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Compulsory Schooling, the Family, and the “Foreign Element” in the United States, 1880-1900

Jeff Lingwall¹

Abstract

This paper uses exogenous variation in family schooling decisions induced by compulsory schooling laws to study the intra-family allocation of education among children. Using both administrative and self-reported data from the census, evidence is given that compulsory schooling laws increased attendance between 1880 and 1900, particularly for the children of immigrants who were a special target of attendance laws. The effect of the compulsory schooling law on the individual is then compared to the influence of the average number of children in the home affected by a law. Evidence is given that some increased attendance came at the cost of siblings’ attendance, with family allocation of schooling determining the ultimate impact of the law. The family effects of the law are strongest on the children of immigrants.

1 Introduction

The development of the educational system of the United States in the late 19th century has long been of interest to scholars, both from a historical perspective and as a laboratory for policy experiments. Economists have used variation in state policies affecting children to explore fertility decisions (Puerta, 2009), the results of child labor laws (Moehling, 1999), and the effects of compulsory schooling (Margo and Finegan, 1996). One area that has recently received attention is the use of variation in state laws to

¹jlingwal@andrew.cmu.edu
www.andrew.cmu.edu/user/jlingwal

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explore how the employment of children affects the labor market decisions of their siblings. Manacorda (2006) compares households with varying numbers of children under child labor restrictions in 1920. He finds evidence of positive spillovers from the labor supply of children which affected the school attendance and employment of siblings.

Do families strategically allocate schooling among children? Understanding how families distribute schooling and work among children improves understanding of the consequences of legislation targeting children. For compulsory schooling and child labor legislation to be effective, and to understand whether such restrictions should exist at all, policy makers must make assumptions about the causes and consequences of school attendance and child labor. If policy makers assume that “child labor in developing countries is nearly always a form of child abuse (Edmonds, 2004),” and that compulsory schooling or child labor bans might shift economies toward more desirable equilibria (see Basu and Van, 1998), then legislation appears justified. On the other hand, if parents use the labor of some children to advance the interests of their siblings, then public policy interventions might be less justified and even backfire, as families are unable to use the labor of some children to fund the education of others.

This paper contributes to the literature on household allocation of schooling by examining how families responded to exogenous pressure from compulsory schooling laws. The paper focuses on the era when initial compulsory schooling laws were introduced in the majority of states outside the South, and when historical evidence begins to point at their effect, from 1880 to 1900. In many ways, the United States during this period serves as an ideal setting for examining how families respond to legislation. Many children were in school, but attendance rates still had room to grow in response to attendance laws. Considerable variation existed across the laws themselves, with exactly half of states outside the South containing a law in 1880. Finally, recently released census samples for these years are large enough to allow good examination of minority groups, such as the children of immigrants.

Using difference-in-difference estimation across states, the paper first

gives evidence that compulsory schooling laws increased attendance, especially among immigrant families. Given that many compulsory schooling laws were created with the intent to assimilate immigrant children, the “foreign element,” it is surprising that the literature on the history of compulsory schooling largely ignores this group. Historical evidence exists that immigrant children were brought into school by the efforts of truancy officers in some states, and the econometric evidence from compulsory schooling laws confirms this. I find that compulsory schooling increased attendance of immigrant children by up to 9% over the two decades considered.

Using this variation in family schooling decisions induced by the law, the effect of compulsory schooling laws on the individual is then compared to the influence of the average number of children in the home affected by a law. An economic model of parental decision making predicts that compulsory schooling laws should change the cost and benefit of schooling for all children in the home. Using census data on household relationships, evidence is given that some increased attendance came at the cost of siblings’ attendance. Specifically, as children came under a compulsory schooling law, the average number attending in the family increased by less than would be predicted by the individual effect of the law, showing that family allocation of schooling determined the ultimate impact of the law.

2 *A model, the laws, and the data*

2.1 *A model of parental decision making*

A simple model of parental decision making, adapted from Edmonds (2007), illustrates the basic reasoning behind the analysis in the paper. Consider a family of one parent with some exogenous income Y and utility over the family’s standard of living and the quality of children. For simplicity, fertility is exogenous.² The parent’s utility function is represented with $u(S, \overline{V}_K)$,

²The number of children k could be incorporated into the model of section 2.1 by considering two different periods of decision making. In period 1 parents decide on the number of children to have based on their expectations about future standards of living and costs of schooling. For example, in the first period, if all future children were expected

where S is the current standard of living, and $\overline{V}_K = V_1 + \dots + V_K$ is the sum of the future welfare V_k of each of the K children. Each child's time constraint is represented by $E + M + H = 1$, where E is time spent on education, M is market work, and H is household work.

