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Froebel's Gifts:
How the Kindergarten Movement Changed the American Family

Abstract

Nineteenth-century social reformers promoted the establishment of kindergartens as a remedy for the problems associated with industrialization and immigration. We evaluate the impact that the rollout of the first kindergartens in American cities had on mothers and their children. Consistent with the predictions of a quantity-quality trade-off model, immigrant families exposed to kindergartens significantly reduced fertility. Their offspring at age 10-15 were more likely to attend school, they worked less, and as adults, they had fewer children. We also unveil positive language spillover effects of kindergarten education on immigrant mothers illustrating the importance of kindergartens for social integration.

JEL-Codes: N310, J130, I250, O150.

Keywords: kindergarten education, fertility transition, child labor, school attendance, quantity-quality trade-off.

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I. Introduction

High birth rates and reliance on income from child labor were key features of working-class families in industrializing America. In the late 19th century, children often spent no longer than three years in school because they started working in factories when they were as young as ten years old. By 1880, every third boy aged 10-15 was working and white women had around 4 children. Over the next fifty years, the U.S. experienced a dramatic decline in child labor and fertility rates, while school attendance rates were increasing, especially for immigrants. Such pattern is consistent with theoretical predictions that parents will reduce fertility and invest more in the education of their children when child labor is declining (Hazan and Berdugo, 2002; Doepke, 2004; Doepke and Zilibotti, 2005; Moav, 2005). The aim of this paper is to contribute to a better understanding of the underlying factors that triggered this substantial social change in the U.S. at the turn of the 20th century.

Our analysis focuses on evaluating the impact of the roll-out of the first kindergartens in American cities on fertility, child labor, and school attendance. The establishment of kindergartens was part of a progressive reform movement in the U.S. that was increasingly alarmed by the social challenges that rapid industrialization, immigration, and urbanization brought to traditional family life. Influential educational reformers, such as Elisabeth Peabody or William T. Harris, saw in kindergartens a remedy for the problems with an increasing number of children growing up in cities surrounded by poverty, ignorance, and crime (Klein, 1992; Beatty, 1995). Between 1880 and 1910, more than 7,000 kindergartens opened their doors in various American cities, thus increasing the number of children enrolled in kindergartens from a few thousand in 1880 to more than 350,000 in 1910 (U.S. Office of Education, 1899; U.S. Bureau of Education, 1914). Contemporary observers such as Nina C. Vandewalker, a kindergarten specialist for the U.S. Bureau of Education in the 1920s, even regarded the establishment of kindergartens in American cities as one of the most fundamental movements in American education (Vandewalker, 1908). Yet, rigorous quantitative studies assessing the impact of the *kindergarten movement* on social change at the turn of the 20th century in the U.S. are lacking.[1]

The main goal of this paper is to explain how the historical roll-out of kindergartens in rapid industrializing American cities affected parental fertility choices and their decision to invest in children's

[1] The only other empirical study on the kindergarten movement in the U.S. we are aware of is an unpublished PhD thesis chapter by Haimovich (2015). For a linked sample of males (1900/10-1940), Haimovich finds positive long-term effects of exposure to public kindergartens on schooling and occupation-based earnings in 1940. Further evidence on positive long-term effects of large-scale historical preschool or childcare programs in the U.S. comes from Cascio (2009a) who evaluates the introduction of state funding for public school kindergartens during the 1960s and 1970s, and Herbst (2017) who studies the long-term consequences of the Lanham Act of 1940---a universal child care policy that operated during WWII. We refer the readers to the surveys by Duncan and Magnuson (2013), Almond et al. (2018), and Cascio (2021) for a detailed overview of the economics literature on early childhood education programs.

education. Kindergarten teachers prepared 5- to 6-year-old children for primary school but also regularly arranged home visits and mothers' meetings, where they lectured working-class mothers on the importance of child rearing, home economics, and the value of early childhood education (Ross, 1976; Shapiro, 1983). Besides anecdotal evidence emphasizing the social benefits of such interactions between mothers and teachers, development studies have argued that providing low-income parents with valuable information about education influences their decision to increase the investment in their children's education, which, in the context of a trade-off between child quantity and quality, would also affect their fertility choices (e.g., Rosenzweig and Wolpin, 1980; Nguyen 2008; Jensen, 2010; Dizon-Ross, 2019).

Our results suggest that the availability of kindergarten education played an important role, especially for the fertility choices of immigrant families and their decision to invest more in the education of their children. The roll-out of kindergartens explains up to 14 percent of the overall fertility decline that immigrant families experienced between 1880 to 1910. Since, on average, every forth city dweller during our sample period was foreign-born, this effect is economically relevant. Full exposure to kindergartens increased school attendance of 10 to 15-year old immigrant children by 9 percentage points and led at the same time to a 7 percentage point decline in the likelihood of reporting a gainful occupation. We also find that access to kindergarten education improved immigrant children's English fluency.[2] Overall, our findings are in line with anecdotal evidence from contemporary surveys that children attending kindergartens were generally better prepared for primary school. In particular, public school instructors argued that teaching the use of English at such an early age would offer immigrant children a fair start in school (e.g., Palmer, 1915; Waite, 1926; Berg, 2004).

Our empirical evidence is consistent with the predictions of an augmented quantity-quality model of fertility which explicitly allows households to invest in kindergarten education. The key insight of this model is that parents would unambiguously reduce fertility if complementarities between preschool education and other forms of human capital investment like schooling exist. We rule out other competing explanations for the observed fertility decline associated with the roll-out of kindergartens. In particular, we show that the general expansion of the public education system in American cities during the late 19th and early 20th centuries cannot explain away the negative association between the kindergarten roll-out and fertility. Yet we also find that the expansion of the public school system contributes to the fertility decline, which is consistent with a well-established literature emphasizing the importance of human capital for the fertility transition (e.g., Galor, 2011). The fertility decline associated with kindergarten exposure is also not driven by changes in child mortality, a delay of marriage, or increased female labor

[2] A number of economists have argued that English proficiency is a crucial factor for immigrants' success in the workplace (e.g., Bleakley and Chin, 2004; Chiswick and Miller, 2015).

force participation. The latter result is in line with Aaronson et al. (2021), who find no systematic relationship between fertility and female labor supply in the U.S. before WWI.[3]

Our empirical analysis of the kindergartens' impact on fertility begins with a case study. In 1873, William T. Harris, superintendent of schools in St. Louis, Missouri, initiated the first large-scale involvement of a public-school system in kindergarten education in the U.S. The St. Louis case study 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 effects of Froebel's system" (St. Louis Annual Report, 1875, p. 195). The annual reports of the Board of public schools in St. Louis contain detailed information about the location of kindergartens starting in 1873. We geo-referenced the location and opening date of every kindergarten between 1873 and 1886 and combined this information with the fertility history of 18- to 44-year-old white women living in St. Louis in 1880. We then employ a difference-in-differences approach, which exploits the different timing of kindergarten openings across enumeration districts and the fact that women gave birth at different points in time, to test whether and to what extent the fertility pattern of women changed after the opening of kindergartens across enumeration districts. The estimates reveal a striking pattern: Women in treated enumeration districts gradually reduce fertility after a kindergarten opening, while there are no fertility differences between treated and untreated women before the event. We obtain similar results if we consider proximity to the closest kindergarten as treatment criterion instead. Women that live in close proximity to a kindergarten (within 250 meters) gradually decrease fertility compared to women living further away (between 250-1,000 meters) but only so after the kindergartens are open. This finding is consistent with the notion that households' proximity to a kindergarten and kindergarten exposure are closely related. Importantly, our results also hold when accounting for potential heterogeneous treatment effects across time periods and the potential presence of spatial spillover effects.

Next, we analyze whether the kindergarten movement also contributed to the fertility transition in other American cities. Starting in the 1870s, the annual U.S. Bureau of Education's annual report contains, for most years, kindergarten statistics at the city level. We digitize these reports and construct a city and time varying measure of kindergarten exposure which we combine with complete count U.S. Census microdata for the decades 1880-1910. Based on this sample, we find that 18- to 44-year-old white women exposed to kindergartens at the time when they conceive a child significantly reduce fertility. The fertility decline associated with kindergarten exposure is driven by families who had at least one child of

[3] Several studies based on modern data investigate the effect of access to public school kindergartens on maternal labor supply with generally mixed evidence depending, for example, on different eligibility criteria (e.g., Gelbach, 2002; Cascio, 2009b; Havnes and Mogsted, 2011).

kindergarten age at the time of future family planning. Instead, families who have only young children (under age 5) at the time of the census enumeration do not adjust their fertility behavior. Our findings suggest that mothers' personal interactions with the kindergarten teachers play an important role for changing fertility behavior. While our model is silent about how parents learn about potential complementarities between kindergarten education and schooling, it implies that parents change their fertility behavior once they realize such complementarities exist. Hence, our finding that parents learn about the returns to education from personal interactions with the kindergarten teacher is consistent with the predictions of our model. These results are robust to including city-specific linear time trends and relevant individual characteristics, such as literacy, age, and birthplace. Our most demanding specifications control for city-by-year fixed effects to capture time-varying locally confounding factors that could have coincided with the timing of the kindergarten roll-out, such as public health interventions.

Since the kindergarten movement specifically targeted poor families who had more children and relied on child labor for their family income, one would expect a stronger impact of kindergarten exposure on fertility in cities that relied more on child labor in 1880. Our results indicate that this is indeed the case: Families exposed to kindergartens in cities with an initial share of child labor above the median substantially reduce fertility once they have a child of kindergarten age. The estimated fertility decline associated with kindergarten exposure is substantially larger compared to families in cities with an initial share of child labor below the median. The negative relationship between kindergarten exposure and fertility is mainly driven by immigrant families—the main target group of the kindergarten movement according to contemporary sources. School reports, discussions in general education journals, or more specialized outlets such as the *Kindergarten Magazine* reveal that home visits and mothers' meetings were key for kindergarten teachers to gain access into immigrant homes (Shapiro, 1983; Klein, 1992; Berg, 2004). These visits and meetings aimed to change the mothers' perception about the value of early childhood education in general and, hence, one would expect a decline in fertility once immigrant mothers received more information about the value of kindergarten education. Consistent with this interpretation, we find that immigrant households substantially reduce fertility once direct contact with the kindergarten teacher is possible.

We further investigate whether kindergarten exposure affects the fertility behavior of exposed children as adults. Based on a linked sample of males from 1900-1920 and 1910-1930, we show that those who were exposed to kindergartens at age 5-6 live as adults in smaller families and have fewer children. This finding is again driven by children with immigrant background and reveals that exposure to kindergartens exposure influenced the fertility decisions of immigrant families over, at least, two generations.

Our results on the mechanisms explaining the fertility decline are based on an analysis of 10 to 15-year old children living in cities with different exposure to kindergartens at the time when they were of kindergarten-age. First, we find a significantly higher school attendance rate if they were exposed to kindergartens at age 5-6. The increase in school attendance is mainly driven by immigrant children. Those also reduce the supply of child labor if they were exposed to kindergartens at age 5-6. Overall, these findings reveal that kindergartens leave a trace on children's outcomes approximately 5-10 years after exposure. Our estimation strategy accounts for state-by-birth cohort fixed effects, and when comparing outcomes of children from immigrant homes relative to children of native parents we include city-by-birth cohort fixed effects, besides a wide set of individual and parental controls. Including state (city)-by-birth cohort fixed effects reduces the concerns that state legislation, such as the introduction or modification of laws concerning child labor and compulsory schooling, could confound our results.[4] Second, we also show that access to kindergartens improves English fluency of foreign-born children from non-English-speaking sending countries. Exploiting for identification the children's year of arrival in the U.S. together with variation in kindergarten exposure across cities when the child was age 5-6, we find that children who arrived early enough to attend a kindergarten are more likely to speak English in cities with a higher kindergarten exposure.

We also unveil positive language spillover effects of kindergarten attendance on mothers. In this case, we exploit the differential effect that kindergarten attendance might have on foreign-born mothers and fathers from non-English speaking sending countries. Our identification strategy is based on family fixed effects and rests on the assumption that mothers are more directly affected compared to fathers because of the stronger relationship with the child and the personal interactions with the kindergarten teacher during home visits and mothers' meetings. The estimates indicate that having a child attending a kindergarten reduces the gap in English fluency between immigrant mothers and fathers between 10-17 percent. Even if this model cannot fully account for selection, it provides important insights whether mothers from culturally different countries benefit from the kindergarten education of their children.

Overall, our findings are consistent with the predictions of a quantity-quality trade-off model in which households reduce fertility because the roll-out of kindergartens increased the returns to education and

[4] Economic historians generally questioned the effectiveness of compulsory schooling laws (Landes and Solmon, 1972) and child labor laws (Moehling, 1999) that were enacted in the late 19th century and early 20th century, while recent work by Clay et al. (2021) finds, based on complete count data, modest effects of these laws on educational attainment consistent with Lleras-Muney (2002) and Goldin and Katz (2011). In terms of school attendance, Lleras-Muney and Shertzer (2015) find positive effects of compulsory schooling laws for children aged 10-16 in 1910-1930 and Margo and Finegan (1996) for children age 14 in 1900, but only when compulsory schooling was combined with an age-compatible child labor law. On reducing child labor, Feigenbaum and Russo (2020) find both laws to be effective.

the costs of child rearing due to lost household income from child labor.[5] Our results also relate to proponents of unified growth theory emphasizing the role of human capital for the fertility decline during the second phase of the industrial revolution (Galor and Weil, 2000; Galor, 2011). We show that besides having broader access to public education, the roll-out of kindergartens contributed significantly to the transition from high to low fertility in American cities. More generally, our empirical evidence suggests that the availability of kindergarten education facilitated the rapid accumulation of human capital in the U.S. at the beginning of the 20th century (Goldin and Katz, 1999; 2008). Overall, the kindergarten movement improved the prospects of working-class and immigrant children and contributed to falling fertility rates in rapidly industrializing American cities at the turn of the 20th century.

II. Historical Background

In this section, we describe the historical context of our study, beginning with how kindergartens spread in the U.S. during the last decades of the 19th century. We then provide a brief account of their organization and finally discuss the importance of home visits that kindergarten instructors regarded as key elements in building up a relationship with working-class mothers.

A. The Spread of the Kindergarten Movement in the United States

The kindergarten as an institution of early childhood education goes back to 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 in a child's life were the most important for their future development. His principle of educating little children rejected traditional didactic education and focused instead on children's 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 teaching methods aimed to educate 3- to 6-year-old children and were applicable to all children independent of their social background. With daily sessions of 3-4 hours, Froebel's concept represented a compromise between family-based and fully institutionalized child rearing (Allen, 2017; Klein, 1992; Lascarides and Hinitz, 2013).

Froebel's kindergarten concept was transplanted to the U.S. when a number of highly educated liberal political leaders (the "Forty-Eighters") fled from Germany as a consequence of the political oppression following the failed revolution in 1848-49.[6] One of them was Margarethe Schurz, an enthusiastic advocate of Froebel's teaching methods, who opened the first kindergarten on American soil in Watertown,

[5] A number of empirical studies provide compelling evidence that such a tradeoff during (or even before) the demographic transition existed (e.g., Bleakley and Lange, 2009; Becker et al., 2010; Aaronson et al., 2014; and Ager et al., 2020).
[6] See Bauernschuster and Falck (2015) for further insights on the early spatial diffusion of kindergartens in Germany.

