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THE CAUSAL EFFECT OF REGULATIONS ON ECONOMIC GROWTH

EVIDENCE FROM THE US STATES

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ABSTRACT

We exploit variation in stage age across US states to estimate the effect of regulatory accumulation on economic growth. Regulatory levels are measured using QuantGov's State RegData. The identification strategy is based on institutional sclerosis, the hypothesis that stable societies become stagnant over time as interest groups seek to impose restrictions on the economy, slowing its capacity to adapt to changing conditions. We find that a higher level of regulation's exogenous component significantly reduces GDP growth.

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DISCLAIMER

The Causal Effect of Regulations on Economic Growth

Evidence from the US States

1. Introduction

We typically think there is some tradeoff between regulation and efficiency. But until the past several years, the extent to which an economy is regulated, let alone regulation's effect on economic growth, has been difficult to estimate. The two main issues constraining research in the literature had been, first, the absence of data that could directly capture the size and variation of regulations at each level of government. In contrast, this study leverages datasets that are generated from textual analysis programs and machine learning algorithms that quantify regulations at degrees of directness and scale not achieved by research in the past. The second issue had been that very few of the studies in the literature had identified an exogenous variation in regulatory levels, limiting their ability to infer a causal relationship. To that end, we draw on the institutional sclerosis hypothesis to justify the use of a given US state's age as an instrumental variable. State age refers to the years since a US state's most recent admission to the union. (The most recent admission date is used for Southern states readmitted after the Civil War.) This allows us to obtain an exogenous component of regulatory accumulation.

We find that a higher level of regulation reduces the growth of GDP at the state level. Specifically, a 10 percent increase in regulatory restrictions is estimated to cause real GDP to fall by 0.37 percentage point—about one-seventh of a typical state's annual real GDP growth. This finding implies that moving across the interquartile range in restriction count (i.e., reducing regulatory restrictions by 42 percent) would increase aggregate real GDP by 1.54 percentage points.

Broadly defined, regulations are mandates that limit the domain of permissible actions of economic actors and that are ostensibly designed and implemented to achieve some specific outcomes. One view of regulation is that it is a policy vehicle for addressing market failures (such as externalities and information asymmetries) and maximizing social welfare. However, regulation can increase costs and subject potential entrants to barriers of entry. Tullock argued that in addition to deadweight loss, interventions in general entail compliance costs and invite attempts to capture transfers, which redirect factors from their more productive uses.[1] Similarly, Stigler hypothesized that regulation is captured by industry and that, by producers' monopolistic design, it is intended to restrict output.[2]

Olson argued that on top of individual regulations, the phenomenon of *regulatory accumulation* can exacerbate the aforementioned costs of regulation. When barriers to entry are ubiquitous, they can in general slow the rate at which resources are reallocated to more profitable sectors that spring up in response to technological change.[3] Regulatory complexity increases the size of government required to enforce said rules, encourages allocation of legal resources to discover loopholes, creates specialists who lobby against simplification, and spawns further regulations.[4] On a similar note, regulatory accumulation increases the likelihood of contradiction

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[1] Gordon Tullock, "Welfare Costs of Tariffs, Monopolies, and Theft," *Western Economic Journal* 5, no. 3 (June 1967): 225–26.

[2] George Stigler, "The Theory of Economic Regulation," *Bell Journal of Economics and Management Science* 2, no. 1 (Spring 1971): 3–21.

[3] Mancur Olson, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities* (New Haven: Yale University Press, [1982], reprint edition 1984), 65–68.

[4] Olson, *Rise and Decline of Nations*, 73–74.

(or what Steiner would term "incompossibility"), which can lead to indeterminate evaluations of the legality of actions. Ambiguous laws require judicial interpretation, in turn creating rules that are more ad hoc and potentially more arbitrary.[5] We return to Olson's hypothesis of institutional sclerosis in section 3.1, where we discuss its use as an instrumental variable.

Dawson and Seater made one of the first attempts to directly measure the quantity of regulations.[6] Before Dawson and Seater, most studies resorted to using indices of regulatory severity (either self-constructed or constructed by organizations such as the Organisation for Economic Co-operation and Development, or OECD),[7] which can limit the scope of regulation evaluated (to only those such as licensing requirements, product safety requirements, or employee health and safety) or the number of industries considered, in addition to introducing measurement errors. Dawson and Seater captured the growth of the *Code of Federal Regulations* by using page counts, creating a single time series of regulatory accumulation for federal regulations. Running simulations with an endogenous growth model, the authors estimated that if the pages of regulations had been unchanged since 1949, the economy would have grown 2.2 percentage points more annually—or an increase of $38.8 trillion to GDP by 2011.[8]

In one of the first applications of RegData, Bailey and Thomas investigated the effect of regulation as a potential "cost" to the economy by examining three metrics of entrepreneurship: firm births, firm deaths, and new hires.[9] (RegData will be described in more detail in section 2.) The study also examined whether varying levels of regulation affect these entrepreneurship outcomes differently for small firms than for large firms. Overall, they found a small but statistically significant negative relationship between regulation and entrepreneurship, with some of the effects more pronounced for small firms compared to large firms.

