Journal of Economic Growth
https://doi.org/10.1007/s10887-026-09271-z

Froebel's gifts: how the kindergarten movement changed the immigrant family

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Received: 2 February 2026 / Accepted: 24 June 2026

Abstract
Nineteenth-century social reformers promoted the establishment of kindergartens to facilitate the integration of immigrant children in the U.S. society. Exploiting newly collected data on historical kindergarten statistics, we evaluate the impact that the roll-out of the first kindergartens in U.S. cities had on immigrant families. We find that immigrant women exposed to kindergartens experienced a significant reduction in fertility. Their offspring were more likely to attend school, less likely to work during adolescence, and went on to have fewer children as adults. Kindergarten exposure also helped children of non-English-speaking households to acquire English proficiency, thereby illustrating the importance of kindergartens in the social integration of immigrant families. The observed fertility decline, in response to higher returns to education, is consistent with a child quantity–quality tradeoff.

Keywords Kindergarten education · Immigration · Fertility transition · Child labor · School attendance · Social integration

JEL Classification N31 · J13 · I25 · O15

1 Introduction

High birth rates, low levels of education, and reliance on income from child labor were common features of poor families during U.S. industrialization, and they still are in less developed countries today. The industrialization process in the U.S. was coupled with an unprecedented inflow of European immigrants, which increased poverty and concerns over

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assimilation. Despite many contemporary observers feared that industrialization and immigration amplified social problems, poor families started by the end of the nineteenth century to have fewer and better-educated children and relied less on child labor as a source of income. This 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 (Galor, 2005; Hazan and Berdugo, 2002; Doepke and Zilibotti, 2005; Moav, 2005).

Several reforms 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 U.S. cities during the late nineteenth century was part of such reforms. Between 1880 and 1910, more than 7,000 kindergartens opened in U.S. 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 immigrants (Berg, 2004). Influential educational reformers, such as Elisabeth Peabody or William T. Harris, saw in kindergartens a remedy for 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 U.S. 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 U.S. cities contributed to the transformation of immigrant families at the turn of the twentieth 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. Our main result is that exposure to kindergartens reduced fertility among immigrant families. Their children, if exposed at ages five to six, were more likely to attend school and speak English, and less likely to work during adolescence. Kindergarten-exposed immigrant children also had fewer children as adults, suggesting that access to kindergartens affected the fertility choices of immigrant households over multiple generations.

Our research strategy exploits two different data sources for identification. 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 (Mo.) Board of Education, 1874, 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.

For identification, we use the event-study design proposed by Sun and Abraham (2021), which exploits the staggered roll-out in kindergarten openings across St. Louis' enumeration districts. We find that immigrant 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. Importantly, this result also holds when using an alterna-

tive identification strategy based on street proximity to public schools. The latter approach deals with the issue that households that live closer to schools might have different preferences for children and education. This result is robust to accounting for the expansion of the public school system.

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 U.S. cities. We find that the roll-out of kindergartens explains up to 12 percent of the overall fertility decline that immigrant families experienced between 1880-1910. Since, on average, every fourth city dweller during our sample period was foreign-born, this effect is economically relevant.

These data also allow us to evaluate the effect of the kindergarten movement on children's outcomes. Exposure to kindergartens at ages five to six increased school attendance of immigrant children by up to 8 percentage points and reduced their likelihood of working as adolescents by up to 5 percentage points. In our estimation strategy, we include city and time fixed effects, and we control for individual characteristics such as literacy, age, and birthplace. Importantly, we also account for state-by-birth-year fixed effects, thus reducing the concerns that state legislation, such as the introduction or modification of laws concerning child labor and compulsory schooling, could confound our results.[1] When we contrast results between mothers with and without children of kindergarten age, we can also control for city-by-birth year fixed effects to capture time-varying local confounding factors. We also control for nationality-specific changes in cultural norms across cities that may have influenced fertility independently of the kindergarten movement (Beach and Hanlon, 2023).

Our results on the fertility decline of immigrant households and the increased school enrollment of their children 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 formal schooling, exist. In particular, we show that the general expansion of the public education system during the late nineteenth 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). We also show that the fertility decline associated with kindergarten exposure is not driven by changes in child mortality, a delay of marriage, or increased female labor force participation.

[1] 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 nineteenth century and early twentieth 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 aged 14 in 1900. Cinnirella and Harka (2025) find positive effects of compulsory schooling law in the context of nineteenth century Italy. On reducing child labor, Feigenbaum and Russo (2020) find both laws to be effective.

How did immigrant parents learn about the value of kindergarten education? Kindergarten teachers used home visits to inform immigrant mothers about the importance of child-rearing, home economics, U.S. customs, and the value of early childhood education. School reports, discussions in general education journals, or more specialized outlets such as the Kindergarten Magazine reveal that home visits were key for kindergarten teachers to gain access into immigrant homes (Shapiro, 1983; Klein, 1992; Berg, 2004).[2] Home visits helped reduce information frictions regarding the value of education and effective child-rearing practices, thereby increasing parents' perceived returns to education. Hence, in the context of a tradeoff between child quantity and quality, home visits would also affect fertility choices.

Our results indirectly suggest that personal interactions with kindergarten teachers, indeed, were very important for changing the fertility behavior of immigrant families. We find that only mothers who already had a child of kindergarten age at the time of family planning reduced fertility. Instead, mothers with only young children at the time of the census enumeration do not adjust their fertility behavior. Although 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.

We then study the returns to kindergarten education in terms of children's outcomes and show that they are consistent with a child quantity–quality tradeoff. In particular, we examine whether adolescents are more likely to attend school and less likely to work if they had access to kindergartens at ages five to six. We find that this is indeed the case, with the effects primarily driven by immigrant children. Children of immigrant mothers who were exposed to kindergartens at ages five to six were more likely to attend school at ages 13–15, whereas for children of U.S.-born mothers, school attendance increased at ages 16–18 following exposure to kindergartens at ages five to six. These results suggest that complementarities between kindergarten and subsequent schooling operate at different points in the educational trajectory depending on maternal origin. For children of immigrant mothers, access to kindergarten appears to increase the likelihood of completing—or at least remaining enrolled in—regular schooling, while for children of U.S.-born mothers it primarily increases the likelihood of attending high school.[3] Results for child labor broadly mirror these patterns.

We also show that, for a similar age cohort, access to kindergartens at ages five to six improved English fluency among foreign-born children from non-English-speaking households. This finding aligns with anecdotal evidence from contemporary surveys, which suggest that kindergarten access enhanced immigrant children's English skills and better prepared them for school (e.g., Palmer, 1915; Waite, 1926; Berg, 2004).

Overall, our results reveal that having access to kindergartens affected children's outcomes approximately ten 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.

[2] Besides anecdotal evidence emphasizing the social benefits of such parent-teacher interactions, development studies have shown that providing low-income parents with relevant information about education can influence their decisions to invest more in their children's education (e.g., Jensen, 2010; Dizon-Ross, 2019).

[3] See also Ager et al. (2025), who examine the impact of free public kindergartens in the early twentieth-century United States on intergenerational mobility among children of both immigrant and native parents.

1.1 Related literature

Our paper is the first to assess the large-scale effects of the kindergarten movement on immigrant families in the United States during the Age of Mass Migration (1850–1920).[4] This period was characterized by (i) the inflow of millions of immigrants (e.g., Ager and Brueckner, 2013; Abramitzky and Boustan, 2017; Abramitzky et al., 2023) and (ii) a general decline in fertility (e.g., Bailey and Hershbein, 2018; Hacker and Roberts, 2019). Our historical 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 tradeoff 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).[5]

Our findings 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 twentieth century (i.e., Moehling, 1999; Goldin and Katz, 2008; Abramitzky et al., 2014, 2020, 2021b).

While we examine how access to kindergartens affects mothers' fertility, given that the fertility transition was a key event at the end of the nineteenth century, the modern early childhood education literature instead focuses on maternal labor supply and parenting practice. This literature generally finds small effects of kindergarten access on maternal employment (e.g., Gelbach, 2002; Cascio, 2009b) but documents that children's participation in early childhood education programs positively influences the parenting practices of low-income households (e.g., Gelber and Isen, 2013; Barr and Gibbs, 2022).[6] In our context, female labor supply is not a driving force for our fertility results.[7] Instead, home visits constituted an important service provided by historical kindergartens: they helped remov-

[4] The only other empirical study on the U.S. kindergarten movement that we are aware of is an unpublished PhD thesis chapter by Haimovich (2015). He finds positive long-term effects of exposure to public kindergartens on schooling and occupation-based earnings in 1940. Broadly related is also the work by Cascio (2009a) on the introduction of state funding for kindergarten programs in the 1960s and 1970s. She documents positive long-term effects for white children, including lower high school dropout rates and reduced institutionalization.

