Fewer Kids, More School: Evidence from the Early 20th Century U.S. Birth Control Movement

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Abstract

What caused the rapid rise in human capital accumulation in the early 20th century? This paper examines the role of the birth control movement, led by Margaret Sanger, which facilitated the roll out of over 600 birth control clinics across the United States. Using a staggered difference-in-differences design, we find that children whose mothers gained access to birth control were less likely to have younger siblings, more likely to attend school and be literate, and less likely to report working. These findings are consistent with a quantity-quality tradeoff, in which investments in education came at the high opportunity cost of forgone income via child labor.

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1 Introduction

Early 20th-century America was a period of profound economic and social transformation. Among these changes was a sharp increase in human capital accumulation, as secondary school graduation rates rose from 10% in 1910 to over 50% by 1940 (Goldin, 1998). While legislative reforms – such as compulsory schooling and child labor laws – contributed to this shift, existing evidence suggests their impact was modest (Goldin and Katz, 2011; Lleras-Muney, 2002). Another key trend during this period was the decline in fertility, marking the end of the United States' demographic transition as birth rates fell toward replacement levels (Bailey and Hershbein, 2018). Could this decline in fertility – by increasing families' resources available per child – have helped drive the surge in educational attainment?

In this paper, we leverage the opening of Margaret Sanger's birth control clinics across the United States 1910-1940 – which recent work shows accounted for 5.4-6.5 percent of this fertility decline (Bauernschuster et al., 2025) – to study how a reduction in family size influenced children's schooling and work outcomes. Using a staggered difference-in-differences design, we first document that half of the birth control clinics' negative impact on fertility was driven by the "intensive margin," or the number of kids to which existing mothers gave birth. We then document that school attendance increased for both younger children (ages 5–14) and older youths (15–18) in response to clinic openings. Consistent with these children's family size playing a role, we find that children of both age groups are also less likely to have toddler-aged siblings as a result of the clinics. A back-of-the-envelope calculation suggests that access to birth control clinics can explain roughly 2% of the rise in years of educational attainment between 1910 and 1940.

Children's education during this period came at a cost. Schooling would have imposed some direct costs on parents, such as expenses for books or private school tuition. And, more importantly, it was not uncommon for a substantial portion of a families' income to rely on children's wage income or, domestically, contributions to household production. School attendance therefore implied forgoing income or valuable contributions at home (Goldin, 1979; Horrell and Humphries, 1995). While the minimum age at which children began working rose throughout the 19th century, this opportunity cost was still large and binding for youths at the turn of the 20th century (Doepke et al., 2023). The financial sacrifices associated with investing in children's human capital meant that child "quality" was costly, implying a steep quantity-quality ("QQ") tradeoff for parents. That is, holding family income constant, having more children meant fewer resources to spend on each child's education (Doepke, 2004; Hazan and Berdugo, 2002).

Provided that the QQ tradeoff described above holds, a reduction in family size

would have shifted children from work into school. However, families' ability to control fertility was constrained by the absence of effective birth control technologies and by legal barriers, particularly the Comstock laws. The birth control movement spearheaded by Margaret Sanger aimed to alleviate these constraints and enable families to make more deliberate fertility choices. Through the establishment of over 600 clinics nationwide, the movement facilitated women's access to new birth control technologies (e.g., the diaphragm) and, more generally, knowledge about the relative efficacy of various contraceptive methods. Although the technology was still nascent and far less effective than modern hormonal contraceptives, empirical evidence suggests that these clinics contributed to 5.4-6.5% of the overall fertility decline during this period (Bauernschuster et al., 2025).

We examine how the clinics, through a QQ channel, influenced education and employment outcomes among children born to women who had access to the clinics. To do so, we use a difference-in-differences approach to compare changes in children's outcomes in counties that gain access to clinics to those who did not. While clinics were not randomly assigned to locations (e.g., cities were more likely to receive clinics), the event study estimates suggest that control counties' outcomes were evolving in parallel to treated counties leading up to the event, conditional on controls.

We begin by replicating the finding that the introduction of clinics reduced birth rates. We find that, similar to findings in Bauernschuster et al. (2025), counties with access to a clinic saw a decrease of 0.010 in the ratio of children to women. We show that approximately half of this impact is driven by the intensive margin, or children born to mothers, and half is driven by the extensive margin, or the propensity for a woman to become a mother. This finding suggests that access to birth control technologies were used to moderate the size of a family conditional on having one. It is consistent with the historical context suggesting that the clinics primarily targeted women who were married or already had children, and the fact that the clinics appeared to help women both delay motherhood and space births (Bauernschuster et al., 2025; Hajo, 2010).

We then shift focus to children over the age of 5 and study how their mothers' access to a clinic influenced their school attendance and employment. After the introduction of the clinics, children in both age groups shift towards "full-time" school attendance, meaning attending school and not reporting any employment. Among youths (ages 15-18), this increase is coupled with an equally sized decrease in full-time employment (defined analogously). This finding is consistent with the notion that youth education had a large opportunity cost, but as their moms gained access to contraception, the family could afford to invest in the child's education. Among younger children (ages 5-14), the shift towards full-time schooling is primarily driven by a shift away from full-time "leisure," or reporting neither school nor employment. This finding is more consistent with the idea that parents found it increasingly worthwhile to shift younger

children from domestic work to school or to start investing in children's outcomes earlier. We also find positive impacts on literacy rates for both age groups, suggesting higher rates of school attendance translated into substantive human capital gains.

Next, we more directly test whether children in these age groups were less likely to have younger siblings as a result of the clinics. This analysis helps confirm the mechanism of reduced family size, which is a particularly important to test for the 15–18 age group, whose mothers would have been older and thus less likely to have additional births even in the absence of birth control. For both age groups, we find that access to a clinic significantly reduced the likelihood of having a younger sibling.

Higher investments into children as family size decreases rests on the assumption that educational investments are costly. To verify that such educational costs shape the QQ tradeoff between family size and educational investments, we exploit state-level variation in children's access to schooling and work opportunities. During our study period, states varied in the scope and strength of schooling laws that either encouraged or required children to attend school rather than work, thus creating regional differences in the opportunity cost of education. We focus on the three types of reforms studied by Goldin and Katz (2011): compulsory schooling laws, continuation school mandates, and educational requirements for work permits. We find that lower costs of education weaken the QQ tradeoff. Counties where such laws were already in place experience muted responses to the opening of birth control clinics compared with counties with fewer regulations. For example, counties without any educational requirements for work permits experienced a 0.07 increase in the share of children ages 15-18 attending school full-time as a result of the clinics; for every additional year of education required by a work permit law, this effect was smaller by nearly 0.01. This result suggests that the QQ-tradeoff is less relevant in settings where education is more accessible, as parents can more easily (or are forced to) provide education to their children (Doepke et al., 2023).

Our results contribute to the body of work seeking to identify if – and in what circumstances – a QQ tradeoff exists. The QQ tradeoff was first formalized by Becker and Lewis (1973). Since then, a large literature tests whether plausibly exogenous increases in family size – typically induced by twin births or the gender composition of existing children – affect various measures of child "quality" (Liu, 2015).[1] Evidence from developed countries is mixed: some studies find that larger family size reduces children's educational outcomes (Black et al., 2010; Cáceres-Delpiano, 2006; Conley and Glauber, 2006), while others find no significant relationship (Angrist et al., 2010; Black et al., 2005). In contrast, studies from developing countries more consistently doc-

[1]To our knowledge, only one other paper uses shocks to the price of child quality as a test of QQ tradeoffs: Bleakley and Lange (2009) show that the eradication of hookworm disease from the American South in the early 20th century led to a decline in fertility.

ument negative effects of family size on children's educational attainment (Ponczek and Souza, 2012; Rosenzweig and Wolpin, 1980; Rosenzweig and Zhang, 2009) and on parental investments in child health and education (Lee, 2008; Liu, 2014; Millimet and Wang, 2011). One way of reconciling these findings is that QQ tradeoffs are weak or absent in contexts where child labor is prohibited and high-quality K-12 education is publicly funded. In such settings, the relevant margin of child quality becomes college education, which can be financed through loans and occurs after children leave the household, reducing direct competition for household resources (Doepke et al., 2023; Liu, 2015). We provide credibility to this unifying hypothesis by directly testing whether the QQ tradeoff, induced by the opening of birth control clinics, is mitigated in areas that were beginning to develop such institutions.

Applying the QQ tradeoff framework to this period reveals an additional mechanism that contributed to human capital accumulation in the early 20th century. Past work has documented that various state-level compulsory schooling laws or child labor laws had modest impacts on secondary school educational attainment (Goldin and Katz, 2011; Lleras-Muney, 2002; Lleras-Muney and Shertzer, 2015). Specifically, estimates from Goldin and Katz (2011) suggest that the laws can explain 6% of the 2.45 year increase in completed years of schooling or 6-7% of the 50.4 percentage point increase in the high school enrollment rate. We find access to birth control clinics explain 5% of the increase in school attendance among youths, which translates into roughly 2% of the rise in years of educational attainment.

This paper also contributes to a closely related literature examining how access to contraceptives or abortion affects children's well-being. The literature generally finds that women's access to contraception or abortion improves a range of outcomes for these women's children. Outcomes studied include health indicators such as infant mortality and low birth weight (Anukriti et al., 2022; Flynn, 2024a,b; Gruber et al., 1999; Lahey and Wanamaker, 2025; Rau et al., 2021), educational attainment and future employment (Ananat et al., 2009; Bailey, 2013; Londoño-Vélez and Saravia, 2025; Mølland, 2016; Pop-Eleches, 2006; Rau et al., 2021), involvement in criminal activity (Donohue III and Levitt, 2001, 2008), and socioeconomic conditions during childhood, such as poverty status and two-parent household structure (Ananat and Hungerman, 2012; Bailey et al., 2019; Gruber et al., 1999).[2] To explain these impacts, much of the lit-

[2]Some nuance is worth noting. Ananat and Hungerman (2012) find that while access to the pill improves long-run outcomes, in the short run it increased the share of children born into poor households and with low birth weight. They attribute this to positive selection into pill use by women seeking to delay childbearing. Anukriti et al. (2022) find that while abortion access in India reduces infant mortality among girls, it also increases the number of "missing girls" as a result of sex-selective abortions. Pop-Eleches (2006) find that Romania's abortion ban in the 1960s led to an increase in children's average educational attainment. But, this effect is explained by the fact that, prior to the ban, educated urban women were more likely to access abortion. Once this selection is accounted for, the ban's impact on educational outcomes among the affected cohorts is negative.

erature has focused on the "wantedness" channel, where reducing the price of controlling fertility results in more positively selected children, typically because women with fewer resources are more likely to use contraception or abortion. Other mechanisms explored include changes in cohort size and the direct impact on familial resources.[3] In contrast to most existing work, we study how access to birth control affects the contemporaneous educational outcomes of children who are already born, as well as their likelihood of gaining a younger sibling. These outcomes give us visibility on parents' decisions to invest in their children's human capital and allow us to directly test for the family size channel, which implies that we can rule out selection into lower birth rates among under-resourced moms as a mechanism. These findings – though in a very different setting – echo those of Londoño-Vélez and Saravia (2025), who show that being denied an abortion in modern day Columbia lowers school attendance among already-born children and increases their likelihood of working.

A complementary literature studies the impacts of birth control on women's fertility and well-being. Much of the focus is on modern contraceptive methods. Women with access to the pill tend to reduce or delay childbirth and marriage, instead opting for investing in their education and careers, which in turn has lowered the gender wage gap (e.g., Bailey (2006, 2010); Bailey et al. (2012); Goldin and Katz (2002)). Access to subsidized family planning resources leads to delays in childbearing and reductions in completed fertility, particularly among resource-constrained women (Bailey, 2012; Bailey et al., 2023). A smaller literature examines how access to abortion or shifting cultural attitudes explains historical trends in birthrates in developed countries throughout the 19th century (e.g., Beach and Hanlon (2023); Lahey (2014)). We build on recent work focusing on early 20th-century U.S. birth control clinics (Bauernschuster et al., 2025), except our focus is the downstream impacts on children.

