Does Compulsory Schooling Affect Innovation?

Evidence from the United States

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Does Compulsory Schooling Affect Innovation? Evidence from the United States

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Abstract

U.S. states adopted compulsory schooling laws between 1852 and 1929. Compulsory schooling laws increase the number of completed years of schooling, which could lead to social benefits such as increases in workforce productivity and technological innovation. In theory, additional schooling could lead to a more educated populace that possesses the skills that lead to more inventions and technological innovation. However, the historical impetus around compulsory schooling included a well-meaning desire to assimilate diverse populations and to control children's educations. It is possible that a more homogenous and regimented system of schooling could dampen entrepreneurial and innovative thinking relative to less structured forms of education. To our knowledge, this is the first study to empirically test the historical relationship between state-level compulsory schooling laws and measures of innovation and entrepreneurship such as the number of patents per capita and output per worker. Our results suggest that the adoption of compulsory schooling in the U.S. reduced patents per capita and output per worker over time.

Keywords: compulsory schooling; innovation; productivity; patents; homeschooling

JEL Classifications: I28, I20

Introduction

The late 19th century witnessed most U.S. states adopting their first compulsory schooling law mandating school attendance by children. By the end of 1929, all 50 states and the District of Columbia had compulsory education laws in place. Proponents of these laws argued for schools that were more broadly available to students regardless of social class (Katz ,1976). Public schools also offered a way to assimilate newly arrived immigrants to the predominant culture (Dewey, 1916; Katz, 1976; Mann, 1855; Rush, 1786) and to allow children with negligent parents to receive an education (Taylor, 2010). More recent education scholars similarly argue that public schooling is necessary to teach children how to become good citizens (Gutmann, 1999; Saltman, 2000).

We consider how these initial compulsory schooling laws affect innovation. The effect of compulsory schooling on innovation is theoretically ambiguous. Educational attainment tends to be positively associated with measures of innovation (Akcigit, Grigsby, & Nicholas, 2017a); compulsory schooling increases some groups' educational attainment, which could potentially increase their innovation. On the other hand, changing schooling from voluntary to compulsory may change the nature of schooling, affecting innovation (see, for example, the arguments in Gatto, 2000; 2002).

Compulsory schooling laws could also change the structure of education by reducing the proportion of children being educated in the home. Increases in conventional schooling could lead to reductions in academic and social outcomes if children were receiving meaningful educations in their homes. Although much of the empirical evidence on the subject has limited internal validity, the literature suggests that homeschooled children tend to fare better academically and socially than their otherwise similar peers in public schools (Burke, 2019; Cogan, 2010; Hamlin, 2019; Medlin, 2013; Ray, 2017).

Potential social benefits of compulsory public education include a more educated populace, positive socialization, and more civic knowledge (Gutmann, 1999). However, it is also possible that particular types of schooling lead to a less educated populace and more obedience relative to alternative forms of education such as homeschooling or private schooling (DeAngelis, 2019). More obedience could be viewed as a social benefit since it could lead to less crime, but obedience could also be viewed as a social cost if it reduces creativity and innovation (DeAngelis, 2018; Glenn, 1988). The traditional schooling model could also have differing effects on cognitive skills such as reading comprehension and noncognitive skills such as creativity and effort (DeAngelis, 2019; Kim, 2011). Lleras-Muney and Shertzer (2015) find that English-only laws and compulsory schooling laws had no detectable effect on the socialization of immigrants; their results imply that compulsory schooling may be limited in its ability to change cultural attitudes.

We empirically test the relationship between schooling and innovation by using the number of patents per capita by state as a proxy for innovation. We estimate declines in patenting that begin around the prime patenting years (ages 36 to 55) of the first cohorts affected. We also consider the effect of compulsory schooling laws on real output per work as a measure of productivity.