[5] Several empirical studies provide compelling evidence that such a tradeoff during (or even before) the demographic transition existed (i.e., Bleakley and Lange, 2009; Becker et al., 2010, 2012; Aaronson et al., 2014; Ager et al., 2020).

[6] 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, 2016). 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).

[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 nineteenth century stood at less than 5 percent (Goldin, 1977). This finding is also consistent with Aaronson et al. (2021), who find no systematic relationship between fertility and female labor supply in the U.S. before WWI.

ing 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.[8]

Several important studies in this literature have also evaluated the short- and long-term benefits of early childhood education programs on a wide range of cognitive and non-cognitive outcomes of participating children.[9] For studies outside the United States, see, e.g., Havnes and Mogstad (2011); Engle et al. (2011); Araujo et al. (2016); Rossin-Slater and Wüst (2020). One particular focus was on the dynamic skill complementarities between early childhood education and formal schooling (e.g., Heckman, 2006; Cunha and Heckman, 2007). We have incorporated these insights from this literature into our augmented quantity-quality tradeoff model.

Much of the empirical evidence evaluating early childhood education programs in the U.S. has had limited geographic coverage, small sample sizes (e.g., the Perry Preschool and Abecedarian programs), and relied on relatively small-scale survey data (for further references, see Cascio, 2021; Duncan et al., 2023). More recently, a few studies have used large, representative samples to evaluate childcare and preschool programs in the U.S., including Ager and Malein (2024), who evaluate the long-term impact of charity nurseries on disadvantaged children in early twentieth-century New York City; Herbst (2017) and Derrington et al. (2021), who study the long-term effects of the 1940 Lanham Act—a universal childcare policy implemented during World War II; and Bailey et al. (2021), who examine the long-term effects of Head Start. Our empirical analysis is based on full-count individual census data, including over 200 American cities. 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 10-18 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 by nationality, and (iii) to consider the effect of kindergarten exposure over two generations.

Finally, we contribute to a vast literature on the evolution of cultural norms and preferences of immigrants in host countries (e.g., Fernández and Fogli, 2009; Algan and Cahuc, 2010; Giuliano and Tabellini, 2020). Economic historians found that fertility levels of immigrant families at the turn of the twentieth century were higher compared to similar U.S. native households, but that this gap diminishes in the second generation (e.g., Guinnane et al., 2006; Hacker and Roberts, 2019). Indeed, our analysis based on a linked sample of second-generation immigrant males from the 1900-1920 and 1910-1930 Censuses shows that immigrant children who were exposed to kindergartens at ages five to six have fewer children as adults. This finding suggests that kindergarten exposure influenced the fertility decisions of immigrant families over (at least) two generations and contributed to the convergence towards native fertility norms.

[8] Similarly, home-visiting components in modern preschool programs for children below kindergarten age are considered an important policy tool for reducing information frictions about education and improving child-rearing practices in low-income households (e.g., Cunha et al., 2013; Elango et al., 2016).

[9] For the United States, studies include, among others, Currie and Thomas (1995); Garces et al. (2002); Fitzpatrick (2008); Deming (2009); Heckman et al. (2010); Carneiro and Ginja (2014); Heckman and Karapakula (2019); De Haan and Leuven (2020).

2 Historical Background

In this section, we describe the historical context of our study, beginning with the diffusion of kindergartens across the U.S. during the final decades of the nineteenth century. We then provide a brief account of the social role of kindergartens and their organizational structure, and conclude by discussing the importance of home visits conducted by kindergarten instructors.

2.1 The Kindergarten movement in the U.S.

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 were designed to educate children aged three to six and were intended to be applicable to all children, regardless of their social background. With daily sessions lasting three to four hours, Froebel's concept represented a compromise between family-based child-rearing and fully institutionalized care (Allen, 2017; Klein, 1992; Lascarides and Hinitz, 2013).

Froebel's kindergarten concept was transplanted to the U.S. when several 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.[10] One of them was Karl Schurz's wife, Margarethe, an enthusiastic advocate of Froebel's teaching methods, who opened the first kindergarten on U.S. soil in Watertown, Wisconsin, in 1856.[11] A few other German-speaking kindergarten pioneers followed Schurz to the U.S. and set up more kindergartens and trained instructors according to Froebelian principles. During the early phase of the movement, kindergartens were mainly tuition-based private institutions that 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

[10] Bauernschuster and Falck (2015) discuss the early spatial diffusion of kindergartens in Germany.

[11] 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).

general principles of kindergartens. By 1890, about 15,000 children were enrolled in the schools of 115 free kindergarten associations. 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 (Shapiro, 1983; Klein, 1992).

By the late 1880s, many large cities had gradually incorporated free kindergartens into the public school system, which 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; Vandewalker, 1908). Public kindergartens also served a socialization function, particularly by promoting the Americanization of children from non-English-speaking families. Classroom activities such as singing English-language songs, reading English stories, learning U.S. cultural customs, and receiving instruction in English were all intended to accustom immigrant children to the "American" way of life (Allen, 2017; Berg, 2004; Klein, 1992).

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 5-6-year-old children 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).

2.2 The social role of kindergartens

By the end of the nineteenth century, U.S. educators generally acknowledged the importance of education in the child's first years of life. Kindergarten advocates argued that kindergarten education was important for the child's development of 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, 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. For example, Waite (1926, p. 37), in her summary of several surveys on kindergarten training in city schools, emphasized that the inability to use the English language was a serious cause of delays in later schoolwork.

More generally, several contemporaneous surveys documented observations from teachers about the beneficial effects of kindergarten training. The survey by Holden (1905) 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 education, especially for foreign children (U.S. Bureau of Education, 1914, p. 93). These observations resonate with the survey by Palmer (1915), 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. It suggests that attending kindergarten 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 child-rearing costs, such as expenditures for proper clothes; it meant forgone income from child labor if the child stayed longer 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, if complementarities between kindergarten education and regular schooling existed, households would reduce fertility even if the unit cost of preschool investment per child declined. In Section 3, we present a theoretical model consistent with these notions.

2.3 The organization of kindergartens

Kindergarten instructions were offered on weekdays for around three to four hours per day. The classes were usually relatively small. In public kindergartens, one or two teachers instructed, on average, 25 children per room (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).[12] While tuition-based and free kindergartens served a specific segment of society, public school sponsorship contributed to the universal provision of kindergarten education. By 1910, almost 90% of kindergartens were publicly funded, and 85% of the enrolled children attended a public kindergarten (U.S. Bureau of Education, 1914).

[12] These enrollment numbers for tuition-based and free kindergartens are from the Reports of the Commissioner of Education (U.S. Office of Education, 1880, 1887).

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 mainly saw the focus on the child in class (Klein, 1992). 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).

2.4 The home and the kindergarten

Beyond caring for the well-being of young children, an important element of kindergarten pedagogy was to establish relationships with mothers. Kindergarten teachers arranged home visits and mothers' meetings. These meetings primarily targeted working-class and immigrant mothers, aiming to socialize them and elevate their social status. They were intended to enlighten immigrant mothers about general child-rearing principles, hygiene, nutrition, the value of kindergarten education, and home economics, as well as to familiarize them with English songs, stories, and classroom materials (Berg, 2004; Fisher, 1905; Klein, 1992). These meetings became later an integral part of the public school system and are considered forerunners of the modern Parent Teacher Associations (Klein, 1992; Ross, 1976).

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 U.S. 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).

Anecdotal evidence suggests that some kindergarten teachers visited each family monthly, while others aimed to make a set number of visits such as fifty per month (Berg, 2004). Hewes (1985) reports that these teachers appear to have been widely accepted by parents into their homes. Ross (1976) further explains that the high frequency of home visits contributed to teachers' acceptance, which in turn paved the way for acceptance of their ideas, including the belief that play was developmentally appropriate. The importance of home visits is further highlighted by the U.S. Bureau of Education's recognition that scheduling afternoon classes directly conflicted with teachers' ability to conduct home visits (Palmer, 1915, p. 25).

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.

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 three to four hours per day in the classroom, also involved regular interactions between teachers and mothers. Home visits and mothers' meetings provided 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. Below, we show that households adjusted fertility consistently with the prediction of an augmented quantity-quality model of fertility.