Coffey, McLaughlin, and Peretto, meanwhile, built upon the initial Dawson and Seater approach. Using industry-specific regulatory data from RegData, Coffey, McLaughlin, and Peretto specified an endogenous growth model in which (1) growth depends on lagged knowledge investment and its interaction with regulation, and (2) knowledge investment depends on past growth and regulation.[10] Exploiting variation in the level of regulation across industries and over time, they found that the economy would have grown 0.8 percentage points more annually if federal regulation had remained at 1980 levels—or a $4 trillion increase to GDP by 2012.[11]

More recently, Coffey and McLaughlin studied the case of regulatory budgeting in British Columbia, Canada, which reduced its count of regulations by just over one-third in three years.[12] The authors found that a 10 percent increase in regulatory stringency (i.e., a restriction count weighted by industry relevance) is associated with a 0.25 percentage point decrease in GDP per

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[5] Hillel Steiner, *An Essay on Rights* (Oxford: Blackwell, 1994), 81–85.

[6] John W. Dawson and John J. Seater, "Federal Regulation and Aggregate Economic Growth," *Journal of Economic Growth* 18, no. 2 (June 2013): 137–77.

[7] For example, see Norman V. Loayza, Ana María Oviedo, and Luis Servén, "Regulation and Macroeconomic Performance" (Policy Working Paper 3469, World Bank, Washington, DC, September 2004).

[8] Dawson and Seater, "Federal Regulation and Aggregate Economic Growth," 160.

[9] James B. Bailey and Diana W. Thomas, "Regulating away Competition: The Effect of Regulation on Entrepreneurship and Employment," *Journal of Regulatory Economics* 52, no. 3 (2017): 237–54.

[10] Bentley Coffey, Patrick A. McLaughlin, and Pietro Peretto, "The Cumulative Cost of Regulations," *Review of Economic Dynamics* 38 (2020): 1–21.

[11] Coffey et al., "Cumulative Cost of Regulations," 14–15.

[12] Bentley Coffey and Patrick A. McLaughlin, "Regulation and Economic Growth: Evidence from British Columbia's Experiment in Regulatory Budgeting" (Mercatus Working Paper, Mercatus Center at George Mason University, May 2021).

capita.[13] There was additional causal evidence from a difference-in-differences synthetic control setup, which found that the reform (of reducing regulations by one-third) increased growth by 1.4 percentage points.[14] However, the latter is estimated with a difference-in-differences setup that, although it did exploit a natural experiment and a synthetic control, ultimately relied on discrete rather than continuous variation in levels of regulation across province and time and the assumptions required of difference-in-differences estimation (e.g., parallel trends and no confounding events). This illustrates the difficulty of designing a study in which the shift in regulatory stringency (1) is exogenous and (2) can be measured with a continuous measure of regulation like RegData. To this end, we introduce the concept of institutional sclerosis, which we argue provides a source of exogenous variation in regulation.

Coffey and McLaughlin make important points not only about the potential growth effects of regulatory accumulation or red tape reduction, but also about the possibility that regulations do not have to correlate with population or the amount of economic activity in a given jurisdiction.[15] Other research has demonstrated a fairly robust correlation between state population and the quantity of regulation,[16] and between industry size and the quantity of federal and state regulation.[17] Coffey and McLaughlin as well as Jones and McLaughlin present the counterargument that such correlations are not an obligatory byproduct of economic growth but instead a result of inattention to regulatory accumulation by policymakers.[18]

The paper proceeds as follows. Section 2 describes the datasets on which this study relies. In section 3, we describe the main model being estimated and justify the use of different variables. The results are shown in section 4. Section 5 covers various robustness tests. Section 6 concludes.

2. Data

This study combines several datasets. Table 1 reports summary statistics. First, economic outcomes by state are pulled from the US Bureau of Economic Analysis's National Income and Product Accounts (BEA-NIPA).[19] Our main outcome of interest is a given state's chained real GDP.

Second, a measure of state-level regulations is provided by QuantGov's State RegData 2023.[20] RegData measures the *count* of *restrictions* in each state's regulatory codes *and* statutes, at the document level. Not every line of regulation constitutes a restriction. Instead, each occurrence of one of five specific restrictive phrases—namely, "shall," "must," "may not," "required," and "prohibited"—counts as one restriction. We aggregate restrictions at the state level, creating a sample of 77 observations for our main specification. This provides us with a snapshot of most states' restrictions within each year, from 2021 to 2023.

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[13] Coffey and McLaughlin, "Regulation and Economic Growth," 36.

[14] Coffey and McLaughlin, "Regulation and Economic Growth," 35.

[15] Coffey and McLaughlin, "Regulation and Economic Growth."

[16] James Bailey, James Broughel, and Patrick A. McLaughlin, "Larger Polities Are More Regulated," *Journal of Public Finance and Public Choice* 36 (2021): 233–43.

[17] Marc T. Law and Patrick A. McLaughlin, "Industry Size and Regulation: Evidence from US States," *Public Choice* 192 (2022): 1–27.

[18] Coffey and McLaughlin, "Regulation and Economic Growth"; Laura Jones and Patrick A. McLaughlin, "Measurement Options for Regulatory Budgeting," *Harvard Journal of Law and Public Policy Per Curiam* 1, no. 25 (2022): 43–60.

