The Long-term Effects of Charity Nurseries: Evidence from Early 20th Century New York*

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Abstract

This paper examines the long-run effects of charity nurseries on disadvantaged children in early 20th-century New York City. Using a large sample of linked individual-level census data, we show that access to charity nurseries incorporating kindergarten instruction significantly increased educational attainment and reduced the likelihood of low-skilled employment in adulthood. These gains were particularly pronounced among children from non-English-speaking immigrant families. Our findings highlight the lasting benefits of early childhood education for upward mobility and immigrant assimilation, especially when childcare is paired with structured educational content.

Keywords: Charity nurseries, long-term outcomes, immigrant assimilation, early childhood education
JEL Codes: N31, J13, J24, I21

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1 Introduction

Several influential studies underscore the critical role of child care and early childhood education programs in fostering skill development and enhancing long-term outcomes for children from low-income families (Duncan and Magnuson, 2013; Cascio, 2021; Duncan et al., 2023). In the U.S., participation in these programs varies significantly by age, family income, and ethnicity.[1] Most early childhood programs in the U.S. remain targeted rather than universal, and policy debates continue over whether to expand publicly funded universal care, strengthen targeted interventions, or limit public involvement altogether.[2]

These debates echo longstanding concerns about the public role in the care and education of young children from low-income families—concerns that also shaped social reform efforts more than a century ago (Addams, 1990; Leff, 1973; Moehling, 2007; Ager and Cinnirella, forthcoming). We investigate the long-term effects of charity nurseries that emerged in U.S. cities in the late 19th century to provide center-based child care and kindergarten instruction for disadvantaged children. Rapid industrialization, mass immigration, and rising urban poverty strained the traditional role of mothers in low-income households as primary caregivers, as many were compelled to work. In the absence of government involvement in child care, families typically relied on informal arrangements with relatives or neighbors; otherwise, children were often left unsupervised. In response, charitable organizations established hundreds of nurseries in impoverished urban neighborhoods. Did these charity nurseries improve long-term outcomes for disadvantaged children, and were they cost-effective?

We show that charity-sponsored day nurseries significantly and cost-effectively improved the educational attainment and labor market outcomes of disadvantaged children. Our analysis draws on large, representative linked samples of historical U.S. Census records from the Census Tree database, which enable a long-term follow-up of charity nursery attendees into adulthood (Price et al., 2021; Buckles et al., 2023). Our historical framework and findings contribute to the

[1] Nearly 60 percent of children aged 3 to 5 in the U.S. are enrolled in preschool or kindergarten programs; see National Center for Education Statistics (2024).

[2] See, for example, the articles in the New York Times, The Atlantic, and The Economist.

literature evaluating the long-term effects of early childhood programs in the U.S. (Garces et al., 2002; Ludwig and Miller, 2007; Deming, 2009; Gray-Lobe et al., 2023). First, we evaluate the long-term impact of charity nurseries in a historical context when child care was unregulated and publicly provided care did not exist. Second, large-scale linked data enable us to identify the effects on participants by nativity—allowing for an evaluation of whether the charity nurseries contributed to immigrant assimilation by narrowing gaps in educational attainment and wages between immigrants and U.S.-born residents (Figure 1). Finally, few U.S. studies to date have used large-scale data to evaluate the long-term impacts of child care and early childhood education programs (Herbst, 2017; Bailey et al., 2021; Derrington et al., 2021).

We focus on New York City—the major immigration hub and a leading center of the historical charity organization movement in the United States. Our empirical analysis draws on a newly constructed database based on the rich information provided by the *New York Charities Directories* for 1883 to 1924. Figure 2 illustrates the main source of variation in our sample: the share of enumeration districts in the boroughs of Brooklyn and Manhattan with access to charity nurseries increased from approximately 5 percent in 1883 to around 45 percent by the early 1920s. Over this period, charitable and religious organizations in New York City sponsored over a hundred nurseries and kindergartens (Figure 3). We georeferenced the locations of the charity nurseries and kindergartens and combined them with Census Tree data for the years 1900/10/20–1940. The resulting sample includes approximately one million individuals born between 1883 and 1910 who spent their childhood in New York City.

We follow these individuals into adulthood, where we observe their completed education and labor market outcomes. Information on place and date of birth, residence, and race allows us to identify the targeted population. We leverage the age eligibility criteria of charity nurseries and their highly localized access, defined as a 350-meter radius around each institution. Because nurseries served only young children, we compare age-eligible children to those just above the eligibility cutoff within the same enumeration district—the smallest Census-defined administrative unit at that time. Children in enumeration districts without access to nurseries serve as additional

control units (see Figure 4). Enumeration district fixed effects are expected to absorb time-invariant unobservable neighborhood characteristics that can explain systematic differences between control and treated groups.

The key identifying assumption is that the relationship between a child's age at the time a nursery began operating and their later-life outcomes reflects the causal effect of nursery exposure. To assess this, we use an event-study design in which treatment status is assigned to individuals who were age-eligible and lived in an enumeration district within 350 meters of a nursery when it first opened (see Figure A1). We also show that our results are not simply driven by broader increases in school enrollment in treated districts. In addition, we conduct a series of robustness checks, including exploiting within-family variation, performing placebo tests, and using a matched sample that includes only treated and control units with similar neighborhood characteristics, all of which confirm our main findings and reinforce the credibility of our identification strategy. Furthermore, we show that treatment effect heterogeneity does not confound our results.

We find that access to charity nurseries improved later-life outcomes, *but only* when the nurseries provided kindergarten instruction. Exposed children attained 0.14 additional years of education and were 2.7 percentage points more likely to complete schooling beyond the compulsory level. However, we find no evidence of completing high school. In terms of labor market outcomes, participating children were 1.2 percentage points less likely to work as laborers in adulthood. We also find a 2 percent increase in wages, but only among those who earned above the median.[3] Additionally, we show that the costs of charity nurseries were substantially lower than those of more recent targeted early childhood programs in the U.S., such as the Perry Preschool Program (Heckman et al., 2010). Overall, our results suggest that charitable investments in targeted child care programs with an educational component promoted human capital accumulation and improved economic opportunities for disadvantaged children in a cost-effective manner.

Our results also reveal substantial heterogeneity in effects across racial and immigrant groups. The primary goal of the charity nurseries was to provide daycare and early childhood education to

[3]These figures represent intention-to-treat (ITT) effects. We refer to Section 5.3. for the corresponding average treatment effect on the treated (ATT).

children from disadvantaged families, many of whom had immigrant backgrounds (Davis, 1984). We find that children of immigrants from non-English-speaking countries—particularly those from Southern and Eastern Europe—experienced the largest long-term gains from attending charity nurseries. We show that acquiring English language skills played a key role in mediating the impact of charity nurseries on immigrant children's educational attainment and occupational choices. Overall, our estimates suggest that charity nurseries with kindergarten instruction reduced the gaps in educational attainment and wages between first- and second-generation immigrants and U.S.-born natives in New York City.

Moreover, we exploit the fact that some charities provided child care and education services exclusively for Black children. Blacks were among the poorest and most discriminated groups in the early 20th century, and racial gaps in income and education were substantial (Margo, 2016). Blacks in our sample generally had fewer years of education and worse labor market outcomes. However, access to charities for Black children significantly contributed to closing the racial gap. Black children who had access to charities remained in school for approximately 0.8 additional years and were about 10 percent less likely to be employed in laborer occupations in adulthood. These findings align with existing research that highlights the long-term benefits of targeted early childhood education programs for Black children (e.g., Elango et al., 2015; García et al., 2023).[4]

Related Literature

Our paper contributes to a large body of literature evaluating the long-term effects of targeted early childhood education programs in the U.S., such as Head Start, the Perry Preschool Program, and the Abecedarian Project (e.g., Duncan and Magnuson, 2013; Cascio, 2021; Duncan et al., 2023).[5] Existing evidence on the long-term benefits of these targeted programs relies either on small-sample experimental settings (Perry, Abecedarian) or, in the case of Head Start—with the

[4]Our results also relate more broadly to studies that find positive long-term effects of education programs for Black children in the US South after the Civil War (the Freedmen's schools) and the Rosenwald Rural Schools Initiative at the beginning of the 20th century (Aaronson and Mazumder, 2011; Eriksson, 2020a; Jones and Schmick, 2022).

[5]The Perry Preschool and Abecedarian Projects are well-known small-scale "model" programs, each serving fewer than 130 children, whereas Head Start is a federal initiative that has served more than 38 million children since 1965.

notable exception of Bailey et al. (2021)—relatively small survey samples (e.g., Johnson and Jackson, 2019; De Haan and Leuven, 2020; Pages et al., 2020).

In contrast, we use linked Census data to evaluate the long-term impact of charity-funded nurseries on disadvantaged children in New York City. In terms of scale, our setting lies between small experimental programs and large federal initiatives such as Head Start, offering sufficient statistical power to detect meaningful long-term effects across heterogeneous population groups. We find that while mere access to childcare had limited long-term effects, nurseries that incorporated formal kindergarten instruction significantly improved disadvantaged children's long-term outcomes. These findings highlight the importance of early educational content, consistent with evidence from Head Start and the Perry and Abecedarian programs, while echoing results showing that childcare without structured learning components tends to have weaker developmental impacts on younger children (Duncan et al., 2023).[6]

Our historical setting further contributes to this literature by examining a period in which childcare provision was unregulated and publicly provided care did not yet exist. Our findings on the importance of kindergarten instruction in charity nurseries resonate with Ager and Cinnirella (forthcoming), who find that access to kindergarten education in American cities in the late 19th and early 20th centuries promoted school attendance and English proficiency among immigrant children aged 10–15.[7] Outside the United States, Rossin-Slater and Wüst (2020) find that a Danish public preschool program targeting poor children in the mid-20th century had positive long-term effects on adult educational attainment and earnings; however, they also show that the program's value-added diminished when children were exposed to a nurse home-visiting program in infancy.[8] Moreover, our setting allows for an important contrast with studies of the Lanham Act—a federally

[6]There is a large body of work, beyond the focus of this article, evaluating the impact of universal early childhood education programs mostly outside the US, which generally finds positive effects on child development and long-term outcome. For younger children, the evidence on universal child care comes from outside the US and is more mixed. We refer the readers to Duncan et al. (2023) for a recent overview.

[7]Cascio (2009) shows that state subsidies for universal kindergarten programs in the South during the 1960s and 1970s reduced institutionalization and high school dropout rates among affected southern-born white children, but had no comparable effects for southern-born Black children.

[8]Several researchers have highlighted the beneficial long-term effects of access to home visits and child health care centers in Scandinavian countries that were rolled out during the 1930s (Bütikofer et al., 2019; Bhalotra et al., 2017; Hjort et al., 2017).

supported universal childcare program during WWII. While Herbst (2017) and Derrington et al. (2021) find positive long-term effects of the Lanham Act on educational and labor market outcomes, our study focuses specifically on the efficacy of targeted charity-based interventions for the most disadvantaged children in the late 19th and early 20th centuries.

Finally, we relate to the historical literature on immigrant assimilation (Abramitzky et al., 2014, 2020a; Lleras-Muney and Shertzer, 2015; Alexander and Ward, 2018; Collins and Zimran, 2023). It is well documented that newly arriving immigrants often settled in existing immigrant neighborhoods (Bartel, 1989; Ager and Brückner, 2013; Abramitzky and Boustan, 2017; Tabellini, 2020). These immigrant enclaves provided important support networks—offering access to jobs, housing, and community services—but may have also impeded immigrants' assimilation into society (Wegge, 1998; Eriksson, 2020b; Abramitzky et al., 2025; Gagliarducci and Tabellini, 2025). For example, Abramitzky et al. (2024) show that leaving immigrant enclaves in New York City at the beginning of the 20th century was positively associated with socioeconomic outcomes, and that these gains persisted into the next generation.

In contrast to the emphasis on residential mobility, we find that educational programs in charity nurseries helped children achieve better economic outcomes later in life even if they remained within these neighborhoods. In particular, English instruction in the nurseries appears to have played a key role in the assimilation of immigrant children, consistent with both historical and contemporary studies that highlight language acquisition as a central driver of immigrant integration into the labor market (Bleakley and Chin, 2004, 2010; Chiswick and Miller, 2015; Abramitzky et al., 2023; Ward, 2020). Our findings therefore suggest that improving local educational opportunities can serve as a powerful alternative pathway to successful assimilation, even in the absence of geographic mobility.

2 Historical Background

In the latter half of the 19th century, child care and kindergarten institutions emerged in U.S. cities as a response to the complex social challenges associated with urbanization, immigration, and widespread poverty. Charitable organizations played a key role in this development, introducing a range of programs aimed primarily at supporting destitute mothers and low-income immigrant families. Providing child care and early education was a central strategy in their broader efforts to combat poverty and address the lack of social infrastructure in urban areas. Between 1880 and 1915, approximately 700 day nurseries and over 500 free kindergarten associations were established (Tank, 1980; Davis, 1984; Durst, 2005; Ager and Cinnirella, forthcoming).

New York City, a major hub of immigration during this period, emerged as one of the leading centers of the charity organization movement in the United States. Between 1900 and 1940, the city's population more than doubled from 3.4 million to nearly 7.5 million residents. At the height of the era of mass migration before WWI, 40 percent of New York City's residents were foreign-born, accounting for nearly 15 percent of the total foreign-born population in the United States. A disproportionate share of Eastern and Southern European immigrants—who were, on average, poorer, less educated, and often non-English-speaking—lived in New York's immigrant neighborhoods, frequently under poor housing and living conditions (Riis, 1890; Abramitzky et al., 2024; Ager et al., 2024).[9] Charitable organizations provided critical support in these poverty-stricken neighborhoods by establishing center-based child care services and providing kindergarten instruction (Tank, 1980; Durst, 2005).

The primary purpose of the charity nurseries was to prevent the institutionalization of children and to support working mothers. These institutions focused on ensuring the health and safety of children, providing food and clothing, and educating mothers about child-rearing, hygiene, and home economics. Most of them operated daily from 7:00 a.m. to 7:00 p.m. While the size of charity nurseries varied, it was not uncommon to care for more than 50 children between the ages

[9]The painting, made by American painter George Bellows in 1913, depicts the miserable living conditions of the new arrivals, including Italian, Jewish, Irish, and other ethnic groups residing in tenement houses on the Lower East Side (see Appendix Figure B1).

of 0–6 years. Some charities charged a nominal fee—typically five cents per day (equivalent to the price of a loaf of bread)—from families who could afford to pay.[10] In addition to child care, some nurseries offered employment services for mothers in need of work. In New York City in 1914, less than one-quarter of the families served by charity nurseries were headed by widows. Close to 20 percent were headed by women who had been deserted, 45 percent were two-parent households in which both parents worked, and the remaining cases involved women whose husbands were sick, drunkards, or otherwise "unable" to work (Durst, 2005).

Several charity nurseries started to introduce kindergarten instruction since the charity workers believed that kindergarten education has a positive influence on the cognitive and social development of the child (Tank, 1980). This combined treatment spread throughout New York City in the early 20th century, but charity-sponsored kindergartens also operated as independent institutions. The target group was mainly young immigrant children (ages 3-6) from the poorest neighborhoods. Classroom activities included learning American cultural customs and teaching children English language skills, morals, and values to prevent delinquency. Charity workers also reached out to the children's homes with the goal of "Americanizing" and educating mothers about child-rearing practices and hygiene (Klein, 1992; Durst, 2005; Berg, 2004). The assimilation function of charity-sponsored kindergartens and nurseries was an important activity in New York and other large U.S. cities that experienced a massive inflow of immigrants from Eastern and Southern Europe, who often arrived without financial means, had on average lower proficiency in English, and lower socioeconomic status. Overall, charitable organizations familiarized the public with the general principles of child care and preschool education and gained widespread recognition as child-saving institutions (Tank, 1980; Klein, 1992; Durst, 2005).

While charity nurseries offered a service that allowed women to work and raise children simultaneously, the general public did not view women's incorporation into the labor force as a desirable goal. The concern was that by assisting poor women, the nurseries would undermine the values of a traditional family model. The introduction of state-specific mothers' pension laws—a

[10]This information is taken from the day nurseries listed in the NYC charities directory in 1902.

government-sponsored welfare program for poor families with dependent children—between 1911 and 1931 reflected the prevailing view that dependent mothers should be the primary caretakers of their children and were not expected to be the main breadwinners (Leff, 1973; Moehling, 2007; Aizer et al., 2016).[11] Despite widespread opposition to center-based care, approximately 500 charity-sponsored day nurseries still operated in the U.S. by the early 1920s (Tank, 1980).[12]

In parallel to these developments, improving child hygiene became an important objective of public health departments in the early twentieth century. At the municipal level, New York City was the first to establish a separate Division of Child Hygiene in 1908. In 1912, the federal government created the United States Children's Bureau, a national agency dedicated to the well-being of children and their mothers. During the 1910s and early 1920s, New York and other states followed by establishing statewide divisions of child hygiene. Federal support through the Sheppard-Towner Act in the 1920s enabled states to fund public health clinics, home visiting programs, and maternal education initiatives, contributing to improvements in maternal and child health (Duffy, 1974; Moehling and Thomasson, 2012, 2014).[13] However, despite efforts by public health and welfare departments to regulate child care standards in the 1920s and 1930s, there was no systematic government sponsorship of child care facilities prior to the 1930s. This changed only with the Works Progress Administration, which sponsored emergency nursery schools, and later with the Lanham Act, which funded a large-scale system of child care centers in the 1940s (Tank, 1980; Cahan, 1989; Herbst, 2017).

[11] Heavily opposed by charity organizations, New York introduced a mother's pension program in 1915.

[12] Global trends, measured by the relative frequency of the bigram "Day Nursery" in a corpus of English historical books, indicate a rapid increase in the popularity of nurseries after 1890, peaking around 1920 (Appendix Figure A2).

[13] The Sheppard–Towner Act (1921–1929) was a short-lived program that provided federal matching grants to states for maternal and infant health services (Lemons, 1969).

3 Data

3.1 Main variables

Our empirical analysis combines individual-level U.S. Census data with geo-referenced information on the locations of charity-sponsored day nurseries and kindergartens in Manhattan and Brooklyn, which we digitized from New York City charity directories.[14]

Individual-level data: Our analysis draws on complete-count U.S. Census records from IPUMS (Ruggles et al., 2021). The records provide information such as place of birth, current residence, race, occupation, wages (reported only in 1940), literacy, and educational attainment (also reported only in 1940). We link individuals across censuses using crosswalks provided by the Census Tree Project (CTP). The quality of the CTP links is high and has been independently verified (Buckles et al., 2023; Price et al., 2021). As a robustness check, we also apply the links provided by other existing publicly available crosswalks, specifically, the Census Linking Project (Abramitzky et al., 2020b) and the links identified by FamilySearch users.[15]

For each linked individual in our sample, we use the information on place of residence (i.e., the enumeration district of the individual in Manhattan and Brooklyn), race, age, birthplace, school attendance, and parental and household characteristics from the censuses of 1900, 1910, and 1920. Information on educational and labor market outcomes is retrieved from the 1940 Census. We keep only unique matches in our sample based on the earliest available link.[16] The sample is restricted to individuals born between 1883 and 1910 who had completed their education by 1940. We exclude foreign-born individuals who arrived too late in the US to be eligible for childcare and preschool education. The resulting linked sample is used in our empirical analysis to assess the long-term effects of having access to day nurseries during childhood.

[14] In 1910, the population of the boroughs of Manhattan and Brooklyn was nearly four million–more than 80 percent of the city's total population (Source: Population of New York City).

[15] The CTP provides researchers with fully anonymous crosswalks between each pair of historical U.S. censuses (1850-1940). These crosswalks can be merged by HISTID, a consistent individual-level identifier provided by IPUMS that uniquely identifies individuals within a single census year, though not across censuses. The CTP linking method yields a high match rate of individuals and includes systematic links for women over time. We refer the readers to Abramitzky et al. (2021) for further information on methods used to link historical U.S. Census data.

[16] For example, if an individual can be matched to 1940 from 1900 and 1910, we keep only the 1900-1940 match.

Data on charity nurseries and kindergartens: We digitized information on charity-sponsored day nurseries and kindergartens in the boroughs of Manhattan and Brooklyn from the charity directories of the Charity Organization Society of the City of New York. These directories list the addresses (exact street and house number), which we geo-referenced, along with information on the target group (e.g., infants and young children of poor working mothers), the age range of admitted children, the type of service provided (i.e., whether the organization operated only a day nursery, or offered both daycare and kindergarten instruction), and whether the service was tailored to a specific groups (e.g., African Americans).[17] We collected this information for the years 1883 (the first published volume) to 1924, although volumes are missing for the years 1884, 1886, 1889, 1891, 1893-94, 1904, and 1908. For the missing years, we assume that a nursery remained active if it is observed in the volumes both before and after the missing year.

We can distinguish between three different types of charity organizations:

(i) Day nurseries: organizations admitting children, typically aged 0-6, that provide only daycare for children without kindergarten instruction.

(ii) Day nurseries with kindergarten instruction: organizations admitting children aged 0-6, and providing a combination of daycare for younger children (below age 3) and kindergarten instruction for children aged 3-6.[18]

(iii) Kindergartens: organizations admitting children, typically aged 3-6, that provide kindergarten instruction.

Appendix Figure B2 shows examples of all three types of institutions listed in the directories. We focus in our empirical analysis on the first two types of institutions. This allows us to estimate the daycare effect and the joint effect of daycare and kindergarten instruction. Importantly, we always control for (iii) when evaluating the effects of (i) and (ii) on educational attainment and adult labor market outcomes of exposed children. Appendix Table A1 reports summary statistics.

[17] Some charities provided services specifically targeted to Black communities, such as the "Free Kindergarten Association for Colored Children" or the "Hope Day Nursery for Colored Children".