3 An augmented quantity-quality tradeoff model

Motivated by the dynamic skill complementarity literature (Cunha and Heckman, 2007; Heckman, 2006), we extend the quantity–quality tradeoff model in Galor (2012) by incorporating two types of potentially complementary investments—preschool and other human capital investments (e.g., formal schooling)—to illustrate how and why increased access to kindergarten education could negatively affect fertility. Let us consider a household utility function of the following form

$$U = (1-\gamma)\ln c + \gamma[\ln n + \beta \ln h(k,e)]$$

where $0 < \gamma < 1$ and $0 < \beta < 1$ are constant parameters. The household derives utility from consumption (c), the number of children (n), the level of human capital of each child (h), and the preferences are represented by a log-linear utility function. $h = h(k,e)$ indicates the level of human capital of a child determined by both k (kindergarten) and schooling e. h is an increasing concave function of the parental time investments in both types of education. The household faces a budget

$$y \geq c + yn(\tau^q + \tau^k k + \tau^e e)$$

where y is the potential income generated by the household if it supplies its entire unit of time to the labor market. Let τ_q be fraction of household's unit-time endowment required to raise a child with no quality investment (only quantity), τ^k and τ^e the fraction of the household's unit-time endowment required for each unit of preschool k (kindergarten) and formal education e, respectively. Thus, the household's potential income y is divided between expenditure on consumption c, and on child rearing, both in terms of quantity and quality. From solving the household's optimization problem with respect to n, we obtain the optimal number of children:

$$n^* = \gamma/(\tau^q + \tau^k k + \tau^e e).$$

Treating k and e as endogenous variables which are affected by the unit cost of preschool investment τ^k, results in

$$\frac{dn}{d\tau^k} = \frac{-\gamma\left[\left(\frac{dk}{d\tau^k}\right)\tau^k + k + \tau^e\left(\frac{de}{d\tau^k}\right)\right]}{(\tau^q + \tau^k k + \tau^e e)^2}.$$

If fertility falls due to a decline in preschool costs, one needs to assume that $dn/d\tau^k > 0$. This would require that $(dk/d\tau^k)\tau^k + k + \tau^e(de/d\tau^k) < 0$. If there is no complementarity between preschool and schooling (i.e., $de/d\tau^k = 0$), then this condition would amount to $(dk/d\tau^k)(\tau^k/k) < -1$. 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 $\tau^k k$, which causes a decline in fertility. This result is similar to the baseline model as outlined in Galor (2012) for changes in the cost of child quality. Even if a decline in τ^k decreases $\tau^k k$, fertility may still decline if $de/d\tau^k$ is sufficiently negative. This would be the case if strong complementarities between kindergarten education (k) and formal education (e) 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 the baseline model in Galor (2012).

Note that our model is silent on 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 indicates, the observed fertility decline would be in line with the prediction of the augmented quantity-quality tradeoff model outlined above.

4 The effect of kindergartens on fertility

4.1 St. Louis: a quasi-natural experiment

In 1873, the first public kindergarten in the U.S. opened at the Des Peres School in St. Louis. In this period, St. Louis had a population of around 350,000 inhabitants with a large share of first and second-generation immigrants which reached about 70% in 1880. This kindergarten started literally as an experiment to study "*the practical effects of Froebel's system*" (St. Louis (Mo.) Board of Education, 1874, p. 195). After the experience was deemed successful "*beyond expectations*", in the next year "*it was resolved to try the experiment in two schools near the centre of the town*" (St. Louis (Mo.) Board of Education, 1876, p. 95). By 1875, kindergarten education was already offered in seven schools with about 450 pupils regularly attending (St. Louis (Mo.) Board of Education, 1876, p. 98). To finance the expansion of the kindergarten system, a quarterly fee of one dollar was charged, with indigent children exempted, starting in the school year of 1876–77. Charges were dropped again in 1878. The Board of Education ended the experimental stage of the kindergartens in 1878 and integrated them permanently into the public school system (Troen, 1972).

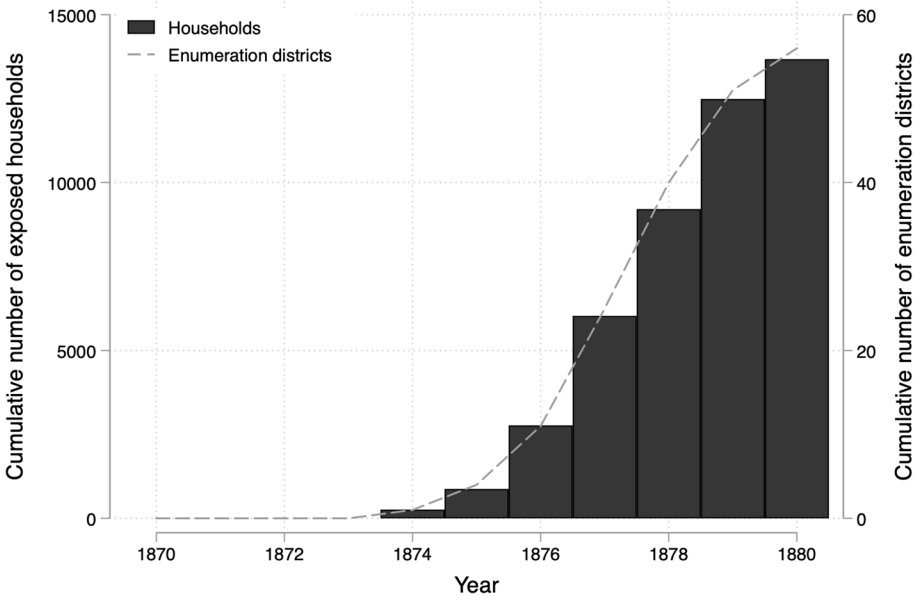

Fig. 1 The Roll-out of Kindergartens in St. Louis 1873-1880. NOTE.— This figure displays on the left y-axis (right y-axis) the cumulative number of households (enumeration districts) exposed to a kindergarten in St. Louis between 1870 and 1880

Despite not being mandatory, enrollment increased from 68 pupils in 1873 to 7,828 children in 1880 (St. Louis (Mo.) Board of Education, 1881, pp. 152–153). By this time, most schools were already involved in kindergarten activities and by 1886 more than 50 kindergartens (all public) operated within the city limits of St. Louis. Figure 1 illustrates the roll-out of kindergartens in St. Louis from 1873 until 1880.

The establishment of a public kindergarten system in St. Louis was a major step toward the universal acceptance of kindergartens in the U.S. 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 U.S. cities over the next decades. St. Louis demonstrated that kindergartens can be successfully integrated into the public school system and provides an interesting case in point to study the effect that the roll-out of kindergartens had on fertility. Data limitations restrict our investigation to the effects of kindergarten openings on fertility and on the "school" attendance of five- to six-year-old children.[13]

4.1.1 Data

For our case study, we collected information from the annual reports of the St. Louis public school board which contain information about the exact location and opening date of every public kindergarten that operated in St. Louis for the years 1873 to 1886. We geo-

[13] Most children with potential kindergarten exposure would be too young in 1880 to report a gainful occupation in the Census.

referenced the locations of the kindergartens operating in St. Louis between 1873 and 1886 and assigned them to their corresponding enumeration district (those roughly correspond in population size to contemporary census tracts) in the 1880 Census. IPUMS also includes the exact geo-referenced location of households for St. Louis in 1880.[14] Figure 2 depicts the locations of St. Louis' public kindergartens together with georeferenced households and enumeration districts as reported in the 1880 Census.

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

4.1.2 Results

We use the quasi "mother panel" to evaluate whether women adjusted their fertility behavior after having access to a kindergarten in their enumeration district. To test whether this is the case, we use a difference-in-differences design which exploits the fact that kindergartens opened in different enumeration districts at different points in time. One potential threat to identification would be if the fertility pattern in treated enumeration districts had already evolved differently before the kindergarten opened. Since we know the exact establishment date of every kindergarten in the sample, we can conduct an event study to observe the dynamic effects of kindergarten openings on fertility and, at the same time, test whether the coefficient of interest indicates any violation of the parallel trends assumption before treatment occurs.[16]

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

$$y_{iet} = \alpha_i + \alpha_t + \sum_{j \in T} \beta_j \, \text{Kindergarten}_{et}^{\tau+j} + \Gamma X_{iet} + \varepsilon_{iet} \tag{1}$$

