FROEBEL'S GIFTS:

HOW THE KINDERGARTEN MOVEMENT CHANGED THE AMERICAN FAMILY*

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Abstract: Nineteenth-century social reformers promoted the establishment of kindergartens as a remedy for the problems associated with industrialization and immigration. Using newly collected data on historical kindergarten statistics, we evaluate the impact that the roll-out of the first kindergartens in American cities had on poor families. We find that immigrant women exposed to kindergartens significantly reduced fertility. Their offspring were more likely to attend school, they worked less at age 10-15, and they had fewer children as adults. Kindergarten exposure also helped children and mothers of non-English-speaking households to acquire English proficiency thereby illustrating the importance of kindergartens in the social integration of immigrant families.

Keywords: Kindergarten Education, Immigration, Fertility Transition, Child labor, School Attendance, Social Integration.

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1. Introduction

High birth rates, low levels of education, and reliance on income from child labor were common features of poor families in industrializing America of the 19th century and they still are in less developed countries today. The industrialization process in the United States was coupled with an unprecedented inflow of European immigrants which increased poverty and concerns over immigrant assimilation. Despite many contemporary observers' fears that industrialization and immigration amplified social problems, poor families started by the end of the 19th century to have fewer and better-educated children and relied less on child labor as a source of income.[1] Economic historians and sociologists attributed this social change to several reforms which aimed at facilitating the assimilation of immigrants, restricting child labor, and elevating the status of poor families in a rapidly industrializing society (Landes and Solmon, 1972; Davis, 1984; Moehling; 1999; Goldin and Katz, 2008).

The large-scale introduction of kindergarten education in American cities during the late 19th century was part of such reforms. Between 1880 and 1910, the kindergarten movement opened more than 7,000 kindergartens in 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). A substantial number of the enrolled children were coming from immigrant homes (Berg, 2004). Influential educational reformers, such as Elisabeth Peabody or William T. Harris, saw in kindergartens a remedy for the problems of an increasing number of children growing up in immigrant neighborhoods surrounded by poverty, ignorance, and few educational opportunities (Klein, 1992; Beatty, 1995). Nina C. Vandewalker, a kindergarten specialist for the U.S. Bureau of Education in the 1920s, regarded the roll-out of kindergartens as one of the most fundamental movements in American education (Vandewalker, 1908). To our knowledge, this is the first paper assessing whether and to what extent the roll-out of the first kindergartens in American cities contributed to the social transformation of poor families at the turn of the 20th century.

We use newly collected kindergarten statistics from various official education reports for the period 1880 to 1910 combined with full-count decennial census records to address this question. Our main result is that poor urban families reduced fertility after they were exposed to kindergartens. This finding is driven by immigrant households who were among the poorest at that time and the main target group of the kindergarten movement according to contemporary sources. Immigrant households not only reduced fertility, but their children were also more likely to attend school, they were more likely to speak

[1] 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 and Zilibotti, 2005; Moav, 2005).

English, and they worked less at age 10-15 if they were exposed to kindergartens at age 5-6. Exposed immigrant children had also fewer children as adults suggesting that access to kindergartens affected the fertility choices of immigrant households over multiple generations.

Our results on the fertility decline of immigrant mothers and the increased school enrollment of their offspring are consistent with the predictions of an augmented quantity-quality tradeoff model that explicitly allows households to invest in kindergarten education and reduce fertility. 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 such as schooling exist. We rule out other competing explanations for the observed fertility decline that could be associated with the roll-out of kindergartens. In particular, we show that the general expansion of the public education system during the late 19th century cannot explain away the negative association between the kindergarten roll-out and fertility. Yet, we find that the expansion of the public school system mattered for the urban fertility decline consistent with well-established literature emphasizing the importance of human capital for the fertility transition (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 force participation.

How did immigrant parents learn about the value of kindergarten education? Like today, kindergarten teachers prepared 5- to 6-year-old children for primary school, but one important distinctive feature was that they also regularly arranged home visits and mothers' meetings to gain access to immigrant homes. Kindergarten teachers used home visits and mothers' meetings to inform immigrant mothers about the importance of child-rearing, home economics, American customs, and the value of early childhood education.[2] Thus, an important feature of our study is that our treatment captures access to kindergartens which includes classroom activities and home visits from kindergarten teachers. The home-visiting component of the kindergarten movement constitutes an important contrast to many modern kindergarten programs where this component is not offered. 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. In the context of a trade-off between child quantity and quality, this would also affect their fertility choices (e.g., Rosenzweig and Wolpin, 1980; Jensen, 2010; Dizon-Ross, 2019).

[2] 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).

Our results suggest that personal interactions with kindergarten teachers appear very important for changing the fertility behavior of immigrant families. We find that it was only mothers *who had already a child of kindergarten age at the time of future family planning* that reduced fertility. Instead, mothers with *only small children (under age 5)* at the time of the census enumeration do not adjust their fertility behavior. While our model is silent about how parents learn about potential complementarities between kindergarten education and schooling, it implies that families 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 kindergarten teachers is consistent with our model predictions.

We then turn our focus to children's outcomes which serve also as additional mechanisms in explaining the observed fertility decline. Our analysis of children's outcomes first asks whether 10 to 15-year-olds are more likely to attend school and less likely to work in case they had access to kindergartens at age 5-6. We find this to be the case, and that these results are driven by immigrant children. We also show for the same age cohort that access to kindergartens at ages 5-6 improves the English fluency of *foreign-born* children from non-English-speaking sending countries. This result is in line with anecdotal evidence from contemporary surveys showing that access to kindergarten education improved immigrant children's English fluency and helped them to be better prepared for school (e.g., Palmer, 1915; Waite, 1926; Berg, 2004). Overall, our results reveal that having access to kindergartens affected children's outcomes approximately 5-10 years after exposure. Immigrant children stayed longer in school and were less likely to work at young ages, as the progressive reformers at that time had hoped for.

Next, we also provide evidence that having a child attending a kindergarten facilitated the assimilation process of their mothers. This analysis 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. Our estimates unveil positive language spillover effects of kindergarten attendance on mothers from non-English speaking sending countries. Having a child attending a kindergarten reduces the gap in English fluency between immigrant mothers and fathers by 10-17 percent. This finding provides important insights that immigrant children's access to kindergarten education accelerated the assimilation process of their parents through "learning" English from them and/or from interactions with the kindergarten teachers (Kuziemko, 2014).[3]

The final part of our empirical analysis speaks to a vast literature on the evolution of cultural norms and preferences of immigrants in host countries (e.g., Fernandez and Fogli, 2009; Algan and Cahuc, 2010; Abramitzky and Boustan, 2017; Giuliano and Tabellini, 2020). Economic historians found that fertility

[3] 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).

levels of immigrant families at the turn of the 20th century were higher compared to similar U.S. native households, but that this gap diminishes in the second generation (e.g., Guest, 1982; Morgan et al., 1994; Guinnane et al., 2006). Our analysis based on a linked sample of second-generation immigrant males from the 1900-1920 and 1910-1930 Censuses suggests that this was indeed the case. We show that immigrant children who were exposed to kindergartens at ages 5-6 have fewer children when married as adults. This result reveals that exposure to kindergartens influenced the fertility decisions of immigrant families over (at least) two generations and contributed to the convergence towards native fertility norms.

Our research strategy exploits two different data sources for identifying effects. The first data source contains detailed information about kindergarten openings within the city of St. Louis. William T. Harris, St. Louis' Superintendent of Schools, initiated in 1873 the first large-scale involvement of a public school system in kindergarten education in the United States. 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 between 1873 and 1886 which we georeferenced and combined with the fertility history of women living in St. Louis in the 1880 Census. We use a difference-in-differences approach exploiting 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 districts gradually reduce fertility after a kindergarten opening, while there are no fertility differences between treated and untreated women before the event. This result is driven by immigrant households, and it also holds when accounting for potential heterogeneous treatment effects across time. We also show that only mothers who already had a child of kindergarten age at the time of future family planning reduced fertility.

