a crop type (e.g., Paddy Aman) among all varieties of that crop type (e.g., coarse or fine). Profits are equal to revenues net of the cost of inputs (e.g., seeds, fertilizers, pesticides, irrigation, tilling, and labor for cultivation). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.10: ITT Effects of Consumption Shares by Sector

	(1) Agriculture	(2) Manufacturing	(3) Services
Treatment	-0.01	-0.00	0.01*
	(0.01)	(0.00)	(0.01)
% chg. rel. to mean	-2.6	-2.5	5.8
Mean	0.52	0.19	0.25
Baseline controls	Y	Y	Y
Observations	2013	2013	2013

Notes: The table presents estimates of the effect of the MCH-FP on 2014 consumption outcomes aggregated to the MHSS1 household-level. Consumption is measured at the MHSS2-household level, and is summed across MHSS2 households to the MHSS1 household level. The sample is restricted to MHSS1 households where MHSS2 consumption was observed within at least one household. Consumption goods classified into sectors based on United Nations (2018). *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.11: Mincer Regressions, Returns to Experience and Education by Sector

	(1) Agriculture	(2) Manufacturing	(3) Services
Years of education	0.025**	0.047***	0.062***
	(0.010)	(0.008)	(0.004)
Age	0.019	0.129***	0.057***
	(0.024)	(0.023)	(0.015)
Age squared	-0.000	-0.002***	-0.001***
	(0.000)	(0.000)	(0.000)
Average wage (Taka)	46	58	91
Average years of education	3.6	6.7	7.0
Average age	46.9	36.3	39.9
Observations	1129	650	2465

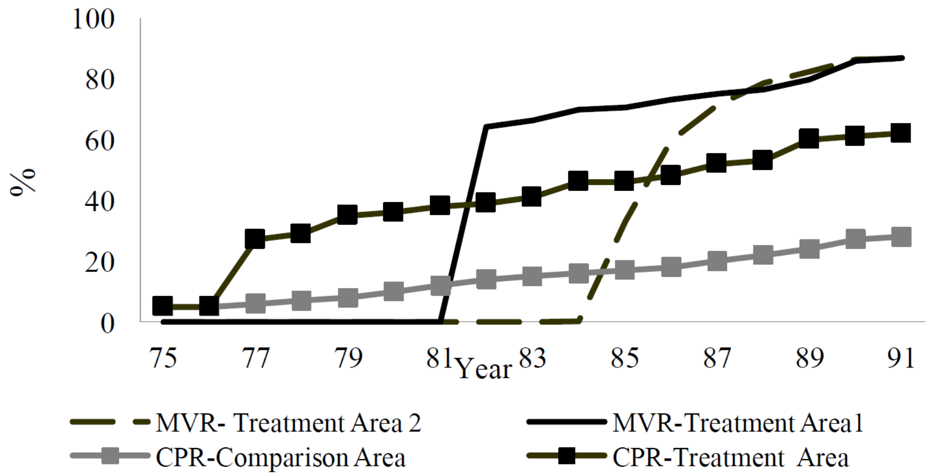
Notes: The table presents estimates from a Mincer wage regression by sector. The dependent variable—log hourly wage—is calculated by dividing total sectoral earnings by hours worked in the sector. The sample includes MHSS2 respondents who are MHSS1 household members or their descendants, and is restricted to men born between 1947 and 1988. Heteroskedasticity-robust standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.12: ITT Effects of MCH-FP on Years of Education

	All Adults			Adults Born 1982–1988		
	(1) Pooled	(2) Men	(3) Women	(4) Pooled	(5) Men	(6) Women
Treatment	-0.002	0.049	-0.011	0.406**	0.695**	0.147
	(0.132)	(0.158)	(0.134)	(0.200)	(0.279)	(0.205)
% chg. rel. to mean	-0.0	0.8	-0.2	5.7	9.8	2.0
Mean	5.51	6.06	4.79	7.18	7.08	7.20
Baseline controls	Y	Y	Y	Y	Y	Y
Observations	2483	2358	2373	1463	935	946

