



State-level economic policy uncertainty

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ABSTRACT

We quantify and study state-level economic policy uncertainty. Tapping digital archives for nearly 3500 local newspapers, we construct three monthly indexes for each state: one that captures state and local sources of policy uncertainty ($EPU - S$), one that captures national and international sources ($EPU - N$), and a composite index that captures both. $EPU - S$ rises around gubernatorial elections and own-state episodes like the California electricity crisis of 2000–01 and the Kansas tax experiment of 2012. $EPU - N$ rises around presidential elections and in response to 9–11, Gulf Wars I and II, the 2011 debt-ceiling crisis, the 2012 fiscal cliff episode, and federal government shutdowns. Close elections elevate policy uncertainty much more than the average election. VAR models fit to pre-COVID data imply that upward shocks to own-state EPU foreshadow weaker economic performance in the state, as do upward EPU shocks in contiguous states. The COVID-19 pandemic drove huge increases in policy uncertainty and unemployment, more so in states with stricter government-mandated lockdowns.

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1. Introduction

Policy uncertainty and its economic consequences are salient concerns in the United States and around the world. High uncertainty can depress economic activity by causing firms to defer investments that are costly to undo, by raising credit spreads and risk premiums (thereby dampening business investment and hiring), and by prompting consumers to postpone purchases of durable goods.¹ Several studies provide evidence that uncertainty increases around political elections and that election-related uncertainty has material effects on corporate investment, capital flows, precautionary savings, and

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¹ See, for example, Bernanke (1983) and McDonald and Siegel (1986) on the value of waiting to invest when uncertainty is unusually high; Christiano et al. (2014); Gilchrist et al. (2014) and Arellano et al. (2019) on uncertainty effects that work through credit spreads and risk premiums; and Eberly (1994) on how uncertainty affects consumer expenditures on durable goods. Fernandez-Villaverde et al. (2015) show how fiscal policy uncertainty depresses output in a New Keynesian model by intensifying monopoly pricing distortions. Bloom (2014) reviews the larger literature. Pastor and Veronesi (2013) and Arbatli et al. (2022) consider the two-way interplay between policy uncertainty and economic performance.

stock price volatility.² Baker et al. (2014) document an upward drift in U.S. policy uncertainty since the 1960s that broadly coincides with rising political polarization and growth in government spending, taxes, and regulations.

Previous research focuses on national measures of policy uncertainty and national outcomes, as in Alexopoulos and Cohen (2015); Baker et al. (2016); Davis (2016); Ozturk and Sheng (2018), and Ahir et al. (2018). Uncertainty related to sub-national policy also matters for business and household decision-making. State and local governments differ greatly in the design of their tax systems and their choice of tax rates. Their spending amounts to almost 15 percent of U.S. GDP (Nunn et al., 2019). They also determine land-use policies, business and occupational licensing rules, education standards, minimum wages, unemployment benefits, eligibility rules for social programs, environmental regulations, health and safety regulations, and more. Indeed, Justice Louis Brandeis famously characterized the states as ‘laboratories of democracy.’³

Whether experimentation *per se* is the intent, the power to set policies and change them is a source of economic uncertainty. States also differ in industry mix, energy sources, population characteristics, and the economic footprint of the federal government. As a consequence, states are differently exposed to federal tax policy, defense spending, energy prices, and other economic developments influenced by federal policy.⁴ As an example, national policy efforts to promote a shift from fossil fuels to renewable energy sources have profoundly uneven effects across the fifty states. As another example, hikes in the federal minimum wage may matter little in high-wage states while materially raising the cost of low-wage labor in other states.

In light of these remarks, we utilize the digital archives of nearly 3500 local newspapers to construct three monthly indexes of economic policy uncertainty for each state: one that captures state and local sources of policy uncertainty ($EPU - S$), another that captures national and international sources ($EPU - N$), and a composite index ($EPU - C$) that captures both state + local and national + international sources. Half the articles that feed into our composite indexes discuss state and local policy, confirming that sub-national matters are major sources of policy uncertainty.

Our EPU measures exhibit enormous increases in the wake of the COVID-19 pandemic, in line with evidence for a wide range of other uncertainty indicators in Altig et al. (2020). Looking over our full sample (which for a dozen states extends back to 1985), $EPU - S$ rises around presidential and own-state gubernatorial elections and in response to own-state episodes such as the California electricity crisis of 2000–01 and the Kansas tax experiment of 2012. $EPU - N$ rises around presidential elections and in response to the 9–11 terrorist attacks, the July 2011 debt-ceiling crisis, federal government shutdowns, and other “national” events. Close elections (winning vote margin under 4 percent) elevate policy uncertainty much more than less competitive elections. Using statistical models that include controls for common trends, seasonal effects, and state-level economic conditions, a close presidential election contest raises $EPU - N$ by 60 percent and a close gubernatorial contest raises $EPU - S$ by 35 percent.

More broadly, the relative importance of state and national sources of policy uncertainty differs greatly across states and over time. As a simple metric, consider the ratio of $EPU - S$ to $EPU - N$ for a given state. The time-averaged value of this ratio ranges from 0.35 in the District of Columbia to 1.51 in Alaska. The cross-state average value rose from 0.65 in the pre-pandemic years to 1.1 in the period from March 2020 to June 2021. Since the timing, stringency, and duration of gathering restrictions, school closure orders, business closure orders, and shelter-in-place orders during the pandemic were largely set by state and local authorities, it makes sense that $EPU - S$ saw an especially large increase after February 2020.⁵

Our state-level measures make it feasible to leverage sub-national variation when studying policy uncertainty and its effects, while also controlling for local institutions and economic conditions.⁶ We provide the first evidence on the relative importance of state and national sources of state-level policy uncertainty, how these sources differ across states, and how they vary over time within states. By exploiting the richness of our state-level data, we show that closer elections for political leaders bring greater policy uncertainty. We also show, in several ways, that state-level policy uncertainty matters for state-level economic performance. Using VAR models, we find that upward innovations in own-state $EPU - C$ foreshadow higher unemployment, lower employment, and weaker housing markets. Extending the VAR models to allow for spatial spillovers, we also provide the first evidence that upward policy uncertainty shocks in neighboring states negatively affect own-state activity.⁷ And we develop the first evidence on how government-mandated lockdowns during the pandemic impacted policy uncertainty differently across the 50 states.

Four other studies feature sub-national measures of policy uncertainty. Shoag and Veuger (2016) examine state-level indicators of policy uncertainty in the Great Recession. Their measures lack a time-series dimension. Rauh (2019) develops text-based uncertainty indicators for Canadian provinces and territories. In concurrent work, Elkamhi et al. (2021) also use

² See Canes-Wrone and Park (2012); Giavazzi and McMahon (2012); Hassan et al. (2019); Julio and Yook (2012, 2016); Kelly et al. (2016) and Baker et al. (2020), among others. Many other studies investigate the economic effects of policy uncertainty more generally. See, for example, Baker et al. (2016) and Gulen and Ion (2016).

³ *New State Ice Co. v. Liebmann*, 285 U.S. 262 (1932).

⁴ For evidence, see Albuoy (2009); Davis et al. (1997), and Nakamura and Steinsson (2014). Mumtaz et al. (2018) provide evidence that national uncertainty shocks have heterogeneous effects across the states.

⁵ See Arnon et al. (2020); Coibion et al. (2020) and Goolsbee et al. (2020) on the prevalence of restrictive orders issued by state and local governments during the pandemic.

⁶ Our data are freely available at www.policyuncertainty.com/state_epu.html. We update our state-level EPU measures on a regular basis, following our customary practice for the national EPU measures featured on the same site.

⁷ This aspect of our results complements work on the spillover of national (policy) uncertainty shocks across countries. See, for example, Bhattarai et al. (2020); Colombo (2013); Klossner and Sekkel (2014), and Arbatli et al. (2022).

newspapers to construct state-level EPU measures. We differ from their work in building indexes that *separately* quantify national and state sources of state-level policy uncertainty. Like [Elkamhi et al. \(2021\)](#), we find a great deal of state-specific time variation in policy uncertainty. And like them, we find that upward shocks in state-level EPU foreshadow weaker economic performance in the state. Their measures end in 2019, before the COVID-19 pandemic and the huge increases in policy uncertainty that came with it. Finally, [Ash et al. \(2021\)](#) consider state-level EPU – as measured by the frequency of “economic policy uncertainty” in local newspapers – to test one implication of their incomplete-contracts theory of legislative detail.

The next section explains how we construct our state-level indexes and summarizes their behavior. [Section 3](#) explores several drivers of state-level EPU, with particular attention to election-related uncertainty and a variety of state, national, and international episodes that involve high levels of policy uncertainty. In [Section 4](#), we use pre-COVID data to estimate panel VAR models that relate state-level economic activity to state-level EPU. We apply the models to characterize the dynamic response of state-level activity to own-state and border-state EPU shocks, with special attention to California in view of its size, policy-induced electricity crisis in 2000–01, and gubernatorial recall in 2003. [Section 5](#) focuses on developments during the pandemic. We find that states with stricter lockdowns had bigger rises in policy uncertainty relative to 2019, bigger rises in unemployment, and bigger falls in employment – all conditional on pandemic severity, as measured by COVID deaths per capita. [Section 6](#) offers concluding remarks.

2. State-level economic policy uncertainty indexes

To measure state-level economic policy uncertainty (EPU), we turn to local newspapers in each state and track the fraction of articles that discuss policy-related economic uncertainty. We follow the approach of [Baker et al. \(2016\)](#) in constructing our measures but develop an extensive collection of new term sets to disentangle state and local from national (and international) sources of policy uncertainty.

2.1. Why newspapers?

Newspapers have many attractive attributes for our purposes. They publish frequently, facilitating the creation of monthly, weekly and even daily measures. Their timely character allows for the production of forward-looking uncertainty indicators in real time, which is especially valuable amidst novel developments like the COVID-19 pandemic ([Altig et al., 2020](#)). Digital newspaper archives often extend back for decades, letting us create panel data with a long time-series dimension. Newspaper coverage of a particular topic expands and contracts as concerns and news flow related to the topic wax and wane. Moreover, the richness of newspaper text lets us drill into the forces that drive uncertainty in response to particular state, national and international developments. By using multiple newspapers in a given state and month, we average out much of the idiosyncratic noise present in the coverage of a single newspaper, and we reduce biases that might arise from slanted coverage in particular papers. Finally, newspapers offer one of the few sources for creating sub-national uncertainty measures on a frequent and timely basis. In contrast, uncertainty proxies based on option prices or financial market volatility are difficult to create for sub-national units. And survey data typically lack the combination of spatial granularity, frequency, and topical density that is easily achieved with newspapers.