[19] US Bureau of Economic Analysis, "Table: SQGDP9 Real GDP in Chained Dollars," BEA Data API, accessed September 2, 2024.

[20] Patrick A. McLaughlin et al., "State RegData 2023" (dataset), QuantGov, Mercatus Center at George Mason University, 2023.

TABLE 1. Summary statistics

Characteristic	*N* = 77
Restriction Count, $t - 2$	
Mean (SD)	276,130 (145,830)
(Minimum, IQR, maximum)	(98,015, 184,228, 315,669, 889,795)
Chained real GDP growth, 1-year	
Mean (SD)	0.024 (0.015)
(Minimum, IQR, maximum)	(−0.016, 0.015, 0.032, 0.054)
State age	
Mean (SD)	172 (44)
(Minimum, IQR, maximum)	(64, 134, 207, 236)
Population at admission (million)	
Mean (SD)	0.11 (0.15)
(Minimum, IQR, maximum)	(0.00, 0.01, 0.14, 0.69)
State area at admission (million km^2)	
Mean (SD)	0.10 (0.10)
(Minimum, IQR, maximum)	(0.00, 0.04, 0.15, 0.57)
Date of first constitution	
Mean (SD)	1,835 (50)
(Minimum, IQR, maximum)	(1,776, 1,777, 1,878, 1,959)
Euclidean distance from Washington, DC (latitude-longitude)	
Mean (SD)	19 (16)
(Minimum, IQR, maximum)	(0, 6, 28, 81)

Note: IQR = interquartile range; km = kilometers; SD = standard deviation.

Finally, the effective admission date of a state to the United States, which we use to compute state age, is provided by the US Census Bureau's "Historical Statistics of the United States."[21] This dataset also contains a state's population and geographical area near or at the time of admission—which allows us to control for factors that may affect the independence of state age as an instrument (see section 3.1).

3. Model

The main findings of this paper are derived using two-stage least squares (TSLS) estimation, applied to the following two equations that characterize a given state i at time t:

[21] Susan B. Carter et al., "Historical Statistics of the United States: Millennial Edition" (dataset), Cambridge University Press, 2006, originally published by the US Census Bureau, https://hsus.cambridge.org/HSUSWeb/HSUSEntryServlet.

$$\ln\left(\frac{y_{it}}{y_{i,t-1}}\right) = \ln Reg_{i,t-2}\beta + \vec{w}_i\vec{\gamma} + \epsilon_{it}, \quad (1)$$

$$\ln Reg_{i,t-2} = Age_i\delta + \vec{w}_i\vec{\eta} + \xi_{it}. \quad (2)$$

In equation 1, $\ln y_i$ is the natural logarithm of chained real GDP of a given state i. Since we are estimating regulatory accumulation's effect on its production, we are interested in moments of output change. Specifically, we estimate the first differences of log output, which gives the one-period continuous compounding growth rate of GDP. We are also more interested in the growth of GDP than in that of per capita GDP. This is because insofar as regulation affects incomes, it can do so by two mechanisms: by reducing efficiency of existing residents' economic activity or by reducing net immigration to a state. Aggregate GDP captures both dynamics. All Greek letters represent parameters to be estimated.

Reg_{it} refers to the restriction count of a given state i in year t. The count of regulatory restrictions vary widely across states, with a mean of 276,130 and values ranging between 98,015 and 889,795.[22] Figure 1 shows that the distribution of restriction count is skewed to the right. For this reason, we use a log-transformation of restriction count as the main treatment variable. This will help enforce homoskedasticity when we are estimating equation 2. Additionally, by emphasizing variation at lower values, log-transformation has the desirable property of implementing the assumption that regulatory accumulation matters more at the lower levels. This is the idea that moving from 400,000 to 500,000 restrictions may have far less of an effect than from 100,000 to 200,000. We test this assumption later.

In our main setup, we regress GDP growth from $t-1$ to t on restriction count in $t-2$.[23] The lag is meant to reflect that regulatory accumulation, insofar as it has any effect, requires time to permeate into economic activity. Later, we relax this assumption and allow regulations to have a more instantaneous effect.

We use $\vec{w}_i$ as a vector of controls of a given state i. The vector includes (a) a state's population recorded on the decennial census subsequent to statehood and (b) the geographical area of a state at admission, both of which could determine how early a given territory sought statehood. Our data constitute an unbalanced panel, including one year of observation for 19 states and two years of observations for 29 states.