The second data source contains information about kindergarten statistics at the city level from the U.S. Bureau of Education covering the period of the kindergarten movement. We digitize these reports and construct a city and time-varying measure of kindergarten exposure, which we combine with the complete count U.S. Census records for the decades 1880-1910. The individual census data combined with temporal variation in kindergarten exposure at the city level allows us to show that the kindergarten movement contributed to the fertility transition of immigrant families in American cities. We find that the roll-out of kindergartens explains up to 12 percent of the overall fertility decline that immigrant families experienced between 1880 to 1910. Since, on average, every fourth city dweller during our sample period was foreign-born, this effect is economically relevant. This data also allows us to evaluate the

effect of the kindergarten movement on children's outcomes. Exposure to kindergartens at age 5-6 increased school attendance of immigrant children by up to 9 percentage points and reduced their likelihood of working at age 10-15 by up to 7 percentage points. In our estimation strategy, we control, besides fixed effects for cities and time and relevant individual characteristics, such as literacy, age, and birthplace, for state-by-birth year fixed effects. This 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] When contrasting results between mothers with and without children of kindergarten age or children from immigrant and native households, we can also control for city-by-birth year fixed effects to capture time-varying local confounding factors, such as city-specific public health interventions, that could have coincided with the timing of the kindergarten roll-out. We also account for national-specific changes in cultural norms across cities which could have influenced fertility choices independently of the kindergarten movement (Beach and Hanlon, 2022).

Our paper is the first in evaluating the large-scale effects of the kindergarten movement on poor urban families during the U.S. fertility transition.[5] It complements a large modern literature on the impact of early childhood education programs in the United States (e.g., Duncan and Magnuson; 2013; Elango et al., 2016; Cascio, 2021).[6] There is ample evidence of how these programs affected parental outcomes. Since the ongoing fertility transition is a key event during our period, we focus on how access to kindergartens affects mothers' fertility, while modern studies primarily evaluate whether early childhood education programs influence maternal labor supply and parenting practices. Generally, the modern literature finds small effects of access to kindergartens on maternal employment (e.g., Gelbach, 2002; Cascio, 2009b). Even if access to kindergartens in our period could have affected female labor

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

[5] 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) and Derrington et al., (2021), who study the long-term consequences of the Lanham Act of 1940---a universal child care policy that operated during WWII.

[6] See Herbst (2022), for a recent overview of the childcare literature in the United States, and the reviews by Almond and Currie (2011) and Almond et al. (2018) on shocks to the childhood environment and other early childhood interventions before kindergarten age. This large literature goes beyond the focus of this paper, which is about evaluating the effects that the roll-out of the first kindergartens ever operated in the United States had on poor urban families.

participation, we show that this channel is not a driving force for our fertility results.[7] This finding is also consistent with Aaronson et al. (2021), who find no systematic relationship between fertility and female labor supply in the United States before WWI. On the other hand, several studies show that the participation of children in early childhood education programs positively influences the parenting practices of low-income households (e.g., Gelber and Isen, 2013; Barr and Gibs, 2022).[8] In our context, home visits were an important service that the historical kindergartens provided. They contributed to the removal of information frictions about the value of education and child-rearing practices and, thus, constitute an important trigger of the historical fertility decline in U.S. cities.[9]

Several important studies have also evaluated the short- and long-term benefits of early childhood education programs in the United States on a wide range of cognitive and non-cognitive outcomes of participating children.[10] Until recently, much of the existing evidence is based on evaluating programs with geographic limited coverage, such as the Perry Preschool or the Carolina Abecedarian program, and relying on small sample sizes of longitudinal studies of targeted programs.[11] Compared to these studies, our empirical analysis is based on *full-count* individual census data including over 200 large cities across the United States. Besides parental outcomes, we can observe the outcomes of exposed children at different points in time over their life cycle. That is, our sample includes over 4 million boys and girls at ages 10-15 when we investigate the impact of kindergarten exposure on child labor and school attendance and between 300,000-500,000 observations when we follow linked samples of boys into adulthood to analyze their fertility decisions. The large sample and the rich set of information available allow us (i) to better control for parental characteristics and local geography, (ii) to explore the heterogeneity of treatment effects, and (iii) to consider the effect of kindergarten exposure over two generations.

Another important feature of our study is that we investigate how the kindergarten movement contributed to the assimilation of immigrants during the Age of Mass Migration (Abramitzky and

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[7] This result is perhaps not so surprising, since the labor force participation rate of white married women in the U.S. at the end of the 19th century stood at less than 5 percent (Goldin, 1977).

[8] Relatedly, one important question in the modern literature is whether being enrolled in a preschool program, such as Head Start, is better compared to home care (e.g., Kline and Walters, 2014). In our context, the alternative to kindergarten attendance for poor (immigrant) children was the street, and inadequate or no home care at all (Troen, 1972; Berg, 2004).

[9] There are a few preschool programs for children below kindergarten age in the U.S. that include a home-visiting component which is regarded as an important tool to remove information frictions about education and improve child-rearing practices in poor households (e.g., Cunha et al. 2013; Elango et al. 2016).

[10] For the United States studies include, among others, Currie and Thomas (1995); Garces, Thomas and Currie (2002); Fitzpatrick (2008); Deming (2009); Heckman et al. (2010); Carneiro and Ginja, (2014); Heckman and Karapakula (2019), Bailey et al., (2020); De Haan and Leuven, (2020); and Barr and Gibs (2022). For studies outside the United States, see, e.g., Havnes and Mogstad (2011); Engle et al. (2011); Aurajo et al. (2016); or Rossin-Slater and Wüst (2020).

[11] The studies of Herbst (2017) and Derrington et al. (2021) evaluating the long-term effects of the 1940 Lanham Act are recent examples of exploiting a universal child care/preschool program based on large representative samples. See also Cascio (2021) for further references.

Boustan, 2017). This period was characterized by the inflow of *millions of immigrants* and a general decline in fertility, hence understanding whether and to what extent the kindergarten movement affected the cultural and economic assimilation of immigrants in U.S. cities is of first-order importance. Our study provides a consistent quantification of the effect of kindergarten exposure on the fertility decline, the rise in school attendance, and the decline of child labor in immigrant households. These 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 the costs of child-rearing due to lost household income from child labor (e.g., Doepke, 2004).[12] They relate to proponents of unified growth theory, who emphasize the role of human capital in the fertility decline during the second phase of the industrial revolution (Galor and Weil, 2000; Galor, 2011). More generally, our empirical evidence suggests that the availability of kindergarten education facilitated the accumulation of human capital of the urban poor, accelerated the economic and cultural assimilation of immigrants into the U.S. society, and contributed to the social change that U.S. cities experienced at the beginning of the 20th century (e.g., Moehling; 1999; Goldin and Katz, 2008; Abramitzky et al., 2014, 2020, 2021).

2. Historical Background

In this section, we describe the historical context of our study, beginning with how kindergartens spread in the United States 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 and immigrant 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).

[12] 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).

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.[13] One of them was Karl Schurz's wife Margarethe, an enthusiastic advocate of Froebel's teaching methods, who opened the first kindergarten on American soil in Watertown, Wisconsin, in 1856.[14] A few other German-speaking kindergarten pioneers followed Schurz to the United States 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 educating 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 for kindergarten education due to a lack of funding and insufficient 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 also served 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

[13] See Bauernschuster and Falck (2015) for further insights on the early spatial diffusion of kindergartens in Germany.
[14] 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).

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 kindergartens increased from 42 with 1,252 enrolled pupils to 7,557 kindergartens with 353,546 pupils enrolled in 1912. Nationwide, kindergarten enrollment rates went up from close to zero in 1880 to approximately 9% in 1912 (U.S. Bureau of Education, 1914). Yet the kindergarten movement was an *urban phenomenon*. 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).[15] While both tuition-based and free kindergartens served a specific segment of society, public school sponsorship contributed to the universal provision of kindergarten education. By 1910,

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

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 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 society, the kindergarten curriculum largely followed Froebel's teaching principles. The daily program was rather similar, consisting of Froebel's gifts and occupations, circle games, free play, songs, and talks (Fisher, 1905, p. 718).

