Draft November 2025

Measuring the Effect of Blue Sky Laws on Capital Formation

Paul Mahoney
University of Virginia School of Law
pmahoney@law.virginia.edu

Jianping Mei
Cheung Kong Graduate School of Business
jpmei@aol.com

Abstract

"Blue sky" laws (BSLs) of the early 20th century required a regulator's permission to sell securities to the public in the relevant state, which the regulator granted or denied after conducting a "merit review". Previous studies find that BSLs have positive effects on publicly traded companies. We measure their effect on capital formation. We find that BSLs are associated with lower growth in manufacturing capital. While BSLs may have prevented some fraudulent companies from selling securities to the public, they may also have prevented enough sound companies from doing so to more than offset those benefits.

Keywords: Securities regulation; Blue Sky laws; Progressive Era
JEL Codes: G24, G38, K22

We thank Bernard Black, Andrei Shleifer, and seminar participants at Brandeis University, the University of California at Berkeley, the University of Oxford, and Five Star Asia Pacific Workshop for comments.

Abstract

"Blue sky" laws (BSLs) of the early 20th century required a regulator's permission to sell securities to the public in the relevant state, which the regulator granted or denied after conducting a "merit review". Previous studies find that BSLs have positive effects on publicly traded companies. We measure their effect on capital formation. We find that BSLs are associated with lower growth in manufacturing capital. While BSLs may have prevented some fraudulent companies from selling securities to the public, they may also have prevented enough sound companies from doing so to more than offset those benefits.

Keywords: Securities regulation; Blue Sky laws; Progressive Era

JEL Codes: G24, G38, K22

"Blue Sky" laws (BSLs), adopted and enforced at the state level, were the first comprehensive system of securities regulation in the United States. The statues typically required companies to file information with a state official and receive its permission to sell securities to the public in that state. In most states, the official gave or denied permission after conducting a "merit review" of the fairness of the offering to investors.

These statutes differed from modern securities laws in two important respects. First, they did not require disclosure to investors, but instead to a state official. Relatedly, permission to sell the securities was based not on the adequacy of disclosure, but on the state official's assessment of the quality of the securities.

Investor protection laws can in principle reduce a firm's cost of capital (Shleifer and Wolfenzon 2002; Lambert, Leuz and Verrecchia 2007; Houston, Lin and Xie 2018; Hao 2024). For the reasons just stated, one might doubt that BSLs were effective investor protection measures. Critics argued that administering officials used opaque and possibly political criteria to grant or deny approval (Parrish 1970). When the federal government decided to regulate primary offerings of securities in 1933, it turned to a British model of public disclosure rather than the BSL model of merit review.

Nevertheless, Agrawal (2013) finds that BSLs had a positive impact on publicly traded companies in the early 20th century. BSLs are associated with greater equity issuance, firm growth, and dividend payouts. Using data from 2001 to 2010, Brüggemann *et al.* (2018) also find that companies subject to modern BSLs are more liquid in states with stricter BSLs. Their sample consists of securities offerings not subject to registration with the Securities and Exchange Commission, primarily of small companies traded over the counter.

Given the questionable design of the BSLs, what explains these findings? One possibility is that tests limited to publicly traded firms do not reveal the BSLs' full costs and benefits. Those costs may be realized principally in investments that are not made and firms that do not appear in the data.

This effect could be substantial if the administrators of BSLs often rejected proposed offerings by non-fraudulent companies. It seems likely that large companies that were already publicly traded would be more likely to receive permission than smaller, less well-known (and likely riskier) companies. Publicly traded companies might also more easily raise capital out of state, making the home state's BSL less binding on them. BSLs could therefore have a positive impact on publicly traded firms but a negative impact overall.

To the best of our knowledge, no prior paper has attempted to assess the impact of BSLs on capital formation. Data availability for the early 20th century presents a challenge. We lack state-level data on securities offerings or industrial production. The best available measure is one of aggregate capital employed in the manufacturing sector at 5-year intervals from 1899 to 1919. We use it as our primary proxy for capital formation.

Using heterogeneous difference-in-differences (DiD), we find that BSLs have a negative impact on capital. We interpret the result as indicating that administrators denied permission to a sufficiently large number of non-fraudulent companies to hamper the growth of capital in their states. Consistent with this interpretation, we also find that dividend payouts are lower and more businesses fail after enactment of a BSL.

Finally, we make a case study of Kansas, the first state to adopt a BSL. The Kansas bank commissioner was a driving force behind the first generation of BSLs. He proposed that Kansas regulate securities sales and urged other states to follow its lead. His own public statements and a later study indicate that he used his discretionary authority enthusiastically and perhaps overbroadly to prevent sales of securities in Kansas. We use a synthetic controls approach and find that manufacturing capital in Kansas lags substantially behind that of its synthetic twin over the three years following enactment of the statute.

We conclude that the positive impact of BSLs on publicly traded companies found in prior studies does not carry over to the entire commercial sector. The likely reason is that smaller and riskier companies found it difficult to gain permission to raise external capital through securities sales. These same companies would also have found it difficult to raise long-term capital from banks. Bank involvement in long-term finance in that era tended to be through purchases of securities rather than long-term loans (Mehrling 2011, p. 34). Immature companies were therefore likely to grow more slowly in states with BSLs.

The paper contributes to the literature on the optimal design of securities regulation. Prior papers discuss the benefits of mandated disclosure generally (Grossman and Hart 1980; Dye 1990; Admati and Pfleiderer 2000; Jiang, Xin and Xiong 2023) while raising some caveats (Kripke 1979; Ben-Shahar and Schneider 2014; Haeberle and Henderson 2018). Numerous papers assess the effects of specific changes to U.S. disclosure rules (Bushee and Leuz 2005; Ferrell 2007; Honigsberg, Jackson and Wong 2015; Leuz and Wysocki 2016; Chiu, Guan and Kim 2018; Honigsberg 2019).

Our paper studies an earlier approach to investor protection that relied on screening by a government official and finds that it was ineffective and indeed counterproductive. The finding has ongoing relevance because some developing countries today continue to rely on screening of potential offerings by a regulator. Our results are also related to those of Ewens, Xiao and Xu (2024), who find that excessive regulatory costs induce sub optimal investment.

The paper also contributes to a growing literature on the Progressive Era policy environment. After the New Deal, most aspects of the regulatory system for financial and product markets were federalized and therefore uniform throughout the United States. Previously, many of these policies were either set or implemented at the state level and were not uniform.

Studying the consequences of financial policies during the Progressive Era is challenging because of the paucity of data. Several recent papers, however, make creative use of available information to draw lessons from differences in policy substance or implementation across states (Dehejia and Lleras-Muney 2007; Rajan and Ramcharan 2011; Agrawal 2013; Calomiris and Jaremski 2022; Hilt, Jaremski and Rahn 2022; Del Angel and Richardson 2024). We similarly find that cross-state policy differences had measurable economic effects.

INSTITUTIONAL BACKGROUND OF THE BLUE SKY LAWS

Kansas enacted the first blue sky law in 1911.[1] With some exceptions, the statute made it illegal for any person to sell securities in Kansas unless it first filed information with the state's bank commissioner and received his permission. The statute instructed the bank commissioner to permit the sale only if "in his judgment [the security] promises a fair return."

Twenty-four other states adopted BSLs by the end of 1913. Twelve of them followed the Kansas statute closely. The remainder required pre-clearance of public offerings, licensing of dealers, or both. By the early 1930s, every state except Nevada had adopted a BSL.

[1] An Act to provide for the regulation and supervision of investment companies and providing penalties for the violation thereof, Kansas Session Laws, 1911, chapter 133, pp. 210-219.

Figure 1 shows the pattern of adoption. The earliest and largest wave, from 1911 to 1913, was concentrated in southern and Great Plains states. The largest northeastern states were late adopters.

Although state governments likely had limited enforcement resources, non-compliance with a BSL would have been risky. Unless and until the regulator approved an application, it was a criminal offense for the company not only to sell securities, but to engage in any business in the state. We examined correspondence of the Missouri Bank Commissioner from the early 20th century and found that investors, lawyers, and accountants sometimes inquired whether a specific company had received clearance under the state's BSL. These inquiries would bring potential noncompliance to the administrator's attention. Opinions in reported cases demonstrate that state governments brought criminal prosecutions against suspected violators.

Even a successful application could impose substantial and uncertain costs. Under a typical statute, the administrator had the right to inspect the company's properties and operations at the company's expense, including travel expenses for the Commissioner or staff.

There was some uncertainty about which state's BSL applied to transactions that crossed state borders, as when a customer in Kansas received a solicitation letter from a New York broker and responded with an order. The most common, although not universally agreed, answer was that the contract was created where the acceptance took place (New York in this situation) and only that state's BSL applied (Loss and Cowett 1958, Chap. V).

