DISCUSSION PAPER SERIES

DP15629
(v. 2)

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PUBLIC ECONOMICS

CEPR

ISSN 0265-8003

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Discussion Paper DP15629
First Published 03 January 2021
This Revision 25 April 2022

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JEL Classification: N/A

Keywords: N/A

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This paper analyzes the conditions under which more 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 output causes more economic growth. Guided by a simple model of reform decision-making under uncertainty, we find that the effect is driven by contingent clauses, that the effect is concave in the pre-existing stock of legislation, and that the effect size is increasing with economic policy uncertainty.

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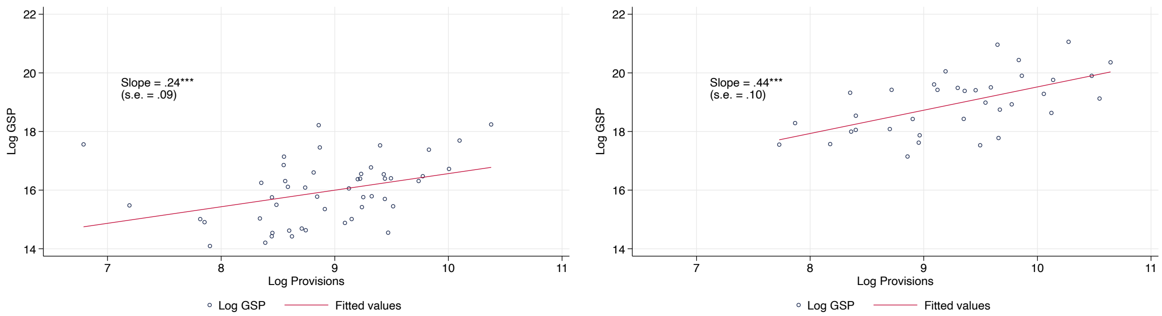
Figure 1: State GDP and Legislative Output, 1966 and 2012

A) State GDP vs. Provisions, 1966 (B) State GDP vs. Provisions, 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 legislative output and GDP in U.S. states, illustrated in Figure 1, 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 laws mechanically (as, for example, more industries need to be regulated), it could also be that more legislation (if well-designed) causes economic growth. Consider the introduction of detailed property rights protections, for example, or 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 lawmaking could hinder economic growth by increasing compliance costs (Niskanen, 1971, Botero et al., 2004). Even in an ideal world of benevolent legislators, therefore, one could postulate an optimal level of legal complexity given the current state of the economy, where moving toward the optimum from either side would increase growth.

Motivated by this debate, we explore the relationship between legislative output and economic output in the context of U.S. states, for the years 1965 through 2012. For each state and biennium, we produce a measure of legislative output from the text of state laws. The measure draws on recently developed methods in computational linguistics to detect *provisions,* legally relevant requirements in statutes (Vannoni et al., 2019). These provisions extract more information than coarser measures based on words or phrases. 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), which isolates exogenous variation in new provisions. Analogous to standard shift-share instruments that use *sector-specific economic flows* interacted with *pre-period sector shares*, we construct our instrument using *topic-specific legislative flows* interacted with *pre-period 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 at the state level. In other words, we assume that topic-specific national legislative flows are exogenous to each particular state, 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 (Borusyak and Jaravel, 2017, Adao et al., 2019, Goldsmith-Pinkham et al., 2020).

Our main result is that more state-level legislation due to the shift-share instrument tends to boost the state economy on average. 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).

Why do more laws increase growth rather than decrease it in the context of U.S. states? Previous work in other contexts, such as Italy, has found the opposite (e.g. Gratton et al., 2021). In the case of U.S. states, there are some important factors that could contribute to a beneficial effect of shocks to legal output: (1) the competition between states to attract businesses, (2) the greater information about reforms attainable from the adoption of such reforms from other states,[1] and (3) a relatively low value of reelection for state politicians (e.g. Diermeier et al., 2005). These factors contribute to a greater implied benevolence or alignment of state-level legislators, relative to legislators at the national level. In particular, as shown in Gratton et al. (2021), at the national level the signaling incentives of politicians tend to over-production of laws and a consequent negative relationship between legislative output and economic performance. At the state level, meanwhile, signaling incentives of politicians are not

[1] This diffusion aspect of state legislating is specifically captured by our shift-share instrument.

large enough to distort towards over-production.

To better understand the implications of greater alignment and implied benevolence of state-level legislators to voters' needs, we provide a formal model of the equilibrium consequences of benevolent legislating under uncertainty. In our model, the decision maker (DM henceforth) makes decisions about whether to enact proposed reforms. Due to noisy signals about the quality of the reform, the DM may make Type 1 or Type 2 errors. We derive conditions under which on average the DM approves "good" laws and then take the associated predictions to the data.[2]

First, we assess the level of precision in new legislation. In particular, reforms with contingent clauses are more targeted but have a larger expected implementation cost. Hence, the DM will more frequently filter the quality signal; conditional on passing, such reforms are more likely to be welfare-improving. To test this prediction, we provide separate textual measurements for new legislation containing contingent clauses (those containing "if", "except", etc.). Consistent with the prediction, we find empirically that the effect of increased legislating on growth is driven by contingencies.

A second condition noted in the model is on the complexity or difficulty in understanding a legislative proposal. In particular, when the stock of laws on a policy area is already quite large, the additional proposed reforms tend to be more technical and thus more difficult to interpret. Hence, the signal on proposal quality is more noisy, and the DM makes more mistakes. The resulting empirical prediction is that, on average, a boost in the number of reforms are more likely to contribute to growth in states where the existing stock of regulation is relatively less developed. That is, there should be a concave relationship between legislative output and growth.[3] Correspondingly, in the data, we find that the effect is stronger for states with a low-legislation baseline.

Third and finally, the model yields an additional prediction about the role of economic policy uncertainty (Baker et al., 2016). When such uncertainty is high, the likelihood that the reforms proposed have contingency clauses is higher. Because the DM is more selective for contingent clauses (as described above), the enacted reforms under uncertainty are more likely to be good for growth. Taking this prediction to the

[2]Appendix G provides an alternative and less formal model following the literature on endogenously incomplete contracts (Battigalli and Maggi, 2002, 2008). In that approach, we frame legislating as contract writing by a benevolent principal, who has to choose the level of completeness given the marginal benefit and the writing costs. The alternative model generates similar predictions about when and where the marginal increments in legislating can have positive effects on growth.

[3]Of course, there could be other ways to formalize concavity. A simple and intuitive alternative is decreasing marginal benefits in legislative detail.

data, we adopt the text-based approach of Baker et al. (2016) to local newspapers to produce a measure of economic policy uncertainty by state over time. The effect of increased legislation – 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, Davis, 2017). The main argument is that the costs of complying with regulation hinder new firm formation, competition, and innovation (Fonseca et al., 2001, Nicoletti and Scarpetta, 2003, Ciccone and Papaioannou, 2007, Braunerhjelm and Eklund, 2014). On top of that, regulatory compliance costs could disincentivize skill acquisition (Ciccone and Papaioannou, 2007). More nuanced work includes Kawai et al. (2018), who highlight the importance of complementarity in reforms, and Foarta and Morelli (2020), who identify conditions under which higher complexity could be either good or bad for the economy.

Perhaps reflecting the mixed theoretical results, the empirical literature is also mixed (e.g. Parker and Kirkpatrick, 2012). One set of papers document a positive correlation between legislative output and growth. For example, Mulligan and Shleifer (2005) show that population is positively related to the volume 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. A number of papers have used indexes for regulatory quality and shown a correlation with economic growth across countries or over time.[4]

[4]This literature concludes that better regulation and administrative simplification are good for the economy (Gørgens et al., 2004, Loayza et al., 2005, Djankov et al., 2006, Jalilian et al., 2007, Jacobzone et al., 2010). This strand uses different indices of regulatory burden from OECD surveys, World Bank‘s Doing Business, World Bank Governance Indicators, Amadeus database, UNIDO 3-IndStat, and Fraser Institute‘s Economic Freedom Index. All these measures rely either on the expert assessment of the regulation in a particular country/region/industry or objective measures such as the

A second set of papers present evidence for the negative view. Botero et al. (2004) show in a cross-country comparison 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. (2021) suggest that electoral incentives may create excessive reformism and deterioration of the quality of legislation, which negatively impacts economic growth.[5]

In the set of papers on the negative view, there are two recent papers using quantitative text analysis. Dawson and Seater (2013) show that in the U.S. time series, the number of pages in the Code of Federal Regulations is negatively related to overall national growth. Coffey et al. (2020) produce a panel dataset across industries since 1980, and find that stricter industry-specific regulation is associated with lower industry growth. Our model and discussion can help us understand why the effects are different in the context of U.S. states.

To recap, our paper contributes to this literature in a number of ways. First, we provide a new approach to text as data, employing a linguistically motivated measure of legal detail rather than simple word counts or page counts. Second, we provide a novel shift-share instrument based on legal-topic shares, so that our estimates have a causal interpretation. Third, we test a set of more subtle theoretical predictions that highlight the key features of our setting, as well as the relevance of contingency, concavity, and economic uncertainty.

The paper is organized as follows: Sections 2, 3, and 4 describe the data, text analysis methods, and empirical approach, respectively. Section 5 reports the main results and robustness checks on laws and growth. Section 6 introduces a model of legislative reform decisions and explores the more subtle theoretical predictions on contingency, concavity, and uncertainty. Section 7 concludes.

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number of procedures needed to start up a firm in a country/region/industry.

[5]Another important factor in settings like ours, with multiple neighboring jurisdictions, is that laws that affect growth could also potentially have spillover effects on these neighbors. Our empirical strategy requires that such spillovers are not in turn affecting our instrument. We return to this issue in the empirical strategy and conclusion.

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 Output)	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.

2 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 output, and control variables. The main summary statistics are reported in Table 1. A full list of variables with descriptions is shown in Appendix Table A.1. Additional summary statistics are shown in Appendix Table A.2.

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 $t-1$ 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. As mentioned, the laws are published annually or biennially. To ensure consistency, the dataset is built biennially, with the data point for year t including the laws from t and $t-1$.

The statutes can include new laws, amendments to existing laws (revisions), and repeal of existing laws (deletions). Ideally, one could distinguish the effects of amending and repealing provisions in terms of their effect on the stock of laws. In particular, repeal of clauses usually has a negative effect on the stock of laws, while amending of clauses could have a negative, neutral, or positive effect depending on what they replace. Unfortunately, our corpus does not provide a machine-readable indication of the original text that is being amended or repealed, so we cannot precisely determine the size of removals.[6] Hence, our main measure of legislative volume includes all types of provisions and does not distinguish amendments or repeals. Through qualitative inspection of

[6]Similarly, we cannot cleanly distinguish clauses that add regulations or remove them. So some of our estimated effects could be due to clauses that deregulate rather than regulate. An example of such a "deregulating" law is Texas Utilities Code Title 2.C Ch. 65, "Deregulation of certain incumbent local exchange company markets", enacted in 2005. While that law is taking away power from a telecommunications regulator, it still contains a number of quite detailed provisions. See `https://statutes.capitol.texas.gov/Docs/UT/htm/UT.65.htm`.

samples from the corpus, however, we could determine that amendments and repeals are a relatively small share of the text in the state session laws. Quantitatively, we proxy for the share of amendments and repeals by scanning for associated text signifiers ("amend*" or "repeal*"). Appendix Figure A.6 shows the time series for these shares over time, and they are relatively infrequent (about 3 percent repealing and less than 1 percent amending). In any case, the presence of amendments and repeals is not a problem for our empirical analysis as long as their frequency is not confounded with the instrument. Appendix Figure A.15 shows that, reassuringly, the instrument is not correlated with the share of either type of clause.[7]

The next issue is that the text from the state session laws corpus is produced from optical character recognition (OCR) applied to printed laws. From inspecting samples, the OCR is high quality. Appendix Figure A.5 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. To investigate this, we computed a proxy for OCR as the misspelling rate for common (non-proper) nouns. Appendix Figure A.6 shows the time series in the misspelling rate and it is low (about 3 percent) and smooth over time. These errors could add measurement error to the legislative output measure. Again, 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 legislating. We found that our instrument is not correlated with the misspelling rate (Appendix Figure A.14).[8]

Demographics. We link the data on economics and law to demographic data at 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).

[7]Appendix Table A.33 shows that we can also control for these variables in our regressions and it makes no difference.

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

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. Appendix Figure A.4 shows that our measure is highly correlated with a state-level measure recently developed by Baker et al's team for recent years (rank correlation coefficient = 0.41).

3 Text Analysis Methods

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

3.1 Measuring Legislative Output

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.3 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.7. 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.4. 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 output W_{st} is the number of legal provisions counted in the session laws for a state at biennium t. To assess proportional changes in provisions, 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. Word or page counts 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.2 shows that provision counts and word counts are correlated. In Appendix Table A.20 we explore variations on our analysis using word counts or page counts.

3.2 Allocating Laws to Topics

An essential ingredient in our analysis is to assign statutes to topics. For this purpose, 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 six (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.19 we 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 easier 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.5. Appendix Figure A.8 shows the legislation shares across these four categories over time.

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.

3.3 Measuring Contingency in Legal Language

Contingencies are a prominent feature of legal language because they impose more precise conditions under which legal actions will be made (Crawford and Ostrom, 1995, Frantz and Siddiki, 2022).[9] We measure contingency using a simple lexicon-based approach. Starting with several lists developed by linguists to indicate contingency, we searched for examples in the statutes to check which 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*}.

To see what this distinction looks like in context, Appendix Table A.6 shows examples of contingent provisions, while Appendix Table A.7 shows examples of non-contingent provisions. These are randomly sampled from the corpus to represent different states, years and topics. One can clearly see that non-contingent clause impose rigid requirements, while contingent clauses depend on some environmental factor.

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 3.2, we also compute topic-specific counts of contingent and non-contingent provisions by state-biennium.

Summary statistics are reported in Appendix B.4. About one-fifth of clauses are contingent. Appendix Table A.8 shows the changes in contingency across decades, showing that the share of contingent clauses has decreased over time, from 19.3% in the 1970s to 18.6% in the 2000s. Appendix Figure A.10 shows the time series for the share of contingencies by the four policy categories. Economic-regulation clauses have consistently had the highest degree of contingency.[10]

[9]For a more detailed discussion of this issue, see Appendix B.4.

[10]Consistent with our model (see Section 6.1), if economic regulations are the most complex and

4 Empirical Approach

4.1 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 output 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.[11]

Under strong identification assumptions, OLS estimates for ρ would procure a causal effect of legislative output 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. Our empirical strategy is designed to address these confounders.

4.2 Shift-Share Instrument for Legislative Output

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

require the most contingencies, the decision-maker is more likely to pass them only after receiving good information. This descriptive statistic could help explain why contingencies are the most effective for economic growth.

[11]The set of variables included differs by specification. For example, in the main 2SLS results (Table 4), we report results with pre-period economic covariates interacted with biennium effects (initial growth, initial GSP, and initial GSP per capita); controls for initial sector shares interacted by biennium; demographic characteristics (share of urban, foreign, and population) measured in the pre-treatment period interacted with biennium fixed effects; topic share controls; lagged government expenditures; and the lagged dependent variable. The specific variables in each column are listed in the respective table notes. Descriptions of the covariates with data sources are shown in Appendix Table A.1.

for legislative output. The instrument is constructed from the LDA topic shares (described in Section 3.2 above), as fully enumerated here.