2.2. Tracking newspapers

To construct our EPU measures, we draw on the digital archives provided by the Access World News Newsbank service. We include daily and weekly newspapers, ranging from small, local papers to flagship newspapers that circulate throughout the state. We exclude papers with a strong national reach like the New York Times and the Wall Street Journal. Our sample runs from January 1985 onward, is shorter for many states, and covers all states since 2006. All told, we use about 3500 newspapers, and the median number of papers per state-month observation is 49.

The average coverage duration for the newspapers in our sample is about 14 years. Once included, coverage usually extends through the present day, although about 250 papers disappear from the archives by 2010. These sample exits typically occur because the paper ceased operations or was absorbed by a competitor (who is often in our sample). Papers that disappear from the sample tend to have smaller circulation and lower article counts. There are no instances in which the state's largest newspaper exits the sample. Appendix Table A.2 provides additional statistics on newspaper counts, circulation, and sample start dates by state.

2.3. Using term sets to flag relevant newspaper articles

We flag newspaper articles that contain at least one of ‘economic’ or ‘economy’ (E); and at least one of ‘uncertain’, ‘uncertainties’, or ‘uncertainty’ (U); and at least one term in a policy set that differs between $EPU - N$ and $EPU - S$ to reflect their different objectives. In devising the policy sets for $EPU - N$ and $EPU - S$, we avoid terms like ‘taxes’ and ‘tax policy’ that refer to shared responsibilities among federal, state, and local governments.⁸

⁸ We investigated whether the inclusion of tax-related terms improve our $EPU - N$ and $EPU - S$ indexes. Not surprisingly, tax-related terms are unhelpful in distinguishing between articles about national sources of policy uncertainty and articles about state and local sources. Moreover, conditional on our

The policy term set for $EPU - N$ mainly contains terms for national policy-making institutions and regulatory agencies but also includes ‘monetary policy’ and terms that refer to the election of federal officials. See Appendix Table A.1. For $EPU - S$, we tailor the policy sets to cover relevant state and local officials, policy-making bodies, and regulatory agencies. Thus, our state-specific policy sets include terms like ‘governor’, ‘mayor’, ‘state senate’, ‘city council’, and the like. To assemble these terms, we consulted government websites for the titles of officials and names of legislatures and state bodies with authority over regulations pertaining to the environment, labor and unemployment, gambling, transportation, energy and utilities, banking, and other financial services. We include ‘zoning’ in our $EPU - S$ policy sets, because zoning functions as an important policy lever that is exercised mainly by state and local governments. We also include ‘referendum’ for states that have provisions for putting direct votes on policy matters before their citizens.⁹ For comparison, Table A.1 also reports the “BBD” policy term set developed by Baker et al. (2016).

2.4. EPU index construction

Having flagged a suitable set of articles, we compute the raw state-level indexes by calculating

$$RawEPU-N_{s,t} = \frac{(\text{\#of Articles with E, U and National Policy Terms})_{s,t}}{(\text{Total\#of Articles in Same Newspapers})_{s,t}}, \quad (1)$$

$$RawEPU-S_{s,t} = \frac{(\text{\#of Articles with E, U and Own-State Policy Terms})_{s,t}}{(\text{Total\#of Articles in Same Newspapers})_{s,t}}, \text{ and} \quad (2)$$

$$RawEPU-C_{s,t} = \frac{(\text{\#of Articles with E, U and (Own-State or National Policy Terms)})_{s,t}}{(\text{Total\#of Articles in Same Newspapers})_{s,t}}, \quad (3)$$

where s and t index the state and monthly time period, respectively. To obtain our final indexes, we divide each raw index value by the average of $Raw EPU - N$ from 2006 to 2019 for the state in question and multiply by 100. This normalization preserves information about the relative magnitudes of $EPU - N$, $EPU - S$ and $EPU - C$ within each state.¹⁰ We take the same approach in calculating our $EPU - BBD$ measures for each state.

2.5. A First look at the sources of state-level policy uncertainty

Fig. 1 displays a quarterly series for the equal-weighted average value of $EPU - C$ and its three-way decomposition by source of policy uncertainty. Our decomposition reflects the content of articles the feed into the $EPU - C$ indexes – specifically, whether they discuss only national (and international) policy matters, only state and local policy matters, or both. From 1985 to February 2020, articles that discuss only national policy matters account for 50% of the average $EPU - C$ value. Articles that discuss only state and local policy matters account for 30%, and those that discuss both account for 20%. From March 2020 to June 2021, the shares are 38% for national only, 40% for state and local only, and 22% for articles that discuss both. These results establish the importance of state and local policy matters as sources of state-level policy uncertainty.

The sources of EPU also vary greatly across states. At the extremes, articles that mention only state and local policy matters account for 45% of $EPU - C$ in Alaska from 1985 to 2021 but only 18% in South Dakota and 10% in Washington, DC. Articles that mention only national and international policy matters account for 75% of $EPU - C$ in Washington, DC and 62% in Pennsylvania but only 26% in Wyoming and Alaska. We offer many examples of shocks and developments that drive common and state-specific movements in our EPU measures in the analysis below.

2.6. State-level EPU behavior over time and across states

Fig. 2 displays monthly values for the cross-state averages of $EPU - N$ and $EPU - S$. Gulf Wars I and II, close presidential elections, financial crises, major political conflicts over fiscal policy, the June 2016 Brexit referendum, trade policy tensions during the Trump presidency, and Trump’s impeachment in December 2019 all leave visible marks on the indexes, especially $EPU - N$. The pandemic pushed (average) $EPU - N$ to 2.7 times its pre-COVID peak and pushed $EPU - S$ to more than four times its previous peak. The times-series correlation between the two measures displayed in Fig. 2 is 0.74 from 1985 to 2019 and 0.88 when using data through June 2021.

other policy terms, the inclusion of tax-related terms flags few articles about policy uncertainty that we otherwise miss. On the margin, tax-related terms yield a low ratio of true positives to false positives in flagging articles. Baker et al. (2016) reached the same conclusion, based on a large-scale human audit study, regarding the potential use of tax-related terms in their national EPU index.

⁹ Table A.1 reports our policy set for Michigan. The full collection of state-specific policy term sets is available on the Economic Policy Uncertainty website at https://policyuncertainty.com/state_epu_terms.html.

¹⁰ Our normalization method equalizes the level of $EPU - N_{s,t}$ across states over the normalization period. We make this choice deliberately based on our sense that cross-state variation in average levels is heavily influenced by differences in newspaper practices across states, e.g., the share of newspaper articles devoted to sports or weather.

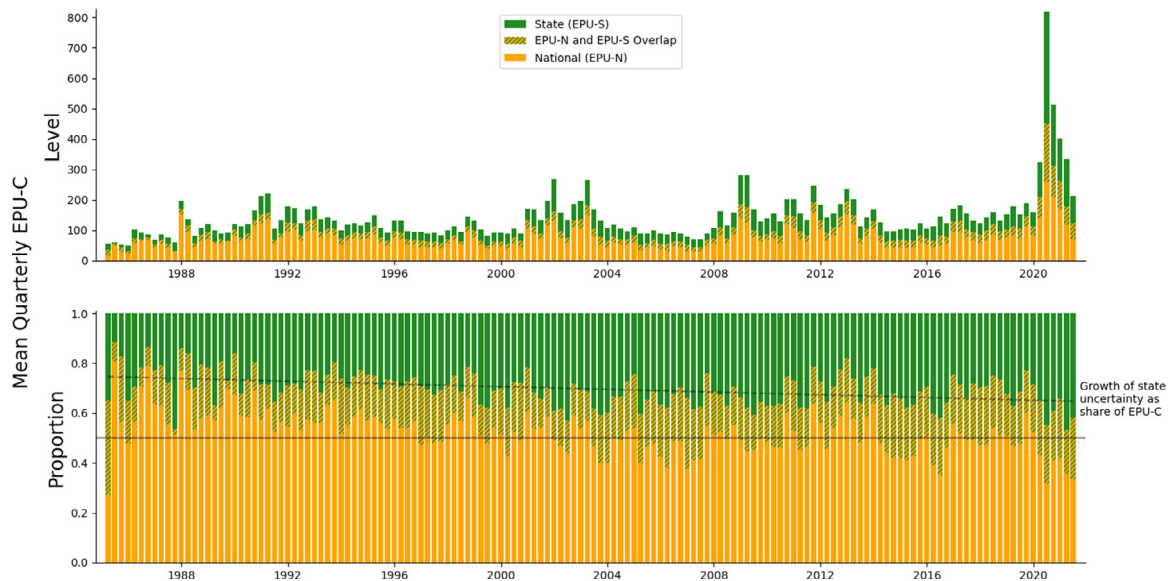


Fig. 1. The level and composition of state-level EPU index values, cross-state averages, quarterly data. *Notes:* The height of each bar is the equal-weighted mean over states of the state-level $EPU - C$ values (composite index) in the indicated quarter. The breakdown within each bar reflects the contribution of articles that contain only national policy terms, those that contain only state and local policy terms, and those that contain both.

