Cost of Regulatory Accumulation: US States' Age as Identification

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Abstract

We exploit variation in statehood recency 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.

1 Introduction

We typically think there is some tradeoff between regulation and efficiency. But until the last 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 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 which are generated from text-scraping programs that count regulations at degrees of directness and scale not achieved by research in the past. The second issue had been that the literature had generated mixed results, with most studies not having identified an exogenous variation in regulatory levels. 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. 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 restrictions is estimated to cause GDP to fall by 0.37 percentage point–about one-seventh of a typical state's yearly growth. This implies that moving across the interquartile range in restriction count (i.e., reducing restrictions by 42 percent, or about 130,000 restrictions) would increase aggregate GDP by 1.54 percentage points.

Broadly defined, regulations are mandates that limit the domain of permissible actions of economic actors, typically designed and implemented to achieve some specific outcome(s). Regulation is a 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. Gordon Tullock argued that in addition to deadweight loss, interventions in general entail compliance cost and invite attempts to capture transfers, which redirect factors from their more productive uses.[1] Similarly, George Stigler hypothesized that regulation is captured by industry and that it is by producers' monopolistic design intended to restrict output.[2]

Mancur Olson has 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 (Olson 1984, 73-4). Substantial volumes of regulations can also raise the cognitive cost of entrepreneurship. Or put differently: "Regulations in this view are like pebbles tossed into a stream. Each pebble in isolation has a negligible effect on the flow but toss enough pebbles and the stream is dammed."[4] More will be said on Olson's hypothesis of institutional sclerosis shortly in Section 3.1. It should also be noted that increasing the number of rules increases the likelihood of contradiction (or what Hillel Steiner would term as "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]

John Dawson and John Seater made one of the first attempts to directly measure the amount of regulations.[6] Prior to Dawson and Seater, most studies resorted to using indices of regulatory severity (either self-constructed or by organizations such as the OECD),[7] which can limit the scope of regulation evaluated (to only e.g., 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 though page counts. With a general equilibrium model, the authors estimated that if the pages of regulations had been unchanged since 1949, the economy would have grown 2.2 percent more annually—or an increase of $38.8 trillion to GDP by 2011 (Dawson and Seater 2013, 160).

Nathan Goldschlag and Alex Tabarrok were one of the first to use RegData to estimate regulatory cost. (RegData will be described in more detail in Section 2.) In particular, they exploited variation in federal regulation across industries and found that regulatory stringency is statistically insignificant for industry value-add (Goldschlag and Tabarrok 2018, 23). In fact, measures of regulatory stringency correlated *positively* with entrepreneurship under several specifications (Goldschlag and Tabarrok 2018, 24; 26; 28-9; 31).

[1] Gordon Tullock, 'Welfare Costs of Tariffs, Monopolies, and Theft', *Western Economic Journal* 5, no. 3 (June 1967): 225-6.

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

[3] Mancur Olson, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities* (New Haven: Yale University Press, [1982] 1984): 65-8.

[4] Nathan Goldschlag and Alex Tabarrok, 'Is regulation to blame for the decline in American entrepreneurship?', *Economic Policy* (January 2018).

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

[6] John W. Dawson and John J. Seater, 'Federal regulation and aggregate economic growth', *Journal of Economic Growth* 18, no. 2 (June 2013).

[7] For example, see Norman V. Loayza, Ana María Oviedo, and Luis Servén, 'Regulation and Macroeconomic Performance', *World Bank* (September 2004).

Bentley Coffey et al., meanwhile, took a similar approach to Dawson and Seater while using RegData.[8] The authors specified a general equilibrium model where growth depends on lagged knowledge investment and its interaction with regulation, and where knowledge investment depends on past growth and regulation. They found that the economy would have grown 0.8 percent more annually if federal regulation remained at 1980 levels—or a $4 trillion increase to GDP by 2012 (Coffey et al. 2020, 14-5).

More recently, Bentley Coffey and Patrick McLaughlin studied the case of regulatory budgeting in British Columbia, Canada, which reduced its count of regulations by one-third in three years.[9] 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 capita (Coffey and McLaughlin 2021, 36). There was additional causal evidence from a difference-in-difference synthetic control setup which found that the reform (of reducing regulations by one-third) increased growth by 1.4 percentage points (Coffey and McLaughlin 2021, 35). However, the latter is estimated with a difference-in-difference setup across province and time that did not leverage direct measurement of regulatory levels. This illustrates the difficulty of designing a study where the shift in regulatory stringency is exogenous and where this shift can be measured with RegData. To this end, we will now introduce the concept of institutional sclerosis which we will argue provides a source of exogenous variation in regulation.

