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 around the world in the last 60 years and across U.S. states in the 19th century, generate plausibly exogenous variation in fertility. In each of these three empirical analyses, lower fertility raises the agricultural employment share. Improving human capital can therefore offset the effect of fertility declines on the agricultural employment share.

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

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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 jobs in 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 nearly every country (Delventhal et al. 2021), and with global population expected to peak within the next 60 years (United Nations 2024), understanding the role of declining fertility on structural transformation is increasingly important. Yet, credible empirical evidence remains limited, in part due to the lack of exogenous policy variation occurring sufficiently 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 a fixed factor—land—a declining population reduces land congestion, increasing returns to labor in farming and potentially 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 labor-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 which 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 can lead to costly children. Parents can reduce the number of children they have and the associated costs of investing in their human capital 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 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 is endogenous to economic development and the structure 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, demands 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 overcome each of these challenges in three complementary empirical exercises. Exogenous variation in fertility comes from a quasi-experimentally disbursed program in Bangladesh, and abortion policy changes around the world in recent decades and in 19th century America. The Bangladesh context offers rich household and individual data over a long-time horizon, allowing us to examine mechanisms at the level of decision makers. The quasi-experiment only treated a small area of Bangladesh, therefore yielding insight only into partial equilibrium effects. The global and U.S. analyses, by contrast leverage on decades of aggregated data to shed light on the effect of fertility change in agricultural employment share in light of general equilibrium adjustment to prices.

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 in the medium run. 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 analysis 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 that improved adolescent human capital (Barham 2012; Barham et al. 2021). The program accelerated the demographic transition by reducing fertility and improving child survival and health (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 of population size and the child quantity-quality tradeoff.

Our findings illustrate that the program-accelerated demographic transition slowed structural transformation. Thirty-five 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: population size and human capital. First, we use the program's quasi-experimental variation to estimate the effect of household size on sectoral labor allocation. Adding an adult male to the average household more reduces the share of work time spent in agriculture by 26 percentage points. In levels, however, the marginal male increases total household hours worked in manufacturing and services, with little effect on agricultural labor, suggesting that fertility-driven household growth delays structural transformation primarily through relative rather than absolute shifts.

Second, we estimate the effect of human capital using the rollout of the program over time leading to variation in exposure to the intensive child health phase of the program. Men born during this phase, compared to just before it, attained higher levels of schooling and cognitive achievement (Barham 2012; Barham et al. 2021). We provide suggestive evidence that they were 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 labor-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. 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 leverage variation in national abortion policy to estimate the long-run impact of fertility decline on sectoral employment. Specifically, we implement an event study of the effect of abortion access on the agricultural employment share, using a staggered dynamic difference-in-differences (De Chaisemartin and d'Haultfoeuille 2024) to account for the staggered timing of abortion policy changes across countries. A nearly one standard deviation increase in abortion accessibility is associated with a 5 percentage point increase in the agricultural employment share three decades later. These results align with the Bangladesh evidence: fertility decline slows the reallocation of labor out of agriculture. These estimates suggest that in the long run the population size 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 (De Chaisemartin and d'Haultfoeuille 2024). Event study estimates reveal that abortion restrictions accelerate structural transformation in subsequent decades. On average, 19th century abortion restrictions, which increased fertility rates, decreased the agricultural employment share by about 5 percentage points three decades later, consistent with our cross-country results.

Finally, we use the model and estimated elasticities to conduct two back-of-the-envelope calculations. First, we quantify how much human capital investment is required to offset the Malthusian land congestion mechanism highlighted in the empirical analysis. In Matlab, Bangladesh, we estimate that human capital would need to increase by a factor of three to offset Malthusian land congestion effects. Second, we consider variation in required offsetting human capital investment across the development spectrum, leveraging the fact that 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 household-level evidence on the effect of demographic transition on structural transformation, two central features 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 include 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 rather than exploiting exogenous variation in fertility for causal identification,

[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 changes in ethnic minorities, 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, we allow endogenous population growth, a quantity-quality tradeoff, and a role for family planning technologies to affect fertility.

limiting their ability to isolate behavioral 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 interventions. The few growth models jointly featuring endogenous fertility, the quantity-quality tradeoff, and family planning technologies developed by Strulik (2017) and Cavalcanti et al. (2021) do not include multiple sectors, nor a fixed factor of production. As a result, the Malthusian land congestion mechanism that we emphasize is absent in their models.

Finally, our model emphasizes the role of land—a fixed factor in agricultural production—as a countervailing force that limits the growth-enhancing potential of fertility decline.[4] 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 that the land congestion mechanism is stronger than that of the quantity-quality tradeoff.

Furthermore, we contribute to the literature on the child quantity-quality tradeoff by quantifying the net effect of fertility decline and the associated 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.[5] 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.

[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.

[4]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.

[5]Our Bangladesh quasi-experiment features a package of interventions targeting both family planning and child health. The child health interventions improve child quality, as should family planning via the 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 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, but 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.

2.1 Setup

2.1.1 Production

Consider a small open economy that trades agricultural and manufacturing goods with the world economy.[6] 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.[7] As in Caselli and Coleman (2001) and Porzio et al. (2022), per household human capital h_t only yields returns outside of agriculture.[8]

In many developing economies, employment is inefficiently high in agriculture (Gollin

[6] 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.9 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 partial equilibrium Bangladesh context.

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

[8] 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.

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 $w_{mt} \equiv w_t = w_{at}/\xi$.

2.1.2 Households

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

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

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.[9] 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 family planning technologies used. Households may use contraception or abortion to limit their 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

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

[9]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 reducing family planning price p_{ft} on fertility and education.

given child rearing cost ϕ and the price p_{ft} of a unit of the family planning technology. The world price of agriculture is p_{at} and of manufacturing 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:

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

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:

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

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. 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. We consider a reduction of the price of the family planning technology p_{ft}.[10] The

[10] 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

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:

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

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:

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

That is, the direction of the change in labor hours depends on the relative strength of quantity-quality tradeoff. On the one hand, parents have fewer children to raise and therefore less demand on their parenting time. On the other, parents invest more time educating each child. 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. 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 of a family planning and vaccine program on labor supply and find an imprecisely estimated modest effect.

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. The net 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, as seen in equation (4). The net effect of more accessible family planning technology on agricultural employment share depends on the relative strength of the human capital channel versus the labor supply channel.

We show that our predictions are robust to alternative production functions in Appendix

we focus on it for that reason.

A. In Appendix Section A.1, we show our results hold when adding an additional factor of production, imported intermediate inputs.[11] We further show in Appendix Section A.2 that our main results hold if we allow intermediate inputs and labor to be arbitrarily substitutable.

When trade costs are sufficiently high, the economy becomes closed and must rely on local production. Hence, the food problem (Schultz 1953) becomes salient and reverses our baseline model's prediction: a larger population raises demand for agriculture, thus shifting a greater share of workers into that sector. Hence the relative closedness of the agricultural sector in many developing economies (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 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 by icddr,b (formerly known as the International Centre for Diarrhoeal Disease Research, Bangladesh). We estimate the effect of the program, which included family planning and maternal and child health services, on structural transformation. While the study area's small size precludes quantifying general equilibrium forces resulting from changes in fertility, such as changes in wages and prices, the intervention provides rare causal identification and rich, long-running household- and individual-level data to estimate mechanisms.

[11] 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 makes it intractable, as noted by Galor (2005).