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, 2016), 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., 2020). 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 (Borusyak and Jaravel, 2017, Jaeger et al., 2018, Adao et al., 2019, Goldsmith-Pinkham et al., 2020).

To link our setting to that of 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 output 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: $\frac{W_{s0}^k}{W_{s0}}$.[12]

[12]We include all topics in constructing the instrument, as recommended by Borusyak and Jaravel

The global "shifter" in our case is nationwide growth in topic-specific legislating, 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, $\frac{1}{49}\sum_{r\neq s}\Delta\log W_{rt}^{k}$, where r indexes the other 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 output 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.[13] The first stage equation for legislative output 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

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

that is, regressing the outcome directly on the instrument.

4.3 Instrument Validity

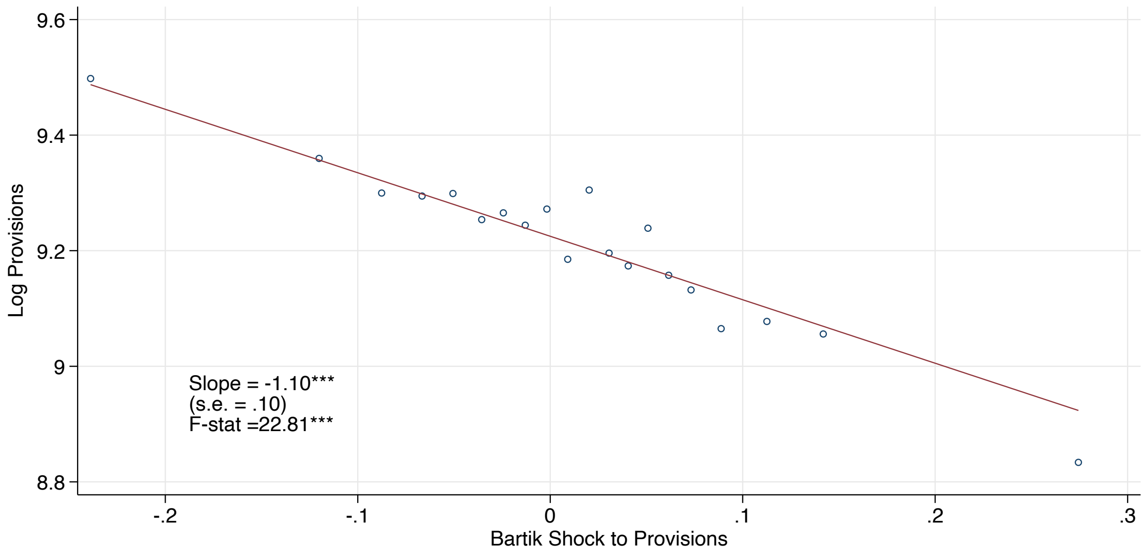
Figure 2 illustrates the first-stage relationship. The first stage statistics are consistent with instrument relevance. The estimate of ψ is statistically significant ($p = .003$). The Kleibergen-Paap first-stage F-statistic in the baseline specification is 22.8.

The first-stage relation between legislative flow and the instrument is negative. This is different from standard shift-share instrument for economic shocks, where the effect

(2017), relative to a situation where only a subset of shares is used for the instrument (as in Autor et al., 2016). Moreover, the use of pre-treatment shares is advisable in situations where shocks are serially correlated and shares are affected by lagged shocks.

[13] See Appendix Table A.9 for summary statistics on the instrument and endogenous regressor by decade.

Figure 2: First Stage: Impact of Shift-Share Legislative Shock on Legislative Output



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.

is positive. Our interpretation is that when a state had initially low detail on a topic, then it is more likely to increase legislating 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.11).

There are two approaches to identification in shift-share designs. In the first approach, one assumes that the pre-period shares are conditionally exogenous (Jaeger et al., 2018, Goldsmith-Pinkham et al., 2020). In this view, the exclusion restriction hinges 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. Using the definition of the instrument (2), Equation (5) implies instrument exogeneity. Equation (5) is a relatively strong requirement in most empirical contexts, however. 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 assumption is difficult to justify, since the pre-period legislation could be drafted in expectation of 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. Still, we show that we can pass the checks proposed by Goldsmith-Pinkham et al. (2020) and Jaeger et al. (2018) in the framework that assumes exogeneity of pre-treatment shares. Appendix Table A.12 shows that the instrument is uncorrelated with pre-treatment state characteristics. Appendix Figure A.13 shows that pre-treatment topic shares are uncorrelated with subsequent growth trends. These statistics lend support to the 'exogeneity of shares' assumption, which would suffice for instrument validity.

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. (2016), 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 and technicalities 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).[14]

[14]There are two additional identification issues that should be discussed. First, there is the issue of shared economic or political shocks across multiple states, which drive both legislation and economic growth. Economic crises like the Great Recession and the Covid Pandemic would be examples of such events. A similar issue is there for the classic Bartik (1991) instrument for employment and growth. All of our validity checks, for example the placebos for time and other variables, are designed to support our assumption that such joint shocks are second order, once integrated into the constructed instrument.

A second issue is how the instrument, and the resulting nudge to detail, impacts other neighboring states. There could be positive spillovers in the outcome due to gains from trade, for example, or

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.12. 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 132.8 and we reject the weak instrument null at 5 percent significance. Third, Appendix Table A.11 reports the following placebo test: we regress economic growth on future values of the legislative-growth instruments. The estimates are not statistically significant.[15] Fourth, we run a balance test by regressing the instrument on some potential confounders. Appendix Table A.13 shows the instrument is not correlated with current or lagged values for relevant state characteristics.

5 Main Results: Legislation and Growth

This section reports the main results. We provide an empirical test for whether greater legal output causes greater growth, or lower growth, at the margin.

5.1 Effect of Legislation on Economic Growth

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

negative spillovers due to migration of labor or capital. There may also be spillovers in the effect on legislative output. We assume that to the extent that these spillovers exist, they are second-order to the main effect of the instrument. Further exploring such spillovers is an important area for future work, as discussed in the conclusion below.

[15] See also Appendix Table A.16 for additional results on leads and lags of the effect.

Table 3: First Stage, OLS, and Reduced Form

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Provisions		Effect on Real GDP Growth Per Capita			
	FS	FS	OLS	OLS	RF	RF
Legislative Output			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 (FS) (Equation 3). Columns 3 and 4 show the results for OLS estimates of Equation 1). Columns 5 and 6 give the Reduced Form (RF) 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.

on growth, from Equation (4). As previously discussed, the reduced-form coefficient is negative, reflecting that lower pre-treatment detail on a topic is associated with a positive shock to legislative output. Additional specifications for OLS and reduced form are shown in Appendix Table A.14 and Appendix Table A.15, respectively.

2SLS estimates for ρ, the effect of legislative output 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 output 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 positive effect of legislative output on growth is robust. In U.S. states, 1965-2010, a 10 percent increase in legislative output would increase the per capita economic growth rate by .001 to .002, relative to a mean of 0.03. The 2SLS estimates have a similar magnitude to the OLS estimates from Table 3 (see

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Growth Rate Per capita						
Legislative Output	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. Descriptions of the covariates with data sources are shown in Appendix Table A.1.Standard errors clustered by state. **p<.01; *p<.05; +p<.1.

also Appendix Table A.14).

5.2 Robustness Checks for the Effect of Laws on Growth

In the Appendix we run a series of checks to the specification and to assess alternative channels for the results. First, Appendix Table A.16 reports regression estimates for leads and lags of the growth effect of increased legislative output. As with the main regressions, the current-period effect is positive and significant. The placebo lead (effect of next biennium's legislating) is a precisely estimated zero, especially upon the inclusion of controls (Columns 1-3). Meanwhile, the lagged effect is positive, suggesting an additional delayed effect in the subsequent biennium. The lagged effect is not statistically significant in 2SLS (Columns 4-7) but significant at the 10 percent level in the reduced form (Column 8).[16]

Next, we report a number of robustness checks in regard to the topics. 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.18). The results are not driven by any single topic (Appendix Figure A.16). Next, our results are not sensitive to the number of topics used in the construction of the instrument. Appendix Table A.19 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.17 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.20 to check alternative measures of legislative output. 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 (Column

[16]In particular, we note that in the specification with additional controls, the p-value for the lead is 0.86 (Column 3), while the p-value for the lag is 0.27 (Column 7).

Table 5: Effect of Legislative Output on Additional Economic Variables

	(1) GDP (Total)	(2) Population	(3) Employment	(4) Profits	(5) Wages	(6) Establishments
Legislative Output	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.

4).

5.3 Unpacking the Effect of New Laws

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 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.29 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.

Our instrument identifies an average effect that combines many factors across many

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

	(1)	(2)	(3)	(4)
	Effect on Real GDP Growth Per Capita			
Policy Category	*Fiscal*	*Economic Regulation*	*Social Regulation*	*Procedural*
Legislative Output	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.

different types of legislation. To further unpack the effect, we would like to know what category of policy is most important. As described in Section 3.2, we divide the LDA topics into the four more interpretable categories: fiscal policy, economic regulation, social regulation, and procedural. Thus we have four separate endogenous regressors W^l_{st}, 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 4.2, 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^l_{st} 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^l_{st} and instrument Z^l_{st} 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.29 that our effect is not driven by changes in government expenditures. So the fiscal-policy effect could instead be due to legal changes in how money is spent (for example imposing more monitoring or controls), rather than the amount spent.

So far, our analysis has left out potentially important additional sources of laws:

bureaucratic regulations and the courts. Appendix E provides a detailed analysis of the relevance of these alternative legal sources. We built auxiliary corpora of state regulations and state court cases to assess their relevance for our instrumental-variables analysis. We find that our instrument does not have a direct effect on these other legal sources, and that our main results hold when controlling for the volume of text from these other sources. Thus, we can rule out that our effects are driven by regulations or caselaw.[17]

6 Contingency, Concavity and Economic Uncertainty

This section provides a model of legislative reform decisions to motivate exploration of additional dimensions of the data.

6.1 A Simple Model of Legislative Reform

We analyze the task of a decision maker (DM henceforth) who decides every period whether to adopt a reform y. The reform can be understood as incremental legislation or regulation of an industry. The implementation costs of the reform $c > 0$ are known at the time of the decision; these costs can for example depend on the capacity of the relevant bureaucracy. The benefits of y are uncertain. The DM receives a signal (from experts, from her understanding, or from the relevant staff) on the potential benefits of the reform and then decides on adoption. The DM's calculus assesses whether the expected growth benefits of y outweigh the implementation costs, especially if being monitored by voters or other institutions creates accountability incentives to consider such costs.

Formally, there are two states of the economy θ: one where the reform y is *good* (θ^G) and one where it is *bad* (θ^B). The state is unknown to the DM. In turn, the DM has to assess the effect of the reform on the basis of her prior that the reform is good, $\kappa \in (0, 1)$, and on the basis of her signal q, which can be either good (q^G) or bad (q^B). The signal identifies the true state with precision $1 - z$, where $z \in [0, \frac{1}{2}]$ captures

[17]The dynamic regression from Appendix Table A.16 showing there is a significant impact effect, with a smaller lagged effect in the next biennium, and not much effect two bienniums later, is further in line with an effect of statutes, rather than regulations or caselaw, which would take longer. The effect timing also suggests that the effect is due to an increase in firm productivity, as measured in current profits and wages, rather than through investments, which would take longer to affect growth.

the difficulty of appraising the benefits of the reform on the basis of the signal.[18] The likelihood of the good state is updated according to Bayes' rule:

$$\kappa(q^G) = \frac{(1-z)\kappa}{(1-z)\kappa + z(1-\kappa)}, \qquad \kappa(q^B) = \frac{z\kappa}{z\kappa + (1-z)(1-\kappa)}.$$

If the DM rejects the reform, the status quo is maintained, and her payoff is normalized to zero (and so are the changes in growth prospects). If she adopts the reform, the resulting utility depends on the realized benefits and implementation costs and varies by state. Under the "reform is bad" state, the reform has a negative economic impact ($-l < 0$) and the DM receives negative utility $u(y|\theta^B) = -\ell - c$. Under the "reform is good" state, there is an economic benefit $v > 0$ and hence DM utility is $u(y|\theta^G) = v - c > 0$. Hence the DM is assumed to be *benevolent* in the sense that utility follows the growth prospects of the industry or economic factor targeted by the reform. The DM is rewarded when the reform is adopted appropriately in the good state of the world, while she is penalized if the reform is adopted when the state of the world turns out to be bad.

The DM maximizes expected utility. Hence, the DM adopts the reform if she expects positive effects on the economy net of implementation costs relative to the status quo – that is, if and only if

$$\mathbb{E}[u|q] = \kappa(q)u(y|\theta^G) + (1-\kappa(q))u(y|\theta^B) > 0 \quad \Leftrightarrow \quad \kappa(q)v - (1-\kappa(q))\ell > c.$$

We can thus characterize the decision of the DM as a function of her signal, as follows:

Lemma 1: *The reform y is adopted under s^G when*

$$\mathbb{E}[u|q^G] > 0 \quad \Leftrightarrow \quad (1-z)\kappa v - z(1-\kappa)\ell > c$$

Notice that noise z, which correlates with the complexity of the stock of legislation, depresses the expected benefits of reforms adopted on the basis of signal q^G. In words, when signals are noisy, the DM puts a lower weight on positive signals received in her updating.

[18] When $z = \frac{1}{2}$, the signal is uninformative, while when $z = 0$, the signal is perfect.

Lemma 2: *Under the bad signal q^B, the reform is adopted only when*

$$\mathbb{E}[u|q^B] > 0 \quad \Leftrightarrow \quad z\kappa v - (1-z)(1-\kappa)\ell > c.$$

Comparing the two conditions in the two lemmas, one can immediately observe that if reforms are adopted only when the signal is good, this should imply a larger expected growth benefit when the reform is adopted. However, the short-run incentives of the DM may induce her to adopt the reform even when the signal is bad – i.e., whenever the inequality in lemma 2 is satisfied.

A reform is accepted under the bad signal q^B if and only if the complexity parameter z satisfies the following condition:

$$z > \hat{z} \equiv \frac{(1-\kappa)(c+\ell)}{\kappa(v-c)+(1-\kappa)(c+\ell)} \text{ and } \hat{z} < 1/2. \tag{7}$$

$\hat{z} < 1/2$ requires that $(1-\kappa)(l+c) < \kappa(v-c)$, i.e.,

$$c < \kappa v - (1-\kappa)l. \tag{8}$$

In contrast, the reform is accepted under signal q^G if and only if

$$z < z^* \equiv \frac{\kappa(v-c)}{\kappa(v-c)+(1-\kappa)(c+\ell)}. \tag{9}$$

Thus:

Proposition 1:

1. *If (8) is violated (high enough c), then the reform is adopted if and only if the DM receives a good signal and $z < z^*$.*

2. *If (8) holds, then: (a) the DM adopts the reform under both signals for $z \in (\hat{z}, 1/2]$; and (b) the DM adopts the reform only under a good signal when $z \leq \hat{z}$.*

For a given implementation cost c, the prediction is that when κ and $v-l$ are sufficiently high, then there exists a range of complexity values under which, due to noise, the DM makes the reform regardless of the signal. On the other hand, if complexity is low enough, then the DM approves only after a good signal. In such cases, there is an expected increase in growth prospects conditional on the reform passing.