Wisconsin, in 1856.[7] A few other German-speaking kindergarten pioneers followed Schurz to the U.S. and set up more kindergartens and trained instructors according to Froebelian principles. During the early phase of the movement, kindergartens were mainly tuition-based private institutions catered to educate privileged children from wealthy families (Beatty, 1995; Allen, 2017).

The movement gained popularity with the establishment of *free* kindergartens during the 1870s, which the public regarded as child saving agencies at times of rapid industrialization, immigration, and urbanization. Several philanthropists, churches, and other charitable societies, increasingly concerned about the virtues of children growing up in poverty, established and funded kindergarten associations to offer tuition-free kindergarten classes. Created as an institution of the "urban slum", these kindergartens also fulfilled a social function by saving poor children from the dangers of the street, providing food and clothing, and teaching them morals and values to prevent delinquency (Klein, 1992). The so-called free kindergarten associations became the engine of the movement during the 1880s, when kindergarten instruction was still in a rather experimental phase (Vandewalker, 1908). During this period, kindergartens became widely recognized as an institution for the urban poor and the work of the associations further familiarized the public with the general principles of kindergartens. By 1890, about 15,000 children were enrolled in the schools of 115 free kindergarten associations (Shapiro, 1983). While the number of free kindergarten associations peaked around 1900 with over 500 associations (U.S. Office of Education, 1899), they started struggling to meet the public needs of kindergarten education due to lack of funding and organizational resources (Klein, 1992).

Already by the late 1880s, Boston and many other larger cities gradually incorporated free kindergartens into the public school system (Vandewalker, 1908), which gradually shifted the focus from the social and urban reform functions of free kindergartens to granting universal access to kindergarten education with the primary goal of preparing children for school (Lazerson, 1971a; Beatty, 1995; Klein, 1992). For immigrant children, the public kindergarten served also a socialization function. Public educators regarded the kindergarten as an important instrument to Americanize children of non-English-speaking families, who arrived in large numbers in American cities at the turn of the 20th century. Classroom activities included singing English songs, reading English stories, learning American cultural customs, and teaching English, which all aimed to accustom immigrant children to the "American" way of life (Klein, 1992; Berg, 2004; Allen, 2017).

The integration of kindergartens into the public school system fueled the expansion of the movement between 1890 and 1910. Since the first year of official kindergarten statistics in 1873, the number of

[7] Schurz's kindergarten was small-scale (only six children, including her own daughter, attended) and it closed just a few years later when the Schurz family moved away from Watertown (Beatty, 1995, pp. 53–54).

kindergartens increased from 42 with 1,252 enrolled pupils to 7,557 kindergartens with 353,546 pupils enrolled in 1912. Yet the kindergarten movement was an *urban phenomenon*. Nationwide, kindergarten enrollment rates went up from close to zero in 1880 to approximately 9% in 1912 (U.S. Bureau of Education, 1914). The outstanding reason for the still relatively low enrollment was that kindergarten coverage in the sparsely populated rural areas was very limited (e.g., Allen, 2017; Vandewalker, 1925). As the Massachusetts Board of Education (1903, p. 94) stated, kindergartens are "*hardly practicable in rural communities, outside of the villages, since the children are few and widely separated.*"

In cities, a different pattern emerged because the public school system broadened the access to kindergarten education. The number of cities with publicly sponsored kindergartens increased from 137 in 1892 to 867 in 1912. Yet this process occurred gradually, and coverage was not universal, mainly because kindergartens entailed high maintenance costs (Klein, 1992). In some instances, the costs even exceeded the expenditures per pupil in primary school (Lazerson, 1971b). In fact, as a response to the increasing cost pressure, many city school systems introduced so-called double sessions: one session in the morning and another in the afternoon, usually with a different group of children. Double sessions allowed kindergartens to adjust to capacity problems because more children could be enrolled without having larger classes (Lascarides and Hinitz, 2013). By 1910, the attendance rate of 5- to 6-year-olds in cities stood at approximately 60 percent. At this time, most urban public school systems had integrated the kindergarten as a (voluntary) first class of the elementary school (Ross, 1976).

B. The Organization of Kindergartens

Kindergarten instructions were offered on weekdays for around 3-4 hours per day. The classes were usually relatively small. In public kindergartens, one or two teachers instructed, on average, 25 children per room for about three hours (Foos, 1909). In tuition-based kindergartens the class size was somewhat smaller (around 20-25 enrolled pupils), whereas association kindergartens had generally larger classes (around 50 pupils).[8] While both tuition-based and free kindergartens served a specific segment of the society, public school sponsorship contributed to the universal provision of kindergarten education. 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).

Compared to the multiple functions of free kindergartens assisting the urban poor, private kindergartens mainly served an educational function, focusing on preparing children of affluent households for primary school. Public school administrators also mainly saw the kindergarten's focus on the child in class (Klein 1992). The U.S. Bureau of Education (1914, p. 10) describes the mission of the

[8] These enrollment numbers for tuition-based and free kindergartens are from the Reports of the Commissioner of Education (1880; 1886–87).

public 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. While all kindergarten sponsors pursued different goals and served different segments of the society, the kindergarten curriculum had largely followed Froebel's teaching principles. For example, the director of public kindergartens in Boston, Laura Fisher, describes the daily program as rather similar in many kindergartens, consisting of Froebel's gifts and occupations, circle games, free play, songs, and talks (Fisher, 1905, p. 718). Davis (1925) analyzed the teaching schedules of 137 kindergartens across 34 states and found that kindergarten teachers devoted 33 percent of their time in sessions to work with materials, 36 percent to physical education (such as games and plays), 9 percent to language, 6 percent to music, and 16 percent to general assemblies.

One important element of kindergarten pedagogy beyond caring for the well-being of little children was to build up a relationship with their mothers. Kindergarten teachers arranged home visits and mothers' meetings. These meetings mainly targeted working-class and immigrant mothers to socialize with them and elevate their social status (Klein, 1992; Berg, 2004). In the next section, we argue that the personal interactions with mothers were a central building block of the kindergarten movement in the late 19th century, with the result of directly influencing family planning of the urban poor.

C. The Home and the Kindergarten

Home visits and mothers' meetings were intended to enlighten mothers about general child-rearing principles, the value of kindergarten education, home economics, and familiarizing them with songs, stories, and materials used in class (Fisher, 1905; Berg, 2004). Kindergarten associations introduced home visits as a community service: Teachers of free kindergartens reached out to the deprived homes of their pupils, explaining to mothers how to engage with their offspring (Lazerson, 1971b; Shapiro, 1983). Overall, home visits catered to the needs of the children and their families and included "lectures" about hygiene, nutrition, and child rearing. The mothers' meetings were another service offered by kindergarten associations that aimed at establishing a bond between teachers and mothers. These meetings became later an integral part of the public school system and are considered forerunners of the modern Parent Teacher Associations (Ross 1976; Klein, 1992).

The home visits and mothers' meetings provided invaluable services, especially for immigrant mothers. Berg (2004) provides ample anecdotal evidence that kindergarten teachers aimed to integrate immigrant mothers into the society by teaching them to emulate the domestic life of middle-class American women of that time. The *Kindergarten Circular* emphasized the importance of these services as being *"[…] instrumental in helping foreign mothers to understand and appreciate the customs and standards of the new country"* (U.S. Bureau of Education, 1918, p. 1) and stated that *"the kindergarten teacher can render service to the immigrant*

mother in helping her plan for the education of her children, in showing the advantages of keeping the children in school regularly, and of having them continue their studies, in keeping her informed on the kinds of employment available for her children" (U. S. Bureau of Education, 1919, p. 5*)*. One positive side effect of the home visits and mothers' meetings is that they might have improved the English skills of immigrant mothers. In Section IV.D, we show that such language spillover effects in fact existed.

Overall, the historical narrative suggests that the interaction between kindergarten teachers and mothers was a crucial element of the kindergarten pedagogy. It implies that the kindergarten treatment, besides the 3-4 hours per day in the classroom, also involved regular interactions between teachers and mothers. Home visits and mothers' meetings provided mothers with information about child-rearing practices and conveyed the value of early childhood education and schooling which presumably affected mothers' perception about the return to schooling and the costs of child rearing. Hence, the kindergarten movement also affected households' budget constraints by altering the time costs of raising children. In Section IV, we show that households adjusted fertility broadly consistent with the prediction of an augmented quantity-quality model of fertility.

III. Data

Our empirical analysis draws on a series of official education reports that contain detailed information about kindergartens. For our case study, we digitize annual reports of the St. Louis public school board for the years 1873 to 1886. These reports contain, among other things, enrollment numbers and the exact location and opening date of every public kindergarten in the city. We geo-reference the locations of the kindergartens operating in St. Louis between 1873 and 1886.[9]

Our city-level analysis is based on newly digitized kindergarten records collected by the U.S. Bureau of Education for the years 1874, 1880, 1886-87, 1890-91, annually from 1895-1896 to 1909-1910, and for 1912 from Bulletin No. 6 of the U.S. Bureau of Education in 1914.[10] While the reports before 1888 contain information per kindergarten and their corresponding sponsor (associations, public or private kindergartens), the later reports usually include only information about the total number of public kindergartens, teachers, and pupils in cities with more than 4,000 or 8,000 inhabitants. Exceptions are the reports for the years 1886-87, 1890-91, 1897-98, 1901-02, and 1912, which also list the locations and number of free kindergartens operated by charity organizations. The location of private kindergartens at

[9] The first public kindergarten in St. Louis opened its doors in September 1873 (St. Louis Annual Report, 1875, p. 195), which falls into the school year 1873-74 (at that time the school year in St. Louis ended on August 1). By 1886, 52 kindergartens operated within the city borders of St. Louis. The kindergarten at Lowell school is the only one we could not locate. We thank Adele Heagney from the St. Louis Public Library for helping us geolocating the St. Louis kindergartens.
[10] The reports of the Commissioner of Education do not contain detailed kindergarten statistics between 1892-1895.

the city level was only reported in detail before 1890 and in 1912. We construct a city-level kindergarten panel dataset based on these reports covering the years 1874-1880 and 1887-1912.

The second main data source is a digitized collection of historical complete-count census records provided by IPUMS (Ruggles et al., 2020). Our main sample consists of repeated cross-sections of about 8.5 million white women aged 18-44, who are listed as household head/spouse in the census and resided in cities during the period 1880-1910 (no data are available for 1890). We also used the complete-count census data to study the outcomes of children. The census data are then merged with the kindergarten data based on year and location. Other secondary datasets are introduced in the relevant sections of the empirical analysis below. Appendix Table 1 presents detailed summary statistics.

IV. The Effect of Kindergartens on the American Family

A. The Kindergarten Movement in St. Louis – A Quasi-Natural Experiment

Our empirical analysis begins with a case study that evaluates the roll-out of the first publicly sponsored kindergartens in the U.S.: the kindergarten "experiment" in the St. Louis public school system. St. Louis, with a population of around 350,000 in 1880, was one of the main commercial and industrial centers in the American Midwest during the late 19th century. Like other rapidly industrializing cities, St. Louis faced the problems of a rapidly growing city (the population of St. Louis almost doubled between 1860 and 1870). Families living in St. Louis were often poor, relied on child labor, and many of them were immigrants. A survey of St. Louis' neighborhoods conducted in the late 1860s revealed that children of the levee and factory districts spent only three years, or fewer, in school because they started working in factories when they were as young as ten years old (Troen, 1972).

The initiator of this survey, William T. Harris, school superintendent in St. Louis (1868–1880) and later U.S. Commissioner of Education (1889–1906), was alarmed about these children's short school life and suggested that the Board of Education introduce the Froebelian kindergarten concept as a solution to this problem. He believed that kindergarten education could facilitate entry into the public school system, increase years of schooling, and avoid the early transition to child labor. Kindergartens, Harris argued, would remove poor children from the street, build their character, and train them in the necessary skills to become industrious persons later in life (St. Louis Annual Report, 1877, pp. 79-119).

In 1873, Harris appointed Susan E. Blow, an enthusiastic kindergarten teacher and advocate of Froebel's teaching methods, to operate at the Des Peres School in St. Louis the first public kindergarten in the U.S. This kindergarten started as an experiment to study *"the practical effects of Froebel's system"* (St. Louis Annual Report, 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 Annual Report 1876, p. 95). By 1875, kindergarten education was already offered in seven schools with about

450 pupils regularly attending (St. Louis Annual Report 1876, p. 98).[11] Impressed by the pupils' progress, 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 Annual Report 1881, pp. 152-53). At that time, most schools were already involved in kindergarten work; by 1886 more than 50 kindergartens operated within the city borders of St. Louis (Troen, 1972; Lascarides and Hinitz, 2013). Figure 1 illustrates the roll-out of kindergartens in St. Louis from 1873 until 1886.

The establishment of a public kindergarten system in St. Louis was a major step towards the universal acceptance of kindergartens in the U.S. In the following years, St. Louis became a center from which the kindergarten movement spread across the country. 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 up in other American cities over the next decades. St. Louis demonstrated that kindergartens can be successfully integrated into the public school system (Vandewalker, 1908; Troen, 1972; Lascarides and Hinitz, 2013) and provides an interesting case in point to study the effect that the roll-out of kindergartens had on fertility.

We geo-reference each kindergarten listed in the annual school reports and assign them to their corresponding enumeration district in the 1880 Census.[12] The kindergarten locations are then merged with the complete count U.S. census data, who contain the exact location of households in St. Louis together with other important information on socioeconomic characteristics at the individual and household level, such as age, gender, enumeration district, occupation, birthplace, and the number of children in 1880. Our sample consists of every 18- to 44-year-old white woman in St. Louis in 1880 who is listed as a household head or spouse. Since the census reports the age for 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". In order to avoid potential issues associated with children leaving their 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.[13] Hence, the

[11] In order 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).
[12] We used the website https://stevemorse.org/census/unified.html?year=1880 of Steven Morse and Joel Weintraub to assign the location of 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; see also Figure 2.
[13] 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 (NCHILD) a woman reported in the 1880 census. We only consider own children in

"mother panel" spans the years 1870 to 1880 period containing the cumulative number of births per woman in a given year. Figure 2 depicts the location of public kindergartens in our sample together with geo-referenced households and enumeration districts in St. Louis as reported in the 1880 Census.

We first present a balancing test that shows whether there are any substantial mean differences in observable characteristics of interest for women in the sample by treatment status, i.e., if a woman's enumeration district had a kindergarten by 1880 (the 1880 Census is the first containing information on enumeration districts). The mean values presented in Appendix Table 2 reveal that women were of similar age and foreign background. Yet women in enumeration districts with a kindergarten were somewhat less likely to work but were more likely to be German immigrants. Their husbands had a somewhat higher occupational income score, were more likely to be foreign-born, but less likely to work in blue-collar skilled occupations or as day laborers.[14] It is therefore important to account for these baseline differences by including individual fixed effects in the empirical analysis below.