The child's future welfare is a function of education, given by $V_k = R(E_k)$ with $R' > 0$, $R'' < 0$. In other words, education has an increasing but diminishing effect on each child's returns to education and future welfare. In the market each child can earn wage w_k , where the wage is increasing with the age of the child. Each child also has costs the family p_k to support. The household consumes a good c , which must be greater than some subsistence level \underline{c} and faces a budget constraint net of all child income and costs

$$\underline{c} \leq c \leq Y + \sum_k w_k M_k - \sum_k p_k \quad (1)$$

The standard of living S is a function of consumption of c and the sum of home production H , $S = F(c, \sum_k H_k)$ The parent thus faces the following problem:

$$\max_{E_k, M_k, H_k, K} u(F(Y + \sum_k w_k M_k - \sum_k p_k, \sum_k H_k), \sum_k R(E_k)) \quad (2)$$

subject to the constraint on each child's time use and the budget constraint. If the parent places no value on the future welfare of the children then children will perform market work unless home production is more valuable.³ More generally, a child will attend school instead of working if

$$\frac{\partial u}{\partial V} \frac{\partial R}{\partial E_k} \geq \frac{\partial u}{\partial S} \frac{\partial F}{\partial c} w_k \quad (3)$$

that is, if the marginal utility from the extra consumption brought by the

to work, then the parent would find $\frac{\partial E(S)}{\partial k} > 0$, and might be expected to have more children than a parent in later years when child labor declined and compulsory schooling laws made it increasingly costly to have children. Puerta (2009) discusses the effect of compulsory schooling on fertility in the United States.

³This model omits the possibility of immediate returns to education, that is, if a child would have immediately higher wages as a consequence of some school attendance then parents would have an additional reason to send them to school.

child's work is less than the marginal utility from their school attendance.

Since w_k is increasing with age, older children will be more likely to work. As the older children work family income rises, so that younger children will face a weaker budget constraint and be more likely to attend school. Since policies targeting some children in the household change the marginal cost and benefit of schooling for all children, policies targeting a subset of the children indirectly affect their siblings. We also expect different effects of policies on immigrant families, because of both income differences between native and immigrant families, and since it is widely believed that immigrant families in the early United States had different valuations of the benefits of schooling (for instance, see LaGumina, 2000).

2.2 *State compulsory schooling laws*

One of the most common policies targeting children over this period was compulsory schooling: legislators believed children ought to be in school up to a certain age, and they expressed this age through compulsory schooling laws. Compulsory schooling laws gave an entry age to be in school and a maximum age to leave school, with possible exceptions. The laws differed widely between states in their age limits and exemptions. For example, New Jersey required 20 weeks from 7-12 year olds in 1900, while Kentucky required 8 weeks from 7 to 14 year olds (United States Department of Education, 1901).

In many state, compulsory schooling laws were particularly aimed at the children of immigrants. Catholic immigrants from Europe needed education in public schools– the foreign born needed assimilation into American Protestant culture. (see Richardson, 1980; Eisenberg, 1988 for a discussion on the passage of compulsory schooling laws). Immigrant children were more likely to work than their native counterparts, and much less likely to attend school during their teenage years. Compulsory schooling could be seen by legislatures as solving two problems at once: the evils of child labor and the dangers of alien culture and religion.

Table 1 shows the distribution of the maximum age of compulsory school-

Table 1: The maximum age for compulsory schooling

Age	1860		1870		1880		1900		1910	
Region	N	S	N	S	N	S	N	S	N	S
0	31	17	30	16	16	16	2	13		7
12							1	1	2	4
13							2			
14	1		2	1	12	1	20	3	29	6
15					2		3			
16					2		4		1	

Notes: Each cell shows the number of states in the North (N), i.e. outside the South, and in the South (S) with that age of compulsory schooling law.

ing across time. Each cell shows the number of states with the given maximum age for compulsory schooling. N shows laws in the North (outside the South), S shows laws in the South. In many states, compulsory schooling laws contained an exemption that allowed working children to leave school earlier than others. In these cases, I have used the lower “dropout” age.

The modal age of the state laws in most years was 14, the age later pushed by the National Child Labor Committee as a standard age at which children should be allowed to work. The South lagged the rest of the country in the adoption of compulsory schooling laws, the District of Columbia being the earliest adopter in the South, decades before any other state. For 1900, and 1910 detailed compilations of state compulsory schooling laws are available from Goldin and Katz (2003). I have supplemented this with age limits from reports in the Commissioner’s Report on Education in various years. Table 2 in the Appendix shows the details on the age limits of laws across states and years.

Compulsory schooling laws were often complemented with other laws. For example, child labor laws restricted employment in industries for children under a certain age; industries likely to be targeted were factories and mercantile establishments. Legislation also removed children from dangerous occupations, such as mining and tight-rope walking, and from morally hazardous occupations, such as jobs in breweries or performing “messen-

ger duty to houses of ill-repute (Stigler, 1950 and U.S. Dep. of Education, 1900).” As of 1900, about half of the states with child labor laws had age limits that coincided exactly with the compulsory schooling age, and for simplicity this paper focuses on compulsory schooling alone.