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 personal interactions with mothers were a central building block of the kindergarten movement in the late 19th century, with the result of directly influencing the family planning of the urban poor.

c. The Home and the Kindergarten

Home visits and mothers' meetings were intended to enlighten immigrant mothers about general child-rearing principles, the value of kindergarten education, home economics, and to familiarize them with English songs and 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 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*).* If these meetings changed immigrant mothers' perception of 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. Our empirical results suggest that these personal interactions were indeed important. One further positive side effect of the home visits and mothers' meetings is that they might have improved the English skills of immigrant mothers thereby accelerating the assimilation process of the immigrant household. Below, 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 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. We show that households adjusted fertility consistently with the prediction of an augmented quantity-quality model of fertility once the personal interactions between kindergarten teachers and mothers corrected informational frictions about the returns to education.

3. 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. 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 (no such data exist between 1892-1895), and annually from 1895-1896 to 1909-1910, and for 1912 from Bulletin No. 6 of the U.S. Bureau of Education in 1914. 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 in 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 the city level was only reported in detail before 1890 and in 1912. We construct a city-level kindergarten panel 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 a household head/spouse in the census and resided in cities during the period 1880-1910 (no data are available for 1890).[16] 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.

4. The Effect of the Kindergarten Movement on Fertility

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

Our empirical analysis begins with a case study that evaluates the kindergarten "experiment" in the St. Louis public school system, the first publicly sponsored kindergarten in the U.S. With a population of around 350,000 in 1880, St. Louis was one of the main commercial and industrial centers in the American Midwest during the late 19th century. Like other rapidly industrializing cities, it faced the problems of a rapidly growing city (St. Louis' population almost doubled between 1860 and 1870). The residents were often poor and had an immigrant background. 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 an early transition to child labor. Kindergartens, Harris argued, would remove poor children from the street and train them in the necessary skills to become industrious persons in the American society later in life (St. Louis Annual Report, 1877, pp. 79-119). In 1873, Harris appointed Susan E. Blow, an enthusiastic kindergarten teacher and an 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

[16] We consider only white women since their children were the main target group of the kindergarten movement.

Report, 1874, p. 195).[17] 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).[18] 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 and by 1886 there were 52 kindergartens operating 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 toward 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 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 poor families. Due to data limitations, we can only investigate how families adjusted fertility in response to kindergarten openings and whether "school" attendance of 5- to-6-year-old children with access to kindergarten increased (for the effect of kindergarten exposure on children's outcomes see Section 5 below).[19]

We geo-reference each kindergarten listed in the annual school reports and assign them to their corresponding enumeration district in the 1880 Census.[20] The kindergarten locations are then merged with the complete count U.S. census data, which contain the geo-referenced location of households in St. Louis together with other important information on socioeconomic characteristics at the individual level, such as age, gender, occupation, birthplace, number of children, and the enumeration district (this

[17] 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).
[18] 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).
[19] Children with potential kindergarten exposure would be too young in 1880 to report a gainful occupation in the Census.
[20] We thank Adele Heagney from the St. Louis Public Library for helping us geolocating the St. Louis kindergartens (the kindergarten at Lowell school is the only one of the 52 kindergartens we could not locate). 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.

is the first Census containing information on enumeration districts) in 1880. Our sample consists of every white woman aged 18-44 in St. Louis in 1880 who is listed as a household head or spouse. Since the Census reports the age of every enumerated person and lists every child in a household together with the household head (and spouse), we can reconstruct the fertility history of every woman in the sample and compile a quasi "mother panel". To avoid potential issues associated with children leaving the parents' household, our panel only includes children up to the age of 15 in 1880. We further require women to be at least 18 at the time when they were having a child. We then obtain the cumulative fertility history by calculating the number of children before 1870 and subsequently adding the births between 1870 and 1880 for every woman in the sample.[21] Hence, the "mother panel" spans every year from 1870 to 1880 containing for each year the cumulative number of births per woman. 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 use the "mother panel" to evaluate whether women adjusted their fertility behavior after having access to a kindergarten in their enumeration district. To test whether this is the case, we use a difference-in-differences 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}, denotes the total number of children of woman i residing in enumeration district e in year t.[22] The parameter τ refers to the year of a kindergarten opening in enumeration district e. $Kindergarten_{et}^{\tau+j}$ is

[21] 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 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.

[22] For our estimation approach, we need to assume that the household location observed in 1880 remained the same in the whole period under consideration.

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 slow-moving over time, and year fixed effects, α_t, which account for year-specific shocks common to all women in the sample. Standard errors are clustered at the enumeration district level to account for correlations within an enumeration district in a given year and over time.

Figure 3 depicts the event study based on estimating equation (1); the corresponding estimates are reported in column (1) of Appendix Table 2. Reassuringly, we find that for all j < 0, $\beta_j \approx 0$, which supports the key 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 2. Consistent with the historical narrative described in Section 2, our estimates reveal that the negative effect of kindergarten openings on fertility is primarily driven by women with an immigration background.

The key identifying assumption of our difference-in-differences approach is that the fertility behavior in the treatment and control groups would have moved in a parallel fashion in the absence of 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 and/or 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, besides the controls used in Figure 3,

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 2 and column (3) of Appendix Table 2 confirm the pattern observed in Figure 3. Reassuringly, 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.

One further concern is that households with stronger preferences for education could have lived near kindergartens and drive our results. We believe that selection into kindergarten districts is not a threat to our identification: First, kindergartens operated in the buildings of district schools, and we already control in estimating equation (1) for the years since an enumeration district had access to these schools. Second, it is also extremely unlikely that (poor) immigrant households with stronger preferences for kindergarten education would have been selected into the relevant school districts that offered kindergarten education since the practical effects of kindergarten education were literally unknown at that time. That is, parents likely learned about the kindergarten effects only after their child attended one. Third, we present a modified approach using proximity to the closest district school as a treatment criterion (this is possible since the 1880 IPUMS Census data contain the geo-referenced locations of households in St. Louis). To do so, we only include households within a 1,000-meter radius of a district school in the sample. Households are only considered as treated in year *t* if they were living within 250 meters of a district school *with* an active kindergarten. Besides fixed effects for mother's age, school district, year, and the years since a district school was in operation, this specification also includes a dummy for whether a household lived within a 250-meter radius of a district school (independent of whether a kindergarten was in place or not) to account for the concern that households with a likely stronger preference for education drive the result. Standard errors are clustered at the school district level. The event-study estimates of the proximity analysis are based on 40 district schools where we could identify the school district borders and are presented in Appendix Figure 3. The pattern of the fertility decline in Appendix Figure 3 resembles Figure 3 (column (4) of Appendix Table 2 reports the point estimates). Treated households experienced a fertility decline, but only after a kindergarten was in operation (see Appendix Figure 4 for an illustration of the identification strategy based on a household's distance to a district school).

Next, we test whether kindergarten openings indeed 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 for 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.[23] 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 kindergartens operating in 1880 for more than 5 years, we group them together). Figure 4 summarizes the result. The estimates reveal that school attendance of children aged 5-6 increased significantly in the first year after the opening and remained constant over the next 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.

