More Laws, More Growth? Evidence from U.S. States

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Abstract

This paper analyzes the conditions under which more detailed legislation contributes to economic growth. In the context of U.S. states, we apply natural language processing tools to measure legislative flows for the years 1965-2012. We implement a novel shift-share design for text data, where the instrument for legislation is leave-one-out legal-topic flows interacted with pre-treatment legal-topic shares. We find that at the margin, higher legislative detail causes more economic growth. Motivated by an incomplete-contracts model of legislative detail, we test and find that the effect is driven by contingent clauses, that the effect is concave in the pre-existing level of detail, and that the effect size is increasing with economic policy uncertainty.

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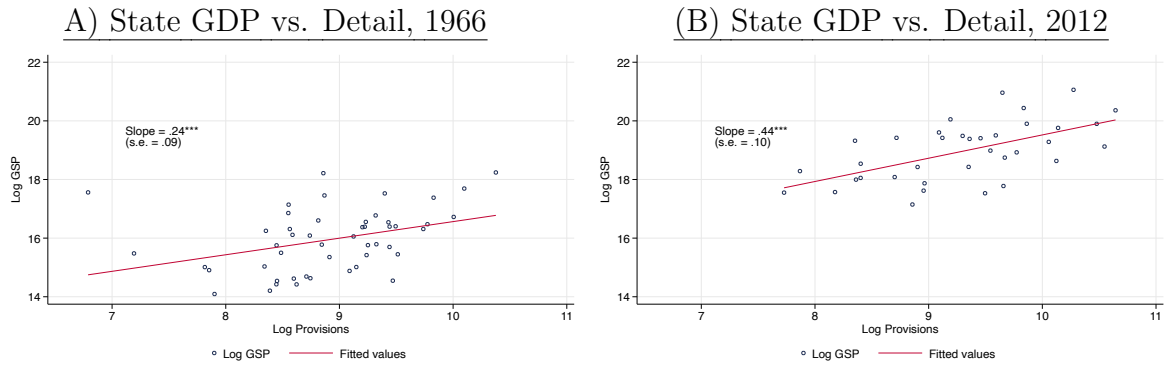
Figure 1: State GDP and Legislative Detail, 1966 and 2012



Notes. Scatter-plots for the relationship between (log) provisions and (log) state GDP at the beginning of our time period (1966) and the end (2012).

1 Introduction

In the cross section, states with larger, more complex legal systems also tend to have larger, more productive economies. The correlation between statute detail and GDP in U.S. states, illustrated in Figure 1 Panels A and B, provides a clear example of this empirical regularity. A key question is whether these correlations reflect causal links.

While a larger economy could lead to more detail mechanically (as, for example, more industries need to be regulated), it could also be that more legislation (if well-designed) could also cause economic growth. Consider the introduction of detailed property rights protections, and establishment of the rule of law (Dam, 2007). These institutions could help markets run more efficiently, encourage investment, and increase growth. On the other hand, excessive regulation could hinder economic growth (Niskanen, 1971, Botero et al., 2004). Hence, even in an ideal world of benevolent legislators, one could postulate the existence of an optimal level of regulatory complexity given the current state of the economy, where moving toward the optimum from either side would increase growth.

Taking inspiration from the literature on endogenously incomplete contracts, this paper offers a set of predictions about when and where the marginal increments in legislative detail can have positive effects on growth. Theoretically, we view legislation drafting as contract writing by a benevolent principal, who has to choose the level of completeness given the marginal benefit and the writing costs. The main prediction of the theory based on Battigalli and Maggi (2002, 2008) framework is that when legislative

details on an issue or sector increase through an increase in contingent clauses, then the effects of such an increase in legislative details is positive for growth. Second, the model predicts that the effect should be concave, in the sense that legislation would have a larger effect starting from a low-detail baseline. Third, the effect is predicted to be larger when there is higher economic uncertainty (and hence more contingencies are needed).

We take these predictions to data in the context of legislation and economic output in U.S. states for the years 1965 through 2012. For each state and biennium, we produce a measure of legislative detail from the text of state laws. The measure draws on recently developed methods in computational linguistics to detect legally relevant requirements from legislation (Vannoni et al., 2019), extracting more signal than coarser measures based on word or character counts. Further, we use a topic model to measure the allocation of provisions across legal categories (Blei et al., 2003).

Our empirical strategy is a shift-share instrumental variables design, based on Bartik (1994), that isolates exogenous variation in legislative detail. Analogous with standard shift-share instruments that use *sector-specific economic flows* interacted with *pre-treatment sector shares*, we construct our instrument using *topic-specific legislative flows* interacted with *pre-treatment topic shares*. The exclusion restriction is based on the orthogonality of shifters: we assume that common (e.g. technological) factors across states drive them to legislate on a topic and these factors are unrelated to economic growth. In other words, we assume that topic-specific legislative flows are exogenous, in line with recent econometric work by Borusyak and Jaravel (2017) and Adao et al. (2019). Our design passes a number of checks recently developed by econometricians for probing the exogeneity of shift-share instruments (Goldsmith-Pinkham et al., 2020, Borusyak and Jaravel, 2017, Adao et al., 2019).

Our main result is that more state-level legislation tends to boost the state economy. This effect is robust to a range of alternative specifications and inclusion of covariates. The growth effect is reflected in both increased wages and increased profits. The effect is driven by fiscal and regulatory policy, rather than by social policy (e.g. crime) or procedural issues (e.g. electoral districting).

Next we test some of the more subtle predictions of the model. First, we find that the effect of detail on growth is driven by contingent clauses (those containing "if", "except", etc.). Second, we find that the effect is stronger for states with a low-detail (low-regulation) baseline (the concavity effect). Third, we extend the approach of Baker et al. (2016) to local newspapers to produce a measure of economic policy uncertainty

by state over time. The effect of detail – and in particular contingent clauses – is stronger during periods of higher economic policy uncertainty.

These results contribute to the centuries-long debate on how government regulation (as opposed to expenditure) is related to the functioning of the economy. One strand of literature – the *positive view* – stresses that a certain level of regulation is likely needed for the economy to grow (Di Vita, 2017). There is legislation needed to regulate externalities, define the tax base, and allocate government expenditures. Some of the economic literature on tax legislation suggests that more legislation could be better for the economy, to the extent that it reduces legal uncertainty (Slemrod, 2005, Graetz, 2007). In this sense, incomplete laws can be understood as incomplete social contracts (Weisbach, 2002, Holtzblatt and McCubbin, 2003, Givati, 2009).

On the other hand, the *negative view* of public choice theory holds that excessive regulation could hinder economic growth (Niskanen, 1971). The main argument is that regulation deters growth by hindering new firm formation, competition, and innovation (Fonseca et al., 2001, Nicoletti and Scarpetta, 2003, Ciccone and Papaioannou, 2007). There are also indirect costs, by disincentivising people to acquire skills (Ciccone and Papaioannou, 2007). Braunerhjelm and Eklund (2014) suggest that the burden of complex taxes has a negative effect on market entry. Kawai et al. (2018) show that failing to consider complementarity in reforms (kludges) can have negative effects. Finally, Foarta and Morelli (2020) provide a more nuanced theoretical explanation identifying conditions under which higher detail could be either good or bad for the economy.

Perhaps reflecting the mixed theoretical results, the empirical literature is also mixed. On the one side, there are a set of papers documenting a positive correlation between legislative detail and growth. For example, Mulligan and Shleifer (2005) show that population is positively related to the detail of legislation in U.S. states (as measured by number of pages). In Japan, Fukumoto (2008) finds that economic growth is associated with higher volume of legislation over time. Kirchner (2012) finds a similar effect for Australia. These papers cannot make strong causal claims, however.

On the other side, a number of papers find some evidence for the negative view. Botero et al. (2004) show in a cross-country c9omparison that regulation of labor is associated with lower labor force participation and higher unemployment. Similarly, Campbell et al. (2010) argue that regulation often does not provide efficient solutions to conflicts and, therefore, does not foster economic development. In a comparison between Italian regions, Di Vita (2017) finds that regulatory complexity is related to lower economic growth and per capita income. Also in Italy, Gratton et al. (2018)

suggest that electoral incentives may create excessive reformism and deterioration of the quality of legislation, which negatively impacts economic growth.

Our paper builds on this literature in a number of ways. First, we leverage text as data in a new way, employing a linguistically motivated measure of legal detail rather than simple word counts or page counts. Second, we use exogenous variation in legislative detail, so that our estimates have a causal interpretation. Third, we test a set of more subtle theoretical predictions that provide support for an incomplete contracts model of legislating, in particular highlighting the importance of contingency and economic uncertainty.

The paper is organized as follows: Section 2 explains how the theory of endogenous incomplete contracts generates the four main hypotheses that we test in this paper. Sections 3, 4, and 5 describe the data, text analysis methods, and empirical approach, respectively. Section 6 reports the main results and robustness checks on detail and growth, while Section 7 explores the more subtle theoretical predictions on contingency, concavity, and uncertainty. Section 8 concludes.

2 Legislating as Incomplete Contracting

In this section we describe how the writing costs approach of Battigalli and Maggi (2002, 2008) can be adapted to the legislative process in order to derive a set of hypotheses on the causes and consequences of legislative detail. We start by using the logic of Battigalli and Maggi (2002) to describe the law as an incomplete contract; we will then describe how some insights can also be derived from Battigalli and Maggi (2008), where the focus is not on the degree of completeness of a law but on the type of clauses (contingent or spot) and on their evolution over time. We will derive from this framework a set of hypotheses that can be tested using our proposed methodology.

2.1 What can we learn from the writing costs approach

A law can be viewed as an incomplete contract between the legislator (the principal) and the citizens (agents), with an efficiency objective. Incompleteness can take the form of rigidity (non-contingent clauses) or discretion (empty clauses). The optimal degree of incompleteness depends on writing costs, that could be the following: the cost of figuring out the relevant contingencies and obligations, the cost of thinking how to describe them, the cost of time needed to write the law. Thus these are all costs

related to the details and precision of the language of the law.

The language of the law consists of primitive sentences that describe (1) elementary events and (2) elementary actions, plus logical connectives (e.g., "not," "and," "or"). This language can be used to describe state- dependent constraints on behavior, or in other words, a correspondence from states to allowable behaviors. Each primitive sentence has a cost and the total cost of writing the contract is a function of the costs of its primitive sentences about events and actions . It follows naturally that contingent clauses are more costly than non-contingent clauses.

A contingency is a formula about the environment, i.e., could include different events with different logical connectives, so a contingency could be event 1 or event 2, and another contingency could be event 1 and 3. An instruction is a formula of behavior, i.e. a set of actions with some logical connectives, like take action 1 and or 2.[1] Omitting from the text of a law an elementary sentence about the possible events or situations that could occur saves on the cost of describing contingencies, but makes the contract rigid. Omitting from the contract an elementary action saves on the cost of describing behavior, but gives discretion to the agent.

Adjusting Battigalli and Maggi (2002) main characterization result about the optimal contract to our context, we can informally restate their proposition 1 saying that an optimal law should have contingent clauses for the most important decisions regulated by such a law, while less important decisions can be regulated by rigid or non-contingent clauses and the least important decisions can be left to discretion.

In more uncertain environments the optimal law (proposition 2(II) in Battigalli and Maggi (2002)) contains more contingent clauses, fewer rigid clauses, and leaves more discretion to the agent. This is intuitive: when uncertainty is higher the efficiency cost of ignoring low-probability events and writing rigid clauses is higher, hence the number of rigid clauses is lower. Moreover, when uncertainty is higher, both contingent clauses and missing clauses increase in number.

While in the above summary of the static Battigalli and Maggi (2002) model the states of the world without a precise instruction are described as cases of agents' discretion, Battigalli and Maggi (2008) allow such discretion cases to be regulated by informal contracts or spot clauses – this becomes a possibility because of repeated play.

[1] An important assumption in the framework in Battigalli and Maggi (2002) is that the language just described is common-knowledge for the parties and the courts, and hence states of the world and actions are perfectly verifiable by courts. This ensures that there are no problems of ambiguous interpretation of the law in this efficiency framework.

When the cost of describing contingencies is low relative to the cost of (re-)negotiating actions after each unregulated contingency, then contingent clauses are optimal to begin with; a spot approach is optimal when this relative cost is high; and an enrichment approach (where when a new unregulated contingency occurs it induces an enrichment of the contingent clauses in the law) may be optimal when this relative cost takes intermediate values.

2.2 Deriving testable predictions

In this section we use the framework described above to derive a set of testable predictions.

Completeness. The first aggregate prediction coming out of the optimal contract framework is that if more legislative details are added by a benevolent principal, it must be because they are beneficial in the context where they are added. In other words:[2]

H0: Given the benevolence assumption on legislators, the greater the completeness of law, the better the economic outcomes to be expected – the **completeness** hypothesis.

Contingency. The second prediction that we can derive from the Battigalli and Maggi (2002, 2008) framework related to contingent clauses. Suppose that for each issue or topic there are plenty of contingencies that one could potentially differentiate, but each contingency requires a constant marginal writing cost. Even if the marginal writing cost of an extra contingency is constant, the marginal benefit depends on many things that could vary a lot from state to state and from year to year, as well as some common component that relates to technological changes or other exogenous transformations of the topic to be regulated.[3] As a result, given a fixed marginal writing cost

[2]Given benevolence and rationality of the designer, in contract theory we take it for granted that in the absence of costs of describing contingencies, a complete contract specifying what would happen in all possible realizations of the states of the world would be better than leaving the contract incomplete (Dye, 1985).

[3]For example, in a state where all employees are in one or two sectors without many differentiations of skills, the marginal benefit from new contingent statements related to different sectors, seniority, education or other observables would be low. Hence, that state might have relatively simple labor laws and tax laws with non-contingent statements. On the other hand, in a state where skill differentiation

but wide variation in the benefit function, the state legislators choose different levels of contingent legislative detail across states.

The optimal level of completeness of contracts is increasing in the marginal benefit of adding contingencies. Hence we should expect the relation between contingent clauses and growth to be stronger than for other clauses. That is, clauses along these lines: "if a worker has such characteristics... then a firm with such other characteristics could employ him or her with a special tax treatment, transfer, labor law relaxation, etc..." should be expected to have a positive effect given that it is more costly to write and hence a rational legislator who has decided to introduce it must have anticipated a high marginal benefit from it. The testable hypothesis that corresponds to this reasoning is:

H1: The changes in legislative detail that most affect the growth prospects of a state are additional contingent clauses – the **contingency** hypothesis.

If at some point in time comes a shock such as the advent of internet and new exogenous elementary events and actions arise, the existing legislation is not optimal to maximize the surplus. As a result, legislators write more clauses (as we have more events and actions) and more specifically write more contingent clauses (as there are more combinations). Now clauses like: "if there is good internet connection, the worker shall work from home" could be added and be beneficial. We expect that contingent statements to matter most for economic performance. A side prediction would be that contingent clauses would be even more beneficial in states with greater economic complexity – more sectors, more levels, more segmentation, more strategic incentives to be given, etc.

Concavity. We now turn to a third implication of the Battigalli and Maggi (2002) framework. Assume for simplicity that each contingent clause has the same cost c. Thus, a law that includes l contingent clauses has cost cl. The state j's **marginal** benefit from adding a contingent clause is a function $B(l, t, w_j)$, where $t \in R_+$ is a parameter capturing a common factor (like technological change) and $w_j \in R_+$ is a state specific parameter capturing the degree of complexity of the economy to be regulated in state j. Let $\frac{\partial B}{\partial t} > 0$, $\frac{\partial B}{\partial w_j} > 0$, and $\frac{\partial B}{\partial l} < 0$ (the latter capturing a concavity assumption).[4]

matters, there is a higher marginal benefit from more clauses as, for example, the planner might find it important to give incentives to workers to switch from one sector to another.

