Kindergartens and Fertility: Evidence from the first publicly funded kindergartens in the United States*

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Abstract

We examine the impact of the first publicly funded kindergartens on fertility in the late 19th century US exploiting a policy experiment conducted in St. Louis. Following mothers with 5–6 year old children at the time of the kindergarten openings, we find that mothers whose children attended kindergarten reduced the number of children ever born but not the number of surviving children. We show that a reduction in child mortality can explain this result. While access to kindergarten education was not a significant factor in the historical fertility transition, it played a relevant role in saving children's lives.

*We thank Dan Aaronson, Doug Almond, Sonja Bhalotra, Hoyt Bleakley, Leah Boustan, Davide Cantoni, Liz Cascio, Karen Clay, Carl-Johan Dalgaard, Matthias Doepke, Kathrine Eriksson, James Feigenbaum, James Fenske, Joe Ferrie, Oded Galor, Paola Giuliano, Claudia Goldin, Walker Hanlon, Casper Worm Hansen, Lauren Hoehn-Velasco, Dan Hungerman, Markus Lampe, Lars Lønstrup, Bob Margo, Bhash Mazumder, Kris Mitchener, Petra Moser, Nathan Nunn, Sebastian Oettinger, Alex Roehrkasse, David Weil, Ludger Woessmann, and seminar and workshop participants for their helpful comments and suggestions on earlier versions of the paper (this article replaces our previous version with the title "Froebel Gifts: How the Kindergarten Movement changed the American Family").

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1 Introduction

This paper studies how access to kindergarten education influences fertility decisions. Education is widely believed to have a critical impact on fertility and increased demand for human capital is seen as an important driver of the transition to a low-fertility regime (e.g., McCrary and Royer, 2011; Galor, 2012; Aaronson et al., 2014). Economic models of fertility suggest various mechanisms through which the provision of kindergarten education can affect the fertility decisions of mothers (Doepke et al., 2023). Access to kindergarten education can reduce the opportunity cost of having children, thereby increasing mothers' fertility and labor supply. On the other hand, parents would reduce fertility if kindergarten education increased their children's educational returns, as predicted by a quantity-quality trade-off model (Becker and Lewis, 1973). Kindergarten teachers can also provide mothers with valuable advice about parenting practices, nutrition, and hygiene that impact children's health and fertility decisions (Grossman, 1972). Ultimately, assessing the effects of access to kindergarten education on fertility is an empirical question.

We examine the short- and long-term effects of the first public kindergarten program in the US on fertility. Kindergartens were part of the expansion of the US public school system towards the end of the 19th century when women started to have fewer children (Bailey and Hershbein, 2018). The number of cities with publicly sponsored kindergartens increased from one in 1873 with 68 children enrolled to 867 in 1912 with more than 300,000 children enrolled (U.S. Bureau of Education, 1914).[1] Our focus is on St. Louis (MO), where the first large-scale involvement of a public school system in kindergarten education began in 1873. The St. Louis public kindergarten program provides a quasi-natural experiment to investigate the effect of the roll-out of kindergartens on fertility, since the first public kindergarten started, literally, as an experiment to study *"the practical e ects"* of kindergartens (St. Louis Board of Education, 1875, p.195). We geo-referenced the exact location of kindergartens that opened in St. Louis during the 1870s and combined this information with complete-count census records and newly publicly available crosswalks that systematically link *women* across Censuses. With these data at hand, we can study the short and long-run fertility responses of women living in St. Louis at the time when the kindergarten program was introduced.

For the short-run analysis, we use an event-study design proposed by Sun and Abraham (2021) that exploits the staggered roll-out in kindergarten openings across St. Louis' enumeration districts. We find that women in treated districts gradually reduced fertility after a kindergarten opening, while there were no fertility differences between treated and

[1] Influential educational reformers at that time saw in kindergarten education a tool to prepare children for elementary school and to promote social integration of immigrant children (Klein, 1992; Beatty, 1995).

untreated women before the event. Importantly, this result holds also when using an alternative identification strategy based on street proximity to public schools. This approach deals with the issue that households who live closer to schools might have different preferences for children and education. Moreover, our results are robust to accounting for the expansion of the public school system in St. Louis.

Next, we examine whether the short-term decline in fertility is due to a delay in births or whether women decided to have fewer children. We link women with a 5–6 year old child in St. Louis in 1880 to the 1900 US Census, which contains information on completed fertility. Specifically, we test whether kindergarten exposure had an impact on the number of children ever born (gross fertility) and the number of surviving children (net fertility) in 1900 by comparing mothers with a 5–6 year old child who attended/not attended kindergarten in 1880. Our estimation strategy only compares mothers in the treatment and the control group *within* the same enumeration district of St. Louis in 1880 and we further include a wide set of individual-level and household-level controls to reduce the concern of selection of children into kindergartens. We find that women whose children attended kindergarten in 1880 had significantly fewer children ever born but no fewer surviving children in 1900, which suggests that the optimal number of children was not different for households whose children attended kindergarten in St. Louis. These findings are not driven by differences in maternal labor supply or a delay in marriage.

As a final step, we investigate a possible mechanism that can explain the different results for gross and net fertility. We find that reduction in child mortality for the sample of mothers with a 5–6 year old child in 1880 attending a kindergarten can explain such a difference. This result is entirely consistent with the actual functioning of kindergartens in St. Louis as kindergarten teachers were expected to regularly meet with the mothers of enrolled children to explain the benefits of kindergarten education and to lecture them about hygiene, nutrition, and child-rearing (Klein, 1992). Quantitatively, the estimates are sizable as women with a child attending kindergarten in 1880 had 0.12 fewer dead children (a 6.3 percent reduction of the sample mean). Our findings are not driven by higher school attendance rates of children in kindergarten households or major public health interventions. We also obtained similar results using a similar estimation strategy for a linked sample of women living in large U.S. cities at the beginning of the 20th century, when many kindergartens were opening, which strengthens the external validity of the St. Louis case study.

Our paper is the first to evaluate the effects of the first publicly funded kindergarten program in US history on fertility during the historical demographic transition.[2] Demographers and economists argue that economic incentives played an important role in explaining

[2]See Bailey and Hershbein (2018) for a recent overview of the history of childbearing in the US.

the historical fertility transition (Guinnane, 2011).[3] Those include increases in the direct and indirect costs of having children, such as declines in the value of child labor and increased labor market opportunities for women. Proponents of unified growth theory have emphasized the role of human capital in the fertility decline during the second phase of the industrial revolution (Galor, 2012).[4] Our findings suggest that access to kindergarten education removed important information frictions about child-rearing practices which reduced gross fertility, but the negligible effect on the number of surviving children suggests that access to kindergarten was not a major driver of the historical fertility decline in the US. This result is consistent with the theoretical predictions made by Galor (2012) and empirical evidence by Ager et al. (2018), that declines in infant and child mortality did not play an important role in the historical fertility transition.

Our work relates also to a large modern literature on the impact of early childhood education programs in the US (e.g., Duncan and Magnuson, 2013; Elango et al., 2015; Cascio, 2021). There is ample evidence of how these programs affected parental outcomes. Much of this work focuses on evaluating whether early childhood education programs influence maternal labor supply and parenting practices. Overall, the modern literature finds generally small effects on maternal employment (Gelbach, 2002; Cascio, 2009) and we also rule out that this channel is driving our fertility results.[5] On the other hand, our finding that access to kindergarten education reduces child mortality speaks to studies showing that children's participation in early childhood education programs can have a positive impact on parenting practices, e.g., via parent-teacher interactions (Gelber and Isen, 2013; Barr and Gibbs, 2022).

2 Historical Background

2.1 The Origins of Kindergarten Education in the United States

The idea of a kindergarten goes back to the educational reformer Friedrich Wilhelm August Froebel (1782–1852), who founded "an institution for the education of little children" in Bad Blankenburg (Germany) in 1837. Froebel realized that the first years of a child's life were the most important for their future development. His teaching methods focused on children's

[3] Recent studies also emphasize changes in cultural and social norms as important determinants for the historical fertility decline (e.g. Spolaore and Wacziarg, 2022; Beach and Hanlon, 2023).

[4] Several empirical studies found a negative link between education and fertility during the historical fertility transition (e.g. Becker et al., 2010; Bleakley and Lange, 2009).

[5] This is not surprising, since the labor force participation rate of white married women in the US at the end of the 19th century stood at less than 5 percent (Goldin, 1977), and it is consistent with Aaronson et al. (2021), who show that there was no systematic relationship between fertility and female labor supply in the US before WWI.

interests and needs. Froebel developed specially designed educational toys ("gifts"), prescribed activities ("occupations"), games, and songs to stimulate the manual and cognitive abilities of little children. Froebel's kindergarten concept represented a compromise between family-based and fully institutionalized child-rearing (Lascarides and Hinitz, 2013).

