THE LONG-TERM RETURN TO EARLY CHILDHOOD EDUCATION: EVIDENCE FROM THE FIRST US KINDERGARTENS

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JOB MARKET PAPER

Abstract: Public investment in universal early childhood education programs is increasing rapidly in many countries, yet the benefits of these programs are not well documented. This paper studies the long-term effects of one of the first early education programs in the US – the Kindergarten Movement (1890-1910). I collected unique data on the opening of public kindergartens across cities in the US during this period. I then link over 100,000 children living in these cities to subsequent Censuses where their adult outcomes can be observed. I find that kindergarten attendance had large effects on adult outcomes. On average, the affected cohorts had about 0.6 additional years of schooling and six percent more income (as measured by occupational score). These effects were substantially larger for second generation immigrant children. The effects of this early intervention are most likely due to language acquisition and the attainment of various "soft skills" early in childhood.

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I. Introduction

Public investment in early childhood education programs is increasing rapidly in most OECD countries. For instance, the Obama administration's "Preschool for All" initiative has budgeted $75 billion over the next decade in order to expand the supply of preschool education to both poor and middle-class children in the United States. The theoretical argument for these investments is very intuitive (Heckman et al., 2010). Key cognitive skills (such as mathematical reasoning and language skills) and non-cognitive skills (like sociability and discipline) are

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thought to be more easily developed at early ages. Moreover children that start school with low levels of cognitive and non-cognitive skills may learn less thereafter as a result. Thus any initial skill gap may widen over time if initially disadvantaged children are not able to benefit fully from each stage of the educational system. This cumulative skill gap will eventually be reflected in lower quality employment opportunities and lower wages in adulthood.

Although the theoretical case for investment in early childhood education is strong, most of the empirical evidence on the long-term effects of pre-school attendance is based on small samples (see, for instance, Barnett and Masse, 2007; Heckman et al., 2009; and Anderson, 2008). In addition, studies tend to focus on high-quality model programs that are targeted to the poor, such as the Perry pre-school study, which may not generalize to the full population.

This paper studies the long-term effects of one of the first early education programs in the US – the Kindergarten Movement (1890-1910). During this period, hundreds of cities and towns built their first public kindergartens in order to help children in their transition from home to school. These early kindergartens, which were typically available to students between the ages of four and six, resemble pre-school programs today, in that they focused on socialization and play rather than academic training (such as basic arithmetic, writing, reading, etc.). Because the children who benefitted from these programs were born before 1910, I can follow them over time using historical Census data and study how the kindergarten affected their long term outcomes. To do so, I link over 100,000 white children living in cities that opened public kindergartens to subsequent Censuses where their adult outcomes can be observed. I then estimate the effects of kindergarten on occupational earnings and highest grade of completed education by comparing the cohorts that were eligible to attend kindergarten, with those that were slightly older and therefore just missed the enrollment cutoff.

There are a number of advantages to studying the Kindergarten Movement in this period. First, the early twentieth century was an era of tremendous improvement in the human capital of the US labor force. At the same time, the US was catching up to and overtaking the leading European economies in terms of GDP per capita. The US advantage in human capital acquisition was partially due to the High School Movement, which resulted in near-universal secondary

school attendance (Goldin, 1998; Goldin and Katz, 2008). Investment in kindergartens was another potentially important and understudied component of this human capital revolution.

Second, in this historical setting, I am able to rule out certain mechanisms that might explain the positive relationship between kindergarten attendance and adult outcomes in contemporary data (e.g., Cascio, 2009; Havnes, et al., 2011).[1] Today, sending children to pre-school or kindergarten often frees up mothers to re-enter the labor force, resulting in an associated income effect in the household.[2] Yet, in this period, the labor force participation rates of married, white women was negligible (less than 5 percent), suggesting that any income effect is likely to be small. Furthermore, knowing that mothers were the most likely care providers before the Kindergarten Movement allows me to provide a clear interpretation of the estimates. My estimates indicate the value of kindergarten attendance relative to staying home with a family member, whereas contemporary estimates compare kindergarten attendance to a combination of private daycare and family care.[3]

Finally, studying the Kindergarten Movement allows me to assess the role of early education programs in the assimilation of second-generation immigrants. The Kindergarten Movement coincided with the mass arrival of immigrants from Europe, many of whom had very limited English skills. Kindergartens provided an opportunity for children of immigrants to have early interactions with native children and adults through a simple play-based curriculum. This environment may have fostered the development of language skills at a critical age, allowing children of immigrants to start elementary school with a smaller language handicap (as most of the anecdotal evidence suggest).[4] Indeed part of the motivation for the expansion of

[1] Cascio (2009) studies the introduction of state-level grants funding kindergarten education in the United States during the 1960s and 1970s. She finds evidence of positive effects only for whites and then only for two outcomes (the probability of being a high school drop-out or institutionalized in adulthood). However, she finds no effect on years of completed schooling, earnings, employment and public assistance receipt. These mixed results could be explained by the large crowding-out rates that she estimates for both private kindergartens and Head Start. On the other hand, Havnes et al. (2011) study the long-term effects of a childcare reform in Norway in 1975. They find stronger evidence of positive effects on both educational and labor market outcomes, perhaps because, in this context, the increase in formal childcare largely displaced informal care arrangements (e.g. babysitters).

[2] In addition, providing free public kindergartens could generate an income effect today by reducing expenditures on private childcare arrangements. In the past, few households paid for private childcare.

[3] The predominance of maternal care was reinforced by both the limited supply of private kindergartens and the prevalent philosophical view that it was the mother's responsibility to educate young children. (See for instance Shimoni, 1990).

[4] Indeed, language skills fit very well the dynamic model of skill formation developed by Heckman et al. (2010) summarized above. Studies have shown that children can more easily learn a foreign language at early ages

kindergartens was to assimilate new immigrants into American society. Studying the long-term effects of "early Americanization" interventions remains important for countries such as the US that have large immigrant populations.

I study the long-term effects of kindergarten education by linking city-level data on the timing of kindergarten construction to census data on adult outcomes. I collected unique data on the dates of kindergarten construction and kindergarten enrollment from different reports of the Bureau of Education. Data on final educational attainment is drawn from the recently assembled complete-count of the 1940 census, the first census to collect data on the highest grade of education completed. Data on labor market outcomes come from the 1900-1940 census samples.[5] Census observations during adulthood do not contain information on the detailed location of birth or of residence during early childhood, which is a key variable for my identification strategy. In my main estimation sample, I assume that individuals were born in the town in which they currently reside, but I check the validity of this assumption by matching individuals to an earlier census wave to observe their city of residence during childhood.[6]

I estimate that, in the average city, enrollment in public kindergartens grew by 26 percentage points in the three years following the construction of the first public kindergarten.[7] To identify the effect of exposure to kindergarten, I exploit this sharp variation (within cities and cohorts) in the number of public kindergartens available at relevant ages. Within each city or town, I compare cohorts that were slightly older than the entry age cutoff when a public kindergarten was first introduced (and hence were not able to attend), with cohorts that were

(Bleakley and Chin, 2004, 2008, 2010). Severe language deficits at the beginning of elementary school may translate into poor performance in the first years of education, which are believed to be crucial for future success in both later stages of the educational system and the labor market. In other words, a language handicap could severely affect the child's readiness to learn at the beginning of elementary school with negative consequences for long-term outcomes.

[5] I use repeated cross-sections for labor market outcomes in order to be able to control for both age-earnings profiles and national trends.

[6] Due to sample size issues this later sample is not my preferred sample. I use a standard matching algorithm based on first and last name, age, and state of birth to match individuals from the 1900 and 1910 Census samples to the 1940 complete-count Census (see, for example, Ferrie, 1996; Abramitzky, Boustan and Eriksson, 2012; Ferrie and Long, 2013).

[7] I estimate the effect of kindergarten construction in a sample of small- and medium-sized cities and towns (i.e. below the (weighted) population median of the cities with kindergartens). On the contrary, the date of the construction of the first public kindergarten is not associated with enrollment gains in large cities. Cities like Chicago and New York City introduced public kindergartens in a very slow and experimental way. In New York City, for instance, 10 years after the first public kindergarten was built, enrollment was still less than 5 percent. For this reason, I excluded the cities above the population median from the analysis.

slightly younger. The two key identification assumptions are that there are no preexisting trends in child well-being in the cities that built kindergartens, and that kindergarten construction was not correlated with other policies that differentially affected children of kindergarten age. I carry out several falsification experiments that provide strong evidence that these identification assumptions are valid.

I find that kindergarten attendance had a large effect on adult outcomes. On average, the affected cohorts had about 0.6 additional years of schooling and six percent more income (as measured by occupational score). Furthermore, the estimated effects are at least twice as large for children whose mother came from a non-English speaking country. These children gained about 1.1 additional years of schooling and 15.5 percent more income with exposure to kindergarten. Previous research indicates that the returns to schooling for immigrants were close to 14 percent during this period (see Lleras-Muney and Shertzer 2014, and Clay, et al., 2012). Thus a back-of-the-envelope calculation suggests that almost all the income gains are explained by the effect of kindergarten attendance on the highest grade of completed education.

These findings have important policy implications. The results indicate that even a simple play-based early intervention, with no indirect income effects, can have a very large returns, particularly for non-native speakers. Indeed, other policies of the time that also were aimed at the assimilation of immigrants had substantially smaller impacts on adult outcomes. For example, "English-only" laws – that required English as the exclusive language of instruction of schools – had negligible effects on immigrants' educational and labor market outcomes (see Lleras-Muney and Shertzer, 2014), possibly because these laws targeted older children.

The rest of the paper is structured as follows. Section II describes the Kindergarten Movement. Section III documents my main data sources for both adult outcomes and kindergarten education. Section IV explains my identification strategy. Section V analyzes the main empirical results. Section VI performs several robustness checks. Section VII discusses the main mechanisms that could drive the results. Section VIII concludes.

II. The Kindergarten Movement

A. Historical Background

The concept of a kindergarten was first conceived in Germany in 1837 by Friedrich Fröebel. The literal meaning of the German word kindergarten – "garden of children" – accurately captures the philosophy of the first kindergartens, which were aimed at providing a safe environment where children could grow and develop. The original kindergarten curriculum was play-based with a large emphasis on socialization.

In the US, the first kindergartens were introduced by German immigrants around the year 1860. The main objectives were to help the socialization of the immigrants' children and to preserve German culture and language. As German Kindergartens grew in the US (and in Europe), they captured the attention of several educators and superintendents of schools. The city of Saint Louis, Missouri—which received a large inflow of German immigrants between 1860 and 1870—was the first city in incorporate kindergartens into the public educational system in 1873 (Shapiro, 1983). The superintendent of the city's schools, William Harris, was very attracted by the idea of smoothing children's transition from home to school. Indeed, he described kindergartens as a "transition between the life of the family and the severe discipline of the school" (Beatty, 1995).

However, at the national level, Kindergarten attendance remained negligible until 1890. A national Kindergarten Movement, led by women's associations, educators and superintendents of schools, gained strength at the turn of the century (for instance, see Shapiro 1983, and Bryant et al. 1992). Through educational magazines, conferences, fairs and expositions, the Movement successfully advocated for the full integration of kindergartens into the public school systems. As a result, during the years 1890-1910 there was a boom in kindergarten enrollment in specific cities, largely fostered by the construction of public kindergartens.

Figure 1 shows the evolution of kindergarten enrollment at the national level for children aged 4 to 6 (the target age group). Before 1890, national kindergarten enrollment was around 1 percent but it reached almost 8 percent by 1912. This percentage masks substantial heterogeneity across states and cities. As shown in Figure 2, 10 states lead the Kindergarten Movement with

enrollment rates of between 15-30 percent (CA, NJ, DC, NY, WI, CT, RI, MI, IL and CO), while more than half of the states had enrollment rates below 4 percent. These differences are even more striking at the city level. By the year 1912, only 852 cities and towns had kindergartens integrated into the public education system, with an estimated median enrollment rate in these early-adoption cities of 47 percent (see right panel of Figure 2).[8]

The typical kindergarten targeted children aged 4 to 6. Most kindergarten teachers were high school graduates with two years of specific training that included children psychology, music, and children literature. Kindergarten sessions lasted for 2-3 hours and were typically carried out in the mornings.

