Estimating the Cost of Regulatory Accumulation: US States' Age as Identification Strategy

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Abstract

We exploit ages of 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 personal income. Specifically, a 10 percent increase in restrictions is estimated to cause personal income to fall by 0.469 percent—one-sixth of the average state's yearly growth.

1. Introduction

Regulation is recognized to come at some tradeoff to economic growth. But until the last several years, the extent to which an economy is regulated, let alone the scale and significance of regulation's cost to 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 a scale not achieved by most research in the past. The second issue had been that most studies used statistical models that lacked specifications to eliminate bias, thereby generating insignificant evidence or that which is significant but correlational at best. To that end, we draw on the institutional sclerosis literature to justify the use of a given US state's age as an instrumental variable. This allows us to obtain an exogenous component of regulatory accumulation.

We find that a higher level of regulation reduces the growth of personal income at the state level. Specifically, a 10 percent increase in restrictions is estimated to cause personal income to fall by 0.469 percent—which is around one-sixth of the average state's yearly growth and no small amount. This implies that moving across the interquartile range in restriction count (i.e., reducing restrictions by 46 percent, or -139,772 restrictions) would increase aggregate personal income by 3.1 percent.

Broadly defined, regulations are government mandates that limit the domain of permissible actions of economic actors, typically designed and implemented to achieve some specific

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outcome(s). Regulation is a vehicle for addressing market failures (such as externalities and information asymmetries) and maximizing social output. 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 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, which in turn demand judicial intervention, leading to rules that are more ad hoc and potentially more arbitrary.[5]

While the attempts to estimate the impact of regulations on aggregate output or growth have been numerous, one of the first attempts to directly measure the amount of regulations was made by John Dawson and John Seater.[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 (e.g., licensing requirements, product safety requirements, and 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 Chapter 2.) In particular,

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[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).

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). 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) would decrease GDP per capita by 0.238 percent (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 percent (Coffey and McLaughlin 2021, 35). However, this result was not estimated with RegData and therefore could not be compared with the first estimate, which was not causal. 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. Chapter 2 describes the datasets on which this study relies. In Chapter 3, we describe the main model being estimated and justify the use of various variables. The results are shown in Chapter 4. Chapter 5 covers various tests conducted in response to possible concerns about the model. Chapter 6 concludes.

2. Data

This study combines several datasets. 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 personal income. Other outcomes of interest include finer sub-components, such as per capita personal income, population, capital income, and transfers.

Second, a measure of state-level regulations is provided by QuantGov's State RegData 2.0.[11] RegData measures the *count* of *restrictions* in each state's regulatory codes. 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 from each state's regulatory texts to the state level,

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[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. SAINC4 Personal income and employment by major component', BEA Data API (accessed November 18, 2023).

[11]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.

creating a sample of 46 observations. State RegData 2.0 is collected from March to June 2020, which provides us with a snapshot of states' restrictions that is lagged relative to the most recent observations in our series of economic outcomes. This allows us to implement the notion that regulatory accumulation, insofar as it has any effect, requires time to permeate into economic activity. One drawback of using State RegData 2.0 (as opposed to the latest 4.0) is that the restriction count only covers administrative rules issued by agencies, and not statutes created through legislation.

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 near the time of admission— which allows us to control for factors that may affect the independence of state age as an instrument (Section 3.1).

3. Model

The core results of this paper is obtained through two-stage least squares (2SLS) estimation of the following two equations:

$$lny_{i,2022} - lny_{i,2022-t} = lnReg_{i,2020}\beta + X_i\delta + e_i, t = 1, 2 \tag{1}$$

