

On the Validity of Compulsory Education Laws as  
Instruments for Educational Attainment:  
Evidence from 1920-1969

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\*We would like to thank the faculty and the participants of the American Economic Association Summer Program and Minority Scholarship Program for their help and encouragement. We would like to give special thanks to Dr. Kelly Bedard and Dr. Douglas Steigerwald for their contributions and advice. In addition, we would like to thank Mariana Carrera for her encouragement and help. Views expressed herein are strictly those of the authors.

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## **Introduction**

Compulsory schooling laws have a long history in the world and play an important part in the current political discourse. In the United States compulsory schooling laws were first put in place in Massachusetts in 1852, and with time others states followed. These laws were passed individually by each state, and so there is significant level of variation of the number of years of education required across states, and in the years that these laws were passed.

An education system requires a great amount of public and private investment, and as such calculating the returns to schooling, both private and social, occupies a considerable part of the economic literature. Several papers have explored the consequence of education on crime<sup>1</sup>, mortality<sup>2</sup> and labor productivity.<sup>3</sup> The problem that arises with trying to measure the effect of schooling on these areas is that educational attainment tends to be endogenous (i.e. there exist unobservables that affect both education and the dependent variable<sup>4</sup>).

To tackle this endogeneity problem, compulsory schooling laws are used as an instrument for the level of formal education. These laws are arguably exogenous and as such should be valid instruments for education. The idea behind using these laws is that an individual who planned to drop out of school at age 13 and is “forced” to stay in school until age 16 (because of the law), will have extra years of education for reasons unrelated to unobservables.

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<sup>1</sup> See Lochner and Moretti (2004)

<sup>2</sup> See Lleras-Muney (2005) also Mazumder, Bhashkar (2006)

<sup>3</sup> See Acemoglu and Angrist (2000), Angrist and Krueger (1991), Card (2001)

<sup>4</sup> The most common example involves education and income. Both are highly correlated, but it could be the case that people with high ability tend to earn higher incomes and obtain higher education.

The use of these laws as an instrumental variable is increasingly relevant. A growing body of the economic literature uses these laws to control for education for policy relevant issues such as those mentioned above. Given the important policy consequences, the purpose of this paper is to analyze whether state compulsory laws are a valid instrument for education attainment and to identify in what periods of time these laws can be used as instruments for education.

Previous work has been done in this regard. Acemoglu and Angrist (2000) analyze cohorts who were 14 years old between 1914 and 1964 using two groups of census data, one from 1950-1980 and the other from 1950-1990. They find that compulsory schooling laws have a statistically significant impact on an individual's schooling, with a larger impact when the 1950-1990 census data is used, but find no evidence of impact on individuals with 12 years or more of education.

Similarly, Lleras-Muney (2002) finds that one more year of compulsory schooling increases years of education by about 5%. She analyzes cohorts from 1915-1939 using 1950-1970 census data and finds that the coefficients are robust to the addition of other state controls. These estimates are similar to those reported by Lleras-Muney (2005), Lochner and Moretti (2004), Eisenberg (1988), Oreopoulos et al (2006), Angrist and Krueger (1991), and Lang and Kroop (1986).

While the results of many previous papers have pointed to a significant and robust effect of compulsory schooling laws on education, many do so by measuring their effect on average educational attainment. A problem with this approach is that the effect of compulsory schooling laws might not be correctly identified. Compulsory schooling laws

would only be a valid instrument for education if their effect on educational attainment is specific to the group whose behavior they were intended to change. Even if the laws have a significant impact on average educational attainment, it is necessary to estimate their impact on groups with different education levels.

For this reason, this paper does not focus solely on the *average* effect of compulsory schooling laws on education. In contrast to most other papers, it estimates the effect that these laws have on various groups with different levels of education, this approach is similar to Acemoglu and Angrist (2000) with the difference that a wider range of individuals is used, and birth cohorts are observed in different censuses. For example, a law changing the years of schooling required from 9 to 10 years will increase average education of the population. However, it is more important to test whether this law change has a significant impact on the group it is posited to affect (i.e. people who were going to drop out by the end of 9<sup>th</sup> grade). Thus, instead of estimating the impact of such a law on average education, the impact of the law change estimate for people who finished 10 or more years of education, and then the same synthetic cohort is observed in a different census to be able to compare the effect of the laws on specific cohorts. It would be farfetched for a law of this nature to have a significant effect on the number of people with 12 or more years of education, since this group should not be affected by this law.

Another goal of this paper is to estimate how the effectiveness of compulsory education laws varies across different time periods. In contrast with the existing literature, as mentioned above, the effect of compulsory laws is estimated during different years and using data from different censuses. Previous studies focus on specific time

periods; Lleras-Muney (2002) uses data from the 1950 and 1960 census to focus on the cohorts from 1914 to 1939, and then argues that the laws were not effective after 1940. On the other hand, Acemoglu and Angrist (2000) focus on cohorts from 1924 to 1954 and argue that the laws were effective during this time period. This paper will use data from the 1960, 1970 and 1980 censuses to check for time periods where the laws had a significant effect on education. Knowing in which time periods the laws were relevant and had an impact on education will indicate in what cases using compulsory schooling as instruments for education is valid.

### **Data Considerations**

This paper employs data from the 1960, 1970, 1980 censuses. The data was obtained from the Integrated Public Use Microdata Series (IPUMS). For 1960 and 1970, data from 1% sample of these years was used. For 1980, the 5% sample is employed. Observations from Alaska and Hawaii are eliminated because they were not states during the first half of the century. Thus, observations for individuals of the 48 states in the continental U.S. plus the District of Columbia are used. Table 1 presents several descriptive statistics per cohort.

