More Laws, More Growth? Evidence from US States

Elliott Ash

ETH Zurich and Centre for Economic Policy Research

Massimo Morelli

Bocconi University, Centre for Economic Policy Research, and Luxembourg Institute of Socio-Economic Research

Matia Vannoni

King's College London

This paper analyzes the conditions under which more legislation contributes to economic growth. In the context of US 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 pretreatment legal topic shares. We find that at the margin, higher legislative output causes more economic growth. Consistent with more complete laws reducing ex post holdup, we find that the effect is driven by the use of contingent clauses, is largest in sectors with high relationship-specific investments, and is increasing with local economic uncertainty.

We acknowledge financial support from the European Research Council (Ash: Starting Grant 101042554; Morelli: Advanced Grant 694583) and the Framework for Attraction and Strengthening of Excellence Grant. We also acknowledge support from the Baffi Centre on Economics, Finance, and Regulation (CAREFIN) and Innocenzo Gasparini Institute for Economic Research (IGIER) centers. We thank Massimo Anelli, Benjamin Arold, Pierpaolo Battigalli, Kirill Borusyak, Quentin Gallea, Livio di Lonardo, Xavier Jaravel, Giovanni Maggi,

Electronically published May 8, 2025

Journal of Political Economy, volume 133, number 7, July 2025.

https://doi.org/10.1086/734874

I. Introduction

In the cross section, jurisdictions with larger, more complex legal systems also tend to have larger, more productive economies. The correlation between legislative output and GDP in US states (illustrated in fig. 1) provides a clear example of this empirical regularity. A fundamental question is whether these correlations reflect causal links.

While a larger economy could lead to more laws mechanically (as, e.g., 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. In particular, a more complete "legislative contract" could lead to more investment by making laws more enforceable and reliable (e.g., Williamson 1979; Hart and Moore 1988). Conversely, legislation might instead consist mainly of favors to special interest groups, coming at the cost to overall growth and welfare (e.g., Grossman and Helpman 2001). Even with good-natured legislators in charge, excessive lawmaking could hinder economic growth by increasing compliance costs (Niskanen 1971; Botero et al. 2004). Given the current state of the economy, one might postulate an optimal level of legal detail, with movement toward the optimum from either side leading to growth.

Motivated by these debates, we explore empirically the relationship between legislative detail and economic output in US states, from 1965 to 2012. Our research question is whether and how laws impact the economy. In brief, we show that increasing legal detail does lead to more growth. On mechanisms, we report supporting evidence that higher legislating reduces legal uncertainty, leading to more business activity via relationship-specific investments.

The first step is to measure detail in legislation. 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, Ash, and Morelli 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, Ng, and Jordan 2003).

Our empirical strategy introduces a novel text-based shift-share instrument for legal detail (e.g., Bartik 1994). Analogous to standard shift-share

and Piero Stanig for helpful comments. We also thank seminar participants at European University Institute, Autonoma de Barcelona, Columbia Political Economy Conference, ETH Political Economy Workshop, King's College London, and Stockholm School of Economics for helpful comments. We thank David Cai, Claudia Marangon, and Emiliano Rinaldi for helpful research assistance. This paper was edited by Harald Uhlig.

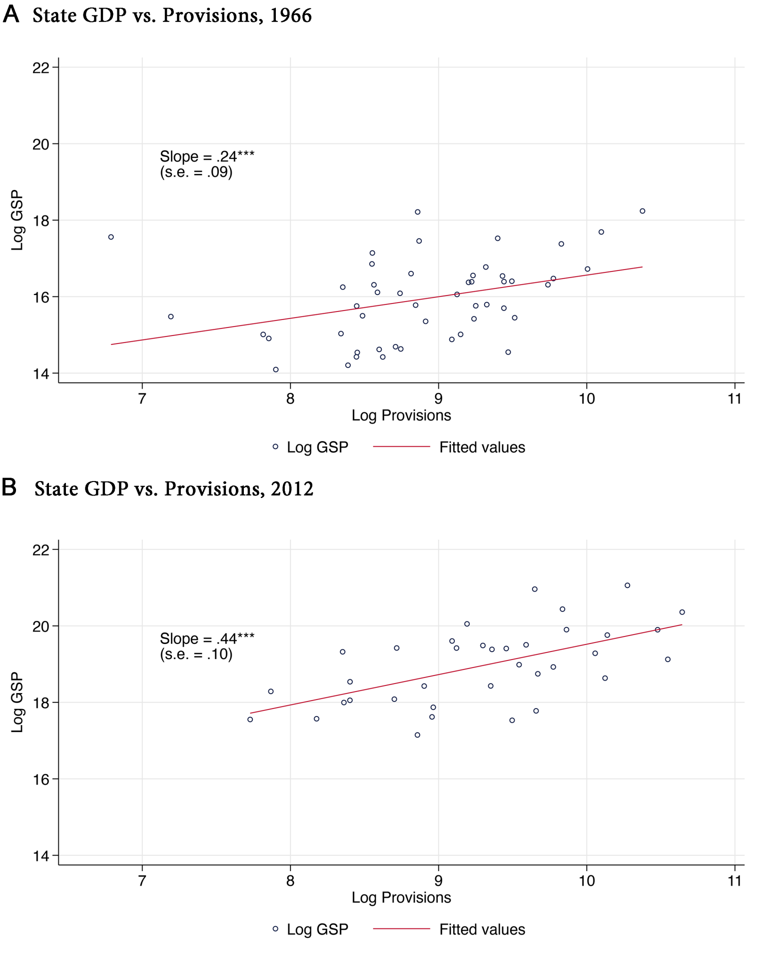

Fig. 1.—State GDP and legislative output, 1966 and 2012. Scatterplots show the relationship between (log) provisions and (log) state GDP at the beginning of our time period (1966; *A*) and the end (2012; *B*).

instruments that use *sector-specific economic flows* interacted with *preperiod sector shares*, we construct our instrument using *topic-specific legislative flows* interacted with *preperiod topic shares*. For identification, we assume that topic-specific national legislative flows are exogenous to each particular state, in line with recent econometric work by Adao, Kolesar, and Morales (2019) and Borusyak, Hull, and Jaravel (2022). Intuitively, we think of states as borrowing language on policies enacted in other states (the shifters), especially when they have relatively low existing detail on that topic (the shares). Our design passes a number of checks recently developed by econometricians for probing the exogeneity of shift-share instruments

(Adao, Kolesar, and Morales 2019; Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2022).

The main empirical finding 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 choices for text processing, regression specification, and statistical inference. We can rule out a number of alternative channels for the results besides an effect of legislation on the economy, including changes in population, taxes/expenditures, intervening political conditions, and changes in the actions of state regulatory agencies or state courts.

To understand why more laws lead to more growth, consider the following essential feature of our setting and design: the instrument captures state-to-state diffusion on legal topics where borrowing states have not yet legislated. As publication of laws in other states reduces the cost of enacting similar laws, that expands the legislative choice set over beneficial laws while allowing legislators to disregard unsuitable ones. Hence, the instrument is likely to bring efficiency-enhancing laws under minimal assumptions on the benevolence of state legislators. These assumptions seem reasonable in light of previous work showing that states tend to borrow the more successful policies from each other (e.g., Volden 2006; Pacheco 2012; Butler et al. 2017).

What is the economic mechanism underlying the law's boost to growth? In an extensive mechanisms analysis, we focus on an "incomplete contracts" interpretation of legislating and the economy. To summarize, we think of state governments as creating legislation that businesses must abide by, which necessitates specialized investments and supply chain structures. Ambiguous or incomplete legislation can generate uncertainty about enforcement, which deters businesses from making investments in the first place. More comprehensive and clear legislation reduces this holdup problem and encourages such specific investments, thus stimulating economic growth.

We report a series of empirical findings in support of legislative completeness and holdup reduction as a key mechanism. First, laws that regulate the economy (e.g., land/property rights) have a larger effect on growth than laws regulating social issues (e.g., family law). Second, provisions that produce contingencies—actions or outcomes conditioned on events, reducing uncertainty over those events—are more helpful than noncontingent provisions (Battigalli and Maggi 2002, 2008). Third, there is decreasing economic benefit to legislating when the existing stock is already relatively detailed. Fourth, the law's effect on growth is focused on sectors relying more on customized inputs requiring relationship-specific investments (Nunn 2007). And fifth, increasing legal detail is most beneficial under higher local economic uncertainty (Baker, Bloom, and Davis 2016).

Further, the estimated effect coefficients are economically meaningful. The regressions suggest that a 10% increase in legislative flows due to text borrowing increases the state GDP growth rate (per capita) by about 0.15 percentage points, relative to a mean of 3.1 percentage points. For contingent provisions, the estimated effects are even larger—a 10% increase in the flow of contingencies would lead to about a 0.6 percentage point boost to the growth rate. For comparison, achieving a 0.15 percentage point increase in growth through fiscal stimulus would require about a 0.1 percentage point increase in net-of-tax government spending (e.g., Nakamura and Steinsson 2014; Chodorow-Reich 2019).