Margarethe Schurz, a German immigrant, brought Froebel's ideas to the US, when she opened the first kindergarten in Watertown, Wisconsin, in 1856.[6] A few other German-speaking kindergarten pioneers followed Schurz to the US and set up more kindergartens and trained instructors according to Froebelian principles in the 1860s.[7] But it wasn't until the 1870s that the kindergarten movement began to gain popularity in the United States. The year 1873 marked the birth of public kindergartens in the US, when William T. Harris, the school superintendent in St. Louis (1868-1880) and later U.S. Commissioner of Education (1889-1906), initiated a publicly funded kindergarten education program within the St. Louis' public school system.

Around the same time, several philanthropists and charitable societies funded kindergarten associations throughout the US to offer tuition-free kindergarten classes for children from poor households (Vandewalker, 1923). Starting in the late 1880s, these tuition-free kindergartens were integrated into the public school system and the number of cities with publicly sponsored kindergartens increased from 137 in 1892 to 867 in 1912. By 1910, almost 90% of kindergartens were publicly funded and 85% of the enrolled children attended a public kindergarten (U.S. Bureau of Education, 1914). Overall, the number of kindergartens increased from 42 with 1,252 enrolled pupils in 1873 (the first year of official kindergarten statistics) to 7,557 kindergartens with 353,546 pupils enrolled in 1912—an enrollment rate of approximately 9% (U.S. Bureau of Education, 1914).

Kindergarten services were offered on weekdays for 3-4 hours per day and targeted mainly 5–6 year old children. One or two kindergarten teachers usually instructed, on average, 25 children per room for about three hours. The kindergarten curriculum largely followed Froebel's teaching principles. The U.S. Bureau of Education (1914, p. 10) describes the mission of the kindergarten "*[. . .] as a mediating element, in which it is sought to provide for the children of the people the best kind of nurturing and scientific care, to give them the best kind of physical, mental, social, and spiritual training*" which aimed at preparing children for primary school. Beyond caring for the well-being of children, kindergarten teachers aimed to build up a relationship with the mothers by arranging home visits and mothers' meetings.[8] They educated mothers about hygiene, nutrition, and parenting practices that would affect

[6] See Ager and Cinnirella (2021) for a more detailed historical background on kindergartens in the US.

[7] During the early phase of the movement, kindergartens were mainly tuition-based (Beatty, 1995).

[8] These meetings became later an integral part of the public school system and are considered forerunners of the modern Parent Teacher Associations (Klein, 1992).

fertility decisions and the health of their children. These personal interactions were invaluable, especially for immigrant mothers. Historians provide ample anecdotal evidence that kindergarten teachers aimed to integrate immigrant mothers into society by teaching them to emulate the domestic life of middle-class American women (Klein, 1992; Berg, 2004).

2.2 The Kindergarten Movement in St. Louis

In 1873, the first public kindergarten in the US opened at the Des Peres School in St. Louis. This kindergarten started literally as an experiment to study *"the practical e ects of Froebel's system"* (St. Louis Board of Education, 1874, p. 195). After the experience was deemed successful *"beyond expectations"*, in the next year *"it was resolved to try the experiment in two schools near the centre of the town"* (St. Louis Board of Education, 1876, p. 95). By 1875, kindergarten education was already offered in seven schools with about 450 pupils regularly attending (St. Louis Board of Education, 1876, p. 98). To finance the expansion of the kindergarten system, a quarterly fee of one dollar was charged, except from the indigent, starting in the school year of 1876–77. Charges were dropped again in 1878 (Troen, 1972). The Board of Education ended the experimental stage of the kindergartens in 1878 and integrated them permanently into the public school system (Troen, 1972). Despite not being mandatory, enrollment increased from 68 pupils in 1873 to 7,828 children in 1880 (St. Louis Board of Education, 1881, pp. 152-53). By this time, most schools were already involved in kindergarten activities and by 1886 more than 50 kindergartens (all public) operated within the city limits of St. Louis. Figure A.1 illustrates the roll-out of kindergartens in St. Louis from 1873 until 1880.

The establishment of a public kindergarten system in St. Louis was a major step toward the universal acceptance of kindergartens in the US. Other school superintendents regarded St. Louis as a role model for operating and managing public kindergartens, and people trained in St. Louis introduced or supervised the work in public kindergartens that opened in other American cities over the next decades. St. Louis demonstrated that kindergartens can be successfully integrated into the public school system and provides an interesting case in point to study the effect that the roll-out of kindergartens had on fertility (Vandewalker, 1923; Troen, 1972; Lascarides and Hinitz, 2013).

3 Data

Our empirical analysis draws on three data sources: (i) a series of official education reports from St. Louis that contain detailed information about kindergartens; (ii) a digitized collection of the historical complete-count census records provided by IPUMS (Ruggles et al., 2023); and (iii) newly publicly available crosswalks of individuals that are linked across Censuses from the Census Tree Project (Price et al., 2021; Buckles et al., 2023).

From the annual reports of the St. Louis public school board, we collected information about the exact location and opening date of every public kindergarten that operated in St. Louis for the years 1873 to 1886. We geo-referenced the locations of the kindergartens operating in St. Louis between 1873 and 1886 and assigned them to their corresponding enumeration district (those are comparable in population size to census tracts) in the 1880 Census. IPUMS also includes the exact geo-referenced location of households for St. Louis in 1880.[9] Figure A.2 depicts the location of public kindergartens together with geo-referenced households and enumeration districts in St. Louis as reported in the 1880 Census.

For the short-run analysis, we include information from the full count U.S. Census data about fertility and other socioeconomic characteristics at the individual level. Our sample includes every white woman aged 18-44 listed as a household head or spouse in St. Louis in 1880. Since the Census reports the age of every enumerated person and lists every child in a household together with the household head (and spouse), we can reconstruct the fertility history of every woman in the sample and compile a quasi "mother panel". To avoid potential issues associated with children leaving the parents' household, our panel only includes children up to the age of 15 in 1880. We further require women to be at least 18 at the time when they were having a child. We then obtain the cumulative fertility history by calculating the number of children before 1870 and subsequently adding the births between 1870 and 1880 for every woman in the sample.[10] Hence, the "mother panel" contains, for each year between 1870 and 1880, the cumulative number of births per woman.

For the long-run analysis, we use a measure of completed fertility—the Census collected in 1900 information on the number of children ever born and the number of children that were still alive on the Census day of ***ever-married*** women (age 12 and older)—as our outcome variable. We follow a sample of 25–44 years old white women ***who had a 5–6 year old child*** in the 1880 Census in St. Louis to 1900.[11] This linked sample of women is constructed from the publicly available crosswalk files provided by the Census Tree (CT) Project (https:

[9]We thank Adele Heagney from the St. Louis Public Library for helping us geolocating the St. Louis kindergartens (the kindergarten at Lowell school is the only one that we could not locate). We used the website https://stevemorse.org/census/unified.html?year=1880 to geolocate every kindergarten to its corresponding enumeration district. Note that a kindergarten can border with multiple enumeration districts depending on the exact location of the kindergarten.

[10]We calculated the existing number of children before 1870 by subtracting the total number of births between 1870 and 1880 from the number of own children a woman reported in the 1880 census. We only consider own children in the household.

[11]Close to 90 percent of women with a 5–6 year old child in St. Louis in 1880 were between age 25 and 44. Of those women, 95% were already married (or widowed) in 1880.

//www.censustree.org/). The CT project contains publicly available crosswalks, including systematic links for women, between decennial censuses based on the 1850-1940 complete count US census records provided by IPUMS.[12] The quality of the CT links is high and they were independently verified (Buckles et al., 2023). The sample for the long-term analysis includes approximately 7,000 women (a match rate of 58.9%). To address concerns about the external validity of our results, we also constructed a linked sample based on the CT crosswalks for the census years 1900 and 1910 (most cities opened kindergartens between 1890 and 1910).[13] As in St. Louis, our sample includes white women aged 25–44 who lived in any city with 25,000+ inhabitants in 1900 with a 5–6 year old child. This linked sample includes about 575,000 women in 606 cities.

4 Estimation Strategy

Our goal is to estimate the causal effect of access to kindergarten education on maternal fertility when kindergartens were established (short-run), and to estimate the effect of having a child in kindergarten on completed fertility (long-run).

For the short-run analysis, we use the quasi "mother panel" to evaluate whether women adjusted their fertility behavior after having access to a kindergarten in their enumeration district. To test whether this is the case, we use a difference-in-differences design which exploits the fact that kindergartens operated in different enumeration districts at different points in time. One potential threat to identification would be if the fertility pattern in treated enumeration districts had already evolved differently before the kindergarten opened. Since we know the exact establishment date of every kindergarten in the sample, we can conduct an event study to observe the dynamic effects of kindergarten openings on fertility and, at the same time, test whether the coefficient of interest indicates any violation of the parallel trends assumption before treatment occurs.[14]

More formally, we use the following event study design to evaluate the dynamic effects of public kindergarten openings on fertility:

$$y_{iet} = \alpha_i + \alpha_t + \sum_{j \;\; T} \beta_j Kindergarten_{et}^{\tau+j} + X_{iet}\Gamma + \;{}_{iet} \quad (1)$$

[12]Compared to other existing publicly available crosswalks (the Census Linking Project (CLP) and the IPUMS Multigenerational Longitudinal Panel project (MLP)) the CT project achieves a substantially higher match rate and includes systematic links for women over time (Buckles et al., 2023).