[18] We do not have information whether younger and older children spent time together and participated in joint activities.

Treatment assignment: To construct our treatment measure, we geo-reference all addresses of charity-sponsored nurseries and kindergartens listed in the directories between 1883 to 1924. Historical addresses are identified with Google API and matched to their corresponding enumeration district. On average, an enumeration district in our sample had a population of nearly 2,000 residents, of whom approximately 12.5% were children aged 0–6.[19] This matching procedure assumes that the historical addresses of charity organizations align with their modern counterparts as geolocated by our automated algorithm. We then combine this information with the shapefiles of enumeration district boundaries in Manhattan and Brooklyn for 1900, 1910, and 1920 census years.[20] Figure 3 displays the locations of charity nurseries and kindergartens in 1910 across Manhattan and Brooklyn.

Based on the geo-references locations, we calculate the distances from each district centroid to the nearest charity nursery in every year from 1883 to 1924. Since we only observe an individual as a child either in the Census of 1900, 1910, or 1920 (duplicates are excluded), we only need to rely on the enumeration district and its proximity to the nearest nursery in that given census year. Hence, for the assignment of treatment, we do not require enumeration districts to have stable boundaries over time.

Figure 4 presents two example enumeration districts to illustrate how proximity to the nearest charity institution can change over time. In 1898, the distance fell below 350 meters—the threshold we use to define treatment exposure.[21] Accordingly, we assign 1898 as the treatment year for this district. Individuals residing in the district are considered treated if they are age-eligible. For instance, those aged 3 or younger in 1898 would be exposed to the full education treatment in day nurseries with kindergarten instruction, while older cohorts would be classified as untreated. Appendix Figure A1 illustrates our treatment assignment rule.

[19] There was a significant variation in the size of enumeration districts. For instance, in one of the most densely populated areas of East Side Manhattan in 1910, enumeration districts populations ranged from 739 to 3,012 residents.

[20] We verified the addresses geolocated by Google with the crosswalks between historical and modern NYC street names provided by Steve Morse. The shapefiles containing the enumeration district boundaries for New York City are from the Urban Transition Historical (UTH) GIS Project; see category "Northern Cities, 1900-1930" for the corresponding enumeration districts for New York City.

[21] Appendix Figure B3 provides a visualization of the distance threshold on a modern map of Manhattan.

3.2 Validation of treatment assignment

While we do not have enrollment data for charity nurseries, we can validate our measure of treatment exposure by testing whether the establishment of a nursery with kindergarten instruction affects school attendance at specific ages. During our sample period, compulsory schooling in New York City did not begin before age 7, and children under six were not generally admitted to school except in kindergarten classes (Palmer, 1905; Stambler, 1968). Using our baseline event-study framework, described in detail below, we find that the establishment of such a nursery increases attendance rates among the target group of children aged 3 to 6 (Figure 5).[22]

However, one potential concern is that our estimates may reflect broader trends in school enrollment within treated enumeration districts. Reassuringly, we do not observe any meaningful impacts on attendance among children of compulsory school age (7-12). Table 1 supports this finding: attendance among children aged 3 to 6 increased when they had access to charity nurseries offering kindergarten instruction, but not when nurseries provided only daycare services (columns 1–3). These effects hold regardless of nativity status. Consistent with the event-study results in Figure 5, access to charity institutions is not associated with higher attendance among children aged 7 to 12 (column 4), suggesting that the estimates do not simply capture improved access to elementary or middle schools in treated enumeration districts.

To further assess the localized impact of charity nurseries, we regress a school attendance indicator on age dummies interacted with the distance to the nearest nursery offering kindergarten instruction. If access to these institutions is indeed local, we would expect distance to influence attendance only within our treatment threshold of 350 meters, and not beyond. Consistent with this expectation, Appendix Figure A3 shows that distance predicts school attendance exclusively for age-eligible children residing within 350 meters of a nursery. For those living beyond this range, the estimated coefficients are statistically insignificant and close to zero across most age groups.

[22] Instructions regarding school attendance varied across censuses. In 1910 and 1920, the question formally applied to individuals aged 5 to 21; however, enumerators were instructed to record "yes" for individuals outside this range if they were in fact attending school (see IPUMS documentation for the variable "SCHOOL"). For instance, in 1910, approximately 1% of three-year-olds were reported as attending school, compared to 4% of four-year-olds, 36% of five-year-olds, and 75% of six-year-olds.

This pattern suggests that treatment exposure affects only children in immediate proximity to a charity nursery. These findings align with Ager and Cinnirella (forthcoming), who document that the effects of kindergarten exposure were similarly confined to households located near kindergartens.

4 Empirical strategy

This section describes our identification strategy to estimate the long-term effects of children obtaining access to charity nurseries. The outcomes of interest include educational attainment and labor market outcomes as adults. We exploit variation in access to charity nurseries at the enumeration district level across different birth cohorts. Treatment is assigned to eligible individuals residing in an enumeration district within 350 meters of any active charity nursery. In our analysis, we distinguish between charity nurseries with and without kindergarten instruction.

We run the following event-study-model using data from individuals born between 1883 and 1910 as outlined by estimating equation (1):

$$y_{idt} = \alpha_d + \alpha_t + \sum_{k}^{T} \beta_k \mathbb{1}[t - E_{idt} = k] + X'_{idt}\Gamma + \mu_w \times t + \epsilon_{idt}, \tag{1}$$

where y_{idt} is the outcome variable for individual i who belongs to birth cohort t and resides in enumeration district d. The variable E_{idt} captures the year when the enumeration district, in which individual i resides, first gained access to a charity nursery. The variable of interest, $[t - E_{idt} = k]$, is an indicator of an individual's birth year being k years away from treatment. Our reference category (omitted) refers to individuals who were age 8 at the time of treatment.[23]

We further include enumeration district fixed effects (α_d) and birth cohort fixed effects (α_t) to control for time-invariant neighborhood characteristics and cohort-specific factors, respectively. The estimating equation also includes a vector of individual controls X'_{idt}, which accounts for

[23] Our event-study graphs display estimates for relative ages $-10 <= k <= 16$. We choose eight-year-old children as a reference group to account for potential fuzziness in age eligibility for treatment. Parents could misreport the age of their children to pass eligibility criteria. Even though our "first-stage" results in Figure 5 and Table 1 do not support it, seven-year-old children could be still affected by treatment.

individual's birthplace, race, family size, gender, years spent in the U.S. (only for foreign-born), and parental characteristics (including indicators of each parent's birthplace and literacy).[24] We include individual-level controls to account for the potential changes in the demographic composition of the population over time and also demand for education (parental literacy). Additionally, we include ward-specific linear trends to capture differential trends across wards, which were historically administrative and electoral divisions in New York City. Specifically, the specification allows each ward w to have its own linear time trend by interacting a ward indicator with a continuous measure of time $\mu_w \times t$. This absorbs gradual ward-level changes—such as population growth, demographic transformation, housing development, or local economic change—that may affect educational outcomes and evolve at different rates across wards. Standard errors are clustered at the ward level. In the robustness section, we test the sensitivity of our results to specific controls such as ward-specific linear trends.

To estimate equation (1), we combine data from three linked samples: 1900/10/20-1940. Accordingly, we interact fixed effects for enumeration districts, birthplace, and birth year with census-year indicators to account for differences between individuals linked from different census years. Since the openings of the charity nurseries and kindergartens are likely to be correlated, we *always* control for individuals having potential access to different types of treatment throughout our regression analysis. In other words, when estimating the effect of access to charity nurseries with kindergarten instruction, we control for access to both charity nurseries without kindergarten instruction and charity kindergartens.[25]

The estimate of β, obtained from estimating equation (1), captures the impact on age-eligible children residing in enumeration districts with access to charity nurseries under the standard parallel trends assumption (i.e., in the absence of treatment, outcomes of children in treated and untreated districts would have followed similar trajectories). As an initial check to support our

[24] We consider a mother (identified by the variable MOMLOC in IPUMS) and all her children residing within one household as one family. If parents are absent, we substitute the missing values of parental characteristics with zeroes and include the corresponding indicators of the missing mother and father in the regression specification.

[25] Specifically, we add indicators for whether an age-eligible individual lived in an enumeration district with access to charity nurseries without kindergarten instruction or charity kindergartens.

identification strategy, we compare baseline characteristics of enumeration districts by treatment status. We focus on characteristics that were likely important factors for charities to open up a nursery, such as the share of foreign-born, the share of tenement dwellers, and the share of white-collar occupations, the latter serving as a proxy for neighborhood income levels.

The balancing test in Appendix Figure A4 reports the mean difference of these characteristics between treated and control districts. On average, treated enumeration districts had a larger immigrant population, lower-income residents, and a higher prevalence of rental housing. This pattern is consistent with the mission of charity nurseries, which aimed to serve poverty-stricken neighborhoods. Our econometric specification includes a rich set of individual-level controls and enumeration district fixed effects to account for time-invariant neighborhood differences between treatment and control areas. Additionally, our event-study design enables us to test for potential violations of the parallel trends assumption. To further add credibility to our identification strategy, we present several placebo tests and a series of robustness checks in Section 5.2.

5 Empirical results

This section presents the results of our empirical analysis. The main findings are summarized in Section 5.1. Based on estimating equation (1), we find that access to a charity nursery offering kindergarten instruction significantly increased educational attainment and improved labor market outcomes in adulthood for affected children. These effects are particularly pronounced among non-English-speaking immigrants and African Americans—who constituted some of the poorest populations in New York City at the time. In contrast, we generally detect no significant effects from access to charity nurseries that provided only daycare services. Section 5.2 validates these findings by estimating models with an extended set of controls (e.g., family fixed effects) and conducting placebo tests. We also apply the alternative estimator proposed by Sun and Abraham (2021) to account for potential treatment effect heterogeneity. Furthermore, we use a matched sample that restricts the analysis to treated and control units with similar neighborhood characteristics,

such as the proportion of foreign-born residents and housing conditions. Section 5.3 assesses the magnitudes of our estimates and relates them to existing studies. Finally, Section 5.4 discusses potential mechanisms driving the observed effects.

5.1 Main Findings

We begin our analysis by evaluating the impact of children's exposure to charity nurseries on educational attainment, using estimation equation (1). In what follows, we distinguish between two types of exposure: charity nurseries that offered kindergarten instruction and those that provided only daycare services.

Figure 6 displays the treatment coefficients and the corresponding 95-percent confidence intervals for various education outcomes. These coefficients capture the impact of access to charity nurseries with kindergarten instruction (left column) and without kindergarten instruction (right column) for different relative age cohorts (e.g., a child born two years after the first opening is denoted with age "-2" in the event-study graph). Eight-year-old children represent the reference group (omitted category). When a nursery opened within 350 meters of the enumeration district, we would expect effects on educational attainment among children age-eligible for exposure to kindergarten instruction.[26] In contrast, no effects are expected for children aged 8 and older at the time of the opening, as they would have been too old to attend. As mentioned in Section 4, we always control for individuals having potential access to different types of treatment.

For children exposed to charity nurseries with kindergarten instruction, we find a positive effect on years of schooling (Panel a) and the likelihood of completing education beyond the compulsory level (Panel c).[27] These effects are concentrated among children who were aged four or younger at the time of the nursery's opening. Reassuringly, there is no indication of pre-existing trends among older cohorts (age 8 and above), which alleviates concerns that parents with more favorable views on education systematically moved to areas where nurseries and kindergartens

[26] For simplicity, we treat all children within 350 meters who are age-eligible as exposed.

[27] The 1903 Law in New York required all children aged 8 to 14 to stay in school. The 1909 Law extended the compulsory schooling age to seven years old (Stambler, 1968).

had recently opened.[28] Moreover, we find no evidence that access to kindergarten instruction affected the educational trajectories of girls differently from those of boys (see Appendix Figure A5 and Table A3). Nevertheless, we always control for gender using a dummy variable in our regression analysis.

For children exposed to charity nurseries without kindergarten instruction, we do not find a significant effect on years of schooling (Panel b), though there is some evidence of a positive impact on completing education beyond the compulsory level (Panel d). Furthermore, we find no significant effect of either type of nursery on completing high school (Panels e and f of Figure 6). This likely reflects the limited capacity and high opportunity costs associated with attending high school for poor families in Manhattan and Brooklyn in the early 20th century (Palmer, 1905; Stambler, 1968).

Panels (a)-(f) of Figure 7 present the results for various adult labor market outcomes. Children exposed to charity nurseries with kindergarten instruction (left column) earned higher wages in adulthood, but only if their annual earnings exceeded the median wage level in 1940 (USD 1,300); see Panels (a) and (c).[29] Access to charity nurseries with kindergarten instruction is also associated with a reduced likelihood of working as laborer (Panel e).[30] In contrast, there is no robust relationship between access to day nurseries without kindergarten instruction (right column) and adult labor market outcomes; see Panels (b), (d), and (f).

The heterogeneous effect on wages may have several explanations. First, it may reflect limited wage variation resulting from minimum wage legislation and labor union agreements that

[28] To illustrate the issue, consider two households observed in the 1900 census, each with a just-born child living in different enumeration districts, ED_1 and ED_2. Suppose that ED_1 gains access to a nursery in 1901, while ED_2 does so only in 1908. This means that the child from ED_1 receives treatment, whereas the child from ED_2 does not. If parents value preschool education, they might choose to move from ED_2 to ED_1 to access the nursery earlier. However, due to the nature of the data, we observe each household only once at the time of the census. Because enumeration district boundaries change between census years, we cannot track population movements by linking households across censuses. Moreover, direct indicators of residential mobility were not reported in U.S. censuses until 1940. Consequently, we cannot distinguish movers from stayers in our analysis. But, if such selective migration occurred—where parents strategically relocated to areas with earlier nursery openings—it would bias our estimates downward, since older cohorts could move into treated areas before treatment occurred. In Section 5.2, we provide further evidence that our results are unlikely be driven by selection of parents into treatment districts.

[29] Appendix Figure A6 shows the effect without splitting the sample between high and low wage earners

[30] Laborers are classified by the IPUMS occupation classification OCC1950 = 970.

establish lower-bound thresholds for earnings. Figure A7 supports this interpretation by showing a floor–ceiling pattern in wages among individuals with education at the compulsory level. An alternative explanation is that treatment exposure enhances cognitive and language skills that are not fully captured by formal educational attainment. Another possible mechanism involves the complementarity between early childhood treatment and subsequent schooling. This implies that, for a given number of years of education, treated individuals may have acquired more effective knowledge or achieved higher academic performance, which we cannot directly observe in the data. Consequently, the treatment effect could materialize primarily in high-skilled occupations that reward stronger cognitive and communication abilities. We present some empirical evidence in favor of this in the section, discussing the mechanism.

Table 2 provides a summary of the "event-study" estimates. Column (2) reports the "intention-to-treat" (ITT) effects. The ITT effect for cohorts receiving access to charity nurseries with kindergarten instructions is 0.14 additional years of schooling, a 2.7 percentage point increase in the likelihood of completing education above compulsory level, a 2 percent increase in wages for those whose earnings exceeded the median wage level, and a 1.2 percentage point decrease in having an unskilled occupation. To obtain these estimates, we report the simple arithmetic average of the cohort-specific effects shown in the event-study figures.[31] Furthermore, we find no significant effects on employment status and number of weeks worked in the previous year. This suggests that the observed wage gains are likely driven by a selection of individuals to more productive occupations rather than by increased employment and working hours.[32] We discuss the average treatment effect on the treated (ATT), reported in columns (3)-(4), in Section 5.3.

Our findings suggest that access to charity nurseries offering kindergarten instruction increased educational attainment and improved long-term economic opportunities for children. As

[31] An alternative way to summarize the cohort-specific effects is to estimate a model with a single treatment indicator that distinguishes all treated cohorts from untreated ones. The two approaches are not equivalent. The treatment-indicator specification yields a weighted average of the cohort-specific effects, with weights roughly proportional to each cohort's relative size in the sample. This distinction is important in our setting because the event-study estimates indicate that the treatment effect increases over time and is larger for later-treated cohorts than for earlier-treated cohorts. Since the later-treated cohorts are relatively small in our sample, the estimate obtained from the treatment-indicator specification is smaller than the simple arithmetic average of the cohort-specific effects.

[32] Note, the wage regressions always control for the number of weeks worked previous year.

these nurseries primarily served disadvantaged children from immigrant households, we next examine whether immigrant children experienced the greatest gains from access to such charitable services. To do so, we estimate a standard difference-in-differences (DD) specification that examines outcomes by nativity, comparing U.S.-born natives (i.e., both the child and mother were born in the U.S.) to first- and second generation immigrants. This modified specification includes treatment indicators for access to charity nurseries (with and without kindergarten instruction) and charity kindergartens, along with the same set of individual and parental controls, enumeration and birth cohort fixed effects, and ward-specific linear trends as in estimating equation (1). These indicators equal one for age-eligible children. To capture potential heterogeneous treatment effects, we interact these treatment indicators with a dummy for being U.S.-born using immigrant children as the reference group.

Table 3 presents the results. Columns (1)–(5) report estimates for educational outcomes, while columns (6)–(7) present results for adult labor market outcomes. Among immigrant children with access to charity nurseries that included kindergarten instruction, the average years of schooling increased by 0.08 years (column 3), and the likelihood of attaining education beyond the compulsory level rose by 1.3 percentage points (column 4). However, there is no evidence of an increased likelihood of completing high school (column 5). In terms of labor market outcomes, access to these nurseries was associated with a 0.4 percentage point reduction in the likelihood of being employed as laborers and a 1.2 percent increase in wages among those earning above the median. In contrast, access to charity nurseries without kindergarten instruction did not lead to statistically significant improvements in either educational or adult labor market outcomes for immigrant children. "For children of U.S.-born mothers, we observe similar effects on labor market outcomes; however, the pattern for educational outcomes is less consistent. While access to charity nurseries offering kindergarten instruction increased their likelihood of pursuing education beyond the compulsory level, it did not lead to an overall increase in total educational attainment.

Table 4 explores whether the effects of having access to charity nurseries with kindergarten instructions differs between individuals who remained in Manhattan and Brooklyn and those who

moved elsewhere. To examine this, we interact the treatment indicator with a dummy variable indicating whether individuals observed in the 1940 Census had moved out of Brooklyn and Manhattan, and we split the sample by nativity—Panel A focuses on first- and second-generation immigrants, while Panel B reports estimates for native-born (third-or-higher generation). Moving out was generally associated with better socioeconomic outcomes. However, the positive effect found in Table 3 on educational attainment is driven by immigrant children who stayed in Brooklyn and Manhattan. This finding suggests that improving conditions within ethnic enclaves, such as expanding access to quality childcare and kindergarten instruction, can offer a viable alternative to resettlement programs in promoting the social and economic integration of immigrants.

Finally, we evaluate the effects of charitable institutions that specifically provided child care and kindergarten education in Black communities in New York City. At the turn of the 20th century, most Black residents lived in concentrated enclaves such as San Juan Hill—a historic neighborhood in Manhattan's Upper West Side—and Harlem. These neighborhoods were among the poorest in the city at the time. Because charities directories mention whether an institution exclusively served Black children, we are able to assess whether access to these services improved educational attainment and adult labor market outcomes for Black children.[33] Due to the limited number of organizations serving Black children, we grouped them into one broad category—Black charity institutions (all nurseries and kindergartens). The estimation equation includes the same set of controls as in Table 3.

Table 5 summarizes the results.[34] On average, Blacks individuals completed 1.5 fewer years of schooling and earned 55 percent less than their White counterparts. Despite these substantial racial disparities in education and income, access to race-specific charitable institutions appears to have had positive effects for Black children. Columns (1)-(3) report the impact on educational outcomes. The results are striking: Black children with access to a charity institution completed 0.8 additional years of schooling, they were 13 percentage points more likely to stay in school

[33]For example, the *Free Kindergarten Association for Colored Children* on 202 West 63rd Street offered free education for black children under six years of age.

[34]We classify Blacks based on the IPUMS *race* variable. The share of Blacks in our linked sample equals 2% and corresponds to 22,726 individuals.

beyond the compulsory level, and they were 7.5 percentage points more likely to complete high school (12th grade).

In terms of adult labor market outcomes, although we find no significant wage gains for Black children with access to a charity institution, they were 10 percent less likely to work as laborers (columns 4-5). Importantly, access to these race-specific charity institutions primarily affected the targeted Black children with no significant impact for non-Black children (except in column 4). This pattern suggests that our findings are unlikely to be driven by omitted variables that are spatially or temporally correlated with access to charitable services. If such confounding factors were present, we would expect to see statistically significant impacts also for non-Black children, which was not the case.

Overall, our results align with the observations of sociologists, who have documented social and economic upward mobility of immigrants in New York City (e.g., Glazer and Moynihan, 1970; Berrol, 1976). The efforts of charitable organizations helped poor immigrant children to achieve better labor market outcomes as adults. In contrast, for Black children, structural barriers—including labor market discrimination and rising residential segregation—constrained the extent to which gains in educational attainment translated into economic advancement (Lieberson, 1980; Sundstrom, 1994; Logan et al., 2015).

5.2 Robustness

In this subsection, we perform a series of robustness checks. In particular, we deal with potential threats to identification by (i) testing sensitivity of the results to alternative regression specifications (ii) estimating a family fixed effects model and restricting treated group only to "late adopters" to account for the selection of parents into treatment; (iii) implementing a robust estimator to address the issue of potential treatment heterogeneity when using a staggered adoption design; (iv) implementing placebo tests that substantiate the credibility of our estimation strategy; and (v) using a matched sample which includes only treatment and control districts that share similar characteristics.