The historical record on the effect of compulsory laws is mixed. In some states, the laws were apparently not well-enforced. For example, the 1890 Report of the Commissioner of Education of the United States provides summaries of compulsory schooling in many states up to that point. The compulsory schooling law was a “dead letter” in California, Kansas, Montana, Nevada, New Hampshire, Washington, Washington DC, and Wyoming. This view dominates the perspective of traditional historians (see Stambler, 1968 and Tyack, 1987).

In other states, however, schooling laws appear to have been taken seriously. In Colorado, “Compulsory education has been much more effectively enforced” by 1896 (CRE, 1898). The Connecticut Board of Education reported success in using truant officers to bring Italian immigrants to school (Connecticut, 1894). In Kentucky, the 1894 school report remarked that “this largely increased enrollment and attendance . . . were undoubtedly due, in a large measure, to the Hiles’ Compulsory Law.” In Pennsylvania a “strenuous effort” was made to enforce compulsory schooling on the children of immigrants (Pennsylvania, 1906). In Utah, pressure from the federal government helped make schools both free and compulsory in 1890, and the Governor reported in 1892 that, “In Salt Lake City the number of pupils seeking admission is beyond the capacity of the school buildings, and the trustees are compelled to rent private buildings.”

Compulsory schooling laws became more effective as they were gradually revised and better enforced over time (CRE, 1918). For example, in Massachusetts the first compulsory schooling law was passed in 1852, which apparently attracted little attention. Truant laws were formed into a law in about 1859. As late as 1866 as there was no penalty annexed to the failure to appoint truant officers the requirement was largely ignored. Yet as time passed the law was taken seriously. In 1873 town treasurers were fined for neglect to enforce the law. In 1876 a child labor law was passed that

worked in tandem with the compulsory schooling law. In 1878 a provision was passed that towns could lose funds if they did not comply with the truant law. In 1898 Massachusetts’s child labor, attendance, and truancy laws underwent revision after a study commissioned by the state legislature, after which they were further modified (Ensign, 1921).

This paper focuses on the introduction, and changes to, compulsory schooling laws between 1880 and 1900, since almost all states outside the South contained compulsory schooling laws by 1900, and the historical evidence first begins to confirm that laws were effective in some states during this period. Since the South differed dramatically, both in terms of public educational systems and compulsory schooling laws, the paper focuses on the effect of laws outside the South.

2.3 *Census data*

Data on the schooling and employment of children come from the IPUMS public use samples of the United States Census (Ruggles *et al.*, 2008). Each census contained a question about school attendance to the effect of “Did this person attend school?” The 1900 census was an exception to this rule, as months of school attendance were recorded. For comparability between years, I measure school attendance in 1900 as an individual being listed with positive months of attendance and school attendance in other years as a positive response to the school question.

The timeframe considered in the response to the school question differed by year, from the previous four months to the previous twelve months. Families were also asked about the employment of their children (for children over 9 in 1880 and 1900). I have counted children listing “Student” as their occupation as having attended school at some point over the last year. Table 2 summarizes the data used for each census year. For 1880 and 1900, the years focused on in the paper, the samples contain information on between 1 in 10 and 1 in 20 children in the United States.

The census data are not without problems. Moehling (2003) notes the emphasis of the occupation question changed between census years and that

Table 2: Census data on child labor and schooling

Year	Sample	Enumeration	School time period	Occupation
1870	1 in 100	June	June to June	All children
1880	1 in 10	June	June to June	Children over 9
1900	1 in 20	June	June to June	Children over 9
1910	1 in 100	April	September to April	All children
1920	1 in 100	January	September to January	All children

Notes: Census data are from the IPUMS samples of the United States census (Ruggles *et al.*, 2004).

in 1910 children were more particularly questioned about occupation, introducing possible differences between years due to changes in wording or enumerator instructions. Margo (1990) finds evidence of an undercount of black school attendance in 1900 based on the wording of the attendance question. Also, it is impossible to tell from the census how a child divided his or her time: whether a child went to school full-time or part-time.⁴ Each of the censuses recorded children’s occupations, but I cannot tell whether those who reported both employment and school attendance engaged in these sequentially or simultaneously. For example, a child may have been employed during the summer vacation in 1900, and thus be listed with an occupation while he or she studied full-time during the school year. Also, due to the short term requirements, a child in 1910 may have already finished a full-time school term and found employment by the April 15th enumeration date.

To give a brief description of attendance across the country over this period, Figure 1 uses the census data to show the attendance rates of white children in 1880, separated by native and immigrant children and by urban

⁴The enumeration instructions for students in 1900 were, “Report a student who supports himself by some occupation according to the occupation, if more time is given to that, but as a student, of more time is given to study. Thus report a student who does stenographic work as a student unless more of his time is spent in stenography. Report a salesman in a grocery store, who attends a night school as ‘salesman, groceries,’ because most of his day is spent in the store (enumerator instructions in Ruggles *et al.*, 2008).”

and rural areas. A child is counted as foreign if either parent, or the child, was born outside the United States. The profiles of attendance by age differ markedly between native and foreign born children at ages over 10, with foreign born children attending less frequently than their native counterparts, especially among older children. A marked difference also persists between urban and rural attendance, with younger urban children more likely to attend than rural children, but older rural children more likely than rural children. This reflects the difference between urban and rural schooling. Urban schooling was more likely to be in a graded system help for a long period of time during the year. Rural schooling involved fewer months of attendance, mostly during the summer and winter, in a one-room schoolhouse (Fischel, 2009). As a result, they attended for fewer months of the year than urban children, but also until a later age.