Compulsory Schooling Laws

Figure 1 illustrates the number of states (plus Washington, D.C.) adopting compulsory schooling laws over time. Massachusetts, with its long history of compulsory education, was the initial adopter in 1852. Post-Civil War, the growing urbanization and increased number of immigrants led many more states to follow (Lleras-Muney & Shertzer, 2015; Katz, 1976; Tyack, 1974; Vinovskis, 1995). By 1929, all states and territories had adopted compulsory schooling laws of varying lengths.

Ample research uses the modern lengthening of compulsory schooling as an instrumental variable for years of schooling completed (e.g. Angrist & Krueger, 1991; Oreopoulos, Page, & Stevens, 2006). These evaluations rely on the binding nature of modern compulsory schooling laws for the strength of the instrument. In this study, we consider only the initial passage of compulsory schooling laws. These laws plausibly have two effects: increasing educational attainment and changing the nature of schooling.

The initial evidence on whether the original compulsory schooling laws increased educational attainment is mixed. Many of the original laws were not well enforced, partly because there was not a sufficient supply of seats for students to attend schools and partly because more-rural parts of the country found it exceedingly difficult to enforce attendance without an administrative body (Katz, 1976). In the early decades of the 20th century, states developed the bureaucracy necessary to enforce the compulsory schooling laws, including adopting child labor laws. Goldin and Katz (2011), for example, estimate that state compulsory schooling laws account for at most 6 to 7 percent of the change in high school enrollment in the early 20th century. Akcigit, Grigsby, and Nicholas (2017a) document that patentors around the turn of the 20th century were more educated. To the extent that compulsory schooling increases educational attainment, it plausibly increases patenting.

Recent evidence provides a more nuanced understanding of the effect of compulsory schooling on educational attainment. Rauscher (2014) replicates the finding that early compulsory schooling laws in the U.S. did not increase average school attendance. However, she demonstrates that this average conceals interesting compositional changes in attendance. Early compulsory schooling laws increased educational attainment for non-whites and for boys from lower socioeconomic status backgrounds and reduced attendance among white and higher socioeconomic status boys and girls. This compositional change narrowed the race and class gaps in educational attainment by 20 percent.

When the composition of students changed, the nature of schooling under a compulsory system also likely changed. Bowles and Gintis (2002), revisiting their 1976 work, describe the system of mass schooling as a system for cultural transmission. In particular, schools cultivate children to become good workers, as a “well-socialized labor force” for employers (p. 20). The socialization they describe in mass schooling, echoed by popular commentators such as John Gatto (2000; 2002), emphasizes obedience to authority and conformity―behaviors unlikely to lead to innovations (McDonald & Gray, 2019).

In his account of the history of the public school system, Currie-Knight (2020) pointed out that “schools inculcated students with habits that would fit them for industrial wage work.” Additionally, Bowles and Gintis (1976) argued that the founder of American public education, Horace Mann, “was a supporter of the industrial system,” and that public schools were meant to train students to take “on as their own the values and objectives of those in authority.”

Patents and Productivity

We consider two measures of innovation: state-level, annual data on patents issued by state and state-level decennial data on real output per worker.

We retrieved annual patent data for each state and the District of Columbia from the U.S. Department of Commerce Patent and Trademark Office from 1883 to 2015.[1] The U.S. Patent Office provides state-level counts of patents by year. For Figure 2, we aggregate these counts by year for a national trend in patenting beginning in 1883. Patents in the U.S. typically number between 3 and 4 per 10,000 people with no over-arching trend over this period. Given the unusual behavior in patenting during World War II, we end our sample in 1940.

The real output per worker data are from Haines and ICPSR (no date) for the Census years. Figure 3 presents the available data. Productivity generally is increasing through this period, with a dip in the middle of the Great Depression. Worker productivity may be influenced by compulsory schooling laws in at least three ways. First, the factory model of schooling implied by compulsory schooling laws may lead to more-compliant workers relative to a system of home education, which would increase their output. Second, compulsory schooling laws may increase education, increasing innovation and productivity. Third, compulsory school laws and the resulting factory model of schooling may stifle creativity, innovation, and productivity. Because of these mechanisms potentially pushing in opposite directions, we might expect that the effect of compulsory schooling laws on real output per worker are weaker than their effects on patenting.