We evaluate the stability of our findings across alternative regression specifications. We begin with a plain model that includes only enumeration district and cohort fixed effects. We then extend it by adding individual-level baseline characteristics, namely place of birth, number of years spent in the US, indicators of birth order, and family size. Next, we include indicators of parental literacy, followed by variables capturing the mother's employment status and whether the father held a white-collar occupation. We also compare specifications with and without ward-specific linear trends. Figure A8 shows that introducing baseline individual controls reduces the magnitude of the estimated coefficients. However, the overall pattern of the coefficients (e.g., no visible pre-trends) remains stable. The results also remain stable when we further include parental literacy, maternal employment, and paternal occupational status. The robustness of the estimates to controlling for parental human capital and social status suggests that our findings are unlikely to be driven by selection into treatment—that is, by parents with stronger preferences for education moving into areas where nurseries and kindergartens were established.

Appendix Table A2 presents the estimates from the family fixed effects model. This specification enables comparisons between siblings within the same family—specifically, those who could and could not attend charity nurseries—thereby controlling for all shared parental characteristics that might influence both selection into treatment and subsequent educational or labor market outcomes. While leveraging within-family variation improves the identification strategy, it requires restricting the sample to families with at least two children. Despite this limitation, the results broadly align with our earlier findings, showing a positive impact of charity nurseries with kindergarten instruction on educational attainment and occupational choice. Importantly, these results are not driven by the changes in sample composition—with one exception: we do not observe significant effects on wages. In that case, it is unclear whether it is due to the inclusion of family fixed effects or the reduction in sample size.

To further address potential selection concerns, we re-estimate our baseline specification on a restricted sample that includes only (i) individuals from districts that never received treatment ("never adopters") and (ii) individuals born in districts that adopted nurseries/kindergartens only

after their birth. For example, we exclude an individual if she was born in a district where a nursery/kindergarten had already been established *before* her birth year (e.g., born in 1900 in a district treated in 1899 or earlier). However, we keep an individual if treatment occurred in the year of birth or later. This strategy prevents the "retrospective" assignment of treatment and potential selection issue that could arise if parents with stronger educational preferences had strategically moved into already-treated districts. As shown in Figure A9, the results are consistent with the baseline estimates: we continue to observe positive effects for individuals who were age 0–4 when the first nursery opened, and no significant effects for older cohorts. This finding suggests that the estimated treatment effects are unlikely to be driven by selective migration of parents into areas with pre-existing nurseries.[35]

Next, we address concerns about using event study designs when units are treated at different times (see, e.g., Baker et al. (2022) for a detailed survey of the recent econometrics literature). To address the issue of potential heterogeneous treatment effects, we implement an alternative estimator proposed by Sun and Abraham (2021). For this robustness check, we aggregate outcomes to the enumeration district—the unit to which treatment is assigned.[36] The results based on this alternative estimator are consistent with our results using the standard event-study design with individual-level data (see Appendix Figure A10).

In Appendix Figure A11, we implement a placebo test by assigning treatment status to age-eligible children who lived outside the 350 meters threshold at the time the first charity nursery with kindergarten instruction opened. As shown by Appendix Figure A3, charity nurseries that are too far away from the enumeration districts of age-eligible children do not affect their attendance. Therefore, we expect the placebo treatment to yield no significant effects on educational attainment and occupational choice. Reassuringly, the estimated coefficients from this placebo event study

[35]The alternative way to address selection concerns would be to restrict the sample to individuals who did not change their place of residence ("non-movers"). However, two data limitations prevent us from implementing this approach. First, we only observe individuals at a single point in time in each census year (1900, 1910, 1920), rather than continuously. Second, enumeration district boundaries changed between census years, making it impossible to reliably link the same location across censuses. As a result, identifying non-movers is not feasible, and we therefore rely on the alternative strategy described above.

[36]We use units (enumeration districts) that never receive treatment as a control group. All computations are performed using the STATA command *eventstudyinteract*.

reveal no statistically significant pre- and post-treatment trends for either outcome. This supports the interpretation that the true treatment effects are localized around charity nurseries with kindergarten instruction. It also helps to rule out confounding influences from contemporaneous education or health interventions implemented by city authorities (e.g., at the ward-level) that may have coincided with the expansion of the child care and kindergarten services provided by charitable organizations.

Additionally, we randomly reshuffle the dates on which enumeration districts gained access to nurseries within each year. Note that this procedure preserves the annual count of districts with nursery access. For each set of falsely assigned treatment dates, we estimate our baseline event-study specification and record the average coefficient for the treated cohorts. We repeat this 1,000 times. Appendix Figure A12 shows that most of the placebo estimates cluster near zero and never exceed the effect estimated for the actual treatment. This suggests our results reflect actual variation in treatment timing.

Moreover, we address potential imbalances between treated and control units as shown in Appendix Figure A4. Since charity nurseries were intentionally located in poorer neighborhoods, observable characteristics—such as the share of foreign-born residents—are unevenly distributed across the two groups. To correct for this imbalance, we implement nearest-neighbor matching on the propensity score. Specifically, we first estimate a logit model of the treatment indicator based on the covariates shown in Appendix Figure A4, and then match each treated unit to the nearest control unit whose propensity score differs by no more than 0.1. As illustrated in Appendix Figure A4, this matching procedure largely eliminates the differences between the two groups. Finally, we re-estimate our event-study model on the matched sample. The results, presented in Appendix Figure A13, closely mirror our main findings.

Appendix Figure A14 shows results for all types of charity institutions. These estimates reveal a positive effect for institutions that provided kindergarten instruction. Notably, the estimate effects for charity kindergartens and nurseries with kindergarten instructions are very similar. This suggests the absence of dynamic complementarity between early childhood daycare (before

age 3) and subsequent educational treatment (ages 3–6).[37] In other words, the observed gains in educational attainment and occupational choice appear to be driven solely by the educational component of the intervention.

Finally, we demonstrate that our results hold when using alternative methods for linking individuals across historical US censuses. Specifically, we apply crosswalks provided by the Census Linking Project (CLP) and also restrict the sample to individuals identified by users of the FamilySearch platform. The latter should provide the highest-quality matches, as they were manually verified by relatives. Figure A15 presents our results. Importantly, we also verify that our conclusion—that the educational component of the treatment primarily affected first- and second-generation migrants—holds across different automated linking methods (Table A4).

5.3 Magnitudes and Discussion

It is important to interpret our findings within the broader context of educational trends in New York City. Panel (a) of Figure 1 shows that the initial educational gap—approximately 2.5 years of schooling—between U.S.-born Whites and first- and second-generation immigrants among cohorts born before 1889 had largely closed by 1910. This notable convergence suggests that New York City's education policies, along with the efforts of charitable institutions, were effective in narrowing the educational gap for a substantial portion of the city's population.[38] The estimates reported in Table 3 suggest that the charity nurseries' provision of kindergarten instruction to immigrant children contributed approximately to a 4% reduction in the educational gap between immigrants and natives in New York City.[39]

[37] Investments at different stages of a child's skill development process could have synergy effects, in the worst case they could be redundant (i.e., these investments are either dynamic complements or dynamic substitutes). See Cunha and Heckman (2007) and Bailey et al. (2020) for more details on the concept of dynamic complementary and dynamic substitutes. For empirical evidence on the existence of dynamic complementarities and dynamic substitutability, see, e.g., Johnson and Jackson (2019) and Rossin-Slater and Wüst (2020).

[38] It is also important to consider the changing composition of the immigrant population during this period. In the late 19th and early 20th centuries, New York City experienced a significant influx of less educated and poorer immigrants from Southern and Eastern Europe, which likely posed additional challenges for integration into the education system (Abramitzky and Boustan, 2017).

[39] This calculation is based on the ratio of the average treatment effect reported in Table 3 (ranges between 0.08 and 0.11) to the change in the educational gap between immigrants and natives, computed as (9.5-7) - (10.9-10.7).

A different pattern emerges when examining the evolution of the Black-White gap in schooling in Panel (a) of Appendix Figure A16. A substantial racial schooling gap existed for cohorts born in the 1880s. While convergence in years of schooling occurred among the cohorts in our sample—and appears to accelerate after the introduction of Black charitable institutions in 1902—the gap had not closed by 1910. The persistence of this disparity can likely be attributed to the rise of residential segregation and the entrenchment of racist institutions, which limited the speed and extent of convergence. Our estimates reported in Table 5 suggest that access to Black charity institutions reduced the racial gap in schooling. However, the few tailored charity institutions for Black children that we observe in New York City were far from sufficient to close the racial gap in schooling observed in Appendix Figure A16. Notably, Panel (b) of Appendix Figure A16 shows a considerable racial wage gap. The evolution of this wage gap over time does not mirror the pattern in educational attainment shown in Panel (a), suggesting that other factors—such as racial differences in the quality of schooling, labor market discrimination, or occupational segregation—may have contributed to the persistent wage penalty faced by Black workers.

Next, we calculate the average treatment effect on the treated (ATT). Since we do not observe whether treated individuals actually attend a charity nursery, our estimates represent intent-to-treat (ITT) effects. Table 2 and Appendix Table A2 show that access to charity nurseries with kindergarten instruction is associated with an increase of 0.11–0.14 years of schooling. These estimates are similar to Bailey et al. (2021) and Rossin-Slater and Wüst (2020), though somewhat larger than Havnes and Mogstad (2011), reflecting that the NYC charity organizations specifically targeted poor children—those most likely to benefit from access to kindergarten instruction.

To estimate the ATT, we use information from the *New York Charities Directories* to approximate the capacity of charity nurseries with kindergarten instruction, assuming 50 pupils per nursery. We take this number as a proxy for the number of children receiving kindergarten instruction in these institutions. We then calculate the average number of children aged 3–6 per enumeration district in Manhattan and Brooklyn using U.S. Census data from 1900, 1910, and 1920, which is an average of approximately 340 children per district. This allows us to construct a "first-stage"

estimate of access to charity kindergartens on enrollment rates: 50/340 = 0.15.

This estimate closely aligns with the share of children aged 3–6 reported as attending educational institutions in New York City according to the 1900 U.S. Census (0.161). It is also consistent with our own first-stage estimates (see Figure 5).[40] To obtain the ATT, we divide our ITT estimates by 0.15. Accordingly, our ITT estimates of 0.11-0.14 years of schooling translate into a 0.73-0.93 year increase in schooling for the average individual who attended a charity nursery with kindergarten instruction (see also Table 2).

These estimates lie between the effects of Head Start, which is associated with a 0.65-year increase in schooling (Bailey et al., 2021), and those of a targeted preschool program for low-income children in mid-20th-century Denmark, which led to roughly a one-year increase in highest grade attainment (Rossin-Slater and Wüst, 2020). Importantly, our estimated impacts are notably larger for the most disadvantaged groups (Black children). These results are comparable to programs explicitly targeting ethnic minorities. For instance, the Carolina Abecedarian Project showed substantial long-term benefits for poor, predominantly African American children, with gains of 1.8–2 additional years of schooling for treated individuals (García et al., 2017).

In terms of labor market outcomes, our estimates suggest that children who participated in charity nurseries with kindergarten instruction were 8 percentage points less likely to work as laborers in adulthood. For those earning above a median salary in 1940, program participants also experienced a 13 percent increase in wages (see Table 2).[41]

Finally, we provide an estimate of the cost-efficiency of the charity nurseries with kindergarten instruction. To do so, we take the daily cost per pupil at the Halsey Day Nursery in 1901, 1905, and 1910, which were 15, 20, and 25 cents, respectively.[42] Taking the average costs per pupil

[40]We do not rely on the census-based estimate as our primary first-stage proxy because it is unclear whether the IPUMS *school* variable fully captures attendance among age-eligible children in charity nurseries with kindergarten instruction. At the time, nurseries and kindergartens may not have been consistently classified as educational institutions, leading to potential underreporting by census enumerators.

[41]We obtain these estimates by converting the ITT effect reported in Figure 7 to ATT and then expressing it in percentages relative to the pre-treatment mean.

[42]We use data from the Halsey Day Nursery, which was established in 1897 and operated at 227 East 59th St, because it reported the operational costs rather than the fees paid by enrolled families. The nursery provided both daycare and kindergarten instruction to children under 6 years of age of working mothers. The average daily attendance was 45 pupils (Source: Charities Directory of the City of New York, various years).

(20 cents) and assuming that nurseries operated six days per week, we obtain an estimate of the yearly per pupil cost as 0.2*310 = $62 in 1905, or $2,355 in 2024$.[43] This estimate is comparable to the per-child cost of attending preschool in Denmark during 1949–1950 obtained by Rossin-Slater and Wüst (2020), which ranged between $2,230-$3,130 (in 2024$). In comparison to more recent targeted preschool interventions, such as the Perry Preschool Project and the Carolina Abecedarian Project, our cost estimates are substantially lower (Duncan et al. 2023). Taken together, our estimate suggests that the cost-efficiency of early 20th-century charity nurseries with kindergarten instruction in New York City may have exceeded that of many modern programs. As such, it can offer an appropriate benchmark for the evaluation of similar charity childcare initiatives in developing countries today.

However, several limitations should be noted. First, our estimate of the average treatment effect on the treated (ATT) is sensitive to the estimated "first-stage" effect of access to charity kindergartens on enrollment rates. Relatively small changes in this estimate can lead to substantial differences in the implied ATT. Second, because comprehensive data on nursery operating costs are unavailable, our calculations necessarily rely on evidence from a limited number of case studies. Given these limitations, the estimated ATT should be viewed as providing an order-of-magnitude assessment of the program's effects rather than a precise point estimate.

5.4 Mechanism

In this section, we explore potential mechanisms underlying the positive impact of access to charity nurseries with kindergarten instruction on educational attainment and adult labor market outcomes among immigrant children. We highlight the importance of gains in English language skills that children began acquiring in the nurseries and discuss how this aligns with the observed fertility decline among affected mothers. Finally, we argue that direct health improvements or changes in the mortality environment are unlikely to be a key driver of our findings.

[43] For a historical comparison, yearly expenditures for public kindergartens were $27 (in 1914) per pupil in Boston and $39.77 (in 1915) in Cambridge, Massachusetts (Lazerson, 1971). For converting the $62 to current US dollars, we use the purchasing power calculator from Measuring Worth. In assuming operational activity of nurseries as 310 days, we follow Costa 2000

Our motivation for exploring the English language skills channel stems from existing research showing that early childhood is a critical period for acquiring the host country's language (Bleakley and Chin, 2004, 2010; Isphording, 2015; Abramitzky et al., 2023). There was substantial variation in the ability to speak English across immigrant groups, reflecting, among other things, differences in linguistic distance, ethnic segregation, literacy levels, and the high cost of language acquisition. In particular, English illiteracy was relatively high among immigrant households from Eastern and Southern Europe at the turn of the 20th century. We therefore hypothesize that attending a charity nursery with kindergarten instruction helped young immigrant children acquire English more efficiently than their peers who were primarily exposed to their native language within the family and neighborhood.[44] Consequently, immigrant children exposed to charity nurseries with kindergarten instruction would have had an advantage upon entering elementary school.

To test this hypothesis, we construct a sample of first- and second-generation immigrant children aged 10–18 from the U.S. Censuses of 1900, 1910, and 1920.[45] We estimate an event-study model as outlined in Section 4. Panel (a) of Figure 8 shows that exposure to day nurseries with kindergarten instruction leads to substantial improvements in English proficiency among children from immigrant families. The results are striking, while we observe no significant differences in English illiteracy between treated and non-treated enumeration districts for children who were too old (above age 8) at the time of treatment, age-eligible children (i.e., those aged 6 or younger) were substantially less likely to report being unable to speak English.[46] Our ITT estimates indicate that individuals exposed to treatment at age 3 had, on average, an English illiteracy rate 2 percentage points lower than their older, untreated counterparts. Panel (b) of Figure 8 shows that there is *no effect* of exposure to charity nurseries without kindergarten instruction on English proficiency.

To further explore the importance of language acquisition, Table 6 shows that access to charity nurseries with kindergarten instruction had a positive effect on the likelihood of holding an occupation with a higher cognitive skills score (decomposed into reasoning, math, and language

[44] See Ager and Cinnirella (forthcoming) for further discussion of the role of kindergarten instruction in improving the English skills of immigrant children in the early 20th-century U.S.

[45] The question *"Speaks English"* was asked only of individuals aged 10 years or older.

[46] A joint test of pre-trends yields a p-value of 0.608, providing no evidence of statistically significant pre-trends.

components).[47] This effect is present only for immigrants from non-English-speaking countries and is highly statistically significant. These results suggest that acquiring English language skills was a key channel through which charity nurseries had a long-lasting impact. Importantly, the stronger effect of treatment on non-English speaking minorities is unlikely to be driven by differences in school attendance rates. As shown in Table 1 ("first stage"), children aged 3–6 had similar school attendance rates across groups. Overall, by facilitating early English acquisition, charity nurseries with kindergarten instruction better prepared children from non-English-speaking households to understand teachers' instructions and learning materials, thereby improving their academic performance and shaping their occupational choices later in life.

Interestingly, adding years of schooling as an additional control substantially reduces the magnitude of the estimated treatment effect (Table 6, Panel B). This suggests that higher educational attainment may mediate the impact of nursery exposure on occupational upgrading. The remaining part of the effect (e.g.., for wages) may be attributed to non-cognitive abilities developed at early ages. Factors such as perseverance, motivation, time preference, and self-control can directly influence earnings, which is consistent with findings in the literature (see Heckman 2008).

If kindergarten instruction in charity nurseries increased the returns to education, one might also expect to observe a fertility decline of affected mothers—consistent with a quantity-quality trade-off. In this context, we draw on recent work by Ager and Cinnirella (forthcoming), who develop a *quantity-quality trade-off* model with two types of potentially complementary investments—kindergarten education and other forms of human capital investments, such as formal schooling. Their framework suggests that access to kindergarten education can lead to a reduction in fertility when complementarities between kindergarten and formal schooling are present. Our findings support this theoretical prediction.

Panel (a) of Figure 9 presents estimates based on equation (1), applied to a sample of first- and second-generation immigrant women aged 16-35 at the time of the Census enumeration (with

[47] To derive occupation-specific cognitive scores, we link occupations in the 1940 U.S. Census to the 1971 Current Population Survey (CPS) based on a sample of 60,441 workers. The survey data contains information on the level of cognitive skills (reasoning, math, and language) associated with each occupation (see details here).

women aged 36 serving as the reference category). The results indicate a negative impact of charity nurseries with kindergarten instruction on fertility, measured by the number of children under age five. Notably, we do not observe a similar effect for nurseries that offered only daycare services in Panel (b).[48] These findings support the hypothesis that access to kindergarten instruction encouraged parents to reduce fertility and increase educational investment in their children.

Finally, while the statistically insignificant effects of access to charity nurseries without kindergarten instruction suggest that health impacts are unlikely to be a major driver of the observed improvements in educational attainment and labor market outcomes, we lack individual-level health data to test this directly. However, we can examine whether access to charity nurseries improved child survival among immigrants. Using U.S. Census data on children ever born and surviving children, we compute a child survival ratio for each ever-married woman residing in Manhattan and Brooklyn in 1900 and 1910.[49] Appendix Figure A19 shows no statistically significant effect of nursery access on child survival. However, these findings should be interpreted with caution. Immigrant families often lived in the city's poorest, most overcrowded, and unsanitary neighborhoods—areas with some of the highest infant and child mortality rates. Living conditions in parts of New York City at that time were abysmal (Duffy, 1974; Ager et al., 2024). Even if the charity nurseries provided hygiene and child-rearing instructions to mothers, the spread of infectious diseases—the major cause of death at that time—did not "respect" enumeration district borders and may have affected treated and untreated households at similar rates.

6 Conclusion

Charities are, today and even more so in the past, an important provider of social services to the urban poor in the United States. Charity organizations emerged in U.S. cities in the late 19th century as a response to rapid industrialization, immigration, and urban poverty. One of their primary objectives was to provide center-based child care and preschool education for children

[48] Our results remain robust when using the total number of children as an outcome (see Appendix Figure A18).

[49] Data on children ever born and surviving children were collected only in the 1900 and 1910 Censuses. Information on surviving children was only collected from ever-married women.

from impoverished neighborhoods.

Our analysis focuses on New York City—the main immigration hub and a leading center of the historical charity organization movement in the U.S. We show that charity nurseries with kindergarten instruction improved the educational attainment and labor market outcomes of disadvantaged children in a cost-effective manner. The effects were strongest for immigrant children from non-English-speaking countries, including some of the poorest ethnic groups in the U.S. at the time (such as immigrants from Southern and Eastern Europe). These educational opportunities offered by charity nurseries significantly contributed to narrowing the gap in educational attainment and wages between immigrants and natives. Overall, our findings underscore the importance of early childhood education for economically disadvantaged groups.

Finally, our findings contribute to the ongoing discussion about whether public preschool programs crowd out private child care providers. While we cannot directly test for crowding out, our study demonstrates that long-term benefits for disadvantaged children were achieved cost-effectively even when child care providers operated without regulation and the public sector was not involved in child care (e.g., through subsidies or investments). Our cost estimates suggest that charity day nurseries with kindergarten instruction in the late 19th century operated with efficiency comparable to that of publicly funded targeted preschool programs in the mid-20th century (Rossin-Slater and Wüst, 2020). Although our study of charity-sponsored center-based care is based on historical data, the findings may offer relevant insights for developing countries where the public sector often lacks the funding and regulatory capacity to operate or subsidize targeted preschool programs effectively.