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

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

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

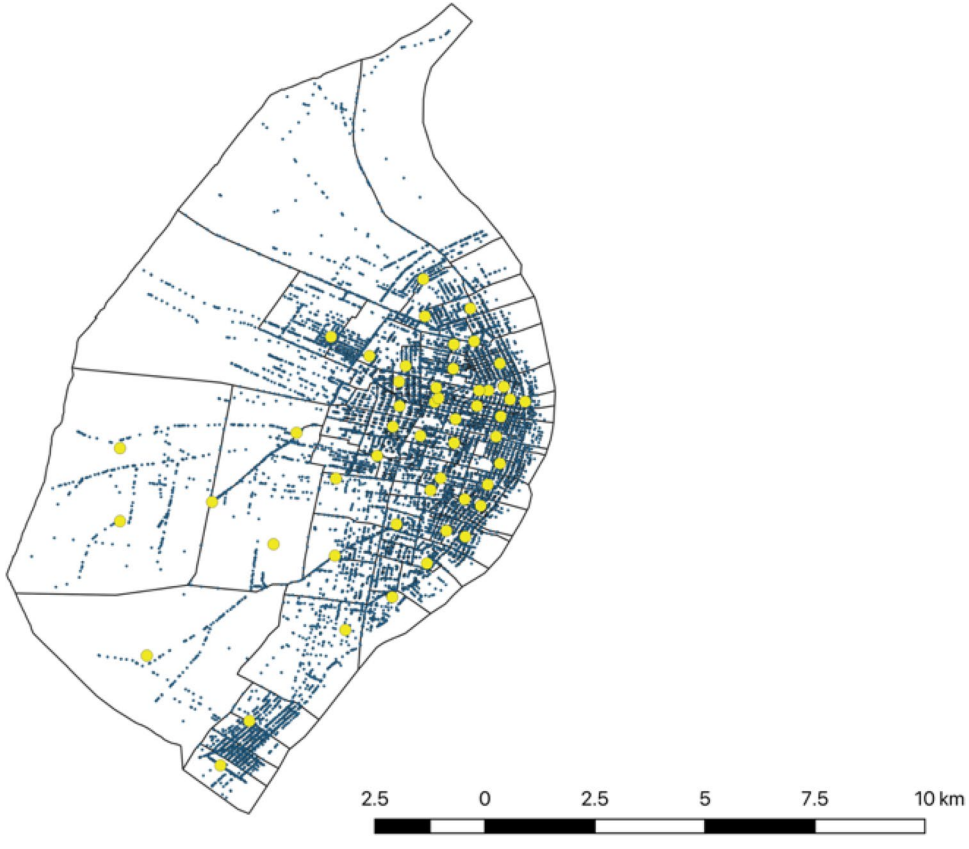

Fig. 2 Location of Public Kindergartens and Households in St. Louis 1880. NOTE.— This map displays households (blue dots) together with the 1880 enumeration districts (gray lines) of St. Louis (see the Urban Transition Historical GIS project at https://s4.ad.brown.edu/Projects/UTP2/ncities.htm for further details). The kindergarten locations in 1886 (yellow dots) are based on the historical map of St. Louis in 1882 (https://collections.leventhalmap.org)

where $T = \{-4, \ldots, -2, 0, \ldots, 4\}$. 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.[17] The parameter τ refers to the year of a kindergarten opening in enumeration district e. $\text{Kindergarten}_{et}^{\tau+j}$ is an indicator equal to 1 when $t = \tau + j$ and 0 otherwise. In order to capture the fertility response four and more years prior (after) the kindergarten opening, we define an indicator $\text{Kindergarten}_{et}^{\tau-4} = 1$ if $t \leq \tau - 4$ ($\text{Kindergarten}_{et}^{\tau+4} = 1$ 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.[18] Standard errors are clustered at the enumeration district level to account for correlations within an enumeration district in a given year and over time.

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

We begin our analysis by assessing whether the opening of kindergartens had any short-term impact on fertility. Figure 3, panel (a), reports the estimated coefficients of the relative time to a kindergarten opening based on equation (1).[19] For the periods before a kindergarten opening, we find that the estimated coefficients are very close to zero, which supports the parallel trends assumption. After treatment, the estimated coefficients become negative and statistically significant four years after the opening of a kindergarten, implying that the establishment of kindergartens caused a decline in fertility. This decline occurred gradually, which is reasonable as it takes time until mothers fully internalize the benefits of kindergarten education for their children. Moreover, the effect of a kindergarten opening after four years is economically relevant as it explains about 14 percent of the fertility variation within women in our sample.

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

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

[18] We argue that preferences for child quality are more likely to change between generations than within generations. This is also supported by the evidence presented in Section 6.

[19] The corresponding estimates are reported in column (1) of Table A.2.

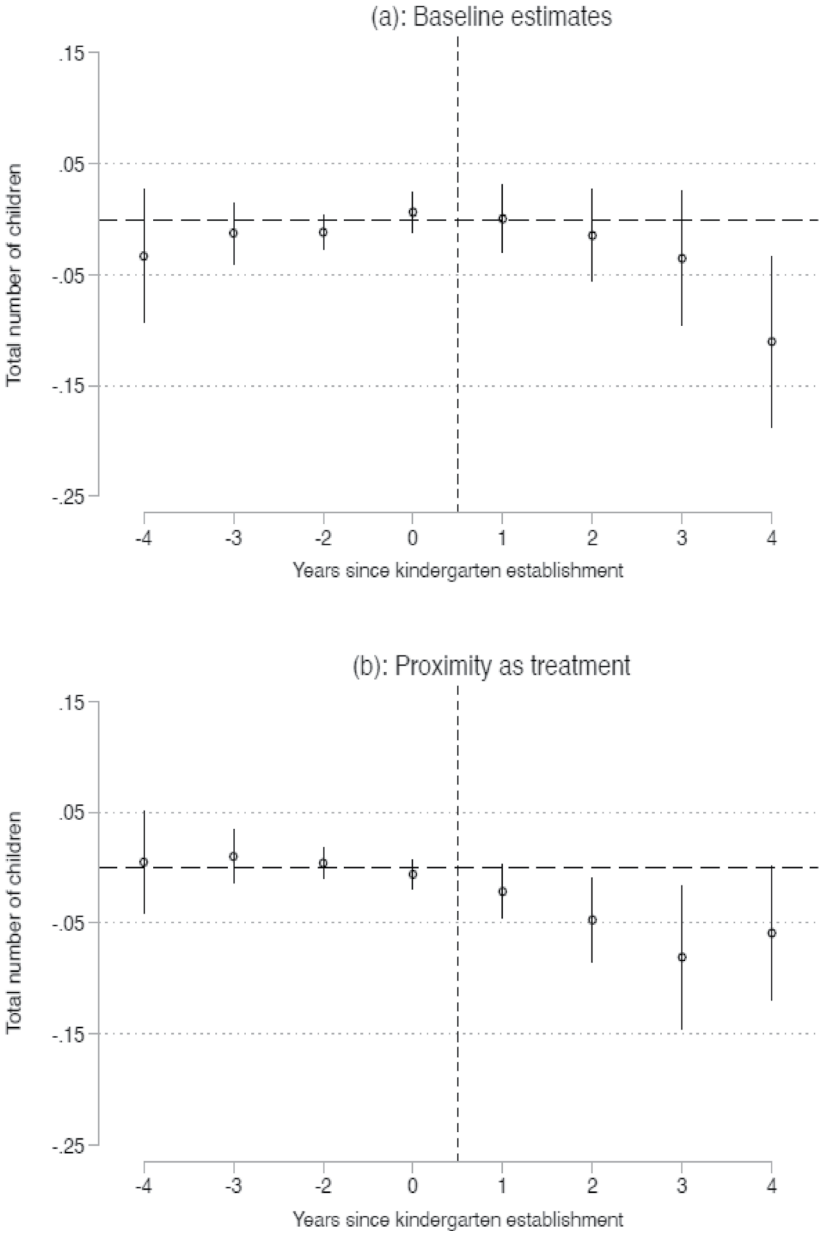

Fig. 3 The Effect of the Kindergarten Roll-out on Fertility in St. Louis. NOTE.— This figure shows the dynamic effect of the kindergarten roll-out on fertility in St. Louis. Panel (**a**) reports results for the whole sample; Panel (**b**) for households within 1,000 meters of a public school. The x-axis measures the years since the kindergarten opened in enumeration district *e*. Estimated coefficients (dots) including 95 percent confidence intervals (solid lines) of kindergarten exposure on fertility are reported relative to the base year -1 (omitted). Standard errors are clustered at the enumeration district level

unknown at that time. Furthermore, we present a different estimation approach using proximity to the closest public school as a treatment criterion.

To do so, we only include households within a 1,000-meter radius of a public school in the sample. Within this sample, households are considered as treated in year t if they were living within 250 meters of a public school with an active kindergarten (we always control for the main effect of living within 250 meters of a public school).[20] Figure A.1 illustrates the identification strategy based on a household's distance to a public school. The estimates of the specification with proximity to a school as treatment are shown in Fig. 3, panel (b). The pattern of the fertility decline is consistent with Fig. 3, panel (a). If anything, when using proximity as treatment, we observe a decline in fertility, which is even more pronounced. Overall, our results suggest that mothers significantly reduced fertility in the short term in response to kindergarten openings. We also find a fertility decline similar to Fig. 3, panel (a), for the sample of women with a five- to six-year-old child in 1880 (Fig. A.2).