Now assume that the reform can be either simple or complex, y_s or y_c, with $c(y_c) > c(y_s)$. That is, the implementation cost of a more complex reform is higher. In our setting, a complex reform is one where more contingent clauses are added. These additional contingencies and exceptions need to be implemented and monitored by the bureaucracy.

Corollary 1: *When the reform considered is more complex, it is more likely that (8) does not hold. Hence, it is more likely that the DM adopts the reform only after a good signal. In other words, conditional on being adopted, a reform with more contingent clauses yields higher growth prospects.*

6.2 Testable Predictions

We have assumed that the DM is benevolent – proposals are enacted to maximize the public welfare. There are three reasons to believe that U.S. state legislators are relatively close to acting benevolently, especially when they are adopting legislation consistently with our shift-share instrument. First, there is a lower value of holding a state politician seat versus a federal politician seat, which reduces the value of signaling. Second, state legislators act in a federalist system with significant competition to attract businesses. Third, our instrument captures learning and diffusion of information across states, making it easy to borrow the best laws and policies.[19]

Since the DM is benevolent, she adopts the reform (adding the necessary legislation) only when it benefits the economy in expectation. Reforms can occasionally incur losses ex post, but effects should be *on average positive* if the DM's expectations are unbiased. Thus, if the DM is benevolent, the first baseline prediction is

H0 *(Benevolence hypothesis)*: *On average, legislative reforms are good for the economy.*

This hypothesis is expected to hold for U.S. state legislators because of the low signaling incentives, competition across states, and the diffusion of information. All our results in section 5 confirm this hypothesis.

Corollary 1 allows us to introduce the first hypothesis emerging from the model:

[19]This situation is different from settings with signaling incentives, like in Italy due to political instability, or like in the U.S. federal case. In those cases, the benevolence assumption may not hold. Note that while lobbying is not explicitly modeled in our framework, it is likely still important in this context (Bombardini and Trebbi, 2020). Lobbying could be added either through distortions of the DM's objective function, or through creation or elimination of noise in the information (higher or lower z in the model). That would not change the main comparative statics. Empirically, we show that our results are not sensitive to controlling for lobbying rules in Appendix Table A.35.

H1 *(Contingency hypothesis): The reforms that are best in expectation for growth prospects are those that add more contingent clauses, making legislation or regulation go in the direction of a more complete contract.*

A second hypothesis emerging from our model is about the marginal expected impact of a reform for different status quo conditions:
H2 *(Concavity hypothesis): A higher baseline stock or complexity of previous legislation yields noisier signals about which elements need reforms. Hence, the expected benefits from a reform are decreasing in the previous stock of enacted legislation.*

More noise in the DM's information reduces the expected benefits of the reforms (when $z > \hat{z}$). Simple environments yield more precise signals. This should imply higher benefits *on average* when the existing stock of legislation is low.[20]

Another source of concavity of the expected benefit function is that contingent clauses should be added in order of importance. In scarcely regulated markets, a number of interventions with high κ and precise signal (z low) are still to be taken. Indeed, in an incremental framework, if there are n possible reforms $(y_k)_{k=1}^n$ associated with information $((\kappa_k, z_k, s_k))_{k=1}^n$, the reforms with the highest expectated benefit will be adopted first.[21]

A final hypothesis that we can bring to the data concerns the role of increases in economic uncertainty. When economic uncertainty increases, the benefits from a greater completeness of the law typically increase because more contingencies need to be covered. Given that reforms that include a greater number of contingent clauses can be expected to be more complex, and hence have higher implementation costs, the same logic of corollary 1 applies. Thus:
H3 *(Uncertainty hypothesis): The greater is economic uncertainty, the greater the likelihood that adopted reforms can induce growth.*

Summing up, all the above testable predictions derived from our model are related

[20] A source of decreasing net returns from additional details may be due to the greater difficulty to assess the effects of regulations due to the interactions with other laws in the baseline environment. Some laws are complementary while others may not go well together, incurring reduction of benefits or even negative marginal effects. In a more detailed baseline legislative environment, it is more difficult to draft laws that are complementary and do not harm each other, reducing the *average* marginal benefit of additional complexity.

[21] The states with a low level of regulation typically have more precise signals on the reforms that are good to take, and they will begin with those with the highest κ. A state with a high level of regulation have already taken the best measures, and it becomes more difficult to understand where to intervene because remain the newly proposed policies are less certain (lower κ, more difficult to assess, i.e., higher z).

to an important, perhaps counter-intuitive, idea: when a situation requires an elaborate reform, the reform should be expected to be beneficial, because more elaborate state laws need more careful consideration and the DM is expected to use all potential signals. In such circumstances, the DM writes new rules for key edge cases, rather than writing a larger number of (perhaps shorter) laws without filtering.

We note that an incomplete contracting framework can deliver similar testable predictions. As discussed in further detail in Appendix G, a law can be viewed as an incomplete contract between the legislator (the principal) and the citizens (agents). In this setup, a benevolent principal adopts reforms when such incremental reforms bring the menu of contracts closer to a level of optimal completeness given the implementation costs. For example, rules with contingencies require anti-fraud agencies to ensure that agents indeed satisfy the claimed contingencies, thus requiring monitoring, more complex record-keeping, and human resources to make the contingency-dependent implementation effective. It follows that the more contingent clauses there are, the more costly is the administrative apparatus needed for their correct implementation. Given any level of implementation capacity, such costs may rise in a convex fashion with the level of complexity adopted. Proposition 1 in Battigalli and Maggi (2002) tells us that a good reform should have contingent clauses for the most important topics, and gives an additional robustness to our corollary 1 and related hypotheses: when an important reform is being considered, it is typically more complex in terms of contingent clauses, and hence the DM adopts the reform under the good signal. Finally, even H3 is broadly consistent with the results of Battigalli and Maggi: the more uncertain are the relevant situations, the more important it should be to account for different possible contingencies.

6.3 Contingency

The **contingency** hypothesis (**H1**) states that the effect of legislating on growth should be driven by contingent (rather than non-contingent) clauses. As described in Subsection 3.3, 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 non-contingent measures of laws as joint endogenous regressors. The second stage is

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho_C \log W_{st}^{C} + \rho_N \log W_{st}^{N} + X_{st}'\beta + \varepsilon_{st} \qquad (10)$$

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

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

$$\log W_{st}^N = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi_C Z_{st}^C + \psi_N Z_{st}^N + X_{st}'\beta + \eta_{st}^N \tag{12}$$

where all terms are as above. Appendix H reports additional checks and results 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

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho_{CN}(\log W_{st}^C - \log W_{st}^N) + X_{st}'\beta + \varepsilon_{st} \tag{13}$$

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:

$$(\log W_{st}^C - \log W_{st}^N) = \alpha_s + \alpha_t + \alpha_s \cdot t + \psi_C Z_{st}^C + \psi_N Z_{st}^N + X_{st}'\beta + \eta_{st} \tag{14}$$

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 (10) with two endogenous regressors (contingent and non-contingent), instrumented by first stages (11) and (12). 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 (13) with first stage (14). Consistent with the separate-treatments specification, there is a large positive effect of relative use of contingency.

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						
Contingent Provisions	0.0638**	0.0590**					
	(0.0226)	(0.0215)					
Non-Contingent Provisions	-0.0559*	-0.0511*					
	(0.0242)	(0.0228)					
Contingent -			0.0752**	0.0697**	0.0501*	0.0379*	0.0773**
Non-Contingent			(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 10) and First Stages 11 and 12), 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 13) and First Stages 14). 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.

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 provisions – three to four times as large. This is additional support that the part of legislative output that contributes to growth are contingent clauses.[22]

Appendix H reports a number of supporting results. Appendix Table A.30 reports additional specifications with the differenced treatment variable, showing that it is robust to inclusion of other variables. Appendix Table A.31 shows the effects for other intermediate economic outcomes. Appendix Table A.32 shows the results when using contingency and non-contingency counts by themselves as the endogenous regressor.

Finally, Appendix Table A.34 shows that there is effect heterogeneity by the structure of party control in the state government branches (House, Senate, or Governor). The effect is larger when Democrats have control of more branches. It is also stronger under divided government (when each party controls at least one branch). The former result could be interpreted that Democrats are more likely to accept and enact diffused legislative proposals that are helpful for growth. The latter suggests that divided government more greatly constrains the enactment of legislation, such that the instrumented push for laws has a larger effect.

6.4 Concavity

Next, the concavity hypothesis (**H2**) states that the effect of adding laws should be larger in contexts with a relatively low pre-existing stock of laws. Unfortunately, historical data on the stock of legislation (the annotated code) is not available. Instead, we proxy for the stock using recent levels of the flows – in particular the number of provisions issued in the state over the last five bienniums (ten years). The idea is that, at any given point, the ranking of states by the historical flow of provisions approximates the ranking of states by the total stock of provisions. Correspondingly, we rank the the state-biennium observations by recent detail and then split the sample into three terciles by that ranking.

We then estimate the baseline 2SLS system (Equations (3) and (1)), but subsetting

[22] From Appendix Table A.8 and Appendix Figure A.9, we see that the log difference in contingencies and non-contingencies is actually slightly decreasing. So the overall aggregate predicted change in output due to changes in legislative volume over this period may be negative.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Real GDP Growth Per Capita						
Recent Legal Detail	*Low*			*Medium*		*High*	
Legislative Output	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 output (previous five biennia). Columns 1 through 3 report results for states with lower tercile recent legislative output. Columns 4 and 5 report results for those with average recent legislative output and Columns 6 and 7 states with recent legislative output 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.

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 (14) and (13)). According to **H2**, we would expect a larger effect of new laws in the tercile with lowest previous detail.

Results are shown in Table 8. Consistent with our prediction, we find that the effect of new laws on economic growth is stronger for states with low recent legal volume (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). Overall, these findings provide support for the concavity hypothesis (**H2**). Indeed, the evidence suggests this mechanism is quite important: at any given year, most states have too large a stock for additional laws to help with growth.

Appendix Section I provides additional specification checks for the concavity analysis. In particular, Appendix Table A.37 shows that we get similar results when the concavity thresholds are computed after residualizing on the state and year fixed effects.

6.5 Uncertainty

Finally, we test the **uncertainty** hypothesis (**H3**), which states that legislative output – in particular, contingent clauses – should boost growth more under higher economic uncertainty. Using the measure of local economic uncertainty described in Subsection 2, 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 new provisions 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. 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) and Table 7 (contingency).

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 output, 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 I provides additional specification checks for the uncertainty analysis. In particular, Appendix Table A.36 shows that concavity and uncertainty recover independent dimensions in the dataset. In addition, Appendix Table A.38 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.39 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

Table 9: Testing Uncertainty: Effect of Laws 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 Output	0.00448		0.00699		0.0373*	0.0391*				
	(0.0111)		(0.0111)		(0.0153)	(0.0176)				
Contingent Provisions							0.145*	0.170*		
							(0.0560)	(0.0672)		
Non-Contingent Provisions							-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.

uncertainty measure picking up the business cycle. Also consistent with this point: Appendix Table A.40 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 legislative output 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.

7 Conclusion

This paper explores what makes new laws 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 – although that average result conceals important heterogeneity. This heterogeneity is revealed by additional empirical analysis motivated by a model of legislative reform. We find that the positive impact on growth is higher when the additional legislation is in the form of contingent clauses, when starting from lower initial levels of legal detail, 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 output from the text of state laws based on tools from computational linguistics. Second, we implement a text-based shift-share instrumental variables strategy that isolates exogenous variation in legislative output to examine causality.

Although the external validity of our empirical results is an open question, the theoretical mechanisms should apply more broadly to other domains and to other countries. In principle, similar results could hold at the country level or even at the global level, with a shock to growth across countries mediated by legislative reforms. The recent wave of public health legislation during a global pandemic, for example, could have diffused to other countries at low cost. Yet external validity to other countries is questionable due to different institutions. As shown in Gratton et al. (2021), signaling incentives can have a strong effect on the quantity and quality of laws. The predictions on the effects of legislative output 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 help of specialized agencies in the drafting of legislative proposals (Bendor, 1995), or the selected pool of proposals considered (or already passed) in other states. Overall, more research is needed to understand the broader applicability of these results across time and across countries and to situate them in the broader research agenda on laws and growth.

Our methods and results open doors to research in a number of new directions. Methodologically, there could be further extensions on the econometrics of shift-share instruments using legal diffusion, and text-based instruments more generally. Such explorations could use simulations and other methods to better understand the robustness of such instruments, and in particular their sensitivity to different pre-processing or featurization steps.

Substantively, it could be interesting to extend the approach to allow for spillover effects of laws on neighboring states. The economic policies identified in our study could have both positive spillovers – for example through gains from trade – and negative spillovers – for example through displacement of labor and capital. Understanding these spillovers would make an important contribution but would also pose a formidable empirical challenge. One could use data on county development at state borders, for example, potentially using job-to-job transfers and residence location versus work location data. Beyond the U.S. states, the text methods and empirical approach could be applied to a number of new domains. For example, it could be possible to test some of the predictions from the literature on endogeonusly incomplete contracts (e.g. Battigalli and Maggi, 2002). In patents or social media, the methods could be applied to understand the diffusion and impacts of the spread of innovations and ideas.