2.4 *Were compulsory schooling laws effective?*

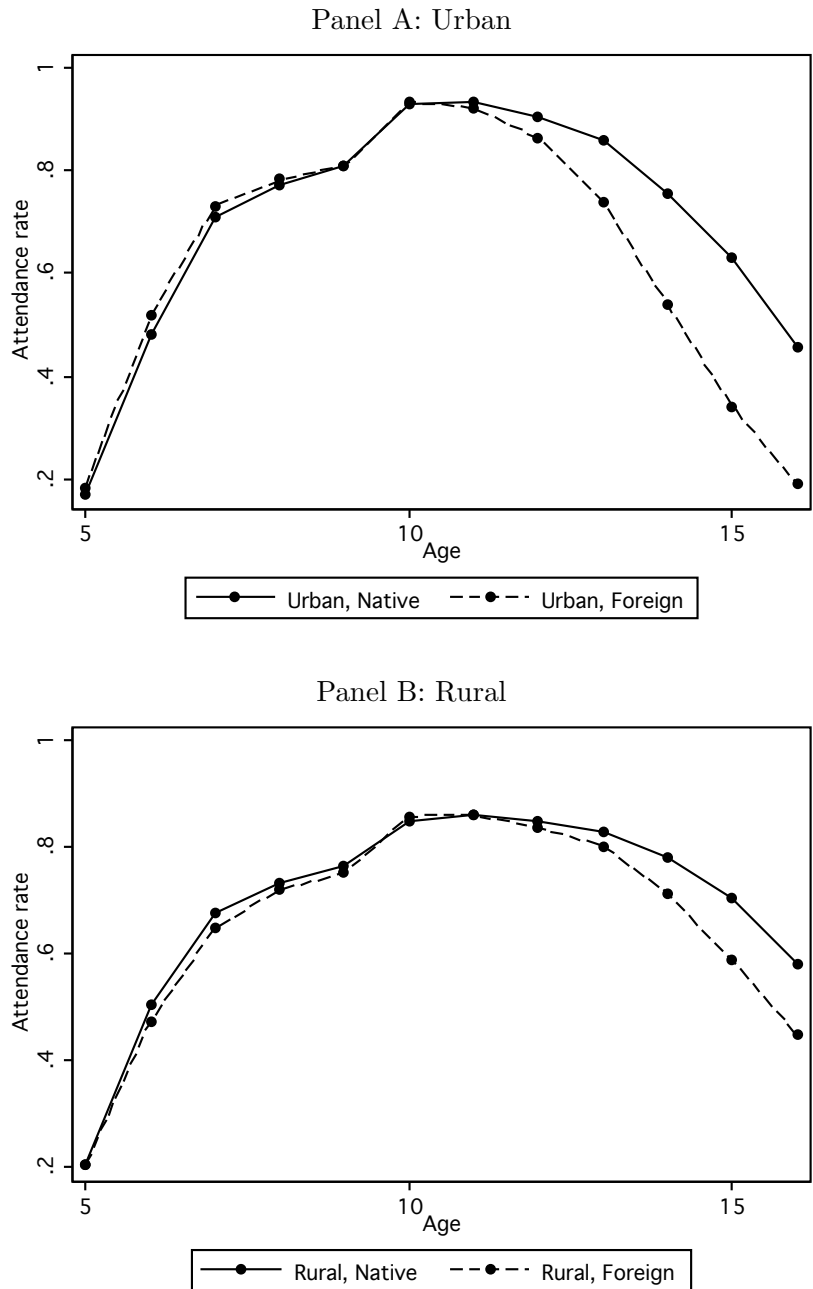
This section uses difference-in-difference estimation to examine whether compulsory schooling laws were effective, with particular emphasis on the children of immigrants. The general specification is

$$y_{asti} = \beta_0 + \beta_1 Law_{ast} + state_s \times age_a + year_t \times age_a + X_{asti} + u_{asti} \quad (4)$$

where y_{asti} is the school attendance (0/1) of a child i of age a in state s , at time t . Law_{ast} indicates whether a compulsory schooling law affected children of that age, in each state and year. State and year fixed effects are included, each interacted with age, so that the coefficient on Law is the pooling of a difference-in-difference estimate on each age. The estimate compares the change in attendance for children of age a in states that passed laws affecting them with the change in rates for children of age a in states that did not pass laws. The year by age fixed effect serves as the counterfactual change in attendance, what the treated children of age a would have experienced in the absence of passing a law.