According to the prediction of a standard quantity-quality framework of fertility (e.g., Galor, 2011), one would expect that access to kindergarten education, by increasing both the returns to education and the direct costs of having children (e.g., by higher child-rearing cost and reduced household income from child labor), should incentivize parents to reduce the optimal number of children. We test this prediction using a difference-in-differences approach, which exploits the fact that kindergartens opened their doors in different enumeration districts at different points in time. One potential threat to identification would be if the fertility pattern in treated enumeration districts would have already evolved differently before the kindergarten opened. Since we know the exact establishment date of every kindergarten in the sample, we conduct an event study to observe the dynamic effects of kindergarten openings on fertility and, at the same time, to test whether the coefficient of interest shows any sign of existing pre-trends.

More formally, we use the following estimation equation to evaluate the dynamic effects of kindergarten openings on fertility:

$$y_{iet} = \alpha_i + \alpha_t + \sum_{j \in T} \beta_j \, Kindergarten_{et}^{\tau+j} + \Gamma X_{iet} + \epsilon_{iet} \qquad (1),$$

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},

the household. These are indicated in IPUMS by the variable RELATE == 3. Children-mother pairs are identified using the variable MOMLOC. See IPUMS for detailed variable descriptions.

[14] We refer to professionals, managers, clerical and sales workers as white-collar jobs (IPUMS variable OCC1950 codes 0-490, excluding farmers and farm managers) and to craftsmen as blue-collar skilled occupations (codes 500-595). The skill classification of occupations follows Katz and Margo (2014).

denotes the cumulative number of births of a woman *i* residing in enumeration district *e* in year *t*.[15] The parameter τ refers to the year of a kindergarten opening in enumeration district *e*. $Kindergarten_{et}^{\tau+j}$ is an indicator equal to 1 when $t = \tau + j$ 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 persistent 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.

Figure 3 depicts the event study based on estimating equation (1); the corresponding estimates are reported in column (1) of Appendix Table 3. We find that for all j < 0, $\beta_j \approx 0$, which supports the identifying assumption of common pre-trends. After treatment, the estimated coefficients become negative and statistically significant, implying that the establishment of kindergartens caused a decline in fertility. From Figure 3, it is also apparent that the fertility decline occurred gradually, which seems reasonable as it takes some time until mothers fully internalize the benefits of kindergarten education for their children. Appendix Figure 1 shows the event-study results for the subsample of first- and second-generation immigrant mothers (about 80 percent of the sample); the corresponding estimates are displayed in column (2) of Appendix Table 3. Our estimates reveal that the negative effect of kindergarten openings on fertility is primarily driven by immigrant households, which is consistent with the historical narrative that the kindergarten movement targeted especially immigrant households.

In order to test the robustness of our results, we also consider proximity to the closest kindergarten instead of the enumeration district as treatment criterion. In particular, we define households as being treated if they were living within 250 meters of an open kindergarten, and we restrict the sample such that only households located between 250-1,000 meters of an open kindergarten serve as control units. Appendix Figure 2 shows that the negative effect on fertility is driven by households that were living in close proximity to a kindergarten (see Appendix Figure 3 for an illustration of the identification strategy based on household's distance to a kindergarten). The result suggests that exposure is closely connected

[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.

to households' proximity to kindergartens. We also used this setting to test for the presence of potential spatial spillover effects. For example, households in the control group could have taken up treatment as well, which would lead to an underestimation of the true kindergarten effect. We thus adopted a so-called "*distance bins approach*" defining the control group at different distance bins, namely as being between 500-1,000 or 750-1,000 meters away from the kindergarten. The results in Appendix Figure 4 and columns (3)-(5) of Appendix Table 3 show that spatial spillover effects are not a major issue in our context.

The key identifying assumption of our difference-in-differences approach is that fertility behavior in the treatment and control group would have moved in a parallel fashion in the absence of the treatment. While the common trends assumption cannot be directly tested, our pre-treatment estimates provide strong support to this assumption. Yet one recent issue discussed in the econometrics literature is that difference-in-differences models can produce unreliable estimates of average treatment effects if heterogeneous effects are present across groups or/and time (e.g., de Chaisemartin and D'Haultfoeuille 2020; Sun and Abraham, 2020; Goodman-Bacon, 2021). To address this concern, we follow Cengiz et al. (2019, Online Appendix D) and consider each event separately. In particular, we created 58 datasets, one for each treated enumeration district and defined as "clean controls" those enumeration districts which did not experience any kindergarten opening during the event window. Successively, we stack the datasets and perform a "*stacked event study*" analysis including individual fixed effects interacted by event fixed effects to account for the fact that the same mother could appear multiple times in the stacked dataset. Standard errors are clustered at the enumeration district level. The results of the stacked event study reported in Appendix Figure 5 and column (6) of Appendix Table 3 confirm the pattern observed in Figure 3. The absence of a pre-treatment trend is not a statistical artifact due to time-varying treatment and the negative effect of kindergarten openings on fertility is also present using the stacked approach.

Finally, we test whether kindergarten openings affected the attendance rate for children of kindergarten age. Since the 1880 Census asked whether an individual attended school within the past year, we can construct a dummy variable for whether a 5 to 6-year-old child attends school. These children are the main target group of kindergartens according to the official school reports and, hence, attending "school" at that age would very likely mean attending a kindergarten. For the analysis, we keep the sample of mothers, but only include those with a 5 to 6-year-old child in 1880. The outcome variable is a dummy whether their 5 to 6-year-old attends school. Since we only observe school attendance in 1880, the specification cannot control for individual and time fixed effects. Instead, we keep the fixed effects for the years since an enumeration district had access to a district school and mother's age. We also add fixed effects for mother's birthplace and literacy status, and a set of dummy variables whether her husband works in a white-collar/blue-collar skilled occupation (see footnote 14 for more details). The attendance

dummy is then regressed on these controls 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 (since there are only a few cases of kindergartens operating in 1880 for more than 5 years, we group them together). Figure 4 displays the coefficients of interest. The estimates reveal that the probability to attend a kindergarten at age 5-6 increased significantly in the first year after the opening and remained fairly constant in 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 12-17 percentage points compared to enumeration districts without a kindergarten. Relative to a mean of 33 percent, the estimated effect is substantial.

In the next subsection, we turn our focus to the kindergarten movement in other American cities. We will see that for mothers adjusting fertility in exposed cities, it was crucial to have a child of kindergarten age to fully internalize the costs and benefits of a kindergarten education.

B. The Kindergarten Movement in American Cities between 1880–1910

Did mothers adjust their fertility behavior in response to the roll-out of kindergartens during the late 19th and early 20th centuries in other American cities? Since the kindergarten statistics published by the U.S. Bureau of Education did not provide such detailed information as the St. Louis school reports, we do not present event-study estimates as in the previous subsection.[16] Yet these reports contain detailed statistics on the total number of kindergartens, allowing us to exploit temporal and spatial variation across cities in the intensity of kindergarten exposure for identification.

Our empirical analysis starts in 1880, the decade referring to the onset of the kindergarten movement, and it ends in 1910 before the disturbances associated with the outbreak of WWI and the takeoff of the high school movement (Goldin and Katz, 2008). The main sample consists of repeated cross-sections of white woman aged 18-44 listed either as household head or spouse in a given city *c* and census year *t*. Our baseline econometric model is outlined by the following equation:

$$y_{ict} = \alpha_c + \alpha_t + \beta Kindergarten\ Exposure_{ct} + \Gamma X_{ict} + \epsilon_{ict} \qquad (2).$$

The main outcome variable, y_{ict}, is a woman's number of own children under age 5 (see, e.g., Bleakley and Lange, 2009; Ager et al., 2020), but we also consider school attendance of 5 to 6-years-old as an outcome.[17] Our measure of interest, *Kindergarten Exposure*, reflects for a woman of childbearing age the

[16] While the Bureau of Education collected information on the establishment date of the first public kindergarten in a city in some years, it is incomplete and for some locations opening dates are missing.

[17] For 1880 and 1910, we used the IPUMS variable "SCHOOL" and "SCHLMNTH" for 1900 (due to some error, IPUMS requested full count users to construct school attendance in 1900 based on the variable "SCHLMNTH").

expanded opportunity for sending a child to a kindergarten at the time when she conceived a child. It broadly follows the concept of Aaronson et al. (2014) and is constructed as:

$$Kindergarten\ Exposure_{ct} = \frac{1}{5}\sum_{k=1}^{K=5} \frac{Kindergarten\ Capacity_{c,t-k}}{Children\ Age\ 5\ to\ 6_{c,t-k}} \quad (3),$$

where *Kindergarten Capacity* denotes the number of kindergartens in a given city multiplied by the average enrollment number of kindergarten pupils.[18] The capacity is normalized by the target population (children aged 5-6), which we obtain retrospectively based on the age of the children at the time of the census year. For a given city *c* and census year *t, Kindergarten Exposure* is the average of the normalized kindergarten capacity over the five years preceding the census.

For the fertility regressions, we add to equation (2) fixed effects for city, α_c, and census year, α_t, a city-specific linear time trend, and a set of individual controls, X_{ict}, which includes fixed effects for birthplace interacted by census year and by city, fixed effects for year of birth interacted by census year and by state of residence, dummy variables for literacy and marital status, and a set of spouse characteristics: These include the occupational income score (in logs) of the husband,[19] a set of dummy variables whether the husband worked in a white-collar/blue-collar skilled occupation was foreign-born, literate, or whether his occupation was still not classified by IPUMS.[20] In some specifications, we can exploit also variation *within cities* allowing us to control for city-by-year fixed effects. These additional set of fixed effects account for city-specific time varying demand shocks or any city-specific legislation implemented in different years, such as public health interventions, that could have changed fertility behavior at the same time as the kindergarten roll-out occurred. All specifications report standard errors that are clustered at the city level to account for correlations within a city in a given year and over time.

a. City Characteristics and Kindergarten Exposure

Before evaluating whether the roll-out of the first kindergartens affected fertility in American cities, it is informative to explore whether certain initial (1880) city characteristics predict kindergarten exposure in the subsequent census years. Appendix Table 4 summarizes the results, where we regress kindergarten exposure in 1900 or 1910 on a set of city-level socio-economic covariates in 1880 and state fixed effects. The set of covariates includes cities' average occupational score, the share of white-collar and blue-collar

[18] For 1900-10, we used 50 pupils as the average capacity for free and public kindergartens and 25 pupils for private ones, reflecting their smaller scale. These numbers are based on the average enrollment rates over the period 1887-1912 by kindergarten type. For 1880, we used 30 pupils as the average capacity based on the 1874 and 1880 report.

[19] Since the census did not collect individual income data before 1940, we proxy husbands' income by the occupational income score from IPUMS (e.g., Jones and Tertilt, 2008).

[20] The complete-count data for the census years 1900 and 1910 still contain some occupation strings that IPUMS has not yet classified for the variable OCC1950 (code 979). We flag these observations in all our regressions.

skilled workers, the share of 10 to 15-year-old children working, the share of foreign-born, the crude birth rate, log city size, and the share of 18- to 44-year-old women that are working and married. These measures intend to capture the economic and demographic structure of a city. We further add the literacy rate, the share of 5- to 21-year-old attending school, and the number of teachers per capita as proxies for human capital. It turns out that most of these covariates are not systematically related to kindergarten exposure and statistically insignificant. Only log city size and teachers per capita are correlated with kindergarten exposure in 1900 and 1910. The inclusion of city-by-year fixed effects in the main analysis aims at accounting for these factors. Furthermore, we show in Subsection IV.B.d that our results are not explained away by the expansion of the public school system that also took place at this time.

b. Kindergarten Attendance

Next, we check whether our kindergarten exposure measure, as defined in equation (3), is related to actual attendance of kindergarten-age children. The analysis in this subsection focuses on 5 to 6-year-old white children, who according to official school reports are considered the main target of kindergartens during our sample period. One would expect that kindergarten exposure is positively correlated with the probability of 5 to 6-year-old children attending a kindergarten ("attending school") in the census year. Appendix Figure 6 displays a binned scatterplot of this relationship after controlling for year and county fixed effects, which is positive and highly statistically significant indicating that our kindergarten exposure measure captures also actual attendance. The positive association between kindergarten exposure and attendance remains highly statistically significant and quantitively unchanged after adding controls for individual and parental characteristics. We present this results in column (1) of Table 1. Columns (2)-(3) reveal that there is no significant difference in kindergarten attendance by gender. The remaining columns of Table 1 report sample splits by age. The estimates reveal that the relationship between kindergarten exposure and attendance is the strongest for 5 to 6-year-old children as one would have expected.

c. Kindergarten Exposure and Household Fertility

After having verified that our kindergarten measure actually captures school attendance of kindergarten-age children, our focus turns to the relationship between kindergarten exposure and household fertility. The fertility measure we use in this subsection is the number of children under age 5 in the own household. Our analysis is based on the full-count sample of 18- to 44-year-old white women, who resided in American cities during the period 1880-1910. We estimate equation (2) by ordinary least squares including, besides kindergarten exposure, fixed effects for city and year, the city-specific linear time trend, and the set of individual controls, X_{ict}, as outlined above.

The baseline results are summarized in column (1) of Table 2 (Panel A). The estimated coefficient on *Kindergarten Exposure* is negative but modest in size and only statistically significant at the 15-percent level

(the *p-value* is 0.136). The point estimate suggests that full exposure to kindergartens is associated with a 0.04 decline in the number of children below age 5, which is approximately 6 percent of the sample mean. The next two columns of Table 2 (Panel A) reveal why the overall effect of kindergarten exposure on fertility is relatively modest. The result in column (2) shows that household fertility substantially declines in cities with more kindergarten exposure if the family has a child of kindergarten age, while kindergarten exposure does not affect fertility decisions in families that *only* have small children (below age 5). The point estimate on kindergarten exposure in column (3) is close to zero and not statistically significant.

We obtain the result presented in column (2) by adding to estimating equation (2) an interaction term of kindergarten exposure with a dummy variable for whether a household has a 6- to-11-year-old child at the time of the census enumeration. Note that these children were of kindergarten age in the five previous years before the census enumeration; the period we consider kindergarten exposure to be relevant for family planning. In this slightly modified specification, we also always control for the direct effect of having a 6- to 11-year-old child in the household and we further include a dummy variable if the household has any older children (i.e., above age 11). For a given level of kindergarten exposure, families with a 6- to 11-year-old child experience a larger fertility decline compared to families facing the same exposure but without a 6- to 11-year-old child. The estimated coefficient on the interaction term is negative and statically significant at the 1-percent level, while for the reference group (i.e., families without a 6- to 11-year-old child) there is no statistically significant association between kindergarten exposure and fertility. The estimated coefficients on kindergarten exposure and the interaction term are statistically different from each other at the 1-percent level. The point estimate suggests that full exposure to kindergartens for households with a 6- to 11-year-old child is associated with a 0.11 decline in the number of children below age 5, which is approximately 16 percent of the sample mean.