[13]We cannot use the US census of 1890 since it was lost due to a fire and we cannot use 1920 to keep the same spacing between linked censuses, since 1910 was the last historical US census with systematic information on the number of surviving children and children ever born.

[14]While the parallel trends assumption is not directly testable, the absence of pre-trends before the kindergarten opening would suggest that the identifying assumption is reasonable.

where T = {-4, . . . , -2, 0, . . . , 4}. We omit j = -1 (the base year) such that the post-treatment effects are relative to the year before the kindergarten opening in enumeration district e. The outcome variable, y_{iet}, denotes the total number of children of woman i residing in enumeration district e in year t. The parameter τ refers to the year of a kindergarten opening in enumeration district e.[15] Kindergarten$_{et}^{\tau+j}$ is an indicator equal to 1 when t = τ + j and 0 otherwise. In order to capture the fertility response four and more years prior (after) the kindergarten opening, we define an indicator $Kindergarten_{et}^{\tau+-4} = 1$ if t $\leq \tau$ - 4 ($Kindergarten_{et}^{\tau+-4} = 1$ if t $\geq \tau + 4$) and 0 otherwise.

The estimated coefficients β_j trace out the dynamic effects of the kindergarten roll-out on fertility. The set of controls, X_{iet}, contains fixed effects for women's age and the years since an enumeration district had access to a district school. We further control for individual fixed effects, α_i, which account for unobserved time-invariant heterogeneity across women, such as cultural traits or preferences for child quality which tend to be slow-moving over time, and year-fixed effects, α_t, which account for year-specific shocks common to all women in the sample. Standard errors are clustered at the enumeration district level to account for correlations within an enumeration district in a given year and over time.

Recent literature in econometrics has shown that staggered treatment can lead to misleading estimates of standard two-way fixed-effect (TWFE) models due to heterogeneous treatment effects and negative weighting. Therefore, we adopt the interaction-weighted estimator proposed by Sun and Abraham (2021) which estimates the underlying weights that depend only on the distribution of cohorts and the relative time indicators. In particular, we define the women experiencing the opening of a kindergarten strictly after 1880 (the census year) as the control group of never-treated women (the next kindergarten opened in 1885).

The long-run analysis is based on the linked sample of women described in Section 3. Our estimation strategy exploits differences in completed fertility between mothers with a 5–6 year old child in 1880 who attended/not attended kindergarten. Table A.1 compares women in our matched sample to those in the 1880 Census who cannot be matched forward. Women in the linked sample were slightly younger, 3 percentage points less likely to be foreign-born, almost 4 percentage points less likely to be the household head, about 3 percentage points more literate, they had slightly more children (4.3 vs 4), their husbands were more likely to work in a white-collar occupation (about 6 percentage points), and they had a slightly higher household income. To improve external validity to the full population of women in St. Louis who had a 5–6 year old child in 1880, our main results are reweighted by these baseline

[15] For our estimation approach, we need to assume that the household location observed in 1880 remained the same in the whole period under consideration. This is a reasonable assumption given the relatively short time window considered.

characteristics. Columns 4-6 in Table A.1 demonstrate that the re-weighting procedure substantially balances the matched and unmatched women on observable attributes.[16] We also report unweighted results as a robustness check.

Moreover, the richness of the 1880 Census data allows us to control for a wide range of observable household characteristics and enumeration district fixed effects to account for unobservable neighborhood characteristics. At first, we examine whether the sample in 1880 is balanced between mothers whose children attended kindergarten (treatment group) and those who did not (control group). Table A.2 reports differences in means between the treatment and the control group for the linked sample of women ($N = 7,247$). There are some differences between the treatment and the control group. Mothers whose children attend kindergarten are slightly older (age 33.7 vs 33.2), more literate (96.1% vs 95%), they and their spouses are more likely to be of foreign origin (64% vs 59%; 79.5% vs 73.6%), and they have slightly more children (4.5 vs 4.2), but there are no significant differences in the likelihood that the mother works, her husband works in a more skilled occupation, and in household income (measured by the occupational income score). As a result of these imbalances, we only compare mothers in the treatment and the control group within the same enumeration district and we further include individual-level and household-level controls to reduce the concern of selection of children into kindergartens.

The relationship between completed fertility and mothers whose 5–6 year old child attended kindergarten in St. Louis in 1880 can be estimated as follows:

$$y_{i,1900} = \alpha_{e,1880} + \beta Attend_{i,1880} + X_{i,1880}\Gamma + u_{i,1900} \quad (2)$$

where $y_{i,1900}$ denotes the outcome of interest (children ever born, surviving children) for woman i in the year 1900. The variable of interest, $Attend_{i,1880}$, is a binary variable for whether the 5–6 year old child of woman i attended a kindergarten in St. Louis in 1880. Hence, the coefficient β can be interpreted as an estimate of the impact of kindergarten attendance on the fertility behavior of women who had a 5–6 year old child in 1880. Equation (2) further includes fixed effects for women's enumeration districts ($\alpha_{e,1880}$) in 1880 and a set of baseline controls, $X_{i,1880}$, including fixed effects for woman i's age in 1880, birthplace, the birthplace of her parents, and the number of children in the household in 1880. The enumeration district fixed effects account for any neighborhood characteristics in 1880 that could directly affect the fertility behavior of women. To further address concerns about potential omitted variable bias, we control in extended specifications for woman i's literacy

[16] Coefficients in columns 4-6 are weighted by the propensity of being matched $P_i(M_i = 1|X_i)$, which is calculated from a probit of match status on the covariates (e.g., age). Observations are reweighted by $(1 - P_i(M_i = 1|X_i))/P_i(M_i = 1|X_i) \times q/(1 - q)$, where q is the proportion of records linked.

and work status, the duration of the current marital status, household income (measured by the occupational income score), the skill level of the husband's occupation (indicators for whether he worked in 1880 in a white-collar or blue-collar skilled occupation) and fixed effects for his birthplace.[17] Standard errors are clustered at the enumeration district level.

5 Main Results

5.1 Short-Run Results

We begin our analysis by assessing whether the opening of kindergartens had any short-term impact on fertility. Figure 1, panel (a), reports the estimated coefficients of the relative time to a kindergarten opening based on equation (1).[18] For the periods before a kindergarten opening, we find that the estimated coefficients are very close to zero, which supports the parallel trends assumption. After treatment, the estimated coefficients become negative and statistically significant four years after the opening of a kindergarten, implying that the establishment of kindergartens caused a decline in fertility. This decline occurred gradually, which is reasonable as it takes time until mothers fully internalize the benefits of kindergarten education for their children. Moreover, the effect of a kindergarten opening after four years is economically relevant as it explains about 14 percent of the fertility variation within women in our sample.

One potential concern is that households with stronger preferences for education could have lived near kindergartens, thus leading to selection bias. We believe that selection into districts with kindergartens is not a threat to our identification: First, kindergartens operated in school buildings and in estimating equation (1) we already control for the number of years since an enumeration district had access to public schools. Second, it is also unlikely that households with stronger preferences for kindergarten education would select into districts with kindergartens since the practical effects of kindergarten education were literally unknown at that time.

Furthermore, we present a different estimation approach using proximity to the closest public school as a treatment criterion. To do so, we only include households within a 1,000-meter radius of a public school in the sample. Within this sample, households are considered as treated in year t if they were living within 250 meters of a public school with an *active* kindergarten.[19] Figure A.3 illustrates the identification strategy based on a household's

[17] The skill classification of occupations follows Katz and Margo (2014).

[18] We report the coefficients for the whole sample of women while the corresponding estimates are reported in column (1) of Table A.3.

[19] In the context of the interaction weighted estimator of Sun and Abraham (2021), we define as control

distance to a public school. The estimates of the specification with proximity to a school as treatment are shown in Figure 1, panel (b). The pattern of the fertility decline is consistent with the one shown for the baseline estimates in panel (a). If anything, when using proximity as treatment we observe a decline in fertility which is statistically more significant. Overall, our results suggest that mothers significantly reduced fertility in the short term in response to kindergarten openings.

As a plausibility check, we also show in Figure A.4 that kindergarten openings led to increased kindergarten attendance. For this exercise, we keep the sample of mothers but we only include those with a 5–6 year old child in 1880. The outcome is a dummy variable for whether their 5–6 year old child attended "school" in 1880.[20] Hence, differently from the previous specifications, we cannot include fixed effects for years and individuals as we have only cross-sectional variation. The estimates show that kindergarten attendance increased significantly in the first year after the kindergarten opening and remained constant over the successive years. The point estimates are statistically significant at the 5-percent level. The establishment of a kindergarten in an enumeration district increased attendance between 14-19 percentage points compared to enumeration districts without a kindergarten. Relative to a mean of 32 percent, the estimated effect is substantial. We also find a fertility decline similar to Figure 1 for the sample of women with a 5–6 year old child in 1880 (Figure A.5).