The data was restricted to white male individuals who were 25-54 years old during the census years. White males are used because previous literature finds that these laws did not have a significant effect on black and women in the first half of the 20<sup>th</sup> century,<sup>5</sup> and this period is included in the data. The age range was designed so that

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<sup>5</sup> Lleras-Muney (2002)

individuals are observe when they were old enough to be most likely finished with their education, but young enough so that deaths due to age did not affect our sample<sup>6</sup>.

Table 1

<b>Variables</b>	<b>1920-1929</b>	<b>1930-1939</b>	<b>1940-1949</b>	<b>1950-1959</b>	<b>1960-1969</b>
Average years of education	10.15	11.23	11.95	12.78	13.49
Minimum education attainment	5.00	8.92	9.53	10.60	11.78
Maximum education attainment	13.57	14.33	13.89	14.66	15.07
% Completed at least 8 years of education	0.81	0.89	0.92	0.96	0.98
% Completed at least 9 years of education	0.61	0.76	0.83	0.91	0.96
% Completed at least 10 years of education	0.54	0.71	0.78	0.87	0.94
% Completed at least 11 years of education	0.46	0.63	0.71	0.82	0.91
% Completed at least 12 years of education	0.41	0.57	0.65	0.78	0.88
% Completed at least 13 years of education	0.18	0.25	0.32	0.41	0.53

Note: Average years of education, minimum and maximum education attainment, is computed per state and cohort

Since individuals who are affected by the laws tend to be teenagers, cohorts were created based on the year at which individuals were 14 years old. For example, our data from the 1960 census contains observations of individuals born between the years 1905 and 1925. These individuals were 14-years old in the years 1920 to 1939. So for the 1960 census data there are 25 cohorts of individuals who were 14 years old at any given year from 1920 to 1939. These cohorts are referred to as ‘year-at-14’ cohorts for the rest of the paper.

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<sup>6</sup> Certain types of individuals could start dying past age 55. If for example people with low levels of education died early, then our observations would overestimate the number highly educated individuals.

For compulsory schooling laws data dating back from 1920 to 1978<sup>7</sup> is employed. Figure 1 shows the change in the average education level as a function of the number of years of compulsory schooling. It provides tentative evidence of compulsory schooling laws affecting average education, more years of education required seem to be correlated with higher educational attainment. However, these also reflect other differences across states.

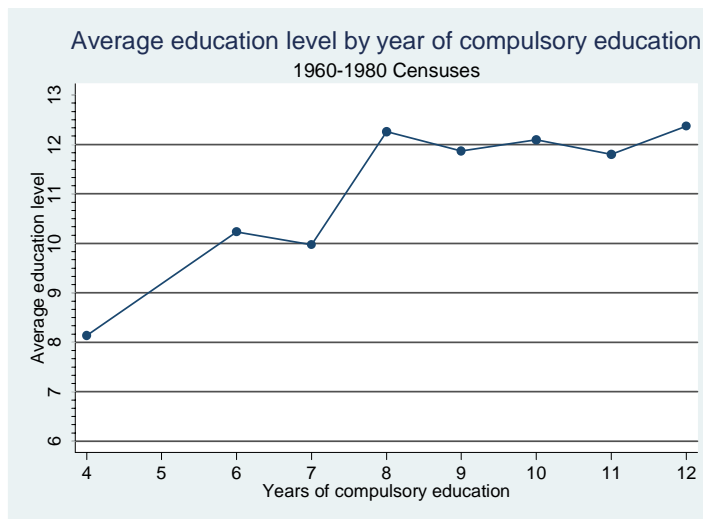


Figure 1

Figure 2 shows the same information but for selected cohorts. It can be seen from this figure that younger cohorts tend to have higher levels of education than older cohorts, regardless of the compulsory schooling laws.

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<sup>7</sup> Figure 1 (Appendix A) shows the changes in the laws throughout these years.

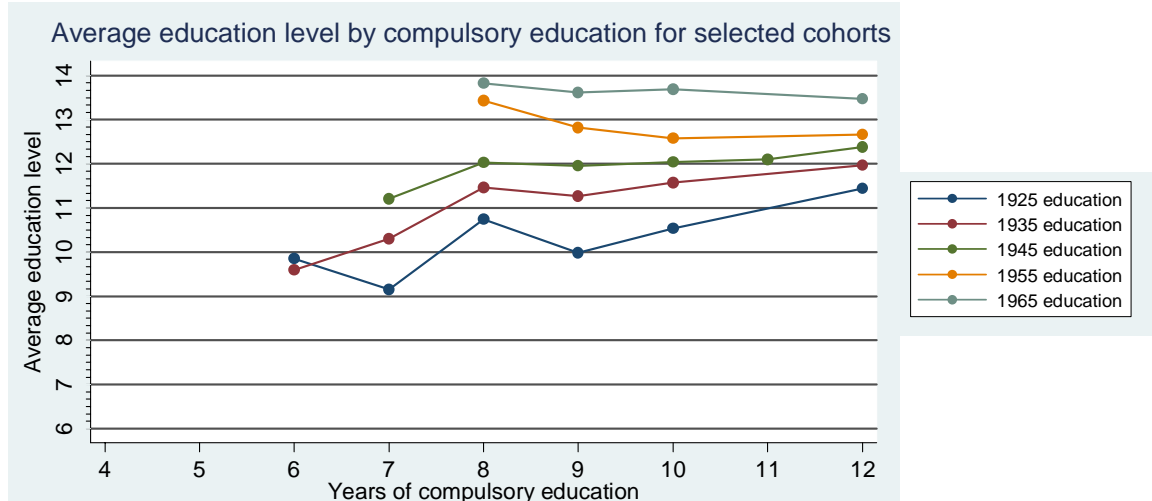


Figure 2

The IPUMS data was collapsed to weighted means by cohort and state-of-birth and then compulsory schooling laws are merged to the collapsed data. Regressions are conducted using collapsed data, and weighted by the population size of each state. The data for all the censuses merged included 4.05 million observations. After collapsing the data there are 2,450 observations, 50 observations per state