We also investigated whether a specific age cohort of children matters particularly for the fertility decline. The regression is similar to the specification in column (2) but instead of a single interaction term we use a set of dummy variables that equal to one if a household has a child of age 5, 6, …, 11, or 12 and older interacted with kindergarten exposure (we always control for the direct effect of the age group). The reference group are households with children under age 5 or without any children. Appendix Figure 7 plots the estimated coefficients of interest by age and the corresponding 95-percent confidence intervals. There is no statistically significant relationship between fertility and the reference group or with kindergarten exposure interacted with the age 5 dummy (the estimated coefficients are close to zero). As before, for a given level of kindergarten exposure, families with a 6- to 11-year-old child reduced fertility, but the size of the estimated coefficient is largest (in absolute terms) for families with a 6-year-old and gradually declines afterwards. The interaction term between kindergarten exposure and having children

of age 12 and older is still negative and statistically significant but quantitatively rather small. These findings illustrate that kindergarten exposure mattered for fertility once families could have been directly in contact with kindergarten education but not beforehand.

Since the 1880s the kindergarten movement primarily focused on addressing the problems in poor urban neighborhoods where birthrates were high and child labor common. Hence, one would expect a larger fertility decline in cities with a higher initial child labor share. We test whether this was the case in columns (4)-(5) of Table 2 (Panel A), where we split the sample by cities with a child labor share in 1880 below and above the median (the median child labor share in the sample was 0.129 in 1880). In both specifications, households significantly reduce fertility in cities with higher kindergarten exposure once they have a child of kindergarten age. The estimated coefficient on the interaction term is negative and statistically significant at the 1-percent level. Yet the point estimate on the interaction term in column (5) for households in cities with a child labor share above the median was about 2.5 times as large. This suggests that fertility rates of targeted poor families were more responsive to kindergarten exposure.

Our next goal is to evaluate whether the fertility decline associated with the roll-out of kindergartens was mainly driven by immigrant households, who often resided in the poorest neighborhoods of a city (Ward, 1971). The particular focus on immigrant households is also motivated by the historical narrative (see Section II for details): Contemporary school reports and discussions in general educational journals or more specialized outlets such as the *Kindergarten Magazine* revealed that home visits and mothers' meetings were key for kindergarten teachers to gain access into immigrant homes (Berg, 2004). If these meetings changed mothers' perception about the importance of child rearing and the value of early childhood education in general, one would expect fertility to change once the mothers were in direct contact with kindergarten teachers. Given the potentially higher returns to education for immigrant children and the more stringent budget constraint of immigrant households, it is plausible to expect larger fertility reductions for mothers born abroad. The final two columns of Table 2 (Panel A) show that this was the case. Although U.S.-born and foreign-born mothers significantly reduce fertility in cities with higher kindergarten exposure once they have a child of kindergarten age (the estimated coefficient on the interaction term is statistically significant at the 1-percent level), the size of the estimated coefficient on foreign-born mothers is about twice as large. The stronger fertility decline for foreign-born women is consistent with the results shown in Subsection IV.C that children of immigrant families experienced large returns from kindergarten education. The estimate presented in column (7) is also sizeable: the roll-out of kindergartens explains about 12 percent of the overall fertility decline immigrant mothers experienced over the sample period.

One important technical advantage of this modified specification is that we can include city-by-year fixed effects and thereby exploit only the impact that kindergarten exposure has on different types of households (with and without 6- to 11-year old children) within the same city and census year. In this way, we account for any time varying city-specific shock that could coincide with the timing of the kindergarten roll-out. This also implies that the direct effect of kindergarten exposure is absorbed due to the inclusion of city-by-year fixed effects. Hence, any remaining threat to identification would need to differentially affect households with and without 6- to 11-year-olds. We present these results in Panel B of Table 2. The estimating equation is (2) and the method of estimation is ordinary least squares. The set of controls also includes now the interaction terms of birthplace, year and city fixed effects and birth cohort-by-city fixed effects. Reassuringly, the estimated coefficients based on this more demanding specification are quantitatively similar to the estimates presented in Panel A of Table 2. The point estimate on the interaction term is always negative and statistically significant at the 1-percent level, thereby mitigating the concerns of unobserved time varying city-specific factors driving our results.

Overall, our results show an economically meaningful negative effect of kindergarten exposure on fertility. Direct contact of mothers with kindergarten teachers appears to have played a crucial role in changing fertility behavior in American cities, especially for immigrant families. Our results that mothers reduce fertility once they have a child of kindergarten age in cities with higher kindergarten exposure are consistent with this notion. Nevertheless, one remaining concern is that our findings might reflect the general expansion of the public-school system occurred towards the end of the 19th century. The inclusion of city-by-year fixed effects should mitigate this concern to a great extent, but we cannot rule out that an expanding public-school system had a differential impact on our target groups. The next subsection deals with this particular issue in detail.

d. Robustness to the Expansion of Public Schools

At the time of the kindergarten roll-out, the U.S. experienced a general expansion of the public school system which aimed to promote the education of the masses (Meyer et al, 1979; Goldin and Katz, 2008; Parman, 2011). Annual expenditure per pupil increased between 1880 and 1910 from 8 to 25 US dollars. While the expenditure figures are denoted in current dollars, the corresponding increase of the enrollment rate of 5- to 17-year-old children in public schools from 65.5 to 74.2 percent indicate that the expansion was real. Pupils also went to school for more days a year: The average length of the school term increased from 130.3 to 156.8 days over the same time period (Snyder, 1993). While the spectacular increase in secondary enrollment rates took place between 1910-1940, several cities, most of them located in New England, already operated high schools during our sample period. Despite high school graduation rates were still below 10 percent in 1910, the expansion of the public school system could have triggered a

general fertility decline, since it allowed parents from all social classes to invest more in the education of their children (Goldin, 1998; Black and Sokoloff, 2006; Galor, 2011).

Is the fertility decline in poor and immigrant households that we associate with exposure to kindergartens driven by a general expansion of the public school system during our sample period? To address this question, we digitize city-level data on the number of public school and high school teachers from the reports of the Commissioner of Education for the years 1880, 1900/01 and 1910/11. Based on these data, we construct measures of exposure to high schools and public schools. For every census year, city-level exposure to public schools is defined as the number of public school teachers (net of public kindergarten teachers) multiplied by an assumed class size of 35 relative to the number of children between the ages 5 to 21. High school exposure is calculated similarly with high school teachers multiplied by an assumed class size of 35 relative to the number of children over the ages 14-18.[21] Both measures are also interacted with a dummy variable for whether a household has a 6- to-11-year-old at the time of the census enumeration. We use the same specification as in column (1) of Table 2 (Panel B), but constrain the sample to observations where data on public schools and high schools are available.

The results accounting for exposure to public schools and high schools are summarized in Table 3. Column (1) shows the baseline result to facilitate comparison across different specifications. In column (2), we add our measure of public school exposure, while in column (3) we control for exposure to high schools. The specification presented in column (4) includes both measures together—they enter with a negative sign and are highly statistically significant. These estimates suggest a negative relationship between the rise of mass education and fertility during the second phase of the industrial revolution in the U.S. The point estimate on our measure of interest, the interaction term of kindergarten exposure and having a 6- to 11-year-old child, shrinks by about 40 percent but it is still statistically significant at the 5-percent level. Importantly, even after accounting for the general expansion of the public school system, kindergarten exposure still substantially reduced fertility of families living in cities with a high initial child labor share (column 6) and in immigrant homes in particular (column 8). In both columns, the point estimate is at least statistically significant at the 5-percent level.

e. Further Robustness Checks

Despite only 9 percent of the women in our sample work and recent empirical evidence finds no systematic relationship between fertility and female labor supply in the U.S. before WWI (Aaronson et al., 2021), one immediate question is whether a change in the labor supply of affected mothers can explain away the effect of kindergarten exposure on fertility if the time children spent in classroom freed up

[21] An assumed class size of 35 reflects the average pupil-teacher ratio between 1880-1910 (Snyder, 1993; Table 14).

maternal labor supply (albeit the historical narrative suggests that mothers simply left children unattended in the streets while being at work).[22] Column (1) of Appendix Table 5 shows that, once we add to the baseline specification of column (2) of Table 2 (Panel B) a dummy variable for whether a woman is working, the point estimate on the interaction term remains qualitatively unchanged. In column (2), we show that controlling for the duration of marriage does not affect our results (since information on duration of marriage is only available for 1900-10 the point estimates need to be compared to column (6) of Appendix Table 6).[23] The 1900-10 Censuses also include information about the number of children ever born and surviving to each ever-married woman. We use this information to construct a measure of child mortality that we include as a control in column (3). The estimated coefficient on the interaction term changes very little, suggesting that variation in child mortality is not driving our results. Finally, we obtain a similar result in column (4) including all three controls (only for years 1900-10). Overall, our findings suggest that changes in maternal labor supply, a delay of marriage, or changes in child mortality cannot explain away the negative relationship between kindergarten exposure and fertility.

One drawback of using the IPUMS city identifier is that not all cities are identified across all years; as a result, we have 220 cities in 1880 and around 600 cities in 1900 and 1910. Appendix Table 6 illustrates that our results also hold when using a balanced panel of cities (columns 1-2) or when considering only the census years 1880-1900 (columns 3-4) or 1900-1910 (columns 5-6). Interestingly, the fertility decline is stronger in cities with higher kindergarten exposure during the early phase of the kindergarten movement. We further show in Appendix Table 7 that our results are not sensitive to normalizing kindergarten capacity by the number of 18- to 44-year-old women instead of 5- to 6-year-old children. As a final robustness check, we evaluate in Appendix Table 8 whether our results are driven by regional differences between northern and southern cities (columns 1-2) or by city size (columns 3-4). While the fertility decline associated with kindergarten exposure is similar across northern and southern cities, it mostly affected households in larger cities (the point estimate is 3 times as large).

C. Fertility Decisions of Exposed Children as Adults

Before we discuss the mechanisms that can explain the fertility decline associated with the roll-out of kindergartens in American cities between 1880-1910, we are interested in whether children exposed to kindergartens have fewer children as adults. For example, kindergarten exposure could have influenced occupational choices later in life and thus also the opportunity cost of having own children, or the negative effect of kindergarten exposure on their mothers' fertility might create preferences for small

[22] There is anecdotal evidence of a few businesses providing kindergartens for their workforce (Vandewalker 1908).
[23] The 1900 and 1910 censuses asked currently married persons how long they had been married to the present husband or wife (see IPUMS variable DURMARR for further details).

family size that persist across generations. While it is beyond the scope of this paper to explore these potential channels, we think it is worth studying whether such a pattern existed.

Recent advances in automated linking methods allow researchers to follow individuals across census years. The Census Linking Project (Abramitzky, Boustan, and Rashid, 2020) provides the crosswalks of linked males used in this analysis.[24] We use linked samples spanning the period 1900-20 and 1910-30. The sample is restricted to white boys aged 5-15 with a 20 to 55-year-old mother in the starting year, and we require that they had a spouse aged 18-44 in the terminal year. The estimates are based on estimation equation (4) using ordinary least squares. Panel A (B) of Table 4 reports the results based on the 1900-20 (1910-30) linked sample. Columns (1)-(3) summarize our results using the number of children below age 5 as outcome variable. In both samples, the relationship between childhood kindergarten exposure and fertility is driven by children with a foreign-born mother. The point estimate of the interaction term is always statistically significant at the 5-percent level and implies that full kindergarten exposure during childhood leads to a 0.05-0.07 decline in the number of children below age five. The results on family size shown in columns (4)-(6) reveal that males exposed to kindergartens during their childhood also live as adults in smaller families. This finding is also driven by second-generation immigrants.

Overall, our results reveal that the roll-out of kindergartens impacted the fertility decisions of immigrant families over two generations. Immigrant mothers reduced fertility once they were in direct contact with the kindergarten system; their offspring, who was exposed to kindergartens during their childhood, also decided to have fewer children later in life.

D. Mechanisms

So far, we have provided compelling evidence that the roll-out of kindergartens contributed to the fertility decline in American cities over the period 1880-1910. In what follows, we study the mechanisms that could explain the negative effect of kindergarten exposure on fertility. We have already shown that changes in maternal labor supply, delay of marriage, or changes in child mortality cannot explain away the negative association between kindergarten exposure and household fertility. Instead, we argue that the observed fertility decline associated with kindergarten exposure is consistent with the prediction of theories which emphasize the importance of human capital to the fertility transition during the second phase of the industrial revolution (Galor, 2011). If kindergarten attendance increased the returns to education, one would expect parents to invest more in the education ("quality") of their children but at the same time reducing the number of children ("quantity").

[24] We used the so-called "ABE-NYSIIS" standard links; see Abramitzky et al. (forthcoming) for details on the linking methods.

We first discuss the effects that kindergarten education was expected to have on enrolled children according to contemporary American educators. Based on the historical narrative, we develop a quantity-quality trade-off model of fertility as outlined in Galor (2011) but allow explicitly for investments in kindergarten education. According to the augmented model, parents would unambiguously reduce fertility if complementarities between kindergartens and regular schooling exist. We show in this section that such complementarities were likely at play. Children were more likely to attend school instead of taking up a gainful occupation at age 10-15, if they faced a higher exposure to kindergartens at age 5-6. Immigrant children from non-English-speaking countries further benefited from kindergarten education in terms of gaining English skills. Overall, we regard our results as broadly consistent with a standard quantity-quality trade-off mechanism according to which households reduce fertility in response to an increase in the returns to education and foregone family income from child labor.

a. The Socioeconomic Effects of Kindergarten Education

Towards the end of the 19th century, American educators generally acknowledged the importance of education in the child's first years of life. Kindergarten advocates argued that kindergarten education was important for the child's development of the practical, cognitive, and social skills which helped the young child in preparing for primary school but also for the work life as an adult (Lazerson, 1971b; Berg, 2004; Allen, 2017). In order to offer children from immigrant families a fair start, free and public kindergarten teachers also considered the socialization function of the kindergarten, in particular teaching the use of the English language, as a key aspect of their work (Berg, 2004). Exposing the children to the English language at such an early age would prepare them better for later schoolwork. Waite (1926, p. 37), for example, highlights in her summary of several surveys about kindergarten training in city schools that the inability of using the English language is a serious cause of slowing later schoolwork.

More generally, several contemporaneous surveys documented observations from school teachers about the beneficial effects of kindergarten training. Holden's (1905) survey on the effects of kindergarten training for primary school revealed that teachers regarded kindergarten training as good preparation for school studies. An inquiry from the U.S. Commissioner of Education sent to supervisors of schools, primary supervisors, and first-grade teachers in 127 cities revealed overwhelmingly positive sentiments towards kindergarten training, especially for foreign children (U.S. Bureau of Education, 1914, p. 93). These observations resonate with Palmer's (1915) survey, containing responses from superintendents, principals, and primary teachers on whether kindergarten children are better prepared for school. One of the most common observations was that children with kindergarten training have better soft skills, are more fluent in language, and are better to work with others. Overall, the historical narrative suggests that

the kindergarten training offered in many American cities at the turn of the 20th century increased the returns to education, especially for children from immigrant homes.