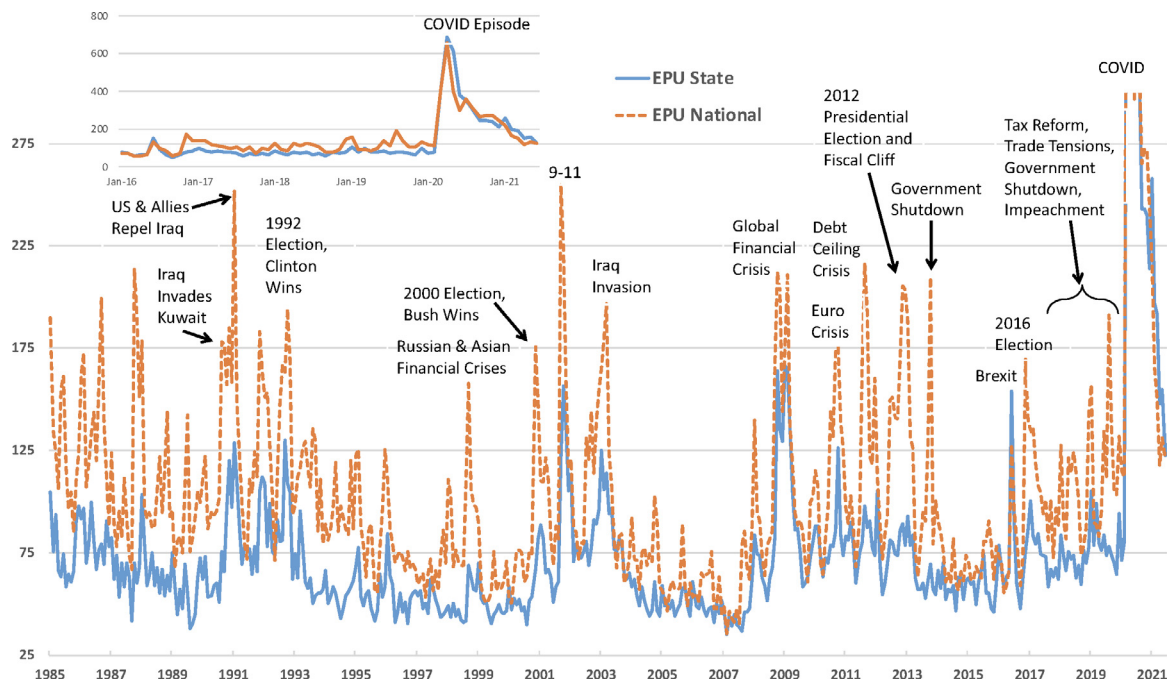


Fig. 2. Average values of $EPU - S$ (solid blue) and $EPU - N$ (dashed orange) by month. *Notes:* The figure plots equal-weighted cross-state average values of $EPU - S$ and $EPU - N$ by month. We cover all states from January 2006 onward, 38 states from 1996 onward, and 12 states throughout the period from January 1985 to June 2021. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Even before the pandemic struck, policy uncertainty levels had drifted upward over time: The average $EPU - N$ value is 114 from 2008 to 2019, as compared to 96 from 1985 to 2007. This pattern broadly aligns with the newspaper-based evidence in Baker et al. (2016). They also find an upward drift in policy uncertainty measures derived from the periodic Beige Books compiled by economists in the Federal Reserve System and in descriptions of “Risk Factors” in the annual 10-K filings of listed firms. Hassan et al. (2019) find an upward drift in political risks facing firms based on their analysis of quarterly earnings conference calls. Aside from the COVID episode, the cross-state average $EPU - N$ measure fluctuates with

greater amplitude than average $EPU - S$, because many state-level sources of EPU are idiosyncratic and average out in the cross section.

Appendix Fig. A.1 plots the average between-state EPU correlations in rolling samples of the last 60 observations. The 9–11 attacks, the financial crisis in September 2008, the onset of the COVID pandemic, and (to a lesser extent) the November 2016 election victory of Donald Trump brought abrupt increases in the average pairwise correlations. This illustrates how major political, geopolitical, and economic developments can drive simultaneous increases in state-level EPU.

Nevertheless, there is much state-specific time variation in our EPU measures. The average pairwise time-series correlation between states is only 0.23 for $EPU - S$ and 0.39 for $EPU - N$ for the period ending in 2019. The first principal component accounts for only 26% of the monthly time-series variation in the state-level $EPU - S$ measures, and the next four components account for another 27%. Not surprisingly, the $EPU - N$ measures exhibit more commonality: The first principal component accounts for 47% of the state-level variation, and the next four components account for another 16%.¹¹ There is also much index-specific variation. Looking within states over time in the pre-pandemic period, the correlation between $EPU - N$ and $EPU - S$ ranges from 0.17 to 0.76, with a mean of 0.45.

To investigate whether and how co-movements in the state-level EPU measures depend on spatial proximity, we estimate dyadic regression models of the following form:

$$C_{ij} = \alpha_i + \alpha_j + \beta_1 B(i, j) + \beta_2 \text{Dist}(i, j) + \epsilon_{ij}, \quad (4)$$

where C_{ij} denotes the pairwise correlation between EPU measures for states i and j over the longest available time span, α_i and α_j are fixed effects for states i and j , $B(i, j)$ is an indicator function that equals 1 when states i and j share a border, and $\text{Dist}(i, j)$ is the log distance between i and j (hundreds of miles). There are $51(50)/2 = 1275$ pairwise correlations for each EPU measure. When we fit (4) by least squares to the pairwise $EPU - N$ correlations, we obtain $\hat{\beta}_1 = 0.001$ [0.005] and $\hat{\beta}_2 = 0.006$ [0.011].¹² Similarly, fitting (4) to the pairwise $EPU - S$ correlations also yields small and statistically insignificant values for $\hat{\beta}_1$ and $\hat{\beta}_2$. Thus, we find no evidence that state-level EPU movements are more similar for states in closer proximity.

There is, however, a geographic structure to state-level EPU co-movements. Specifically, Fig. A.2 shows how each state-level EPU measure correlates over time with the corresponding national average EPU value. EPU measures for the Mountain states, Alaska, the Dakotas, Nebraska, Kansas, Oklahoma, several Southern states (but not Texas and Florida), Maine, Vermont and New Hampshire correlate more weakly with national average values than other states. Roughly speaking, “hinterland” states exhibit more idiosyncratic EPU variation than other states.

In summary, there is much commonality in measured EPU fluctuations across states and over time within states, but there is also a great deal of state-specific and index-specific variation. This idiosyncratic variation is quite useful in downstream econometric applications, a fact we exploit in the rest of the paper. Perhaps surprisingly, we find no evidence that states in closer physical proximity exhibit stronger EPU co-movements.

3. Drivers of state-level economic policy uncertainty

3.1. Election-related uncertainty

Elections are an obvious source of policy uncertainty. Indeed, Baker et al. (2020) find that national EPU indices exhibit a clear tendency to rise in the months leading up to national elections in a sample of 23 countries around the world. We now investigate whether and how our state-level EPU measures respond to U.S. presidential and own-state gubernatorial elections.

In Table 1, we regress our monthly state-level EPU indexes on election indicators that equal one in an election month and the prior month, zero otherwise. Column (1) reports a least-squares regression of $\log EPU - N_{s,t}$ on these indicators.¹³ We control for state fixed effects and time-varying economic conditions, as measured by the state’s contemporaneous unemployment rate and a summary measure of its coincident economic indicators. On average and conditional on the controls, $EPU - N_{s,t}$ is 33 log points higher around presidential elections, with a t -statistic of 13.

Gubernatorial elections also raise $EPU - N_{s,t}$, but the effect is much smaller and only marginally significant. When we add common year effects (controlling for any drift over time in the underlying level of EPU) and common month effects (controlling for unobserved seasonal forces that affect EPU in Novembers, for example), the coefficient on the presidential election indicator is nearly unchanged, but the gubernatorial election indicator becomes smaller and statistically insignificant. We find strong statistical evidence that presidential and gubernatorial elections have sizable positive effects on $EPU - S_{s,t}$ in columns (4) and (5). Notably, we find substantially higher effects of gubernatorial elections on $EPU - S_{s,t}$ relative to $EPU - N_{s,t}$.

Close elections (winning margin less than 4 percent) yield especially large increases in our state-level policy uncertainty measures, as shown in columns (3) and (6). The total estimated effect of a close presidential election on $EPU - N_{s,t}$ is

¹¹ We use a balanced panel of 44 states from January 2001 to July 2021 in the principal components analysis.

¹² We adjust standard errors for the dyadic structure following Fafchamps and Gubert (2006).

¹³ In practice, we use the inverse hyperbolic sine of the state-level EPU measures. The inverse hyperbolic sine transformation handles zero values while closely approximating the natural log transformation. See Bellemare and Wichman (2020).

Table 1
Election effects on state-level policy uncertainty measures.

Variables	(1) ln(EPU-N)	(2) ln(EPU-N)	(3) ln(EPU-N)	(4) ln(EPU-S)	(5) ln(EPU-S)	(6) ln(EPU-S)
Presidential Election	0.32*** (0.025)	0.33*** (0.029)	0.15*** (0.037)	0.15*** (0.024)	0.15*** (0.028)	0.18*** (0.041)
Gubernatorial Election	0.049 (0.033)	0.033 (0.030)	0.022 (0.034)	0.061** (0.030)	0.15*** (0.030)	0.11*** (0.036)
Close Presidential Election			0.31*** (0.049)			-0.053 (0.049)
Close Gubernatorial Election			0.032 (0.056)			0.11** (0.052)
Observations	17,000	17,000	17,000	17,000	17,000	17,000
R ²	0.133	0.329	0.331	0.225	0.375	0.375
Economic Controls	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES
Year and Month FE	NO	YES	YES	NO	YES	YES

Notes: Presidential (gubernatorial) election variables equal one in the month of and the month before a presidential (own-state gubernatorial) election, zero otherwise. Economic controls are the monthly state-level unemployment rate and the monthly state-level Coincident Economic Indicator from the St. Louis Federal Reserve (not available for the District of Columbia). Observations are weighted by state population. The sample runs from January 1985 to June 2021 (unbalanced across states). In practice, we use the inverse hyperbolic sine transformation for the dependent variable, which closely approximates the natural log transformation. Heteroskedasticity-robust standard errors in parentheses.

47 log points, or 60 percent. The total effect of a close, own-state gubernatorial election on $EPU - S_{s,t}$ is 30 log points, or 35 percent.

The results in Table 1 tell us that elections have powerful effects on state-level policy uncertainty, and that they alter the mix between $EPU - N$ and $EPU - S$ over time. We isolate the mix effect in Fig. 3, which plots the average time path of $EPU - S$ relative to $EPU - N$ in the months around presidential and gubernatorial elections, conditional on state-level economic conditions and other controls. Gubernatorial elections raise the ratio of $EPU - S$ relative to $EPU - N$, with a peak estimated effect of about 18 log points in the election month. Presidential elections pull down the ratio of $EPU - S$ relative to $EPU - N$, with a peak estimated effect of about -31 log points.

3.2. Selected national and international events

We now investigate how our state-level EPU measures respond to several national and international events that created policy uncertainty directly or that raised profound questions about policy responses: the terrorist attacks on the World Trade Center in New York City and the Pentagon building on 11 September 2001; the U.S. debt-ceiling crisis in July 2011; the June 2016 Brexit referendum in the United Kingdom, which reverberated through financial markets around the world; Donald Trump's election victory in November 2016, the biggest U.S. presidential election surprise since Harry Truman defeated Thomas Dewey in the 1948 contest; and partial federal government shutdowns that commenced in October 2013 and December 2018.

Fig. A.3 displays histograms of one-month log changes in state-level policy uncertainty measures, $\ln(EPU - N_{s,t}) / EPU - N_{s,t-1}$, where s indexes states as before, and t is the event month. All six events drove increases in $EPU - N$ in most states, and the increases are often quite large. For example, the November 2016 presidential election outcome triggered $EPU - N$ increases in 86 percent of states, with a median jump of 80 log points. Four states (Georgia, Maryland, Maine, and Rhode Island) had $EPU - N$ jumps of 200 log points or more in reaction to Trump's election win. The debt-ceiling crisis and the two government shutdowns drove $EPU - N$ increases in 80 percent or more of states, and the 9–11 attacks raised $EPU - N$ in all but three states.¹⁴ The median $EPU - N$ spike ranges from 45 to 120 log points across these four episodes. Lastly, in reaction to the surprise Brexit referendum outcome, $EPU - N$ rose in 84 percent of states, with a median jump of 64 log points. As the Brexit case shows, major policy developments in other countries can drive large, heterogeneous changes in state-level policy uncertainty. Fig. 2 identifies other foreign developments that drove large increases in $EPU - N$.