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Online Appendix

A Data Appendix

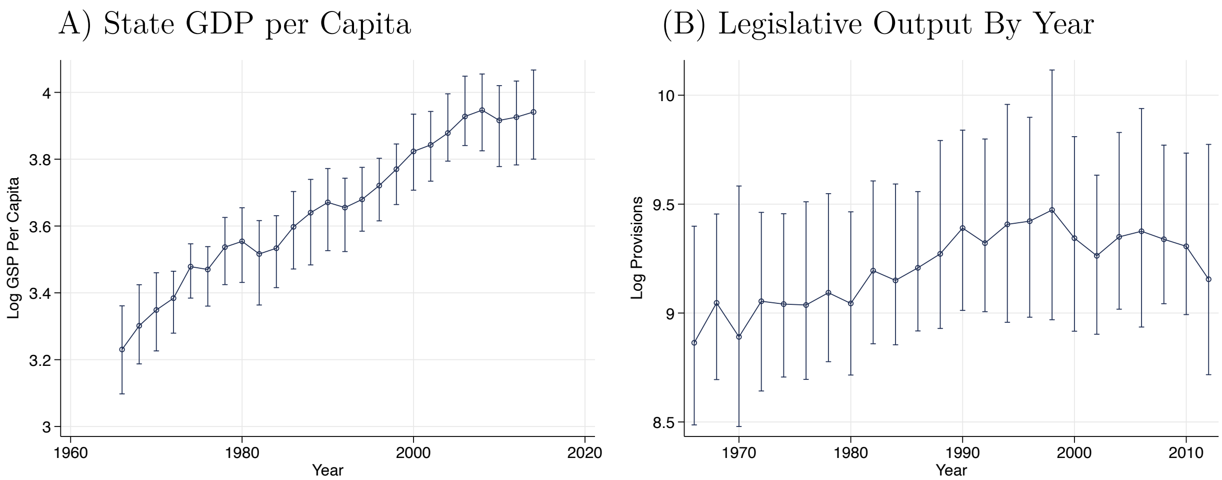
Table A.1: List of Variables with Source and Description

Variable	Data Source	Description
Economic Outcomes		
Log Real GSP	BEA Regional Accounts	Logged gross State Product deflated ti 2007 values using state CPI
Log Real GSP Growth	BEA Regional Accounts	Biennial growth in log real GSP
Log Real GSP Per Capita	BEA Regional Accounts	Log of GSP divided by population
Log Real GSP Per Capita Growth	BEA Regional Accounts	Biannial growth in Log Real GSP per capita
Growth Number Estabilishments	County Business Patterns	Growth of logged number of establishments
Log Establishment Profit Growth	County Business Patterns	Growth of logged establishment profits
Log Employment Growth	County Business Patterns	Growth of logged employment
Legislation		
Shock to Provisions	State session laws	Instrument for logged number of legal provisions in state statutes
Shock to Contingent Provisions	State session laws	Instrument for logged contingent legal provisions in state statutes
Shock Non-Contingent Provisions	State session laws	Instrument for logged non-contingent legal provisions in state statutes
Log Provisions	State session laws	Logged number of legal provisions in state statutes
Log State Statute Words	State session laws	Logged number of words in state statutes
Log Contingent Provisions	State session laws	Logged number of contingent legal provisions in state statutes
Log Non-Contingent Provisions	State session laws	Logged number of non-contingent legal provisions in state statutes
Log Share Amend Sentences	State session laws	Logged share of sentences that contain 'amend'
Log Share Repeal Sentences	State session laws	Logged share of sentences that contain 'repeal'
Log Statute Misspelling Rate	State session laws	Logged OCR error rate in in state statutes
Covariates		
Log Population	Klarner (2013)	Logged population
Urban Population	Ujhelyi (2014)	Logged urban population
Democratic Control	Klarner (2013)	Number of bodies under democratic control
Log Income	Klarner (2013)	Logged labour income
Log Expenses	Klarner (2013)	Logged government expenditures (in 1000s current dollars)
Log Legislative Expenses	Klarner (2013)	Logged legislative expenditures (in 1000s current dollars)
Log State News Uncertainty	newspapers.com	Logged number of articles mentioning the phrase 'economic uncertainty'
Log Real Tax Per Capita	Klarner (2013)	Logged per capita taxation deflated to CPI
Log Real General Exp p. Capita	Klarner (2013)	Logged per capita expenses deflated to CPI
Log Campaign Contributions	opensecrets.org	Logged dollars spent in campaign contributions
Campaign Finance Regulation	Book of the States	Openness of campaign finance contribution regulation
Regulation and Courts		
Log Federal Regulation	QuantGov	Federal Regulation and State Enterprise (FRASE) index
Log State Regulation Words	LexisNexis	Log word count in state regulations, from the Regulation Report database
Log State Court Opinion Words	LexisNexis	Log word count in state court opinions, from Court Opinions database

Table A.2: Extended Summary Statistics

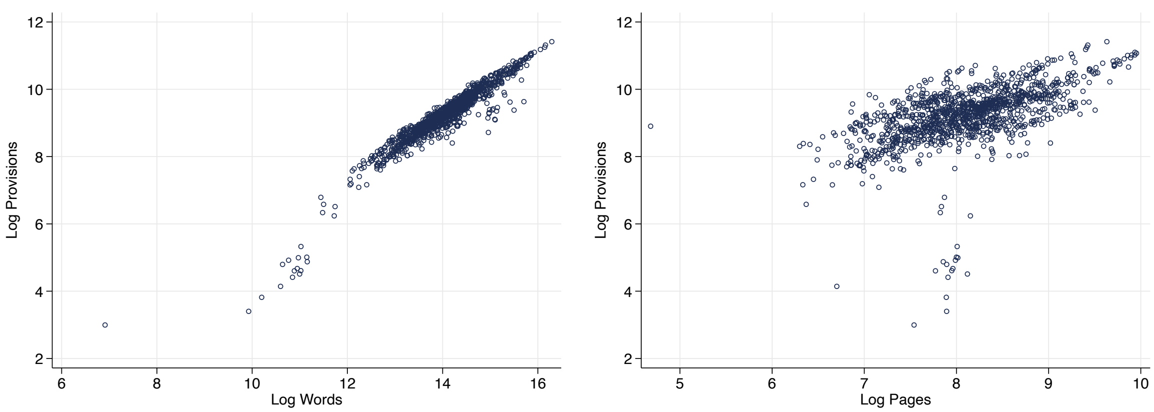
Variable	N	mean	std dev	min	max
Economic Outcomes					
Log Real GSP	1,250	17.79	1.436	14.09	21.54
Log Real GSP Growth	1,250	0.134	0.0701	-0.0870	0.665
Log Real GSP Per Capita	1,250	3.652	0.281	2.803	4.844
Log Real GSP Per Capita Growth	1,249	0.0314	0.0502	-0.174	0.332
Growth Number Estabilishments	823	0.0451	0.0584	-0.146	0.409
Log Establishment Profit Growth	550	0.163	0.109	-0.403	0.818
Log Employment Growth	823	0.0568	0.0643	-0.151	0.930
Legislation					
Shock to Provisions	1,183	0.0131	1.025	-2.191	2.563
Shock to Contingent Provisions	1,183	0.00371	1.023	-2.035	2.803
Shock Non-Contingent Provisions	1,183	0.0100	1.031	-2.218	2.629
Log Provisions	1,183	9.211	0.887	2.996	11.42
Log State Statute Words	1,183	14.03	0.833	6.912	16.29
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
Log Share Amend Sentences	1,159	-3.619	0.576	-7.321	-2.098
Log Share Repeal Sentences	1,159	-5.496	0.897	-11.79	-2.240
Log Statute Misspelling Rate	1,183	0.0306	0.00699	0.0131	0.0649
Covariates					
Log Population	1,250	14.94	1.029	12.51	17.47
Urban Population	1,248	0.635	0.144	0.359	0.887
Democratic Control	1,127	1.802	1.057	0	3
Log Income	1,250	3.479	0.267	2.563	4.144
Log Expenses	1,250	15.57	1.471	11.89	19.46
Log Legislative Expenses	1,250	9.410	1.384	5.176	12.73
Log State News Uncertainty	1,208	-4.628	0.348	-6.044	-3.479
Log Real Tax Per Capita	1,250	0.721	0.370	-0.466	2.715
Log Real General Exp p. Capita	1,250	1.348	0.418	-0.0226	2.879
Log Campaign Contributions	659	16.32	1.674	9.179	20.26
Campaign Finance Regulation	1,199	0.990	0.712	0	2
Regulation and Courts					
Log Federal Regulation	450	13.75	0.184	13.24	14.43
Log State Regulation Words	444	14.55	1.757	0	16.84
Log State Court Opinion Words	1,183	14.77	1.049	12.335	18.267

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



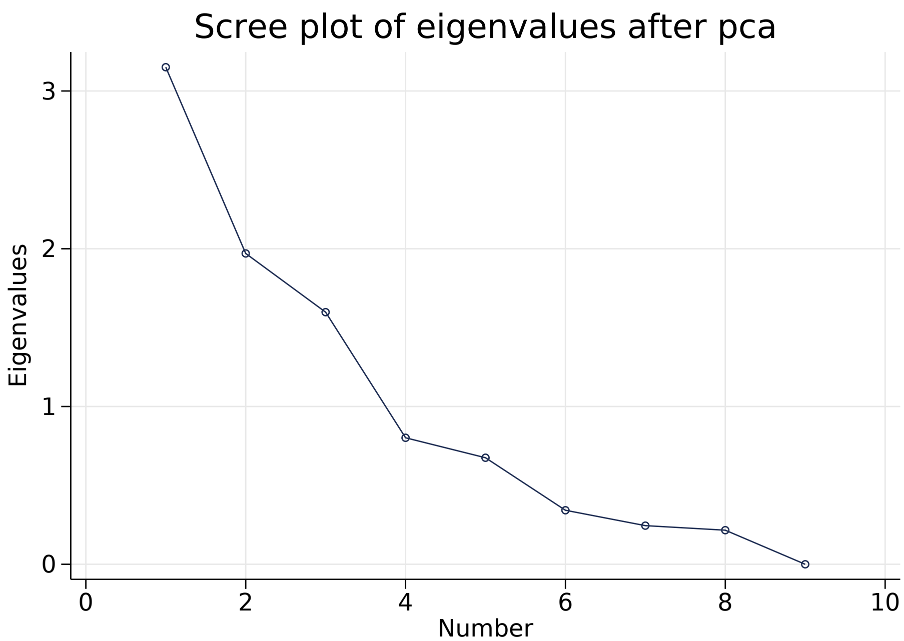
Notes. Line graphs showing the mean of (log) private GDP (Panel A) and (log) provisions (Panel B) across states over time. Error spikes give 90% confidence intervals from standard errors of the mean.

Figure A.2: Scatter Plots of Provisions vs. Word Counts and Page Counts



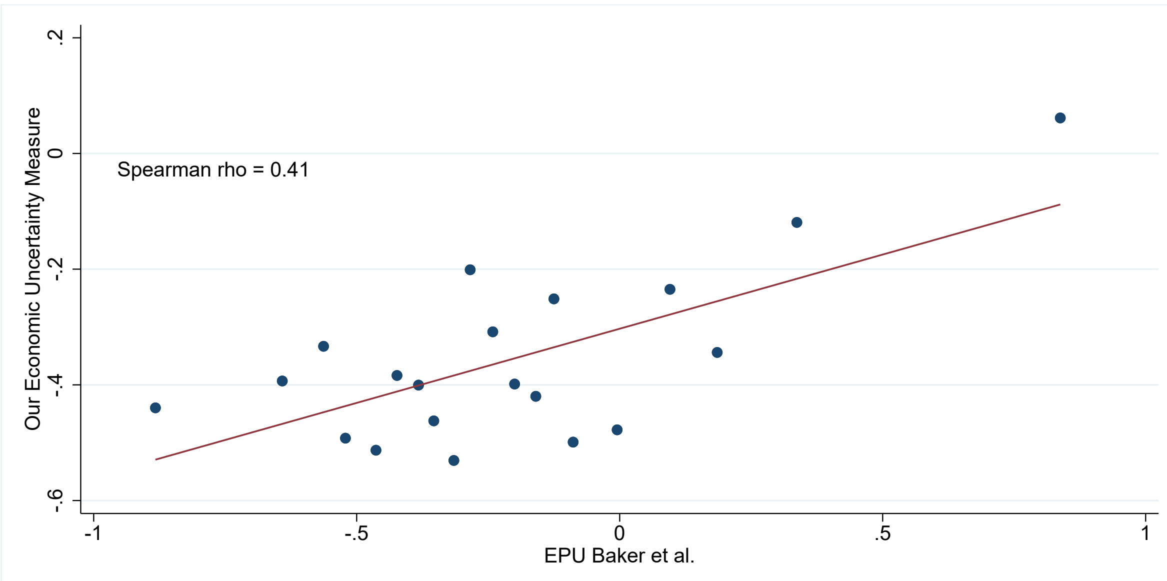
Notes. The figure shows a scatter plot for the relationship between (logged) words in the left panel and (logged) pages in the right panel and (logged) provisions, respectively.

Figure A.3: 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.

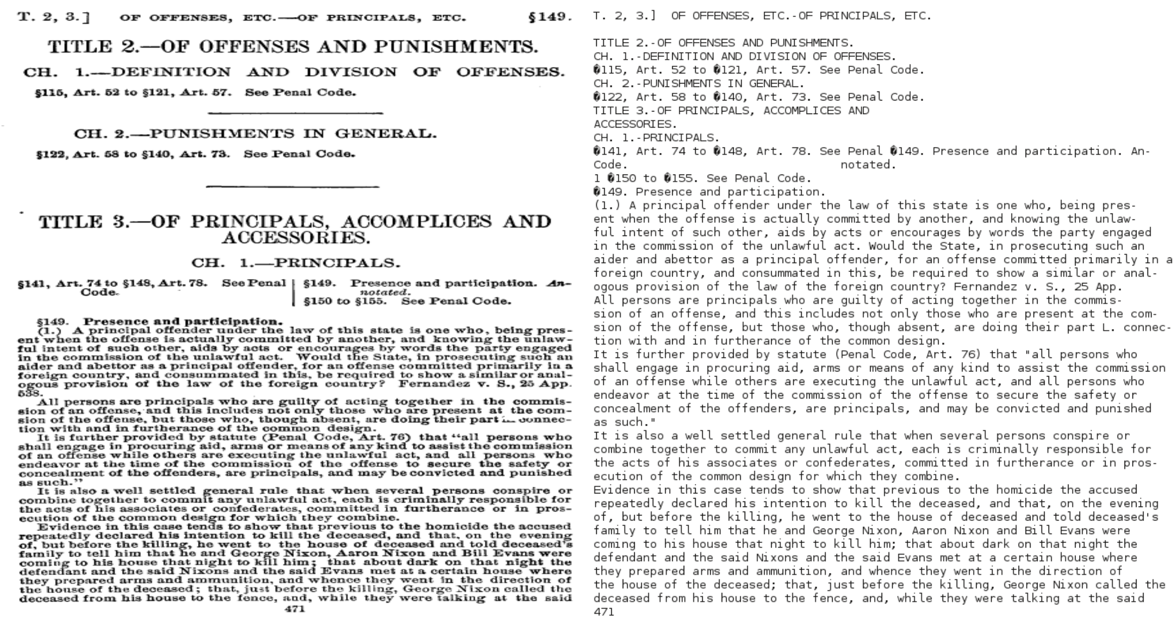
Figure A.4: Economic Policy Uncertainty Index



Notes. Binned scatterplot showing the cross-sectional relationship between our measure of economic uncertainty and the state-level one from policyuncertainty.com, available since the 1990s for most states. The regression model represented includes year FE. The Spearman rank correlation coefficient is 0.41 (rejecting the null hypothesis that the ranks of the two variables are independent).