After we unveiled that the establishment of a kindergarten substantially increased the school attendance rate of 5- to-6-year-old children, we evaluate whether the observed fertility decline associated with kindergarten openings is driven by mothers who had already a child of kindergarten age at the time of future family planning. To do so, we use an estimation approach based on the St. Louis cross-section of white women aged 25-34 in 1880 that is comparable to the city-level fertility analysis in the next section:

$$y_{id} = \alpha + \beta Kindergarten\ Exposure_d + \Gamma X_i + \epsilon_{id} \qquad (2),$$

where y_{id} is the number of own children under age 5 of women *i* in school district *d*, which is a retrospective cumulative measure of all births a woman had in the four years before the Census enumeration.[24] The variable of interest, $Kindergaten\ Exposure_d$, is a dummy which equals to one if a woman living in school district *d* had access to a kindergarten by 1880. Equation (2) further includes fixed effects for birth year, birthplace, father's and mother's birthplace, enumeration district, and the years since the district school was established. To capture potential social interactions of mothers with the kindergarten teacher, we add to equation (2) an interaction term between kindergarten exposure and a dummy variable for whether a woman has a 6- to-11-year-old child at the time of the census

[23] 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).

[24] The number of children under age 5 is used in other empirical studies on the U.S. fertility transition (see, e.g., Bleakley and Lange, 2009; Ager et al., 2020).

enumeration. These children were of kindergarten age in the five years before the census enumeration, i.e., the period we consider kindergarten exposure to be relevant for family planning when using the number of children under age 5 as the fertility measure. In the specification with the interaction term, we always control also for family size, i.e., we account for the direct effect of having a 6- to 11-year-old child in the household and we further include a dummy variable for whether the household has any older children (i.e., above age 11). Standard errors are clustered at the school district level.

Table 1 presents the cross-sectional results for 16,213 women aged 25-34 living in St. Louis in 1880. The estimates are based on equation (2) and our estimation method is ordinary least squares. To compare the cross-sectional findings with our event-study estimates, column (1) shows estimates without including the interaction term. The estimated coefficient on kindergarten exposure is negative and statistically significant at the 1-percent level. Women with access to kindergartens have significantly fewer children under age 5, which resonates with our event-study results based on the mother panel presented in Figure 3. Having access to a kindergarten by 1880 reduces the number of children under age 5 by 0.065, or 6 percent of the sample mean (the average number of children below age 5 in our sample is 1.11).

Columns (2) of Table 1 presents the results when adding the interaction term between kindergarten exposure and having a child aged 6-11. 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. Exposed mothers with a 6- to 11-year-old child reduced fertility while this was not the case for exposed mothers without a 6- to 11-year-old child. This result remains robust when we add to estimating equation (2) spouse controls (column 3) and school district fixed effects (column 4), which absorb the direct effect of kindergarten exposure and other (unobserved) school-level characteristics such as the quality of the district school on fertility. Hence, any remaining threat to identification would need to differentially affect mothers with and without a child aged 6-11 within the same school district. Reassuringly, when we account for school district fixed effects in column (4) of Table 1, the point estimate on the interaction term remains negative and statistically significant at the 5-percent level, thereby mitigating the concerns of some unobserved confounding factors driving our results.

Overall, our results show an economically meaningful negative effect of kindergarten exposure on fertility. Social interactions of mothers with kindergarten teachers likely played a key role in changing the fertility behavior of poor families in St. Louis, since mothers with access to kindergartens only reduced fertility if they had a child of kindergarten age at the time of future family planning.

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

Now we turn our focus to the kindergarten movement in other American cities. As in St. Louis, we will see that for mothers adjusting fertility in cities with access to kindergartens, it was crucial to have a child of kindergarten age at the time of future family planning to fully internalize the costs and benefits of a kindergarten education. Since the city-level kindergarten statistics do not provide such detailed information as the St. Louis school reports, we do not present event-study estimates as in the previous subsection.[25] Yet these reports contain detailed city-level statistics on the total number of kindergartens, allowing us to exploit 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 women 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:

$$y_{ict} = \alpha_c + \alpha_t + \beta Kindergarten\ Exposure_{ct} + \Gamma X_{ict} + \epsilon_{ict} \quad (3),$$

where the main outcome variable, y_{ict}, is a woman's number of own children under age 5. As in the St. Louis case study, we also consider school attendance of 5 to 6-years-old as an outcome.[26] Our measure of interest, *Kindergarten Exposure*, reflects for a woman of childbearing age the expanded opportunity for sending a child to a kindergarten at the time when she conceived a child. It follows the concept of Aaronson et al. (2014) and is constructed as:

$$Kindergarten\ Exposure_{ct} = \frac{1}{5}\sum_{k=1}^{K=5} \frac{Kindergarten\ Capacity_{c,t-k}}{Children\ Age\ 5\ to\ 6_{c,t-k}} \quad (4),$$

where *Kindergarten Capacity* denotes the number of kindergartens in a given city multiplied by the average enrollment number of kindergarten pupils.[27] 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. This exposure measure varies across cities and time.

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[25] 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.

[26] 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").

[27] 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.

For the fertility regressions, we add to equation (3) 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,[28] 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.[29] In some specifications, we can also exploit variation across households *within a city* which allows us to control for city-by-birth 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 the fertility behavior of a specific cohort in a city at the same time as the kindergarten roll-out occurred. In addition, our strictest set of controls also includes the full interactions of city-by-year-by-birthplace fixed effects. These should account for changes in cultural norms by nationality groups across cities over time, such as a specific change in the fertility behavior of culturally British households as documented in Beach and Hanlon (2022) or a specific role of the German community that varies by city and over time. 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.

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 3 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.[30] The set of covariates includes cities' average occupational score, the share of white-collar and blue-collar skilled workers, the share of 10 to 15-year-old children working, the share of foreign-born, the share of Germans (1st and 2nd generation), 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 the estimates are insignificant.

[28] 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).
[29] 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.
[30] Since there are no data by enumeration district available, we cannot perform a similar analysis for the St. Louis analysis.

Only log city size and teachers per capita are correlated with kindergarten exposure in 1900 and 1910. The inclusion of city-by-birth year fixed effects in the main analysis aims at accounting for these factors. Furthermore, we show in robustness Section 4c that our results are not explained away by the general expansion of public city-school systems.

Next, we check whether our kindergarten exposure measure, as defined in equation (4), is related to the 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 group 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. We show that this was the case in column (1) of Table 2. Columns (2)-(3) reveal that there is no significant difference in kindergarten attendance by gender. The remaining columns of Table 2 report sample splits by age 4-7. The estimates reveal that the relationship between kindergarten exposure and attendance is the strongest for 5 to 6-year-olds as one would have expected.

After having verified that our kindergarten measure captures school attendance of kindergarten-age children, our focus turns to estimating the relationship between kindergarten exposure and fertility. Our analysis is based on the full-count sample of white women aged 18-44, who resided in American cities during the period 1880-1910. We estimate equation (3) by ordinary least squares including, besides kindergarten exposure, fixed effects for city and year, a 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 3 (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 3 (Panel A) reveal why the overall effect of kindergarten exposure on fertility is relatively modest. Column (2) shows that household fertility substantially declines in cities with more kindergarten exposure *if the family has a child of kindergarten age*. As for the St. Louis case study, we obtain this result by adding to estimating equation (3) 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 (for further details see pages 17-18). Importantly, in the specifications with the interaction term, we always control also for family size, i.e., we account for the direct effect of having a 6- to-11-year-old child and we include a dummy whether a 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 statistically 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. Reassuringly, this result does not depend on the rich set of control variables added. When only including fixed effects for city and year and controlling for family size, the estimated coefficient on the interaction term is -0.117 with a standard error of 0.03 which is very close to the result reported in column (2).

Moreover, column (3) also shows that kindergarten exposure does not affect fertility decisions in families that *only have small children* (under age 5). The point estimate on kindergarten exposure is close to zero and statistically insignificant. This also mitigates the concern that the kindergarten roll-out is capturing some underlying unobserved city-specific factors that triggered a general fertility decline across all households.

In columns (4)-(5) of Table 3 (Panel A), 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) to test whether cities with a higher initial child labor share experienced a stronger fertility decline. 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.