5.2 Long-Run Results

Next, we evaluate whether the observed fertility decline associated with the opening of kindergartens is long-lasting. To do so, we keep the sample of women with a 5–6 year old child in St. Louis in 1880 that can be linked to 1900. Table 1 reports whether kindergartens had a long-term impact on completed fertility. The estimates are based on estimating equation (2) and the estimation method is weighted least squares. Panel A summarizes the results on the number of children ever born (gross fertility). The first column of Table 1 only includes the variable of interest, $Attend_{i,1880}$, and the following individual baseline controls: fixed effects for the mother's age, her birthplace, the birthplace of her parents, and the number of children in 1880. In column (2), we add fixed effects for 167 enumeration districts to account for unobserved neighborhood characteristics. This ensures that we only compare the fertility behavior among mothers with similar social backgrounds. In column (3) we further add controls for the household's income, whether the mother worked and was literate, and her husband's characteristics (age, birthplace, type of occupation) in 1880 including the

group women treated after 1880 or more than 250 meters away from a school.

[20] Since elementary school in St. Louis began at age 7, "attending school" at age 5–6 captures kindergarten attendance.

duration of the marriage (measured in 1900).[21] In columns (4) and (5), we split the sample by parentage (native vs. foreign-born).

The results in Panel A show that women whose children attended kindergarten in 1880 had significantly fewer children ever born. The estimated coefficient on $Attend_{i,1880}$ is statistically significant at the 5% level. Importantly, including fixed effects for enumeration districts (columns 2-5), and additional mother and household characteristics (columns 3-5) cannot explain the result away. This suggests that neighborhood effects or differences in income as well as mother's literacy and work status are unlikely to account for the negative association between kindergarten attendance and gross fertility. The estimated coefficient remains statistically significant at the 5% level and similar in size. Using our most restrictive specification (column 3) as benchmark, the estimated coefficient implies that having a child attending kindergarten in 1880 resulted in 0.183 fewer children compared to women whose child did not attend kindergarten in 1880; or a -2.7 percent reduction of the sample mean (6.8 children). A quantitatively modest but noticeable decline. This result is driven by immigrant mothers—the main target group of St. Louis' kindergartens (column 5).

Did the lower number of children ever born also mean that these women had fewer surviving children (net fertility)? While the estimated coefficient on $Attend_{i,1880}$ in Panel B of Table A is negative in all columns, it is about 1/3 of the size of the estimated coefficient reported in Panel A and always insignificant. This implies that mothers with a child attending kindergarten in 1880 did not adjust the optimal number of children compared to women whose children did not attend kindergarten in 1880.

A substantial reduction in child mortality can potentially explain the decline in gross fertility. The historical narrative suggests that kindergarten teachers educated mothers about hygiene, nutrition, and parenting practices, which we would expect to affect child mortality. The results reported in Panel C of Table 1 suggest that this was the case. Using our benchmark specification (column 3), the estimated coefficient implies that having a child attending kindergarten in 1880 resulted in 0.12 fewer dead children; or a 6.3 percent reduction of the sample mean (1.9 dead children)—a quantitatively sizable result. The decline in child mortality is also exclusively driven by immigrant mothers (column 5). It seems unlikely that the reduction in child mortality was driven by general improvements in public health that would have affected mothers of native and foreign parentage alike. Sanitary interventions in St. Louis, such as the chlorination and filtration of water, were introduced during the 1910s, and the city adopted bacteriological standards for milk only in the 1920s (Anderson et al., 2022). Moreover, the inclusion of enumeration district fixed effects accounts for the concern that the mortality environment for children was, on average, different across neighborhoods.

[21] The duration of the marriage was not asked in the 1880 Census.

We further report a series of robustness checks in Table A.4, which are extensions of our baseline specification (column 3 of Table 1). First, column 1 shows that our results on fertility and child mortality are not driven by a higher school attendance rate of children (aged 7-15 in 1880) in kindergarten households. Second, column 2 reports a specification that includes street fixed effects. In this specification, we only compare fertility differences of mothers with 5–6 year old children who lived in the same enumeration district and ***on the same street*** in 1880. Columns 3-6 further show that our results do not depend on weights, or on removing matches outside a 5-year or 1-year birth year window, and using the MLP linking method instead of all the links provided by the CT project. Overall, we find robust evidence that mothers of kindergarten children reduced gross fertility but not net fertility. This result was mostly driven by a reduction in child mortality.

How generalizable are our findings? The public kindergarten system of St. Louis served as a role model for other cities that established kindergartens between 1880 and 1910. But do we observe a similar fertility and child mortality pattern in other US cities? To answer this question, we use the city sample (25,000+ inhabitants in 1900) that contains women with a 5–6 year old child in 1900 linked to 1910. The estimates are based on estimating equation (2) and the estimation method is weighted least squares.[22] Standard errors are clustered at the city level.

The results for the city sample presented in Table 2 are very similar to those reported in Table 1 suggesting St. Louis was not a special case in this regard. For the number of children ever born (Panel A), the estimated coefficient on $Attend_{i,1900}$ is negative and statistically significant at the 1% level and similar in size irrespective of the set of control variables added. Quantitatively, the 2.4 percent reduction of the sample mean is similar to what we observed in St. Louis. However, the point estimates on $Attend_{i,1900}$ for the number of surviving children (Panel B) are more precisely estimated. These are now statistically significant at the 1% level, but the effect sizes remain quantitatively small (about 1 percent of the sample mean). As in St. Louis, the decline in gross fertility is mostly driven by a substantial reduction in child mortality. The link between kindergarten attendance and child mortality is statistically significant at the 1% level. The point estimate reported in column (3) implies that mothers whose children attended a kindergarten in 1900 had about 0.09 fewer dead children or a 7.6 percent reduction of the sample mean (1.1 dead children).[23]

[22]The time indices in equation (2) are replaced accordingly. We use the same re-weighting procedure as for the St. Louis sample (see Table A.5) and report for the linked sample a balance table by treatment status (i.e., whether a 5–6 year old child attends kindergarten) in Table A.6.

[23]Table A.7 presents the same robustness checks as Table A.4. We cannot show results including fixed effects for streets in the city sample, as this information is not available in the public use complete count samples from IPUMS.

6 Concluding Remarks

In this paper, we examined the impact of the first publicly funded kindergartens in the US on fertility. Using newly publicly available crosswalks that link women across historical Censuses, we show that at the turn of the 20th century, fewer children died in households whose children attended kindergarten. However, the number of surviving children did not increase, because households adjusted their gross fertility accordingly. Although households might have reduced net fertility to some extent, the effects are quantitatively small, which is consistent with the theoretical predictions of Doepke (2005) and Galor (2012) that reductions in infant and child mortality were not a major trigger for the historical fertility transition. However, our results suggest that the interplay between mothers and kindergarten teachers saved children's lives at a time when child mortality was still extraordinarily high.

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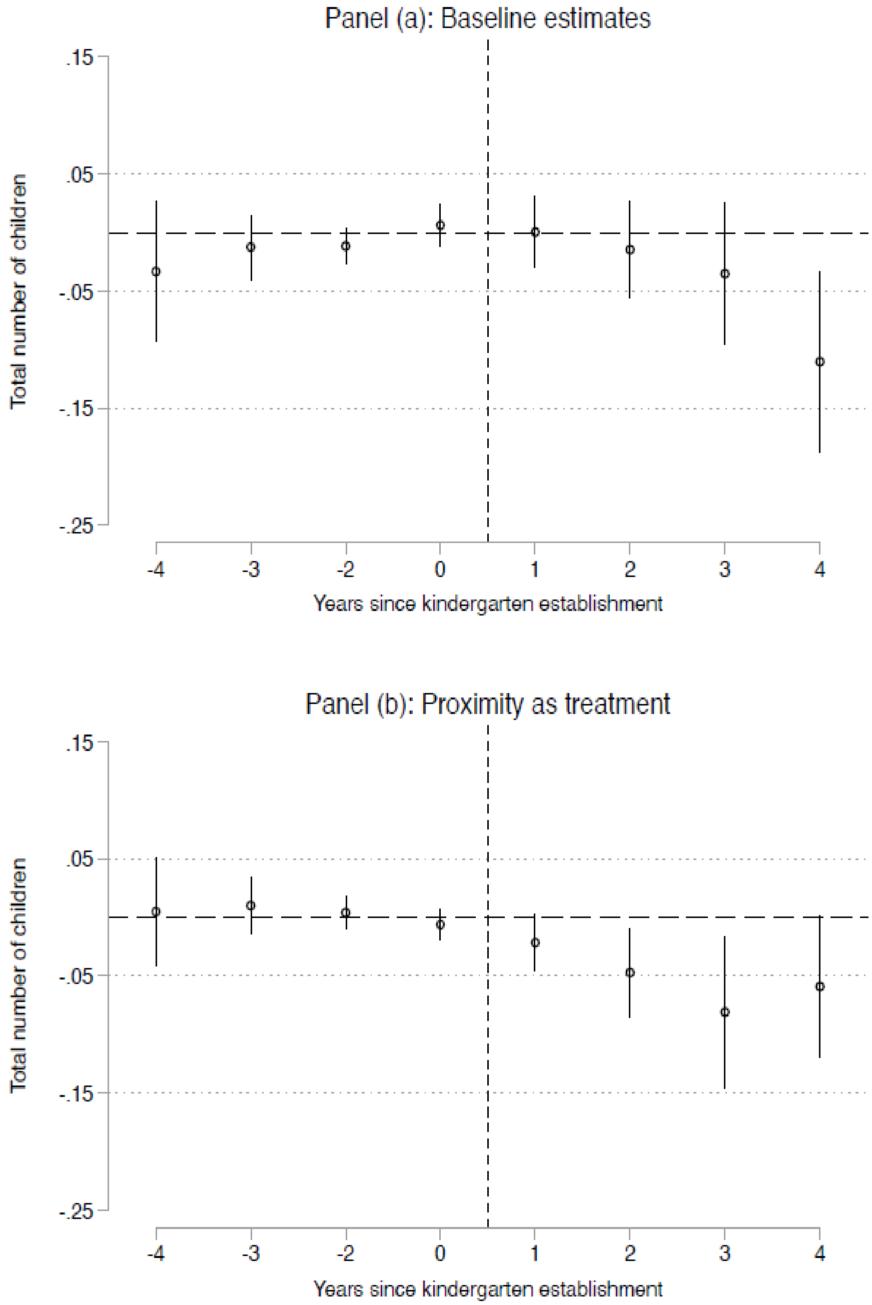
Tables and Figures