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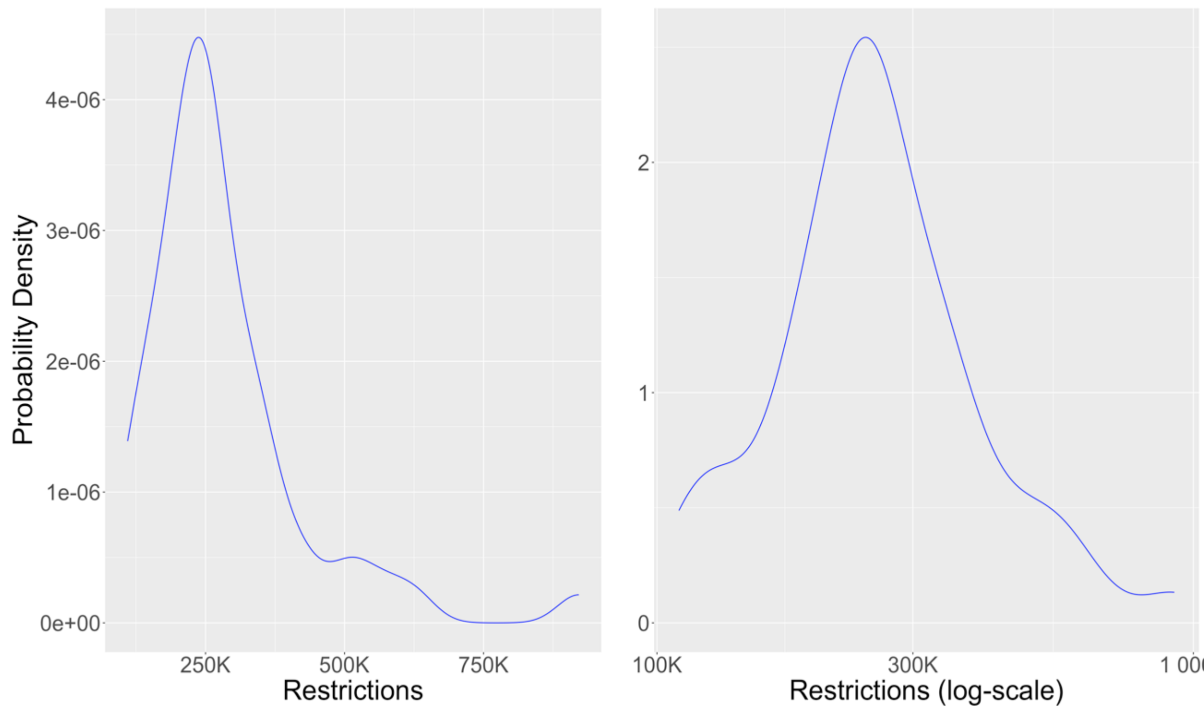
[22] Note that, as mentioned in the previous section, we are using the sum of regulatory restrictions contained in both state administrative codes and state statutes.

[23] In alternative specifications, we explored using personal income as the dependent variable and reached qualitatively similar results. However, those analyses did not correct for regional purchasing parities, which other researchers show to have dramatic effect on the bottom 10 percent of income distributions across regions. For more, see Vincent Geloso and Youcef Msaid, "Adjusting Inequalities for Regional Price Parities: Importance and Implications," *Journal of Regional Analysis and Policy* 48, no. 4 (2018): 1–8, and Justin T. Callais and Jamie Bologna Pavlik, "Does Economic Freedom Lighten the Blow? Evidence from the Great Recession in the United States," *Economics of Governance* 24 (2023): 357–98. Since our main focus is GDP growth, we leave exploration of income effects to future research.

FIGURE 1. Density of restriction count and log of restriction count



3.1 State age as an instrument

Estimating equation 1 alone would lead to bias. For example, a classic case of simultaneity would be a state pursuing regulatory reform when facing low growth. With a valid instrument that is uncorrelated with ϵ_i, we can estimate equation 1 using TSLS. Age_i is the years since a state's admission to the United States, with the exception of Southern states, for which the variable is defined as years since their readmission to the United States in 1868, as the Civil War (in addition to reconstruction) likely disrupted or inhibited the development of interest groups.[24] We use this as the instrument for obtaining an exogenous component of Reg_i. Figure 2 shows the distribution of state age.

Olson offered the institutional sclerosis hypothesis to explain why affluent societies become stagnant with time. The main components of his hypothesis are as follows:[25]

1. Stable societies with unchanged boundaries tend to accumulate more collusions and organizations for collective action over time.
2. On balance, special-interest organizations and collusions reduce efficiency and aggregate income in the societies in which they operate and make political life more divisive.
3. Distributional coalitions slow down a society's capacity to adopt new technologies and to reallocate resources in response to changing conditions, and thereby they reduce the rate of economic growth.