Yet one might wonder why the establishment of kindergartens did not provide incentives for families to have more children. Even if free kindergartens did not charge a tuition fee and public schools financed the kindergarten mainly via local school funds, it does not imply that kindergarten attendance for poor households was costless. Kindergarten enrollment increased childrearing costs, such as expenditures for proper clothes, shoes, and hygiene; it meant foregoing household income from child labor if the child stayed in the school system; and busy mothers needed to spend extra time with the kindergarten teacher (e.g., Lazerson, 1971b; Berg, 2004; Allen, 2017). In case complementarities between kindergarten education and regular schooling exist, households will reduce fertility *even* if the unit cost of preschool investment per child declines. We will develop this argument in the next subsection in more detail.

b. A Quantity-Quality Model of Fertility with Investments in Preschool

We set up a simple quantity-quality model of fertility with two types of potentially complementary investments, preschool and other investments in human capital (e.g., schooling), to illustrate how and why increased access to kindergarten education might negatively affect fertility. Let's consider a utility function of the following form $U = (1-\gamma)ln(c) + \gamma ln(n) + \delta ln(h(p,s))$ with household budget $c = y - n(\tau + \theta p + \sigma s)$, where c is consumption, y is income, n is the number of children, τ is the rearing cost of one child with no quality investment (possibly dependent on y), p is preschool investment per child (e.g., kindergarten education), s is investment in schooling (or other forms of investments in human capital of children), θ is the unit cost of preschool investment per child and σ is the unit cost of schooling per child.

We obtain the optimal number of children, $n^* = \gamma y/(\tau + \theta p + \sigma s)$, from solving the household's optimization problem with respect to n. Treating p and s as endogenous variables which are affected by the unit cost of preschool investment θ, results in $dn/d\theta = -[\gamma y((dp/d\theta)\theta + p + \sigma(ds/d\theta)]/[(\tau + \theta p + \sigma s)^2]$. If fertility falls due to a decline in preschool costs, one needs to assume that $dn/d\theta > 0$. This would require that $(dp/d\theta)\theta + p + \sigma(ds/d\theta) < 0$. If there is no complementarity between preschool and schooling (i.e., $ds/d\theta = 0$), then this condition would amount to $(dp/d\theta)(\theta/p) < -1$. That is, the elasticity of preschool investment with respect to preschool costs must be greater than 1 in absolute terms. If this is the case, and the unit cost of preschool per child declines, the associated increase in the demand for preschool would increase θp, which causes a decline in fertility. This result is similar to the one outlined in Galor (2011) for changes in the cost of child quality. Even if a decline in θ *decreases* θp, fertility may still decline if $ds/d\theta$ is sufficiently negative. This would be the case if strong complementarities between preschool education (p) and formal schooling (s) exist, such that an increase in the preschool investment per child

will induce parents to increase schooling, which then increases the costs of having a child. This effect is absent from Galor's (2011) baseline model.

Note that our model is silent about how parents learn about potential complementarities between kindergarten education and schooling (e.g., through direct contact with the kindergarten teacher). It only states that parents change their fertility behavior once they realize that such complementarities exist. If complementarities between investments in kindergarten education and schooling existed during our sample period, as the historical narrative already indicates, the observed fertility decline would be in line with the prediction of the augmented quantity-quality model of fertility as outlined above. The following subsection provides empirical evidence that suggests this was the case.

c. Empirical Evidence for a Quantity-Quality Tradeoff

We explore in this subsection whether exposure to kindergarten education increases the returns to education and, as a byproduct, leads to a decline of child labor as progressive educators at that time had hoped for. We thus turn our focus to study the effect of kindergarten education on school attendance and child labor by looking at 10 to 15-year-old children at the time of the census enumeration. Our estimation approach utilizes annual variation in kindergarten exposure across cities at the time when these children were age 5-6, which allows us to test whether kindergarten education left a trace on children's outcomes about 5-10 years after exposure. Since this approach does not require to link individuals over time, we can consider the impact of kindergarten exposure on both boys and girls.[25]

The econometric model of this subsection is described by the following equation:

$$y_{ibct} = \alpha_c + \alpha_t + \beta Kindergarten\ Exposure_{bct} + \Gamma X_{ibct} + \omega_{\mathrm{bs}} + \epsilon_{ict} \qquad (4),$$

where y_{ibct} is a dummy variable if child *i* born in year *b* living in city *c* in census year *t* attends school or is working. All 10 to-15-year-old included in our sample are listed as children at the time of the census enumeration. We only consider (i) U.S.-born children that lived in 1900-1910 in the state of birth; and (ii) foreign-born children who arrived in the U.S. early enough to be exposed to kindergarten education (i.e., they arrived at age six or earlier). This analysis is only based on the 1900-10 censuses for two reasons: Almost certainly, none of the 10 to 15-year-olds in 1880 would have attended a kindergarten, and the census did not ask questions about the year of immigration before 1900.

The variable of interest, $Kindergarten\ Exposure_{bct}$, is the kindergarten capacity (see page 17 for further details) of city *c* normalized by the number of children age 5-6 at the time when a child of cohort *b* was of age 5-6 (e.g., a 12-year-old child in 1910 is assigned the average kindergarten exposure of 1903-

[25] The basic assumption is that children stayed at the place of kindergarten exposure as early adolescents. In fact, if there is no selective migration induced by kindergarten exposure, as it is reasonable to assume at that time, migration would attenuate the results towards zero.

04). All specifications include fixed effects for city and census year. The set of controls, X_{ibct}, includes fixed effects for birthplace interacted by year and by city, birth cohort, gender, and a set of parental controls including fixed effects for mother's birthplace, father's birthplace, parents' joint occupational score as a proxy for household income, a set of dummy variables including mother's literacy, whether the mother was working, father's literacy, whether the father worked in a white-collar/blue-collar skilled occupation, and whether the father was absent at the time of the census enumeration. We also include a dummy variable whether each parent's occupation was still not classified by IPUMS. Estimating equation (4) further includes state-by-birth cohort fixed effects, ω_{bs}, which account for state-specific factors that would affect cohorts across states and their outcomes differentially, such as state-specific legislation regulating child labor and compulsory education that could directly affect child labor and school attendance (e.g., Lleras-Muney, 2002; Lleras-Muney and Shertzer, 2015; Clay et al., 2021). Since we are particularly interested in the impact of kindergarten exposure on educational outcomes of immigrant children relative to children of native parentage, we can replace in some specifications ω_{bs} with city-by-birth cohort fixed effects. In this case, we identify the differential effect of kindergarten exposure on immigrant children by exploiting only variation within the same city. We cluster standard errors at the city level to account for correlations within a city in a given year and over time.

Table 5 (Panel A) presents the results for school attendance based on estimating equation (4). The method of estimation is ordinary least squares. Column (1) shows that children previously more exposed to kindergartens are more likely to attend school at age 10-15. The estimated coefficient on kindergarten exposure is statistically significant with a p-value of 0.07. Since we observe a stronger fertility decline for immigrant households, we additionally interact kindergarten exposure with a dummy variable for whether the mother was foreign-born in the remaining columns of Table 5 (Panel A). Consistent with a quantity-quality tradeoff interpretation of our findings on fertility, the estimates in column (2) reveal that the increase in school attendance was mainly driven by children from immigrant homes. Column (3) repeats the previous specification, but we replace ω_{bs} with city-by-birth year fixed effects (hence, the main effect of kindergarten exposure is absorbed). Results change very little: the estimated coefficient on the interaction term remains sizeable and it is statistically significant at the 1-percent level. Full kindergarten exposure when children were 5 to 6-years-old increased their likelihood of attending school as young adolescents by 9 percentage points. The remaining columns split the sample by age and reveal that this result is mainly driven by the oldest cohort (age 14-15).

Next, we consider whether the increase in school attendance of immigrant children is also reflected in a decline in child labor at age 10-15.[26] Since child labor was more common in immigrant homes and youth employment rates were gradually falling between 1880 and 1910 (Carter and Sutch, 1996), it is interesting to evaluate whether the roll-out of kindergartens contributed to this decline.[27] According to a standard quantity-quality framework of fertility, an accompanied increase in the direct costs of having children due to reduced household income from child labor would reinforce the fertility decline (Galor, 2011). The results presented in Table 5 (Panel B) show that this is the case. The specifications are the same as in Table 5 (Panel A), but the outcome variable is a dummy for whether a 10 to 15-year-old reported a gainful occupation.[28] Again, results are driven by children from immigrant homes (columns 2-3). Those are substantially less likely to work if they were more exposed to kindergartens at age 5-6. This effect is a result of older children reducing their labor supply (columns 4-6) and accounts for a substantial decline in immigrant child labor in American cities between 1900 and 1910.

In the remainder of this subsection, we evaluate whether kindergarten education helps children of non-English-speaking households acquiring English proficiency. One major goal of the kindergarten movement in the U.S. was to facilitate the integration of immigrant children by teaching English through songs, rhymes, and stories. Since the 1900-10 Censuses provide information on whether an immigrant 10 years of age and over is able to speak English, we can test whether exposure to kindergartens increases the likelihood of immigrant children speaking English. In case kindergarten education promotes English fluency, which is an important input for immigrants to acquire human capital in the host country, one would expect an increase in the returns to schooling for immigrant children.[29]

For this analysis, we restrict the sample to 10- to 15-year-old immigrants from non-English-speaking countries without imposing any restriction on their year of arrival. This information will be crucial for identifying a potential language effect from kindergarten education. In particular, we add to estimating equation (4) an interaction term between kindergarten exposure and a dummy whether a child arrived early enough to be exposed to kindergarten education in the U.S. (a child in our sample is regarded as "eligible" if it arrived at age six or earlier). We also always control for the direct effect of arriving in the U.S. earlier in life (i.e., the "eligible" dummy). This specification allows us to control for city-by-cohort

[26] Note, there is no mechanical relationship between school attendance and working, as children could attend school and still report a gainful occupation at the same time, or they chose to be "idle" (no activity) instead.
[27] In our sample, the share of 10 to 15-year-old with foreign (native) parentage reporting a gainful occupation fell form 20 (11) percent in 1880 to 9 (6) percent in 1910, a decline by about 55 (25) percent over 30 years.
[28] We further excluded children from the analysis that had their occupations still not yet classified by IPUMS.
[29] The economic gains of acquiring English skills in the U.S. today are well documented (Bleakley and Chin, 2004; Chiswick and Miller, 2015), but recent evidence cast doubt whether the returns to English fluency were as high historically (Ward, 2020).

fixed effects since we exploit variation between treated and non-treated immigrant children within the same city. We further include the same set of control variables as in Table 5.

Table 6 summarizes the effect that kindergarten exposure has on the probability of foreign-born children speaking English. The results presented in column (1), which for expositional purpose include state-by-cohort fixed effects instead of city-by-cohort fixed effects, are striking. We only find a positive and statistically significant effect of kindergarten exposure for treated cohorts while the estimated coefficient on kindergarten exposure, which captures the effect on non-eligible children and acts as placebo, is insignificant. In column (2), we add city-by-cohort fixed effects to estimating equation (4). Reassuringly, the effect of the interaction term on the likelihood of speaking English is similar to column (1). Columns (3)-(5) present the results by age. In contrast to the results in Table 5, the effect of kindergarten exposure on English fluency is driven by the youngest cohort in our sample (age 10-11).

E. Language Spillover Effects to Mothers

The results presented in Table 6 reveal that kindergarten education increases English fluency of foreign-born children from non-English-speaking sending countries. Next, we investigate whether language spillover effects from children attending a kindergarten to their mothers existed. The historical narrative emphasized frequent interactions between mothers and kindergarten teachers through home visits and mothers' meetings. It is therefore plausible to assume that mothers could have been directly or indirectly affected by the kindergarten education of their children. We focus on one particular possible spillover effect, that is, whether kindergarten attendance of children from non-English-speaking households enhances their mother's ability to speak English.

As before, we limit the sample to households from non-English-speaking sending countries in 1900-10. We impose the following additional constraints: We consider only eligible households, i.e., parents must have a 5- to 6-year-old child, both parents need to be younger than 50 years, and they must have been at least 14 years of age or over when they arrived to the U.S. This avoids the possibility that both parents may have themselves benefited from being educated in the U.S. In order to establish the existence and magnitude of spillover effects, we exploit the differential effect that kindergarten *attendance* can have on mothers. In particular, we assume that if such spillover effects exist, they were larger for mothers compared to fathers because of the predominant role of women in child rearing at that time.

We can therefore estimate a model with household fixed effects which has the advantage of accounting for time-invariant family characteristics like preferences for education. We further control for occupation type, occupational income score, and literacy status of both parents that could affect the decision to send their children to the kindergarten. Even if this model cannot fully account for selection, it provides

important insights whether immigrant mothers, who send their children to the kindergarten, integrate faster in terms of acquiring basic English language skills.

We run separate regressions for the years 1900 and 1910 using the following econometric model:

$$y_{if} = \alpha_f + \beta mother_i + \gamma\left(mother_i \times child\ attends_f\right) + \Gamma X_{if} + \epsilon_{if} \qquad (5),$$

where the dependent variable is a dummy variable if a parent speaks English. Besides family fixed effects, α_f, a mother dummy, and the above-mentioned controls, we also include fixed effects for age and birthplace of both parents. In equation (5), treatment refers to the own 5- to 6-year-old child *attending* kindergarten. The coefficient of interest is γ, which captures the effect of kindergarten attendance on mother's English proficiency compared to the father. Table 7 presents the results on language spillover effects from kindergarten attendance. The estimation equation is (5) and the method of estimation is ordinary least squares. Columns (1) and (3) report results without household fixed effects, while columns (2) and (4) include them. The coefficient of interest is always statistically significant at the 1-percent level, but the estimate is smaller in the specifications including family fixed effects indicating that it is important to account for unobserved family characteristics that could influence a parent's decision to send their children to the kindergarten. Mothers per se are between 9-19 percentage points less likely to speak English compared to fathers. Yet, the gap in the likelihood of speaking English language is reduced by about 2 percentage points if the child attends a kindergarten. We regard our results as suggestive evidence that the home visits or the classroom experiences that the children bring to their homes accelerate the assimilation of immigrant mothers in terms of acquiring basic English-language skills.[30]

V. Conclusion

What was the socioeconomic impact of kindergartens when they were introduced for the first time? Historians of education vividly describe the positive influence of kindergarten education on young children and their parents, but rigorous quantitative evidence on the role of kindergartens for historical development is still scarce. We made use of a unique historical experiment in which, towards the end of the 19th century, thousands of kindergartens opened their doors in various American cities within less than thirty years. At that time, most kindergartens targeted poor urban children and their families who were one of the most disadvantaged groups in the American society of the late 19th century. The kindergartens' impact on these families was fundamental: Our empirical analysis revealed that kindergarten exposure led to a fertility decline in American cities, in particular where child labor was most common. The observed fertility decline was mainly driven by immigrant families. Since these households

[30] Our finding relates also to a debate whether immigrant parents "lean" or "learn" from the human capital acquisitions of their children (such as learning English in school); see Kuziemko (2014).

constituted a substantial part of the city population, the effect is economically relevant and contributed to the fertility transition in American cities. Consistent with the prediction of a quantity-quality tradeoff model, households reduce fertility since access to kindergartens increases the returns to education and reduces family income from child labor as the progressive educators at that time had hoped for.