Figure 1: The Effect of the Kindergarten Roll-out on Fertility in St. Louis



Notes: This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis. Panel (a) reports results for the whole sample; Panel (b) for households within 1,000 meters of a public school. The x-axis measures the years since the kindergarten opened in enumeration district e. Estimated coefficients (dots) including 95 percent confidence intervals (solid lines) of kindergarten exposure on fertility are reported relative to the base year -1 (omitted). Standard errors are clustered at the enumeration district level.

Table 1: Kindergarten, Fertility and Child Mortality
St. Louis – Linked sample 1880-1900

	(1) all	(2) all	(3) all	(4) Natives	(5) Immigrants
Panel A			No. children ever born		
Attends	-0.186**	-0.188**	-0.183**	0.0164	-0.223**
	(0.0774)	(0.0802)	(0.0806)	(0.196)	(0.0875)
R-squared	0.257	0.283	0.347	0.597	0.320
Mean(Y)	6.781	6.781	6.779	5.745	6.997
Effect Size (%)	-2.737	-2.767	-2.702	0.286	-3.187
Panel B			No. surviving children		
Attends	-0.0667	-0.0723	-0.0624	-0.0253	-0.0737
	(0.0559)	(0.0556)	(0.0550)	(0.154)	(0.0584)
R-squared	0.344	0.364	0.417	0.652	0.398
Mean(Y)	4.864	4.864	4.864	4.314	4.980
Effect Size (%)	-1.372	-1.486	-1.283	-0.586	-1.479
Panel C			No. dead children		
Attends	-0.119**	-0.115**	-0.121**	0.0417	-0.149**
	(0.0493)	(0.0537)	(0.0565)	(0.148)	(0.0613)
R-squared	0.073	0.099	0.147	0.424	0.146
Mean(Y)	1.917	1.917	1.915	1.431	2.017
Effect Size (%)	-6.200	-6.016	-6.306	2.914	-7.403
Observations	7,183	7,183	7,162	1,186	5,895
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Enumeration FE	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes

Notes: This table reports the impact of kindergarten attendance on gross fertility (Panel A), net fertility (Panel B), and child mortality (Panel C). The sample includes white women aged 25-44 with a 5–6 year old child in St. Louis in 1880 that could be linked to 1900. Column (1) includes the following baseline control variables all measured in 1880: fixed effects for the mother's age, birthplace, parents' birthplaces, and the number of children. Column (2) adds fixed effects for enumeration districts. Column (3) further adds household income, fixed effects for the duration of the marriage (in 1900), the husband's age, his birthplace, the birthplaces of his parents, whether he worked in a white/blue collar skilled occupation, and indicators for the mother's literacy status, whether she worked, and whether she was the household head in 1900. Columns (4)-(5) split the sample by parentage. Standard errors (in parentheses) are clustered at the enumeration district. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

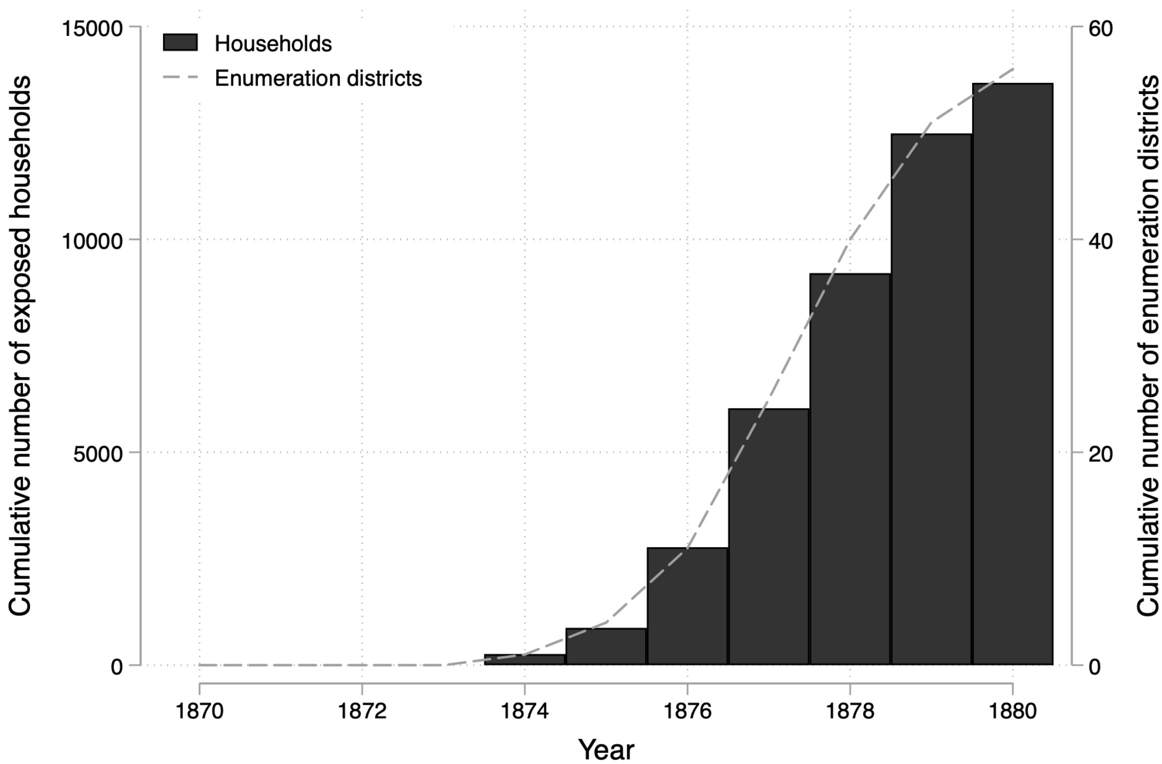
Table 2: Kindergarten, Fertility and Child Mortality
U.S. cities – Linked sample 1900-1910

	(1) all	(2) all	(3) all	(4) Natives	(5) Immigrants
Panel A			No. children ever born		
Attends	-0.149***	-0.148***	-0.136***	-0.0897***	-0.146***
	(0.00827)	(0.00677)	(0.00682)	(0.0105)	(0.00820)
R-squared	0.497	0.518	0.532	0.591	0.502
Mean(Y)	5.652	5.652	5.669	4.764	6.044
Effect Size (%)	-2.644	-2.620	-2.405	-1.883	-2.410
Panel B			No. surviving children		
Attends	-0.0631***	-0.0641***	-0.0493***	-0.0399***	-0.0497***
	(0.00420)	(0.00441)	(0.00461)	(0.00837)	(0.00561)
R-squared	0.595	0.608	0.616	0.661	0.595
Mean(Y)	4.511	4.511	4.528	3.901	4.787
Effect Size (%)	-1.398	-1.420	-1.088	-1.022	-1.038
Panel C			No. dead children		
Attends	-0.0864***	-0.0840***	-0.0870***	-0.0499***	-0.0960***
	(0.00649)	(0.00491)	(0.00490)	(0.00829)	(0.00581)
R-squared	0.107	0.142	0.177	0.205	0.178
Mean(Y)	1.141	1.141	1.142	0.863	1.257
Effect Size (%)	-7.572	-7.364	-7.625	-5.779	-7.637
Observations	576,966	576,904	559,625	163,444	395,280
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Enumeration FE	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes

Notes: This table reports the impact of kindergarten attendance on gross fertility (Panel A), net fertility (Panel B), and child mortality (Panel C). The sample includes white women aged 25-44 with a 5–6 year old child in U.S. cities in 1900 that could be linked to 1910. Column (1) includes the following baseline control variables all measured in 1900: fixed effects for cities, the mother's age, birthplace, parents' birthplaces, and the number of children. Column (2) adds fixed effects for enumeration districts. Column (3) further adds household income, fixed effects for the duration of the marriage, the husband's age, his birthplace, whether he worked in a white/blue collar skilled occupation, and indicators for the mother's literacy status, whether she worked, and whether she was the household head in 1900. Columns (4)-(5) split the sample by parentage. Standard errors (in parentheses) are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