Kindergarten was not conceived as an extension of the elementary grades but as an intermediate step between home and school. The key distinctive characteristic of the curriculum was the large emphasis on socialization. Through a "play-based" program, children were expected to learn from the interaction with other children and adults, and to develop their creativity through "self-chosen activities."[9] In the process of incorporation to the public schools, additional goals were added, including the inculcation of cultural values and norms, and the improvement of children discipline (Lee, et al., 2006; Bryant and Cliffort, 1992). The emphasis on play activities and child interaction is well captured in this extract from a report of the Bureau of Education (1920)[10]:

> *A large part of kindergarten education consists in furnishing the right kind of play material and the boys and girls to play with. The ability to work and play with other people, respecting their rights and enjoying their companionship, is one of the most valuable lessons anyone can learn. No child can be educated alone. (…) Teach children by children!*

Not only did early kindergartens rely on a play-based curriculum, but the basic academic training included in many modern kindergarten classrooms - which emphasizes math reasoning, reading and writing skills - was believed to be detrimental for children during early years of life, and thus was strongly rejected by the advocates of the kindergarten movement (see Lee et al,

[8] Author's calculations based on United States Bureau of Education (1914).
[9] Examples of the activities carried out encompass playing group games, listening to stories, singing songs, learning manual arts, playing with didactic toys ("gifts"), etc.
[10] *"The Child and The kindergarten,"* Julia Abbot, Bureau of Education (1920).

2006). These academic activities would be only incorporated into the curriculum after the 1960s.[11]

Kindergartens were also considered a powerful tool for the assimilation of immigrants, in particular for those coming from non-English speaking countries. It was argued that young children had personal traits that were "still plastic" and that they could be easily "molded as to grow up Americans, to absorb by natural process, by normal unconscious assimilation" the American culture and values (Beatty, 1995). In fact, many kindergartens included specific activities aimed at this goal such as listening to patriotic stories, singing national songs, conducting exercises with the flag, and so on.

In addition, early access to the English language was expected to improve immigrants' communication skills before the advent of formal academic training, providing them "a fair start." This benefit of kindergarten education was emphasized in a Bureau of Education study:

> *"The kindergarten is the best place to begin the removal of these language handicaps. Probably more can be accomplished in this during a kindergarten year than in any subsequent year. This initial achievement gives the child of foreign parentage something like a fair start." Bureau of Education, 1922*

The emphasis of the curriculum on soft-skills and language over academic training is also manifested in survey about the benefits of kindergarten education carried out in 1915 (see Palmer, 1915). In this survey, primary teachers and superintendents reported that the child trained in the kindergarten shows an advantage over the non-trained child in several dimensions. The top answers included the formation of good school habits, (e.g. regularity, punctuality, capacity of paying attention, ability to work with other children, etc.) and fluency in language. On the contrary, less than 10% of the teachers and superintendents reported that children that attend to kindergarten were able to "read and write more quickly" (see Figure 3).[12]

[11] Interestingly, the modern emphasis on the importance of early interventions to develop non-cognitive skills was already stressed by advocates of this movement. For instance, a report of the Bureau of Education pointed out that kindergarten protect a child from "the regressive tendency toward anger, self-feeling, suspicion, isolation, sullenness, and nervousness, and fosters good nature, open-mindedness, sociability, self-confidence, cheerfulness, and the habit of being happy." (Abbot, 1923).

[12] It is important to notice that contemporary teachers' surveys also stress the importance of soft skills for child's "readiness to learn" (see for instance Heaviside and Farris, 1993)

B. Who attended kindergartens?

Although kindergartens were not targeted to particular socio-economic groups (see Beatty, 1995), enrollment was far from universal. By 1912, the median enrollment of children aged 4 and 5 in cities with kindergartens was about 47 percent. Given that kindergarten attendance may have been particularly beneficial to the children of immigrants, we may expect that enrollment would be highest in immigrant households. In order to assess the determinants of enrollment, I examine the effect of access to a public kindergarten on enrollment of children in different age and socio-economic groups.

Specifically, I collected data on the number of public kindergartens in 1912 in each city or town and linked this data to the 1910 1 percent IPUMS sample by city name. My analysis proceeds in two steps. First, I confirm that the density of kindergartens in a city only affects the enrollment of relevant age groups (that is, 4 and 5 year olds). Second, I test whether the "program take-up" was heterogeneous by family background. In particular, I estimate the following linear probability model:

$$I(enrolled_{iacs} = 1) = \sum_a D_a.(\#\ of\ Public\ Kindergartens/pop;_c)\ .\ \boldsymbol{\beta_a}$$

$$+ \alpha_a + \delta_s + f(a, X_c) + \varepsilon_{iacs} \quad (1)$$

where i indexes a child of age a, in city c, and state s, *I(enrolled=1)* is a dummy variable that equals 1 if the child attended any educational institution in the academic year,[13] D_a is a dummy that equals 1 if the children was age a at the beginning of the academic year,[14] and $\#\ of\ Public\ Kindergartens/pop$ measures the number of public kindergartens per thousand inhabitants. I also include both age and state fixed effects. Given that my main variable of interest (*# of Public Kindergartens*) is divided by the city population, I include a fourth order polynomial for the population size interacted with a full set of age dummies to deal with potential model misspecification.

[13] Although the specific question was "Attended school any time since September 1, 1909", "school" was defined as any school, college, or educational institution.

[14] Since the 1910 census was carried out in April 15th, the best proxy for age at the beginning of the academic year = age - 1

The coefficient of interest β_a captures the effect of new kindergarten construction on the probability of being enrolled in "any educational institution" by age *a*. These coefficients are plotted in Figure 4. The first interesting results is that the kindergarten stock only appears to affect the enrollment of targeted children (i.e. those aged 4 and 5), which suggest that other educational policies (for example, the construction of high school buildings) do not seem to be correlated with the construction of kindergartens—these other education policies would have presumably affected older as well as younger children.

To test for the presence of heterogeneous effects on enrollment, I interact a dummy variable for being in the relevant age range (4 or 5 years old) with two indicators of socio-economic background: a dummy equal to one if the child's father's occupational score is below the population median and a dummy for being a second-generation immigrant. The first column of Table 1 reports the main effect (i.e. with no interaction) and the results imply that building one kindergarten per thousand of inhabitants increases the likelihood of school attendance for 4 and 5 year olds by 44 percentage points. Columns 2 and 3 show that neither father's occupation nor mother's birthplace are statistically significantly correlated with kindergarten attendance. However, the coefficient corresponding to the latter interaction is large and negative, suggesting that the increment in the kindergarten enrollment was 12 percentage points smaller for second generation immigrants.

III. Data

A. Kindergarten data

I collected data on kindergarten construction and enrollment from three different set of reports published by the Bureau of Education. First, I collected data from a kindergarten survey carried out in 1912 at the city/town level. This survey included data for all the cities and towns with public kindergartens by that year, including the year that the first public kindergarten was established (a key input for my empirical strategy), the number of public kindergartens and children enrolled in the city, the number of teachers, their minimum and maximum wages, the formal training required, and so on.

Second, I collected data on enrollment and the number of public kindergarten schools during the period 1890-1910 from the statistical tables of the city schools systems.[15] This data is useful for two reasons. First, it allows to estimate the immediate increase in enrollment after kindergartens are first incorporated into the public school system (i.e. how quickly did cities build kindergartens). Second, it allowed me to verify the reported year of kindergarten incorporation in the 1912 survey. For the most part, these reports collected data only for cities and towns larger than 4,000 inhabitants.

Third, I collected data on both public and private enrollment in kindergarten (at the state level) in the period 1897-1912 from statistics assembled in the corresponding Reports of the Commissioner of Education. From this data, I estimated the state-level crowding-out rates as the share of the increase in public enrollment that was compensated by a decrease in private enrollment. Figure 5 reports the rate of private crowding out for the 25 states with largest increase in public enrollment during the period.[16] Whereas states like New York and New Jersey show a crowding-out rate of about 2 percent, in states like California and Minnesota this rate rise to around 50 percent. In order to have a more clear interpretation of my results, I drop from my main sample the states with the largest crowding-out rates.

Sub-sample of cities with good data on kindergarten construction

Combining the data on kindergarten construction and enrollment above, my main analysis sample consists of 220 small and medium cities with consistent data. I focus on cities in states with low crowding out rates of private kindergartens. The robustness section considers a set of alternative samples.

In the main sample, I excluded the cities/towns for which: (a) the year that the first kindergarten built is missing (about 170 places); (b) population was below 4,000 residents- (around 300 places); (c) the reported year of first kindergarten construction in the 1912 survey was inconsistent with the enrollment statistics of the city school system (around 90 places); or (d) the states had a high crowding-out rates of private kindergartens (around 30 places). Finally, I

[15] For several years: 1890, 1892, 1897,1901,1905, and 1912

[16] For each state I estimated the crowding-out rates of private kindergarten enrollment as the ratio between the the reduction in private kindergarten enrollment and the increment in public kindergarten enrollment ($\nabla\, priv.\, enrollment / \Delta pub.\, enrollment$) in the period 1897-1912.

also dropped the largest cities in my sample, i.e. those with a population above the weighted population median (around 40 places), since most of those cities introduced kindergarten very slowly.[17]

B. Outcomes

The study focuses on white males born in the United States.[18] I evaluate the impact of kindergarten exposure on two outcomes: (a) occupational based earnings, and (b) highest grade of education. The first outcome is evaluated using pooled cross-sectional samples of the 1900-1940 censuses. With repeated cross-sections, I can control for both the earnings age profile and time trends. The impact on highest grade of completed education is assessed using the 1940 full census count, which is the first census that collected data on this variable.[19]

B.1 Occupational earnings

Occupational earnings ("occupational score") are computed by IPUMS as the median income for an individual in a given job category in 1950. To study the impact of kindergarten exposure on occupational earnings, I pool five cross-sectional samples of the 1900-1940 censuses using the IPUMS public use samples and the sample line of the 1940 full census count (described below).

I restrict the sample to white males aged 25 to 45. I link these observations to the kindergarten data using their current city of residence ("adult city" from now on). Using the adult

[17] Knowing the year that the first kindergarten was built does not provide useful information for most of these cities. As will be discussed in the identification section, my empirical strategy exploits a sharp variation in the stock of public kindergartens. However, this was not the case in the largest cities of the country. Cities like Chicago and New York City (NYC) introduced public kindergartens in a very slowly and experimental way, probably because they were among the pioneers and for coordination issues. In NYC, for instance, 10 years after the first public kindergarten was built, enrollment was still less than 5%. On the other hand, in cities with a population below the median (i.e. below 130,000 inhabitants), I estimate that enrollment grew about 26 percentage points in the next 3 years that followed the introduction of kindergartens. All the results are robust to using alternative population cutoffs (e.g. 200,000 inhabitants, 150,000 inhabitants, 100,000 inhabitants, etc.) and are available upon request.

[18] Black males were excluded from this project. They only represent around 5% of the population in the cities considered.

[19] The cohorts studied in this paper are aged 30 to 66 by 1940. Hence, given that for most the people the education process already finished by age 25, it is possible to control for trends using the individual age and a single cross-section. For labor market outcomes, this is not the case. Furthermore, the strong correlation between age and missing data around age 50 to 66, makes the single 1940 cross-section inadequate to study the impact of kindergarten exposure on labor market outcomes (these statistics are available upon request). With the repeated cross-sections 1900-1940, however, I can observe the cohorts when they are younger. In particular, I can restrict the sample to people aged 25 to 45 to limit the correlation between age and entry/exit from the labor market.

city to measure exposure to kindergarten could bias my estimates either upward or downward. The coefficients will be biased upward if residents who stood to benefit the least from kindergarten were the more likely to migrate in adulthood (for example, children with educated parents). On the contrary, the estimates will be biased downward if residents who stood to benefit the most from kindergarten were more prone to migrate or even if migration is "random" (i.e. by the attenuation bias implied by migration). I am able to address selective migration in my linked sample, which is described below.

