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Duration dependence, monetary policy asymmetries, and the business cycle

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Duration dependence, monetary policy asymmetries, and the business cycle*

Abstract

We produce business cycle chronologies for U.S. states and evaluate the factors that change the probability of moving from one phase to another. We find strong evidence for positive duration dependence in all business cycle phases but find that the effect is modest relative to other state- and national-level factors. Monetary policy shocks also have a strong influence on the transition probabilities in a highly asymmetric way. The effect of policy shocks depends on the current state of the cycle as well as the sign and size of the shock.

- *Keywords*: Duration analysis, hazard rates, business cycles, monetary policy asymmetries.
- *JEL Codes*: C23, C25, E32, E52.

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1 Introduction

Macroeconomic growth consists of a series of alternating distinct phases: economic expansions are followed by declines in economic activity, recessions. The behavior of macroeconomic indicators is strikingly different across the two phases. It is no surprise, then, that a large literature studies the movement of the economy from one phase to the next.[1] A typical approach is to use either parametric or non-parametric statistical tools to make inference about the probability of moving from one phase of the business cycle to another, using the NBER's chronology of expansions and recessions in the United States. However, because there are only a handful of business cycle phases in the United States since WWII, inference made using business cycle data is necessarily uncertain. Previous studies generally cannot reject a null hypothesis that the hazard rate of the business cycle is flat, leading to the well-known aphorism that 'economic expansions do not die of old age.'[2]

This paper revisits the estimation of the hazard rate of the business cycle. Our analysis is distinct from the previous literature in two ways. First, our object of interest is the business cycle in U.S. states. Although state-level chronologies share a common national factor, they exhibit substantial heterogeneity, which allows us to estimate the hazard much more precisely than the literature that evaluates national-level chronologies.[3] Secondly, whereas most of the previous literature focused on the shape of the hazard rate of the business cycle, because of the panel nature of our data, we are able to evaluate other risk factors for the business cycle. One obvious confounding factor is the stance of monetary policy, but we also consider the effect of oil prices, housing prices, and the flexibility of the local labor market on business cycle dynamics. We also explore whether the initial conditions of a particular business cycle phase affect its duration, such as the duration or depth of the previous phase.

[1] Diebold & Rudebusch (1990), Sichel (1991), Sichel, Diebold & Rudebusch (1993), catalyzed the literature of analyzing the hazard rate of business cycle phases. More recently, Zuehlke (2003) and Castro (2010) have revisited the literature, the latter analyzing the hazard rate from a panel of international business cycle chronologies. A related literature is devoted to forecasting or identifying in real-time the move of the macroeconomy from expansion to recession. See Hamilton (2011) for a recent overview of the issues confronted by this literature.

[2] Kim & Nelson (1998) find positive duration dependence in an estimated common factor with regime switching behavior. Filardo & Gordon (1998) similarly find the transition probabilities of a Markov-switching model of U.S. business cycles are time varying and related to an index of leading indicators, although they do not explicitly evaluate duration dependence. Zuehlke (2003) estimates a variety of parametric hazard functions and also finds evidence of positive duration dependence since 1945, while Castro (2010) finds evidence of positive duration dependence in OECD countries.

[3] Other papers that have used U.S. state level data to make inference about business cycles include Michael T. Owyang and Jeremy M. Piger and Howard J. Wall (2005), Owyang, Piger & Wall (2015), Hamilton & Owyang (2012), Gonzalez-Astudillo (2017), and Francis, Jackson & Owyang (2018).

Our empirical approach consists of two steps. In the first step, we apply the Bry & Boschan (1972) algorithm to identify business cycle chronologies from labor market variables. Taking the chronologies as given, we then estimate hazard rate models of the business cycle. When we apply the approach to the U.S. at the national level, we replicate very closely analysis performed using the NBER business cycle chronology (Sichel 1991). When we estimate the hazard rate of state-level business cycles, we find that the hazard rate for all stages of the business cycle is upward-sloping, that is, business cycle phases become more likely to end as they progress. However, the economic significance of the effect is relatively modest. In our preferred specification for expansions, we find that an expansion of 170 months (about as long an expansion as is observed in our sample) is about 35 percent more likely to end than an expansion of 42 months (the median duration of expansions in our sample). Assuming that all covariates are at their mean level, we estimate that a state currently in an expansion of duration 42 months has probability of 3.3 percent of turning to recession. In contrast, an expansion of duration 170 months has transition probability of around 4.5 percent.