This paper is organized as follows. Section 2 describes the data sources and sample construction. Section 3 provides the historical background around the birth control clinic movement and education reforms during the time period of 1910 to 1940. Section 4 outlines the empirical strategy of using a staggered difference-in-differences design. Section 5 presents our results, and Section 6 concludes.

2 Data

This paper draws on two main data sources. The first is the Full Count Census from 1910, 1920, 1930, and 1940. From these data, we construct county-level information on women's fertility, children's school attendance, and children's employment to form

[3]Smaller cohorts can decrease exposure to infectious diseases (Lahey and Wanamaker, 2025) and reduce school crowding (Pop-Eleches, 2006). Greater access to family planning can also allow parents to invest more in their own careers or relationships (Bailey et al., 2019).

the main outcomes. We additionally use the 1910 Census to construct county-level characteristics that form control variables: the share of households living in an urban area, the share of farm households, the share of households living in a big city, the ratio of children ages 0-4 to women ages 18-33, the share of adults who are literate, the share of the population that is white, black, or another race, and the share of the population that is foreign-born. Throughout the analysis, our sample is limited to individuals who do not live in group quarters. To maintain consistent county boundaries across all four census years, we follow the longitudinal template of Horan and Hargis (1995) and aggregate counties to their 1910 borders in each period.

Following Carlana and Tabellini (2024), we measure women's fertility in three ways: the ratio of children to women, the ratio of children to mothers, and the ratio of mothers to women. The first measure captures overall fertility, while the latter two capture the intensive and extensive margin, respectively.[4] The overall fertility measure allows us to replicate the main findings in Bauernschuster et al. (2025), and, crucially for our purposes, we complement these findings with analysis along the extensive and intensive margin separately in order to assess the role of family size as a mechanism. We restrict the sample to women in their prime child-bearing years, ages 18–33, and identify each woman's children using the mother–child links provided by IPUMS for individuals in the same household. Our main measures of fertility focuses on children aged 0-4, which proxies for the number of children born,[5] but we also present the results for all children aged 0-18 for thoroughness.

We construct measures of children's schooling and employment similar to Goldin and Katz (2011). Specifically, for each county-year, we calculate the share of children who attend school only ("full time" work), attends school and works ("part time" school), works only, or neither attends school nor works. Work is defined as having listed an occupation. School attendance is defined as having attended school at any point during a reference period of a few months, defined slightly differently across Census years.[6] Because school attendance is imperfectly measured, we additionally evaluate the impacts on the share of children who are literate, defined as being able to

[4] To further understand this breakdown along the extensive and intensive margin, consider the fact that:

$$\frac{\text{\# children ages 0-4}}{\text{\# women ages 18-33}} = \underbrace{\frac{\text{\# children ages 0-4}}{\text{\# mothers ages 18-33}}}_{\text{intensive}} \times \underbrace{\frac{\text{\# mothers ages 18-33}}{\text{\# women ages 18-33}}}_{\text{extensive}}$$

where mothers are defined as a woman with a child in her household of any age.

[5] The ideal measure of fertility would be the number of children ever born to each woman, but these data are only available in 1900, 1910, and 1940 among ever-married women.

[6] In 1910-1930, enumerators asked about school attendance since September 1 of the previous year. The Census day was April 15, January 1, and April 1 in 1910, 1920, and 1930, therefore capturing a period of 7.5 months, 4 months, and 6 months, respectively. In 1940, the Census information was collected on April 1 and enumerators asked about school attendance since March 1, resulting in a reference period of 1 month.

read and write in any language. However, this variable is not available in 1940 and can therefore only be evaluated over a shorter time span. We limit the sample to children who live with a mother, which allows for sample continuity when we further analyze whether these children are less likely to have a younger sibling (whom we identify based on co-residency and sharing the same mother).

We analyze children's outcomes separately for children aged 5-14 and 15-18. This serves two purposes. First, education was more optional for high school aged children (as will be discussed in greater detail below) and, thus, age 14 or 15 would be a natural point at which the tradeoff between work and education changes discretely. Second, work outcomes are not reported for children under age 14 in 1940, likely due to the Fair Labor Standards Act of 1938, which raised the minimum working age to 14 and thus rendered such data collection unnecessary (at least in principle). Splitting the age groups therefore allows us to track outcomes for a longer period of time among the subgroup of youths aged 15-18. Analyses are not sensitive to alternative reasonable age range splits, such as ages 5-13 and 14-17.

The second data source is a list of 632 birth control clinics, their locations, and their opening dates compiled by Hajo (2010). Information on clinics was compiled via national directories, reports, or ephemera from archival records associated with nonprofit organizations operating within the birth control movement (which will be discussed in greater detail in the next section).[7] Clinic locations were recorded by city, which we crosswalked to counties using the Census data to identify which counties each city belonged to.[8] Following Bauernschuster et al. (2025), we consider a county to be (permanently) treated as soon as any clinic opens either within the county itself or a neighboring county. We ignore clinic closures, as treatment effects may continue to accumulate through information diffusion or shifts in cultural norms, and, in most cases, a new clinic would eventually open in the same area following a closure. We identify neighboring counties using the county adjacency files compiled by National Bureau of Economic Research and the Census Bureau (NBER, 2017). The results are the same if we consider a county treated only when a clinic opens within its own boundaries.

[7] Organizations include the American Birth Control League, the Birth Control Clinical Research Bureau, and the Birth Control Federation of America (see Table 1 on page 24 of Hajo (2010)). The list of clinics can be found online at this link under "Supplemental Materials": https://www.press.uillinois.edu/books/?id=p077258#pane-3. The original list includes 649 clinics, but we drop 17 clinics in Hawaii, Puerto Rico, and in an unidentifiable city.

[8] Census data records both the city and county of residence for each individual and can therefore be used to form a crosswalk between counties and cities. 53 clinics were located in cities (e.g., parts of New York City) that mapped to multiple counties. In these cases, we assign the clinic to all counties to which it mapped, therefore casting a wide net on locations the clinic may have served.

3 Historical Background, 1910-1940

3.1 Birth control movement

As discussed in Bailey and Hershbein (2018), between 1800 and 1940, the number of live births per woman declined from approximately seven children to just above two. The decline was especially sharp in the 1920s, with fertility falling by about one child per woman, ultimately reaching a low in the late 1930s. Scholars have proposed a range of explanations for this transition to lower birth rates, including the rising opportunity cost of women's time as factory employment expanded and falling mortality rates (which reduced the number of births needed to reach a desired family size). And, even in the absence of modern contraceptives, nascent forms of birth control likely played a role in this fertility decline. Demographers and historians have highlighted the spread of birth control practices – including withdrawal and extended breastfeeding – as contributing factors. Empirical evidence also suggests that emerging birth control technologies (e.g., abortifacients) played a role, at least during the 19th century.

More recently, work by Bauernschuster et al. (2025) has pointed to the significance of the early 20th century birth control movement, which is the focus of this project. Both Bauernschuster et al. (2025) and Hajo (2010) provide detailed overviews of the movement and its effects on fertility, which we briefly summarize here for context. In the early 20th century, access to birth control information and methods was limited by the Comstock Act, which constrained or prohibited the circulation of "obscene" materials, such as information about contraceptive practices. These practices, however, had the potential to benefit maternal health by, for example, allowing for more birth spacing or preventing advanced maternal age pregnancies. Motivated in part by these concerns, Margaret Sanger became a leading activist for birth control access. Her primary contribution as an activist was as a founder of the American Birth Control League (ABCL) in 1921. Among other activist activities, the ABCL facilitated the opening of birth control clinics nationwide. Key to the movement's success, the clinics strategically dodged Comstock laws by employing physicians, who were often exempt from Comstock law restrictions. The primary service offered by these clinics was the fitting of diaphragms, a newer birth control technology that Sanger seen used in Europe and was inspired to bring to the U.S.. A fitting of the diaphragm was also usually accompanied by instructions on proper use and annual follow-up visits to reinforce best practices and inspect the device for wear.

As alluded to in Bauernschuster et al. (2025), whether a clinic opened in a given county was shaped by a complex set of local factors, such as the presence of motivated activists, the availability of willing medical professionals, and a community's fundraising capacity. These factors often made urban areas more attractive. Table A.1 compares

counties that received a clinic to those that did not and provides evidence consistent with this pattern. In addition to being more urban, counties in which a clinic opened also had lower child-to-woman ratios and a larger share of foreign-born residents.[9] In our empirical approach, we use counties that never receive a clinic as a control group for the treated counties. Our identification strategy requires that we assume the outcomes (e.g., children's enrollment in school) would have evolved in parallel across counties in the absence of these clinics. To make this assumption more plausible, we control for the 1910 characteristics in Table A.1 (via county fixed effects) and how these characteristics impact trends (by including interactions between these characteristics and year dummies).

Figure 1(a) depicts time trends in the fertility measures as described in Section 2. Consistent with a decline in completed fertility over this time period, the ratio of young children to women declined by 0.2. Bauernschuster et al. (2025) estimate that the emergence of clinics associated with the movement accounted for 5.4-6.5% of the decline in fertility between 1920-1940. Importantly, Figure 1(a) reveals that most of the decline is along the intensive margin, i.e. ratio of young children to mothers.

3.2 Child labor and education

In the 1910's, it was not uncommon for youth age – or even younger – children to work in low skill jobs. In 1910, approximately 50% of 15-18 year olds and 9% of 5-14 year olds reported an occupation. Table A.2 shows common occupations reported among boys and girls separately. Unsurprisingly, the occupations listed include many low skill or manual labor positions, such as farm labor, manufacturing, store clerks, and seamstresses.

In the decades that followed, during what is now known as the "high school movement," child labor declined as school attendance took its place. Figure 1(b) depicts these trends separately for 5-14 and 15-18 year olds. Between 1910 and 1940, the fraction of youths in school full time increased by 25 percentage points. This increase coincided with a 10pp decrease in full time work and a 15pp decrease in part time work/school.

As described in Goldin and Katz (2011), a confluence of factors, such as increasing income or returns to schooling, may have contributed to this rapid rise in human capital accumulation. State-level laws requiring school attendance and regulating child labor are often cited as key drivers of this shift. These laws varied in scope and content, but generally fell into three categories: child labor laws (e.g., minimum level of school required before a child could work), compulsory schooling laws (e.g., setting a school

[9]For additional context, Figure A.1 shows the number of clinic openings by year, and Figure A.2 provides a map of clinic openings across counties over time. These figures and Table A.1 are very similar to the ones produced in Bauernschuster et al. (2025) but presented here for context.

entry and leave age), and continuation (or part-time) school regulations (e.g., require that school hours occur during normal weekdays rather than evenings/weekends). However, empirical assessments of the impacts has been modest (e.g., Goldin and Katz (2011); Lleras-Muney (2002)), and the full set of forces behind the decline in child labor and rise in schooling remains an open question.

In this paper, we study the role of improved access to birth control and subsequent reductions in family size in explaining this trend. As discussed in Doepke et al. (2023), the option to send a child to work instead of school would imply that education has a large opportunity cost, one that would bind more in larger families where there are more mouths to feed. To assess the plausibility of the presence of such quantity-quality tradeoffs during this time period, Figure 2 documents how children's employment and school enrollment varies with the number of younger siblings (<5) in 1910.[10] The more likely a child is to have a toddler sibling, the more likely they are to work instead of (or in addition to) attend school. This tradeoff is steeper among older children, for whom work was a more viable or lucrative option. School enrollment, depicted in the second panel, exhibits the opposite pattern.[11]

4 Empirical Strategy

To study how the clinics influenced women's fertility and children's outcomes, we use a two-way fixed effects difference-in-differences (DiD) design at the county-year level. To ensure the estimates compare treated individuals to untreated (rather than already-treated) individuals, we apply recently developed methods and employ a "stacked" event study design similar to those deployed by Cengiz et al. (2019) and Deshpande and Li (2019). Specifically, for each year between 1910-1940, we create a dataset comprised of all county-year observations associated with counties that were newly treated in that year and never-treated counties (which form the control group). We then stack these individual datasets – each of which forms a separate "experiment" – and estimate a typical DiD with additional fixed effects indicating the experiment. The estimation equation is:

$$y_{itg} = \beta_0 + \beta_1 ClinicOpened_{itg} + \gamma_{ig} + \gamma_{tg} + \gamma_{s(i)tg} + \gamma_g + (\vec{X}_{1910,i} \times \gamma_{tg})'\alpha + \varepsilon_{itg} \quad (1)$$

[10] We focus on younger siblings aged 0-4 to provide continuity with the later results, where we assess the impact of the clinics on children's likelihood of having a toddler sibling. Moreover, the presence of toddlers may be the channel through which family size affects investments into children because young children require more resources (including time that could otherwise be spent working). Nevertheless, we find a similar pattern in Figure A.3, where we plot these outcomes against the number of siblings of any age on the x-axis.