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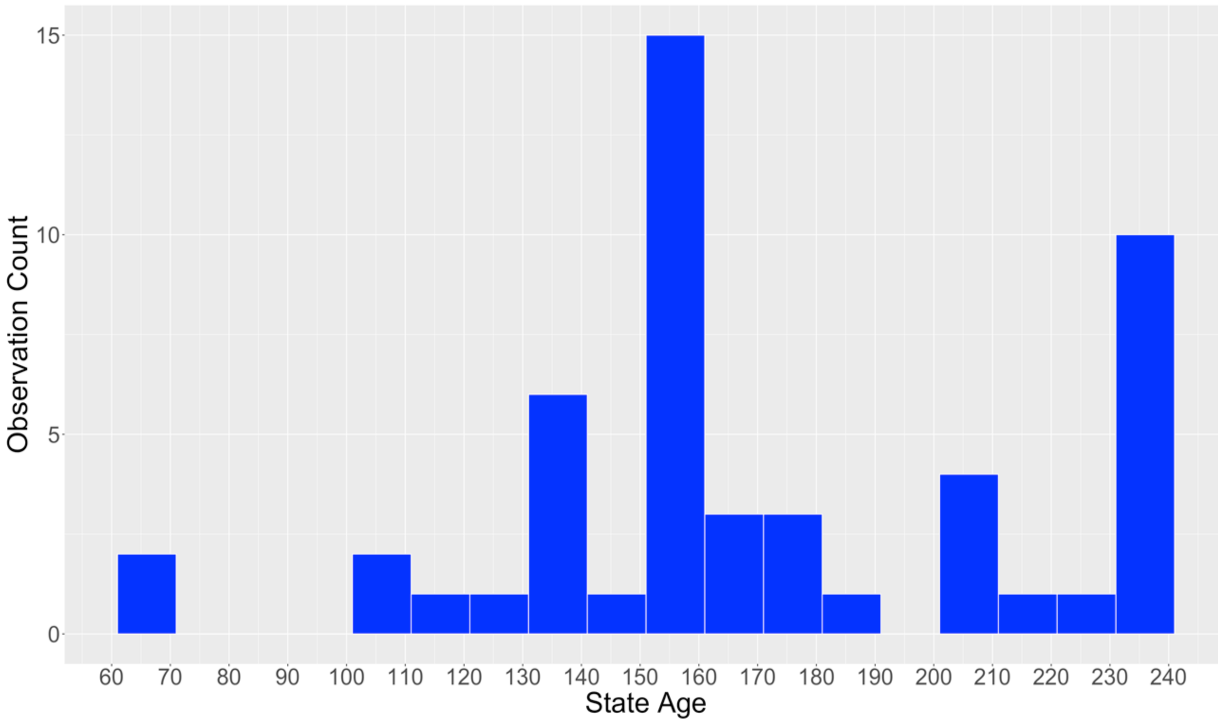
[24] Olson, *Rise and Decline of Nations*, 98.

[25] Olson, *Rise and Decline of Nations*, 76.

FIGURE 2. Histogram of state age, years



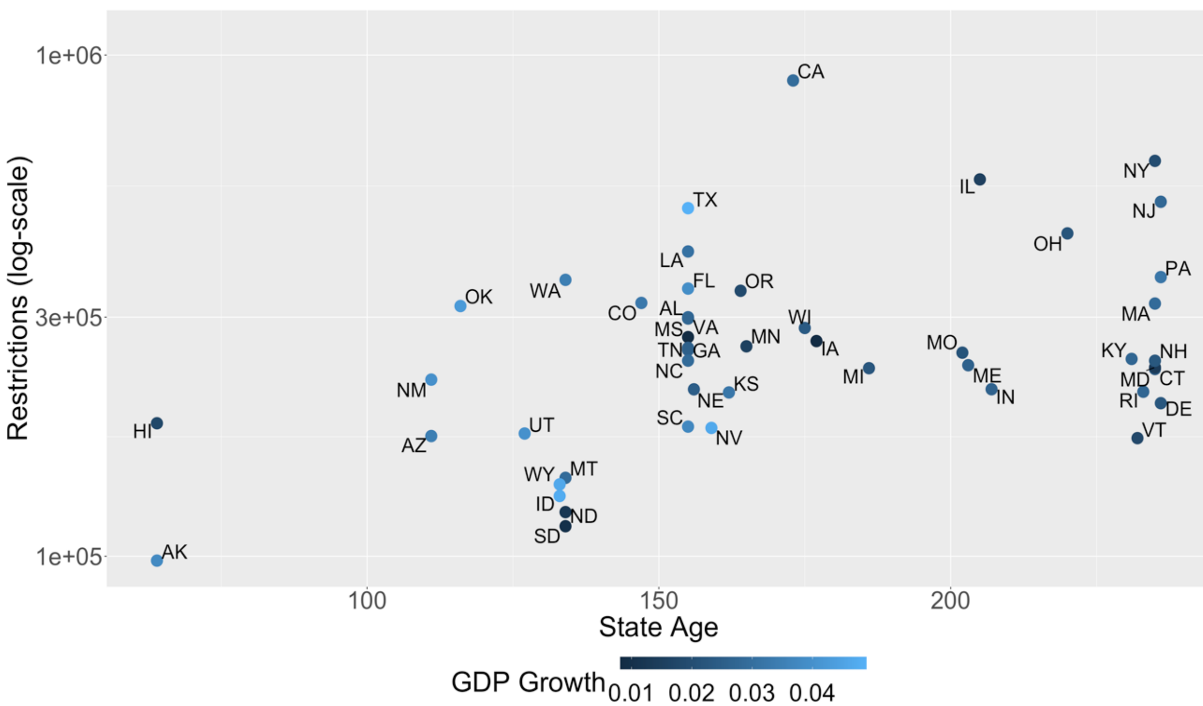
We briefly elaborate on the first two components. The first rests on the notion that bargaining costs are high. Specifically, the organizing required to create special-interest groups requires preconditions such as leadership, risk appetite, previously established social networks, or a combination of these for bargaining costs to be overcome, and such preconditions are highly congruent with a stable environment.[26] The second component illustrates a collective action problem: Suppose an interest group constituted some small share s of total income. If faced with whether to effect a transfer R at the cost of reducing total income by C, the group will find it rational to proceed as long as $R > sC$. Thus, even if C exceeds R by a large multiple, each given interest group will still find it optimal to lobby for regulations that limit entrants or to organize a cartel.[27]

[26] Olson, *Rise and Decline of Nations*, 43–44.