$$lnReg_{i,2020} = Age_i b + X_i\delta + u_i \tag{2}$$

Here, lny_i is the income component of a given state i. Since we are estimating regulatory accumulation's affect on the income process, we are interested in moments in income change. Specifically, we estimate the first differences of log income, a common measure used in labor economics,[13] i.e., $lny_t - ln_{t-1} = ln(y_t/y_{t-1})$. Since BEA-NIPA data ends at 2022, even if we assume that only half a year is necessary for regulation to affect the income process, the longest horizon at which we can estimate first-differences is two years, starting in 2020. Thus, one- and two-year first differences as at 2022 are reported for various income components. Table 1 reports summary statistics. We are also more interested in the growth of personal income (PI) than that of per capita personal income (PCPI). 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. Since PI is mechanically defined as PCPI times population (Pop), change in PI captures both dynamics. That is, $\frac{PI_t}{PI_{t-1}} = \frac{PCPI_t}{PCPI_{t-1}} \cdot \frac{Pop_t}{Pop_{t-1}}$.

Reg_i refers to restriction count in 2020 of a given state i (a.k.a. the treatment). The count of regulatory restrictions widely vary across states, with a mean of 254,600 and values ranging between 64,000 and 791,100. 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

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

[13]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).

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. X_i is a given state's population around the time of admission, which could determine how early a given territory sought statehood.
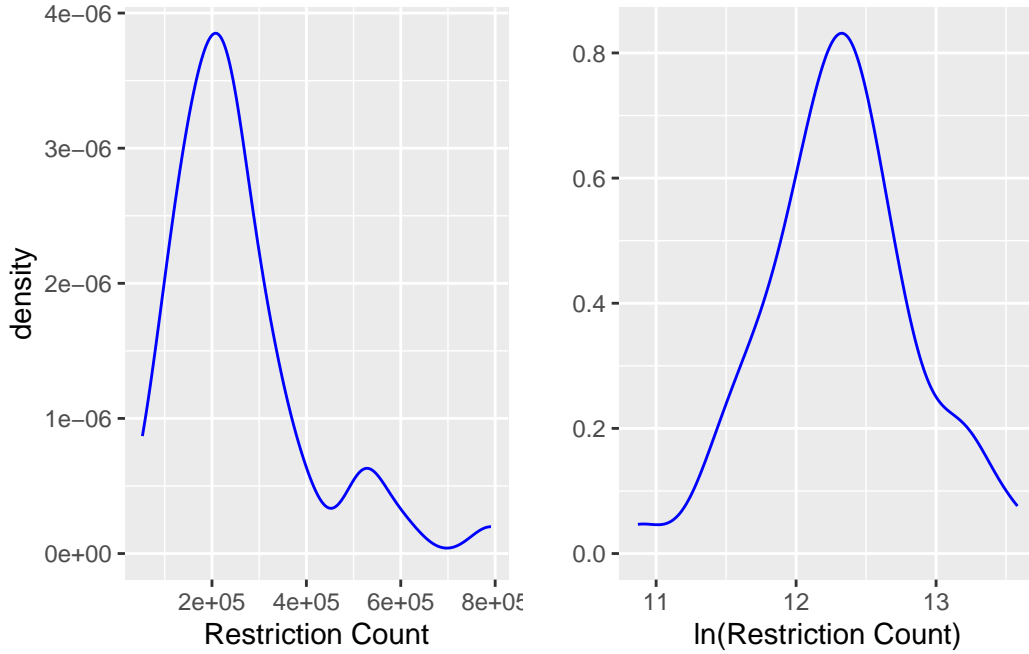

Figure 1: Densities of Restriction Count and Log of Restriction Count

Estimating Equation 1 alone would lead to bias due to omitted variables, simultaneity (outcome having an effect on the treatment), and measurement error in the treatment. However, with a valid instrument that is uncorrelated with ϵ_i, we can estimate Equation 1 using 2SLS. 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.

