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Three Essays on the Economics of Education and Early Childhood

A dissertation submitted in partial satisfaction

of the requirements for the degree

Doctor of Philosophy in Economics

by

Francisco Haimovich Paz

2015

ABSTRACT OF THE DISSERTATION

Three Essays on the Economics of Education and Early Childhood

by

Francisco Haimovich Paz

Doctor of Philosophy in Economics

University of California, Los Angeles, 2015

Leah Michelle Boustan, Co-chair

Adriana Lleras-Muney, Co-chair

In these essays, I study the long-term effects of education policies and birth order on educational and labor market outcomes.

In my first chapter I study 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.

In my second chapter I study the long-term effects of an educational reform in Argentina. In the nineties Argentina implemented a large education reform (*Ley Federal de Educación* – LFE) that mainly implied the extension of compulsory education in two additional years. The timing in the implementation substantially varied across provinces, providing a source of identification for unraveling the causal effect of the reform. The estimations from difference-in-difference models suggest that the LFE had a positive impact on years of education and the probability of high school graduation. The impact on labor market outcomes —employment, hours of work and wages— was positive for the non-poor youths, but almost null for the poor.

In my third chapter I use US historical data to empirically test whether long-term birth order effects differ across the leading and lagging regions of the country in the Pre-War World II period. To do so, I create a large panel dataset by linking more than two million children across the 1920 and the 1940 full census counts, and to the World War II army enlistment records. I then study birth order effects on various long-term outcomes (with emphasis on educational outcomes). I find that in general, birth order effects are positive in the "developing" south—i.e. younger siblings do better than older siblings— and negative in the relatively modern north, which is consistent with the available evidence from contemporary data for developed and developing countries. I then exploit state level variation to show that birth order effects are positively correlated with the share of rural population, child labor rates and negatively correlated with the level mechanization in agriculture. I also show that, regardless the state of birth, the effects tend to be larger for the poor. Finally, I complement the analysis by looking at birth order effects on earnings and adult height. While I find relatively similar results for

earnings, I find no birth order effects on adult height, which suggests that we can rule out improvements in health or nutrition as the potential mechanisms behind the effects on education and labor outcomes.

The dissertation of Francisco Haimovich Paz is approved.

Dora Luisa Costa, Committee

Sarah J Reber, Committee

Leah Michelle Boustan, Co-chair

Adriana Lleras-Muney, Co-chair

University of California, Los Angeles

2015

To my wife Veronica,

and to my parents Maria and Francisco.

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ACKNOWLEDGMENTS

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CHAPTER 1

The Long-term Return to Early Childhood Education:

Evidence From the First US Kindergartens

1.1 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 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

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[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).

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 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]

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[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 (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.

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 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).

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[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.

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.

1.2 The kindergarten movement

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 to 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 1.1 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 1.2, 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 1.2).[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*

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[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).

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, 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:

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[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).

"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 1.3).[12]

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.

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[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)

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:

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

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

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.

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 1.4. The first interesting results is that the kindergarten stock only appears to

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[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

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 1.1 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.

1.3 Data

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 1.5 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

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[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.

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 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]

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

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[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.

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]

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 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.

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[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.

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]

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).

1.4 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

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[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 1.6 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 $Exposed\ to\ Kindergarten_{iac}$, which measures a cohort's "exposure" to kindergarten education in city c in the following way. Let $Year\_K_c$ be the year that kindergartens were incorporated into the public education system in city c. Then:

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

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

with B equal to 5 in the benchmark case. This is illustrated in the left panel of Figure 1.6. 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.

Clearly 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

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[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.

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:

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

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 jth 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:

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

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.

Which cities built the first public kindergartens?

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[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)

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. 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

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[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)

incorporation. Finally, all results are robust to dropping the largest and smallest places of my sample.

1.5 Results

Table 1.2 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]

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[26] Nevertheless, it is likely that I am underestimating the earnings effects by using the "adult city" instead of the "childhood city". Indeed, Table 1.2 indicates that the impact on highest grade attained grows from 0.11 grades to

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 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 1.3 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 1.5 reports alternative estimates of the effect of kindergarten exposure, both

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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.

[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

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.

1.6 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 1.6 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]

(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.

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 1.6 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 1.7 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

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[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 1.8, 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 1.9. The first column in Table 1.9 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 1.9). 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.

1.7 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

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

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 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 1.7). As a result, mothers were the most likely counterfactual providers of care in the absence of

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[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).

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 have 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 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

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[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.

[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.

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.

1.8 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 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 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).

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[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).

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. .

Figures and Tables

**Figure 1.1: National enrollment in Kindergarten 1870- 1920**
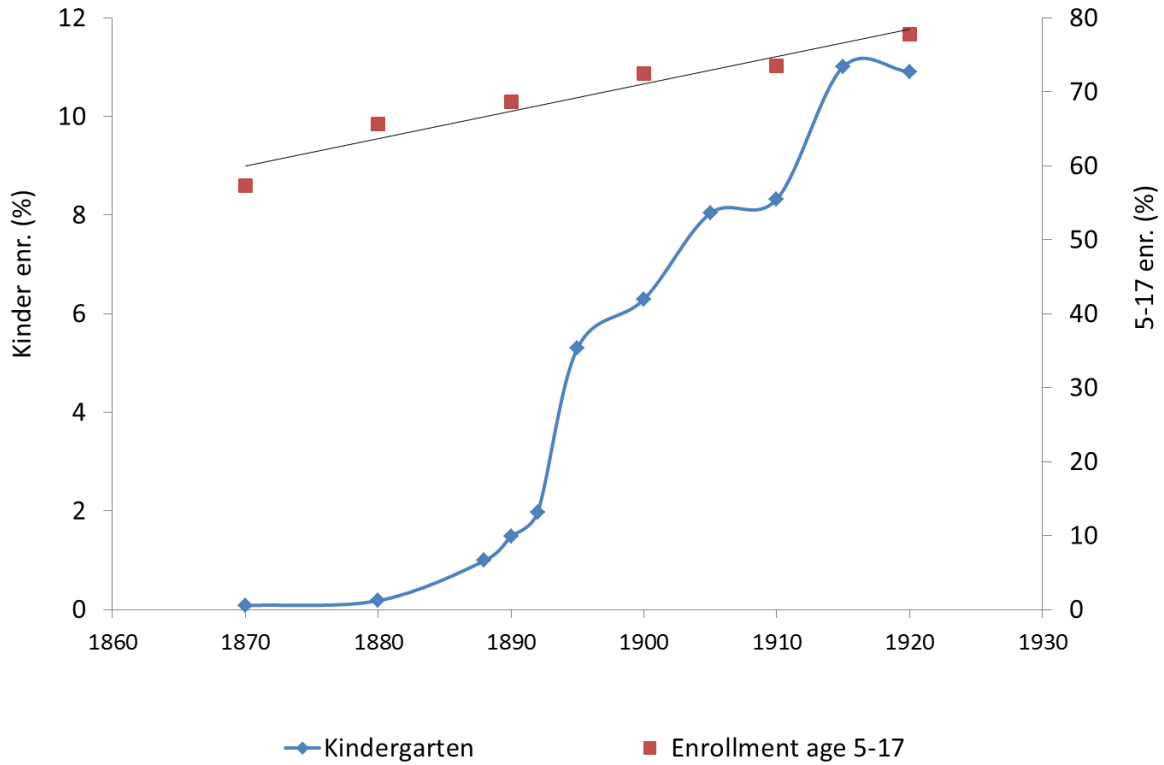
Note: The left axis measures the percentage of children aged 4 to 6 enrolled in Kindergarten. The right axis measures the percentage of children aged 5 to 17 enrolled in public schools. Source: Reports of the Commissioner of Education, several years.

Figure 1.2: Kindergarten enrollment in 1912, Heterogeneity across states and cities
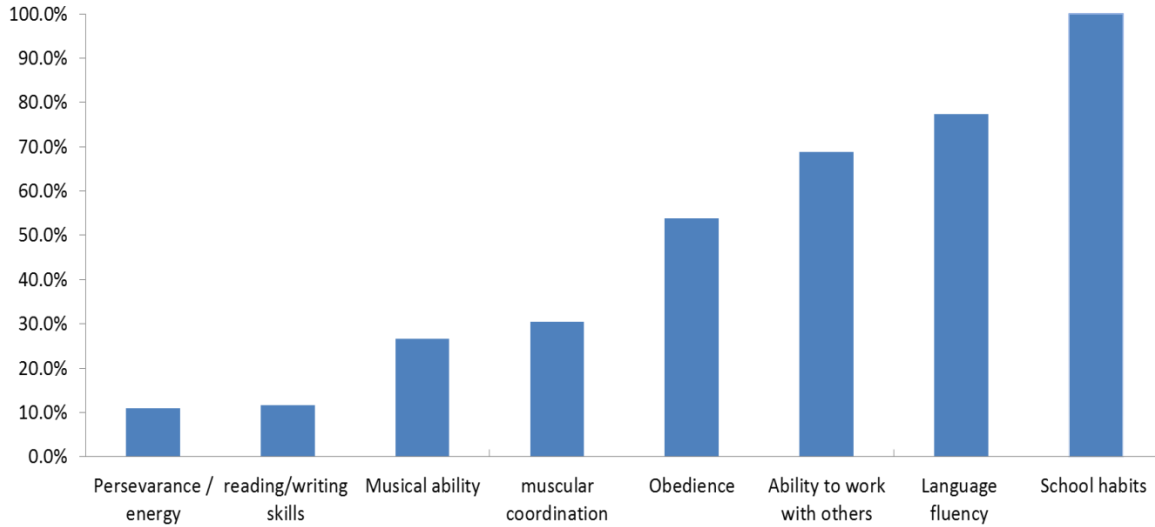
(a) State level
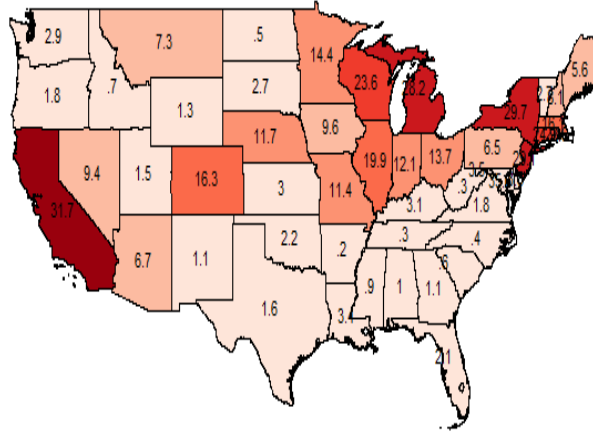
(b) City level
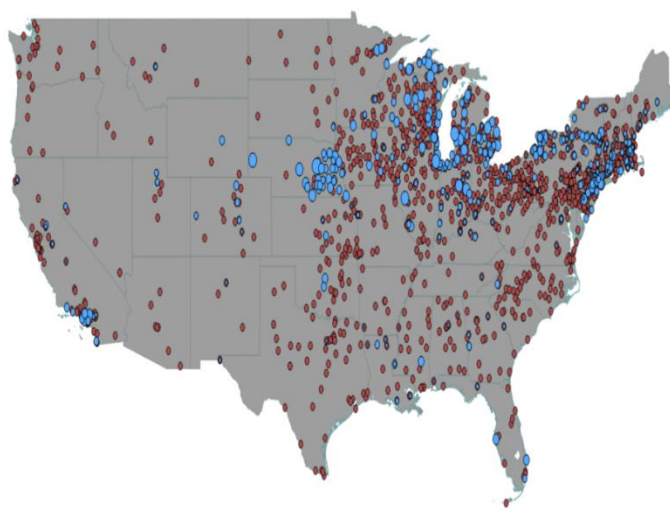
● City w/o public kindergartens   ● City with public kindergartens

Note: larger blue dots means larger enrollment

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

Figure 1.3: 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 1.4: Number of public kindergartens and probability of enrollment in "any educational institution" (by age)
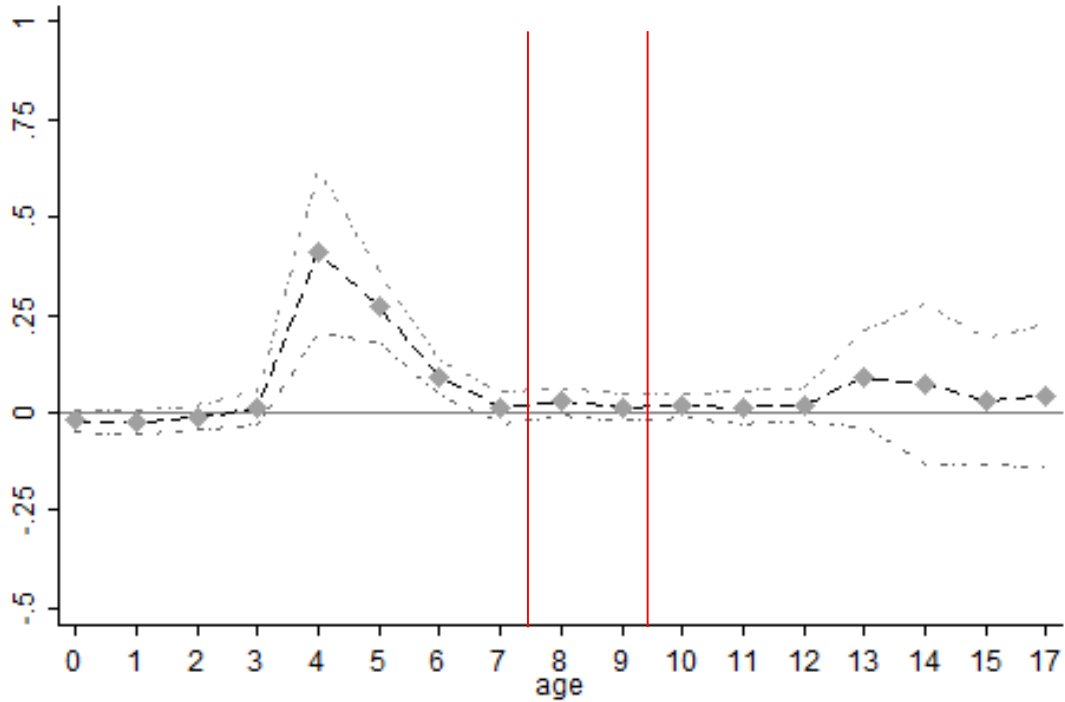
$$I(enrolled_{iacs} = 1) = \sum_a D_a \cdot (\# \ of \ Public \ Kindergartens/pop;\ _c) \cdot \boldsymbol{\beta_a} + (\dots)$$