While the goal of this paper was to understand the contribution of kindergartens for historical development, some of our findings are also relevant for policymakers. For example, the establishment of kindergartens in developing countries can potentially reduce population pressure and reduce the evils of child labor if they are targeted at economically disadvantaged families. Our result that kindergarten exposure increased English proficiency of immigrant children and their mothers from non-English-speaking countries also indicates that kindergartens can play an important role for social integration.

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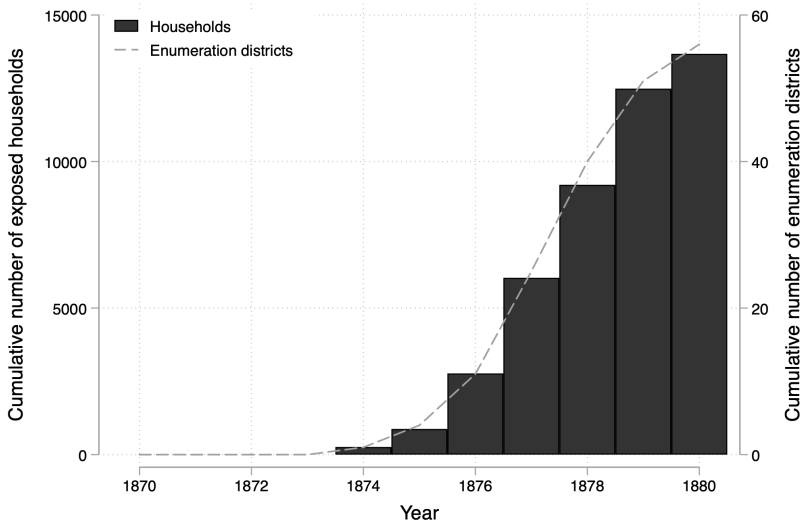
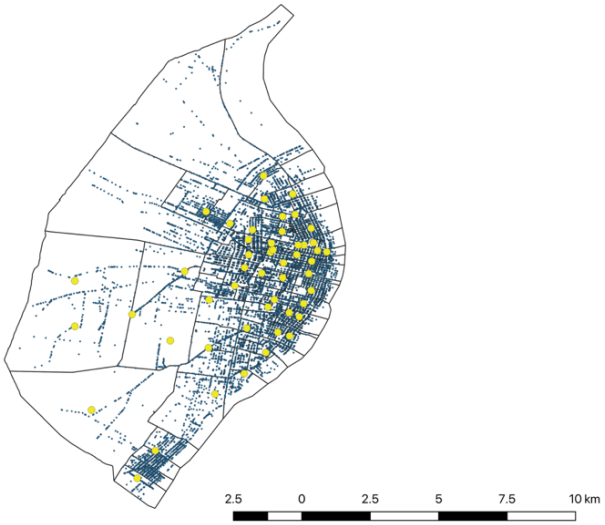
TABLES AND FIGURES

Figure 1:
The Roll-out of Kindergartens in St. Louis 1873-1886



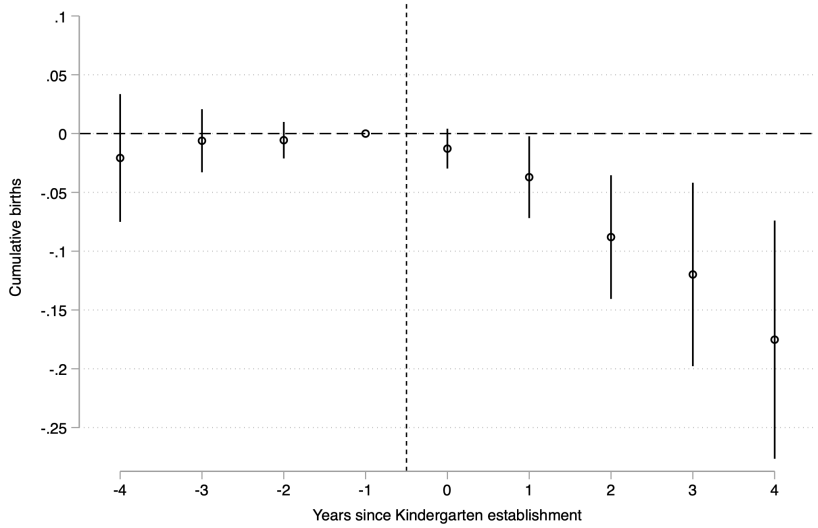
NOTE.— This figure displays on the left y-axis (right y-axis) the cumulative number of households (enumeration districts) exposed to a kindergarten in St. Louis between 1870 and 1880.

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



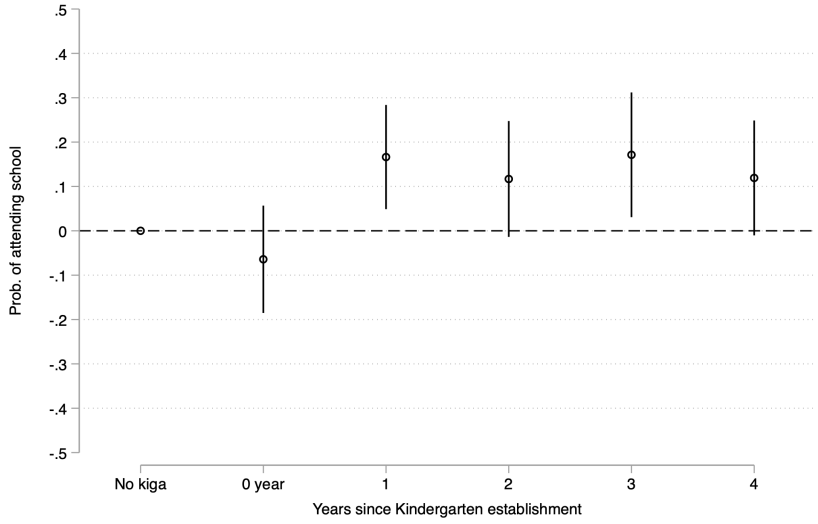
NOTE.— This map displays households (blue dots) together with the 1880 enumeration districts (gray lines) of St. Louis (see the Urban Transition Historical GIS project at https://s4.ad.brown.edu/Projects/UTP2/ncities.htm for further details). The kindergarten locations in 1886 (yellow dots) are based on the historical map of St. Louis in 1882 (https://collections.leventhalmap.org).

Figure 3:
The Effect of the Kindergarten Roll-out on Fertility in St. Louis



NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis. 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 fertility relative to the base year (the year before opening). The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

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



NOTE.— This figure shows the effect of the kindergarten roll-out on school attendance for children age 5-6. 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.

Table 1: Kindergarten Exposure and Attendance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable: Attends School						
	Age 5-6	Age 5-6	Age 5-6	Age 4	Age 5	Age 6	Age 7
$Kindergarten\ Exposure_{ct}$	0.259***	0.260***	0.259***	0.092***	0.390***	0.125***	0.021
	(0.037)	(0.039)	(0.038)	(0.015)	(0.054)	(0.031)	(0.032)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE × Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	Boys	Girls	All	All	All	All
Observations	2,414,765	1,210,550	1,202,196	1,246,005	1,208,556	1,204,039	1,156,673
R-squared	0.254	0.257	0.255	0.023	0.178	0.175	0.172

NOTE.— This table shows the impact of kindergarten exposure on attendance for the census years 1880 to 1910. The dependent variable is a dummy whether a child attends school. Column (1) is based on the sample of white children age 5-6; columns (2)-(3) present results by gender; and columns (4)-(7) present results separately by age. The variable of interest, $Kindergarten\ Exposure_{ct}$, is calculated as described in equation (3). All specifications include city, census year, and state-by-birth cohort fixed effects. Individual controls include fixed effects for birthplace interacted by year and by city, birth cohort, gender, and a set of parental controls including fixed effects for mother's birthplace, father's birthplace, parents' joint occupational score, a set of dummy variables including mother's literacy, whether the mother was working, father's literacy, whether the father worked in a white-collar/blue-collar skilled occupation, and whether the father was absent at the time of the census enumeration. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 2: Kindergarten Exposure and Household Fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable: Children below Age 5						
	All	All	Only children Below Age 5	Below Median % Child Labor	Above Median % Child Labor	U.S. Born	Foreign Born
	Panel A: Without City FE × Time FE						
Kindergarten Exposure$_{ct}$	-0.042	0.002	-0.007	-0.006	0.041	-0.015	0.045
	(0.028)	(0.024)	(0.022)	(0.026)	(0.033)	(0.026)	(0.045)
Kindergarten Exposure$_{ct}$ ×		-0.110***		-0.080**	-0.207***	-0.079***	-0.172***
Has Kid Age $6-11_i$		(0.029)		(0.037)	(0.056)	(0.020)	(0.044)
Observations	8,579,001	8,579,001	1,664,855	2,146,718	5,002,495	5,297,340	3,281,648
R-squared	0.157	0.179	0.059	0.178	0.180	0.145	0.187
	Panel B: With City FE × Time FE						
Kindergarten Exposure$_{ct}$ ×	—	-0.128***	—	-0.090**	-0.226***	-0.094***	-0.198***
Has Kid Age $6-11_i$		(0.031)		(0.039)	(0.055)	(0.021)	(0.046)
Observations	—	8,575,961	—	2,145,791	5,001,578	5,296,889	3,277,382
R-squared	—	0.183	—	0.183	0.182	0.151	0.195
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend (Panel A)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE x Year FE (Panel B)	—	Yes	—	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.— This table shows the impact of kindergarten exposure on fertility for the census years 1880 to 1910. The dependent variable is the number of own children below age 5. The variable of interest, *Kindergarten Exposure*$_{ct}$, is calculated as described in equation (3). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11 in columns (2) and (4)-(7). These specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). Columns (1)-(2) are based on the whole sample of 18 to 44-year-old white women; column (3) only includes mothers with children below age 5; columns (4)-(5) split the sample below/above median share of child labor in 1880; and columns (6)-(7) split the sample by nativity. All specifications include fixed effects for city and census year. Panel A further includes a city-specific linear time trend, while Panel B includes city-by-year fixed effects instead. Individual controls include fixed effects for birthplace interacted by census year and by city, fixed effects for birth cohorts interacted by census year and by state of residence, dummy variables for literacy and marital status, and a set of spouse controls. These include the occupational income score (in logs) of the husband, a set of dummy variables whether the husband worked in a white-collar/blue-collar skilled occupation, is foreign-born, literate, and whether his occupation was still not classified by IPUMS. Panel B further includes all interactions of birthplace, year and city fixed effects and city-by-birth cohort fixed effects. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 3: Accounting for the Public School Expansion, 1880-1910

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Children below Age 5							
	All	All	All	All	Below Median % Child Labor	Above Median % Child Labor	U.S. Born	Foreign Born
Kindergarten Exposure$_{ct}\times$ *Has Kid Age* $6-11_i$	-0.127***	-0.084***	-0.070**	-0.072**	-0.023	-0.169***	-0.056***	-0.146**
	(0.035)	(0.031)	(0.033)	(0.032)	(0.037)	(0.056)	(0.021)	(0.058)
Public School Exposure$_{ct}\times$ *Has Kid Age* $6-11_i$		-0.211***		-0.139***	-0.206***	-0.104	-0.117***	-0.197***
		(0.023)		(0.032)	(0.059)	(0.068)	(0.027)	(0.074)
High School Exposure$_{ct}\times$ *Has Kid Age* $6-11_i$			-0.201***	-0.095***	-0.023	-0.133*	-0.065**	0.015
			(0.021)	(0.030)	(0.059)	(0.077)	(0.025)	(0.090)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,643,933	7,643,933	7,643,933	7,643,933	1,945,942	4,639,735	4,650,491	2,992,507
R-squared	0.182	0.182	0.182	0.182	0.182	0.181	0.149	0.193

NOTE.— This table shows the impact of kindergarten exposure on fertility accounting for the expansion of the public school system for the census years 1880 to 1910. The specification in this table is based on Panel B of Table 2, column 2. Columns (1)-(4) present results based on the whole sample where information on public schools and high schools was available. The remaining columns present sample splits by initial share of child labor (columns 5-6) and nativity of the women (columns 7-8). The dependent variable is the number of own children below age 5. The variable of interest, *Kindergarten Exposure*$_{ct}$, is calculated as described in equation (3). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11. All specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). Public school exposure is calculated as the number of public school teachers multiplied by an assumed class size of 35 relative to the number of children between the ages 5 to 21. High school exposure is calculated similarly with high school teachers multiplied by an assumed class size of 35 relative to the number of children over the ages 14 to 18. Both measures are also interacted with a dummy variable for whether a household had a 6- to-11-year-old at the time of the census enumeration. This table includes the same set of individual and spouse controls as Table 2 (Panel B, column 2); see notes to Table 2 for further details. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 4: Fertility and Family Size of Exposed Children as Adults

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Children below Age 5			*Dependent Variable: Family Size*		
			Panel A: Linked Sample 1900-1920			
$Kindergarten\ Exposure_{bct}$	-0.071*	-0.048		-0.070	-0.022	
	(0.042)	(0.042)		(0.069)	(0.068)	
$Kindergarten\ Exposure_{bct} \times$		-0.084**	-0.073**		-0.181***	-0.165***
$Has\ Immigrant\ Mother_i$		(0.033)	(0.032)		(0.059)	(0.060)
Observations	336,969	336,969	336,848	336,969	336,969	336,848
R-squared	0.039	0.039	0.055	0.051	0.051	0.066
			Panel B: Linked Sample 1910-1930			
$Kindergarten\ Exposure_{bct}$	-0.028	-0.015		-0.024	0.003	
	(0.033)	(0.033)		(0.060)	(0.061)	
$Kindergarten\ Exposure_{bct} \times$		-0.050**	-0.048**		-0.101***	-0.096***
$Has\ Immigrant\ Mother_i$		(0.020)	(0.020)		(0.037)	(0.037)
Observations	510,712	506,371	506,357	510,712	506,371	506,357
R-squared	0.011	0.032	0.044	0.025	0.055	0.066
City FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE x Birth Cohort FE	Yes	Yes	No	Yes	Yes	No
City FE x Birth Cohort FE	No	No	Yes	No	No	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.— This table shows the impact of kindergarten exposure on fertility and family size for the linked samples 1900-1920 (Panel A) and 1910-1930 (Panel B). The dependent variable in columns (1)-(3) is the number of own children below age 5 and family size in columns (4)-(6). $Kindergarten\ Exposure_{bct}$, is city c's kindergarten capacity normalized by the number of children age 5-6 at the time when a child of cohort b was of age 5-6. Kindergarten exposure is also interacted with a dummy variable whether the mother was foreign-born. This table includes the same set of individual and parental controls as Table 5 (see notes to Table 5 for further details). Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 5: Kindergarten Exposure, School Attendance, and Child Labor