As a plausibility check, we also show in Fig. A.3 that kindergarten openings led to increased kindergarten attendance. For this exercise, we keep the sample of mothers, but we only include those with a five- to six-year-old child in 1880. The outcome is a dummy variable for whether their five- to six-year-old child attended "school" in 1880.[21] Hence, differently from the previous specifications, we cannot include fixed effects for years and individuals as we have only cross-sectional variation. The estimates show that kindergarten attendance increased significantly in the first year after the kindergarten opening and remained constant over the successive years. The point estimates are statistically significant at the five-percent level. The establishment of a kindergarten in an enumeration district increased attendance by 14-19 percentage points compared to enumeration districts without a kindergarten. Relative to a mean of 32 percent, the estimated effect is thus substantial.

After we unveiled that the establishment of a kindergarten substantially increased the school attendance rate of five- to six-year-old children, we evaluated whether the observed fertility decline associated with kindergarten openings is driven by mothers who already had 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:

$$y_{id} = \alpha + \beta \,\text{Kindergarten Exposure}_d + \Gamma X_i + \varepsilon_{id} \tag{2}$$

where y_{id} is the number of own children under age five 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.[22] The variable of interest, $\text{Kindergarten Exposure}_d$, is a dummy which equals 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

[20] In the context of the interaction weighted estimator of Sun and Abraham (2021), we define women treated after 1880 or more than 250 meters away from a school as the control group.

[21] Since elementary school in St. Louis began at age seven, "attending school" at ages five to six captures kindergarten attendance.

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

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 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 five as the fertility measure. In the specification with the interaction term, we also control 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 one-percent level. Women with access to kindergartens have significantly fewer children under age five, which resonates with our event-study results based on the mother panel presented in Fig. 3. Having access to a kindergarten by 1880 reduces the number of children under age five by 0.065, or six percent of the sample mean (the average number of children below age five in our sample is 1.11).

Column (2) 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 statistically significant at the 1-percent level, while for the reference group

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 five. 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 indicating 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

(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 five-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.

4.2 The Kindergarten Movement in U.S. Cities, 1880-1910

Now we turn our focus to the kindergarten movement in other U.S. 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.[23] Yet these reports contain detailed city-level statistics on the total number of kindergartens, allowing us to exploit temporal variation across cities in the intensity of kindergarten exposure for identification.

4.2.1 Data

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 U.S. Bureau of Education (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 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.

Our city-level 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

[23] 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.

cities during the period 1880-1910.[24] 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. Table A.1 presents detailed summary statistics.

4.2.2 Results

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*. We estimate the following equation:

$$y_{ict} = \alpha_c + \alpha_t + \beta\,\text{Kindergarten Exposure}_{ct} + \Gamma X_{ict} + \varepsilon_{ict} \tag{3}$$

where the main outcome variable, y_{ict}, is a woman's number of own children under age five. We also consider school attendance of five- to six-year-old children as an outcome.[25] 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:

$$\text{Kindergarten Exposure}_{ct} = \frac{1}{5}\sum_{k=1}^{k=5}\frac{\text{Kindergarten Capacity}_{c,t-k}}{\text{Children age 5-6}_{c,t-k}} \tag{4}$$

where *Kindergarten Capacity* denotes the number of kindergartens in a given city multiplied by the average enrollment number of kindergarten pupils.[26] The capacity is normalized by the target population (children aged five to six), 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.

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

[24] We consider only white women since their children were the main target group of the kindergarten movement. Census data for 1890 went lost in a fire and thus are not available.

[25] 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").

[26] 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 reports.

income score (in logs) of the husband,[27] a set of dummy variables indicating 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.[28] In some specifications, we can also exploit variation across households *within city*, which allows us to control for city-by-birth year fixed effects. This additional set of fixed effects accounts 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 (2023) 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 U.S. cities, it is informative to explore whether certain initial (1880) city characteristics predict kindergarten exposure in the subsequent census years. Table A.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.[29] The set of covariates includes cities' average occupational score, the share of white-collar and blue-collar skilled workers, the share of 10-15-year-old children working, the share of foreign-born, the share of Germans (1st and 2nd generations), the crude birth rate, log city size, and the share of 18-44-year-old women who 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 5-21-year-olds 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. 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 Section 4.2.3 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 five- to six-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 five- to six-year-old children attending a kindergarten ("attending school") in the census year. In column (1) of Table 2, we show that this was indeed the case. Columns (2)-(3) reveal that there is no significant difference in kindergarten attendance by gender. The remaining columns of Table 2 report results for sample splits by ages four to seven. The estimates reveal that

[27] 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).

[28] 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.

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

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 × 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 ages five to six; 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						
			Only children	Below Median	Above Median		
	All	All	Below Age 5	% Child Labor	% 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

Table 3 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable: Children below Age 5						
			Only children	Below Median	Above Median		
	All	All	Below Age 5	% Child Labor	% Child Labor	U.S. Born	Foreign Born
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 five. 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 indicating 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

the relationship between kindergarten exposure and attendance is the strongest for five- to six-year-old children, as one would have expected.

After having verified that our kindergarten measure captures school attendance of five- to six-year-old 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 U.S. 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 (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 five, 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 the estimating equation (3) an interaction term of kindergarten exposure with a dummy variable for whether a household has a 6-11-year-old child at the time of the census enumeration. Importantly, in the specifications with the interaction term, we always also control for family size, i.e., we account for the direct effect of having a 6-11-year-old child, and we include a dummy indicating whether a household has any older children (i.e., above age 11). For a given level of kindergarten exposure, families with a 6-11-year-old child experience a larger fertility decline compared to families facing the same exposure but without a 6-11-year-old child. The estimated coefficient on the interaction term is negative and statistically significant at the one-percent level, while for the reference group (i.e., families without a 6-11-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, compared to U.S.-born households, immigrant households' fertility responded more strongly to kindergarten exposure. Although both 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 one-percent level), the size of the estimated coefficient on foreign-born mothers is about twice as large. The stronger fertility decline for foreign-born women is consistent with the results presented in Section 5 that children of immigrant families experienced large returns from kindergarten education. Results are similar when including only women aged 25-34 (available upon request).

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 6-11-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. Any remaining threat to identification would need to differentially affect households with and without 6-11-year-old children. We present these results in Panel b of Table 3. 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 one-percent level, thereby mitigating the concerns that unobserved time-varying city-specific factors are driving our results. Moreover, the estimate presented is also economically sizable: the roll-out of kindergartens in column (7) explains about 12 percent of the overall fertility decline that immigrant mothers experienced over the sample period.[30]

Moreover, the detailed information provided by the Census allows us to estimate differential effects of kindergarten exposure by nationality. It is possible that various ethnic/cultural groups reacted differently to the opening of a kindergarten. Such a heterogeneous response could be due to differences in cultural norms regarding preschool education and/or different attitudes toward the social assimilation of children. In particular, we estimate separate effects for the following groups: Scandinavian, UK, Ireland, Germany, Western Europe (excluding Germany), Southern Europe, and Eastern Europe using U.S. born mothers as the baseline. The nationality of a household is defined on the basis of the birthplace of the mother. The estimated coefficients (and the corresponding 95% confidence interval) for each group are shown in Fig. 4. As one can see, the point estimates are negative for all groups, except for Southern Europeans. Our estimates indicate that the largest fertility response was from mothers coming from the UK, Ireland, Germany, and Eastern Europe. Scandinavians and Western Europeans show a comparatively smaller effect, similar in size to U.S. born mothers (in red).

[30] The mean kindergarten exposure for households with 6-11-year-old children is 0.049, hence fertility would decline by 0.01 (i.e., $-0.198 \times 0.049 = -0.01$). The average number of children below age 5 for foreign-born women in the sample fell from 0.915 in 1880 to 0.830 in 1910.