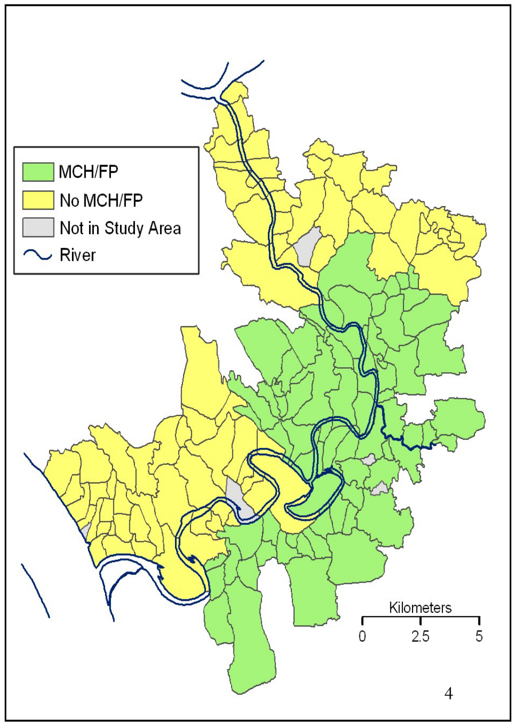
3.1 The Intervention

The MCH-FP program was implemented in Matlab, Bangladesh, in two distinct phases. The first phase, between October 1977 and December 1981, the intervention focused on family planning and maternal health. Locally recruited 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 the treatment area, trained female community health workers provided services directly to households. 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.

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, then rose steadily thereafter. Contraceptive use rose more slowly in the comparison area. The measles vaccination rate rose substantially to 60 percent after it was introduced in the second half of the program; rates for vaccination coverage for diseases targeted by the program increased throughout the program duration. There is no data available on vaccination in the camparison 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.

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.

3.2 Data and Analysis Sample

Data Sources. We draw on the extraordinarily rich data available for the Matlab study area. We focus on 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.[12] 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 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.

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

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 the entire 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. We provide additional details about the Matlab data in Appendix Section C.1.

Intent-to-Treat Assignment. Access to the MCH-FP program was based on the village of residence of the individual/household during the program period. We cannot use the area where the household or individual lived at the time of survey or even when some of the individuals in our individual sample were born because the household'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 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 DSS household head, etc.) present in the 1974 census. For an individual, the ITT variable takes the value of 1 if the 1974 census-linked individual or 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 unit of analysis. We then 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, 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 restrict our sample to individuals born in 1947 or later, and were thus 65 or younger at the start of MHSS2 surveying, and those who were born in 1988 or earlier, to focus on the set of individuals born before or during the MCH-FP program. 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 a migration rate out of the Matlab study area of approximately 60 percent for men, 25 percent of whom migrated internationally.

3.3 Empirical Strategy

3.3.1 Pre-Program Balance and Trends

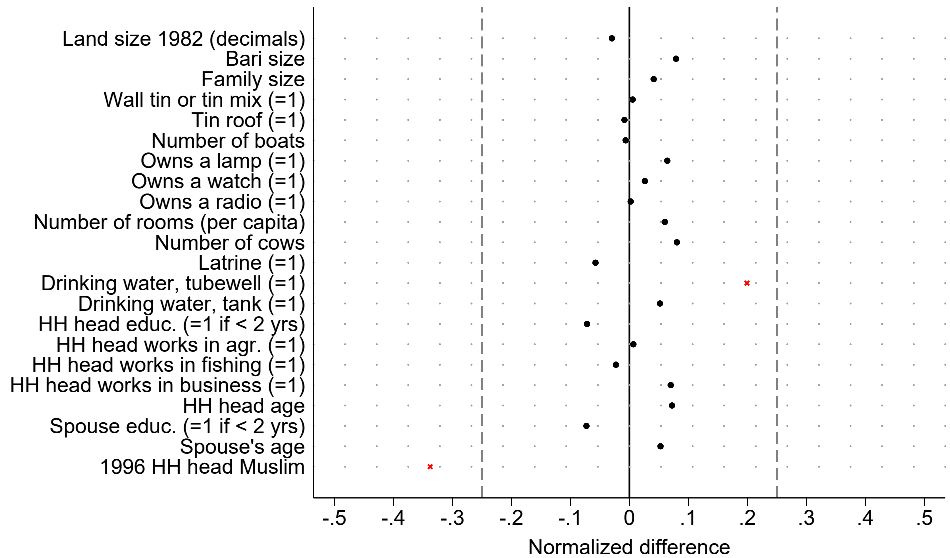
This analysis takes advantage of the treatment and comparison areas that were built into the program and designed to be socially and economically similar and geographically insulated from outside influences (Phillips et al. 1982). Prior studies document balance between treatment and control villages 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.[13] These normalized differences provide an in-

[13] Appendix Table D.1 presents the means for the treatment and comparison group separately and the

dication 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.

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.

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 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 access at 0.20 standard deviations is close to the cut off of 0.25. This difference in access to tubewell water is not a result of household decisions, but rather the rollout of a government program. While ground water may be considered cleaner, there is widespread groundwater arsenic contamination in the tubewells in Bangladesh (Chowdhury et al. 2000) and arsenic is a health concern and has been shown to reduce IQ among school

level differences in means between the two groups.

aged Bangladeshi children (Wasserman et al. 2006). Barham (2012) explores the potential for tubewell access to bias estimates of the program's effect on human capital and does not find any evidence.

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.[14] Prior 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.[15] 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 intent-to-treat (ITT) model. We estimate the household-level specification,

$$Y_h = \omega_0 + \omega_1 T_h + \zeta X_h + \varepsilon_h \tag{7}$$

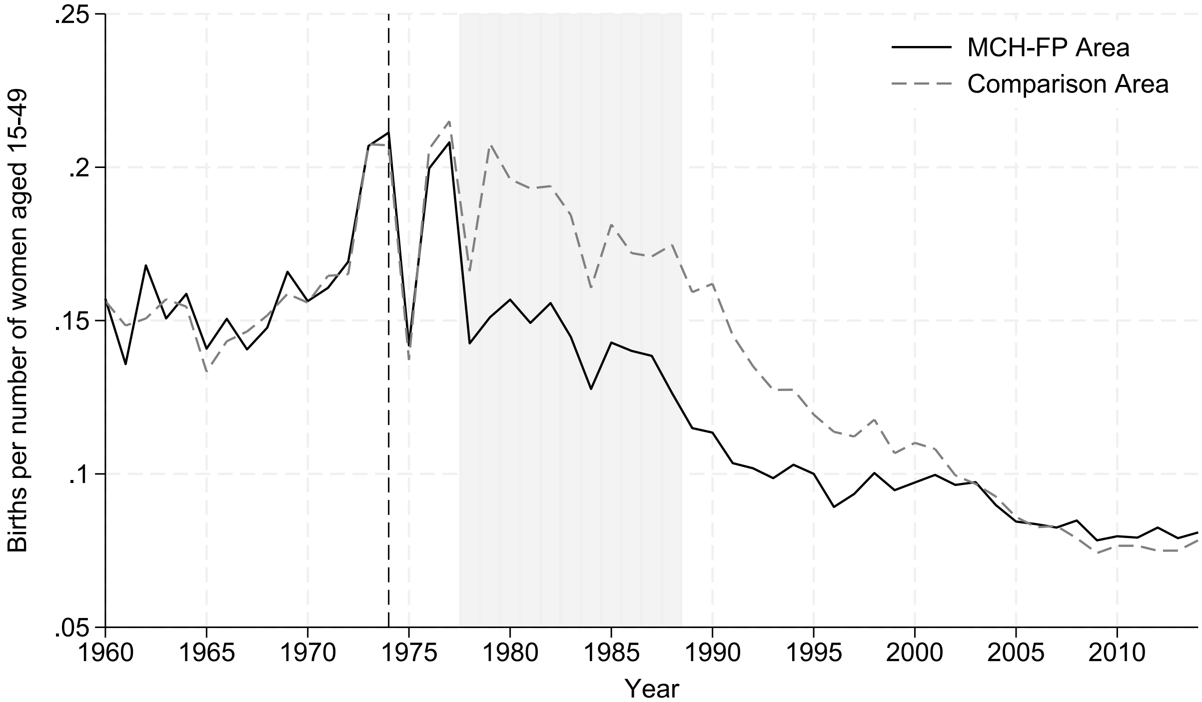
where T_h is an indicator for whether household h is considered treated (as defined in Section 3.2) and X_h is the vector of demographic and baseline characteristics detailed in Figure 2. The coefficient of interest, ω_1, measures the difference in average outcomes between treatment and comparison area 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 1974 village of the household head of h or his antecedents.