[1] Patent data from 1883 to 1962 were retrieved from Kreps, Ancker-Johnson, Dann, & Lawson (1977). Patent data from 1963 to 2015 were retrieved from the U.S. Patent and Trademark Office Patent Technology Monitoring Team Extended Year Set – Patent Counts By Country, State, and Year (December 2015): https://www.uspto.gov/web/offices/ac/ido/oeip/taf/cst_utlh.htm?fbclid=IwAR1G14Bzf6D4f1pI4dJVPofabjpmFgXu6wJu2WIzX51CvKGKNOrlTwZbiT8

What drives innovation is an ongoing question for entrepreneurs and policymakers. Lerner (2002) reviews the international and historical evidence and concludes that, once the specification controls for trends in patenting, changes in patent law are uncorrelated with patent activity. Boldrin and Levine (2013) summarize the literature on patents, concluding that patents are more common in competitive industries confirming the results in Stigler (1956). Boldrin, Correa, Levine, and Ornaghi (2011) find no correlation between patent activity and productivity. This suggests that our two measures capture different aspects of innovation.

Some recent evidence suggests a link between institutions and innovation. Wagner and Pavlik (2019) find that U.S. metropolitan areas with more economic freedom experience more patents per capita, across a more diverse set of products, and from a more diverse set of innovators. These results are consistent with a correlation between patents and industry competition. Beeson (2017) observes more research and development investment in U.S. states adopting the Uniform Trade Secrets Act, at least in some industries. Next, we estimate the role compulsory schooling may have played in patenting and productivity.

Empirical Specification

Compulsory schooling laws affect the outcomes of adults many years later. Akcigit, Grigsby, and Nicholas (2017a) provide some background as to the type of people who become innovators and inventors. They match patents with Census records around the turn of the 20th century. They document several relevant empirical regularities: densely populated, financially developed, and geographically connected states were more inventive; inventors were more educated; and inventors were most productive between the ages of 36 and 55. We use this last fact to motivate the lag-structure of our specification. Early laws typically required attendance between ages 8 and 14, although there is substantial variation. This implies about a 25- to 40-year lag in effects on patenting as illustrated in Figure 4.

Given the timing of the likely effect, we consider several specifications. We have data only in Census years for the productivity measure. The limited number of available years and resulting degrees of freedom restrict the flexibility we can include in the specification. We estimate a standard difference-in-differences specification for state *s* in year *t*, lagging the indicator for adoption of the law by 20 years.

We also estimate a specification allowing the effect of the law to change non-linearly over time.

$$outcome_{st} = \beta_1 law_{t-20} + \beta_2 years\ since\ law_{t-20} + \beta_3 years\ since\ law_{t-20}^2 + X'\delta + \gamma_s + \tau_t + \varepsilon_{st}$$

We have annual data on patents by state. The large increase in the number of observations—and degrees of freedom—allow us to estimate a more flexible specification. We estimate an event study framework:

$$outcome_{st} = \sum_{p=-10}^{45} \beta_p law_{t-p} + X'\delta + \gamma_s + \tau_t + \varepsilon_{st}$$

The event study more flexibly considers the timing of the law's effect on future patent behavior of the affected children.

All specifications include state fixed effects, γ_s, and year fixed effects, τ_t. We also include vector *X* of the following controls: the percent of bordering states with compulsory schooling laws, the number of years since the state entered the union, the average age of the population (linearly interpolated between Census years when necessary), and the farmland per worker in the state.[2] Summary statistics for these variables are presented in Table 1.

Akcigit, Grigsby, and Nicholas (2017b) show that immigrant inventors were more productive than native-born inventors in the U.S. The impetus for compulsory schoolings laws, in part, stemmed from a backlash against wider spread immigration and a desire to compel the new immigrants to assimilate (Katz 1976). To the extent that laws were passed earlier in states with more immigrants, and the higher inventiveness of immigrants, our results are biased toward finding positive effects of compulsory schooling laws on patenting.