B Details on Text Features

Figure A.5: State Session Laws Corpus



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

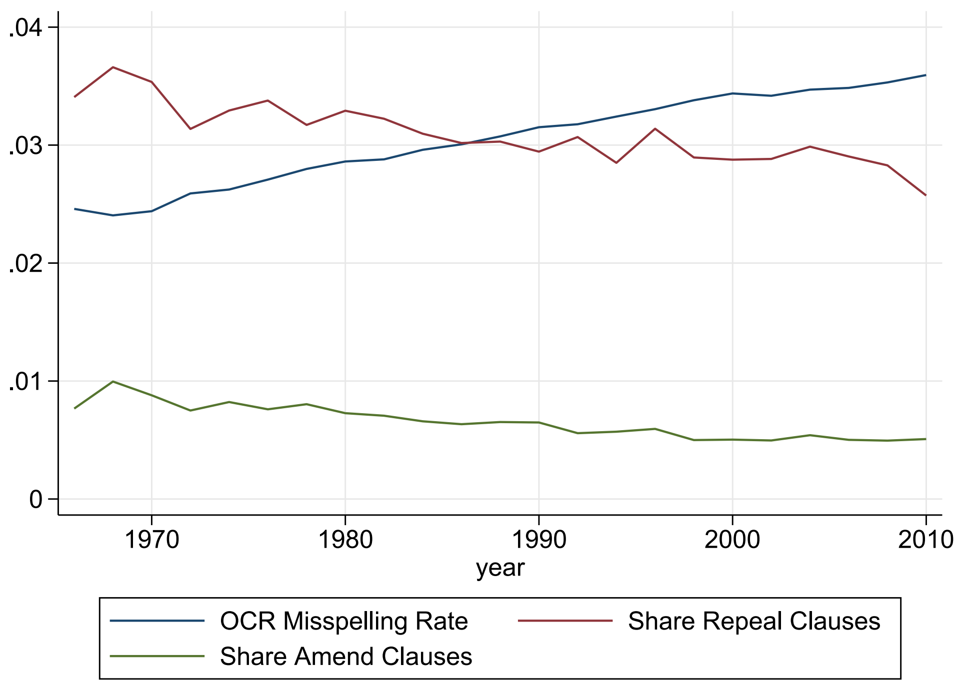
TITLE 2.-OF OFFENSES AND PUNISHMENTS.
CH. 1.-DEFINITION AND DIVISION OF OFFENSES.
�115, Art. 52 to �121, Art. 57. See Penal Code.
CH. 2.-PUNISHMENTS IN GENERAL.
�122, Art. 58 to �140, Art. 73. See Penal Code.
TITLE 3.-OF PRINCIPALS, ACCOMPLICES AND
ACCESSORIES.
CH. 1.-PRINCIPALS.
�141, Art. 74 to �148, Art. 78. See Penal �149. Presence and participation. An-
Code.                                   notated.
1 �150 to �155. See Penal Code.
�149. Presence and participation.
(1.) A principal offender under the law of this state is one who, being pres-
ent when the offense is actually committed by another, and knowing the unlaw-
ful 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 anal-
ogous provision of the law of the foreign country? Fernandez v. S., 25 App.
All persons are principals who are guilty of acting together in the commis-
sion of an offense, and this includes not only those who are present at the com-
sion of the offense, but those who, though absent, are doing their part L. connec-
tion 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 pros-
ecution 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
```

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

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.

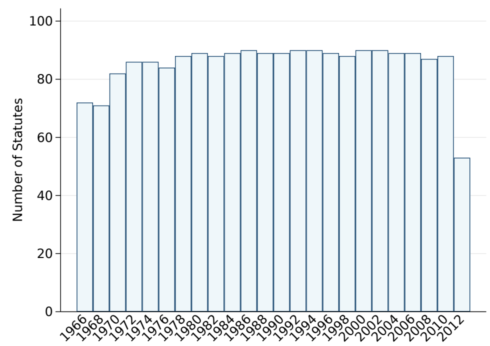
Figure A.6: Time Series: Amend Share, Repeal Share, and OCR Error Rate



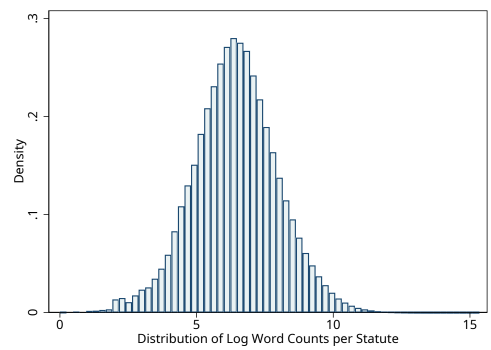
Notes. Time series for the share of amending sentences, share of repeal sentences, and the OCR misspelling rate in the state session laws corpus, over the time period of our analysis. OCR error rate computed as share of common nouns (identified with automated POS tagger) that are not in the open-source dictionary WordNet.

Table A.3: Summary Statistics on Statute Segmentation

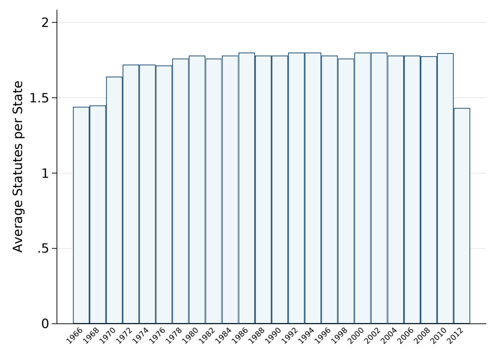
(A) Histogram: Number of Statutes by Biennium



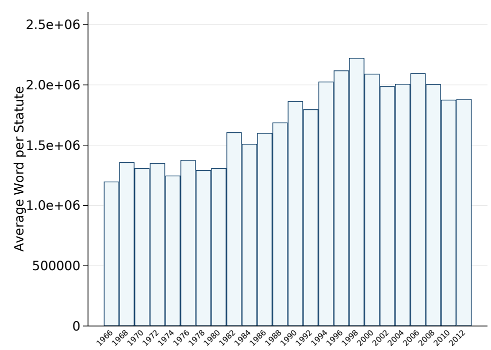
(B) Histogram: Number of Words per Statute



(C) Statutes per State, by Biennium

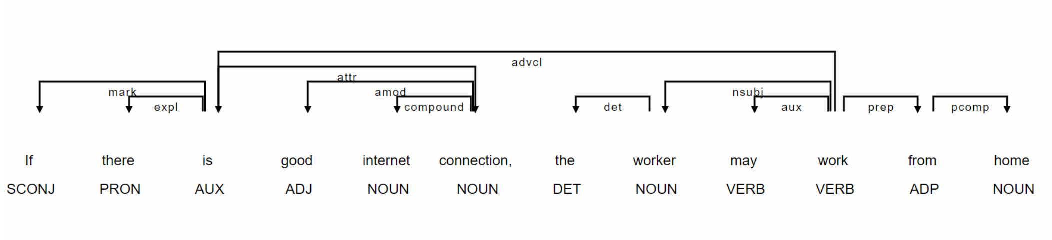


(D) Average Words per Statute, by Biennium



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).

Figure A.7: 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.

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. Table A.4 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.

Figure A.7 shows the result of the syntactic parser. The dependency parser tells us whether 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).

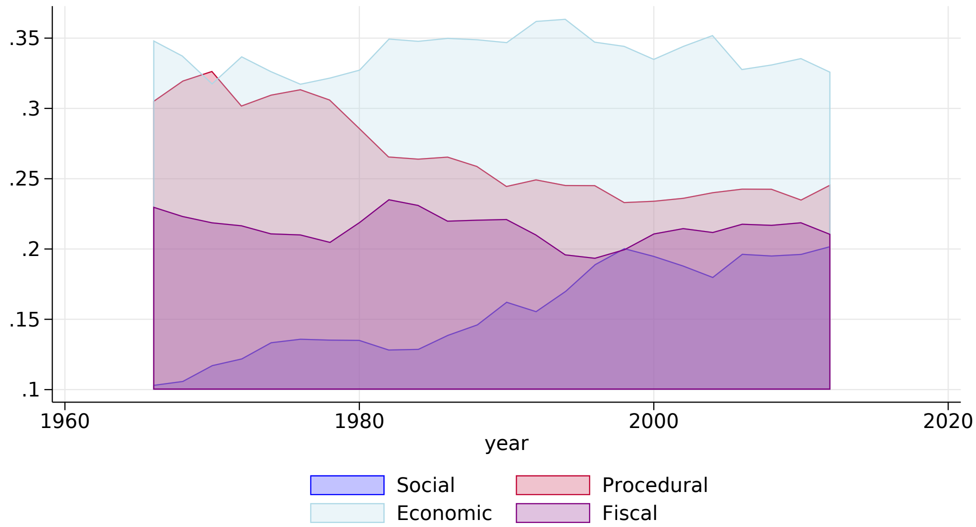
Table A.4: Types of Legal Provisions, with Extraction Rules

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.7is a permission: "The worker may work".

Figure A.8: 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.

B.3 Details on Legislative Topics

Table A.5 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.

Table A.5: 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

B.4 Details on Contingency

An established literature on policy design and legal linguistics has emphasized the special relevance of contingencies in legal texts. In particular, the so-called "institutional grammar" has been used to study how legislation is written. This approach, which builds on the seminal paper by Crawford and Ostrom (1995), extracts relevant semantic features of the language in legislation. One of these semantic features are contingencies (what some of the literature calls "conditions"), which define the scope of application of the provision.

Take as an example the following sentence from U.S. state organic farming legislation (Frantz and Siddiki, 2022) as an example:

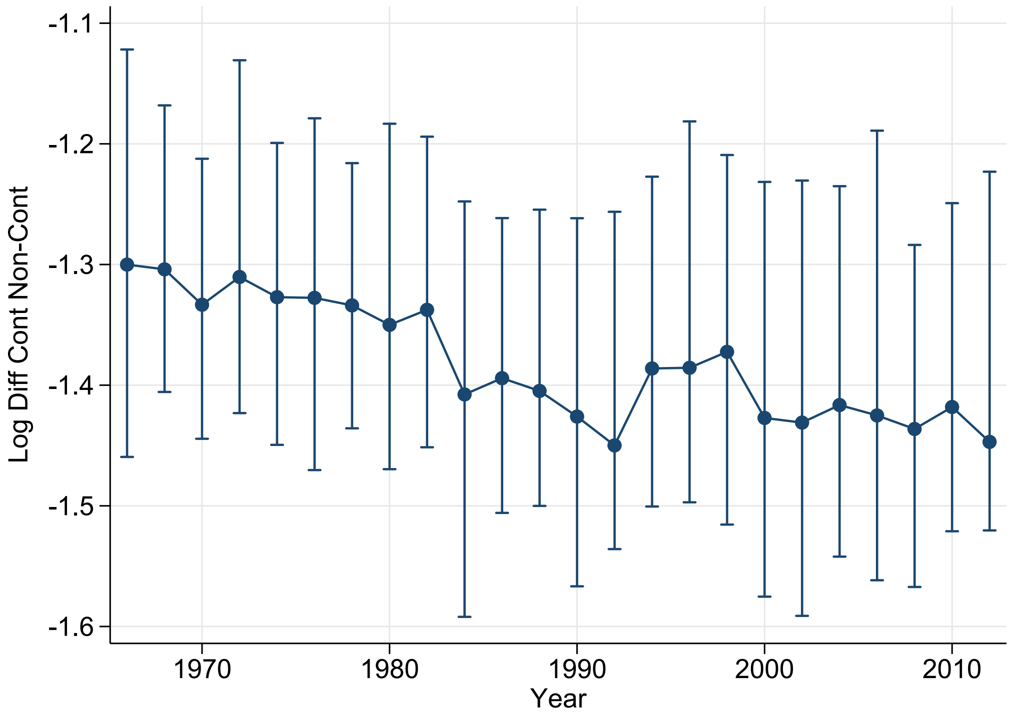
> "A certified operation or a person responsibly connected with an operation whose certification has been revoked will be ineligible to receive certification for a period of 5 years following the date of such revocation, except that Secretary may, when in the best interest of the certification program, reduce or eliminate the period of ineligibility."

In this sentence, there is a contingency that signals an exception: "except that Secretary may...". More specifically, this is a so-called activation condition (Frantz and Siddiki, 2022), namely a condition under which the statement activates – e.g., "if X, then Y". Activation conditions specify the context of the provision, thus making legislation more precise. In the example above, the provision operates in a context with two scenarios: one where the Secretary does not do anything, and another where the Secretary takes action and changes the process of certification. The former would be the only scenario if the condition was not present. In conclusion, the presence of this activation condition (what we call a contingency) provides a more detailed, precise set of rules than a situation where there is no contingency.

As discussed in Section 2 on the corpus, our legislative text output measurement does not distinguish statutes that increase regulations or decrease them (deregulate). An example of a "deregulating" law is Texas Utilities Code Title 2.C Ch. 65, "Deregulation of certain incumbent local exchange company markets", enacted in 2005.[23] That statute has many contingent clauses, reflecting that more contingent clauses does not necessarily mean more intense regulation.

[23] See `https://statutes.capitol.texas.gov/Docs/UT/htm/UT.65.htm`.

Figure A.9: Net Contingencies by Biennium



This figure shows the difference in log contingencies and log non-contingencies over time in our dataset. Error spikes give the 25th and 75th percentiles.

Table A.6: Examples of Contingent Provisions

State	Year	Topic	Provision Text
UT	2009	0	(iv) if liability under the bond filed by the applicant with the division pursuant to Section 40-10-15 shall be for the duration of the underground mining operations and until the requirements of this Subsection (2) and Section 40-10-16 have been fully complied with.
MD	1992	1	Unless authorized by the Board, the consumer member of the Board may not participate in any activity related to examinations under this subtitle.]
TN	2005	2	The prescribing optometrist must sign the handwritten prescription order on the day it is issued unless it is a standing order issued in a hospital, a nursing home or an assisted care living facility as defined in SS68-11-201.
TX	1985	3	The transcription shall be in narrative form unless a party gives written objection to the use of narrative form not later than the fifth day after receiving notice of the request for a statement of facts.
OR	1985	4	Roadside vehicle emergency lighting must be lighted and placed upon the highway where they are clearly visible to the drivers of approaching vehicles for a distance of 500 feet and according to the following.(A)
KS	1987	5	If any provision or clause of this act or application thereof to any person or circumstances is held invalid, such invalidity shall not affect other provisions or applications of the act which can be given effect without the invalid provision or application, and to this end the provisions of this act are declared to be severable.
IL	1979	6	If the taxpayer's average monthly tax liability to the Department under this Act, the "Use Tax Act", the "Service Occupation Tax Act", the "Service Use Tax Act", the "Municipal Retailers' Occupation Tax Act", the "Municipal Service Occupation Tax Act", the "County Retailers' Occupation Tax Act" and the "County Service Occupation Tax Act" was $25,000 or more during the preceding 4 complete calendar quarters or was $10,000 or more if such 4 quarter period ended on or after Mp-ch 31, 1977, he shall file a return with the Depar t-m-1.
CA	2006	7	With respect to each foreign disappearing other business entity previously registered for the transaction of intrastate business in this state, the filing of the agreement of merger pursuant to subdivision (f) automatically has the effect of a cancellation of registration for that foreign other business entity as of the date of filing in this state or, if later, the effective date of the merger, without the necessity of the filing of a certificate of cancellation.
CA	1996	12	The court shall continue the case only if it finds that there is a substantial probability that the minor will be returned to the physical custody of his or her parent or guardian within six months or that reasonable services have not been provided to the parent or guardian.
SD	1994	8	If a draft is payable at a fixed period after sight and the acceptor fails to date the acceptance, the holder may complete the acceptance by supplying a date in good faith.(d)
IN	2010	9	If the electronic mail address or the fax number provided by the voter does not permit the county voter registration office to send the voter an application not later than the end of the first business day after the county voter registration office receives the communication, the county voter registration office shall send the application to the voter by United States mail.
FL	1976	10	(1) DEFINITION.--"Industry trade products" means all food products having any-nenda~ry-pweduet-wh~eh-as the semblance of milk or a milk product defined in this chapter but which does not come within the definition of milk, a milk product, ea filled milk, or filled milk product.(2) LABELING.--Industry trade products shall be labeled with a fanciful name or any other descriptive name that accurately describes the product, but in no case shall an "industry trade roduct" be labeled as an imitation of any product defined in this chapter.

Table A.7: Examples of Non-Contingent Provisions

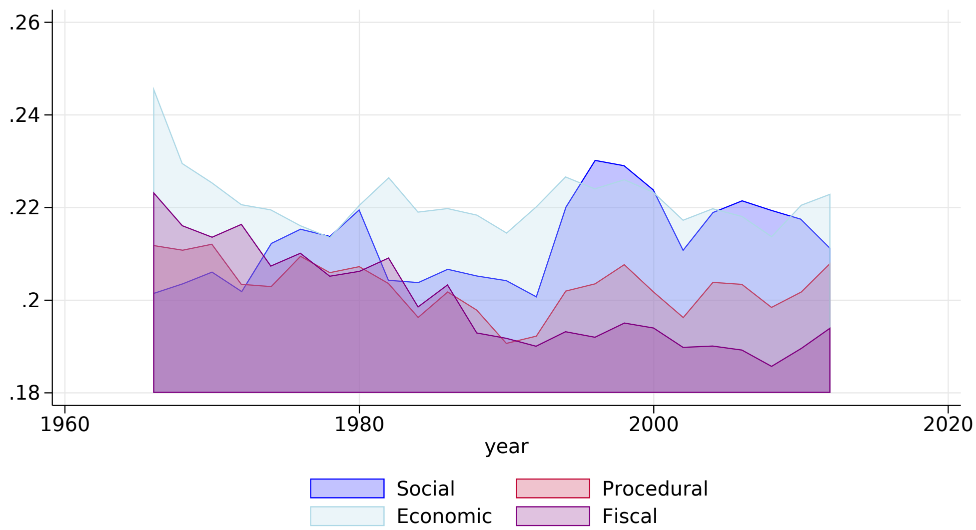
State	Year	Topic	Provision Text
UT	2009	0	these water impoundments will not result in the diminution of the quality or quantity of water utilized by adjacent or surrounding landowners for agricultural, industrial, recreational, or domestic uses.
MD	1992	1	A member may not serve more than [two] 2 consecutive full terms.
TN	2005	2	Nothing in this section shall be construed to prevent a physician assistant from issuing a verbal prescription order.
TX	1985	3	The clerk shall note the payment of the fee on the docket of the court.
OR	1985	4	A rear mounted lighting system shall have a green light, a yellow light and a red light.
IL	1979	6	Such determination shall be subject to review and revision by the Department in the manner hereinafter provided for the correction of returns.
CA	2006	7	The agreement of merger shall be approved on behalf of each other party by those persons authorized or required to approve the merger by the laws under which it is organized.
CA	1996	12	The court shall order that those services be initiated, continued, or terminated.(f)