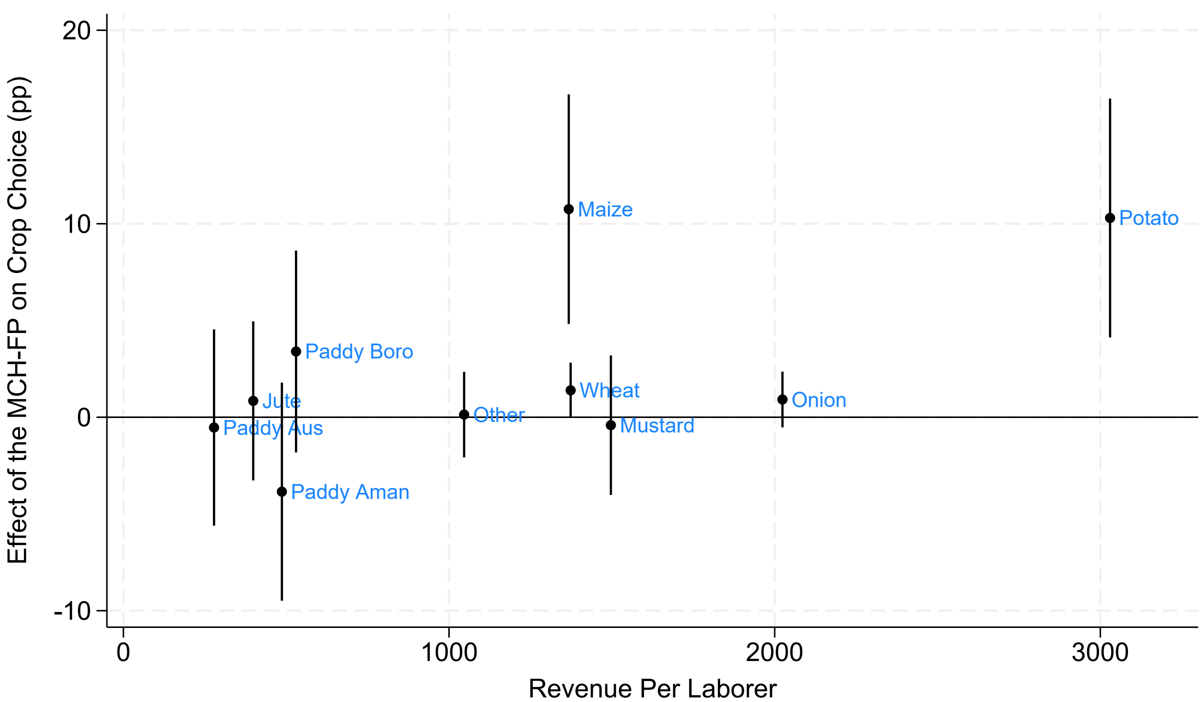
Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure D.1: Trends in contraceptive prevalence rate (CPR) and measles vaccination rates (MVR) for children 12-59 months by calendar year



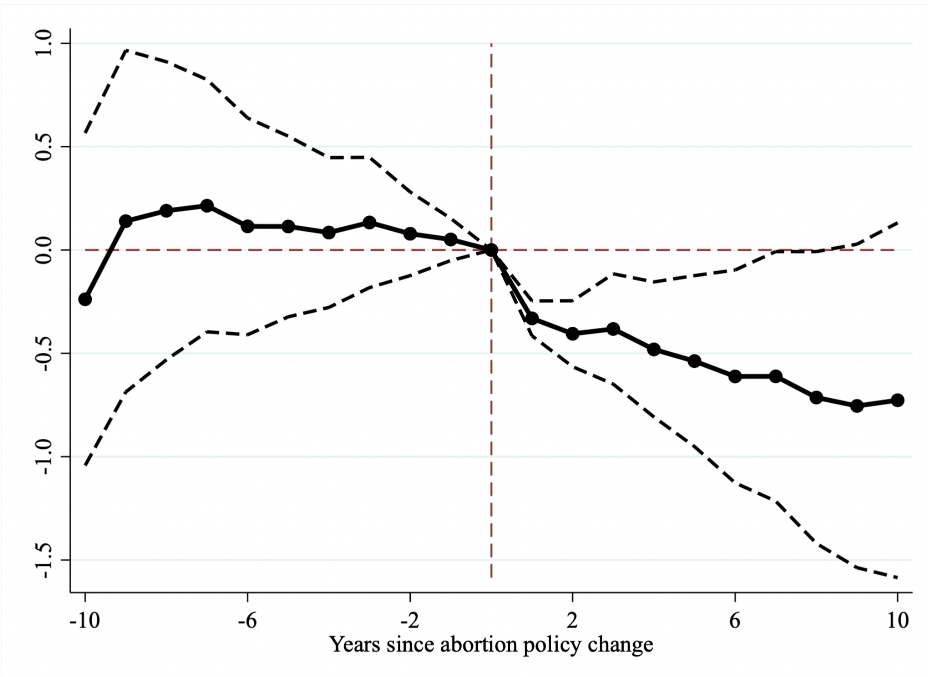
Source: Replicated from Figure 2 in Barham et al. (2023).

