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 national, subnational, and household-levels. Abortion policy changes around the world in the last 60 years and across U.S. states in the 19th century, and a quasi-experimental family planning program provided to households, generate plausibly exogenous variation in fertility. In each of these three empirical analyses, lower fertility raises the agricultural employment share. Improving human capital, however, can 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, in which fertility and mortality fall, and structural transformation, in which workers leave agriculture for manufacturing and service jobs. A large literature examines how growth and structural transformation drive demographic transition (Galor and Weil, 1996, 2000; Chatterjee and Vogl, 2018; Ager et al., 2020). Little is known, however, about how demographic transition drives structural transformation. Given farmers' reliance on a fixed factor of production, land, a decline in population could result in more workers staying in agriculture (Malthus, 1798; Lewis, 1954).

Unified models of the growth process typically hold, however, that endogenous technological change will swamp the Malthusian force keeping workers in agriculture (Boserup, 1965; Galor and Weil, 2000). But if the pace of technological advancement is slowing (Bloom et al., 2020) or frictions restrict some countries' access to the technological frontier (Gancia and Zilibotti, 2009; Buera and Oberfield, 2020), Malthusian forces may once again become salient. With fertility declining in virtually every country on earth today (Delventhal et al., 2021) and world population expected to peak within the next 60 years (Nations, 2024), understanding the impact of fertility decline on structural transformation is crucial.

Lower fertility may affect human capital, and therefore structural transformation, via the quality-quantity tradeoff (Barro and Becker, 1989). If nonagriculture more intensively uses human capital, then an increase in human capital will raise nonagricultural employment. The net effect of a fertility reduction on structural transformation—depending on both the Malthusian force of land in agriculture and the quality-quantity tradeoff—is therefore ambiguous. We formalize this logic in a simple two sector model in Section 2.

Testing models relating fertility and structural transformation is challenging for three reasons. First, fertility changes endogenously with a region's economic growth and structural transformation. Second, there may be a substantial lag between changes in fertility and resulting impacts, as cohorts must grow up before entering the labor market. This requires consistent data for a lengthy period of time. Third, understanding the mechanisms by which fertility affects structural transformation is difficult when using regionally aggregated data.

We test whether fertility drives subsequent structural transformation in three distinct empirical contexts. In each approach, we find that falling fertility slows down structural transformation. Hence, the population size effect dominates the quantity-quality induced human capital improvements. Our results imply that governments seeking to transform their economy away from agriculture should pair family planning programs with investments in human capital.

We begin our empirical analysis by leveraging cross-country variation since 1960 in Section 3. We estimate an event study relating changes in abortion policies to the agricultural employment share. A policy which makes abortion more accessible, and thus reduces fertility rates, increases agricultural employment share by about 5 percentage points three decades later.

The cross-country analysis has the advantage of estimating the relationship of interest: how fertility changes affect an economy's agricultural employment share in the face of general equilibrium effects, such as changes in wages in prices. There are two key drawbacks, however. First, cross-country regressions are inherently challenging to cleanly estimate Durlauf et al. (2005), for example because harmonizing data across countries is exceedingly difficult. Second, the use of aggregate data precludes analysis of mechanisms at the level of decision makers: households and individuals. We therefore turn to two additional analyses that address these shortcomings.

In our second empirical analysis, we estimate the long-run effect of abortion restrictions passed by U.S. states in the 19th century in Section 4. Event study estimates reveal that abortion restrictions accelerate structural transformation in subsequent decades. A policy which makes abortion *less* accessible, and thus increases fertility rates, decreases agricultural employment share by about 5 percentage points three decades later.

In our third empirical analysis, we estimate the long-run impact of a quasi-random intervention in Bangladesh that distributed modern contraception and childhood vaccines 50 years ago in Section 5. The intervention accelerated the demographic transition by first inducing a fall in birth rates inside the treatment area during the program period (Joshi and Schultz, 2007). Several years into the program vaccines were rolled out, reducing early-childhood death rates and raising the cognitive abilities and education of treated cohorts (Barham, 2012; Barham et al., 2021b). Treatment was assigned by village, with treatment and control villages well balanced across a wide range of pre-intervention characteristics. We leverage highly detailed microdata collected across four decades in rural Bangladesh to understand the long-run effect on structural transformation and the corresponding mechanisms.

We find that the faster demographic transition induced by the program slowed down the movement of workers out of agriculture decades later. Treated households allocated a 19 percent higher share of work hours to agriculture, but 11 percent less to manufacturing.

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

Second, households on average sent higher human capital sons to work outside of agriculture. We obtain quasi-exogenous variation in human capital by comparing those born during the intensive child health phase of the intervention to those born before it. Vaccines raised affected cohorts' human capital (Barham, 2012; Barham et al., 2021b). Treatment area men born during the intensive child health phase of the program worked more in the service sector where human capital returns are likely higher.

This paper contributes to a growing literature on the consequences of fertility decline for economic growth (Ashraf et al., 2013; Cavalcanti et al., 2021; Jones, 2022; Hopenhayn et al., 2022).[1] Unified growth models emphasize that declining fertility raises per capita income growth by freeing up resources to invest more in human capital (the quality-quantity tradeoff) and raising the ratios of labor-to-capital and labor-to-land (Galor and Weil, 2000; Galor, 2005). Relative to previous work, we emphasize the role of the fixed factor of land in agriculture as a countervailing force against the growth-enhancing effects of fertility decline. We provide direct evidence that the Malthusian force of land in agriculture outweighs the offsetting effect of the quality-quantity tradeoff in keeping workers in agriculture as fertility falls.

We are the first to empirically establish a causal link leading from the demographic transition to structural transformation, two central features of economic development (Kuznets, 1957). Many studies focus on how structural transformation and productivity growth lead to demographic transition (Greenwood and Seshadri, 2002; Wanamaker, 2012; Ager et al., 2020). A notable exception which explores how population growth shapes structural transformation is Leukhina and Turnovsky (2016). They, however, rely on calibrated macroeconomic models and aggregate time series data, making causal identification and the parsing of different mechanisms challenging.[2]

We contribute to the literature on the child quality-quantity 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. 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 mechanism that we focus on in this paper.

[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] Fertility and agricultural employment share may commove due to changes in skill-biased technical change, which affect the returns to child quality investments (relative to quantity) and the returns to employment in agriculture.

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.[3] In total there are T units of land, which is only used in agriculture.

Production of agricultural output is Cobb-Douglas:

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

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_a 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.[4] As in Caselli and Coleman (2001) and Porzio et al. (2022), per household human capital h_t only yields returns outside of agriculture.[5]

[3]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.4. We also show in Table D.2 that the quasi-experimental intervention in Bangladesh that we study in Section 5 did not induce any changes in consumption shares across sector, suggesting that demand-side factors are not driving sectoral reallocations in our Bangladesh context.

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

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

2.1.2 Households

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

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

where h_{t+1} is human capital per child in the following period, s_t is the amount of sex had by the household, σ is the desire for sex, n_t is the number of births per household, c_t^a is consumption of the agricultural good, and c_t^m is consumption of the manufacturing good per household.[6] 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

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

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

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

[6] 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) in his appendix shows that Stone-Geary preference for consumption would not change the effect of reducing family planning price p_{ft} on fertility and education.