[11] The share of children aged 5-8 in school is as low as the share for 15-16 year olds. This low share is driven by children aged 5-6 who have not necessarily started school.

where i indexes counties, $t \in [1910, 1920, 1930, 1940]$ indexes year, s indexes state, and g indexes the experiment associated with each treatment cohort. The outcomes (y_{itg}), described in Section 2, include various measures of fertility and children's education or work outcomes. $ClinicOpened_{itg}$ is an indicator variable equal to 1 if a clinic has opened in county i or any of its neighboring counties in year t or earlier. γ_{ig}, γ_{tg}, and $\gamma_{s(i)tg}$ denote county, year, and state-by-year fixed effects, respectively, each interacted with experiment fixed effects, which are also included in the specification (γ_g). β_1 therefore captures how y_{itg} changes after a clinic opens in a treated county, relative to the change over the same period in a different county (in the same state) that does not receive a clinic. The experiment fixed effects and their interactions ensure the estimated treatment effect represents the weighted average of treatment effects estimated separately for each treatment cohort. To help address the imbalance in depicted in Table A.1, we also control for 1910 county-level characteristics interacted with (experiment-specific) year dummies ($\vec{X}_{1910,i} \times \gamma_{tg}$). We weight observations by the 1910 total population, and standard errors are robust and clustered on county.

5 Results

5.1 Women's fertility

Table 1 presents the results for women's fertility. The first two columns report the results for the overall ratio of children to women (either ages 0-4 or ages 0-18), and the subsequent columns decompose these changes along the intensive and extensive margin.

After a clinic opens, the ratio of toddlers to women falls by 0.010, corresponding to a 1.7% decline relative to the 1910 average (Column 1, Table 1). Given that 70% of the population would have been exposed to a clinic in their own or neighboring county, these estimates imply that clinics were responsible for 5% of the 0.15 decline in this ratio between 1910 and 1940. Reassuringly, this decline is comparable to estimates reported in Bauernschuster et al. (2025), who employ a slightly different empirical approach and sample.[12]

Columns 3 and 5 indicate that intensive and extensive margins contribute roughly equally to this decrease: mothers have 0.010 fewer children on average (a 0.8% drop from 1910), and women are 0.005 less likely to become mothers at all (a 1.09% drop). Thus, for every 100 fewer children born because of access to a clinic, about half reflect

[12]They focus on married women aged 15-39 and define treatment as the number of years a woman is exposed to a clinic in her county or a neighboring county since her 15th birthday. They estimate a 0.004 decline in toddlers per married woman for each year of exposure. By 1940, women were exposed for an average of 2.745 reproductive years, implying a total effect of $2.745 \times 0.004 = 0.01$, which is 1.3% of their 1920 baseline ratio of toddlers per married woman.

mothers having fewer children, and half reflect fewer women becoming mothers in the first place. That the intensive margin plays a substantial role is consistent with the notion that the clinics aim to primarily serve married women, especially those with children, and that they had an impact on delaying motherhood as well as spacing births Bauernschuster et al. (2025) and Hajo (2010). [13]

Using the ratio of *young* children to women in their prime childbearing years is our preferred proxy for women's fertility. Counting children aged 5-18 risks conflating mortality with birth rates, making it a less reliable measure of new births. Moreover, because we study how a clinic opening affects contemporaneous outcomes, we expect the impact to be concentrated among younger children. Nevertheless, Columns 2, 4, and 5 show a similar pattern: a clinic reduces the ratio of children (ages 0-18) to women by 0.016 (a 1.5% decline relative to the 1910 mean), with roughly 73% of this effect driven by the extensive margin and 27% by the intensive margin.

These sizable intensive-margin effects align with the fact that the clinics primarily served married women who already had children. Serving unmarried women (who likely had no children) would have jeopardized her credibility and, consequently, the broader momentum of the social movement. And, the reduction in children per mother suggests that family size could be a key mechanism behind the results in the following section, which we examine in greater detail below.

5.2 Children's work and education

In Table 2, we examine how the clinics affected children's school attendance and employment. Panel A presents results for high school–age children (15–18), and Panel B focuses on younger children (5–14). The outcomes are mutually exclusive and collectively exhaustive: the share of children attending only school, both school and work, only work, and neither school nor work. Because work outcomes for the younger group are only available from 1910 to 1930, we exclude 1940 from Panel B.

After their mothers gain access to a birth control clinic, children in both age groups attend school at higher rates. For older youths (15-18), a clinic opening raises the share attending school by 0.019 (Column 1). For younger children (5-14), the corresponding increase is 0.026. As depicted in Figure 1(b), the share of children attending school full time increased by 0.25 among youths and by 0.06 among younger children between 1910-1940. Given that 70% of the population was exposed to a clinic opening either in their own or neighboring clinic, these estimates imply that the clinics were responsible

[13] In line with this idea, Bauernschuster et al. (2025)'s sample only includes married women. Moreover, Bauernschuster et al. (2025) implicitly explore the impacts of the clinics on the mother-to-woman ratio (extensive margin) by examining the impact of the clinics on an indicator for not having any children in the household separately by the woman's age. They find positive impacts on younger women in their sample, suggesting that the women used birth control to delay motherhood.

for 5% (29%) of the schooling increase among youths (younger children).

How does the impact of the clinics compare to the impact of compulsory schooling and child labor laws? Goldin and Katz (2011) estimate that these laws account for 6% (0.145 years) of the 2.45-year increase in educational attainment among birth cohorts who would have been exposed to the reforms. To place these estimates in a comparable framework, we follow Lleras-Muney and Shertzer (2015) and assume that school attendance translates to an additional year of completed schooling distributed evenly across children within each age group. Under this assumption, our findings imply a 0.05 (0.18) year increase in educational attainment among youths (younger children) due to the clinics.

Columns 2-4 of Table 2 help unpack which activities children substituted away from as they became more likely to attend school. Among youths, the increase in school attendance is coupled with a similarly sized decrease in share employed of 0.018 (Panel A, Column 3), suggesting that previously working teens switched to attending school. In contrast, the rise in younger children's school attendance appears driven mainly by a reduction in those doing neither school nor work. If neither school nor work, what were these children doing, and why would their mother's ability to have fewer children affect them? It is possible these children would have been engaged in unpaid domestic work, such as helping on the family farm, in which case this shift also reflects a tradeoff between labor and educational investments. Alternatively, parents may have chosen to refrain from even the minor financial costs of school (e.g., books, tuition) if, lacking a way of preventing births, they anticipated that the child would likely work instead of attend high school, in which case the returns to a primary school education are lower.[14]

School attendance alone may not necessarily translate into higher educational attainment, especially given that the data only indicate whether a child attended school at any point during the reference period, regardless of duration. The fact that most of the impact reflects an increase in "full-time" schooling (school only), rather than "part-time" attendance (school & work), supports the notion that clinics facilitated meaningful educational improvements. Nevertheless, we also examine the effect on literacy, which the 1910–1930 Censuses measured for children aged 10 and older. Table 3 presents these results. Although literacy rates were already high in 1910 (over 90% for both age groups), access to a clinic raises these rates by about 1 percentage point in each group. While this increase is small, it aligns with the school attendance findings and suggests that the observed rise in school enrollment indeed translated into substantive gains in human capital.

[14]Other mechanisms outside of the quantity-quality tradeoff are possible. For example, Bauernschuster et al. (2025) find suggestive evidence that women with access to birth control were more likely to enter the workforce. Such women may have found it more beneficial to send their children to school at an earlier age insofar as it provided childcare.

5.3 Prevalence of younger siblings

The results from Section 5.1 suggest that mothers had fewer children (aged 0-4) as a result of the clinics, and the results from Section 5.2 suggest school and work outcomes for children above age 5 improved. Taken together, these findings are consistent with school-age childrens' mothers birthing fewer younger siblings, thereby reducing family size and allowing educational investments per school-age children to increase. But, given that many high school age children's mothers may have already aged out of their fertile years, we now test this mechanism more directly.

Table 4 reports the effect of clinic access on the prevalence of toddler age siblings among 15-18 and 5-14 year olds separately (Panels A and B, respectively). We assess the impact on the share of children with any toddler sibling (Column 1) and the average number of younger siblings (Column 2). The bottom row in each panel reports the average for each outcome in 1910. As expected, youths are much less likely to have a toddler aged sibling (30% versus 52%). Nevertheless, we find that a clinic opening lowers the likelihood that both 15-18 year olds and 5-14 year olds have a toddler-aged sibling (by 0.5 and 0.7 percentage points, respectively).

5.4 Heterogeneity by pre-existing schooling institutions

In this section, we test whether the QQ tradeoff varies in places by the cost of education (or child quality). In the QQ framework, having fewer children increases the resources spent on each child's education. The existence of this relationship crucially depends on education being costly—either through direct schooling costs (like books) or forgone child time (like working in the labor market). For example, if all children are already enrolled in full-time schooling even before the introduction of birth control clinics, declines in fertility may not translate into additional educational investments.

To explore this heterogeneity, we re-estimate Equation 1, fully interacting the treatment indicator with a variable capturing whether the county's state had schooling laws in place in the year prior to treatment. Because our empirical strategy relies on a stacked difference-in-differences design that uses never-treated counties as controls, we assign to each control county the schooling law status corresponding to the treatment cohort's pre-treatment year. For example, among the control counties corresponding to the counties treated in 1938, we assign them their state's schooling law status in 1937. The interaction term between the clinic treatment indicator and the schooling law variable therefore captures whether the treatment effect of the clinics differs between counties located in states that had schooling laws and those that did not at the time of treatment.

We use the replication data available from Goldin and Katz (2011) and, following their methodology, we construct variables intended to summarize three different

(complex and varied) types of schooling laws. The first represents the number of years a child needed to be in school as dictated by compulsory schooling laws, which often had a minimum (or "school entrance") age and maximum (or "school leaving") age. The second measures the years of education required for a child to enter the workforce, defined as either the number of years of schooling necessary to obtain a work permit or the minimum age for a work permit minus the school entry age – whichever constraint is more binding. The third is an indicator for the presence of laws that either mandated or enabled continuation schools – part-time schools targeting youth who had left school but were below the school leaving age. We interpret these schooling laws as lowering costs or barriers to education. For example, states that impose a higher minimum age for child labor effectively reduce the opportunity cost of schooling for children below that threshold because the outside option of working is unavailable.

We test for this heterogeneity in the treatment effects of the clinics on children's full-time schooling separately for each of these types of laws. The results are presented in Table 5, separately for children ages 15-18 (Panel A) and children ages 5-14 (Panel B).[15]. The treatment effects do not vary by the presence of compulsory schooling laws (Column 1). However, among children ages 15-18, the positive impact of clinics on full-time schooling is muted in counties with work permit requirements (Column 2). That is, while clinics increased the rate of full-time schooling among youths by 0.07, this was reversed by 0.01 for each additional year of education required in order to obtain a work permits. Among children ages 5-14, the presence of continuation schools also muted the treatment effect on full-time schooling by nearly two-thirds. Continuation schools also mute the treatment effect for children ages 15-18 by half, but the interaction term is not statistically distinguishable from zero (p-value of 0.140).

Taken together, these results suggest that the QQ tradeoff manifests differently depending on the educational setting. If the opportunity cost of education is low, the tradeoff weakens.

5.5 Extensions and robustness checks

In this section, we test for heterogeneous impacts, parallel trends, and assess the sensitivity of our results across specifications.