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

Figure 1.5: Crowding-out of Private Enrollment in Kindergartens

State level, 1897-1912
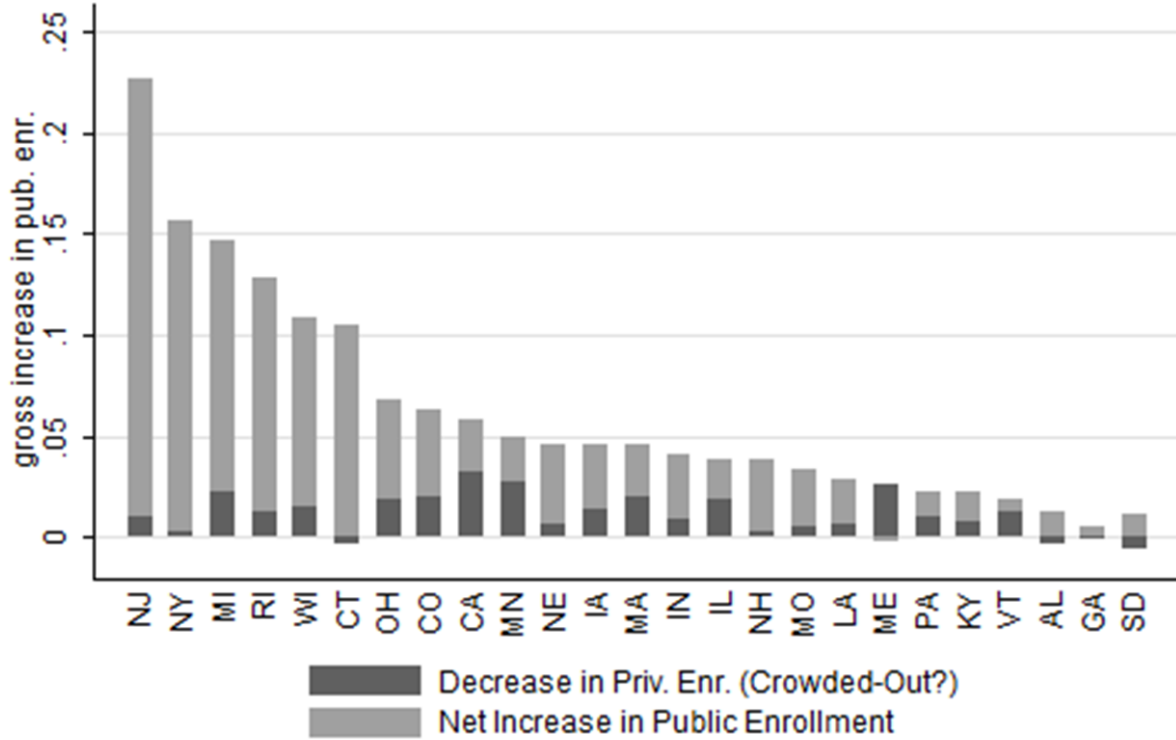
Note: this figure report share of the increase in enrollment in public kindergartens that was compensated by a decrease in private enrollment between 1897 and 1912. The height of the bars indicates the total increment in public enrollment between 1897 and 1912. The darker are represents the decrease 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.
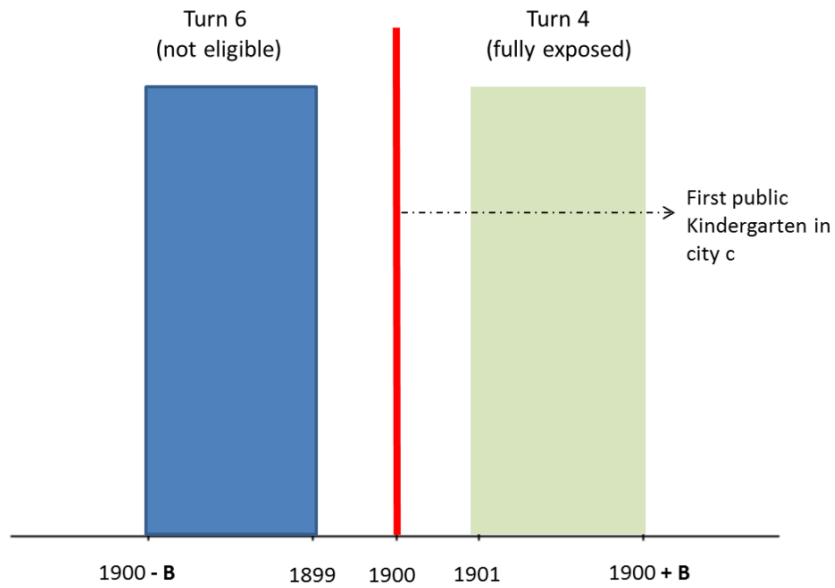
Figure 1.6: Identification strategy

Panel A: Enrollment growth (public kindergartens)

Sample of cities, New York 1888-1910
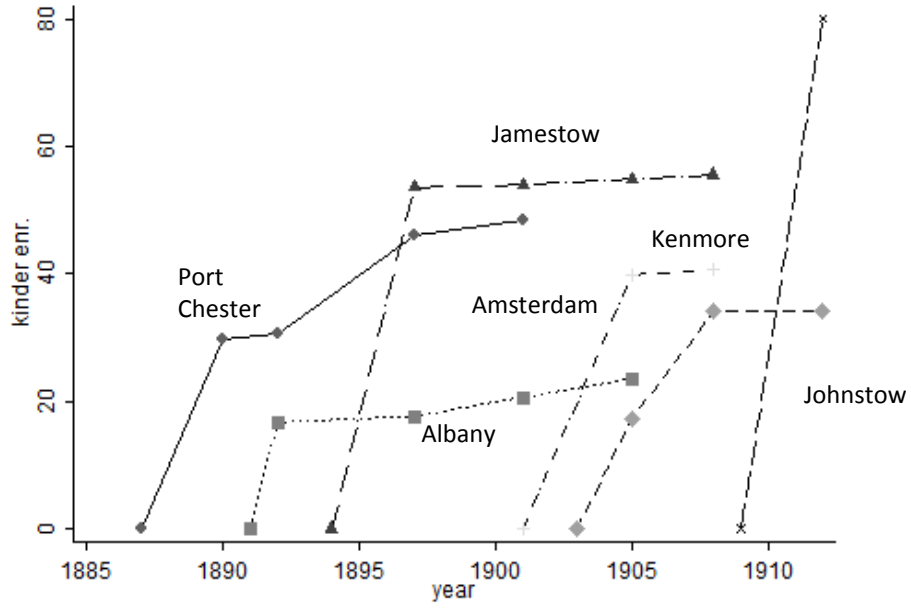

Panel B: Kindergarten exposure

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 [$Year\_K_c + 1$; $Year\_K_c + B$], and equals 0 if the children turned 6 in [$Year\_K_c - B$; $Year\_K_c - 1$], 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 1.7: Labor force participation of white married women aged 25 to 45

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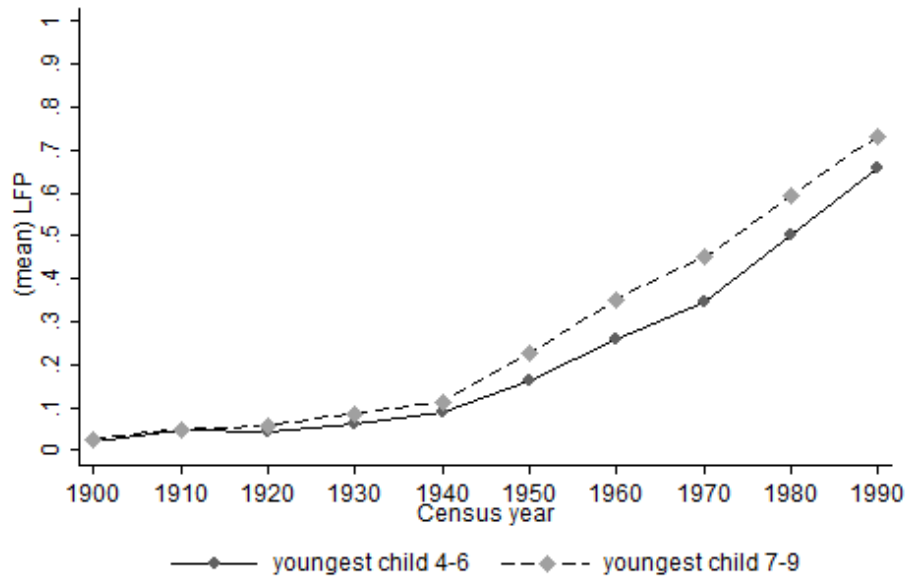
Source: Author's calculation based on IPUMS 1900-1990

Table 1.1: 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 1.2: 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 1.3: 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 (1)	5 yrs earlier (2)	**real year (3)**	5 yrs later (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 1.4: 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 (1)	5 yrs earlier (2)	real year (3)	5 yrs later (4)
Panel A: Adult city [(a)]				
Exposed to Kindergarten	0.01 [0.047]	0.00 [0.044]	**0.11 [0.044]****	-0.03 [0.035]
Panel B: childhood city [(b)]				
Exposed to Kindergarten	-0.03 [0.070]	0.02 [0.052]	**0.18 [0.056]*****	n/a
Panel C: childhood city [(b)] **- by mother language**				
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.03 [0.094]	-0.06 [0.073]	**0.29 [0.061]*****	n/a
(Exposed to Kindergarten)*(English Mother Tongue)	-0.04 [0.071]	0.04 [0.054]	**0.14 [0.059]****	n/a

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 1.5: 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]**
(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 1.6: Effects of kindergarten exposure in states with large crowding-out of private enrollment and cities with inconsistent data

	Main Sample	Main sample &		Only cities from	
		↑ 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 1.7: 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 1.8: 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 1.9: 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 [0.0130]***	0.035 [0.0122]***	0.040 [0.0130]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.010 [0.0068]	0.009 [0.0057]	0.011 [0.0064]*
Panel B: Max. grade attainment			
(Exposed to Kindergarten)*(Non-English Mother Tongue)	0.29 [0.061]***	0.17 [0.051]***	0.26 [0.058]***
(Exposed to Kindergarten)*(English Mother Tongue)	0.14 [0.059]**	0.03 [0.039]	0.10 [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 $(d(\text{Inc.})/dP)$ 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

(5)

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

(1) Crowding-out
of parental time

(2) Crowding-out of
Alternative Care

(3) Income effect

Equation (5) ill   th   o 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.

CHAPTER 2

Education Reform and Labor Market Outcomes: The Case of Argentina's *Ley Federal de Educacion*[1]

2.1 Introduction

In 1993 the Argentine Congress passed a law (*Ley Federal de Educación*, LFE henceforth) aimed at changing some important characteristics of the educational system. Chief among them was an extension in the years of compulsory education, along with a change in the structure of the educational curricula. While in the previous system a child was obliged to attend seven years of primary school, under the new legislation that compulsory educational level was extended to nine years.

By increasing the obligatory number of years of education, the government sought to force mostly poor children to increase their human capital accumulation, and induce some of them to continue studying in the secondary level, and then, hopefully, into college. More educated youths are expected to perform better in the labor market, and hence have a lower probability of falling into poverty. There are, however, scenarios in which these links may be weak. The return to an additional year of education could be very small for the poor if there exist complementarities with other educational investments, if the poor are less likely to work in the formal sector, or if there are large network effects (see for instance Cunha, Heckman

and Schennach 2010; Almond and Currie 2011; and Zimmerman 2013).[2] Only the empirical evidence can settle the issue of the relationship between time spent at school and improvements in labor market outcomes. While evidence on this relationship is well established for developed countries, evidence for developing countries is much scarcer (Duflo 2001).

In this paper we evaluate the impact of a large education reform in Argentina (the LFE) on several educational and labor outcomes by exploiting the regional heterogeneity in the timing of the reform. Argentina is a federal country where primary and secondary public education are administered and financed at the provincial level. Although the LFE was a federal law to be complied with in all provinces, there was flexibility for provincial governments to decide on the timing of the reform. While in some provinces the reform was quickly implemented after the LFE was passed, in others the pace of the changes was slower. In fact, in some districts many central aspects of the reform were never implemented. We take advantage of this source of variation in the exposure to the reform to study its impact on different educational and labor market outcomes. In particular, we are interested in evaluating whether poor youngsters who had to attend two additional school years were more likely to finish high school, and performed better in the labor market.

The rest of this paper is organized as follows. The education reform is described in Section II. Section III presents the methodology and describes the data sources. Section IV presents the main results on the effects of the LFE on education and labor market variables. Finally, section V closes with some remarks.