2.2 Equilibrium

Labor markets clear so

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

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

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

The equilibrium agricultural employment share is

$$\frac{L_{at}}{L_t} = \left(\frac{\theta p_{at} A_{at}}{p_{mt} A_{mt} h_t} \right)^{\frac{1}{1-\theta}} \frac{T}{L_t}. \tag{3}$$

Assuming an interior solution, each household's optimal choice of fertility and child education are as follows:

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

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

2.3 Effects of Abortion and Contraception Access

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}. The price includes both monetary and non-monetary costs associated with accessing the family planning technology. 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 human capital h_t and current adult population n_{t-1} are unchanged as a result of the program. 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 quality-quantity 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 suggest that the magnitude may not be very large.

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 ℓ_t may change, the land-labor ratio rises, increasing the returns to labor in agriculture, as seen in equation (3). 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 A. In Appendix Section A.1, we show our results hold when adding an additional factor of production, imported intermediate inputs.[7] Our main results also hold when adding a nontradable service sector, as we show in Appendix Section A.3.

We discuss when our results hold if trade is costly in Appendix Section A.4. If trade costs are sufficiently high, the economy becomes closed and must rely on local production. Hence, the food problem (Schultz, 1953) becomes salient and reverses our baseline model's prediction: a larger population raises demand for agriculture, thus shifting a greater share of workers into that sector. Hence the relative closedness of the agricultural sector in many developing economies (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. Tombe (2015), however, 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; Farrokhi and Pellegrina, 2023; Gollin et al., 2025). Moreover, we show in our subsequent empirical analyses that the demographic transition slows down the movement of workers out of agriculture, implying that agricultural sectors are on average sufficiently open

[7]One can instead think of this additional factor as capital when the economy is open to the global capital market. We further show in Appendix Section A.2 that our main results hold if we allow intermediate inputs and labor to be arbitrarily substitutable. Introducing capital to the model makes it intractable, as noted by Galor (2005).

to drive open economy effects.

3 Cross-Country Analysis

We start to test our theory by looking at variation across countries in abortion policies and the agriculture employment share. The cross-country analysis has two main advantages. First, we establish that the relationship predicted by our theory holds even when accounting for general equilibrium forces at the country level, 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.

3.1 Cross-Country Data

We construct a cross-country panel dataset of agricultural employment share and abortion policy changes. To measure agricultural employment share we rely primarily 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).[8] We collapse specific policy changes into an index that varies between 1 and 5 as in Elías et al. (2017).[9] 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. 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.

3.2 Cross-Country Specification

Fertility rates and agricultural employment share are very likely endogenously determined, each influencing the other. For example, an improvement in nonagricultural productivity may pull workers away from the farm and raise the returns to human capital, inducing parents to switch away from child quantity and into child quality (Galor, 2005). We therefore need an

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

[9] Bloom et al. (2009) instead construct a 7 point index.

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

$$AES_{ct} = \alpha_c + \alpha_t + \sum_{\tau=T_0}^{T} \beta_\tau Abortion_{ct} + \epsilon_{ct} \tag{4}$$

where AES_{ct} is country c's agricultural employment share in year t. $Abortion_{ct}$ is equal to the magnitude of the change in 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 (4) following De Chaisemartin and d'Haultfoeuille (2024).

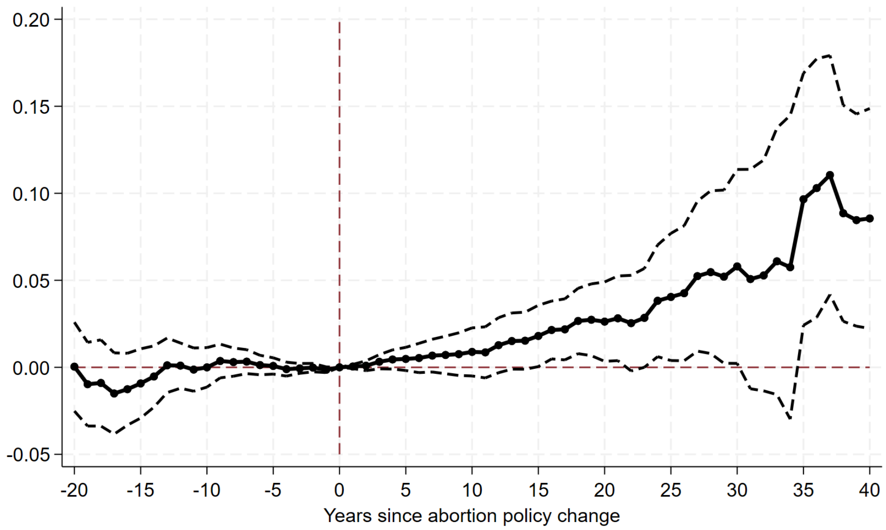
3.3 Cross-Country Results

We show the results of estimating equation (4) in Figure 1, which shows the event study plot. We do not find evidence of pretrends. 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 15 to 40 years later is a 5 percentage point increase in agricultural employment share. Relative to a mean share of 0.37, this represents a 14 percent drop.

We also estimate the effect of abortion policy on the birth rate using equation (4). Appendix Figure D.2 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 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 quality-quantity tradeoff found by Rosenzweig and Zhang (2009). Hence, the population

Figure 1: 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. 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).

size effect dominates.

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). To address this concerns, we turn next to a within-country analysis.

4 Regional U.S. Analysis

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.[10]

As surgical abortions became more prevalent in the U.S. in the 1800s, a backlash followed, driving widespread implementation of abortion restrictions across the country. Lahey (2014) finds that the passage of these laws was not correlated to the immigrant population

[10]Other U.S. reproductive policy changes may come to mind but are not suitable for our analysis. The liberalization of abortion access in the 1960s and 1970s yields too little across-state variation over time, as most states were treated all at once with the 1973 Roe v. Wade Supreme Court decision. Regarding the 'power of the pill,' Myers (2017) argues that the rollout of oral contraception across the U.S. had little impact on fertility.

share, literacy rate, pre-law child-to-woman ratio, and, importantly for the present study, the urbanization rate. Lahey (2014) estimates that the abortion restrictions increased fertility by 5 to 15 percent.

To measure agricultural employment share, we use the decadal data compiled by Craig and Weiss (1998) for the period 1800 to 1900. These data are drawn from decennial census tabulations computed by the U.S. Census as well as estimates based on the Census microdata for the 1870 to 1900 waves. Imputations were necessary, especially in earlier census periods.[11] The dependent variable drawn from these data is the ratio of male agricultural workers ages 10 and older to the total population.[12] We provide additional details on the data and their construction in Appendix Section C.1.

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

Figure 2 shows the resulting event study plot of our estimates. There are no differential trends in agricultural employment share prior to the implementation of abortion restrictions. After restrictions are in place, a negative effect on agricultural employment share begins to emerge, becoming statistically significantly negative four decades later. The delayed effect is consistent with the fact that affected cohorts must age into the labor market, and mirrors our findings in the cross-country estimates shown in Section 3. The implication is that increased fertility—a slower demographic transition—speeds up the movement of workers out of agriculture. In terms of the magnitude, agricultural employment share falls by almost 5 percentage points four decades after abortion was restricted, a 27% reduction. If the average abortion policy reduced fertility by 10%, the midpoint of the estimates by Lahey (2014), then the resulting long-run fertility-agricultural employment share elasticity is 2.7.