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 assump-

[14] 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 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.

[15] 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.

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.

tion, we demonstrated in Section 3.3.1 that the treatment and comparison areas had similar birthrate trends in the nearly two decades leading up to program rollout and document that a wide-range of pre-intervention covariates were balanced between the two areas.

3.4 Effects on Sectoral Employment

We first estimate the effects of the MCH-FP on the share of time spent working in each sector at the household level. Results are shown in Table 1. We separate the estimates into medium-run effects (columns 1 and 2) measured in the 1996 MHSS1 survey, and long-run effects (columns 3 through 6) measured in the 2012–2015 MHSS2 survey.

In the medium-run, we estimate a negligible effect of the program on structural transformation. The dependent variable measured in MHSS1 is the share of months spent per year in each sector.[16] As of 1996, 19 years after the MCH-FP program started, we find no significant effect of the program on sectoral employment (columns 1 and 2). The estimated treatment effect on the agriculture share is 0.7 percentage points (SE=2.1). The effect of the program on non-agricultural employment is similarly small, with an estimated effect of 0.4 p.p. (SE=2.1).

In the long-run, 35 years after the program started, we see significant effects consistent with the Malthusian land congestion mechanism. In MHSS2, the dependent variables are the share of total annual hours worked by household members in each sector (columns 3 though 5),[17] and the average annual number of hours worked by household members (column 6). The MCH-FP raised the share of time household adults spent working in agriculture by 4.1 p.p. (SE=1.4 p.p), representing a 20 percent increase over the comparison area mean (column 3). The share of time household members spent working in manufacturing falls by 3.2 p.p. (SE=1.4), a 16 percent reduction relative to comparison area households (column 4). In services, we find a very small effect of -1.3 p.p. (SE=1.8), a 3 percent reduction relative to comparison households (column 5). Column 6 suggests changing sectoral hours allocations are not driven by changes in total hours worked.

Given the importance of rural-to-urban migration in the development process (Lagakos 2020; Lagakos et al. 2023), we explore its role in shaping our baseline estimates. We re-estimate equation (7) by sector, but further split the dependent variable of work hours share by rural or urban location of employment. We report results in Appendix Table D.2, with the effect on hours worked share in urban areas reported in columns 1–3, and in rural areas

[16] Because we measure time at the monthly level, share spent working in agriculture and non-agriculture may sum to more than one if individuals worked in both sectors in the same month.

[17] Employment hours shares do not sum to 1 for two reasons. First, we do not report results for two small sectors, construction and mining. Second, we code the small set of households who do not work as spending 0 percent of their time working in each sector.

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 (7) 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 (column 6). Employment hours shares do not sum to 1 for two reasons. First, we do not report results for two small sectors, construction and mining. 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 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 columns 4–6. Our main results are driven by treated households engaging more in rural agriculture and less in urban manufacturing relative to comparison households, underlining the importance of rural-to-urban migration in structural transformation in Bangladesh.

Population pressure is determined by the land-to-labor ratio, with land being fixed at the local level. At the household level, by contrast, households may buy or sell land in response to changes in household size. We find no evidence the program changed the amount of acres owned by households (columns 2 and 4 of Appendix Table D.3). Moreover, land transactions outside the family are quite rare (less than 6% of households engage in them annually as of MHSS1) and are typically modest in size.

Given the importance of entrepreneurship for development (McMillan and Woodruff 2003; Buera et al. 2011, 2021), we explore whether the patterns observed in employment are matched by sector-specific entrepreneurship. We report the results in Appendix Table D.4. Columns 1 through 3 show a similar pattern as in Table 1: increased agricultural entrepreneurship, with no change in manufacturing or services entrepreneurship.

As large firms, especially factories, drive structural change and growth (Buera and Kaboski 2012), we also explore how the MCH-FP affected employment across firm types in columns 4–6 of Appendix Table D.4. Employment at factories among treated households lagged behind comparison area households (columns 4 and 5), as did employment at large firms (column 6).

Our results indicate that the MCH-FP program reduced the speed of structural transformation. Interpreted through the lens of Section 2's model, this suggests that the Malthusian land congestion mechanism outweighs the effects of both the quantity-quality tradeoff and any direct human capital improvement resulting from the child health interventions. After discussing robustness of our baseline results, we leverage our rich data to provide evidence for the strength of each mechanism.

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

We first explore whether the MCH-FP's effect persists when looking at the extensive margin of farming. Columns 1 and 3 of Appendix Table D.3 show results consistent with our baseline estimation in Table 1. In the medium run, the program had negligible effects on farming in 1996 (column 1). By contrast, in the long-run the program kept more households in farming relative to control households. By 2014, treatment area households were 4 percentage points more likely to have a household member engaged in farming relative to comparison area households (column 3).[18]

We assess the concern that information spillovers along the border of the treatment and control zones may reduce our estimated effect. That is, recipients of program interventions such as contraception advice and vaccine benefits may pass along information to comparison households. To gauge the importance of such spillovers, we restrict our sample to those living in a village prior to the intervention which has a centroid within 3km of the border. In Panel B of Appendix Table D.5, we show that our results are very similar in magnitude to our baseline estimates when applying this restriction.

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

Finally, we address one other asymmetry between treatment and control areas: the only urban center in the study area, Pourashava, exists in the treatment area. In Panel C of Appendix Table D.5, we show that our results are largely unchanged when we remove households who resided in Pourashava prior to the intervention.

[18]We consider a household to be farming in MHSS2 if at least one subhousehold engages in farming.

3.5 Mechanisms

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

3.5.1 Family Size

We start by testing the prediction of our model from Section 2 that a reduction in the labor force will increase the share of employment in agriculture. We test this prediction by estimating how household size shapes agricultural employment share. The land to labor ratio, T/L_t, in the model's agricultural employment share Equation (4) is the key term that characterizes that Malthusian land congestion mechanism. A necessary condition for the mechanism to work is that land is fixed. This is certainly true at the local level; furthermore, at the household-level, we find the program induces no changes in land holdings as seen in Appendix Table D.3.