Results

Table 2 presents the difference-in-differences results with the law indicator lagged 20 years. The data on real output per worker are available in the Census years leading up to 1929 and then annually. We use the Census years from 1890 to 1940 to estimate these specifications, giving us six years of observations for each state (1890, 1900, 1910, 1920, 1930, and 1940). For comparability between the two outcome variables, we first use these same years for patents. The six observations per state provide only limited degrees of freedom; we estimate parsimonious specifications.

The specification in Table 2 assumes the law takes at least 20 years to affect patenting or productivity. Columns (1) and (5) include only the state fixed effects, year fixed effects, and law indicators. These estimates suggest that patenting is higher and output per worker lower in years after a compulsory schooling law. Columns (2) and (6) allow the effect of the law to change the longer the law is in place. We continue to observe higher levels of patenting post-compulsory

[2] Data on farmland, labor force, and output are from Turner, Tamura, and Mulholland (2013).

schooling laws, although the effect is not statistically significant. Once we allow the effect to change the longer the law is in place, we observe statistically significant declines in real output per worker once the law has been adopted for a least a few years.

The remaining columns add a limited number of control variables and produce qualitatively similar estimates. Patents are lower in years following the compulsory schooling laws, although the estimates are not generally statistically significant. When the effect is allowed to change over time, real output per worker is lower once the law has been in effect for a few years.

Because we have annual data on patents, we can estimate more-expansive specifications. The first four columns of Table 3 present estimates for logged patents per 10,000 using the annual observations and the Table 2 specifications. These specifications suggest that the initial effect of compulsory schooling is to increase patents, although the magnitude of the increase slowly declines over time. The estimated effects are not sensitive to adding this limited set of control variables. Columns (5) through (8) also include state-specific linear time trends. Adding the state-specific trends changes the picture: although compulsory schooling laws initially raise patenting, over time, the effect quickly turns negative, with or without the added control variables in the specification.

Figure 5 presents the event study estimates. The omitted year is the year of adoption. Each variable represents five-year time spans prior to or following adoption. The coefficients gradually become more negative after about 20 years, becoming statistically less than zero at the 30- to 34-year mark, when the initial cohorts would be around 45 years of age.[3] The last lagged

[3] In 1910, the median years of schooling completed was 8 years. Assuming those in school ranged from aged 6 to 13 or 14 years old, the laws induced this cohort to stay in school longer and changed the nature of their schooling and peers. Thirty years later, those students are 36 to 43 or 44 years old; 35 years later, they are 41 to 48 or 49 years old.

term is 55 years or more. The coefficient on this variable is somewhat less negative than the 50 to 54 year variable, consistent with the non-linear response shown in Table 2.

Taken together, the results suggest that the adoption of compulsory schooling led to lower rates of patenting 30 years following their adoption—when the initial cohorts would be roughly in their mid-forties, prime years of patent activity for innovators (Akcigit, Grigsby, & Nicholas, 2017a). Estimates using output per worker also suggest declines in output per worker following the adoption of compulsory schooling laws.

Conclusion

Economists such as Herb Gintis and Samuel Bowles, as well as experienced public school teacher and popular author John T. Gatto (2002), have long argued that mass schooling is "dumbing us down." We consider how the initial adoption of compulsory schooling affected state-level rates of patenting and real output per worker, finding declines in both once the originally affected cohorts reach middle age. Compulsory schooling laws may have reduced innovation and productivity by reducing home education in favor of formal schooling or by changing the nature of formal schooling. Because this is the first empirical study linking compulsory schooling laws to innovation and productivity, more research is needed on the topic. Research on the specific mechanisms linking formal schooling and homeschooling to innovation is especially welcome.