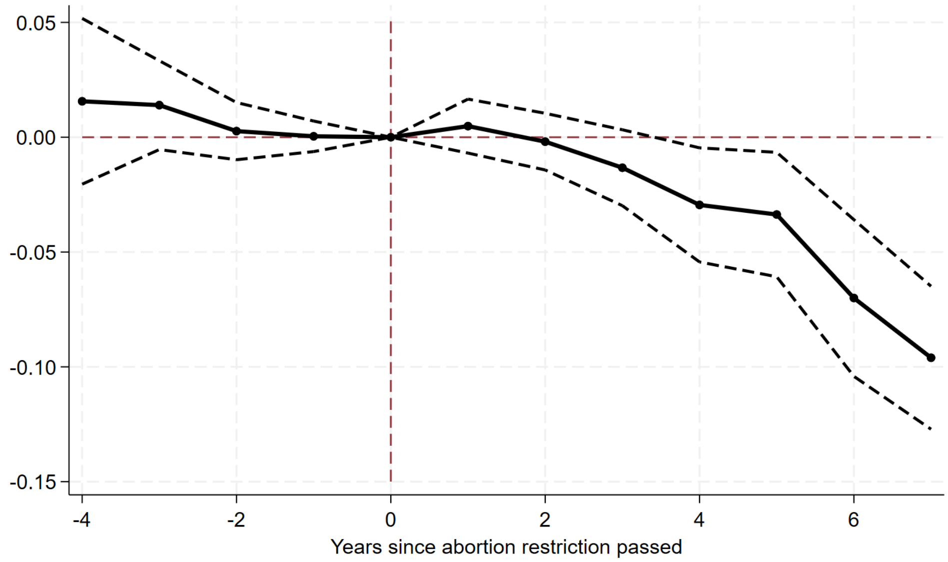
We conduct two additional robustness checks of our main results. Appendix Figure D.4 shows the event study plot when excluding states that passed abortion restriction laws prior to 1840. Laws passed prior to 1840 were often part of larger bills and not enforced until much later. While fewer states and years are included, we still see a statistically and economically significantly negative effect of abortion restrictions on agricultural employment share four decades later.

Next, to better understand how our proposed mechanisms work at the level of decision

[11] We redo the estimation using the 1850–1900 full count census waves to construct agricultural employment share and our results do not change; see Appendix Figure D.3. See Appendix Section C.1 for more on the imputations implemented by Craig and Weiss (1998).

[12] 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).

Figure 2: Effect of Abortion Restriction on Agricultural Employment Share, U.S. States



Notes: Data on state-level agricultural employment shares 1800-1900 comes from Craig and Weiss (1998). Timing of abortion restrictions come from Lahey (2014) and Lahey 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).

makers, we turn to a quasi-experiment in Bangladesh in which richly detailed household and individual level data are available.

5 Bangladesh Quasi-Experiment

The Maternal and Child Health and Family Planning (MCH-FP) program was introduced in the Matlab subdistrict in Bangladesh in 1977 to treatment villages by icddr,b (formerly known as the International Centre for Diarrhoeal Disease Research, Bangladesh). The program included family planning and maternal and child health services.

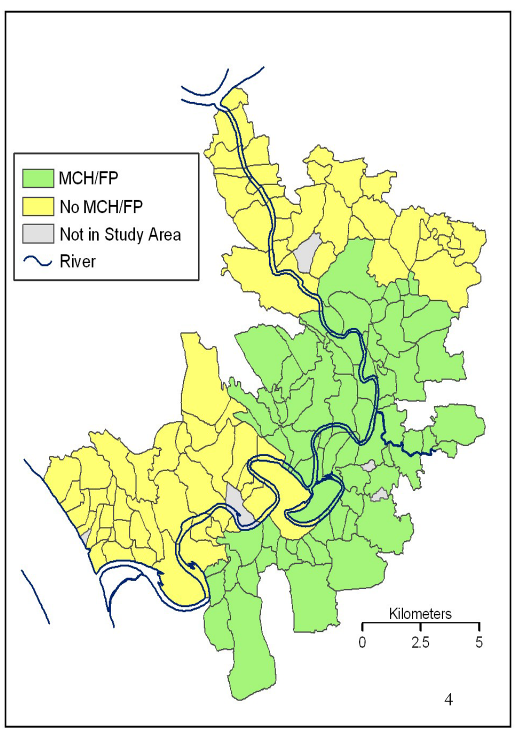
5.1 Program Details

Program interventions were rolled out over time starting with access to and advice on using modern contraception for women and tetanus toxoid vaccines for pregnant women in 1977. Intensive child health interventions started in 1982 with the measles vaccine and other child health interventions were introduced in 1985 including vaccination against measles, tetanus, pertussis, polio, and tuberculosis were distributed for children starting in 1985.

In the comparison area, then-standard government health and family planning services were available, but family planning services were only available at clinics, not in the home, and some of the childhood services, such as vaccinations, were not readily available in clinics until 1989 or later, providing an experimental period, 1977–1988, to evaluate the program.

The MCH-FP program was introduced to half of Matlab, with the remaining half serving as an untreated comparison. We depict treatment and comparison villages in Figure 3. The program covered about 200,000 people in 149 villages, with the population split evenly between the two areas. The program was placed in a single block of contiguous villages, with a block of comparison villages on two sides. The block design was intended to reduce potential contamination of the comparison area with information about the family planning interventions (Huber and Khan, 1979) and spillovers from positive externalities generated by vaccination. The comparison villages were socially and economically similar to the treatment villages and geographically insulated from outside influences (Phillips et al., 1982). Treatment and comparison blocks were chosen in order to balance the average distance to transport and health infrastructure between the blocks. We thus refer to the placement of this intervention as quasi-random and draw further support for our identification strategy from the evidence shown in Section 5.3.1 of pre-program similarities between treatment and comparison areas.

Figure 3: 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. Taken from Barham (2012).

The program was successful in driving rapid take up of the two key interventions: family planning and the measles vaccine (see Appendix Figure D.5). 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 much 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. Rates for the comparison area were much lower throughout the period. We provide additional details about the MCH-FP in Appendix Section B.

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

Previous research demonstrates that the MCH-FP program had significant effects on fertility and human capital. Barham et al. (2021a) show that completed family size was between 0.52 and 0.67 smaller in the treatment than the comparison area depending on the number of reproductive years a woman was exposed to the MCH-FP Program. Joshi and Schultz (2007) use a different research design and also find schooling increased for boys.

Regarding human capital, Barham (2012) finds that adolescent boys born during the vaccine phase of the program in the treatment area experienced significant improvements in height, cognitive functioning, and schooling. There was no effect on those born prior to the introduction of intensive child health interventions for those born between 1977-1981. In a follow-up paper, Barham et al. (2021b) show that effects on height and education persisted into adulthood for those born between 1982-88. The persistence of the effect on human capital is strongest for affected men.