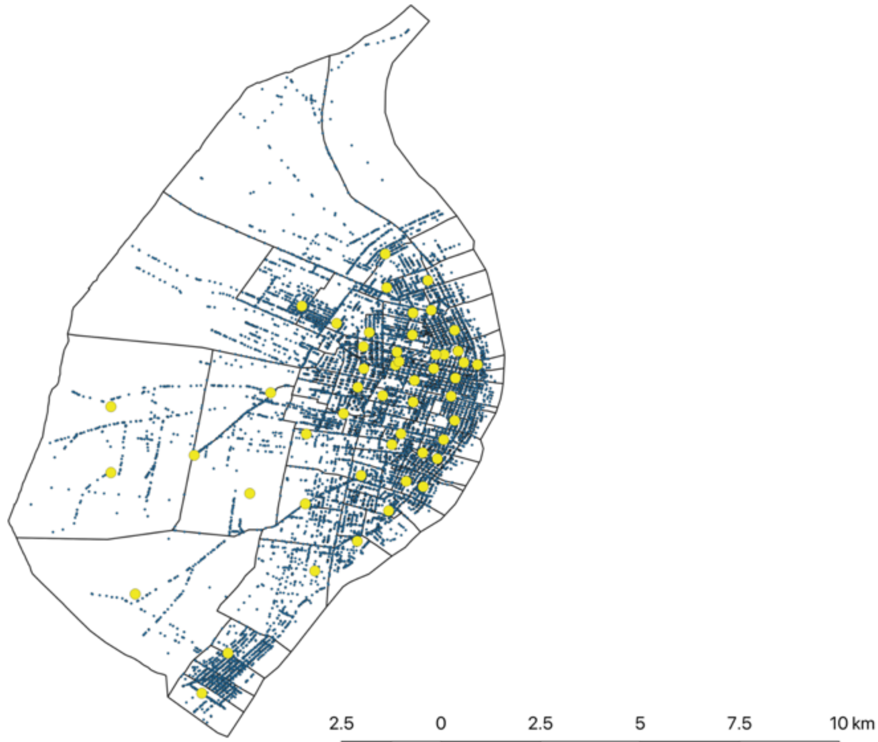
Online Appendix

Figure A.1: The Roll-out of Kindergartens in St. Louis 1873-1880



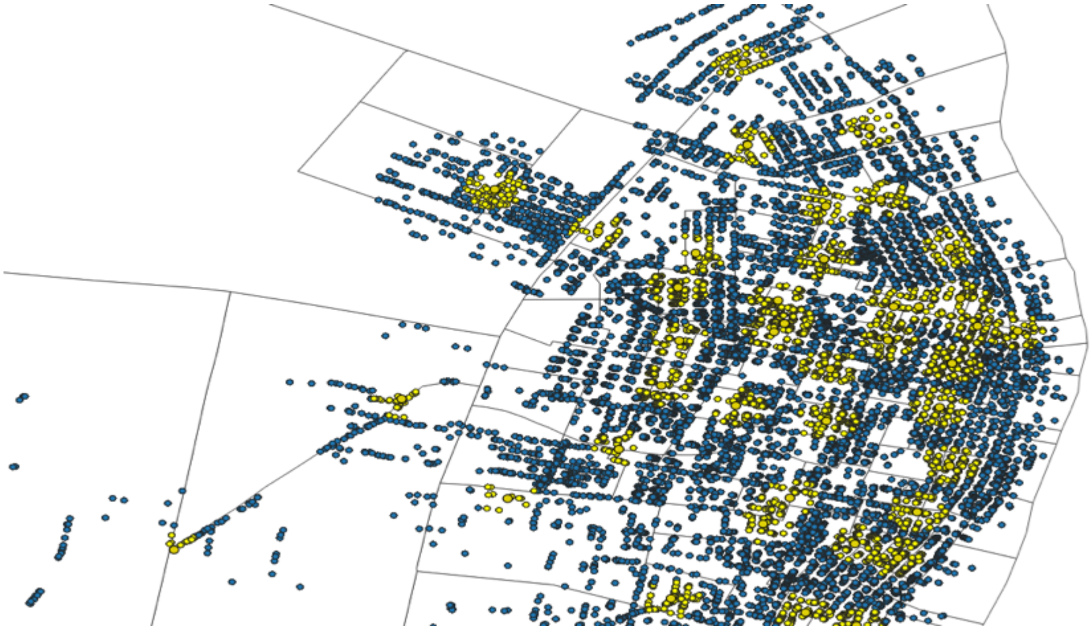
Notes: This figure displays on the left y-axis (right y-axis) the cumulative number of households (enumeration districts) exposed to a kindergarten in St. Louis between 1870 and 1880.

Figure A.2: Location of Public Kindergartens and Households in St. Louis 1880



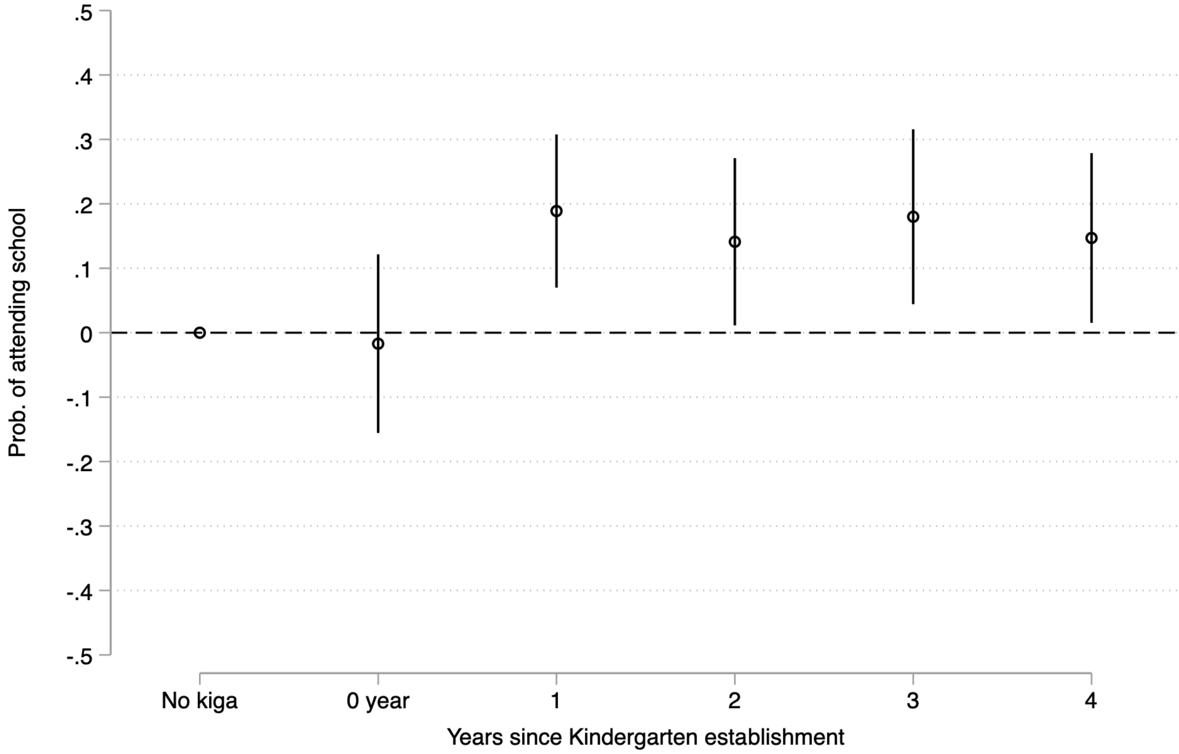
Notes: This map displays households (blue dots) together with the 1880 enumeration districts (gray lines) of St. Louis (see the Urban Transition Historical GIS project for further details). The kindergarten locations in 1886 (yellow dots) are based on the historical map of St. Louis in 1882 (https://collections.leventhalmap.org).