3.3. The California electricity crisis of 2000–01

California experienced a spectacular electricity crisis from May 2000 to June 2001 after a multi-year effort to reform its wholesale and retail markets.¹⁵ Wholesale prices rose by a factor of six from the second half of 1999 to the second half of 2000. Average spot prices for wholesale power in the first few months of 2001 were ten times their levels in 1998 and 1999. Regulators froze retail electricity prices in the summer of 2000, before letting them rise in early 2001. Even then, they

¹⁴ Alaska, Maine, and Nevada, where it rose with a 1–2 month delay.

¹⁵ Our summary of what was a multi-faceted and highly contentious regulatory, political, and economic crisis draws on Joskow (2001); Wolak (2003), and Bushnell (2004). See those sources for a fuller discussion of the crisis.

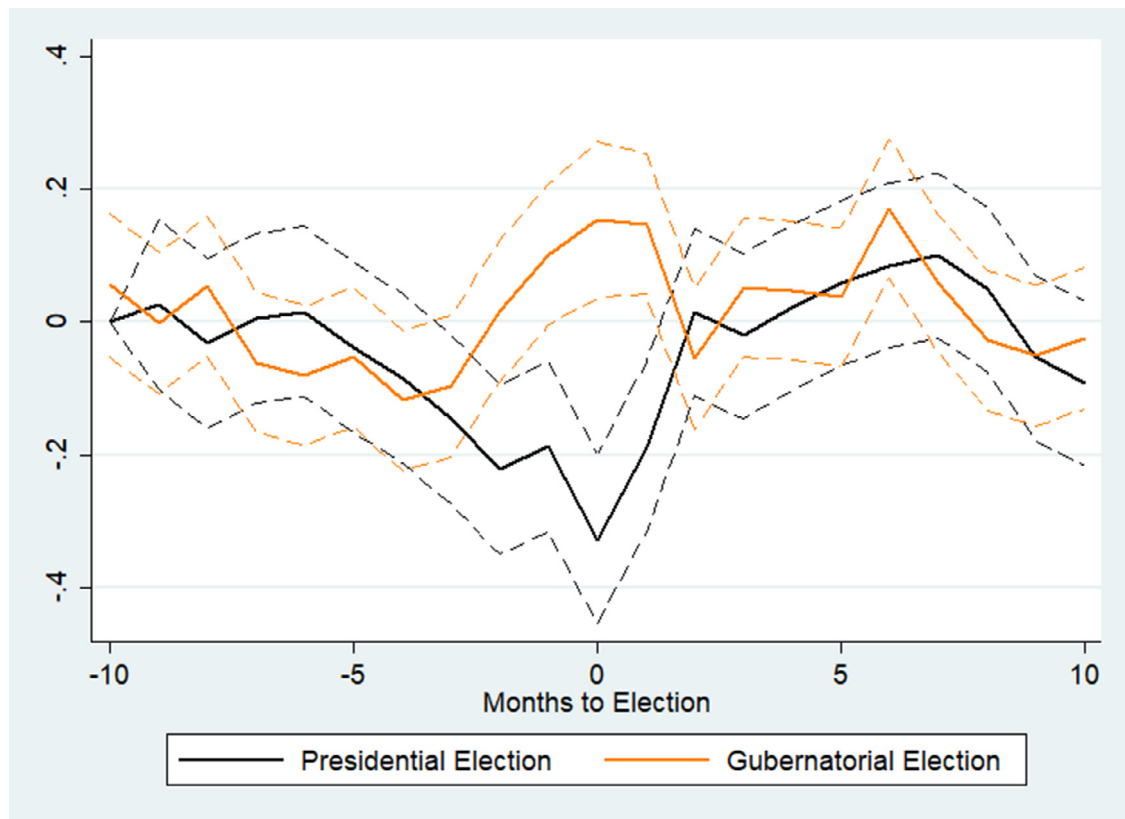


Fig. 3. Ratio of $EPU - S$ to $EPU - N$ around elections. *Notes:* This chart plots the coefficients from two regressions of state-month values of $(EPU - S / EPU - N)$ on a collection of indicator variables for $-10, -9, \dots, 9, 10$ months to or from an election of the indicated type. Both regressions include controls for the contemporaneous values of state-level unemployment rates and state-level Coincident Economic Indicators produced by the Federal Reserve Bank of St. Louis. The regression for “Presidential Elections” also includes controls for common year effects. The regression for “Gubernatorial Elections” include controls for both common year effects and common month effects. Observations are weighted by state population. Sample runs from January 1985 to June 2021 (unbalanced across states). Dashed lines show 95% confidence intervals with heteroskedasticity-robust standard errors.

let retail prices rise much less than wholesale power costs. By January 2001, California’s two largest utilities were insolvent and had ceased paying their bills for wholesale power. Governor Gray Davis declared a state of emergency on January 17th, and the state began purchasing power directly at very high costs to head off widespread blackouts. Many Californians had already experienced rolling blackouts and mandatory cutbacks in electricity consumption.

The electricity crisis was a principal factor in the ultimately successfully effort to recall Governor Davis. That effort began in early 2003 and gained momentum in May 2003, when U.S. Representative Darrell Issa announced he would contribute substantial sums to help gather signatures to force a recall election. By July 2003 the recall campaign had gathered enough signatures to put the question on the ballot. The recall election on October 7, removed Governor Davis and elected Arnold Schwarzenegger as his successor. This episode remains one of only two successful gubernatorial recall efforts in U.S. history, the other taking place in North Dakota in 1921.

California’s electricity crisis and gubernatorial recall are clearly visible in the top-left panel of Fig. 4, which plots the quarterly paths of $EPU - S$ and $EPU - N$ for the state from 1996 to 2006. Its $EPU - S$ levels during the electricity crisis and around the recall campaign and election are more than twice as high as in the preceding and following years. Its $EPU - S$ index rose much more during this period than the average of $EPU - S$ index values, as seen by comparing to Fig. 2. $EPU - N$ also rose to high levels from 2000 to 2004, largely because of national and international developments that affected all states. California-centric aspects of federal policy actions also contributed to the state’s high $EPU - N$ values in this period. Specifically, the Federal Energy Regulatory Commission (FERC) played a significant role in California’s electricity crisis.¹⁶

¹⁶ FERC initiated a formal investigation into California’s wholesale electricity markets in August 2000, identified “structural reforms that must be addressed” and placed a ceiling on electricity price bids in California’s wholesale markets in November, and waived rules for generating facilities to increase power supplies in December. Later in December, FERC took additional steps to regulate pricing and other aspects of California’s wholesale power markets and to create independent “Governing Boards” to monitor those markets. In March 2001, FERC issued its “first refund order directing sellers to provide refunds of excess amounts charged for certain electric energy sales during the month of January 2001.” That same month, FERC staff issued a proposal

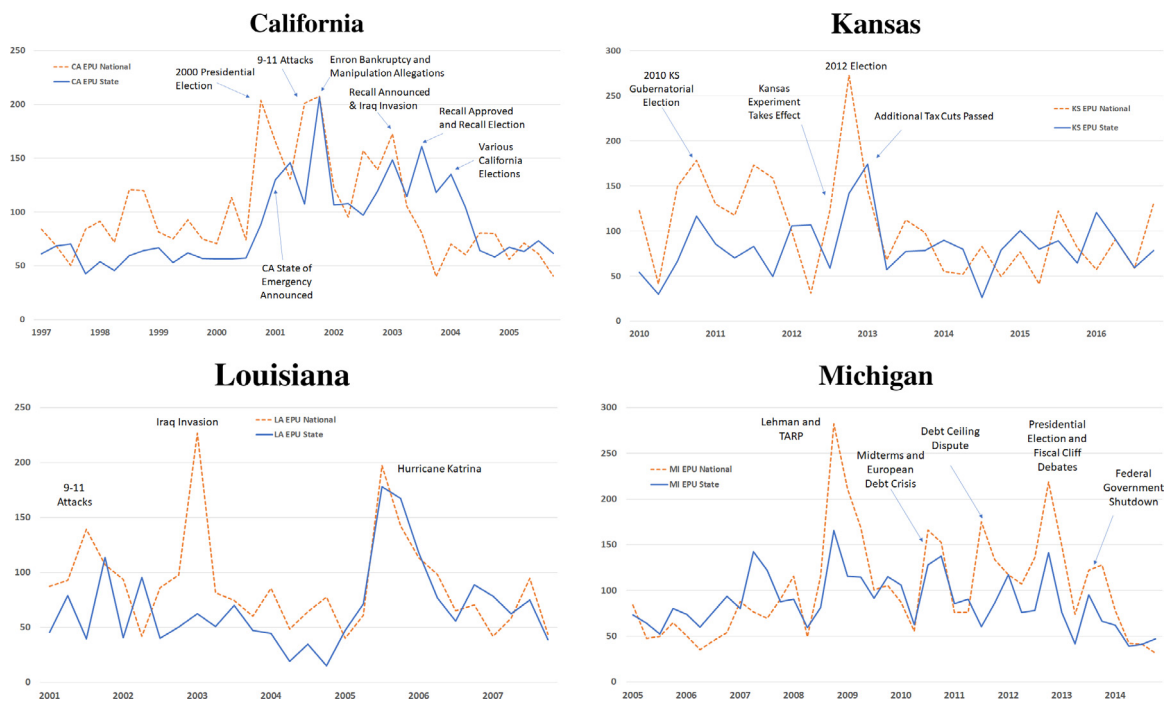


Fig. 4. $EPU - S$ and $EPU - N$ in California, Kansas, Louisiana, and Michigan. Notes: This figure plots quarterly averages of monthly $EPU - S$ and $EPU - N$ values for California, Kansas, Louisiana, and Michigan in selected time periods.

3.4. Other illustrative episodes with high state-level EPU

Fig. 4 also highlights selected episodes with high levels of EPU in three other states. The chart for Kansas shows that $EPU - S$ more than doubled in late 2010, when Sam Brownback prevailed in the state's gubernatorial election. He ran on a platform that stressed tax reform and, once elected, followed through. The 'Kansas Experiment' to reduce income taxes, enacted in 2012, and the passage of additional tax cuts in early 2013 are both clearly visible in the state's $EPU - S$ index.

The chart for Louisiana illustrates a different example. Hurricane Katrina struck the state in late August 2005, causing widespread destruction of private property and public infrastructure and a near-total exodus from New Orleans. The devastation wrought by the hurricane also brought great uncertainty about how local, state, and federal policymakers and officials would respond. Accordingly, Louisiana's $EPU - S$ and $EPU - N$ series rose dramatically in the wake of Katrina and, in the case of $EPU - S$, remained elevated through 2007. As revealed by a glance at Fig. 2, nothing similar happened to the cross-state average values of $EPU - S$ and $EPU - N$ in this period. Indeed, the average EPU series fluctuated around unusually low levels in and around 2005.