Heterogeneous impacts: In 1910, children with a foreign born mother were more likely to be working and not in school relative to those with a native born mother (12% vs. 8%). Additionally, boys were more likely than girls to be engaged in work (11% vs. 7%). These differences could indicate that foreign-born children or boys were

[15] Appendix Tables A.4 and A.3 present the results for all outcomes – full-time schooling, part-time schooling, full-time work, and neither school nor work.

differentially likely to be on the margin of opting for school instead of work, in which case the clinics could have had a differently sized effects on their school attendance.[16]

Figures A.4 and A.5 present the schooling and employment results separately by the mother's foreign-born status and by child gender, respectively. Among children aged 5–14, the increase in school attendance appears to be driven primarily by those with foreign-born mothers. Among youths, the shift from part-time to full-time schooling is largely concentrated among girls. However, neither of these heterogeneous impacts are statistically distinguishable from zero.

Parallel trends: For β_1 from Equation 1 to capture the causal effect of clinics on our outcomes, we must assume that, absent the clinics, treated and control counties would have followed parallel trends over time. Table A.1 showed that treated counties indeed differ systematically from the control counties. While we partially account for these differences through the inclusion of fixed effects and county-level characteristics interacted with year dummies, the possibility of non-parallel trends remains. Therefore, to assess the plausibility of parallel trends in the outcome variables prior to the event, we also present the results for an event-study version of Equation 1:

$$y_{itg} = \beta_0 + \sum_{k \notin [-3,-1]} \mu_k ClinicOpened_{itg,k} + \gamma_{ig} + \gamma_{tg} + \gamma_{s(i)tg} + \gamma_g + (\vec{X}_{1910,i} \times \gamma_{tg})'\alpha + \varepsilon_{itg} \tag{2}$$

where k indexes years since a clinic was opened, binned in intervals of three, following the event study specification in Bauernschuster et al. (2025). We omit the three-year period preceding the event, and therefore estimates of μ_k capture changes in the outcome in period k relative to the 1-3 year period prior to the entry of a clinic. However, because each county is only observed four times (and only a subset are observed in $k \in [-3, -1]$), each $\hat{\mu}_k$ only represents a subsample of counties. This imbalance implies that variation across $\hat{\mu}_k$ may partially reflect variation in underlying sample characteristics. We therefore rely on the simpler pre–post comparison from Equation 1 as our primary specification and use these estimates solely to test for parallel trends.

Figure A.6 report the event study results for the main fertility outcomes. Consistent with the parallel trends assumption, both measures of women's fertility (Panel A) and children's likelihood of having a toddler aged sibling (Panel B) evolve similarly over time across treated and untreated counties leading up to the event. Interestingly, the event study estimates also reveal that the treatment effect does not emerge until 6-11

[16] It is not *ex ante* obvious whether we would expect the effects to be larger or smaller among these groups. Higher base rates for a group could indicate that their contributions to family income were larger and, therefore, families would be more reluctant to opt for their schooling over work. Alternatively, higher base rates could indicate that the there are more children at the margin of selecting into schooling.

years after the opening of a clinic, at which point there is a clear trend break in the outcomes.

Figure A.7 report the event study results for children's schooling and work outcomes separately by 5-14 year olds (Panel A) and 15-18 year olds (Panel B). While outcomes do not necessarily evolve in parallel leading up the event, reassuringly, there does not appear to be a discernible pattern. And, mirroring the fertility outcomes, the treatment effects (particularly among youths) become larger with time.

Sensitivity to specification: Figure A.8 revisits the main specification (Equation 1) and reports the estimated treatment effect across a range of alternative specifications to assess the robustness of the results. We report women's fertility outcomes in Panel A, sibling presence outcomes in Panel B, and children's school and work outcomes in Panels C and D, where estimates are grouped by outcome. To provide a benchmark, we report the treatment effect from the main specification at the top of each outcome group.

The main specification includes controls for 1910 county characteristics interacted with year fixed effects, which control for the fact that counties with different initial characteristics may experience different shocks to fertility or children's schooling. To assess the presence of these shocks, we ask: how sensitive are the results to the inclusion of these controls? We find that the estimates are smaller (or in some cases flip signs) when we drop these controls (labeled "drop 1910 controls"). This sensitivity is especially strong for the fertility outcomes. Upon further inspection, the most influential control appears to be baseline fertility levels, measured as the ratio of toddlers to women.[17] One interpretation is that counties with different initial fertility levels experienced systematically different fertility shocks over time, which may have, in turn, shaped local demand for clinic openings. If so, this result highlights the importance of accounting for demand-side shocks when estimating the impact of clinics. The inclusion of 1910 characteristics interacted with year fixed effects, along with state-by-year fixed effects, is intended to address this concern.

One of the clinics in the dataset corresponds to Sanger's first clinic, which was open for only a few days in New York City in 1916 (see Section 3). Although her activism continued and she later opened a second clinic in the city, we test whether the results are being driven by this early, short-lived clinic. To do so, we exclude both treatment and control observations associated with the 1916 cohort ($g = 1916$) and re-estimate the effects. The estimates are unchanged relative to the benchmark.

As discussed in Section 3, many states enacted compulsory schooling and child labor laws during this period. Because these reforms were often championed by progressive activists – many of whom also supported the birth control clinic movement

[17] Results available upon request.

– it is possible that the timing of clinic openings was correlated with the introduction of these laws. The state-by-year fixed effects account for this potential source of omitted variable bias. However, directly testing this possible confounding factor can lend additional credibility to the assumption that clinic openings were exogenous to the outcomes. The next two estimates present treatment effects with and without controls for state-level education and labor laws,[18] omitting state-by-year fixed effects in both cases. The estimates are nearly identical, suggesting that the impact of the clinics was independent of the impact of these laws.

Finally, we interpret the impacts on children's schooling and employment outcomes under the assumption that these children were already born and thus not subject to fertility-related selection due to birth control. By focusing on contemporaneous outcomes, we can estimate the effects of birth control access on human capital investments without confounding from changes in average cohort quality. However, some counties are observed many years after a clinic opening (e.g., 10 years later), in which case older children may reflect selection effects. For example, children aged 10 and above might have been born after the clinic opened. To address this, we re-estimate the treatment effects using only county-year observations where the time since clinic opening is less than the minimum age in the child's age group. That is, for children aged 5–14, we restrict to observations within four years of a clinic opening; for those aged 15–18, we restrict to within 14 years. Reassuringly, the resulting estimates (labeled "event distance < age") are very similar to the baseline results.

6 Conclusion

This paper provides evidence that the early 20th-century birth control movement enhanced children's educational outcomes by allowing families to reduce fertility and allocate more resources per child. The analysis in this paper concludes in 1940. Since then, some have argued that the U.S. has undergone a second demographic transition as birth rates have returned to pre-baby boom levels (Bailey et al., 2014). The modern context differs in important ways: for example, effective birth control technologies and high quality public education are now widely accessible. These changes may have altered the nature of the tradeoff between fertility and investments in children (Doepke et al., 2023). In line with the importance of such institutional factors, we find that the clinics' impacts were muted in areas with pre-existing schooling institutions. Whether and how modern institutions have reshaped the tradeoffs documented here remains an important question for future research.

[18] Following Goldin and Katz (2011), we use their replication files to construct three variables that vary at the state by year level: the number of school years mandated by child labor laws, the minimum years required by compulsory schooling laws, and a dummy indicating a continuation school law.

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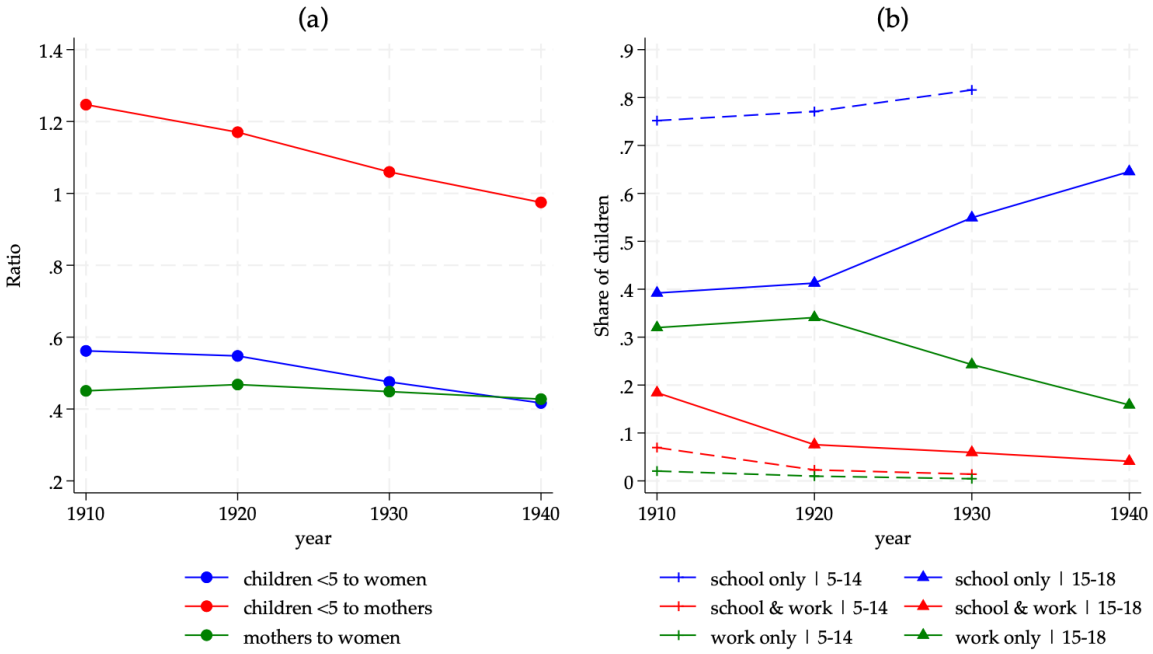
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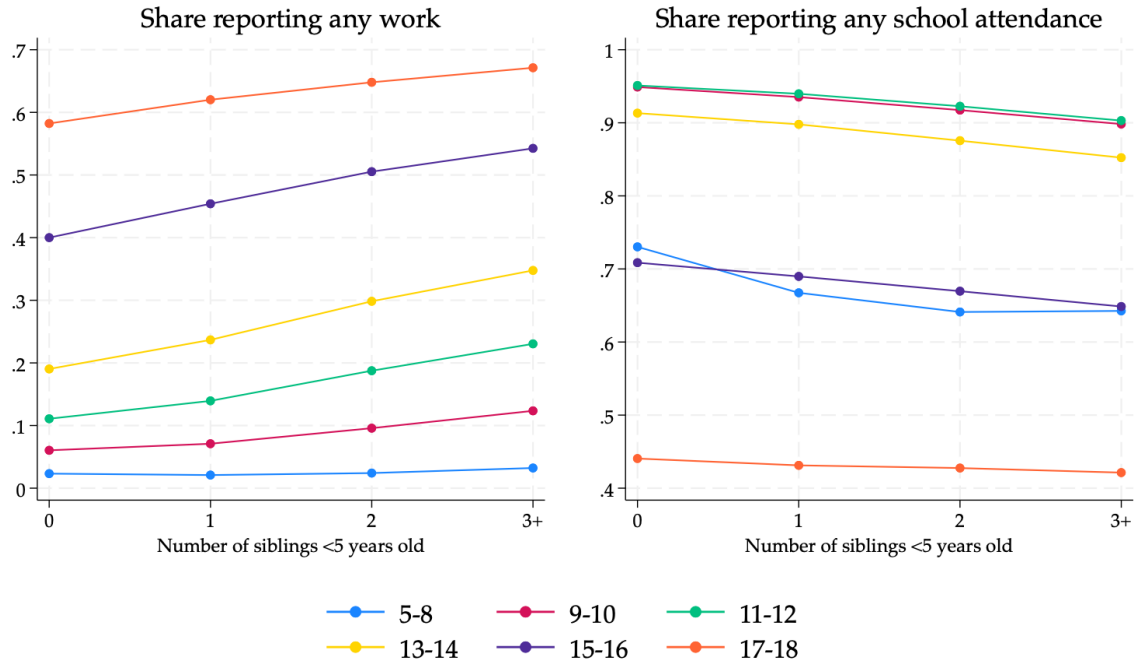
7 Main Figures and Tables

FIGURE 1: FERTILITY AND CHILDREN'S WORK/SCHOOLING 1910-1940



Notes: data from 1910, 1920, 1930, and 1940 full count census. Panel (a) reports various measures fertility: the ratio of the number of toddlers to the number of women, the number of toddlers to the number of moms, and the number of moms with toddlers to the number of women (i.e., the share of women who recently gave birth). Toddlers are defined as children aged 0–4. The sample of women is limited to those in their prime childbearing years, aged 18–33. Mothers are defined as the subset of these women with at least one child of any age living in the household. Panel (b) reports the share children who both attend school and work (*"school + work"*), only attend school (*"school only"*), and only work (*"work only"*) separately for ages 5-14 and 15-18. School attendance refers to any attendance at school over since the reference period, which shifts slightly across the years. A child is counted as working if they list an occupation. In Panel (b), the time series for young children stops in 1930 because occupation data was not collected in 1940 for children less than 14 years old.