Figure D.2: ITT Effects of MCH-FP on Crop Choice and Average Crop Productivity



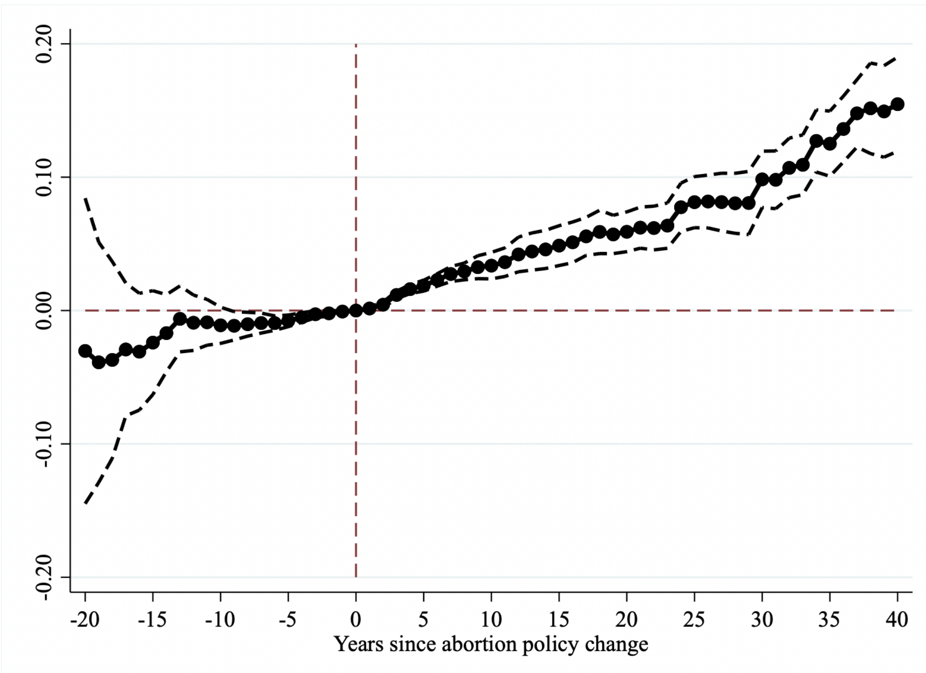
Notes: The figure reports estimates of equation (8). The vertical axis reports the ITT effect of the MCH-FP on whether the household grew the given crop. The horizontal axis reports the average revenue per unit of labor when producing the crop, which comes from national Bangladeshi statistical yearbooks 2012–2014. Vertical bars represent the 95% confidence intervals.

Figure D.3: Effect of Abortion Policy Changes on Crude Birth Rate



Notes: The figure shows event study coefficient estimates for the effect of country-level abortion policy changes on the crude birth rate. Dashed lines depict 95% confidence intervals with standard errors clustered at the country level. Annual data on crude birth rates come from the World Bank Development Indicators as compiled by Delventhal et al. (2021). Abortion policy change database compiled by Bloom et al. (2009). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

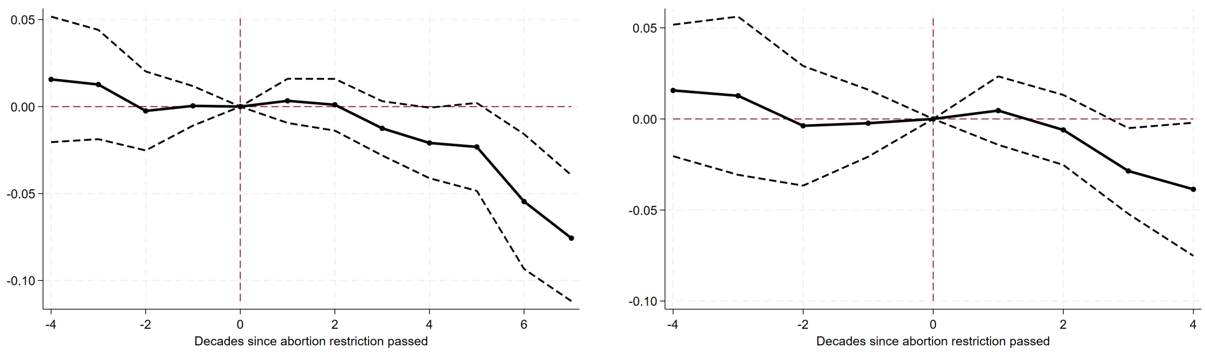
Figure D.4: Effect of Abortion Policy Changes on Agricultural Employment Share Using Dummy for Abortion Freely Accessible