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[2] For instance, Cunha, Heckman and Schennach (2010) develop a human capital model with dynamic complementarities between educational investments. In their model, key cognitive and non-cognitive skills are developed early in life, and these skills considerably increase the productivity of the educational investments carried out later in life. Hence, as a corollary of this model, a given educational investment during high school might be less productive for disadvantaged populations.

2.2 The education reform

In the early nineties Argentina decentralized the provision of schooling services, previously in hands of the federal government.[3] The enactment of the *Ley Federal de Educación* (LFE) on April 14th, 1993 (Law 24195) introduced a second set of reforms, among which the extension of mandatory education stands out.[4] While in the old system a child was obliged to attend seven years of primary school, under the new legislation that compulsory educational level was extended to nine years. In fact, the LFE implied the reorganization of the levels in which the educational system in Argentina is divided. The main changes were: (i) pre-primary education for children aged five became compulsory; (ii) the primary level was extended from seven years to nine years, and was renamed *Educación General Básica* (*EGB*); and (iii) the five years of high school education were replaced by a three-year level called *"Polimodal"*. Table 2.1 shows the structure of the educational system before and after the reform. The first column reports the age in which the child/youth is supposed to be attending each level.

One of the main goals of the LFE was reducing the high dropout rate in the initial years of secondary school, especially by disadvantaged students (Braslavsky, 1999).[5] Under the new structure youths were encouraged to stay two years more in school. Advocators of the LFE argued that this extension might also induce many of them to complete the, now shorter, high school level, and hopefully to get into the tertiary level. Other authors were more skeptical about the enforcement of the law. Rivas (2003), among others, suggests that

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[3] Decentralization has been one of the main recent institutional innovations in developing countries. Galiani, Gertler and Schargrodsky (2007) find that decentralization in the provision of schooling in Argentina increased (decreased) test scores in richer (poorer) districts. Madeira (2006) and Rodriguez (2010) find that decentralization had a positive effect on test scores in Brazil and Colombia, respectively.

[4] This is a somehow different change from the one observed in developed countries, which increased the age for which school is mandatory. The policy in Argentina was to increase the number of compulsory years, regardless of age.

[5] The year Congress passed the law, the net enrolment rate in secondary school was around 65% for all (urban) Argentina, while it was below 50% in the bottom quintile of the income distribution (CEDLAS 2012).

the increase in the enrollment rate during mandatory education may be compensated later with a higher dropout rate in the non-compulsory stage.

The increase in the years of compulsory education was accompanied by other institutional changes also aimed at keeping youths at school for longer. The change in the curricula included some flexibility to choose among several specializations in the *Polimodal,* which also had the objective of smoothing the transition to higher education.

An important point for our analysis is that the new legislation was implemented with a substantial variation in terms of timing and intensity across provinces. Argentina is a federal country where primary and secondary public education are administered and financed at the provincial level. Although the LFE was a federal law to be complied with in all provinces, there was flexibility for provincial governments to decide on the timing of the reform. In fact, provinces were allowed to phase the implementation of the reform along the period 1995-1999. While in some provinces the reforms were quickly and massively implemented, in others the changes were put into effect more gradually, involving a much smaller percentage of schools (Rivas 2003; Crosta 2008). Moreover, in some districts some central aspects of the reform were never implemented (city of Buenos Aires, and the province of Río Negro). Table 2.2 reports for each province the year of implementation of the LFE and the modality (full, gradual, or null). By year 2000 the majority of the Argentina's provinces were complying with the new legislation.

The main objective of the reform was to reduce the high dropout rates in the first years of high school and to contribute to improve labor market outcomes. There is a great deal of literature studying the effects of additional schooling on subsequent gains later in life, related mainly to labor market outcomes (Angrist and Kruger 1991 and Acemoglu and Angrist 2000 for the US, Harmon and Walker 1995 for the UK and Oreopoulos 2006 for

Canada). Also, there are some studies that look at other outcomes such as crime (Lochner and Moretti 2004) and teen pregnancy (Black, Devereux and Salvanes 2005). However, as argued by Oreopoulos (2006), these studies look at changes in compulsory schooling laws that took place many decades ago in developed countries and the studies affecting dropout at that time may be different from the ones affecting dropouts today. Furthermore, the above-mentioned studies look at changes in the age a student should remain in school. Our paper looks at a somehow different change, since we examine the number of years that the individual must remain at school, regardless of her age. Our paper is more closely related to Duflo (2001), who studies the effect of a large school construction program in Indonesia —aimed at increasing primary school enrollment in poor areas during the seventies— on labor market outcomes.

2.3 Methodology

The implementation of the LFE was not accompanied by any strategy to evaluate its impact. This situation forces us to rely on observational data to derive our results. Our analysis seeks to identify the effect of the LFE on several educational and labor outcomes by exploiting the variation in the implementation of the reform across Argentine provinces.

Figures 2.1 and 2.2 help to motivate this strategy. Figure 2.1 shows that while enrollment for children aged 6 to 12 remained almost universal during the period under analysis, enrollment rates for youths aged 13 to 15 substantially increased after provinces started implementing the reform in 1996.

Figure 2.2 shows enrollment rates for ages 13-15 according to the degree (massive vs. gradual) of the implementation of the educational reform. Enrollment rates seem to have

strongly increased for those youngsters living in areas where the LFE was quickly and fully implemented.

One of the basic points of the paper is to evaluate whether individuals who were affected by the LFE performed better in certain dimensions (e.g., the labor market) than their peers who were not affected, either because they were born in provinces that did not implement the reform quickly, or because they were not affected by the LFE as they were just leaving primary school when the law was passed.

We use a difference-in-difference (diff-in-diff) approach for our estimations. Specifically, we use fixed-effects methods to control for unobserved heterogeneity across both cohorts and urban areas. Essentially, fixed-effects identification strategy uses repeated observations of the unit of analysis to control for unchanged unobservable characteristics that can be correlated with both causal variables and outcomes of interest. Our strategy is similar to that of Duflo (2001), who analyzes the impact of an extended school construction program, using the interaction between cohort indicators and program intensity as an instrument for schooling. Formally, the basic model is:

$$Y_{ijk} = C + \alpha_j + \beta_k + \theta X_i + (P_j * T_i).\gamma + e_{ijk} ,$$

(1)

where Y_{ijk} is the outcome of interest of individual i, living in city j, belonging to cohort k; α_j is a city fixed effect, β_k is a cohort fixed effect and X_i represents a vector of individual characteristics. T_i is a treatment variable, equal to 1 if the individual is young

enough to have been exposed to the reform, while P_j is a measure of the program intensity in the city.

Treated age cohorts are defined in terms of the likelihood to be fully exposed to the reform (see below for further discussion on the definition of cohorts). Besides, there is substantial variability in terms of treatment intensity among the young cohorts. Differences in the timing of the implementation of the new law imply that a given cohort could have been exposed to a variable extension in mandatory education according to the city of residence. In practice, we introduce the intensity of the reform P_j through a single binary variable equal to 1 for those individuals living in a city that fully implemented the LFE (instead of gradually or not implemented), according to the classification in Table 2.2.

We are interested in measuring the impact of the LFE on human capital accumulation and labor market performance (our left-hand-side variables Y). As outcome indicators for education we consider years of formal education and a dummy for secondary school graduation. With respect to the labor market performance, the main outcomes considered are employment, hours worked and wages.

Data

Our primary source of information is the *Encuesta Permanente de Hogares* (EPH) from 2003 to 2006, the main household survey in Argentina. The EPH covers 32 urban areas, with at least one observation from Argentina's twenty-four provinces listed in Table 2.2. Although the EPH covers only urban population, and hence it is not nationally representative, the share of rural population in Argentina is, unlike most developing countries, small (13%). In addition, the available evidence drawn from other data sources suggests only small

differences between urban and rural areas in terms of poverty and other social variables (Gasparini 2005).

The EPH gathers information on individual's socio-demographic characteristics, employment status, hours of work, wages, incomes, type of job, and education. The EPH includes information on about 100,000 individuals. Though the units of observation in our research are the individuals, the sources of variability in exposure to treatment are both the city of residence[6] and the cohort.

Exogeneity

One of the major methodological concerns about the approaches that exploit the regional variability in the timing or intensity of a policy intervention is that the choice of the local governments as to when and how to implement the reform may be correlated with unobservable factors which also affect outcomes. In our case, for instance, one may conjecture that poorer provinces with lower enrollment rates could have been more eager to put into effect the changes, since they will be granted resources from the central government.

In order to better understand the timing of the implementation of the LFE, we estimate a hazard model (Jenkins 1995) of the probability of implementing the reform. We are interested in examining whether there are factors that could be both affecting labor market/educational outcomes and the probability of implementing the reform. In Table 2.3 we present the estimates of the hazard model. We model the probability that a province implements the reform at a given period of time as a function of time-varying provincial

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[6] If migration is important, the region of residence used for the estimations may be different from the region the individual was living in at the time of the reform. Internal migration is, however, relatively low in Argentina. Since the EPH has information on migration during the past five years, the estimations were replicated using the previous residence: all the results in the paper hold under this alternative.

variables. There are several specifications for the left-hand-side variable.[7] Among the explanatory variables we consider proxies for regional GDP per capita, the Gini coefficient, the unemployment rate, population, fiscal deficit, poverty (percentage of individuals with unmet basic needs), and a political party dummy which takes the value 1 if the province is governed by the same party than the national government at the time of the reform. All these covariates were significantly different for the gradual and the full implementers, but they do not evolve differently over time between both groups.

The only variable that is significant in most of the specifications is the political party, which means that a province was more likely to implement the LFE if its ruling party was the same as the national one. Given this situation, we control for this variable in our estimations. As mentioned above, the rest of the variables, which are correlated with economic shocks and could be also correlated with our outcome variables of interest are uncorrelated with the probability of reform. If the reform is uncorrelated with observed time-varying factors, it is less likely that it is correlated with unobserved time-varying factors that could be also affecting our outcomes of interest.

Table 2.4 performs some checks in order to support our identification strategy. Based on individuals' ages and region of residence we split our sample according to exposure to the reform.

In Panel I we examine the simple difference in years of education between provinces that implemented massively vs. the ones that did not, for the subsample of males. While young cohorts (ages 8 to 12) are the ones exposed to the new law, old cohorts (ages 14 to 18) are not, since they were born before they could be affected by the education reform. The

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[7] We considered "implementation" for several different thresholds: 33% percent of implementation and 90% of implementation of EGB and Polimodal.

double difference between these two groups amounts to 0.91 years of education and is statistically significant.

To contribute to the assessment of the likelihood of our identification assumption, in panels II and III we run false experiments or *placebos*, in which we evaluate the program impact over age cohorts that were not affected by the program. In both panels, both groups (young and old) are comprised by people not exposed to the educational reform. The double difference in both panels is not statistically significant, which supports our claim that our results are driven by the reform under study and not by other factors.

2.4 The results

We carry out the estimations using several samples and different cohorts' definitions. Since Argentina has a high rate of individuals who graduate at a later age, we build the cohorts using several age ranges for robustness (see Table 2.5).[8] While cohort A includes a broader age range, cohort C considers that all individuals graduate on time.

The number of observations under the definition A of cohorts is 60,825, while it drops to 48,486 for definition B and 36,522 for definition C. The number of observations used for the estimations slightly decreases due to missing variables for some individuals (see Tables 2.6 and 2.7 below).

We carry out the analysis for four samples: all individuals, males, poor, and poor males. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. We performed the estimations using Unmet Basic

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[8] We define the cohorts with different age ranges due to two reasons. First, we cannot observe the exact date of birth of the individual, so we do not know the exact age at which she should have entered school. Secondly, we cannot observe grade promotion/repetition, so our measure of exposure has some noise.

Needs as definition of poverty and the results hold. As left-hand-side variables, we consider two measures of educational attainment —years of education and a dummy for complete high school— and a set of labor variables related to the labor market —employment (measured by a dummy of being employed), weekly hours worked and labor income (log of hourly wages).

Educational outcomes

The impact of increasing mandatory years of schooling on actual years of formal education may not be straightforward. While the extension in the number of years is mandatory and punished by law, such laws are difficult to enforce (Angrist and Krueger 1991). That is especially true in a context of high labor informality and credit constraints to the access to education. Poor individuals may be secluded into working in the informal sector, where returns to education are usually smaller, and proof of mandatory schooling is not required. Also, if credit markets have frictions, individuals may not go to school even when is compulsory.

Table 2.6 shows the results of the impact of the reform on educational variables. As explained above, the effect is captured by the interaction of a dummy identifying the "young cohorts" (i.e., those individuals young enough to be exposed to the reform) with a dummy variable that measures the intensity of the reform (=1 for those individuals living in a city that massively implemented the LFE). Besides the typical set of controls including socioeconomic and demographic characteristics, we also include political party in the regressions, given its significance in the hazard models of Table 2.3. Standard errors shown in the table are clustered at the province level. We report the results using different cohort definitions according to Table 2.5, and different samples. Results vary more across samples than across definitions of cohort.

The LFE seems to have had a significant effect on some basic school enrollment outcomes. The coefficients of the treatment variable in the regressions for years of education are positive and significant for all samples and cohort definitions. Youths fully exposed to the LFE ended up with more years of education than those not fully exposed to the reform. Coefficients range from 0.58 to 0.92 extra years of education as a result of the reform. Most coefficients are also positive and significant in the case of the binary variable for complete high school. In particular, they are positive for poor people, implying at least a partial success of the reform: poor youngsters exposed to the reform ended up with better educational outcomes than those not fully exposed to the reform.[9]

The increase in years of education is somewhat larger for the sample of all people than for the poor. One possibility behind this result is that the reform caused some poor teenagers to finish high school, but few of them to go beyond that. Instead, the impact could have been more intense on non-poor youths, who probably live in an environment more prone to education, and have higher opportunities to continue studying after high school.