I drop white males residing in cities and towns that did not have a public kindergarten by 1912. In addition, I exclude men who were born outside their current state of residence to limit concerns about using adult city as a proxy for childhood location.[20]

B.2 Highest grade of education

As mentioned above, the 1940 full census count is the first census to collect data on the highest grade of education "attended or completed." Given the large number of observations in this census (more than 140 million of observations), I can use two alternative methods to link individuals to the kindergarten data. First, as I do with the IPUMS samples, I link individuals to the kindergarten data using the contemporaneous city of residence (i.e. the "adult city"). Second, to deal with potential selective migration, I match individuals from the 1940 census to either the 1900 or the 1910 full census counts to identify the location of their childhood household.[21] Matching across census waves is conducted by first and last name, age and state of birth (the matching algorithm is described in the web appendix I).

IV. Identification strategy

My empirical strategy exploits sharp variation (within cities and cohorts) in the number of local public kindergartens. For the typical city in my sample, I estimate that kindergarten enrollment grew by 26 percentage points in the three years following the construction of the first

[20] Although the highest grade of completed education was not collected in 1900-1930 census samples, they include data on literacy skills. However, literacy is a very poor measure of educational attainment during the period studied, especially for those individuals born in the US. In fact, about 99% of the sample analyzed reports having literacy skills.

[21] Identifying small cities in the 1900/1910 full census counts is more complex than in the IPUMS samples because the names of incorporated municipalities were not digitized in some cases. I describe an algorithm to identify such cities and towns in appendix I.

kindergarten. Within these cities, all of which had kindergartens, I compare cohorts that were slightly older than the entry age cutoff at the time when kindergartens were introduced (and hence not able to attend), with those that were slightly younger. The fact that there was substantial heterogeneity in the timing of kindergarten construction across cities allows me to control for any national policies that may have targeted the cohorts eligible to enter kindergarten. Furthermore, even if cities made other investments at the same time as they started public kindergartens (for example, building high schools or hospitals), these new institutions would likely affect both the "control" and the "treatment" age-groups.

Figure 6 illustrates the timing and geographic variation in kindergarten exposure for a sample of cities in New York state. Two key points can be seen. First, even within the same state, there was substantial heterogeneity in the timing of kindergarten construction. For instance, whereas Port Chester built the first public kindergartens around 1890, Kenmore established the first kindergartens only in 1910. Second, the increase in enrollment in the years following the incorporation of public kindergartens was very rapid in many cities, ranging from 20 percentage points to 80 percentage points in the cities included in the figure.

In a typical town, local children between the ages of four and six were allowed to attend the new public kindergarten. In theory, then, the first cohort to be fully exposed to kindergarten was four years old when the first kindergarten was built, and the last cohort to miss out on kindergarten attendance was six years old at the time. However, in my benchmark case, I allow for a +1/-1 measurement error in the year that the first public kindergarten was built, thereby excluding children who were between the ages of four and six in the year that the kindergarten was reportedly incorporated.[22] Nevertheless, I show in the robustness section that the key results do not depend on excluding these "noisy cohorts."[23]

Formally, I construct a dummy variable named $Exposed\ to\ Kindergarten_{iiac}$, which measures a cohort's "exposure" to kindergarten education in city c in the following way. Let

[22] This one-year band also allows for measurement error in reported age for children in the Census. In web appendix II, I show that there was some measurement error in both the year that the first kindergarten was established and children's age (in part because the most censuses do not collect data on month of birth).

[23] In the robustness section, I assume that (a) there was no measurement error neither in age nor in the year of kindergarten incorporation, (b) children aged 4 to 6 faced a probability p of receiving kindergarten training, which p decreasing in age. All the key results are robust to these scenarios.

$Year\_K_c$ be the year that kindergartens were incorporated into the public education system in city c. Then:

$$Exposed\ to\ Kinder._{iac} = 1 \text{ if the children turned 4 in } [Year\_K_c + 1\ ;\ Year\_K_c + B\ ]$$

$$= 0 \text{ if the children turned 6 in } [\ Year\_K_c - \mathrm{B}\ ;\ Year\_K_c - 1] \quad (2)$$

with B equal to 5 in the benchmark case. This is illustrated in the left panel of Figure 6. For instance, suppose that the first kindergarten in a city was built in the year 1890. In this case, all children who turned 4 between the years 1891 and 1895 are considered to be fully exposed to kindergarten, while all children who turned 6 in the years 1885 through 1889 before the kindergarten was built were not exposed to kindergarten.

Cleary there is a tradeoff regarding the choice of bandwidth B. A very large B would raise concerns over the comparability of the cohorts, but a small B may not allow for enough time for the town to build a significant number of public kindergartens, and would heavily rely on the accuracy of reported ages. In addition, B must be large enough to allow sufficient power to study heterogeneous effects on a particular small subsample (second-generation immigrants whose mothers were born in Non-English speaking countries). I deal with the concerns regarding the comparability of the cohorts by considering alternative values for B in the robustness section.

For the analysis using the full census count data, I estimate the following equation:

$$Y_{iac} = \alpha_c + \beta.Kinder_{iac} + \sum_j \gamma_j\ (age_{iac})^j + \varepsilon_{iac} \quad (3)$$

where i indexes children of age *a* in city *c*, Y is a long-term outcome, $Kinder$ is a dummy equal to 1 if the cohort was exposed to kindergarten (as defined above), α_c is city fixed effect, and ε_{ics} is an error term. I also fit a j^{th} order polynomial in age to control for any non-linear trends in the outcome Y by age.

For the analysis using the 1900-1940 cross-sectional samples, I estimate the following model:

$$Y_{iacst} = \alpha_s + Kinder_{iacst}.\beta + \sum_j \gamma_j\ (age_{iac})^j + \delta_t + \gamma X_{(1880)c} + \varepsilon_{iacst} \quad (4)$$

Given the smaller sample size (about 6 percent of the 1940 full count sample), I control for state fixed effects instead of city fixed effects.[24] In lieu of city fixed effects, I control for a set of county characteristics in the year 1880 (such as median occupational score and average school enrollment). This specification also includes year fixed effects. Finally, I restrict the sample to people aged 25-45 to limit potential bias due to the correlation between age and entry/exit from the labor market.

A. Which cities built the first public kindergartens?

Part of my identification strategy exploits heterogeneity in the timing of public kindergarten construction across cities. Therefore, I investigate the characteristics of cities that are correlated with early kindergarten provision. In particular, I evaluate characteristics of cities in 1880, 10 years before the Kindergarten Movement gathered strength. I find that the average income of the cities (as measured by the median occupational earnings) was not correlated with the year of incorporation. However, places with a larger share of immigrants and bigger cities were more likely to build early public kindergartens.

Two channels could explain why cities with a large immigrant share were first to establish kindergartens. First, city officials may have been influenced by a demonstration effect linked to the fact that immigrants (in particular German immigrants) were usually the first in establish private kindergartens. Second, as explained before, kindergartens were considered a powerful tool for the Americanization of immigrants, and hence the demand for "early Americanization" was potentially larger in these cities and towns.[25]

Larger places may have been more likely to construct kindergartens because the conferences and expositions at which the idea was first promoted were carried out in large cities (see Vandewalker, 1908). In addition, to some extent, kindergartens were designed to provide a safe environment for urban children to play who otherwise might be unsupervised on the streets.

[24] Since I focus on small and medium cities (for identification purposes), the number of observations per city or town is small in the IPUMS samples. In a typical place there are around 50 observations in the relevant age range in the 1900-1940 data (i.e. 10 observations per city/year cell)

[25] Additional regressions not reported here indicate that, on average, the demand for "early Americanization" might be stronger since when breaking down the share of immigrants on German immigrants and non-German immigrants, the coefficients are larger for the latter share. Other possibility is that towns with a large enough German population might organize the private provision of kindergartens (reducing the demand for public kindergartens)

Nevertheless, the fact that larger places or places with more immigrants were more likely to build kindergartens earlier is not threat to my identification strategy because my results are robust to including city fixed effects. Further, although the population size in 1880 is correlated with kindergarten, population growth between 1880 and 1910 is not correlated with the timing of incorporation. Finally, all results are robust to dropping the largest and smallest places of my sample.

V. Results

Table 2 reports estimates of the long-term effect of being exposed to kindergarten education on adult outcomes for my main sample. Columns (1) and (2) consider the relationship between kindergarten exposure and occupational earnings in the pooled cross-section. The first column shows that being exposed to kindergarten education increases occupational earnings for the average resident in the relevant age cohort by 1.5 percent. However, the results are heterogeneous by mothers' language. Whereas there is no significant impact on males whose mother's first language is English (either because she was native born or because she was born in an English-speaking country), earnings are about 4 percent larger for those whose mothers come from non-English speaking countries (column 2).

Columns (3)-(5) report the impact of kindergarten exposure on the highest grade of completed education using the complete count 1940 Census. In column (3), I match individuals to their likely kindergarten exposure according to their current location("adult city"). I find that kindergarten exposure increases the highest grade of completed education by 0.11 grades. Column (4) instead uses the linked census sample to match individuals to kindergarten construction in their childhood place of residence ("childhood city"). In this case, I instead find that exposure to the treatment increases the highest grade of completed education by 0.18 grades, suggesting that estimations based on the "adult city" are probably biased downward due to the measurement error associated with migration.

In the last column of the table, I assess whether the impact on educational outcomes is also heterogeneous by the language spoken at home. I find a large effect of kindergarten exposure on final educational attainment for children whose native language is English (0.14

additional grades of completed education), but the impact is twice as large for those children whose mother's first language is not English (0.29 grades).

The previous results correspond to intention-to-treat effects (ITT)—that is they capture the effect of having kindergartens in a city at the right time—they do not estimate the effect of kindergarten attendance. In order to get a measure of the treatment effects, I re-scale the coefficients taking into account the fact that only a fraction of the children was able to attend to the new public kindergartens. In other words, I divide the ITT effects by the average increase in kindergarten enrollment faced by the exposed cohorts (about 26 percentage points). The re-scaled estimates indicate that children with mothers coming from non-English speaking countries gained 1.1 grades of completed education and 15.5 percent more occupational income. On the other hand, children whose first language is English gained 0.52 grades of completed education, which is similar to other estimates from the literature (see Havnes et al. 2011, and Galiani et al 2008).

For non-native speakers, if it is assumed that all the increase in earnings is driven by the additional grades of schooling, the implicit returns to educations are 14%. Other estimates of the return to education for a similar population and historic period have found identical returns, suggesting that 100% of the earning increment is driven by the better performance in school (see Lleras-Muney and Shertzer 2014 and Clay, et al., 2012).[26]

Potential threats to validity: Pre-existing trends

One of the main threats to my identification strategy is that exposure to kindergarten (β in equation 4) might be correlated with pre-existing trends at the local level that particularly affect younger cohorts. For instance, within a given city, younger cohorts might have had higher education levels than older cohorts (beyond national trends by age) due to other polices that were expanding around the time of the Kindergarten Movement, such as public health and sanitation

[26] Nevertheless, it is likely that I am underestimating the earnings effects by using the "adult city" instead of the "childhood city". Indeed, Table 2 indicates that the impact on highest grade attained grows from 0.11 grades to 0.18 grades when I recover the childhood city (columns 3 and 4). If we assume a similar "underestimation rate" for occupational earnings (i.e. 0.11/0.18), then around 60% (instead of 100%) of the earning increment would be driven by the improvement in schooling. The real percentage probably lies in-between 60% and 100% since the individuals of the pooled cross-sections are much younger (and hence probably less likely to have migrated) than the individuals in the 1940 Full Census Count. The data needed to estimate the earnings effects using the "childhood city" is being manually collected and will be available shortly.

programs and investments in the quality or quantity of public schooling at older ages. I perform a few empirical exercises to rule out this possibility. First, I conduct several falsification experiments ("placebos") in which I assume that kindergartens were built a few years in advance of (or a few years later than) the real years of construction. Table 3 and 4 report sets of placebo coefficients for each of the previous regressions. All of the placebo coefficients are statistically insignificant. Moreover, the magnitudes of the coefficients are very small in comparison to the estimated effects.