[4]Note also that in an optimal contracting framework with constant marginal writing cost of contingent clauses, such contingent clauses should be added in order of importance -- another source of

A state prescribing a rigid clause to always be at the office from 9 to 5 could be optimal in a state j with a low w_j while a state k with a greater $w_k > w_j$ may display already a contingent clause that working from home is possible when some condition on traffic or weather is met. In other words, state k with high w_k may optimally have $l_k^* > l_j^*$. Suppose that this is the case at time 0 with common technology t_0. Consider an exogenous shock at time 1 determining $t_1 > t_0$ (like the invention of internet), such that $l_k^*(t_1) = l_k^*(t_0) + 1$ and $l_j^*(t_1) = l_j^*(t_0) + 1$.

It follows naturally, given the concavity assumption, that the effects must be bigger in state j. When a change in t makes it convenient for both states to add a contingent clause like "if there is good internet connection, the worker shall work from home" then this addition benefits relatively more state j.

H2: An exogenous increase in legislative completeness will have a greater growth differential in the states with lower initial level of legislative detail – the **concavity** hypothesis.

Uncertainty. The fourth implication of the Battigalli and Maggi (2002) framework concerns the role of uncertainty. That is, it is plausible that the marginal benefit of contingent clauses is higher in states that are exposed to greater uncertainty. The more uncertain are the relevant situations, the more important should be to account for different possible contingencies as far as important issues are concerned, whereas more discretion could be allowed in low importance issues. The functional form for the marginal benefit of an additional contingent clause could be enriched by adding an additional parameter $u_j \in R_+$ capturing the degree of uncertainty in state j. The simple hypothesis to be tested is that indeed the marginal benefit of more contingent clauses when u_j is higher is positive.

H3: The greater or the more frequent the sources of **uncertainty** in a state, the greater will be the growth benefit from higher legislative detail, and especially from more contingent clauses.

We will be able to test this hypothesis only at the state aggregate level, whereas testing it in particular on the high importance issues when such issues suffer from greater uncertainty would require some non trivial agreement on importance ranking, something that we could study in future extensions of this research.

concavity.

Table 1: Summary Statistics

Variables	(1) Obs	(2) Mean	(3) SD	(4) Min	(5) Max
Economic Output Variables					
Log Real GSP per Capita	1,250	3.652	0.281	2.803	4.844
Log Real GSP	1,250	17.79	1.436	14.09	21.54
Log Real GSP per Capita Growth	1,249	0.031	0.050	-0.174	0.332
Log Real GSP Growth	1,250	0.134	0.070	-0.087	0.665
Log Employment Growth	823	0.057	0.064	-0.151	0.930
Log Number of Establishments Growth	823	0.045	0.058	-0.146	0.409
Log Establishment Profit Growth	550	0.163	0.109	-0.403	0.818
Statute Text Variables					
Log Provisions (Legislative Detail)	1,183	9.211	0.887	2.996	11.42
Log Contingent Provisions	1,183	7.528	0.983	0.405	9.859
Log Non-Contingent Provisions	1,183	8.908	0.893	2.890	11.03
Covariates					
Log Population	1,250	14.94	1.029	12.51	17.47
Democratic Control	1,127	1.802	1.057	0	3
Log Income	1,250	3.479	0.267	2.563	4.144
Log Govt. Expenditure	1,250	15.57	1.471	11.89	19.46
Log Legis. Expenditure	1,250	9.410	1.384	5.176	12.73

Notes. Summary statistics for the main variables. The different number of observations is due to the availability of different years in the different datasets/sources we use.

3 Data Sources

This section describes the data and provides summary statistics. The variables can be roughly divided into three categories: data on economic output and growth, statute text data and legislative detail, and control variables. The main summary statistics are reported in Table 1.

The dataset for our empirical analysis ranges from 1965 through 2012. This period is determined by the beginning of the economic growth variables (in 1965) and the ending of the legislative text variables (in 2012). The data are constructed by biennium (two-year periods), since many states publish their compiled statutes once every two years.

Economic Activity. We have a rich array of variables on the economic conditions by year in each of the 50 states. These data are assembled from the Bureau of Economic Analysis Regional Accounts, County Business Patterns, Klarner (2013), and Ujhelyi (2014).

As our empirical analysis looks at how legal flows impact economic growth, the key variable Y_{st} is local growth, measured by the change in log per capita Gross State Product (GSP) in state s between year $t-2$ and year t (as the data are at the biennium level). Appendix Figure A.1 shows the evolution of this variable over the time period of the data. The data on the numerator (total real GSP) and the denominator (total population) will also be used separately. All economic variables denominated in dollars are deflated to 2007 values using the state-level CPI.

We have a number of additional measures of economic activity. On the worker side, we have labor income and employment. On the firm side, we have number of establishments and profits.

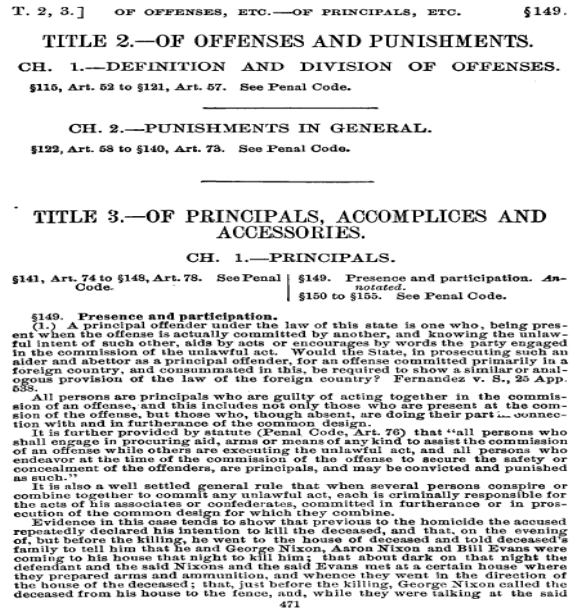
State Session Laws Corpus. The dataset on legislation includes the full text of U.S. state session laws. This corpus consists of the statutes enacted by each state legislature during each session. The statutes modify the text in the state's compiled legislative code. They therefore can include new laws, revisions to existing laws, and repeal of existing laws. As mentioned, the laws are published annually or biennially; to ensure consistency, the dataset is built biennially.

The text from this corpus is produced from optical character recognition (OCR) applied to printed laws. From inspecting samples, the OCR is high quality. Figure 2 shows the scanned copy of a page from a statute enacted in the Texas Legislature for the 1889 session. As can be seen, although the statute is old, the quality of the digitized version is quite good.

Still, as with any historical digitized corpus there are a significant number of OCR errors. At a minimum, OCR errors add measurement error to the legislative detail measure. This is not a major problem for our empirical analysis as long as the OCR error rate is not correlated with either the outcome or the instrument for detail. If it is classical measurement error, it would attenuate estimates. To investigate this, we computed a proxy for OCR as the misspelling rate per word. We found that our instrument is not correlated with the misspelling rate (Appendix Figure A.11).[5]

[5]In addition, controlling for OCR error rate in our main results does not change them.

Figure 2: State Session Laws Corpus



T. 2, 3.] OF OFFENSES, ETC.—OF PRINCIPALS, ETC. §149.

TITLE 2.—OF OFFENSES AND PUNISHMENTS.

CH. 1.—DEFINITION AND DIVISION OF OFFENSES.

§115, Art. 52 to §121, Art. 57. See Penal Code.

CH. 2.—PUNISHMENTS IN GENERAL.

§122, Art. 58 to §140, Art. 73. See Penal Code.

TITLE 3.—OF PRINCIPALS, ACCOMPLICES AND ACCESSORIES.

CH. 1.—PRINCIPALS.

§141, Art. 74 to §148, Art. 78. See Penal Code. | §149. Presence and participation. *Annotated.* | §150 to §155. See Penal Code.

§149. **Presence and participation.**
(1.) A principal offender under the law of this state is one who, being present when the offense is actually committed by another, and knowing the unlawful intent of such other, aids by acts or encourages by words the party engaged in the commission of the unlawful act. Would the State, in prosecuting such an aider and abettor as a principal offender, for an offense committed primarily in a foreign country, and consummated in this, be required to show a similar or analogous provision of the law of the foreign country? Fernandez v. S., 25 App. 538.
All persons are principals who are guilty of acting together in the commission of an offense, and this includes not only those who are present at the comsion of the offense, but those who, though absent, are doing their part i... connection with and in furtherance of the common design.
It is further provided by statute (Penal Code, Art. 76) that "all persons who shall engage in procuring aid, arms or means of any kind to assist the commission of an offense while others are executing the unlawful act, and all persons who endeavor at the time of the commission of the offense to secure the safety or concealment of the offenders, are principals, and may be convicted and punished as such."
It is also a well settled general rule that when several persons conspire or combine together to commit any unlawful act, each is criminally responsible for the acts of his associates or confederates, committed in furtherance or in prosecution of the common design for which they combine.
Evidence in this case tends to show that previous to the homicide the accused repeatedly declared his intention to kill the deceased, and that, on the evening of, but before the killing, he went to the house of deceased and told deceased's family to tell him that he and George Nixon, Aaron Nixon and Bill Evans were coming to his house that night to kill him; that about dark on that night the defendant and the said Nixons and the said Evans met at a certain house where they prepared arms and ammunition, and whence they went in the direction of the house of the deceased; that, just before the killing, George Nixon called the deceased from his house to the fence, and, while they were talking at the said
471

T. 2, 3.] OF OFFENSES, ETC.-OF PRINCIPALS, ETC.

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1 §150 to §155. See Penal Code.
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471

Notes. Scanned image and associated OCR for example page from State Session Laws corpus.

Demographics. We link the data on economics and law to demographic data the state level. Besides population, we use census information on the age distribution, the fraction of urban population, and the share of foreign born population.

State government finances. We use a set of data on local government revenues and expenditures from the state government finances census. These include total government expenditures (in 1000s current dollars), and legislative expenditures (in 1000s current dollars).

Politics. Next, we use measures of state political conditions. In particular, we have a measure of Democratic Control, which is the number of governing bodies (lower chamber, upper chamber, and governor) controlled by Democrats. This ranges from zero to three.

Local economic uncertainty. Finally, we have information on state-year-level economic uncertainty constructed from the text of newspaper articles. For this purpose, we use the searchable local newspaper archive `newspapers.com`, which can programmatically provide counts by state and year for articles meeting search criteria. Following

Baker et al. (2016), we count the number of articles mentioning the phrase 'economic uncertainty' in a state in a given biennium. We construct a frequency by taking this count divided by the total number of news articles.

4 Text Analysis Methods

This section summarizes our methods for extracting useful measures from the statute texts.

4.1 Measuring Legislative Detail

Using the digitized text of the state session laws, we start by segmenting the text for each biennium into statutes. Roughly speaking, a "statute" is a singular, coherent enacted bill or policy. It usually corresponds to a "chapter" in the compiled legislative code, which is the second level of organization beneath titles. Appendix Figure A.1 Panel A shows the distribution of the number of statutes by biennium. Panel B shows the distribution of the number of words per statute. Panels C and D respectively show the time series for the number of statutes, and number of words per statute, over time.

Next, the statutes are segmented into sentences using a sentence tokenizer. For each sentence, we extract legally relevant statements following the method in Vannoni et al. (2019) and Ash et al. (2020). The method works as follows, with more detail provided in Appendix B.2.

We apply a syntactic dependency parser to construct data on the grammatical relations among words in each sentence (Dell'Orletta et al., 2012, Montemagni and Venturi, 2013), as illustrated in Appendix Figure A.5. The dependency parse identifies the main verb in a sentence segment, along with the associated subject, object, helping verb, and information on negation.

To extract legally relevant statements, we define a set of legislative provision types (also called legal frames), including obligations, definitions, modifications, and so on (Soria et al., 2007, Saias and Quaresma, 2004). We extract dependency tags associated with each legislative provision type (van Engers et al., 2004, Lame, 2003); for instance, a constraint is characterized by three potential structures: a negative structure with a modal, such as 'the Agent shall not'; a negative structure with a permission verb, such as 'the Agent is not allowed'; or a positive structure with a constraint verb, such as 'the Agent is prohibited from'. The set of provision types, with tagging rules, are listed in

Appendix Table A.2. Vannoni et al. (2019) and Ash et al. (2020) use this method to count provisions across different agent types. Here, the aim is less targeted – we count the number of legal provisions by state and over time.

Our measure of legislative detail W_{st} is the number of legal provisions counted in the session laws for a state at biennium t. To assess proportional changes in detail, we use the log of the counts. The evolution of this measure, by year, is illustrated in Figure A.1. Counting provisions should provide a cleaner measure of the flow of legal requirements than would be obtained by a coarser measure, such as word counts or page counts. The latter type of measure would be noisier because they include a lot of non-legislative or otherwise less informative content. Vannoni et al. (2019) provide some validation against human annotations that our parser-based measure does a better job than simpler measures in identifying legally relevant statements. Appendix Figure A.3 shows that provision counts and word counts are correlated. In Appendix Table A.11 we explore variations on our analysis using word counts or page counts.

As shown above in Figure 1 and in in the binned scatterplots in Appendix Figure A.2, growth and detail are positively related, both across-states in the cross section and within-state over time. While growth in the economy and growth in laws tend to co-occur, they could do so for many non-causal reasons. Our empirical strategy is designed to address these confounders.

4.2 Allocating Laws to Topics

An essential ingredient in our analysis is to assign statutes to topics. To learn and assign topics, we apply the Latent Dirichlet Allocation (LDA) model described in Blei et al. (2003). This algorithm, by now well known in the literature on text data in political economy (Grimmer and Stewart, 2013, Hansen et al., 2018), assumes that every document is a distribution over topics, which in turn is a distribution over words and phrases. A document is generated by drawing topic shares, and then the words of the document are drawn from those topics.

We trained LDA on our corpus at the statute level using the Mallet wrapper from the Python gensim package. The main tunable hyperparameter in LDA is the number of topics K. Starting with $K = 6$ topics, we increased the number by multiples of 6 (12, 18, ..., etc) to find the topic count that maximized the topic coherence score. This score was maximized at $K = 42$. We also inspected the topics subjectively, and we agreed that the specification with $K = 18$ topics was a good balance for a relatively small

number of intuitive, coherent topics. After producing our main empirical results for all topic counts $K \in \{6, 12, ..., 48\}$, we found that the instrument constructed with $K = 18$ topics (more details below) generates the most consistent estimate across specifications with different sets of predetermined covariates. Therefore, we have two preferred LDA models: 18 topics and 42 topics. For our main results, however, the topic number choice is not important. In Appendix Table A.10we show consistent results for all LDA models produced ($K \in \{6, 12, ..., 48\}$).

The baseline specification for the main text uses the LDA model with $K = 18$ topics. The list of 18 topics is reported in Table 2, sorted by most to least frequent in the state session laws corpus. The model produces clearly interpretable topics for vehicle regulation, licensing, courts, project funding, childcare services, trusts and estates, employment law, taxes, land regulation, retirement regulation, etc. These are the types of legal policy areas that one would expect to arise in the business of U.S. state government.

The 42-topic LDA model is mainly used to flesh out our results by policy type. These more granular topics were more easy than the 18-topic model to divide into broader policy areas: economic regulation, fiscal policy, social regulation, and procedural. To make this assignment to policy groups, all three of the co-authors annotated the topics and we assigned the majority annotation, with some discussion under disagreement. The list of topics, with broader category assignments, is reported in Appendix Table A.3. Appendix Figure A.6 shows the legislation shares across these four categories over time.