The impact of the reform on educational outcomes seems to have been higher for males than for females. This is consistent with the fact that in Argentina, as in most Latin American countries, high-school drop-out rates are higher for men than for women. CEDLAS (2012) reports that in 2006 while 84% of females in secondary school age are attending that educational level, the share for males is 78%.

Labor market outcomes

The educational reform under analysis had mainly the objective of facilitating permanence of young individuals within the schooling system. By achieving higher levels of

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[9] The larger effect in the group of poor youths compared to the rest is mainly driven by the fact that high school graduation rates in this group are substantially lower.

education it was believed that labor market perspectives would improve. We find that the results of the reform in terms of labor market outcomes are mostly positive and statistically significant (Table 2.7). Youths fully exposed to the reform when they were teenagers have now higher probability of being employed, work more hours and earn higher wages. The probability of employment in the sample of all individuals increases between 4.3% and 7.2%.

The effect for poor individuals and poor males is also positive, but not statistically significant at the conventional levels. The same pattern applies to hours worked: while in the entire sample hours worked per week increased between 2 and 3 hours,[10] the effect for poor individuals is negligible and not statistically significant. Labor incomes for treated youths are around 16% higher than for their non-treated counterparts. The impact is higher for the sample of males, but almost completely vanishes in the sample of poor youths. The reform seems to have had no effect on the labor outcomes of income-deprived people.

In summary, the reform seems to have had an overall positive impact on education and labor outcomes. On average, youths fully exposed to the LFE have more years of education, were more likely to have completed secondary school, have higher probability of finding a job, work more hours and earn higher salaries. In contrast, the impact of the reform on the labor outcomes of poor youths turns out to be almost null. Poor teenagers fully exposed to the reform apparently did not experience improvements in their labor outcomes, compared to their counterparts in the control group.

One possible explanation for the differences across groups runs as follows. Poor people have very limited access to jobs with high returns to education. Most of them are construction workers, domestic servants, or are self-employed in the commerce sector. The

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[10] For an average working week of 35 hours, the increase in hours amounts from 5.7% to 8.5%.

environment where they grow (low social capital, scarce contacts) implies a substantial constraint to the access to jobs where education makes a big difference. In contrast, the gains were larger for the non-poor given the types of jobs that these people are more likely to hold (e.g., civil servants).

Returns to education

Our identification assumption allows us to estimate the impact of the program; if in addition we assume that the effect of the program on wages comes only through higher educational attainment, we can use the program to construct instrumental variables to estimate the impact of additional years of education on wages (Duflo 2001). In reality, the reform could have had an impact on both the quantity and the quality of education, and hence wages could have been affected through both channels. The impact of the education reform on quality has been studied by Galiani et al. (2007) and Bet (2008). The evidence is mixed: while the first paper shows that quality increased (decreased) in richer (poorer) districts, the second paper shows no change in quality as measured by standardized tests, other than an almost negligible improvement in Spanish scores. In what follows we assume that the change in quality was on average very small, so we can interpret our estimations as returns to education.

In our estimations we use equation (1) as a first stage in a Two Stages Least Squares estimation of the returns to education. We use the instrumented years of education in equation (2):

$$w_{ijk} = A + \phi_j + \beta_k + \delta X_i + \theta S_i + \eta_{ijk},$$

(2)

where w_{ijk} are log of hourly wages of individual i of city j of cohort k, X_i are individual characteristics, S_i are schooling years and η_{ijk} is the error term. Returns to education are presented in Table 2.8.

Point estimates for the whole sample are between 15.8% and 17.6%; they increase to 20.4% to 24.1% in the case of males. These results seem high but are in line with the ones estimated by Lopez Bóo (2010) for Argentina. Consistent with our previous findings, the returns are much lower for the group of poor individuals. The substantial difference between returns for poor and non-poor can be explained by the fact that still the majority of poor individuals do not finish high school and are severely limited to find a job in the formal labor market, where wages are higher.

2.5 Concluding remarks

High dropout rates in developing countries have motivated changes in educational systems in order to keep individuals in school. In most developing countries, education still remains an important policy for leveling off different labor market opportunities. While evidence on the (sometimes causal) relationship of time spent at school and improvements in labor market is well established for developed countries, evidence for developing countries is much scarcer. It is believed, however, that increasing the average years of education for individuals will enhance their labor market opportunities.

In spite of the heated debate about the educational reform in Argentina, there has not been solid evidence on its causal effect over educational and labor market outcomes. This

paper contributes to the measurement of the impact of the reform, by taking advantage of the variation in the implementation of the reform across provinces. Using a diff-in-diff methodology, we show the effect of the reform on several educational and labor outcomes. We also perform some robustness checks to argue that our estimates can have a causal interpretation.

When we look at the complete sample of individuals affected by the reform, our results suggest positive effects in some educational outcomes (years of education and high school completion) and labor outcomes (employment, hours and wages). Results also hold for the sub-sample of males. The same can be said for returns to education, which are high, but in line with previous literature for Argentina. However, the reform seems to have been only partially successful, as the impact on labor outcomes for the poor was almost null, possibly as a consequence of a very limited access of poor youths to jobs with high returns to education.
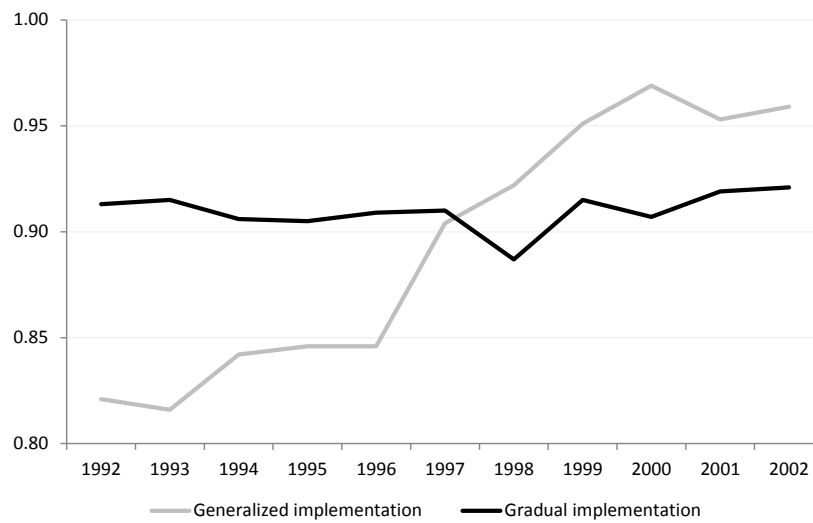
Figures and Tables

Figure 2.1: Gross enrollment rates by age group
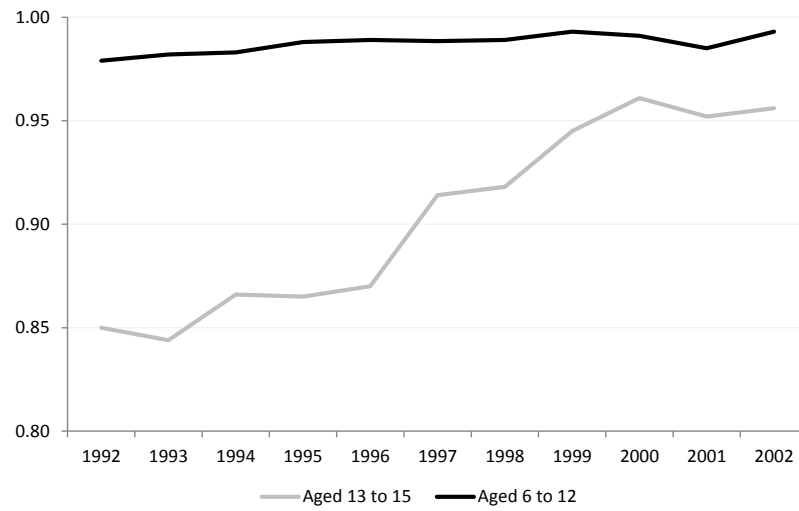
Note: The gross enrollment rates measure the share of each age group attending school. Source: own calculations based on microdata from EPH (INDEC).

Figure 2.2: Gross enrollment rates by degree in the implementation of the reform

Notes: The gross enrollment rates measure the share of each age group attending school. The provinces that followed each modality of implementation (*Gradual* or *Generalized*) are listed in Table 2.2. Population aged 13 to 15. Source: own calculations based on microdata from EPH (INDEC).

Table 2.1: Educational structure before and after the reform

Age	Before the LFE			After the LFE		
	Levels	Year	Compulsory?	Levels	Year	Compulsory?
3	Pre-primary	1	No	Pre-primary	1	No
4	Pre-primary	2	No	Pre-primary	2	No
5	Pre-primary	3	No	Pre-primary	3	Yes
6	Primary	1	Yes	EGB	1	Yes
7	Primary	2	Yes	EGB	2	Yes
8	Primary	3	Yes	EGB	3	Yes
9	Primary	4	Yes	EGB	4	Yes
10	Primary	5	Yes	EGB	5	Yes
11	Primary	6	Yes	EGB	6	Yes
12	Primary	7	Yes	EGB	7	Yes
13	Secondary	1	No	EGB	8	Yes
14	Secondary	2	No	EGB	9	Yes
15	Secondary	3	No	Polimodal	1	No
16	Secondary	4	No	Polimodal	2	No
17	Secondary	5	No	Polimodal	3	No

Note: LFE=*Ley Federal de Educación*; EGB=*Educación General Básica*.

Table 2.2: Year and degree of implementation of LFE by province

Province	Year	Degree
Buenos Aires	1996	F
Catamarca	1999	G
City of Buenos Aires	N.I	
Chaco	1997	G
Chubut	1999	G
Córdoba	1996	F
Corrientes	1997	F
Entre Ríos	1997	F
Formosa	1998	F
Jujuy	1998	G
La Pampa	1997	F
La Rioja	1999	G
Mendoza	2000	G
Misiones	1998	F
Neuquén	1998	G
Río Negro	N.I	
Salta	1998	G
San Juan	1997	F
San Luis	1998	F
Santa Cruz	1998	F
Santa Fé	1997	F
Santiago del Estero	1998	F
Tierra del Fuego	1998	G
Tucumán	1998	F

Source: Crosta (2008). N.I: not implemented. F: full implementation since the beginning. G: gradual implementation

Table 2.3: Hazard model: time of implementation

Dependent variable	33% polimodal implemented		90% polimodal implemented		33% EGB implemented		90% EGB implemented	
Variables	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
GDP per capita	-0.001	-0.001	-0.002	0.000	0.000	-0.000	-0.000	-0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]
Gini coefficient	0.607	-15.299	4.650	-8.219	-28.235*	-52.549**	-10.541	-27.035
	[11.691]	[14.903]	[14.009]	[18.726]	[14.433]	[23.218]	[15.655]	[21.443]
Unemployment rate	0.074	-0.031	0.137	0.243*	0.120	-0.170	-0.067	-0.216*
	[0.071]	[0.080]	[0.100]	[0.135]	[0.097]	[0.159]	[0.085]	[0.120]
Political party	-0.088	0.341	1.705**	2.349**	2.096**	0.509	2.134**	3.019***
	[0.603]	[0.664]	[0.758]	[0.947]	[0.843]	[1.185]	[0.840]	[1.153]
Population	0.005	0.019	0.005	-0.044	0.026	0.055	0.005	0.024
	[0.018]	[0.016]	[0.029]	[0.037]	[0.036]	[0.035]	[0.017]	[0.026]
Fiscal deficit	-0.001	-0.001	-0.001	-0.005**	0.000	0.002	-0.002	-0.000
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.004]	[0.002]	[0.003]
Time trend	1.224		2.690		1.401**		1.662*	
	[0.772]		[1.763]		[0.621]		[0.858]	
Poverty	0.024	0.044	-0.083	-0.080	0.110**	0.125**	0.099*	0.128
	[0.049]	[0.056]	[0.054]	[0.058]	[0.044]	[0.061]	[0.060]	[0.083]
Constant	-5.340	-16.769***	-8.716	-18.83	3.328	0.027	-3.284	-13.930
	[4.925]	[6.182]	[7.193]	[0.000]	[5.839]	[10.396]	[5.947]	[0.000]
Year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Observations	141	141	181	181	96	96	131	131

Notes: Each panel of this table reports a hazard model for the probability of implementing the reform under alternative definitions of "implementation". The alternative definitions differ either in the thresholds employed (i.e. percentage of schools that implemented the reform) or in the educational level considered (EGB or polimodal). The explanatory variables include the following time-varying covariates at the provincial level: GDP per capita, Gini coefficient, unemployment rate, population, fiscal deficit, poverty (percentage of individuals with unmet basic needs), and a political party dummy which takes the value 1 if the province is governed by the same party than the national government at the time of the reform. Source: own calculations based on microdata from EPH (INDEC).

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Table 2.4: Double differences between groups in years of education

Panel I: experiment of interest

	Intensive	Non-Intensive	Difference
Young	10.183	10.500	-0.317
	[0.001]	[0.001]	[0.044]
Old	10.991	12.218	-1.227
	[0.001]	[0.001]	[0.063]
Difference	-0.809	-1.718	**0.910**
	[0.043]	[0.054]	**[0.077]**

Panel II: Control experiment 1

	Intensive	Non-Intensive	Difference
Young	9.425	10.452	-1.026
	[0.001]	[0.001]	[0.074]
Old	9.118	10.217	-1.098
	[0.001]	[0.001]	[0.082]
Difference	0.307	0.235	**0.072**
	[0.055]	[0.079]	**[0.110]**

Young= 19, 20, 21, 22, 23, Old=24, 25, 26, 27, 28.