Beyond considering the shape of the hazard, we also consider the effect of additional risk factors, including national-level macroeconomic and financial variables but also state-level business and institutional characteristics. The strongest effects on the hazard rate come from the slope of the yield curve and other interest rate spreads. Among the state-specific covariates, house prices, regional mortgage rates, and the business cycle phase of nearby states change the hazard in both expansions and contractions. States that have enacted right-to-work legislation exit recessions more quickly, on average, than states that have not enacted such legislation, though we find no impact of this kind of legislation on the hazard rate of expansions.

Lastly, we study monetary policy shocks as a risk factor for transition between different business cycle phases, focusing on potential asymmetries in the effects of monetary policy shocks.[4] We show that monetary policy shocks have differential effects depending on the current state of the business cycle, whether the shock is expansionary or contractionary, and whether the shock is small or

[4] Carlino & Defina (1998) examine the differential impact of monetary policy across U.S. states and regions and find that manufacturing regions experience larger reactions to monetary policy shocks than industrially-diverse regions. Francis et al. (2018) also study the effects of changes in the federal funds rate on the speed of recovery after a recessions, but they focus on the changes in the federal funds rate, while we take into consideration only the unexpected part of monetary policy change. Weise (1999), Lo & Piger (2005), Santoro, Petrella, Pfajfar & Gaffeo (2014), Tenreyro & Thwaites (2016), and Barnichon & Matthes (2018) all study monetary policy asymmetries.

large. When the economy is in an expansion, the effect of policy shocks depends strongly on the sign of the shock. Contractionary policy shocks raise the hazard—i.e., increase the probability of moving into a recession—while expansionary shocks lower it. Further, contractionary shocks have a larger impact on the hazard rate than expansionary ones. We find similar asymmetries when states are in recession. Conditional on being in a recession, expansionary shocks raise the hazard. However, contractionary shocks also raise the hazard rate, albeit by less than expansionary shocks. Additionally, we test for asymmetries in the size of the monetary policy shock. This asymmetry is most prevalent in recessions, where surprisingly small shocks are relatively more powerful than large ones. One possible explanation is that small shocks have stronger signaling effects about the current and future economic growth.

The remainder of the paper proceeds as follows. In section 2, we outline our empirical strategy. Section 3 presents the results, first at the national level and then using the state-level data. Section 4 considers monetary policy shocks as a risk factor. We conclude with a brief discussion.

2 Empirical approach

We wish to estimate the hazard rate of business cycle phases from a panel of chronologies from U.S. states. Since there are no official state-level recession chronologies, our first task is to produce them. With the chronologies in hand, we then turn to modeling the hazard rate.

2.1 Identifying business cycle phases

Economic cycles typically pervade throughout an economy, so that economists typically look at a wide range of indicators to identify business cycles (Burns & Mitchell 1946). For example, when producing the business cycle chronology for the United States, the Business Cycle Dating Committee of the National Bureau of Economic Research uses quarterly real GDP alongside several monthly indicators. Unfortunately, obtaining a long time-series of multiple indicators of economic conditions at the state level is not possible with standard data sources.

Thus, we use a single variable, the unemployment rate, to identify business cycle turning points in each state. Specifically, we apply the Bry & Boschan (1972) algorithm to identify peaks and troughs in each state's unemployment rate.[5] We add one restriction to an otherwise standard application of the algorithm. Because the sampling error of state-level unemployment rates can be sizable, state unemployment rates often tick higher or lower in movements clearly unrelated to the business cycle.[6] Since do not want to mischaracterize these small movements as a change in the business cycle phase, we require that a business cycle trough correspond to a cumulative rise in the unemployment rate of at least one-half percentage point from the previous peak. With the Bry-Boschan peaks and troughs in hand, we then define the latent state of the local economy, S_{it}, where $S_{it} = 0$ denotes that month t in state i is an economic expansion, whereas $S_{it} = 1$ denotes a recession instead.