5.2 Data and Treatment Assignment

Data Sources. We draw on the extraordinarily rich data available for the Matlab study area. We focus on household- and individual-level sectoral employment. To measure these outcomes, we use both the 1996 Matlab Health and Socioeconomic Survey wave 1 (MHSS1) (Rahman et al., 1999) and the 2012–2015 Matlab Health and Socioeconomic Survey wave 2 (MHSS2).[13] Questions changed significantly between survey rounds, and the MHSS2 offers a

[13]

richer set of questions about sectoral employment (see Appendix SectionC.2 for more details on our sectoral employment classification). In particular, we can measure the share of months worked by sector in MHSS1 but the share of hours worked by sector in MHSS2.

MHSS2 was conducted between 2012 and 2014 and has low attrition rates, with the loss of less than 10 percent of the target sample.[14] 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. International migrants not interviewed in Matlab were instead contacted by phone.

We use two supplementary data sources: periodic censuses in 1974 and 1982 (icddr,b, 1974, 1982), and 1974–2014 Matlab demographic surveillance site (DSS) data on the universe of vital events (e.g., births, marriages, deaths, in and out migrations) collected by the International Center for Diarrhoeal Disease Research, Bangladesh (icddr,b). The MHSS1 and MHSS2 are a panel of a random sample of households from the study area, while the census and DSS data cover the entire study area. A key feature of all these data is that individuals can be linked across different data sources 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.2.

Analysis Sample and Attrition. We consider two primary units of analysis. In our baseline estimation, we look at households, the unit at which decisions about member's employment are typically made in Bangladesh. Moreover, households often jointly make migration decisions for individual members. Because household composition may change over time in response to the MCH-FP, we consider 1996 MHSS1 households as our unit of household analysis. That is, we aggregate MHSS2 households into the household in which survey respondents resided in 1996. Household composition at this early stage is unlikely to be shaped by the program since the children born during the program were not yet of age to form their own households. Only 0.5 percent of MHSS1 households have no members who can be tracked to the MHSS2 survey round.

When assessing the role of human capital in the MCH-FP's total effect, we analyze employment outcomes at the individual level. The sample of individuals includes those who were randomly selected for individual interviews in an MHSS1 primary sample household or

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

were a pre-1996 migrant into Matlab. Including death and any other type of non-response, the attrition rate is 7 percent. This is a low attrition rate compared to other long-term effects studies with shorter follow-up periods despite a migration rate of approximately 60 percent for men (25 percent international) in this highly-mobile population.

Intent-to-Treat and Baseline Variables. Access to the MCH-FP program was based on the village of residence of the individual/household during the program period. We cannot use the area where the household or individual lived at the time of survey or even when some of the individuals in our individual sample were born because the household may have moved into the village after the start of the program, and therefore post-1977 location might be endogenous (Barham and Kuhn, 2014). We determine treatment at the household and individual level by exploiting the Demographic Surveillance System and census data, tracing back an individual in the MHSS2 2012–2014 survey back through their family tree to find where the household head lived prior to the program.

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

5.3 Empirical Strategy

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

5.3.1 Baseline Balance and Trends

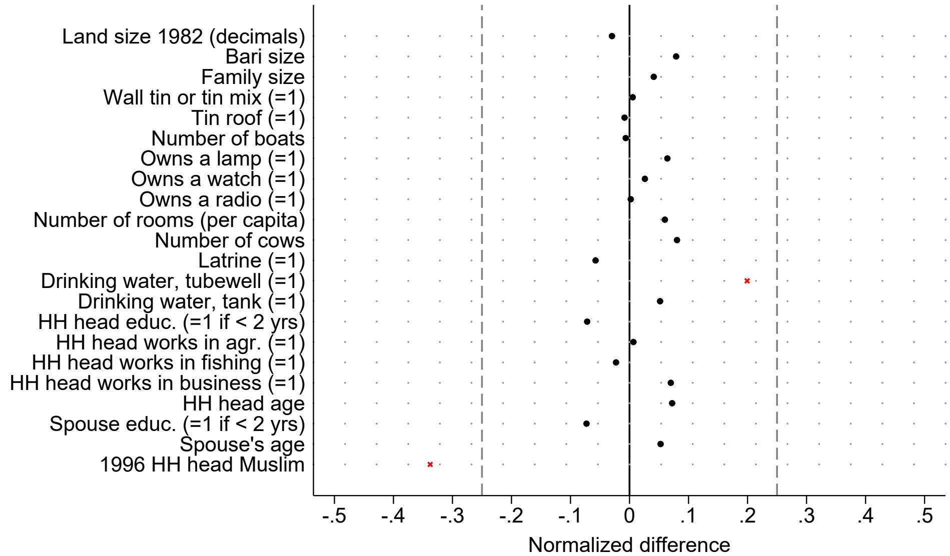
Because our identification strategy uses variation between treatment and comparison villages, we now show that pre-intervention characteristics were well balanced between these two areas. Prior studies have shown that the treatment and control villages are extremely well-balanced across a range of variables. Importantly, balance holds across several important dimensions including mortality rates, fertility rates, and pre-intervention household and household head characteristics (Koenig et al., 1990; Menken and Phillips, 1990; Barham, 2012; Joshi and Schultz, 2013). In addition, migration stocks and flows were similar between the treatment and comparison area at the start of the program and through to 1982, for a cohort of individuals most likely to migrate at the start of the program, showing good baseline balance (Barham and Kuhn, 2014). Barham et al. (2023) further show that for men born between 1977 and 1988, the labor market outcomes for their antecedent households were similar in 1974 and the trends were similar in the early years of the program between 1974 and 1982. Finally, Barham (2012) also shows that cognitive functioning, height, and education were similar across the treatment and comparison areas in 1996 for those who were old enough that their human capital and height were not likely to have been affected by the program.

Much of the previous literature examined baseline balance at the individual level. We further explore the baseline balance between the treatment and comparison area at the household level in Table 2 using 1974 census data. We depict the normalized differences in means (difference in the means divided by the standard deviation of the comparison area) of preintervention household characteristics in Figure 4. Appendix Table D.1 presents means for the treatment and comparison group separately and the differences in means between the two group. The normalized difference provides an indication of the magnitude of mean differences, since a small difference in means can be statistically significant with large sample sizes (Imbens and Wooldridge, 2009). Normalized differences bigger than 0.25 standard deviations are generally considered to be substantial. In Figure 4, any difference which is statistically significant at the 5% level is indicated with a red X.

Differences in means are 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. Since we test balance across 22 variables it is not surprising that a few are statistically different. With the exception of religion and tubewell water for drinking water, the normalized differences are less than 0.12 standard deviations demonstrating that the differences that do exist are relatively small. In our main specification, we control for all baseline variables.

The difference in tubewell access is close to the cut off at 0.20 standard deviations. There is widespread groundwater arsenic contamination in the tubewells in Bangladesh (Chowdhury et al., 2000) and arsenic is a health concern and has been shown to reduce IQ among school aged Bangladeshi children (Wasserman et al., 2006). 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 for this. In sum, our baseline balance results mimic previous research and show that the two areas are similar across a wide variety of household and household head

Figure 4: Baseline Balance in Normalized Differences



Notes: The chart plots normalized differences in baseline 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.

characteristics.