Panel III: Control experiment 2

	Intensive	Non-Intensive	Difference
Young	9.849	11.076	-1.227
	[0.001]	[0.001]	[0.086]
Old	9.490	10.515	-1.026
	[0.001]	[0.001]	[0.096]
Difference	0.360	0.561	**-0.202**
	[0.066]	[0.089]	**[0.129]**

Young= 16, 17, 18; Old=20, 21, 22.

Notes: Each panel reports for different regions ("Intensive" or "Non-intensive") and cohorts ("Young" or "Old") the average number of years of education for the subsample of males. "Intensive" and "Non-intensive" refer to whether the provinces implemented massively the reform or not, respectively. In panel I, "Young" denotes cohorts that where young enough to be exposed to the reform (8 to 12), whereas "Old" denotes cohorts that were slightly older, and hence not exposed (14 to 18). In Panel II and III, both cohort groups (young and old) are comprised by people not exposed to the educational reform. The numbers in bold in each panel correspond to simple difference-in-difference estimates. Standard errors in brackets.

Table 2.5: Cohort definitions

	Age
Cohort A	
Young	8,9,10,11,12
Old	14,15,16,17,18
Cohort B	
Young	8,9,10,11
Old	15,16,17,18
Cohort C	
Young	11,12,13
Old	14,15,16

Notes: Age in 1996. For each cohort definition, "Young" refers to the cohorts exposed to the reform, whereas "Old" refers to control cohorts.

Table 2.6: Impact of educational reform on educational outcomes

	Years of education			Complete High School		
	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
All	0.895***	0.920***	0.756***	0.024	0.027*	0.035***
	[0.211]	[0.235]	[0.132]	[0.015]	[0.015]	[0.013]
Observations	59449	47339	35850	60002	47799	36089
Males	0.838***	0.887***	0.704***	0.029	0.035*	0.037**
	[0.195]	[0.248]	[0.127]	[0.019]	[0.020]	[0.018]
Observations	29128	23213	17693	29432	23466	17824
All Poor	0.779***	0.859***	0.575***	0.078***	0.087***	0.083***
	[0.198]	[0.227]	[0.122]	[0.021]	[0.025]	[0.017]
Observations	32485	26002	19065	32852	26304	19230
Poor males	0.856***	0.924***	0.614***	0.083***	0.095***	0.093***
	[0.198]	[0.236]	[0.129]	[0.022]	[0.030]	[0.019]
Observations	15521	12446	9085	15722	12612	9176

Notes: Standard errors clustered at the province level in brackets. * significant at 10%, ** significant at 5%, ***significant at 1%. This table reports the reduced form estimation of the impact of the reform on educational outcomes (i.e., the parameter γ of equation (1)) for alternative samples and cohorts definitions. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 2.5. The educational outcomes include: (a) years of formal education, and (b) a dummy variable that equals 1 if the individual completed high school education.

Table 2.7: Impact of educational reform on labor outcomes

	Employed			Hours Worked			Labor Income		
	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C	Cohort A	Cohort B	Cohort C
All	0.060**	0.072**	0.043**	2.980***	3.181**	1.997***	0.157***	0.169***	0.160***
	[0.028]	[0.034]	[0.019]	[1.118]	[1.318]	[0.747]	[0.042]	[0.038]	[0.048]
Observations	60006	47803	36089	60006	47803	36089	15705	12391	10428
Males	0.056*	0.063*	0.055**	3.074**	3.168**	3.371***	0.205***	0.230***	0.218***
	[0.030]	[0.036]	[0.023]	[1.266]	[1.486]	[1.222]	[0.034]	[0.038]	[0.050]
Observations	29434	23468	17823	29434	23468	17823	9742	7692	6474
All Poor	0.025	0.022	0.029	0.774	0.712	0.087	0.015	0.051	0.051
	[0.026]	[0.028]	[0.018]	[1.021]	[1.143]	[1.044]	[0.047]	[0.053]	[0.066]
Observations	32854	26306	19230	32854	26306	19230	9151	7150	6098
Poor males	0.031	0.019	0.043	1.366	1.142	0.768	0.064	0.129***	0.065
	[0.033]	[0.034]	[0.035]	[1.442]	[1.642]	[1.800]	[0.046]	[0.043]	[0.053]
Observations	15724	12614	9176	15724	12614	9176	5941	4666	3928

Notes: Standard errors clustered at the province level in brackets. * significant at 10%, ** significant at 5%, ***significant at 1%. This table reports the reduced form estimation of the impact of the reform on labor market outcomes (i.e., the parameter γ of equation (1)) for alternative samples and cohorts definitions. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 2.5. The labor market outcomes include: (a) a dummy variable that equals 1 if the individual was employed, (b) weekly hours worked, and (c) labor income (log of hourly wages).

Table 2.8: Returns to education

	Cohort A	Cohort B	Cohort C
All	0.158***	0.169***	0.176***
	[0.042]	[0.047]	[0.056]
Observations	15657	12359	10396
Males	0.204***	0.238***	0.241***
	[0.045]	[0.058]	[0.069]
Observations	9709	7672	6450
All Poor	0.013	0.048	0.053
	[0.049]	[0.050]	[0.093]
Observations	9116	7126	6075
Poor males	0.074	0.149***	0.088
	[0.063]	[0.057]	[0.085]
Observations	5917	4652	3910

Notes: * significant at 10%, ** significant at 5%, ***significant at 1%. This table reports the implicit returns to education under the assumption that the reform only affected hourly wages by increasing the years of formal education (i.e., parameter θ of equation (2)). This was estimated with Two Stages Least Squares, using the models estimated in table 2.6 as the first stage for each sample and cohort definition. We consider a person to be poor if (s)he belongs to the bottom three quintiles of the household equivalent income distribution. Cohorts are defined in Table 2.5. Standard errors clustered at the province level in brackets.

CHAPTER 3

Birth Order Effects and Economic Development

3.1 Introduction

A great deal of the inequality in both educational and labor market outcomes is actually explained by intra-household variation. For instance, in the Philippines only 49% of the variation in educational attainment is explained by the differences between families (see Ejrnaes et al., 2004). Moreover, in many countries, birth order profiles play a large role in shaping the intra-family distribution of outcomes. In Norway, for example, second and the third born children accumulate on average about 0.3 and 0.5 fewer years of formal education than first born children (see Black et al, 2005).

While birth order effects have been widely studied, less attention has been paid to the relationship between these effects and economic development. There are good reasons to expect that birth order effects would be larger (positive) in developing economies. For example, in economies with high child labor rates, one could expect that younger siblings would benefit (in relative terms) from the additional income generated by their older siblings.[11] Furthermore, an underdeveloped credit market would limit the possibility of smoothing the household's consumption (and investments) over the life cycle, which could also generate positive birth order profiles.

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[11] Moreover, a simple extension of the child labor model developed by Basu et al (1998) predicts that low income households are less likely to send the younger children to the labor market when they reach the legal working age. In other words, younger siblings not only benefit from older siblings wages but they are also more likely to stay in school at any given age.

Indeed, a quick review of the literature shows that birth order effects are more likely to be positive in developing economies. Most of the studies carried out using contemporary data from developed countries – including the United States, United Kingdom and Norway— report large negative birth order effects. In those countries, younger siblings usually accumulate less human capital and earn lower wages than their older siblings (see Black et al., 2005; Kantarevic et al., 2004; and Booth et al., 2008). Conversely, most of the empirical studies for developing countries point in the opposite direction, though data limitations introduce important caveats. In countries like Brazil, Ecuador, the Philippines and Egypt— younger siblings tend to do better than their older siblings, particularly in terms of short-term educational indicators like grade-for-age (see Ejrnaes et al., 2004 ;Emerson et al., 2012 ;De Haan et al., 2012 ;and Tenikue, 2012).

In this paper, I empirically test whether birth order effects differ across regions with different levels of economic development. Specifically, I study long-term birth order effects in a historic period in the United States that is characterized by substantial geographic heterogeneity in terms of the level of "modernization" (such as the share of population living in rural areas). To do so, I create a large panel dataset by linking more than two million children across the 1920 and the 1940 full census counts, and to the World War II army enlistment records. I then estimate long-term birth order effects on years of education, earnings and adult height. In particular, I carry out a detailed comparison between the birth order profiles in the relatively modern North and the "developing" South. Then, I exploit state level variation to further analyze the relationship between birth order effects and variables measuring key dimensions of economic development, such as the rate of child labor, the level of mechanization of agriculture, and the share of rural population, to mention but a few. Finally, I show suggestive evidence indicating that the differences in the birth

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order profiles are not only driven by the specific institutions of the straggling states but also to the fact that the population in those states is poorer.

This paper has two important contributions. First, I thoroughly document birth order effects in relatively poor states that in many aspects (e.g. child labor rates) resemble developing countries today. While most of the available evidence for developing economies relies on cross-sectional data or short term educational outcomes, I am able to analyze long-term effects and a broader set of indicators (including adult height and earnings) with a large panel dataset. Second, to the best of my knowledge, this is the first paper that explicitly tests whether birth order effects are negatively correlated with variables measuring key dimensions of economic development.

In this version of the paper, I focus most of the analysis on the educational outcomes of white natives. I first divide the country into three regions with substantial differences in their levels of economic development by 1920: the Urban North, the Rural North, and the South. Whereas the Urban North represents the most advanced area of the country, the South lags behind in most of the key development indicators. By comparing these regions at one point in time, it is possible to observe the full transition from a "developing" to a "developed" birth order profile. I find that in the relatively modern urban north, the birth order effects on educational outcomes remarkably resemble the profiles estimated in developed countries using contemporary data (in terms of the negative sign, convexity and magnitude of the birth order effects). The picture changes dramatically, however, when I move to the rural north and the south. Whereas the rural north shows an inverted-U shaped birth order profile (with a flatter slope), the birth order effects becomes mostly positive when considering the less developed south. While there is still small penalty for second born children, the penalty reverses and even became a "premium" for the children born later. Indeed, for most family

sizes, the last born children accumulate around half year more of completed education than the first born children.

I then turn to the state level regressions. Using a two-stage approach—in the first stage I estimate birth order effects at the state level and in the second stage I regress these effects on state characteristics — I find additional evidence that birth order effects are larger in developing economies. States with larger share of rural population, higher child labor rates and less agriculture mechanization tend to have larger birth order effects.

These results could be explained by specific institutions of the "developing states", but they could also be explained by the fact that the populations of those states are just poorer. Indeed, some of the key mechanisms that could generate a positive birth order profile (e.g. large child labor rate or credit constraints) are more likely to be relevant for the poor. In order to explore this hypothesis, I test whether birth order effects are heterogeneous by socio-economic background even after controlling by state fixed effects. Regardless of the variable selected to approximate poverty, I find that poorer children have larger birth order effects. For instance, children whose fathers have lower earnings, rent their dwelling, or are illiterate have (on average) larger birth order effects.

I then expand the analysis to consider birth order effects on earnings and adult height. The results are, for the most part, similar for earnings. Birth order effects in this dimension are larger in less developed regions, and larger for the poor, etc. However, in the north, the sign changes, switching now to positive (although relatively small) birth order effects. For instance, a younger sibling earns on average 0.4% more than the immediately older sibling in the urban north. One possible explanation for this is that the older sibling spends more time on household chores, which ends up eroding the return to education (since

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it could limit the time they can invest in education related activities). Regardless of the mechanism, the conflicting results in the north highlights the importance of considering a broader set of indicators beyond educational outcomes when considering birth order effects, something that has been mostly ignored in the developing literature.

I also find that there are no birth order effects in terms of adult height. The coefficients are not only statistically insignificant but also small in economic terms. This is important since it suggests that we can rule out improvements in health or nutrition as the potential mechanisms behind the effects on education and labor outcomes.

Lastly, I extend the analysis to study second generation immigrants (classified by their mothers' primary language) and blacks. I find interesting heterogeneous profiles. Immigrants whose mothers came from English speaking countries have a birth order profile similar to that of northern white natives. Although younger siblings tend to accumulate less formal education than older siblings, this does not affect their relative earnings. On the other hand, children whose mothers came from Non-English speaking countries have a profile similar to that of southern white natives. Later born children have more education and larger earnings than their older siblings. Furthermore, the magnitude of the birth order effects is relatively large for this group. For instance, a higher birth order position increases (on average) the years of completed education by 0.2 and earnings by 3%. Finally, I do not find significant birth order effects for blacks on education, earnings and height. However, this conclusion is not robust. For some specifications I find a positive "last-born" effect on both education and earnings.

These results have important implications. They suggest that we should expect a large transition in the birth order profiles as countries grow, from positive to negative effects,

which could significantly affect the within-family distribution of outcomes (which explain a large share of the overall inequality in many countries). Furthermore, this can provide an alternative explanation for the demographic transition, since the optimal number of children could be significantly smaller when birth order effects are negative.

The rest of the paper is structured as follows. Section II summarizes the literature. Section III discusses alternative mechanisms that could generate different birth order profiles in developed and developing countries. Section IV documents my main data sources. Section V reports the main empirical results. Section VI discusses the next steps.