Table 1: Summary Statistics

Statistic	Min	Pctl(25)	Mean	Pctl(75)	Max	St. Dev.
State Age	64	149	170.7	204.5	236	42.0
Restrictions/1000	52.6	162.3	254.6	302.1	791.1	146.2
ln(Restriction Count)	10.9	12.0	12.3	12.6	13.6	0.5
Population (Admission)/1m	0.001	0.01	0.1	0.1	0.7	0.1
PI, 1yr Diff	−1.6	1.2	2.8	4.0	8.5	2.2
Population, 1yr Diff	−0.01	−0.001	0.004	0.01	0.02	0.01
Per Capita PI, 1yr Diff	−0.7	1.3	2.4	3.3	7.1	1.7
Per Capita Capital Inc, 1yr Diff	2.7	5.5	6.9	8.3	12.1	2.3
Per Capita Transfers, 1yr Diff	−24.9	−15.7	−13.0	−11.5	2.9	4.3
PI, 2yr Diff	5.5	8.8	11.5	13.9	17.8	3.2
Population, 2yr Diff	6.7	9.3	10.7	12.4	15.5	2.1
Per Capita PI, 2yr Diff	−0.02	−0.002	0.01	0.02	0.05	0.01
Per Capita Capital Inc, 2yr Diff	−16.5	−6.4	−3.4	0.1	13.2	5.7
Per Capita Transfers, 2yr Diff	10.0	13.8	15.9	17.9	22.6	3.0

Note: All differences are log-differences.

3.1. State Age as Instrument

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 first elaborate on each component briefly. (1) rests on the notion of 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). Finally, (3) argues that as restrictions in the economy accumulate, societies will find their markets rigid and reallocation deliberate in spite of changing economic conditions.

The significance and magnitude of Olson's hypothesis have been extensively tested, starting with Olson himself along with Kwang Choi. Olson and Choi found that a state's founding year (or what I call 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 ($p = 0.022$), as Figure 2 also illustrates.

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[14]—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.[15] Mark Crain and Katherine Lee estimated a significantly negative relationship between the same outcome and business associations' revenue as a share of income.[16]

Given the ostensible relevance of a state's age to explaining its economic growth, we propose using state age as an instrumental variable. Insofar as there is some unobserved variable U that biases any direct estimate of regulation's effect on growth, state age can provide an exogenous variation in regulation as long as $Cov(StateAge, U) = 0$. We posit that it is implausible for state age, other than through the channel of institutional sclerosis, to affect present economic growth. And insofar as state age may be influenced by early industrial activity, we include a given state's population around the time of admission to control for this possible source of endogeneity.

4. Results

The 2SLS results are reported in Table 2. The outcome of interest here is the one-year first-difference of log personal income. Column (1) reports the results from a simple OLS regression of the outcome on log restriction count. Log restriction count as a predictor is statistically significant at the 1 percent level. The negative sign of the estimated coefficient is consistent with expectations as well. A 10 percent increase in the number of restrictions is associated with a 0.146 percent decrease in personal income ($y_t/y_{t-1} - 1 = (Reg_t/Reg_{t-1})^{\beta} - 1 = (1 + 10\%)^{-0.0153} - 1$).

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[14]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.

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

[16]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).
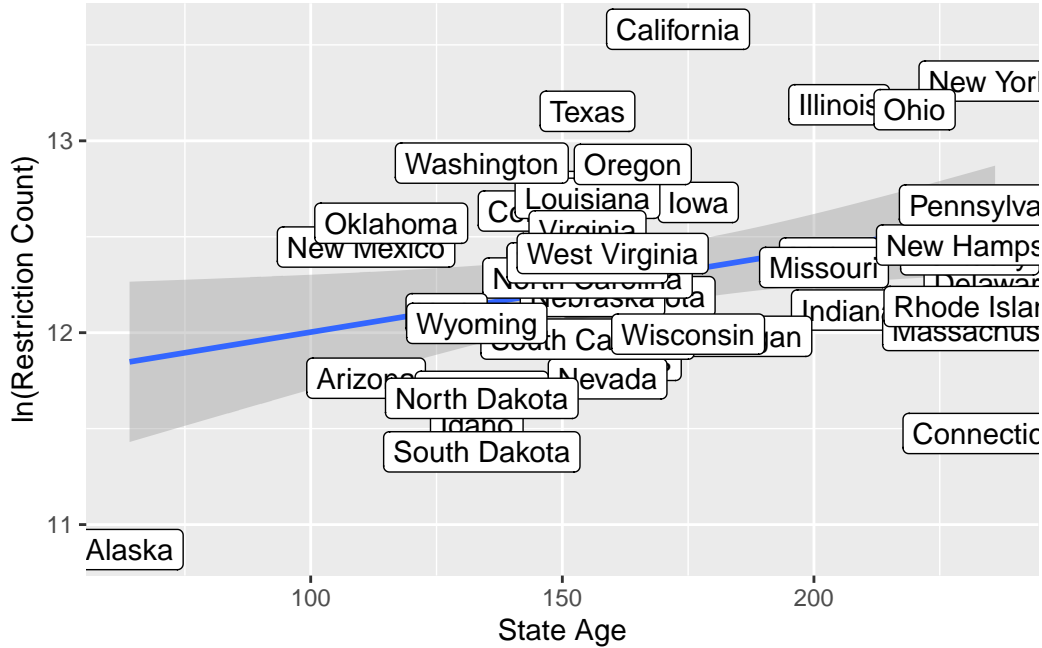
Figure 2: Relationship Between Treatment and Instrument