Finally, the chart for Michigan considers the period before, during, and after the global financial crisis and Great Recession. National developments figure prominently in the behavior of $EPU - S$ and $EPU - N$ for Michigan, partly because the cyclically sensitive auto industry is a major part of Michigan's economy. That makes Michigan unusually exposed to policy uncertainty that causes or responds to national economic fluctuations. For example, the federal government played a major role in rescuing U.S. automobile manufacturers (and their employees) during the Great Recession. The economic uncertainty associated with that rescue effort was accentuated for Michigan by virtue of the state's industrial structure.

In summary, the examples highlighted by Fig. 4 illustrate that high state-level EPU can arise in several ways – as a consequence of shocks interacting with bad policy design (California's electricity crisis) and the resulting political turmoil (gubernatorial recall), policy reforms designed to lower taxes and promote growth (the Kansas tax experiment), devastating natural disasters that raise policy issues about whether and how a state will rebuild (Katrina), and state economies that are unusually exposed to national developments and sources of policy uncertainty by virtue of their industrial structure (Michigan in the Great Recession).

for "monitoring and mitigating prices prospectively" in California's wholesale power markets. See the chronology of FERC actions from 2000 to 2002 at <https://www.ferc.gov/sites/default/files/2020-05/pr-07-26-00.pdf> and the links therein.

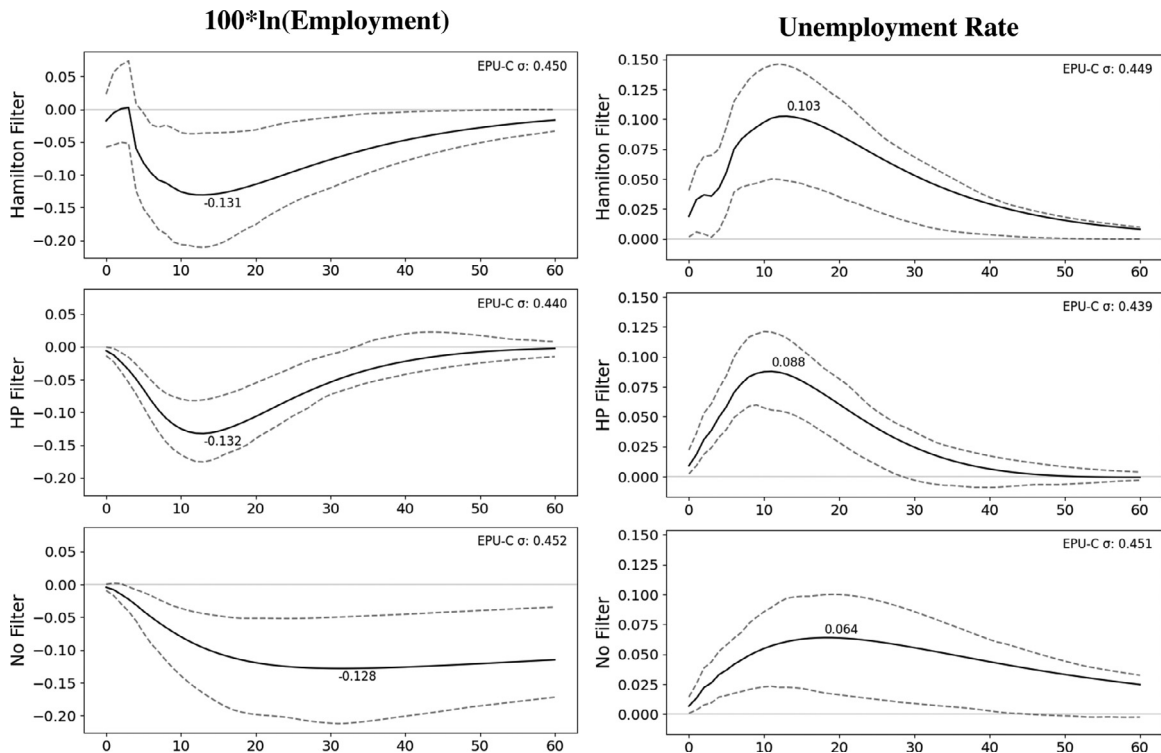


Fig. 5. Employment and unemployment rate responses to unit standard deviation $\ln(EPU - C)$ shocks. Notes: Each panel shows estimated dynamic responses of the activity measure to a unit standard $EPU - C$ shock (with 95% confidence intervals), the peak response, and standard deviation of the identified shocks. To obtain these results, we filter the data as indicated, fit a two-equation panel VAR model by least squares to monthly state-level data for 44 states, and place $EPU - C$ first in a Cholesky ordering. The VAR system has six lags of each variable and state-specific intercepts. The estimation sample runs from July 2001 to December 2019 when using raw and HP-filtered data, and from June 2004 to December 2019 when using Hamilton-filtered data.

3.5. The main locus of policy uncertainty shifted after the pandemic

Recall from Figs. 1 and 2 that our state-level policy uncertainty measures rose enormously in the wake of the pandemic. What's easy to overlook in these figures is the simultaneous shift in the predominant source of policy uncertainty. Fig. A.4 highlights this shift, showing that the ratio of $EPU - S$ to $EPU - N$ rose in all but a few states after the pandemic. The ratio rose by a factor of two or more in Georgia, Illinois, Kentucky, Nevada, New Jersey, New Mexico, Oklahoma, Tennessee, Texas and Utah, while falling slightly in Delaware, Maine, Mississippi, New Hampshire, Rhode Island, and South Dakota. The cross-state average value of this ratio rose from 0.65 in the pre-pandemic part of our sample to 1.1 in the period from March 2020 to June 2021.

It is perhaps no surprise that the locus of policy uncertainty shifted to state and local policy matters after the pandemic struck. State and local authorities largely determined the parameters of government-mandated restrictions on economic and social activity during the pandemic. The scale of the shift is remarkable nonetheless. It constitutes a major break in the sources of policy-related uncertainty within the federal system of American government. Whether, and how fully, this shift will persist remains to be seen.

4. State-level EPU and economic performance

This section investigates how economic activity responds to state-level policy uncertainty shocks in vector autoregressive (VAR) models. The results yield clear evidence that upward own-state policy uncertainty shocks foreshadow weaker activity in the state, and in contiguous states. As we illustrate for California, states sometimes experience a sequence of EPU shocks that drive – or at least foreshadow – material movements in the state-level unemployment rate.

4.1. Panel VAR analysis

We fit a panel VAR to monthly, state-level data on $\ln(EPU - C)$ and economic activity. We use the composite EPU measure here, because we aim to capture all relevant sources of policy uncertainty for the state – whether due to local, state, national or international developments. For now, our activity measure is either the state's unemployment rate or the natural

log of its employment multiplied by 100. Unless noted otherwise, all specifications include state-specific intercept terms and six lags of state-level activity and $\ln(EPU - C)$ in each equation. To identify shocks, we use a Cholesky decomposition with $\ln(EPU - C)$ placed first in the recursive causal ordering. We also consider results when placing $\ln(EPU - C)$ last in the causal ordering. Appendix A.3 sets forth an explicit statement of our structural VAR and its identification.

We estimate the VAR by least squares, using data for 44 states from January 2001 to December 2019. Starting in January 2001 maximizes the number of state-month observations in a balanced panel design.¹⁷ We stop in December 2019 in view of the economy's highly atypical dynamic response to the COVID shock and attendant uncertainty: the lag between the COVID shock and the trough was extraordinarily short, the recession was extremely short-lived, and the early pace of recovery was unusually rapid.¹⁸ Thus, it makes little sense to include the COVID episode when seeking to characterize the normal dynamic relationship of EPU to economic activity.

Fig. 5 displays estimated dynamic responses to unit standard deviation $\ln(EPU - C)$ shocks, along with 95% confidence intervals. Our preferred specification uses the filter proposed by Hamilton (2018) with his recommended look-back horizon ($h = 24$) and one year of lags ($p = 12$). We also report results using the Hodrick-Prescott (HP) filter with $\lambda = 129,600$, and using unfiltered data.¹⁹ Since we lose six observations for lags, our estimation sample runs from July 2001 to December 2019 for VARs fit to raw data or to HP-filtered data. The Hamilton filter uses an extra 35 observations, yielding an estimation sample that runs from June 2004 to December 2019.

As the figure shows, own-state policy uncertainty shocks foreshadow lower employment and higher unemployment in the state, with hump-shaped dynamic responses that peak after about one year. The EPU shock responses are modest in size but estimated with good precision, owing to the richness of our state-level data. Using the Hamilton filter, the unemployment rate response to a unit standard deviation upward innovation in $\ln(EPU - C)$ peaks 11 to 14 months later at 0.103 percentage points, with a 95% confidence interval of 0.05 to 0.15 points. The peak employment response is -0.131 log points after about one year. HP-filtered data yield similar results. Unfiltered data also yield similar results, except the impulse responses are more persistent.

The dynamic response to a $\ln(EPU - C)$ shock in Fig. 5 is qualitatively similar to what Baker et al. (2016) find in a 12-country panel VAR but smaller in magnitude. They estimate a peak unemployment rate response of 0.25 percentage points one year after a 90-point shock to the level of EPU, which amounts to 65 log points on an EPU base of 100.²⁰ Scaling up to account for the larger EPU shock in their study, our panel VAR yields a peak unemployment response of $(0.65/0.45)(0.103) = 0.15$ percentage points. Thus, our peak unemployment rate response to a same-size EPU shock is only 60 percent as large as theirs. One possible reason is that state-level data are noisier than national data, leading to more attenuation of estimated effects in our setting. Especially for less populous states, monthly activity measures are subject to sampling variability and our EPU measures draw on fewer newspapers. In fact, re-estimating our panel VAR using only the 12 largest states yields a peak unemployment rate response of 0.155 percentage points to a unit standard deviation policy uncertainty shock of size 0.369.²¹ Re-scaling the shock size as before, our 12-state panel VAR yields a peak unemployment response of $(0.65/0.369)(0.155) = 0.27$ percentage points, slightly larger than Baker et al. (2016) find.

4.2. Robustness checks on the baseline VAR

We explored several alternative specifications and assumptions. First, we fit analogous VAR models to each state separately. As shown in Appendix Fig. A.5, the pattern in Fig. 5 holds in the vast majority of states and is not driven by extreme behavior in a few states. Second, we obtained similar results using shorter and longer lags in the VARs. See Fig. A.6, which displays unemployment responses when using the Hamilton filter. Third, placing $\ln(EPU - C)$ last in the Cholesky ordering also yields similar results, but peak responses are attenuated. When using the Hamilton filter, placing $\ln(EPU - C)$ last reduces the peak unemployment response by about 12% (Fig. A.7). Fourth, replacing $EPU - C$ with $EPU - N$, $EPU - S$, or $EPU - BBD$ in our VAR models yields qualitatively similar dynamic response functions but smaller peak responses in all cases. For example, when using Hamilton-filtered data, the peak unemployment response to a unit policy uncertainty shock is 23% greater for $\ln(EPU - C)$ as compared to $\ln(EPU - N)$, 18% greater as compared to $\ln(EPU - S)$, and 37% greater as compared to $\ln(EPU - BBD)$. This pattern indicates that the broader (and tailored) nature of our $\ln(EPU - C)$ measure captures relevant information that the other measures miss.