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 10-15	Age 10-15	Age 10-15	Age 10-11	Age 12-13	Age 14-15
			Panel A: Dependent Variable: Attends school			
$Kindergarten\ Exposure_{bct}$	0.033*	-0.006				
	(0.018)	(0.018)				
$Kindergarten\ Exposure_{bct} \times$		0.090***	0.088***	0.008	0.054***	0.090***
$Has\ Immigrant\ Mother_i$		(0.013)	(0.013)	(0.005)	(0.009)	(0.016)
Observations	4,308,792	4,308,792	4,308,784	1,499,385	1,445,137	1,360,733
R-squared	0.229	0.229	0.240	0.128	0.163	0.230
			Panel B: Dependent Variable: Child works			
$Kindergarten\ Exposure_{bct}$	-0.009	0.020				
	(0.012)	(0.013)				
$Kindergarten\ Exposure_{bct} \times$		-0.070***	-0.066***	-0.005**	-0.033***	-0.043***
$Has\ Immigrant\ Mother_i$		(0.013)	(0.013)	(0.002)	(0.005)	(0.015)
Observations	4,163,005	4,163,005	4,162,997	1,485,814	1,421,542	1,252,141
R-squared	0.196	0.197	0.212	0.032	0.071	0.159
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE x Birth Cohort FE	Yes	Yes	No	No	No	No
City FE x Birth Cohort FE	No	No	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTE.— This table shows the impact of kindergarten exposure on school attendance (Panel A) and on working in a gainful occupation (Panel B) at age 10-15 for the census years 1880 to 1910. The dependent variable is a dummy whether a child attends school in Panel A, and a dummy whether a child was working in Panel B. Columns (1)-(3) are based on the whole sample; columns (4)-(6) present results by age 10-11, age 12-13, and age 14-15, respectively. $Kindergarten\ Exposure_{bct}$, is city c's kindergarten capacity normalized by the number of children age 5-6 at the time when a child of cohort b was of age 5-6. Kindergarten exposure is also interacted with a dummy variable whether the mother was foreign-born. All specifications include fixed effects for city, census year, state-by-birth cohort fixed effects (columns 1-2), and city-by-birth cohort fixed effects (columns 3-6). Individual controls include fixed effects of birthplace interacted by year and by city, birth cohort, gender, and a set of parental controls including fixed effects for mother's birthplace, father's birthplace, parents' joint occupational score, a set of dummy variables including mother's literacy, whether the mother was working, father's literacy, whether the father worked in a white-collar/blue-collar skilled occupation, and whether the father was absent at the time of the census enumeration. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 6: Kindergarten Exposure and English Fluency of Immigrant Children

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Speaks English				
	Age 10-15	Age 10-15	Age 10-11	Age 12-13	Age 14-15
$Kindergarten\ Exposure_{bct}$	-0.030				
	(0.039)				
$Kindergarten\ Exposure_{bct} \times$	0.081***	0.088***	0.136***	0.048	0.004
$Eligible_i$	(0.026)	(0.028)	(0.039)	(0.037)	(0.046)
$Eligible_i$	0.094***	0.099***	0.107***	0.085***	0.095***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.009)
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE x Birth Cohort FE	Yes	No	No	No	No
City FE x Birth Cohort FE	No	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes
Parent Controls	Yes	Yes	Yes	Yes	Yes
Observations	366,017	365,195	112,828	122,308	126,920
R-squared	0.154	0.165	0.182	0.163	0.167

NOTE.— This table shows the impact of kindergarten exposure on whether a 10 to 15-year-old child from a non-English speaking sending country speaks English. The sample spans the census years 1900 and 1910. The dependent variable is a dummy whether a child speaks English. Columns (1)-(3) are based on the whole sample; columns (4)-(6) present results by age group. $Kindergarten\ Exposure_{bct}$, is city c's kindergarten capacity normalized by the number of children age 5-6 at the time when a child of cohort b was of age 5-6. Kindergarten exposure is also interacted with a dummy variable whether the child arrived before age 6 in the U.S. (= $Eligible_{it}$). This table includes the same set of individual and parental controls as Table 4 (see notes to Table 4 for details). Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

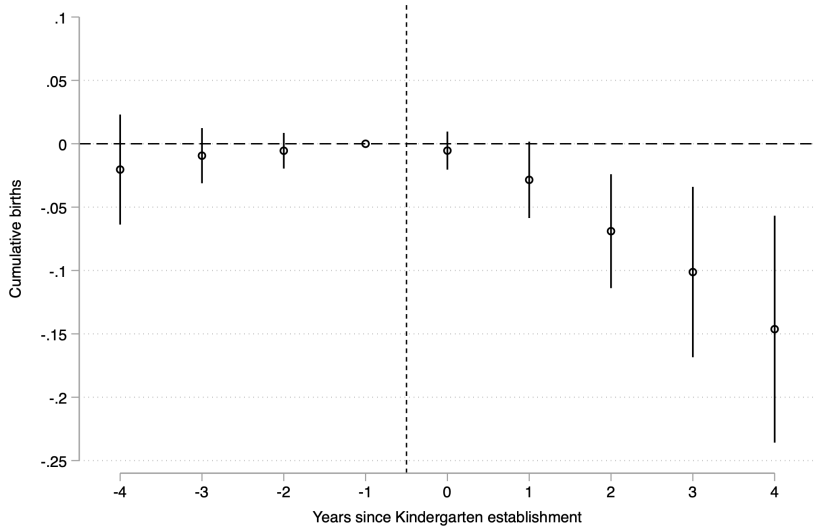
Table 7: Language Spillover Effects on Immigrant Mothers

	(1)	(2)	(3)	(4)
	Dependent Variable: Speaks English			
	Year 1900	Year 1900	Year 1910	Year 1910
$Mother_i$	-0.095***	-0.119***	-0.160***	-0.190***
	(0.007)	(0.006)	(0.005)	(0.004)
$Mother_i \times Child\ Attends_f$	0.022***	0.021***	0.029***	0.019***
	(0.006)	(0.005)	(0.005)	(0.004)
$Child\ Attends_f$	0.017***		0.036***	
	(0.003)		(0.003)	
City FE	Yes	No	Yes	No
Household FE	No	Yes	No	Yes
Individual Controls	Yes	Yes	Yes	Yes
Observations	405,619	305,856	531,141	414,712
R-squared	0.227	0.739	0.230	0.803

NOTE.— This table shows the spillover effects of kindergarten attendance on the likelihood of a mother from a non-English speaking sending country speaking English. The dependent variable is a dummy variable if a parent speaks English. $Mother_i \times ChildAttends_f$, denotes the effect on mothers if her 5 to 6-year-old child attends a kindergarten. Columns (1) and (3) include city fixed effects and a control whether the child attends a kindergarten. Columns (2) and (4) include family fixed effects (the direct effect of attendance is absorbed). All specifications further control for each parent's occupation type (white collar and blue collar skilled dummies), the occupational income score, literacy as well as fixed effects for age and birthplace. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

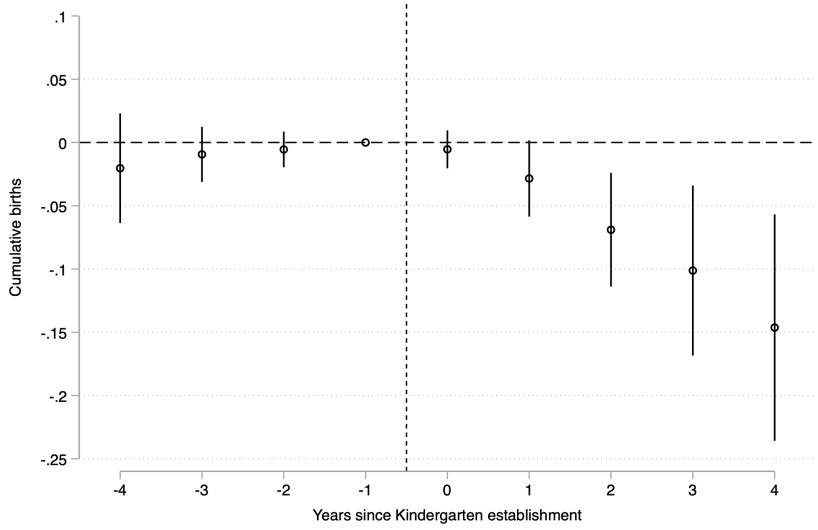
ONLINE APPENDIX TABLES AND FIGURES

Appendix Figure 1:
Replication of Figure 3 – First and Second Generation Immigrant Mothers



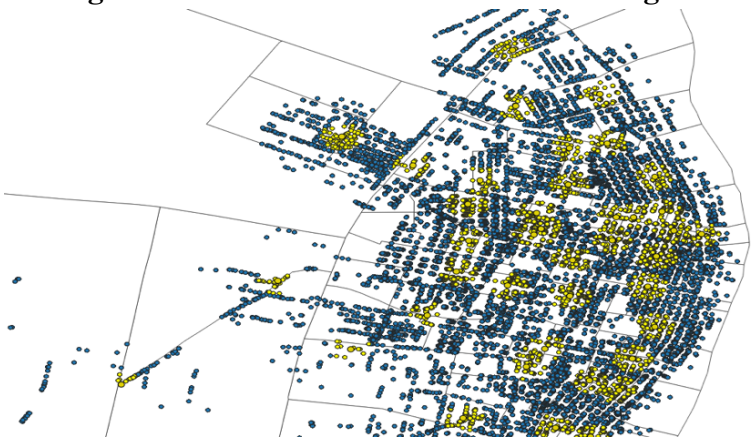
NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis restricting the sample to first and second generation immigrant mothers. The x-axis measures the number of years since the kindergarten opened in enumeration district *e*. The dots depict the estimated coefficients of kindergarten exposure on fertility relative to the base year (i.e., the year before the opening). The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

Appendix Figure 2:
Replication of Figure 3 using Proximity as Treatment



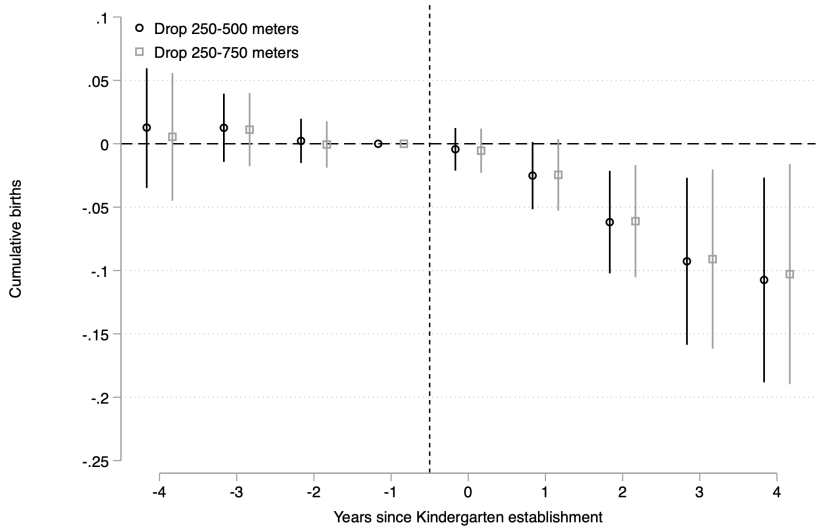
NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis using proximity as treatment. Treated households are those living within 250 meters of an open kindergarten whereas households located between 250-1000 meters of an open kindergarten serve as control units. 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 fertility relative to the base year (i.e., the year before the opening). The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

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



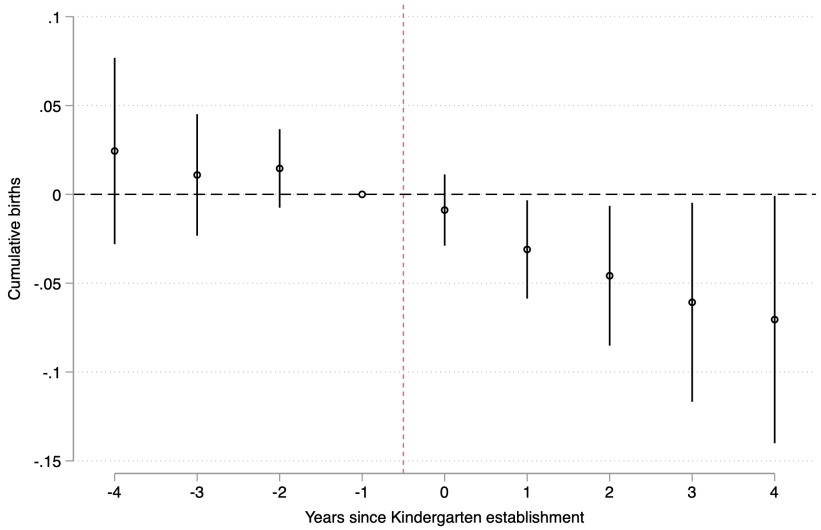
NOTE.— This map displays an excerpt of the sample of households within 1,000 meters radius from the closest kindergarten in St. Louis together with the 1880 enumeration districts. The households in yellow are located within 250 meters of a kindergarten and are considered as "treated" group while households in blue are located between 250–1,000 meters away from a kindergarten and serve as "control" group.