Moving onto the 2SLS first-stage results in Column (2), we see that state age is significant at the 5 percent level as a predictor for log 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 neither affects our instrument nor has significance—assuaging our 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. Again, the level of regulation reduces the personal income growth of a state. The magnitude of the coefficient is more than three times larger than that in Column (1), confirming our suspicion that a simple OLS estimation captures upward bias. In this case, it may be that states which anticipate lower growth tend to deregulate. When we account for this bias, the negative relationship between growth and regulation becomes stronger. The estimate from Column (3) implies that a 10 percent increase in restrictions will reduce personal income by 0.469 percent. This is rather significant as states' personal income only grew 2.8 percent in 2022 (cross-sectional average), which would make our estimate one-sixth of yearly growth. For an alternative interpretation: moving across the interquartile range in restriction count (i.e., reducing restrictions by 46 percent, or -139,772 restrictions) would increase personal income by 3.1 percent $(= (162.3/302.1)^{-0.0493} - 1)$.

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 2SLS 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 ($p = 0.012$). The 90 percent confidence interval for the estimated coefficient is $[-0.572, -0.015]$. This puts even the weaker end of

the interval equivalent to the result from Table 2 Column (1), confirming our exogenous component of regulation as a highly significant and relevant predictor.

Table 2: Estimating Personal Income Growth on Restrictions Using State Age as Instrument

	Outcome is first-difference of log personal income (2021-22)		
	Outcome OLS	log(Reg) 1st Stage	Outcome 2nd Stage
	(1)	(2)	(3)
ln(Restriction Count)	-0.0153^{***}		-0.0493^{**}
	(0.0042)		(0.0230)
State Age		0.0041^{**}	
		(0.0019)	
Population (Admission)/1m		0.2324	0.0097
		(0.5365)	(0.0284)
Constant	0.2124^{***}	11.5853^{***}	0.6340^{**}
	(0.0523)	(0.3224)	(0.2817)
Observations	46	46	46
R^2	0.2284	0.1177	-0.2774
Adjusted R^2	0.2108	0.0766	-0.3368
Residual Std. Error	0.0153 (df = 44)	0.5144 (df = 43)	0.0254 (df = 43)
F Statistic	13.0210^{***} (df = 1; 44)	2.8673^{*} (df = 2; 43)	

Note: *p<0.1; **p<0.05; ***p<0.01

R-squared from 2nd-stage regression should not be interpreted.

5. Robustness

5.1. Different Income Components as Outcomes

The results obtained in Chapter 4 are robust across different specifications of various income components as outcomes. Table 3 Column (5) shows an estimate of the same relationship, but where the log-first difference of personal income is computed across two years (2020 to 2022). The estimated effect of restrictions on growth is similar to that in Table 2 Column (3); a 10 percent increase in restrictions is predicted to decrease personal income by 0.432 percent $(= [(1 + 10\%)^{-0.0908}]^{1/2} - 1)$ per year on average. The relationship is weaker than those shown in Table 2, but this is most likely caused by the overlap of State RegData 2.0's data gathering window and BEA-NIPA's measurement window.