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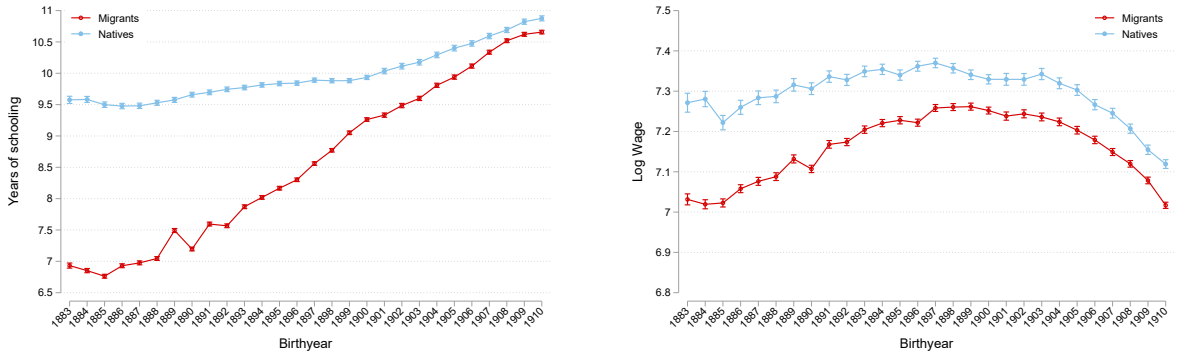
Tables and Figures

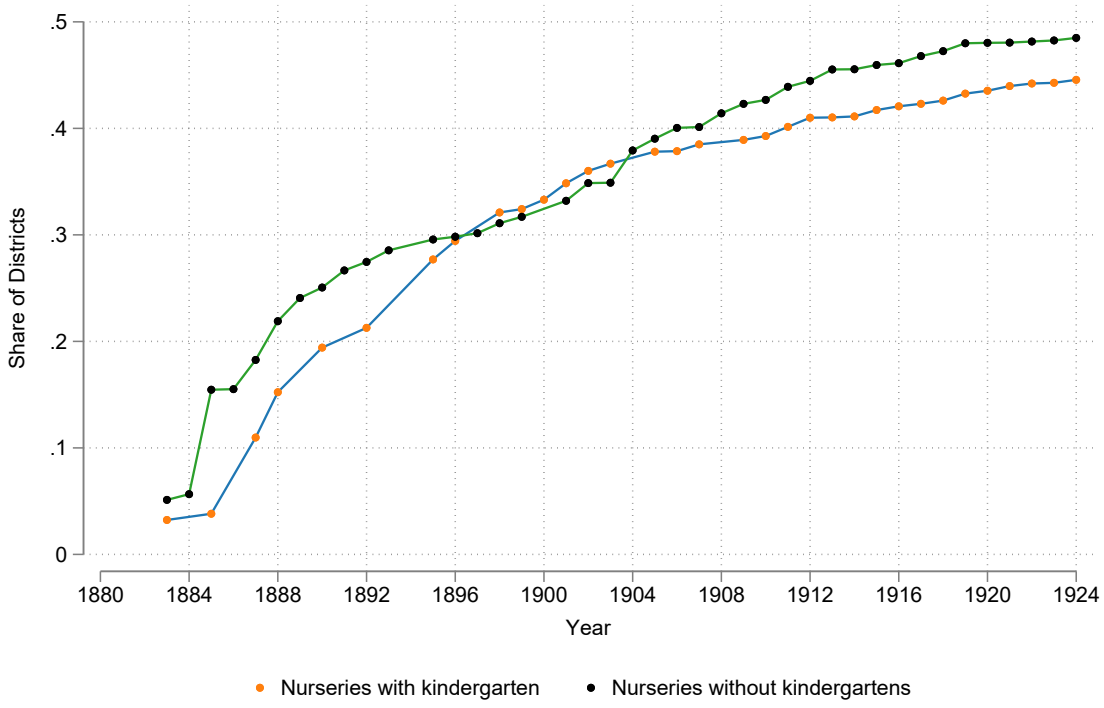
Figure 1: Average Years of Schooling and Wages by Cohort: Natives vs Immigrants



(a) **(b)**

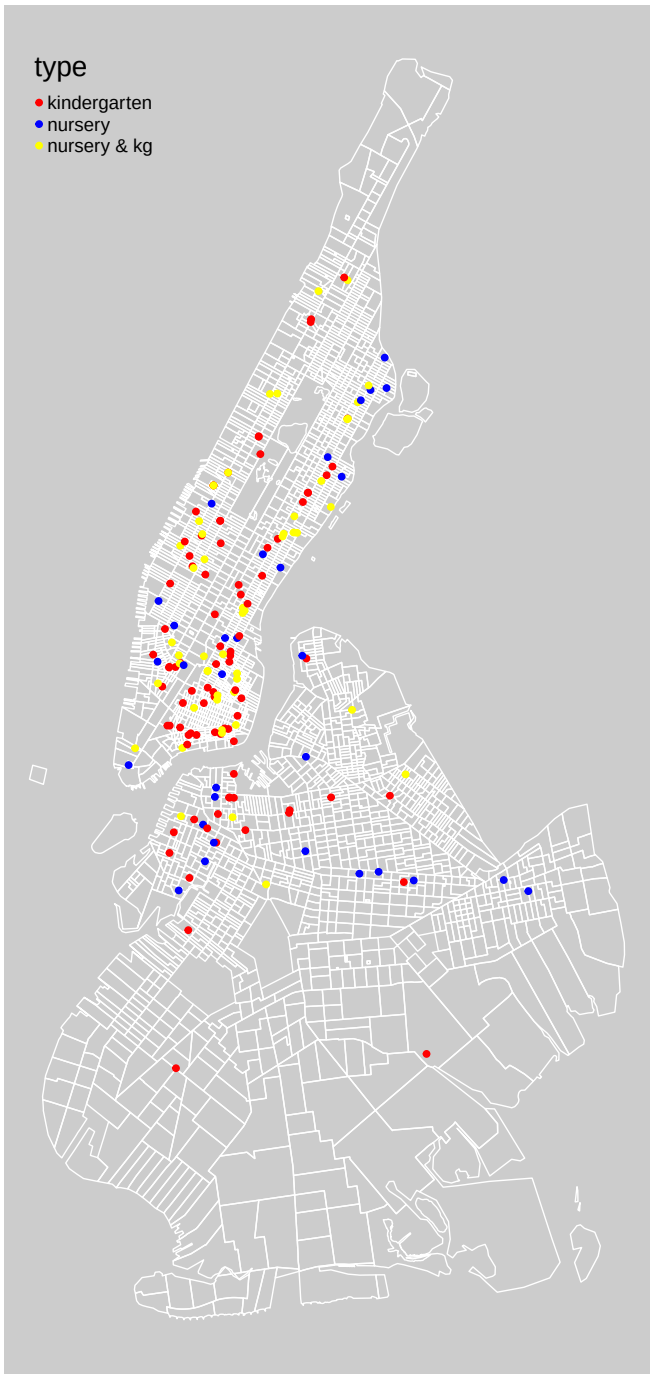
NOTE.— This figure shows the average years of schooling and wage income for individuals for cohorts born in 1883-1910, who grew up in New York City, by U.S.-born natives (the individual and their mother were born in the U.S.) and first- and second-generation immigrants.

Figure 2: Access to Charity Nurseries in Brooklyn and Manhattan



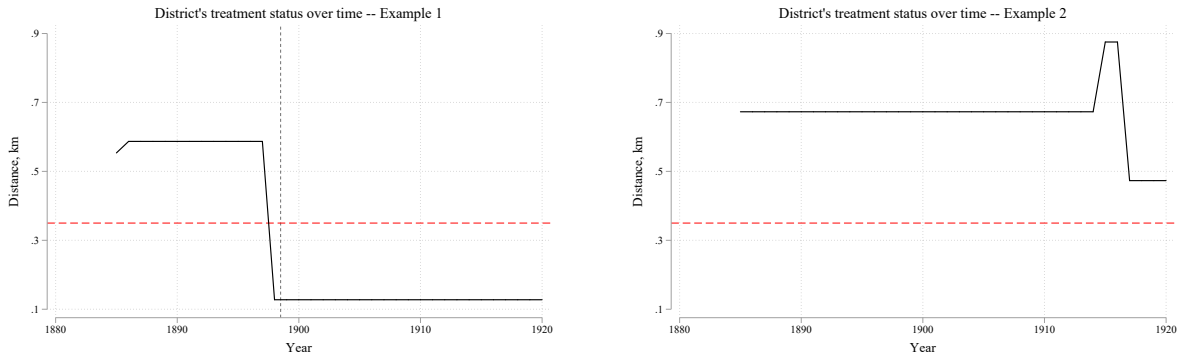
NOTE.— This figure shows the share of enumeration districts in Manhattan and Brooklyn that had access to a charity nursery within 350 meters of their centroid between 1883 and 1924. Each district could have access to a day nursery with and without kindergarten instruction.

Figure 3: Map of Charity Nurseries and Kindergartens in Brooklyn and Manhattan in 1910



NOTE.— This figure shows the location of charity nurseries (with and without kindergarten instruction) and charity kindergartens in Manhattan and Brooklyn in 1910. The enumeration district boundaries correspond to those used in the 1910 Census.

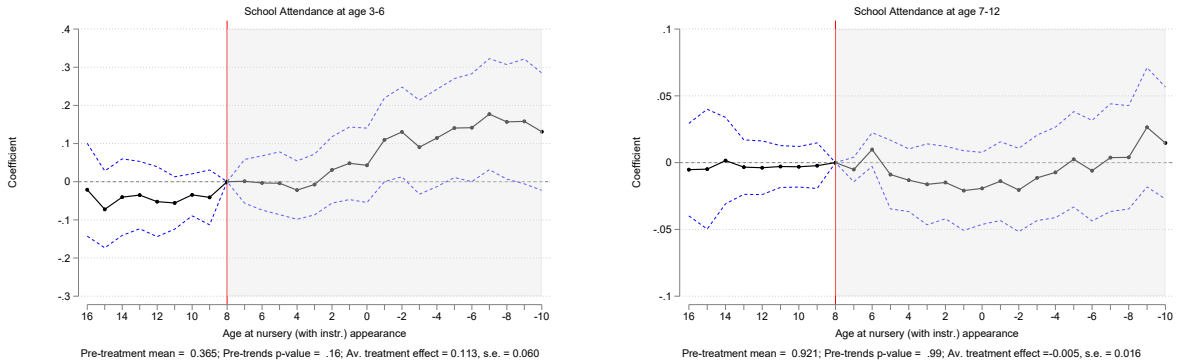
Figure 4: Treatment Status



(a) **(b)**

Note.— This figure illustrates the treatment assignment rule by showing how an enumeration district's proximity to the nearest charity nursery can vary over time. Panel (a) presents an example of a district where the distance fell below the 350-meter threshold in 1898, resulting in treatment status from that year onward. Panel (b) shows a district whose distance to the nearest nursery never crossed the threshold, and thus remained untreated throughout the period.

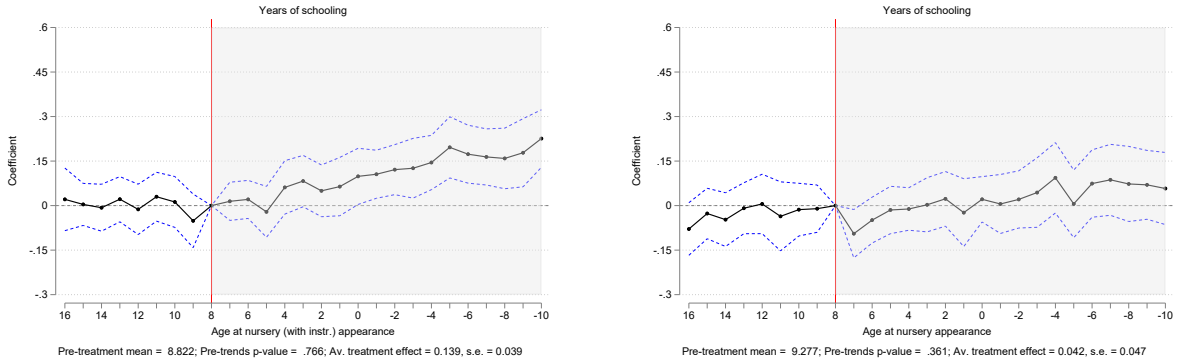
Figure 5: Effect on School Attendance (by age group)



(a) **(b)**

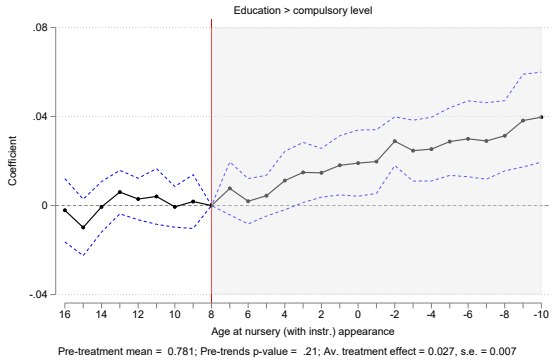
Note.— Panels (a) and (b) display the coefficients on relative time indicators corresponding to an individual's age when the first day nursery (with kindergarten instruction) was established within 350 meters of the enumeration district centroid. The outcome variable is a binary indicator for school attendance, based on the IPUMS variable *school*, which is available in the 1910 and 1920 censuses. Panel (a) shows results for individuals aged 3–6 at the time of the census, while Panel (b) includes individuals aged 7–12. Both subsamples consist of individuals linked between the 1910 or 1920 Census and the 1940 Census. We exclude the 1900 Census from the estimation sample, as enumerators in that year were instructed to collect school attendance data only for individuals of school age. The specification controls for access to charity nurseries without kindergarten instruction and charity kindergartens. Additional controls include fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.

Figure 6: The Effect of Charity Nurseries on Educational Outcomes

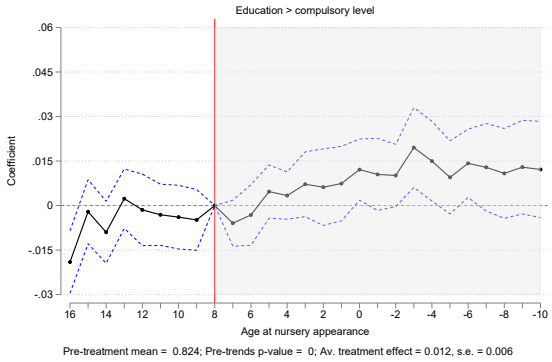


(a)

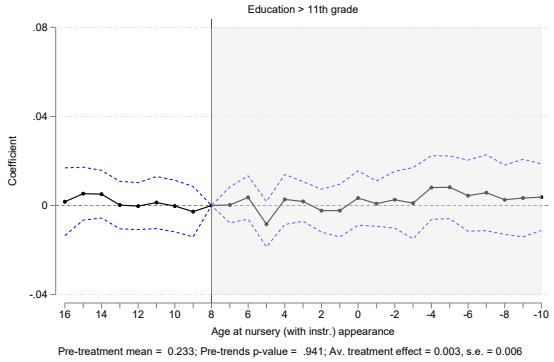
(b)



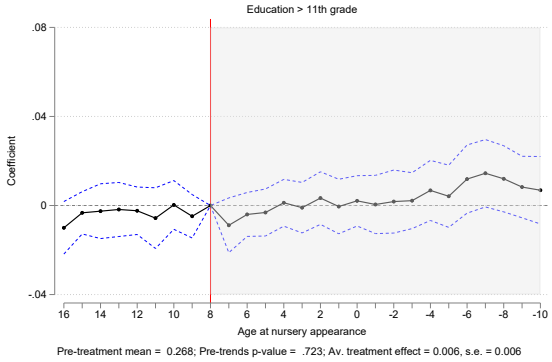
(c)



(d)



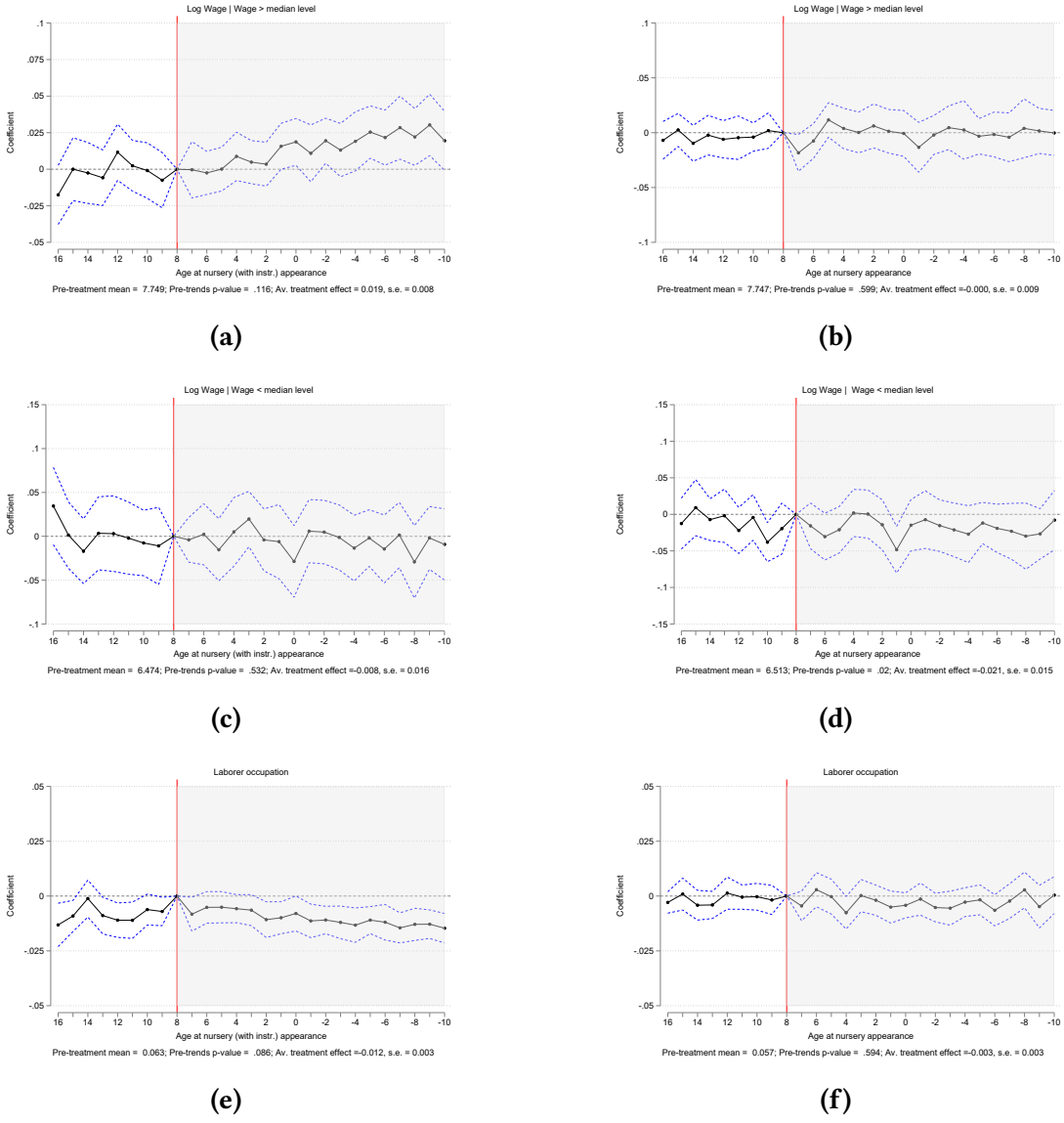
(e)



(f)

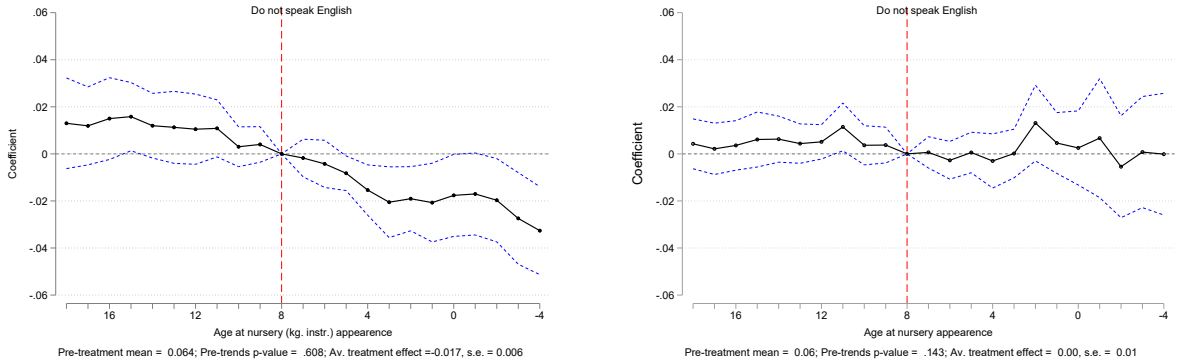
NOTE.— This figure presents the regression coefficients for relative time indicators based on the individual's age at the time the first charity nursery (with and without kindergarten instruction) appeared within 350 meters of the enumeration district centroid. The left column presents estimates for charity nurseries that included kindergarten instruction, while the right column shows estimates for those that did not offer this service. The vertical line denotes the reference category—individuals aged 8. The plot legend reports p-values for the Wald test of the following hypothesis: $\beta_t = 0, 8 < t <= 16$ (i.e., no pre-trends). It also reports the sample outcome mean for the reference cohort ($\beta_{t=8} = 0$). Additionally, the legend reports the average of the post-treatment coefficients: $(\sum_{t=2}^{10} \beta_t)/T$. The sample includes individuals born between 1883 and 1910 residing in Manhattan or Brooklyn. We pool data from the 1900, 1910, and 1920 U.S. Censuses and link individuals to their records in the 1940 Census. The outcome measures include years of schooling, an indicator for education beyond the compulsory level (above 7th grade), and an indicator for high school completion (above 11th grade). Panels (a), (c), and (e) control for access to charity nurseries without kindergarten instruction and charity kindergartens, while Panels (b), (d), and (e) control for access to charity nurseries with kindergarten instruction and charity kindergartens. All specifications include fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.