2.2 Modeling the hazard rate

With state-level chronologies in hand, we turn to modeling the hazard rate. Let τ_i denote the random variable that defines the duration of phase i of the business cycle. The hazard function is the probability that a phase ends at duration t, given that it has persisted until that point. The hazard function is closely related to the survival function $S(\tau)$, the probability that a phase has duration longer than τ months. Note that $S(\tau) = 1 - F(\tau)$, where $F(\tau)$ is the cdf of τ, with corresponding pdf $f(\tau)$. Denoting the hazard function $h(\tau)$, the hazard function is $h(\tau) = [F(\tau + \Delta\tau) - F(\tau)]/[1 - F(\tau)]$, and after taking the limit $\Delta\tau \to 0$, $h(\tau) = f(\tau)/S(\tau)$.[7] We use the parsimonious but flexible Weibull form to parameterize the hazard function: $h(\tau) = \alpha\lambda\tau^{\alpha-1}$.[8] The parameter α governs the shape of the hazard. When α is one, the hazard rate is independent of τ, while α great (less) than one implies that the hazard rate increases (decreases) with duration. We add controls for other potentially important macroeconomic indicators by parameterizing λ as $\lambda = \exp(x'\beta)$, where x denotes an $(N+1) \times 1$ vector of observables and β measures a proportionate increase or decrease in risk associated with the set of characteristics x.

Our estimation strategy allows for time-variation in the covariates. That is, instead of specifying

[5] For details of the algorithm, see Bry & Boschan (1972). Harding & Pagan (2002) and Stock & Watson (2010a, b) provide recent applications to macroeconomic data. We have also performed our analysis using a state-level coincident index as our measure of economic activity, and our results are qualitatively unchanged (results available upon request).

[6] See (Bureau of Labor Statistics 2017) for a description of state-level sampling error.

[7] For a textbook treatment see, for example, Greene (2011) or Jenkins (2005).

[8] Sichel (1991) and Sichel et al. (1993) investigate Weibull hazard models. Diebold & Rudebusch (1990) provide a non-parametric evaluation, while Zuehlke (2003) compares different parametric forms for the hazard rate.

$\lambda(x_i)$, where x_i is a vector of characteristics associated with phase i, we model $\lambda(x_{it})$, where x_{it} denotes a sequence of covariates observed within phase i, $t \in 1, ..., \tau_i$. Each month, we observe whether a spell is completed and a vector of covariates associated with that phase, x_{it}. If a phase is not completed in month t, then we consider x_{it} as a right-censored observation. A censored observation contributes its survival to the likelihood, since the probability of survival until period j is the product of probabilities of *not* exiting in each period up to and including j:

$$\begin{aligned} Pr(\tau_i > j) &= S(\tau_i = j) \\ &= \prod_{t=1}^{j}(1 - h(t, x_{it})) \equiv \prod_{t=1}^{j} f_c(t, x_{it}). \end{aligned}$$

An observation where the spell ends at j contributes:

$$\begin{aligned} Pr(\tau_i = j) &= h(\tau_i = j)S(\tau = j - 1) \\ &= h(j, x_{ij}) \prod_{t=1}^{j-1}(1 - h(t, x_{it})) \\ &= \frac{h(j, x_{ij})}{1 - h(j, x_{ij})} \prod_{t=1}^{j}(1 - h(t, x_{it})) \equiv \prod_{t=1}^{j} f_u(t, x_{it}). \end{aligned}$$

To construct the likelihood function, let I be an indicator that denotes whether an observation is uncensored, with $I_{it} = 0$ indicating that phase i has not ended in month t, and $I_{it} = 1$ that that it is complete. Then the likelihood for the sample is

$$\begin{aligned} \mathcal{L} &= \prod_{i=1}^{N} \left[Pr(\tau_i = j)\right]^{I_i} \left[Pr(\tau_i > j)^{1-I_i}\right] \\ &= \prod_{i=1}^{N} \left[\prod_{t=1}^{j_i} f_u(t, x_t)\right]^{I_{it}} \left[\prod_{t=1}^{j_i} f_c(t, x_t)\right]^{1-I_{it}}, \end{aligned} \quad (1)$$

where f_c and f_u are defined above and N is the number of phases observed in the sample.