¹⁷ The omitted states are Arkansas, Delaware, Hawaii, New Mexico, South Dakota, West Virginia, and Wyoming. While we have EPU data for New Mexico and West Virginia from 1996, the sample of newspapers (and articles) for these states is thin and variable, resulting in very noisy EPU series.

¹⁸ U.S. and global stock markets fell about 40% from 17 February to 23 March 2020 in reaction to the escalating COVID-19 pandemic, and economic activity underwent a spectacular collapse by mid-April (Davis et al., 2021). According to the NBER Business Cycle Dating Committee, the COVID recession lasted only two months, making it the shortest on record back to 1854. See the Committee's announcement on 19 July 2021 at www.nber.org/research/business-cycle-dating.

¹⁹ Hamilton (2018) argues that the HP filter produces spurious dynamic relations, and that standard choices of λ , which are almost universally drawn from Hodrick and Prescott's original work, are not justified by the data. Despite Hamilton's criticisms, the HP filter remains in widespread use, so we report results for both filters. As it turns out, the two filtering approaches yield similar results in our setting.

²⁰ Baker et al. (2016) normalize each country's mean EPU level to 100, but the country-specific normalization period does not always coincide with the sample period they use in their VAR estimation. In addition, their multi-country VAR system includes two variables that are unavailable at the state level: a national stock price index, and an industrial production index. For these reasons, our comparison to their EPU shock response magnitude is not exact.

²¹ The 12 largest states are California, Florida, Georgia, Illinois, Michigan, North Carolina, New Jersey, New York, Ohio, Pennsylvania, Texas, Virginia.

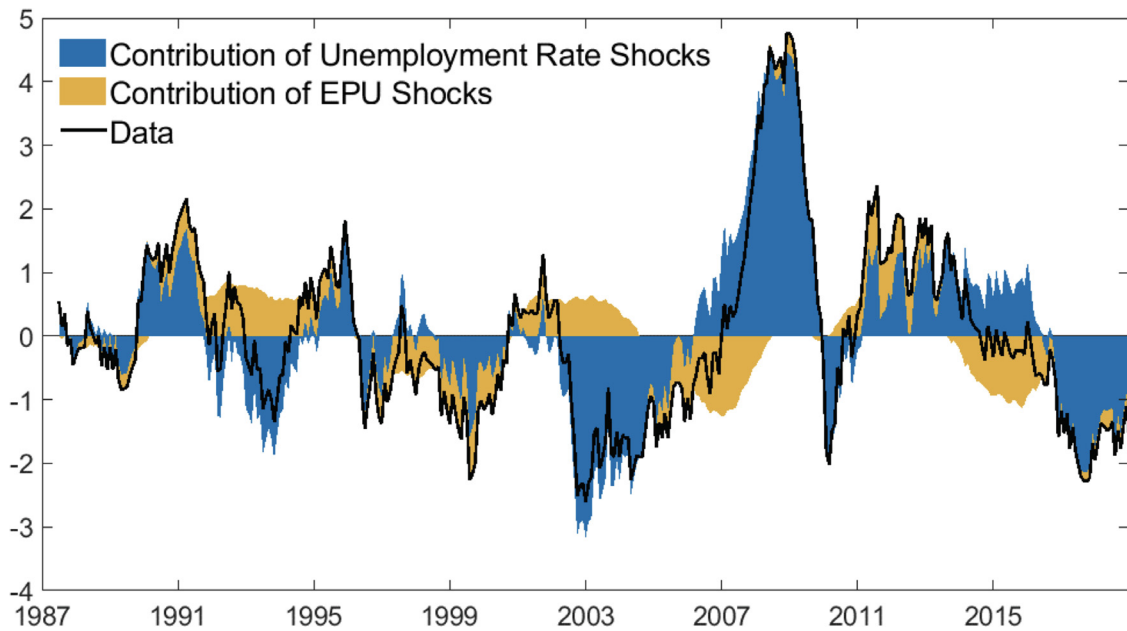


Fig. 6. Historical decomposition of California's unemployment rate movements from June 1988 to December 2019. *Notes:* This figure shows the historical decomposition of unemployment rate movements implied by a structural VAR system with six lags fit to Hamilton-filtered monthly data on the unemployment rate and $\ln(\text{EPU}-C)$ for California from June 1988 to December 2019. In filtering the raw data (available from January 1985), we adopt Hamilton's recommendation to look back over a two-year business cycle ($h = 24$) with one year of lags ($p = 12$), which uses 35 observations. Since we lose six additional observations due to lags in the VAR specification, our estimation sample runs from June 1988 to December 2019. We recover structural shocks from the reduced-form VAR by placing $\ln(\text{EPU}-C)$ last in a recursive causal ordering of the reduced-form VAR innovations. Reversing the causal ordering yields a very similar decomposition.

In summary, the estimated dynamic responses to state-level policy uncertainty shocks are of modest size, estimated with good precision, and qualitatively robust to a range of alternative specifications and identification assumptions. As we show next, realized EPU shocks can have material effects on state-level unemployment rate movements.

4.3. An illustration: EPU shocks and their effects in california

California is the largest state in the union, and its recent history features a good deal of policy-related economic uncertainty. Motivated by these facts, we now consider the role of EPU shocks as a driver of unemployment rate movements in the state.

As a first step, we re-fit our six-lag VAR model using monthly Hamilton-filtered data on the unemployment rate and $\ln(\text{EPU} - C)$ for California. Here, we exploit the fact that our data for California extend back to January 1985. After accounting for lags and filtering, our estimation sample runs from June 1988 to December 2019. In identifying the structural VAR, we place $\ln(\text{EPU} - C)$ last in the recursive causal ordering. This amounts to assuming that policy uncertainty shocks have no same-month impact on the state's unemployment rate, while allowing the unemployment rate shock to contemporaneously affect $\ln(\text{EPU} - C)$.

Fig. A.8 displays the estimated dynamic response of California's unemployment rate to the identified $\ln(\text{EPU} - C)$ shock. The shape is similar to the response function in the upper right panel in Fig. 5, but the implied peak effect for a same-size shock is nearly twice as large. To see this, multiply the peak estimated response of California's unemployment rate (0.12) by the ratio of the shock sizes to obtain $(0.12)(0.45/0.28) = 0.19$. Recall that the peak response obtained from the identified panel VAR is only 0.10, even with $\ln(\text{EPU} - C)$ placed first in the causal ordering. Thus, the results for California reinforce our earlier conclusion that larger states yield larger estimated effects of policy uncertainty shocks.

We now use the structural VAR to examine the contribution of policy uncertainty shocks to movements in California's unemployment rate. Inspecting the time path of structural $\ln(\text{EPU} - C)$ shocks (not shown), we find large positive values of 59 log points in October 2000 and 42 log points in November, followed by essentially no shock in December (-4 log points) and a sequence of positive shocks from January to May 2001 that average 19 log points. This pattern aligns well with our earlier discussion of the California electricity crisis in 2000–01. Surprisingly though, we find little trace of California's gubernatorial recall in the path of the structural $\ln(\text{EPU} - C)$ shocks.

Next, we feed the realized shock sequences through a Wold moving-average representation of our structural VAR. This exercise delivers a VAR-based historical decomposition of monthly movements in California's unemployment rate, expressed as deviations from the model-implied equilibrium values. As seen in Fig. 6, the electricity crisis produced one of two episodes

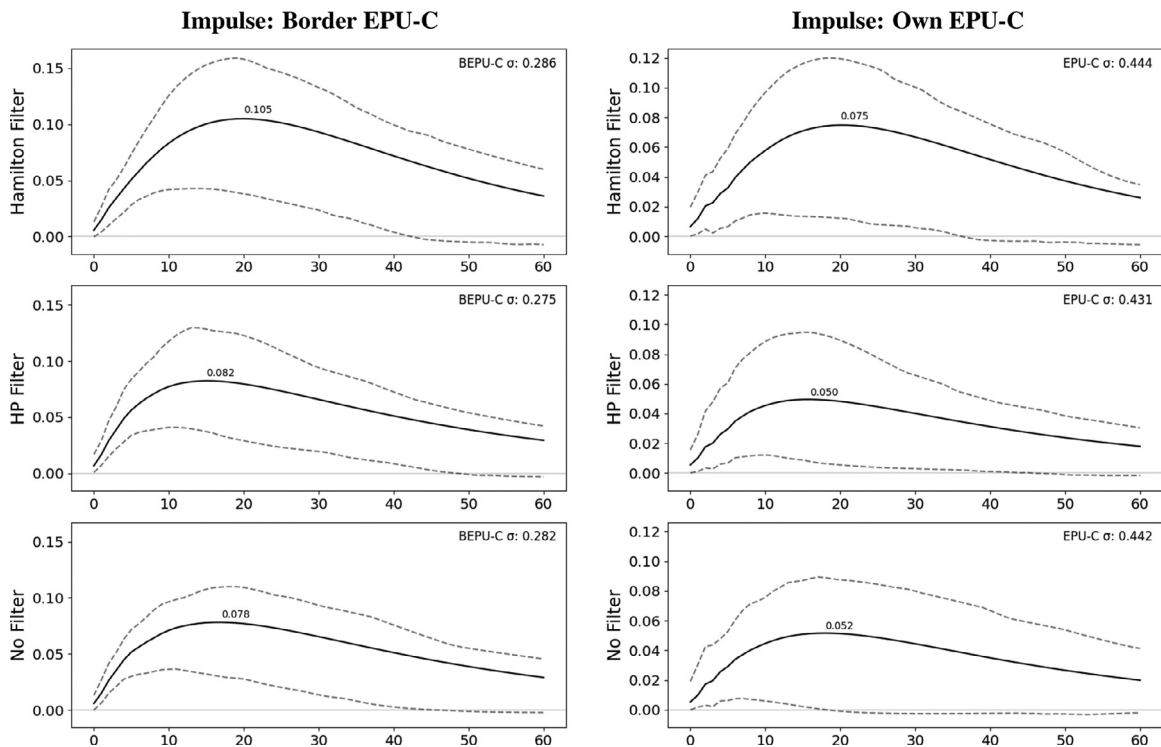


Fig. 7. Unemployment rate responses to unit standard deviation $\ln(EPU - C)$ and $\ln(BEPU - C)$ shocks. *Notes:* Each panel shows estimated responses (with 95% confidence intervals) of the state-level unemployment rate to a unit standard deviation shock to $EPU - C$ or $BEPU$, the peak response, and the standard deviation of the identified shock. To obtain these results, we filter the data as indicated, fit a three-equation panel VAR model by least squares, place $EPU - C$ first in the Cholesky ordering, and place $BEPU$ second. The VAR system has six lags of each variable and state-specific intercepts. We use the same samples as in Fig. 5 except for dropping Alaska.

in which large upward policy uncertainty shocks elevated California's unemployment rate by 50–60 basis points or more for an extended period. The maximal swing in California's unemployment rate due to policy uncertainty shocks is about two percentage points. In short, California's experience shows that policy uncertainty shocks materially affect state-level economic performance.