Figure 7: Effect of Charity Nurseries on Labor Market Outcomes



Note.— This figure displays event-study estimates for labor market outcomes. We estimate wage regressions for two subsamples - above and below the median wage threshold of USD 1,350. These subsamples exclude individuals with zero wages (not in the labor force or self-employed). Panels (a), (c), and (e) control for access to charity nurseries without kindergarten instruction and charity kindergartens, while Panels (b), (d), and (e) control for access to charity nurseries with kindergarten instruction and charity kindergartens. All specifications include fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. For the wage regressions, we also include the yearly number of working weeks as an additional control variables.

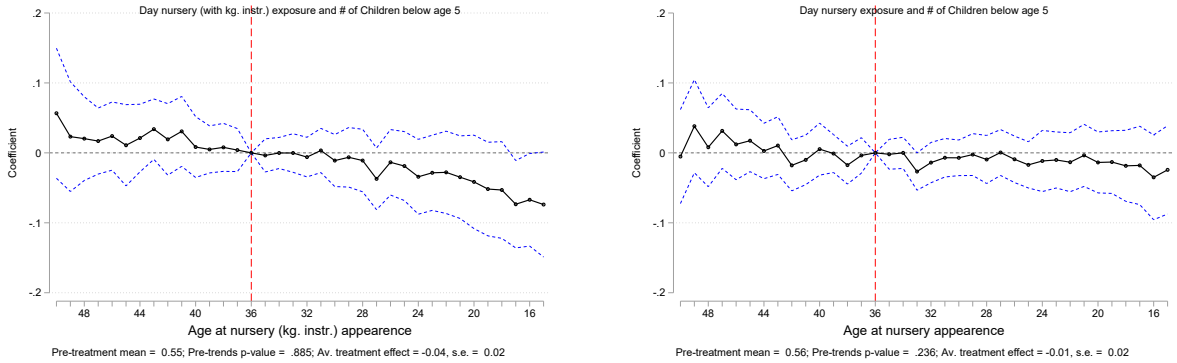
Figure 8: The Effect of Day Nurseries on English Illiteracy



(a) **(b)**

NOTE.—This figure plots regression coefficients on relative time indicators showing the individual's age when the first Day nursery (with and without kindergarten instruction) appeared within 350 meters from the district centroid. The outcome is a binary indicator switching on if an individual reports to *"not speak English"* (based on SPEAKENG variable in IPUMS). The sample consists of first and second-generation immigrant children aged 10-18. We pool data from 1900, 1910, and 1920 (full-count Census data). The specification further includes fixed effects for individual and parental birthplace, birthyear, enumeration district, race, and family size. Further, we control for parental literacy and the individual's number of years spent in the US. All fixed effects are interacted with a census year dummy. We also include ward-specific linear trends. Standard errors are clustered at the ward level.

Figure 9: The Effect of Day Nurseries on Fertility (# of children below age 5)



(a) **(b)**

NOTE.— This figure plots regression coefficients on relative time indicators showing the individual's age when the first Day nursery (with and without kindergarten instruction) appeared within 350 meters from the district centroid. An outcome is the number of children aged 5 and below residing with an individual (IPUMS variable nchlt5). The estimation sample consists of first and second-generation immigrant women aged 16-35. The specification further includes fixed effects for individual and parental birthplace, birthyear, enumeration district, race, and family size. Further, we control for parental literacy and the individual's number of years spent in the US. All fixed effects are interacted with a census year dummy. We also include ward-specific linear trends. Standard errors are clustered at the ward level.

Table 1: Effect on School Attendance (by institution's type and origin)

	(1)	(2)	(3)	(4)
	School attendance			
	age 3–6			age 7–12
Day Nursery (with instr.) β_1	0.038***		0.039***	-0.004
	(0.013)		(0.013)	(0.006)
Day Nursery (with instr.)*Native β_2	0.001		0.000	-0.002
	(0.006)		(0.008)	(0.004)
Day Nursery (no instr.) β_3		-0.016	-0.016	0.003
		(0.029)	(0.028)	(0.007)
Day Nursery (no instr.)*Native β_4		0.001	0.001	-0.003
		(0.007)	(0.010)	(0.004)
$\beta_1 + \beta_2$	0.039***		0.039***	-0.007
	(0.013)		(0.014)	(0.007)
$\beta_3 + \beta_4$		-0.015	-0.015	-0.001
		(0.032)	(0.034)	(0.009)
Observations	113,958	113,958	113,958	145,549
R-squared	0.501	0.501	0.501	0.169

NOTE.— This table reports estimates of the effect of access to different types of institutions on school attendance for first- and second-generation immigrant children compared to the rest of the sample (labeled "Natives"). The outcome variable is a binary indicator for school attendance, based on the IPUMS variable *school*, available in the 1910 and 1920 censuses. "Native" is defined as a binary indicator equal to one if both the individual and their mother were born in the United States *Sum of the coefficients* represents the sum of the coefficients on the main effect and interaction term coefficient. All specifications control for access to charity kindergartens (and its interaction with the Native indicator), fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.*** p<0.01, ** p<0.05, * p<0.1.

Table 2: Summary of the "Event-Study" Estimates of the Day Nursery Impact

	(1)	(2)	(3)	(4)
Dependent variable	Control Mean	ITT	ATT	ATT % change
	PANEL A: Day Nurseries with kindergarten instruction			
Years of schooling	8.82	0.139	0.927	10.5
		(0.39)		
Educ level > compulsory	0.78	0.027	0.18	23
		(0.007)		
Educ level > 11th grade	0.23	0.003	0.02	8.7
		(0.006)		
Wage $	1761.8	34.93	232.87	13.2
		(16.59)		
Log Wage	7.21	0.006	0.04	
		(0.011)		
Log wage | wage > median (1300$)	7.73	0.02	0.133	
		(0.008)		
Log wage | wage < median (1300$)	6.44	-0.008	-0.053	
		(0.016)		
Works as a laborer	0.063	-0.012	-0.08	127
		(0.003)		
Weeks worked last year	30.5	0.079	0.53	1.7
		(0.272)		
Has employment	0.64	0.004	0.026	4.1
		(0.005)		
	PANEL B: Day Nurseries without kindergarten instruction			
Years of schooling	9.28	0.042	0.28	3.0
		(0.047)		
Educ level > compulsory	0.82	0.012	0.08	9.8
		(0.006)		
Educ level > 11th grade	0.27	0.006	0.04	15.0
		(0.006)		
Wage $	1771.6	18.84	125.6	7.1
		(23.5)		
Log Wage	7.23	0.005	0.033	
		(0.013)		
Log wage | wage > median (1300$)	7.75	-0.000	-0.007	
		(0.009)		
Log wage | wage < median (1300$)	6.47	-0.021	-0.14	
		(0.015)		
Works as a laborer	0.057	-0.003	-0.02	35.1
		(0.003)		
Weeks worked last year	32.37	-0.148	-0.987	3.0
		(0.219)		
Has employment	0.67	-0.004	-0.027	4
		(0.005)		

NOTE.— This table provides a summary of event-study estimates of the effect of Day nurseries (with and without kindergarten instruction) on educational and labor-market outcomes. To calculate the ITT estimate, we take the average of event-study coefficients for the cohorts fully exposed to treatment $-10 <= k <= 2$. To calculate the control mean, we average outcome values across individuals in the relative age group $k = 8$. The ATT estimate in column 3 divides the ITT effect in column 2 by the estimated effect of getting access to Day Nursery on school enrollment (0.15, see Section 5.3). Column 4 computes the percentage increase implied by the ATT relative to the control mean (the ratio of column 4 to column 1). The compulsory level of schooling refers to the 7th grade. Laborer refers to occupations that require raw physical labor. Standard errors are clustered at the ward level.

Table 3: Effect on Immigrants vs. Natives

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Years of schooling			Educ. > compulsory	Educ. > 11th grade	Log(wage) if wage > $1300	Laborer
Day Nursery (with instr.)	0.108***		0.080***	0.013***	0.002	0.012***	-0.004**
	(0.023)		(0.021)	(0.004)	(0.003)	(0.003)	(0.002)
Day Nursery (with instr.)*Native	-0.129***		-0.079***	-0.006**	-0.008*	-0.003	0.000
	(0.038)		(0.029)	(0.003)	(0.004)	(0.004)	(0.002)
Day Nursery (no instr.)		0.068**	0.010	0.003	0.001	-0.006	-0.002
		(0.030)	(0.025)	(0.003)	(0.003)	(0.004)	(0.001)
Day Nursery (no instr.)*Native		-0.079*	0.041	0.004	0.005	0.001	0.002
		(0.042)	(0.032)	(0.003)	(0.005)	(0.005)	(0.002)
$\beta_1 + \beta_2$	-0.021		0.001	0.007**	-0.005	0.009**	-0.004**
	(0.031)		(0.026)	(0.003)	(0.005)	(0.004)	(0.002)
$\beta_3 + \beta_4$		-0.011	0.051*	0.007**	0.006	-0.005	0.000
		(0.032)	(0.03)	(0.003)	(0.005)	(0.005)	(0.002)
Observations	1,092,155	1,092,155	1,092,155	1,092,155	1,092,155	381,060	820,211
R-squared	0.220	0.220	0.220	0.143	0.152	0.134	0.052

NOTE.—This table shows the heterogeneity of the treatment effect by nativity. "Native" is defined as a binary indicator equal to one if both the individual and their mother were born in the United States. *Sum of the coefficients* represents the sum of the coefficients on the main effect and interaction term coefficient. All specifications control for access to charity kindergartens (and its interaction with the Native indicator), fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect on Movers vs. Stayers

	(1)	(2)	(3)	(4)	(5)
	Years of schooling	Educ. > compulsory	Educ. > 11th grade	Log(wage) if wage > $1300	Laborer
	Panel A: 1st&2d generation immigrants				
Day Nursery (with instr.) β_1	0.077***	0.014***	0.002	0.009***	-0.005**
	(0.024)	(0.004)	(0.004)	(0.003)	(0.002)
Day Nursery (instr.) × Mover β_2	-0.069**	-0.009**	-0.010**	0.007	0.003
	(0.029)	(0.003)	(0.004)	(0.007)	(0.002)
Mover	0.485***	0.006**	0.086***	0.045***	0.009***
	(0.023)	(0.003)	(0.003)	(0.003)	(0.001)
$\beta_1 + \beta_2$	0.009	0.005	-0.007	0.017***	-0.002
	(0.0320)	(0.0045)	(0.0045)	(0.006)	(0.002)
Observations	689,215	689,215	689,215	233,731	528,679
	Panel B: third-and-higher generation native-born				
Day Nursery (with instr.) β_1	0.038	0.007	0.003	0.007	-0.005*
	(0.034)	(0.005)	(0.004)	(0.007)	(0.003)
Day Nursery (instr.) × Mover β_2	-0.085**	-0.006	-0.012*	-0.007	0.004
	(0.041)	(0.004)	(0.006)	(0.009)	(0.003)
Mover	0.681***	0.014***	0.117***	0.074***	-0.001
	(0.046)	(0.005)	(0.005)	(0.004)	(0.002)
$\beta_1 + \beta_2$	-0.047	0.001	-0.009	0.0005	-0.002
	(0.041)	(0.004)	(0.006)	(0.009)	(0.004)
Observations	402,701	402,701	402,701	146,860	291,228

NOTE.—This table shows the heterogeneity of having access to a charity nursery with kindergarten instructions by mover status and nativity. Panel A presents results for first- and second-generation immigrants, while Panel B reports results for native-born (third-or-higher generation). "Mover" is defined as a binary indicator equal to one if the individual moved out of Brooklyn and Manhattan by 1940. *Sum of the coefficients* represents the sum of the coefficients on the main effect and interaction term coefficient. All specifications control for access to charity nurseries without kindergarten instructions and charity kindergartens (and its interaction with the "Mover" indicator), fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, race, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.*** p<0.01, ** p<0.05, * p<0.1

Table 5: The Long-Term Effects of Black Charities

	(1)	(2)	(3)	(4)	(5)
	Years of sch	Educ > compulsory level	Educ > 11th grade	Log(wage)	Laborer occupation
Black institution*Black	0.812***	0.130***	0.075***	0.070	-0.095***
	(0.102)	(0.041)	(0.012)	(0.058)	(0.010)
Black institution*Non-Black	-0.043	0.007	0.005	0.032***	0.001
	(0.065)	(0.009)	(0.005)	(0.008)	(0.008)
Black	-1.530***	-0.169***	-0.151***	-0.554***	0.098***
	(0.089)	(0.008)	(0.010)	(0.015)	(0.005)
t-test p-val	0.000***	0.0145**	0.000***	0.435	0.000***
Observations	1,092,155	1,092,155	1,092,155	659,952	820,211
R-squared	0.220	0.143	0.152	0.394	0.052

Note.— This table shows the impact of Black charities on education and labor market outcomes. Black is a binary indicator switching on if the individual belongs to the corresponding minority. Black is a binary indicator equal to one if an individual is classified as Black in the census. Black institution is a binary indicator equal to one if the individual was exposed to a Black charity. We report the p-value of the following test-statistics: null hypothesis Black institution*Black = Black institution*Non-Black. All specifications include fixed effects for enumeration district and birth year, as well as controls for individual's birthplace, gender, and years spent in the U.S., parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census year dummy. Standard errors are clustered at the ward level.*** p<0.01, ** p<0.05, * p<0.1.

Table 6: The Effect of Day Nurseries on Wages, Occupational Choice and Cognitive Skills: Natives vs. English and Non-English Speakers

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of schooling	Log(wage) if wage > $1300	Laborer	Reasoning score	Math score	Language score
	Panel A: Education, Wages and Occupational Upgrade					
Day Nursery (with kg) β_1	-0.010	0.009*	-0.003	-0.002	0.004	-0.003
	(0.027)	(0.004)	(0.002)	(0.009)	(0.010)	(0.010)
Day Nursery (with kg)*Eng β_2	0.004	-0.005	-0.001	-0.008	-0.009	-0.005
	(0.029)	(0.007)	(0.002)	(0.010)	(0.011)	(0.011)
Day Nursery (with kg)*Non-Eng β_3	0.123***	0.005	-0.002	0.013	0.015	0.019**
	(0.034)	(0.005)	(0.002)	(0.009)	(0.009)	(0.010)
$\beta_1 + \beta_2$	-0.006	0.004	-0.003	-0.01	-0.006	-0.008
	(0.031)	(0.006)	(0.003)	(0.009)	(0.01)	(0.011)
$\beta_1 + \beta_3$	0.113***	0.014***	-0.005***	0.012**	0.019***	0.016***
	(0.024)	(0.004)	(0.002)	(0.005)	(0.006)	(0.006)
Observations	1,092,155	373,101	802,476	791,635	791,635	791,635
R-squared	0.222	0.138	0.052	0.089	0.096	0.104
	Panel B: Wage and Occupational Upgrade (cond. on education)					
Day Nursery (with kg) β_1		0.010**	-0.003	-0.000	0.005	-0.002
		(0.004)	(0.002)	(0.006)	(0.007)	(0.007)
Day Nursery (with kg)*Eng β_2		-0.005	-0.001	-0.009	-0.010	-0.005
		(0.007)	(0.002)	(0.008)	(0.009)	(0.008)
Day Nursery (with kg)*Non-Eng β_3		-0.001	-0.001	0.002	0.004	0.005
		(0.005)	(0.002)	(0.007)	(0.008)	(0.007)
Years of school.		0.039***	-0.009***	0.101***	0.092***	0.126***
		(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
$\beta_1 + \beta_2$		0.005	-0.003	-0.009	-0.005	-0.007
		(0.006)	(0.003)	(0.008)	(0.008)	(0.009)
$\beta_1 + \beta_3$		0.009***	-0.004**	0.002	0.009*	0.003
		(0.003)	(0.002)	(0.004)	(0.005)	(0.005)
Observations		373,095	802,471	791,630	791,630	791,630
R-squared		0.232	0.067	0.242	0.211	0.275

Note.—This table (Panel A) shows the effect of day nurseries (with kindergarten instruction) on education, and labor-market outcomes, including occupation-specific cognitive skills. The interaction terms compare effects for individuals whose mother or themselves were born in English- or non-English-speaking countries. The specification includes fixed effects for individual and parental birthplace, birth year, enumeration district, race, and family size. It also controls for parental literacy and the individual's years spent in the U.S. All fixed effects are interacted with census year dummies, and ward-specific linear trends are included. The wage regression is estimated for individuals earning above $1300 and controls for weeks worked. Panel B includes an individual's years of schooling as an additional control. Standard errors are clustered at the ward level.
*** p<0.01, ** p<0.05, * p<0.1

A Appendix. Additional Figures and Tables

Table A1: Summary Statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Kindergarten / nursery exposure					
Nursery (with instr.) dummy	1136,320	0.239	0.427	0.000	1.000
Nursery dummy	1,136,320	0.222	0.416	0.000	1.000
Natives	1,136,320	0.370	0.483	0.000	1.000
Black	1,136,320	0.020	0.140	0.000	1.000
Linked sample outcomes – all groups					
Years schooling	1,106,885	9.444	3.378	0.000	20.00
Educ > compulsory level (7th Grade)	1,106,885	0.834	0.372	0.000	1.000
Educ > 11th Grade	1,106,885	0.290	0.454	0.000	1.000
Log wage	671,113	7.225	0.826	0.000	8.517
White-collar	832,747	0.543	0.498	0.000	1.000
Laborer	832,747	0.051	0.221	0.000	1.000
Weeks worked last year	1,136,320	31.36	23.54	0.000	52.00
Has employment	1,136,320	0.659	0.474	0.000	1.000
Cognitive skills (Reasoning)	821,573	3.576	0.789	1.556	6.000
Cognitive skills (Math)	821,573	2.665	0.826	1.000	5.925
Cognitive skills (Language)	821,573	3.039	0.935	1.000	6.000
Natives					
Years schooling	410,877	9.976	3.201	0.000	20.000
Educ > compulsory level (7th Grade)	410,877	0.873	0.334	0.000	1.000
Educ > 11th Grade	410,877	0.345	0.475	0.000	1.000
Log wage	248,166	7.276	0.852	0.000	8.517
White-collar	298,172	0.568	0.495	0.000	1.000
Laborer	298,172	0.052	0.222	0.000	1.000
Weeks worked last year	420,517	30.736	23.905	0.000	52.000
Has employment	420,517	0.641	0.480	0.000	1.000
Cognitive skills (Reasoning)	294,240	3.615	0.780	1.556	6.000
Cognitive skills (Math)	294,240	2.692	0.834	1.000	5.925
Cognitive skills (Language)	294,240	3.091	0.922	1.000	6.000
1&2d generation immigrants					
Years schooling	696,008	9.130	3.439	0.000	20.000
Educ > compulsory level (7th Grade)	696,008	0.812	0.391	0.000	1.000
Educ > 11th Grade	696,008	0.258	0.437	0.000	1.000
Log wage	422,947	7.195	0.808	0.000	8.517
White-collar	534,575	0.528	0.499	0.000	1.000
Laborer	534,575	0.051	0.220	0.000	1.000
Weeks worked last year	715,803	31.729	23.309	0.000	52.000
Has employment	715,803	0.670	0.470	0.000	1.000
Cognitive skills (Reasoning)	527,333	3.554	0.793	1.556	6.000
Cognitive skills (Math)	527,333	2.650	0.821	1.000	5.925
Cognitive skills (Language)	527,333	3.010	0.941	1.000	6.000
Unlinked sample – Immigrants					
Survived children ratio	444,165	0.842	.269	0.000	1.000
# Children below age 5	1,701,710	0.45	0.78	0.000	9.000
Don't speak English	1,060,126	0.053	0.22	0.000	1.000

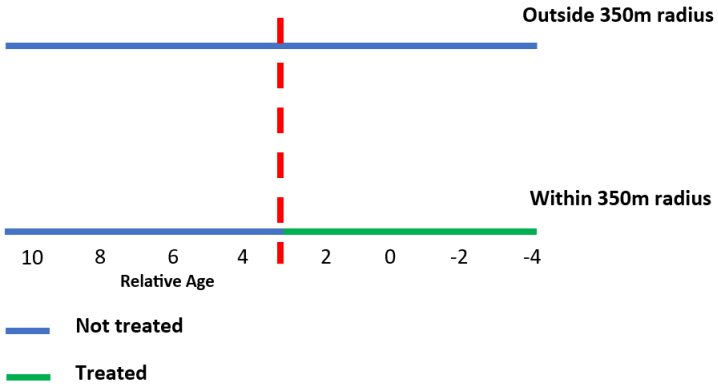
NOTE.— The sample includes male individuals residing in Manhattan and Brooklyn who are linked from 1900/1910/1920 to the 1940 US Census with excluded duplicate matches. The sample includes only cohorts born in 1883-1910. Note that the information on the following variables: *Survived children ratio*, *Children below age 5*, and *Don't speak English* refers to an unlinked sample of individuals, observed in 1900/1910/1920 US censuses.