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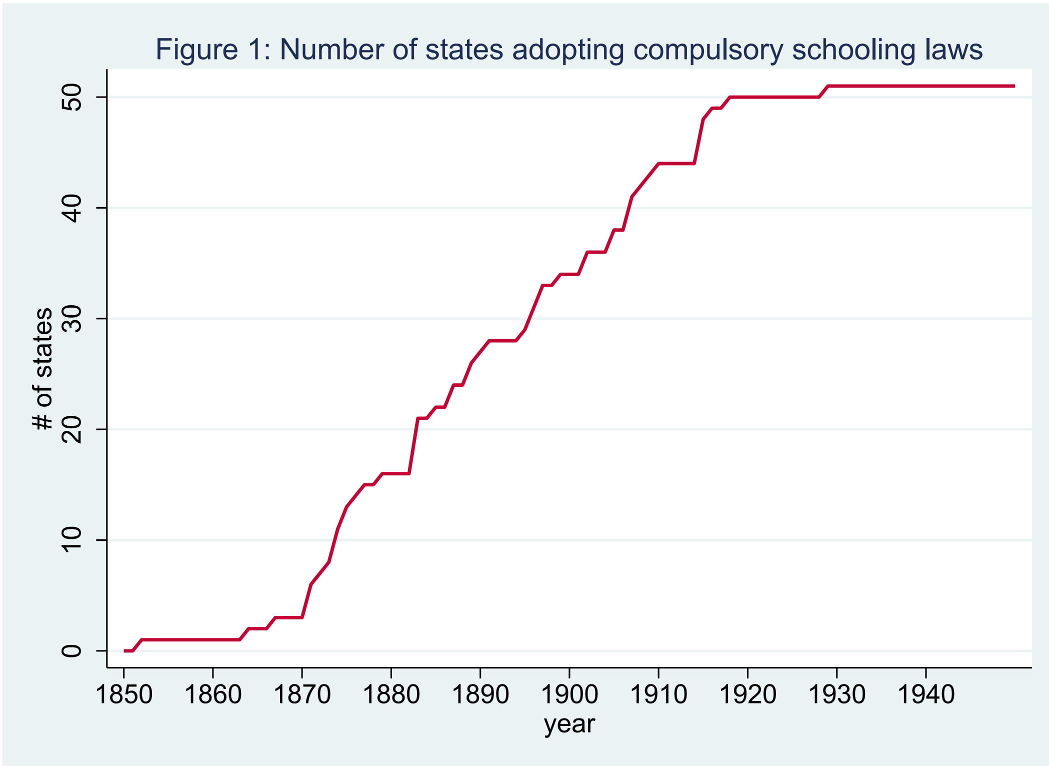


Figure 1: Number of states adopting compulsory schooling laws
# of states
0
10
20
30
40
50
1850
1860
1870
1880
1890
1900
1910
1920
1930
1940
year