Decomposing personal income into its main sub-components of population and PCPI shows that both hypothesized channels of reduced efficiency and lower net migration play a role (Columns (1), (2), (6), (7)). The estimated relationship between regulation and PCPI also allows us to directly compare the results here with the established literature. Coffey and McLaughlin estimated that reducing restrictions by 33 percent increased British Columbia's growth by 1.4 percent annually. According to Table 3 Column (2), a 33 percent *decrease* in restrictions would increase growth by 1.384 percent $(= (1 - 33\%)^{-0.0339} - 1)$–essentially an identical result.

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Table 3: Estimating Income Components Growth on Restrictions Using State Age as Instrument (2nd Stage Results)

	Outcome is log-first difference of income component								
	Pop	PCPI	Capital	Transfers	PI	Pop	PCPI	Capital	Transfers
		1-Year Difference					2-Year Difference		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln(Restriction Count)	−0.0154*	−0.0339**	−0.0285	−0.0916*	−0.0908*	−0.0282*	−0.0626*	−0.0641*	−0.1791**
	(0.0085)	(0.0167)	(0.0197)	(0.0463)	(0.0456)	(0.0150)	(0.0335)	(0.0355)	(0.0877)
Population (Admission)/1m	0.0037	0.0060	−0.0374	0.0347	0.0009	0.0059	−0.0050	−0.0408	0.0498
	(0.0105)	(0.0207)	(0.0243)	(0.0572)	(0.0564)	(0.0186)	(0.0414)	(0.0439)	(0.1084)
Constant	0.1934*	0.4405**	0.4236*	0.9929*	1.2326**	0.3541*	0.8785**	0.9528**	2.1651*
	(0.1037)	(0.2046)	(0.2409)	(0.5669)	(0.5588)	(0.1838)	(0.4099)	(0.4352)	(1.0738)
Observations	46	46	46	46	46	46	46	46	46

Note: *p<0.1; **p<0.05; ***p<0.01

Capital refers to capital income. All outcomes except Pop and PI are per capita.

5.1.1. Capital Income

We now briefly turn to per capita capital income—a subcomponent of PCPI, defined as dividends, interest, and rent. The relationship between regulation and capital income is significant at the 10 percent level at the two-year horizon, but not one-year (Table 3 Columns (3), (8)). One could attribute this to the short-term volatility of capital returns, but overall these results are rather ambiguous. One concern given the slight significance of capital income is that our results may be driven by catch-up by new states, as neoclassical growth models predict. If this is true, it would violate exclusion restriction. To that end, we repurpose the BEA-NIPA data and find that there is no relationship between present and ten-year lagged per capita capital income ($p = 0.391$). Figure 3 illustrates the same point. This is contrary to the notion that newer states are still experiencing "catch-up growth" today.
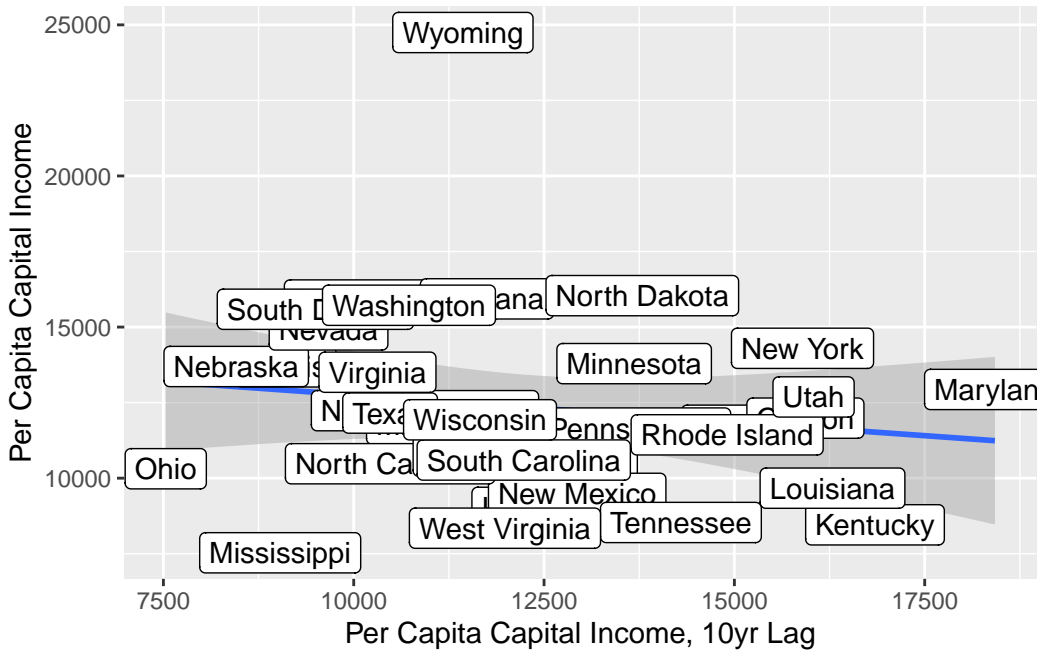