This issue was important because states with major financial centers were late adopters and because New York's BSL did not require pre-clearance of public offerings. It therefore seems likely that BSLs imposed greater restraints on small, less prominent businesses reliant on local capital than on publicly traded companies, whose shares might be dealt in by brokers in

New York, Boston, or Philadelphia. We attempt to assess the impact on all businesses, not just those publicly traded.

DATA AND EMPIRICAL STRATEGY

Our principal proxy for the cost of external capital is manufacturing capital as collected in the quinquennial census of manufacturing and reported in the *Statistical Abstracts of the United States*. The census defines capital as the sum of fixed assets, inventory, and receivables.[2] The level accordingly measures the main tangible assets of the manufacturing sector, while changes over time measure investors' willingness to commit capital, either by providing new funds or permitting the firm to retain earnings.

The quinquennial capital series begins in 1899 and ends in 1919. Although these data are low-frequency and cover a limited period, growth in manufacturing capital is the best measure of new capital formation available at the state level.[3] The limitation to manufacturing firms is not concerning because other important sectors, including railroads, utilities, and financial services, were subject to separate regulatory systems and sometimes exempted from BSLs.[4]

We begin by examining pretrends in the outcome measure to assess the plausibility of the parallel trends assumption underlying DiD. Figure 2 shows the evolution of manufacturing capital in treated states (those that adopted BSLs before 1919) and the remainder, or control

[2] U.S. Department of the Interior, Census Office, Report of Manufacturing Industries in the United States at the Eleventh Census: 1890. Part I, p. 10..

[3] Agrawal (2013) uses data on authorized capital from a limited number of states. Authorized capital, however, was a regulatory and not a market measure (Manning and Hanks 2013). State corporate laws required varying levels of minimum capital as a protection for creditors. The authorized capital identified in a company's charter bore no necessary relation to invested capital. It is plausible, moreover, that states with strict BSLs also had stricter minimum capital provisions.

[4] For example, Section 3 of the California BSL, adopted in 1913, exempted railroads, banks, insurance companies, and "mutual water companies".

states. Capital in the control states increases more than in the treatment states from 1899 to 1909, calling the parallel trends assumption into question.

We attempt to deal with this by identifying time-varying controls that partly explain the growth of manufacturing capital. We hypothesize that infrastructure, human capital, and natural resources are all associated with the size of the manufacturing sector. We can identify one measure of each in data available at the state level for the period 1899-1919. We also use covariates that predict the early adoption of a BSL. We discuss pretrends in more detail in connection with the empirical results below.

Figure 2 also indicates that the starting levels of manufacturing capital are substantially different in the treated and control states. Capital in the control states is approximately 2.5 times that of the treatment states in 1899. DiD results can therefore be sensitive to the underlying assumption about how the outcome variable evolves absent treatment (Kahn-Lang and Lang 2020). Consistent with observed trends in the growth of the aggregate assets of U.S. nonfinancial businesses over time, we assume that manufacturing capital grows exponentially:

$$E[Capital_{t+1}] = Capital_t e^r \tag{1}$$

where E is the expectation operator and the growth rate, r, is conditional on the covariates. We therefore estimate our models in logs rather than levels.

Our measure of infrastructure is the density of the state's railroad network, calculated as the number of railroad miles per 100 square miles of land area. A denser railroad network should reduce transportation costs and thereby encourage the formation of manufacturing businesses. Our measure of human capital is per-pupil educational spending, or total expenditure on primary and secondary education divided by the school-age population. Our measure of natural resources is coal production. Twenty-eight of the 48 states were coal producers. Coal was an important energy source and a spur to the development of heavy industry in states like Pennsylvania, Alabama, and Ohio. All three measures are available at annual intervals and hand-gathered from the *Statistical Abstracts*.

Infrastructure, human capital, and natural resources should influence the growth rate of manufacturing capital. The reverse could also be true; growth in manufacturing capital could cause an increase in the covariates. We expect this effect to appear with a delay, however. As an additional protection against endogeneity, we lag each covariate by one year. Regressing the growth rate of manufacturing capital from 1899 to 1909 against the growth rates of railroad density, per pupil spending, and coal production from 1898 to 1908 produces positive and significant coefficients on all three independent variables and results in an adjusted r-squared of 70%, indicating that these measures explain a substantial part of the growth rate r in Equation (1).

Mahoney (2003) finds that states that adopted other regulatory legislation advocated by progressive politicians were more likely to adopt BSLs in the early wave. Macey and Miller (1991) argue that lobbying by small unit banks was a substantial factor in the enactment of BSLs.

As covariates predicting BSL adoption, therefore, we use an index of progressive legislation derived from Fishback and Kantor (1998), consisting of a state-year count of the number of progressive statutes on the books, out of a possible nine. We download the data from Price Fishback's website. We also use the average assets of a state's depositary institutions as a measure of the importance of small unit banks. The number and assets of national, state, and private banks, trust companies, and mutual and stock savings banks are hand gathered from the *Annual Reports* of the Office of the Comptroller of the Currency.

Beginning in 1917, the Internal Revenue Service published data on incomes subject to taxation in the *Statistics of Income*, which we use in some of the tests. We divide some of our outcome measures by population. Annual population estimates by state are taken from the FRED database maintained by the Federal Reserve Bank of St. Louis.

Dun's Statistical Service published quarterly counts of commercial firms (comprising trade and manufacturing) that failed in each state. The *Statistical Abstracts* aggregated these on an annual basis and reported them as a percentage of beginning of year commercial establishments. We use both the number of failed firms and the number of establishments in some of the tests.

Our source for the substance and enactment dates of BSLs is the *HeinOnLine* legal database, which contains digitized versions of state laws. We assume that enforcement begins in the year following the statute's enactment and therefore consider the state treated beginning in that year. Treated states for our DiD analysis of capital therefore include all states that adopted BSLs from 1911-1918, inclusive. The Data Appendix includes the date of adoption of each state's initial BSL and a citation to the relevant state's session laws.[5]

Table 1 provides summary statistics for the outcome measures, covariates, and population observed in 1909, prior to the first BSL adoption, or 1917, the first available year, in the case of income tax data. The table separately shows summary statistics for the 30 states treated before 1919 and the 18 states treated later or never. These measures indicate that the treated states are, on average, smaller and less economically developed than the control states, but with substantial variation within each category.

[5] Mahoney (2003) contains a table of BSL adoptions and characteristics. The present authors used subsequently available machine reading technology to do full-text searches of state session laws and statutory compilations, allowing us to expand on the Mahoney table as well as identify and correct a few errors.

The treated states for our main test fall into two cohorts: those that adopt a BSL before 1914 and those that adopt between 1914 and 1919. Recent papers caution that the standard two-way fixed effects DiD model can produce biased estimates when treatment occurs at different times and treatment effects vary by cohort (Goodman-Bacon 2021; Sun and Abraham 2021).

We therefore use the heterogeneous DiD framework of Callaway and Sant'Anna (2021), which allows treatment effects to vary by cohort. The two treatment cohorts are identified by $g \in [1, 2]$. Indicator variables G_{ig} equal one if state i is part of cohort g and zero otherwise. For each cohort-year pair (g,t), there is an associated control group indicated by the variable C_{gt}.

We estimate treatment effects using a doubly robust augmented inverse propensity weighted estimator employing separate covariates to predict treatment and outcome (Glynn and Quinn 2009; Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2021). The first step is to estimate the following regression for a data set consisting of each untreated state-period observation:

$$m_{it} = \alpha + \beta \boldsymbol{x_{it}} + \varepsilon_{it} \tag{2}$$

defining $m_{it} = y_{it} - y_{i0}$, period 0 = t-1 for $t<g$ and g-1 for $t>g$, and $\boldsymbol{x}$ as a set of covariates that affect outcomes. Intuitively, this equation estimates the effect of covariates on outcomes absent treatment.

The second step uses a logit regression to estimate a propensity score, p_{igt}, or the probability that state i is a member of cohort g conditional on a separate group of covariates, $\boldsymbol{z}$, given that i is not a member of any prior cohort:

$$log\left(\frac{p_{igt}}{1-p_{igt}}\right) = \beta \boldsymbol{z}_{it} + \varepsilon_{it} \tag{3}$$

The fitted values $\widehat{m}_{it}$ from equation (3) and $\hat{p}_{igt}$ from equation (4) are then used to estimate the average treatment effect on the treated for each cohort-period:

$$\widehat{ATET}_{gt} = \frac{1}{i}\sum_i \left[\frac{G_{ig}}{\frac{1}{i}\sum_i G_{ig}} - \frac{\frac{\hat{p}_{igt}C_{gt}}{1-\hat{p}_{igt}}}{\frac{1}{i}\sum_i \frac{\hat{p}_{igt}C_{gt}}{1-\hat{p}_{igt}}} \right] \left[y_{ig} - y_{i,g-1} - \widehat{m}_{it} \right] \quad (4)$$

MAIN RESULTS

Results for the heterogeneous DiD test using manufacturing capital as the outcome variable are shown in Table 2. The estimated treatment effect for the early cohort and for the two cohorts combined are negative and significant. A joint test that all treatment effects are zero in the pre-treatment period does not reject the null (p=0.17), consistent with the parallel trends assumption.

