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

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Abstract

I 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. I find that a higher exogenous level of regulation 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, I draw on the institutional sclerosis literature to justify the use of a given US state's age as an instrumental variable. This allows me to obtain an exogenous variation in regulatory accumulation

I 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

2 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, I estimate the first differences of log income, a common measure used in labor economics,[1] 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, as our regulatory data was gathered between March and June 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

[1] For example, see Richard Blundell, Luigi Pistaferri, and Ian Preston, 'Consumption Inequality and Partial Insurance', *American Economic Review* 98, no. 5 (2008): 1887-1921.

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, $\frac{PI_t}{PI_{t-1}} = \frac{PCPI_t}{PCPI_{t-1}} \cdot \frac{Pop_t}{Pop_{t-1}}$.

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.

Reg_i refers to restriction count in 2020 of a given state i (a.k.a. the treatment). This measure of state-level regulation is provided by QuantGov's State RegData 2.0.[2] 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. I aggregate restrictions from each state's regulatory texts to the state level, creating a sample of 46 observations.

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, I use a log-transformation of restriction count as the main treatment variable. This will help enforce homoskedasticity when estimating

[2]Patrick A. McLaughlin and Jonathan Nelson, 'State RegData Definitive Edition (dataset)', QuantGov, Mercatus Center at George Mason University (2021).

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 vector of controls, which only consists of a state's population around the time of admission.

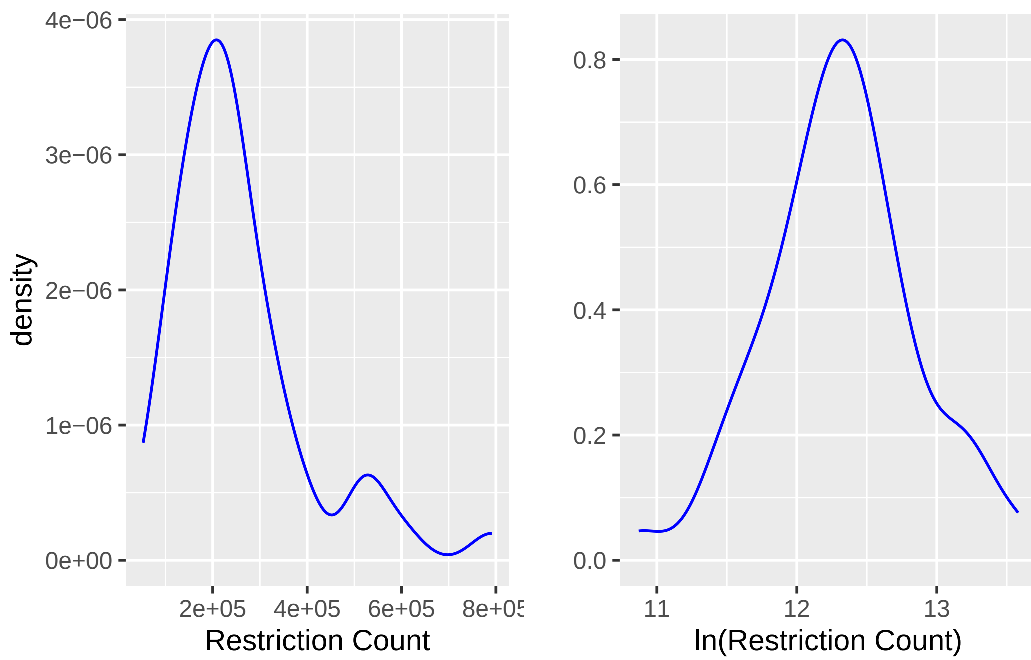

Figure 1: Distribution of Restriction Count vs. Its Log Transformation

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 estimate of Reg_i. The predicted outcome from Equation 2 ($\widehat{Reg_{i,2020}}$) will be used as a regressor for Equation 1.

2.1 State Age as Instrument

Mancur Olson (1984) offered the institutional sclerosis hypothesis to explain why affluent societies become stagnant with time.[3] 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.