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.6. Consistent with earlier research and Figure 3, we find the program reduced household size. In particular, the program reduced the number of males per household aged 24 to 34 by 16 percent, and decreased the number of females per household in the same age range by 11 percent.[19]

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 those household's subsequent sectoral employment choices. We focus on males because of their stronger labor market attachment. In particular, we estimate an equation of the form

$$Y_h = \alpha_0 + \alpha_1 Num.\ males\ age\ 24\ to\ 34_h + \gamma X_h + \epsilon_h \tag{8}$$

where Y_h is either the share of household work hours by sector or the number of hours by

[19]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 two small sectors, construction and mining. 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 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.

sector. Because the program directly affects the number of males born during the experimental period, we instrument for $Num.\ males\ age\ 24\ to\ 34_h$ using the treatment dummy T_h.

We present our results in Table 2, with estimates for the share of hours by sector appearing in Panel A and for the number of hours by sector in Panel B.

Starting with Panel A, we find that larger households spend a smaller share of time working in agriculture (column 1). One more male born during the program period reduces the share of household work time spent in agriculture by nearly 40 percentage points. Conversely, larger households spend a higher fraction of time working in manufacturing (column

2) and services (column 3), though the effect is less precisely estimated for services.

In Panel B we explore the effect on the household number of hours worked by sector. Doing so allows us to quantify how much of the decrease in available son labor corresponds to a change in the level of agricultural labor relative to the level of non-agricultural labor. We find a negative but imprecisely estimated effect on agricultural hours worked due to having an additional son. By contrast, we observe a substantial increase in hours worked in both manufacturing and services. Our results are therefore consistent with a story in which the marginal son is sent to work in the non-agricultural sector. Overall, Table 2 indicates that fertility changes induce a stronger relative effect than a level effect on agricultural employment. That is, the employment level in agriculture appears to be in a fixed proportion with the amount of land available.

3.5.2 Human Capital

When returns to human capital are higher outside agriculture, an increase in human capital draws workers out of agriculture. We test this hypothesis by leveraging the rollout of the child health arm of the MCH-FP and cross-cohort variation in exposure.

First, we confirm in our context that returns to human capital are in fact higher outside agriculture. We show this by estimating a Mincer equation relating the log wage to education and potential experience by sector. Appendix Table D.10 displays the results. Consistent with Caselli and Coleman (2001) and Porzio et al. (2022), returns to education are lower in agriculture relative to non-agriculture, and highest in the service sector.

Past research on the effects of the MCH-FP by Joshi and Schultz (2007), Barham (2012), and Barham et al. (2021) have found pronounced effects on human capital for the cohorts born between 1982 and 1988 and negligible effects for those born between 1977 and 1981. Effects were strongest among men. We estimate the effect of the MCH-FP on years of education as of the MHSS2 within our household sample, with results shown in Appendix Table D.11. The average adult across all cohorts experienced a negligible effect of the program. In light of the program's focus on vaccinating only young children, this is unsurprising. As expected, and consistent with earlier research, we find that adults born during the vaccine period of the MCH-FP obtained 5.7% more years of education than the average comparison household member born during the same period. The effect is stronger for men than for women. In what follows, we therefore take as given that cohorts born into the vaccine arm of the MCH-FP (between 1982 and 1988) experience a significant human capital boost relative to other cohorts.

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

$$Y_i = \alpha_{y(i)} + \gamma_1(T_i \times Born_i^{77-81}) + \gamma_2(T_i \times Born_i^{82-88}) + \gamma_3(T_i \times Born_i^{Pre-77}) + \nu X_i + \epsilon_i \quad (9)$$

where T_i is an indicator for whether i is treated as defined in Section 3.2; $\alpha_{y(i)}$ is a set of indicator variables for i's birth year; and X_i is the vector of pre-intervention demographic and baseline characteristics detailed in Figure 2.[20] The outcomes Y_i we consider 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 the relevant age group. γ_1 captures the effects of the family planning and maternal health interventions combined with any spillovers of having younger siblings exposed to the intensive child health interventions, and γ_2 is the combined effect of all program interventions, including the childhood vaccination programs. γ_3 captures any indirect spillover effects of the program on older generations.

Table 3 reports results at the individual level among men.[21] We find that, consistent with our household-level estimates, treated individuals increase the share of hours worked in agriculture (column 1) and reduce it in manufacturing (column 2).

There is, however, considerable heterogeneity in program effects across cohorts. To interpret these differences across cohorts, recall that the 1977–81 cohort in the treatment area only directly experienced the effects of smaller family sizes via the contraception arm of the MCH-FP. By contrast, the cohorts born between 1982 and 1988 experienced both smaller family sizes and improved early-life health from vaccinations, which translated into higher later-life human capital (Barham 2012; Barham et al. 2021).

Men born during the human-capital enhancing phase of the program, between 1982 and 1988, worked more in the service sector and less in manufacturing (first row of coefficients). While the coefficient on services share is imprecisely estimated, coefficients across each row (including the two sectors we exclude from the table: construction and mining) must sum to 0. Cohorts born before 1982 modestly reduce their hours share in services (column 3). Cohorts of men born before 1982 increased their agricultural hours share. Our results can be

[20] 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 $\alpha_{y(i)}$.

[21] We show results for women in Appendix Table D.7. The program did not affect hours worked for women. Vaccine-treated women work more in agriculture and less in non-market activities. Given women's much lower labor supply, we focus on men in our analysis.

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

	Share hours by sector			
	(1)	(2)	(3)	(4)
	Agriculture	Manufacturing	Services	Hours worked
Treatment × Born 1982–1988	0.01	-0.08**	0.05	-24.20
	(0.02)	(0.03)	(0.04)	(87.49)
Treatment × Born 1977–1981	0.05*	-0.06**	-0.01	-64.32
	(0.02)	(0.03)	(0.04)	(85.68)
Treatment × Born Pre-1977	0.04*	-0.00	-0.04	-145.84**
	(0.02)	(0.01)	(0.02)	(61.92)
% 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 as well as erosion exposure. The dependent variable in columns (1) through (3) is the fraction of total hours worked by sector. See Appendix C.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 two small sectors, construction and mining. 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.

understood to the extent that the returns to human capital are higher in the service sector than in agriculture or manufacturing (Appendix Table D.10), and that families optimally allocated sons to sectors based on their human capital.

3.5.3 Agricultural Adjustment

We next examine household-level effects of the program on agriculture. Since treated households are smaller, less family labor is available for use on the farm. Farming households may therefore switch into growing less labor-intensive crops. We estimate the effect of the MCH-FP on crop choice and show the results in Figure Table D.2. The program induced a shift towards crops which produce more revenue per unit of labor input. Roughly put, farmers shifted away from rice and into potatoes.

We also assess whether observable measures of farm productivity change as a result of the program. With the human capital rising due to the child health component of the MCH-FP, and child health-treated women working more in agriculture (Appendix Table D.7), farmers may raise their per-acre farm productivity if they increasingly use more complex inputs.

Our proxy for per acre productivity is revenue and profit per acre. To compute the value of output, we first need data on crop prices. Lacking farmgate prices for each household in the MHSS2 data, we instead draw upon the Bangladesh statistical yearbooks for 2012 through 2014. These yearbooks list prices at the variety level (e.g., coarse paddy boro or fine paddy boro), not the crop level (e.g., paddy boro). Hence we take prices in two ways: either the minimum price within crop across varieties, or the maximum.