3.2 Literature review

Table 3.1 summarizes the literature on birth order effects. I classify the papers in two groups, depending on whether they refer to developed or developing countries. Three key points stand out. First, whereas birth order effects are negative in developed countries (e.g. younger sibling accumulate less human capital than older siblings), in developing countries the available evidence suggests that the pattern is the opposite, i.e. on average younger children perform better than they older siblings. As will be discussed later, different explanations have been proposed for these results, including the implicit subsidy that younger siblings receive from the older siblings child labor, imperfect credit market, to mention but a few.

Second, whereas the evidence for developed countries relies on panel data, most of the available evidence for developing countries is based on cross-sectional data. The latter has important limitations to study birth order effects. On the one hand, it limit the analysis to short term outcomes, like grade-for-age, which could be misleading to analyze long term

birth order effects. On the other hand, in many developing countries a large share of the children leave their families at early ages in order to work as child workers. For instance, in Ghana about 50% of the children have left their family by age 15.[12] This can considerable bias the short-term birth order effects estimated using cross-sections due to attrition bias.

Third, the outcomes considered in the studies for developing countries are more limited. These studies mostly focus on school enrollment or the probability of being a child worker. To the best of my knowledge, this literature has no considered birth order effects for labor market outcomes or adult height.

3.3 Birth order effects and economic development: a simple theoretical framework

In this section I describe a simple theoretical framework that could generate different birth order profiles for developing and developed countries. The basic idea is that the per capita resources available to invest in children at a given age may increase or decrease with birth order, and that these constraints (or their returns) can look different in developing and developed countries.

On the one hand, some resources –like siblings' or parents' earnings—could increase with birth order. This is more likely to happen in developing countries either due to larger child labor rates or because a larger share of the population is credit constrained (either because the credit market is underdeveloped or because more people have informal jobs) and then faces more difficulties to have access to future flows of income.

On the other hand, for key early ages, some fixed resources like the mother's time or financial resources decrease (in per capita terms) with birth order. Whereas this is true for

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[12] Owns calculations based on IPUMS-international

both developing and developed countries, one could speculate that this constraint could be less relevant in developing countries, either because mothers are on average less educated (or because the skills developed by mothers' care have smaller returns when most of the available jobs are unskilled), or because the family fixed resources are in general smaller in developing countries, to mention but a few.

To be more specific, let assume that for a given child i the long-term outcome Y_i can be written as a function of the investments I_{ia} carried out by parents at different ages a :

$$Y_i^{Adult} = F\left(I_{i0}, I_{i1}, I_{i2,}, \dots, I_{iA}\right)$$

Let further assume than I_{ia} includes financial resources X_t that can be transferred across time (such as income) and fixed resources $\overline{M}$ that cannot be transferred across time (such as mother's time).

Then, it is straight forward to notice that as the number of siblings S increases, the per capita fixed resources $m = \overline{M}/S$ decreases at early ages. The red line of Figure 3.1 describes –for a given early age a— how would m evolve with birth order. Since younger siblings will have to share the fixed resource with more siblings, the later the child is born, the smaller m is. However, the "penalty" increases at a decreasing rate. For instance, if $\overline{M}$ is mother's care and each kid is born every two years (and if mothers divide equally their time between children), by age two the first born child will have 100% of the mother's time, the second born will have only 50% of her time since he has to share it with his older brother, the third born only 33%, and so on. The fixed resource story is interesting because it could

perfectly replicate the birth order profile found in developed countries, i.e. birth order effects that are negative but convex.[13]

On the other hand, the present value of the financial resources that can be transferred across time (X_t) can be expressed as $X = \sum_t (X_t)/(1 + r)^t$, where r stands for the real interest rate. If the household is not credit constrained, the investments in each child should not depend on X_t but on X. However, if the household is credit constrained, it won't be able to smooth consumption (and the investments in each child) over the life cycle. Since credit constrains do not impede households to save but only to borrow from future income, per capita financial resources are more likely to increase with birth order (at any given age) due to credit constrains. In developing countries, this is further exacerbated by larger child labor rates (X_t would increase faster with birth order as older siblings enter the labor market).

Finally, in economies with high child labor rates, younger siblings could also benefit from having relatively more time to invest in education. A simple extension of the child labor model developed by Basu et al (1998) predicts that low income households are less likely to send younger children to the labor market when they reach the legal working age. The intuition is that later born siblings do not need to enter the child labor market in order to support younger siblings (as the per capita income of the household increases when older siblings enter the labor market). In other words, younger siblings not only benefit from older siblings wages but they are also more likely to stay in school at any given age.

The last two arguments indicates that either due to an underdeveloped credit market, higher share of informal jobs, or higher child labor rates, the amount of resources available to

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[13] In other words, there is a big penalty for second born children in comparison to their older siblings, an additional penalty (but smaller) for third born children vs. second born children, and so on.

invest in each child are more likely to increase with birth order in developing countries. This is illustrated by the green line of figure 3.1.

3.4 Data

Birth order, family background, and long term outcomes

In order to study long-term birth order effects on adult outcomes I need to follow individuals from their childhood –to observe their birth order and family background— into their adulthood. In other words, I need a long-term panel. The great advantage of using historic data is that after 72 years the census manuscripts become public, which allows researchers to have access to individuals' variables that are usually restricted, like their names, and follow them across census waves. Taking advantage of this feature, I link more than two million of children across the 1920 and 1940 Full Census Counts (FCC from now on) by using a standard matching algorithm based on first and last name, age, and state of birth (see, for example, Ferrie, 1996; Abramitzky, Boustan and Eriksson, 2012; Ferrie and Long, 2013). I then further link a subsample of these individuals to the World War II Army Enlistment Records to gather data on additional adult outcomes.

I restrict the sample to male children born in the US between 1895 and 1920, and that were still living with their families in the year 1920. There are two important caveats related to this sample. First, some families have not finished their fertility decisions by 1920, which could affect family size and the relative birth order of the children.[14] Second, some children might have already left their families, which could also bias the results. In order to limit both concerns, I further restrict the sample to children with mothers aged 35 to 45, since those

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[14] For instance, the "last born" children observed in 1920 might not be the actual if we are interested in comparing first born with last born children.

mothers are old enough to have likely finished their fertility decisions, but young enough to still be living with all their children.[15]

1920 Full Census Count

The 1920 Full Census Count gathers rich data on the demographic composition of the children's families (for the complete population of the US) , including their birth order, number of siblings, and their age and sex. Furthermore, recently, important variables measuring the family socio-economic background have been also digitalized, encompassing the occupation of each family member, whether they own their dwelling and whether they are illiterate or not.

1940 Full Census Count

As explained above, I link the children of the 1920 FCC to the 1940 FCC count where I can observe their adult outcomes at ages 20 to 45. The 1940 Full Census Count gathers data on both educational and labor market outcomes. The two outcomes considered in this study are the highest grade of completed education and earnings. Since the exact earnings are only reported for wage earners, I approximate the earnings of self-employed workers by their occupational earnings ("occupational score") computed by IPUMS, i.e. the median income for an individual in a given job category in 1950.

WWII army enlistment records

WWII army enlistment records contain data for more than nine million of individuals who enlisted in the army between 1938 and 1945. Among other variables, it reports the adult

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[15] All the key results reported in this paper are robust to further restricting the sample to children with mothers aged 35 to 40.

height of the individuals, which is a key variable to explore the channels that could be driving the birth order profiles.

States characteristics

One of the main objectives of this paper is to explore the relationship between birth order effects and the level of economic development of each state. I gather state level data on variables measuring key dimensions of economic development from both NHGIS and IPUMS. These variables include the population living in urban and rural areas, the level of mechanization of agriculture[16], and share of children employed as child workers, among others.

3.5 Results

Highest Grade of Completed Education

Given that most of the literature for developing countries has focused on educational outcomes, I start by analyzing birth order effects on the highest grade of educational attainment. In the following sections I also restrict the sample to white natives, but I extend the analysis in the last section to include blacks and immigrants.

North versus South

I first divide the country in three regions with substantial differences in their levels of economic development by 1920: the Urban North, the Rural North, and the South. Whereas the Urban North represents the most advanced area of the country, the South lags behind in

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[16] I define "mechanization of agriculture" as the share of the total assess of farms that is represented by "machines and implements":   mechanization of agriculture $\equiv$ (Implements and machinery / farms' total assets)

most of the key development indicators, such as the share of population living in urban areas, child labor and average education of the population.

In order to explore differences in birth order effects across these regions, I start by estimating a simple model for each region r and for each family size n:

$$YEDU_{iarN} = \alpha_a + \sum_{j=1,\ldots,N} I(birth\ order_{iarN} = j).\beta_j + AMB_{iarN}.\gamma + \varepsilon_{iarN} \quad (1)$$

$$R = Urban\ North; \quad Rural\ North; \quad and\ South$$

$$N = 2,3,\ldots,7 \quad \text{(number of siblings)}$$

Where $YEDU_{iaRN}$ stands for the highest grade of completed education of individual i of age a in region r and living in household with n children, α_a is an age fixed effect, AMB is the age of mother at birth, and ε is an error term. The main coefficient of interest is β_j, which captures the (non-linear) birth order effect for the sibling born in the jth order.

Figure 3.2 plots β_j for each region and each family size. This figure summarizes one of the main findings of this paper. By comparing these regions at one point in time, it is possible to observe the full transition from a "developing" to a "developed" birth order profile. I find that in the relatively modern urban north, the birth order effects resemble the sign and magnitude of the results found in development countries using contemporary data. Second born children face a relatively large penalty in comparison to the oldest sibling in the family. They accumulate between 0.2 to 0.4 years of education less than the first born children. The penalty increases with birth order but at a decreasing rate. For instance, third born children accumulate on average between 0.1 to 0.2 less years of education than the second born children, but the children born in the fourth place or later do not seem to face any additional penalty. As mentioned above, these findings are remarkably similar to those of

Kantarevic et al.(2004) and Booth et al.(2008) that use contemporary data for the US and Norway, respectively.

The picture changes dramatically, however, when I move to the rural north and the south. Whereas the rural north shows an inverted-U shaped birth order profile (with a flatter slope), the birth order effects becomes mostly positive when considering the south. When there is still small penalty for second born children, the penalty reverses and even became a "premium" for the children born later. Indeed, for most family sizes, the last born children accumulate around half year more of completed education than the first born children.

To sum up, when moving from the relatively modern urban north to the less developed south, the birth order profile switch from negative to positive birth order effects, with an inverted U-shape profile for the "hybrid" rural north

State level variation

To further analyze the relationship between birth order effects and economic development, I run state level regressions that relate these effects to a set of variables measuring key dimensions of development, such as the share of rural population or the child labor rate in the state.

I proceed in two steps. First, I estimate birth order effects at the state level. In this version of the paper I consider two summary measures "π_S" of the birth order effects: (i) the gap between last and first born children ($\beta_S^N - \beta_S^1$ using the notation of equation 1, where s indexes the state), and (ii) linear birth order effects (β_S^{Lin}).

Second, I regress:

$$\widehat{\pi_S} = \alpha \quad + \theta_1.(share\ rural_s) + \theta_2.(child\ labor_s)$$

$$+ \theta_3.(agriculture\ mechanization_s) \quad + \theta_4.(VA\ manufacturing_s)$$

$$+ \theta_5.(Popualtion_s) + \varepsilon_S \qquad (2)$$

with:

$$\widehat{\pi_S} = \widehat{\beta_S^N} - \widehat{\beta_S^1} \quad \text{(last - first born gap)}$$

$$\text{or} \quad \widehat{\pi_S} = \beta_S^{Lin} \quad \text{(linear birth order effects)}$$

I include as regressors the share of rural population in the state ($share\ rural_s$), the proportion of children aged 12 to 15 that were working during 1920 ($child\ labor_s$), a measure of agricultural mechanization ($agriculture\ mechanization_s$), the value added of the manufacture sector per thousands of inhabitants ($VA\ manufacturing_s$), and the population of the state in thousands of inhabitants.

To illustrate the variation exploited in the regressions, in figure 3.3 I plot the correlation between each measure of birth order effects and the share of population living in rural areas, which is probably one of the best indicators of the level of economic development of each state by 1920. This figure shows a strong positive correlation in both cases. In particular, the correlation coefficients are 0.59 and 0.63 for the last-born/first-born gap and the linear birth order effects, respectively.

Table 3.2 shows the estimated regressions.[17] Column (i) shows the results corresponding to the last born versus first born gap. As expected, this gap is larger in states with a large share of rural population. The coefficient is not only statistically significant but

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[17] In this version of the paper I do not adjust the standard errors to take into account that π_S was estimated in the first stage. This will be corrected in future versions of the paper.

the magnitude is relatively large. According to the estimations, the transition from a fully rural state to a fully urban state would increase this gap by about one year of completed education. In addition, states with higher child labor rates and less agriculture mechanization also have larger educational gaps between last and first born children. On the other hand, the value added by the manufacture sector (per thousand of inhabitants) is not correlated with this indicator. The results are similar when considering linear birth order effects, although child labor rates are no longer correlated with this measure.[18]

Poor institutions or poor population?

In the previous section I found that the states and regions that are lagging behind in terms of economic development are more likely to have larger (positive) birth orders effects, which is consistent with the findings for developing countries. These results could be explained by specific institutions of the "developing states", but they could also be explained by the fact that the populations of those states are just poorer. Indeed, some of the key mechanisms that could generate a positive birth order profile (e.g. large child labor rate or credit constraints) are more likely to be relevant for the poor.