Figure A.3: Map of Kindergartens and Households in St. Louis using Proximity as Treatment



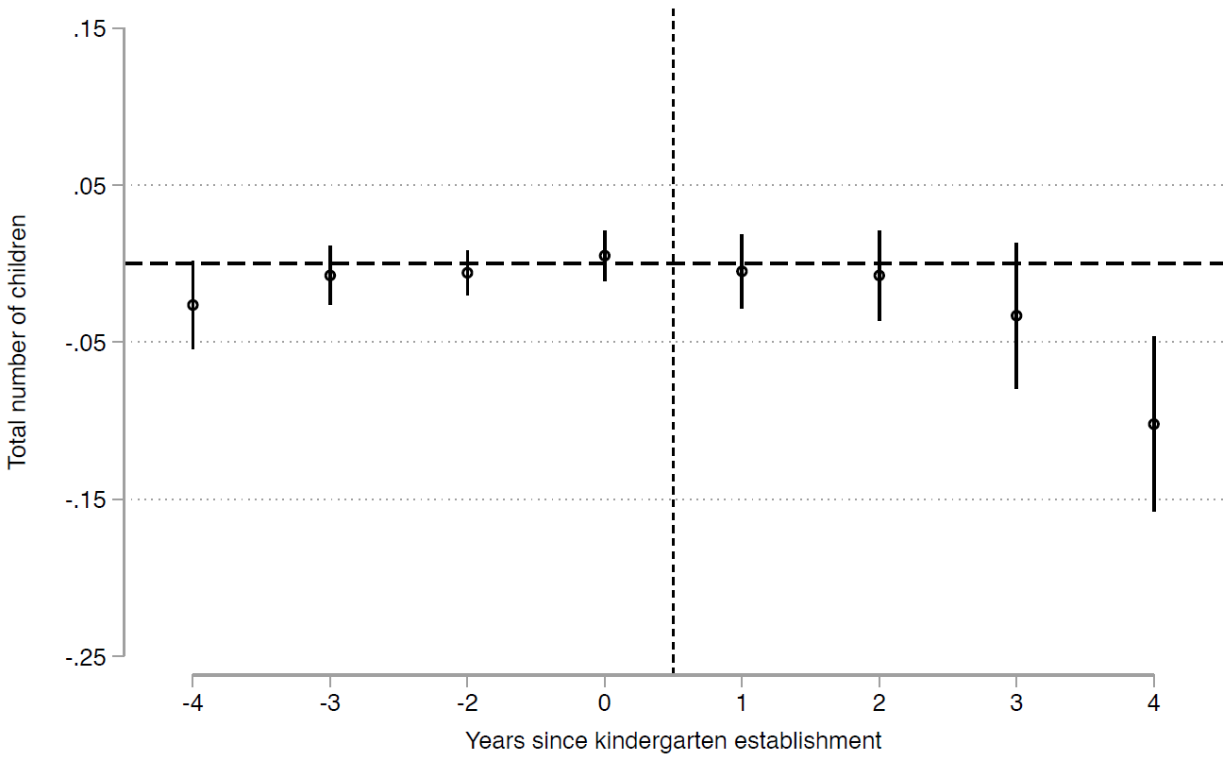
Notes: This map displays an excerpt of the sample of households within 1,000 meters radius from the closest district-school in St. Louis together with the 1880 enumeration districts. The households in yellow are located within 250 meters of a district-school with opened kindergarten and are considered as "treated".

Figure A.4: The Effect of the Kindergarten Roll-out on Attendance in St. Louis



Notes: This figure shows the effect of the kindergarten roll-out on school attendance for children age 5–6. The attendance dummy is regressed on fixed effects for the number of years since an enumeration district had access to a school and for the mother's age. We also add fixed effects for the mother's birthplace and literacy status, and a set of dummy variables for whether her husband works in a white-collar/blue-collar skilled occupation and a series of binary variables indicating the number of years since a kindergarten operated in the enumeration district where the household was residing in 1880. The x-axis measures the number of years since the kindergarten opened in enumeration district e. The dots depict the estimated coefficients of kindergarten exposure on school attendance relative to enumeration districts without an kindergarten ("no kiga"). The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

Figure A.5: Robustness Figure 1 Panel (a) – Women with 5-6 year old child in 1880



Notes: This figure replicates Panel (a) of Figure 1 for women with a 5-6 year old child in 1880. The x-axis measures the years since the kindergarten opened in enumeration district e. Estimated coefficients (dots) including 95 percent confidence intervals (solid lines) of kindergarten exposure on fertility are reported relative to the base year -1 (omitted). Standard errors are clustered at the enumeration district level.

Table A.1: Balancing Table Linked/Non-Linked Women in St. Louis 1880

Variable	(1) 0 Mean	(2) 1 Mean	(3) (1)-(2) t-test	(4) 0 Mean	(5) 1 Mean	(6) (4)-(5) t-test
Age	33.662	33.332	0.331***	33.662	33.736	-0.074
	(0.084)	(0.063)		(0.084)	(0.074)	
Foreign-born	0.630	0.608	0.022**	0.630	0.630	0.000
	(0.011)	(0.012)		(0.011)	(0.013)	
Mother foreign-born	0.815	0.826	-0.011	0.815	0.817	-0.002
	(0.010)	(0.012)		(0.010)	(0.013)	
Father foreign-born	0.824	0.834	-0.010	0.824	0.826	-0.002
	(0.010)	(0.012)		(0.010)	(0.012)	
Works	0.172	0.166	0.006	0.172	0.170	0.002
	(0.027)	(0.027)		(0.027)	(0.026)	
Household head	0.078	0.039	0.039***	0.078	0.078	0.001
	(0.005)	(0.002)		(0.005)	(0.004)	
Literate	0.929	0.954	-0.025***	0.929	0.930	-0.001
	(0.009)	(0.006)		(0.009)	(0.009)	
No. children	3.970	4.293	-0.322***	3.970	4.016	-0.046
	(0.030)	(0.024)		(0.030)	(0.026)	
Foreign-born (spouse)	0.765	0.756	0.009	0.765	0.766	-0.001
	(0.010)	(0.012)		(0.010)	(0.011)	
Mother foreign-born (spouse)	0.861	0.855	0.006	0.861	0.861	0.000
	(0.009)	(0.012)		(0.009)	(0.011)	
Father foreign-born (spouse)	0.865	0.858	0.007	0.865	0.865	-0.000
	(0.009)	(0.011)		(0.009)	(0.010)	
White collar occ (spouse)	0.242	0.309	-0.066***	0.242	0.244	-0.002
	(0.012)	(0.014)		(0.012)	(0.013)	
Blue collar occ (spouse)	0.258	0.266	-0.009	0.258	0.257	0.000
	(0.008)	(0.008)		(0.008)	(0.008)	
Household occ score	30.388	32.741	-2.353***	30.388	30.381	0.007
	(1.111)	(1.133)		(1.111)	(1.037)	
Number of observations	5051	7247	12298	5051	7247	12298

Notes: This table reports mean differences in baseline characteristics (columns 3 and 6) between women that are linked in our sample (columns 2 and 5) and those who are not (column 1 and 4). Results in columns 4-6 are re-weighted by the propensity of being matched (see footnote 19 in the manuscript for details). The sample includes white women age 25–44 who had a 5–6 year old child and lived 1880 in St. Louis. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.2: Comparison of Linked Women by Treatment Status in St. Louis 1880

Variable	(1) 0 Mean	(2) 1 Mean	(3) (1)-(2) t-test
Age	33.150	33.694	-0.543***
	(0.075)	(0.117)	
Foreign-born	0.593	0.639	-0.046***
	(0.014)	(0.014)	
Mother foreign-born	0.812	0.855	-0.043***
	(0.014)	(0.013)	
Father foreign-born	0.822	0.858	-0.036***
	(0.013)	(0.013)	
Works	0.158	0.181	-0.024
	(0.027)	(0.031)	
Household Head	0.035	0.048	-0.013**
	(0.003)	(0.005)	
Literate	0.950	0.961	-0.011*
	(0.007)	(0.006)	
No. Children	4.187	4.504	-0.317***
	(0.029)	(0.037)	
Foreign-born (spouse)	0.736	0.795	-0.059***
	(0.013)	(0.013)	
Mother foreign-born (spouse)	0.841	0.884	-0.043***
	(0.013)	(0.011)	
Father foreign-born (spouse)	0.846	0.883	-0.037***
	(0.012)	(0.011)	
White collar occ (spouse)	0.315	0.296	0.019
	(0.015)	(0.017)	
Blue collar occ (spouse)	0.268	0.262	0.006
	(0.009)	(0.011)	
Household occ score	32.608	33.006	-0.398
	(1.131)	(1.271)	
Number of observations	4828	2419	7247

Notes: This table reports mean differences in baseline characteristics (columns 3) between women whose children between the ages of 5 and 6 attend a kindergarten in St. Louis in 1880 (columns 2) and those who are not (column 1). The sample includes women in St. Louis in 1880 that can be linked to 1900. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.3: Estimated Coefficients for St. Louis Event Study Regressions

Dependent variable:	Total number of children	
	Baseline	Proximity
	(1)	(2)
Kindergarten establishment ($\tau \leq -4$)	-0.033	0.005
	(0.030)	(0.023)
Kindergarten establishment ($\tau - 3$)	-0.012	0.010
	(0.014)	(0.012)
Kindergarten establishment ($\tau - 2$)	-0.012	0.004
	(0.008)	(0.007)
Kindergarten establishment ($\tau - 1$)	*Baseline*	*Baseline*
Kindergarten establishment (τ)	0.006	-0.006
	(0.009)	(0.007)
Kindergarten establishment ($\tau + 1$)	0.000	-0.022*
	(0.015)	(0.012)
Kindergarten establishment ($\tau + 2$)	-0.015	-0.047***
	(0.021)	(0.019)
Kindergarten establishment ($\tau + 3$)	-0.035	-0.081***
	(0.031)	(0.033)
Kindergarten establishment ($\tau \geq 4$)	-0.110**	-0.059**
	(0.039)	(0.031)
Mother FE	Yes	Yes
Year FE	Yes	Yes
Mother's age FE	Yes	Yes
Years since district school	Yes	Yes
Observations	372105	332195
R-squared	0.92	0.92