As a second approach, I explicitly control for proxies of alternative health and educational policies. Specifically, for several years, I collected city-level data on the number of public schools, the number of seats available in public schools, and the number of deaths for children under age 1 and under age 5 (which I then used to compute mortality rates for the same age groups).[27] Table 5 reports alternative estimates of the effect of kindergarten exposure, both including and excluding these measures. Results are very similar even after controlling for these variables. Whereas the educational effects are identical, the impact on occupational earnings is slightly larger for both native and non-native speakers.

VI. Robustness checks

Effects of kindergarten exposure in city sub-samples

Thus far, I have restricted my sample to cities with consistent data on the dates of kindergarten construction and to states with a small crowding-out rate of pre-existing private kindergartens. One would expect that the estimated effect of kindergarten exposure would be smaller or non-existent in cities without these characteristics, and I consider each in turn. First, I include data for the 29 cities and towns in the five states with the largest crowding-out rate of private enrollment in the period 1897-1912.[28] In these cities, the first public kindergartens were most likely replacing existing non-public options, and hence we would expect the effects of exposure to public kindergartens to have a smaller effect in these areas. Column (2) of Table 6

[27] The data on other educational and health policies was available for 60% - 80% of the sample (depending on the variable). For the cities with missing data I used either the state or the national average for the corresponding cohort (depending on availability). Finally, I interpolated these variables for the years that were not included in the data collection. The data was linked to the individuals using their city and the year in which they turned age 4.

[28] The five states with the highest density of private kindergartens were Maine, Vermont, California, Minnesota, and Illinois. I define "private" kindergarten enrollment to include all children enrolled in non-public kindergartens.

reports the results when adding these cities to my main sample. The estimated effects are between 10 to 15 percent smaller for non-native speakers (although very similar for those who speak English at home). Furthermore, when restricting the sample only to the five states with high rates of crowding-out (column 4), none of the coefficients are significant, and for the most part their absolute value is small in comparison to the main effects.[29]

A similar pattern is found when incorporating cities with inconsistent data. Around 90 cities and towns reported a year of first public kindergarten establishment that was inconsistent with the enrollment statistics reported in the statistical tables of the city schools systems (e.g. some cities appear with a positive number of public kindergartens before the year in which they supposedly built the first public kindergarten). Therefore, the reported year of first establishment is probably inaccurate. Column (3) of Table 6 shows the results including this set of cities in the main sample. The estimated ITT effects are about 10 to 30 percent smaller for non-native speakers. Moreover, when restricting the analysis only to the 90 cities with inconsistent data, all the coefficients are small and not statistically significant (column 5).

Alternative age trends

The main threats to my identification strategy involve potential pre-existing trends by age cohort within cities. Therefore, it is important to document that my results are robust to alternative specifications of the age effects. Table 7 shows that results are robust to employing alternative age trends. The baseline estimates include a quartic age trend. When I instead add a quadratic trend or age fixed effects, the estimated effect of kindergarten exposure on the children of non-native speakers remains identical (a 4.1 percent increase in occupational earnings).

Alternative treatment and control age bands

The benchmark model uses five-year age bands before and after the construction of the first kindergarten ("B" in equation 2) to define the treatment and control cohorts. As was discussed before, there is trade-off in choosing the bandwidth. On the one hand, a small band improves the comparability of the treatment and control cohorts, who were then more likely to be exposed to similar local and national policies. However, on the other hand, restricting the sample

[29] This is true for all ITT effects but for the impact on educational outcomes of non-native speakers.

to a few years after the first kindergarten allows for less time to build kindergartens, magnifies the importance of measurement error in age/date and reduces sample size.

In Table 8, I explore whether the results are sensitive to the selection of the band width. In particular, I consider 5-year, 4-year and 3-year age bands (columns 1, 2 and 3, respectively). I report the estimated effect of kindergarten exposure on the log of occupational earnings and the maximum grade of completed education, respectively, in panels A and B. In both panels, the sample sizes are reduced by almost 40 percent when using a 3-year band instead of a 5-year band. Nevertheless, the confidence intervals of each of the estimates overlap and, for the most part, the estimated effects remain stable or increase. Results are particularly robust for non-native speakers. For instance, the maximum grade of completed education increases by 0.29 grades when using a 5-year band and by 0.33 grades when using a 3-year band. A similar pattern is observed for occupational earnings (a 4.1 percent vs. 3.8 percent increment, respectively). On the other hand, the results are less robust for non-native speakers. In particular, the maximum grade of completed education increases by 0.14 grades when using the largest band, and by 0.23 grades when using the smallest band. However, the standard error also increase significantly (it is around 60 percent larger for the smaller sample)

Noisy cohorts

My main sample focuses on children who were older than six at the time of first kindergarten construction (not exposed), or who were younger than four in that year (potentially exposed). I did not include children ages four to six in the year that the first public kindergarten was built in each city (Y* hereafter) because even slight measurement error in Y* or in the reported age of the individual in the Census makes the exposure of this group to a kindergarten education unclear. This section re-introduces this "noisy" cohort under a variety of different assumptions. To start off with, I assume that all variables are perfectly measured and therefore that the probability of exposure to kindergarten was zero for those aged six at Y*, 0.50 for those aged five (because they received half of the treatment), and one for those aged four. Alternatively, I assume that both variables (Y* and age) are imperfectly measured, and hence that children aged four to six when the first kindergarten was opened face a probability between

zero and one of being exposed to the treatment. Specifically, I assume that the probability was 0.25 for those aged six, 0.50 for those aged five, and 0.75 for those aged four at Y*.

The results are reported in Table 9. The first column in Table 9 corresponds to my benchmark specification, which drops the "noisy cohorts." The second and third columns correspond to the assumptions above of either perfectly-measured or imperfectly-measured age/date data. The first panel shows that the "intention to treat" effects of kindergarten exposure on occupational earnings are very robust to any of these alternative assumptions. For children whose mothers come from non-English speaking countries, for instance, kindergarten exposure is estimated to increase occupational income by 4.1 percent in the benchmark specification, and to increase occupational income by between 3.5 and 4.0 percent under the two alternative assumptions. A similar pattern is observed for those children whose native language is English.

The coefficients measuring the impact on educational outcomes are somewhat more sensitive (see panel B of Table 9). For instance, whereas I estimate than non-native speakers exposed to kindergarten gain 0.29 grades when excluding the noisy cohorts (column 1), the gain is 0.17 grades and 0.26 grades under the alternative assumptions (columns 2 and 3). Yet simple calculations that incorporate the estimated measurement error in age (and assume that Y* is perfectly measured) can account for most of the drop in the coefficients (see web appendix II). For children with English-speaking mothers, the effects on grade attainment fall below the conventional level of statistical significance under the assumption of perfect measurement for both Y* and age, but this assumption seems unrealistic, particularly in historical data.

VII. Mechanisms

The effect of early education on adult outcomes depends critically on three factors: (1) what skills are developed by the program, (2) what is the program replacing (i.e. who is the counterfactual provider of childcare), and (3) does the program have any indirect effects on the household (e.g., by contributing to increases in parental income). Today, public kindergartens may have particularly large effects on children because they replace low-quality (and potentially expensive) private day care centers, or because they free up mother's time to re-enter the labor force, thereby adding to household income. In the historical context of the Kindergarten Movement, these mechanisms were unlikely to be operative: married women with children had

extremely low rates of labor force participation and most children stayed home with their mother (or another family member) until beginning elementary school. Therefore, the Kindergarten Movements provides a useful setting for estimating the direct effect of kindergarten attendance and skill-building on adult outcomes, with little interference from other more indirect mechanisms.[30]

Curriculum and skill-development during the Kindergarten Movement

Modern kindergarten curriculum is designed to develop children's "soft skills" (such as language fluency, socialization, discipline, punctuality, etc.), while also building their academic training in basic arithmetic, reading, writing, etc. However, as discussed above, early kindergartens were focused on soft skills, and deliberately excluded academic skills from the curriculum (see for instance Lee et al., 2006). Given this emphasis, we can interpret the estimates as revealing the effect of investment in soft skills during childhood on adult outcomes.[31]

Counterfactual care provider and indirect income effects

The potential returns to attending kindergarten depend not only on the curriculum but also on the child's alternative use of time. That is, if a public kindergarten had not been available in the child's town, would he have been home with a parent or would he have been cared for in another more informal arrangement? The effect of crowding-out informal care arrangements (e.g. babysitters) might differ from the effect of replacing parental time.

The counterfactual provider of care is intrinsically connected to parents' (chiefly mothers') employment decisions. Two extreme examples can illustrate this point. First, consider an economy where most of the parents (including mothers) are employed. In this case, increasing the stock of kindergartens is very unlikely to crowd-out parent's time since working mothers must have already made alternative care arrangements. Second, in the other extreme, consider an economy where most of the mothers are out of the labor force. In this case, it is much more

[30] See appendix II for a more formal discussion of the mechanisms.

[31] Moreover, most of the empirical work on the long-term effects of early education interventions focuses on programs that provide a bundle of services to the children (academic training, socialization, food, health controls, etc.). The fact that the program studied focused only on soft skills allows me to isolate the effect of this component, which is believed to be key for school readiness (see for instance Heaviside and Farris, 1993).

likely that public kindergartens replace parental time, in particular if the supply of private kindergartens is low.

A great advantage of studying the Kindergarten Movement is that, during this historical period, the labor force participation of mothers was negligible (less than five percent, see Figure 7). As a result, mothers were the most likely counterfactual providers of care in the absence of public kindergartens. If mother's care is preferable to the other types of informal arrangements that might be more prevalent today, we would expect to find smaller effects of exposure to public kindergartens in the past. Yet, I find that exposure to kindergarten generated an economic return in adulthood even when replacing (high-quality) mother's care.

In addition, in a context with high rates of female labor force participation (as today), access to public kindergarten could be associated with large increases in household income. First, some mothers may choose to enter the labor force if a public kindergarten is provided because kindergartens offer free or low-cost childcare, thereby lowering the opportunity cost of working. Second, public kindergartens could crowd-out existing care arrangements made by the mothers that were already employed (e.g., babysitters), thereby reducing the household expenditures on childcare services. Several papers has shown that the effect of public childcare on available household income could potentially be very large (see, for instance, Gelbach, 2002; Baker et al., 2008; Berlinski et al., 2007; Cascio, 2009; and Black et al., 2012). If household income itself has a direct influence on child's outcomes later in life, modern studies might conflate the effect of kindergarten attendance with the potential effects of household income. In my historical context, such income effects were likely to be very small or non-existant, suggesting that any estimated effect of kindergarten exposure is likely to come from human capital acquisition in the classroom.

Comparison to existing literature on long-term effects of early education programs

To the best of my knowledge, there are only two other papers that examine the impact of early education programs on adult outcomes using large samples.[32] Cascio (2009) studies the introduction of state-level grants to fund kindergarten education in the United States during the

[32] A few papers have studied the long-term effects of early education with small- or medium-sized samples: Barnett and Masse, 2007; Heckman et al., 2009; and Anderson, 2008 ; Garces et al.,(2002), Deming (2009); In addition, all these papers refer to programs targeted to disadvantaged populations.

1960s and 1970s. She finds some evidence of positive effects of kindergarten exposure in adulthood, but only for whites and only for two outcomes (the probability of being a high school drop-out or of being institutionalized as an adult). However, she finds no effect on grade retention, earnings, employment, or the receipt of public assistance. These null results might be explained by the large crowding-out rates that she estimates between public kindergartens and a series of alternative care arrangements, including private kindergartens and Head Start programs.[33]

On the other hand, Havnes, et al. (2011) study the long-term effects of a childcare reform in Norway in 1975. They find stronger evidence of positive effects for exposure to kindergarten on both educational and labor market outcomes in adulthood. In the Norwegian context, nearly all the mothers that took the program were already employed in the labor market. Public childcare primarily displaced informal care arrangements (e.g. unlicensed care providers, friends, etc.).