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 various variables. The results are shown in Section 4. Section 5 covers various tests conducted in response to possible concerns about the model. 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).[10] Our main outcome of interest is a given state's GDP.

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	

[8] Bentley Coffey, Patrick A. McLaughlin, and Pietro Peretto, 'The cumulative cost of regulations', *Review of Economic Dynamics* 38 (2020).

[9] Bentley Coffey and Patrick A. McLaughlin, 'Regulation and Economic Growth: Evidence from British Columbia's Experiment in Regulatory Budgeting', *Mercatus Working Paper* (May 2021).

[10] U.S. Bureau of Economic Analysis, 'Table. SQGDP9 Real GDP in chained dollars', BEA Data API (accessed September 2, 2024).

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 D.C. (Latitude-Longitude)	
Mean (SD)	19 (16)
(Minimum, IQR, Maximum)	(0, 6, 28, 81)

Second, a measure of state-level regulations is provided by QuantGov's State RegData 2023.[11] 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', '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.

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 (HSUS).[12] 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 a instrument (see Section 3.1).

3 Model

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

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

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

$\ln y_i$ 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

[11] Patrick A. McLaughlin, Michael Gilbert, Jonathan Nelson, and Thurston Powers, 'State RegData 2023 (dataset)', QuantGov, Mercatus Center at George Mason University (2023).

[12] Susan B. Carter, Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright, 'Historical Statistics of the United States: Millennial Edition', Cambridge University Press (2006), originally published by the U.S. Census Bureau, https://hsus.cambridge.org/HSUSWeb/HSUSEntryServlet.

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 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 widely vary across states, with a mean of 2.7613×10^5 and values ranging between 98015 and 889795. 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 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.

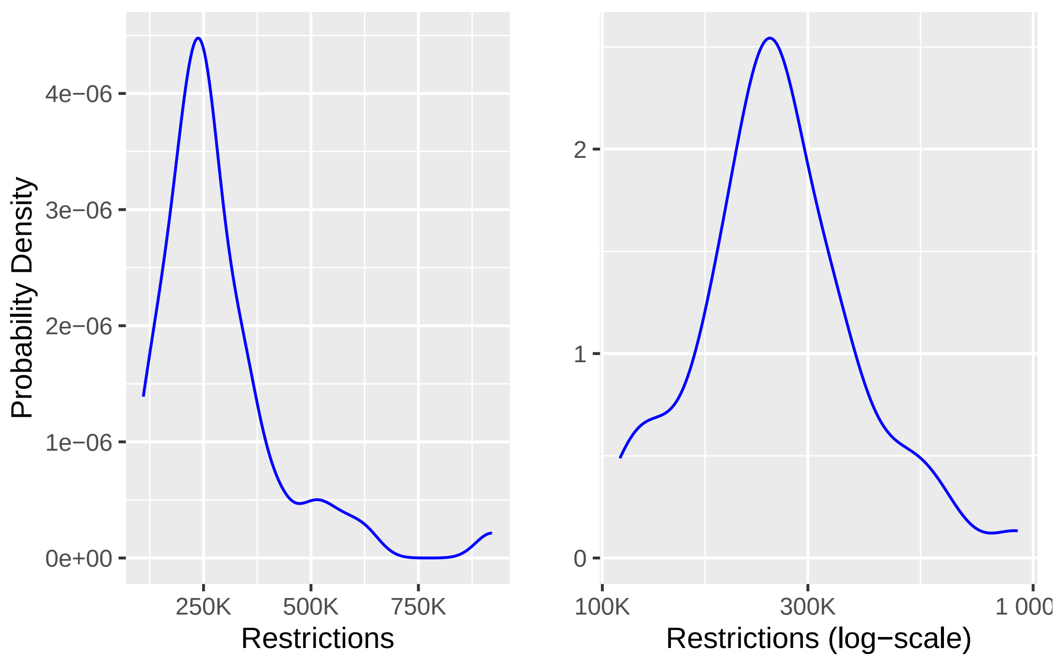

Figure 1: Density of Restriction Count and log of Restriction Count

In our main setup, we run GDP growth from $t-1$ to t on restriction count in $t-2$. 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.

$\vec{w}_i$ is a vector of controls of a given state i. The vector includes (i) a state's population recorded on the decennial census subsequent to statehood and (ii) the geographical area of a state at admission, both of which could determine how early a given territory sought statehood. Figure 2 shows the number of observations in our sample, by state.