These results add to a long-running debate on how laws and regulations influence growth prospects (e.g., Parker and Kirkpatrick 2012). Overall, the results are consistent with the "positive view" that legislation is needed to regulate externalities, define the tax base, and allocate government expenditures, and usually it helps the economy grow (e.g., Dam 2007). Empirical work documenting a positive correlation between legislative output and growth includes Mulligan and Shleifer's (2005) cross-sectional comparison of US states, Fukumoto's (2008) time-series comparison in Japan, and Kirchner's (2012) time-series comparison in Australia.[1] Conversely, our evidence goes against the "negative view" of public choice theory that regulation is usually excessive and tends to hinder economic growth (e.g., Niskanen 1971; Davis 2017); in particular, the effect of compliance costs in hindering business formation, competition, innovation, and skill acquisition (Fonseca, Lopez-Garcia, and Pissarides 2001; Nicoletti and Scarpetta 2003; Ciccone and Papaioannou 2007; Braunerhjelm and Eklund 2014). Related empirical work emphasizing compliance costs includes Botero et al.'s (2004) cross-country comparison on the regulation of labor, Di Vita's (2017) comparison of regulatory complexity across Italian regions, and Gratton et al. (2021), also in Italy, pointing to electoral incentives as a mechanism for overproduction of laws and negative impacts on economic growth. Finally, more nuanced models describing conditions under which higher legal complexity helps or hurts includes Kawai, Lang, and Li (2018) and Foarta and Morelli (2022).

Two closely related papers, in terms of both the topic and the method, have used text analysis to assess federal regulations in the United States.

[1] A number of papers have used indexes for regulatory quality and shown a positive correlation with economic growth across countries or over time (Gørgens, Paldam, and Würtz 2004; Loayza, Oviedo, and Serven 2005; Djankov, McLiesh, and Ramalho 2006; Jalilian, Kirkpatrick, and Parker 2007; Jacobzone et al. 2010). The different indexes include measures of regulatory burden from Organization for Economic Cooperation and Development (OECD) surveys, the World Bank's Doing Business project, World Bank Governance Indicators, the Amadeus database, United Nations Industrial Development Organization (UNIDO) 3-IndStat, and Fraser Institute's Economic Freedom Index.

Dawson and Seater (2013) show that in the US time series, the number of pages in the Code of Federal Regulations is negatively related to overall national growth. Coffey, McLaughlin, and Peretto (2020) produce a panel dataset across industries since 1980 and find that stricter industry-specific regulation is associated with lower industry growth. There could be many reasons for these different results, including the discussion earlier on how our legislation instrument is driven by borrowing of successful policies among state legislators.

The mechanisms analysis around legislative completeness and investment is relevant to the large literature in labor economics and contract theory on holdup and the theory of the firm (e.g., Williamson 1979; Grossman and Hart 1986; Hart and Moore 1988). Here we extend that idea to the legislative "social contract." The public finance literature on tax compliance has made a similar connection to incomplete laws as incomplete social contracts (Weisbach 2002; Holtzblatt and McCubbin 2003; Givati 2009); consistent with our results, more detailed tax legislation can help the economy by reducing legal uncertainty (Slemrod 2005; Graetz 2007). Another related paper is Nunn (2007), who finds that industries relying on inputs requiring relationship-specific investments tend to cluster in countries with more effective legal institutions and better contract enforcement. The most recent empirical work on legal uncertainty and economic activity includes Giommoni et al. (2023) and Bamieh et al. (2025).

The rest of the paper is organized as follows. Sections II, III, and IV describe the data, text analysis methods, and empirical approach, respectively. Section V reports the main results on laws and growth, while section VI reports the extensive mechanisms analysis on what types of laws matter and under what economic conditions the effect is largest. Section VII concludes.

II. 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. A full list of variables with descriptions is shown in table A.1 (tables A.1–A.42 are available online). Summary statistics are shown in 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 (2-year periods), since many states publish their compiled statutes once every 2 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} represents local growth, measured by the change in log per capita gross state product (GSP) in state s between year $t - 2$ and year t (as the data are at the biennium level). Figure A.1 (figs. A.1–A.21 are available online) 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 consumer price index.

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 law corpus.—The dataset on legislation includes the full text of US 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 even year t including the laws from t and $t - 1$.

The statutes can include new laws, amendments to existing laws (revisions), and repealing 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.[2] Hence, our main measure of legislative volume includes all types of provisions and does not distinguish amendments or repeals. Through qualitative inspection of 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*"). Figure A.6 shows the time series for these shares over time, and they are relatively infrequent (about 3% repealing and less than 1% amending). In any case,

[2] Similarly, we cannot cleanly distinguish clauses that add regulations or remove them. Thus, 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.

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. Figure A.15 shows that, reassuringly, the instrument is not correlated with the share of either type of clause.[3]

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. 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 (nonproper) nouns. Figure A.6 shows the time series in the misspelling rate, and it is low (about 3%) and smooth over time. These misspellings 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 legislative output. Fortunately, the instrument is not correlated with the misspelling rate (fig. A.14).[4]

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 government revenues and expenditures from the state government finance census. These include total government expenditures (in thousands of current dollars) and legislative expenditures (in thousands of current dollars).

Politics.—Next, we use measures of state political conditions. For each state and year, 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.

Relationship specificity.—We measure the importance of relationship-specific investments using the data from Nunn (2007). Building on Rauch (1999), Nunn identifies inputs that are sold on an organized exchange. The idea is that exchange goods have an elastic supply on the global market, and they can be purchased in flexible quantities without established relationships. Other goods (not on exchanges) are more specialized and depend on private relationships—and relationship-specific investments.

[3] Table A.22 shows that we can also control for these variables in our regressions and it makes no difference.

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

Nunn (2007) scores industry sectors by the relative share of inputs from the latter category—that is, inputs requiring relationship-specific investments. We match those scores with Bureau of Economic Analysis data on yearly state GDP by sector, using Autor, Dorn, and Hanson's (2013) crosswalk files to match different industry classifications. The resulting dataset has information on relationship specificity for 30 sectors (three-digit North American Industry Classification System [NAICS] code). Those sectors, ordered by relationship specificity, are listed in table A.3. Intuitively, the lowest-scoring industries are oil/gas extraction and primary metal manufacturing, while the highest-scoring industries are computers/electronics and publishing.

Local economic policy uncertainty (EPU).—Finally, we have information on state-year-level EPU, constructed from the text of newspaper articles based on the approach from Baker, Bloom, and Davis (2016). 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. We count the number of articles mentioning the phrase "economic uncertainty" in a state in a given biennium and then construct a frequency by taking this count divided by the total number of news articles. 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).

III. Text Analysis Methods

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

A. 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. Figure A.4*A* shows the distribution of the number of statutes by biennium. Figure A.4*B* shows the distribution of the number of words per statute. Figure A.4*C* and A.4*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, Ash, and Morelli (2019) and Ash et al. (2020). The method works as follows, with more detail provided in appendix section 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 figure A.7. The dependency parse identifies the main verb in a sentence segment, along with the associated subject, object, verb, and information on negation.

To extract legally relevant statements, we define a set of legislative provision types (also called legal frames), including delegations, constraints, and so on (Saias and Quaresma 2003; Soria et al. 2007). We extract dependency tags associated with each legislative provision type (Lame 2003; van Engers, van Gog, and Sayah 2004); 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, is listed in table A.5. Vannoni, Ash, and Morelli (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} represents 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 nonlegislative or otherwise less informative content. Vannoni, Ash, and Morelli (2019) provide some validation against human annotations that our parser-based measure does a better job than simpler measures in identifying legally relevant statements. Figure A.2 shows that provision counts and word counts are correlated. Table A.21 explores variations on our analysis using word counts or page counts.

B. *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, Ng, and Jordan (2003). This algorithm, by now well known in the literature on text data in political economy (Grimmer and Stewart 2013; Hansen, McMahon, and Prat 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 the LDA model 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 $K \in \{6, 12, \ldots, 48\}$, we found that the instrument constructed with $K = 18$ topics (more details are given 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 table A.20, we show consistent results for all LDA models produced ($K \in \{6, 12, \ldots, 48\}$).

The baseline specification for the main text uses the LDA model with $K = 18$ topics. The list of 18 topics is reported in table 1, 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, and so on. These are the types of legal policy areas that one would expect to arise in the business of US state government.[5]

The 42-topic LDA model is used mainly 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, social regulation, fiscal policy, and procedural. To make this assignment to policy groups, all three of the coauthors 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 table A.6. 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} represent 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 $\vec{v} \ni v_i^k, \ \forall \ k \in \{1, \ldots, K\}$, where $v_i^k \geq 0$ and $\Sigma_k v_i^k = 1$. Then define legislative flows for topic k in state s during t as

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

[5] Some example provisions with topic tags are listed in tables A.7 and A.8.