IN	2010	9	A voter may not submit a registration application by fax or an electronic transmission except.
FL	1976	10	This act shall take effect October 1, 1976.
NY	1992	11	This act shall take effect on the same date as a chapter of the laws of 1992 amending the state law, relating to creating assembly and senate districts, as proposed in legislative bill number S. 7280 - A. 10111 takes effect.
NY	1969	13	Such notes may, among other things, be issued to provide funds t,.
CA	1990	14	The city council shall, within 10 days after the establishment of the district, invite bids for the making of the improvement by ordering a notice of the invitation to be published by two successive insertions in a daily or weekly newspaper published or circulated in the city and designated by the city council for that purpose.
IL	1953	15	Bonds shall be held at their book value.
VA	2002	16	The State Council shall report on the status of the Generalist Initiative to the House Appropriations and Senate Finance Committees at their regularly scheduled meetings in November.2.

Table A.8: Descriptive Statistics on Contingency, by Decade

	Mean	Standard Deviation
1960s		
Log Contingent	7.418	1.0050
Log Non-Contingent	8.779	.8894
Cont-Noncont Diff	-1.361	.274
Share of Contingent	.193	.0406
1970s		
Log Contingent	7.374	.7940
Log Non-Contingent	8.701	.7650
Cont-Noncont Diff	-1.326	.209
Share of Contingent	.1952	.0324
1980s		
Log Contingent	7.490	.8744
Log Non-Contingent	8.8693	.8136
Cont-Noncont Diff	-1.378	.210
Share of Contingent	.189	.0331
1990s		
Log Contingent	7.707	1.087
Log Non-Contingent	9.111	.9601
Cont-Noncont Diff	-1.404	.271
Share of Contingent	.1886	.0397
2000s		
Log Contingent	7.619	1.091
Log Non-Contingent	9.046	.9599
Cont-Noncont Diff	-1.427	.296
Share of Contingent	.18640	.0447

Notes. . **p<.01; *p<.05; +p<.1. This table shows the descriptive statistics for the logged number of contingent and non-contingent provisions by decade.

Figure A.10: 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.

C Instrument Checks

Table A.9: Descriptive Statistics on Endogenous Regressor and Instrument, by Decade

	Mean	Standard Deviation
1960s		
Shock to Legislation	.447	1.342
Log Provisions	9.08	.8867
1970s		
Shock to Legislation	-.1276	1.094
Log Provisions	9.023	.7584
1980s		
Shock to Legislation	.1469	.875
Log Provisions	9.173	.8073
1990s		
Shock to Legislation	.127	.7515
Log Provisions	9.402	.9554
2000s		
Shock to Legislation	-.4175	.872
Log Provisions	9.334	.9568

Notes. . **p<.01; *p<.05; +p<.1. This table shows the descriptive statistics for the instrument and the endogenous regressor by decade.

Table A.10: First Stage Estimates are Stable Over Time

	(1)	(2)	(3)
	Effect on Provisions		
	1960s-1970s	1980s-1990s	2000s
Shock to Legislative Output	-1.153**	-1.292**	-1.591**
	(0.391)	(0.0814)	(0.165)
Observations	348	500	249
State FE	X	X	X
Time FE	X	X	X

Notes. . **p<.01; *p<.05; +p<.1. This table shows the first stage regressions separately by time period.

Figure A.11: 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

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

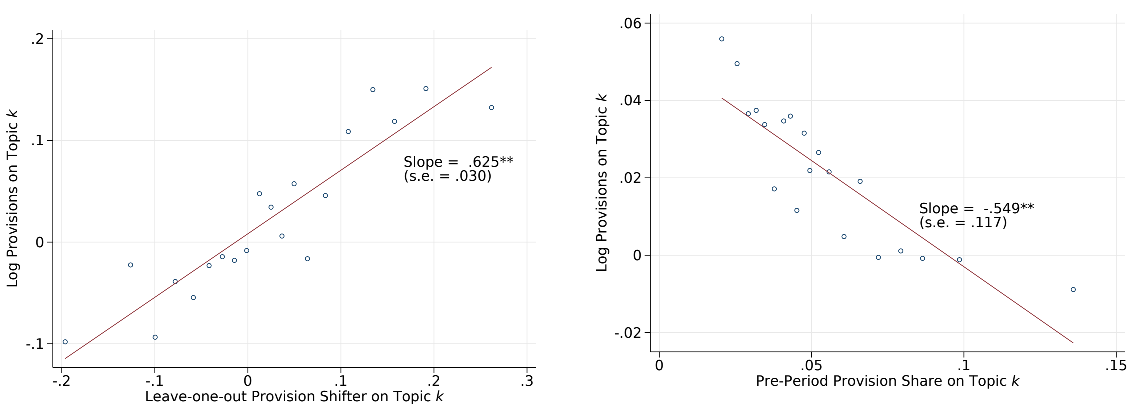
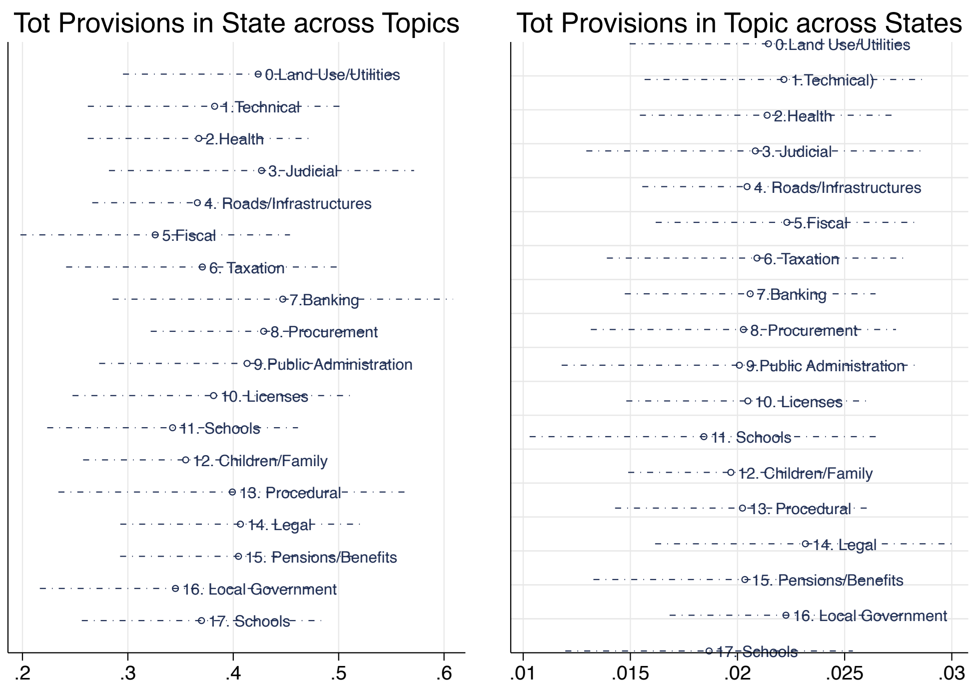
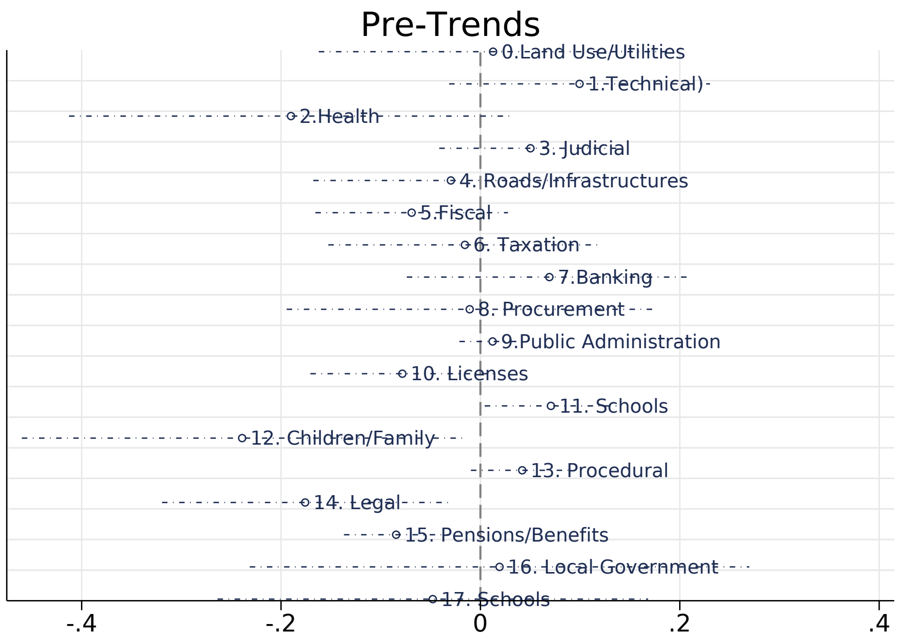


Figure A.12: All Topics Contribute to Instrument



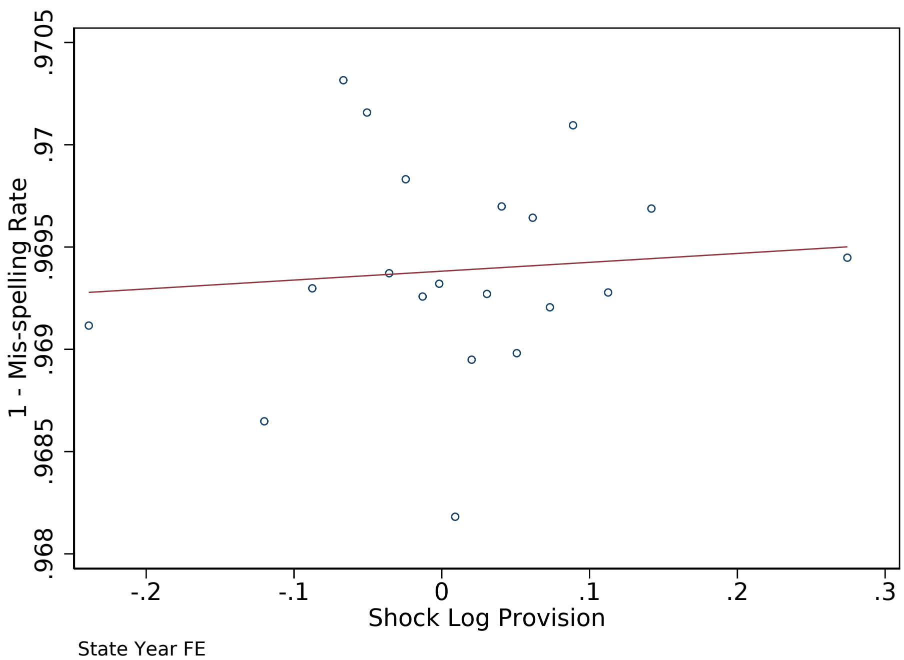
Notes. To check that the relevance of the shift-share instrument is driven by a majority of topics, we regress the increase in provisions related to a topic in a state on the increase in the total provisions related to that topic across states and the increase in the legal provisions in that state, for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

Figure A.13: 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.

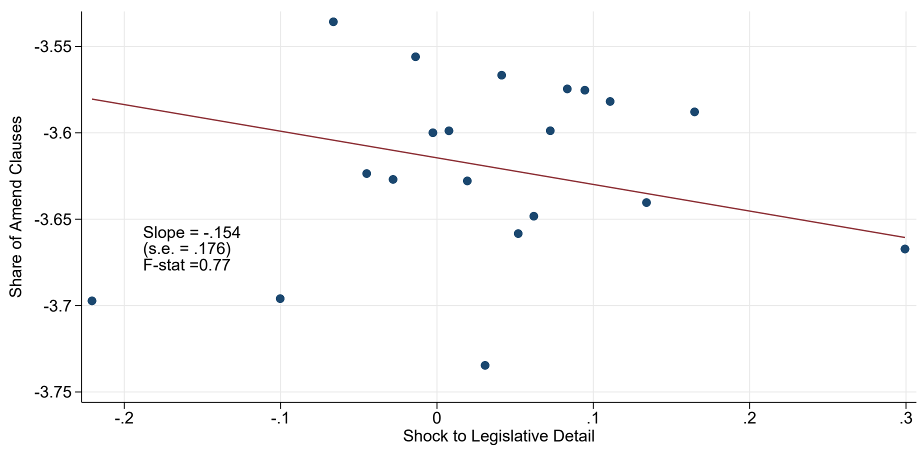
Figure A.14: Instrument is Uncorrelated with OCR Error Rate



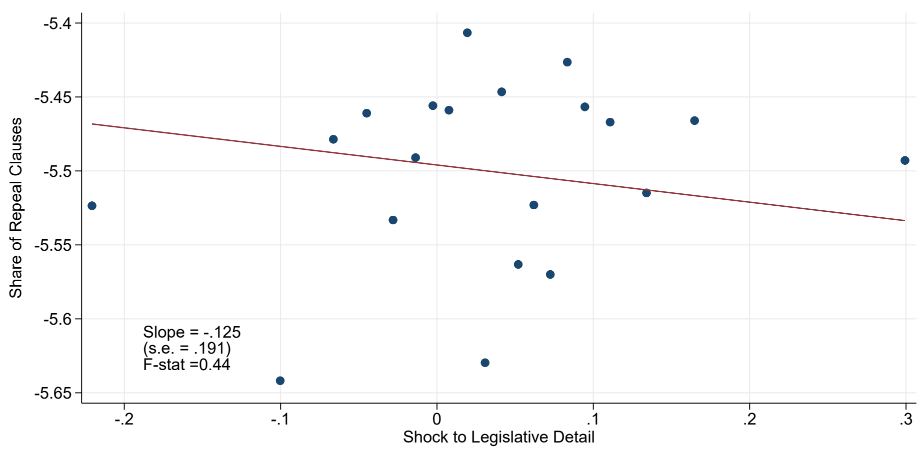
Notes. The Figure shows that the correct spelling rate (computed as the proportion of common nouns that are found in the WordNet dictionary) is not correlated with the instrument.

Figure A.15: Instrument is Uncorrelated with Share of Amending / Repealing Clauses

A. RF Effect of Instrument on Amending-Clause Share



B. RF Effect of Instrument on Repealing-Clause Share



Notes. Binned scatter plot of the relationship between legislative output shock instrument and the share of sentences containing amendment language (Panel A) and the share of sentences containing repealing language (Panel B). These are defined as the presence of the patterns "amend*" and "repeal*", respectively, where * indicates any word suffix.

Table A.11: Placebo Test: No Lead Effect of Legislative Output 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.12: Instrument 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)	
Sample (Years)	All	First Ten	All	First Ten	First	First
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 instrument balance test, 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 for the first principal component of the pre-treatment topic shares. All specifications are with biennium fixed effects, as well as standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.13: 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.

D Robustness Checks on Main Results

Table A.14: Effect of Legislative Output on Economic Growth (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Growth Rate Per capita						
Legislative Output	0.0146+	0.0152	0.0140*	0.0121*	0.00401+	0.0117+	0.00558*
	(0.00832)	(0.0123)	(0.00608)	(0.00512)	(0.00237)	(0.00680)	(0.00233)
Observations	1,182	1,182	1,182	1,182	1,134	1,074	1,086
R^2	0.431	0.446	0.561	0.746	0.628	0.473	0.862
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

Table A.15: Effect of Legislative Output on Economic Growth (Reduced Form)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Effect on Growth Rate Per Capita						
Legislative Shock	-0.0200*	-0.0205*	-0.0169*	-0.0150*	-0.0132*	-0.0216*	-0.0118+
	(0.00883)	(0.00940)	(0.00670)	(0.00660)	(0.00626)	(0.00832)	(0.00627)
Observations	1,182	1,182	1,182	1,182	1,134	1,182	1,087
R^2	0.420	0.440	0.552	0.739	0.629	0.430	0.855
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

Table A.16: Effect of Legislative Output on Economic Growth, Leads and Lags

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Effect on Real GDP Growth Per Capita (2SLS)							Reduced Form
Next Biennium	0.00664	0.00426	0.0009					
(Lead)	(0.00909)	(0.00838)	(0.00525)					
Legislative Output	0.0173*	0.0161+	0.00834+	0.0176*	0.0302	0.0160*	0.0103*	-0.0238*
	(0.00862)	(0.00841)	(0.00436)	(0.00787)	(0.0190)	(0.00720)	(0.00482)	(0.0114)
Last Biennium				0.00453	0.0146	0.00394	0.0033	-0.0123+
(Lag)				(0.00689)	(0.0134)	(0.00641)	(0.00296)	(0.00683)
Two Bienniums Ago					0.0128			
(2nd Lag)					(0.0128)			
First Stage F-stat	8.596	10.17	12.06	9.026	0.962	10.68	19.94	
Observations	1,130	1,130	1,038	1,179	1,176	1,179	1,085	1,179
State FE	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
State Trends		X	X			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				X	

Notes. Columns 1, 2, and 3 show the results with the placebo lead and the contemporaneous effect together. Columns 4, 6, and 7 include together the lag and the contemporaneous effects. Column 5 includes two lag effects and the contemporaneous one together. Column 8 is the reduced form, where the indicated endogenous regressors are replaced with the associated instruments. Specification include state and biennium fixed effects, state specific trends, and additional covariates, as indicated. All specifications have standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.17: Effect of Laws on Growth (2SLS): Alternative Clustering of Standard Errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Effect on Real GDP Growth Per Capita										
Clustering	*None (Robust SEs)*		*Two-Way: State & Year*		*Initial Topics (12)*		*Initial Topics (16)*		*Initial Topics (20)*		AKM
Legislative Output	0.0182*	0.0168*	0.0182*	0.0168+	0.0182+	0.0168+	0.0182+	0.0168+	0.0182*	0.0168+	0.014**
	(0.00872)	(0.00808)	(0.00854)	(0.00879)	(0.00986)	(0.00921)	(0.00873)	(0.00856)	(0.00835)	(0.00804)	(0.0011)
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	1,182	1,182	1,182	1,182	1,182
Time FE	X	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	

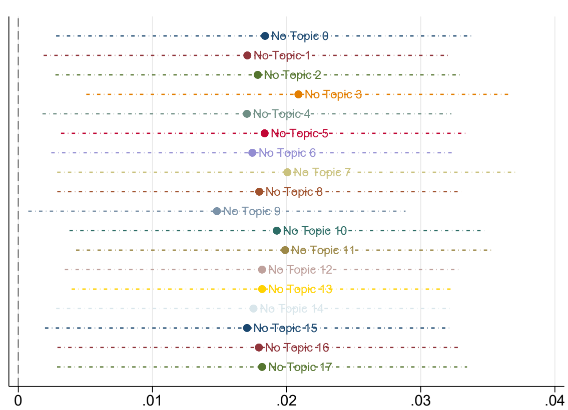
Notes. Columns 1 and 2 report the estimates for the effect of legislative output on growth per capita using robust standard errors. Columns 3 and 4 use standard errors clustered at the state and year level. Columns 5 to 10 use standard errors clustered at the initial topic level, with 12, 16 and 20 topics. All specifications include a first column with time and state fixed effects and a second column with the addition of state specific trends. Column 11 uses standard errors from the ivreg_ss command by Adao et al. (2019), using the default settings and initial share of topic 2 dropped due to collinearity and year fixed effects. 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.18: Effect of Legislative Output on Economic Growth - Topic Controls

	(1)	(2)	(3)	(4)
	Effect on Growth per Capita			
	2SLS	2SLS	2SLS	2SLS
Legislative Output	0.0182+	0.0168+	0.0182*	0.0168+
	(0.00905)	(0.00864)	(0.00903)	(0.00863)
Observations	1182	1182	1182	1182
First Stage F-stat	22.78	22.11	22.84	22.17
State FE	X	X	X	X
Time FE	X	X	X	X
State Trends		X		X
Frequent Topic Shares	X	X		
PCA			X	X
Growing Topic				

Notes. The table shows the results for baseline 2SLS estimate controlling for the share of the most frequent topics in columns 1 and 2, and for the first principal component in columns 3 and 4. 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.

Figure A.16: 2SLS Results are not driven by any single topic



Notes: This is a coefficient plot showing the results of the main 2SLS model, with the instrument constructed by leaving one topic out at a time. State and time FE and clustered SE

Table A.19: Effect of Legislative Output 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 Output	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.20: Effect of Laws on Growth – Adjusting for Words or Pages

	(1)	(2)	(3)	(4)
	Effect on Real GDP Growth Per Capita			
Legislative Output			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 output 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.

E Analysis of Regulations and Caselaw

The analysis in this paper has focused on the state session laws – the legislation or statutes enacted by state legislatures to be added to the statutory code. There are two major additional sources of rules for governing the state economy: state regulations and state caselaw.