Appendix Table D.8 shows no evidence of the program raising farm productivity per acre. We estimate the effect of the MCH-FP for the subset of households which grow crops. In columns 1 and 2 we look at the effect on potential revenue per acre, while we estimate the effect on profits per acre in columns 3 and 4. Across all outcomes, we can not statistically rule out a null effect. This result is consistent with our individual-level estimates in Table 3 which shows that the men whose human capital was improved most by the program (i.e., were born during the vaccine arm of the MCH-FP) left agriculture to work in services.

While the MCH-FP Matlab quasi-experiment sheds light on the partial equilibrium effects of fertility changes and vaccine accessibility, it cannot on its own be informative about the general equilibrium effects. In particular, wages, prices, and technology may change as a result of large, widespread changes in fertility and human capital (Acemoglu 2010). To understand the general equilibrium effect of fertility changes, taking into account general equilibrium forces, we first leverage cross-country and cross-state variation in the next section, and then do some rough back-of-the-envelope calculations leveraging the model from Section 2.

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 over the last 60 years. 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 agriculture 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).[22] We collapse specific policy changes into an index that varies between 1 and 5 as in Elías et al. (2017). A value of 1 indicates that there is no law regulating abortion; an index value of 2 indicates that abortion is prohibited unless it would save the mother's life; a value of 3 that abortion is only allowed to protect the mother's physical or mental health; a value of 4 that additionally abortion is allowed if there are fetal abnormalities and in the case of rape or incest; and a value of 5 indicates that abortion is freely permitted. Hence, a higher value of the index indicates that abortion is more accessible. We find similar results when constructing the abortion policy variable as a dummy equal to 1 when the index equals 5 and 0 otherwise (see Appendix Figure D.4). The median value of the index is 3, indicating that the typical country only allows abortion to protect the mother's health. The standard deviation of the index is 1.3, close to the one point index change used to interpret our event study coefficients. 56 countries make at least one abortion policy change during the sample period; 6 countries experienced two abortion policy changes, with no countries experiencing more than two changes.

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

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

quantity and into child quality (Galor 2005). We therefore need an exogenous shifter of fertility rates which is uncorrelated with factors shaping the agricultural employment share, conditional on controls.

We leverage variation in country policy changes to abortion access. Specifically, we estimate an event study of the effect of abortion policies on the agricultural employment share. Our specification is

$$AES_{ct} = \alpha_c + \alpha_t + \sum_{\tau=T_0}^{T} \beta_\tau Abortion_{ct} + \epsilon_{ct} \tag{10}$$

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 and α_t a vector of year fixed effects. Given the continuous nature of the treatment—since multiple abortion policies may change at once, and abortion may become more or less accessible—we estimate equation (10) following De Chaisemartin and d'Haultfoeuille (2024). As robustness, we estimate the effect of abortion using a binary indicator when that equals one when the index is 5 and zero otherwise, with the results shown in Appendix Figure D.4.

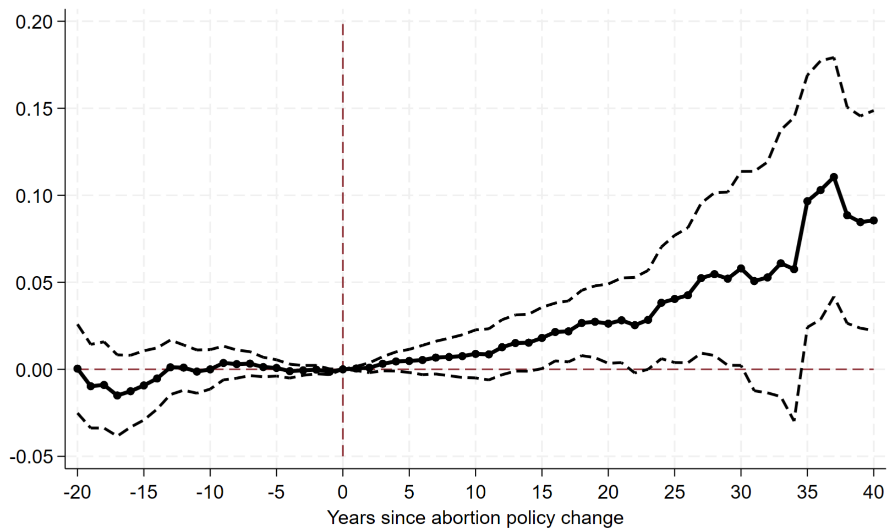
4.1.3 Cross-Country Results

We show the results of estimating equation (10) in Figure 4, which plots the β_τ coefficients. We do not find evidence of pretrends, indicating that abortion policy changes do not correlate to pre-policy changes in the agriculture employment share. The effect of abortion policy changes on 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, nearly a one standard deviation change in the abortion policy index, is a 5 percentage point increase in agricultural employment share 15 to 40 years later. Relative to a mean share of 0.37, this represents a 14 percent drop.[23]

We also estimate the effect of abortion policy on the birth rate using equation (10). Appendix Figure D.3 shows the result. We do not find strong evidence of pretrends, with 8 of 10 pre-policy change coefficients 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

[23] 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).

becoming more accessible) reduces the birth rate by 0.33 children per 1,000 population. Relative to a mean birth rate of 30, this implies a 1.3% reduction. This magnitude is very close to the 1.1 percent decline estimated by Bloom et al. (2009), whose sample differs slightly 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.

There are two main drawbacks to our cross-country analysis. First, data may not be directly comparable across countries, and may require various assumptions and imputations to harmonize (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, 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.[24]

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 1900. These data are drawn from decennial census tabulations computed by the U.S. Census as well as estimates based on the Census microdata for the 1870 to 1890 waves. Imputations were necessary, especially in earlier census periods.[25] 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.[26]

4.2.2 Cross-State Empirical Specification

As surgical abortions became more prevalent in the U.S. in the 1800s, a backlash followed, driving widespread implementation of abortion restrictions across the country. Lahey (2014) finds that the passage of these laws was not correlated with the immigrant population share, literacy rate, pre-law child-to-woman ratio, and, importantly for the present study, the urbanization rate. Lahey (2014) leverages the staggered rollout of these laws across U.S. states to estimate that abortion restrictions increased fertility by about 10 percent.[27]

We estimate the causal effect of abortion restrictions on agricultural employment share over time. Specifically, we estimate the staggered dynamic difference-in-differences following

[24] 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.

[25] 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.

[26] 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).

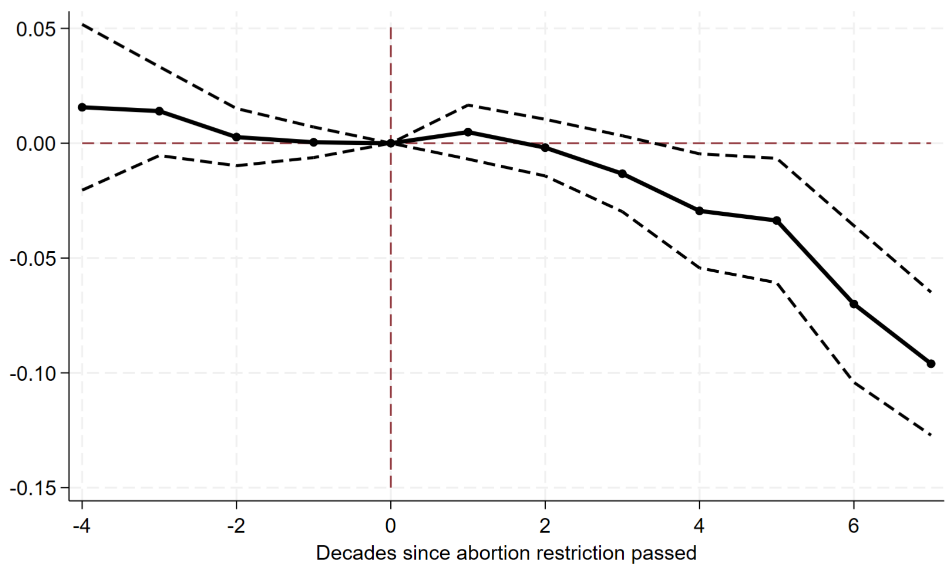
[27] Measures of abortion use across states and over time do not exist for 19th century America, unfortunately. Still, the responsiveness of fertility that Lahey (2014) finds is similar to magnitudes in other studies. For example, Fischer et al. (2018) estimate that policies reducing funding to family 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.