5.3.2 Empirical Specification

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

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

where T_h is an indicator for whether household h is considered treated (as defined in Section 5.2) and X_h is the vector of demographic and baseline characteristics detailed in Table D.1. We cluster standard errors by the village of the household head of h or his antecedents in 1974.

5.4 Main Results

We first estimate the effects of the MCH-FP on the share of work time spent in each sector at the household level. Results are shown in Table 1. We separate the estimates into medium-

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

	MHSS1 (1996)		MHSS2 (2012–2014)		
	(1) Agriculture	(2) Non-agricultural	(3) Agriculture	(4) Manufacturing	(5) Services
Treatment	-0.003	0.011	0.039***	-0.024*	-0.010
	(0.022)	(0.022)	(0.014)	(0.014)	(0.018)
% chg. rel. to mean	-0.5	3.0	18.7	-12.2	-2.0
Mean	0.68	0.36	0.21	0.20	0.48
Baseline controls	Y	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y	Y
Observations	2534	2534	2484	2484	2484

Notes: The table presents estimates of equation (5) for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. Columns (1) and (2) measure outcomes in the 1996 MHSS1, while Columns (3) through (5) measure outcomes in the 2012–2015 MHSS2. MHSS1 dependent variables are the share of working months in the year in which household members could work allocated to each sector. MHSS2 dependent variables are the share of hours worked by sector within the household. See Appendix C.2 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.

run effects (Panel A) measured as of the 1996 MHSS1 survey, and long-run effects (Panel B) measured as of the 2012–2015 MHSS2 survey. The dependent variable in panel A is the share of months spent per year in each sector; in panel B, the dependent variable is the share of annual work hours spent in each sector.

As of 1996, 19 years after the MCH-FP program started, we find no significant effect of the program on sectoral employment, as shown in Panel A. The estimated treatment effect is -0.2 percentage points (SE=2.2). The effect of the program on non-agricultural employment is similarly small, with an estimated effect of 0.9 p.p. (SE=2.2).

Next, we turn to the long-run effects of the MCH-FP, 35 years after it started. Panel B of Table 1 reports our results at the time of the 2012–2015 MHSS2 survey. The MCH-FP raised the share of household adults working in agriculture by 3.9 p.p. (SE=1.4 p.p), representing a 19 percent increase over the comparison area (column 1). The share of household members in manufacturing fell by 2.4 p.p. (SE=1.4), a 12 percent fall relative to comparison households (column 2). In services, we find a very small effect of -0.7 p.p. (SE=1.9), a 1.4 percent reduction relative to comparison households (column 3). Hence, the MCH-FP program reduced the speed of structural transformation.

We separately explore the MCH-FP's effect on the extensive margin of farming and land ownership. Appendix Table D.5 reports the estimates. The program had negligible effects on farming in 1996 (columns 1–2). In particular, treated households were no more likely to farm than comparison households in 1996 (column 1). We also do not detect any statistically significant medium-term effect of the program on the number of acres owned per capita (column 2).

By contrast, the program induced treated households to remain in farming relative to control households. By 2014, treatment area households were 3.2 percentage points more likely to farm relative to comparison area households (column 3), consistent with our theoretical predictions. Households in both areas owned a similar number of acres per member (column 4).

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. The results are reported in Appendix Table D.3. Columns 1 through 3 show the same pattern as in Table 1: increased agricultural enterprise founding, less in manufacturing, and no change in service entrepreneurship.

Given the importance of large firms, especially factories, in driving 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.3. Employment at factories among treated households lagged behind comparison area households (columns 4 and 5), as did employment at large firms (column 6).

Next, 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 (5) by sector, but further split the dependent variable of work hours share by rural and urban location of employment. We report results in Appendix Table D.7, with the effect on hours worked share in urban areas reported in columns 1–3, and in rural areas 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.

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

We assess the concern that information spillovers along the border of the treatment and control zones may reduce our estimated effect. To do so, we restrict our sample to those living in a village prior to the intervention which has a centroid within 3km of the border. In Panel B of Appendix Table D.6, we show that our results are very similar in magnitude to our baseline estimates when applying this restriction.

Given our finding in Section 5.3.1 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.6. 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.6, we show that our results are largely unchanged when we remove households who resided in Pourashava prior to the intervention.

5.5 Mechanisms

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

5.5.1 Family Size

We start by testing how household size shapes our results, a key mechanism highlighted by our theoretical model. Fauveau (1994), Joshi and Schultz (2013), and Barham et al. (2023) have all found significant effects of the MCH-FP in reducing fertility. We also estimate the effect of the program on the number of men and women born during the experimental period, with results shown in Table D.4. Consistent with earlier research, we find the program reduced household size. In particular, we find the program reduced the number of males per household aged 24 to 34 by 16 percent, and decreased the number of females per household in the same age range by 11 percent.[15]

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

$$Y_h = \alpha_0 + \alpha_1 Num.\ males\ age\ 24\ to\ 34_h + \gamma X_h + \epsilon_h \tag{6}$$

Because the number of males born during the experimental period is an outcome of the

[15]The difference in number of 24-34 year olds by gender is statistically indistinguishable. The effect size on fertility is 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
No. males aged 24–34	-0.260**	0.165	0.065
	(0.113)	(0.108)	(0.117)
% chg. rel. to mean	-125.9	82.2	13.4
Mean	0.21	0.20	0.48
First-stage F-stat.	11.2	11.2	11.2
Baseline controls	Y	Y	Y
Embankment controls	Y	Y	Y
Observations	2484	2484	2484

Notes: The table presents 2SLS estimates for outcomes measured in the 2012–2015 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 is the share of hours worked by sector within the household. See Appendix C.2 for more details on how we classify workers into sectors. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

program, we instrument for $Num.\ males\ age\ 24\ to\ 34_h$ using the treatment dummy.

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

5.5.2 Human Capital

The simple model outlined in Section 2 posits that an increase in human capital will draw workers out of agriculture. We test that theoretical prediction leverage the rollout of the vaccine arm of the MCH-FP and cross-cohort variation in exposure.

Past research on the effects of the MCH-FP by Barham (2012) and Barham et al. (2021b) 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. In what follows, we therefore take as given that cohorts born into the vaccine arm of the MCH-FP (that is, 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:

$$Y_i = \alpha_{y(i)} + \gamma_1(T_i \times Born_i^{77-81}) + \gamma_2(T_i \times Born_i^{82-88}) + \gamma_3(T_i \times Born_i^{Pre-77}) + \nu X_i + \epsilon_i \quad (7)$$

where T_i is an indicator for whether i is treated as defined in Section 5.2; $\alpha_{y(i)}$ is a set of indicator variables for i's birth year; and X_i is the vector of pre-intervention demographic and baseline characteristics detailed in Table D.1.[16] 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. For each cohort, we also report the cohort's mean outcome in the comparison area, and the percent change relative to the cohort comparison mean.