A comparison of Cascio (2009) and Havnes, et al. (2011) illustrates the importance of understanding children's alternative use of time when estimating the effect of early childhood education. If public education crowds out high-quality alternatives, the effect of kindergarten attendance might be quite small (e.g., in Cascio). But, if public education displaces informal or low-quality alternatives, the effect of these public options will likely be larger (e.g., in Havnes, et al.). In my context, kindergarten primarily replaced mother's care, which is often thought to be salutary for human capital acquisition, providing a particularly stringent test for kindergarten effectiveness.

VIII. Final comments

The amount invested in universal early education programs is growing rapidly in many countries, yet evidence on the long-term benefits of these investments is inconclusive. In this paper, I study the long-term effects of one of the first early education programs in the United States – the Kindergarten Movement (1890-1910). I collected unique data on the openings of public kindergartens across cities and towns during that period. I then link more than 100,000

[33] In a related paper, Cascio (2009b) estimates that these grants had a very large effect on the labor force participation of single mothers. Specifically, four out of ten mothers with no younger children entered the work force with public school enrollment of a five-year-old child.

children living in those cities across census waves, creating a panel dataset that includes adult outcomes. By comparing the cohorts within each city that were eligible to attend to kindergarten with those that were slightly older, I identify the effects of kindergarten exposure on long-term outcomes.

I find that kindergarten attendance had a significant effect on educational and labor market outcomes. On average, the affected cohorts received about 0.6 additional years of schooling and six percent more income (as measured by occupational score). These effects were substantially larger for second generation immigrant children. In particular, I estimate that children whose mothers came from a non-English speaking country gained about 1.1 additional years of schooling and 15.5 percent more income with exposure to kindergarten. To the best of my knowledge, this is the first paper that assesses the role of early education programs in the process of immigrant assimilation in the labor market.[34]

One of the advantages of studying this historical setting is that I am able to rule out certain mechanisms that might explain the positive relationship between kindergarten attendance and adult outcomes in contemporary data. The combination of negligible labor force participation of mothers during the period (less than five percent) and the simple play-based curriculum of the kindergartens during this era allows me to provide a clear interpretation of the estimates: they are most likely due to the acquisition of language and various soft skills early in childhood, rather than to earlier acquisition of academic skills or to the indirect effects of kindergarten on household income (via mother's employment).

Three interesting extensions of these results are in progress. First, it is possible that children whose mothers were born in non-English speaking countries benefited more from kindergarten exposure simply because their families were poorer. I am manually collecting data on household income (as proxied by father's occupation) to disentangle the effects of socio-economic status and language.

Second, if the main channel by which kindergarten exposure improves long-term outcomes is language acquisition and the development of soft-skills, one would expect that the

[34] A few papers have looked at the short- and middle-term effects of early education on Hispanic children (see for instance Currie and Thomas,1995; and Gormley and Gayer,2006). In addition, Deming(2009) studies the impact of Head Start on an index of young adult outcomes (around age 20).

cohorts exposed to kindergartens would be more likely to be employed in jobs that particularly reward those skills (e.g. white collar jobs). In the next version, I will explore whether this was actually the case by creating measures of the skills used in each occupation according to occupation dictionaries (e.g. O*NET).

Third, part of the theoretical case for investing in early education is based on the potential complementarities between early and later educational investments. The intuition is that disadvantaged children who did not develop key cognitive and non-cognitive skills during early childhood, may not be able to take full-advantage of future stages of the education system (e.g. high school). However, it is also possible that later educational investments have a smaller return for those children who managed to develop the skills they need for the labor market early on. I will exploit a unique characteristic of this historic period to test for complementarities between early and later educational investments. In particular, during the 1900s, both kindergarten and high school education were rapidly expanding, but these investments followed different time paths in different sets of cities and states. By interacting measures of exposure to each educational stage, I will be able to test whether these investments are complements or substitutes.
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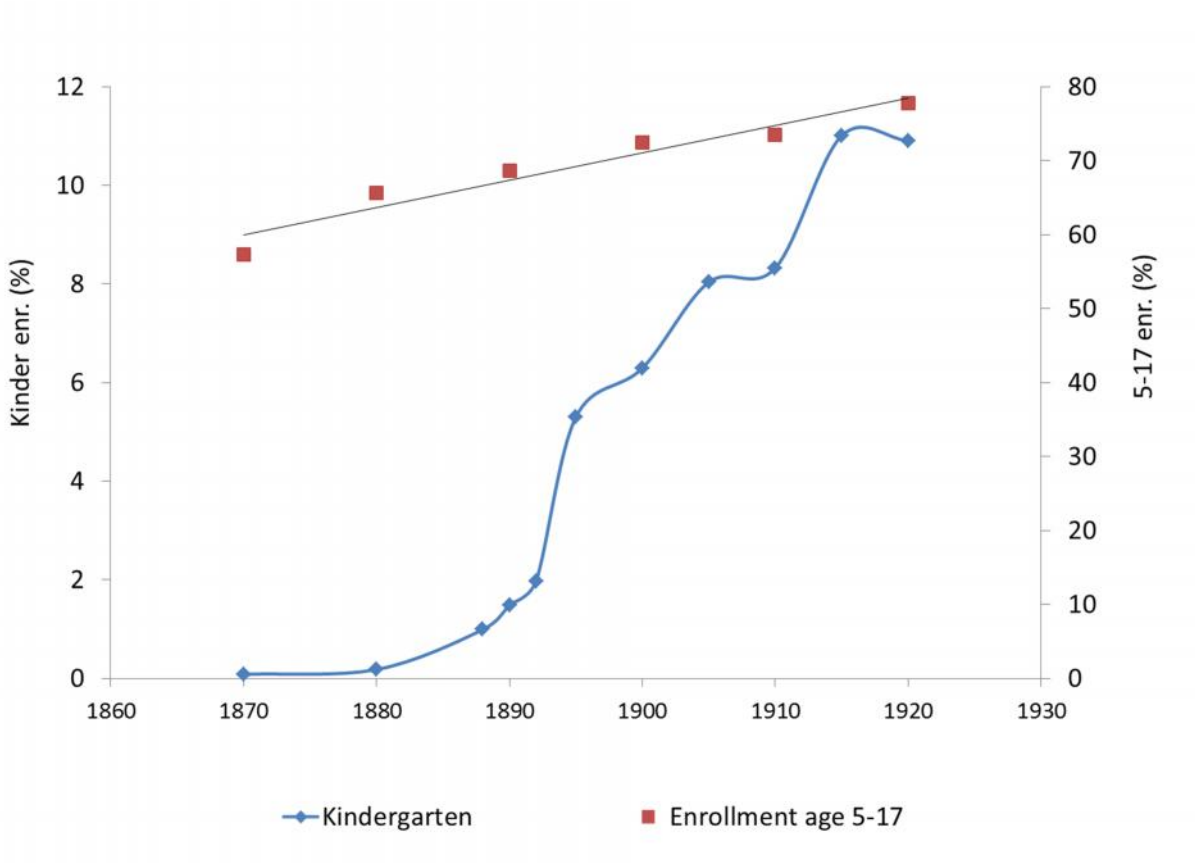
Reports of the Commissioner of Education, several years (1890-1912)

FIGURES

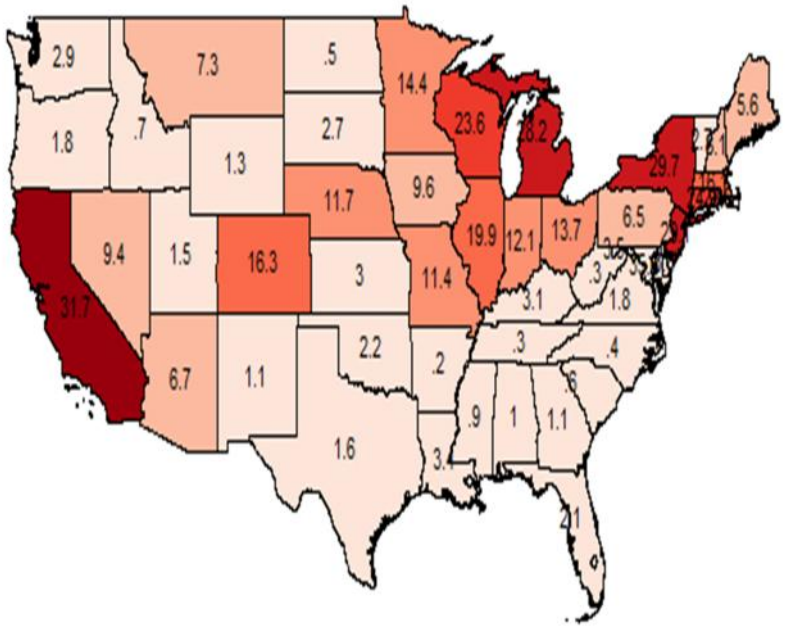
Figure 1: National enrollment in Kindergarten 1870- 1920



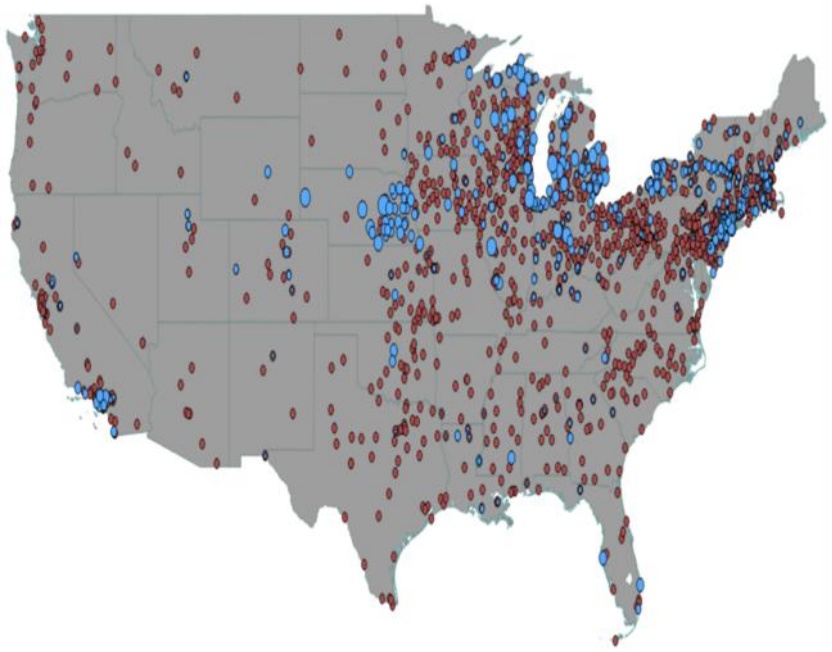
Note: The left axis measures the percentage of children aged 4 to 6 enrolled in Kindergarten. The right axis measures the percentage of children aged 5 to 17 enrolled in public schools. Source: Reports of the Commissioner of Education, several years

Figure 2: Kindergarten enrollment in 1912, Heterogeneity across states and cities