The estimated treatment effect for the late cohort standing alone is insignificant. The cohort consists of only five states, which may limit the power of the test. Alternatively, there may be something unique about the early cohort such that we cannot generalize the negative result for that cohort to all BSLs. We address this possibility using outcome measures that are available for later periods.

A few less populated states had initially low levels of manufacturing capital but large percentage increases over the sample period. As a robustness check, we winsorize the annual growth rate of manufacturing capital at the 5% and 95% levels and re-run the model. The untabulated results remain qualitatively unchanged; the point estimates are lower in absolute magnitude but with correspondingly smaller standard errors.

Another assumption underlying DiD is that treatment affects outcomes only in the treated states and does not spill over into the control states, known as the stable unit treatment value assumption (Angrist, Imbens and Rubin 1996). In our setting, the regulated activity is the sale of securities, while the outcome measure is capital deployed in the relevant state. If, for example, in 1915 a company doing business in Missouri, which had a BSL, sold securities in Illinois, which did not have a BSL, the resulting capital increase would be "credited" to Missouri in the data. There would be no spillover.

Spillovers would arise if existing businesses chose to migrate from treatment to control states or entrepreneurs shifted new business formation to control states. Such moves on more than a modest scale are implausible for two reasons. First, additional states were adopting BSLs throughout our sample period, meaning that a company could not assume that moving to a new state would avoid BSLs. Second, in the early 20th century, business activity was still localized to a substantial extent. Moving to another state would disrupt customer and supplier relationships or, in the case of an entrepreneur, support networks.

We nevertheless test for outmigration of business enterprises from treated to control states. Card and Krueger (1994) study the effects of state-level minimum wage laws on restaurant employment in a setting like ours where national chains and their franchisees might decide to open more new establishments in states with a low minimum wage. They document that the rate of new openings is not sensitive to differences in state minimum wage laws and therefore conclude that the SUTVA is not violated.

In the same spirit, we test for changes in the growth rate of business establishments in treated and control states after treatment. We use the same procedure as for the tests of manufacturing capital, but with the log of business establishments as the outcome variable.

Because these data are annual, there are more cohorts of treated states. The sample period begins in 1904 and ends in 1922, the last year for which all the covariates are available.

Table 3 shows results. There are seven treated cohorts totaling 38 states. Because of the small number of never-treated states, we use each state not treated at time *g* as a control group for cohort *g*. The results are robust to using only the 10 never-treated states as a control group for each treated state. Aggregating all 38 treated states, the estimated treatment effect is both statistically and economically insignificant. Only one cohort individually has a significant estimated treatment effect, but it is positive. There is accordingly no evidence of outmigration of businesses from treated to control states. Finally, we assess pretrends visually through Figure 3, an event study plot showing estimated treatment effects by length of exposure to treatment. There are no noticeable trends either before or after treatment.

A plausible explanation for the results obtained so far is that businesses in the treated states, particularly smaller and riskier businesses, found it hard to obtain permission to sell securities. If so, these firms would be capital constrained relative to similar firms in control states. Among other things, this should lead them to pay fewer dividends. Conversely, if BSLs improved investor protection, firms in BSL states would have greater access to external capital and should pay more dividends (Shleifer and Wolfenzon 2002).

We therefore use data on dividends reported on tax returns by each state's residents from the *Statistics of Income*. Under the assumption that investors have a home-state bias, this measure should proxy for the dividend payout rates of the state's corporations. A state's BSL might also reduce the number of out-of-state companies in which its residents could invest, reducing dividend payments to residents through this channel as well.

These data begin after more than half of the states had already adopted BSLs. This reduces the sample to 18 states that adopted BSLs after 1917 or not at all. Nevertheless, the test permits us to examine the effect of BSL adoption for later cohorts of adopting states, including some of the most economically developed ones.

Prior to treatment, there are substantial differences in dividend income among the treatment cohorts. In 1917, residents in the 1922 cohort of states (Massachusetts, New Mexico, New York, and Rhode Island) received $703 million in dividends in aggregate, whereas those in the 1920 cohort (Alabama, Oklahoma, Utah, and Wyoming) received $28 million. We assume that dividend income should grow exponentially over time and therefore estimate the model in logs using the heterogeneous DiD model of Equations (2)-(4).

We control for time-varying regional economic trends using region by year fixed effects. The Office of the Comptroller of the Currency divided the country into six regions, and we follow its definitions. We use the normal covariates to predict BSL adoption.

Table 4 and Figure 4 show results. The estimated treatment effect is negative for three of the four cohorts and not statistically distinguishable from zero for the fourth. Although the pre-treatment data are noisy, visual inspection reveals no pretrends. There is a clear downward trend after the BSLs come into force. The results are robust to different specifications, specifically using dividends divided by population or by total taxable income as the outcome measure.

These results suggest that the negative effects found in prior tests are not limited to the cohort that adopted BSLs during the early wave of 1911-1913 but extends to late adopters. They also support the hypothesis that BSLs imposed capital constraints on some firms.

If BSLs denied external financing to some firms in treated states, those firms should be more likely to fail for lack of capital. We test this hypothesis using the *Dunn's* business failure data from 1904 to 1929, inclusive. We employ the heterogeneous DiD model of Equations (2)-(4) to test for differences in failures between BSL and control states, with treatment cohorts *g* identified by the year after BSL adoption. Because the data now extend to 1929, by which all but two states had adopted BSLs, we use all states that had not yet adopted a BSL by year *g* as a control group for cohort *g*.

Following Del Angel and Richardson (2024), who also study business failures during this period, we use the Palmer Drought Severity Index and the number of farm failures as covariates predicting higher commercial failures. We download the Palmer data from the National Oceanic and Atmospheric Administration website and the farm failure data from the Del Angel and Richardson online appendix. We use the normal covariates to predict BSL adoption.

Because the data span a 25-year period and there are many treatment cohorts, we use an event study methodology, aggregating the cohort-year estimates by the length of time exposed to treatment. The results are shown in Table 5 and Figure 5. There is no persistent trend in the differences between BSL and control states before treatment, but a clear rise in failures in treated relative to control states after treatment. The aggregate estimated treatment effect of 2.63 additional failures per year is statistically significant at the 5% level. It is also economically significant in comparison to the average of 8.95 failures per 1,000 establishments in the pre-treatment year 1909 shown in Table 1.[6]

---

[6] It is worth noting that the changes in the number of business establishment equals to new business starts plus net migration and minus business failures. Thus, our results in Table 3 and Table 5 imply that the treatment effect is not significant for the growth of business establishment, but significant for higher business failure rates. But we are not making any statement on either new business starts nor net migration to the treatment states.

CASE STUDY: KANSAS

We now make a case study of Kansas's pioneering BSL. The motivating force behind the statute was J.N. Dolley, a small-town banker and former state legislator appointed as the state's bank commissioner in 1909. After surveying the state's banks, he argued that customers were withdrawing deposits to invest in securities (Loss & Cowett 1958). He then worked with the legislature to draft and enact the BSL. Next, he successfully urged other states to follow Kansas's lead (Mulvey 1916).

Dolley's public statements indicate that he was motivated by a belief that competition from capital markets reduces the profitability and therefore the stability of the banking sector. After enactment of the BSL, he claimed to have rejected most of the applications filed to sell securities in Kansas. Mulvey (1916) examined the records of the Kansas Bank Commissioner for the period March 1911, when its BSL became effective, through April 1913. He found that 111 companies applied to sell securities in Kansas, of which only 49 were approved.

The timing of Kansas's enactment and Dolley's zeal for enforcement make it likely that any positive or negative effects on capital formation would be realized by the time of the 1914 census of manufacturing. We hypothesize that his rejection of many applications to sell securities hindered capital formation in Kansas.

We test the hypothesis using the synthetic controls estimator of Abadie, Diamond and Hainmueller (2010, 2015). These papers apply the synthetic controls approach in case study settings resembling ours, with a single aggregate entity (state or country) subject to a policy change. Rather than comparing the affected state to a mean of unaffected control states, the synthetic controls approach constructs a synthetic twin for the affected unit consisting of a weighted average of untreated states that resembles the treated unit in a sense to be described.