First, there are the regulations that bureaucratic agencies enact to help implement statutes. These are often much more detailed than statutes. For example, Davis (2017) documents that in the case of the U.S. federal government, regulatory texts dwarf the legislative texts in volume and complexity. McLaughlin and Sherouse (2017) look at the particular case of the Dodd-Frank Act, which by itself resulted in tens of thousands of provisions to be added to the corpus of federal regulations. Federal regulations could be having an important economic impact at the state level, and the states themselves also issue regulations.

Second, the judiciary has an important role in economic governance. First, legislation and regulations have to be interpreted by judges for enforcement. When a regulator challenges a company action, or companies file suit against each other, the state courts are there to adjudicate and also to issue opinions clarifying legal rules. In a common law system like the United States, moreover, judges are often responsible for the rules themselves (e.g. Gennaioli and Shleifer, 2007). For example, Autor et al. (2007) shows that common-law rules on wrongful discharge had effects on employment and firm structure in U.S. states (see also MacLeod and Nakavachara, 2007).

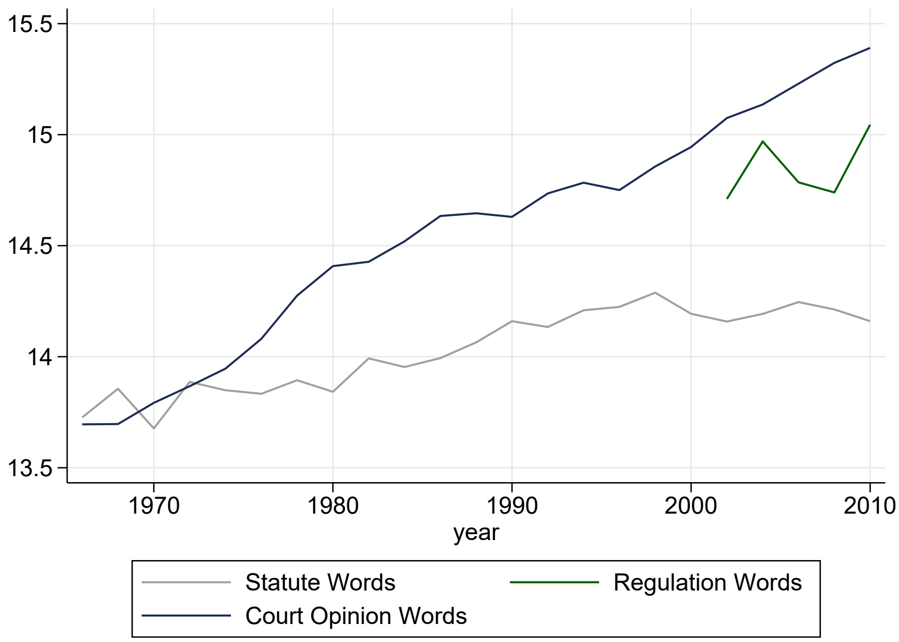
It could be that our main approach, focusing on legislation, leaves out some growth-relevant legal features in these other legal domains. In particular, it could be that our instrument affects growth not just through its effect on legislation, but also through its effect on regulations or caselaw. That would be a violation of the exclusion restriction and call into question the causal interpretation of the 2SLS regressions.[24]

To address these issues, we collected three additional measures of state-level legal output. We built two new corpora on state laws – a corpus of recent state regulations, and a corpus of state appellate court cases, both from Lexis Nexis. Third, we have a measure of the intensity of federal regulations across states from McLaughlin and Sherouse (2016).

State Regulations. We gained access to the proprietary State Net Regulatory

[24] The reduced-form regressions would still be causal, but difficult to interpret since it is a shift-share instrument whose units do not have a clear economic meaning.

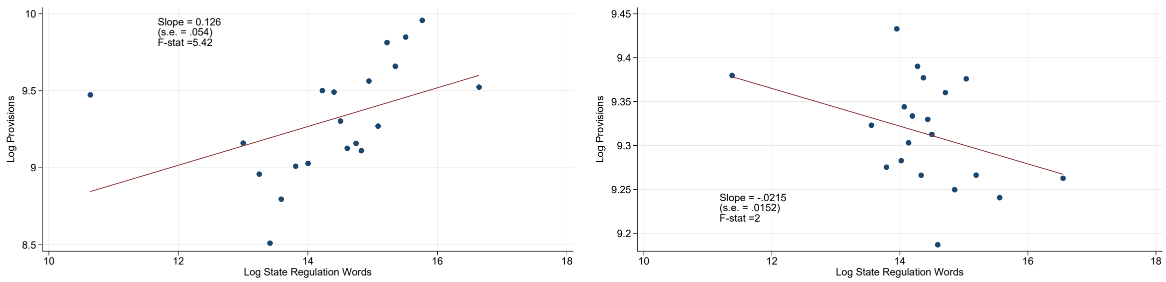
Figure A.17: Comparison of Legal Volume in U.S. States: Statutes, Regulations, and Court Opinions



Notes. This figure shows the time series of the log word counts (average across states by biennium) for statutes (gray), regulations (green) and court opinions (black). See text for additional details on data sources.

Figure A.18: State Legislation and State Regulations

With Year FE　　　　With Year FE and State FE



Notes. The relationship between logged number of provisions and logged state regulation words, controlling for year FE (left panel) and state and year FE (right panel). SE clustered at state level.

Text corpus from Lexis Nexis. This is a corpus of regulatory activity by state, available for some states starting in 1998 and most states starting in 1999, with complete coverage starting in 2002, up until 2017. It contains a rich collection of records corresponding to regulatory actions, for example new tax rules issued by state tax agencies. The database contains 20GB of XML files, which we processed to extract the regulatory text content. The processed corpus contains 642 thousand documents, adding up to 1.8 billion words. We computed the log word counts in regulations by state-biennium and merged them to our main dataset (through 2010). Figure A.17 plots the average log biennium word counts in regulations across states in the green time series. In the gray time series, we have for comparison the average log biennium word counts in statutes (the state session laws) across states. The regulation word count is about .7 to .9 higher in log scale, reflecting that the volume of regulations is about double the volume of statutes.

Figure A.18 shows binscatter plots relating regulatory output to legislative output. In the cross section (left panel), there is a positive and statistically significant effect. Intuitively, states with more complex legislation also have more complex regulations. When adding in state fixed effects and looking at within-state changes in legislative / regulatory flows, the slope flips sign and is no longer significant.

Table A.21 shows the reduced-form effect of our legislative shock on state regulations. Reassuringly, there is no effect. This means that regulatory detail is likely not a major mediator between our instrument and economic growth. Further, adding recent regulatory detail, averaged by state, and then interacted with time FE, does not change our results (Table A.22).

Table A.21: Reduced Form Effect of Legislation Instrument on State Regulatory Output

	(1)	(2)
	Effect on State Regs	
Shock to Legislation	-0.256	0.116
	(1.053)	(0.949)
Observations	329	329
State FE	X	X
Time FE	X	X
State-Specific Trends		X

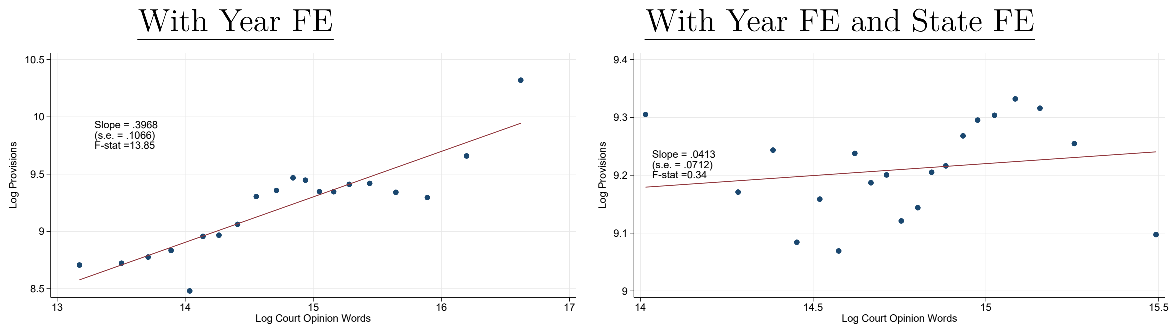
Notes. The table shows the results of regressing the logged state regulation word count on the instrument. **p<.01; *p<.05; +p<.1.

Table A.22: Controlling For State Regulation, Interacted with Time FE

	(1)	(2)
	Effect on Growth per Capita	
	2SLS	2SLS
Legislative Output	0.0190+	
	(0.0103)	
Contingent - Non-Contingent		0.0778**
		(0.0256)
Observations	1,182	1,182
First Stage F-stat	20.06	22.35
State FE	X	X
Time FE	X	X
State Reg x Time	X	X

Notes: Main 2SLS results controlling for a state's state regulation in 2002-2010, interacted with time.

Figure A.19: Log Provisions and Log Court Opinion Words



Notes. The relationship between logged number of provisions and logged state court opinion words, controlling for year FE (left panel) and state and year FE (right panel). SE clustered at state level.

Case Law. The second part of the legal process to explore is the judiciary. We gained access to the corpus of state appellate court opinions from LexisNexis. This corpus contains opinions from state intermediate and high courts from their inception, with the earliest opinion from 1658 and the latest in 2017. These cases are common-law decisions which can interpret statutes/regulations, apply precedents to new cases, or make new precedents. The raw dataset is 230GB of XML files, which we processed to extract the opinion texts The processed corpus contains 9.7 million written opinions, adding up to 10.5 billion words. We computed the log word counts in cases by state-biennium and merged them to our main dataset. Figure A.17 plots the average log biennium word counts in court opinions across states in the black time series. We can see that at the beginning of the sample, the volume of text was similar between statutes (gray series) and caselaw. But over the last decades, the volume of caselaw has increased more rapidly, such that in recent years the caselaw word count is about 1.3 higher on a log scale.

Figure A.19 shows the descriptive relationship between legislative volume and the volume of laws from courts. As with regulations, in the cross-section there is a positive and statistically significant relationship. This reflects that states with more legislation also have more published court opinions. In the panel, there is still a positive relationship but it is no longer statistically significant. Changes in legislative text flows are not strictly related to changes in judicial text flows.

Table A.23 shows the reduced form effect of the legislative instrument on judicial opinion text volume. Unlike with the regulations, there is a small positive effect of the instrument on court output, but only when including state trends. However, we

Table A.23: Reduced Form Effect of Legislation Instrument on Judicial Opinion Output

	(1)	(2)
	Effect on Growth per Capita	
	State Court Opinion Words	State Court Opinion Words
Shock to Legislative Output	-0.00632	0.0676+
	(0.0807)	(0.0353)
Observations	1,183	1,183
State FE	X	X
Time FE	X	X
State-Specific Trends		X

Notes. The table shows the results of regressing the logged state court case word count on the instrument. **p<.01; *p<.05; +p<.1.

ran our main specifications controlling for the measure of judicial text output (Table A.24) and the results are identical. Further, we found that results are similar when we control for number of cases, number of opinions, and average number of words per opinion. Overall, these results suggest that caselaw is not an important mediator for our economic-growth results.

Finally, we would like to net out any influence of the federal judicial system. For this purpose, we allow for separate trends by federal judicial circuit – groups of 3-7 states that share a federal circuit court (the intermediate court below the U.S. Supreme Court). Table A.25 shows the main results after adding circuit-year interacted fixed effects. Results are robust and estimates are very similar to those from the main models.

FRASE Index for Federal Regulation. We add in information on the FRASE index from McLaughlin and Sherouse (2016). FRASE (Federal Regulation and State Enterprise) measures the impact of federal regulation on the private-sector industries in each state's economy. Cross-state variation is given by the differences in industry composition. Hence, a state's FRASE score represents the degree of impact federal regulations have on a state's economy relative to federal regulations' impact on the national economy.

FRASE is available only for recent years. Figure A.20 shows a positive relationship between state statutory legislation and FRASE in these overlapping years, but that relationship is not statistically significant. Table A.26 shows that the shift-share instrument for legislation has no effect on FRASE, suggesting that federal regulations

Table A.24: Controlling for Legal Detail in State Court Opinions

	(1)	(2)
	Effect on Growth per Capita	
	2SLS	2SLS
Legislative Output	0.0166+	
	(0.00864)	
Contingent - Non-Contingent		0.0695**
		(0.0230)
Observations	1,182	1,182
First Stage F-stat	22.18	36.57
State FE	X	X
Time FE	X	X
Court Opinion	X	X
State Trends	X	X

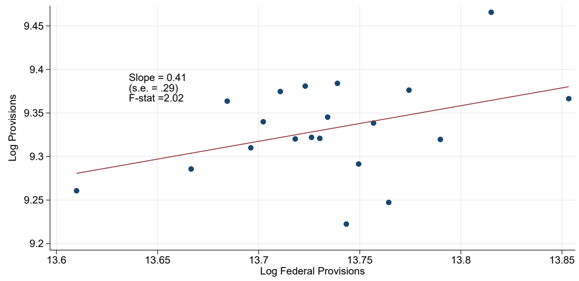
2SLS results when controlling for logged state court opinion words. Notes. . **p<.01; *p<.05; +p<.1.

Table A.25: Federal Judicial Circuits

	(1)	(2)	(3)	(4)
	Effect on Growth per Capita			
	2SLS	2SLS	2SLS	2SLS
Legislative Output	0.0110*	0.0163*		
	(0.00446)	(0.00792)		
Contingent - Non-Contingent			0.0495+	0.0576*
			(0.0274)	(0.0259)
Observations	1,180	1,180	1,180	1,180
First Stage F-stat	17.78	67.95	9.349	14.44
CircuitxTime FE	X	X	X	X
State FE		X		X

Notes. 2SLS results when controlling for circuit-year fixed effects. **p<.01; *p<.05; +p<.1.

Figure A.20: Statutory Legislation and FRASE Federal Regulation Index