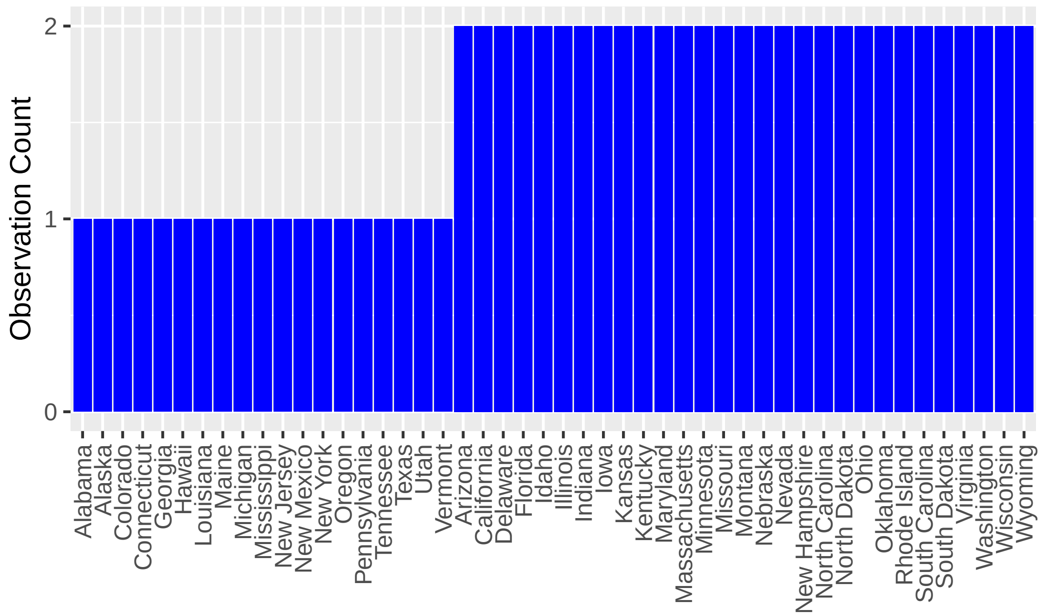

Figure 2: Number of Observations in Main Sample, by State

3.1 State Age as 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 US, with the exception of Southern states, where the variable is defined as years since their re-admission to the US in 1868, as the Civil War (in addition to reconstruction) likely disrupted or inhibited the development of interest groups (Olson 1984, 98). This is the instrument for obtaining an exogenous component of Reg_i. Figure 3 shows the distribution of state age.

Mancur Olson (1984) offered the institutional sclerosis hypothesis to explain why affluent societies become stagnant with time. The main components of his hypothesis are as follows (76):

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 reduce the rate of economic growth.

We will elaborate on the first two components briefly. (1) 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, and/or previously established social networks for bargaining costs to be overcome, and such preconditions are highly congruent with, if not implies, a stable environment (Olson 1984, 43-4). (2) 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 organize a cartel (Olson 1984, 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 1965-78 and between 1946-78. This result is particularly noteworthy, given that most US states were founded at least a century before the period for which income was measured (Olson 1984, 104-6; 114). Furthermore, state age is positively and significantly correlated with one measure of interest group accumulation, specifically union membership as a percentage of employees (non-agricultural) (Olson 1984, 107-8). State age is also a significant predictor of log-transformed restriction count ($p = 0.0027$), as Figure 4 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 less restrictions, tend to exhibit higher growth today.