(b) State level



(a) City level



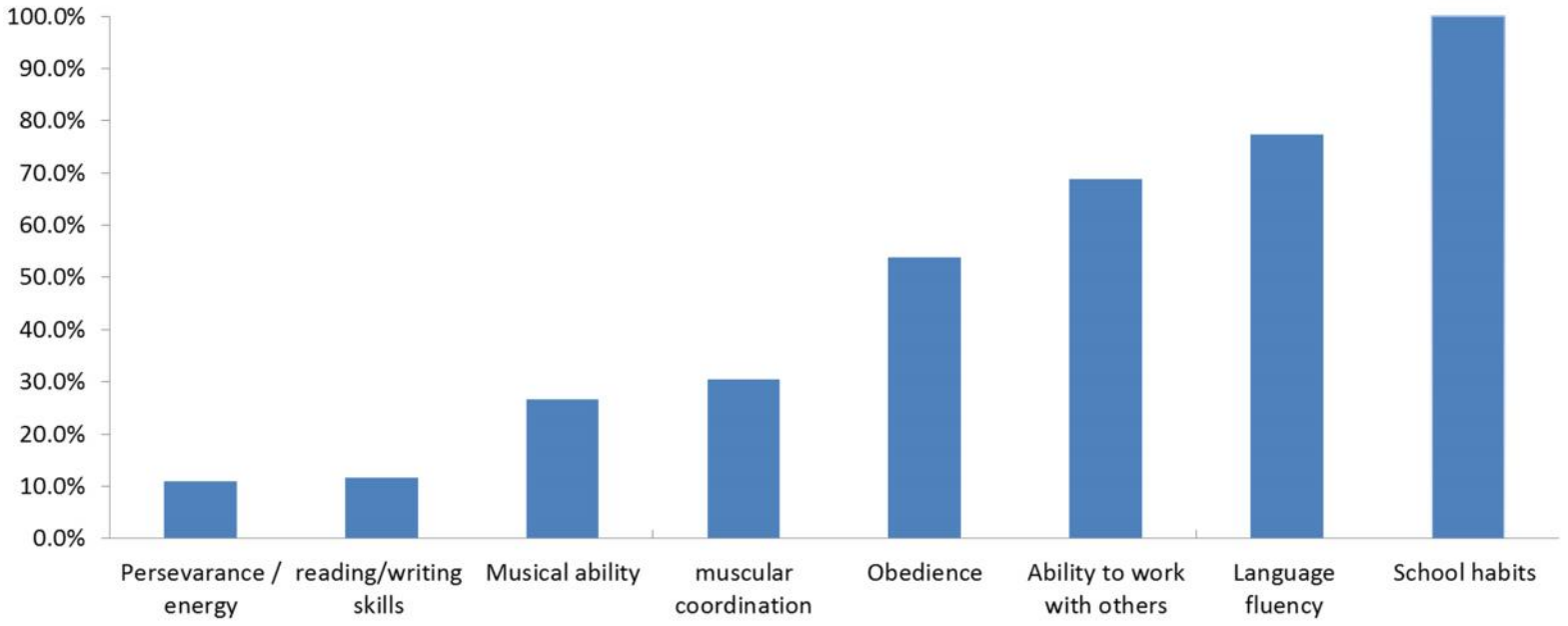
Source: Author's calculation based on Bureau of Education (1914)

City w/o public kindergartens City with public kindergartens

Note: larger blue dots means larger enrollment

Figure 3: Teachers survey (1915)

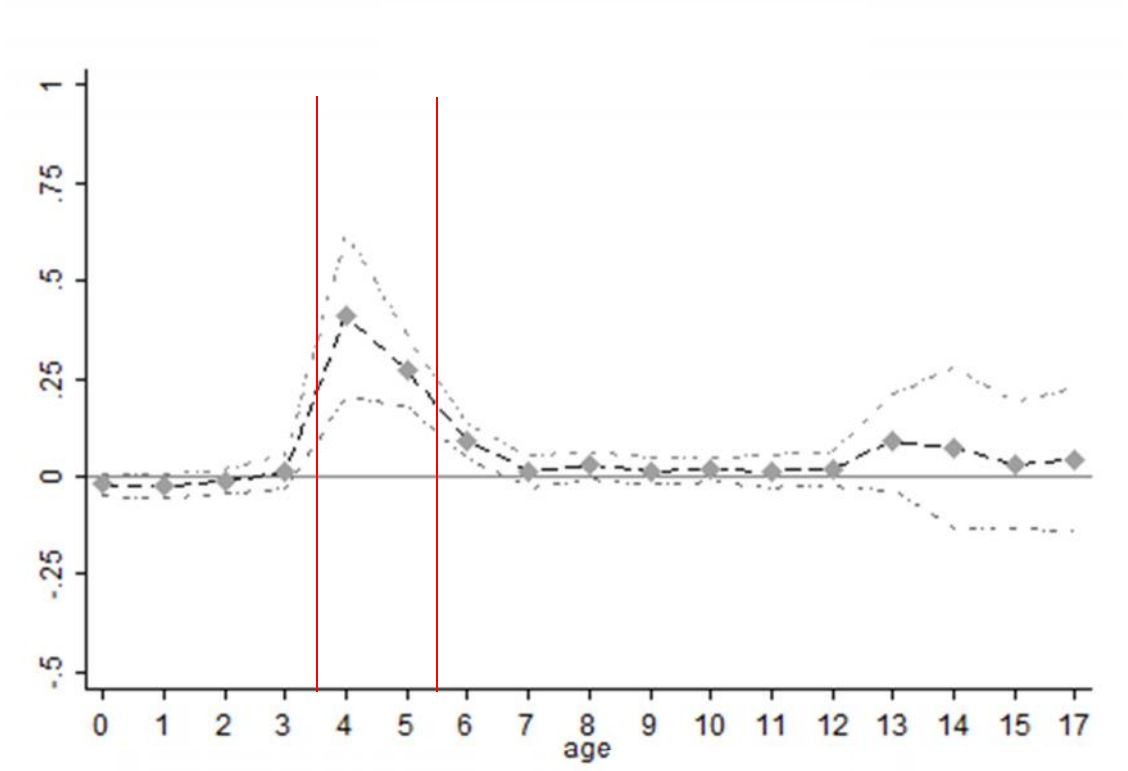
Advantages of children with kindergarten training



Note: The left axis reports a proxy for the percentage of teachers and superintendents of school that answered that the children with kindergarten training had an advantage in each dimension. The percentages were estimated as the number of teachers selecting each answer divided by the number of teachers who selected "school habits" (top answer). Source: Palmer (1915).

Figure 4: Number of public kindergartens and probability of enrollment in "any educational institution" (by age)

Sample: white children aged 0-17 living in cities and towns with kindergartens by 1912, IPUMS 1910 1% sample

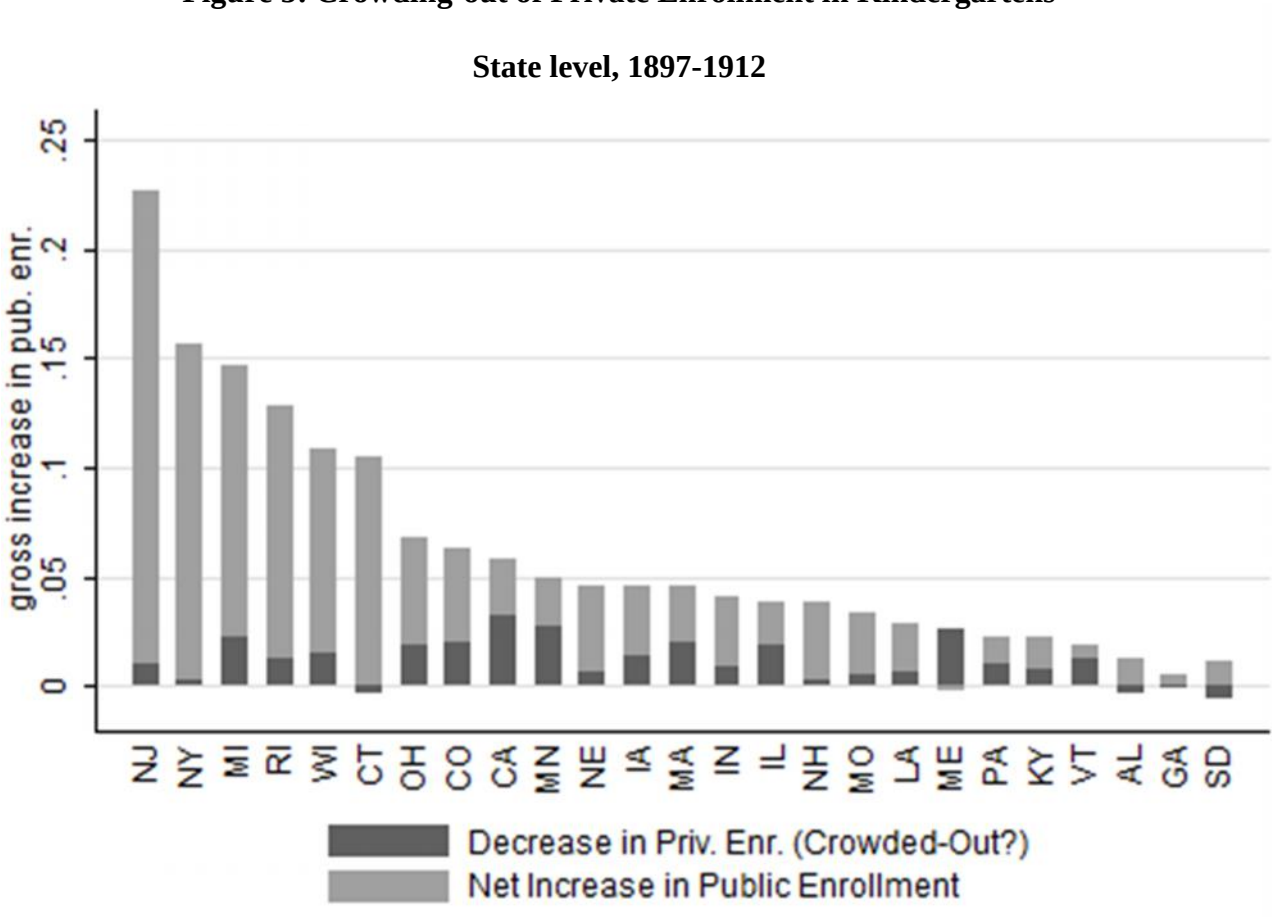


$$I(enrolled_{iacs} = 1) = \sum_a D_a . (\#\ of\ Public\ Kindergartens/pop;_c) . \boldsymbol{\beta_a} + (\ldots)$$

Note: The graph plots the coefficients β_a of equation (1). These coefficients were obtained from an OLS regression of attendance on the number of kindergartens per thousand inhabitants in each city or town by 1912 ("# of kindergartens/pop") interacted with a full set of age dummies (Da). The model also include a full set of age dummies, state fixed effects, and a forth order polynomial in the city population interacted with the full set of age dummies. Standard errors were clustered at the city level.

Figure 5: Crowding-out of Private Enrollment in Kindergartens

State level, 1897-1912

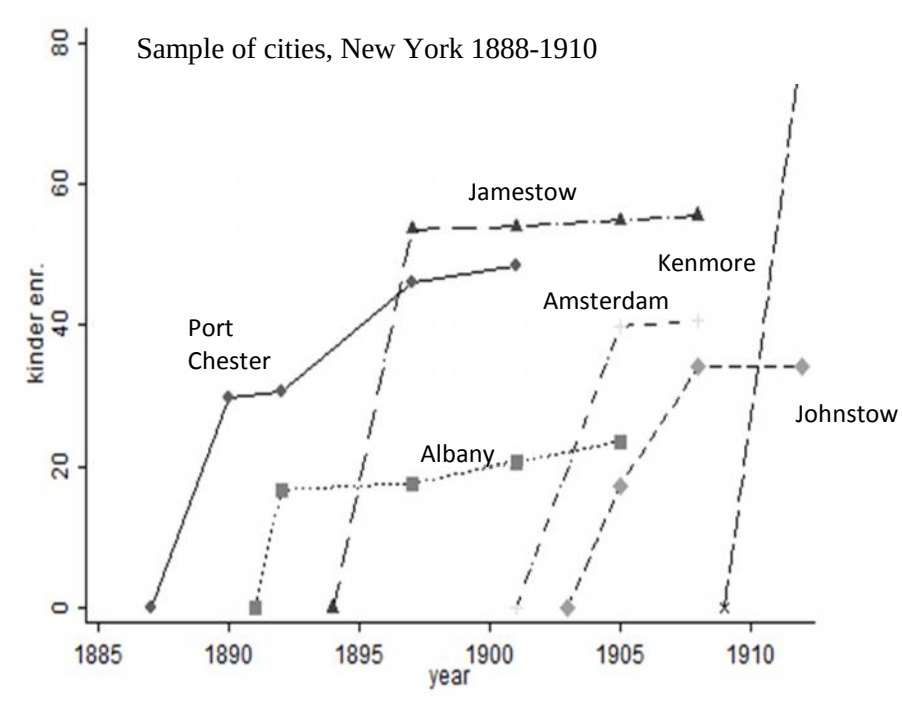


Note: this figure report share of the increase in enrollment in public kindergartens that was compensated by a decrease in private enrollment between 1897 and 1912. The height of the bars indicates the total increment in public enrollment between 1897 and 1912. The darker are represents the decrease in private enrollment in the period 1897-1912 (negative numbers imply an increment in private enrollment). Source: Author's calculations based on reports of the Bureau of Education