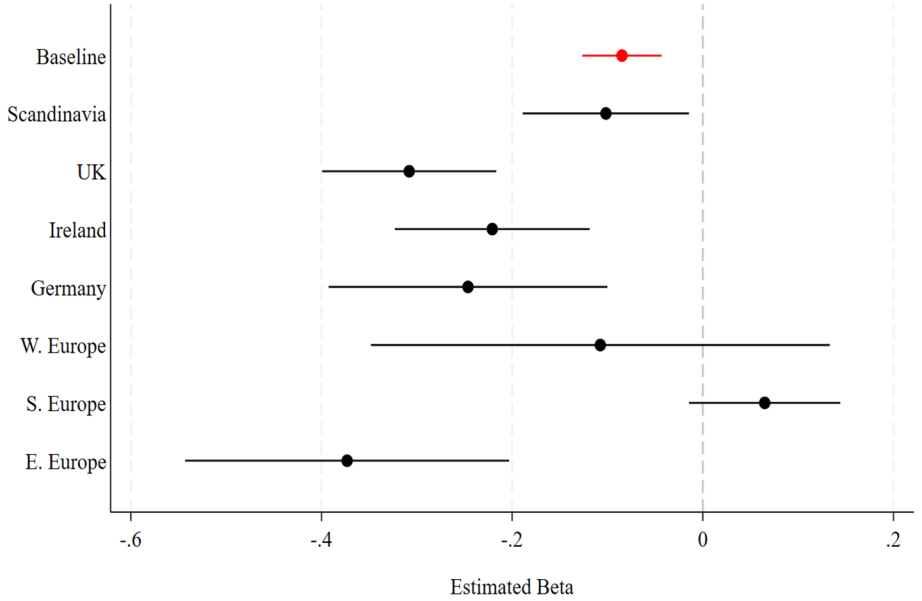

Fig. 4 Kindergarten Exposure and Fertility by Nationality – City Level Analysis. NOTE.— The figure shows the impact of kindergarten exposure on fertility for the census years 1880 to 1910 by nationality. The dependent variable is the number of own children below age five. The variable of interest, $Kindergarten\ Exposure_{ct}$ is calculated as described in equation (4) and interacted with a dummy variable for whether a woman has a child aged 6-11. The baseline coefficient reports the results for U.S. born mothers. The nationality specific coefficients are the joint effects of the baseline estimate $Kindergarten\ Exposure_{ct} \times Has\ Kid\ Age\ 6-11_i$, and the nationality specific coefficient $Kindergarten\ Exposure_{ct} \times Has\ Kid\ Age\ 6-11_i \times Birthplace_i$. The specification includes women born in the U.S., Scandinavia, Great Britain, Ireland, Germany, Western Europe (excl. Germany), Southern Europe, or Eastern Europe. The specification includes the same controls as in column (2) of Table 3 (Panel B). 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) are also interacted with the corresponding nationality dummies. Estimated coefficients (dots) include 95 percent confidence intervals (solid lines). Standard errors are clustered at the city level

Overall, our results show an economically meaningful negative effect of kindergarten exposure on fertility for immigrant households. 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.

4.2.3 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 nineteenth 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 (Goldin and

Katz, 2008; Parman, 2018). 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 in the public school enrollment rate of 5-17-year-old children from 65 to 74 percent indicates that the expansion was real. Pupils also went more days to school: The average length of the school term increased from 130 to 157 days over the same period (Snyder, 1993). While the spectacular increase in secondary enrollment rates took place between 1910-1940, several cities already operated high schools during our sample period. Despite high school graduation rates below ten 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 (Black and Sokoloff, 2006; Galor, 2011; Goldin, 1998).

Was the fertility decline that we associate with the roll-out of 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 ages 5 -21 (ages 14-18).[31] Both measures are also interacted with a dummy variable for whether a household has a 6-11-year-old child 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 6-11-year-old child, shrinks by about 40 percent, but it is still statistically significant at the five-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 including 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. Even if only 9 percent of the women in our sample work, changes in the labor supply of affected mothers could explain away the effect of kindergarten exposure on fertility if the time children spent in the classroom freed up maternal labor supply (albeit the historical narrative suggests that mothers simply left children unattended in the streets while being

[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.

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							
					Below Median	Above Median		
	All	All	All	All	% Child Labor	% Child Labor	U.S. Born	Foreign Born
$Kindergarten\ Exposure_{ct} \times$	-0.127***	-0.084***	-0.070**	-0.072**	-0.023	-0.169***	-0.056***	-0.146**
$Has\ Kid\ Age\ 6-11_i$	(0.035)	(0.031)	(0.033)	(0.032)	(0.037)	(0.056)	(0.021)	(0.058)
$Public\ School\ Exposure_{ct} \times$		-0.211***		-0.139***	-0.206***	-0.104	-0.117***	-0.197***
$Has\ Kid\ Age\ 6-11_i$		(0.023)		(0.032)	(0.059)	(0.068)	(0.027)	(0.074)
$High\ School\ Exposure_{ct} \times$			-0.201***	-0.095***	-0.023	-0.133*	-0.065**	0.015
$Has\ Kid\ Age\ 6-11_i$			(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 3, 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 five. The variable of interest, $Kindergarten\ Exposure_{ct}$, is calculated as described in equation (4). Kindergarten exposure is also interacted with a dummy variable indicating 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 of 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 child 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

at work).[32] Column (1) of Table A.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. 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. Table A.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, Fig. 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]

Finally, we show in Table A.6 that our results are not sensitive to normalizing kindergarten capacity by the number of 18-44-year-old women instead of children aged five to six. We also evaluate in Table A.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 three times as large).

5 The returns to kindergarten education

In this section, we study the effect of kindergarten exposure on children's outcomes. We argue that the fertility decline associated with kindergarten exposure documented in Section 4 is consistent with the prediction of theories emphasizing 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").

[32] Only a few businesses provided kindergartens for their workforce (Vandewalker, 1908).

[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 Fig. 2 of Goodman-Bacon (2021) but we regard the results presented in columns (3)-(5) of Table A.5 as suggestive evidence that potential heterogeneous treatment effects will not substantially confound our results.

In this context, we investigate the effect of kindergarten exposure at ages five to six on subsequent adolescent outcomes, such as school attendance, child labor, and English proficiency. We show that kindergarten exposure increased school attendance and reduced child labor, with particularly pronounced effects among children from immigrant households. For these children, kindergarten exposure also improved English proficiency. Taken together, these findings are consistent with the predictions of our augmented quantity–quality trade-off model. However, we acknowledge that other factors (e.g., through social imitation) might have also contributed to the observed fertility decline.

Our estimation approach exploits annual variation in kindergarten exposure across cities at the time when children were aged five to six, which allows us to test whether kindergarten education left a trace on children's outcomes about ten 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:

$$y_{ibct} = \alpha_c + \alpha_t + \beta\,\text{Kindergarten Exposure}_{bct} + \Gamma X_{ibct} + \omega_{bs} + \varepsilon_{ibct} \tag{5}$$

where y_{ibct} is a dummy variable if child i born in year b living in city c in census year t attends school, is working, or speaks English. We include all white children aged 10–18 in our sample at the time of the census enumeration. This analysis is based exclusively on the 1900–1910 censuses, as none of the 10–18-year-old children observed in 1880 would plausibly have attended kindergarten. Moreover, for English proficiency (only asked in the 1900-10 Census), the sample includes only 10–15-year-old immigrants from non-English-speaking countries.

The variable of interest, $\text{Kindergarten Exposure}_{bct}$, is the kindergarten capacity (see equation (4) and Section 4.2.2 for details) of city c normalized by the number of children aged five to six at the time when a child of cohort b was of age five to six (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 for each parent's occupation not classified by IPUMS (occ1950 = 979). 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). We cluster standard errors at the city level to account for correlations within a city in a given year and over time.

[35] The basic assumption is that children received kindergarten exposure at the same place where they were listed in the Census at ages 10-18. In fact, if there is no selective migration induced by kindergarten exposure, which is reasonable to assume at that time, migration would attenuate the results towards zero.

5.1 School attendance

First, we assess whether exposure to kindergarten education at ages five to six had a positive effect on school attendance during adolescence. Table 5 presents the results based on estimating equation (5) using ordinary least squares as the estimation method. Column (1) shows that children fully exposed to kindergarten at ages five to six are 3.1 percentage points more likely to attend school at ages 10-18. 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. Consistent with previous results, the estimates in column (2) reveal that the increase in school attendance was driven by children from immigrant homes. In columns (3)-(5), we investigate whether there is a differential effect by age group and maternal origin, splitting the sample by 10-12, 13-15, and 16-18-year olds. We find that kindergarten exposure had no statistically significant effect for the 10–12 age group, regardless of maternal origin (column 3); for ages 13–15, it had no effect for children of U.S.-born mothers but a strong positive effect for children of immigrant mothers (column 4); and for ages 16–18, it significantly increased school attendance in the upper grades of high school for children of U.S.-born mothers, while the overall effect for immigrant children was statistically insignificant (column 5).

In sum, these patterns suggest that complementarities between kindergarten and subsequent schooling arise at different stages of the educational trajectory depending on maternal origin. For children of immigrant mothers, access to kindergarten appears to increase the likelihood of completing—or at least remaining enrolled in—regular schooling, while for children of U.S.-born mothers it primarily increases the likelihood of attending high school. These findings are consistent with our theoretical framework.

Finally, we also investigate the kindergarten effect on school attendance by nationality using the same groups as in Fig. 4. These estimates are shown in Fig. 5. Except for the baseline (children of U.S. born parents), the coefficients display a positive sign, indicating a positive effect of kindergarten exposure on school attendance for all groups. However, the largest effects are for Germans, Western, Southern, and Eastern Europeans.