Next, we evaluate whether the observed fertility decline associated with the kindergarten movement is mainly driven by immigrant households. 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 foreign-born mothers. The final two columns of Table 3 (Panel A) show that immigrant households' fertility responded stronger to kindergarten exposure. 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 estimate presented in column (7) is sizeable: the roll-out of kindergartens explains about 12 percent of the overall fertility decline immigrant mothers experienced over the sample period. The stronger fertility decline for foreign-born women is consistent with the results presented in Section 5 that children of immigrant families experienced large returns from kindergarten education.

One important econometric advantage of the modified specification which contrasts the impact that kindergarten exposure had on fertility for different types of households (i.e., with and without 6- to 11-

year-old children) is that we can include city-by-birth year fixed effects. 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-birth year fixed effects. Hence, any remaining threat to identification would need to differentially affect households with and without 6- to 11-year-old children. We present these results in Panel B of Table 3. The estimating equation is (3) and the method of estimation is ordinary least squares. Besides the previous set of controls and adding city-by-birth year fixed effects, we also include all possible interaction terms between birthplace, year, and city fixed effects. Reassuringly, the point estimate on the interaction term is always negative and statistically significant at the 1-percent level, thereby mitigating the concerns that unobserved time-varying city-specific factors are driving our results. Results are similar when restricting the sample to include only women aged 25-34 (available upon request).

Overall, our results show an economically meaningful negative effect of kindergarten exposure on fertility. Direct interactions with the kindergarten teacher likely played an important role in changing the fertility behavior of urban working-class and immigrant families, since we find mothers in cities with high kindergarten exposure only reduced fertility once they had a child of kindergarten age.

c. Robustness Checks

One potential concern is that our findings simply reflect the general expansion of the public-school system that 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. We deal with this issue in this subsection.

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 indicates 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 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).

Was the fertility decline 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 similar to equation (4). For every census year, city-level exposure to public (high) schools is defined as the number of public (high) school teachers (net of public kindergarten teachers) multiplied by an assumed class size of 35 relative to the number of children between age 5 -21 (age 14-18).[31] 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 (2) of Table 3 (Panel B), but we 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 4. Column (1) shows the baseline results based on this sample to facilitate the 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, exposure to kindergartens still substantially reduced the fertility of families living in cities with a high initial child labor share and in immigrant households (columns 6 and 8). Results are similar when restricting the sample to include only women aged 25-34 (available upon request).

Next, we show that the negative association between kindergarten exposure and fertility is not driven by changes in female labor supply, the age of marriage, or child mortality. 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), changes in the labor supply of affected mothers could explain away the effect of kindergarten exposure on fertility if the time children spent in classroom freed up maternal labor supply (albeit the historical narrative suggests that mothers simply left children unattended in the streets while being at work).[32] Column (1) of Appendix

[31] An assumed class size of 35 reflects the average pupil-teacher ratio between 1880-1910 (Snyder, 1993; Table 14). Note that we subtract the number of kindergarten teachers in public schools from the overall number of public teachers in a given city.
[32] There is anecdotal evidence of a few businesses providing kindergartens for their workforce (Vandewalker 1908).

Table 4 shows that once we add to the baseline specification of column (2) of Table 3 (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 result.[33] The 1900-10 Censuses 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), when including all three controls (only possible for the sample 1900-10). The estimated coefficient on kindergarten exposure is similar to the results without adding the three additional controls but restricting the sample to 1900-10 (column 5).

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 5 illustrates that our results also hold when using a balanced panel of cities (columns 1-2) or when considering a decomposition exercise in the spirit of Goodman-Bacon (2021, Figure 2). Reassuringly, the negative association between kindergarten exposure and fertility is also present when comparing treated cities to "clean" control cities. This comparison can be made using a sample that only contains early-treated cities (receiving the first treatment between 1880-1900) and untreated cities (column 3), late-treated cities (receiving the first treatment between 1900-1910) and untreated cities (column 4), and early treated cities and late treated cities where we restrict the sample to include only the years 1880-1900 (column 5).[34]

We further show in Appendix Table 6 that our results are not sensitive to normalizing kindergarten capacity by the number of 18- to 44-year-old women instead of children aged 5-6. We also evaluate in Appendix Table 7 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 where most of the immigrant population at that time lived (the point estimate is 3 times as large).

5. Mechanisms

So far, we have provided compelling evidence that the roll-out of kindergartens contributed to the fertility decline in St. Louis and other American cities over the period 1880-1910. In this section, we study the effect of kindergarten exposure on children's outcomes which serve also as potential mechanisms to

[33] 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).

[34] Since we have a continuous treatment measure, we cannot exactly replicate Figure 2 of Goodman-Bacon (2021) but we regard the results presented in columns (3)-(5) of Appendix Table 5 as suggestive evidence that potential heterogenous treatment effects will not substantially confound our results.

explain the observed fertility decline. 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 fertility decline associated with kindergarten exposure is consistent with the prediction of theories which emphasize the importance of human capital for the fertility transition (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 reduce the number of children ("quantity").

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 we 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 at age 10-15 were more likely to attend school and did not work if they were exposed to kindergartens at age 5-6. 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 increased returns to education and the loss of income from child labor.

a. The Returns of Kindergarten Education

American educators generally acknowledged towards the end of the 19th century 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 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 immigrant 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 schoolteachers 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 at working 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 the loss of 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). Moreover, 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 baseline model as 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., via personal interactions 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 suggesting that 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 studying 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 aged 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 linking individuals over time, we can consider the impact of kindergarten exposure on both boys and girls.[35]

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

$$y_{ibct} = \alpha_c + \alpha_t + \beta Kindergarten\ Exposure_{bct} + \Gamma X_{ibct} + \omega_{\mathrm{bs}} + \epsilon_{ict} \qquad (5),$$

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.

[35] The basic assumption is that children received kindergarten exposure at the same place where they were listed in the Census at age 10-15. 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.

The variable of interest, $Kindergarten\ Exposure_{bct}$, is the kindergarten capacity (see page 18 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 living in city *c* in 1910 is assigned the average kindergarten exposure of 1903-04 of this city). 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 year, 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 of whether each parent's occupation was still not classified by IPUMS. Estimating equation (5) further includes state-by-birth year 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 year 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 (5). The method of estimation is ordinary least squares. Column (1) shows that children more exposed to kindergartens at ages 5-6 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, 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. Kindergarten exposure when children were 5 to 6-years-old increased their likelihood of attending school as young adolescents by up to 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.[36] 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.[37] 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.[38] Again, results are driven by children from immigrant homes (columns 2-3). Those are substantially less likely to work at age 10-15 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.

d. Kindergarten Exposure and English Proficiency – A Measure of Social Assimilation

One major goal of the kindergarten movement in the U.S. was to facilitate the social 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 can speak English, we can test whether exposure to kindergartens increases the likelihood of immigrant children (and their parents) 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.[39]

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 (5) 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-birth year

[36] 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.
[37] 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.
[38] We further excluded children from the analysis that had their occupations still not yet classified by IPUMS.
[39] 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 purposes 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. Results remain almost unchanged in column (2) when adding city-by-cohort fixed effects. Columns (3)-(5) present results by age. In contrast to Table 5, the effect of kindergarten exposure on English fluency is driven by the youngest cohort in our sample (age 10-11). This finding is consistent with the notion that these differences disappear the longer the children stayed in the U.S.

Next, we investigate whether language spillover effects from children attending kindergarten to their mothers existed. We focus on one particular spillover effect, that is, whether kindergarten attendance and the potential interaction with kindergarten teachers enhance 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 in 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.

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

$$y_{if} = \alpha_f + \beta mother_i + \gamma\left(mother_i \times child\ attends_f\right) + \Gamma X_{if} + \epsilon_{if} \qquad (6),$$

where the dependent variable is a dummy variable for whether 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 (6), 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 (6) 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.