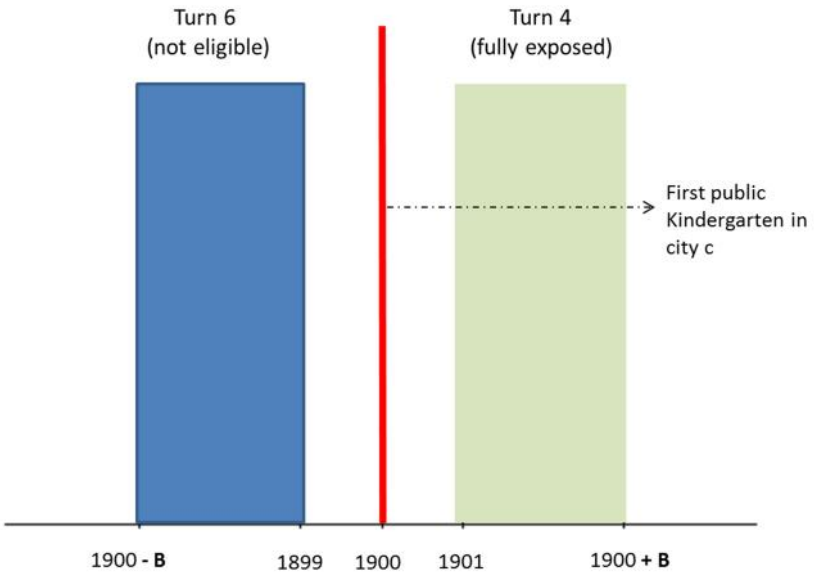
Figure 6: Identification strategy

Panel A: Enrollment growth (public kindergartens)

Sample of cities, New York 1888-1910



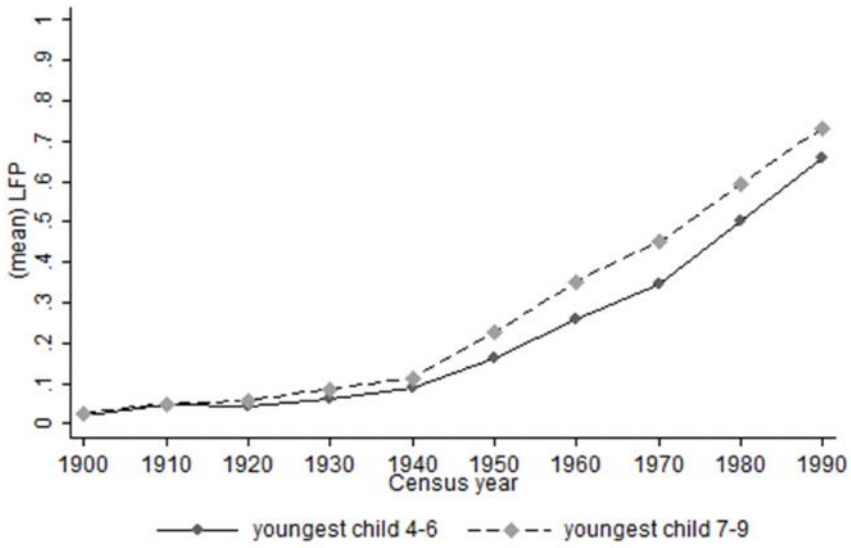
Panel B: Kindergarten exposure



Note: panel A shows for a sample of cities of New York the increment in enrollment in public kindergartens in the years following the construction of the first public kindergarten (Source: Author's calculations based on several reports of the Bureau of Education). Panel B illustrates how "exposure to kindergarten" is defined for a given city C (in the example, I assume that city C built the first public kindergarten in the year 1900). Formally: Exposed to Kindergarten equals 1 if the children turned 4 in $[\text{Year\_K}_c + 1\,;\ \text{Year\_K}_c + \text{B}\,]$, and equals 0 if the children turned 6 in $[\,\text{Year\_K}_c - \text{B}\,;\ \text{Year\_K}_c - 1]$, where Year_K_c represents the year that kindergartens were incorporated into the public education system (Year_K_c is equal to 1900 in the example) and B=5 in the benchmark case.

Figure 7: Labor force participation of white married women aged 25 to 45

United States, 1900-1990



Source: Author's calculation based on IPUMS 1900-1990

TABLES

Table 1: Determinants of kindergarten enrollment

Dependent variable = 1 if "attended any educational institution"	OLS		
	(i)	(ii)	(iii)
(# of Kindergartens) x (Age= 4 or 5)	0.438	0.457	0.476
	[0.087]***	[0.091]***	[0.097]***
(# of Kindergartens) x (Age= 4 or 5) x (low income)		-0.071	
		[0.080]	
(# of Kindergartens) x (Age= 4 or 5) x (immigrant mother)			-0.122
			[0.086]
Observations	58404	58404	54127
R-squared	0.63	0.63	0.65

Note: The table presents the coefficients obtained from an OLS regression of attendance on the number of kindergartens per thousand inhabitants in each city or town by 1912 (# of Kindergartens/pop), a full set of age group dummies, state fixed effects, and a forth order polynomial in the city population interacted with the full set of age group dummies.(see equation 1). “Low income” is a dummy variable that equals 1 if the father’s occupational earnings is below the median. “Immigrant mother” is a dummy variable that equals 1 if the child mother was born in a foreign country. The sample consists of white children aged 0-17 living in cities and towns with kindergartens by 1912. Data: IPUMS 1910 1% sample. Standard errors were clustered at the city level.

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level

Table 2: OLS effects of kindergarten exposure on earnings and education attainment

	Log(earnings)		Maximum grade attainment		
			adult city [(a)]	childhood city [(b)]	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Exposed to Kindergarten	0.015		0.11	0.18	
	[0.006]**		[0.044]**	[0.056]***	
(Exposed to Kindergarten)*(Non-English Mother Tongue)		0.04			0.29
		[0.013]***			[0.061]***
(Exposed to Kindergarten)*(English Mother Tongue)		0.01			0.14
		[0.007]			[0.059]**
Non-English Mother Tongue		-0.05			-0.89
		[0.010]***			[0.068]***
State fixed effects	Y	Y			
Year fixed effects	Y	Y			
County characteristics -1880	Y	Y			
Quartic age trend	Y	Y	Y	Y	Y
City fixed effects			Y	Y	Y
Observations	20,263	20,263	239,390	100,488	100,488
R-squared	0.02	0.02	0.01	0.01	0.01
Age range (sample)	25-45		30-66	30-66	

Note: The table presents the intention-to-treat effects of kindergarten exposure on labor market and educational outcomes. The coefficients were obtained from an OLS regression of each outcome on a dummy identifying the cohorts exposed to kindergarten (in each city). I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. The data used in columns (1) and (2) corresponds to the pooled cross-sectional samples 1900-1940. The data used in column (3) corresponds to the unlinked 1940 Full Census Count (kindergarten data is matched using the contemporary city in 1940). The data used in column (4) and (5) corresponds to the linked 1900-1910-1940 Full Census Counts (kindergarten data is matched using the contemporary city in 1900 or 1910). The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city.

(a) "Adult city" is the individual's contemporary city in the year 1940

(b) "Childhood city" is the individual's contemporary city in the years 1900 or 1910

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 3: Placebo tests to evaluate the presence of pre-existing trends in the cities that built kindergartens. Outcome: log(occupational earnings)

Dependent variable: log(occupational earnings)	Effects of kindergarten opening …			
	10 yrs earlier	5 yrs earlier	**real year**	5 yrs later
	(1)	(2)	**(3)**	(4)
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.004	0.014	**0.041**	0.004
	[0.014]	[0.012]	**[0.013]*** **	[0.011]
(Exposed to Kindergarten)*(English Mother Tongue)	0.012	0.006	**0.010**	0.008
	[0.009]	[0.007]	**[0.007]**	[0.007]
Observations	12195	16418	**20263**	23867
R-squared	0.02	0.02	**0.02**	0.02

Note: The table presents the intention-to-treat effects of kindergarten exposure on labor market outcomes. The coefficients were obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes year fixed effects, state fixed effect, county characteristics in 1880, and quartic age trends. County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years. The sample consists of white males born between 1874 and 1910 in small and medium cities. Dataset: pooled cross-sectional Census samples 1900-1940. Standard errors were clustered by city.

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 4: Placebo tests to evaluate the presence of pre-existing trends in the cities that built kindergartens. Outcome: maximum grade attainment

Dependent variable: maximum grade attainment	Effects of kindergarten opening …			
	10 yrs earlier	5 yrs earlier	**real year**	5 yrs later
	(1)	(2)	**(3)**	(4)
Panel A: Adult city[(a)]				
Exposed to Kindergarten	0.01	0.00	**0.11**	-0.03
	[0.047]	[0.044]	**[0.044]****	[0.035]
Panel B: childhood city[(b)]				
Exposed to Kindergarten	-0.03	0.02	**0.18**	n/a
	[0.070]	[0.052]	**[0.056]*****	
Panel C: childhood city[(b)] - by mother language				
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.03	-0.06	**0.29**	n/a
	[0.094]	[0.073]	**[0.061]*****	
(Exposed to Kindergarten)*(English Mother Tongue)	-0.04	0.04	**0.14**	n/a
	[0.071]	[0.054]	**[0.059]****	

Note: The table presents the intention-to-treat effects of kindergarten exposure on educational outcomes. The coefficients were obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes city fixed effects and quartic age trends. I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). The data used in Panel A corresponds to the unlinked 1940 Full Census Count (kindergarten data is matched using the contemporary city in that year). The data used in Panel B and C corresponds to the linked 1900-1910-1940 Full Census Counts (kindergarten data is matched using the contemporary city in 1900 or 1910). Colum 4 cannot be estimated in these panels because some of the children needed for that "placebo" were not born by 1910. Mother's language is proxied by mother birthplace. The interaction with mother's language cannot be computed in panel A because the 1940 Full Census Count only asked about mother's birthplace to the individuals included in the Census sample line. The sample consists of white males born between 1874 and 1910 in small and medium cities.

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

(a) "Adult city" is the individual's contemporary city in the year 1940

(b) "Childhood city" is the individual's contemporary city in the years 1900 or 1910

Table 5: is the effect of kindergartens due to other city level policies?

	Log(occupational earnings)		Max. grade attainment	
	(1)	(2)	(3)	(4)
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.041	0.043	0.29	0.29
	[0.0130]***	[0.0131]***	[0.061]***	[0.061]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.010	0.011	0.14	0.14
	[0.0067]	[0.0066]*	[0.059]**	[0.058]**
$(\text{Educ. and health policies})^{(a)}_{ct}$	N	Y	N	Y

Note: The table presents the intention-to-treat effects of kindergarten exposure on log(occupational earnings) and maximum grade attainment. Columns (1) and (2) present the coefficients obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The baseline model includes year fixed effects, state fixed effect, county characteristics in 1880, and quartic age trends (see equation 4). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. Columns (3) and (4) present the coefficients obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The baseline model includes city fixed effects and quartic age trends (see equation 3). I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). The data used in columns (1) and (2) corresponds to the pooled cross-sectional samples 1900-1940. The data used in columns (3) and (4) corresponds to the linked 1900-1910-1940 Full Census Counts. The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

(a) Proxies for other educational and health policies include: (1) Number of publics schools, (2) Number of seats in public schools, (3) Mortality rate under 1, (4) Mortality rate under 5 (available by city/year).