TABLE 1
List of Topics, 18-Topic Specification

Label	Frequency	Most Associated Words
Courts	.0724	court, judgment, attorney, case, appeal, civil, petition, sheriff, trial, circuit court, district court, such person, complaint, counsel, brought, circuit, warrant, paid
Pensions	.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	.0645	development, local, project, budget, government, cost, grant, research, center, local government, data, transfer, governor, is the intent, develop, urban, review, biennium
Procurement	.0621	director, contract, work, review, civil, labor, contractor, attorney general, bureau, final, perform, audit, receipt, status, exempt, panel, government, firm, bid, prepared
Elections	.0612	district, town, petition, charter, special, ballot, mayor, voter, township, precinct, cast, referendum, census, elector, case, town council, said district, such district
Banking	.0604	loan, trust, bank, agent, partnership, institution, foreign, stock, mortgage, deposit, surplus, interest, merger, credit union, partner, case, credit, gift, branch, transact
Licensing	.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	.0576	real, interest, sale, owner, contract, claim, lien, payment, transfer, instrument, seller, holder, issuer, debtor, claimant, buyer, pay, broker, settlement, receipt, money
Bonds	.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	.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	.0551	governor, council, government, chief, fire, appoint, personnel, compact, conflict, perform, shall consist, invalid, parish, successor, volunteer, membership, head, travel
Healthcare	.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	.0535	child, court, minor, children, parent, age, probation, crime, victim, parole, guardian, adult, petition, placement, youth, case, social, legal, child support, obligor, home
Taxes	.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 and energy	.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	.0474	school, school district, state board, district, student, institution, higher, teacher, special, aid, pupil, children, school year, tuition, high school, school board

TABLE 1 (*Continued*)

Label	Frequency	Most Associated Words
Traffic 1	.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	.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

NOTE.—This table shows the 18 topics, along with their frequency and the most associated keywords. As can be seen, the distribution is rather dispersed and no topic is predominant. The most frequent topics across states and years are courts, pensions, and local projects, whereas the least frequent are education, traffic 1, and traffic 2.

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

C. *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).[6] 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, table A.7 shows examples of contingent provisions, while table A.8 shows examples of noncontingent provisions. These are randomly sampled from the corpus to represent different states, years, and topics. One can clearly see that noncontingent clauses impose rigid requirements, while contingent clauses depend on some environmental factor.

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

Summary statistics related to contingency are reported in appendix section B.4. About one-fifth of provisions are contingent. Table A.9 shows the changes in contingency across decades, showing that the share of

[6] For a more detailed discussion of this issue, see app. sec. B.4.

contingent clauses has decreased slightly over time, from 19.3% in the 1970s to 18.6% in the 2000s. Figure A.10 shows the time series for the share of contingencies by the four policy categories. Economic regulation clauses have usually had the highest degree of contingency.

IV. Empirical Approach

This section outlines the main features of our research design for estimating a causal effect of legislative output on economic growth. We use a shift-share instrumental variable design based on topics in legal texts. We describe how the instrument is constructed and present evidence and checks on its validity.

A. 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 let $\Delta \log Y_{st}$ equal the log change in real per capita GDP, in s during t. We assume a linear model

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho \log W_{st} + X_{st}'\beta + \epsilon_{st}, \tag{1}$$

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, preperiod state characteristics interacted with the time fixed effects—for use in robustness specifications.[7]

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.

[7] The set of variables included differs by specification. For example, in the main two-stage least squares (2SLS) results (table 3), we report results with preperiod 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 pretreatment 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 table A.1.

B. Shift-Share Instrument for Legislative Diffusion

The baseline OLS model (1) is likely to produce biased estimates given confounders and reverse causality. We address this issue using an instrumental variable approach that isolates exogenous variation in legislative output due to sharing of legal texts across US states. This section describes the source of identifying variation and formalizes the construction of the instrument.

A classic motivation for federalism institutions is that the constituent units—for example, US states—can act as laboratories of democracy in the discovery and adoption of good policies (Burgess et al. 2016). When states adopt good policies through legislation, legislators in other states can learn from that example and adopt similar policies in their laws. As discussed in previous work, one of the main drivers of policy diffusion across US states is policy success; successful policies are more likely to spread across states (Volden 2006; Pacheco 2012; Shipan and Volden 2014; Butler et al. 2017). For example, Yu, Jennings, and Butler (2020) find that in the case of drunk driving laws, only policies that reduce the total fatality rate tend to spread across states. Souza, Rasul, and de Paula (2019) document tax competition between states, while DellaVigna and Kim (2022) find policy diffusion between neighboring states and states with similar political makeups.

As these policies are embodied in legislative text, state-to-state policy diffusion typically consists of borrowing of text (Burgess et al. 2016). Other things equal, it is cheaper to use previously used text than draft something from scratch. This borrowing mechanism is likely to be strengthened in the case of US states because the state legislatures are relatively resource-constrained (e.g., Malhotra 2006). Burgess et al. (2016) document borrowing of text, showing that in recent years some of that is driven by organizations publishing model legislation (see also Hansen and Jansa 2021).

To summarize, previous work shows that state legislators borrow legal provisions from other states. That borrowing is due in part to resource constraints, suggesting that there are drafting costs for new legislation. Hence, we would expect more borrowing on topics where a given state has not yet legislated in much detail. Further, there is a tendency to selectively borrow successful provisions. Hence, the legislation that diffuses by borrowing is positively selected in terms of its impact on society and the economy.

These ideas motivate the construction of our instrument. Formally, we adapt a shift-share instrumental variable design, often attributed to Bartik (1991, 1994) but 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 pretreatment 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, Dorn, and Hanson 2013, 2016), market size and drug innovation (Acemoglu and Linn 2004), small-business lending and economic growth (Greenstone, Mas, and Nguyen 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 (Jaeger, Ruist, and Stuhler 2018; Adao, Kolesar, and Morales 2019; Goldsmith-Pinkham, Sorkin, and Swift 2020; Borusyak, Hull, and Jaravel 2022).

To link our setting to that of more traditional shift-share designs, we 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 represents the number of statements on topic k in s at t.

The local "shares" are a state's preperiod stock of legislative output on each topic, analogous to preperiod employment shares across sectors, or preperiod immigrant population shares across origin countries. Formally, we construct the pretreatment legislative topic shares as the average of topic shares over the decade before our analysis (1955–64), represented as period zero: W_{s0}^k / W_{s0}.[8]

[8] Note that we divide a state's cumulative statutes by topic, W_{s0}^k, by the total statute output in those years, W_{s0}. This is needed to normalize variation by state, such that instrument variation is driven by topic variation within state. We cannot use W_{s0}^k itself as the shares (the level, rather than the share) due to the large level differences across states, which then produces a very noisy instrument. In line with that, the shift-share instrument based on levels does not get a strong enough first stage (F-statistic < 2.5). The estimated 2SLS coefficients are the same sign and similar in magnitude ($\hat{\rho} \approx 0.03$) but noisy and not statistically significant. We include all topics in constructing the instrument, as recommended by Borusyak, Hull, and Jaravel (2022), relative to a situation where only a subset of shares is used for the instrument (as in Autor, Dorn, and Hanson 2016). Moreover, the use of pretreatment shares is advisable in situations where shocks are serially correlated and shares are affected by lagged shocks.

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, $(1/49)\Sigma_{r\neq s}\Delta \log W_{rt}^{k}$, where r indexes the other 49 states. Borusyak, Hull, and Jaravel (2022) 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 pretreatment 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.[9] 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

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

that is, regressing the outcome directly on the instrument.

C. *First Stage*

In the classic shift-share instrument, it is expected that the first-stage effect of Z_{st} on the endogenous regressor (e.g., employment), ψ, is positive, as having high previous shares of sectors that are increasing nationally will tend to get pulled upward. In our setting, however, the effect of previous shares goes in the other direction. As outlined in the discussion above, it is states with relatively low previous legislating on a topic that will be pushed most by national upward shifts on that topic. Hence, we expect that $\psi < 0$.

Figure 2 illustrates the first-stage relationship, which is statistically significant ($p = .003$) and produces a Kleibergen-Paap first-stage F-statistic of 22.8 in the baseline specification. As expected, the first-stage relation between legislative flow and the instrument is negative. When a state had

[9] See table A.10 for summary statistics on the instrument and endogenous regressor by decade.

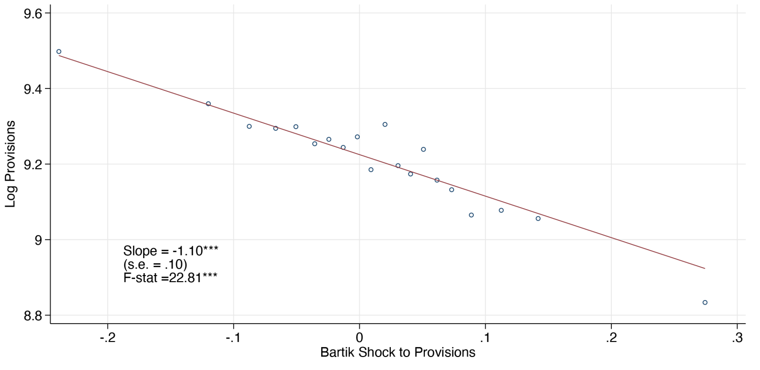

FIG. 2.—First stage: impact of shift-share legislative shock on legislative output. Shown is a binned scatterplot for the first-stage relationship (eq. [3]) between the shift-share instrument (x-axis) and the log number of provisions (y-axis). State and year fixed effects are absorbed.

initially low detail on a topic, then it was more likely to increase legislating in response to national trends on that topic. 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 (fig. A.11).