We separately estimate the model via maximum likelihood for four business cycle phases: expansion, contraction, peak-to-peak, and trough-to-trough. In the tables that follow, regression results are reported as hazard ratios (that is, βs are exponentiated). The β estimates therefore report the percent increase or decrease in the duration of the business cycle phase, holding all other covariates fixed.[9]

3 Results

To clarify the chronologies produced by the Bry-Boschan algorithm, and revisit the conclusions of previous literature with the benefit of 20 additional years of data, a first step in our analysis is to apply our two-step estimation procedure to the national economy.

3.1 Application to national-level data

Table 1 compares business cycle durations for national recessions as defined by the NBER to the recession dates produced by applying the Bry & Boschan (1972) algorithm to the U.S. unemployment rate. The two chronologies are also shown in figure 1. Since the unemployment rate is highly cyclical, the two chronologies are quite similar but there are some notable differences. The BB algorithm selects one fewer recession than the NBER, namely, the BB algorithm identifies the double-dip recession of the early 1980s as a single, prolonged downturn. An NBER-defined expansion lasts from August 1980 to June 1981, and while the unemployment rate falls from 7.8 to 7.2 percent over this period, its behavior is erratic and does not display a distinct peak-trough pattern that the BB algorithm requires to identify a turning point.

The other important difference between the chronologies is that downturns produced by the BB algorithm are longer on average than NBER-defined recessions, especially for the recessions since 1990, the "jobless recoveries." According to the NBER, both the 1990-1991 and 2001 recessions had a duration of 9 months. However, the unemployment rate continues to increase following the NBER-defined troughs, so that the BB algorithm identifies recessions that last more than 30 months. For the entire sample, according to the NBER, only 15 percent of months are recessionary, whereas nearly one-third are within Bry-Boschan downturns. In contrast, complete cycles (peak-to-peak and trough-to-trough), are of roughly the same duration.

However, a higher incidence of recessions using the BB algorithm need not imply that the shape of the hazard rates will be different. Indeed, table 2 indicates that the estimated hazard rates of

[9] To see this, consider the hazard ratio of two vectors, x_1 and x_2, $\frac{\lambda(\tau,x_1)}{\lambda(\tau,x_2)}$ =exp$(\beta(x_1 - x_2))$. If the only difference between x_1 and x_2 is a one-unit change in variable k, then $\frac{\lambda(\tau,x_1)}{\lambda(\tau,x_2)} = \exp(\beta_k)$, and the percent change in the hazard is approximately $\exp(\beta_k) - 1$.

Table 1: Comparison of U.S. recession chronologies.

	N	Median	Std. dev.	Minimum	Maximum
Business Cycle Dating Committee					
Expansions	10	50	36	11	119
Recession	11	11	4	7	19
Peak-to-peak	10	65	37	18	128
Trough-to-trough	10	60	35	28	128
Bry-Boschan Restricted Dating Algorithm					
Expansions	9	46	31	10	101
Recession	10	26	10	16	44
Peak-to-peak	9	67	39	27	141
Trough-to-trough	9	76	37	34	132

Notes: Table compares NBER business cycle dating committee's chronology to the chronology produced by applying the Bry-Boschan algorithm to the U.S. unemployment rate, 1948–2015. Median, minimum and maximum denote the duration of business cycle phase, in months.

Table 2: Parameter estimates of baseline Weibull hazard model.