Finally, X contains a vector of covariates that might affect school atten-

Figure 1: School attendance rates of white children outside the South, 1880



Source: IPUMS sample of the United States census

dance. Covariates include whether the child was foreign born or had foreign born parents, lived in an urban area, lived on a farm, the number of siblings, the age of the household head, the occupational class of the household head, and the proportion of children in the home in each age by sex cell. The standard errors are clustered at the state level. These regressions are for white children ages 10-16, who lived with both parents, both to parallel the later family level regressions, and because poverty exemptions in schooling laws explicitly allowed children of widows out of school. Since immigrant children were often the target of compulsory schooling, the regressions interact *Law* with immigrant status, counting a child as foreign if the child or either parent was born outside the United States.

Tables 4 contains the difference-in-difference estimates. Columns (1) and (2) show the aggregate results. Column (1) includes fixed effects but no additional covariates, and Column (2) includes all covariates described above. The laws appear to increase attendance by around three percentage points, a result consistent with literature examining the laws in later periods (Lleras-Muney, 2002, Goldin and Katz, 2003). Column (3) interacts law with an indicator for immigrant status. The effect of compulsory schooling on native children becomes insignificant, while a large effect, around 9%, is shown for immigrant children. To further probe the experience of immigrant families, Column (4) shows regression results from interacting the foreign born variables with a dummy for whether the family lived in an urban area. The effects on foreign children are largest in urban areas, consistent with the tradition that enforcement of compulsory schooling worked best in the cities. The Appendix contains regression results for additional decades, with and without the South.

To confirm the results of the difference-in-difference estimation, an additional analysis is run using biyearly administrative from the Commissioner's Report on Education. State reports to the Bureau of Education are available at least every two years between 1870 and 1920 and cover several school-related variables such as enrollment, attendance, and spending, but offer no demographic information.⁵ In general, administrative data is likely to be

⁵Data on attendance, enrollment, school term, teachers, and expenses were mainly

Table 3: Effect of compulsory schooling on attendance, 1880-1900

	(1)	(2)	(3)	(4)
Law	0.028** (0.012)	0.027** (0.011)	-0.014 (0.012)	-0.014 (0.014)
Law * Foreign			0.088*** (0.011)	0.068*** (0.011)
Law * Urban				0.007 (0.011)
Law * Foreign * Urban				0.036*** (0.013)
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Demographic Controls	N	Y	Y	Y
Observations	644,644	644,644	644,644	644,644
R^2	0.166	0.299	0.302	0.302

Notes: The dependent variable is school attendance (0/1) of children. Regressions are for white children ages 10 to 16, with standard errors clustered by state.

of higher quality than self-reported data for general attendance numbers, though the lack of family structure and demographic-specific attendance rates limits its use to this paper. In some years, states report numbers as estimates, which I have taken at face value.⁶

The results from the state-aggregated administrative data for 1880 to 1900 are shown in Table 4. Here, in addition to state and year fixed effects, a linear state time trend is included for each state. Demographic controls for percentage white, urban, and foreign born are interpolated from the census data to the bi-yearly level. The dependent variables examined are public school enrollment, average daily attendance levels, average days of school attended in the state (out of 180), the average number of days school was held, and per-child public school expenditures. The Bureau of Education data give aggregate enrollment numbers for these variables across all ages, which were then divided number of 5 to 17 year olds (the widely held “school age” of the time) interpolated from the census.

The coefficient on enrollment levels, per 5-17 year old in the state, is positive and significant, indicating an increase in public school enrollment of around 2% caused by the laws. This increase comes after adjusting for individual state time trends, and state and year fixed effects accounting for differences in attendance across states and over time. The coefficients on attendance of enrolled children and other educational outcomes are all positive but insignificant in the regressions.

In summary, difference-in-difference regressions show the laws seem to have significant effects on school attendance of 10-16 year olds, with the effect coming mostly from children who were foreign born or had foreign born parents. This result should be of interest to researchers studying compulsory schooling, as the direct effect of the law on immigrant children has never been

entered from the Commissioner’s Report on Education for the years from 1881 to 1899. Data were entered for every odd school year. Due to difficulty obtaining the complete set of reports, the data were entered from the printed volumes, scans from the Hathi Trust Digital Library, Google Books, and reprints of the data in the Statistical Abstracts of the United States Census. In some cases, when data in later years of the Commissioner’s Report varied from the data in the original report, the later data were assumed to be more accurate.

⁶Additionally, separate data for North and South Dakota do not exist for many years.