D. *Exogeneity and Exclusion*

There are two approaches to identification in shift-share designs. In the first approach, one assumes that the preperiod shares are conditionally exogenous (Jaeger, Ruist, and Stuhler 2018; Goldsmith-Pinkham, Sorkin, and Swift 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, Hull, and Jaravel 2022). In our case, this assumption could be formally stated as

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

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 preperiod legislative topic shares are uncorrelated with subsequent trends in economic growth during the treatment period. This assumption is difficult to justify, since the preperiod 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 Jaeger, Ruist, and Stuhler (2018) and Goldsmith-Pinkham, Sorkin, and Swift (2020) in the framework that assumes exogeneity of pretreatment shares. Table A.13 shows that the instrument is uncorrelated with pretreatment state characteristics. Figure A.13 shows that pretreatment 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 Adao, Kolesar, and Morales (2019) and Borusyak, Hull, and Jaravel (2022), relies on different, arguably weaker, assumptions. In these frameworks, the exclusion restriction follows from the conditional exogeneity of the current-period shifters, rather than from the pretreatment shares. No assumption is needed with respect to the pretreatment 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, Dorn, and Hanson (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

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

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, Hull, and Jaravel 2022).[10]

In line with Adao, Kolesar, and Morales (2019) and Borusyak, Hull, and Jaravel (2022), we take a number of steps to assess the validity of Z_{st} as an

[10] 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-19 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—e.g., 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, e.g., positive spillovers in the outcome due to gains from trade or 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.

instrument for log W_{st} (see app. sec. C). First, to check that the relevance of the shift-share instrument is driven by a majority of topics, for every topic (including state and year fixed effects and clustering standard errors by state) 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. We find that topic growth is statistically significant in the great majority of topics, as shown in figure A.12. Second, we use the test for weak instruments, robust to heteroskedasticity, 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-statistic is greater than 23.1. In our data, the effective F-statistic equals 132.8 and we reject the weak instrument null at 5% significance. Third, table A.12 reports the following placebo test: we regress economic growth on future values of the legislative growth instruments. The estimates are not statistically significant.[11] Fourth, we run a balance test by regressing the instrument on some potential confounders. Table A.14 shows that the instrument is not correlated with current or lagged values for relevant state characteristics.

V. Main Results

This section reports the main empirical results. We start in section V.A with an empirical test for whether greater legal output causes greater or lower growth at the margin. We then report some robustness checks and supporting results on the main effect in section V.B.

A. *Main Results on Legislation and Growth*

Table 2 presents the first results for legislative output and growth. 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 on growth, from equation (4). As discussed above, the reduced-form coefficient is negative, reflecting that lower pretreatment detail on a topic is associated with a positive shock to legislative output. Additional specifications for OLS and the reduced form are shown in tables A.15 and A.16, respectively.

2SLS estimates for ρ, the effect of legislative output on growth, are reported in table 3. Column 1 gives the baseline 2SLS estimate with state fixed effects and biennium fixed effects. It is positive and statistically significant, similar in magnitude to the OLS, meaning that at the margin an

[11] See also table A.17 for additional results on leads and lags of the effect.

TABLE 2
FIRST STAGE (FS), OLS, AND REDUCED FORM (RF)

	Effect on Provisions		Effect on Real GDP Growth per Capita			
	FS (1)	FS (2)	OLS (3)	OLS (4)	RF (5)	RF (6)
Legislative output			.0146*	.0152		
			(.00832)	(.0123)		
Instrument (Z_{st})	−1.099***	−1.221***			−.0200**	−.0205**
	(.230)	(.259)			(.00883)	(.00940)
Observations	1,183	1,183	1,182	1,182	1,182	1,182
R^2	.813	.9	.431	.446	.420	.440
State fixed effects	X	X	X	X	X	X
Time fixed effects	X	X	X	X	X	X
State-specific trends		X		X		X

NOTE.—Columns 1 and 2 show the estimates for the first stage (eq. [3]). Columns 3 and 4 show the results for OLS estimates of eq. (1). Columns 5 and 6 give the reduced-form specification (eq. [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 are clustered by state.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

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; neither does the set of pretreatment controls, interacted with fully saturated time effects, added in columns 3–5. The results are not sensitive to controls for current-period topic shares (col. 6). Finally, we can take everything together and add the lagged dependent variable (col. 7), still producing a positive and statistically significant coefficient but with a slightly smaller magnitude when including endogenous controls.

These robust positive effects of legislative output on growth are economically meaningful in their magnitudes. The estimates suggest that a 10% increase in borrowed legislation—the approximate change triggered by a 1 standard deviation change in the residualized instrument[12]—would increase the per capita economic growth rate by 0.1–0.2 percentage points, relative to a mean of 3.1 percentage points. To give some intuition for this magnitude, recent work in empirical macro suggests that achieving a 0.15 percentage point increase in the growth rate through a fiscal stimulus would require about a 0.1 percentage point increase in net-of-tax government spending (e.g., Nakamura and Steinsson 2014; Chodorow-Reich

[12] More precisely, after residualizing out state and biennium fixed effects, a 1 standard deviation change in the instrument is 0.12. Multiplying that by the first-stage coefficient 1.1 generates a predicted change of 0.13, or a 13% increase in legislative output.

TABLE 3

EFFECT OF LEGISLATIVE OUTPUT ON ECONOMIC GROWTH (2SLS)

	EFFECT ON GROWTH RATE PER CAPITA						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Legislative output	.0182**	.0168*	.0152**	.0134*	.0116*	.0222**	.0094*
	(.00903)	(.00863)	(.00704)	(.00687)	(.00602)	(.0106)	(.00507)
First-stage							
F-statistic	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 fixed effects	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
State trends		X					X
Economic variables × time			X				X
Sector shares × time				X			X
Demographic variables × time					X		X
Topic shares						X	X
Lagged government expenditures							X
Lagged dependent variable							X

NOTE.—Results for the 2SLS model (second stage [1] and first stage [3]). All specifications include state and biennium fixed effects. Column 2 adds state-specific linear trends. Column 3 adds a set of preperiod 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 col. 5 adds demographic characteristics (share of urban, foreign, and population) measured in the pretreatment 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 table A.1. Standard errors are clustered by state.

* $p < .10$.

** $p < .05$.

2019). Hence, a 10% increase in statute flows is equivalent in its growth effect to a 0.1% increase in government spending flows.

B. Robustness Checks on Main Results

This section enumerates a series of checks to assess robustness of the specification and evaluate alternative channels for the results. First, table A.17 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 (cols. 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 (cols. 4–7) but significant at the 10% level in the reduced form (col. 8).[13] The zero estimate for the preshock placebo lead and a positive estimate for the postshock lag provide some additional reassurance on the validity of the instrumental variable design.

Next, we report a number of robustness checks in regard to the topics. In line with Borusyak, Hull, and Jaravel (2022), we show that results are robust to the inclusion of topic share controls, both in levels and in changes (see table A.19). The results are not driven by any single topic (fig. A.16). Next, the results are not sensitive to the number of topics used in the construction of the instrument. Table A.20 shows results for six, 12, 24, 30, 36, 42, and 48 topics. The main results hold regardless of the topic count for constructing the instrument.

To check further that our syntax-based measure of legislative provisions is capturing the legally and economically relevant component of legislative text, we run our analysis using alternative measures of legislative detail for the instrument and treatment. Table A.21 shows that when using number of words rather than number of provisions as the endogenous regressor (and for constructing the instrument), we obtain positive 2SLS estimates that are not statistically significant (cols. 1, 2). This result supports our argument from above that our neurolinguistic programming method is needed to extract legally relevant information from the statute texts. In line with this idea, our main result is robust to including the number of pages in the published statutes as a control (col. 4).

To better understand the effect on growth, appendix section D.3 provides supporting results for the effect of legislative detail on some alternative outcomes. Table A.24 shows that the estimated coefficient is identical (yet less precise) when using growth in GDP as the outcome rather

[13] In particular, we note that in the specification with additional controls, the *p*-value for the lead is .86 (col. 3), while the *p*-value for the lag is .27 (col. 7).

than growth in GDP per capita (col. 1), as there is no effect on population (col. 2). Further, we can rule out that the effects on growth are driven by changes in employment (col. 3) or the number of establishments (col. 6). Meanwhile, there are positive effects on other signifiers of economic expansion, including profits (col. 4) and labor income (col. 5). Looking to other government activities besides legislation, table A.26 shows that 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 or taxes suggests 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 the policymaking procedure. The null effect on party control means that there does not appear to be intervening effects in the state political environment.

So far, our analysis has left out some potentially important additional sources of laws: bureaucratic regulations and the courts. Appendix section D.4 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 case law.

Finally, table A.18 reports the baseline specification with alternative clustering of standard errors. The results are robust to not clustering (cols. 1, 2) as well as two-way clustering by state and year (cols. 3, 4). Following Adao, Kolesar, and Morales (2019), we apply *k*-means clustering on the preperiod 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 (cols. 5–10).

VI. Legal and Economic Mechanisms

This section provides additional evidence to unpack the legal and economic factors that are most relevant to the impact of laws on growth. First, section VI.A lays out our conceptual framework for analyzing laws and growth, founded in how more complete laws can increase economic activity through more relationship-specific investments. We then report evidence on the resulting predictions. These include heterogeneity by type of policy (sec. VI.B), by contingent versus noncontingent clauses (sec. VI.C), by preexisting level of detail (sec. VI.D), by relationship specificity across industrial sectors (sec. VI.E), and by levels of EPU (sec. VI.F).