Beyond these word lists, we further interpret the topics by producing lists of sentences that are most distinctive of each topic. Lists of these sentences are provided in Appendix B.3. Overall they are quite intuitive and show that meaningful policy categories are being recovered by LDA.

Using the trained models, we assign to every statute a distribution over topics based on the words and phrases in that statute. For each state-biennium, the number of provisions by topic is computed by the sum of provisions in that state-biennium's statutes, weighted by the topic share of each statute. Formally, let L_{st} be the set of laws in state s time t. Each statute $i \in L_{st}$ has a provision count w_i and a distribution over topics $\vec{v} \ni v_i^k$, $\forall k \in \{1, ..., K\}$, where $v_i^k \geq 0$ and $\sum_k v_i^k = 1$. Then define legislative

Table 2: List of Topics, 18-Topic Specification

Label	Frequency	Most Associated Words
Courts	0.0724	court judgment attorney case appeal civil petition sheriff trial circuit_court district_court such_person complaint counsel brought circuit warrant paid
Pensions	0.0653	paid benefit rate payment equal death age credit pay total life pension premium calendar_year loss account case per_cent event membership excess maximum
Local Projects	0.0645	development local project budget government cost grant research center local_government data transfer governor is_the_intent develop urban review biennium
Procurement	0.0621	director contract work review civil labor contractor attorney_general bureau final perform audit receipt status exempt panel government firm bid prepared
Elections	0.0612	district town petition charter special ballot mayor voter township precinct cast referendum census elector case town_council said_district such_district
Banking	0.0604	loan trust bank agent partnership institution foreign stock mortgage deposit surplus interest merger credit_union partner case credit gift branch transact
Licensing	0.0593	license fee dealer sale food sold holder sell valid fish agent distributor milk liquor product such_license livestock game card retail misdemeanor fine
Real Estate	0.0576	real interest sale owner contract claim lien payment transfer instrument seller holder issuer debtor claimant buyer pay broker settlement receipt money
Bonds	0.0574	interest bond payment commonwealth cost sale paid pay project power thereon sold debt pledge local_law event hereof proper said_board real port sell therefrom
Expenditures	0.0569	fund account money paid special pay tile payment transfer for_the_fiscal_year excess trust_fund so_much_thereof deposit state_general_fund auditor tie
Bureaucracy	0.0551	governor council government chief fire appoint personnel compact conflict perform shall_consist invalid parish successor volunteer membership head travel
Healthcare	0.0546	health care treatment health_care physician home human patient mental mental_health drug social condition public_health medicaid dental client review institution
Child Custody	0.0535	child court minor children parent age probation crime victim parole guardian adult petition placement youth case social legal child_support obligor home
Taxes	0.0522	tax paid gross credit return net rate exempt assessor case refund equal sale total calendar_year payment fuel portion sold price retail zone pay such_tax
Land & Energy	0.0512	land water owner control site air solid gas tenant oil park airport forest coal plant environment prevent underground power soil portion landlord condition
Education	0.0474	school school_district state_board district student institution higher teacher special aid pupil children school_year tuition high_school school_board
Traffic 1	0.0423	motor highway driver owner traffic plate test vessel accident weight special sect trailer railroad state_highway stricken feet fine alcohol aircraft carrier
Traffic 2	0.0267	street road feet island river run tract team great highway township center_line park center corner lake beach more_or_less san honor creek high_school

Notes. This table shows the 18 topics, along with their frequency and the most associated keywords. As it can be seen, the distribution is rather dispersed and no topic is predominant. The most frequent topics across states and years are Courts, Pension and Local Projects, whereas the least frequent are Education, Traffic 1 and Traffic 2.

flows for topic k in state s during t as

$$W_{st}^k = \sum_{i \in L_{st}} v_i^k w_i.$$

This process results in a dataset with the number of provisions by topic for the legislation of a state in a biennium.

4.3 Measuring Contingency in Legal Language

We measure contingency using a simple lexicon-based approach. We consulted several lists developed by linguists to indicate contingency. We then searched for examples in the statutes to check that these words almost always indicated contingency . After this inspection process, we settled on a relatively short list of words that were distinctive of contingent clauses. Formally, a provision is contingent if one of the following words (or phrases) appears in the same sentence: {*if*, *in case*, *where*, *could*, *unless*, *should*, *would*, *as long as, so long as*, *provided that*, *otherwise*, *supposing*}.

Let W_{st}^C be the number of contingent provisions in the statutes from state s in year t. Let $W_{st}^N = W_{st} - W_{st}^C$ be the number of non-contingent provisions. Following the same procedure as in Subsection 4.2, we also compute topic-specific counts of contingent and non-contingent provisions by state-biennium.

Summary statistics are reported in Appendix B.4. About 20.86% percent of clauses are contingent. Appendix Figure A.7 shows the time series for share of contingencies by the four policy categories. Economic-regulation clauses have consistently had the highest degree of contingency, which could reflect that policy specificity is most important for that category. An interpretation of this in line with Battigalli and Maggi (2002) is that economic regulation topics are the most important, given they deserve the specification of state-contingent actions. The contingency share of social-regulation clauses has increased over time

5 Empirical Approach

5.1 Linear Regression Specification

Our dataset is at the state-biennium level, for each state s and biennium t. The main research objective is to test whether legislative detail W_{st} increases or decreases

economic growth Y_{st}. More formally, let W_{st} equal the number of legal provisions enacted, and $\Delta \log Y_{st}$ equal the log change in real per capita GDP, in s during t. We assume a linear model

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho \log W_{st} + X'_{st}\beta + \varepsilon_{st} \tag{1}$$

where α_s includes state fixed effects, α_t includes time (biennium) fixed effects, and $\alpha_s \cdot t$ includes state-specific time trends. When estimated by ordinary least squares (OLS), this is a standard two-way fixed effects model. X_{st} includes a set of additional covariates, for example pre-period state characteristics interacted with the time fixed effects, for use in robustness specifications.

Under strong identification assumptions, OLS estimates for ρ would procure a causal effect of legislative detail on growth. The key assumption is that there are no unobserved factors (time-varying at the state level) correlated with both $\log W_{st}$ and $\Delta \log Y_{st}$. This assumption is unrealistic, given that there could be unobserved shocks (e.g., the rise of a new industry) that affect both economic output and legislative output.

Thus, we consider the OLS estimates for ρ as descriptive. As shown in the binned scatterplots in Appendix Figure A.2, growth and detail are positively related, both across-states in the cross section and within-state over time. While growth in the economy and growth in laws tend to co-occur, they could do so for many non-causal reasons. Our empirical strategy is designed to address these confounders.

5.2 Shift-Share Instrument for Legislative Detail

Given the likelihood of confounders in the baseline OLS model (1), we take an instrumental variables approach to obtain causal estimates. We use a shift-share instrument constructed from the LDA topic shares (described in Section 4.2 above), to be fully enumerated here. In the spirit of the theoretical model from Section 2, our shock to detail is driven by a reduction in costs of legislating across topics. The cost reduction comes from national trends in legislating, combined with differences in pre-existing detail across topics in each state.

The shift-share instrumental-variables design is often attributed to Bartik (1991, 1994) but was popularized by Blanchard and Katz (1992). The original application of the approach was meant to address the endogeneity between employment growth and economic growth; that is, more economically prosperous regions tend to attract more

labor. To address this problem, one can instrument local employment growth with the interaction between pre-treatment local employment shares by sector and national employment growth rates by sector. The Bartik approach therefore isolates changes in employment growth due to these labor demand shocks (rather than due to local supply side responses).

While the use in economic growth and employment is still the classic example, more recent applications include migration effects on labor markets Card (2001)(Basso and Peri, 2015), imports and economic growth Autor et al. (2013)(Autor et al., 2016b), market size and drug innovation (Acemoglu and Linn, 2004), small business lending and economic growth (Greenstone et al., 2020), effects of democracy on growth (Acemoglu et al., 2019), and effects of the China shock on nationalism (Colantone and Stanig, 2018) and populism Autor et al. (2016a). In tandem with this diversity of applications, a recent and active literature in econometrics has produced useful results and guidance on how to use these estimators (Goldsmith-Pinkham et al., 2020, Jaeger et al., 2018, Borusyak and Jaravel, 2017, Adao et al., 2019).

To link our setting to those in more traditional shift-share designs, let's conceive the flow of legislative provisions as analogous to the flow of workers or flow of migrants. Analogous to economic sectors (which supply workers) and origin countries (which supply migrants), we have legal policy topics (which supply legislative text). The instrument consists of a "share" factor and a "shift" factor, to be described in turn. As above, W_{st} represents the total number of legislative statements in state s at biennium t, while W_{st}^k represent the number of statements on topic k in s at t.

The local "shares" are a state's pre-period stock of legislative detail on each topic, analogous to pre-period employment shares across sectors, or pre-period immigrant population shares across origin countries. Formally, we construct the pre-treatment legislative topic shares as the average of topic shares over the decade prior to our analysis (1955-1964), represented as period zero: $\frac{W_{s0}^k}{W_{s0}}$.[6]

The global "shifter" in our case is nationwide growth in topic-specific detail, analogous to nationwide growth in employment in a particular sector, or growth in immigration from a particular origin country. Formally, this is the leave-one-out average log change in legislation to topic k in other states, $\frac{\sum_{r \neq s} \Delta \log W_{rt}^k}{49}$, where r indexes the other

[6]We include all topics in constructing the instrument, as recommended by Borusyak and Jaravel (2017), relative to a situation where only a subset of shares is used for the instrument (as in Autor et al., 2016b). Moreover, the use of pre-treatment shares is advisable in situations where shocks are serially correlated and shares are affected by lagged shocks.

49 states. Borusyak and Jaravel (2017) note that the assumptions for identification are relaxed with the leave-one-out specification for the shifter.

Now we combine the "shifters" and the "shares." The instrument for legislative detail is the weighted sum, by topic, of the leave-one-out average legislative flow on that topic in other states, multiplied by this state's pre-treatment topic share:

$$Z_{st} = \sum_{k=1}^{K} \underbrace{\frac{W_{s0}^{k}}{W_{s0}}}_{\text{shares}} \underbrace{\sum_{r \neq s} \frac{\Delta \log W_{rt}^{k}}{49}}_{\text{shifts}} . \tag{2}$$

To assist interpretability of the first-stage and reduced-form estimates, Z_{st} is standardized to mean zero and variance one. The first stage equation for legislative detail is

$$\log W_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi Z_{st} + X_{st}' \beta + \eta_{st} \tag{3}$$

where Z_{st} is given by (2). The other items are the same as Equation (1). Reduced form estimates are produced by

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi Z_{st} + X_{st}' \beta + \epsilon_{st}. \tag{4}$$

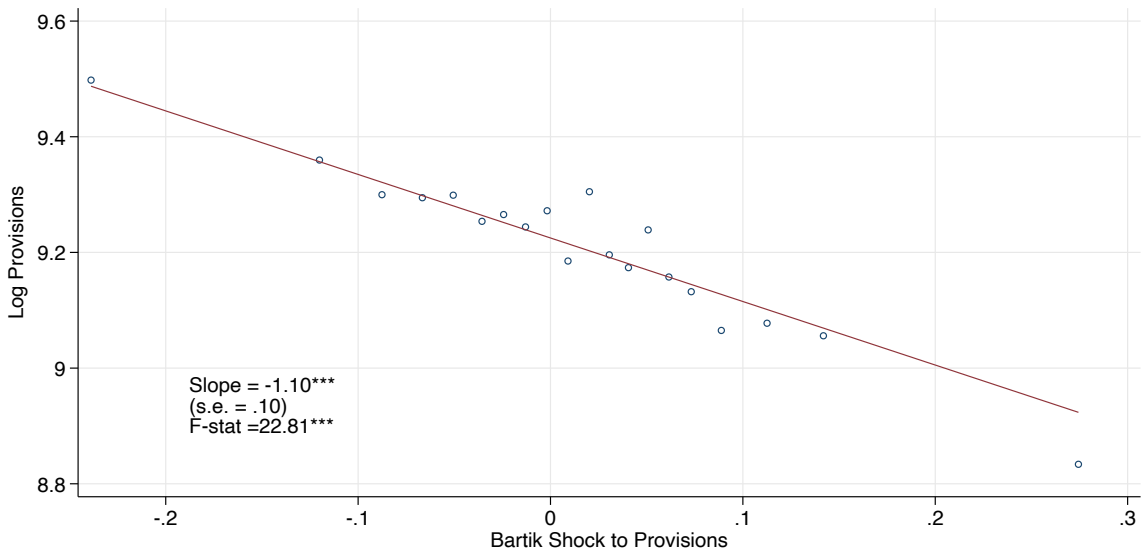
5.3 Instrument Validity

Figure 3 illustrates the first-stage relationship. The first stage statistics are consistent with instrument relevance. The estimate of ψ is statistically significant ($p = .003$). The Kleibergen-Paap first-stage F-statistic in the baseline specification is 22.8.

The first-stage relation between legislative flow and the instrument is negative. The interpretation of our instrument is somewhat different from the standard shift-share instrument for economic shocks. Our interpretation is that when a state had initially low detail on a topic, then it is more likely to increase detail in response to national trends on that topic. This is somewhat intuitive, given that the state can then borrow legislative language at relatively low cost. Consistent with this interpretation, the "shift" term of the instrument is positively correlated with the endogenous regressor $\log W_{st}$, while the "shares" term is negatively correlated (Appendix Figure A.8).

There are two approaches to identification in shift-share designs. In the first approach, one assumes that the pre-period shares are conditionally exogenous (Goldsmith-Pinkham et al., 2020, Jaeger et al., 2018). In this view, the exclusion restriction hinges

Figure 3: First Stage: Impact of Shift-Share Legislative Shock on Legislative Detail



Notes. Binned scatterplot for the first-stage relationship (Equation 3) between the shift-share instrument (horizontal axis) and the log number of provisions (vertical axis). State and year fixed effects absorbed.

on the fact that the shares (normally, sectoral composition, but in our case, topic shares) are as good as randomly assigned conditional on the fixed effects and controls (see Borusyak and Jaravel, 2017). In our case, this assumption could be formally stated as

$$\mathbb{E}\{\frac{W_{s0}^{k}}{W_{s0}} \cdot \epsilon_{st} | \vec{\alpha}_{st}, X_{st}\} = 0, \forall k \tag{5}$$

where $\vec{\alpha}_{st}$ gives the vector of fixed effects. Equation (5) is a relatively strong requirement in most empirical contexts. In our case, this would mean that pre-period legislative topic shares are uncorrelated with subsequent trends in economic growth during the treatment period. This is difficult to justify, since the period legislation could be drafted in preparation for future growth trends. For example, the proportion of legislation on taxes or employment regulation in the 1950s could be correlated with growing more or less quickly in the 1960s or 1970s.

A second approach to identification, taken by Borusyak and Jaravel (2017) and Adao et al. (2019), relies on weaker assumptions. In these frameworks, the exclusion restriction follows from the conditional exogeneity of the current-period shifters, rather than from the pre-treatment shares. No assumption is needed with respect to the pre-treatment shares, and instead this approach assumes that the global shocks are

uncorrelated with the exposure-weighted average of potential outcomes. In the case of Autor et al. (2016b), for example, the identification assumption is that average unobserved determinants of economic growth across states must be unrelated to flows of Chinese imports. With panel data (as in our context), the assumption can be further relaxed. Formally, we have

$$\mathbb{E}\{\sum_{r \neq s} \frac{\Delta \log W_{rt}^{k}}{49} \cdot \epsilon_{st} | \vec{\alpha}_{st}, X_{st}\} = 0, \forall k \tag{6}$$

where the terms are as above. With the inclusion of state and time fixed effects, shocks are allowed to be correlated with exposure-weighted averages of state and time-invariant unobservables, or linearly varying within state given the inclusion of state-time trends (Borusyak and Jaravel, 2017).