Notes. The relationship between logged number of provisions and logged state regulation words, controlling for year FE (left panel) and state and year FE (right panel). SE clustered at state level.

Table A.26: Effect of Legislative Shock on FRASE Federal Regulation Index

	(1)	(2)
	Fed Reg	Fed Reg
Shock to Legislation	-0.0182	0.000935
	(0.0218)	(0.0225)
Observations	385	385
State FE	X	X
Time FE	X	X
State-Specific Trends		X

Notes. The table shows the results of regressing the (logged) FRASE index on the instrument. All models include standard errors clustered at state level. **p<.01; *p<.05; +p<.1.

are not an important mediator for the main effects.

To control for FRASE in the whole sample, we average it by state across years and interact it with time fixed effects. Table A.27 shows the main results of the paper, controlling for FRASE interacted with time effects. Results are robust and the coefficient estimates are very similar to those from the main model.

Table A.27: Adjusting for FRASE Federal Regulation Index

	(1)	(2)
	2SLS	2SLS
Legislative Output	0.0143+	
	(0.00765)	
Contingent Provisions		
Non-Contingent Provisions		
Contingent - Non-Contingent		0.0638**
		(0.0214)
Observations	1,182	1,182
First Stage F-stat	21.67	29.50
State FE	X	X
Time FE	X	X
Fed Reg x Time	X	X

Notes. 2SLS results when controlling for a state's FRASE index interacted with year FE . **p<.01; *p<.05; +p<.1.

F Unpacking the Effect of Legislative Output

Table A.28: Effect of Legislative Output 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 Output	-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 new provisions do 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.29: Legislative Output Shock Does Not Affect Spending, Taxes, or Political Control

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Govt Spending		Legis Spending		Taxes		Dem Control	
Model	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Legislative Output	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.

G Legislating as Incomplete Contracting

In this appendix 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 output. 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.

G.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.[25] Omitting from the text of a law an elementary sentence about the possible events or situations

[25] 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.

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.

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.

G.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 legislation is added by a benevolent principal, it must be because the clauses are beneficial in the context where they are added. In other words:[26]

[26]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).

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.[27] As a result, given a fixed marginal writing cost but wide variation in the benefit function, the state legislators choose different levels of contingent legislative output 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 output 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

[27]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 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.

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).[28]

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 whil6/e 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 stock – 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. Under low uncertainty, a rigid clause that follows the likely state works best. The more uncertain are the relevant situations, the more valuable will it be to account for different

[28]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 concavity.

possible contingencies and state a context-dependent action. 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 output, 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.

H Additional Material for Contingency

Table A.30: 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.31: Effect of Contingencies on Additional Economic Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	GDP (Total)	Population	Employment	Profits	Wages	Establishments
Contingent	0.0771**	0.0192*	0.00445	0.186	0.0517+	-0.00516
	(0.0240)	(0.00944)	(0.0312)	(0.119)	(0.0271)	(0.0131)
Non-Contingent	-0.0694**	-0.0240+	-0.000140	-0.181	-0.0480+	-0.00201
	(0.0256)	(0.0121)	(0.0315)	(0.137)	(0.0283)	(0.0159)
First Stage F-stat	22.27	22.27	36.52	16.24	22.27	36.52
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 with contingent and non-contingent clauses 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.

Table A.32: Effect of Contingent and Non-Contingent Clauses by Themselves

	(1)	(2)	(3)	(4)	(5)	(6)
	Effect on Real GDP Growth Per Capita					
Contingent Provisions	0.0199*	0.0185*	0.0443			0.0018
	(0.00797)	(0.00771)	(0.0575)			(0.0970)
Non-Contingent Provisions			-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.33: Main Results, Controlling for Amend/Repeal Share

	Effect on Growth Rate Per capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Legislative Output	0.0185*	0.0174*			0.0184+	0.0176*		
	(0.00911)	(0.00845)			(0.00920)	(0.00844)		
Cont - Non-Cont			0.0663**	0.0645**			0.0630**	0.0613**
			(0.0216)	(0.0211)			(0.0202)	(0.0202)
First Stage F-stat	23.09	23.97	23.49	33.50	19.91	23.10	25.13	34.97
Observations	1,156	1,156	1,156	1,156	1,156	1,156	1,156	1,156
State FE	X	X	X	X	X	X	X	X
Time FE	X	X	X	X	X	X	X	X
Amend Share	X	X	X	X				
Repeal Share					X	X	X	X
State Trends		X		X		X		X

Notes. This table shows robustness specifications controlling for the current share of amending clauses (Columns 1-4) and share of repealing clauses (Columns 5-8). All specifications are with biennium fixed effects, as well as standard errors clustered by state. **p<.01; *p<.05; +p<.1.

Table A.34: Heterogeneous Effects of Provisions and Contingency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Effect on Real GDP Growth Per Capita							
Sample	Dem	Repub	Divided	Unified	Dem	Repub	Divided	Unified
Log Provisions	0.0211	0.00244	0.0180+	0.0160				
	(0.0127)	(0.00563)	(0.0107)	(0.0196)				
Contingent -					0.0733+	0.00617	0.111*	0.0188
Non-Contingent					(0.0393)	(0.0468)	(0.0497)	(0.0369)
First Stage F-stat	31.67	38.33	22.6	33.61	21.58	16.97	11.28	14.65
Observations	715	403	583	538	715	403	583	538
Time FE	X	X	X	X				X
State FE	X	X	X	X				X
State Trends	X	X	X	X				X

Notes. Effect of provisions, and the difference in contingent and non-contingent clauses – 2SLS estimates – separately by party structure in state government. All specifications include time and state fixed effects, control for state trends and use standard errors clustered at the state level. "Dem" means that democrats control at least 2 branches of state government (House, Senate, or Governor); "Repub" means that Republicans are in control; "Divided" means that both parties are in power; "Unified" means that one party controls all three branches. **p<.01; *p<.05; +p<.1.

Table A.35: Controlling for Campaign Finance Rules

	(1)	(2)	(3)	(4)
	Effect on Growth Rate Per Capita			
	2SLS	2SLS	2SLS	2SLS
Legislative Output	0.0258*	0.0245*		
	(0.0128)	(0.0104)		
Contingent - Non-Contingent			0.0479*	0.0616**
			(0.0230)	(0.0229)
Observations	870	870	870	870
First Stage F-stat	6.779	11.63	37.02	29.68
State FE	X	X	X	X
Time FE	X	X	X	X
Camp. Fin. Rules X Time FE	X	X	X	X
State-Specific Trends		X		X

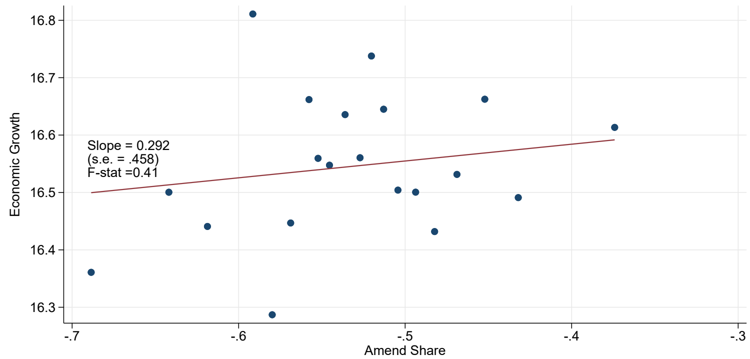
Notes. **p<.01; *p<.05; +p<.1. Regression results controlling for an index for limits on campaign contributions to state races, interacted with time. Smaller sample size is due to some missing years with campaign finance rules data.

Interest groups play a crucial role in proposing and implementing legislation (Bombardini and Trebbi, 2020). If the political system is open to more types of interest groups, then more more diverse proposals will get to legislators. It might also influence the implementation of legislation.

To check whether our results are driven by interest groups, we gathered data on the legislation regulating campaign finance contributions. We inspected the Book of States for the years 1952-2000 and code whether contributions are restricted/prohibited for everyone, restricted/prohibited for some organizations (e.g., corporations or labor unions), or unlimited for everyone. We encode this as a categorical variable in our dataset.

Table A.35 shows the results for our baseline specification and for contingencies when controlling for the campaign finance index fixed effects, interacted with biennium fixed effects. This specification controls flexibly and allows our effects to be different over time depending on these rules. Results are robust and estimates are similar to those from the main models, suggesting that our results are not driven by lobbying efforts.

Figure A.21: Legislation Instrument Does Not Affect Campaign Contributions



Notes. Binscatter diagram of campaign donations to state politicians with the legislative-shock shift-share instrument, for the years 2000-2010 (years for which the campaign donations data is available).

As an additional check, we collected information on campaign contributions to candidates for state government offices. This data is available on a set of web pages at `followthemoney.org`, for elections since the year 2000. We built a programmatic web scraper to collect all of these data and summed them by state and biennium. We then linked it to our main dataset for 2000-2010. Figure A.21 shows that our instrument has no linear effect on these contributions, suggesting that they are not an important mediator for the estimated effects on growth.

I Robustness Checks on Concavity and Uncertainty

Table A.36: 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.37: 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 Output	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 output. Columns 4 and 5 report results for those with average recent legislative output and Columns 6 and 7 states with recent legislative output 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.38: 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 Output	0.0201		0.0146		0.0191	0.0199+				
	(0.0152)		(0.0211)		(0.0132)	(0.0102)				
Contingent Provisions							0.0624	0.0637		
							(0.0561)	(0.0759)		
							-0.0508	-0.0522		
Non-Contingent Provisions							(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.39: 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 Output	-0.0136	0.00796	0.0445*				
	(0.0115)	(0.0112)	(0.0169)				
Contingent Provisions				0.142*	0.151*		
				(0.0567)	(0.0703)		
				-0.126+	-0.131		
Non-Contingent Provisions				(0.0641)	(0.0804)		
Contingent -						0.185**	0.202**
Non-Contingent						(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.40: 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 Output	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.