Table 4: The effect of compulsory schooling laws, bi-yearly administrative data, 1880-1900

Dependent Variable	(1) Enrollment	(2) Attendance	(3) Days	(4) Term	(5) Expenses
Law	0.019** (0.009)	0.004 (0.007)	0.012 (0.007)	3.651 (2.152)	0.037 (0.044)
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
State time trends	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y
Observations	276	276	276	276	276
R^2	0.999	0.998	0.997	0.999	0.999

*** p<0.01, ** p<0.05, * p<0.1

Notes: Regressions use non-South state-aggregated data on average daily attendance, with standard errors clustered by state. Regressions are weighted by the number of children of school age in each state. Demographic controls are interpolated from census data.

shown. As an additional check on whether the laws were effective, a second data source, bi-yearly administrative data, show a positive and significant effect on public school enrollment, and positive, albeit insignificant effects, across a range of other educational outcomes, though if administrative data contained educational variables on immigrant children the results might be somewhat different.

3 *Compulsory schooling and the family*

Since compulsory schooling laws appear to have influenced the school attendance of children, they provide exogenous variation in school attendance among families that can be used to study how families allocate schooling among children. Compulsory schooling laws in families play two roles. First, for a child under the age limit there is a direct effect of compulsion, increasing the probability of school attendance. Second, there is an indirect effect

on the other siblings not under the age limit. Since full-time school attendance restricts the labor of some children, for the family to achieve the same level of income other siblings might need to go to work, thus decreasing their attendance.

In terms of the model from Section 2, compulsory schooling laws change the marginal cost and benefit of schooling for all children in the family, not only the children of ages targeted by the law. A specific example might be the interplay between male and female children. If an age 14 law was passed, then a family which had previously been working a 13 year old boy and not a 15 year girl might switch their roles, sending the girl to work and keeping the boy at home. The average impact of the child labor law is thus mitigated because of correlated family decision making.

To assess this possibility, I follow Manacorda (2006). School attendance is regressed on both individual school compulsion and the proportion of children in the home under a compulsory schooling law. I estimate

$$y_{asti} = \beta_0 + \beta_1 Law_{ast} + \beta_2 \overline{Law}_{sth} + state_s \times age_a + year_t \times age_a + X_{asti} + u_{asti} \quad (5)$$

where y_{asti} indicates whether child i of a household of age a in state s is at school, Law_{ast} is the presence of a law affecting children of age a , and \overline{Law}_{sth} is the average number of children in household h under a compulsory schooling law. The coefficient β_1 is a measure of the direct impact of the law on those targeted by the law. The effect of the laws is identified as a difference-in-difference estimate, as in Section 2. Then, β_2 is a measure of the indirect impact of the law on family members. For control variables, the specification is the same as in Section 2, controlling for household demographics, family structure, and for the proportion of 10-16 year old children in the home in each age by sex cell. This last controls for the effect of age structure of the children on family decision making.

The coefficient β_2 on \overline{Law} tests for indirect effects of the laws on family members, and thus gives evidence of family allocation of schooling changing in response to a law. The coefficient is identified from the interaction of the age structure of the children in the home with compulsory schooling laws.

In order for the same states to identify the direct and indirect effects of the laws, \overline{Law} is constrained to be zero for states with existing laws in 1880 that held the age limit constant until 1900. After controlling for the direct effect of the law on individuals, \overline{Law}_{sth} asks what the effect of average compulsion was in the family. A negative coefficient indicates that trade-offs occurred in the family, with the increased attendance of some children paid for by the decreased attendance of others.

To better understand the identification on β_2 , consider a regression of average attendance on average compulsion within the family.

$$\overline{y}_{sth} = \delta_0 + \delta_1 \overline{Law}_{sth} + state_s \times age_a + year_t \times age_a + X_{sthi} + u_{sti} \quad (6)$$

where δ_1 is an estimate for the effect of average within-family schooling restriction on average attendance rates. The effect of \overline{Law} on the individual, from Equation (5), becomes the difference $\delta_1 - \beta_1 = \beta_2$, the difference between the effect of a law across households and on the individual within households.⁷ Manacorda (2006) attributes this difference between the individual and average effect of the law to positive spillovers in the labor supply of children in the home, with consequences for school attendance.

Table 5 contains estimates of Equation (5) for white children ages 10-16 outside the South. The sample is restricted to those who lived with both parents, since very different factors would have influenced the attendance of orphan children or the children of widows. The structure of the table parallels Table 4. Columns (1) and (2) show the aggregate effect of the law on the family. The estimate on \overline{Law} is negative and significant in Column (1), which contains no covariates, but this aggregate coefficient becomes insignificant in Column (2) as controls for household structure and demographics are introduced. Columns (3) and (4) interact the effect of the law with foreign born and urban status. The coefficients on \overline{Law} are generally negative and significant, consistent with an economic model of the

⁷One can also think of Equation (5) as the reduced-form equation of a two-stage least squares regression of individual school attendance on average school attendance in the family, using the proportion in the family under a law as an instrument for the proportion of children attending school.