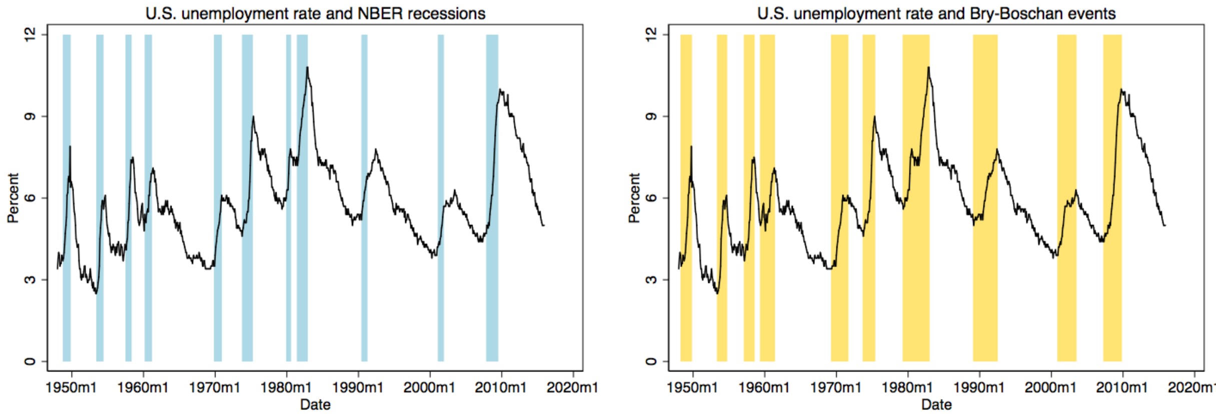
	Expansions		Recessions		Peak-to-peak		Trough-to-trough	
	NBER	BB	NBER	BB	NBER	BB	NBER	BB
α	1.54	1.51	1.70**	2.46**	2.15**	2.33**	2.18**	2.42**
	(0.42)	(0.45)	(0.32)	(0.40)	(0.50)	(0.47)	(0.41)	(0.51)
N phase	11	10	11	10	11	10	11	10
N months	607	484	67	208	740	752	728	734

Notes: Table compares Weibull estimates from post-WWII NBER-defined expansions and recessions to those from the Bry-Boschan (BB) algorithm, 1948–2015. Standard errors robust to heteroskedasticity in parentheses. Asterisks denote significance at the 10 (*) and 5 (**) percent levels of the null that $\alpha = 1$. See text for details.

the two chronologies are quite similar. Like Sichel (1991), we find the hazard rate for business cycle expansions is flat, in the sense that the null that the shape parameter (α) differs from one cannot be rejected. Although the hazard rate for recessions using the Bry-Boschan chronology is, if anything, steeper than the hazard rate from the NBER dates, the uncertainty around the parameter estimates for these models is quite large. The estimated hazard rates for complete business cycles are similar across the two chronologies.

On the whole, we view the evidence presented here as confirming that the Bry-Boschan algorithm produces accurate representations of the business cycle. We next turn to the application to state-level data.

Figure 1: Comparison of NBER and Bry-Boschan U.S. recession chronologies.



Notes: Both panels plot the time series of the U.S. unemployment rate. Blue bars in the left panel indicate NBER recessions. Gold bars in the right panel show recession dates produced by the Bry-Boschan algorithm. See text for details.