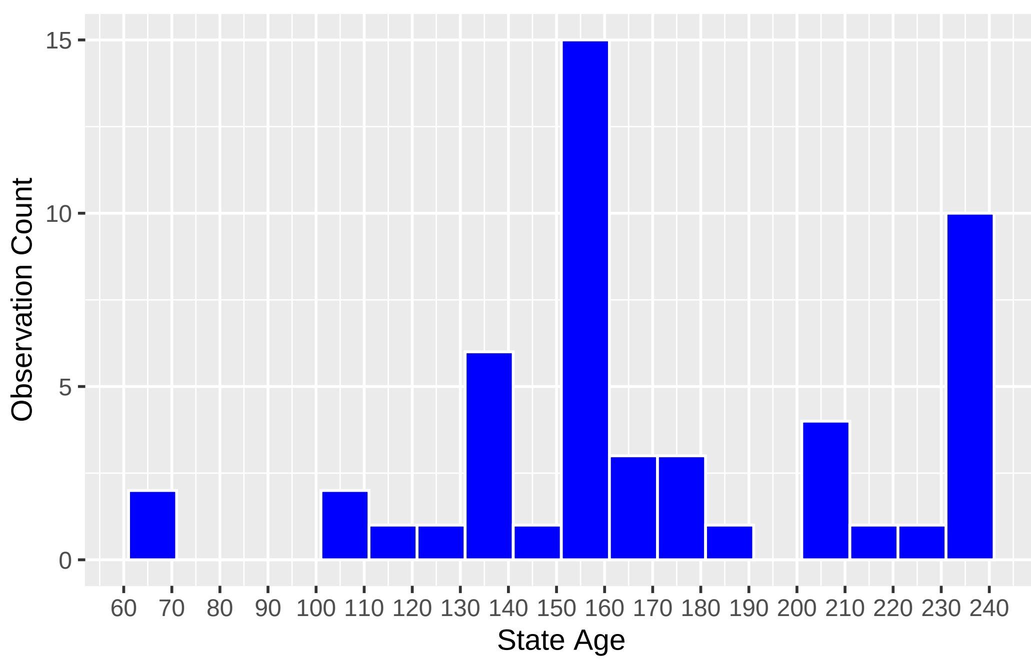

Figure 3: Historgram of State Age

Subsequent scholars have also found evidence to reinforce the process of institutional sclerosis posited here. In a meta-analysis, Jac Heckelman found that subsequent researchers generally concurred with Olson's findings. The proportion of statistical studies (n = 28) which offer support, mixed support, and no support to institutional sclerosis respectively are 57 percent, 18 percent, and 25 percent[13]—though it should be cautioned that the sample of studies surveyed all provide merely correlational evidence. Among studies which focused on US states and the role of interest groups, Richard Vedder and Lowell Gallaway found that state age and union membership are significantly and negatively correlated with per capita income growth.[14] Mark Crain and Katherine Lee estimated a significantly negative relationship between the same outcome and business associations' revenue as a share of income.[15]

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, it certainly is plausible that there are determinants that could have influenced the timing of statehood. Prospective states had to meet population requirements, which in turn could have been driven by early industrial activity that has some persistent effect on current growth. Another determinant is a state's size, since longer travel distance within a state increases the cost to organize, which in turn can affect our endogenous variable. To account for these potential mechanisms, we include both (i) a state's population near admission and (ii) the geographical area of a state at admission. As we will show below, neither covariates are significant in either the first or second stage.

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 D.C. as additional instruments.[16] As we will show below 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 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 one percent level as a predictor for log of restriction count—which offers support to state age being a relevant instrument. In line with expectations, older states experience higher levels of restrictions. Population around the time of admission nor geographical area have significance—assuaging concerns about the endogeneity of state age.

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

[14] Richard Vedder and Lowell Gallaway, 'Rent-seeking, distributional coalitions, taxes, relative prices and economic growth', *Public Choice* 51 (1986): 96.

[15] Mark Crain and Katherine Lee, 'Economic Growth Regressions for the American States: a Sensitivity Analysis', *Economic Inquiry* 37, no. 2 (April 1999): 253. Due to its problematic specifications of interest group power, we omit another study which 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).