Table 5: The effect of compulsory schooling within the family

	(1)	(2)	(3)	(4)
Law	0.048*** (0.006)	0.031*** (0.005)	-0.013 (0.009)	-0.015 (0.009)
$\overline{\text{Law}}$	-0.027** (0.012)	-0.005 (0.011)	0.007 (0.012)	0.012 (0.012)
Law * Foreign			0.096*** (0.009)	0.079*** (0.010)
$\overline{\text{Law}}$ * Foreign			-0.027*** (0.008)	-0.036*** (0.012)
Law * Urban				0.012 (0.011)
$\overline{\text{Law}}$ * Urban				-0.016* (0.008)
Law * Foreign * Urban				0.027* (0.015)
$\overline{\text{Law}}$ * Foreign * Urban				0.026* (0.015)
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Demographic Controls	N	Y	Y	Y
Observations	644,644	644,644	644,644	644,644

Notes: The dependent variable is school attendance (0/1) of children. Regressions are for 10-16 year old children outside the South, in 1880-1900, with standard errors clustered by state. $\overline{\text{Law}}$ is the average number of children in the home under a compulsory schooling law.

family in which there are tradeoffs between the attendance of siblings. This is clearest for the children of immigrants, where the coefficient on $\overline{\text{Law}} * \text{Foreign}$ is around 3% in Columns (3) and (4).

Consider an immigrant family with two children, in which one child comes under a compulsory schooling law. Given the main effect of $\overline{\text{Law}}$ at about zero in Column (3), and the interaction of $\overline{\text{Law}} * \text{Foreign}$ at 3%, this would imply that the probability of attendance of the sibling declined by $0.5 * (0-3)\% = -1.5\%$, as the proportion of children under the law in the family moved from 0.0 to 0.5. Across many households affected by compulsory schooling laws, this represents a large decrease in the number of children attending school, which came in spite of the larger numbers brought to school by the direct effect of the law.

Since child labor and school attendance were twin sides of the same issue, the effect of compulsory schooling laws on employment within the family are considered in Table 6. Compulsory schooling generally lowered employment, though the effects are smaller than for school attendance. One possible reason for this is that children were useful at home, especially in a time of large and predominantly rural families, where some children brought into school by a law didn't necessarily drop out of the workforce to attend. The family effects follow the same general pattern as in Table 5. Significant negative effects exist on child labor show up in aggregate, until family structure is controlled for, and then exist only for immigrant children, where the laws appear to have helped reduce child labor by around 2%, in general. As with school attendance, some of that reduced labor appears to have come at the expense of siblings, whose employment increased. This is consistent with the existence of positive labor spillovers in the employment of children within the family.

4 Discussion

The work closest to this analysis is Manacorda (2006) who uses a similar methodology for 1920. He finds evidence of family effects for child labor and school attendance using a cross-sectional difference-in-difference design,

Table 6: The effect of compulsory schooling on child labor within the family

VARIABLES	(1) 1880-1900	(2) 1880-1900	(3) 1880-1900	(4) 1880-1900
Law	-0.037*** (0.009)	-0.008 (0.006)	0.000 (0.007)	-0.012 (0.007)
$\overline{\text{Law}}$	0.036*** (0.010)	0.003 (0.006)	-0.002 (0.006)	-0.002 (0.006)
Law * Foreign			-0.018** (0.007)	-0.020*** (0.005)
$\overline{\text{Law}}$ * Foreign			0.012* (0.006)	0.024** (0.010)
Law * Urban				0.053*** (0.005)
$\overline{\text{Law}}$ * Urban				-0.003 (0.005)
Law * Foreign * Urban				-0.022*** (0.006)
$\overline{\text{Law}}$ * Foreign * Urban				-0.020* (0.011)
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Demographic Controls	N	Y	Y	Y
Observations	644,644	644,644	644,644	644,644

*** p<0.01, ** p<0.05, * p<0.1

Notes: Regressions are for 10-16 year old children outside the South, in 1880-1900, with standard errors clustered by state. The dependent variable is employment (0/1) of children. $\overline{\text{Law}}$ is the average number of children in the home under a compulsory schooling law.

differencing across ages and states instead of comparing changes in states over time. I find similar evidence of family effects in different years, using a more conventional differencing strategy that uses changes over time to identify the effect of the laws. The existence of family effects on schooling is also consistent with the modern evidence that has begun to be gathered. For example, Emerson and Souza (2002) find that for children in Brazil first born male children are more likely to work and last born male children are more likely to attend school. Chesnakova and Vaithianathan (2006) similarly show that families in Mexico were likely to have the oldest child work in order to fund the education of younger siblings. Khanam and Rahman (2005) find similar results in Bangladesh.

In addition to contributing to the evidence of family effects on school attendance, this paper contributes to the broader literature on compulsory schooling by examining the effect of laws on immigrant children, a group ignored in the prior literature. The laws appear to have been effective in helping move these children into school, and to have done little to influence native children. This also helps explain why previous studies have found little impacts of laws before 1900, since this strong effect on immigrant children would be partially masked in aggregate regression results.