Table A2: Regressions with Family Fixed Effects

	(1)	(2)	(3)	(4)	(5)
	Yrs. school	Educ > compulsory	Educ > 11th grade	Log(wage) | Wage > 1300$	Laborer
	PANEL A: Family FE Yes				
Day Nursery (instr.)	0.105*	0.016**	0.005	-0.001	-0.007**
	(0.056)	(0.007)	(0.006)	(0.008)	(0.003)
Day Nursery	0.073**	0.010**	0.006	-0.013	0.003
	(0.036)	(0.005)	(0.004)	(0.010)	(0.002)
Observations	527,284	527,284	527,284	99,447	351,052
R-squared	0.620	0.537	0.593	0.580	0.473
	PANEL B: Family FE No				
Day Nursery (instr.)	0.144**	0.022***	0.005	0.004	-0.004*
	(0.064)	(0.007)	(0.006)	(0.007)	(0.002)
Day Nursery	0.073	0.012**	0.006	-0.002	0.001
	(0.045)	(0.005)	(0.004)	(0.008)	(0.002)
Observations	527,284	527,284	527,284	99,447	351,052
R-squared	0.150	0.083	0.119	0.155	0.045

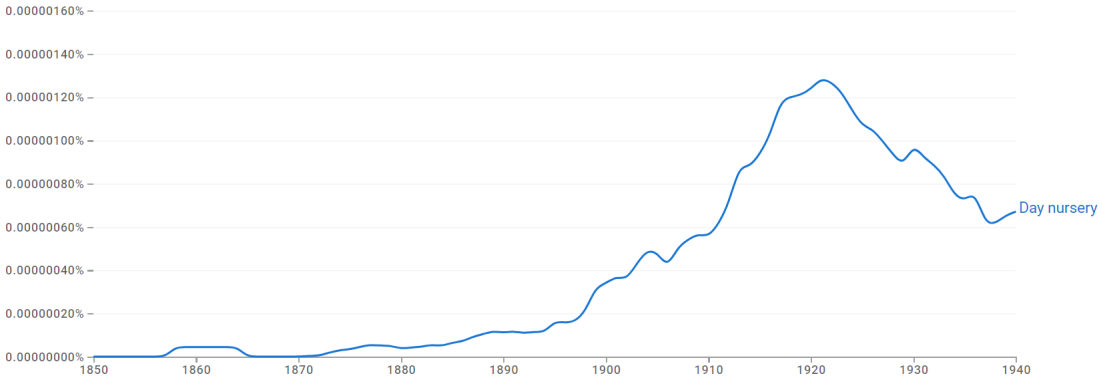
Note.— This table reports the results for the family fixed-effects model. Panel A includes the individual's birthyear and family fixed effects. Panel B keeps the individuals with siblings and applies the baseline regression specification (Equation 1). All specifications control for access to a charity kindergarten. For the wage regressions, we also include the yearly number of working weeks as an additional control variable. Standard errors are clustered at the ward level.*** p<0.01, ** p<0.05, * p<0.1.

Figure A1: Graphical Illustration of the Treatment Assignment



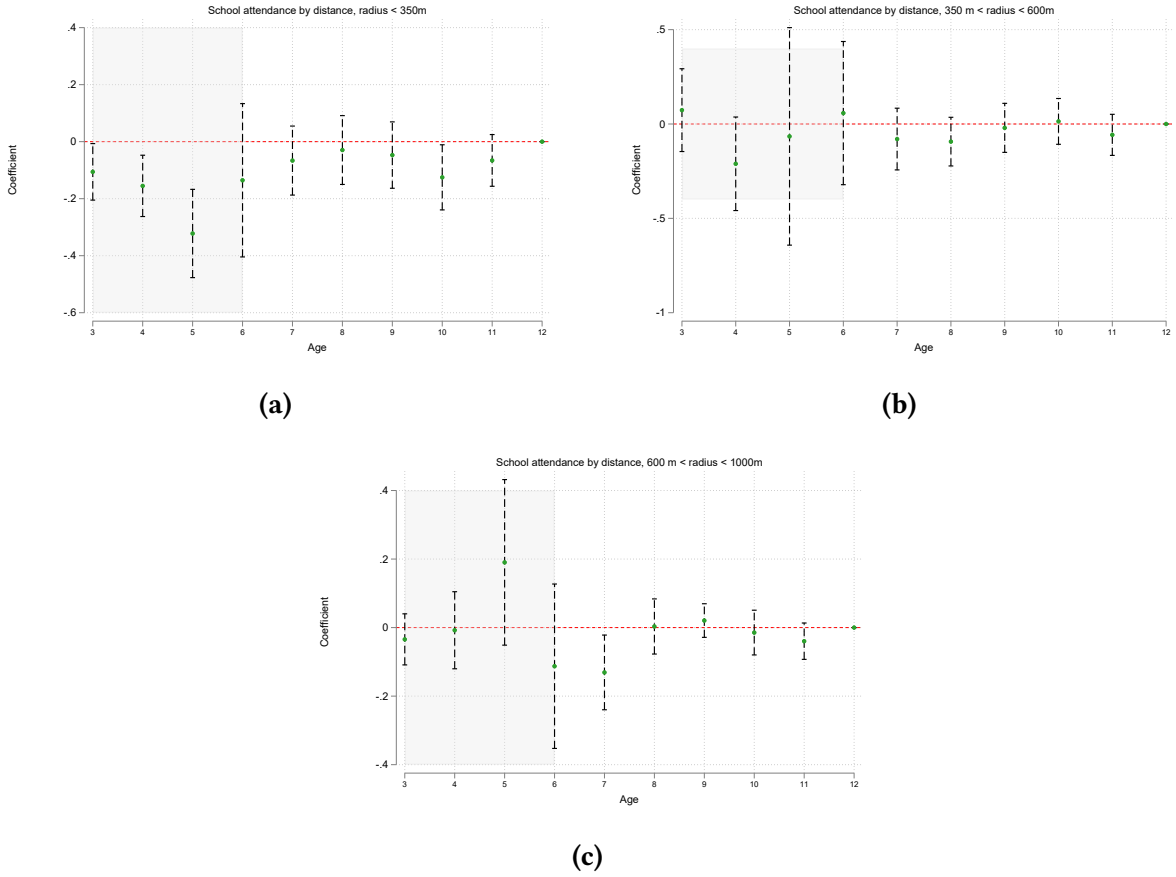
NOTE.— This figure illustrates the treatment assignment rule: to receive the education treatment, an individual must both reside in a district located within 350 meters of the nearest nursery and be three years old or younger at the time the nursery began operating.

Figure A2: Frequency of "Day Nursery" in Google N-gram



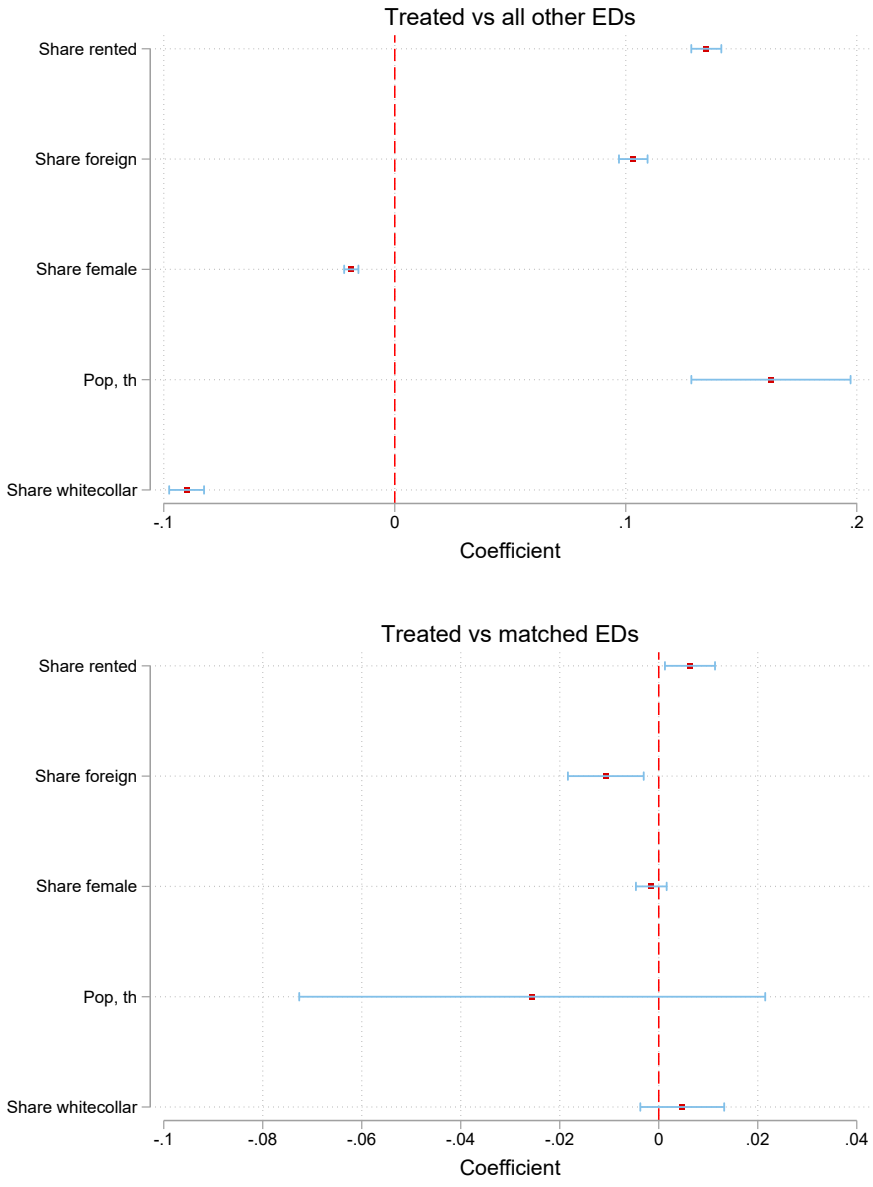
NOTE.— This figure shows the relative frequency of the bigram "Day Nursery" in a corpus of digitized books Books Ngram Viewer.

Figure A3: The Effect of Charity Nurseries with Kindergarten Instruction on School Attendance by Distance and Age Group (1910, 1920 pooled sample estimates)



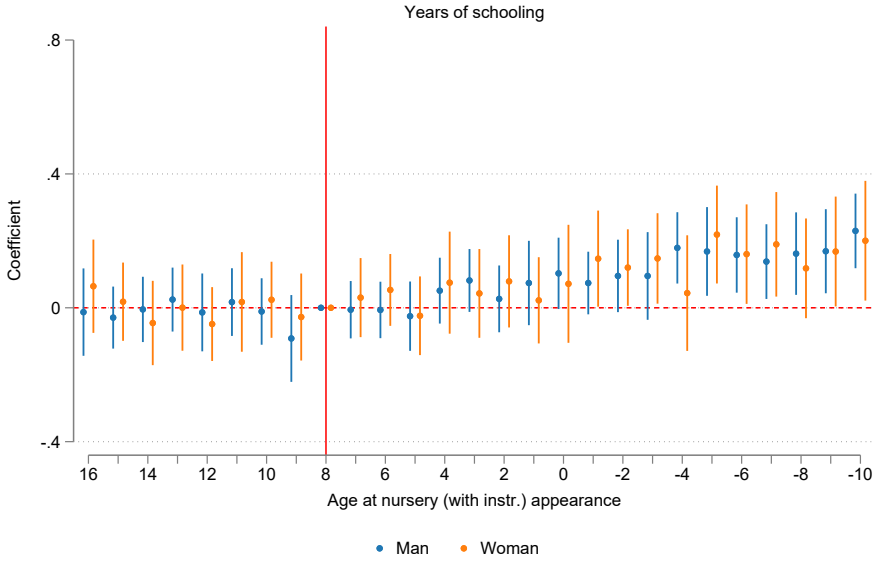
Note.—Panels (a)-(c) show the coefficients of the interaction term *Age*Nursery (with instr.) distance*. The estimates capture the differential impact of proximity to a charity nursery (with kindergarten instruction) on school attendance across different age groups. Children aged 12 serve as the (omitted) reference category. Distance is measured in meters. The outcome variable is a binary indicator of school attendance. The specification includes fixed effects for enumeration districts, as well as indicators for birth year, individual and parental birthplace, gender, and race. All individual-level controls are interacted with a census year dummy. Panel (a) shows estimates for individuals residing within a 350-meter radius; Panel (b) for those residing within 350-600 meter radius; and Panel (c) for those living within a 600–1000 meter radius of the nearest nursery with kindergarten instruction. All subsamples include individuals linked between the 1910/1920 censuses and the 1940 census. Standard errors are clustered at the ward level.

Figure A4: Characteristics of Treated and Control Enumeration Districts in Brooklyn and Manhattan



Note.— This figure shows the mean differences between treated and control enumeration districts before and after nearest-neighbor propensity score matching. To estimate the propensity scores, we fit a logit model of the treatment indicator on each district's characteristics: the share of rented apartments, the share of foreign-born residents, population (in thousands), the share of white-collar workers, and the share of women. The plotted values are the coefficients from separate bivariate regressions of the treatment dummy on each covariate before and after matching.

Figure A5: The Effect of Charity Nurseries with Kindergarten Instructions on Years of Schooling by Gender



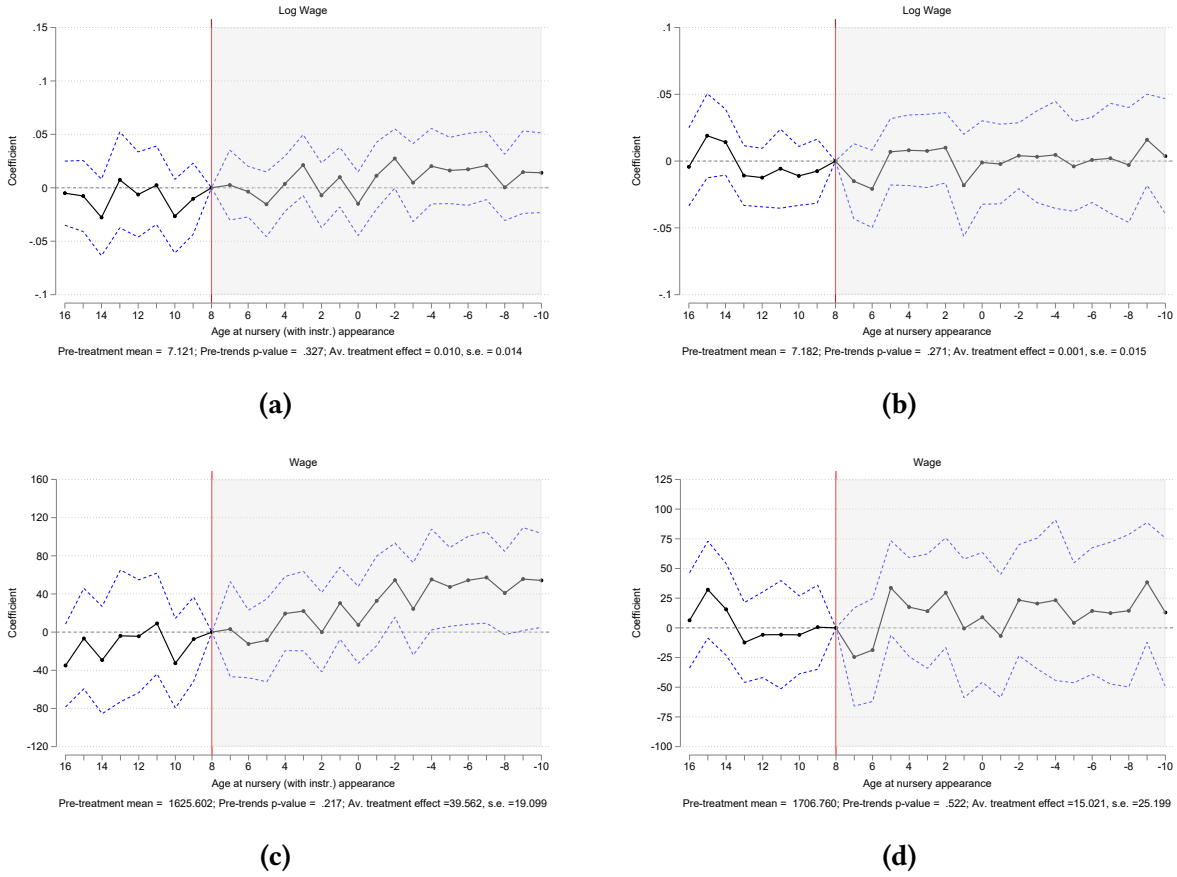
NOTE.— This figure shows the effect of access to a charity nursery (with instr.) on years of schooling by gender (men = blue/women = orange).

Table A3: Effect on Years of Schooling: Immigrants vs. Natives by Gender

	All	Males	Females
	Years of Schooling		
Day Nursery (with instructions) β_1	0.084***	0.072***	0.092**
	(0.022)	(0.023)	(0.037)
Day Nursery (with instructions) × Native β_2	-0.080***	-0.060*	-0.104**
	(0.030)	(0.033)	(0.045)
Day Nursery (no instructions) β_3	0.009	0.022	-0.006
	(0.025)	(0.032)	(0.030)
Day Nursery (no instructions) × Native β_4	0.039	0.029	0.058
	(0.033)	(0.037)	(0.043)
$\beta_1 + \beta_2$	0.004	0.011	-0.012
	(0.026)	(0.035)	(0.034)
$\beta_3 + \beta_4$	0.048	0.051	0.051
	(0.030)	(0.034)	(0.039)
Observations	1,092,150	711,675	374,452
R-squared	0.223	0.204	0.291

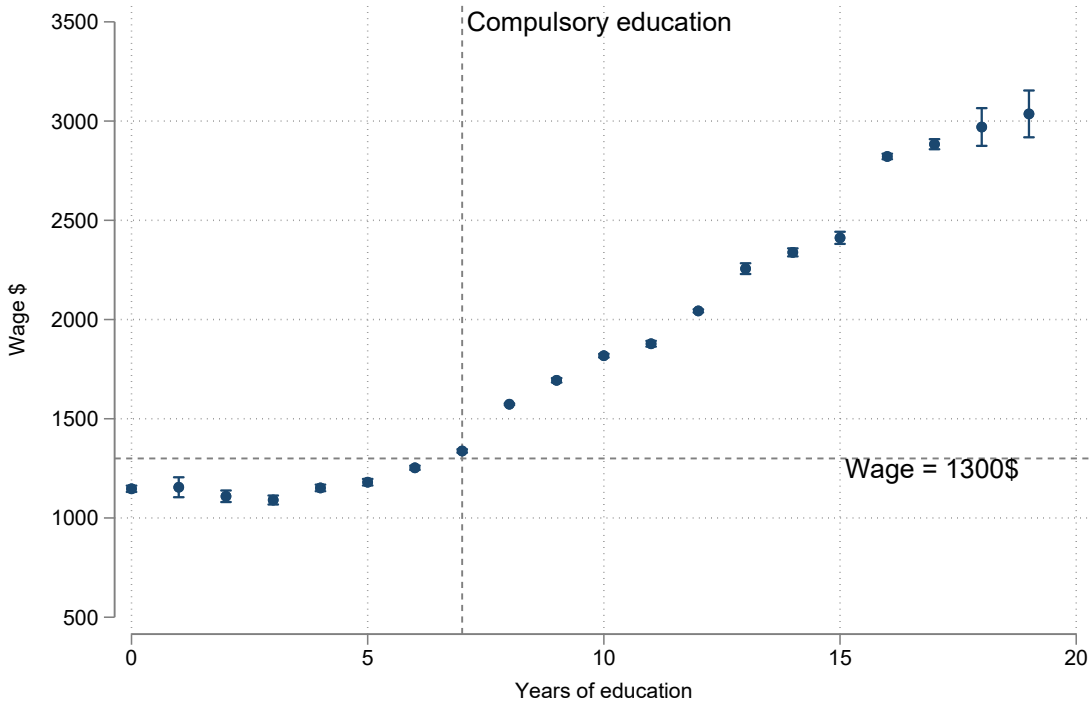
Note.—This table replicates the findings of Table A4 splitting the sample between males and females. "Native" is a binary indicator equal to one if both the individual and their mother were born in the United States. The rows labelled $\beta_1 + \beta_2$ and $\beta_3 + \beta_4$ report the implied treatment effects for natives. All specifications control for access to charity kindergartens (and its interaction with the Native indicator), fixed effects for enumeration district and birth year, individual controls for birthplace, race, gender, years spent in the United States, parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census-year indicator. Standard errors are clustered at the ward level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A6: The Effect of Charity Nurseries on Wages: Full Sample



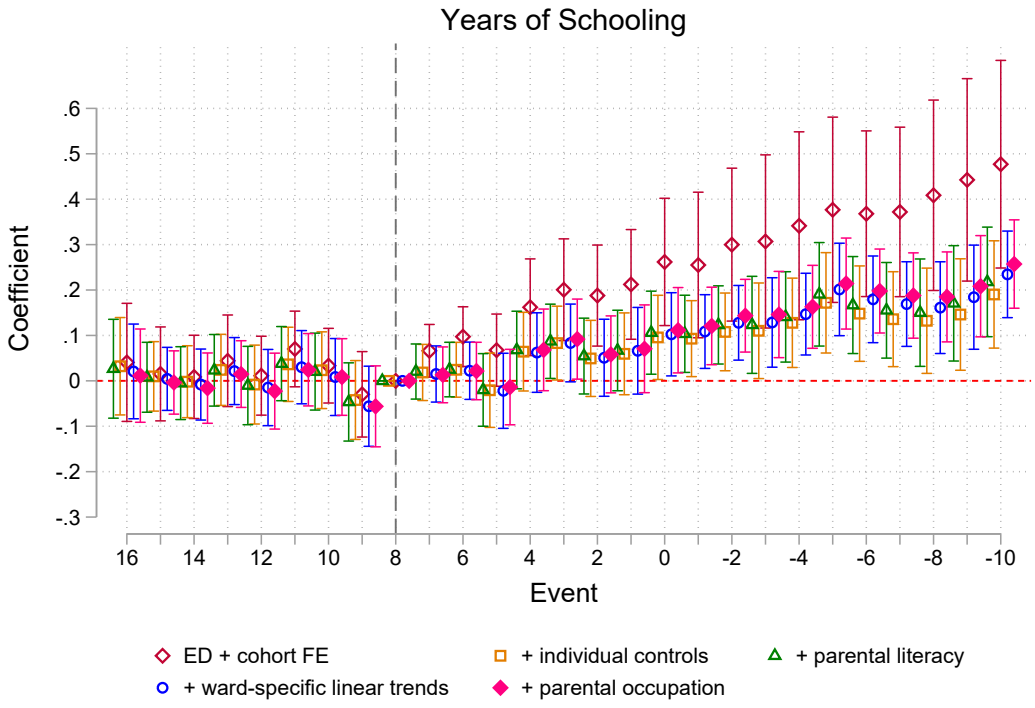
Note.— This figure displays event-study estimates for wages for the whole sample (workers who report positive wages). Panels (a) and (b) report results using log wages (top panel) and Panels (c) and (d) using wages in levels (bottom panel). See table notes to Figure 7 for further details.