The approach begins by specifying a model for the outcome of interest, y_{it}:

$$y_{it} = \boldsymbol{\beta}_x \boldsymbol{X}_{it} + \varepsilon_{it} \tag{6}$$

where $\boldsymbol{X}_{it}$ is a set of predictive variables, which may include lagged values of y. The next step selects a set of weights for units in the donor pool, or the set of untreated states. These weights minimize the root mean squared prediction error (RMSPE) between the pre-intervention outcomes for the affected unit and their predicted values, given the weights and the coefficients on the predictors, subject to the constraint that the weights sum to unity. Details appear in Abadie, Diamond and Hainmueller (2010, p. 496). The weights are then applied to pre- and post-intervention values of y_{it} for the unaffected units to estimate counterfactual values for the affected unit had the policy change not occurred.

The synthetic controls approach relies on the synthetic twin's ability to approximate the pre-treatment outcome path of the treated unit, which requires a sufficient number of pre-treatment observations (Abadie 2021). We accordingly extend the manufacturing capital series back to 1880, adding two additional observations per state. Oklahoma, the Dakotas, and Wyoming are eliminated because of a lack of data.

Kansas's initial manufacturing capital is low compared to most other states, complicating the search for close matches. We therefore divide capital by population and use that as the outcome measure. The untreated group consists of 21 states that adopted a BSL after 1914.

We use railroad network density and per-pupil educational spending as predictors, both as defined in Section 3 and lagged one year.[7] We also include pre-intervention values of the

[7] Eleven of the 21 control states are not coal producers. Adding the coal production variable produces identical results as it does not change the states included in synthetic Kansas or their weights.

outcome variable (for 1890 and 1904) as predictors. The results are robust to the selection of different pre-intervention years or a single pre-intervention year.

Figure 5 shows the path of manufacturing capital scaled by population for Kansas and synthetic Kansas for years 1880, 1890, 1899, 1904, 1909 and 1914. Table 6 shows the states included in synthetic Kansas and their respective weights, along with the average values of the predictor variables for actual and synthetic Kansas. As is common in published studies using synthetic controls, only a small number of untreated states have nonzero weights.

Synthetic Kansas closely reproduces outcomes for actual Kansas during the pre-BSL period. Between 1909 and 1914, however, manufacturing capital per 1,000 population declines sharply in Kansas compared to synthetic Kansas. The actual value is 16% lower than in the estimated no-BSL counterfactual. To check the reliability of the result, we rerun the analysis, but with the BSL (counterfactually) reassigned to be enacted between 1904 and 1909. Once again, the synthetic Kansas closely reproduces the actual outcomes prior to treatment. Unlike the prior analysis, however, there is hardly any difference between Kansas and synthetic Kansas after "treatment."

The estimated effect of the BSL is clearly economically large. We evaluate its statistical significance through placebo studies in which the treatment is reassigned to untreated states (Abadie, Diamond and Hainmueller 2015). We rerun the synthetic controls model 21 times, each time assigning treatment to a different untreated state. For each state, we then divide the absolute magnitude of the difference between the actual and predicted outcome in 1914 by the RMSPE for the period before 1914. The resulting test statistic measures on a common scale the size of the gap between the actual and predicted post-intervention values of the outcome variable.

Figure 6 plots the test statistic for Kansas and each placebo study. The test statistic for Kansas is 2.8 standard deviations above the donor pool mean and is larger than that of any other state in the donor pool. Because there are 21 untreated states, there is a 4.8% probability that a randomly selected state would have the most extreme value.

DISCUSSION

In 1933, the regulatory experts whom President Franklin Roosevelt asked to draft the Securities Act explicitly rejected merit review in favor of mandatory disclosure to investors (Landis 1959). This is important qualitative evidence that knowledgeable observers concluded that BSLs had a flawed design.

This paper studies the quantitative evidence. The best available proxy for the cost of capital is the manufacturing capital data from the quinquennial census of manufacturing. Heterogeneous DiD tests show that capital grew more slowly after adoption of a BSL. Other tests using data on dividends and business failures produce results consistent with the hypothesis that administrators excessively denied permission for securities sales. The impact on manufacturing capital in Kansas, the first state to adopt a BSL and the model for many other states, is strongly negative.

These results support the proposition that securities laws produce positive results primarily through disclosure to potential investors and liability for misleading statements (La Porta, Lopez-De-Silanes and Shleifer 2006). BSLs, by contrast, rely on a state official's ability to distinguish good and bad companies. The incentives facing the official, moreover, would likely lead it to approve too few offerings. The official would bear primary blame for investor losses, but not for general economic underperformance.

The risk of losing access to their home states' capital markets may have induced companies that were already publicly traded to improve their governance practices after enactment of a BSL, as Agrawal (2013) finds. Part of the initial justification for securities regulation was to improve governance practices in publicly traded firms (Pritchard 2018). At the same time, new entrants must have been more likely than incumbent firms to suffer careless or arbitrary rejections. The state official would bear a political cost from shutting down a firm that was already large enough to be publicly traded. Officials may have been less reluctant to deny market access to privately held companies seeking to raise external capital for the first time.

By focusing on the aggregate capital deployed in a state's manufacturing sector, we can capture this negative and likely unintended effect. The resulting difficulty of raising capital should also have produced lower dividend payouts and more business failures. We find evidence that it did. The result is consistent with a recent study by Kalmenovitz (2023) that attempts to measure the costs of paperwork regulations. He demonstrates that regulatory intensity increases the cost of goods sold and induces companies, among other things, to reduce capital investment.

Modern BSLs might not impede capital formation as much as their earlier predecessors. Modern BSLs apply only to offerings too small to require SEC registration. Equally important, BSLs have evolved to look more like the federal Securities Act. Most BSLs are now based on the Uniform Securities Act drafted by the National Conference of Commissioners on Uniform State Laws, which provides for disclosure to potential investors, not just the regulator. The positive effects that Brüggemann *et al.* (2018) find are not, therefore, inconsistent with our conclusions.

While developed capital markets have gravitated to registration-based systems, developing markets often require regulatory permission for securities offerings. Interestingly, China in early 2023 shifted from a permission-based to a registration-based system for initial public offerings. The China Securities Regulatory Commission indicated that the proposed rules were intended to broaden investor choices and make the IPO process more predictable. Press reports described the shift as an attempt to promote capital formation in the public markets. The shift appears to have been short-lived.[8] Our findings indicate that China and other developing markets might improve capital formation by limiting their regulators' remit to disclosure rather than giving them the authority to reject offerings on substantive grounds.

CONCLUSION

This paper uses multiple hand collected data sets to examine the economic impact of "Blue Sky" laws (BSLs) across different American states in the early 1900s. Unlike previous studies that primarily document the BSLs' benefits to publicly traded companies, we measure their effects on capital formation. Using difference-in-differences models, we find that BSLs result in lower growth of capital, lower dividend payouts, and more business failures. The need to persuade a non-specialist regulator that a company was sound and should be permitted to sell securities appears to have imposed costs on non-publicly traded businesses that impeded capital formation. We draw lessons for emerging markets today.

[8] In March 2024, the China Securities Regulatory Commission (CSRC) announced a new and stricter system of access to initial public offering (IPOs) and listings to improve the quality of firms going public. (See "Policy Document Regarding Stricter Issuance and Listing Access Requirements to Improve the Quality of Listed Companies." Source: http://www.csrc.gov.cn/csrc/c100028/c7467848/content.shtml.) Among other things, it declared a policy of permitting new issuances only when the secondary market has the capacity to absorb them and of prohibiting "excessive" financing. It also announced stricter control of IPO pricing to improve investors' "sense of satisfaction." IPO volume in China fell by 65% in the first quarter of 2024 compared to 2023 and 73 companies withdrew IPO applications (Zhang 2024).