5.2 Child labor

Next, we consider whether the increase in school attendance of immigrant children is also reflected in a decline in child labor at ages 10-18.[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 and to what extent the roll-out of kindergartens contributed to this decline.[37] According to a standard quantity-quality trade-off framework of fertility, an 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 6 show that this is the case. The specifications are the same as in Table 5, but the outcome variable is a dummy for whether a 10-18-year-old child

[36] There is no mechanical relationship between school attendance and child labor, as children could attend school and still report a gainful occupation at the same time, or choose to be "idle" (no activity) instead.

[37] In our sample, the share of 10-18-year-old children with an immigrant (U.S. born) mother working fell from 35 (20) percent in 1880 to 23 (16) percent in 1910, a decline by about 34 (20) percent over 30 years.

Table 5 School attendance at age 10-18 – city level analysis

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Attends School				
	Age 10-18	Age 10-18	Age 10-12	Age 13-15	Age 16-18
$Kindergarten\ Exposure_{bct}$	0.031**	-0.004	0.010	-0.007	0.088***
	(0.015)	(0.015)	(0.020)	(0.022)	(0.022)
$Kindergarten\ Exposure_{bct} \times$		0.084***	0.010*	0.100***	-0.069***
$Has\ Immigrant\ Mother_i$		(0.016)	(0.006)	(0.015)	(0.019)
Joint Effect		0.080***	0.019	0.094***	0.019
p-value		0.000	0.365	0.000	0.404
Observations	7,526,514	7,526,514	2,637,165	2,502,103	2,382,134
R-squared	0.386	0.386	0.129	0.247	0.179
Mean(Y)	0.658	0.658	0.896	0.727	0.323
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE x Birth Year FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes
Parental Controls	Yes	Yes	Yes	Yes	Yes

Note.— This table shows the impact of kindergarten exposure on school attendance at age 10-18 for the census years 1900-10. The dependent variable is a dummy whether a child attends school. Columns (1)-(2) are based on the whole sample; columns (3)-(5) present results by age 10-12, age 13-15, and age 16-18, respectively. $Kindergarten\ Exposure_{bct}$, is city *c*'s kindergarten capacity normalized by the number of children age five to six at the time when a child of cohort *b* was of age five to six. Kindergarten exposure is also interacted with a dummy variable whether the mother was foreign-born. All specifications include fixed effects for city, census year, and state-by-birth year fixed effects. 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

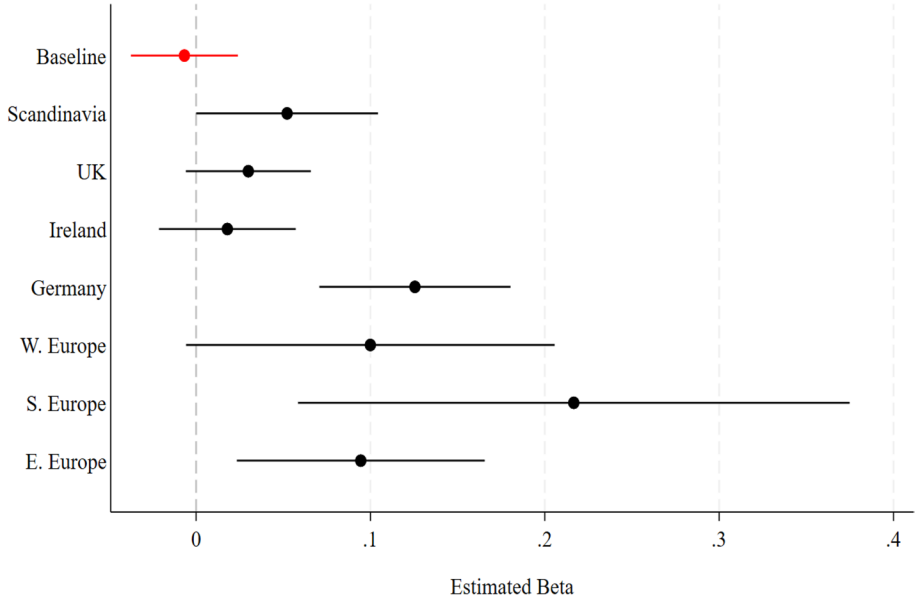

Fig. 5 Kindergarten Exposure and School Attendance by Nationality – City Level Analysis. NOTE.— The figure shows the impact of kindergarten exposure on school attendance for the census years 1900-10 by nationality. The dependent variable is whether a child aged 10-18 attends school. The variable of interest, $Kindergarten\ Exposure_{bct}$ is calculated as described in equation (4) and is also interacted with a dummy variable for whether the mother was foreign-born. The baseline coefficient reports the results for children with U.S. born parents. The nationality-specific coefficients are the joint effects of the baseline estimate $Kindergarten\ Exposure_{bct}$ and the nationality-specific coefficient $Kindergarten\ Exposure_{bct} \times Mother's\ Birthplace_i$. The sample includes children with U.S. born parents, a mother born in Scandinavia, Great Britain, Ireland, Germany, Western Europe (excl. Germany), Southern Europe, or Eastern Europe. The specification includes the same controls as in column (2) of Table 5. Estimated coefficients (dots) include 95 percent confidence intervals (solid lines). Standard errors are clustered at the city level

reported a gainful occupation. As in the previous analysis, the results differ substantially based on maternal origin. The estimates in column (2) show that there is a small but positive effect for children of U.S. born mothers and a relatively large negative effect for children of immigrant mothers. The estimates in columns (3)–(5) provide insight into which age group drives most of these results. In fact, the positive effect for children of U.S. born mothers is driven by the 10-12 year-olds, whereas the overall joint effect for immigrant children is zero (column 3).[38] For the age group 13-15, kindergarten exposure at ages five to six had zero effect for children of U.S. born mothers, but it had a substantial negative effect on immigrant children (column 4). For the age group 16-18, kindergarten exposure had a negative effect on children of U.S. born mothers and an overall insignificant effect for children of immigrant mothers (column 5).

In sum, these results paint a picture broadly consistent with the school attendance findings in Table 5. For children of immigrant mothers aged 13–15, kindergarten exposure increased the probability of staying in school while simultaneously reducing the likelihood

[38] These results, considered together with the previous results on school attendance, seem to suggest that exposed U.S. born kids in this age group worked and attended school at the same time.

Table 6 Child labor at age 10-18 – city level analysis

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Works				
	Age 10-18	Age 10-18	Age 10-12	Age 13-15	Age 16-18
$Kindergarten\ Exposure_{bct}$	-0.002	0.032**	0.013***	0.012	-0.043**
	(0.012)	(0.016)	(0.005)	(0.017)	(0.018)
$Kindergarten\ Exposure_{bct} \times$		-0.081***	-0.007***	-0.063***	0.041***
$Has\ Immigrant\ Mother_i$		(0.022)	(0.002)	(0.012)	(0.014)
Joint Effect		-0.049***	0.006	-0.051***	-0.002
p-value		0.004	0.230	0.006	0.932
Observations	7,526,514	7,526,514	2,637,165	2,502,103	2,382,134
R-squared	0.293	0.293	0.034	0.151	0.120
Mean(Y)	0.199	0.199	0.012	0.149	0.460
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE x Birth Year FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes
Parental Controls	Yes	Yes	Yes	Yes	Yes

NOTE.— This table shows the impact of kindergarten exposure on child labor at age 10-18 for the census years 1900-10. The dependent variable is a dummy whether a child reported a gainful occupation. Columns (1)-(2) are based on the whole sample; columns (3)-(5) present results by age 10-12, age 13-15, and age 16-18, respectively. $Kindergarten\ Exposure_{bct}$, is city *c*'s kindergarten capacity normalized by the number of children age five to six at the time when a child of cohort *b* was of age five to six. Kindergarten exposure is also interacted with a dummy variable whether the mother was foreign-born. All specifications include fixed effects for city, census year, and state-by-birth year fixed effects. 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

of engaging in a gainful occupation. By ages 16–18, however, exposed children of immigrant mothers became less likely to attend high school and more likely to work compared with exposed children of U.S.-born mothers. In contrast, kindergarten exposure at ages five to six significantly increased school attendance and decreased child labor at ages 16–18 for children of U.S.-born mothers.

Regarding the differential effect of kindergarten exposure on child labor by nationality, Fig. 6 presents a pattern broadly consistent with Fig. 5 for school attendance. The largest reductions in child labor due to kindergarten exposure are observed among German, Southern, and Eastern European children.