In line with Borusyak and Jaravel (2017) and Adao et al. (2019), we take a number of steps to assess the validity of Z_{st} as an instrument for $\log W_{st}$ (see Appendix C). First, to check that the relevance of the shift-share instrument is driven by a majority of topics, we regress the increase in provisions related to a topic in a state on the increase in the total provisions related to that topic in other states and the increase in all legal provisions in that state, for every topic (including state and year fixed effects and clustering standard errors by state). We find that topic growth is statistically significant in the great majority of topics, as shown in Appendix Figure A.9. Second, we use the test for weak instruments, robust to heteroscedasticity, serial correlation, and clustering, proposed by Olea and Pflueger (2013). A rule of thumb for 2SLS is to reject the null hypothesis of a weak instrument when the effective F is greater than 23.1. In our data, the effective F statistic equals 132.794 and we reject the weak instrument null at 5 percent significance. Third, Appendix Table A.4 reports the following placebo test: we regress economic growth on future values of the legislative-growth instruments. The estimates are not statistically significant. Fourth, we run a balance test by regressing the instrument on some potential confounders. Appendix Table A.6 shows the instrument is not correlated with current or lagged values for relevant state characteristics.

Independently, we also show that we can pass the checks proposed by Goldsmith-Pinkham et al. (2020) and Jaeger et al. (2018) in the alternative framework that assumes exogeneity of pre-treatment shares. Appendix Table A.5 shows that the pre-treatment topic shares are uncorrelated with pre-treatment state characteristics. Appendix Figure A.10 shows that pre-treatment topic shares are uncorrelated with subsequent growth

Table 3: First Stage, OLS, and Reduced Form

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Detail		Effect on Real GDP Growth Per Capita			
	FS	FS	OLS	OLS	RF	RF
Legislative Detail			0.0146+	0.0152		
			(0.00832)	(0.0123)		
Instrument (Z_{st})	-1.099**	-1.221**			-0.0200*	-0.0205*
	(0.230)	(0.259)			(0.00883)	(0.00940)
Observations	1,183	1,183	1,182	1,182	1,182	1,182
R-squared	0.813	0.9	0.431	0.446	0.420	0.440
State FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
State-Specific Trends		X		X		X

Notes. Columns 1 and 2 show the estimates for the First Stage (Equation 3). Columns 3 and 4 show the results for OLS estimates of Equation 1). Columns 5 and 6 give the Reduced Form specification (Equation 4), regressing the outcome (growth per capita) directly on the instrument . All specifications include state and biennium fixed effect, with a second column including state-specific trends. All standard errors clustered by state. **p<.01; *p<.05; +p<.1.

trends. These statistics lend support to the 'exogeneity of shares' assumption, which would independently suffice for instrument validity.

6 Main Results: Legislative Detail and Growth

To begin reporting of results, it is straightforward to test the **completeness** hypothesis (**H0**), which suggests that greater detail causes greater growth at the margin. In our empirical model (Equation 1), this is equivalent to $\hat{\rho} > 0$. We produce causal estimates using 2SLS, with Equation (3) as first stage and Equation (1) as second stage.

6.1 Effect of Legislative Detail on Economic Growth

The first results for legislative detail and growth are reported in Table 3. Columns 1 and 2 show OLS estimates for the first stage, illustrating a negative and significant effect of the instrument on detail. In Columns 3 and 4, we see that OLS estimation of (1) is positive, but not robustly significant. Columns 5 and 6 show a significant reduced-form

Table 4: Effect of Legislative Detail on Economic Growth (2SLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Growth Rate Per capita						
Legislative Detail	0.0182*	0.0168+	0.0152*	0.0134+	0.0116+	0.0222*	0.00938+
	(0.00903)	(0.00863)	(0.00704)	(0.00687)	(0.00602)	(0.0106)	(0.00507)
First Stage F-stat	22.86	22.19	23.11	22.92	44.51	19.69	27.30
Observations	1,182	1,182	1,182	1,182	1,134	1,182	1,086
Time FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
State Trends		X					X
Econ Vars × Time			X				X
Sector Shares × Time				X			X
Demog Vars × Time					X		X
Topic Shares						X	X
Lagged Govt Expend							X
Lagged Dep. Var.							X

Notes. Results for the 2SLS model (Second Stage 1) and First Stage 3). All specs include state and biennium fixed effects. Column 2 adds state-specific linear trends. Column 3 adds a set of pre-period economic covariates interacted with biennium effects (initial growth, initial GSP, and initial GSP per capita). Column 4 controls for initial sector shares interacted by biennium, and Column 5 adds demographic characteristics (share of urban, foreign, and population) measured in the pre-treatment period interacted with biennium fixed effects. Column 6 includes topic share controls. Column 7 includes all covariates and adds lagged government expenditures and the lagged dependent variable. Standard errors clustered by state. **p<.01; *p<.05; +p<.1.

effect of the instrument on growth. As previously discussed, the coefficient is negative, reflecting that lower pre-treatment detail on a topic is associated with a positive shock to legislative detail.

Appendix Figure A.12 visualizes the reduced form relationship, showing a significant negative effect in the current year. The previous-period lead effect is almost flat yet slightly positive. The next-period lag effect is not strongly significant but clearly negative, qualitatively similar to the current-period effect. This impact effect, followed by a weaker next-period effect, is somewhat intuitive and reassuring about the economic impact of the instrument.

2SLS estimates for ρ, the effect of legislative detail on growth, are reported in Table 4. Column 1 gives the baseline 2SLS estimate with state fixed effects and biennium fixed effects. It is positive and statistically significant, meaning that at the margin an exogenous shift in legislative detail due to nationwide text flows is associated with increased economic growth. The rest of the columns provide an array of robustness

checks. Column 2's state-specific linear time trends do not change things. Nor do the set of pre-treatment controls, interacted with fully saturated time effects, added in Columns 3 through 5. The results are not sensitive to controls for current-period topic shares (Column 6). Finally, we can take everything together and add the lagged dependent variable (Column 7).

Across these specifications, the effect of legislative detail on growth is robust. In U.S. states, 1965-2010, 10 percent increase in legislative detail increased the per capita economic growth rate by .1 to .2 percent, relative to a mean of .13. The 2SLS estimates have a similar magnitude to the OLS estimates from Table 3.

6.2 Specification Checks for the Effect of Detail

In the Appendix we run a series of specification checks. First, in line with Borusyak and Jaravel (2017), we show that results are robust to the inclusion of topic share controls, both in levels and in changes (see Appendix Table A.9).

Next, we show that our results are not sensitive to the number of topics used in the construction of the instrument. Appendix Table A.10 shows results for 6, 12, 24, 30, 36, 42, and 48 topics. Our main results are there regardless of how the instrument is constructed.

Appendix Table A.7 reports the baseline specification with alternative clustering of standard errors. The results are robust to not clustering (Columns 1 and 2) as well as two-way clustering by state and year (Columns 3 and 4). Following Adao et al. (2019), we apply k-means clustering on the pre-period topic share vectors to group states according to their initial topic shares. We then cluster standard errors on 12, 16 and 20 initial-topic groups, and results are still robustly significant (Columns 5 to 10).

We also report results in Appendix Table A.11 to check alternative measures of legislative detail. First, we show that using number of words, rather than number of provisions, as the endogenous regressor (and for constructing the instrument) produces a positive 2SLS estimate that is not statistically significant (Columns 1 and 2). This supports our argument from above that our NLP method is needed to extract legally relevant information from the statute texts. In line with this idea, our main result is robust to including as a control the number of pages in the published statutes volume (Column 4).

Table 5: Effect of Legislative Detail on Additional Economic Variables

	(1) GDP (Total)	(2) Population	(3) Employment	(4) Profits	(5) Wages	(6) Establishments
Legislative Detail	0.0199+	-0.00193	0.00481	0.0486+	0.0106+	-0.00877+
	(0.0102)	(0.00240)	(0.0119)	(0.0244)	(0.00536)	(0.00485)
First Stage F-stat	22.81	22.81	14.84	181.3	22.81	14.84
Observations	1183	1183	821	549	1183	821
State FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X

Notes. Results for the 2SLS model (Second Stage 1) and First Stage 3) but with different outcome variables. Column 1 explores the effect on state GDP (not per capita). Column 2 shows there is no effect on population. Column 3 uses employment while column 4 looks at firm profits (value added) within the state. Column 5 looks at wages and Column 6 establishment growth. All specifications include state and biennium fixed effects. Standard errors clustered by state. **p<.01; *p<.05; +p<.1.

6.3 Unpacking the Effect of Detail

Next, in Table 5 we try to better understand what is driving the effect by putting different economic outcomes on the left-hand-side of the second stage. The effect is not sensitive to using GDP rather than GDP per capita (Column 1). We see that there is no effect on population (Column 2), employment (Column 3), or number of establishments (Column 6). However, there are broad effects on other signifiers of economic expansion, including profits (Column 4) and wages (Column 5).

Next we check whether there are effects on other government activities besides legislation. Appendix Table A.13 shows there is no effect on total government expenditures, expenditures on legislative expenses, taxes, or party control (Democrat/Republican) of state government. That there is no effect on government spending means that the effect on growth is not driven by a fiscal shock, where new legislation mechanically causes new spending. That there is no effect on legislative spending suggests that the growth effect is not driven by confounding effects on the legislative process, for example increased quality of policymaking procedures. The null effect on taxes, again, suggests that there is not a confounding fiscal shock. The null effect on party control means that there does not appear to be intervening effects in the state political environment.

To further unpack the effect, we wanted to know what category of policy (fiscal policy, economic regulation, social regulation, or procedural) is most important. As described in Section 4.2, we divide the LDA topics into the four more interpretable

Table 6: What Policies are Driving the Effect of Detail on Growth?

	(1)	(2)	(3)	(4)
	Effect on Real GDP Growth Per Capita			
Policy Category	*Fiscal*	*Economic Regulation*	*Social Regulation*	*Procedural*
Legislative Detail	0.0220*	0.0125+	-0.000564	0.000883
	(0.0107)	(0.00697)	(0.00968)	(0.00920)
First Stage F-stat	18.68	42.53	13.42	49.12
Observations	1,181	1,182	1,182	1,182
Time FE	X	X	X	X
State FE	X	X	X	X

Notes. Results for the 2SLS model (Second Stage 1) and First Stage 3), where the instruments and endogenous regressors are constructed separately by the four larger policy categories. Columns give the respective policy category. All specifications include time and state fixed effects. **p<.01; *p<.05; +p<.1.

categories l. Thus we have four separate endogenous regressors W_{st}^{l}, representing the log number of provisions in state s at biennium t allocated to topics in policy category l. In turn, we produce separate shift-share instruments for each of the four categories. The calculation is the same as in subsection 5.2, except that rather than summing over all topics K, we sum over the subset of topics K_l within each respective policy category. We therefore get a separate instrument Z_{st}^{l} for each policy. We then estimate the baseline 2SLS system (Equations 3 and 1) separately for each of the four categories l, where the category-specific endogenous regressor W_{st}^{l} and instrument Z_{st}^{l} are appropriately slotted in.

The effects across policy categories are reported in Table 6. We can see that the effects are driven by fiscal policy and economic regulation. Rules related to social regulation and procedure (e.g. judicial and electoral administration) are not important for economic growth. Note that even though fiscal policy is important, we know from Appendix Table A.13 that our effect is not driven by changes in government expenditures. So it must instead be due to legal changes in how money is spent (for example imposing more monitoring or controls), rather than the amount spent.

7 Contingency, Concavity and Uncertainty Effects

7.1 Contingency

The **contingency** hypothesis (**H1**) states that the effect of legislative detail on growth should be driven by contingent (rather than non-contingent) clauses. As described in Subsection 4.3, we produce separate counts for contingent provisions (W_{st}^C) and non-contingent provisions (W_{st}^N). To test **H1**, we estimate variants of the 2SLS system (3) and (1), but using the contingent and contingent measures of detail as joint endogenous regressors. The second stage is

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho_C \log W_{st}^C + \rho_N \log W_{st}^N + X'_{st}\beta + \varepsilon_{st} \tag{7}$$

where now we have two endogenous regressors, with the associated causal effects of interest for contingencies (ρ_c) and non-contingencies (ρ_N).

With two endogenous regressors, we need at least two instruments. To that end, we compute two variants of the shift-share instrument using the same formula (2), but where all provisions counts are replaced with contingent provision counts and non-contingent provision counts, respectively. Let Z_{st}^C give the contingency instrument and let Z_{st}^N give the non-contingency instrument. The first stage equations are

$$\log W_{st}^C = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi_C Z_{st}^C + \psi_N Z_{st}^N + X'_{st}\beta + \eta_{st} \tag{8}$$

$$\log W_{st}^N = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi_C Z_{st}^C + \psi_N Z_{st}^N + X'_{st}\beta + \eta_{st} \tag{9}$$

where all terms are as above. Appendix F reports the full battery of shift-share instrument checks for these instruments.

In addition to the joint treatment, we estimate an alternative specification using as a single endogenous regressor the log difference between contingency and non-contingency, $\log W_{st}^C - \log W_{st}^N$. The second stage is

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho_{CN}(\log W_{st}^C - \log W_{st}^N) + X'_{st}\beta + \varepsilon_{st} \tag{10}$$

where the causal effect of interest is ρ_{CN}, giving the effect of contingencies relative to non-contingencies. We use both contingency instruments in the first stage:

$$(\log W_{st}^C - \log W_{st}^N) = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi_C Z_{st}^C + \psi_N Z_{st}^N + X'_{st}\beta + \eta_{st} \tag{11}$$

Table 7: Effect of Contingent and Non-Contingent Clauses on Economic Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Detail (Contingent)	0.0638**	0.0590**					
	(0.0226)	(0.0215)					
Detail (Non-Contingent)	-0.0559*	-0.0511*					
	(0.0242)	(0.0228)					
Contingent - Non-Contingent			0.0752**	0.0697**	0.0501*	0.0379*	0.0773**
			(0.0242)	(0.0229)	(0.0219)	(0.0158)	(0.0219)
First Stage F-stat	22.27	36.82	22.83	36.60	15.13	31.68	23.86
Observations	1,182	1,182	1,182	1,182	1,182	1,182	1,134
Time FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
State Trends		X		X			
Econ Vars × Time					X		
Sector Shares × Time						X	
Demog Vars × Time							X

Notes. Results for the 2SLS model of contingencies. Column 1 and 2 show results for contingent and non-contingent clauses together (Second Stage 7) and First Stages 8 and 9), adding state specific trends in the second column. Columns 3-7 show the results for the difference between contingent and non-contingent clauses (Second Stage 10) and First Stages 11). Column 4 adds state specific trends, Column 5 adds pre-period economic variables interacted by year, Column 6 interacts initial sector shares by biennium, Column 7 initial demographic characteristics interacted by biennium. All specifications include controls for state and biennium fixed effects. **p<.01; *p<.05; +p<.1. Standard errors clustered by state.

which gave a higher first-stage F-statistic than computing a new instrument. We will report first stage statistics for all specifications along with the 2SLS estimates.