## Empirical Model

The following model was used to estimate the impact of compulsory schooling laws on education:

$$EDi_{cs} = \alpha + \beta_1 CA9_{cs} + \beta_2 CA10_{cs} + \beta_3 CA11_{cs} + \phi_c + \Pi_s + \varepsilon_{cs}$$

The dependent variable '**Ed**' indicates the percentage of people who finished at least '*i*' years of education.  $\phi_c$  captures cohort fixed effects and  $\Pi_s$  captures state of birth fixed effects.

The independent variables  $CA$  is constructed based on the definitions given by Acemoglu and Angrist (2000), compulsory schooling laws were constructed using the following formulas:

$$CA = \text{Max}(Req\_sch; drop\_age - enroll\_age)$$

$CA$  then would be the variable measuring compulsory attendance laws, which would be the maximum between the minimum years in school required and the difference between the maximum enrollment age and the minimum dropout age.

In order to differentiate between laws, dummies are constructed based on the  $CA$  variable. In total, 4  $CA$  dummies are constructed:  $CA8$ ,  $CA9$ ,  $CA10$  and  $CA11$ .  $CA8$  equals 1 if eight or less years of education were required by the laws, and equals zero otherwise.  $CA9$  equals 1 if the laws required nine years of education, and equals zero otherwise. In a similar manner,  $CA10$  equals 1 if ten years of education were required and equals zero otherwise. And  $CA11$  equals 1 if eleven or more years of education were required, and equals zero otherwise.

Several types of regressions are done.  $ED8$  through  $ED14$  and  $ED16$  are used as the dependent variables and  $CA8$  through  $CA9$  as the explanatory variables, including  $\Phi_c$  and  $\Pi_s$  to control for cohort and state of birth fixed effects. We then ran the same regression but used another variable called *Education* as a dependent variable. Education is a continuous variable that indicates the number of years of education attained by each individual. We also ran regressions with  $ED8$  through  $ED14$ ,  $ED16$  and *Education* as the dependent variables and the continuous variables  $CA$  as the explanatory variables, ( $CA$  indicates the number of years of schooling attendance required). In all of the regressions observations are weighted by state of birth population.

## **RESULTS**

Regressions are run at the individual level clustered by state of birth and at the aggregate level, and almost identical results are found. The results for the aggregate level<sup>8</sup> are discussed next. Regressions are run with data from each different Census Year (1960, 1970 and 1980 and with combined data from all three censuses (from now on combined census data is referred to as ‘all years’).

The results for the regressions using the ‘all years’ data, using all cohorts from 1920-1969 are shown in Table 2<sup>9</sup>. The results are mostly consistent with the previous literature; most laws do affect educational attainment. The coefficients of the laws for *ED8* through *ED11* are all positive and significant, which makes sense since one would expect laws requiring 9, 10 or 11 years of education to have an impact on the number of people finishing 8<sup>th</sup>, 9<sup>th</sup>, 10<sup>th</sup> and 11<sup>th</sup> grade. Not surprisingly, the laws are jointly significant in all these cases as well.

An interesting finding worth noting is that in all the regressions, the coefficients on *CA9* (laws requiring nine years of schooling) have a significant effect on both *ED11* and *ED12*. While the effect of these laws on *ED10* might be attributed to some sort of signaling<sup>10</sup>, the strong and significant effect of these laws requiring only 9 years of education have on the number of individuals finishing 11<sup>th</sup> and 12<sup>th</sup> is puzzling. It may suggest an identification problem as to who are the laws affecting. It is also important to note that in the same regressions compulsory schooling laws do not have a statistically

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<sup>8</sup> Tables with the results for the individual-level regressions are available upon request.

<sup>9</sup> For all tables refer to Appendix B

<sup>10</sup> Students who were going to drop out after grade 10 could be deciding to stay one extra year now that they see their peers from grade 9 finishing the same number of years of education as them. They would stay with the purpose of signaling that they have more ability than the students who might have dropped by grade 9 had the compulsory schooling laws not being in place.

significant impact on the number of people who attend or finish college (*ED13*, *ED14* and *ED16*), which is reassuring since significant coefficients for these levels of education would be an unheard signaling effect.

To understand what is driving these results, the same regressions are run for *each* census year. Tables 3a to 3c, present the results for 1960, 1970 and 1980 censuses respectively.

For the 1960 census the results are not consistent; *CA9* is not significant for *ED9*, which is the education group that it should affect the most, *CA10* behaves in a similar manner as well, and the coefficients are insignificant for the average level of education for all laws. In fact, almost all the coefficients are insignificant.

The results for the 1970 census are presented in Table 3b. The coefficients are consistent with the results on the ‘all years’ regressions, most coefficients are positive and significant, and the laws have no effect above 13 years of education. The effect on average education, is similar to those of the ‘all years’ regression.