Table 3 reports results at the individual level among men.[17] We find that, consistent with our household-level estimates, treated individuals increase the share of hours worked in agriculture (column 1) and reduce it in manufacturing (column 2).

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

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

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

[17]We show results for women, who work much less than men, in Appendix Table **??**.

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	-0.07**	0.06	-33.14
	(0.02)	(0.03)	(0.04)	(83.64)
Treatment × Born 1977–1981	0.05**	-0.04	-0.03	-61.78
	(0.02)	(0.03)	(0.04)	(88.74)
Treatment × Born Pre-1977	0.04*	0.00	-0.03	-117.09*
	(0.02)	(0.01)	(0.02)	(63.34)
% chg. (1982–88)	16.6	-27.5	11.2	-1.1
% chg. (1977–81)	64.1	-20.8	-5.4	-1.9
% chg. (Pre-1977)	12.9	1.5	-6.1	-4.1
Comparison mean (1982–88)	0.08	0.25	0.51	3040.13
Comparison mean (1977–81)	0.08	0.21	0.59	3185.37
Comparison mean (Pre-1977)	0.29	0.10	0.52	2857.27
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.2 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.

sectors based on their human capital.

5.5.3 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.1. 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 next examine whether observable measures of productivity change as a result of the program. With the human capital rising due to the vaccine component of the MCH-FP, farmers may raise their per-acre farm productivity. As we show in Appendix Table D.8, we see no evidence of the program raising farm productivity per acre.

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, however, list prices at the variety level (e.g., coarse or fine paddy boro), not the crop level (e.g., paddy boro). Hence we take prices in two ways: either the minimum price within crop across varieties, or the maximum.

We 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. If anything, the effects are negative. 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.

6 Conclusion

Fertility decline is an essential process by which countries escape the "Malthusian trap" of excess population growth, economic stagnation and poverty. Yet population growth is also an engine of innovation, and thus fertility decline could reasonably be expected to change the shape of structural transformation. This paper provides the first direct empirical evidence on the effect of demographic transition on structural transformation. We assess this empirical relationship in varying contexts, using distinct sources of exogenous variation and levels of aggregation. The key takeaway across all analyses is that fertility reductions on net slow down structural transformation from agricultural to manufacturing.

Given the severe negative consequences of population growth, these findings do not suggest that fertility control should be avoided. Instead our model and empirical results suggest that the modest effects of fertility decline on slowed structural transformation can be offset by gains in human capital. Countries investing sufficiently in human capital are able to escape the Malthusian trap, as declining fertility enables a take off in per capita incomes. On the other hand, countries experiencing a demographic transition and minimal investment in human capital may find themselves shifting towards a stagnant equilibrium in which a Malthusian trap is averted, but manufacturing and service sector growth stagnates. 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

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

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

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,

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

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.[18]

A.3 Three-Sector Model with Service Sector

We extend our model to allow for a third sector producing nontradable output. We do so to understand whether our baseline model predictions change with an addition that requires modeling demand.

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

$$Q_s = A_s h L_s$$

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

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

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

Because service sector output is not traded, we must model demand. As in Bustos et al.

[18]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.

(2016), we assume a Cobb-Douglas utility function:

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

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

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

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

In equilibrium, we obtain the same analytic results on agricultural employment (equation 3) and therefore the same effect of changes in population size and human capital as in our baseline. For equilibrium services and manufacturing employment share, we obtain

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

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

Hence

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

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

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

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

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

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

A.4 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.

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.5). 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.5. 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 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.[19]

The numerator, the agricultural workforce, is trickier to compute and requires some assumptions and imputations. Craig and Weiss (1998) focus on rural agricultural employment;[20] 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.[21] As a robustness check, we show very similar results to our baseline in Figure D.3 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 together. 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 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).

The total agricultural workforce is computed in the 1820 and 1840 censuses. For the years 1800, 1810, and 1830, Craig and Weiss (1998) state that the computation procedures were, "more complex and roundabout, and differed substantially between the free and slave states. Nevertheless, the estimates were based largely on the evidence from later years."

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

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

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

C.2 Matlab Health and Socioeconomic Survey

For the Matlab Health and Socioeconomic Survey (MHSS) waves, 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.

The Demographic Surveillance System data are collected bi-weekly or monthly and allow determination of exact birth dates and birth place, key inputs to our assignment of treatment status described in Section 5.2.

In neither the MHSS1 nor the MHSS2 surveys, were respondents asked directly about their non-agricultural industry of employment. Therefore, we must classify industry using indirect measures. Moreover, because the survey questions differed between waves, we take slightly different approaches to industry classification for each survey round.

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

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

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

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

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

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)	11.62	(16.00)	11.00	(16.21)	-0.62	-0.69	-0.03
Bari size	8.06	(5.51)	8.86	(5.99)	0.80	1.72	0.08
Family size	6.87	(2.95)	7.01	(2.94)	0.15	1.12	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)	0.832	(0.370)	0.828	(0.375)	-0.005	-0.22	-0.01
Number of boats	0.673	(0.623)	0.666	(0.631)	-0.007	-0.15	-0.01
Owns a lamp (=1)	0.613	(0.485)	0.652	(0.474)	0.039	1.05	0.06
Owns a watch (=1)	0.149	(0.354)	0.160	(0.364)	0.011	0.54	0.03
Owns a radio (=1)	0.080	(0.270)	0.081	(0.271)	0.001	0.05	0.00
Number of rooms (per capita)	0.206	(0.097)	0.212	(0.102)	0.007	1.43	0.06
Number of cows	1.29	(1.73)	1.45	(1.70)	0.16	1.75	0.08
Latrine (=1)	0.863	(0.342)	0.821	(0.381)	-0.042	-1.56	-0.06
Drinking water, tubewell (=1)	0.163	(0.367)	0.322	(0.465)	0.159	4.09	0.20
Drinking water, tank (=1)	0.321	(0.465)	0.393	(0.486)	0.072	1.35	0.05
HH head < 2 years education	0.610	(0.485)	0.564	(0.493)	-0.046	-1.81	-0.07
HH head works in agriculture (=1)	0.591	(0.489)	0.595	(0.488)	0.005	0.15	0.01
HH head works in fishing (=1)	0.063	(0.241)	0.055	(0.227)	-0.008	-0.51	-0.02
HH head works in business (=1)	0.096	(0.293)	0.126	(0.330)	0.030	1.37	0.07
HH head age	46.24	(13.39)	47.18	(13.73)	0.95	1.76	0.07
HH head spouse < 2 years education	0.844	(0.338)	0.808	(0.368)	-0.035	-1.67	-0.07
HH head spouse's age	36.08	(10.35)	36.75	(10.90)	0.67	1.25	0.05
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 Consumption Shares by Sector

	(1) Agriculture	(2) Manufacturing	(3) Services
Treatment	-0.01	-0.01	0.02*
	(0.01)	(0.00)	(0.01)
% chg. rel. to mean	-2.6	-2.6	6.0
Mean	0.52	0.19	0.25
Baseline controls	Y	Y	Y
Embankment control	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.3: 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	18.2	3.5	4.8	-14.7	-21.3	-23.0
Mean	0.22	0.02	0.14	0.15	0.08	0.08
Baseline controls	Y	Y	Y	Y	Y	Y
Embankment control	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.4: ITT Effects of MCH-FP on Household Size and Composition