The 2SLS regression estimates for contingency are reported in Table 7, with the different specifications analogous to those from Table 4. Columns 1 and 2 provide the estimates for the second stage (7) with two endogenous regressors (contingent and non-contingent), instrumented by first stages (8) and (9). We can see in both columns that the 2SLS effect of contingent clauses is positive, while the 2SLS effect of non-contingent clauses is negative.

Next, Columns 3 through 7 show the estimates for the differenced (contingent minus non-contingent) second stage (10) with first stage (11). Consistent with the separate-treatments specification, there is a large positive effect of relative use of contingency.

The effect is robust to including state trends or including pre-treatment characteristics interacted with time fixed effects.

Overall, these results support the contingency hypothesis. Interestingly, the magnitude of the coefficients on contingency clauses is much larger than that for total detail – three to four times as large. This is additional support that the part of legislative detail that contributes to growth are contingent clauses.

Appendix F reports a number of supporting results. Appendix Table A.15 show the results when using contingency and non-contingency counts by themselves as the endogenous regressor. Appendix Table A.14 reports additional specifications with the differenced treatment variable, showing that it is robust to inclusion of other variables.

7.2 Concavity

Next, the concavity hypothesis (**H2**) states that with diminishing marginal returns to detail, the effect of detail should be larger in contexts with relatively low pre-existing detail. To test for this idea, we split the subsample based on legislative detail in the previous five bienniums. After ranking the state-biennium observations by recent detail, we split the sample into three terciles by that ranking.

We then estimate the baseline 2SLS system (Equations (3) and (1)), but subsetting by the three terciles. We also look at concavity in the effect of contingent clauses by estimating the 2SLS system for the effect of the difference in contingencies and non-contingencies (Equations (11) and (10)). According to **H2**, we would expect a larger effect of detail in the tercile with lowest previous detail.

Results are shown in Table 8. Consistent with our prediction, we find that the effect of legislative detail on economic growth is stronger for states with low detail (Columns 1-3) compared to states with medium detail (Columns 4-5) or high detail (Columns 6-7). The effect for low-detail states is robust to state trends (Column 2), and also holds for the effect of contingencies (Column 3). These findings provide support for the concavity hypothesis (**H2**).

Appendix Section G provides additional specification checks for the concavity analysis. In particular, Appendix Table A.17 shows that we get similar results when the concavity thresholds are computed after residualizing on the state and year fixed effects.

Table 8: Testing Concavity: Effect of Detail on Growth by Recent Detail Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Recent Detail	*Low*			*Medium*		*High*	
Legislative Detail	0.0404*	0.0425*		0.00640	0.000205	0.0002	-0.0109
	(0.0167)	(0.0158)		(0.0104)	(0.0107)	(0.00743)	(0.00935)
Contingent - Non-Contingent			0.117**				
			(0.0351)				
First Stage F-stat	66.18	59.26	25.29	48.65	47.87	86.59	67.12
Observations	392	392	392	385	385	382	382
Time FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
State Trends		X	X		X		X

Notes. Results for the 2SLS model (Second Stage 1) and First Stage 3), splitting up the data by terciles in recent legislative detail (previous five biennia). Columns 1 through 3 report results for states with lower tercile recent legislative detail. Columns 4 and 5 report results for those with average recent legislative detail and Columns 6 and 7 states with recent legislative detail in the higher tercile. All specifications include a first column with time and state fixed effects and a second column with the addition of state specific trends. **p<.01; *p<.05; +p<.1.

7.3 Uncertainty

Finally, we test the **uncertainty** hypothesis (**H3**), which states that legislative detail – in particular, contingent clauses – should boost growth more under higher economic uncertainty. Using the measure of local economic uncertainty described in Subsection 3, we rank the state-biennium observations by uncertainty. We then split the sample into three terciles based on the uncertainty ranking.

As done with concavity, we then produce 2SLS estimates for each tercile sample. We do so for the baseline results with total provisions, as well as the contingency analysis using contingent and non-contingent clauses. According to **H3**, the effect of detail should be greater in the higher-uncertainty terciles. Especially, the contingency effect should be largest in the highest-uncertainty terciles.

The results are reported in Table 9. We perform the baseline analysis with all clauses, as well as extending the contingency analysis, with the expectation that contingent clauses should have the biggest interaction with uncertainty. Columns 1 and 2 include estimates for low uncertainty, Columns 3 and 4 with medium uncertainty, and Columns 5 through 10 with high uncertainty. The specifications are the same as those reported in Table 4 (baseline with all detail) and Table 7 (contingency).

Table 9: Testing Uncertainty: Effect of Detail on Growth by Uncertainty Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Effect on Real GDP Growth Per Capita									
Economic Uncertainty	Low		Medium		High					
Legislative Detail	0.00448		0.00699		0.0373*	0.0391*				
	(0.0111)		(0.0111)		(0.0153)	(0.0176)				
Detail (Contingent)							0.145*	0.170*		
							(0.0560)	(0.0672)		
Detail (Non-Contingent)							-0.137*	-0.163*		
							(0.0624)	(0.0775)		
Contingent -		0.0823		0.000182					0.164**	0.189**
Non-Contingent		(0.0692)		(0.0310)					(0.0465)	(0.0568)
First Stage F-stat	65.92	4.251	5.389	12.03	46.50	108.2	10.24	9.433	10.65	10.34
Observations	345	345	373	373	377	377	377	377	377	377
Time FE	X	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X	X
State Trends						X		X		X

Notes. Results for the 2SLS model (Second Stage 1) and First Stage 3), splitting up the data by terciles in economic policy uncertainty. Also includes estimates for the differenced contingency detail measure, as indicated. Columns 1-2 show results for states with lowest tercile uncertainty. Columns 3-4 report results for those with median uncertainty while Columns 5-10 states with uncertainty in the higher tercile. All specifications include state and biennium fixed effects, while for High Uncertainty states, results controlling for state specific trends are also included (as indicated). **p<.01; *p<.05; +p<.1.

First consider Columns 1 through 4, with low or medium uncertainty. These are all zeros, regardless of the specification. The coefficients are all relatively small in magnitude, and none are statistically significant. Note that the first stage is sometimes weak, however.

In contrast, consider Columns 5 through 10, focusing on the highest-uncertainty tercile. Columns 5 and 6 show a positive and significant effect of legislative detail, about twice in magnitude to the full-sample estimate from Table 4. A similar magnified effect is seen for contingency in Columns 7 through 10. Contingent clauses have a relatively large positive effect on economic growth under high uncertainty. Meanwhile, the computed first-stage F-statistics are consistent with a sufficiently strong first stage for all of these regressions. Overall, these estimates provide support for the uncertainty hypothesis (**H3**).

Appendix Section G provides additional specification checks for the uncertainty analysis. In particular, Appendix Table A.16 shows that concavity and uncertainty recover independent dimensions in the dataset. In addition, Appendix Table A.18 shows similar results when the uncertainty variable is residualized on state and year fixed effects before the ranking and division into terciles.

Appendix Table A.19 shows that the uncertainty effect is robust to the inclusion of lagged growth per capita, suggesting that it is not driven just by the economic uncertainty measure picking up the business cycle. Also consistent with this point: Appendix Table A.20 shows that if we split up the sample based on recent growth (rather than recent detail or current economic policy uncertainty), we see effects of detail on growth in both the top and bottom tercile. Overall, these checks suggest the effect heterogeneity from concavity and uncertainty are not driven by confounding business cycle trends.

8 Conclusion

This paper explores what makes legislative detail matter for growth. In the empirical setting of the U.S. states for the years 1965 through 2012, we find that more legislation tends to boost the economy. Consistent with the writing cost models of contract incompleteness, we find that the positive impact on growth is higher when the additional legislative details are in the form of contingent clauses, when starting from lower initial levels, and in periods of greater economic uncertainty.

Our methods build on the previous literature in the field in two ways. First, we introduce a new measure of legislative detail from the text of state laws based on tools from computational linguistics. Second, we implement a shift-share instrumental variables strategy that isolates exogenous variation in legislative output to examine causality.

Our results are likely specific to the institutional context we study. Our shift-share instrument is constructed based on having low detail on a legal topic, meaning that our set of compliers are those who are most likely to benefit from higher detail. More research is needed to understand the broader applicability of these findings across time and across countries, and to situate the results in the broader research agenda on laws and growth.

More specifically, as shown in Gratton et al. (2018), signaling incentives can have a strong effect on the quantity and quality of laws. The predictions on the effects of legislative detail on growth in a system with strong signaling incentives and a large stock of existing laws (like in Italy and other civil law systems) may well differ from the benevolent legislator benchmark. The positive impact we document for U.S. states in this paper could indicate that signaling incentive distortions are not significant in this context. Other possible factors could be the special role of specialized agencies in the crafting of legislative proposals (Bendor, 1995), or the selected nature of proposals considered (or already passed) in other states.

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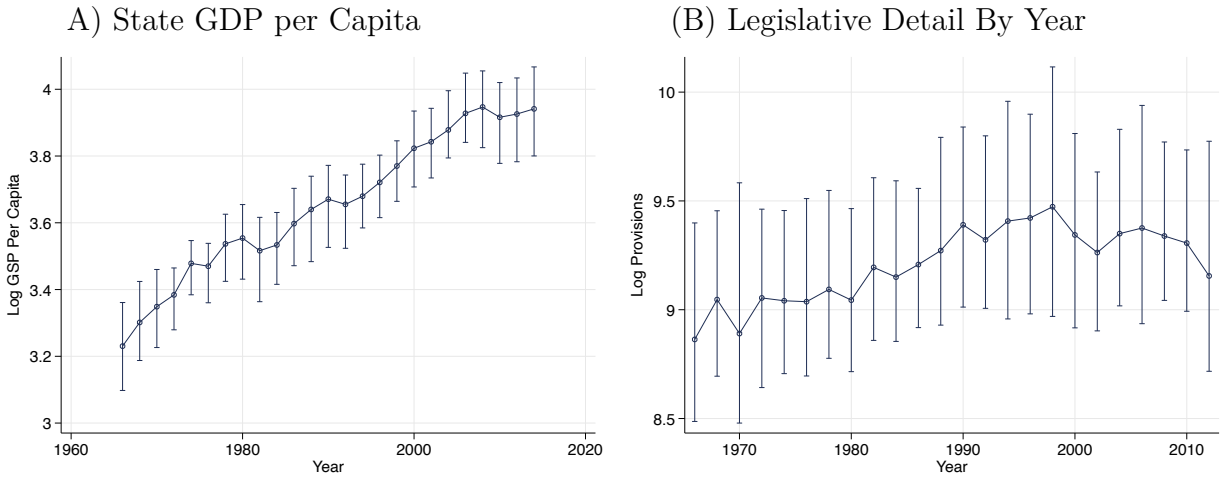
Figure A.1: State-Level Economic Output and Legislative Detail By Year



Notes. Line graphs showing the mean of (log) private GDP (Panel A) and (log) provisions (Panel B) across states over time. Error spikes give 90% confidence intervals from standard errors of the mean.

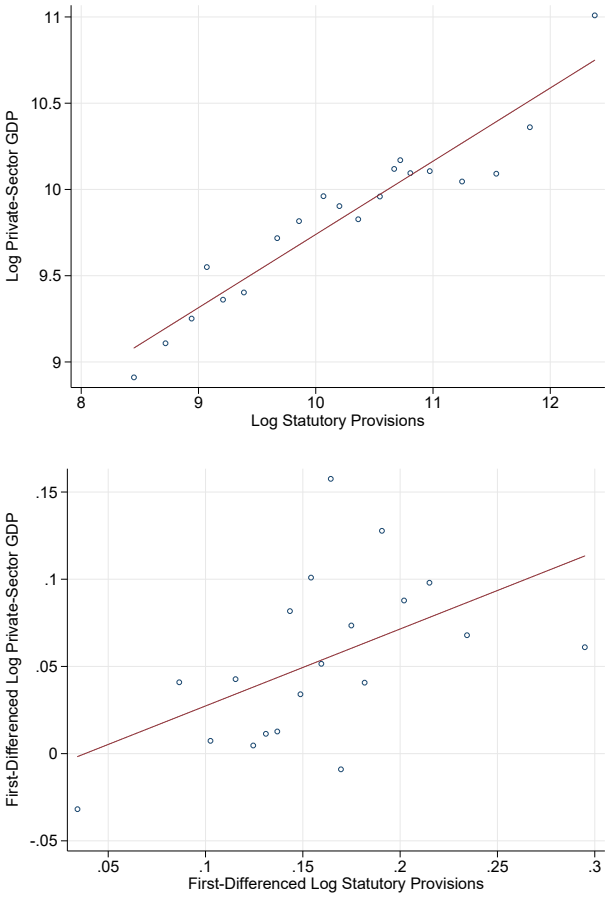
A Data Appendix

B Details on Text Features

B.1 Segmenting Statutes

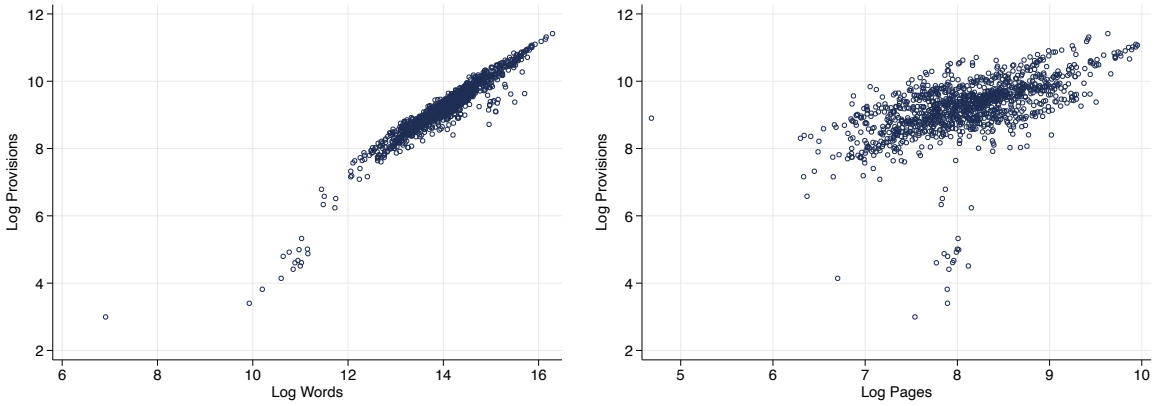
The first step is to merge and process this raw text. A script serves to append pages, remove headers, footers, tables of contents, indexes, and other non-statute material. Then, it segments the text into individual bills, acts, and resolutions using text markers for the start of new statutes. These include indicators for new Chapters, Articles, or Titles, such as a line with CHAPTER followed by a Roman numeral. Some states have their own standard indicators, such as P.A followed by a number to indicate a new Public Act. The script also uses common text for the beginning of a statute preamble (e.g., An act to...) and for enacting clauses (e.g., Be it enacted that...). Research assistants checked samples of the statute segmenter for each state-year to make sure it worked well.