For the 1980 census the results are presented in Table 3c. These estimates are also consistent with the results on the ‘all years’ regressions. All coefficients for *ED8* to *ED12* are positive and significant, and those for *ED13* and above are insignificant as expected. The effect on average education, is similar to those of the ‘all years’ regression, they are just slightly smaller.

The fact that compulsory schooling laws had an insignificant effect in the regressions using data from the 1960 Census is surprising for several reasons. We included year-at-14 cohorts from 1920 to 1949, and previous literature (see Lleras-Muney 2002) argues that the laws were in fact most effective from 1915 to 1939, a period which

is largely included in the data used. In addition to this, the fact that compulsory laws have significant coefficients when using data from the 1970 and 1980 censuses (which include many of the same year-at-14 cohorts as the 1960 census) made the differing results even more perplexing<sup>11</sup>.

One possible explanation seems plausible: that the laws were not effective in some periods that are included in the 1960 census data, and that these periods made the results inconsistent. The estimates for the ‘all years’ regressions are consistent even though they include 1960 data, thus a logical hypothesis would be that what is driving the results in this case is a time period from the 1970 and 1980 censuses when the laws were effective.

To investigate this, regressions are run for three groups of cohorts: one for all cohorts from 1920 to 1939, another for cohorts from 1940 to 1949, and another one for the 1950-1969 cohorts<sup>12</sup>. The results can be found in Table 4. In this case the effect of the laws only holds for cohorts from 1940 to 1949. These results are very concerning. If the results in the ‘all-year’ regression are driven by the cohorts in this ten year period, then such results do not tell us anything about the validity of compulsory schooling laws as an instrument for education. In particular, any use of the laws as an instrument outside this period is of questionable nature.

To test whether the laws significantly affected education in the 1940-1949 period, regressions are run for 10-year cohorts in *each* census. Results are reported in Table 5a to 5c. For the censuses of 1970 and 1980, the only group that is affected by the laws is the one with the cohorts that were 14 between 1940 and 1949, which is consistent to the

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<sup>11</sup> The overlap period between the 1960 and 1970 censuses’ cohorts is 1930-1949. The overlap period between the 1960 and 1980 censuses’ cohorts is 1940-1949.

<sup>12</sup> We also ran separate regressions using 10-year cohort periods and found very similar results.

previous results from the ‘all years’ data. Surprisingly, for the 1960 census the compulsory schooling laws have no effect, and the coefficients for the 1940-1949 cohorts are insignificant. These last results seem to be contradictory to the results for the ‘all years’ data (as well as the 1970 and 1980 censuses data) which show that the laws are effective during the 1940’s.

The possibility exists that these results are driven by the timing of the laws. Changes occurring at the beginning or the end of each decade could potentially be confounding the results in several ways. If a law change occurred on the last year of the time period under study, then any impact the laws could have on education is not being captured in the regressions. The impact would be reflected in the following year (which is not included in the decade in question). Similarly, law changes occurring at the beginning of a time period under study would not take into account the levels of education prior to the change in the law, and thus the effects of such law of educational attainment would not be correctly estimated.

Table 6

Year-at-14	# Law Changes
1929	5
1935	7
1939	4
1946	17
1950	5
1954	3
1959	2
Total	43

Table 6 presents data on compulsory law changes and the year in which they occurred. It can be clearly seen that laws tend to be clustered in specific years. Most importantly, Table 6 shows many of the changes in the laws occurred at the beginning

and end of each decade. In fact, four out of the seven years in which changes in the law occurred happen to be at the end of each decade. To control for this problem, regressions are conducted using different time periods<sup>13</sup>. The time periods are designed so that each includes at least the three years prior to any law change and three years after the change in the laws occurred. The time periods are the following: 1920-1932, 1932-1942, 1942-1953 and 1951-1969. The results for these regressions are consistent with the initial results. For the 1920-1932 and 1932-1942 periods, the coefficients on the laws are mostly insignificant, and the laws again seem to affect cohorts from 1942 to 1953 (roughly the decade in which most of our results hold). For the 1951-1969 period, the laws were again largely ineffective. These results hold even when restricting the dataset to only observations from the 1960 census.

What is remarkable about the previous results is how the 1940-1949 period seems to be the only period where compulsory laws are effective and that any time period that includes this decade will produce significant coefficients of the effects of compulsory schooling laws on education. One thing to note is that 1940-1949 is, if anything, an unusual time period. Not only does it include World War II, but it also includes two thirds of the compulsory schooling laws changes that happened between 1920 and 1969. This decade is not representative of most other time periods and the fact that it seems to be driving the results makes the claim that compulsory laws affect education all the more questionable.

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<sup>13</sup> Results available upon request

## **CONCLUSIONS**

This paper makes two important contributions to the literature on compulsory schooling laws and their use as instruments for education. First, the laws do have a statistically significant effect on average education and on the education distribution (for the most part). However, the laws are affecting groups they are not supposed to affect when the data is disaggregated by educational level. This evidence suggests that there is an identification problem as to which groups these laws affect, and this makes using compulsory schooling laws as instruments for education somewhat questionable.

Secondly, the laws seem to only affect education in the period of 1940-1949. Using both 'all years' data and per-census data the laws did not have statistically significant effects in all other periods. Other meaningful results obtained are from periods that include that 1940-1949 period. Because 1940-1949 is a very peculiar decade, the significant coefficients for the laws in these periods could be caused by other factors like: World War II, GI bill, rapid economic growth, among others.