Overall, exposure to kindergartens helped children of non-English-speaking households acquiring English proficiency, an important skill which increased the returns to schooling. Our results also provide suggestive evidence that the home visits and/or the children's classroom experiences accelerated the assimilation process of immigrant mothers in terms of acquiring basic English-language skills.[40]

e. Fertility Decisions of Exposed Children as Adults

The remaining part of the empirical analysis investigates whether children from immigrant homes exposed to kindergartens have fewer children as adults. For example, kindergarten exposure could have affected the preferences of immigrant children for having smaller families later in life. While it is beyond the scope of this paper to explore potential channels, we think it is worth studying whether such a pattern generally existed. The result can reveal whether access to kindergartens contributed to the convergence of immigrant fertility patterns towards U.S. fertility norms over more than one generation.

Recent advances in automated linking methods allow researchers to follow individuals across census years. The Census Linking Project (https://censuslinkingproject.org) provides the crosswalks of linked males used in this analysis.[41] 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. That is, we only look at the fertility decisions of married couples in 1920 and 1930. The estimates are based on estimation equation (5) using ordinary least squares. Panel A (B) of Table 8 reports the results based on the 1900-20 (1910-30) linked sample. Columns (1)-(3) summarize the 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

[40] Our finding relates 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) and Kuziemko and Ferrie (2014).
[41] We used the so-called "ABE-NYSIIS" standard links; See Abramitzky et al. (2021) for details on the linking methods.

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 second-generation immigrant males exposed to kindergartens during their childhood also live as adults in smaller families.

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

6. Conclusion

What was the 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 until now rigorous quantitative evidence on the role of kindergartens for historical development was 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. Most of those 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.

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 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, immigrant households reduced fertility since kindergarten exposure increased the returns to education for their children and reduced income from child labor as the progressive educators at that time had hoped for. Immigrant children exposed to kindergartens at age 5-6 also had fewer children later in life. We interpret this as evidence that the kindergarten movement in the late 19th century contributed to the closing of the immigrant-native fertility gap.

We believe that 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 of their mothers also indicates that kindergartens can play an important role in the social integration of immigrant families.

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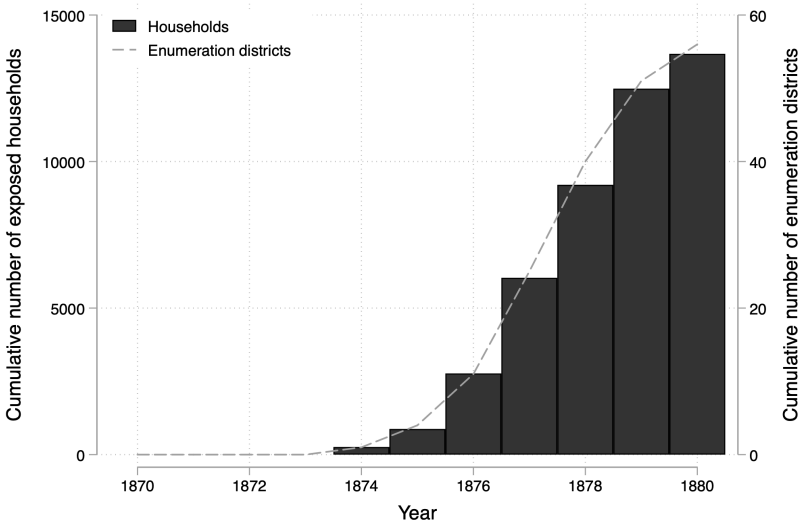
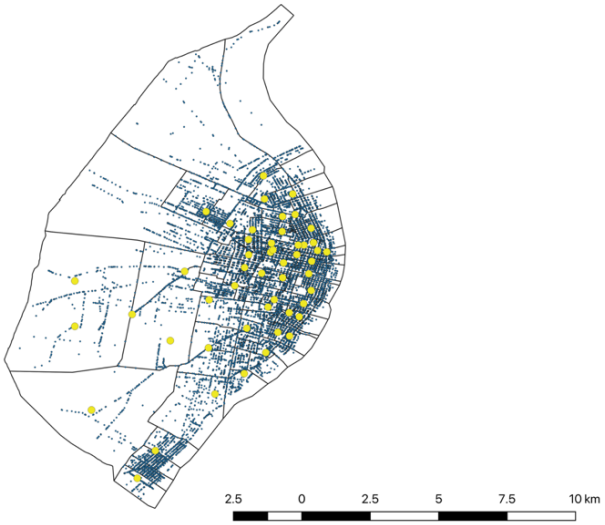
TABLES AND FIGURES

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



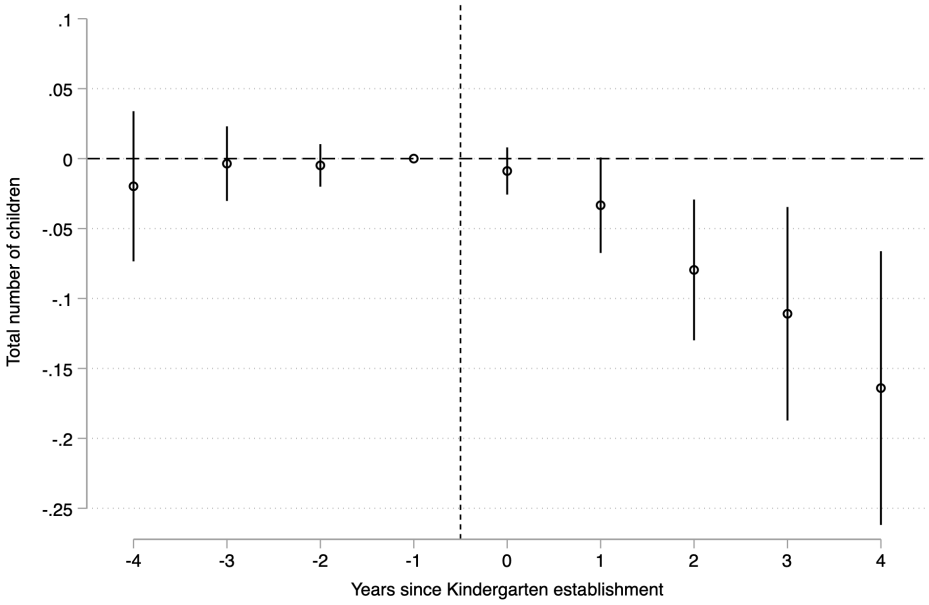
NOTE.— This figure displays on the left y-axis (right y-axis) the cumulative number of households (enumeration districts) exposed to a kindergarten in St. Louis between 1870 and 1880.

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



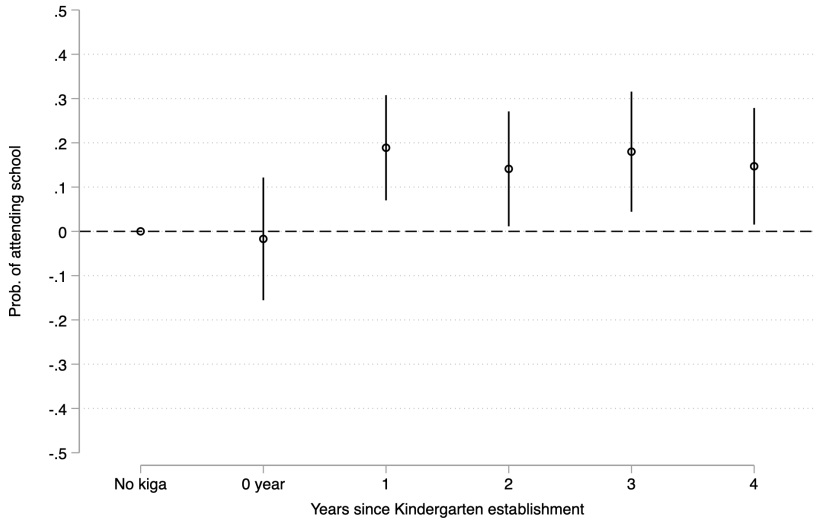
NOTE.— This map displays households (blue dots) together with the 1880 enumeration districts (gray lines) of St. Louis (see the Urban Transition Historical GIS project at https://s4.ad.brown.edu/Projects/UTP2/ncities.htm for further details). The kindergarten locations in 1886 (yellow dots) are based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