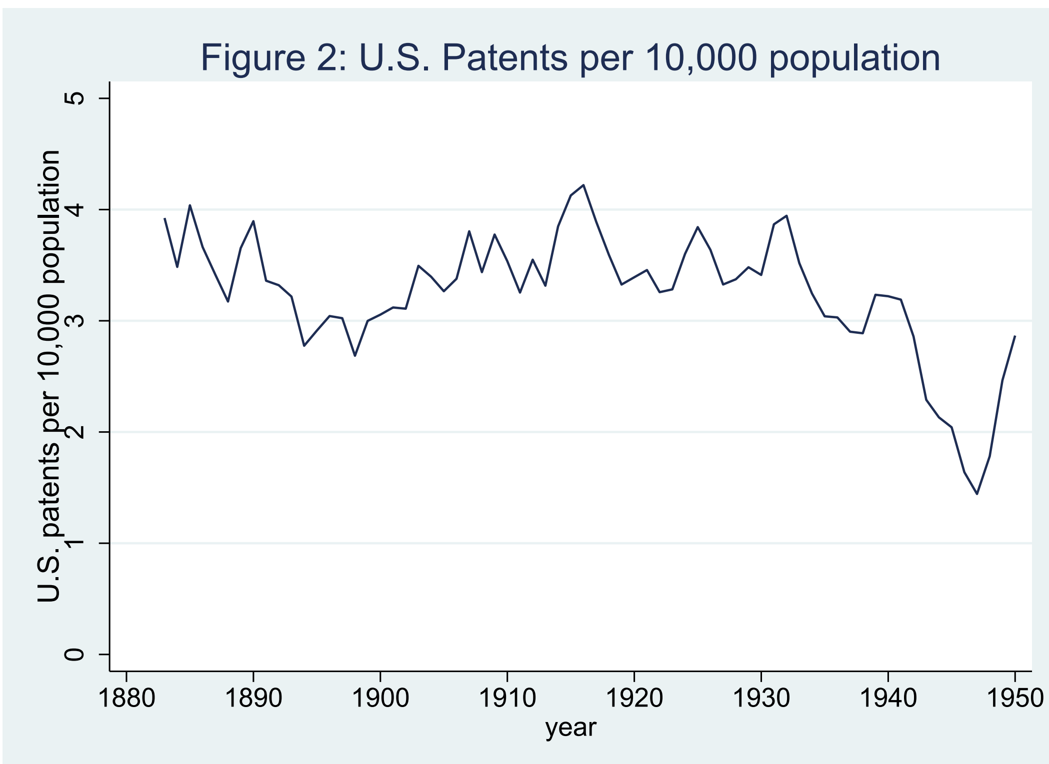


Figure 2: U.S. Patents per 10,000 population
U.S. patents per 10,000 population
0
1
2
3
4