Of course, due to the limited scope of this project we were not able to exhaustively test data for other periods, and thus our results should be seen as indicative rather than definitive. As a possible extension we would like to use data from more censuses to see whether there is an explanation as to why compulsory schooling laws work in some periods and not in others. Another important extension to this work will be to add more controls to our regressions (such as urban/rural ratio in each state/year cohort, labor market characteristics, regional and time effects etc.) in similar fashion to what previous literature has done. Most importantly, given our findings we would have liked to study the 1940-1949 period more in depth to try to understand the reasons why

compulsory schooling worked so effectively during this decade. But despite these limitations, we think that the results obtained here are useful and hopefully will lead to future research in this area.

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### Appendix A

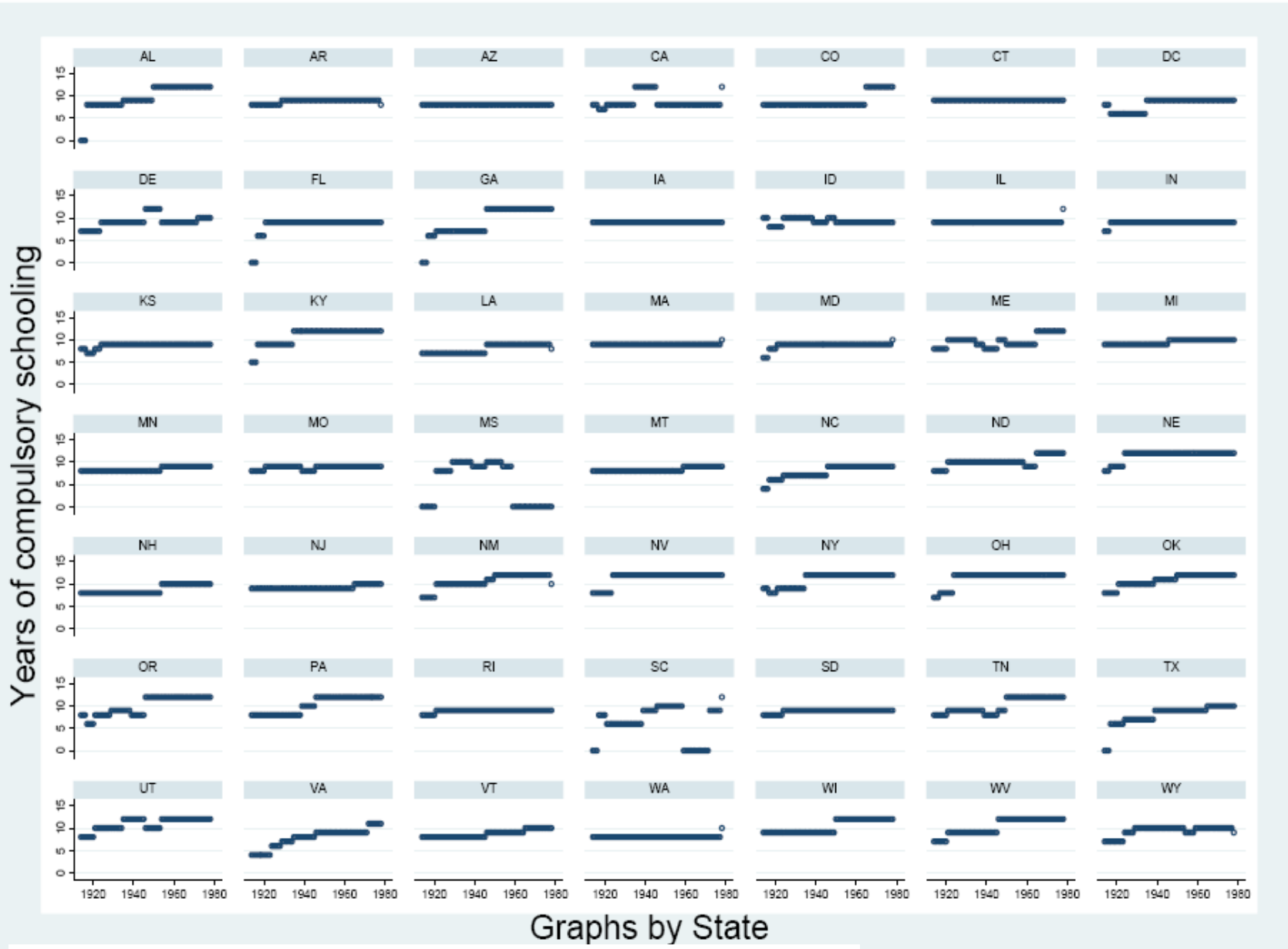


Figure 3

## Appendix B

Table 2

The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old between 1920-1969  
Results from 1960-1980 Census

	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>0.041</b> (0.004)	<b>0.031</b> (0.003)	<b>0.027</b> (0.003)	<b>0.022</b> (0.003)	<b>0.026</b> (0.003)	-0.003 (0.002)	-0.002 (0.000)	-0.002 (0.002)	<b>0.198</b> (0.021)
CA 10	<b>0.0312</b> (0.004)	<b>0.031</b> (0.004)	<b>0.03</b> (0.004)	<b>0.028</b> (0.004)	<b>0.033</b> (0.004)	-0.005 (0.003)	-0.002 (0.002)	-0.003 (0.002)	<b>0.176</b> (0.026)
CA 11	<b>0.044</b> (0.004)	<b>0.054</b> (0.004)	<b>0.052</b> (0.004)	<b>0.048</b> (0.004)	<b>0.05</b> (0.004)	<b>-0.009</b> (0.003)	-0.006 (0.003)	-0.004 (0.002)	<b>0.293</b> (0.029)
F-stat	54.79 (0.000)	58.92 (0.000)	59.77 (0.000)	52.8 (0.000)	56.15 (0.000)	1.46 (0.061)	2.11 (0.097)	1.15 (0.326)	38.94 (0.000)
R-squared	0.842	0.929	0.947	0.960	0.965	0.951	0.966	0.944	0.966

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. F-statistics given with p-values below. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 2450, we collapse the individual data to their weighted means by state of birth and year-at-14.