I 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

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

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.

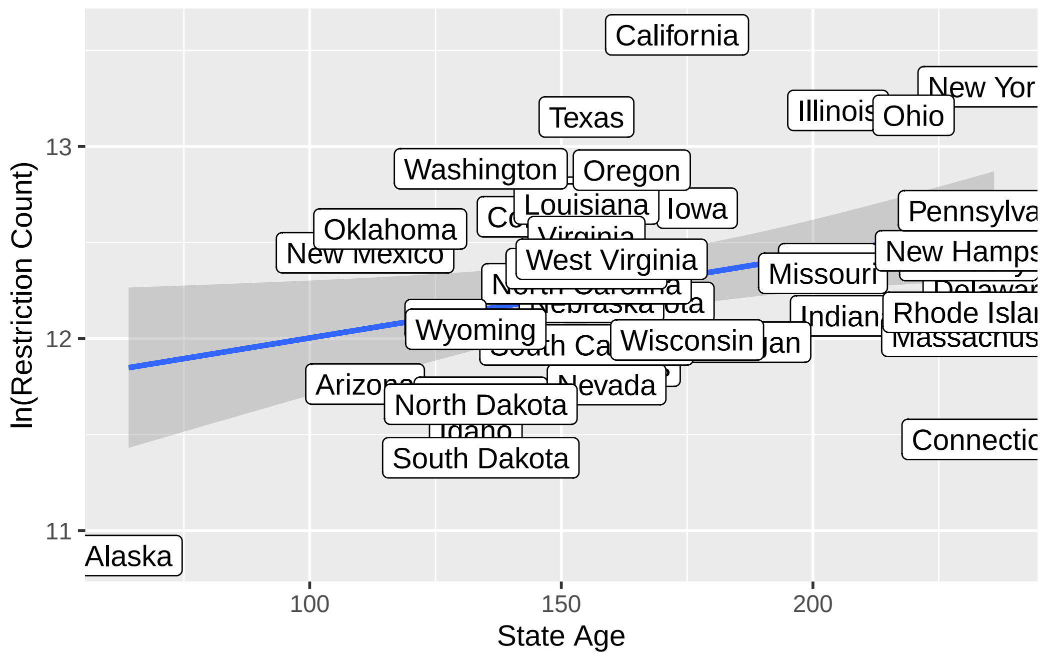

Figure 2: Relationship Between Treatment and Instrument

Given the ostensible relevance of a state's age to explaining its economic growth, I 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 $Cov(StateAge, U) = 0$ (Figure 3).[4] It is implausible that, other than through the channel of institutional sclerosis, how early a state was founded would affect present economic growth. And insofar as state age may be

[4]For a more detailed discussion on identification assumptions, see Scott Cunningham, '7.6 Heterogeneous Treatment Effects', in *Causal Inference: the Mixtape*, https://mixtape.scunning.com/07-instrumental_variables#heterogeneous-treatment-effects.

influenced by early industrial activity, I include a given state's population around the time of admission to control for this possible source of endogeneity.

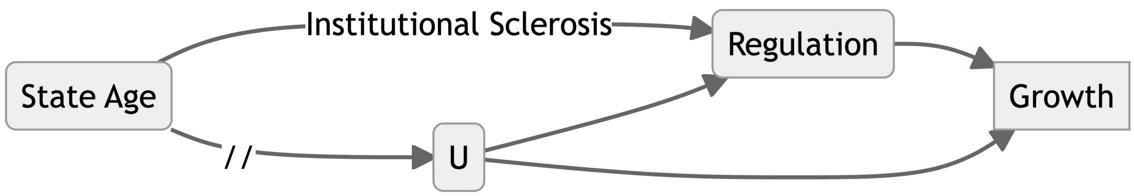

Figure 3: Directed Acyclic Graph

3 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 ($y_t/y_{t-1}-1 = (Reg_t/Reg_{t-1})^{\beta} - 1 = (1+10\%)^{-0.0153} - 1$).