FIGURE 2: SCHOOL, WORK, AND NUMBER OF YOUNG SIBLINGS AMONG CHILDREN AGED 5-18 IN 1910



Notes: data from 1910 full count census. This figure plots the share of children who report an occupation (left panel) or report attending school (right panel) against the number of siblings aged 0-4 in the household. Shares are calculated separately for children aged 5-8, 9-10, 11-12, 13-14, 15-16, and 17-18 as indicated by the legend.

TABLE 1: EFFECT OF CLINIC ACCESS ON FERTILITY

	(1)	(2)	(3)	(4)	(5)
	Children to women		Children to mothers		Mothers to women
	<5	all	<5	all	
Clinic opened	-0.010***	-0.016***	-0.010**	-0.009	-0.005***
	(0.003)	(0.006)	(0.004)	(0.006)	(0.002)
Obs.	102808	102808	102808	102808	102808
R-sq	0.956	0.965	0.950	0.956	0.942
County, Yr, State × Yr FE	x	x	x	x	x
Year × 1910 Cnty Charac.	x	x	x	x	x
Omitting 1940					
1910 Dep. Var. mean	0.574	1.069	1.240	2.296	0.457

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 county population. This table reports results from a regression of various measures of fertility on a dummy indicating a birth control clinic has opened in that county or a neighboring county. Fertility measures include the ratio of children to women (Columns 1-2), the ratio of children to moms (Columns 3-4), and the ratio of moms to women (Columns 5). Columns 1 and 3 count children aged 0-4, and Columns 2 and 4 count children aged 0-18. The sample of women is limited to those aged 18-33 and mothers are defined as the subset of these women with at least one child of any age in the household. Fixed effects for year, county, experiment, experiment by year, and experiment by county are included. Controls include the following 1910 county-level characteristics interacted with year dummies: share of households in urban areas, share of farm households, share of households living in a big city, ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county. These results, especially in columns 1-2, are similar to results in Bauernschuster et al. (2025) but presented here for context.

TABLE 2: EFFECT OF CLINIC ACCESS ON CHILDREN'S SCHOOLING AND WORK OUTCOMES

	(1) school only	(2) school & work	(3) work only	(4) no school or work
	Panel A: Children ages 15-18			
Clinic opened	0.019***	-0.001	-0.018***	0.001
	(0.004)	(0.002)	(0.003)	(0.002)
Obs.	102791	102791	102791	102791
R-sq	0.908	0.902	0.889	0.815
County, Yr, State × Yr FE	x	x	x	x
Year × 1910 Cnty Charac.	x	x	x	x
Omitting 1940				
1910 Dep. Var. mean	0.395	0.180	0.321	0.104
	Panel B: Children ages 5-14			
Clinic opened	0.026***	-0.003	-0.005***	-0.018***
	(0.007)	(0.003)	(0.001)	(0.006)
Obs.	77106	77106	77106	77106
R-sq	0.938	0.889	0.886	0.897
County, Yr, State × Yr FE	x	x	x	x
Year × 1910 Cnty Charac.	x	x	x	x
Omitting 1940	x	x	x	x
1910 Dep. Var. mean	0.768	0.063	0.019	0.151

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from a regression of various measures of schooling and work on a dummy indicating a birth control clinic has opened in that county or a neighboring county. Outcomes include the share of children who: attend school and do not work (Column 1); attend school and work (Column 2); work but do not attend school (Column 3); and neither attend school nor work (Column 4). Children are classified as working if they report an occupation. Panel A (Panel B) reports the results for children ages 15-18 (5-14). The work outcomes are missing for children less than 14 in 1940, hence Panel B drops 1940 from the analysis. Fixed effects for year, county, experiment, experiment by year, and experiment by county are included. Controls include the following 1910 county-level characteristics interacted with year dummies: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

TABLE 3: EFFECT OF CLINIC ACCESS ON CHILDREN'S LITERACY

	Share literate	
	(1)	(2)
	aged	*aged*
	10-14	*15-18*
Clinic opened	0.014**	0.009**
	(0.007)	(0.004)
Obs.	77090	77089
R-sq	0.962	0.969
County, Yr, State × Yr FE	x	x
Year × 1910 Cnty Charac.	x	x
Omitting 1940	x	x
1910 Dep. Var. mean	0.938	0.958

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from a regression of the share of children who are literate on a dummy indicating a birth control clinic has opened in that county or a neighboring county. Column 1 focuses on children aged 10-14 and Column 2 focuses on children aged 15-18. A child is considered literate if they can read and write in any language. Literacy was not collected in 1940 and hence 1940 is excluded from the analysis. Fixed effects for year, county, experiment, experiment by year, and experiment by county are included. Controls include the following 1910 county-level characteristics interacted with year dummies: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

TABLE 4: EFFECT OF CLINIC ACCESS ON LIKELIHOOD OF HAVING YOUNGER SIBLINGS

	(1) sh. with sibling(s) <5	(2) avg num. siblings <5
	Panel A: Children ages 15-18	
Clinic opened	-0.005**	-0.008***
	(0.002)	(0.003)
Obs.	102791	102791
R-sq	0.954	0.953
County, Yr, State × Yr FE	x	x
Year X 1910 Cnty Charac.	x	x
Omitting 1940		
1910 Dep. Var. mean	0.272	0.389
	Panel B: Children ages 5-14	
Clinic opened	-0.007***	-0.014***
	(0.002)	(0.005)
Obs.	102808	102808
R-sq	0.963	0.962
County, Yr, State × Yr FE	x	x
Year × 1910 Cnty Charac.	x	x
Omitting 1940		
1910 Dep. Var. mean	0.529	0.817

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from a regression of two measures of sibling presence on a dummy indicating a birth control clinic has opened in that county or a neighboring county. The outcome in Column 1 is the share of children with siblings aged 0-4 and the outcome in Column 2 is the average number of siblings aged 0-4 per child. Panel A (Panel B) reports the results for children ages 15-18 (5-14). Fixed effects for year, county, experiment, experiment by year, and experiment by county are included. Controls include the following 1910 county-level characteristics interacted with year dummies: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

TABLE 5: EFFECT OF CLINIC ACCESS ON CHILDREN'S FULL TIME SCHOOLING BY PRE-EXISTING PRESENCE OF SCHOOLING LAWS

	Dep. Var.: Share of children in school only		
Schooling law type:	(1) Compulsory Yrs	(2) Work Permit Yrs	(3) Cont. School
	Panel A: Children ages 15-18		
Clinic opened	0.036*	0.073***	0.021***
	(0.021)	(0.026)	(0.006)
Clinic opened × Schooling law in $k = -1$	-0.003	-0.008**	-0.011
	(0.003)	(0.003)	(0.008)
Obs.	102791	102791	102791
R-sq	0.911	0.911	0.910
County, Yr, State × Yr FE	x	x	x
Year × 1910 Cnty Charac.	x	x	x
Omitting 1940			
	Panel B: Children ages 5-14		
Clinic opened	0.066	0.044	0.032***
	(0.049)	(0.051)	(0.011)
Clinic opened × Schooling laws in $k = -1$	-0.006	-0.004	-0.024*
	(0.006)	(0.007)	(0.013)
Obs.	77106	77106	77106
R-sq	0.939	0.939	0.939
County, Yr, State × Yr FE	x	x	x
Year × 1910 Cnty Charac.	x	x	x
Omitting 1940	x	x	x

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from regressions of the share of children who attend school and do not work (i.e., those who report no occupation) on an indicator for the opening of a birth control clinic in the county (or a neighboring county) and its interaction with a measure of the presence or strength of various schooling laws in the year prior to treatment. Column 1 presents heterogeneity by the years of education mandated under compulsory schooling laws, Column 2 by the years of education required before a child may obtain a work permit, and Column 3 by the presence of continuation school mandates. Panel A (Panel B) reports the results for children ages 15-18 (5-14). The work outcomes are missing for children less than 14 in 1940, hence Panel B drops 1940 from the analysis. Fixed effects for year, county, experiment, experiment by year, experiment by county, and each of these fixed effects interacted with the schooling law variable are included. Controls include the following 1910 county-level characteristics interacted with year dummies and with the schooling law variable: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

Appendix

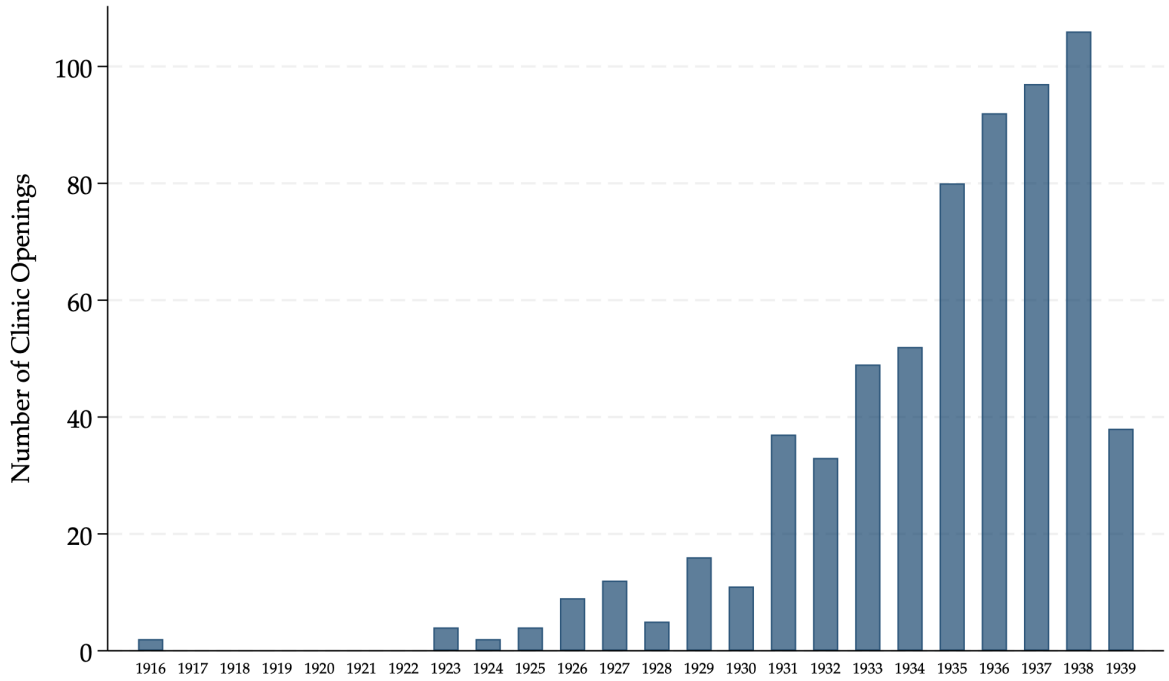
Appendix A Additional Tables and Figures