Notes: The figure shows event study coefficient estimates for the effect of abortion policy changes on the agricultural employment share, using a binary indicator of abortion policy which is 1 when the abortion index is equal to 4 and zero otherwise, indicating the most relaxed abortion policies. Dashed lines depict 95% confidence intervals with standard errors clustered at the country level. Data on country-level agricultural employment shares 1960–2006 comes from Wingender (2014b). Abortion policy change database compiled by Bloom et al. (2009). Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

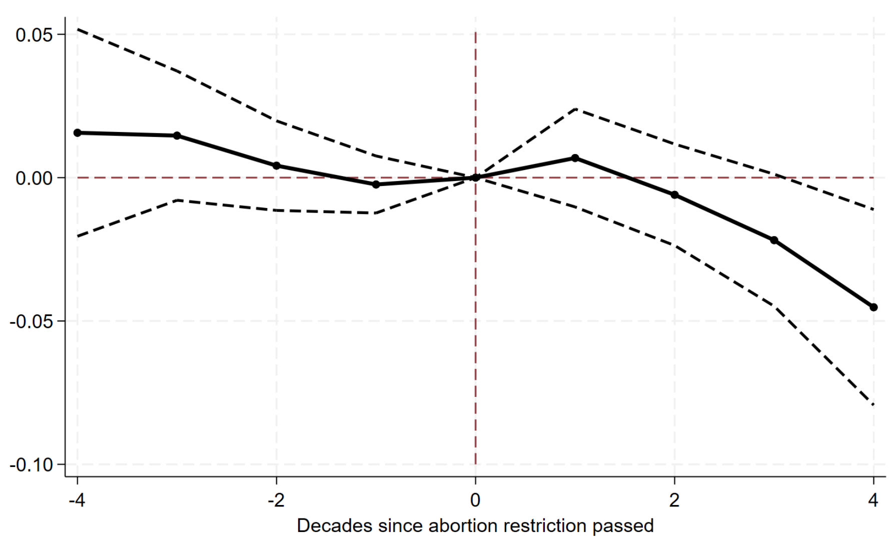
Figure D.5: Effect of Abortion Restrictions on Agricultural Employment Share, U.S. States, using Full Count Census Waves

(a) all abortion policies (b) post-1840 abortion policies



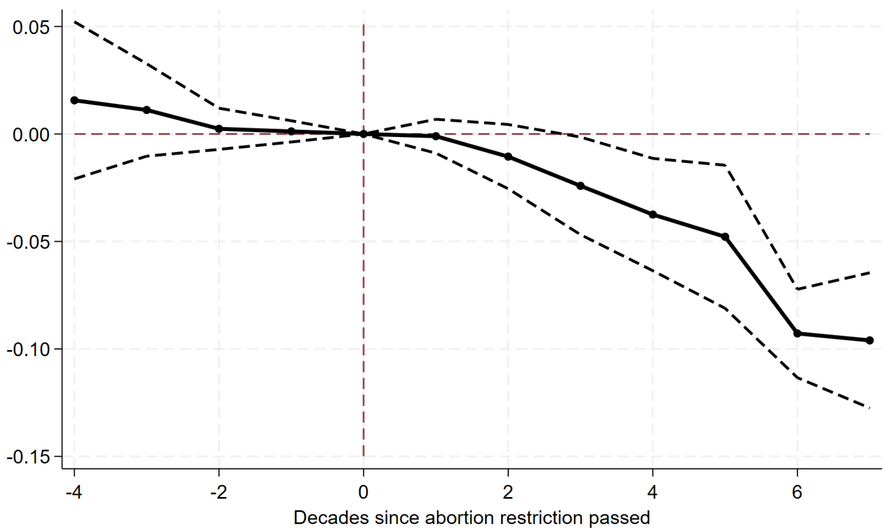
Notes: Data on state-level agricultural employment shares 1800-1840 comes from Craig and Weiss (1998). Agricultural employment shares for 1850–1890 computed from Ruggles et al. (2024). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). Dashed lines depict 95% confidence intervals with standard errors clustered at the state level. Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

Figure D.6: Effect of Abortion Restrictions (excluding those passed before 1840) on Agricultural Employment Share, U.S. States



Notes: Data on state-level agricultural employment shares 1800-1890 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). Dashed lines depict 95% confidence intervals with standard errors clustered at the state level. Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

Figure D.7: Effect of Abortion Restrictions on Agricultural Employment Share, U.S. States Observed in 1800 Only



Notes: Data on state-level agricultural employment shares 1800-1890 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). Dashed lines depict 95% confidence intervals with standard errors clustered at the state level, computed via 1,000 bootstrap repetitions. Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).