A. Conceptual Framework

These supporting analyses are motivated by the idea that more complete legislation can increase location- and relationship-specific investments by reducing ex post holdup. The "holdup model" has a long tradition in the economic literature, showing that contract detail is important for relationship-specific investments (Williamson 1979, 1985; Grossman and Hart 1986). The main idea is input suppliers need to make specific investments to customize the input for the needs of the final good producer. Hence, they need more protection—namely, more detailed and enforceable contracts. If the contracts are not well enforced ex post, because of the lack of details, there will be less investment ex ante (Klein, Crawford, and Alchian 1978; Hart and Moore 1990; Nunn 2007).

Applied to state legislatures, we start with the notion that state government creates legislation that businesses need to comply with. This legislation might require businesses to specialize their investments and structure their supply chains to this legal context, meaning that those investments have less value if moved to other jurisdictions. If legislation is ambiguous or incomplete, then businesses face uncertainty about how the rules will be enforced by regulators or courts. If a business makes relationship-specific investments based on the incomplete legislation but then regulators or courts fail to enforce them as expected, the business can be "held up" where its investments become less valuable or even worthless. Hence, in states with incomplete legislation, businesses will be more hesitant to make full, optimal investments in the first place. This underinvestment limits economic growth and provides a mechanism by which increased completeness in legislation can lead to more growth at the margin.

An implicit assumption in this holdup interpretation is that the adopted laws are helpful to businesses on average. That is, the laws mostly make the economic environment more stable for commerce, rather than do harm to the economy due to mistakes or rent-seeking. In the case of US states, some notable institutional factors contribute to a beneficial effect of more lawmaking. There is competition between states to attract businesses, and there is social learning between states about reforms adopted in other states (Souza, Rasul, and de Paula 2019). In particular, our instrument is likely to be driven by efficiency-enhancing laws and regulations, as it is constructed based on laws that are borrowed from other jurisdictions. The publication of laws in other states reduces the writing costs of enacting those laws. If those laws are helpful, they can be adopted; if they are not a good fit, they can be ignored. Hence, our instrument captures an expansion of the choice set; under minimal benevolence assumptions, the legislative detail triggered by the instrument is likely to increase completeness in the legislative social contract and to help businesses on average. As mentioned above, previous empirical work on state-to-state policy diffusion suggests that successful policies are more likely to diffuse (Volden 2006; Pacheco 2012; Shipan and Volden 2014; Butler et al. 2017; Yu, Jennings, and Butler 2020).

These ideas and empirical predictions are put on a more formal footing in appendix section E. First, we give more formal detail to a writing costs approach to legislating, based on Battigalli and Maggi (2002, 2008). Second, we present an alternative decision theory framework, which models the legislator's choice when to legislate. Both models undergird and complement the holdup model based on relationship-specific investments and contract completeness.

Now we outline a number of additional testable predictions that arise from a holdup model of legislative detail. These can then be taken to data in the subsequent sections.

Heterogeneity across legislative policy topics.—If our results have to do with business investments, then a first expectation is that the clauses that matter most should be those on policies about regulating business. Policies that are less related to business should have less of an effect on growth.

Relative effect of contingent clauses.—A key feature of complete contracts is the inclusion of contingencies, which condition actions and outcomes on the state of the world (Battigalli and Maggi 2002). Contingencies do more than noncontingencies to split up the state space and leave less ambiguity for regulators and courts in the interpretation of laws. Contingencies are especially valuable in long-term relationships that are more likely to involve specific investments (Battigalli and Maggi 2008). Hence, if those contingencies come through borrowing from other jurisdictions, they are even more likely to promote growth than noncontingencies. Noncontingent laws impose rigid requirements or else give discretion to enforcers. Therefore, noncontingent laws may even tend to inhibit business activity.

Concavity in existing legal detail.—In any model of contract completeness (e.g., Battigalli and Maggi 2002), one can rank the topics or clause types by their relative legal and economic importance. The contract designer will write the most important clauses first, and as one moves down the ranking there is a decreasing marginal benefit of adding clauses. Hence, we expect heterogeneity by preexisting detail in response to an exogenous increase in clauses. Starting at a relatively low level of detail, there should be a larger effect on specific investments and economic growth.

Relationship specificity of sector inputs.—If our model is right, we would see an increase in relationship-specific investments between firms in response to an increase in legal detail. But relationship-specific investments cannot be observed directly. As a proxy, following Nunn (2007), we can assess their importance indirectly by looking for heterogeneous effects across sectors based on relationship specificity of the intermediate inputs in that sector. We expect that effects of laws on growth are concentrated among the sectors relying more on relationship-specific investments.

EPU.—A key ingredient of the holdup model is uncertainty about the future. When a rare event (not covered by the contract) becomes more likely due to increased uncertainty, then the expected costs of that event increase and in turn the benefits of describing that event in the contract increase. Adapting this to our legislative context, we expect that an exogenous increase in legal detail would have a larger positive impact on growth when economic uncertainty is higher. More specifically, as discussed in Battigalli and Maggi (2008), we expect that under higher uncertainty more contingencies are more beneficial for growth, as the benefit to conditioning legal outcomes on the state increases.

B. Heterogeneity across Legislative Policy Topics

Our instrument identifies an average effect that combines many factors across many different types of legislative texts. Here we check what types of policies are pivotal for the effect. We expect the effect to be driven by policies related to specific business investments. In particular, we would expect the largest effect from policies that regulate economic activity (e.g., contracts, licensing, property rights), with less of an effect from other policies, such as those regulating social issues (e.g., family law, criminal justice).

As described in section III.B, we divide the LDA topics into the four more interpretable categories: economic regulation, social regulation, fiscal policy, and procedural. Thus, we have four separate endogenous regressors W_{st}^{l}, representing the log number of provisions in state s at biennium t allocated to topics in policy category l. In turn, we produce separate shift-share instruments for each of the four categories. The calculation is the same as in section IV.B, except that rather than summing over all topics K, we sum over the subset of topics K_l within each respective policy category. We therefore get a separate instrument Z_{st}^{l} for each policy. We then estimate the baseline 2SLS system (eqq. [1], [3]) separately for each of the four categories l, where the category-specific endogenous regressor W_{st}^{l} and instrument Z_{st}^{l} are appropriately slotted in.

The effects across policy categories are reported in table 4. We can see, first, that there is a positive and significant ($p < .10$) effect of economic regulations (col. 1) and no effect at all of social regulations (col. 2). This is consistent with the investment hypothesis, where clearer rules about economic issues reduce holdup and lead to more economic activity, while clearer rules about social issues have less of an effect.

Further, we find that fiscal policy rules are impactful for growth (col. 3). This also makes sense from an investment view given that many place-based policies are implemented through taxes and public spending.

TABLE 4
WHAT POLICIES ARE DRIVING THE EFFECT OF LAWMAKING ON GROWTH?

	Effect on Real GDP Growth per Capita			
Policy Category	Economic Regulation (1)	Social Regulation (2)	Fiscal (3)	Procedural (4)
Legislative output	.0125*	−.0006	.0220**	.0009
	(.00697)	(.0097)	(.0107)	(.009)
First-stage F-statistic	42.53	13.42	18.68	49.12
Observations	1,182	1,182	1,181	1,182
Time fixed effects	X	X	X	X
State fixed effects	X	X	X	X

NOTE.—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.
* $p < .10$.
** $p < .05$.

Consistent with that view, recall that the effect of laws on growth is not driven by changes in government expenditures (table A.26). That is, the fiscal-policy effect is driven not by a spending multiplier but rather through legal changes in how money is collected or spent (e.g., targeted tax exemptions or subsidies). Finally, procedural rules (e.g., electoral administration) are not as important for economic growth (col. 4).

C. *Relative Effect of Contingent Clauses*

In the context of making an optimal set of rules or encouraging relationship-specific investments, contingencies are pivotal in moving legislation toward a more complete contract. In Battigalli and Maggi (2002), for example, it is optimal for the most important contract topics to have more contingent clauses (see also Battigalli and Maggi 2008). A more complete contract helps reduce legal uncertainty, and reduction in legal uncertainty generates more stable relationships within and across firms, thereby allowing for better economic outcomes.

As described in section III.C, we produce separate counts for contingent provisions (W_{st}^C) and noncontingent provisions (W_{st}^N). We estimate variants of the 2SLS system (3) and (1) but using the contingent and noncontingent measures of laws as joint endogenous regressors. The second stage is

$$\Delta \log Y_{st} = \alpha_s + \alpha_t + \alpha_s \cdot t + \rho_C \log W_{st}^C + \rho_N \log W_{st}^N + X_{st}'\beta + \epsilon_{st}, \qquad (7)$$

where now we have two endogenous regressors, with the associated causal effects of interest for contingencies (ρ_c) and noncontingencies (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 noncontingent provision counts, respectively. Let Z_{st}^C give the contingency instrument and let Z_{st}^N give the noncontingency instrument. The first-stage equations are

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

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

where all terms are as above. Appendix section D.5 reports additional checks and results for these instruments.

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

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

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

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

which gave a higher first-stage F-statistic than computing a single differenced 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 5, with the different specifications analogous to those from table 3. Columns 1 and 2 provide the estimates for the second stage (7) with two endogenous regressors (contingent and noncontingent), instrumented by first stages (8) and (9). We can see in both columns that the 2SLS effect of contingent clauses is positive, while the 2SLS effect of noncontingent clauses is negative.