5.3 English proficiency

One major goal of the kindergarten movement in the U.S. was to facilitate the assimilation of immigrant children by teaching English through songs, rhymes, and stories. The 1900-10 censuses provide information on whether individuals aged 10 and older can speak English, allowing us to test whether exposure to kindergartens increased the likelihood that immi-

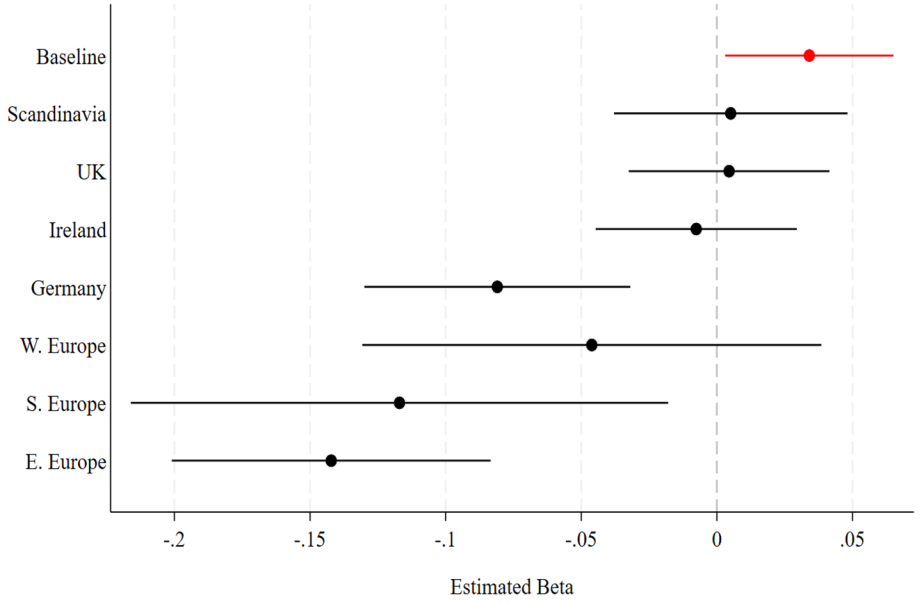

Fig. 6 Kindergarten Exposure and Child Labor by Nationality – City Level Analysis. NOTE.— The figure shows the impact of kindergarten exposure on child labor for the census years 1900-10 by nationality. The dependent variable is whether a child aged 10-18 works. The variable of interest, $Kindergarten\ Exposure_{bct}$ is calculated as described in equation (4) and is also interacted with a dummy variable for whether the mother was foreign-born. The baseline coefficient reports the results for children with U.S. born parents. The nationality-specific coefficients are the joint effects of the baseline estimate $Kindergarten\ Exposure_{bct}$, and the nationality-specific coefficient $Kindergarten\ Exposure_{bct} \times Mother's\ Birthplace_i$. The sample includes children with U.S. born parents, a mother born in Scandinavia, Great Britain, Ireland, Germany, Western Europe (excl. Germany), Southern Europe, or Eastern Europe. The specification includes the same controls as in column (2) of Table 6. Estimated coefficients (dots) include 95 percent confidence intervals (solid lines). Standard errors are clustered at the city level

grant children spoke English. If kindergarten education promotes English fluency, which is a key input for acquiring 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 10-15-year-old immigrant children from non-English-speaking countries. The 1900-10 censuses report their year of arrival, which is crucial for identifying a potential language effect of kindergarten education. In particular, we add to estimation equation (5) an interaction term between kindergarten exposure and a dummy for 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 she arrived at age six or earlier). We also 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 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 Tables 5 and 6.

Table 7 shows 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 find a positive and statistically significant effect of kindergarten exposure on speaking English only for treated cohorts, while the estimated coefficient on kindergarten exposure, which captures the effect on non-eligible children and acts as a placebo, is insignificant. Results remain almost unchanged in column (2) when adding city-by-cohort fixed effects. Columns (3)-(5) present results by age group. 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. Overall, exposure to kindergartens helped children of non-English-speaking households to acquire English proficiency, an important skill that increased the returns to schooling.

6 Fertility decisions as adults

In the remaining part of the empirical analysis, we investigate whether children from immigrant households who were exposed to kindergarten have fewer children in adulthood. 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. In fact, 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) provid es the crosswalks of linked males used in this analysis.[40] We use linked samples spanning the period 1900-20 and 1910-30. The sample is restricted to white boys aged 5-15 with a 20-55-year-old mother in the starting year, and we require that they had a spouse aged 18-44

[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 casts doubt on whether the returns to English fluency were as high historically (Ward, 2020).

[40] We used the so-called "ABE-NYSIIS" standard links; see Abramitzky et al. (2021a) for further details.

Table 7 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 1900-10. 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 five to six at the time when a child of cohort *b* was of age five to six. Kindergarten exposure is also interacted with a dummy variable whether the child arrived before age six in the U.S. (= $Eligible_{it}$). This table includes the same set of individual and parental controls as Tables 5 and 6. 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

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 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) show the results using the number of children below age five as outcome variable. Overall, the relationship between childhood kindergarten exposure and fertility is driven by children of immigrant mothers. The point estimate of the interaction term is always statistically significant at the five-percent level and implies that kindergarten exposure during childhood leads to a 0.05-0.07 decline in the number of children below age five. The family-size results reported in columns (4)–(6) indicate that second-generation immigrant males who were exposed to kindergarten during childhood chose to have smaller families in adulthood.

Overall, these results indicate 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 childhood, also decided to have fewer children later in life.

7 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, toward the end of the nineteenth century, thousands of kindergartens opened across various U.S. cities within a span of less than thirty years. Most of these kindergartens targeted immigrant families, one of the most disadvantaged groups in U.S. society at the time.

Our empirical analysis revealed that kindergarten exposure led to a fertility decline in U.S. cities. 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 U.S. 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 ages five to six also had fewer children later in life. We interpret this as evidence that the kindergarten movement in the U.S. 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 the English proficiency of immigrant children also indicates that kindergartens can contribute to the assimilation of immigrant families.

Table 8 Fertility and family size of exposed children as adults

	(1)	(2)	(3)	(4)	(5)	(6)
	Children below Age 5			*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 (columns 1-3) and family size (columns 4-6) for the linked samples of males in 1900-20 (Panel a) and 1910-30 (Panel b). $Kindergarten\ Exposure_{bct}$, is city *c*'s kindergarten capacity normalized by the number of children age five to six at the time when a child of cohort *b* was of age five to six. Kindergarten exposure is also interacted with a dummy variable indicating whether the mother was foreign-born in columns 2, 3, 5, and 6. This table includes the same set of individual and parental controls as Tables 5 and 6. 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

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10887-026-09271-z.

Acknowledgements We thank the Editor and three anonymous referees for their constructive comments. We thank also Dan Aaronson, Doug Almond, Sonja Bhalotra, Hoyt Bleakley, Leah Boustan, Davide Cantoni, Liz Cascio, Karen Clay, Carl-Johan Dalgaard, Matthias Doepke, Kathrine Eriksson, James Feigenbaum, James Fenske, Joe Ferrie, Oded Galor, Paola Giuliano, Claudia Goldin, Walker Hanlon, Casper Worm Hansen, Lauren Hoehn-Velasco, Dan Hungerman, Markus Lampe, Lars Lønstrup, Bob Margo, Bhash Mazumder, Kris Mitchener, Petra Moser, Nathan Nunn, Sebastian Oettinger, Alex Roehrkasse, David Weil, Ludger Woessmann, and seminar and workshop participants at Aarhus, Bocconi, Belfast, NHH Bergen, Brown, CERGE-EI, Dartmouth, Fed Chicago, Harvard, Ifo Institute Munich, Konstanz, Lund, Mannheim, NTNU Trondheim, NYU Stern, SFI Copenhagen, University of Bonn, UC Davis, University of Copenhagen, University of Southern Denmark, Warwick, WU Vienna, the 2016 NBER Summer Institute (DAE) in Boston, the ASREC 2017 conference in Boston, the 2018 WEHC in Boston, the 2019 SSHA conference in Chicago, the 2019 RES conference in Warwick, the 2019 SAEe conference in Santander, and the 2020 AEA annual meeting in San Diego for useful comments and suggestions. Frederikke Frehr Kristensen, Maja Uhre Pedersen, Torben Skov Dyg Johansen, and Adele Heagney provided capable research assistance. Ager gratefully acknowledges financial support from the Danish Research Council grant 8019-00026B and the German Research Foundation (DFG) through CRC TR 224 (Project A02).

Author Contributions All authors contributed equally to each stage of the study. All authors reviewed and approved the final version.

Funding Open Access funding enabled and organized by Projekt DEAL. Philipp Ager gratefully acknowledges financial support from the Danish Research Council grant 8019-00026B and the German Research Foundation (DFG) through CRC TR 224 (Project A02).

Data Availability Census data have been retrieved from IPUMS. Kindergarten data have been retrieved from the U.S. Bureau of Education.

Declarations

Competing interests The authors declare no competing interests.

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