[27] Olson, *Rise and Decline of Nations*, 49.

The significance and magnitude of Olson's hypothesis have been extensively tested, starting with evidence that Olson compiled with Kwang Choi. Olson and Choi found that a state's founding year (i.e., the additive inverse of state age) is significantly predictive of declines in both aggregate and per capita income growth at the US state level between 1965 and 1978 and between 1946 and 1978. This result is particularly noteworthy, given that most US states were founded at least a century before the period for which income was measured.[28] Furthermore, state age is positively and significantly correlated with one measure of interest group accumulation, specifically union membership as a percentage of employees (nonagricultural).[29] Similarly, we find that state age is also a significant predictor of log-transformed restriction count ($p = 0.0027$), as figure 3 illustrates. The color of each observation is mapped to economic growth from $t - 1$ to t (whereas restriction count is that from $t - 2$; lighter indicates higher growth). It is immediately observable that states admitted more recently, in addition to having fewer restrictions, tend to exhibit higher growth today.

FIGURE 3. Log-scale restrictions versus state age with color gradient representing present GDP growth



[28] Olson, *Rise and Decline of Nations*, 104–6, 114.

[29] Olson, *Rise and Decline of Nations*, 107–8.

Subsequent scholars have also found evidence to reinforce the process of institutional sclerosis posited here. In a meta-analysis, Heckelman found that subsequent researchers generally concurred with Olson's findings. The proportion of statistical studies (n = 28) that offer support, mixed support, and no support to institutional sclerosis, respectively, are 57 percent, 18 percent, and 25 percent[30]—though it should be cautioned that the sample of studies surveyed all provide merely correlational evidence. Among studies that focused on US states and the role of interest groups, Vedder and Gallaway found that state age and union membership are significantly and negatively correlated with per capita income growth.[31] Crain and Lee estimated a significantly negative relationship between the same outcome and business associations' revenue as a share of income.[32]

Given the ostensible relevance of a state's age to explaining its economic growth, we propose using state age as an instrumental variable. We claim that it is implausible for state age, other than through the channel of institutional sclerosis, to affect present economic growth. We offer evidence on this claim in two ways.

First, we control for potential determinants of statehood, and we will later show that these covariates are not significant in either the first or second stage. One covariate is a state's population recorded on the decennial census subsequent to statehood. This is because early population could have been driven by early industrial activity that has some persistent effect on current growth. In addition, territories seeking statehood had to meet population requirements (though they may not necessarily be set at a level that is binding).

The other covariate we control for is the geographical area of a state at admission. The concern there is that longer travel distances within a state can increase the cost to organize, which in turn can affect our endogenous variable.

Second, we overidentify the TSLS model, so as to conduct a J-test for instrument exogeneity. Relying on the same hypothesis, we identify year of initial constitution and Euclidean distance to Washington, DC, as additional instruments.[33] As we will show as well, the hypothesis that instruments are exogenous cannot be rejected.

4. Results

The TSLS results are reported in table 2. Column 1 reports the results from a simple ordinary least squares (OLS) regression of the outcome on log of restriction count. Consistent with the motivation for our identification strategy, log of restriction count as a predictor is not statistically significant.

Moving onto the TSLS first-stage results in column 2, we see that state age is significant at the 1 percent level as a predictor for log of restriction count—which offers support to state age being

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[30] Jac C. Heckelman, "Explaining the Rain: *The Rise and Decline of Nations* after 25 Years," *Southern Economic Journal*, 74, no. 1 (2007): 26, 29.

[31] Richard Vedder and Lowell Gallaway, "Rent-Seeking, Distributional Coalitions, Taxes, Relative Prices and Economic Growth," *Public Choice* 51, no. 1 (1986): 96.

[32] W. Mark Crain and Katherine J. Lee, "Economic Growth Regressions for the American States: A Sensitivity Analysis," *Economic Inquiry* 37, no. 2 (April 1999): 253. Because of its problematic specifications of interest group power, we omit another study that examined US states and interest groups without generating supporting evidence: Virginia Gray and David Lowery, "Interest Group Politics and Economic Growth in the U.S. States," *American Political Science Review* 82, no. 1 (1988): 109–31.

[33] Julia G. Clouse, "Converting the Texts of the U.S. State Constitutions into Quantifiable Data: A Text Analytics Project" (dissertation, George Mason University, 2019); US Census Bureau, "Geographic Areas Reference Files: 2010 Census State Area" (dataset), 2010.

a relevant instrument. In line with expectations, older states experience higher levels of restrictions. Neither population around the time of admission nor geographical area have significance—assuaging concerns about the endogeneity of state age.

The second-stage results—column 3—are encouraging as well. The exogenous component of treatment is statistically significant at the 5 percent level. A higher level of regulation reduces real GDP growth. The magnitude of the coefficient is 19 times larger than that in column 1, consistent with our suspicion that a simple OLS estimation captures upward bias. The estimate from column 3 implies that a 10 percent increase in restrictions will change real GDP by $\beta \cdot \frac{dReg}{Reg} = -0.37$ percentage point. This is rather significant as states' GDP on average grew 2.39 percent, which would make our estimate about one-seventh of yearly growth. For an alternative interpretation: moving across the interquartile range in restriction count (i.e., reducing restrictions by 41.64 percent) would increase real GDP by 1.54 percentage points.