REFERENCES

Abadie, A., 2021. Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects. Journal of Economic Literature 59, 391-425

Abadie, A., Diamond, A., and Hainmueller, J., 2010. Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. Journal of the American Statistical Association 105, 493-505

Abadie, A., Diamond, A., and Hainmueller, J., 2015. Comparative Politics and the Synthetic Control Method. American Journal of Political Science 59, 495-510

Admati, A.R. and Pfleiderer, P., 2000. Forcing Firms to Talk: Financial Disclosure Regulation and Externalities. Review of Financial Studies 13, 479-519

Agrawal, A.K., 2013. The impact of investor protection on corporate policy and performance: Evidence from the blue sky laws. Journal of Financial Economics 107, 417-435

Angrist, J.D., Imbens, G.W., and Rubin, D.B., 1996. Identification of Causal Effects Using Instrumental Variables. Journal of the American Statistical Association 91, 444-455

Ben-Shahar, O. and Schneider, C.E., 2014. The Futility of Cost-Benefit Analysis in Financial Disclosure Regulation. The Journal of Legal Studies 43, S253-S271

Brüggemann, U., Kaul, A., Leuz, C., and Werner, I.M., 2018. The Twilight Zone: OTC Regulatory Regimes and Market Quality. The Review of Financial Studies 31, 898-942

Bushee, B.J. and Leuz, C., 2005. Economic consequences of SEC disclosure regulation: evidence from the OTC bulletin board. Journal of Accounting and Economics 39, 233-264

Callaway, B. and Sant’Anna, P.H.C., 2021. Difference-in-Differences with multiple time periods. Journal of Econometrics 225, 200-230

Calomiris, C.W. and Jaremski, M., 2022. Why Join the Fed? Journal of Economic History 82, 765-800

Card, D. and Krueger, A.B., 1994. Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. The American Economic Review 84, 772-793

Chiu, T.-T., Guan, Y., and Kim, J.-B., 2018. The Effect of Risk Factor Disclosures on the Pricing of Credit Default Swaps. Contemporary Accounting Research 35, 2191-2224

Dehejia, R. and Lleras-Muney, A., 2007. Financial Development and Pathways of Growth: State Branching and Deposit Insurance Laws in the United States, 1900-1940. The Journal of Law & Economics 50, 239-272

Del Angel, M. and Richardson, G., 2024. Independent regulators and financial stability: Evidence from gubernatorial election campaigns in the Progressive Era. Journal of Financial Economics 152, 103773

Dye, R.A., 1990. Mandatory versus Voluntary Disclosures: The Cases of Financial and Real Externalities. Accounting Review 65, 1-24

Ewens, M., Xiao, K., and Xu, T., 2024. Regulatory costs of being public: Evidence from bunching estimation. Journal of Financial Economics 153, 103775

Ferrell, A., 2007. Mandatory Disclosure and Stock Returns: Evidence from the Over-the-Counter Market. The Journal of Legal Studies 36, 213-251

Fishback, P.V. and Kantor, S.E., 1998. The Adoption of Workers' Compensation in the United States, 1900-1930. Journal of Law and Economics 41, 305-342

Glynn, A.N. and Quinn, K.M., 2009. An Introduction to the Augmented Inverse Propensity Weighted Estimator. Political Analysis 18, 36-56

Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. Journal of Econometrics 225, 254-277

Grossman, S.J. and Hart, O.D., 1980. Disclosure Laws and Takeover Bids. Journal of Finance 35, 323-334

Haeberle, K.S. and Henderson, M.T., 2018. A New Market-Based Approach to Securities Law. University of Chicago Law Review 85, 1313-1393

Hao, J., 2024. Disclosure regulation, cost of capital, and firm values. Journal of Accounting and Economics 77, 101605

Hilt, E., Jaremski, M., and Rahn, W., 2022. When Uncle Sam introduced Main Street to Wall Street: Liberty Bonds and the transformation of American finance. Journal of Financial Economics 145, 194-216

Honigsberg, C., 2019. Hedge Fund Regulation and Fund Governance: Evidence on the Effects of Mandatory Disclosure Rules. Journal of Accounting Research 57, 845-888

Honigsberg, C., Jackson, R.J.J., and Wong, Y.-T.F., 2015. Mandatory Disclosure and Individual Investors: Evidence from the JOBS Act. Washington University Law Review 93, 293-334

Houston, J.F., Lin, C., and Xie, W., 2018. Shareholder Protection and the Cost of Capital. The Journal of Law and Economics 61, 677-710

Jiang, X., Xin, B., and Xiong, Y., 2023. The Value of Mandatory Certification: A Real Effects Perspective. Journal of Accounting Research 61, 377-413

Kahn-Lang, A. and Lang, K., 2020. The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications. Journal of Business & Economic Statistics 38, 613-620

Kalmenovitz, J., 2023. Regulatory Intensity and Firm-Specific Exposure. The Review of Financial Studies 36, 3311-3347

Kripke, H., 1979. The SEC and Corporate Disclosure: Regulation in Search of a Purpose. Harcourt Brace Jovanovich, New York.

La Porta, R., Lopez-De-Silanes, F., and Shleifer, A., 2006. What Works in Securities Laws? Journal of Finance 61, 1-32

Lambert, R., Leuz, C., and Verrecchia, R.E., 2007. Accounting information, disclosure, and the cost of capital. Journal of Accounting Research 45, 385-420

Landis, J.M., 1959. Legislative History of the Securities Act of 1933. George Washington Law Review 28, 29-49

Leuz, C. and Wysocki, P.D., 2016. The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research. Journal of Accounting Research 54, 525-622

Loss, L. and Cowett, E.M., 1958. Blue sky law. Little Brown, Boston.

Macey, J.R. and Miller, G.P., 1991. Origin of the Blue Sky Laws. Texas Law Review 70, 347-398

Manning, B. and Hanks, J.J., Jr., 2013. Legal Capital. Foundation Press, St. Paul, Minnesota.

Mehrling, P., 2011. The New Lombard Street: How the Fed Became the Dealer of Last Resort. Princeton University Press, Princeton, NJ.

Mulvey, T., 1916. Blue Sky Law. Canadian Law Times 36, 37-45

Parrish, M.E., 1970. Securities regulation and the New Deal. Yale University Press, New Haven.

Pritchard, A.C., 2018. Corporate Governance, Capital Markets, and Securities Law. In: Gordon JN & Ringe W-G (eds.) The Oxford Handbook of Corporate Law and Governance. Oxford University Press, Oxford, pp. 1063-1083.
Rajan, R.G. and Ramcharan, R., 2011. Land and Credit: A Study of the Political Economy of Banking in the United States in the Early 20th Century. The Journal of Finance 66, 1895-1931
Sant'Anna, P.H.C. and Zhao, J., 2020. Doubly robust difference-in-differences estimators. Journal of Econometrics 219, 101-122
Shleifer, A. and Wolfenzon, D., 2002. Investor protection and equity markets. Journal of Financial Economics 66, 3-27
Sun, L. and Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. Journal of Econometrics 225, 175-199
Zhang, S., 2024. China's first-quarter IPOs plunge 65% as regulator's focus on listing quality saps pipeline. In: South China Morning Post, Hong Kong

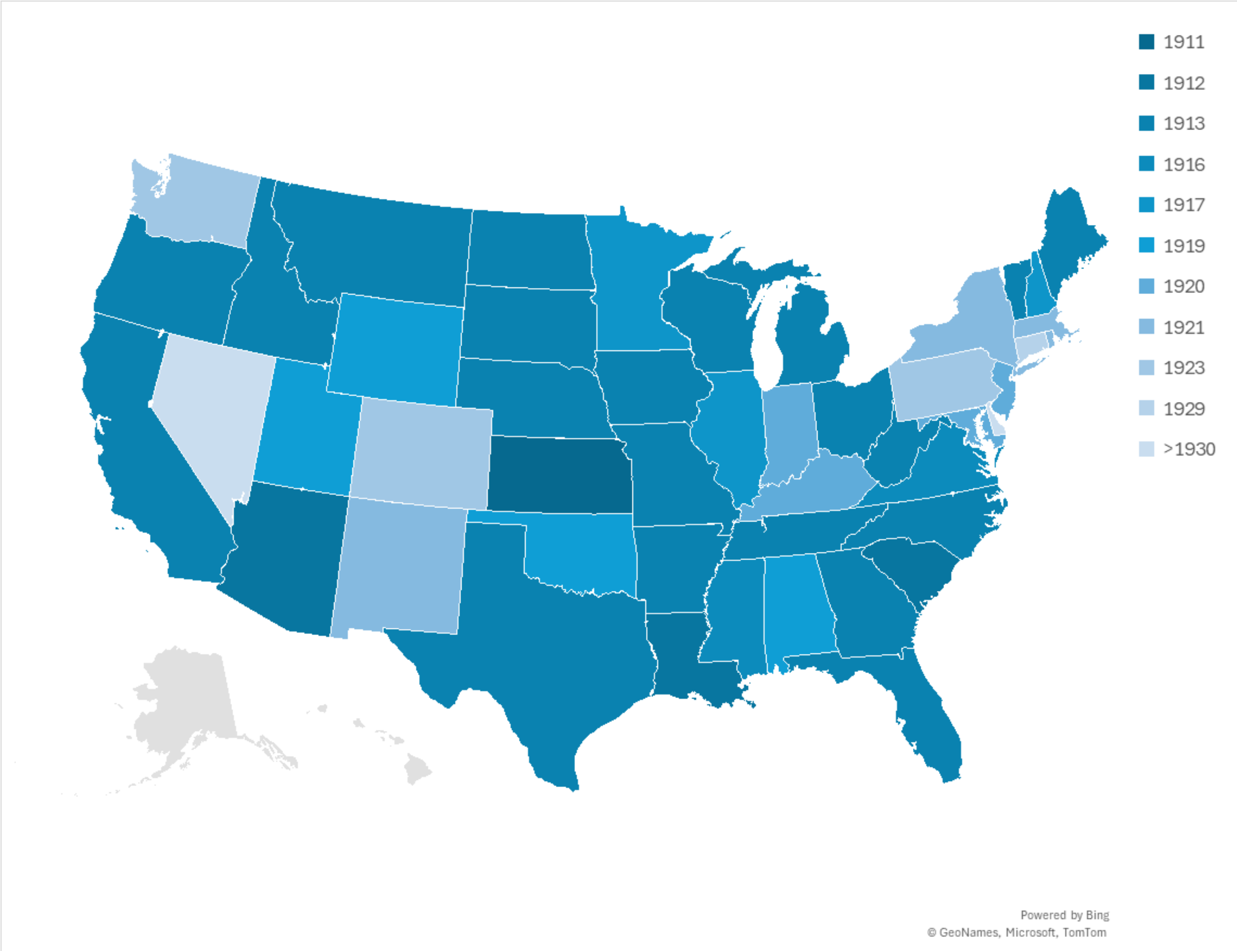

FIGURE 1

ADOPTION OF BLUE SKY LAWS

Notes: The figure shows the year of initial adoption of a BSL for each state, with earlier adopters in darker tones.