	(1)	(2)	(3)	(4)
	Dependent Variable: Children below Age 5			
$Kindergarten\ Exposure_d$	-0.0653***	-0.00585	-0.00174	
	(0.0233)	(0.0297)	(0.0295)	
$Kindergarten\ Exposure_d \times$		-0.0879**	-0.0882**	-0.0904**
$Has\ Kid\ Age\ 6-11_i$		(0.0408)	(0.0409)	(0.0410)
Individual Controls	Yes	Yes	Yes	Yes
Family Size	No	Yes	Yes	Yes
Spouse Controls	No	No	Yes	Yes
School District Fixed Effects	No	No	No	Yes
Observations	16,213	16,213	16,213	16,213
R-squared	0.087	0.136	0.139	0.141

NOTE.— This table shows the impact of kindergarten exposure on fertility for the 1880 cross-section of white females aged 25-34 in St. Louis. The dependent variable is the number of own children below age 5. The variable of interest, $Kindergarten\ Exposure_d$, is a dummy equal to one if a female residing in school district d had access to a kindergarten by 1880. Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11 in columns (2)-(4). These specifications also control for family size, i.e., 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 the women's birthplace, the birthplace of her father and mother, birth year, enumeration district, and the years since the district had access to a public school. Column (2) further controls for literacy and marital status of the women. Column (3) adds the following spouse controls: the occupational income score (in logs) of the husband, a dummy variable whether the husband worked in a white-collar or blue-collar skilled occupation, and whether the husband is foreign-born. Column (4) further includes school district fixed effects. Standard errors (in parentheses) account for arbitrary heteroskedasticity and are clustered at the school district level. ***, **, and * indicate significance at the 1, 5, and 10 percent level.

Table 2: Kindergarten Exposure and Attendance – City Level Analysis

	(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 $\times$ Birth Year 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 (4). All specifications include city, census year, and state-by-birth year fixed effects. Individual controls include fixed effects for birthplace interacted by year and by city, birth year, 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 3: Kindergarten Exposure and Fertility – City Level Analysis

	(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 × Birth Year 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)
$Kindergarten\ Exposure_{ct} \times$		-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 × Birth Year FE						
$Kindergarten\ Exposure_{ct} \times$	—	-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 Birth 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
Family Size	No	Yes	No	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 (4). 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 family size, i.e., 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-birth year fixed effects instead. Individual controls include fixed effects for birthplace interacted by census year and by city, fixed effects for birth year interacted by census year and by state, 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 year 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 4: Accounting for the Public School Expansion – City Level Analysis

	(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 Birth 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
Family Size	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 (4). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11. All specifications also control for family size, i.e., 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 3 (Panel B, column 2); see notes to Table 3 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: Children's Outcomes at Age 10-15 – City Level Analysis

	(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 Year FE	Yes	Yes	No	No	No	No
City FE x Birth Year 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 year fixed effects (columns 1-2), and city-by-birth year fixed effects (columns 3-6). Individual controls include fixed effects of birthplace interacted by year and by city, birth year, gender, and a set of parental controls including fixed effects for mother's birthplace (absorbing the direct effect of whether a child has an immigrant mother), 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 Year FE	Yes	No	No	No	No
City FE x Birth Year 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)-(2) are based on the whole sample; columns (3)-(5) 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 5 (see notes to Table 5 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 birth year 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.

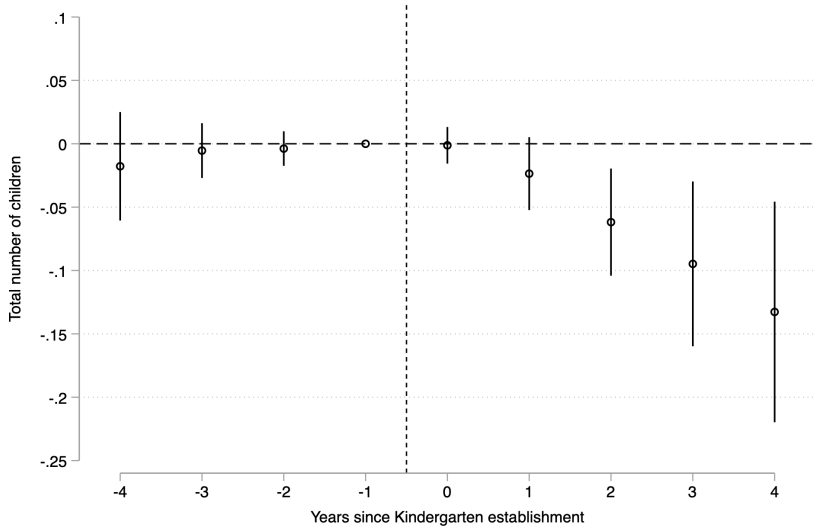
Table 8: Fertility and Family Size of Exposed Children as Adults

	(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 Year FE	Yes	Yes	No	Yes	Yes	No
City FE x Birth Year 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 of males in 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. In columns (2)-(3) and (5)-(6), kindergarten exposure is also interacted with a dummy variable whether the mother was foreign-born (the direct effect is absorbed by the fixed effects for a mother's birthplace). 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.

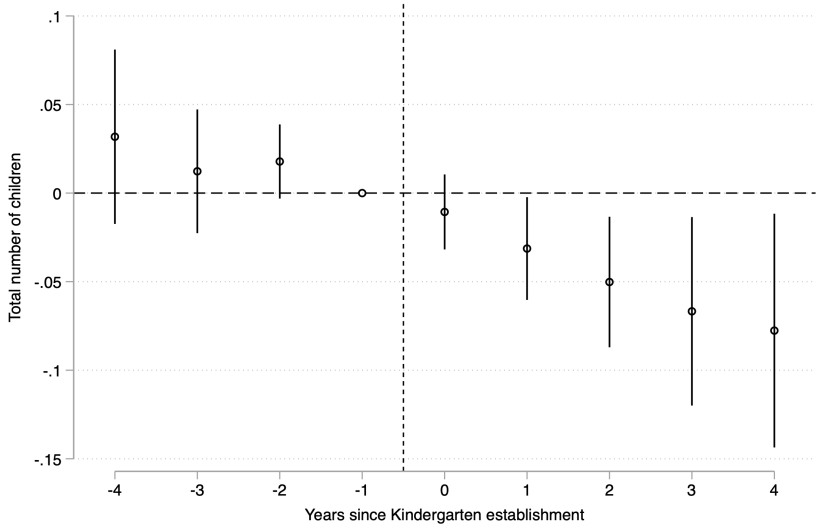
ONLINE APPENDIX TABLES AND FIGURES

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



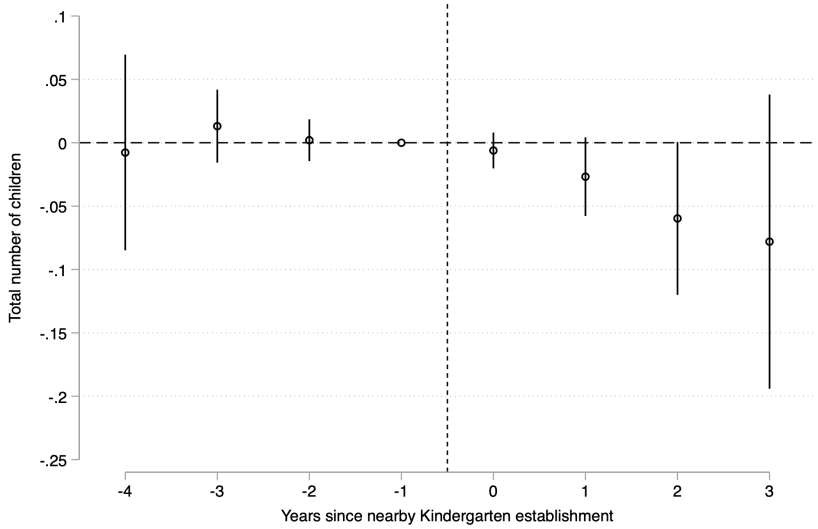
NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis restricting the sample to first and second generation immigrant mothers. The x-axis measures the number of years since the kindergarten opened in enumeration district *e*. The dots depict the estimated 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 Stacked Event-Study Regression