Table 6: Effects of kindergarten exposure in states with large crowding-out of private enrollment and cities with inconsistent data

		Main sample &		Only cities from	
	Main Sample	↑ C.O.R. private enr.[(a)]	inconsist-ent cities[(b)]	↑ C.O.R. private enr.[(a)]	inconsist-ent cities[(b)]
	(1)	(2)	(3)	(4)	(5)
Panel A: log(occupational earnings)					
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.041	0.036	0.028	-0.011	-0.019
	[0.0130]***	[0.0128]***	[0.0122]**	[0.0449]	[0.0247]
(Exposed to Kindergarten)*(English Mother Tongue)	0.010	0.009	0.006	-0.006	-0.021
	[0.0068]	[0.0066]	[0.0060]	[0.0060]	[0.0146]
Panel B: Max. grade attainment					
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.29	0.25	0.24	0.25	0.04
	[0.061]***	[0.058]***	[0.056]***	[0.323]	[0.136]
(Exposed to Kindergarten)*(English Mother Tongue)	0.14	0.14	0.12	0.03	0.07
	[0.059]**	[0.056]**	[0.051]**	[0.242]	[0.102]

Note: The table presents the intention-to-treat effects of kindergarten exposure on log(occupational earnings) and maximum grade attainment. Panel A presents the coefficients obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes year fixed effects, state fixed effect, county characteristics in 1880, and quartic age trends (see equation 4). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. Panel B presents the coefficients obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes city fixed effects and quartic age trends (see equation 3). I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). The data used in Panel A corresponds to the pooled cross-sectional Census samples 1900-1940. The data used in Panel B corresponds to the linked 1900-1910-1940 Full Census Counts. The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city.

[(a)] Cities in the (top 5) states with the largest crowding-out rate (COR) of enrollment in private kindergartens. I estimated the state-level COR as the share of the increase in public enrollment that was compensated by a decrease in private enrollment in the period 1897-1912.

[(b)] Cities with inconsistent data on the year that the first public kindergarten was built (the reported year in 1912 survey does not match the enrollment statistics reported in the statistical tables of the city schools systems).

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 7: Robustness of effects to defining alternative age trends

	Alternative age trends		
	(1)	(2)	(3)
Panel A: log(occupational earnings)			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.041	0.041	0.041
	[0.0131]***	[0.0130]***	[0.0132]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.011	0.010	0.010
	[0.0067]	[0.0067]	[0.0068]
Panel B: Max. grade attainment			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.29	0.29	0.28
	[0.061]***	[0.061]***	[0.062]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.14	0.14	0.13
	[0.059]**	[0.059]**	[0.057]**
Age trends			
Quadratic	Y		
Quartic (main)		Y	
Age fixed effects			Y

Note: The table presents the intention-to-treat effects of kindergarten exposure on log(occupational earnings) and maximum grade attainment. Panel A presents the coefficients obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes year fixed effects, state fixed effect, county characteristics in 1880 (see equation 4). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. Panel B presents the coefficients obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes city fixed effects (see equation 3). I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). The data used in Panel A corresponds to the pooled cross-sectional Census samples 1900-1940. The data used in Panel B corresponds to the linked 1900-1910-1940 Full Census Counts. Each column includes alternative age trends specifications (quadratic, quartic, age fixed effects). The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city.

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 8: Robustness of effects to defining alternative treatment and control age bands

	Alternative age bands		
	5 yrs (main)	4 yrs	3 yrs
	(1)	(2)	(3)
Panel A: log(occupational earnings)			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.041	0.044	0.038
	[0.0128]***	[0.0140]***	[0.0164]**
(Exposed to Kindergarten)*(English Mother Tongue)	0.010	0.009	0.004
	[0.0067]	[0.0069]	[0.0073]
Observations	20,263	16,470	12,588
Panel B: Max. grade attainment			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.29	0.29	0.33
	[0.061]***	[0.074]***	[0.087]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.14	0.15	0.23
	[0.059]**	[0.069]**	[0.095]***
Observations	100,488	81,165	61,537

Note: The table presents the intention-to-treat effects of kindergarten exposure on log(occupational earnings) and maximum grade attainment. Panel A presents the coefficients obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes year fixed effects, state fixed effect, county characteristics in 1880, and quartic age trends (see equation 4). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. Panel B presents the coefficients obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes city fixed effects and quartic age trends (see equation 3). I consider an individual exposed to kindergarten education if he or she turned 4 in the X years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous X years (X = 5, 4 or 3 depending on the column considered). The data used in Panel A corresponds to the pooled cross-sectional Census samples 1900-1940. The data used in Panel B corresponds to the linked 1900-1910-1940 Full Census Counts. The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city.

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Table 9: Alternative assumptions for children aged 4 to 6 (noisy cohorts) when the first public kindergarten was built in each city

	Benchmark[(a)] (1)	Naive[(b)] (2)	Imperf. info.[(c)] (3)
Panel A: log(occupational earnings)			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.041	0.035	0.040
	[0.0130]***	[0.0122]***	[0.0130]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.010	0.009	0.011
	[0.0068]	[0.0057]	[0.0064]*
Panel B: Max. grade attainment			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.29	0.17	0.26
	[0.061]***	[0.051]***	[0.058]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.14	0.03	0.10
	[0.059]**	[0.039]	[0.053]*

Note: The table present the intention-to-treat effects of kindergarten exposure on log(occupational earnings) and maximum grade attainment. Panel A presents the coefficients obtained from an OLS regression of log(occupational earnings) on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes year fixed effects, state fixed effect, county characteristics in 1880, and quartic age trends (see equation 4). County characteristics in 1880 include the mean occupational earnings and school enrollment of children aged 4 to 5. Panel B presents the coefficients obtained from an OLS regression of the maximum grade attainment on a dummy identifying the cohorts exposed to kindergarten (in each city). The model includes city fixed effects and quartic age trends (see equation 3). I consider an individual exposed to kindergarten education if he or she turned 4 in the five years that followed the construction of the first public kindergarten in their city, and not exposed if he or she turned 6 in the previous 5 years (see equation 2). The data used in Panel A corresponds to the pooled cross-sectional Census samples 1900-1940. The data used in Panel B corresponds to the linked 1900-1910-1940 Full Census Counts. The sample consists of white males born between 1874 and 1910 in small and medium cities. Standard errors were clustered by city.

(a) Case 1 (benchmark): drop cohorts aged 4 to 6 at Y* (Y* stands for the year that first the first public kindergarten was built in each city).

(b) Case 2 (naive): Assume that age and Y* are perfectly measured. I consider a probability $0 \leq p \leq 1$ of being exposed to kindergarten for cohorts aged 4 to 6 at Y*, with p=0 for those aged 6, p= 0.50 for those aged 5, and p=1 for those aged 4 at Y* (Y* stands for the year that first the first public kindergarten was built in each city).

(c) Case 3 (imperfect information): Assume that age and Y* are imperfectly measured. I consider a probability $0 < p < 1$ of being exposed to kindergarten for cohorts aged 4 to 6 at Y*, with p=0.25 for those aged 6, p= 0.50 for those aged 5, and p=0.75 for those aged 4 at Y* (Y* stands for the year that first the first public kindergarten was built in each city).

*** Significant at 1% level, ** Significant at 5% level, and * Significant at 10% level.

Appendix I: identifying cities and towns in the 1900-1910 complete census counts

Identifying the cities and towns is not trivial in the 1900-1910 complete census counts since there are no numeric codes for the cities, because the string names are not always homogeneous, and because there are alternative methods that can be used to identify the places. I used the following algorithm:

1) I first identified the cities using one of these 3 variables: (a) the city or town name, (b) the "incorporated place" name, or (c) the enumeration district associated to the incorporated place in the IPUMS samples (5% for 1900, 1% for 1910).

2) Then I collected the population size of the city in 1900 and 1910 either manually from census reports or from IPUMS.

3) To choose between method (a), (b) or (c) in step 1, I selected the method that replicated more accurately the population size of the city (that was collected in step 2). For about 90% of my sample I was able to almost perfectly replicate the city population in the complete census counts.

4) The incorporated place name was not digitalized in the 1910 full census count data. For those cities that I was not able to identify using either the enumeration number or the city name, I manually collected the page numbers corresponding to the incorporated places using the website: http://stevemorse.org/census/unified.html?year=1910

Appendix II: a simple theoretical framework

A key characteristic of early childhood education is that it can provided through a combination of parental time, formal childcare and alternative care arrangements. Assume that children's long-term outcome Y can be modeled as:

$$Y = F(MT,\ CC, AC,\ Income) \quad (1)$$

Where MT stands for mother time, CC stands for formal childcare, AC stands for alternative care arrangement (e.g. babysitter), and Income measures the available household income. Although very simple, this model incorporates key characteristic of early education: the time the child spent with the mother can have a different productivity than the time spent in other care arrangements. In addition, I allow the marginal productivity of mother's care $F_{MT(x)}$ to depend on mother's characteristics x (e.g. mother's native language). Finally, I also allow the marginal productivity of formal childcare $F_{CC(s)}$ to depend on the curriculum taught in the center (e.g. soft skills, hard skills, etc.). Let's further assume that the children face the following time constraint:

$$T = (MT) + (AC) + (CC) \quad (2)$$

which means that the total time of the children T is spent either in one of the 3 care arrangements (mother's care, formal childcare, and alternative childcare). Now let's consider a policy P (e.g. building a public kindergarten). Taking the total derivative of (1) we get:

$$\frac{d(Y)}{dP} = F_{MT}\frac{d(MT)}{dP} + F_{CC}\frac{d(CC)}{dP} + F_{AC}\frac{d(AC)}{dP} + F_{inc}\frac{d(Inc.)}{dP} \quad (3)$$

This expression essentially means that the total change in the child's outcome Y can be decomposed into the change in the time that the child spends in each care arrangement due to the change in the policy P times the marginal productive of each specific care arrangement, plus the change in the income available in the household $(d(Inc.)/dP)$ times the marginal productivity of income. We can take also the total derivative of (2) with respect to P to get:

$$\frac{d(CC)}{dP} = -\left(\frac{d(MT)}{dP} + \frac{d(AC)}{dP}\right) \quad (4)$$

Expression (4) basically means that if the child spends more time in a formal childcare center due to the subsidy, some alternative use of the time must be crowded-out (in this simple model this means that the child spends either less time with the mothers or in alternative care arrangements). Plugging (4) in (3) and rearranging the terms we get:

Impact on adult outcome Y

$$\frac{d(Y)}{dP} = (F_{CC(s)} - F_{MT(x)})\left(-\frac{d(MT)}{dP}\right) + (F_{CC(s)} - F_{AC})\left(-\frac{d(AC)}{dP}\right) + F_{inc.}\frac{d(Inc.)}{dP} \quad (5)$$

(1) Crowding-out of parental time

(2) Crowding-out of Alternative Care

(3) Income effect

Equation (5) illustrates, even within this simple model, the intrinsic complexity to disentangle the mechanisms of early education programs. The final effects depend on the curriculum taught (s), the counter-factual provider of care (mother, babysitter, private childcare, etc.) and the indirect income effects (e.g. some mother may enter the labor force after receiving free childcare). However, as discussed in section VII, in environments with negligible labor force participation of mothers and limited supply of private childcare (such as the Kindergarten Movement), (2) and (3) are very small. Then:

$$\frac{d(Y)}{dP} \approx (F_{CC(s)} - F_{MT(x)})\left(-\frac{d(MT)}{dP}\right) \quad (6)$$

Hence, the particular historic setting of the Kindergarten Movement allows me to provide a clear interpretation of the estimates and to narrow-down the potential mechanisms. My estimates indicate the value of "soft" early education that focused on play and socialization – $F_{CC}(s)$ –relative to staying home with the child's mother – $F_{MT}(x)$ – or a family member. In addition, equation (6) also indicates that the effects are likely to be heterogeneous by family background since the value of mother's care is likely to be heterogeneous by mother's characteristics. In this paper, I focused on a characteristic that has not been considered in the long-term literature on early education: mother's native language.