De Chaisemartin and d'Haultfoeuille (2020), similar to equation (10). Each abortion policy's passage is associated to the subsequent decennial census wave.

4.2.3 Cross-State Results

Figure 5 shows the resulting event study plot of our estimates. There are no differential trends in agricultural employment share prior to the implementation of abortion restrictions. After restrictions are in place, a negative effect on agricultural employment share begins to emerge, becoming statistically significantly negative four decades later. The delayed effect is consistent with the fact that affected cohorts must age into the labor market, and mirrors our findings in 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 almost 5 percentage points four decades after abortion was restricted, a 27% reduction. If the average abortion policy reduced fertility by 10% as suggested by Lahey (2014), then the resulting long-run fertility-agricultural employment share elasticity is 2.7.

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 when 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. 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. As in our baseline analysis, we observe no pretrend and a significantly negative effect of abortion restrictions three plus decades after passage. Because we only observe 20 states as of 1800, we bootstrap the clustered standard errors.

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. As discussed in Section 2.3, we do not expect this force to be quantitatively important. Our results 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? We take a first step towards answering these questions using our stylized framework and making back-of-the-envelope calculations.

Take equation (4) and substitute equation (3) for L_t. 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 we have that changes in the agricultural employment share can be decomposed as follows:

$$\widehat{L_a/L} = -\frac{\hat{h}}{1-\theta} - \widehat{n_{-1}} - \hat{\ell}.$$

where we removed t subscripts for clarity. We consider three scenarios.

First, we compute the model's predicted agricultural employment share change in Matlab in the medium run, in which human capital is unaffected: $\widehat{L_a/L} = -\widehat{n_{-1}} - \hat{\ell}$. As shown in Appendix Table D.11, the program induced no change in average years of education across all adults 35 years later, as the treated cohorts are only a small fraction of the total population.

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

Second, we consider the long-run effect of the program, i.e., allowing population average human capital to change. As the fraction of the population treated by vaccines approaches 1, we would expect years of education to rise in line with Barham (2012), Barham et al. (2021), and Appendix Table D.11. We therefore consider a $\hat{h}$ equal to 0.057, the percent change in years of education induced by the program for vaccine recipients (column 4 of Appendix Table D.11). We also must 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 an 1.8% increase, as wages in nonagriculture rise with higher human capital. Hence our results suggest that in the long-run, the combined effect on agriculture employment share of fertility and early-childhood vaccines will dissipate. Still, a transitional period will occur in the meantime in which the Malthusian population size effect dominates the human capital effects.

Finally, 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 highest income countries' value added share of land in agriculture 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.

6 Conclusion

Fertility decline is an essential process by which countries escape the "Malthusian trap" of excess population growth, economic stagnation and poverty (Galor 2012). Yet technological progress raising the returns to human capital, and thus the investments in children by parents, is necessary to achieve sustained growth. In this paper, we show that fertility decline without sufficient accompanying technological advancement slows down structural transformation out of agriculture. We demonstrate this empirical relationship in varying contexts, using distinct sources of exogenous variation and levels of aggregation.

Our findings do not suggest that developing countries should avoid investing in family planning policies. Instead the modest effects of fertility decline on slowed structural transformation can be offset by investment in human capital. Policymakers should therefore take care to pair family planning programs with education and public health investments that raise human capital.

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Appendix

A Theoretical Appendix

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

A.1 Adding Intermediate Inputs

Assume the production function in agriculture is

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

and in manufacturing, it is

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

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

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

and the agricultural employment share is

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

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 and 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 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:

$$Q_a = A_a \left[\omega Z_a^{\frac{\epsilon-1}{\epsilon}} + (1-\omega) L_a^{\frac{\epsilon-1}{\epsilon}} \right]^{\frac{\theta\epsilon}{\epsilon-1}} T_a^{1-\theta} \tag{A.2}$$

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

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

where $[\cdot]$ is the CES portion of equation (A.2). 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

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

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

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,

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

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

The equilibrium wage plus land market clearing ($T_a = T$, 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 factory sector can be obtained using the labor market clearing constraint, $L = L_a + L_m$.

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

$$\frac{Z_a^*}{L} = \left(\frac{\omega}{1-\omega}\frac{w^*}{p_z}\right)^{\epsilon} \frac{L_a^*}{L}. \tag{A.5}$$

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

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

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

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

Inequality (A.6) 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 population size effects.

For the most developed countries, on the other hand, the model suggests that both forces shift labor into the agricultural sector. This is because human capital increases essentially free-up labor to move into agriculture one labor is sufficiently productive.[28]

A.3 Partially Closed Economy

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

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

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

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

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

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

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

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

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 tables 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 MHSSA 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. All work reported in the AE module was considered 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, etc.) 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 occupation. 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 drive (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 (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.[29]

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

[29] 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.

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 population ratio over time. The denominator, the total population, is readily available from the U.S. Census.[30]

The numerator, the agricultural workforce, is trickier to compute and requires some assumptions and imputations. Craig and Weiss (1998) focus on rural agricultural employment;[31] 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 imput an industry for "non classifiable" workers.[32] As a robustness check, we show very similar results to our baseline in Figure D.5 when using the 1850 to 1900 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. Craig and Weiss (1998) directly observe state-level male agricultural employment for those 16 and older in 1850 and 1860. They 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

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

[31] This is comparable to the use of urbanization rates as a proxy for nonagricultural employment shares by Wingender (2014b).

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

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 using the following procedure. First, he identifies counties within the same census 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 (1974) 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

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

Notes: The table presents estimates of equation 7 for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. The dependent variable is the share of hours worked within the household in different sectors and in different locations. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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

	MHSS1 (1996)		MHSS2 (2012-2014)	
	(1) =1 if household farms	(2) Acres owned	(3) =1 if household farms	(4) Acres owned
Treatment	0.033	-0.044	0.040**	0.017
	(0.028)	(0.108)	(0.017)	(0.097)
% chg. rel. to mean	4.9	-2.7	5.0	1.3
Mean	0.68	1.61	0.80	1.33
Baseline controls	Y	Y	Y	Y
Observations	2525	2525	2484	2482

Notes: The table presents estimates of equation (7) for outcomes aggregated to the MHSS1 household-level and measured in 1996 (columns 1 and 2) and 2014 (columns 3 and 4). Variable means refer to the comparison area. Standard errors are clustered by the 1996 household head's pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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. *, **, 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: 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 7 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 (5) measure outcomes in the 2012–2015 MHSS2. MHSS1 ourcomes 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 excludes households whose pre-program village is within the Matlab town boundary. See Appendix C.1 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table D.6: 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.7: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector: Individual-Level, Women