Notes: This table reports the estimated coefficients of the event studies displayed in Figure 1. The dependent variable is the cumulative number of births. Kindergarten establishment ($\tau + j$) is an indicator equal to one when $t = \tau + j$ and τ is the year in which a kindergarten was established in enumeration district e. The variables Kindergarten establishment ($\tau \leq -4; \tau \geq +4$) capture all leads $\tau \leq -4$ or lags $\tau \geq 4$, respectively. The year before a kindergarten opened in a given enumeration district e is the base year (omitted). Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the enumeration district level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.4: Robustness Checks: St. Louis – Linked sample 1880-1900

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A			No. children ever born			
Attends	-0.156**	-0.158*	-0.184**	-0.185**	-0.168	-0.175*
	(0.0784)	(0.0897)	(0.0740)	(0.0893)	(0.104)	(0.0895)
R-squared	0.351	0.408	0.335	0.356	0.388	0.419
Mean(Y)	6.779	6.777	6.779	6.830	6.896	7.117
Effect Size (%)	-2.296	-2.338	-2.721	-2.704	-2.431	-2.465
Panel B			No. surviving children			
Attends	-0.0627	-0.0264	-0.0770	-0.0372	0.0202	-0.0648
	(0.0553)	(0.0593)	(0.0497)	(0.0616)	(0.0673)	(0.0537)
R-squared	0.420	0.468	0.396	0.436	0.471	0.508
Mean(Y)	4.864	4.863	4.864	4.887	4.945	5.148
Effect Size (%)	-1.289	-0.543	-1.582	-0.760	0.408	-1.258
Panel C			No. dead children			
Attends	-0.0930*	-0.132**	-0.108*	-0.148**	-0.188**	-0.111
	(0.0561)	(0.0616)	(0.0555)	(0.0617)	(0.0809)	(0.0700)
R-squared	0.149	0.228	0.140	0.160	0.188	0.197
Mean(Y)	1.915	1.914	1.915	1.942	1.952	1.969
Effect Size (%)	-4.855	-6.897	-5.615	-7.596	-9.622	-5.621
Observations	7,162	7,005	7,162	5,907	4,253	4,480
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Enumeration FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the following robustness checks by extending our baseline specification (column 3) of Table 1 reporting gross fertility in Panel A, net fertility in Panel B, and child mortality in Panel C. Column (1) reports results controlling for the share of children aged 7-15 in the household that attended school in 1880; Column (2) reports results with street fixed effects; Column (3) reports results without sample weights; Column (4) reports results excluding matches outside a 5-year birth window; Column (5) reports results excluding matches outside a 1-year birth window; and Column (6) uses the crosswalks provided by MLP instead of the CT project. All specifications include the baseline controls reported in column 3 of Table 1. Standard errors (in parentheses) are clustered at the enumeration district. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.5: Balancing Table Linked/Non-Linked Women in US Cities 1900

Variable	(1) 0 Mean	(2) 1 Mean	(3) (1)-(2) t-test	(4) 0 Mean	(5) 1 Mean	(6) (4)-(5) t-test
Age	33.612	33.636	-0.024	33.612	33.665	-0.053**
	(0.034)	(0.041)		(0.034)	(0.048)	
Foreign-born	0.539	0.436	0.103***	0.539	0.536	0.003
	(0.032)	(0.030)		(0.032)	(0.033)	
Mother foreign-born	0.743	0.707	0.036***	0.743	0.742	0.001
	(0.032)	(0.029)		(0.032)	(0.030)	
Father foreign-born	0.757	0.726	0.031***	0.757	0.756	0.001
	(0.030)	(0.028)		(0.030)	(0.028)	
Works	0.057	0.032	0.025***	0.057	0.057	0.000
	(0.002)	(0.001)		(0.002)	(0.002)	
Household head	0.071	0.030	0.040***	0.071	0.070	0.001
	(0.002)	(0.001)		(0.002)	(0.002)	
Literate	0.858	0.932	-0.074***	0.858	0.860	-0.001
	(0.011)	(0.008)		(0.011)	(0.015)	
No. children	3.648	3.847	-0.199***	3.648	3.703	-0.055***
	(0.018)	(0.015)		(0.030)	(0.026)	
Foreign-born (spouse)	0.615	0.500	0.115***	0.615	0.612	0.003
	(0.029)	(0.030)		(0.029)	(0.030)	
White collar occ (spouse)	0.172	0.224	-0.052***	0.172	0.173	-0.000
	(0.006)	(0.003)		(0.005)	(0.003)	
Blue collar occ (spouse)	0.237	0.271	-0.033***	0.237	0.239	-0.002
	(0.006)	(0.003)		(0.006)	(0.005)	
Household occ score	22.573	24.411	-1.838***	22.573	22.596	-0.022
	(0.173)	(0.068)		(0.173)	(0.074)	
Duration of marriage (bins)	3.058	2.921	0.137***	3.058	3.057	0.001
	(0.008)	(0.005)		(0.008)	(0.006)	
Number of observations	166774	580862	747636	166774	580862	747636

Notes: This table reports mean differences in baseline characteristics (columns 3 and 6) between women that are linked in our sample (columns 2 and 5) and those who are not (columns 1 and 4). Results in columns 4-6 are re-weighted by the propensity of being matched (see footnote 19 in the manuscript for details). The duration of marriage is binned by less than 5 years, 6-10 years, 11-15 years, 16-20 years and 21-25 years. The sample includes white women age 25–44 who had a 5–6 year old child and lived 1900 in a city (+25,000 inhabitants). ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.6: Comparison of Linked Women by Treatment Status in US Cities 1900

Variable	(1) 0 Mean	(2) 1 Mean	(3) (1)-(2) t-test
Age	33.497	33.929	-0.432***
	(0.009)	(0.013)	
Foreign-born	0.425	0.460	-0.035***
	(0.003)	(0.003)	
Mother foreign-born	0.695	0.733	-0.038***
	(0.003)	(0.002)	
Father foreign-born	0.714	0.750	-0.036***
	(0.002)	(0.002)	
Literate	0.930	0.937	-0.007***
	(0.001)	(0.001)	
Works	0.032	0.033	-0.001*
	(0.000)	(0.001)	
Household Head	0.030	0.032	-0.003***
	(0.000)	(0.000)	
White collar occ (spouse)	0.221	0.230	-0.009***
	(0.001)	(0.002)	
Blue collar occ (spouse)	0.267	0.278	-0.011***
	(0.001)	(0.001)	
Foreign-born (spouse)	0.489	0.521	-0.032***
	(0.003)	(0.003)	
Household occ score	24.242	24.768	-0.527***
	(0.033)	(0.038)	
No. children	3.808	3.930	-0.122***
	(0.006)	(0.006)	
Duration of marriage (bins)	13.999	14.586	-0.587***
	(0.025)	(0.035)	
Number of observations	393576	187286	580862

Notes: This table reports mean differences in baseline characteristics (column 3) between women whose children between the ages of 5 and 6 attend a kindergarten in 1900 (column 2) and those who are not (column 1). The sample includes women in cities (+25,000 inhabitants) in 1900 that can be linked to 1910. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table A.7: Robustness Checks: U.S. cities – Linked sample 1900-1910

	(1)	(2)	(3)	(4)	(5)
Panel A			No. children ever born		
Attends	-0.131***	-0.125***	-0.130***	-0.130***	-0.141***
	(0.00752)	(0.00617)	(0.00735)	(0.00844)	(0.00748)
R-squared	0.535	0.544	0.550	0.566	0.575
Mean(Y)	5.669	5.669	5.619	5.595	5.844
Effect Size (%)	-2.302	-2.209	-2.312	-2.327	-2.406
Panel B			No. surviving children		
Attends	-0.0522***	-0.0432***	-0.0465***	-0.0444***	-0.0520***
	(0.00487)	(0.00461)	(0.00456)	(0.00535)	(0.00506)
R-squared	559,625	559,625	469,487	366,114	445,038
Mean(Y)	4.528	4.528	4.500	4.503	4.698
Effect Size (%)	-1.153	-0.953	-1.034	-0.986	-1.106
Panel C			No. dead children		
Attends	-0.0783***	-0.0821***	-0.0834***	-0.0858***	-0.0887***
	(0.00514)	(0.00467)	(0.00555)	(0.00617)	(0.00542)
R-squared	0.179	0.161	0.181	0.188	0.190
Mean(Y)	1.142	1.142	1.119	1.092	1.146
Effect Size (%)	-6.860	-7.191	-7.453	-7.858	-7.736
Observations	559,625	559,625	469,487	366,114	445,038
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Enumeration FE	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the following robustness checks by extending our baseline specification (column 3) of Table 2. Column (1) reports results controlling for the share of children aged 7-15 in the household that attended school in 1880; Column (2) reports results without sample weights; Column (3) reports results excluding matches outside a 5-year birth window; Column (4) reports results excluding matches outside a 1-year birth window; and Column (5) uses the crosswalks provided by MLP instead of the CT project. All specifications include the baseline controls reported in Column 3 of Table 2. Standard errors (in parentheses) are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.