	(1) Number of Men Age 24–34	(2) Number of Women Age 24–34
Treatment	-0.15***	-0.09**
	(0.04)	(0.04)
% chg. rel. to mean	-15.2	-10.2
Mean	0.98	0.90
Baseline controls	Y	Y
Embankment control	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.5: ITT Effects of MCH-FP on Farming and Land Ownership

	MHSS1 (1996)		MHSS2 (2012-2014)	
	(1)	(2)	(3)	(4)
	=1 if household farms	Acres owned per cap.	=1 if household farms	Acres owned per cap.
Treatment	0.006	-0.058	0.029*	-0.002
	(0.027)	(0.045)	(0.017)	(0.006)
% chg. rel. to mean	0.8	-16.5	3.6	-1.5
Mean	0.68	0.35	0.80	0.10
Baseline controls	Y	Y	Y	Y
Embankment control	Y	Y	Y	Y
Observations	2525	2518	2484	2482

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

Table D.6: ITT Effects of MCH-FP on Work Time Shares by Sector: Household-Level, Robustness

	MHSS1 (1996)		MHSS2 (2012–2014)		
	(1) Agriculture	(2) Non-agricultural	(3) Agriculture	(4) Manufacturing	(5) Services
Panel A: Full Sample					
Treatment	-0.003	0.011	0.039***	-0.024*	-0.010
	(0.022)	(0.022)	(0.014)	(0.014)	(0.018)
% chg. rel. to mean	-0.5	3.0	18.7	-12.2	-2.0
Mean	0.68	0.36	0.21	0.20	0.48
Observations	2534	2534	2484	2484	2484
Panel B: Within 3km of Treatment Border					
Treatment	-0.008	0.010	0.029*	-0.007	-0.012
	(0.027)	(0.027)	(0.017)	(0.017)	(0.023)
% chg. rel. to mean	-1.2	2.9	13.4	-3.9	-2.6
Mean	0.71	0.34	0.22	0.18	0.48
Observations	1718	1718	1686	1686	1686
Panel C: Only Muslim Households					
Treatment	-0.006	0.014	0.033**	-0.024*	-0.004
	(0.023)	(0.023)	(0.015)	(0.014)	(0.018)
% chg. rel. to mean	-0.8	4.0	16.0	-12.1	-0.8
Mean	0.68	0.35	0.21	0.20	0.48
Observations	2286	2286	2241	2241	2241
Panel D: Exclude Main City					
Treatment	0.004	0.005	0.054***	-0.034**	-0.009
	(0.024)	(0.024)	(0.014)	(0.014)	(0.020)
% chg. rel. to mean	0.6	1.5	25.7	-16.3	-1.9
Mean	0.68	0.35	0.21	0.21	0.47
Observations	2064	2064	2020	2020	2020

Notes: The table presents estimates of equation 5 for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. 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 **??** 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.

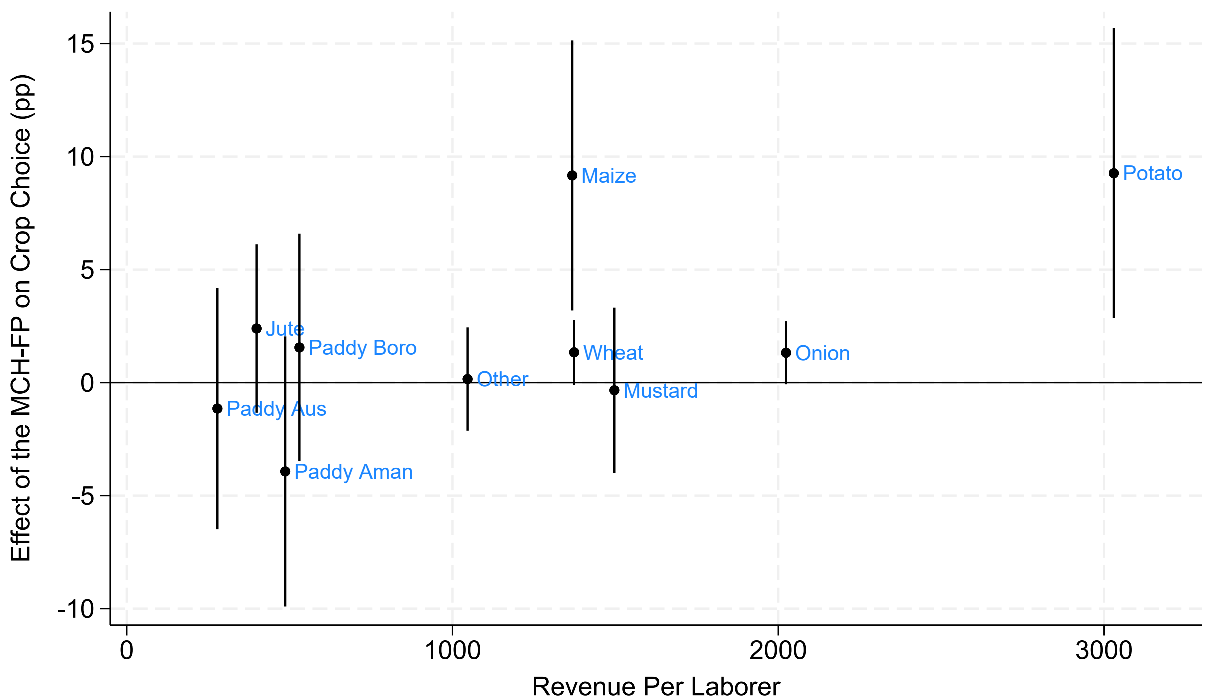
Table D.7: ITT Effects of MCH-FP on Long-term Work Hour Shares by Sector and Urbanicity: Household-Level

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

Notes: The table presents estimates of equation 5 for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. The dependent variable is the share of hours worked within the household in different sectors and in different locations. See Appendix **??** 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.

Notes: The table presents estimates of equation 5 for outcomes at the MHSS1 household-level. Variable means refer to the comparison group. Standard errors are clustered by the 1996 household head's pre-program village. The dependent variable is the share of hours worked within the household in different sectors and in different locations. See Appendix C.2 for more details on how we classify workers into sectors. Due to changes between survey waves, sectors are constructed differently in the MHSS1 and MHSS2, and therefore are not directly comparable. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure D.1: ITT Effects of MCH-FP on Crop Choice and Average Crop Productivity