Table 3a

The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old between 1920-1949  
Results from 1960 Census

	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>0.018</b> (0.004)	0.001 (0.004)	-0.002 (0.005)	-0.002 (0.005)	-0.002 (0.005)	-0.004 (0.316)	-0.001 (0.004)	-0.001 (0.003)	0.051 (0.033)
CA 10	<b>0.009</b> (0.004)	<b>0.015</b> (0.007)	0.014 (0.007)	0.013 (0.007)	0.011 (0.007)	-0.005 (0.260)	-0.002 (0.005)	-0.001 (0.004)	0.059 (0.039)
CA 11	0.004 (0.005)	0.01 (0.006)	0.012 (0.007)	0.013 (0.007)	0.011 (0.007)	-0.0015 (0.772)	0.000 (0.005)	0.003 (0.004)	0.054 (0.043)
F-stat	8.01 (0.000)	2.2 (0.086)	2.86 (0.036)	2.97 (0.031)	2.51 (0.057)	0.65 (0.581)	0.11 (0.957)	0.4 (0.755)	1.11 (0.343)
R-squared	0.908	0.933	0.930	0.921	0.915	0.828	0.808	0.756	0.938

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. F-statistics given with p-values below. The data is from 1960 Census IPUMS, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 1470, we collapse the individual data to their weighted means by state of birth and year-at-14.

Table 3b

The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old between 1930-1959  
Results from 1970 Census

	Completed 8 years or higher	Complete 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>0.036</b> (0.013)	<b>0.028</b> (0.01)	<b>0.026</b> (0.008)	<b>0.02</b> (0.006)	<b>0.037</b> (0.009)	-.008 (0.007)	-0.006 (0.006)	-0.003 (0.005)	<b>0.187</b> (0.054)
CA 10	0.023 (0.012)	<b>0.026</b> (0.009)	<b>0.024</b> (0.008)	<b>0.024</b> (0.007)	<b>0.041</b> (0.012)	-0.011 (0.006)	-0.008 (0.005)	-0.005 (0.004)	<b>0.147</b> (0.066)
CA 11	<b>0.039</b> (0.016)	<b>0.043</b> (0.012)	<b>0.041</b> (0.01)	<b>0.037</b> (0.008)	<b>0.049</b> (0.009)	<b>-0.015</b> <b>(0.006)</b>	-0.011 (0.006)	-0.006 (0.007)	<b>0.241</b> (0.075)
F-stat	3.00 (0.04)	4.57 (0.007)	6.48 (0.001)	7.96 (0.00)	10.49 (0.00)	2.02 (0.124)	1.3 (0.286)	0.4 (0.753)	4.86 (0.005)
R-squared	0.829	0.884	0.901	0.910	0.915	0.867	0.779	0.920	0.829

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. F-statistics given with p-values below. The data are from the Census IPUMS for 1970, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 2450, we collapse the individual data to their weighted means by state of birth and year-at-14.

Table 3c

The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old between 1940-1969  
Results from 1980 Census

	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>0.024</b> (0.005)	<b>0.032</b> (0.004)	<b>0.029</b> (0.004)	<b>0.025</b> (0.004)	<b>0.03</b> (0.005)	.0005287 (0.003)	0.001 (0.003)	-0.002 (0.003)	<b>0.159</b> (0.033)
CA 10	<b>0.026</b> (0.005)	<b>0.034</b> (0.005)	<b>0.032</b> (0.004)	<b>0.025</b> (0.004)	<b>0.031</b> (0.005)	-.0005054 (0.005)	-0.002 (0.005)	-0.007 (0.004)	<b>0.155</b> (0.038)
CA 11	<b>0.038</b> (0.005)	<b>0.06</b> (0.005)	<b>0.058</b> (0.005)	<b>0.053</b> (0.005)	<b>0.056</b> (0.006)	-.0018484 (0.006)	0.000 (0.006)	-0.002 (0.005)	<b>0.32</b> (0.048)
F-stat	18.54 (0.000)	42.12 (0.000)	45.44 (0.000)	34.33 (0.000)	32.89 (0.000)	0.12 (0.950)	0.36 (0.781)	1.85 (0.137)	17.01 (0.000)
R-squared	0.796	0.889	0.928	0.950	0.953	0.9634	0.950	0.924	0.951

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. F-statistics given with p-values below. The data are from the Census IPUMS for 1980, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 2450, we collapse the individual data to their weighted means by state of birth and year-at-14.