Figure A.2: Cross-Sectional Relationship between Legislative Detail and GDP per Capita



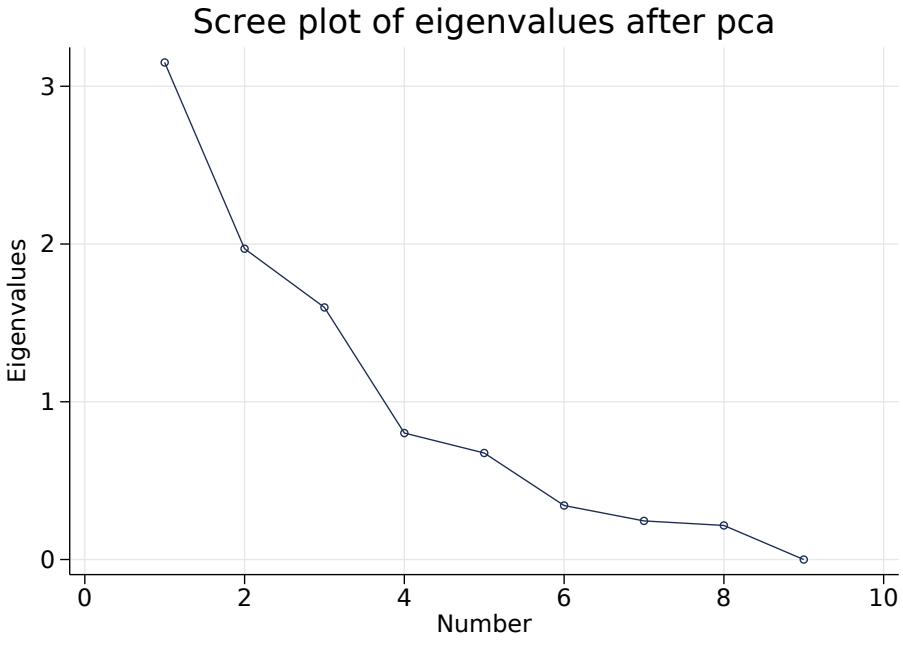
Notes. Binned scatterplots for the relationship between (log) provisions and (log) real GDP per capita in Panel A and first differences in Panel B.

Figure A.3: Scatter Plots of Provisions vs. Word Counts and Page Counts



Notes. The figure shows a scatter plot for the relationship between (logged) words in the left panel and (logged) pages in the right panel and (logged) provisions, respectively.

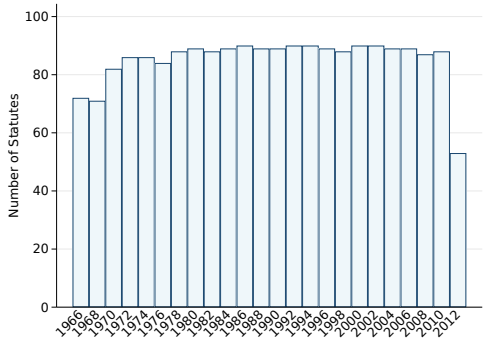
Figure A.4: First Principal Components of Initial Economic Sectors



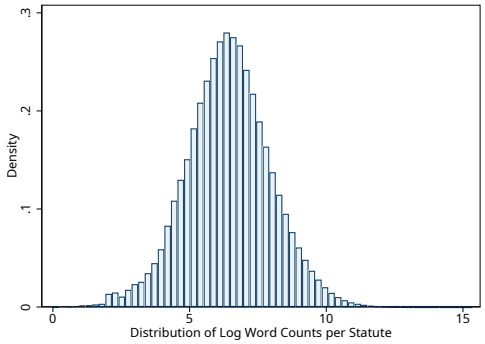
Notes. Screeplot for the principal components in economic sectors. We include the first four, or first six, components, interacted with year, as exogenous covariates in the regression analysis.

Table A.1: Summary Statistics on Statute Segmentation

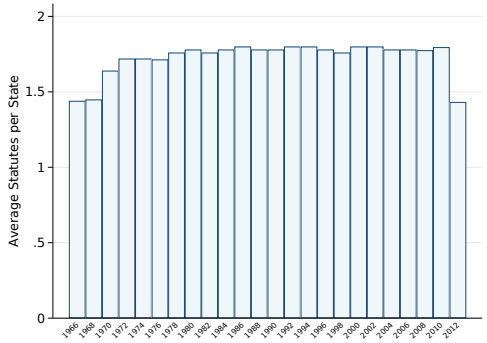
(A) Histogram: Number of Statutes by Biennium



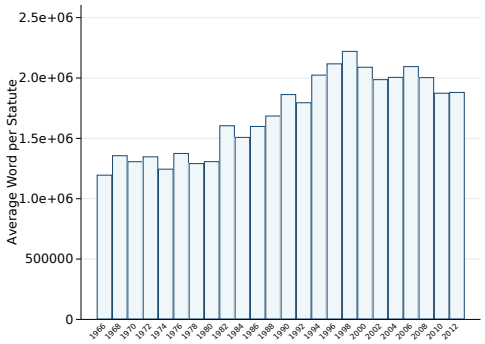
(B) Histogram: Number of Words per Statute



(C) Statutes per State, by Biennium



(D) Average Words per Statute, by Biennium



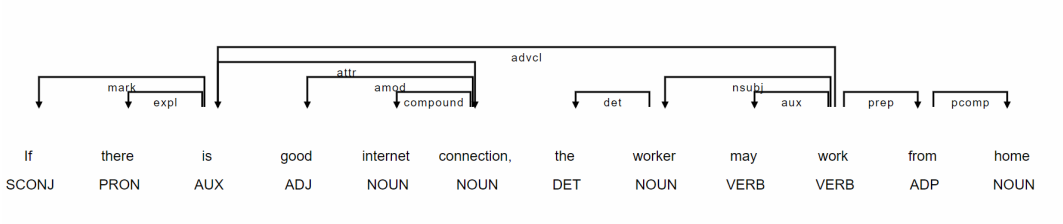
Notes. These figures provide some details on our corpus. The left panels shows the number of statutes by biennium (top) and state (bottom). The left panels show the number of words by statute (top) and the number of words by biennium (bottom).

B.2 Extracting Legal Provisions

Our information extraction approach relies on two stages: the definition of extraction rules and the syntactic parsing of the text. First, we decide the lexical and syntactic features of the provisions we want to extract. We focus on delegation, constraint, permission, and entitlement. A.2 shows the extraction rules, namely the lexical and syntactic rules we expect the main legal provisions above to follow. These are based on large-scale repositories of coded ontologies. These are dictionaries of words and dependencies that have been annotated to serve a theme, such as making a promise. An example of these ontology dictionaries is FrameNet.

A.5 shows the result of the syntactic parser. The dependency parser tells us whether

Figure A.5: Syntactic Parsing for Provision Extraction



Notes. The Figure shows an example of a dependency tree. The letters below the words represent the part of speech (POS) tags. A prerequisite of syntactic dependency parsing, indeed, is POS tagging. The latter assigns labels ('tags') to the tokens in a sentence according to their function, such as noun, verb and adjectives. The arcs above the sentence represent the syntactic relations between words. First of all, the parser identifies the head of the sentence, namely the main verb, in this case 'work'. Then, the parser identifies the subject of the sentence and tells the researcher also that it is a nominal subject, in this case 'worker'. Indeed, in some cases, the subject may by a clause. The subject is then associated to a determiner, 'the'. Then, the parser looks at the other side of the sentence and, in this case, identifies a preposition, namely 'from', and the prepositional complement 'home'. It should be noticed that the verb of the contingent part of the sentence, 'is', is related to the main verb and hence the main sentence with the dependency adverbial clause. The latter is one of the most common syntactic relations that allow identifying a contingency.

a noun is the subject or the object of the sentence. It tells us rich information about the verb -- whether it is the main verb or just an auxiliary, whether it is active or passive, and so on. These annotations provide the ingredients from which our extraction rules build measures of delegation. Our dependencies are produced using the Python package spaCy (Honnibal et al., 2015). The spaCy parser obtains state-of-the-art performance on the standard computational linguistics metrics. Like most parsers, it is trained on corpora of hand-parsed sentences. A detailed discussion of the process of information extraction can be found in Vannoni et al. (2019).

B.3 Details on Legislative Topics

Table A.3 shows the words associated with each topic for the 42-topic specification. We also include the assigned policy category for each topic: economic regulation, fiscal policy, procedural law, or social regulation.

B.4 Details on Contingency

C Instrument Checks

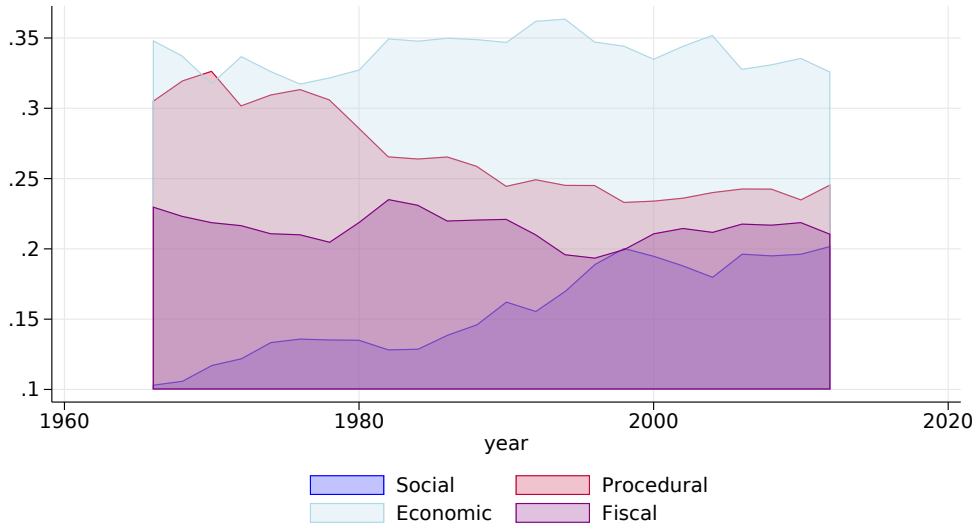
Table A.2: Types of Legal Provisions, with Extraction Rules

Lexical Units
Strict modals: 'shall','must','will'
Permissive modals: 'may','can'
Delegation verbs: 'require', 'expect', 'compel', 'oblige' , 'obligate', 'have to', 'ought to'
Constraint verbs: 'prohibit', 'forbid', 'ban', 'bar', 'restrict', 'proscribe'
Permission verbs: 'allow', 'permit', 'authorize'

Extraction Rules
Delegation: strict modal + active verb + not negation OR not permissive modal + delegation verb + not negation
Constraint: modal + not delegation verb + negation OR strict modal + constraint verb + not negation OR permission verb + negation
Permission: permission verb + not negation OR permissive modal + not special verb + not negation OR constraint verb + negation
Entitlement: entitlement verb + not negation OR strict modal + passive + not negation OR delegation verb + negation

Notes. As enumerated in the table, a delegation is characterized by one of two structures: 1) a non-negated strict modal followed by an active verb ('The worker shall act'), or 2) a non-negated non-permissive modal (either a non-modal or a strict modal) followed by a delegation verb ("The worker is expect to act"). Constraints are characterized by 1) a negated modal ("The worker shall no"), a negated permission verb ("The worker is not allowed), or a non-negated constraint verb ("The worker shall be prohibited from"). Permissions are characterized by a 1) non-negated permission verb ("The worker is allowed to"), 2) a non-negated permissive modal followed by a non-special verb ("The may act"), or a 3) negated constraint verb ("The worker is not prohibited from"). Finally, entitlements are characterized by 1) a non-negated entitlement verb ("The worker retains the power to"), 2) a non-negated strict modal followed by a passive verb ("The worker shall be considered"), or 3) a negated delegation verb ("The worker is not obligated to"). By following these rules, we can see that the sentence in A.5is a permission: "The worker may work".

Figure A.6: Shares across Policy Categories over Time

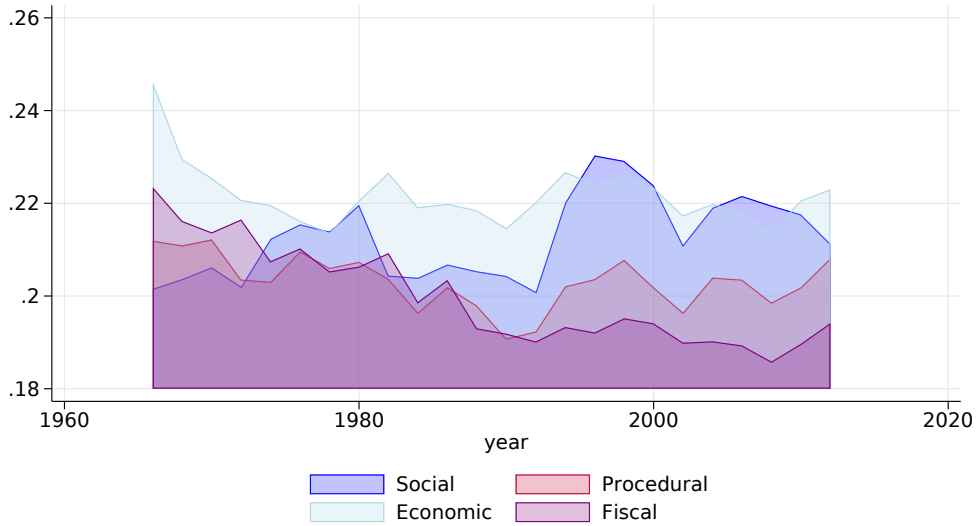


Notes. This Figure shows the shares of topic groups (social, procedural, economic and fiscal) over time. We can see that economic clauses stay relatively stable over time, whereas social clauses increased drastically. Fiscal and procedural clauses, instead, slowly decreased over time.

Table A.3: List of Topics, 42-Topic Specification (with Broader Categories)

Label	**Frequency**	**Category**	**Most Associated Words**
Licensing	0.0318	economic	license fee holder valid such_license card renew proof good age such_person
Energy	0.0267	economic	director control solid gas site air oil coal environment underground tank mine
Partnerships	0.0267	economic	agent partnership foreign partner merger case transact mail demand stock
Payments	0.0258	economic	paid payment pay obligor child_support cost unpaid receipt withheld collect
Credit	0.0241	economic	interest transfer lien instrument issuer debtor seller holder buyer contract
Real Property	0.0227	economic	real loan trust mortgage interest broker common sale lender deed condominium
Traffic	0.0211	economic	motor dealer driver owner plate test vessel trailer weight special accident
Banks	0.0208	economic	institution bank stock deposit higher credit credit_union branch loan account
Insurance	0.0206	economic	life contract small premium carrier surplus risk condition benefit minimum pool
Contracts	0.0205	economic	contract work labor contractor cost repair perform bid job master firm trade
Land	0.0203	economic	land owner park parish port airport forest parcel lot map easement plat portion
Retail	0.0201	economic	sale sold retail sell price distributor fuel product milk liquor aircraft supplier
Torts	0.0201	economic	claim death claimant lieu_thereof loss settlement award case judgment legal
Traffic	0.0182	economic	highway traffic feet railroad state_highway transit load road space front stop
Commodities	0.0176	economic	fish food livestock plant game dog farm seed control sale grain wild owner deer
Land	0.0089	economic	street road feet island run tract river township center_line corner beach
Bonds	0.0336	fiscal	interest bond sale payment sold debt pay cost pledge paid sell interest_thereon
Taxes	0.0294	fiscal	tax gross credit return paid net assessor refund case such_tax homestead state_tax
Budgeting	0.0294	fiscal	budget for_the_fiscal_year so_much_thereof transfer special aid grant biennium
Funding	0.0276	fiscal	fund account money trust_fund transfer special excess deposit state_general_fund
Local Projects	0.0268	fiscal	development project local local_government compact zone urban government cost
Pensions	0.0267	fiscal	age benefit credit paid pension per_cent equal membership death elect final
Taxes	0.0263	fiscal	rate total equal paid calendar_year maximum strikeout subparagraph base excess
Tax Admin	0.0174	fiscal	paid sheriff auditor said_board warrant census audit supervisor cabinet travel
Miscellaneous	0.0202	misc	tile tie sueh lie said_code whieh shal ill supp aid thc tho tle tire aet sha
Courts	0.0390	procedural	court attorney judgment trial case district_court petition circuit_court circuit
Appeals Courts	0.0389	procedural	review appeal final complaint case petition civil receipt mail panel subpoena
Administration	0.0301	procedural	governor chief fire personnel bureau appoint shall_consist volunteer membership
Elections	0.0291	procedural	ballot petition voter township precinct register tenant cast elector referendum
Governance	0.0285	procedural	power invalid control proper event thereon hereof art shall_have_the_power
Policy Research	0.0278	procedural	center data review research staff local access develop implement level task
Elect Districts	0.0217	procedural	district special petition such_district said_district creation portion district_board
Local Govt	0.0207	procedural	council charter mayor special government conflict appoint perform oath organ
Governance	0.0162	procedural	government commonwealth civil attorney_general exempt uniform nonprofit
Local Issues	0.0120	procedural	local local_law new_matter superior such_law event fair race centum thirty-first
Education	0.0291	social	school school_district state_board student teacher pupil school_year tuition
Family Law	0.0275	social	child court parent minor children age guardian placement adult petition youth
Public Health	0.0254	social	health care home health_care social human children medicaid public_health
Healthcare	0.0242	social	treatment physician patient mental drug mental_health dental condition care
Criminal Law	0.0205	social	crime probation fine victim parole jail misdemeanor arrest sex firearm sexual
Water	0.0171	social	town water town_council sewer said_town lake river san town_clerk town_board
Social Issues	0.0087	social	sect team great stricken high_school veteran life honor nation first_paragraph