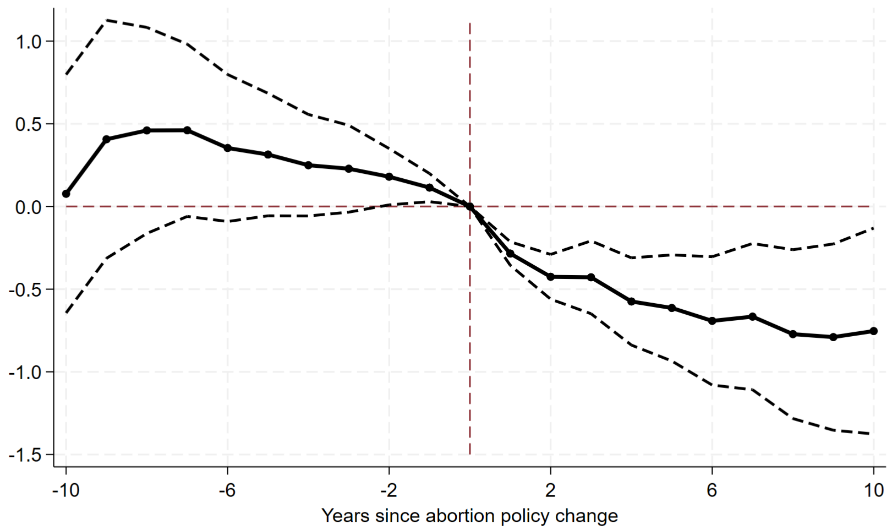
Notes: The figure reports estimates of equation 5. The vertical axis reports the ITT effect on whether the household grew the given crop. The horizontal axis reports the average revenue per unit of labor when producing the crop, which comes from XXX. Vertical bars represent the 95% confidence intervals.

Table D.8: ITT Effects of MCH-FP on Revenue and Profits per Acre

	(1) Revenue per acre (min. price)	(2) Revenue per acre (max. price)	(3) Profit per acre (min. price)	(4) Profits per acre (max. price)
Treated	-0.591	-24.74	-10.63	-34.27
	(39.52)	(143.0)	(52.18)	(144.3)
% chg. rel. to mean	-0.1	16.0	-1.6	-41.4
Mean	446.13	-154.24	683.45	82.84
Embankment controls	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y
Estimation method	OLS	OLS	OLS	OLS
Observations	1411	1411	1411	1411

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

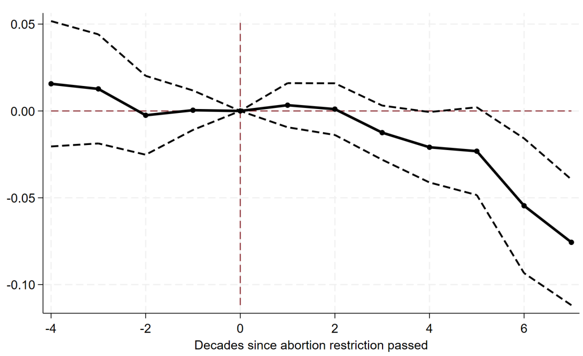
Figure D.2: Effect of Abortion Policy Changes on Crude Birth Rate



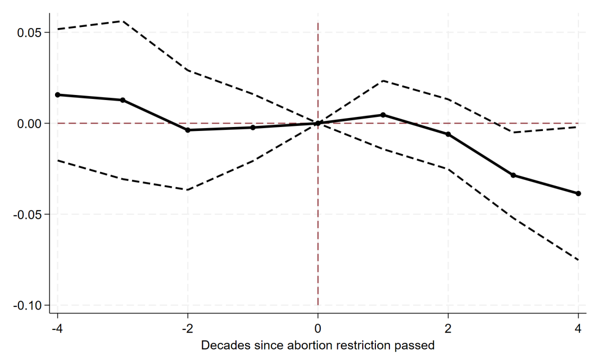
Notes: The figure shows event study coefficient estimates for the effect of abortion policy changes on the crude birth rate. 95% confidence intervals depicted with standard errors clustered at the country level. Annual data on crude birth rate come from the World Bank Development Indicators as compiled 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.3: Effect of Abortion Restrictions on Agricultural Employment Share, U.S. States, Full Count Census 1850–1900

(a) all abortion policies

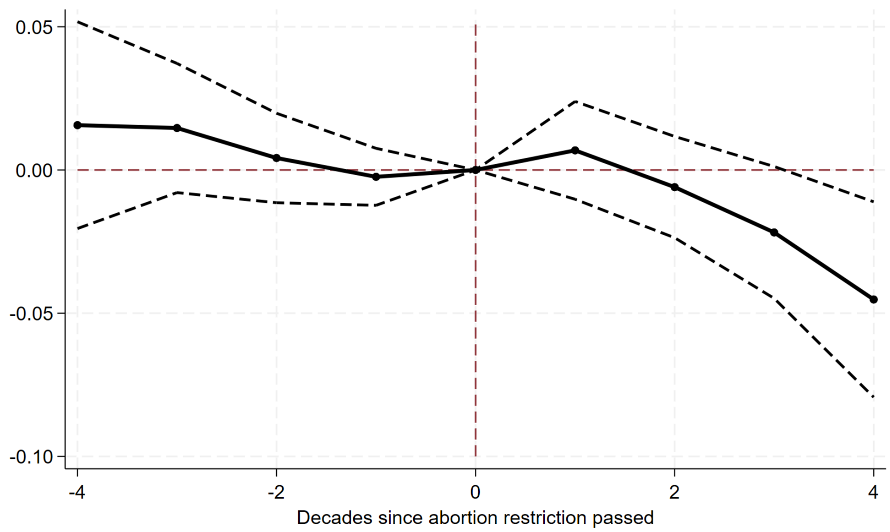


(b) post-1840 abortion policies



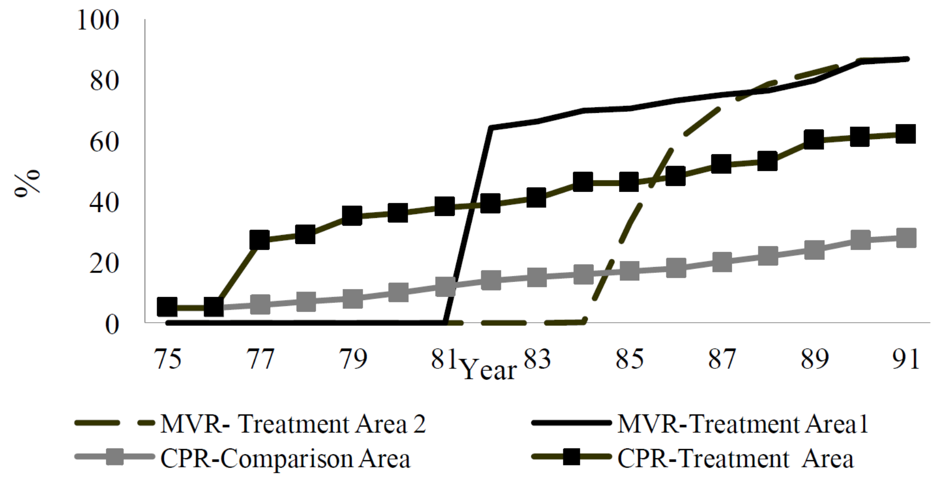
Notes: Data on state-level agricultural employment shares 1800-1840 comes from Craig and Weiss (1998). Agricultural employment shares for 1850–1900 computed from Ruggles et al. (2024). Timing of abortion restriction laws come from Lahey (2014) and Lahey (2014). 95% confidence intervals depicted with standard errors clustered at the state level. Estimated using the Stata command `did_multiplegt_dyn` by de Chaisemartin et al. (2024).

Figure D.4: 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 (2014). 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.5: 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).