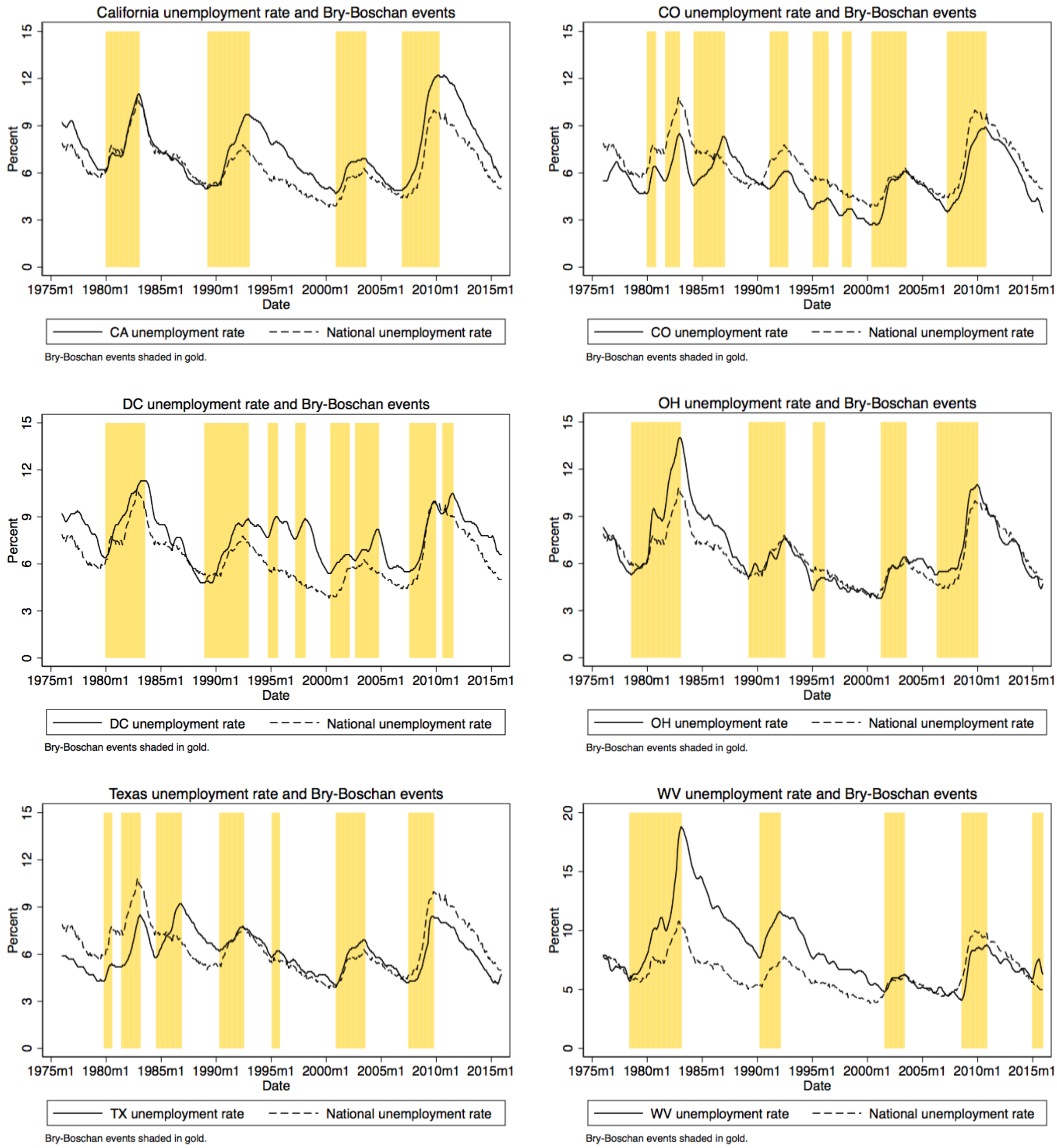
3.2 State-level analysis

Figure 2 shows the Bry-Boschan state-level recession chronologies for six states.[10] For comparison, we include the national unemployment rate in each plot. Unsurprisingly, state-level unemployment rates are highly correlated with the national unemployment rate. Nevertheless, there is a large degree of heterogeneity across states. Unemployment in California is highly correlated to the national unemployment rate, perhaps unsurprisingly so given that California has a large, diverse economy. However, other states in the figure are quite different. For example, Colorado and Texas experienced notable downturns in the mid-1980s, possibly related to a slump in the energy sector and commodity prices. The business cycles of these states have lower concordance indices with the national chronology: about 70 percent for Colorado, and 75 percent for Texas.

Tables 3 and 4 present summary statistics of state-level business cycle chronologies. One difference between the average state chronology (table 3) and the national chronology (table 1) is that the number of observations has increased notably. Beyond that, the summary statistics for the average length of business cycle phases are similar to those using the Bry-Boschan algorithm

[10] The Bureau of Labor Statistics (BLS) provides state-level unemployment rates at a monthly frequency. The data begin in January 1976, and our dataset continues until December 2015. Table A1 in the appendix provides summary statistics of state-level unemployment rates during this period. Our dataset includes the unemployment rate of the 50 U.S. states and Washington, D.C., and is available from the FRED database, `https:\\research.stlouisfed.org/pdl/337`.

Figure 2: State-level recession chronologies from Bry-Boschan algorithm.



Notes: State-level recession chronologies as determined by Bry-Boschan algorithm for California, Colorado, DC, Ohio, Texas, West Virginia. See text for details.

applied to the national unemployment rate; expansions are somewhat longer, on average, than NBER-defined recessions but complete cycles are of similar length. While the average state experienced six recessions between 1976 and 2015 with an average duration of 26 months (table 3), table

4 indicates a considerable degree of heterogeneity across states. Georgia and Louisiana experienced nine recessions during this period, while other states experienced as few as four. Other investigations of state-level business cycles also find notable heterogeneity of state business cycles (Owyang, Piger & Wall, 2005).

Table 3: Summary statistics of state-level business cycle chronologies.