	Share hours by sector				
	(1) Agriculture	(2) Manufacturing	(3) Services	(4) Non-Market	(5) Hours worked
Treatment × Born 1982–1988	0.05***	0.01	-0.01	-0.05*	46.22
	(0.02)	(0.02)	(0.02)	(0.03)	(58.65)
Treatment × Born 1977–1981	-0.03	-0.02	0.03	0.01	-90.23
	(0.03)	(0.02)	(0.02)	(0.04)	(83.72)
Treatment × Born Pre-1977	-0.00	-0.00	0.01	-0.00	-4.31
	(0.02)	(0.01)	(0.01)	(0.02)	(29.08)
% 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. All variables control for the baseline controls listed in Table D.1 as well as erosion exposure. The dependent variable in columns (1) through (3) is the fraction of total hours worked by sector. See Appendix C.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 two small sectors, construction and mining. 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.8: ITT Effects of MCH-FP on Revenue and Profits per Acre

	Revenue per acre		Profit per acre	
	(1) Min. Price	(2) Max. Price	(3) Min. Price	(4) 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 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.9: 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 within MHSS2 households is summed within the MHSS1 household. 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.10: 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

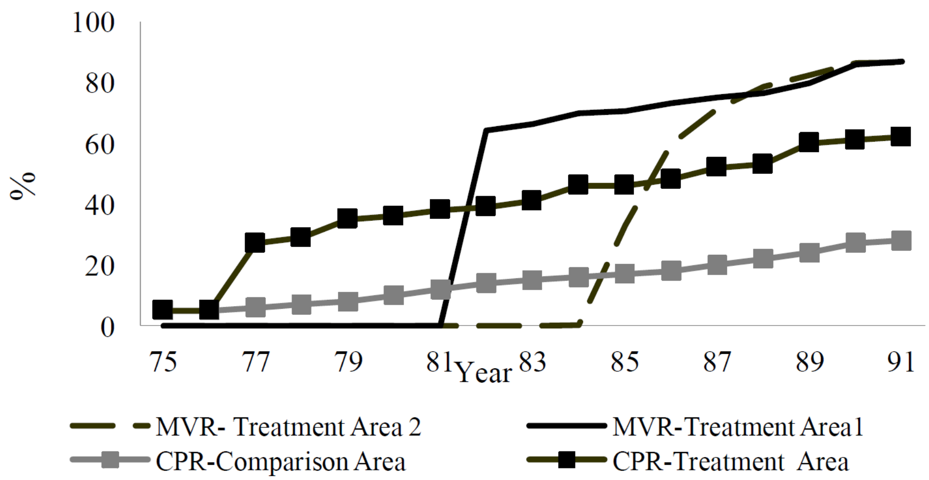
Notes: The table presents estimates from a Mincer wage regression by sector. The dependent variable—log hourly wage—is calculated based on earnings and hours worked in a sector. The sample includes MHSS2 respondents who are MHSS1 household members or their descendants, and is restricted to men born between 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.11: ITT Effects of MCH-FP on Years of Education

	All Adults			Adults Born 1982–1988		
	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Men	Women	Pooled	Men	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

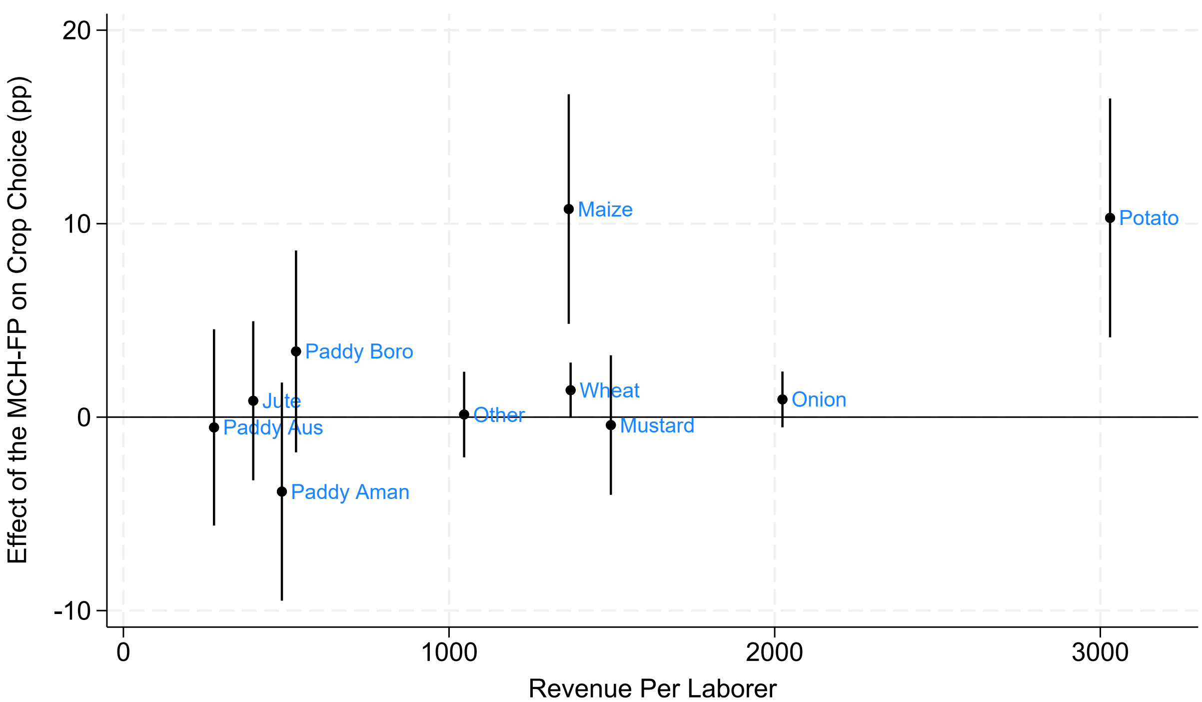
Notes: The table presents estimates of the effect of the MCH-FP on 2014 outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by pre-program village. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

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



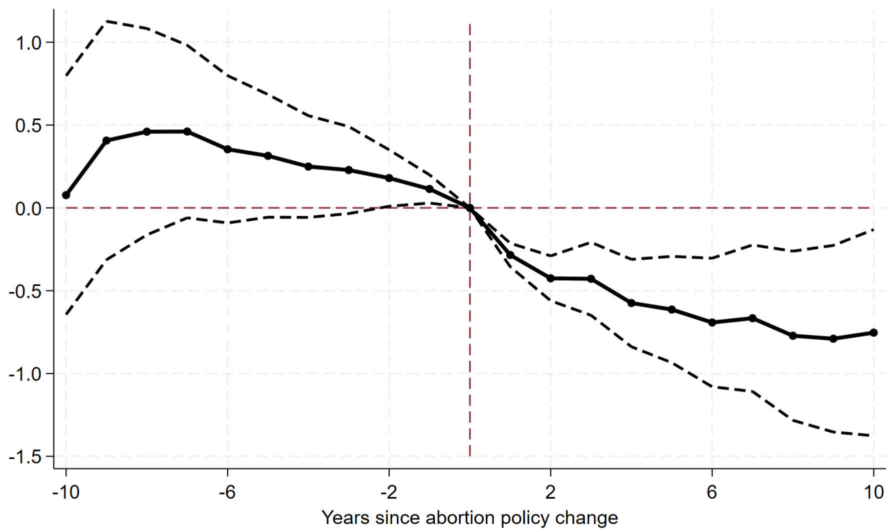
Source: Replicated from Figure 2 in Barham et al. (2023).