Figure A.7: Evolution of Contingent Language by Policy Category

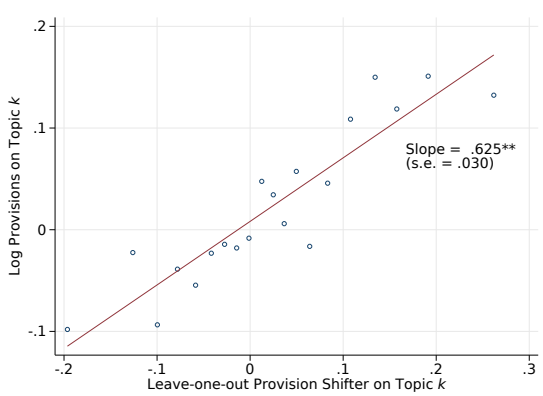


Notes. This Figure shows the trends in the shares of contingent clauses by topic category (social, procedural, economic and fiscal) over time.

Figure A.8: Decomposing First Stage Effects of Shift and Share Terms

(A) First-Stage Effect of Shifter

$$\Delta W_{st}^{k} \sim \sum_{r \neq s} \frac{\Delta \log W_{rt}^{k}}{49}$$



(B) First-Stage Effect of Share

$$\Delta W_{st}^{k} \sim \frac{W_{s0}^{k}}{W_{s0}}$$

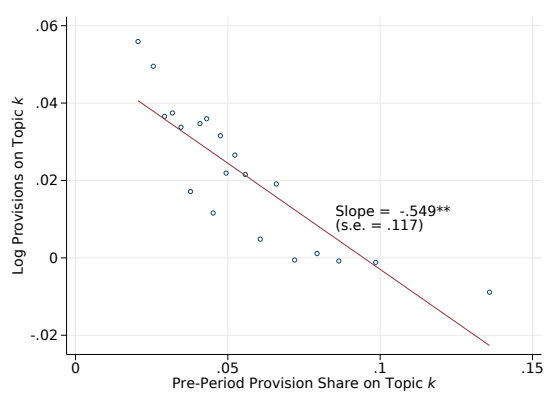
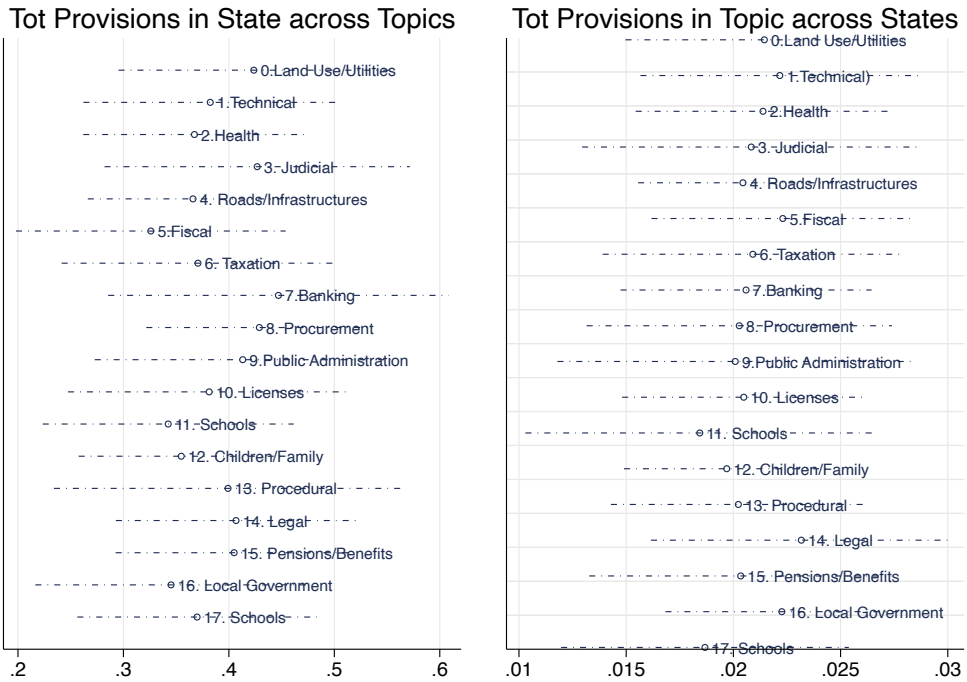
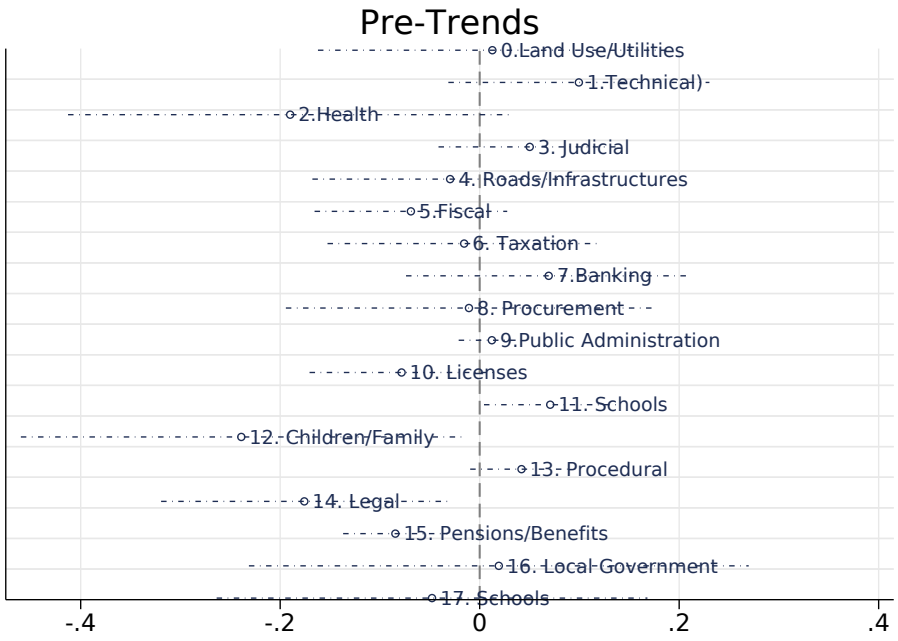


Figure A.9: All Topics Contribute to Instrument



Notes. To check that the relevance of the shift-share instrument is driven by a majority of topics, we regress the increase in provisions related to a topic in a state on the increase in the total provisions related to that topic across states and the increase in the legal provisions in that state, for every topic (including state and year fixed effects and clustering standard errors by state).

Figure A.10: Pre-Treatment Topic Shares do not predict Growth Trends



Notes. We plot the coefficients that show that pre-treatment topic shares are not correlated with growth trends. All specifications include biennium fixed effects and standard errors clustered at the state level.

Table A.4: Placebo Test: No Lead Effect of Legislative Detail on Economic Growth

	(1) OLS	(2) RF	(3) 2SLS
Lead Log Provisions	0.00676	0.0174	-0.0135
	(0.00634)	(0.0164)	(0.0124)
Observations	1132	1132	1132
First Stage F-stat	.	.	15.14
State FE	X	X	X
Time FE	X	X	X
State Trends	X	X	X

Notes. Column 1 shows the OLS estimate with state and biennium fixed effect, and controlling for state specific trends, as well as standard errors clustered by state. Column 2 and 3 shows the same but the reduced form and 2SLS estimates. **p<.01; *p<.05; +p<.1.

Table A.5: Instrument and Pre-Treat Topics Uncorrelated with Initial Characteristics

VARIABLES	(1) Instrument	(2) Instrument	(3) Instrument	(4) Instrument	(5) PCA	(6) PCA
Initial Share of Urban Pop	-0.021	-0.0302				0.688
	(0.0386)	(0.0547)				(3.277)
Initial Share of Foreign Pop	-0.0143	0.126				-12.04
	(0.219)	(0.361)				(18.01)
Initial Log Population	0.079	0.165				2.103
	(0.0753)	(0.123)				(4.902)
Initial Log Population^2	-0.00552	-0.0113				-0.159
	(0.00503)	(0.00824)				(0.316)
Initial Growth per Capita			0.00559	0.0205	0.483	
			(0.0251)	(0.0342)	(1.710)	
Observations	1135	526	1183	548	50	48
Time FE	X	X	X	X	X	X

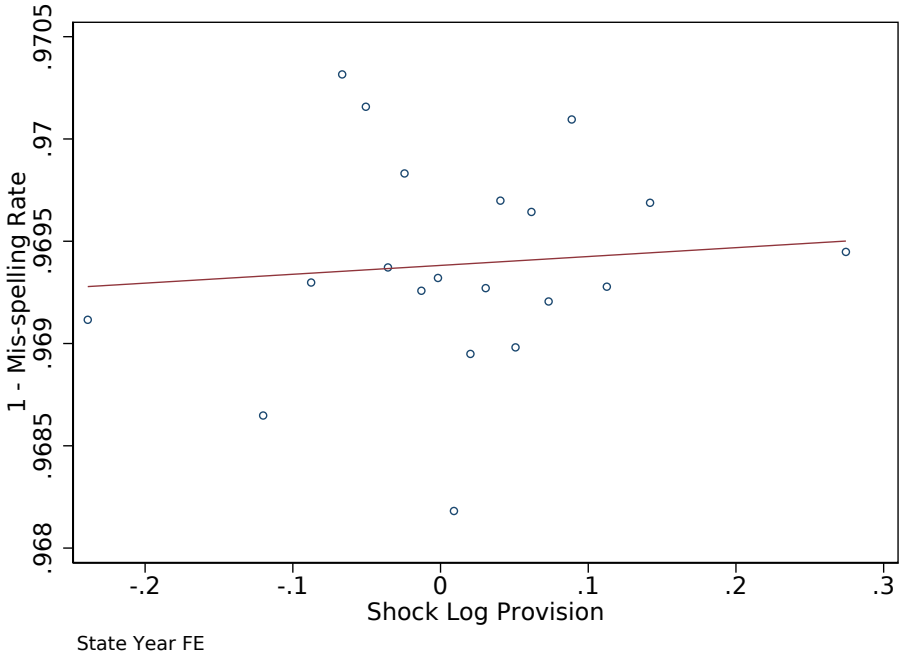
Notes. Columns 1 and 3 show the results for the balance test for the shares, using the whole sample. Column 2 and 4 show the results using only the first 10 years. Columns 5 and 6 show the results for the balance test at the share level for the initial year. All specifications are with biennium fixed effects, as well as standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.6: Instrument Balance Checks for Potential Confounders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Govt Exp	Lagged Govt Exp	Leg Exp	Lagged Leg Exp	Taxes	Lagged Taxes	Party	Lagged Party
Instrument (Z)	0.0523*	0.00789	0.0339	0.0333	0.0318	0.0655	0.0296	-0.0438
	(0.0257)	(0.0326)	(0.0734)	(0.0748)	(0.0535)	(0.0797)	(0.187)	(0.256)
Observations	1,183	1,133	1,183	1,133	1,183	1,133	1,123	1,110
Time FE	X	X	X	X	X	X	X	X

Notes. This table show the results for the balance test, regressing the instrument on the respective variables in each column current general government expenditure, lagged general government expenditure, current legislative expenditure, lagged legislative expenditure, current tax revenue, lagged tax revenue, current Democratic party control of state government, and lagged Democratic party control. Budget variables are in logs. All specifications are with biennium fixed effects, as well as standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Figure A.11: OCR Error Rate not Correlated with Instrument



Notes. The Figure shows that the correct spelling rate (computed from proportion of non-Proper-nouns in dictionary) is not correlated with the instrument.

D Robustness Checks on Main Results

Appendix Table A.8 shows the dynamic effects of detail with leads and lags of the treatment variable, instrumented with the leads and lags of the instrument. First, the lead effect is negative and insignificant, as desired for a placebo (Column 1). Column 2 shows the significant contemporaneous effect (the same as Column 3 from Table 4 for comparison). Column 3 shows the lagged effect included along with the contemporaneous effect, while Column 4 adds an additional lagged effect. The impact effect is always positive and significant, while the lagged effects are positive, and diminishing in magnitude with more bienniums (yet not statistically significant).

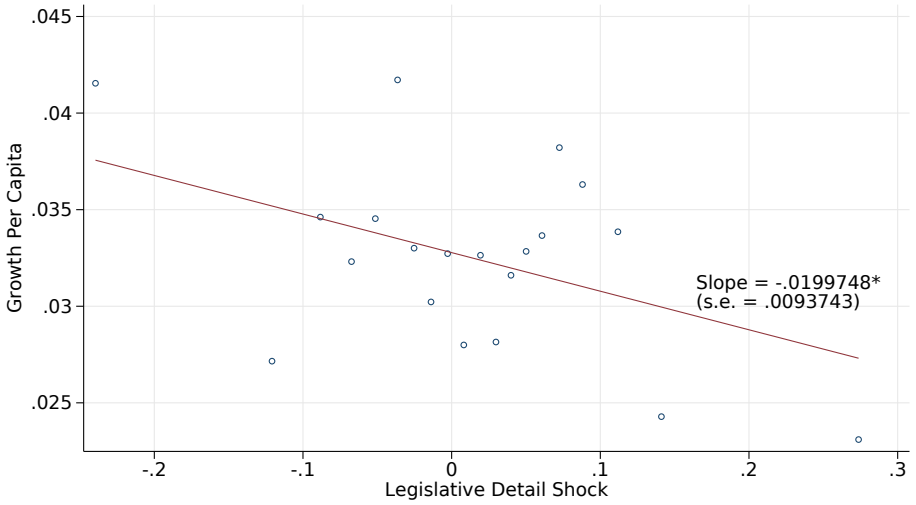
E Unpacking the Effect of Detail

F Additional Material for Contingency

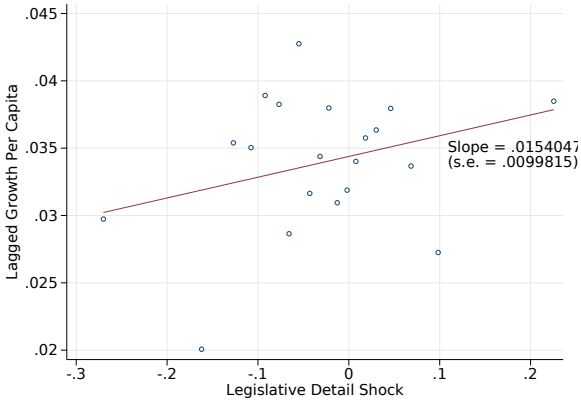
G Robustness Checks on Concavity and Uncertainty

Figure A.12: Reduced Form Effect of Legislative Detail Shock on Growth per Capita

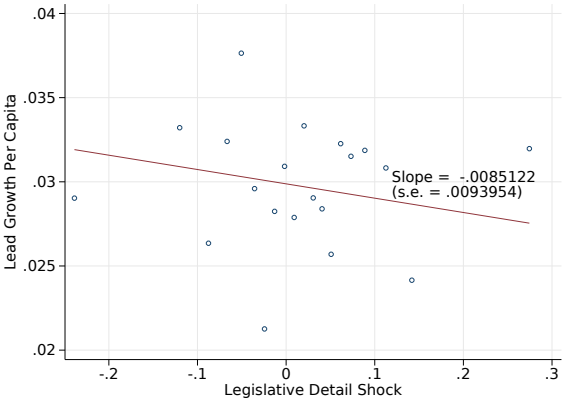
(A) Reduced Form Effect,Current-Period Growth



(B) Reduced Form Effect, Previous Period's Growth



(C) Reduced Form Effect, Next Period's Growth



Notes. Binned scatter plot of the relationship between legislative detail shock and growth per capita. Panel A gives impact effect; Panel B gives the past (placebo) effect; Panel C gives the lagged dynamic effect.