Source: Author's labeling using *HeinOnLine* legal database.

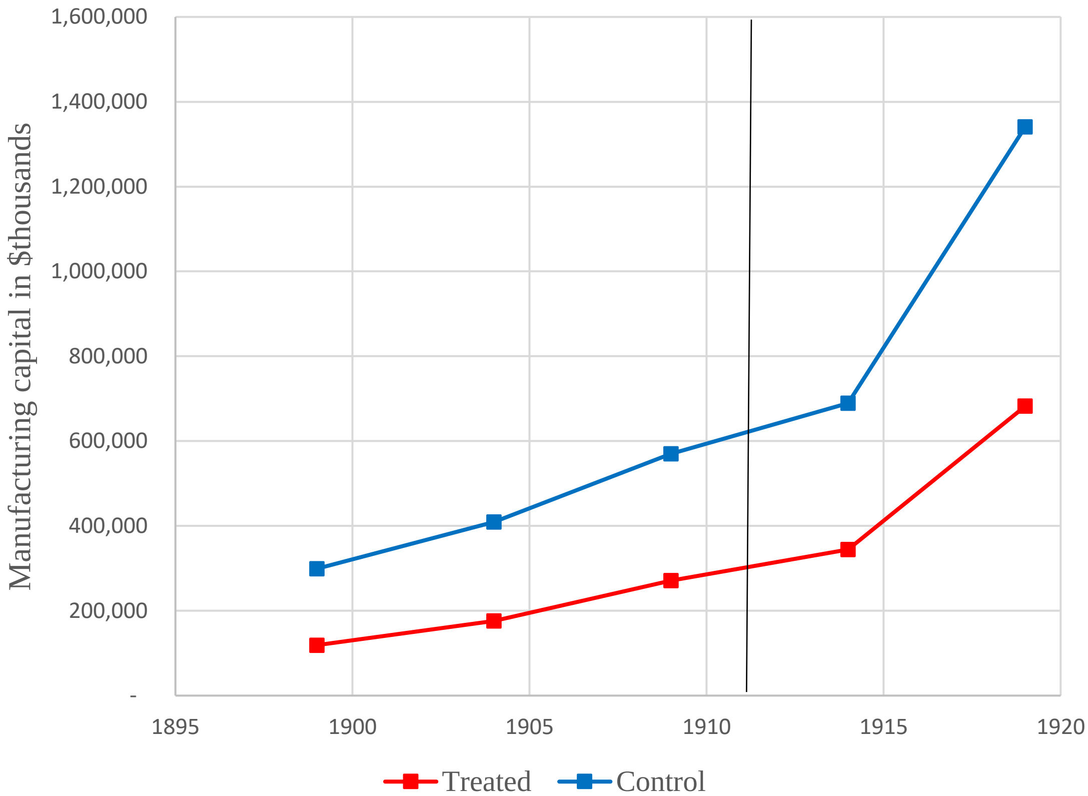

FIGURE 2

MANUFACTURING CAPITAL GROWTH BY TREATMENT STATUS

Notes: The figure plots the growth of manufacturing capital by treatment status, 1904-1919. Treated states are those that adopted a BSL before 1919; the remainder are control states. The horizontal line indicates 1911, the first year of BSL adoption.

Source: Author's calculation as described in text.

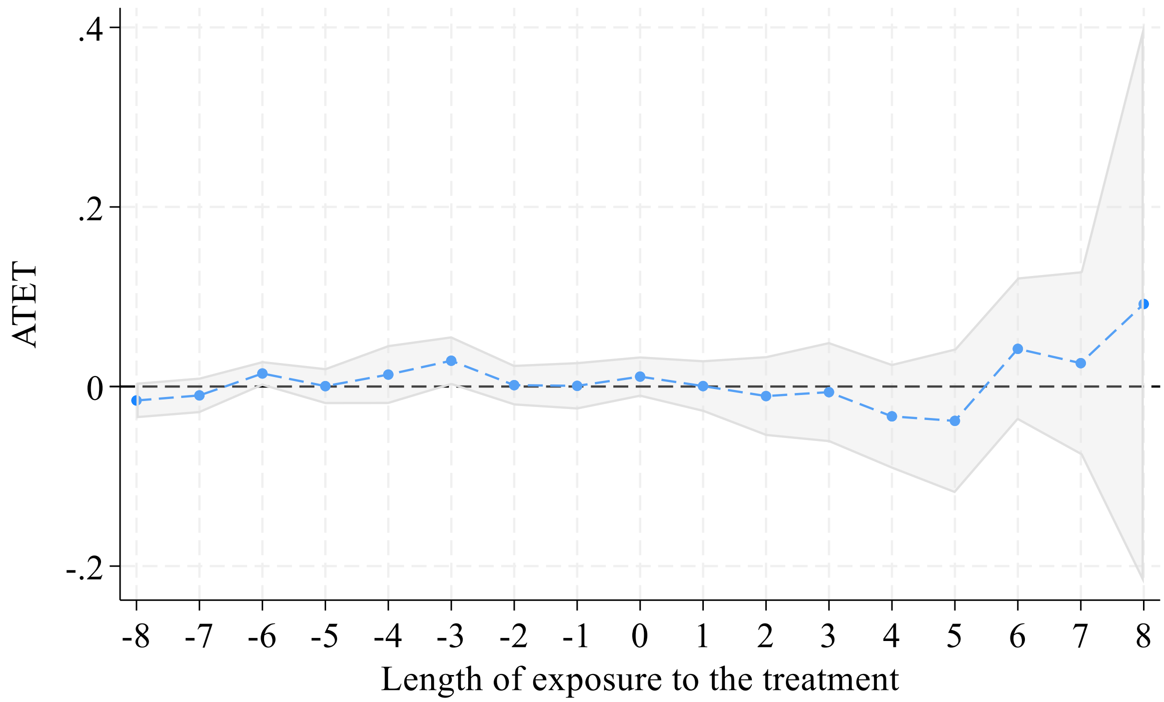

FIGURE 3

EVENT STUDY: BUSINESS ESTABLISHMENTS

Notes: The figure plots estimated treatment effects from the heterogeneous DiD model reported in Table 3, aggregated for all treated states and plotted by length of exposure to treatment, in years. The outcome measure is the log of business establishments. The shaded areas are 95% confidence intervals.

Source: Author's calculation as described in text.