TABLE A.1: 1910 COUNTY CHARACTERISTICS BY CLINIC PRESENCE

	(1) Never clinic	(2) Ever clinic	(3) Diff.
Share urban households	0.114	0.228	0.114***
	(0.182)	(0.268)	(0.008)
Share farm households	0.567	0.459	-0.108***
	(0.195)	(0.226)	(0.008)
Share of households living in large city	0.001	0.029	0.029***
	(0.026)	(0.152)	(0.004)
Ratio of children <5 to women	1.034	0.951	-0.082***
	(0.194)	(0.216)	(0.008)
Share literate	0.887	0.890	0.004
	(0.118)	(0.103)	(0.004)
Share white	0.859	0.873	0.014*
	(0.219)	(0.193)	(0.008)
Share black	0.131	0.120	-0.011
	(0.217)	(0.193)	(0.008)
Share other race	0.010	0.007	-0.003
	(0.055)	(0.041)	(0.002)
Share foreign born	0.072	0.090	0.017***
	(0.089)	(0.098)	(0.003)
Observations	1,525	1,334	2,859

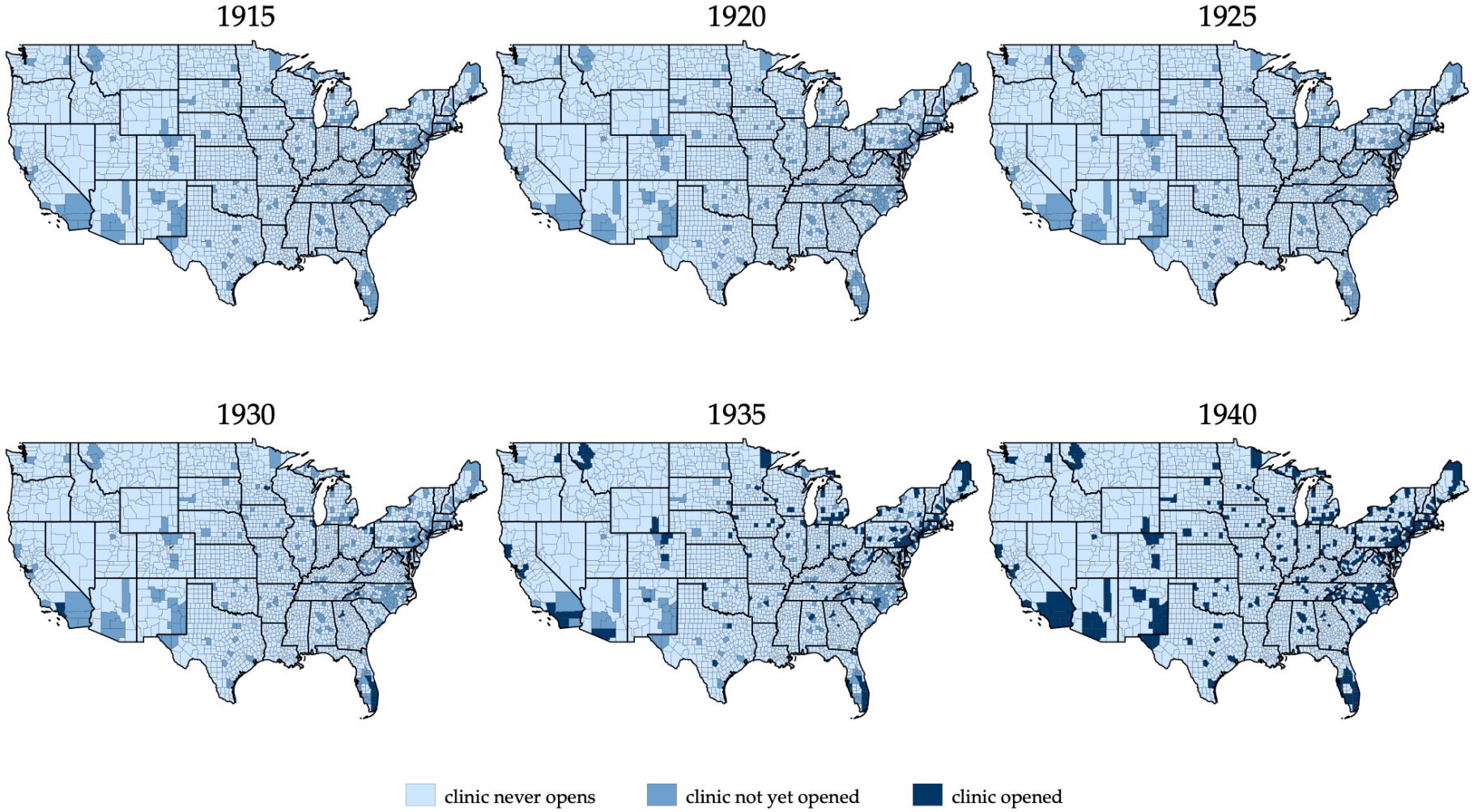
Notes: this table reports average 1910 county characteristics separately by counties that are never exposed to a clinic (Column 1) and those that are exposed at some point between 1910-1940 (Column 2). A county is considered exposed if a clinic opens either within the county or in a neighboring one. Column 3 reports the difference between Columns 1 and 2. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Standard deviations are reported in parentheses. This table is very similar to a table in Bauernschuster et al. (2025) but presented here for context.

FIGURE A.1: NUMBER OF CLINIC OPENINGS IN EACH YEAR



Notes: this figure depicts the number of clinics that opened in each year. This figure is very similar to a figure in Bauernschuster et al. (2025) but presented here for context.

FIGURE A.2: MAP OF CLINIC OPENINGS OVER TIME



Notes: these maps depict the rollout of clinics across counties between 1915-1940 in 5-year intervals. This figure is very similar to a figure in Bauernschuster et al. (2025) but presented here for context.

TABLE A.2: TOP OCCUPATIONS OF WORKING CHILDREN AGED 5–18 IN 1910

Panel A: Boys

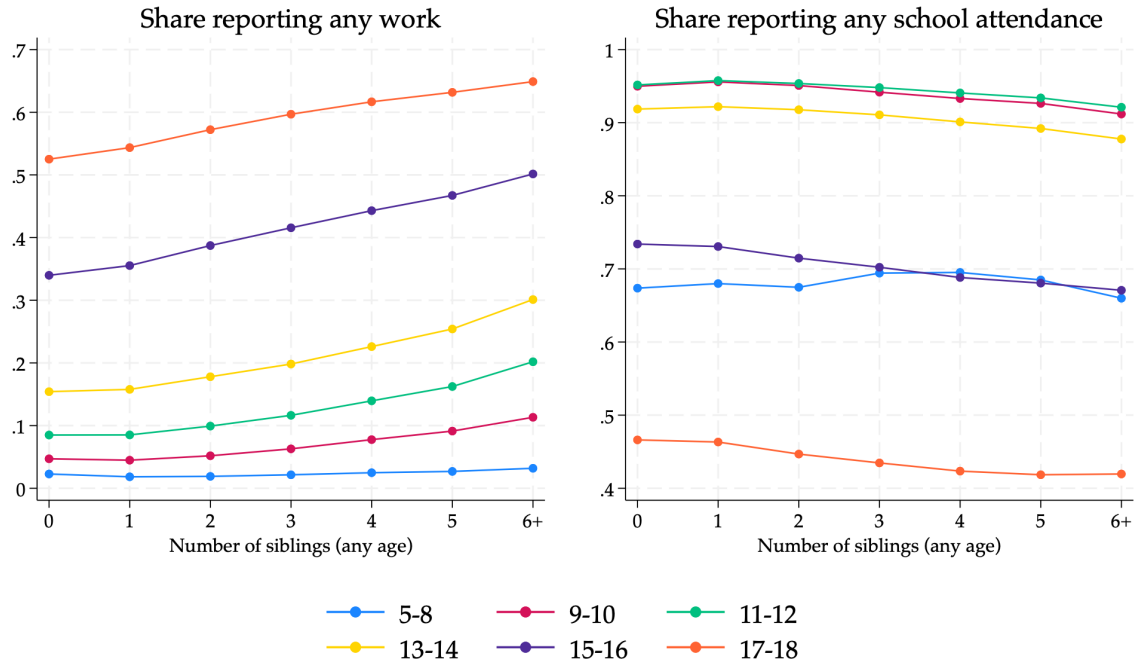
Occupation Title	Share of Workers
Farm laborers, wage workers	0.507
Laborers (n.e.c.)	0.098
Not yet classified	0.064
Operative and kindred workers (n.e.c.)	0.057
Farmers (owners and tenants)	0.035
Salesmen and sales clerks (n.e.c.)	0.029
Clerical and kindred workers (n.e.c.)	0.024
Messengers and office boys	0.019
Mine operatives and laborers	0.017
Deliverymen and routemen	0.010
Newsboys	0.007

Panel B: Girls

Occupation Title	Share of Workers
Farm laborers, wage workers	0.370
Operative and kindred workers (n.e.c.)	0.153
Not yet classified	0.112
Private household workers (n.e.c.)	0.065
Salesmen and sales clerks (n.e.c.)	0.042
Laborers (n.e.c.)	0.030
Stenographers, typists, and secretaries	0.027
Dressmakers and seamstresses, except factory	0.016
Clerical and kindred workers (n.e.c.)	0.016
Telephone operators	0.014
Spinners, textile	0.013

Notes: data are from the 1910 full count census. This table reports the 11 most common occupations (1 of which is "not yet classified") among boys and girls who report an occupation. "n.e.c." indicates the type of work is not elsewhere classified (i.e., "laborers (n.e.c.)" excludes farm laborers, which is grouped into its own occupation). Sample is limited to children aged 5-18 who do not live in group quarters. Occupation titles are based on IPUMS's *occ1950* codes.

FIGURE A.3: SCHOOL, WORK, AND NUMBER OF SIBLINGS AMONG CHILDREN AGED 5-18 IN 1910



Notes: data from 1910 full count census. This figure plots the share of children who report an occupation (left panel) or report attending school (right panel) against the number of siblings (of any age) in the household. Shares are calculated separately for children aged 5-8, 9-10, 11-12, 13-14, 15-16, and 17-18 as indicated by the legend.

TABLE A.3: EFFECT OF CLINIC ACCESS ON CHILDREN'S SCHOOL AND WORK OUTCOMES BY PRE-EXISTING PRESENCE OF SCHOOLING LAWS – AGES 5–14

	school only	school & work	work only	no school or work
	Panel A: Compulsory Years			
Clinic opened	0.066	-0.030	0.003	-0.039
	(0.049)	(0.024)	(0.012)	(0.038)
Clinic opened × Compulsory Yrs	-0.006	0.004	-0.001	0.003
	(0.006)	(0.003)	(0.002)	(0.005)
Obs.	77106	77106	77106	77106
R-sq	0.939	0.892	0.889	0.900
	Panel B: Work Permit Years			
Clinic opened	0.044	-0.025	-0.013	-0.006
	(0.051)	(0.024)	(0.012)	(0.040)
Clinic opened × Work Permit Yrs	-0.004	0.003	0.001	-0.000
	(0.007)	(0.003)	(0.002)	(0.005)
Obs.	77106	77106	77106	77106
R-sq	0.939	0.892	0.889	0.899
	Panel C: Continuation School			
Clinic opened	0.032***	-0.002	-0.007***	-0.023**
	(0.011)	(0.004)	(0.002)	(0.010)
Clinic opened × Cont. School	-0.024*	-0.001	0.006***	0.019*
	(0.013)	(0.006)	(0.002)	(0.011)
Obs.	77106	77106	77106	77106
R-sq	0.939	0.891	0.890	0.899

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from regressions of various measures of schooling and work among 5-14 year olds on an indicator for the opening of a birth control clinic in the county (or a neighboring county) and its interaction with a measure of the presence or strength of various schooling laws in the year prior to treatment. Outcomes include the share of children who: attend school and do not work (Column 1); attend school and work (Column 2); work but do not attend school (Column 3); and neither attend school nor work (Column 4). Children are classified as working if they report an occupation. Panel A presents heterogeneity by the years of education mandated under compulsory schooling laws, Panel B by the years of education required before a child may obtain a work permit, and Panel C by the presence of continuation school mandates. Fixed effects for year, county, experiment, experiment by year, experiment by county, and each of these fixed effects interacted with the schooling law variable are included. Controls include the following 1910 county-level characteristics interacted with year dummies and with the schooling law variable: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