4.4. Spatial spillover effects

We now extend our VAR specification to incorporate EPU shocks in neighboring states. For each state s in month t , we compute $BEPU_{st} = \sum_{b \in B(s)} p_b (EPU - C_{b,t})$, where $B(s)$ is the set of states that border s and p_b is the state- b share of population among states in $s \cup B(s)$.²² This measure has two desirable properties: It weights neighboring states in proportion to their populations, and it gets scaled down for states that are populous compared to neighboring states. This scaling incorporates the assumption that EPU shocks in California, for example, have bigger effects on economic activity in Oregon than the other way round. We add $\ln(BEPU_{st})$ to our baseline panel VAR, placing it after $\ln(EPU - C_{st})$ in the Cholesky ordering and before the unemployment rate. As before, we include six lags of each variable and state-specific intercepts. We also use the same sample as before, except for dropping Alaska.

Fig. 7 displays the estimated responses (and 95% confidence intervals) of state-level unemployment rates to unit standard deviation upward shocks to own-state and border-state policy uncertainty. Both shocks raise state-level unemployment by statistically significant amounts. The response functions have similar shapes for the two EPU shocks, which makes sense given their similar nature. As expected, border-state EPU shocks are smaller in magnitude, as measured by the standard deviation of the structural innovations. For own-state and border-state EPU shocks of equal size, the peak unemployment rate response is about the same in both cases.

How important are policy uncertainty shocks for state-level unemployment rate fluctuations? One way to address this question is to consider their role in generating model-implied forecast errors at a horizon of, say, thirty months. Using our extended panel VAR specification, own-state and border-adjusted EPU shocks jointly account for about one-tenth of the unemployment-rate forecast error variance at this horizon. This result says that EPU shocks account for a modest share of

²² When $EPU - C_{b,t}$ is missing, which happens for some smaller states early in the sample, we redefine $B(s)$ to exclude the state in question.

state-level unemployment fluctuations. Recall, however, two results of our descriptive analysis in Section 2.6. First, policy uncertainty is subject to occasional large upward spikes, as shown in Fig. 2. Second, these extreme episodes also tend to involve greater synchronicity in state-level EPU movements, as shown in Fig. 1 and illustrated in Fig. A.3. Thus, unusually large upward shocks in own-state EPU tend to arrive at the same time as unusually large upward shocks in border-state EPU. For both reasons, policy uncertainty shocks can occasionally drive sizable increase in state-level unemployment rates, even though they play a minor role most of the time.

4.5. Additional outcome variables

We also consider VAR models that include other state-level outcome variables: the (natural) log of housing prices, the log of new permits for private housing units, and the log of business starts.²³ Because these variables are noisier than the corresponding employment and unemployment measures, we focus on the twelve largest states, which yield sharper results. We add one variable at a time to our baseline panel VAR, placing it last in the Cholesky ordering. As before, we include six lags of each variable and state-specific intercepts. The sample start is determined by data availability and continues through December 2019.

Appendix Figures A.9 and A.10 display the estimated dynamic responses to own-state EPU – C shocks. Unemployment responses remain highly significant and are not much affected by the additional variables. Upward EPU shocks reduce new housing permits and home prices. The impact on housing permits is statistically significant but small, peaking at about minus one percent after five months. It vanishes within a year and then reverses. The negative effect on home prices holds for HP-filtered and raw data but not for Hamilton-filtered data. Upward EPU shocks impart a significant negative effect on business formation when using Hamilton-filtered data but not when using HP-filtered or raw data. In short, the evidence favors the view that policy uncertainty shocks depress housing markets, and perhaps business formation as well, but these results are weaker and more fragile than the earlier results for employment and unemployment.

5. EPU and economic performance during the pandemic

COVID-19 led to enormous increases in policy uncertainty (Fig. 2), more so in some states than others. As an indication of this heterogeneity, the cross-state standard deviation of the change in EPU – C from 2019 to the third quarter of 2020 is 44 log points. We now use this state-level variation to develop evidence on how the pandemic raised policy uncertainty and how, in turn, policy uncertainty relates to state-level economic performance during the pandemic.

5.1. What drove policy uncertainty during the pandemic?

We consider two hypotheses about the drivers of state-level EPU during the pandemic:

1. Policy uncertainty rises with the severity of the pandemic's health consequences.
2. Policy uncertainty rises with the extent of government-mandated restrictions on economic and social activities.

The first hypothesis captures the view that greater pandemic severity generates more uncertainty about how policy makers will respond and about the economic consequences of their responses. To operationalize this hypothesis, we measure severity as quarterly COVID deaths per capita in the state.²⁴ Of course, deaths alone do not capture the full range of health consequences associated with COVID-19. We use data on deaths because they are readily available, of relatively high quality, comparable across states and over time, and correlated with other health outcomes associated with COVID. In short, per capita deaths are a reasonable and practical proxy for the severity of a pandemic's health consequences.²⁵

Our second hypothesis reflects the idea that government restrictions on activity create economic uncertainty, and that more extensive restrictions create greater uncertainty. To operationalize this hypothesis, we consider four types of government orders that were widely deployed during the pandemic:

- Shelter-in-place orders (SIPOs) that confine persons to their residences except when performing essential activities.
- Business closure orders (BCOs) that require “non-essential” businesses in multiple industries to halt operations.
- Restaurant closure orders (RCOs) that cover bars, restaurants, taverns, and other eating facilities while typically allowing take-out and delivery services.

²³ As measured, respectively, by the FHFA House Price Index for single-family housing at www.fhfa.gov/DataTools, the Census Bureau's Building Permits Survey at www.census.gov/construction/bps/, and new applications for an Employer Identification Number by businesses with a high propensity to hire. Specifically, we use the seasonally adjusted HBA series available from the U.S. Census Bureau at <https://www.census.gov/econ/bfs/index.html>.

²⁴ Our data on death rates are from the COVID Data Tracker provided by the U.S. Centers for Disease Control and Prevention at <https://covid.cdc.gov/covid-data-tracker#datatracker-home>. We average the daily data to the quarterly level for each state.

²⁵ Alternatively, one might consider COVID case rates per capita. However, using case rates to proxy for pandemic severity is highly problematic for two reasons. First, reported case rates depend on testing rates, which vary greatly across states and over time. Second, conditional on contracting COVID, the likelihood of death or serious illness changed greatly as treatments improved, vaccination rates rose, and the demographic mix of infected persons shifted. For both reasons, case rates are an unsuitable measure for our purposes. That said, our results are not materially different when using reported case rates in place of death rates.

Table 2

Policy uncertainty rose more in states with more extensive government restrictions dependent variable: log change in state-level *EPU-C* value from 2019 to the indicated quarter.

Quarter	(1) 2020Q2	(2) 2020Q3	(3) 2020Q4	(4) 2021Q1	(5) 2021Q2
Lockdown Stringency Index	0.59*** (0.13)	0.76*** (0.14)	0.45*** (0.11)	0.56*** (0.16)	0.34* (0.19)
COVID Deaths	−202.0 (331.8)	−132.0 (800.1)	−1,185** (474.8)	477.2 (951.8)	−274.8 (2,208)
Observations	51	51	51	51	51
<i>R</i> ²	0.164	0.323	0.275	0.173	0.055

Notes: Each column pertains to a separate regression, where the dependent variable is $\ln(EPU-C_{s,q}/EPU-C_{s,2019})$ for the indicated quarter. Explanatory variables are state-level values of the COVID Death Rate per 100,000 persons and the Lockdown Stringency Index value in the indicated quarter. The stringency index aggregate information about shelter-in-place orders, business closure orders, restaurant closure orders and school closure orders, as explained in Section 5.1. See Appendix Table A.3 for summary statistics of the variables. Huber-White robust standard errors are in parentheses.

- School closure orders (SCOs).

We combine data on these four types of orders to create a Lockdown Stringency Index for each state from the second quarter of 2020 through the second quarter of 2021.

Specifically, for a given state and month, we set *SIPO* = 1 when a shelter-in-place order is in effect, 0 otherwise. We assign a fractional value if the SIPO is in effect for a fraction of the month. We define *BCO*, *RCO*, and *SCO* analogously. In using these state-level indicator variables, we treat *RCOs* (i.e., restaurant closure orders) as a limited form of the broader *BCOs*. We treat the combination of a *BCO* and *SCO* as equivalent to a *SIPO*. Thus, we compute the following Lockdown Stringency Index value for each state and month:

$$LSI = \text{Max}\{SIPO, 0.75 * BCO + 0.25 * SCO, 0.25 * RCO + 0.25 * SCO\}, \quad (5)$$

which ranges from 0 to 1. We then average the *LSI* values over months in the calendar quarter to obtain the state's quarterly Lockdown Stringency Index value. Appendix A.4 describes our data sources for government orders and provides more details about our calculations.

To assess hypotheses 1 and 2, we estimate least-squares regressions of the following form:

$$\ln(EPU-C_{s,q}/EPU-C_{s,2019}) = c + \beta_1 CDR_{s,q} + \beta_2 LSI_{s,q} + \epsilon_{s,q}, \quad (6)$$

where the dependent variable is the log change in *EPU-C* for state *s* from 2019 to quarter *q* for *q* = 2020Q2, 2020Q3, ..., 2021Q2; *CDR_{s,q}* is COVID deaths per 100,000 persons in state *s* during quarter *q*; *LSI_{s,q}* is the Lockdown Stringency Index value for state *s* in *q*; and $\epsilon_{s,q}$ is an error term. Since *CDR* and *LSI* equal zero in 2019, (6) is effectively a difference-in-difference regression. We fit (6) separately for each *q* in light of frequent references in the popular discourse to “COVID fatigue,” “mounting resistance to government restrictions,” and the like. These developments may have prompted changes over time in how newspapers reported on pandemic-related policy uncertainty. The regulatory approval of anti-COVID vaccines in December 2020 – widely perceived as a watershed development – may also have altered newspaper coverage of uncertainties associated with the pandemic.