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 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 ($= (1 + 10\%)^{-0.0493} - 1$). 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 ($= (162.3/302.1)^{-0.0493} - 1$).

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.

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, I 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 (Huang et al. 2017, 2473).[5] 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 measure of regulation as a highly significant and relevant predictor.

4 Robustness

4.1 Different Income Components as Outcomes

The results obtained in Chapter 3 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 me 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.[6] 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.

[5] Zhangkai Huang, Lixing Li, Guangrong Ma, and Lixin Colin Xu, 'Hayek, Local Information, and Commanding Heights: Decentralizing State-Owned Enterprises in China', *American Economic Review* 107, no. 8 (2017): 2455-78.

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

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.

4.1.1 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 neoclassical catch-up by new states.[7] This is *prima facie* implausible, given that the first quartile of state age is 149 years. But if it is true, it would violate exclusion restriction, i.e., $Cov(StateAge, U) > 0$ in Figure 3. To that end, I 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 4 illustrates the same point. This is contrary to the notion that newer states are still experiencing "catch-up growth" today.

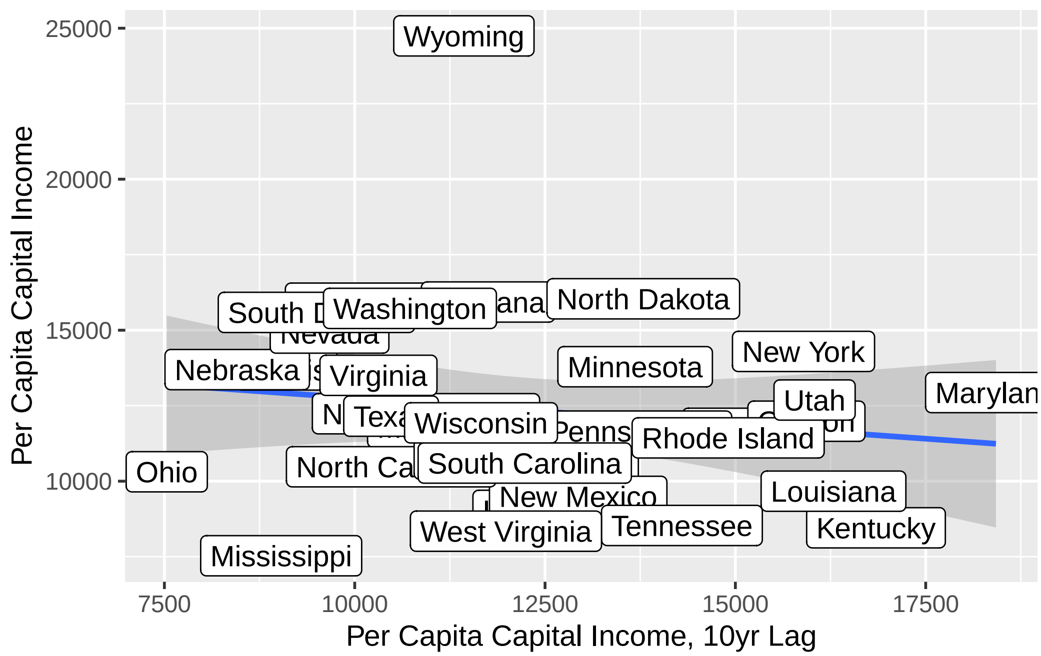

Figure 4: Relationship Between Present and 10-Year Lagged Capital Income

[7]Robert M. Solow, 'A Contribution to the Theory of Economic Growth', *The Quarterly Journal of Economics* 70, no. 1 (Feb., 1956): 65-94.

4.1.2 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 my concern, this would also violate exclusion restriction as illustrated in Figure 3, 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, 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. If this is true, this would be a third mechanism through which regulation affects income growth.

4.2 Geography as Placebo Instrument

I 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

5 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, I 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 various specifications of outcome variables and there was no evidence for alternative explanations. My findings suggest that reducing the aggregate number of regulations at the state level can promote faster economic growth.

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