Next, columns 3–7 show the estimates for the differenced (contingent minus noncontingent) second stage (10) with first stage (11). Consistent with the separate-treatments specification, there is a large positive effect of relative use of contingency. The effect is robust to including state trends or including pretreatment characteristics interacted with time fixed effects.

The magnitude of the coefficients on contingency clauses are also notable. They are much larger than that for total provisions—three to four times as large. Overall, these results support the view that contingent clauses are most important for promoting investment and growth.[14]

[14] From table A.9 and fig. A.9, we see that the log difference in contingencies and noncontingencies is actually slightly decreasing over time, so the overall aggregate predicted change in output due to changes in legislative volume over this period may be negative.

TABLE 5

Effect of Contingent and Noncontingent Clauses on Economic Growth

	Effect on Real GDP Growth Per Capita						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Contingent provisions	.0638***	.0590***					
	(.0226)	(.0215)					
Noncontingent provisions	−.0559**	−.0511**					
	(.0242)	(.0228)					
Contingent − noncontingent			.0752***	.0697***	.0501**	.0379**	.0773***
			(.0242)	(.0229)	(.0219)	(.0158)	(.0219)
First-stage F-statistic	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 fixed effects	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
State trends		X		X			
Economic variables × time					X		
Sector shares × time						X	
Demographic variables × time							X

Note.—Results for the 2SLS model of contingencies. Columns 1 and 2 show results for contingent and noncontingent clauses together (second stage [7] and first stages [8] and [9]), adding state-specific trends in the second column. Columns 3–7 show the results for the difference between contingent and noncontingent clauses (second stage [10] and first stages [11]). Column 4 adds state-specific trends, col. 5 adds preperiod economic variables interacted by year, col. 6 interacts initial sector shares by biennium, and col. 7 shows initial demographic characteristics interacted by biennium. All specifications include controls for state and biennium fixed effects.

** $p < .05$.

*** $p < .01$.

Appendix section D.5 reports a number of supporting results. Table A.34 reports additional specifications with the differenced treatment variable, showing that it is robust to inclusion of other variables. Table A.35 shows the effects for other intermediate economic outcomes. Table A.36 shows the results when using contingency and noncontingency counts by themselves as the endogenous regressor.

D. *Concavity in Existing Legal Detail*

Here we assess a potential concave relationship of legislative detail and economic growth. Since there would be decreasing marginal benefits in completing a legislative contract, the effect of adding laws should be larger in contexts with a relatively low preexisting stock of laws. We take that intuitive idea to the data.

Historical records on the stock of legislation (the annotated code) are 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 (10 years). The idea is that at any given point, the ranking of states by the historical flow of provisions can proxy for the ranking of states by the total stock of provisions.

Correspondingly, we rank 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 (eqq. [3], [1]) but subsetting by the three terciles. We also look at concavity in the effect of contingent clauses by estimating the 2SLS system for the effect of the difference in contingencies and noncontingencies (eqq. [11], [10]). We would expect a larger effect of new laws in the sample with lowest previous detail.

Table A.37 reports the estimates. Consistent with a concave relationship, we find that the effect of new laws on economic growth is stronger for states with low recent legal volume (cols. 1–3) compared with states with medium detail (cols. 4, 5) or high detail (cols. 6, 7). The effect for low-detail states is robust to state trends (col. 2) and also holds for the effect of contingencies (col. 3). Appendix section D.6 provides additional specification checks for the concavity analysis. In particular, table A.39 shows that we get similar results when the concavity thresholds are computed after residualizing on the state and year fixed effects.

E. *Sectoral Relationship Specificity*

Our framework takes a relatively broad view of specific investments in our empirical context, where, for example, an investment could be specific to a state subsidy or banking regulation. That said, firm-to-firm investments are key and do depend on the legal environment. While relationship-specific investments between firms cannot be measured directly, we can test for their importance indirectly by assessing heterogeneity across sectors that vary by relationship specificity. Specifically, we expect that the effect of additional clauses will be larger in those sectors where there are more goods with intermediate inputs that require relationship-specific investments.

For each industry, we have a proxy from Nunn (2007) on the proportion of intermediate inputs that are relationship-specific. That is measured as the proportion of inputs not sold on global exchanges. We calculate state-biennium GDP growth but limited to the sectors with high and low relationship specificity, respectively. We then estimate the 2SLS regressions from above using the separate outcomes. We would expect a larger effect of new laws in the sectors with high relationship specificity.

Table 6 reports the results on heterogeneity by sectoral relationship specificity. First, columns 1–3 show results for sectors with low relationship specificity—that is, sectors such as fossil fuels and primary metals, where inputs are purchased on global exchanges. Exogenous increases in legal detail, overall (col. 1) or through contingencies (cols. 2, 3), have no effect on output in those sectors.

Next, columns 4–9 report the estimates for sectors with high relationship specificity—sectors such as electronics and publishing, where firms have special relationships with suppliers to provide customized inputs that are not sold on global exchanges. We can see here that, in contrast to the low-specificity sectors, there is a positive and statistically significant effect of laws on growth. That holds for overall legislative output (cols. 4, 5) as well as contingencies (cols. 6–9), and it is robust to inclusion of state trends. These results are consistent with relationship-specific investments being an essential mechanism in the effect of laws on growth.

F. EPU

A final supporting analysis is on the moderating role of uncertainty in the economic environment. When uncertainty increases, the benefits from a greater completeness of the law typically increase. Rare events become more frequent and therefore need to be covered by contingencies to avoid holdup.

To measure such uncertainty, we adapt the validated measure of EPU, constructed and explored by Baker, Bloom, and Davis (2016) in the context of the US national economy. We use the state-level annual measure of local EPU described in section II and rank the state-biennium observations by uncertainty. We then split the sample into three terciles based on the uncertainty ranking.

Table 7 reports 2SLS estimates for each tercile in EPU, looking at the baseline results with total provisions, as well as the contingency analysis using contingent and noncontingent clauses. Columns 1 and 2 include

TABLE 6

Heterogeneous Effects by Relationship-Specific Investments

	Effect on Real GDP Growth by Sector Group								
	Low Relationship Specificity			High Relationship Specificity					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Legislative output	.000231			.0488**	.0414*				
	(.0221)			(.0225)	(.0211)				
Contingent provisions		−.00659				.217*	.177*		
		(.0979)				(.109)	(.104)		
Noncontingent provisions		.00864				−.204*	−.164		
		(.117)				(.117)	(.113)		
Contingent − noncontingent			−.00342					.237**	.197**
			(.0795)					(.103)	(.0952)
First-stage F-statistic	22.83	18.2	19.26	22.83	21.74	18.2	34.4	19.26	33.42
Observations	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133
Time fixed effects	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X
State trends					X		X		X

Note.—Results for the 2SLS model (second stage [1] and first stage [3]), where GDP growth (not per capita) is constructed from sectors with below-median relationship specificity (cols. 1–3), or from those with above-median relationship specificity (cols. 4–9). Relationship specificity scores are constructed using data from Nunn (2007) on the share of inputs that are not traded on public exchanges. Also includes estimates for the differenced contingency detail measure, as indicated. All specifications include state and biennium fixed effects and results for state-specific trends are also reported (as indicated).

* $p < .10$.

** $p < .05$.

TABLE 7

Effect of Laws on Growth by the Level of EPU

	Effect on Real GDP Growth per Capita									
	Low Economic Uncertainty		Medium Economic Uncertainty		High Economic Uncertainty					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Legislative output	.00448		.00699		.0373**	.0391**				
	(.0111)		(.0111)		(.0153)	(.0176)				
Contingent provisions							.145**	.170**		
							(.0560)	(.0672)		
Noncontingent provisions							−.137**	−.163**		
							(.0624)	(.0775)		
Contingent − noncontingent		.0823		.000182					.164***	.189***
		(.0692)		(.0310)					(.0465)	(.0568)
First-stage F-statistic	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 fixed effects	X	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X	X
State trends						X		X		X

Note.—Results for the 2SLS model (second stage [1] and first stage [3]), splitting up the data by terciles in EPU (Baker, Bloom, and Davis 2016). Also included are estimates for the differenced contingency detail measure, as indicated. Columns 1 and 2 show results for states with lowest tercile uncertainty. Columns 3 and 4 report results for those with median uncertainty, while cols. 5–10 report results for 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).

** $p < .05$.

*** $p < .01$.

estimates for low uncertainty, columns 3 and 4 include them for medium uncertainty, and columns 5–10 include them for high uncertainty. The specifications are the same as those reported in tables 3 (baseline) and 5 (contingency).

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

In contrast, consider columns 5–10, 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 3. A similar magnified effect is seen for contingency in columns 7–10. 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 view that the effects of law on growth are moderated by higher or lower EPU.

Appendix section D.6 provides additional specification checks for the uncertainty analysis. In particular, table A.38 shows that concavity and uncertainty recover independent dimensions in the dataset. In addition, table A.40 shows similar results when the uncertainty variable is residualized on state and year fixed effects before the ranking and division into terciles.