Table 2 reports the results of estimating (6) for several values of *q*. To our surprise, we find no evidence that greater COVID death rates in a state raised its level of policy uncertainty. The marginally significant coefficient on *CDR* in column (3) says that states with greater COVID death rates in the fourth quarter of 2020 experienced *smaller* increases in *EPU-C* from 2019 to 2020 Q4 – the opposite of the hypothesized effect. None of the other regressions in Table 2 yield a statistically significant effect of COVID death rates on own-state policy uncertainty.

In contrast, we find strong evidence that states with stricter lockdowns had larger increases in policy uncertainty relative to 2019. This pattern holds for each value of *q* in Table 2, with some attenuation by the second quarter of 2021. A unit standard deviation between-state *LSI* differential of 0.32 in the third quarter of 2020, for example, implies an increase in state-level policy uncertainty (relative to 2019) of 24 log points. That is more than one-half the standard deviation of the corresponding dependent variable for column (2) in Table 2. Bin scatters displayed in Appendix Fig. A.11 indicate that this pattern is not driven by a few outliers among the states. In unreported results, we find weaker evidence that states with higher values of *LSI* also experienced an increase in the ratio of *EPU-S* to *EPU-N* after the onset of the pandemic.

In summary, we find no evidence that the severity of COVID-related health consequences affected state-level policy uncertainty during the pandemic. We find strong evidence that policy uncertainty rose more in states with stricter government-mandated lockdowns.

5.2. State-level economic performance during the pandemic

We now investigate how state-level economic performance during the pandemic relates to pandemic severity, lockdown stringency, and policy uncertainty. To do so, we estimate state-level difference-in-difference regressions of the following

Table 3

How state-level unemployment rates relate to lockdown stringency, death rates, and policy uncertainty during the pandemic dependent variable: log change in state-level unemployment rate from 2019 to indicated quarter.

Quarter	(1) 2020Q2	(2) 2020Q3	(3) 2020Q4	(4) 2021Q1	(5) 2021Q2
Lockdown Stringency Index	4.11** (1.97)	2.03*** (0.66)	1.94*** (0.54)	2.39*** (0.44)	1.95*** (0.62)
COVID Death Rate	4051 (3289)	−4647 (4778)	−716.3 (2031)	954.0 (3353)	−54.63 (10,961)
Δ ln(EPU-C) 2020Q2	2.86 (1.91)				
Δ ln(EPU-C) 2020Q3		1.51** (0.70)			
Δ ln(EPU-C) 2020Q4			0.84 (0.68)		
Δ ln(EPU-C) 2021Q1				0.33 (0.43)	
Δ ln(EPU-C) 2021Q2					0.58* (0.33)
Observations	51	51	51	51	51
R ²	0.324	0.286	0.324	0.383	0.251

Notes: Each column pertains to a separate regression, where the dependent variable is the change in the state-level unemployment rate from 2019 to the indicated calendar quarter. Explanatory variables are state-level values of (a) the COVID Death Rate per 100,000 persons in the indicated quarter, (b) the Lockdown Stringency Index value in the indicated quarter, and (c) the log change in the state-level composite EPU index value from 2019 to the indicated quarter. See Appendix Table A.3 for summary statistics of the variables. Huber–White robust standard errors are in parentheses.

form:

$$UN_{s,q} - UN_{s,2019} = a + \gamma_1 CDR_{s,q} + \gamma_2 LSI_{s,q} + \gamma_3 \ln(EPU - C_{s,q} / EPU - C_{s,2019}) + \phi_{s,q}, \quad (7)$$

where the dependent variable is the change in the unemployment rate for state s from 2019 to quarter q for $q = 2020Q2, 2020Q3, \dots, 2021Q2$, and $\phi_{s,q}$ is a regression error.

Table 3 reports the results. There are three messages. First, we find no evidence that pandemic severity matters for a state's relative economic performance. Second, states that imposed stricter lockdowns had bigger unemployment rate increases relative to 2019. As an example, consider the regression for $q = 2020Q2$. Multiplying the LSI coefficient by a unit standard deviation differential in LSI values across states in 2020Q2 gives $(4.11)(0.24) = 0.99$, essentially one percentage point. Analogous calculations for the other values of q yield unemployment responses that range from 0.56 to 0.84 percentage points. Thus, there is clear evidence that stricter lockdowns were associated with sharper state-level unemployment rate increases during the pandemic. Third, there is weaker evidence that state-level unemployment also rose with increases in policy uncertainty during the pandemic (conditional on lockdown stringency and pandemic severity). Multiplying the coefficient on the log change in $EPU - C$ in column (2) by its unit standard deviation differential across states gives $(1.51)(0.244) = 0.66$, nearly two-thirds of a percentage point.

These results suggest that stricter (and longer) lockdowns and higher policy uncertainty both raised a state's unemployment rate during the pandemic. Still, caution is warranted in drawing causal inferences from these regression results. States may differ in unobserved ways that influenced both the vulnerability of their economies to the COVID-19 pandemic and their choices over lockdown policies. In addition, states that imposed stricter lockdowns may also have adopted other policies that raised unemployment. Thus, we see the results in Table 3 as strong motivation for more research into the effects of lockdown mandates and policy uncertainty during the pandemic on unemployment during (and after) the pandemic.

6. Concluding remarks

Using digital archives for thousands of newspapers, we construct three monthly indexes of economic policy uncertainty (EPU) for each U.S. state: one that captures state and local sources of policy uncertainty ($EPU - S$), one that captures national and international sources ($EPU - N$), and a composite index that captures both ($EPU - C$). Our indexes date to 1985 for some states and are freely available at www.PolicyUncertainty.com with regular updates.

Drawing on our indexes and other state-level data, we develop several new findings about the sources of policy uncertainty and its relationship to state-level economic performance :

6.1. The importance of state and local policy

State and local matters are major sources of policy uncertainty. Even before COVID-19, half of all newspaper articles that feed into our composite EPU index discuss state and local policy. That share rose to 62% in the period from March 2020 (when the pandemic struck in force) to June 2021 (the end of our sample period).

6.2. The pandemic and policy uncertainty

The COVID-19 pandemic drove huge increases in policy uncertainty, pushing (average) $EPU - N$ to 2.7 times its pre-COVID peak and (average) $EPU - S$ to more than four times its previous peak. Policy uncertainty rose more sharply in states with stricter lockdowns – as measured by the incidence and duration of shelter-in-place orders, business closure orders, restaurant closure orders, and school closure orders. This lockdown effect is large relative to between-state variation in pandemic-era jumps in policy uncertainty. Surprisingly, policy uncertainty exhibits no discernible response to pandemic severity, as measured by COVID deaths per capita in the state.

6.3. Elections and policy uncertainty

Elections are recurring sources of state-level policy uncertainty. Close elections (vote margin less than four percent) bring especially large increases in policy uncertainty. We estimate that a close presidential election contest raises $EPU - N$ by 60 percent and a close gubernatorial contest raises $EPU - S$ by 35 percent. The richness of our state-level data lets us estimate these election effects with good precision, while controlling for several potential confounders.

6.4. Other sources of policy uncertainty

The cross-state average of our $EPU - N$ indexes rose in response to 9–11, Gulf Wars I and II, major financial crises, the 2011 debt-ceiling crisis, the 2012 fiscal cliff episode, the June 2016 Brexit referendum, trade policy tensions during the Trump presidency, and partial federal government shutdowns that commenced in October 2013 and December 2018. As we illustrate by example, $EPU - S$ can rise sharply in reaction to shocks that interact with poor policy design (California's electricity crisis of 2000–01), political turmoil in the wake of economic mismanagement (California's gubernatorial recall in 2003), major tax reforms that aim to promote economic development (the Kansas tax experiment of 2012), natural disasters that raise questions about how policymakers will respond (Louisiana after Hurricane Katrina in 2005), and state-specific exposures to major national developments and policy actions (Michigan in the Great Recession).

6.5. Upward policy uncertainty shocks foreshadow weaker economic activity

Upward policy uncertainty innovations in VAR models foreshadow higher state-level unemployment rates and lower employment, with peak responses of modest size about one year later. They also foreshadow lower home prices and fewer new permits for housing construction. As illustrated by California's experience, realized EPU shocks can generate sizable swings in state-level unemployment rates – about two percentage points in California's case.

6.6. Policy uncertainty shocks spill over across state borders

Own-state economic activity also deteriorates in the wake of upward policy uncertainty shocks in neighboring states. Own-state and border-state policy uncertainty shocks of equal size have similar effects on own-state unemployment rates.

6.7. Lockdowns, policy uncertainty, and state-level performance during the pandemic

States that imposed stricter lockdowns during the pandemic had bigger jumps in unemployment, conditional on pandemic severity and policy uncertainty. Bigger increases in state-level policy uncertainty during the pandemic also came with bigger increases in unemployment. There are sound reasons for caution in drawing causal inferences from these patterns, as we discuss. Nevertheless, they highlight the value of more research into how lockdown stringency and policy uncertainty during the pandemic affected unemployment and other outcomes.

The foregoing summary underscores the usefulness of our new state-level EPU indexes. We see several fruitful directions for future research:

First, how does political polarization influence election-related policy uncertainty? It's natural to hypothesize that elections generate more uncertainty when the electorate is more polarized. Baker et al. (2020) find support for this view in the behavior of national policy uncertainty around U.S. presidential elections. By exploiting state-level data on EPU, polarization and elections, it becomes feasible to scrutinize this hypothesis more carefully and to differentiate among the effects of different types of political polarization.

Second, why has U.S. policy uncertainty drifted upward since the late 1960s? Baker et al. (2014) advance two explanations for this drift. One stresses growth in government spending, taxes, and regulation. Another stresses increased political polarization and its implications for the policy-making process and policy choices. Both explanations find support in the evidence they amass, but it's hard to develop a convincing evaluation based only on national data. Our work opens the door to a more persuasive assessment that exploits the abundant state-level variation in government growth, polarization, and – now – policy uncertainty.

Third, how does uncertainty affect firm-level investment and employment? Much previous research tackles this question using panel regression designs and identification strategies that exploit election-related uncertainty and national policy uncertainty measures. Prominent examples include Baker et al. (2016); Gulen and Ion (2016) and Jens (2017). Our state-level EPU indexes greatly expand the measured variation that is available to study how firms respond to policy uncertainty.

Lastly, how does the categorical mix of policy uncertainty vary across states and over time within states? Given the volume and topical density of newspaper articles, it is feasible to construct state-level measures of policy uncertainty for particular policy categories. By adapting the newspaper-based methods in Baker et al. (2016); Husted et al. (2020), and Caldara et al. (2020), one could construct state-level measures of uncertainties related to tax policy, government spending, labor market policy, monetary policy, trade policy, and more. Such measures would provide new tools for studying the drivers of policy uncertainty and effects on firm-level and state-level outcomes.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Supplementary material

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