In order to explore this hypothesis, I test whether birth order effects are heterogeneous by socio-economic background. To do so, I estimate the following model:

$$YEDU_{iN} = \alpha_a + \alpha_s + birth\ order_{iN}.\delta_1 + (birth\ order_{iN}).(X_{iN}).\delta_2 + X_{iN}.\delta_3 +$$

$$AMB_{iN}.\gamma + \varepsilon_{iN} \quad (3)$$

$$N = 2,3,\ldots,7$$

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[18] One potential interpretation of this result that the child labor channel is "non-linear", and has a large explanatory power for the gap between oldest and youngest siblings, but not to explain the full birth order effects. It can be shown that this is consistent with child labor models in which the household's decision to send a child to work is non-linear with respect to birth order (e.g. a child is sent to work only if there are no younger siblings to support).

Model (3) is similar to (1), but it includes an interaction term between the linear (for simplicity) birth order effects and some variables X measuring the socioeconomic background of the household in 1920. In particular, X comprises fathers' occupational earnings[19], literacy, and dwelling ownership, as well as a dummy indicating whether the child's mother is a widow. I also control for state fixed effects (α_s) in order to capture the within state variation in the family socio-economic background.

Table 3.3 reports the results. The first column shows the linear birth order effects with no interaction. I find that, on average, linear birth order effects are negative.[20] Columns (2) to (5) report the interactions with alternative characteristics of the parents. Regardless of the variable selected to approximate poverty, I find that poorer children have larger birth order effects. For instance, children whose fathers have lower earnings, rent the dwelling, or are illiterate have much larger birth order effects. Interestingly, these heterogeneous effects tend to vanish for large households (see figure 3.4). Since these estimates include state fixed effects, they suggest that the socio-economic background of the family do have an independent effect from the specific intuitions of the less developed states.

Other long-term outcomes

In this section I study birth orders effects on adult earnings and height. These dimensions have not been considered in the literature for developing countries, probably due to data limitations. For simplicity, I focus on specification (3)— i.e. linear birth order effects—but the conclusions are robust to alternative specifications.[21]

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[19] This variable was standardized to have mean zero and standard deviation equal to 1.

[20] In regressions not reported in this paper I show that indeed the effects are negative for small family sizes and positive (but small) for larger families.

[21] Results are available upon request.

Table 3.4 shows the results. Rows (i),(ii) and (iii) reports the regression coefficients for the urban north, the rural north and the south, respectively. In the first column I re-analyze the birth order effects on years of education with the new specification. It is worth to notice that indeed the linear coefficients depict a regional pattern that is similar to the one found with the non-linear specifications. The effects are negative and large in the urban north, they are much smaller (in absolute value) in the rural north, and they become positive in the south. This is also illustrated in the first panel of figure 3.5.

In the second column I report the results for adult earnings. Interestingly, while the results are consistent for the south, the sign of the birth order effects changes in the north. In this region I find now positive (although small) birth order effects.[22] For instance, a younger sibling earns on average 0.4% more than the immediately older sibling in the urban north. I will discuss in the last section alternative mechanisms that could explain why the additional formal education accumulated by older siblings did not lead to higher income in the north.

It is important to notice that, regardless the sign switch, the results are still consistent with the main hypothesis of this paper: the less developed the region, the larger the birth order effects. Whereas linear birth order effects are 0.4% in the urban north, they increase to 1% and 2.2% in the rural north and the south, respectively. This is illustrated in the second panel of figure 3.5.

The last column of table 3.4 reports the effects on adult height for the subsample matched to the WWII army enlistment records.[23] The variable was standardized to have a

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[22] As mentioned before, this result is robust to considering non-linear birth order effects, as well as alternative specifications (e.g. state fixed effects, etc.) and alternative measures of earnings (only occupational earnings, an hybrid measure that combines occupational earnings for self-employed and wages for wage earners, etc.) . Nevertheless, a closer look suggests that the switch in sign is driven by the larger families (results available upon request).

mean equals to zero and a standard deviation equals to one. I find that there are no birth order effects in terms of adult height. The coefficients are not only statistically insignificant but also small in economic terms. This is important since it allows ruling out some potential mechanisms behind the effects on education and labor outcomes. In particular, it suggests that the previous birth order profiles are not driven by changes in the per capita income of the household at key ages.

Immigrants and blacks

In this paper I focus the analysis mostly on white natives. Nevertheless, in this section I extend the analysis to study the birth order profiles of immigrants and blacks.

Table 3.5 reports the results for these samples. The first two panels show the regression coefficients for second generation immigrants classified by their mother's first language. Panel (a) corresponds to children whose mothers came from English speaking countries. I find that the birth order profile of these children is similar to that of northern white natives. Although younger siblings tend to accumulate less formal education than older siblings, this does not affect their relative earnings.

However, panel (b) shows that children whose mothers came from Non-English speaking countries have a profile similar to that of southern white natives. Later born children have more education and larger earnings than their older siblings. Furthermore, the magnitude of the birth order effects is much larger for this group. For instance, a higher birth order position increases the years of completed education by 0.2 and earnings by 3%. The stronger positive birth order effects for immigrants whose native language is not English

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[23] There could be some selection issues related to this sample since I did not restrict the analysis to the age groups for which enlistment was mandatory. In this version of the paper, I deal with the potential selection by including age fixed effects, but in future versions of this paper I will restrict the sample to the age groups were enlistment was close to universal.

could be explained by the externalities associated to the assimilation of their older siblings or their parents.[24] Alternatively, they could be driven by the lager child labor rates associated to immigrants.

Finally, I do not find significant birth order effects for blacks on education, earnings and height. To some extent this is due to the linear birth order specification. In additional regressions not reported here I do find a positive "last-born" effect for on both education and earnings.

3.6 Final comments

While birth order effects have been widely studied, less attention has been paid to the relationship between these effects and economic development. In this paper, I use US historical data to empirically test whether long-term birth order effects differ across the leading and lagging regions of the country in the Pre-War World II period.

In this version of the paper, in order to compare my findings with those of the developing literature, I focused the analysis on the educational outcomes of white natives. I find that in general, the "developing" south have larger birth order effects than the relatively modern north. In other words, in the south, younger siblings are more accumulate on average more formal education than their older siblings. This finding is consistent with the available evidence from contemporary data for developed and developing countries. I then exploit state level variation to show that birth order effects are positively correlated with the share of rural population, child labor rates and negatively correlated with the level mechanization in

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[24] Indeed, in regressions not reported here, I show that when the sample is restricted to children whose mothers arrived before age five, the birth order effects are reduced substantially.

agriculture. I also show that, regardless the state of birth, the effects tend to be larger for the poor.

I complement the analysis by looking at birth order effects on earnings and adult height. In terms of earnings, while I find consistent results for the south, the sign of the birth order effects changes in the north. In this region I find positive (although relatively small) birth order effects. There are alternative mechanisms that could potentially explain why the additional formal education accumulated by older siblings did not lead to higher income in the north. For instance, it is possible that the older sibling spend more time in household chores, which ends up eroding the return to education (since it could limit the time they can invest in education related activities). Second, education outcomes can be a limited measure of long-term wellbeing in semi-skilled economies. In these economies, it is possible that the more productive children drop earlier from school actually because they developed earlier the skills they need for the labor market. Finally, there could be certain traditions that may limit the returns to education for older siblings, like being less likely to migrate (in order to take care of the parents), or a larger probability of talking care of the family business (where the returns to education could be smaller). Regardless of the mechanism, the conflicting results in the north highlights the importance of considering a broader set of indicators beyond educational outcomes when considering birth order effects, something that has been mostly ignored in the developing literature.

There are at least three interesting extensions for this paper. First, in order to further explore if child labor is the mechanism driving the positive birth order effects in the south (or for the poor in general) I can exploit the rich micro-data on individual occupations (including children) which has been just digitalized for the 1920 full census count. In particular, I can

test whether birth order effects are larger in for those families whose older children are employed (even after controlling by fathers' occupational earnings).

Second, I can test whether credit constrains plays a role in shaping birth order effects. As mentioned above, an underdeveloped credit market may limit the possibility of smoothing the household's consumption (and investments) over the life cycle, which can lead to birth order effects. In regressions not reported in this paper, I tested whether birth order effects are correlated with the number of banks per inhabitant (at the state level). Although I did not find a significant correlation, this variable is a very rough measure of the level of development of the financial system. In future versions of this paper, I will consider richer measures such as the amount of loans per inhabitant.

Third, I find the largest birth order effects for second generation immigrants whose mothers came from non-English speaking countries. As mentioned above, these results could be explained by different mechanisms, including positive externalities at key ages associated to the acquisition of language skills by older siblings (or parents), and to the larger child labor rates among immigrants. To explore these mechanisms, I can compute: (a) a measure of the "linguistic distance" between the mothers' primary language and English (see for instance Isphording et al.,201;3 and Isphording et al.,2014), and (b) child labor specific rates (either at the household level or by sending country). By interacting these two variables with the birth order, I will be able to shed some light on which mechanism is more relevant.

Figures and Tables

Figure 3.1: Per capita resources by birth order at a given age a
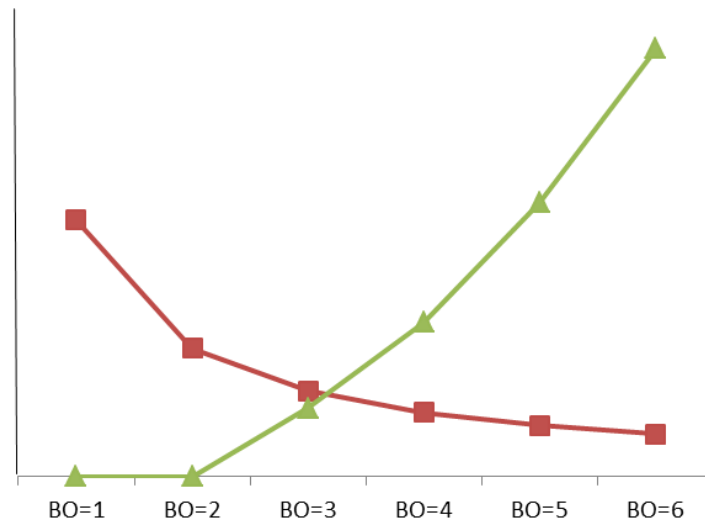
Notes: BO: birth order. Red line: earlier born siblings could benefit from larger per capita fixed resources: $\bar{M}/S$, with $\bar{M}$ = fixed resources, S = number of siblings. Example: $\bar{M} = m$others' time. Green line: later born siblings could benefit from per capita fixed resources that increase with birth order. Examples: older siblings' income, parents' income, more time available (e.g. less likely to be employed as child workers, household chores, etc.)

Figure 3.2: Non-linear birth order effects on highest grade of education, by region
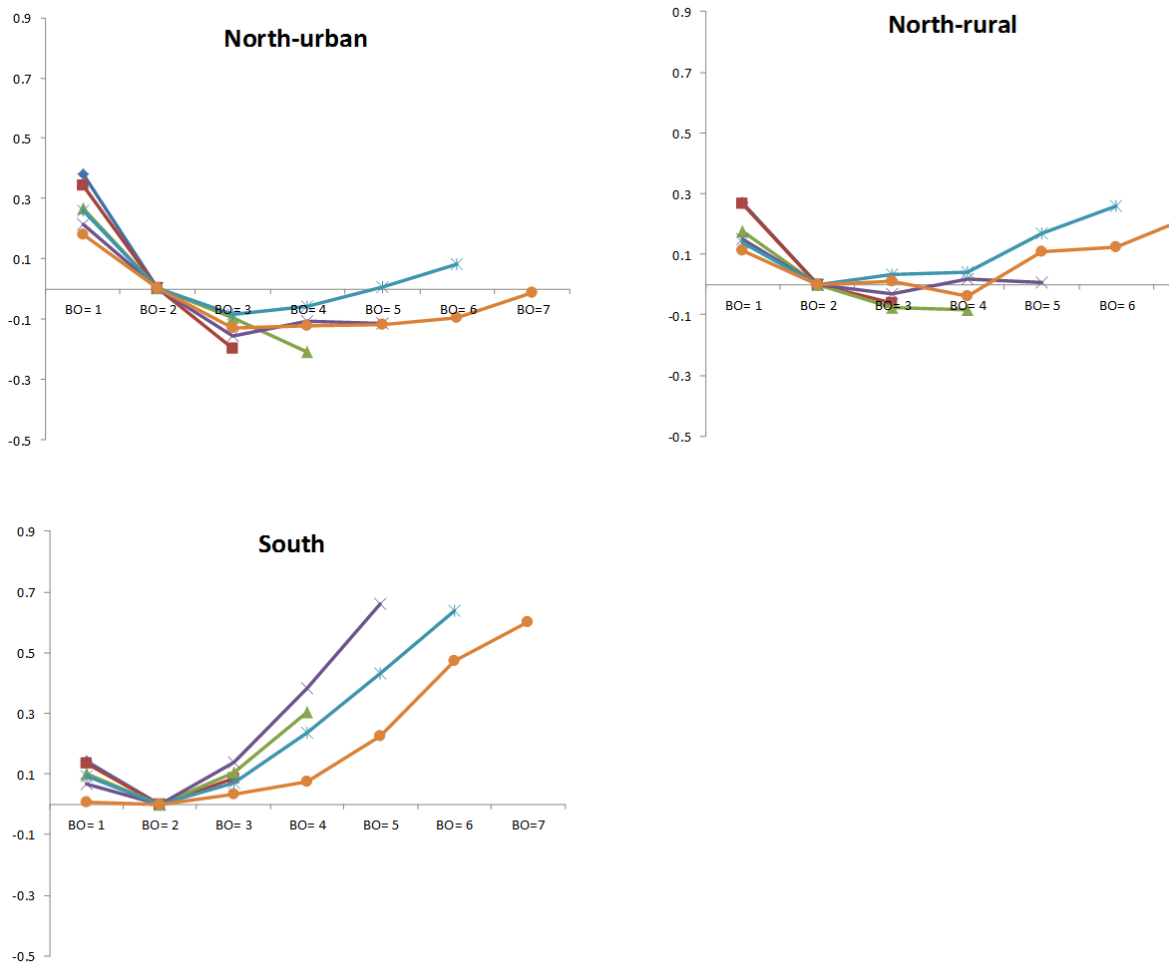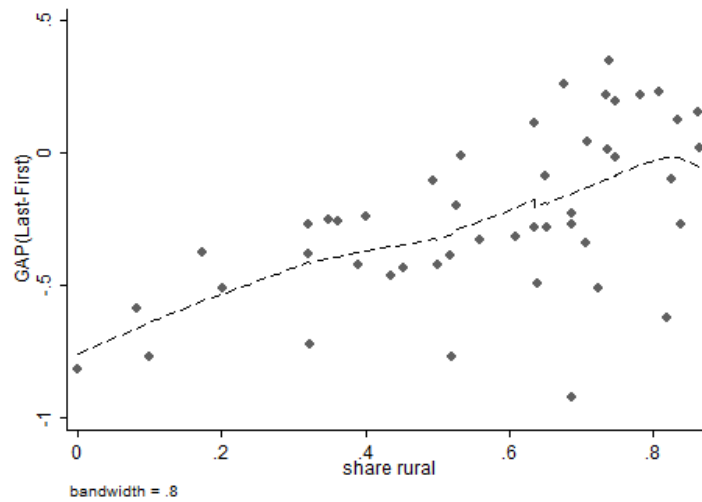