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



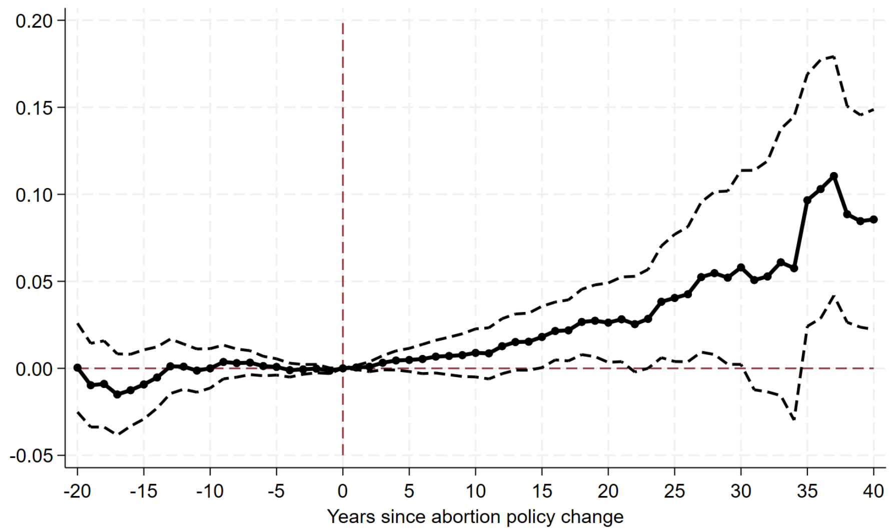
Notes: The figure reports estimates of equation 7. The vertical axis reports the ITT effect on whether the household grew the given crop. The horizontal axis reports the average revenue per unit of labor when producing the crop, which comes from XXX. Vertical bars represent the 95% confidence intervals.

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



Notes: The figure shows event study coefficient estimates for the effect of abortion policy changes on the crude birth rate. 95% confidence intervals depicted with standard errors clustered at the country level. Annual data on crude birth rate 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).

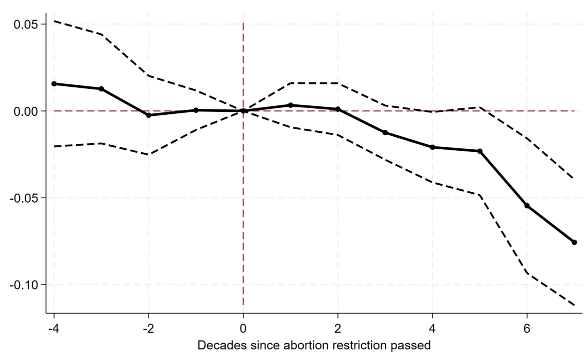
Figure D.4: Effect of Abortion Policy Changes on Agricultural Employment Share Using Indicator of Free Abortion



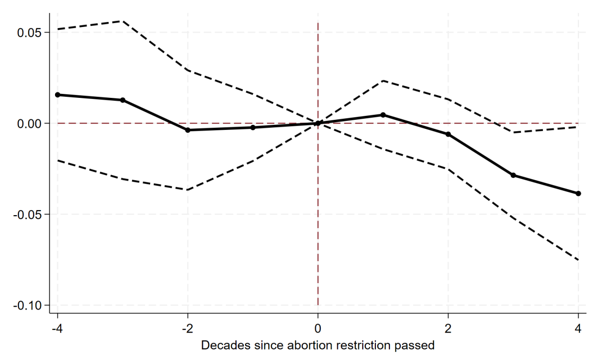
Notes: The figure shows event study coefficient estimates for the effect of abortion policy changes on the agricultural employment share, using an binary indicator of abortion policy which is 1 when the abortion index if 5 and zero otherwise. 95% confidence intervals depicted with standard errors clustered at the country level. Data on country-level agricultural employment shares 1960–2020 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, Full Count Census 1850–1900

(a) all abortion policies

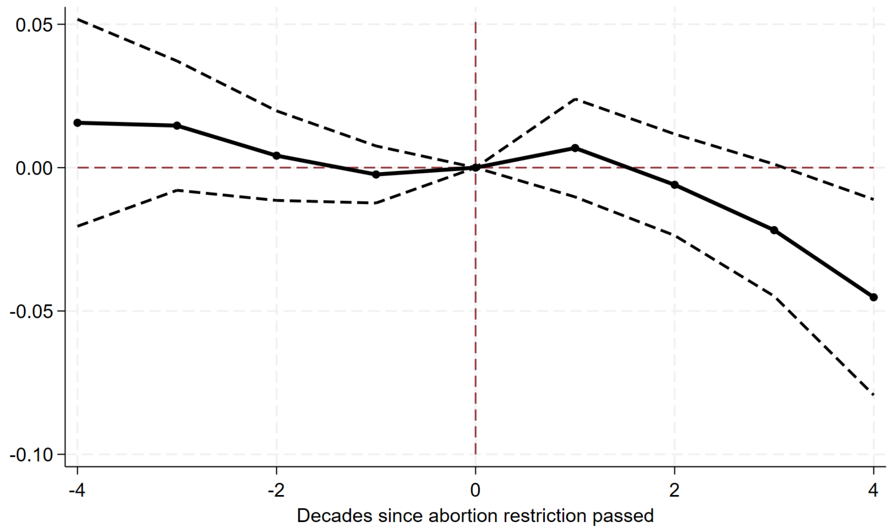


(b) post-1840 abortion policies



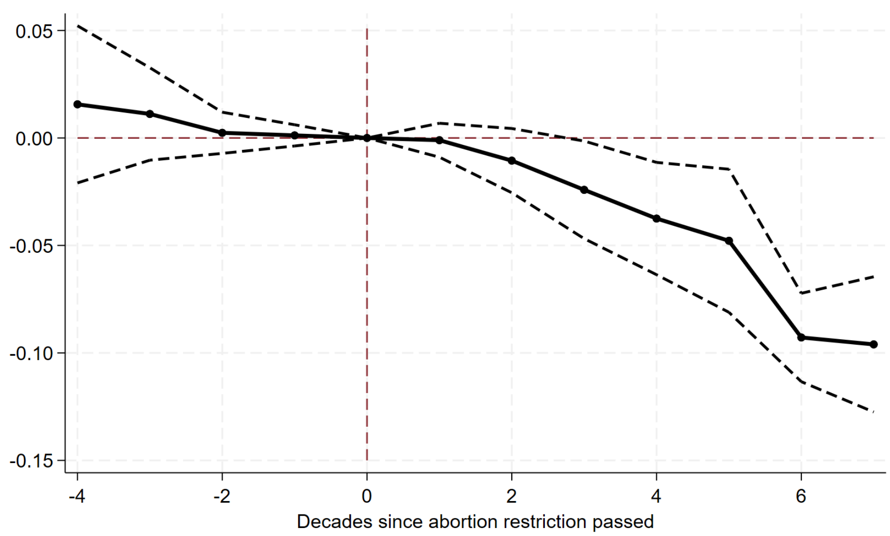
Notes: Data on state-level agricultural employment shares 1800-1840 comes from Craig and Weiss (1998). Agricultural employment shares for 1850–1900 computed from Ruggles et al. (2024). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). 95% confidence intervals depicted 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 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). 95% confidence intervals depicted 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 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restriction laws come from Lahey (2014) and Lahey and Wanamaker (2025). 95% confidence intervals depicted 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).