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

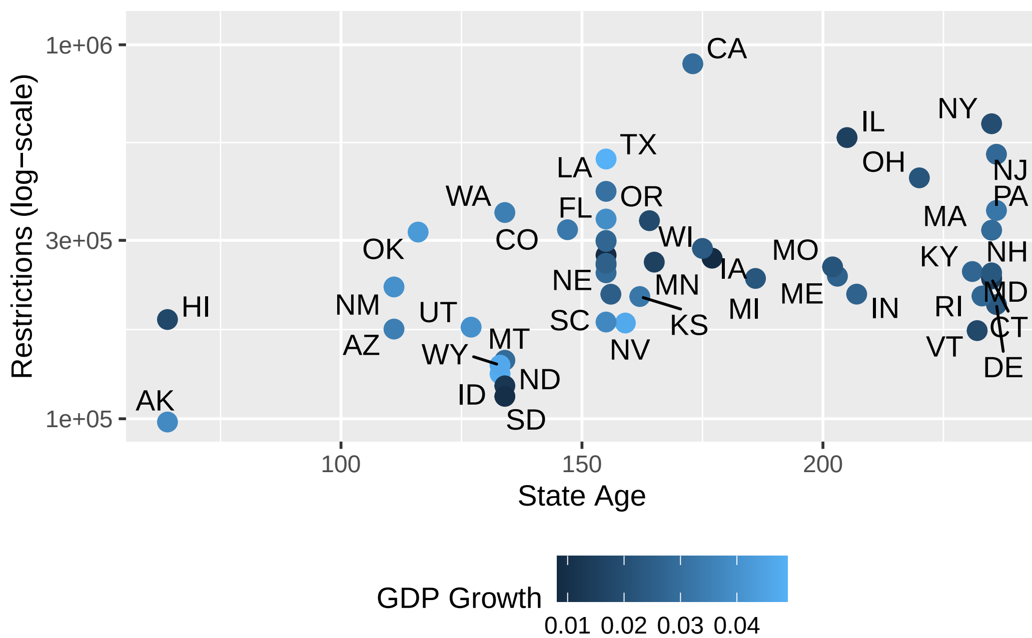

Figure 4: Log-Scale Restrictions vs. State Age with Color Gradient Representing Present GDP Growth

The second-stage results—Column 3—are encouraging as well. The exogenous component of treatment is statistically significant at the five 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 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, or -1.31441×10^5 restrictions) 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.

	%ΔGDP	ln(Restrictions)	%ΔGDP
	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	
Num. obs.	77	77	77

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2: Estimating GDP Growth on Restrictions, OLS vs TSLS with State Age

5 Robustness

5.1 Overidentification

To offer evidence on instrument exogeneity, we now conduct a J-test by overidentifying the first-stage equation, i.e., 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 2nd-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 Julia Clouse (2019). 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. 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 D.C. in the latitude-longitude space. This measure gives us an approximation of a state's distance from D.C. The two key ideas here are that the interaction between where a state ends up located and D.C.'s location is as good as random, but that once both are established, nearness to D.C. 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 2nd-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 IVs are correlated with disturbances in the outcome. Column 1 uses both state age and year of initial constitution as instruments, while Column

2 uses all three variables. While the J-statistic in the latter case does increase in significance, the null cannot be rejected under either overidentified specifications at even the ten percent level. This suggests the main instrument is in fact exogenous.

	2nd-Stage Residuals	
	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 D.C.		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
Num. obs.	77	77

P-value indicates the probability that the null (that instruments are exogenous) is true.

Table 3: Overidentifying Restrictions Test (J-Test)

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 2nd 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, 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.

5.3 Transforming Regulation to Linear or Squared

In Section 3, we noted that we prefer $\ln Reg$ as our endogenous variable as it is approximately normal and implements the idea that at high levels of restrictiveness, one additional regulation has 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 2nd-stage results and Column 1 shows our baseline estimate. Columns 2 and 3 confirm that the significance of our results are 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.

	%ΔGDP				
	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
Num. obs.	77	125	125	77	77

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 4: Estimating GDP Growth on Varying Lags on Restrictions, TSLS

	%ΔGDP		
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
Num. obs.	77	77	77

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 5: Estimating GDP Growth on Varying Specifications of Restrictions, TSLS

5.4 Geography as Placebo Instrument

We would like to consider one final objection, which 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.

	%ΔGDP		
	Longitude	Latitude	Longitude + Latitude
ln(Restriction Count)	0.0045	−0.0383	−0.0272
	(0.1207)	(0.0641)	(0.0518)
Population at Admission	−0.0084	0.0107	0.0058
	(0.0746)	(0.0586)	(0.0560)
Area Size	0.0195	−0.0059	0.0007
	(0.1052)	(0.0854)	(0.0822)
Intercept	−0.0083	0.5244	0.3873
	(1.5017)	(0.7975)	(0.6448)
Controls	YES	YES	YES
Num. obs.	125	125	125

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 6: Estimating GDP Growth on Restrictions, TSLS with Geography

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 Mancur 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 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.

We should acknowledge that the accumulation of regulation does not always equate to an increase in stringency. Goldschlag and Tabarrok (2018), for example, constructed a Herfindahl-Hirschman index to distinguish between general and specific regulations. Though RegData allows one to construct indices of regulatory stringency based on industry relevance, this can introduce considerable noise to one's measure of regulatory variation. One workaround can be drawn from Patrick McLaughlin and Hayden Warlick's (2020) state regulatedness index, which aggregates industry restrictions 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.[17] Future research should explore new measures of stringency that even better approximates a jurisdiction's regulatedness.

`Word Count: There are 4500 words in this document.`

[17] Patrick A. McLaughlin and Hayden Warlick, 'FRASE Index: A QuantGov Data Release', QuantGov (2020).