5
1880
1890
1900
1910
1920
1930
1940
1950
year

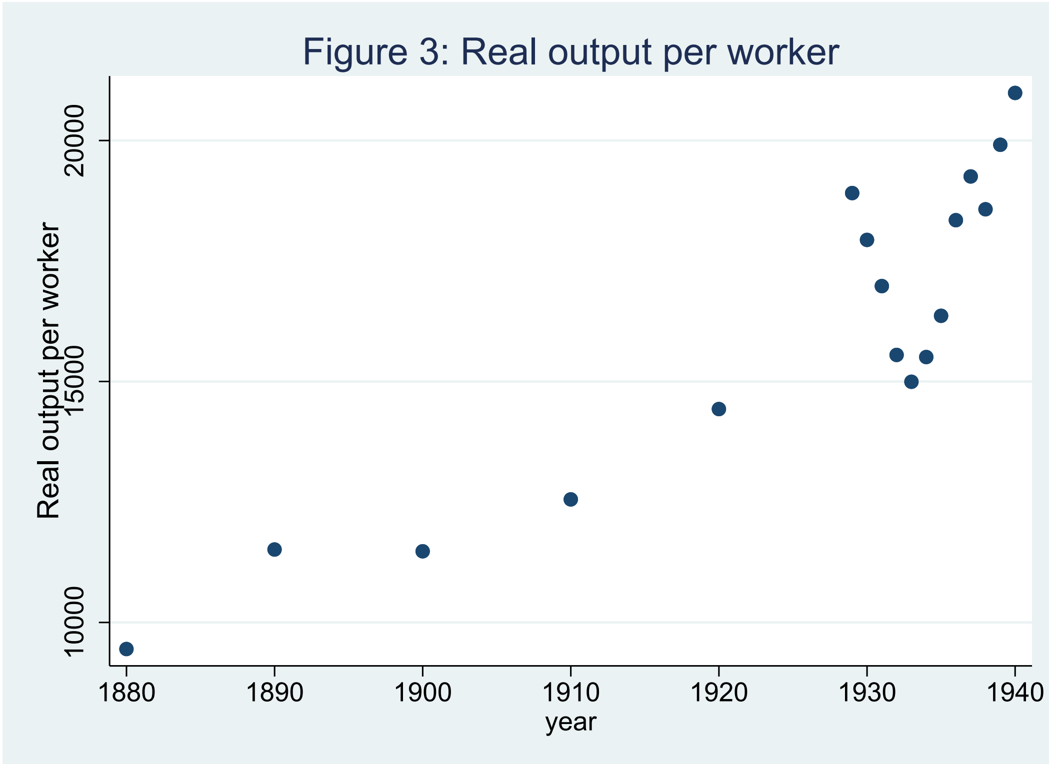
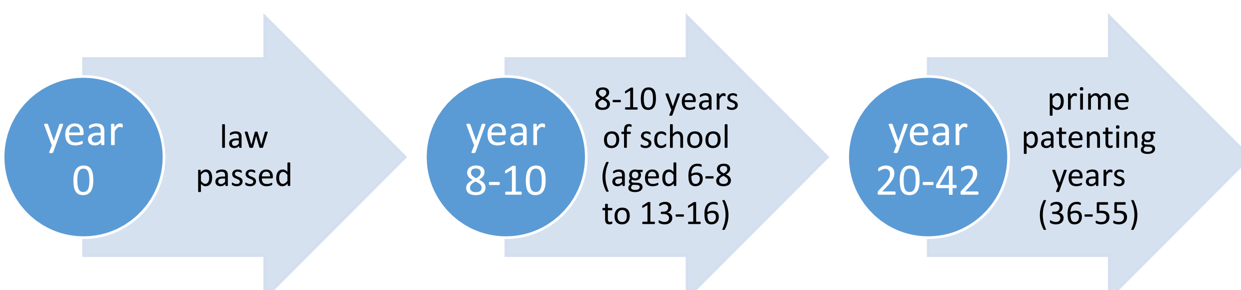