Note: This figure plots non-linear birth order effects on the highest grade of education. Each panel corresponds to a different region, and each color corresponds to an alternative family size. The non-linear birth order effects were obtained from an OLS regression of the highest grade of education on a full set of the birth order dummies. The model also includes age fixed effects and the age of the mother at birth. Sample: white natives born between 1895 and 1920
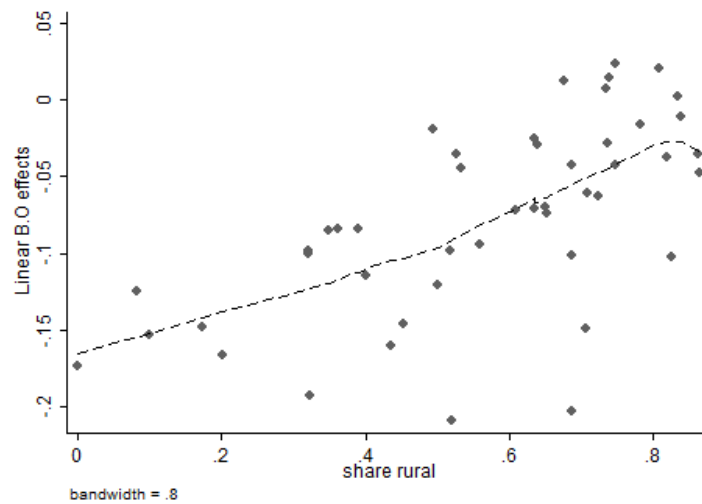
Figure 3.3: Birth order effects on highest grade of education and share of rural population in the state

**Panel (a): Last born- first born**

bandwidth = .8

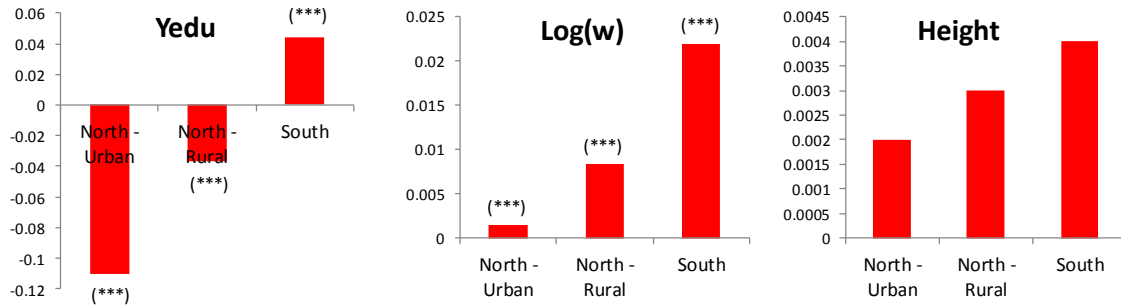**Panel (b): Linear birth order effects**

bandwidth = .8

Note: This figure show the correlation between alternative measures of birth order effects and the share of rural population in each state. The outcome is highest grade of completed education. In panel (a), birth orders effects are measured as the predicted education gap between the last born child and the first born child. In panel (b), I consider linear birth order effects. See the note on table 3.2 for more details. Sample: white natives born between 1895 and 1920

Figure 3.4: Linear birth order effects on highest grade of education. Heterogeneous effects by socio-economic background (for alternative family sizes)
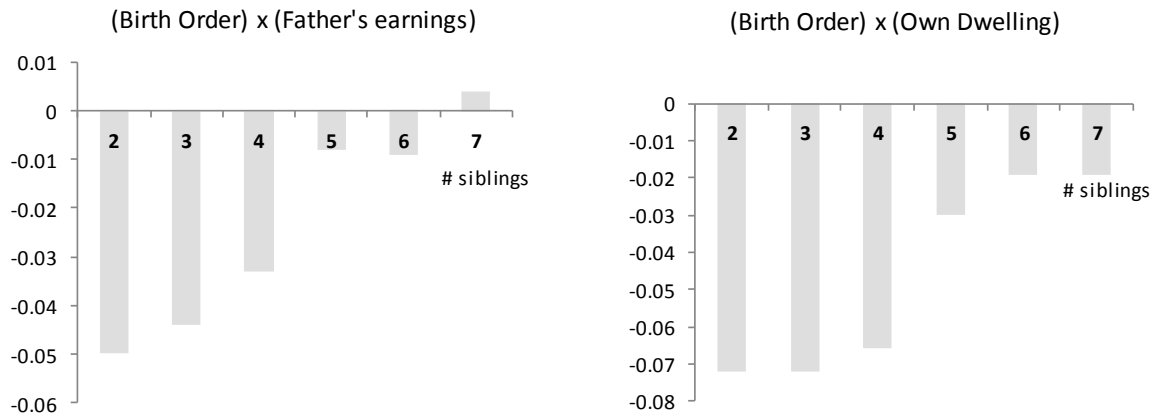
Note: The figure presents the interaction coefficients between a variable measuring birth order and different variables capturing the child's socio-economic background. Whereas the left panel report the interaction between birth order and the father's occupational earnings, the right panel reports the interaction between birth order and a dummy equal to 1 if the family owns the dwelling. Each bar corresponds to an independent regression for each family size. See the note on table 3.3 for more details. Sample: white natives born between 1895 and 1920

Figure 3.5: Linear birth order effects on adult outcomes, by region

Note: Each graph plots the linear coefficient corresponding to birth-order effects on different adult outcomes (for alternative samples). Each bar corresponds to a different OLS regression for each region. See the note on table 3.4 for more details. Sample: white natives born between 1895 and 1920

The outcomes are: Yedu = highest grade of education; Log(w) = log(occupational earnings); Height = Adult Height

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

Table 3.1: Literature review. Economic development and birth order effects

Country	Data	Outcomes	Results
Develpoped countries			
Norway (Black et al, 2005)	Large panel (full pop.)	Years of schooling, earnings, etc.	Negative B.O. effects (non-linear)
US (Kantarevic et al, 2004)	Panel (suplement of PSID)	Years of schooling, earnings, etc.	Negative B.O. effects (non-linear)
UK (Booth et al, 2008)	Panel	Highest educational attainment	Negative B.O. effects (linear)
Developing Countries			
Philippines (Ejrnaes et al, 2004)	Small panel (~800 obs)	Years of schooling, Hours in school	Positive B.O. effects (linear)
Brazil (Emerson et al, 2012)	Cross-section	School Enrollment, Child Labor	Positive B.O. effects (First/last born dummy)
Ecuador (De Haan et al, 2012)	Cross-section	School Enrollment, non-cognitive skills	Positive B.O. effects (non-linear)
Africa; 12 countries (Tenikue, 2012)	Cross-section	Years of schooling	Positive B.O. effects in poor families (linear)

Table 3.2: What explain birth order effects? State level regressions. Outcome: Highest grade of completed education.

	Birth order effects	
	(last-first)[a]	linear[b]
Share Rural	1.048	0.248
	[0.227]***	[0.049]***
Child Labor (12-15)	1.093	0.032
	[0.398]***	[0.086]
Mechanization (Agriculture)	-5.024	-1.006
	[1.528]***	[0.328]***
Value added - Manufactures/ population	-0.002	0.008
	[0.040]	[0.009]
Population (mill)	0.058	0.009
	[0.012]***	[0.003]***
Constant	-0.798	-0.177
	[0.168]***	[0.037]***
Observations	48	48
R-squared	0.75	0.71

Notes: Last-first: predicted education gap between last born child and the first born child (average across alternative family sizes)

(a) linear: predicted linear birth order effects.

The table presents state-level regressions on the determinants of birth order effects. The coefficients were obtained from two-stage OLS regressions. In the first stage, I estimate a measure of birth order effects $\widehat{\pi}_S$ using micro-data from each state (the measure of birth order effects is different in each column). In the second stage, I regress $\widehat{\pi}_S$ on state characteristics, including the share of rural population in the state, the proportion of children aged 12 to 15 that were working in 1920, a measure of agricultural mechanization, the value added of the manufacture sector per thousands of inhabitants, and the population of the state in thousands of inhabitants.

As mentioned above, each column has a different measure of birth order effects. For the first column, I regress in the first stage (for each state) the highest grade of education on a full set of dummies identifying birth order (for alternative family sizes), and then computed the mean predicted "education gap" for the last born child versus the first born child (I averaged this across different family sizes). I also included as controls a full set of age dummies and the age of the mother at birth. In the second column, I computed linear birth order effects. The model is similar to the one used in the first column but I also included a full set of dummies for family size. Sample: white natives born between 1895 and 1920. Standard between brackets.

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

Table 3.3: Heterogeneous birth order effects by socio-economic background. Linear birth order effects on highest grade of completed education.

	No interaction	X= (…)			
		Father's earnings	Own Dwelling	Illiterate Father	Widow Mother
Birth Order	-0.039	-0.033	-0.025	-0.037	-0.035
	[0.003]***	[0.003]***	[0.003]***	[0.003]***	[0.003]***
(Birth Order).(X)		-0.028	-0.024	0.101	0.069
		[0.002]***	[0.003]***	[0.008]***	[0.008]***
Observations	1,547,152	1,316,919	1,510,837	1,432,537	1,547,152
R-squared	0.11	0.16	0.12	0.13	0.11

Note: The table presents the linear birth order effects on the highest grade of education interacted with different variables capturing the child's socio-economic background. The coefficients were obtained from an OLS regression of the highest grade of education on a variable measuring birth order, which was also interacted with: (a) fathers' occupational earnings (standardized to have the mean equals to zero and the standard deviation equals to one), (b) a dummy equal to 1 if the family owns the dwelling, (c) a dummy equal to 1 if the father was illiterate, (d) a dummy equal to 1 if the mother was a widow. The model also includes state fixed effects, age fixed effects, the age of the mother at birth, and a full set of family size dummies. Sample: white natives born between 1895 and 1920. Standard between brackets.

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

Table 3.4: Linear birth order effects on earnings and adult height, by region.

	Yedu	Log(w)	Height
(i) North, Urban	-0.11	0.004	0.002
	[0.005]***	[0.001]***	[0.005]
(ii) North, Rural	-0.037	0.01	0.003
	[0.004]***	[0.001]***	[0.005]
(iii) South	0.044	0.022	0.004
	[0.006]***	[0.002]***	[0.005]

Note: The table presents the linear birth-order effects on different adult outcomes for alternative samples. Each coefficient was obtained from a different OLS regression of the adult outcome on a variable measuring birth order. The model also includes state fixed effects, age fixed effects, the age of the mother at birth, and a full set of family size dummies. The outcomes are: Yedu = highest grade of education; Log(w) = log(occupational earnings); Height = Adult Height. Sample: white natives born between 1895 and 1920. Standard between brackets.

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

Table 3.5: Linear birth order effects on adult outcomes. Immigrants and blacks born between 1895 and 1920

	Yedu	Log(w)	Height
(a) Second generation Immigrants, English			
Linear birth order effects	-0.038	0.004	0.002
	[0.010]***	[0.003]	[0.009]
Observations	91,200	86,602	14,564
R-squared	0.07	0.12	0.02
(b) Second generation Immigrants, Non-English			
Linear birth order effects	0.189	0.032	0.009
	[0.006]***	[0.002]***	[0.005]
Observations	289,846	276,550	41,965
R-squared	0.07	0.13	0.02
(c) Blacks-Natives			
Linear birth order effects	0.015	0.004	-0.011
	[0.010]	[0.003]	[0.009]
Observations	115,396	108,572	15,846
R-squared	0.13	0.05	0.01

Note: The table presents the linear birth-order effects on different adult outcomes for alternative samples. Each coefficient was obtained from a different OLS regression of the adult outcome on a variable measuring birth order. The model also includes state fixed effects, age fixed effects, the age of the mother at birth, and a full set of family size dummies. The outcomes are: Yedu = highest grade of education; Log(w) = log(occupational earnings); Height = Adult Height. Standard between brackets.

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

CHAPTER 4

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