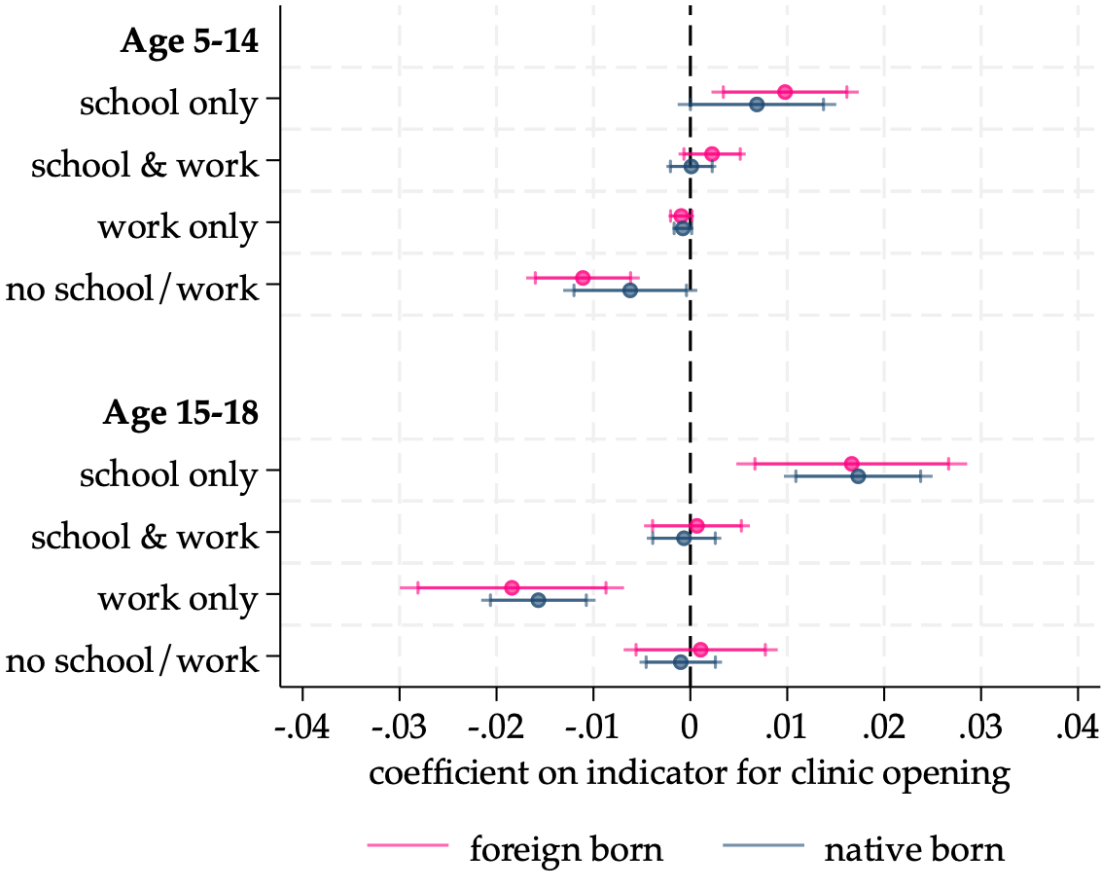
TABLE A.4: EFFECT OF CLINIC ACCESS ON CHILDREN'S SCHOOL AND WORK OUTCOMES BY PRE-EXISTING PRESENCE OF SCHOOLING LAWS – AGES 15–18

	school only	school & work	work only	no school or work
	Panel A: Compulsory Years			
Clinic opened	0.036*	-0.014	-0.019	-0.004
	(0.021)	(0.011)	(0.019)	(0.013)
Clinic opened × Compulsory Yrs	-0.003	0.002	0.001	0.000
	(0.003)	(0.001)	(0.002)	(0.002)
Obs.	102791	102791	102791	102791
R-sq	0.911	0.904	0.893	0.820
	Panel B: Work Permit Years			
Clinic opened	0.073***	-0.026*	-0.052**	0.004
	(0.026)	(0.015)	(0.024)	(0.016)
Clinic opened × Work Permit Yrs	-0.008**	0.003*	0.005	-0.000
	(0.003)	(0.002)	(0.003)	(0.002)
Obs.	102791	102791	102791	102791
R-sq	0.911	0.905	0.893	0.818
	Panel C: Continuation School			
Clinic opened	0.021***	-0.003	-0.021***	0.003
	(0.006)	(0.003)	(0.005)	(0.003)
Clinic opened × Cont. School	-0.011	0.007*	0.009	-0.004
	(0.008)	(0.004)	(0.006)	(0.004)
Obs.	102791	102791	102791	102791
R-sq	0.910	0.904	0.892	0.818

Notes: observations are at the county by year by experiment level. Observations are weighted by 1910 total population. This table reports results from regressions of various measures of schooling and work among 15-18 year olds on an indicator for the opening of a birth control clinic in the county (or a neighboring county) and its interaction with a measure of the presence or strength of various schooling laws in the year prior to treatment. Outcomes include the share of children who: attend school and do not work (Column 1); attend school and work (Column 2); work but do not attend school (Column 3); and neither attend school nor work (Column 4). Children are classified as working if they report an occupation. Panel A presents heterogeneity by the years of education mandated under compulsory schooling laws, Panel B by the years of education required before a child may obtain a work permit, and Panel C by the presence of continuation school mandates. Fixed effects for year, county, experiment, experiment by year, experiment by county, and each of these fixed effects interacted with the schooling law variable are included. Controls include the following 1910 county-level characteristics interacted with year dummies and with the schooling law variable: ratio of children under 5 to women, share of adults who are literate, share white, share black, share other race, and share foreign born. Standard errors are robust and clustered on county.

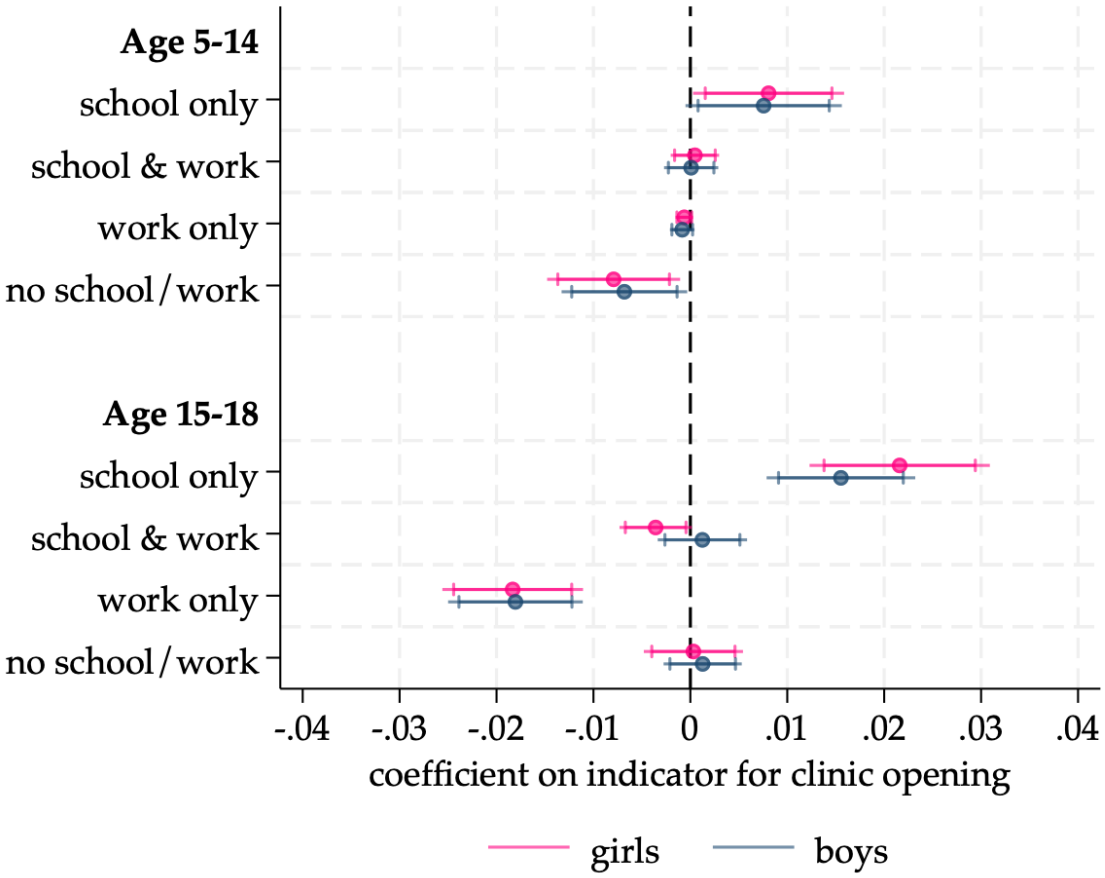
Appendix A.1 Appendix

FIGURE A.4: EFFECT OF CLINIC ACCESS ON SCHOOLING AND WORK AMONG CHILDREN WITH FOREIGN VS. NATIVE BORN MOTHERS



Notes: this figure reports the effect of clinic access on children's schooling and work outcomes estimated separately by the mother's nativity status. The estimate plotted represents the coefficient on a dummy indicating the county was exposed to a clinic (β_1 from Equation 1).

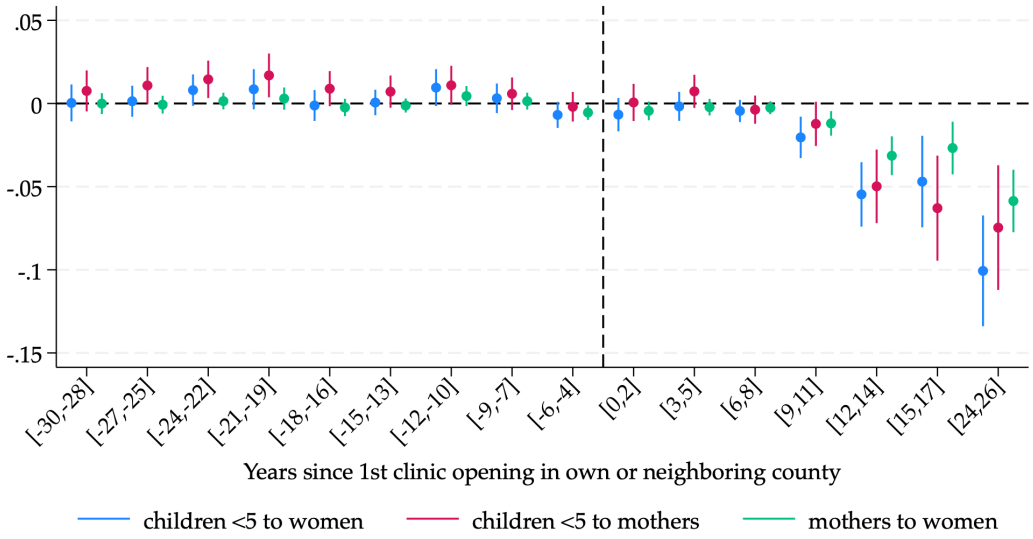
FIGURE A.5: EFFECT OF CLINIC ACCESS ON SCHOOLING AND WORK AMONG GIRLS AND BOYS



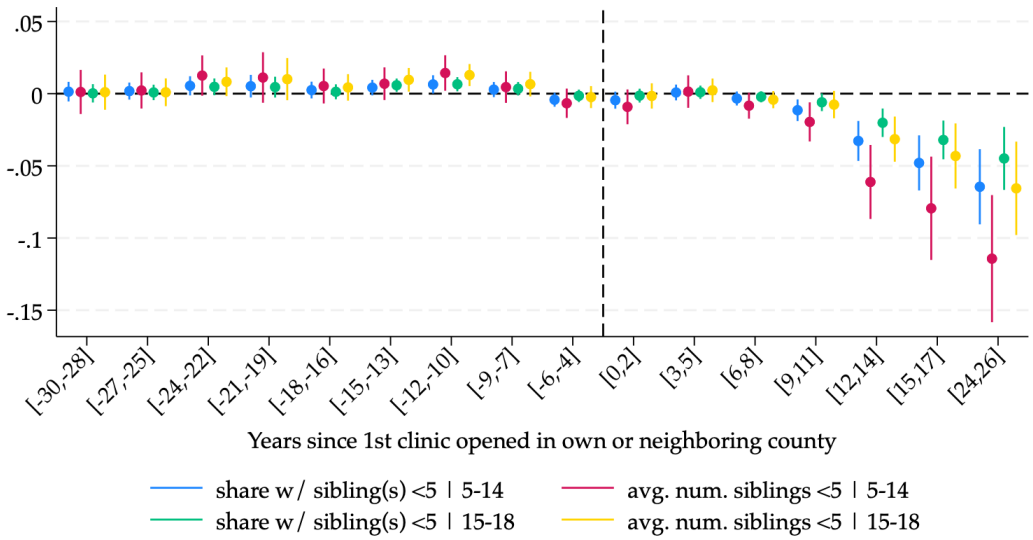
Notes: this figure reports the effect of clinic access on children's schooling and work outcomes estimated separately by child gender. The estimate plotted represents the coefficient on a dummy indicating the county was exposed to a clinic (β_1 from Equation 1).