One concern may be that, at first sight, the instrument appears to be weak, as the *F*-statistic is below 10—see column 2. This raises concerns about the instrument's relevance. To address this, we run the Anderson-Rubin test for TSLS models, which is designed to perform inference on the treatment's coefficient in the presence of a weak instrument. The test statistic is statistically different from zero ($p = 0.0011$). The 95 percent confidence interval for the estimated coefficient is [−0.1167, −0.0131]. This puts even the weaker end of the interval below zero, confirming our exogenous component of regulation as a significant and relevant predictor.

TABLE 2. Estimating GDP growth on restrictions, OLS versus TSLS with state age

	OLS	1st stage	2nd stage
ln(Restriction count)	−0.0019		−0.0370*
	(0.0040)		(0.0168)
State age		0.0038**	
		(0.0012)	
Population at admission	0.0093	0.3871	0.0233
	(0.0123)	(0.3378)	(0.0188)
Area size	0.0051	0.2627	−0.0137
	(0.0194)	(0.5910)	(0.0291)
Intercept	0.0462	11.6983***	0.4823*
	(0.0496)	(0.2520)	(0.2085)
R^2	0.01	0.15	
F-statistic	0.24	4.46	
Number of observations	77	77	77

Note: OLS = ordinary least squares; TSLS = two-stage least squares.
***$p <$ 0.1 percent; **$p <$ 1 percent; *$p <$ 5 percent significance.

5. Robustness

5.1 Overidentification

To offer evidence on instrument exogeneity, we now conduct a J-test by overidentifying the first-stage equation—that is, by including more instruments than endogenous variables. In an overidentified model, not all instruments can be perfectly correlated with the endogenous variables (and thus the second-stage residuals). This in turn allows us to test whether second-stage residuals are correlated with our instruments.

We turn to two additional instruments. The first is the year in which a given state's first constitution was ratified (or "year of initial constitution"), collected by Clouse.[34] The reasoning behind using year of initial constitution is similar to that of state age: a constitution provides a framework for stability that makes organizing, capture, and sclerosis possible. In fact, 27 states saw the establishment of a constitution prior to statehood. The series is therefore largely independent from state age (this is particularly true for Southern states, which by our definition are much younger).

The second is the Euclidean distance between a state's coordinates and that of Washington, DC, in the latitude-longitude space. This measure gives us an approximation of a state's distance from the capital. The two key ideas here are that the interaction between where a state ends up being located and Washington's location is as good as random, but that once both are established, nearness to Washington implies more organizing activity that in turn promotes statehood and sclerosis down the road.

Table 3 shows the results. The J-test is implemented by running the second-stage residuals (from an overidentified TSLS estimation) onto the same set of instruments and control variables. Note that the null hypothesis is that the instruments are jointly exogenous. Thus, a significant J-statistic indicates that the instrumental variables (IVs) are correlated with the estimated error terms. Column 1 uses both state age and year of initial constitution as instruments, while column 2 uses all three variables. Under both specifications, joint exogeneity cannot be rejected. While the J-statistic in the latter case does increase in significance, the null still cannot be rejected at the 10 percent level. This suggests the main instrument is in fact exogenous.

[34] Clouse, "Converting the Texts of the U.S. State Constitutions."

TABLE 3. Overidentifying restrictions test (J-test)

	Two IVs	Three IVs
State age	−0.0000	−0.0000
	(0.0001)	(0.0001)
Year of initial constitution	−0.0000	−0.0001
	(0.0001)	(0.0001)
Euclidean distance from Washington, DC		0.0003
		(0.0003)
Intercept	0.0905	0.2017
	(0.2128)	(0.2136)
J-statistic	0.1846	2.1084
P-value	0.6674 (df = 1)	0.1465 (df = 2)
Controls	Yes	Yes
R^2	0.0026	0.0288
Number of observations	77	77

Note: df = degree of freedom; IV = instrumental variable.

5.2 Different lag specifications

In our main model, we lag restriction count by two periods. We now test other lag specifications. Table 4 shows the TSLS second stage results when different lags are used. Three results are worth highlighting. First, the Lag-1 specification is just as significant as our baseline specification (Lag-2). Second, note the Lag-0 specification, which can be thought of as a placebo measure of regulation, since regulation at t might have little to no overlap with economic activity from $t-1$ to t. As expected, its exogenous component is not a significant predictor of growth. Finally, including restriction levels from two years leads both to become insignificant predictors (columns 4, 5). This is not surprising, as restriction levels, between-years and within-state, should be highly collinear.