Appendix Figure 4:
Replication of Figure 3 – Testing for Spatial Spillover Effects



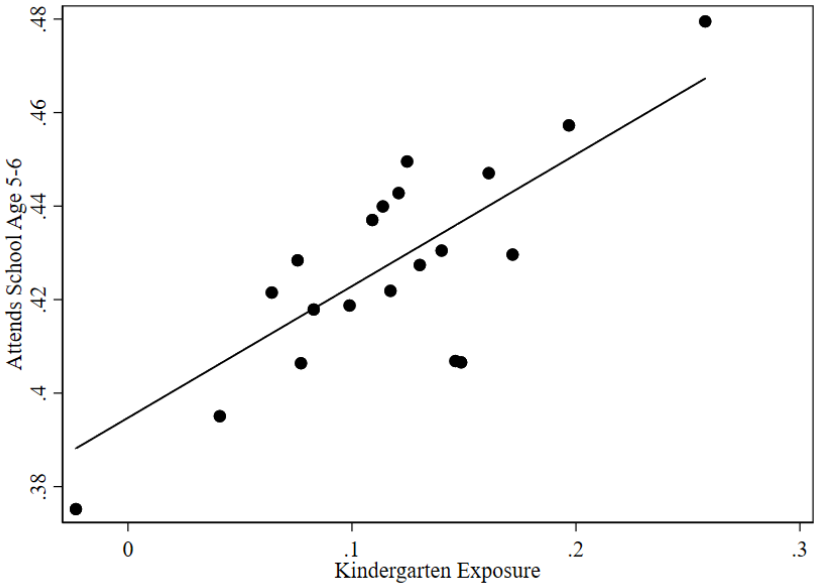
NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis accounting for potential spatial spillover effects. The dots depict the estimated coefficients of kindergarten exposure on fertility relative to the base year (i.e., the year before the opening). The control groups are, respectively, households located between 500-1000 meters (full dots) and between 750-1000 meters (empty dots) from the kindergarten. The x-axis measures the number of years since the kindergarten opened in enumeration district e. The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

Appendix Figure 5:
Replication of Figure 3 using Stacked Event-Study Regression



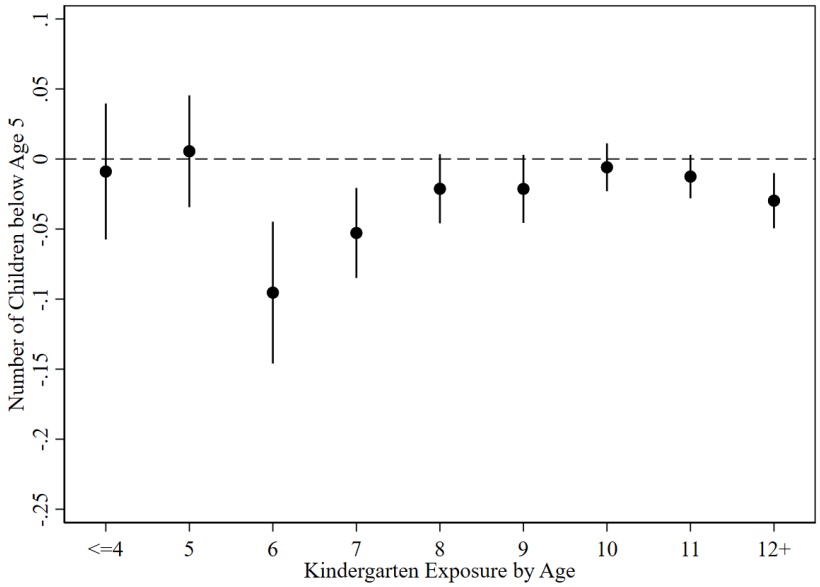
NOTE.— This graph shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis considering each kindergarten opening in an enumeration district as a separate event (stacked regressions). Control units are defined as those enumeration districts which did not experience any kindergarten opening during the event window. The x-axis measures the number of years since the kindergarten opened in enumeration district e. The solid lines indicate 95 percent confidence intervals. Standard errors are clustered at the enumeration district level.

Appendix Figure 6:
Kindergarten Exposure and Attendance in American cities



NOTE.— This binned scatter plot shows the relationship between kindergarten exposure and school attendance of 5 to 6-year-old children in American cities between 1880 and 1910 after controlling for city and year fixed effects. The dependent variable is a dummy variable whether a child of kindergarten age attended school at the time of the Census enumeration. Kindergarten exposure is calculated as described in equation (3).

Appendix Figure 7:
Kindergarten Exposure by Age of the Children and Household Fertility



NOTE.— This coefficient plot shows the relationship between kindergarten exposure and fertility by age of the children. The dependent variable is the number of own children below age 5. Kindergarten exposure is calculated as described in equation (3). The dots depict the estimated coefficients of kindergarten exposure interacted by the age of children (age 5, ..., 12 and older) on the number of children below age 5. The reference group are households with children under age 5 or without any children. The solid lines indicate 95 percent confidence intervals. The figure includes the same set of individual and parental controls as Table 2 (Panel A, column 2) and in addition also controls for the direct effect of the age group. We refer the reader to the notes of Table 2 for further details.

Appendix Table 1: Summary Statistics

	(1) Sample	(2) Obs	(3) Mean	(4) Sd
	Panel A: St. Louis Analysis			
Cumulative births	1880	31,817	3.003	2.056
School attendance age 5-6	1880	12,091	0.329	0.470
	Panel B: City Analysis			
School attendance age 5-6	1880-1910	2,414,765	0.428	0.495
Children below age 5	1880-1910	8,579,001	0.704	0.864
Mother speaks English	1900-1910	451,120	0.644	0.479
Kindergarten Exposure	1880-1910	8,579,001	0.126	0.134
	Panel C: Cohort Analysis (age 10-15)			
School attendance	1900-1910	4,308,792	0.816	0.387
Child works	1900-1910	4,163,005	0.079	0.270
Child speaks English	1900-1910	366,017	0.882	0.323
Kindergarten Exposure	1900-1910	4,308,792	0.105	0.113
	Panel D: Linked Samples			
Children below age 5	1900-1920	336,969	0.783	0.843
Family Size	1900-1920	336,969	3.865	1.577
Children below age 5	1910-1930	506,371	0.690	0.798
Family Size	1910-1930	506,371	3.717	1.513

NOTE.— This table reports summary statistics. In column (1), *Sample* refers to the census year and *sd* in column (4) refers to standard deviation.

Appendix Table 2: Balance test for St. Louis

	No Kindergarten in 1880		*Has Kindergarten in 1880*		*T-test*
Variable	N	Mean/SE	N	Mean/SE	Difference
Characteristics of Females age 18-44					
Age	20,762	33.969 (0.039)	11,055	33.793 (0.053)	0.176***
Labor force (dummy)	20,762	0.205 (0.003)	11,055	0.124 (0.003)	0.082***
Foreign born (dummy)	20,762	0.589 (0.003)	11,055	0.586 (0.005)	0.003
German (dummy)	20,762	0.305 (0.003)	11,055	0.331 (0.004)	-0.026***
Characteristics of their Husbands					
Ln(occupation score)	20,762	2.926 (0.008)	11,055	2.978 (0.010)	-0.052***
White-collar (dummy)	20,762	0.262 (0.003)	11,055	0.282 (0.004)	-0.020***
Skilled blue-collar (dummy)	20,762	0.253 (0.003)	1,1055	0.232 (0.004)	0.022***
Day laborer (dummy)	20,762	0.143 (0.002)	11,055	0.124 (0.003)	0.019***
Foreign born (dummy)	20,762	0.634 (0.003)	11,055	0.645 (0.005)	-0.011*

NOTE.— This table reports a balance test for St. Louis by access to kindergartens in 1880. The first group refers to individuals that resided in an enumeration district without a kindergarten in 1880. The second group refers to individuals that resided in an enumeration district with kindergartens. The last column (t-tests) reports the differences in means between the two groups. Standard errors are reported in parentheses. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Cumulative births					
	Baseline	Foreign	Proximity	Spillover	Spillover	Stacked
Kindergarten establishment ($\tau \leq -4$)	-0.021	-0.020	-0.003	0.013	0.005	0.024
	(0.028)	(0.022)	(0.020)	(0.024)	(0.025)	(0.027)
Kindergarten establishment ($\tau - 3$)	-0.006	-0.009	0.009	0.013	0.011	0.011
	(0.014)	(0.011)	(0.012)	(0.014)	(0.015)	(0.017)
Kindergarten establishment ($\tau - 2$)	-0.006	-0.006	0.002	0.002	-0.001	0.015
	(0.008)	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)
Kindergarten establishment ($\tau - 1$)	*Baseline*	*Baseline*	*Baseline*	*Baseline*	*Baseline*	*Baseline*
Kindergarten establishment (τ)	-0.013	-0.005	-0.002	-0.004	-0.005	-0.009
	(0.009)	(0.008)	(0.007)	(0.009)	(0.009)	(0.010)
Kindergarten establishment ($\tau + 1$)	-0.037**	-0.028*	-0.024**	-0.025*	-0.025*	-0.031**
	(0.018)	(0.015)	(0.011)	(0.013)	(0.014)	(0.014)
Kindergarten establishment ($\tau + 2$)	-0.088***	-0.069***	-0.057***	-0.062***	-0.061***	-0.046**
	(0.027)	(0.023)	(0.018)	(0.020)	(0.022)	(0.020)
Kindergarten establishment ($\tau + 3$)	-0.120***	-0.101***	-0.085***	-0.093***	-0.091**	-0.061**
	(0.040)	(0.034)	(0.029)	(0.033)	(0.036)	(0.028)
Kindergarten establishment ($\tau \geq +4$)	-0.175***	-0.146***	-0.091**	-0.107***	-0.103**	-0.070**
	(0.051)	(0.045)	(0.036)	(0.041)	(0.044)	(0.035)
Mother FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother's age FE	Yes	Yes	Yes	Yes	Yes	No
Years since district school	Yes	Yes	Yes	Yes	Yes	No
Observations	339,611	266,783	233,891	123,497	86,895	1,926,702
R-squared	0.920	0.921	0.920	0.920	0.921	0.935

NOTE.— This table reports the estimated coefficients of the event studies displayed in Figure 3 (column 1), Appendix Figure 1 (column 2), Appendix Figure 2 (column 3), Appendix Figure 4 (columns 4-5), and Appendix Figure 6 (column 6). 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.

Appendix Table 4: Determinants of Kindergarten Exposure

	(1)	(2)
	Dependent Variable: Kindergarten Exposure	
Economic and Demographic Structure		
Average occupation score (logs)	-0.124	-0.114
	(0.181)	(0.257)
% White collar workers	0.469*	-0.062
	(0.254)	(0.288)
% Blue collar skilled workers	-0.145	-0.031
	(0.243)	(0.340)
% 10 to 15-year-old working	0.220	0.042
	(0.197)	(0.269)
% Foreign-born	-0.164	0.091
	(0.190)	(0.275)
Crude birth rate	0.005**	-0.002
	(0.002)	(0.004)
City Size (logs)	0.030***	0.042***
	(0.010)	(0.015)
% Females Working (age 18-44)	-0.027	0.251
	(0.230)	(0.325)
% Married (age 18-44)	-0.250	0.485
	(0.295)	(0.444)
Human Capital Proxies		
Teachers per capita (logs)	0.138***	0.228***
	(0.047)	(0.067)
% Literate	0.283	0.195
	(0.252)	(0.394)
Attendance Rate (age 5-21)	0.171	0.057
	(0.134)	(0.201)
Year	1900	1910
State FE	Yes	Yes
Observations	217	217
R-squared	0.406	0.398

NOTE.— This table shows the correlation betweeen initial (1880) city-level characteristics and kindergarten exposure (as described in equation 3) in 1900 (column 1) and in 1910 (column 2). All specifications include state fixed effects. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Appendix Table 5:
Controlling for Maternal Labor Supply, Duration of Marriage, and Child Mortality

	(1)	(2)	(3)	(4)
	Dependent Variable: Children below Age 5			
Kindergarten Exposure$_{ct}$ ×	-0.133***	-0.064***	-0.061***	-0.062***
Has Kid Age $6-11_i$	(0.031)	(0.024)	(0.019)	(0.019)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City FE x Year FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes
Extra Control	Works	Duration Marriage	Child Mortality	All
Sample	1880-1910	1900-1910	1900-1910	1900-1910
Observations	8,575,961	6,766,944	5,462,489	5,462,489
R-squared	0.185	0.160	0.203	0.210

NOTE.— This table shows that our baseline result in Table 2 (Panel B column 2) is robust to controlling for whether a women is working (column 1); the duration of marriage (column 2); child mortality (column 3); or all the three extra controls together (column 4). Note, the sample in columns (2)-(4) is restricted to the census years 1900 and 1910 because of data availability. The dependent variable is the number of own children below age 5. The variable of interest, *Kindergarten Exposure*$_{ct}$, is calculated as described in equation (3). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11. All specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). This table includes the same set of individual and spouse controls as Table 2 (Panel B, column 2). We refer the reader to the notes of Table 2 for further details. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Appendix Table 6: Sample Modifications of Table 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Children below Age 5					
	Balanced Panel		Only 1880-1900		Only 1900-1910	
$Kindergarten\ Exposure_{ct}$	-0.042		-0.060*		-0.012	
	(0.028)		(0.033)		(0.021)	
$Kindergarten\ Exposure_{ct} \times$		-0.171***		-0.167***		-0.071**
$Has\ Kid\ Age\ 6-11_i$		(0.041)		(0.035)		(0.024)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	No	No	No	No	No
City FE x Year FE	No	Yes	No	Yes	No	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,141,522	7,139,670	4,351,436	4,351,436	7,300,784	7,300,784
R-squared	0.158	0.183	0.151	0.175	0.155	0.175

NOTE.— This table presents various sample modifications of Table 2. Columns (1)-(2) report results for a balanced panel of cities. Columns (3)-(4) and columns (5)-(6) restrict the sample to 1880-1900 and 1900-1910, respectively. The dependent variable is the number of own children below age 5. The variable of interest, $Kindergarten\ Exposure_{ct}$, is calculated as described in equation (3). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11 in columns (2), (4), and (6). These specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). All specifications include fixed effects for city and year, a city-specific linear time trend in column (1) and city-by-year fixed effects in columns (2), (4), and (6). This table includes the same set of individual and spouse controls as Table 2. We refer the reader to the notes of Table 2 for further details. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Appendix Table 7:
Replication of Table 2 with Different Measure of Kindergarten Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable: Children below Age 5						
	All	All	All	Below Median % Child Labor	Above Median % Child Labor	U.S. Born	Foreign Born
$Kindergarten\ Exposure_{ct}$	-0.576*	-0.225					
	(0.322)	(0.286)					
$Kindergarten\ Exposure_{ct} \times$		-1.008***	-1.183***	-0.849*	-2.736***	-0.848***	-2.114***
$Has\ Kid\ Age\ 6-11_i$		(0.354)	(0.390)	(0.456)	(0.766)	(0.250)	(0.635)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	No	No	No	No	No
City FE x Year FE	No	No	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,579,001	8,579,001	8,575,961	2,145,791	5,001,578	5,296,889	3,277,382
R-squared	0.157	0.179	0.183	0.183	0.182	0.151	0.195

NOTE.— This table replicates Table 2 (Panel A, columns 1-2) and (Panel B, columns 2, 4-7) using a different kindergarten exposure measure. The dependent variable is the number of own children below age 5. The variable of interest, $Kindergarten\ Exposure_{ct}$, is calculated as described in equation (3) but normalized by the total number of females aged 18-44 instead of 5 to-6-year-old children. Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11 in columns (2)-(7). These specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). This table includes the same set of individual and spouse controls as Table 2. We refer the reader to the notes of Table 2 for further details. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Appendix Table 8:
Regional Differences and Sample Split by City Size

	(1)	(2)	(3)	(4)
	Dependent Variable: Children below Age 5			
	Northern States	Southern States	Below 25,000 Inhabitants	Above 25,000 Inhabitants
$Kindergarten\ Exposure_{ct} \times$ $Has\ Kid\ Age\ 6-11_{it}$	-0.121*** (0.033)	-0.158** (0.076)	-0.056*** (0.013)	-0.173*** (0.042)
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
City FE x Year FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes
Observations	7,711,682	864,279	1,516,249	7,059,709
R-squared	0.185	0.169	0.192	0.181

NOTE.— This table presents sample splits based on our baseline specification in Table 2 (Panel B, column 2). Columns (1)-(2) present a sample split by region (northern vs southern states). Columns (3)-(4) present a sample split by city population size (above/below 25,000 inhabitants). The dependent variable is the number of own children below age 5. The variable of interest, $Kindergarten\ Exposure_{ct}$, is calculated as described in equation (3). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11. All specifications also control for the direct effect of having a 6- to 11-year-old child in the household and whether the household had any older children (i.e., above age 11). This table includes the same set of individual and spouse controls as Table 2 (Panel B, column 2). We refer the reader to the notes of Table 2 for further details. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the city level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.