FIGURE A.6: EFFECT OF CLINIC ACCESS ON FERTILITY AND LIKELIHOOD OF HAVING YOUNGER SIBLINGS

(A) WOMEN'S FERTILITY



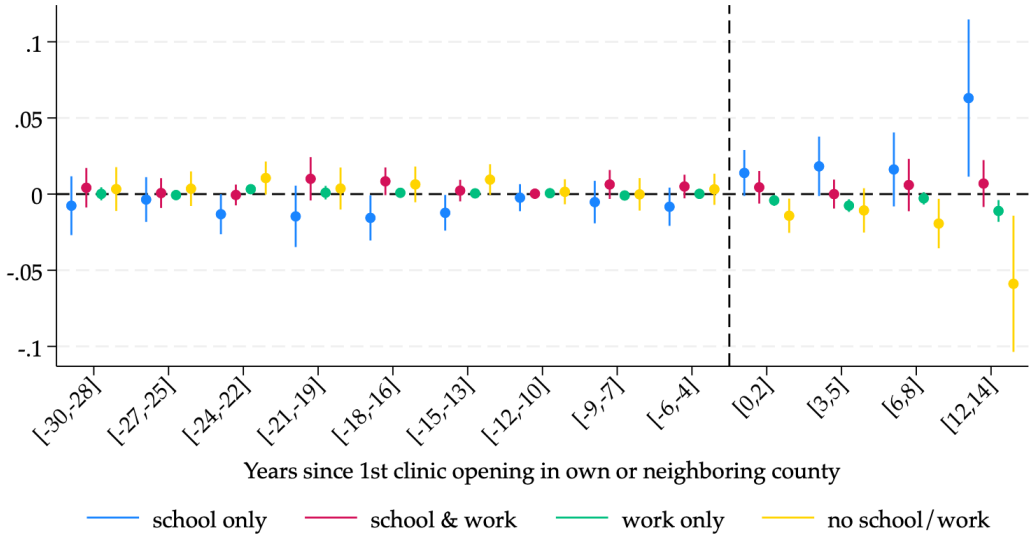
(B) PRESENCE OF YOUNGER SIBLING AMONG CHILDREN



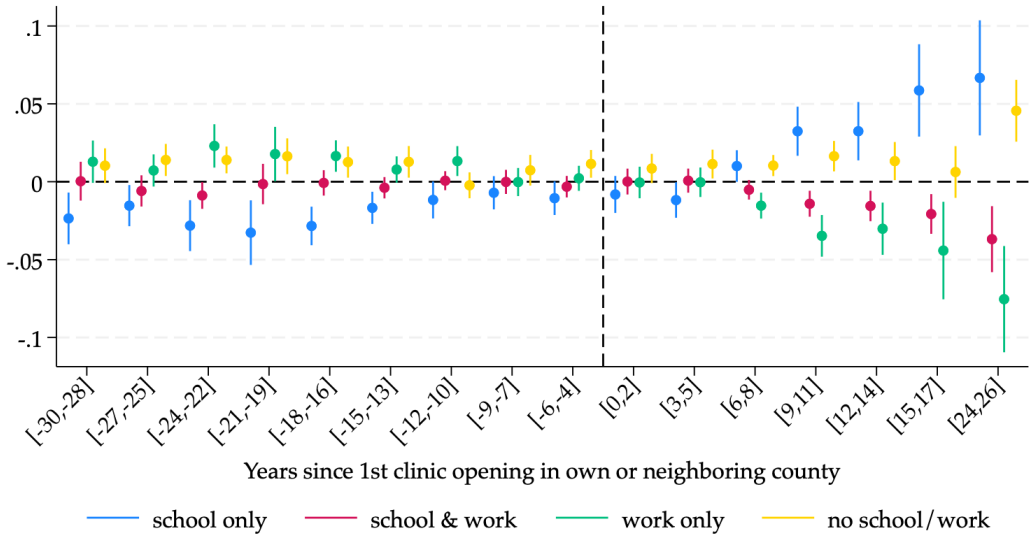
Notes: these figures presents event studies of clinic openings for the main fertility measures. The estimates plotted represent the coefficient on a dummy indicating the number of years since the county was exposed to a clinic (μ_k from Equation 2). Panel (A) reports the results for fertility, with the outcomes defined the same as in Table 1. Panel (B) reports the results for the prevalence of young siblings among children aged 5-14 or 15-18, where the outcomes are defined the same as in Table 4. The results in Panel (A) similar to results in Bauernschuster et al. (2025) but presented here for context.

FIGURE A.7: EFFECT OF CLINIC ACCESS ON CHILDREN'S SCHOOLING AND WORK OUTCOMES

(A) CHILDREN AGED 5-14



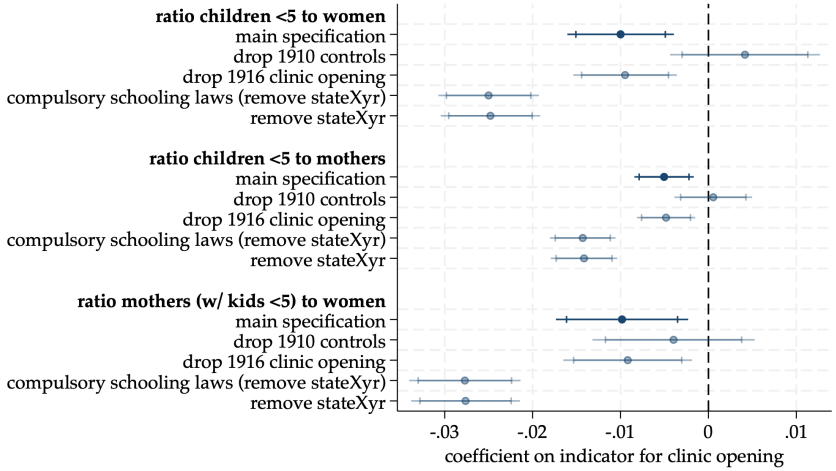
(B) CHILDREN AGED 15-18



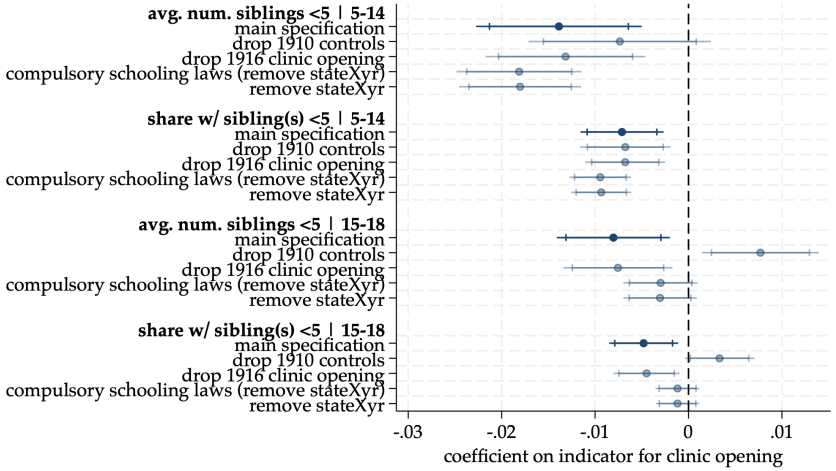
Notes: these figures presents event studies of clinic openings for children's schooling and work outcomes. The estimates plotted represent the coefficient on a dummy indicating the number of years since the county was exposed to a clinic (μ_k from Equation 2). Panel (A) reports the results for children aged 5-14 and Panel (B) reports the results for children aged 15-18. Outcomes are defined the same way as in Table 2. Panel (A) omits 1940.

FIGURE A.8: ROBUSTNESS CHECKS

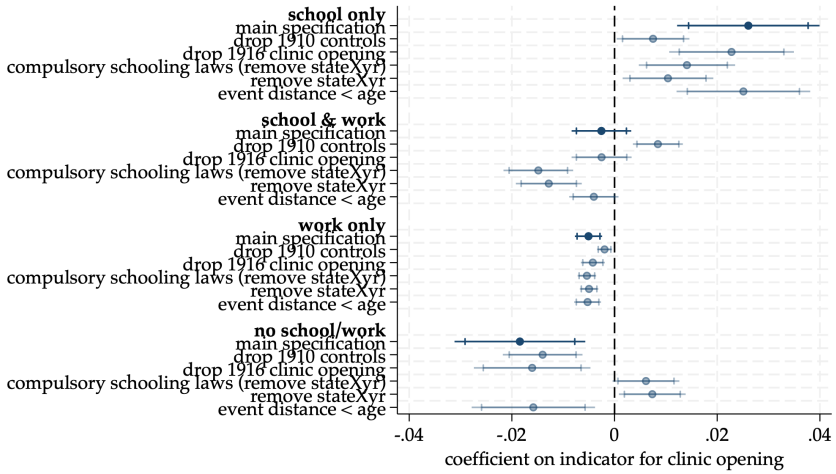
(A) FERTILITY OUTCOMES



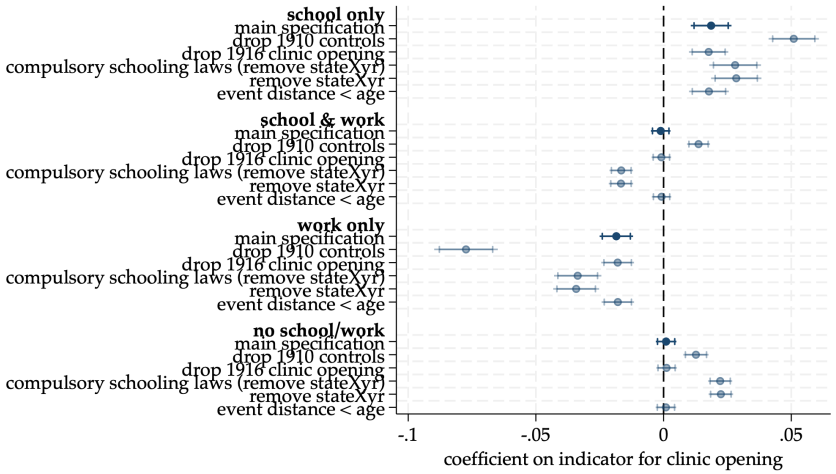
(B) SIBLING OUTCOMES



(C) CHILDREN'S SCHOOLING/WORK OUTCOMES (5-14 YRS)



(D) CHILDREN'S SCHOOLING/WORK OUTCOMES (15-18 YRS)



Notes: these figures report the effect of clinic access on the main outcomes estimated across various robustness specifications. Each estimate plotted represents the coefficient on a dummy indicating the county was exposed to a clinic (β_1 from Equation 1). Within each figure, estimates are grouped by outcome, with outcome labels shown in boldface. For each outcome, the main specification is listed first as a benchmark. In order, the subsequent robustness specifications include: dropping the control set of 1910 county characteristics interacted with year dummies; dropping the 1916 clinic opening (i.e., dropping all treatment and control observations for which $g = 1916$), removing state by year fixed effects while including controls for state-level compulsory schooling and child labor laws, removing state by year fixed effects, and removing observations for which the children represented in the outcome variable would have been born after their mother had access to a clinic.