Figure 4: Timeline of compulsory schooling law and possible effect on patenting



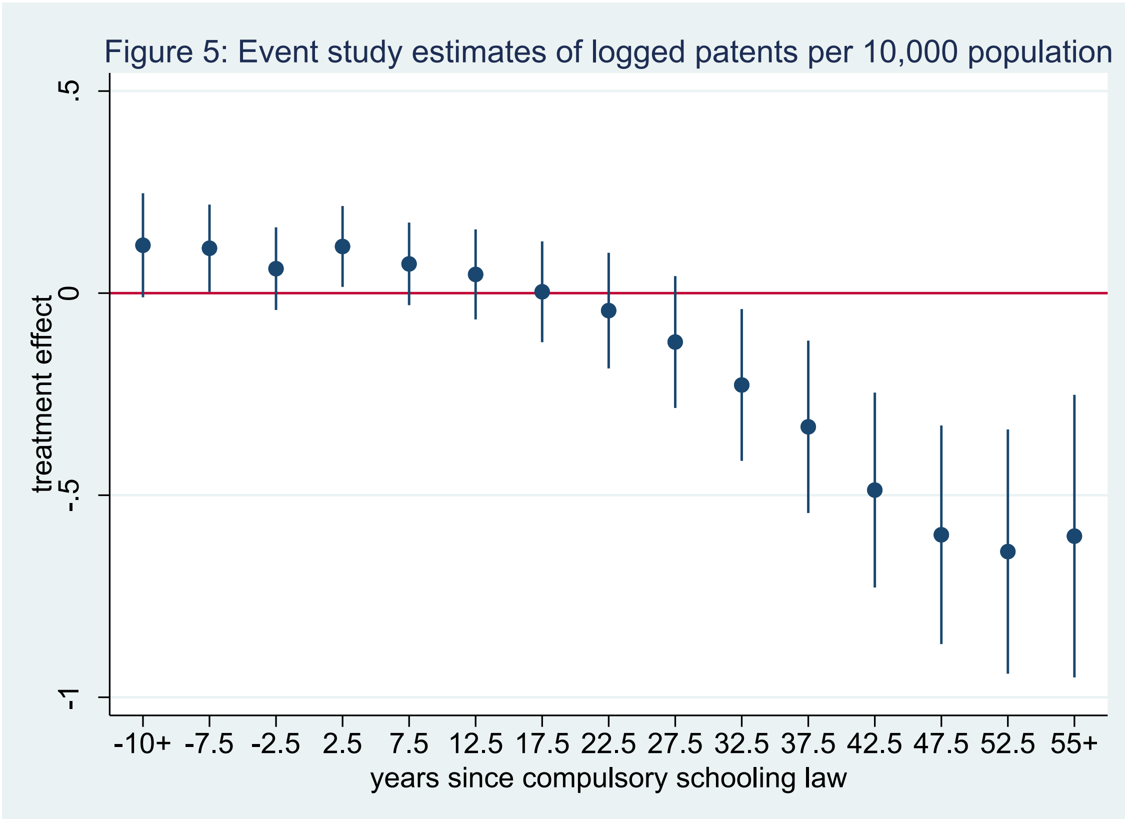


Table 1: Summary statistics

	Mean	Std. Dev	Min	Max
Decennial sample (n = 283)				
Real output per worker	13628.5	5152.1	2870.0	28742.7
Patents per 10,000	3.4	5.6	0.0	33.4
Years in Union	81.8	43.9	-22	153
% Neighbors with Law	0.9	0.3	0	1
Average age in pop	31.4	2.0	28.8	34.7
Farmland per worker	4468.7	627.9	3523.3	5587.3
Annual sample (n = 2772)				
Patents per 10,000	3.3	5.5	0.0	36.7
Years in Union	77.2	44.4	-29	153
% Neighbors with Law	0.8	0.3	0	1
Average age in pop	31.0	1.9	28.0	34.7
Farmland per worker	4320.2	501.8	3523.3	5587.3

Table 2: Difference-in-differences estimates of compulsory schooling laws, patents, and productivity (1880 to 1940)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(patents per 10,000)				ln(real output per worker)			
Compulsory Schooling Law$_{t-20}$	0.098	0.095	0.090	0.0615	-0.015	-0.018	-0.003	0.005
	(0.078)	(0.095)	(0.078)	(0.096)	(0.033)	(0.038)	(0.031)	(0.038)
Years since law$_{t-20}$		0.002		-0.004		-0.011***		-0.006*
		(0.007)		(0.008)		(0.003)		(0.003)
Years since law$^2_{t-20}$		-0.000		-0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Years in Union			0.058***	0.061***			-0.022***	-0.018***
			(0.015)	(0.015)			(0.006)	(0.006)
% Neighbors with Law			-0.193	-0.316*			0.299***	0.216***
			(0.121)	(0.172)			(0.048)	(0.068)
Average age in pop			-0.601***	-0.595***			0.307***	0.298***
			(0.149)	(0.151)			(0.059)	(0.060)
Farmland per worker			0.000	0.000			-0.000	-0.000
			(0.000)	(0.000)			(0.000)	(0.000)
R-squared	0.208	0.208	0.216	0.220	0.728	0.761	0.767	0.771

Notes: There are 283 observations over 48 states. All regressions include state and year fixed effects. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Estimates using annual data on logged patents per 10,000, 1883-1940

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(patents per 10,000)				ln(real output per worker)			
Compulsory Schooling Law$_{t-20}$	0.088***	0.094***	0.088***	0.091***	0.027	0.076***	0.027	0.070***
	(0.025)	(0.030)	(0.025)	(0.030)	(0.020)	(0.027)	(0.020)	(0.027)
Years since law$_{t-20}$		-0.002		-0.003		-0.017***		-0.019***
		(0.002)		(0.003)		(0.003)		(0.003)
Years since law$^2_{t-20}$		0.000		0.000		0.000***		0.000***
		(0.000)		(0.000)		(0.000)		(0.000)
Years in Union			3.752	3.755			3.770	3.792
			(3.500)	(3.500)			(2.548)	(2.526)
% Neighbors with Law			0.001	-0.044			-0.023	-0.102**
			(0.039)	(0.050)			(0.045)	(0.047)
Average age in pop			-33.825	-33.841			-33.784	-33.907
			(31.367)	(31.366)			(22.836)	(22.638)
Farmland per worker			0.005	0.005			0.005	0.005*
			(0.004)	(0.004)			(0.003)	(0.003)
R-squared	0.232	0.233	0.232	0.233	0.600	0.607	0.600	0.607

There are 2,772 observations over 48 states. All regressions include state and year fixed effects. Columns (5) through (8) include state-specific linear time trends. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.