Figure A7: Relationship between Years of Education and Wages



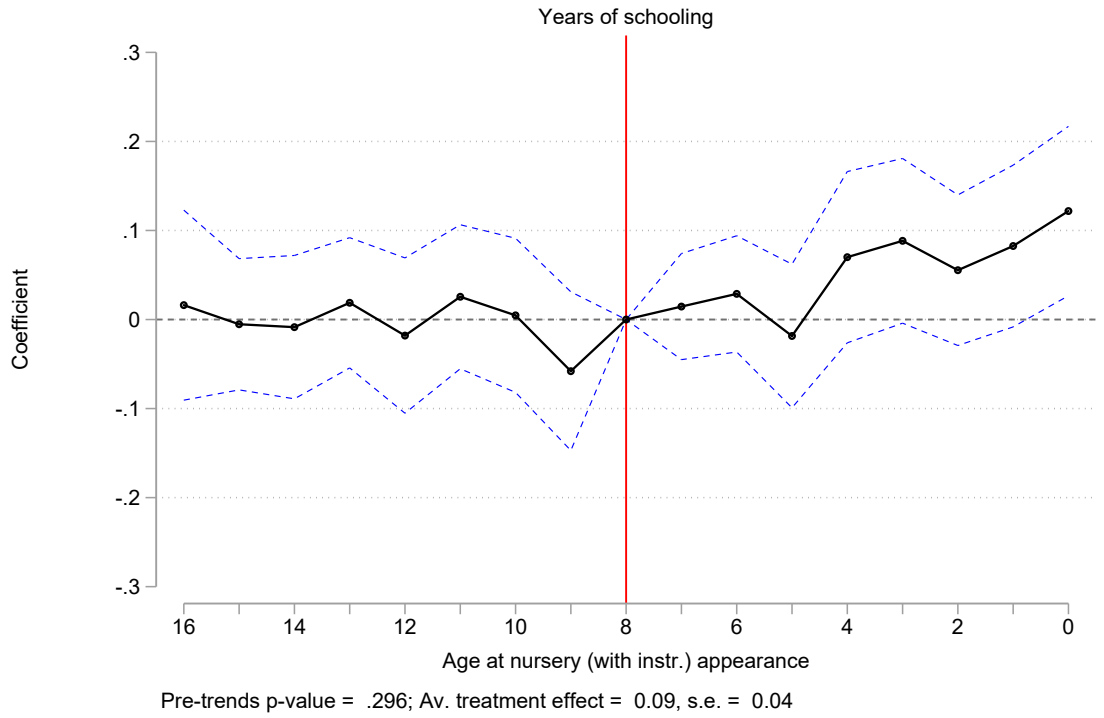
Note.— The binned scatterplot shows average wages for various years of education using information from the 1940 Census. The vertical line represents the level of compulsory education in early 20th-century New York. The horizontal line represents the median wage level in the sample.

Figure A8: Robustness to alternative regression specifications



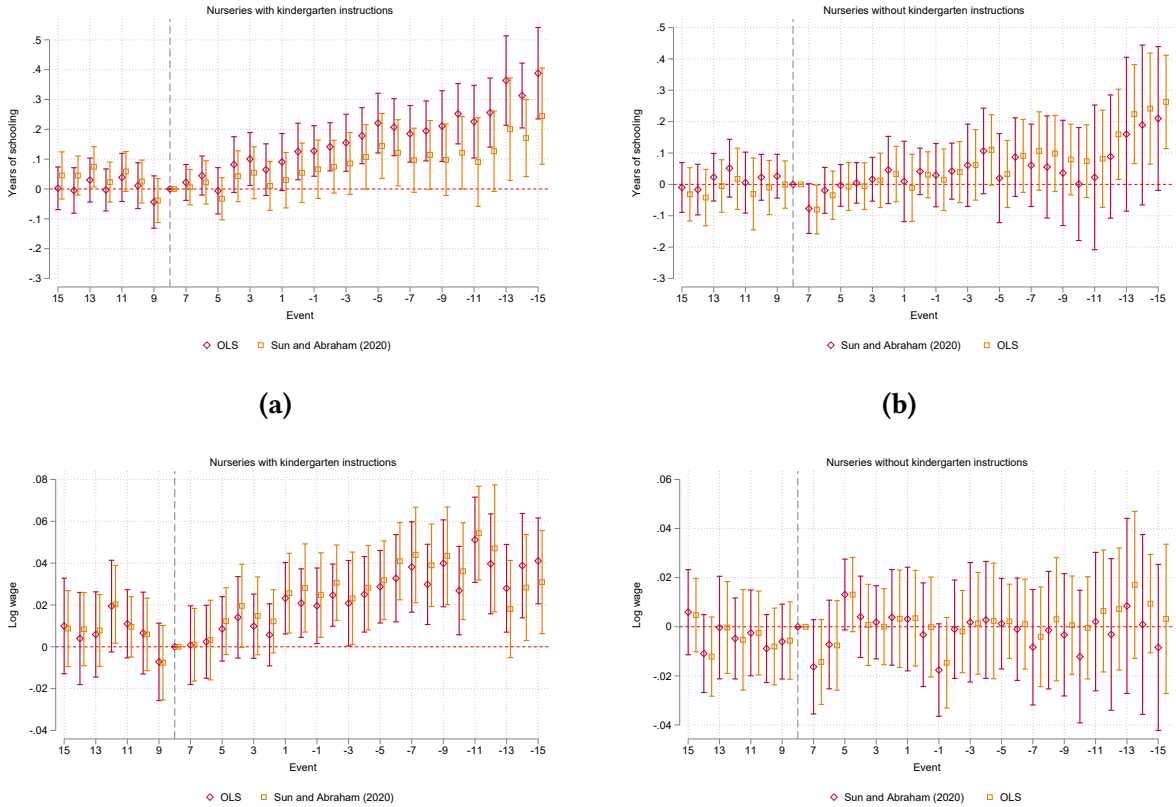
Note.— This figure shows the robustness of the event-study results on education for alternative regression specifications. Specifically. We estimate specification (1) with only enumeration district and birth cohort fixed effects (both interacted with census year dummy); (2) with inclusion of the individual's control, such as years spent in the US, gender indicator, indicators of birthplace, family size, and indicator of the order of birth; (3) adding parental literacy controls, (4) ward-specific linear trends and (5) adding individual's mother working status and an indicator of whether the his/her father works in white-collar occupation. The specification (4) corresponds to our main specification that we use throughout the analysis.

Figure A9: Robustness to excluding "retrospective" treatment assignment



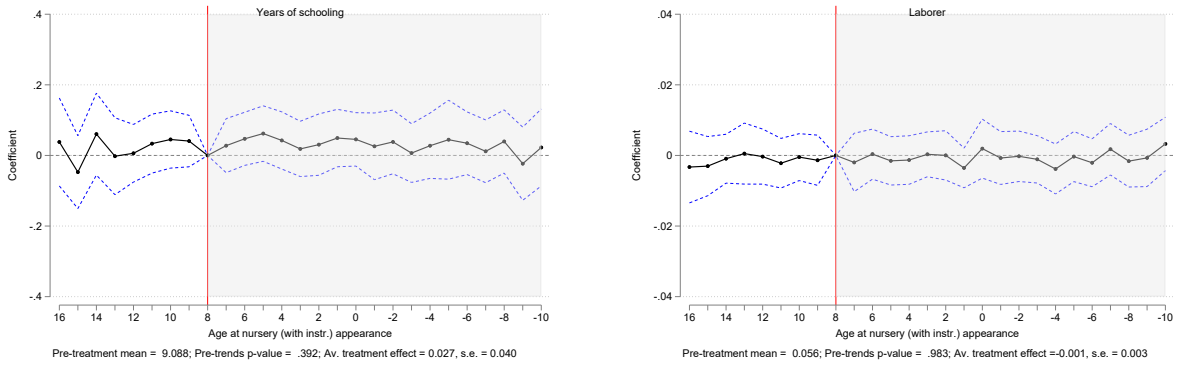
Note.— This figure shows the robustness of the event-study results on education for excluding the individuals with "retrospective" assignment of treatment. The sample consists of only individuals living in the districts that never received the treatment ("never adopters") and those who received treatment in the period following their birth. The regression specification corresponds to our main specification that we use throughout the analysis.

Figure A10: Robustness to Alternative Estimator (Sun and Abraham 2021)



Note.— This figure plots the regression coefficients on relative time indicators corresponding to the individual's age when the first Day nursery (with and without kindergarten instruction) appeared within 350 meters from the district centroid. The outcome is years of schooling by 1940 aggregated to the enumeration district level. Before aggregating individual data to the enumeration district level, we regress each outcome on individual and family characteristics from Equation 1 and enumeration district#birth-year fixed effects. Accordingly, we obtain conditional mean outcomes for each enumeration district × birth-year cohort that we then use as dependent variables in our main regressions. Such an approach reduces the computational burden for estimating the model with relative time and cohort indicators. The event-study coefficients are derived following Sun and Abraham 2021 approach described in Section 5.2.

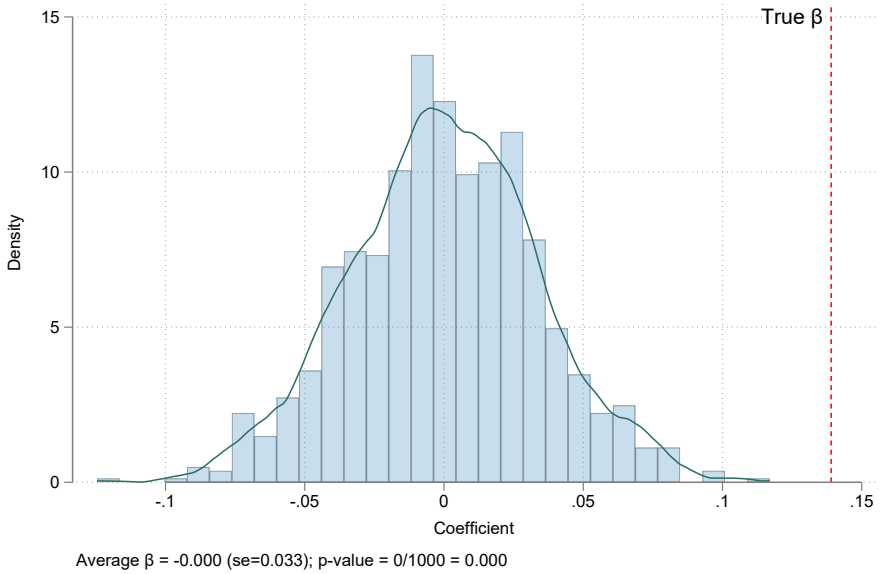
Figure A11: The Effect of Placebo Treatment on Education and Occupational Choice



(a) **(b)**

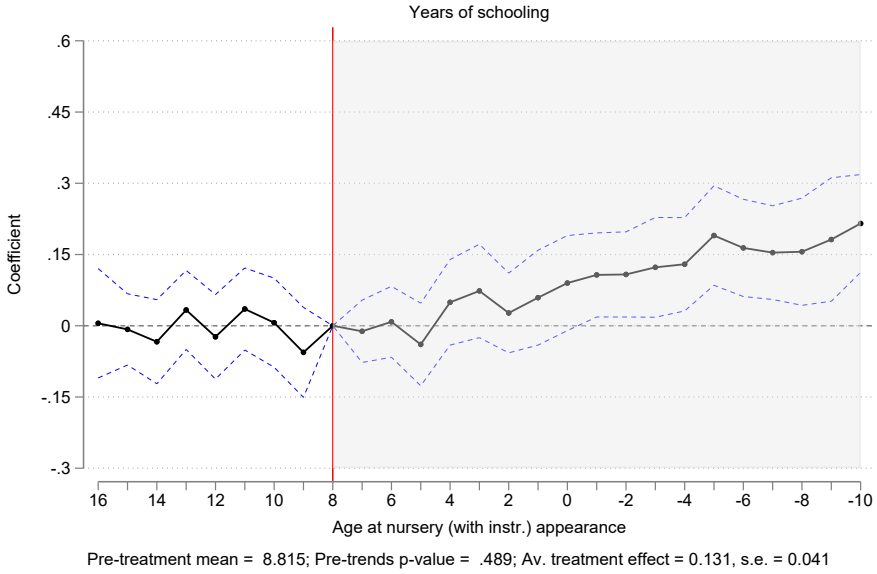
Note.— This figure plots regression coefficients on relative time indicators showing the individual's age when the first day nursery with kindergarten instruction appeared within 500-1000 meters (false treatment) from the district centroid. The specification includes fixed effects for individual and parental birthplace, birthyear, enumeration district, race, and family size. Further, we control for parental literacy and the individual's number of years spent in the US. All fixed effects are interacted with a census year dummy. We also include ward-specific linear trends. Standard errors are clustered at the ward level.

Figure A12: Permutation Test



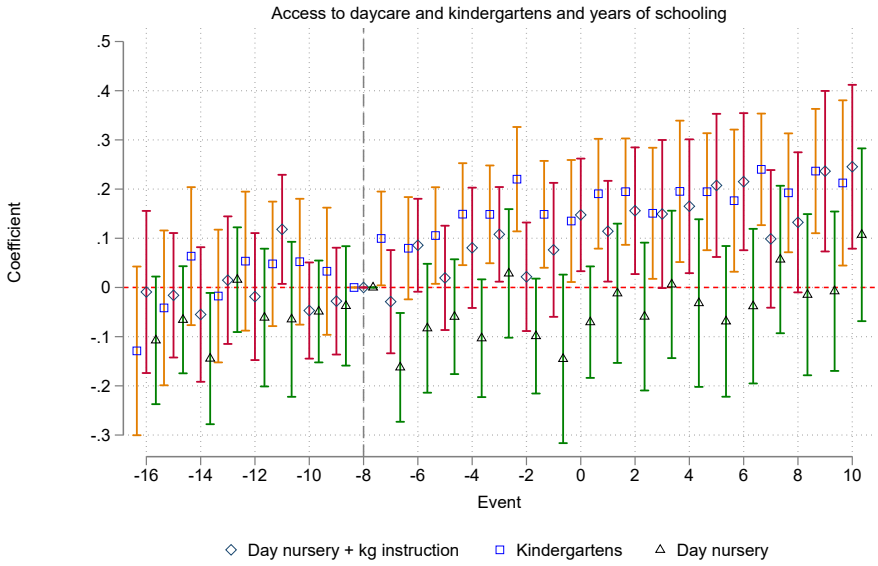
Note.— This figure plots the results of a permutation test, which reshuffles the dates when enumeration districts received access to charity nurseries (with kindergarten instruction) in a given year. The reshuffling procedure preserves the annual count of districts with nursery access. The vertical line presents the estimate of the actual treatment effect reported in Table 2.

Figure A13: The Effect of Day Nurseries on Adult Education (matched sample)



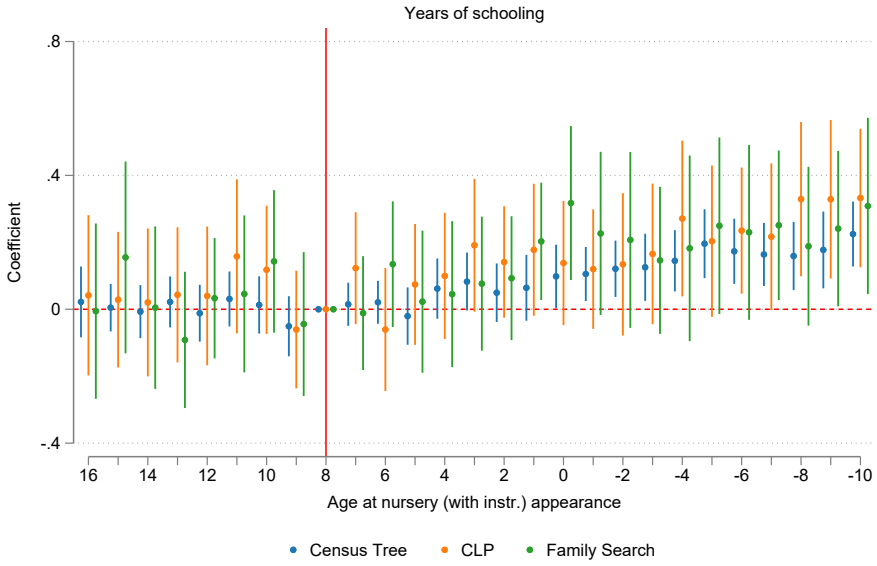
NOTE.— This figure plots the regression coefficients on relative time indicators corresponding to the individual's age when the first charity nursery (with kindergarten instruction) appeared within 350 meters from the enumeration district centroid. The sample is restricted to treated and control units based on nearest neighbor propensity score matching. The set of control variables are the same as in Figure 6.

Figure A14: The Effect of Charity Nurseries and Kindergartens on Educational Attainment



Note.— This figure shows the effect of charity institutions (all types) on years of schooling. The point estimates for the effect of charity nurseries with kindergarten instruction are displayed as diamonds; for charity nurseries without kindergarten instruction as triangles, and for charity kindergartens as rectangles (see the table notes to Appendix Figure A10 for additional details on the specification).

Figure A15: Robustness to Different Linking Methods



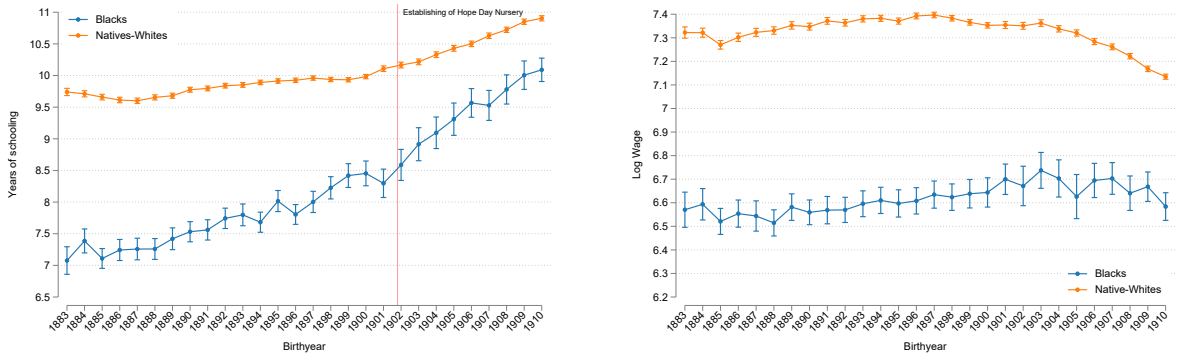
Note.— This figure shows the effect of charity nurseries (with instr.) on the years of schooling for three alternative linking methods: a) the Census Linking Project – CLP; b) Census Tree crosswalk links; and c) links identified by FamilySearch users.

Table A4: Effect on Immigrants vs. Natives: Robustness to Linking Methods

	Baseline	CLP	Family Search
	Years of Schooling		
Day Nursery (with instructions) β_1	0.084***	0.076	0.181***
	(0.022)	(0.049)	(0.061)
Day Nursery (with instructions) × Native β_2	-0.080***	-0.062	-0.114*
	(0.030)	(0.061)	(0.066)
Day Nursery (no instructions) β_3	0.010	0.011	0.039
	(0.025)	(0.062)	(0.057)
Day Nursery (no instructions) × Native β_4	0.039	0.023	-0.005
	(0.033)	(0.064)	(0.067)
$\beta_1 + \beta_2$	0.004	0.014	0.066
	(0.026)	(0.079)	(0.061)
$\beta_3 + \beta_4$	0.049	0.034	0.035
	(0.030)	(0.064)	(0.066)
Observations	1,092,155	211,291	165,599
R-squared	0.222	0.252	0.328

NOTE.—This table replicates the findings of Table 3 using alternative linking methods. Column (1) reports the baseline results based on Census Tree links (Census Tree), while columns (2)–(3) use links from the Census Linking Project (CLP), and manual links identified by FamilySearch users, respectively. "Native" is a binary indicator equal to one if both the individual and their mother were born in the United States. The rows labelled $\beta_1 + \beta_2$ and $\beta_3 + \beta_4$ report the implied treatment effects for natives. All specifications control for access to charity kindergartens (and its interaction with the Native indicator), fixed effects for enumeration district and birth year, individual controls for birthplace, race, gender, years spent in the United States, parental literacy and birthplace, family size, and ward-specific linear trends. All fixed effects are interacted with a census-year indicator. Standard errors are clustered at the ward level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

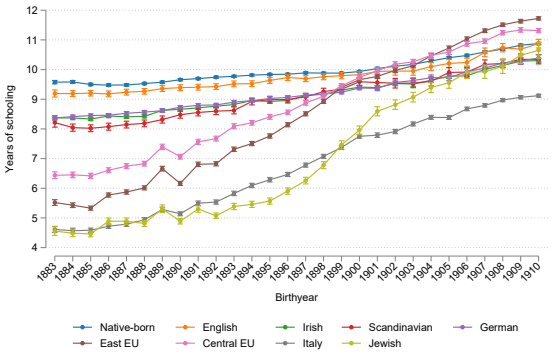
Figure A16: Average Years of Schooling and Wages by Cohorts: U.S.-born Whites vs Blacks



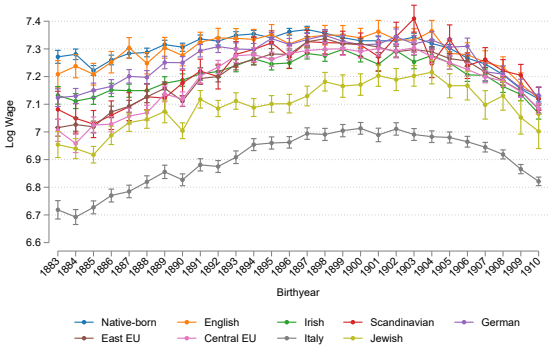
(a) **(b)**

NOTE.— The figure shows trends in years of schooling (Panel a) and log wages (Panel b) for native-born whites (both the individual and his mother were born in the US), and Blacks born in 1883-1910, who grew up in New York City. The vertical line indicates the date of the establishment of Hope Day Nursery - the first-day nursery in our sample that assisted families of African Americans.