Table A.7: Effect of Detail on Growth (2SLS): Alternative Clustering of Standard Errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Effect on Real GDP Growth Per Capita									
Clustering	*None (Robust SEs)*		*Two-Way: State & Year*		*Initial Topics (12)*		*Initial Topics (16)*		*Initial Topics (20)*	
Legislative Detail	0.0182*	0.0168*	0.0182*	0.0168+	0.0182+	0.0168+	0.0182+	0.0168+	0.0182*	0.0168+
	(0.00872)	(0.00808)	(0.00854)	(0.00879)	(0.00986)	(0.00921)	(0.00873)	(0.00856)	(0.00835)	(0.00804)
First Stage F-stat	.	.	.	.	19.06	19.30	20.24	19.81	19.52	19.46
Observations	1,182	1,182	1,182	1,182	1,182	1,182	1182	1182	1182	1182
Time FE	X	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X	X
State Trends		X		X		X		X		X

Notes. Columns 1 and 2 report the estimates for the effect of legislative detail on growth per capita using robust standard errors. Columns 3 and 4 use standard errors clustered at the state and year level. Columns 5 to 10 use standard errors clustered at the initial topic level, with 12, 16 and 20 topics. All specifications include a first column with time and state fixed effects and a second column with the addition of state specific trends. For columns 1 to 4 the first stage F-stat is not generated because of the alternative clustering. **p<.01; *p<.05; +p<.1.

Table A.8: Effect of Legislative Detail on Economic Growth, Leads and Lags

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Real GDP Growth Per Capita					
Detail Next Biennium (Lead)	0.00664	0.00426				
	(0.00909)	(0.00838)				
Legislative Detail	0.0173*	0.0161+	0.0176*	0.0160*	0.0302	0.0232*
	(0.00862)	(0.00841)	(0.00787)	(0.00720)	(0.0190)	(0.0101)
Detail Last Biennium (Lag)			0.00453	0.00394	0.0146	0.00980
			(0.00689)	(0.00641)	(0.0134)	(0.00650)
Detail Two Bienniums Ago (2nd Lag)					0.0128	0.00940
					(0.0128)	(0.00687)
First Stage F-stat	8.596	10.17	9.026	10.68	0.962	4.813
Observations	1,130	1,130	1,179	1,179	1,176	1,176
State FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
State Trends		X		X		X

Notes. Columns 1,2 show the results with the lead and the contemporaneous effect together. Columns 3,4 include together the lag and the contemporaneous effects. Columns 5,6 include two lag effects and the contemporaneous one together. The first specification respectively includes state and biennium fixed effects, and the second adds state specific trends. All specifications have standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.9: Effect of Legislative Detail on Economic Growth - Topic Controls

	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Legislative Details	0.0182+	0.0168+	0.0182*	0.0168+	0.0182	0.0111
	(0.00905)	(0.00864)	(0.00903)	(0.00863)	(0.0332)	(0.0334)
Observations	1182	1182	1182	1182	1179	1179
First Stage F-stat	22.78	22.11	22.84	22.17	6.397	11.1
State FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
State Trends		X		X		X
Frequent Topic Shares	X	X				
PCA			X	X		
Growing Topic					X	X

Notes. The table shows the results for baseline 2SLS estimate controlling for the share of the most frequent topics in columns 1 and 2, for the first principal component in columns 3 and 4, and for the topics with the highest growth rate in columns 5 and 6. All specifications have state and time fixed effect, and standard errors clustered by state. Column 2, 4 and 6 also control for state trends. **p<.01; *p<.05; +p<.1.

Table A.10: Effect of Legislative Detail on Economic Growth - Different Number of Topics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Topic Number	*6*	*12*	*24*	*30*	*36*	*42*	*48*
Legislative Details	0.0150+	0.0158+	0.0168	0.0146+	0.0142+	0.0139+	0.0132
	(0.00771)	(0.00823)	(0.0101)	(0.00834)	(0.00825)	(0.00760)	(0.00832)
Observations	1182	1182	1182	1182	1182	1182	1182
First Stage F-stat	18.43	18.87	28.63	34.23	36.75	39.2	35.77
State FE	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X

Notes. The table shows the results for baseline 2SLS estimate where the instrument is constructed using different number of topics. All specifications have state and time fixed effects, and standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.11: Effect of Detail on Growth – Adjusting for Words or Pages

	(1)	(2)	(3)	(4)
	Effect on Real GDP Growth Per Capita			
Legislative Detail			0.0663	0.0203+
			(0.0632)	(0.0105)
Log Word Count	0.0154	0.0146	-0.0529	
	(0.00933)	(0.00901)	(0.0635)	
Log Page Count				-0.0103
				(0.00768)
First Stage F-stat	12.45	12.35	5.765	20.70
Observations	1,182	1,182	1,182	1,182
State FE	X	X	X	X
Time FE	X	X	X	X
State-Specific Trends		X	X	X

Notes. Columns 1 and 2 report the results for the effect of log of words on growth per capita. Column 3 and 4 report the effect of legislative details on growth per capita controlling for the log of the number of words and pages respectively. All specifications have state and time fixed effect, and standard errors clustered by state. Column 2 to 4 also controls for state trends. **p<.01; *p<.05; +p<.1.

Table A.12: Effect of Legislative Detail on Additional Economic Variables II

	(1)	(2)	(3)	(4)	(5)	(6)
	Small Est	Med Est	Large Est	Profit / Worker	Large Est Ratio	Large/Small Est Ratio
Legislative Detail	-0.00465	0.00896	0.0172	0.0296	-3.99e-05	-8.69e-05
	(0.00486)	(0.00993)	(0.0315)	(0.0188)	(0.000136)	(0.000425)
First Stage F-stat	14.84	14.84	14.84	181.3	11.04	11.04
Observations	821	821	821	549	798	798
State FE	X	X	X	X	X	X
Time FE	X	X	X	X	X	X
State Trends						

Notes. This table reports 2SLS estimates on a range of additional outcomes, showing that detail does not not affect the average firm size or profit per worker. All specifications include state and biennium fixed effect in the first column with the addition of state trends in the second column. Standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.13: Legislative Detail Shock Does Not Affect Spending, Taxes, or Political Control

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Spending		Legislative Spending		Taxes		Democrat Control	
Model	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Legislative Detail	0.0408	-0.0371	0.0339	-0.0309	0.0272	-0.0248	0.0296	-0.0268
	(0.0354)	(0.0326)	(0.0734)	(0.0620)	(0.0587)	(0.0524)	(0.187)	(0.172)
Observations	1183	1183	1183	1183	1183	1183	1123	1123
First Stage F-Stat	.	22.81	.	22.81	.	22.81	.	21.85
State FE	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X

Notes. RF and 2SLS effects on other outcomes. There is no effect on government spending, legislative spending, taxes, or political control. The first specification respectively includes state and biennium fixed effects, and the second adds state specific trends. All specifications have standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.14: Effect of Contingency, Additional Specifications

	(1)	(2)	(3)	(4)	(5)
	Effect on Real GDP Growth Per Capita				
Contingent -	0.0617*	0.0677**	0.120**	0.0637*	0.0642
Non-Contingent	(0.0230)	(0.0215)	(0.0377)	(0.0275)	(0.0390)
First Stage F-stat	31.67	38.33	22.6	33.61	15.24
Observations	1133	1122	1182	1132	1086
Time FE	X	X	X	X	X
State FE	X	X	X	X	X
State Trends	X	X	X	X	X
Lagged Govt Exp	X				X
Democrat Control		X			X
Topic Shares			X		X
Control for Lagged y				X	X
Econ Vars × Time					X
Sector Shares × Time					X
Demog Vars × Time					X

Notes. Effect of the difference in contingent and non-contingent clauses – 2SLS estimates. All specifications include time and state fixed effects, control for state trends and use standard errors clustered at the state level. Column 1 controls for lagged government expenditure. Column 2 controls for democratic control over the state. Column 3 includes the topic shares among the controls. Column 4 includes the lagged dependent variable. Column 5 includes all the aforementioned controls, adding the trends of economics variables, sector shares and demographic variables. **p<.01; *p<.05; +p<.1.

Table A.15: Effect of Contingent and Non-Contingent Clauses by Themselves

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Real GDP Growth Per Capita					
Detail (Contingent)	0.0199*	0.0185*	0.0443			0.0018
	(0.00797)	(0.00771)	(0.0575)			(0.0970)
Detail (Non-Contingent)			-0.0326	0.0166+	0.0153+	0.0133
			(0.0705)	(0.00839)	(0.00794)	(0.110)
First Stage F-stat	43.2	26.34	20.51	22.26	22.12	6.318
Observations	1182	1182	1182	1182	1182	1182
Time FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
State Trends		X	X		X	X
Contingency Control			X			
Non-Contingency Control						X

Notes. Additional contingency 2SLS specifications. There is an effect for contingent clauses by themselves, and a weaker effect of non-contingent clauses by themselves.

Table A.16: Cross-Tabulation: Terciles in Recent Detail and Economic Policy Uncertainty

	Terciles in Economic Uncertainty			
Terciles in Recent Detail	1st	2nd	3rd	Total
1st	83	125	164	372
2nd	107	121	142	370
3rd	179	130	79	388
Total	369	376	385	1130

Notes. This table shows that recent detail (concavity) and economic uncertainty recover different dimensions in the dataset.

Table A.17: Concavity Effects, with Residualized Previous Detail Ranking

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Recent Detail	*Low*			*Medium*		*High*	
Legislative Detail	0.0220+	0.018		0.0141	0.0211	0.00703	0.0173
	(0.0121)	(0.0113)		(0.0122)	(0.0168)	(0.0178)	(0.0237)
Contingent - Non-Contingent			0.0889*				
			(0.0403)				
First Stage F-stat	54.34	55.89	12.68	37.54	35.24	77.57	109.2
Observations	389	389	389	389	389	382	382
Time FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
State Trends		X	X		X		X

Notes. The main concavity results, but the previous detail variable is residualized on state and year fixed effects before making the ranking. Columns 1, 2 and 3 report results for states with lower tercile recent legislative detail. Columns 4 and 5 report results for those with average recent legislative detail and Columns 6 and 7 states with recent legislative detail in the higher tercile. All specifications include a first column with time and state fixed effects and a second column with the addition of state specific trends. **p<.01; *p<.05; +p<.1.

Table A.18: Uncertainty Effects, with Residualized Uncertainty Ranking

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Effect on Real GDP Growth Per Capita									
Economic Uncertainty	Low		Medium		High					
Legislative Detail	0.0201		0.0146		0.0191	0.0199+				
	(0.0152)		(0.0211)		(0.0132)	(0.0102)				
Detail (Contingent)							0.0624	0.0637		
							(0.0561)	(0.0759)		
							-0.0508	-0.0522		
Detail (Non-Contingent)							(0.0676)	(0.0857)		
Contingent -		0.0275		0.0613					0.0847+	0.113+
Non-Contingent		(0.0613)		(0.0547)					(0.0470)	(0.0588)
First Stage F-stat	39.95	3.998	2.42	9.512	6.721	257.2	3.578	4.873	6.87	9.285
Observations	362	362	381	381	355	355	355	355	355	355
Time FE	X	X	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X	X	X
State Trends						X		X		X

Notes. The main economic uncertainty results, but the uncertainty variable is residualized on state and year fixed effects before making the ranking. Columns 1-2 show results for states with lowest tercile uncertainty. Columns 3-4 report results for those with median uncertainty while Columns 5-10 states with uncertainty in the higher tercile. All specifications include state and biennium fixed effects, while for High Uncertainty states, results controlling for state specific trends are also included (as indicated). **p<.01; *p<.05; +p<.1.

Table A.19: Uncertainty Effects, with Lagged Economic Growth Control

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Economic Uncertainty	Low	Medium	High				
Legislative Detail	-0.0136	0.00796	0.0445*				
	(0.0115)	(0.0112)	(0.0169)				
Detail (Contingent)				0.142*	0.151*		
				(0.0567)	(0.0703)		
Detail (Non-Contingent)				-0.126+	-0.131		
				(0.0641)	(0.0804)		
Contingent - Non-Contingent						0.185**	0.202**
						(0.0463)	(0.0601)
Lagged Growth P.C.	0.454**	0.443**	0.186**	0.190**	0.172**	0.206**	0.172**
	(0.0840)	(0.0616)	(0.0616)	(0.0511)	(0.0588)	(0.0588)	(0.0625)
First Stage F-stat	48.45	3.599	24.33	8.971	8.198	9.488	9.484
Observations	335	348	363	363	363	363	363
Time FE	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X
State Trends					X		X

Notes. The main uncertainty results, but adding lagged growth per capita as a control. Columns 1-2 show results for states with lowest tercile uncertainty. Columns 3-4 report results for those with median uncertainty while Columns 5-10 states with uncertainty in the higher tercile. All specifications include state and biennium fixed effects, while for High Uncertainty states, results controlling for state specific trends are also included (as indicated). **p<.01; *p<.05; +p<.1.

Table A.20: Effects when Splitting by Terciles in Recent Growth

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Real GDP Growth Per Capita					
Recent Growth in State	Low		Medium		High	
Legislative Detail	0.0409*		-0.00129		0.00924	
	(0.0157)		(0.00964)		(0.00878)	
Contingent -		0.113*		0.00672		0.0812+
Non-Contingent		(0.0456)		(0.0391)		(0.0431)
First Stage F-stat	30.39	7.111	6.480	11.43	3.658	8.572
Observations	347	347	370	370	408	408
Time FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X

Notes. 2SLS estimates separating out by recent growth. Columns 1-2 show results for states with lowest tercile of recent growth. Columns 3-4 report results for those with median growth while Columns 5-6 states with recent growth in the higher tercile. All specifications include state and biennium fixed effects and use standard errors clustered by state. **p<.01; *p<.05; +p<.1.