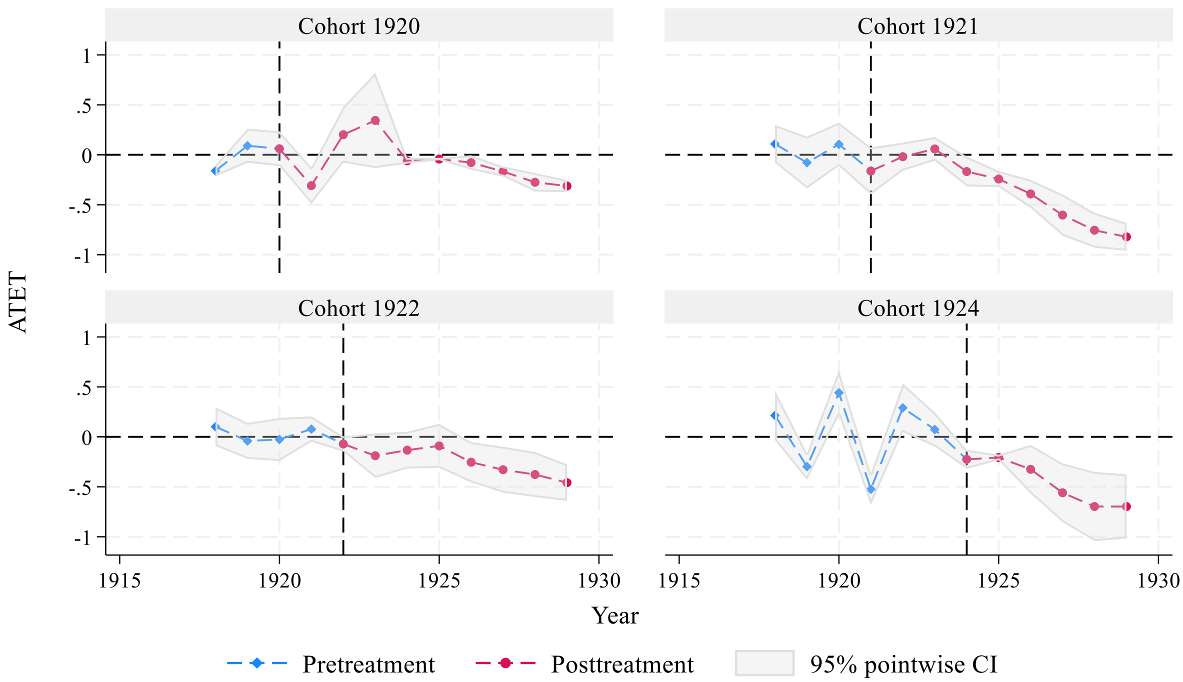

FIGURE 4

DIVIDEND INCOME AROUND THE TIME OF BSLS

Notes: The figure plots by year, for four cohorts of BSL adoption, the difference between the log of dividend income received by residents of the states treated in that cohort and in control states that had not yet adopted a BSL. The data are limited to 1917-1929.

Source: Author's calculation as described in text.

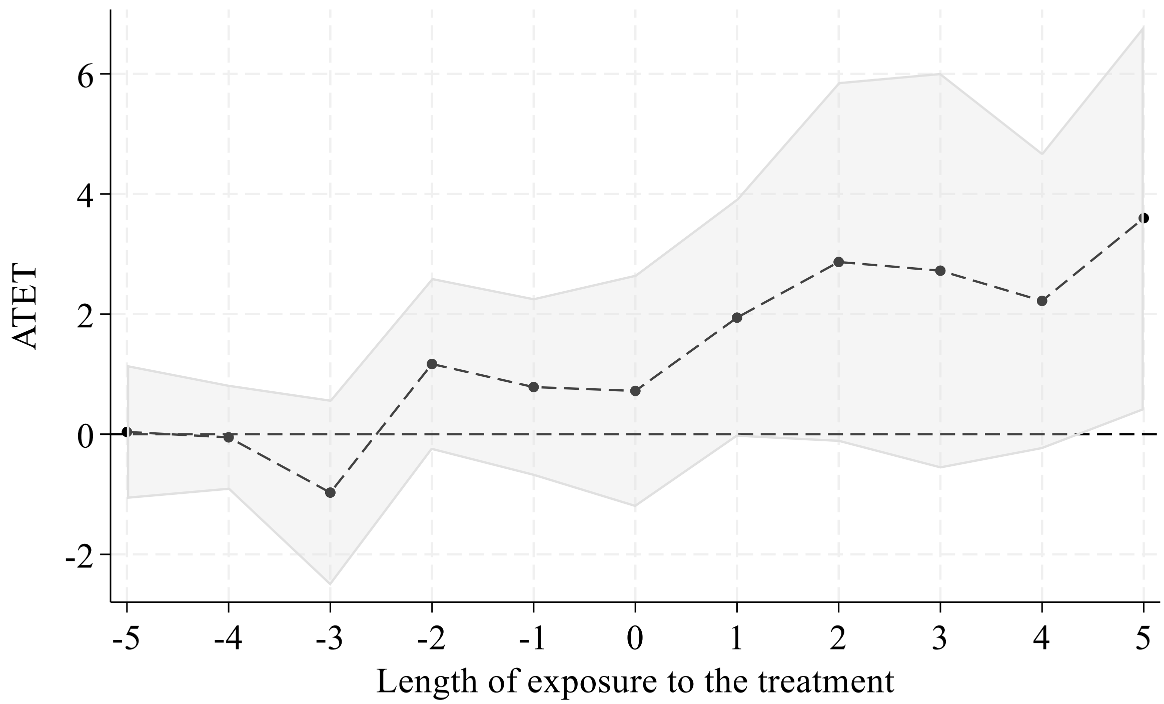

FIGURE 5

EVENT STUDY: COMMERCIAL FAILURES

Notes: The figure plots the difference between BSL and control states in commercial failures per 1,000 establishments for years [-5, +5] relative to treatment. The shaded grey areas are 95% confidence intervals.

Source: Author's calculation as described in text.

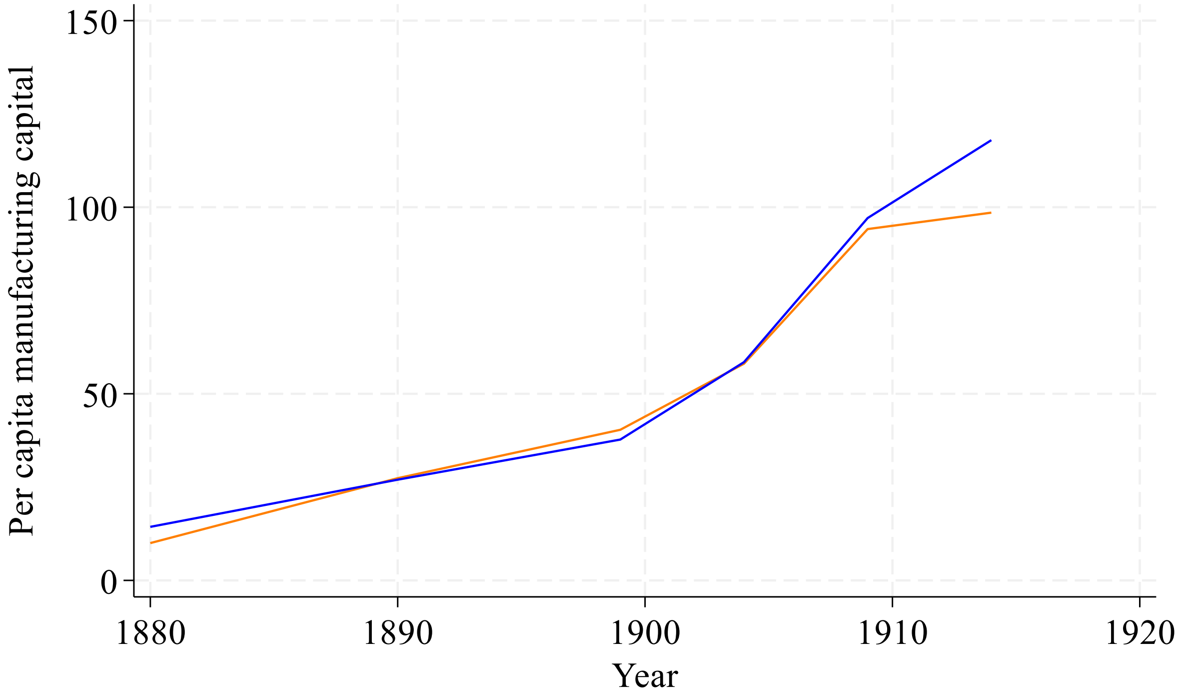

FIGURE 6

PER CAPITA MANUFACTURING CAPITAL IN KANSAS

AND SYNTHETIC KANSAS

Notes: The figure plots the path of manufacturing capital per 1,000 population for Kansas (red line) and synthetic Kansas (blue line), the latter consisting of a weighted average of Indiana (weight = 0.2), Minnesota (0.1), Mississippi (0.5) and Nevada (0.2).

Source: Author's calculation as described in text.