Table 4  
The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old across cohorts  
Results from 1960-1980 Census

	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
<b>1920-1939 Cohort</b>									
CA 9	-0.009 (0.006)	<b>-0.022</b> (0.007)	<b>-0.022</b> (0.008)	<b>-0.017</b> (0.007)	-0.011 (0.006)	0.001 (0.004)	0.001 (0.004)	0.000 (0.004)	-0.083 (0.044)
CA 10	-0.002 (0.007)	0.005 (0.016)	0.011 (0.015)	0.014 (0.012)	0.013 (0.012)	<b>0.014</b> (0.006)	<b>0.016</b> (0.005)	<b>0.016</b> (0.004)	0.088 (0.07)
CA 11	-0.021 (0.006)	-0.014 (0.008)	-0.009 (0.009)	-0.002 (0.009)	0.004 (0.008)	0.002 (0.007)	0.003 (0.006)	0.004 (0.005)	-0.07 (0.053)
<b>1940-1949 Cohort</b>									
CA 9	<b>0.024</b> (0.005)	<b>0.015</b> (0.005)	<b>0.011</b> (0.005)	0.002 (0.005)	0.005 (0.005)	<b>-0.016</b> (0.004)	<b>-0.013</b> (0.004)	<b>-0.007</b> (0.003)	0.056 (0.036)
CA 10	<b>0.022</b> (0.005)	<b>0.018</b> (0.005)	<b>0.014</b> (0.006)	-0.001 (0.006)	0.005 (0.007)	<b>-0.024</b> (0.005)	<b>-0.021</b> (0.005)	<b>-0.013</b> (0.005)	0.023 (0.04)
CA 11	<b>0.022</b> (0.004)	<b>0.02</b> (0.005)	<b>0.019</b> (0.005)	0.009 (0.006)	0.012 (0.006)	<b>-0.029</b> (0.006)	<b>-0.026</b> (0.006)	<b>-0.012</b> (0.006)	0.025 (0.039)
<b>1950-1969 Cohort</b>									
CA 9	<b>-0.015</b> (0.002)	-0.006 (0.005)	-0.003 (0.005)	-0.001 (0.005)	-0.004 (0.005)	-0.005 (0.006)	-0.001 (0.006)	-0.002 (0.005)	<b>-0.08</b> (0.095)
CA 10	<b>-0.017</b> (0.003)	-0.014 (0.005)	-0.008 (0.005)	-0.003 (0.005)	-0.003 (0.005)	-0.003 (0.007)	0.001 (0.006)	0.000 (0.005)	<b>-0.094</b> (0.002)
CA 11	<b>-0.016</b> (0.003)	-0.003 (0.006)	-0.001 (0.006)	0.004 (0.007)	0.002 (0.007)	-0.003 (0.009)	-0.005 (0.008)	<b>-0.02</b> (0.007)	<b>-0.124</b> (0.005)

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. F-statistics given with p-values below. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 980 for 1920-1929 and 1940-1949, and 490 for 1930-1939.

Table 5a  
The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old across cohorts  
Results from 1960 Census

	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
<b>1920-1929 Cohort</b>									
<b>CA 9</b>	-0.013 (0.011)	-0.012 (0.015)	-0.012 (0.017)	-0.003 (0.014)	-0.005 (0.013)	0.004 (0.007)	0.008 (0.008)	0.002 (0.006)	-0.066 (0.081)
<b>CA 10</b>	-0.044 (0.022)	<b>-0.043</b> (0.013)	-0.029 (0.023)	-0.003 (0.025)	-0.011 (0.029)	0.012 (0.018)	0.01 (0.018)	0.013 (0.012)	-0.129 (0.207)
<b>CA 11</b>	-0.018 (0.01)	-0.008 (0.009)	-0.01 (0.011)	-0.007 (0.012)	-0.02 (0.013)	-0.027 (0.014)	-0.004 (0.014)	-0.012 (0.009)	<b>-0.193</b> (0.085)
<b>1930-1939 Cohort</b>									
<b>CA 9</b>	0.003 (0.006)	-0.01 (0.006)	-0.005 (0.007)	0.001 (0.008)	0.004 (0.009)	0.004 (0.007)	0.006 (0.006)	0.007 (0.006)	0.06 (0.053)
<b>CA 10</b>	0.006 (0.005)	0.015 (0.012)	0.025 (0.014)	<b>0.025</b> (0.01)	0.02 (0.011)	<b>0.021</b> (0.007)	<b>0.029</b> (0.007)	<b>0.029</b> (0.006)	<b>0.21</b> (0.069)
<b>CA 11</b>	0.000 (0.007)	-0.007 (0.008)	0.001 (0.01)	0.012 (0.01)	0.012 (0.012)	0.008 (0.008)	0.012 (0.008)	0.016 (0.007)	0.105 (0.065)
<b>1940-1949 Cohort</b>									
<b>CA 9</b>	<b>0.025</b> (0.007)	0.014 (0.009)	0.004 (0.008)	-0.001 (0.009)	-0.005 (0.009)	<b>-0.02</b> (0.008)	-0.013 (0.008)	-0.005 (0.007)	0.064 (0.064)
<b>CA 10</b>	<b>0.023</b> (0.007)	0.008 (0.01)	-0.004 (0.01)	-0.018 (0.012)	<b>-0.027</b> (0.012)	<b>-0.038</b> (0.01)	<b>-0.027</b> (0.011)	-0.012 (0.01)	-0.039 (0.075)
<b>CA 11</b>	<b>0.019</b> (0.007)	<b>0.015</b> (0.008)	0.01 (0.008)	0.003 (0.01)	-0.005 (0.012)	<b>-0.032</b> (0.011)	<b>-0.027</b> (0.013)	-0.005 (0.012)	0.008 (0.078)

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. The data is from the 1960 Census IPUMS, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14.