Table A.41 shows that the uncertainty effect is robust to the inclusion of lagged growth per capita, suggesting that it is not driven simply by the EPU measure picking up the business cycle. Also consistent with this point, table A.42 shows that if we split up the sample based on recent growth (rather than recent detail or current EPU), we see effects of legislative output on growth in both the top and the bottom tercile. Overall, these checks suggest that the effect heterogeneity from high EPU is not driven by confounding business-cycle trends.

VII. Conclusion

This paper explores what makes legislative output matter for growth. In the empirical setting of the US states for the years 1965–2012, 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 the mechanism that we consider most important for the results: making a more complete legislative contract, which reduces ex post holdup and increases ex ante relationship-specific investments. Consistent with this mechanism, we indeed find that the positive impact on growth is driven by economic rather than social regulations, is higher when the additional legislation is in the form of contingent clauses, is larger starting from lower legislative completeness, is strongest for sectors that rely more on relationship-specific inputs, and is concentrated in periods of greater EPU.

Methodologically, we build on the empirical literature in economics through novel use of legal text data in a causal framework. 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. These methods could be useful in other contexts with borrowing of texts between units—for example, diffusion of technologies into patent filings, diffusion of source code between software projects, or diffusion of narratives on social media. Such explorations could use simulations and other methods to better understand the robustness of text-based instruments and in particular their sensitivity to different preprocessing or featurization steps.

Substantively, it could be interesting to extend the approach to allow for spillover effects of laws on neighboring states (Souza, Rasul, and de Paula 2019; DellaVigna and Kim 2022). 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 give a fuller picture of the welfare consequences of legislative borrowing. For example, one could use county-level economic output data at state borders to help get at this question, potentially using data on job-to-job transfers and cross-state commuting.

The external validity of our empirical results is an open question, and it would be interesting and useful to seek similar evidence in other federal systems, such as Canada, Switzerland, or the European Union. The theoretical mechanisms that we have explored could apply more broadly, however, and could help guide future empirical work. In particular, external validity to other contexts would depend on different institutional frameworks. As shown in Gratton et al. (2021), for example, signaling incentives can have a strong effect on the quantity and quality of laws. In a system with strong signaling incentives and a large stock of legislation—for example, Italy—a reduction in legal writing costs may have a very different impact from the case of US state legislators, who have weaker signaling distortions and face competitive pressures tending toward efficiency. Other relevant factors include professionalism among state legislators, the quality of laws in other states, and specialized agencies to support legislative drafting (Bendor 1995). Foarta and Morelli (2022) suggest a theory of complexity and reforms that reconciles some of the different empirical findings. We hope that a combination of more theory and data analysis could bring about a broader understanding of the applicability of these results across different institutional contexts.

Data Availability

Code and data for replication purposes are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/DNSNSU (Ash, Morelli, and Vannoni 2024).

References

Acemoglu, D., and J. Linn. 2004. "Market Size in Innovation: Theory and Evidence from the Pharmaceutical Industry." *Q.J.E.* 119 (3): 1049–90.

Acemoglu, D., S. Naidu, P. Restrepo, and J. A. Robinson. 2019. "Democracy Does Cause Growth." *J.P.E.* 127 (1): 47–100.

Adao, R., M. Kolesar, and E. Morales. 2019. "Shift-Share Designs: Theory and Inference." Working paper.

Ash, E., J. Jacobs, B. MacLeod, S. Naidu, and D. Stammbach. 2020. "Unsupervised Extraction of Workplace Rights and Duties from Collective Bargaining Agreements." Paper presented at the International Workshop on Mining and Learning in the Legal Domain (MLLD), Sorrento, Italy, November 17.

Ash, E., M. Morelli, and M. Vannoni. 2024. "Replication Data for: 'More Laws, More Growth? Evidence from US States.'" Harvard Dataverse, https://doi.org/10.7910/DVN/DNSNSU.

Autor, D., D. Dorn, G. Hanson, and K. Majlesi. 2020. "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure." *A.E.R.* 110 (10): 3139–83.

Autor, D. H., D. Dorn, and G. H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *A.E.R.* 103 (6): 2121–68.

———. 2016. "The China Shock: Learning from Labor-Market Adjustment to Large Changes in Trade." *Ann. Rev. Econ.* 8:205–40.

Baker, S. R., N. Bloom, and S. J. Davis. 2016. "Measuring Economic Policy Uncertainty." *Q.J.E.* 131 (4): 1593–636.

Bamieh, O., D. Coviello, A. Ichino, and N. Persico. 2025. "Effect of Business Uncertainty on Turnover." *J. Labor Econ.*, forthcoming.

Bartik, T. J. 1991. *Who Benefits from State and Local Economic Development Policies?* Kalamazoo, MI: W.E. Upjohn Inst. Employment Res.

———. 1994. "The Effects of Metropolitan Job Growth on the Size Distribution of Family Income." *J. Regional Sci.* 34 (4): 483–501.

Basso, G., and G. Peri. 2015. "The Association between Immigration and Labor Market Outcomes in the United States." Discussion Paper no. 9436, Inst. Labor Econ., Bonn.

Battigalli, P., and G. Maggi. 2002. "Rigidity, Discretion, and the Costs of Writing Contracts." *A.E.R.* 92 (4): 798–817.

———. 2008. "Costly Contracting in a Long-Term Relationship." *RAND J. Econ.* 39 (2): 352–77.

Bendor, J. 1995. "A Model of Muddling Through." *American Polit. Sci. Rev.* 89 (4): 819–40.

Blanchard, O., and L. Katz. 1992. "Regional Evolutions." *Brookings Papers Econ. Activity* 23 (1): 1–76.

Blei, D. M., A. Y. Ng, and M. I. Jordan. 2003. "Latent Dirichlet Allocation." *J. Machine Learning Res.* 3 (January): 993–1022.

Borusyak, K., P. Hull, and X. Jaravel. 2022. "Quasi-experimental Shift-Share Research Designs." *Rev. Econ. Studies* 89 (1): 181–213.

Botero, J. C., S. Djankov, R. L. Porta, F. Lopez-de Silanes, and A. Shleifer. 2004. "The Regulation of Labor." *Q.J.E.* 119 (4): 1339–82.

Braunerhjelm, P., and J. E. Eklund. 2014. "Taxes, Tax Administrative Burdens and New Firm Formation." *Kyklos* 67 (1): 1–11.

Burgess, M., E. Giraudy, J. Katz-Samuels, J. Walsh, D. Willis, L. Haynes, and R. Ghani. 2016. "The Legislative Influence Detector: Finding Text Reuse in State Legislation." In *Proceedings of the 22nd ACM SIGKDD International Conference on Knowledge Discovery and Data Mining*, San Francisco, August 13–17, 57–66.

Butler, D. M., C. Volden, A. M. Dynes, and B. Shor. 2017. "Ideology, Learning, and Policy Diffusion: Experimental Evidence." *American J. Polit. Sci.* 61 (1): 37–49.

Card, D. 2001. "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration." *J. Labor Econ.* 19 (1): 22–64.

Chodorow-Reich, G. 2019. "Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?" *American Econ. J.: Econ. Policy* 11 (2): 1–34.

Ciccone, A., and E. Papaioannou. 2007. "Red Tape and Delayed Entry." *J. European Econ. Assoc.* 5 (2/3): 444–58.

Coffey, B., P. A. McLaughlin, and P. Peretto. 2020. "The Cumulative Cost of Regulations." *Rev. Econ. Dynamics* 38:1–21.

Colantone, I., and P. Stanig. 2018. "Global Competition and Brexit." *American Polit. Sci. Rev.* 112 (2): 201–18.

Crawford, S. E., and E. Ostrom. 1995. "A Grammar of Institutions." *American Polit. Sci. Rev.* 89 (3): 582–600.

Dam, K. 2007. *The Law-Growth Nexus: The Rule of Law and Economic Development.* Washington, DC: Brookings Inst. Press.

Davis, S. J. 2017. "Regulatory Complexity and Policy Uncertainty: Headwinds of Our Own Making." Working Paper no. 2723980, Becker Friedman Inst. Res. Econ., Univ. Chicago.

Dawson, J. W., and J. J. Seater. 2013. "Federal Regulation and Aggregate Economic Growth." *J. Econ. Growth* 18 (2): 137–77.

DellaVigna, S., and W. Kim. 2022. "Policy Diffusion and Polarization across US States." Working Paper no. 30142, NBER, Cambridge, MA.

Dell'Orletta, F., S. Marchi, S. Montemagni, B. Plank, and G. Venturi. 2012. "The SPLeT-2012 Shared Task on Dependency Parsing of Legal Texts." In *Proceedings of the 4th Workshop on Semantic Processing of Legal Texts*, Istanbul, Turkey, May 27, 42–51.

Di Vita, G. 2017. "Institutional Quality and the Growth Rates of the Italian Regions: The Costs of Regulatory Complexity." *Papers Regional Sci.* 97 (4): 1057–82.

Djankov, S., C. McLiesh, and R. M. Ramalho. 2006. "Regulation and Growth." *Econ. Letters* 92 (3): 395–401.

Foarta, D., and M. Morelli. 2022. "The Common Determinants of Legislative and Regulatory Complexity." Working Paper no. 22185, Centre Appl. Res. Internat. Markets Banking Finance and Regulation, Univ. Bocconi, Milan, Italy.