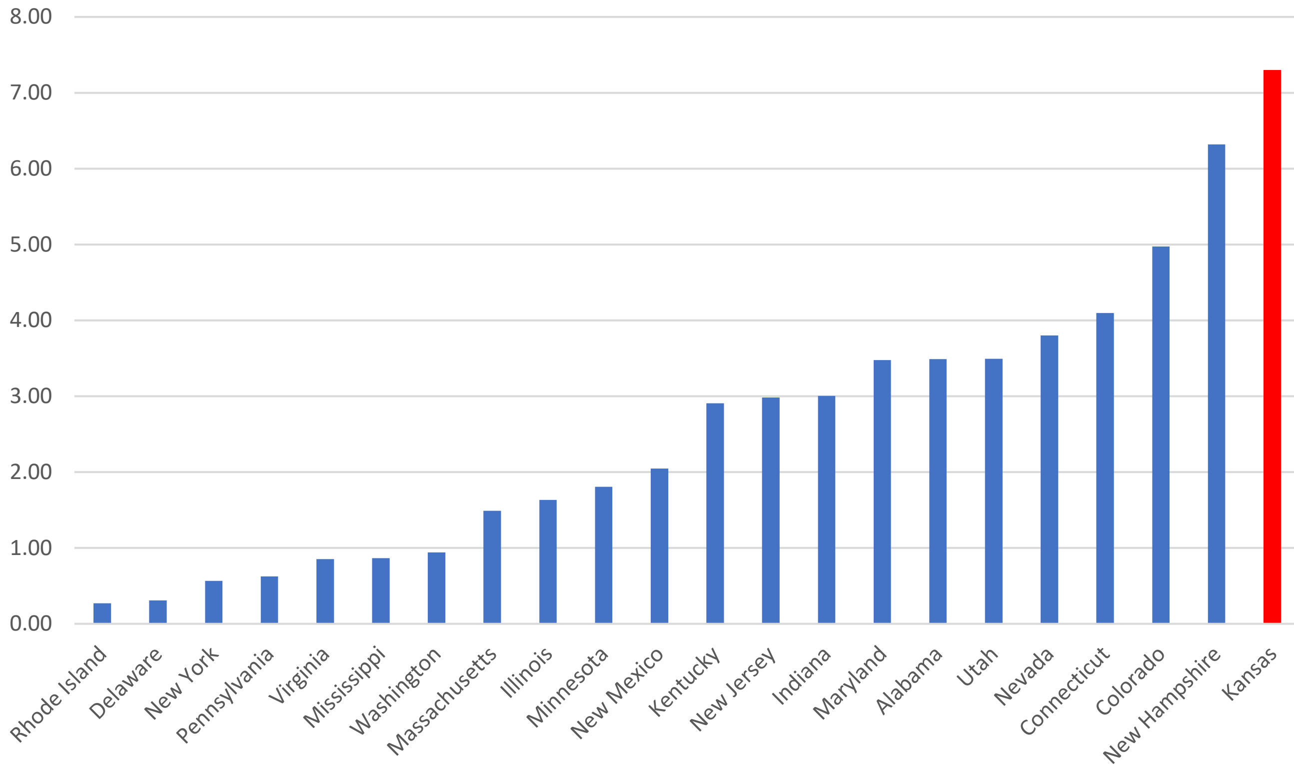

FIGURE 7

INFERENCE FOR SYNTHETIC CONTROL STUDY

Notes: The figure plots test statistics obtained from placebo studies in which the 1911 Kansas BSL is counterfactually assigned to control states. The test statistic is the ratio of the post-treatment prediction error to the pre-treatment root mean squared prediction error.

Source: Author's calculation as described in text.

TABLE 1

DESCRIPTIVE STATISTICS

	Full sample (48 states)	Treated states (N = 30)	Control States (N=18)
Year = 1909			
Population (thousands)	1,878	1,778	2,045
	(1,810)	(1,312)	(2,464)
Manufacturing capital ($ thousands)	383,286	271,231	570,044
	(609,653	(353,313)	(869,221)
Miles of railroad track per 100 square miles of land area	11.19	9.96	13.26
	(7.10)	(5.22)	(9.26)
Educational spending per school-aged population	16.90	14.90	20.21
	(10.72)	(8.73)	(13.00)
Coal production (thousands of tons)	6,187	4,507	8,986
	(16,919)	(10,649)	(24,181)
Progressive laws enacted (out of 9 possible)	2.81	2.80	2.83
	(1.38)	(1.40)	(1.38)
Average assets per bank ($ thousands)	889.65	528.88	1,490.94
	(1141.86)	(338.53)	(1672,35)
Commercial establishments	30,460	25,830	35,493
	(33,887)	(21,559)	(43,536)
Commercial failures per 1,000 establishments	8.95	9.35	8.52
	(4.86)	(6.06)	(3.17)
Year = 1917			
Dividends reported on taxpayer returns	39,991	21,821	61,812
	(84,780)	(28,510)	(116,610)

Notes: The table reports summary statistics for the state-level outcome measures and covariates, as well as population. Treated states are those that adopted a BSL before 1919; the remainder are control states. Values are means, with standard deviations in parentheses.

Source: Author's calculation as described in text.

TABLE 2

HETEROGENEOUS DIFFERENCE IN DIFFERENCES RESULTS FOR MANUFACTURING CAPITAL

A. Cohort-Year Treatment Effects		
	Estimated treatment effect	Standard Error
Early cohort		
1904	-0.197	0.150
1909	-0.153	0.103
1914	-0.179*	0.088
1919	-0.410†	0.217
Late cohort		
1904	-0.087	0.088
1909	-0.074	0.092
1914	-0.139†	0.073
1919	-0.119	0.092
B. Aggregate Treatment Effects		
ATET, Both Cohorts	-0.279*	0.135
ATET for Early Cohort	-0.295*	0.147
ATET for Late Cohort	-0.119	0.092
N (observations)	240	
N (states)	48	

Notes: The table shows estimated average treatment effects either by year (Panel A) or aggregated for the sample period (Panel B) for two different treatment cohorts: states adopting BSLs between 1911 and 1913 (early cohort) or between 1914 and 1919 (late cohort). The outcome variable is the log of manufacturing capital. Each covariate is lagged by one year. Standard errors are clustered by state. († p<.1, * p<.05, ** *p*<.01.)

Source: Author’s calculation as described in text.

TABLE 3

HETEROGENEOUS DIFFERENCE IN DIFFERENCES RESULTS FOR BUSINESS ESTABLISHMENTS

	Estimated treatment effect	Standard error
All treated cohorts aggregated	-0.001	0.021
1912 cohort (N=1)	-0.032	0.021
1913 cohort (N=3)	0.026	0.074
1914 cohort (N=21)	-0.007	0.027
1917 cohort (N=2)	-0.064	0.046
1918 cohort (N=3)	0.011	0.041
1920 cohort (N=4)	0.073**	0.013
1921 cohort (N=4)	0.032	0.028
Number of observations	851	
Number of states	48	

Notes: The outcome variable is the log of business establishments, measured by year from 1904-1922, inclusive. Control states are those that do not yet have a BSL in force in the year for which the treatment effect is estimated. († p<.1, * p<.05, ** *p*<.01.)

Source: Author’s calculation as described in text.

TABLE 4

HETEROGENEOUS DIFFERENCE IN DIFFERENCES RESULTS
FOR DIVIDENDS

	Estimated treatment effect	Standard Error
All treated cohorts aggregated	-0.243**	0.042
1920 cohort (N=4)	-0.063	0.047
1921 cohort (N=4)	-0.345**	0.045
1922 cohort (N=4)	-0.238**	0.078
1924 cohort (N=3)	-0.452**	0.103
Number of observations	234	
Number of states	18	

Notes: The outcome variable is the log of dividends received by the state's residents as reported for tax purposes for years 1917-1929. Covariates predicting the outcome measure are region by year fixed effects. Covariates predicting treatment are the index of progressive laws and average bank size (log of assets per bank), each lagged by one year. The control group for each cohort is the not-yet-treated states. Standard errors are clustered by state. († p<0.1, * p<.05; ** p<.01.)

Source: Author's calculation as described in text.

TABLE 5

EVENT STUDY RESULTS FOR BUSINESS FAILURES

Year relative to treatment	Estimated treatment effect	Standard error
-5	0.038	0.569
-4	-0.051	0.447
-3	-0.972	0.791
-2	1.170	0.731
-1	0.786	0.755
0	0.721	0.987
1	1.941†	1.012
2	2.868†	1.529
3	2.723	1.680
4	2.220†	1.260
5	3.599*	1.632
Five post-treatment years aggregated	2.633*	1.194
Number of observations	1,222	
Number of states	47	

Notes: The outcome variable is the number of business failures per 1,000 establishments for years 1904-1929. Estimated treatment effects are aggregated for all treated states by year relative to treatment. Covariates predicting the outcome variable are the Palmer drought severity index and the number of farm failures. Average bank size (assets divided by the number of banks) and the progressive laws index are used as predictors of treatment. Each covariate is lagged by one year. Standard errors are clustered by state. († p<.1, * p<.05.)

Source: Author’s calculation as described in text.

TABLE 6

SYNTHETIC KANSAS

A. Control Unit Weights

Control State	Weight
Indiana	0.2
Minnesota	0.1
Mississippi	0.5
Nevada	0.2

B. Predictor Balance

Variable	Kansas Value	Synthetic Kansas Value
Railroad miles per square mile of land area	9.41	7.30
Education expenditure per school-aged child	10.31	10.09
Per capita manufacturing capital in 1890	27.39	27.05
Per capita manufacturing capital in 1904	58.07	58.55
Root mean square prediction error	2.48	

Notes: Panel A shows the states that are selected in the synthetic controls model as synthetic Kansas and their relative weights. Panel B shows the pre-treatment values of the outcome and independent variables for Kansas and its synthetic twin.

Source: Author's calculation as described in text.