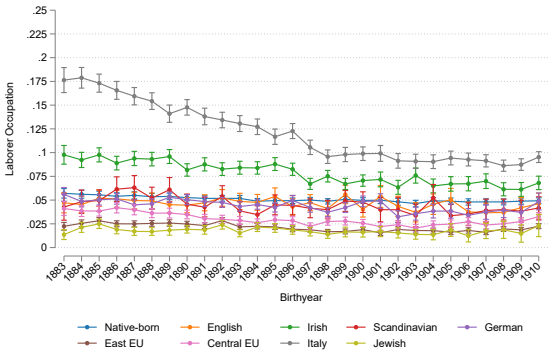
Figure A17: Education and Labor Market Outcomes by Cohorts and Ethnic Groups



(a)



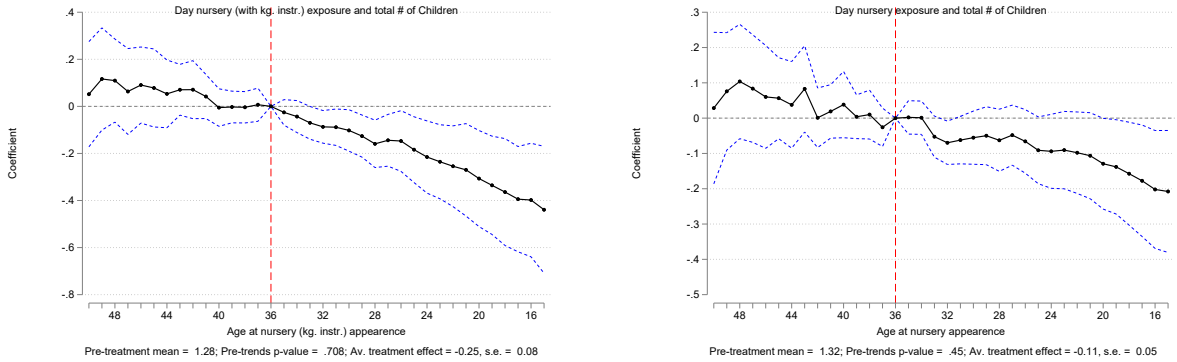
(b)



(c)

NOTE.— This figure plots the average years of schooling, wages, and employment share as a laborer by cohort and ethnicity. The observations are from the estimation sample (the individuals are linked from 1900/10/20 to 1940).

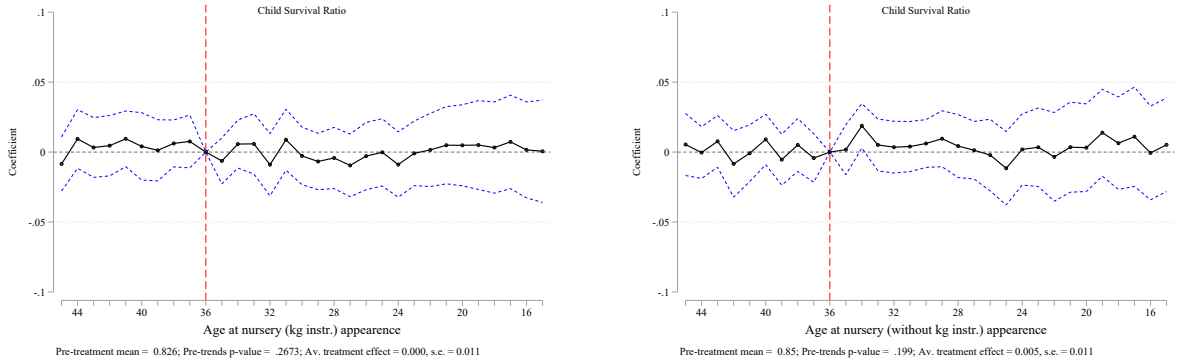
Figure A18: The Effect of Day Nurseries on Fertility (total # of children)



(a) **(b)**

Note.— This figure plots event-study coefficients of the effect of Day nurseries on fertility. An outcome variable is the total number of children residing with an individual (IPUMS variable nchild). See Figure **??** note for estimation details.

Figure A19: Effects of access to day nurseries on survival of children



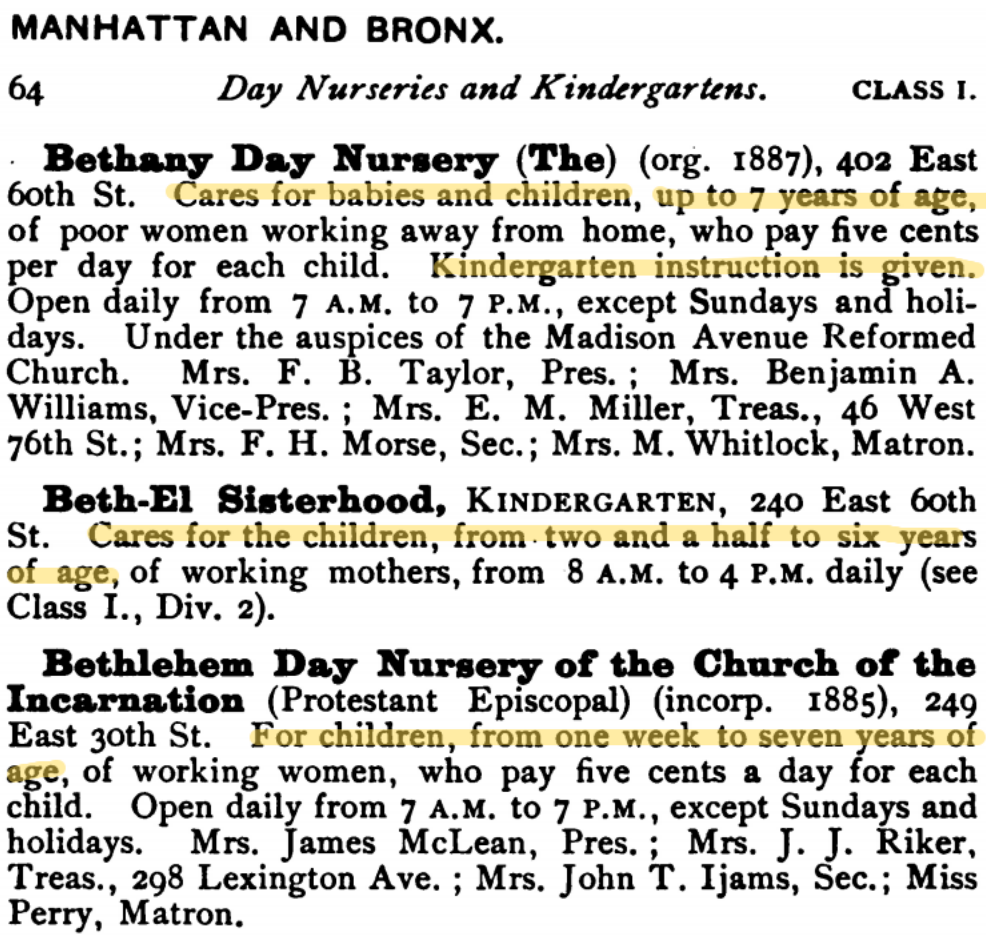
Note.— This figure plots regression coefficients on relative time indicators showing the individual's age when the first Day nursery (with and without kindergarten instruction) appeared within 350 meters from the district centroid. An outcome is the ratio of survived children (IPUMS variables chsurv and chborn). The estimation sample consists of first- and second-generation immigrant women aged 16-35. The regression specification corresponds to 1.

B Appendix. Historical Materials

Figure B1: Early 20th Century New York – Lower East End

Note.— "Cliff Dwellers" by Bellows, depicts the Lower East Side as it was in the early 20th century

Figure B2: NYC Charity Directories – Example of the Data



MANHATTAN AND BRONX.

64 *Day Nurseries and Kindergartens.* CLASS I.

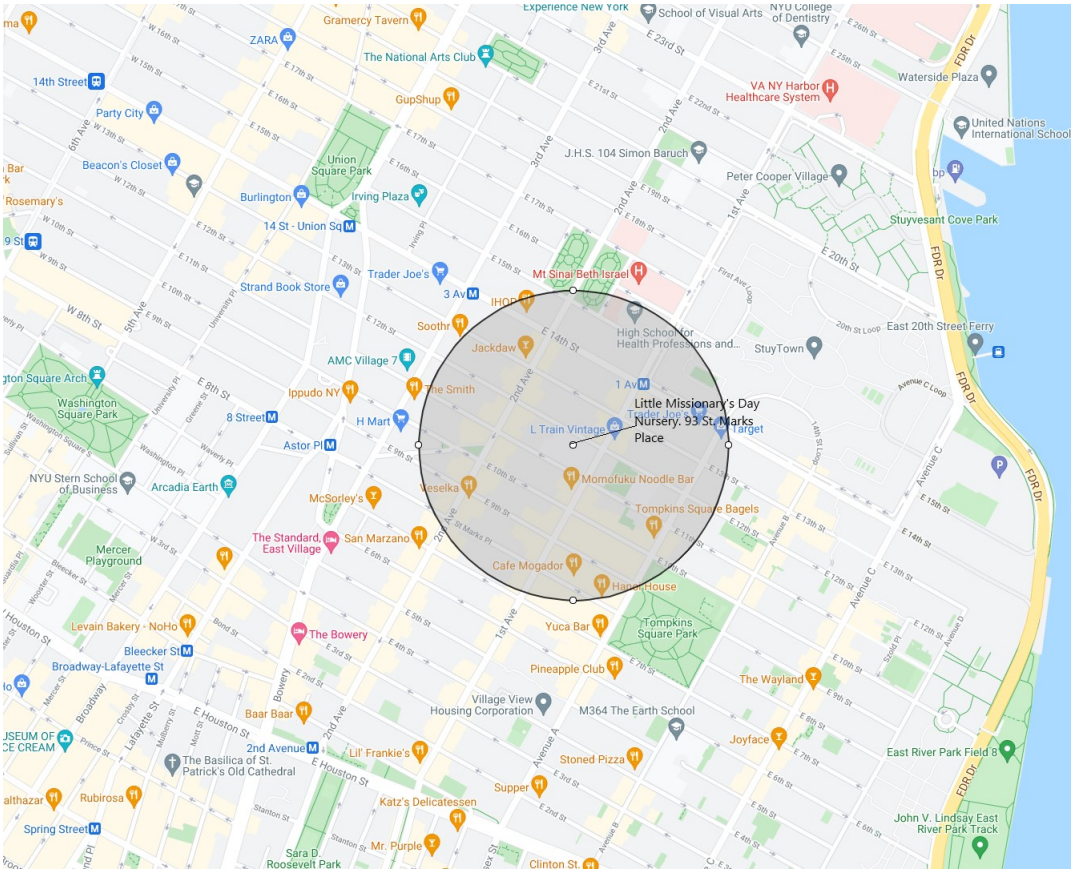
Bethany Day Nursery (The) (org. 1887), 402 East 60th St. Cares for babies and children, up to 7 years of age, of poor women working away from home, who pay five cents per day for each child. Kindergarten instruction is given. Open daily from 7 A.M. to 7 P.M., except Sundays and holidays. Under the auspices of the Madison Avenue Reformed Church. Mrs. F. B. Taylor, Pres.; Mrs. Benjamin A. Williams, Vice-Pres.; Mrs. E. M. Miller, Treas., 46 West 76th St.; Mrs. F. H. Morse, Sec.; Mrs. M. Whitlock, Matron.

Beth-El Sisterhood, KINDERGARTEN, 240 East 60th St. Cares for the children, from two and a half to six years of age, of working mothers, from 8 A.M. to 4 P.M. daily (see Class I., Div. 2).

Bethlehem Day Nursery of the Church of the Incarnation (Protestant Episcopal) (incorp. 1885), 249 East 30th St. For children, from one week to seven years of age, of working women, who pay five cents a day for each child. Open daily from 7 A.M. to 7 P.M., except Sundays and holidays. Mrs. James McLean, Pres.; Mrs. J. J. Riker, Treas., 298 Lexington Ave.; Mrs. John T. Ijams, Sec.; Miss Perry, Matron.

NOTE.— This figure shows a screenshot from Charity Directories and describes three types of institutions. The first type is a day nursery that provides both daycare and kindergarten instruction, admitting children from infancy up to 7 years old (Bethany Day Nursery). The second type is a kindergarten that admits children roughly from 3 to 6 years old (Beth-El Sisterhood). The third type is a day nursery that offers only daycare services for infants and young children, without providing kindergarten instruction (Bethlehem Day Nursery).

Figure B3: Location of the Little Missionary Nursery and surrounding area within 350 m radius



Note.— Since 1896, the Little Missionary Day Nursery has been serving the families of the Lower East Side and the East Village, providing care and education for the young children of this area. It was founded by Sara Curry, who dedicated her life to the care of the neighborhood's impoverished children and their families. Sara Curry (c. 1863-1940) was known locally as The Little Missionary due to both her diminutive stature and her work with the impoverished families of the Lower East Side. She came to New York City from Utica New York in 1896. She started her nursery in two rooms she rented at 204 Avenue C, providing local working parents a place to bring their children where they would be fed and looked after. But her work wasn't restricted to just the nursery. She took it upon herself to go into people's homes and help residents improve their living quarters and aid those who were sick. By 1898 she had moved her nursery to 365 East 10th Street, and her work began to receive attention by the press. With that increased awareness by the public, financial assistance came by way of several wealthy philanthropists.

With the aid of these philanthropists, Sara was able to incorporate the school and purchase the house at 93 St. Mark's Place as a permanent home for the school. Additionally, one of the benefactors donated his home as a summer retreat for sick mothers and their children. An article in 1908 in Harper's Weekly described the scope of the work of the school and the breadth of services.

Here are a few things done during 1907: children received at the Nursery. more than 28,000; poor families visited, 16,000; sick persons nursed, 10,000; families provided with Thanksgiving dinners, 624 (which amounted to 3800 individuals); children and sick mothers received at the summer home at Netherwood, New Jersey, 2423. There were also nightly meetings for girls, boys, mothers, and fathers. All of these services were conducted with the aim to "restore dejected families to moral and physical health and teach them self-support and self-respect" as described in that same article in Harper's Weekly. Sara retired from the school in 1940 and her adopted daughter, Anna Almasy, took over managing the school. Sara died that same year. Well over a century after its founding, the Little Missionary Day Nursery, still located at 93 St. Mark's Place, continues Sara's tradition of serving the community. Source

Figure B4: Little Missionary Nursery - Present View

Figure B5: Fragment of the Article Published in Harper Magazine (1908)

Miss Curry. Soon the work grew, and so many friends rallied to its support that The Little Missionary's Day Nursery was incorporated and installed at No. 93 St. Mark's Place. On that house a mortgage of about $9000 still remains, a heavy burden for which relief is urgently sought. Alexander C. Morgan has provided a summer home at Netherwood, New Jersey, where sick children and their mothers can rest a few weeks in summer.

The ramifications of the work are many, and each one is in consonance with the chief idea—to restore dejected families to moral and physical health and teach them self support and self respect. As good nourishment is the most effective antidote to alcoholism, a fine cooking school is one of the chief features of the Nursery, and in it not only mothers but boys are taught to cook. The good results accomplished are beyond computation.

Here are a few of the things done during 1907: children received at the Nursery, more than 28,000; poor families visited, 16,000; sick persons nursed, 10,000; families provided with Thanksgiving dinners, 624 (which amount to 3800 individuals); children and sick mothers received at the summer home, at Netherwood, New Jersey, 2423.

There are meetings every evening of the week—for girls, for boys, for mothers, for fathers. At one of the latter Mr. Louis B. Rolston, President of the Nursery, told a story illustrating how, by refusing to spend money for drink, a man could save much for food, clothing, rent, etc. A week later Mr. Rolston received the following letter:

"DEAR FRIEND

"Excuse me but I will always look upon you as a friend for you have learned me a valuable lesson in your remarks at Miss Currys last Friday night. I will tell you it might interest you I have been in the habit of buying 10c. worth of whiskey in the evening for years I had the bottle in my pocket at the time you spoke and got my 10c. worth after meeting but when I got home I got

help the needy ones I meet in my visiting. Many a sad and discouraged heart has been lifted up by the substantial help we have been enabled to extend, by God touching the hearts of business men. I am asking God to send us those who will help to pay off the mortgage on our nursery property."

The Boys in this Cooking Class were

Figure B6: Fragment of the Article Published in Harper Magazine (1908)

The Little Missionary and Her Miracles

By
WILLIAM INGLIS

IN a small room on the fourth floor of a crowded tenement near the East River two women were cooking supper by the light of a gleaming kerosene lamp. The stove was newly polished to a degree of brilliance that cheered the eye of the beholder. On three shelves near at hand, which had just been covered with crisp white paper with scalloped edges, rows of clean white plates and dishes were standing. The floor of the small room was bare, but it shone like the deck of a battleship fresh from the holystone. The table in the middle of the room bore a cover of snowy oilcloth, tacked on that afternoon. An appetizing aroma of sizzling steak and fragrant tea made the air delicious and inviting.

The elder of the two women, tall, thin, and bent at the shoulders, suddenly stood erect over the pot in which the potatoes were boiling, her attention arrested by the sound of heavy footsteps ascending the stair. Her eyes were apprehensive and the corners of her mouth drooped.

"That's him!" she cried, anxiously. "Oh, I wonder what'll he say when—"

"Remember, dear," counselled the little woman at her side, "if you are kind to him and show you're not afraid he will surely be good to you."

Miss Sara Curry
"THE LITTLE MISSIONARY"

The tall woman tried to look hopeful, but her lower lip quivered a little. A little boy and a little girl ran into the next room and hid. The heavy footsteps clumped uncertainly to the door, which presently was thrust open by a large and hairy fist. A big longshoreman with a purple neck showing above his blue jumper took two steps into the shining room, sniffed its delightful atmosphere, stared for a moment with dazed eyes at its sparkling cleanliness and the two women in it; then hurriedly turned away.

"Excuse *me*," he apologized as he retreated. "I've come up one flight too high."

"Jim!" the tall woman called after him. "Jim, come on back. Come on."

The big man returned with slow steps. He stood within the threshold and gazed around the room. Mechanically, as from old habit, he tossed his cap into one corner and his empty dinner pail into another, seeming meanwhile every instant to become more puzzled.

"Well," he asked very slowly and without any thought of being profane,—"well—what—the—hell's—been—going—on—here? What does it mean?"

The wife stood dumfounded, but the little woman came forward and looked up at the big man with an expression calm and friendly.

"It means," she said, with a smile,—"it means, my friend, that your home is going to be like this all the time if you care to have it so."

"But where do you come in?" asked the man.

"Oh, I'm a neighbor, and I've been here helping your wife a while," she replied. "I'll see you to-morrow, Mrs. Mannion. Good night."

This incident actually happened a few months ago and was followed by the reclamation of the woman from slatternliness, the man from drink, and their children from a reign of cruelty, ignorance, and neglect which would have either killed them or brought them up to prey on the community. In a word, the family was rescued from hopeless squalor, energized and transformed into a valuable unit of society. And the transformation was accomplished by the expenditure of very little money and a great deal of kindliness. The system whereby the thing was done is unique in the history of benevolent work and well worth studying. It was invented and is now being carried on by one who is known throughout the most congested part of New York as The Little Missionary. Its results, in the physical, moral, and social salvation of families, have been remarkably successful.

On the day before the episode of the startled longshoreman and the renovated tenement his poor wife left her youngest baby at The Little Missionary's Day Nursery, No. 93 St. Mark's Place, so that she might be free to do a day's scrubbing and earn money. Under examination she admitted that she had to work because her husband drank most of his earnings. The Little Missionary called late that afternoon at the woman's home, a tenement of two rooms, and found her frying a steak over a slow fire. For forty-five minutes by the watch she fried and fried and fried.

"What are you going to do with *that?*" the missionary asked, pointing at the hardened remains.

"It's for my husband's supper," the wife replied.

"Huh!" cried the missionary; "I'm not surprised that your husband drinks. I'd drink, myself, if I had to live on such food. My dear, I'll come in to-morrow, and we'll see what can be done. You know, if a man doesn't get good, well cooked food he's bound to take to drink. We'll change him."

On the morrow the missionary brought a pail, a bar of soap, a scrubbing brush, and other cleaning materials, besides a steak, potatoes, and half a pound of tea. She did not show the dejected wife how to clean the home, but on her own knees scrubbed floor and woodwork. She put the fresh crimped paper on the shelves, washed the dishes and polished the stove, while the wife looked on in amazement at a lady doing such things. And as the missionary worked she chatted—pleasantly enough, but always with a fixed purpose.

"Your husband, my dear, really has a great deal of good in him,"

The Nursery Building and one of the Kindergarten Classes