Fonseca, R., P. Lopez-Garcia, and C. A. Pissarides. 2001. "Entrepreneurship, Start-Up Costs and Employment." *European Econ. Rev.* 45 (4): 692–705.

Frantz, C. K., and S. Siddiki. 2022. *Institutional Grammar: Foundations and Applications for Institutional Analysis.* Cham, Switzerland: Springer Nature.

Fukumoto, K. 2008. "Legislative Production in Comparative Perspective: Cross-Sectional Study of 42 Countries and Time-Series Analysis of the Japan Case." *Japanese J. Polit. Sci.* 9 (1): 1–19.

Giommoni, T., L. Guiso, C. Michelacci, and M. Morelli. 2023. "The Economic Cost of Ambiguous Laws." Paper presented at the 10th Workshop on Economic Analysis of Litigation, Catania, Italy, June 22–23.

Givati, Y. 2009. "Resolving Legal Uncertainty: The Unfulfilled Promise of Advance Tax Rulings." *Virginia Tax Rev.* 29:137–75.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift. 2020. "Bartik Instruments: What, When, Why, and How." *A.E.R.* 110 (8): 2586–624.

Gørgens, T., M. Paldam, and A. Würtz. 2004. "How Does Public Regulation Affect Growth?" Working Paper no. 2003-14, Dept. Econ. and Bus. Econ., Univ. Aarhus.

Graetz, M. J. 2007. "Tax Reform Unraveling." *J. Econ. Perspectives* 21 (1): 69–90.

Gratton, G., L. Guiso, C. Michelacci, and M. Morelli. 2021. "From Weber to Kafka: Political Instability and the Overproduction of Laws." *A.E.R.* 111 (9): 2964–3003.

Greenstone, M., A. Mas, and H.-L. Nguyen. 2020. "Do Credit Market Shocks Affect the Real Economy? Quasi-experimental Evidence from the Great Recession and 'Normal' Economic Times." *American Econ. J.: Econ. Policy* 12 (1): 200–225.

Grimmer, J., and B. M. Stewart. 2013. "Text as Data: The Promise and Pitfalls of Automatic Content Analysis Methods for Political Texts." *Polit. Analysis* 21 (3): 267–97.

Grossman, G. M., and E. Helpman. 2001. *Special Interest Politics.* Cambridge, MA: MIT Press.

Grossman, S. J., and O. D. Hart. 1986. "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration." *J.P.E.* 94 (4): 691–719.

Hansen, E. R., and J. M. Jansa. 2021. "Complexity, Resources and Text Borrowing in State Legislatures." *J. Public Policy* 41 (4): 752–75.

Hansen, S., M. McMahon, and A. Prat. 2018. "Transparency and Deliberation within the FOMC: A Computational Linguistics Approach." *Q.J.E.* 133 (2): 801–70.

Hart, O., and J. Moore. 1988. "Incomplete Contracts and Renegotiation." *Econometrica* 56 (4): 755–85.

———. 1990. "Property Rights and the Nature of the Firm." *J.P.E.* 98 (6): 1119–58.

Holtzblatt, J., and J. McCubbin. 2003. "Whose Child Is It Anyway? Simplifying the Definition of a Child." *Nat. Tax J.* 56 (3): 701–18.

Jacobzone, S., F. Steiner, E. L. Ponton, and E. Job. 2010. "Assessing the Impact of Regulatory Management Systems: Preliminary Statistical and Econometric Estimates." Working Paper no. 17, Org. Econ. Cooperation and Development, Paris.

Jaeger, D. A., J. Ruist, and J. Stuhler. 2018. "Shift-Share Instruments and the Impact of Immigration." Working Paper no. 24285, NBER, Cambridge, MA.

Jalilian, H., C. Kirkpatrick, and D. Parker. 2007. "The Impact of Regulation on Economic Growth in Developing Countries: A Cross-Country Analysis." *World Development* 35 (1): 87–103.

Kawai, K., R. Lang, and H. Li. 2018. "Political Kludges." *American Econ. J.: Microeconomics* 10 (4): 131–58.

Kirchner, S. 2012. "Federal Legislative Activism in Australia: A New Approach to Testing Wagner's Law." *Public Choice* 153 (3): 375–92.

Klarner, C. 2013. "State Economic Data." Harvard Dataverse, https://doi.org/10.7910/DVN/KMWN7N.

Klein, B., R. G. Crawford, and A. A. Alchian. 1978. "Vertical Integration, Appropriable Rents, and the Competitive Contracting Process." *J. Law and Econ.* 21 (2): 297–326.

Lame, G. 2003. "Using Text Analysis Techniques to Identify Legal Ontologies' Components." Paper presented at the 2003 International Conference on Artificial Intelligence and Law (ICAIL) Workshop on Legal Ontologies and Web-Based Legal Information Management, Edinburgh, Scotland, June 24–28.

Loayza, N. V., A. M. Oviedo, and L. Serven. 2005. *Regulation and Macroeconomic Performance*. Washington, DC: World Bank.

Malhotra, N. 2006. "Government Growth and Professionalism in US State Legislatures." *Legislative Studies Q.* 31 (4): 563–84.

Montemagni, S., and G. Venturi. 2013. "Natural Language Processing and Legal Knowledge Extraction." Paper presented at Summer School LEX2013, Ravenna, Italy, September 5.

Mulligan, C. B., and A. Shleifer. 2005. "The Extent of the Market and the Supply of Regulation." *Q.J.E.* 120 (4): 1445–73.

Nakamura, E., and J. Steinsson. 2014. "Fiscal Stimulus in a Monetary Union: Evidence from US Regions." *A.E.R.* 104 (3): 753–92.

Nicoletti, G., and S. Scarpetta. 2003. "Regulation, Productivity and Growth: OECD Evidence." *Econ. Policy* 18 (36): 9–72.

Niskanen, W. A. 1971. *Bureaucracy and Representative Government*. Chicago: Aldine-Atherton.

Nunn, N. 2007. "Relationship-Specificity, Incomplete Contracts, and the Pattern of Trade." *Q.J.E.* 122 (2): 569–600.

Olea, J. L. M., and C. Pflueger. 2013. "A Robust Test for Weak Instruments." *J. Bus. and Econ. Statis.* 31 (3): 358–69.

Pacheco, J. 2012. "The Social Contagion Model: Exploring the Role of Public Opinion on the Diffusion of Antismoking Legislation across the American States." *J. Polit.* 74 (1): 187–202.

Parker, D., and C. Kirkpatrick. 2012. "Measuring Regulatory Performance." Expert Paper no. 3, Org. Econ. Cooperation and Development, Paris.

Rauch, J. E. 1999. "Networks versus Markets in International Trade." *J. Internat. Econ.* 48 (1): 7–35.

Saias, J., and P. Quaresma. 2003. "Using NLP Techniques to Create Legal Ontologies in a Logic Programming Based Web Information Retrieval System." Paper presented at the 2003 International Conference on Artificial Intelligence and Law (ICAIL) Workshop on Legal Ontologies and Web-Based Legal Information Management, Edinburgh, Scotland, June 24–28.

Shipan, C. R., and C. Volden. 2014. "When the Smoke Clears: Expertise, Learning and Policy Diffusion." *J. Public Policy* 34 (3): 357–87.

Slemrod, J. 2005. "The Etiology of Tax Complexity: Evidence from US State Income Tax Systems." *Public Finance Rev.* 33 (3): 279–99.

Soria, C., R. Bartolini, A. Lenci, S. Montemagni, and V. Pirrelli. 2007. "Automatic Extraction of Semantics in Law Documents." In *Proceedings of the V Legislative XML Workshop*, edited by C. Biagioli, E. Francesconi, and G. Sartor, 253–66. Florence, Italy: European Press Acad. Publishing.

Souza, P. C., I. Rasul, and Á. de Paula. 2019. "Identifying Network Ties from Panel Data: Theory and an Application to Tax Competition." Working paper.

Ujhelyi, G. 2014. "Civil Service Rules and Policy Choices: Evidence from US State Governments." *American Econ. J.: Econ. Policy* 6 (2): 338–80.

van Engers, T. M., R. van Gog, and K. Sayah. 2004. "A Case Study on Automated Norm Extraction." In *Legal Knowledge and Information Systems*, edited by E. Francesconi, G. Borges, and C. Sorge, 49–58. Amsterdam: IOS.

Vannoni, M., E. Ash, and M. Morelli. 2019. "Legislative Information Extraction: Methods and Application to US State Session Laws." Working paper.

Volden, C. 2006. “States as Policy Laboratories: Emulating Success in the Children’s Health Insurance Program.” *American J. Polit. Sci.* 50 (2): 294–312.
Weisbach, D. A. 2002. “An Economic Analysis of Anti Tax Avoidance Doctrines.” *American Law and Econ. Rev.* 4 (1): 88–115.
Williamson, O. E. 1979. “Transaction-Cost Economics: The Governance of Contractual Relations.” *J. Law and Econ.* 22 (2): 233–61.
———. 1985. “Assessing Contract.” *J. Law, Econ., and Org.* 1 (1): 177–208.
Yu, J., E. T. Jennings, and J. Butler. 2020. “Lobbying, Learning and Policy Reinvention: An Examination of the American States’ Drunk Driving Laws.” *J. Public Policy* 40 (2): 259–79.