Figure 3: Relationship Between Present and 10-Year Lagged Capital Income

5.1.2. Federal Rents

The idea that states are seeking federal rents through federal policies that redistribute resources across states is arguably conceptually similar to institutional sclerosis. Given that interest groups proliferate more in older states, the idea goes, not only would they be able to capture policymaking at the state level, but also the federal level. To our concern, this would also violate exclusion restriction, as state age would affect not only regulation through interest groups, but also redistribution through interest groups.

If federal rent-seeking due to institutional sclerosis is true, we would expect the amount of regulation at the state level to be positively correlated with a state's transfer income. However, Columns (4) and (9) of Table 3 show the opposite: if anything, the income processes of more regulated states depend less, not more, on federal transfers. This suggests that,

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insofar as there is pervasive lobbying for federal rents, older states are less competent at doing so. One speculative explanation is that regulation, in addition to reducing efficiency and net migration, also reduces the demand for transfers—as there may be a substitution effect between the two.

5.2. 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 4 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 instrument. In other words, state age meaningfully captures variation that cannot be explained through the region to which a state belongs.

Table 4: Estimating Personal Income Growth on Restrictions Using Geography as Instrument (2nd Stage Results)

	Outcome is log-first difference of personal income		
	Longitude	Latitude	Longitude + Latitude
	(1)	(2)	(3)
ln(Restriction Count)	−0.0479	−0.0147	−0.0163
	(0.0356)	(0.0136)	(0.0134)
Population (Admission)/1m	0.0089	−0.0092	−0.0083
	(0.0317)	(0.0212)	(0.0211)
Constant	0.6168	0.2098	0.2291
	(0.4354)	(0.1661)	(0.1647)
Observations	46	46	46

Note: *p<0.1; **p<0.05; ***p<0.01

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 reviewed the evidence for Mancur Olson's hypothesis of institutional sclerosis and demonstrated that state age is in fact a relevant instrument. The main results implied that a 10 percent increase in restrictions will reduce personal income by 0.469 percent. Results were robust to alternative specifications of outcome variables. Robustness tests also indicated that the results were not driven by spurious correlation to geography or omitted variables. Our findings suggest that reducing the aggregate number of regulations at the state level can promote faster economic growth.

As we have stated in Chapter 2, given the recency of even the most vintage versions of State RegData, it is not possible to test whether the relationship we have estimated would hold over longer periods of time. This provides researchers a few years from now with an opportunity to revisit and hopefully reaffirm our findings. We should also 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. Future researchers should explore new ways to construct measures of stringency, in addition to tests for evaluating the accuracy of such measures.

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