One additional issue which the paper does not address is fertility, since along with decisions about how children passed their time, the other large decision families faced was how many children to have. Table 7 shows the changes in fertility from 1880 to 1920. Total fertility rates for whites dropped from 4.24 children per woman to 3.17. Column (3) shows the number of 10 to 16 year olds in the home, given that a family had any 10 to 16 year old children. This stays relatively constant over the years of the study. Column (4) shows the average number of children under 10 for these families, which follow TFR and decreases monotonically as time passes.

Despite large changes in fertility, it seems reasonable to assume that fertility decisions are orthogonal to the empirical results. From Table 7, given that a family had a child of age 10 to 16, the average number of children per family stays relatively constant at slightly greater than 2 children for every census year. For this reason, comparing sibling effects between years

Table 7: Fertility in the United States from 1880 to 1930

Year	TFR ¹	10 to 16	Under 10
(1)	(2)	(3)	(4)
1880	4.24	2.07	1.67
1900	3.56	2.15	1.57
1910	3.42	2.15	1.47
1920	3.17	2.14	1.45

Sources: 1. TFR is for whites, from Haines (2008). Averages in (3) and (4) are from the IPUMS samples of each census.

are not confounded by large differences in the number of siblings in the family in the age range considered. As an additional check for the influence of changing family sizes, in the regressions I control for both the number of children under age 10 and the total number of siblings of each child.

The regression results examining the effect of average compulsory schooling restrictions within the family are consistent with an economic model of the family where parents make simultaneous decisions about the schooling and labor of their children. The policy implications for a government concerned with education or child labor are that compulsory schooling laws might have the unintentional effect of decreasing attendance among those children who are not compelled to attend, decreasing overall social welfare gains from the legislation.

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Appendix Table 1: The effect of compulsory schooling laws, difference-in-difference identification

Panel A: Outside the South

	(1)	(2)	(3)	(4)
Decade	1870-1880	1870-1880	1880-1900	1880-1900
Law	-0.011 (0.016)	-0.047** (0.017)	0.027** (0.011)	-0.014 (0.012)
Law*Foreign		0.071*** (0.016)		0.088*** (0.011)
State fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y
Observations	411,980	411,980	652,414	652,414
R ²	0.176	0.177	0.298	0.301

*** p<0.01, ** p<0.05, * p<0.1

Panel B: With the South

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Decade	1870-1880	1870-1880	1880-1900	1880-1900	1900-1910	1900-1910	1910-1920	1910-1920
Law	-0.050** (0.022)	-0.079*** (0.023)	0.008 (0.013)	-0.022 (0.015)	0.037* (0.020)	0.014 (0.022)	0.007 (0.009)	-0.007 (0.010)
Law*Foreign		0.058*** (0.015)		0.077*** (0.010)		0.087*** (0.010)		0.082*** (0.010)
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	553,456	553,456	889,699	889,699	464,381	464,381	188,812	188,812
R ²	0.194	0.194	0.252	0.254	0.274	0.276	0.266	0.269

*** p<0.01, ** p<0.05, * p<0.1

Notes: Regressions are for white children ages 10 to 16, with standard errors clustered by state.

Appendix Table 2: State compulsory schooling laws

State	1870	1880	1900	1910	1920
Alabama	0	0	0	0	14
Arizona	0	14	14	14	14
Arkansas	0	0	0	14	14
California	0	14	14	14	14
Colorado	0	0	14	14	14
Connecticut	0	14	14	14	14
Delaware	0	0	0	14	14
District of Columbia	14	14	14	14	14
Florida	0	0	0	0	12
Georgia	0	0	0	0	12
Idaho	0	0	14	14	14
Illinois	0	0	14	14	14
Indiana	0	0	14	14	14
Iowa	0	0	0	14	14
Kansas	0	14	14	14	14
Kentucky	0	0	14	14	14
Louisiana	0	0	0	0	14
Maine	0	15	15	14	14
Maryland	0	0	0	12	13
Massachusetts	14	14	14	14	14
Michigan	0	14	14	14	15
Minnesota	0	0	16	14	14
Mississippi	0	0	0	0	12
Missouri	0	0	0	14	14
Montana	0	0	14	14	14
Nebraska	0	0	14	14	14
Nevada	0	14	14	14	14
New Hampshire	0	14	16	14	14
New Jersey	0	14	12	14	14
New Mexico	0	0	16	14	14
New York	0	14	14	14	14
North Carolina	0	0	0	12	14
North Dakota	0	0	14	14	14
Ohio	0	14	14	14	15
Oklahoma	0	0	0	14	16
Oregon	0	0	14	14	14
Pennsylvania	0	0	13	14	14
Rhode Island	0	0	15	14	14
South Carolina	0	0	0	0	14
South Dakota	0	0	14	14	15
Tennessee	0	0	12	14	14
Texas	0	0	0	0	12
Utah	0	0	14	16	14
Vermont	14	14	14	12	15
Virginia	0	0	0	12	12
Washington	0	16	15	14	14
West Virginia	0	0	14	12	14
Wisconsin	0	15	13	12	14
Wyoming	0	16	16	14	14