Table 5b  
The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old across cohorts  
Results from 1970 Census

1930-1939 Cohort									
	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>.016</b> <b>0.007</b>	0.005 .006	.004 .007	-.002 .008	.005 .009	.004 .006	-.0004 .007	-.0003 .0083	.073 .038
CA 10	.005 .0067	.015 .008	.013 .009	.016 .010	.012 .014	<b>.017</b> <b>.006</b>	<b>.013</b> <b>.006</b>	.006 .008	.094 .057
CA 11	.009 .007	.008 .008	.012 .011	.003 .013	.010 .014	.005 .009	-.0003 .009	-.0006 .010	.065 .065
1940-1949 Cohort									
CA 9	.020 .013	.015 .011	<b>.018</b> <b>.0083031</b>	.003 .007	.011 .008	-.015 .010	-.015 .010	-.004 .010	.065 .057
CA 10	.019 .011	<b>.021</b> <b>.009</b>	<b>.025</b> <b>.010</b>	.008 .009	.020 .010	<b>-.024</b> <b>.009</b>	<b>-.028</b> <b>.009</b>	-.009 .009	.046 .074
CA 11	<b>.023</b> <b>.009</b>	<b>.021</b> <b>.007</b>	<b>.023</b> <b>.007</b>	.009 .009	.015 .008	<b>-.032</b> <b>.008</b>	<b>-.031</b> <b>.007</b>	-.010 .007	.032 .051
1950-1959 Cohort									
CA 9	-.011 .004	<b>.014</b> <b>.007</b>	<b>.012</b> <b>.005</b>	.010 .006	.00001 .007	.004 .006	.002 .016	-.002 .012	.027 .052
CA 10	-.032 .006	-.009 .006	-.001 .007	-.0005 .007	-.002 .011	<b>.030</b> <b>.012</b>	.023 .018	.031 .019	.016 .082
CA 11	-.030 .018	<b>-.018</b> <b>.006</b>	-.010 .007	-.008 .007	-.020 .011	<b>.028</b> <b>.011</b>	-.0156 .018	.002 .027	-.117 .172

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14.

Table 5c  
The effect of compulsory schooling laws on discrete levels of schooling for white men aged 14 years old across cohorts  
Results from 1980 Census

1940-1949 Cohort									
	Completed 8 years or higher	Completed 9 years or higher	Completed 10 years or higher	Completed 11 years or higher	Completed 12 years or higher	Completed 13 years or higher	Completed 14 years or higher	Completed 16 years or higher	Education
CA 9	<b>0.024</b> (0.004)	<b>0.014</b> (0.004)	<b>0.009</b> (0.004)	0.006 (0.004)	0.010 (0.006)	<b>-0.015</b> (0.004)	<b>-0.012</b> (0.004)	<b>-0.012</b> (0.004)	0.030 (0.036)
CA 10	<b>0.024</b> (0.004)	<b>0.023</b> (0.004)	<b>0.020</b> (0.005)	0.007 (0.005)	<b>0.021</b> (0.006)	<b>-0.012</b> (0.005)	<b>-0.011</b> (0.005)	<b>-0.019</b> (0.004)	0.050 (0.039)
CA 11	<b>0.024</b> (0.004)	<b>0.025</b> (0.004)	<b>0.023</b> (0.005)	<b>0.014</b> (0.005)	<b>0.0246</b> (0.006)	<b>-0.024</b> (0.005)	<b>-0.022</b> (0.005)	<b>-0.022</b> (0.004)	0.0179 (0.041)
1950-1959 Cohort									
CA 9	<b>-0.006</b> (0.003)	0.002 (0.004)	0.004 (0.004)	0.005 (0.005)	0.002 (0.005)	0.0017 (0.006)	-0.003 (0.005)	-0.005 (0.005)	-0.036 (0.027)
CA 10	-0.005 (0.003)	-0.0013 (0.005)	0.001 (0.005)	0.006 (0.006)	0.007 (0.006)	-0.008 (0.007)	-0.014 (0.008)	<b>-0.022</b> (0.007)	<b>-0.098</b> (0.040)
CA 11	-0.002 (0.007)	-0.0002 (0.009)	0.006 (0.010)	0.014 (0.011)	0.004 (0.012)	0.0197807 (0.015)	0.029 (0.016)	-0.013 (0.013)	-0.0008 (0.094)
1960-1969 Cohort									
CA 9	<b>-0.009</b> (0.003)	<b>-0.019</b> (0.006)	<b>-0.019</b> (0.007)	<b>-0.025</b> (0.010)	<b>-0.026</b> (0.010)	<b>-0.054</b> (0.016)	<b>-0.040</b> (0.012)	-0.014 (0.010)	<b>-0.272</b> (0.059)
CA 10	<b>-0.008</b> (0.004)	<b>-0.019</b> (0.006)	-0.016 (0.008)	<b>-0.021</b> (0.010)	-0.021 (0.011)	<b>-0.044</b> (0.017)	<b>-0.038</b> (0.014)	-0.013 (0.012)	<b>-0.247</b> (0.067)
CA 11	<b>-0.074</b> (0.002)	-0.008 (0.004)	<b>-0.009</b> (0.004)	-0.011 (0.006)	<b>-0.011</b> (0.005)	<b>-0.040</b> (0.014)	<b>-0.030</b> (0.009)	<b>-0.024</b> (0.008)	<b>-0.186</b> (0.040)

Note: Robust standard errors in parenthesis. The coefficients in bold are significant at a 5% level. All regressions also contain year-at-14 and state of birth effects. The data are from the Census IPUMS for 1960 through 1980, with the sample restricted to white males aged 25-54 in the census year. Compulsory schooling laws are assigned according to the laws in effect in the individual state of birth when he was 14. The sample size is 980 observations.