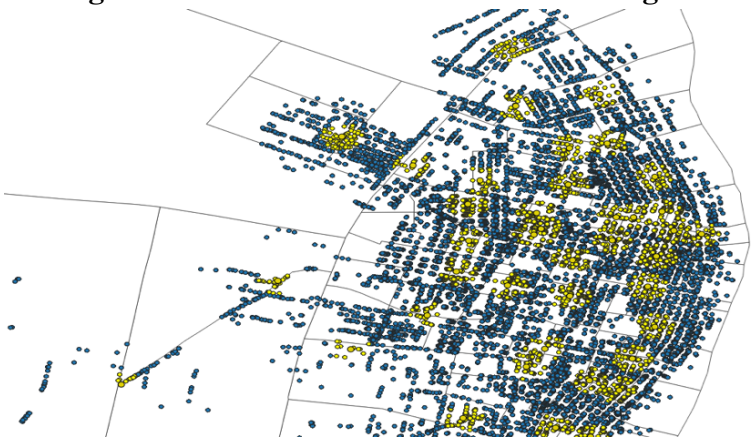
NOTE.— This graph shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis considering each kindergarten opening in an enumeration district as a separate event (stacked regressions). Control units are defined as those enumeration districts which did not experience any kindergarten 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 3:
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 to the nearest district-school as treatment. The sample includes only women within 1,000 meters radius of a district school. Treated women are those living within 250 meters from a given district-school with an active kindergarten in year t. All other women serve as control units. The specification includes fixed effects for a women's age, year, school district, the years since a district school was in business, and a dummy whether a household lived within 250 meters of a district school (independent of whether a kindergarten was in place or not). The x-axis measures the number of years since the treatment occurred. We merge t_{+3} and t_{+4} because of the relatively small number of observations in each group. The dots depict the estimated coefficients of nearby 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 school district level.

Appendix Figure 4:
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 district-school in St. Louis together with the 1880 enumeration districts. The households in yellow are located within 250 meters of a district-school with opened kindergarten and are considered as "treated".

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:
Estimated Coefficients for St. Louis Event Study Regressions

	(1)	(2)	(3)	(4)
	Dependent variable: Total Number of Children			
	Baseline	Foreign	Stacked	Proximity
Kindergarten establishment ($\tau \leq -4$)	-0.020	-0.018	0.032	-0.008
	(0.027)	(0.022)	(0.025)	(0.038)
Kindergarten establishment ($\tau - 3$)	-0.004	-0.005	0.012	0.013
	(0.014)	(0.011)	(0.018)	(0.014)
Kindergarten establishment ($\tau - 2$)	-0.005	-0.004	0.018*	0.002
	(0.008)	(0.007)	(0.010)	(0.008)
Kindergarten establishment ($\tau - 1$)	*Baseline*	*Baseline*	*Baseline*	*Baseline*
Kindergarten establishment (τ)	-0.009	-0.001	-0.011	-0.006
	(0.009)	(0.007)	(0.011)	(0.007)
Kindergarten establishment ($\tau + 1$)	-0.033**	-0.024	-0.031**	-0.027*
	(0.017)	(0.015)	(0.015)	(0.015)
Kindergarten establishment ($\tau + 2$)	-0.080***	-0.062***	-0.050***	-0.060*
	(0.025)	(0.021)	(0.019)	(0.030)
Kindergarten establishment ($\tau + 3$)	-0.111***	-0.095***	-0.067***	-0.078
	(0.039)	(0.033)	(0.027)	(0.057)
Kindergarten establishment ($\tau \geq +4$)	-0.164***	-0.133***	-0.078**	
	(0.050)	(0.044)	(0.033)	
Mother FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mother's age FE	Yes	Yes	Yes	Yes
Years since district school	Yes	Yes	Yes	Yes
Observations	372,105	291,576	1,875,123	339,379
R-squared	0.923	0.924	0.935	0.315

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), and Appendix Figure 3 (column 4). 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). In the proximity analysis in column 4, we collapsed $\tau + 3$ and $\tau \geq +4$ due to a low number of observations. 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 3: 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)
% Germans (1st/2nd gen)	-0.118	-0.081
	(0.106)	(0.137)
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 4) 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 4:
Controlling for Maternal Labor Supply, Duration of Marriage, and Child Mortality

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Children below Age 5				
$Kindergarten\ Exposure_{ct} \times$	-0.133***	-0.064***	-0.061***	-0.062***	-0.071**
$Has\ Kid\ Age\ 6-11_i$	(0.031)	(0.024)	(0.019)	(0.019)	(0.024)
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
City FE x Year FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes
Family Size	Yes	Yes	Yes	Yes	Yes
Extra Control	Works	Duration Marriage	Child Mortality	All	None
Sample	1880-1910	1900-1910	1900-1910	1900-1910	1900-1910
Observations	8,575,961	6,766,944	5,462,489	5,462,489	7,300,784
R-squared	0.185	0.160	0.203	0.210	0.175

NOTE.— This table shows that our baseline result in Table 3 (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); all the three extra controls together (column 4); or none of the three extra controls. Note, the sample in columns (2)-(5) is restricted to the census years 1900 and 1910 because of data limitations. 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 (4). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11. All specifications also control for family size, i.e., 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 3 (Panel B, column 2). We refer the reader to the notes of Table 3 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 5: Balanced Panel and Treatment Heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Children below Age 5				
	Balanced Panel		Early/Never	Late/Never	Early/Late
$Kindergarten\ Exposure_{ct}$	-0.042				
	(0.028)				
$Kindergarten\ Exposure_{ct} \times$		-0.171***	-0.098***	-0.0680*	-0.179***
$Has\ Kid\ Age\ 6-11_i$		(0.041)	(0.036)	(0.038)	(0.038)
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	No	No	No	No
City FE x Birth Year FE	No	Yes	Yes	Yes	No
Individual Controls	Yes	Yes	Yes	Yes	Yes
Spouse Controls	Yes	Yes	Yes	Yes	Yes
Family Size	No	Yes	Yes	Yes	Yes
Sample	1880-1910	1880-1910	1880-1910	1880-1910	1880-1900
Observations	7,141,522	7,139,670	1,789,315	556,974	850,261
R-squared	0.158	0.183	0.187	0.185	0.186

NOTE.— This table presents various sub-samples of the sample used in Table 3. Columns (1)-(2) report results for a balanced panel of cities. Column (3) report results for early treated (between 1880-1900) vs untreated cities. Column (4) report results for late treated (between 1900-1910) vs untreated cities. Column (5) report results for early vs late treated cities (the sample is restricted to include only 1880-1900). Columns (3)-(5) includes only cities if they are listed in all three census years (see Section 4c for further details). 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 (4). Kindergarten exposure is also interacted with a dummy variable whether a woman has a child aged 6–11 in columns (2)-(5). These specifications also control for family size, i.e., 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)-(5). This table includes the same set of individual and spouse controls as Table 3. We refer the reader to the notes of Table 3 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:
Replication of Table 3 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 Birth 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
Family Size	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 3 (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 family size, i.e., 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 3. We refer the reader to the notes of Table 3 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 3 – 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
$Kindergarten\ Exposure_{ct} \times$	-0.121***	-0.158**	-0.056***	-0.173***
$Has\ Kid\ Age\ 6-11_{it}$	(0.033)	(0.076)	(0.013)	(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
Family Size	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 3 (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 family size, i.e., 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 3 (Panel B, column 2). We refer the reader to the notes of Table 3 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.