TABLE 4. Estimating GDP growth on varying lags on restrictions, TSLS

	Lag-2	Lag-1	Lag-0	Lag-1,2	Lag-0,1
ln(Restrictions)			−0.0126		−1.3312
			(0.0139)		(1.9113)
ln(Restrictions), $t-1$		−0.0267*		−0.4997	1.3050
		(0.0121)		(1.3093)	(1.9102)
ln(Restrictions), $t-2$	−0.0370*			0.4637	
	(0.0168)			(1.3086)	
Intercept	0.4823*	0.3537*	0.1766	0.4698*	0.3486
	(0.2085)	(0.1504)	(0.1729)	(0.2271)	(0.3315)
Controls	Yes	Yes	Yes	Yes	Yes
Second IV?	No	No	No	Yes	Yes
Number of observations	77	125	125	77	77

Note: IV = instrumental variable; TSLS = two-stage least squares.
***$p <$ 0.1 percent; **$p <$ 1 percent; *$p <$ 5 percent significance.

5.3 Transforming regulation to linear or squared

In section 3, we noted that we prefer $\ln Reg$ as our endogenous variable because it is approximately normal and implements the idea that at high levels of restrictiveness, one additional regulation would have less impact than if a state were less regulated. We relax this assumption by running our baseline specification, where the treatment is instead either restriction count as measured (Reg) or squared restriction count (Reg^2).

Table 5 shows the TSLS second-stage results and column 1 shows our baseline estimate. Columns 2 and 3 confirm that the significance of our results is not affected by how we transform restriction count. In fact, the exogenous component of squared restriction count is statistically more significant than our baseline specification. However, given that both alternative transformations have lower F-statistics, we prefer the baseline log-transformation, under which state age is a more relevant instrument.

TABLE 5. Estimating GDP growth on varying specifications of restrictions, TSLS

	Model 1	Model 2	Model 3
ln(Restrictions)	−0.0370 *		
	(0.0168)		
Restrictions (millions)		−0.1272 *	
		(0.0621)	
Restrictions^2 (trillions)			−0.1772 **
			(0.0551)
Intercept	0.4823 *	0.0553 **	0.0356 ***
	(0.2085)	(0.0168)	(0.0052)
1st-stage *F*-statistic	4.4593	2.7150	1.7022
Controls	Yes	Yes	Yes
Number of observations	77	77	77

Note: TSLS = two-stage least squares.
****p* < 0.1 percent; ***p* < 1 percent; **p* < 5 percent significance.

5.4 Geography as placebo instrument

One potential concern is that state age is simply proxying for region-based growth in Western or Southern states. What the results indicate then, in fact, is not institutional sclerosis, but some spurious correlation between region, regulation, and growth. If this is true, we might see even stronger results when we directly use geography as a placebo instrument. As table 6 shows, however, this is not the case. Log of restriction count as a treatment loses significance when we use latitude or longitude (or both) as instruments. In other words, state age meaningfully captures variation that cannot be explained through the region to which a state belongs.

TABLE 6. Estimating GDP growth on restrictions, TSLS with geography

	Longitude	Latitude	Longitude + latitude
ln(Restriction count)	–0.0468	0.0167	0.0015
	(0.0435)	(0.0170)	(0.0117)
Population at admission	0.0209	–0.0045	0.0016
	(0.0252)	(0.0155)	(0.0124)
Area size	–0.0279	0.0062	–0.0020
	(0.0371)	(0.0237)	(0.0192)
Intercept	0.6306	–0.1586	0.0298
	(0.5405)	(0.2111)	(0.1459)
Controls	Yes	Yes	Yes
Number of observations	77	77	77

Note: TSLS = two-stage least squares.

6. Conclusion

This paper has presented evidence on how regulatory accumulation affects economic growth by using state age as an instrument that affects regulatory accumulation only through institutional sclerosis. To justify the instrument's validity, we leveraged Olson's hypothesis of institutional sclerosis and further offered evidence that state age is in fact a relevant and exogenous instrument. Our main results indicate that a 10 percent increase in state-level regulatory restrictions will reduce real GDP by 0.37 percentage point. Results are robust to controlling for potential determinants of statehood and alternative specifications of treatment variables. Robustness tests also indicate that the results are not driven by spurious correlation with geography. Our findings suggest that reducing the aggregate number of regulations at the state level can promote faster economic growth—a finding that is consistent with Coffey and McLaughlin; Coffey, McLaughlin, and Peretto; and Dawson and Seater but that represents the first to find this result at the state level in the United States.[35]

We should acknowledge that the accumulation of regulation does not always equate to an increase in stringency. Though RegData allows one to construct indices of regulatory stringency based on industry relevance, this can introduce considerable noise to the measure of regulatory variation. One workaround can be drawn from McLaughlin and Sherouse's and McLaughlin and Warlick's Federal Regulation and State Enterprise Index, which aggregates industry restrictions from federal regulations to the state level using a weighted rather than simple sum, where weights are a given industry's contribution to a given state's output.[36] Future research should explore new measures of stringency that even better approximate a jurisdiction's level of regulation, while distinguishing between different types of regulations.

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[35] Coffey and McLaughlin, "Regulation and Economic Growth"; Coffey et al., "Cumulative Cost of Regulations"; and Dawson and Seater, "Federal Regulation and Aggregate Economic Growth."

[36] Patrick A. McLaughlin and Oliver Sherouse, "The Impact of Federal Regulation on the 50 States" (Mercatus Center at George Mason University, 2016); Patrick A. McLaughlin and Hayden Warlick, "FRASE Index: A QuantGov Data Release," QuantGov, 2020.