	N. phases	Median	Minimum	Maximum
Expansions	247	42	6	179
Recession	292	26	8	66
Peak-to-peak	247	70	19	193
Trough-to-trough	241	69	18	238

Notes: Duration of completed state-level business cycles from Bry-Boschan algorithm. Median, minimum and maximum give the duration of that business cycle phase, in months. Sample period is 1976–2015.

An alternative measure of the heterogeneity of our state-level business cycle chronologies is the concordance measure of Harding & Pagan (2002).[11] When we calculate each state's concordance index relative to the NBER's chronology of U.S. business cycles, we find that the average state has an index value of about 0.70, indicating that the state's economy is in the same phase of the business cycle as the U.S. economy 70 percent of the time. Some states—Louisiana, Delaware, Nebraska and Wyoming—have values as low as 55 percent. California, Florida, Massachusetts, Pennsylvania and Virginia have the highest values, with index values between 80 and 85 percent. When we calculate the concordance index across state-pairs we again see notable heterogeneity. The minimum value is the Delaware–North Dakota pairing, which has a value of 50 percent. California, Pennsylvania, New Jersey and New York all obtain pairwise index values of about 90 percent.

3.3 State-level hazard rates

Columns 1-2 and 5-6 of tables 5 and 6 show baseline hazard rates for the four business cycle phases. These estimates are equivalent to those for the national economy in table 2, although for the state-level data we run two specifications, with and without state fixed effects.

Despite the maxim that 'expansions do not die of old age,' it is not uncommon to find pos-

[11] The concordance between two chronologies is calculated as $C_{ij} = \frac{1}{T}\sum_t S_{it} \times S_{jt} + (1-S_{it})(1-S_{jt})$, and measures the fraction of time two binary indicators agree with one another.

Table 4: Summary statistics for state-level recessions and expansions.

	Recessions					Expansions				
State	Count	Median	Std. dev.	Min.	Max	Count	Median	Std. dev.	Min.	Max
AK	8	21	12	8	40	7	37	18	7	61
AL	5	25	16	17	55	5	69	29	12	89
AR	4	27	13	14	39	4	66	47	47	147
AZ	6	21	7	11	30	6	54	34	10	101
CA	4	39	6	33	46	4	71	23	39	94
CO	8	19	13	10	43	8	25	19	10	62
CT	6	37	19	11	57	6	41	13	18	58
DC	8	24	14	11	48	8	24	21	6	65
DE	7	24	10	11	42	7	28	25	7	70
FL	4	41	11	22	46	4	68	20	49	96
GA	9	11	10	8	36	9	23	23	6	70
HI	6	26	13	10	44	6	42	37	7	101
IA	5	39	17	12	54	5	46	36	17	107
ID	6	29	5	22	37	6	44	33	6	92
IL	7	20	15	13	55	7	34	21	6	71
IN	5	25	8	21	40	5	70	46	7	126
KS	7	20	14	11	55	7	43	26	10	75
KY	6	20	21	12	66	6	37	38	14	107
LA	9	29	15	8	49	9	16	19	8	63
MA	4	33	9	26	47	4	67	24	52	108
MD	6	26	12	12	43	6	45	16	34	70
ME	5	29	7	22	40	5	69	41	8	115
MI	5	27	12	19	47	5	74	40	10	105
MN	5	26	14	19	55	5	74	35	11	93
MO	5	22	20	11	58	5	70	42	10	105
MS	6	20	14	11	47	6	37	37	19	115
MT	6	19	14	14	46	6	42	24	19	84
NC	7	23	10	11	36	6	47	25	11	83
ND	7	18	5	12	26	7	54	27	12	79
NE	6	27	20	16	65	6	36	21	19	74
NH	4	31	16	19	52	4	71	25	55	111
NJ	6	25	14	10	41	6	47	37	8	92
NM	8	25	8	12	36	7	44	19	8	59
NV	4	48	10	38	57	4	63	16	48	87
NY	5	35	16	13	53	5	43	32	16	101
OH	5	40	16	13	54	5	61	21	30	74
OK	8	17	9	8	32	7	35	25	10	82
OR	6	25	10	11	39	6	42	29	9	86
PA	4	38	5	36	46	4	71	19	48	93
RI	4	42	8	31	48	4	71	21	44	95
SC	7	32	14	14	53	7	21	24	7	73
SD	5	23	18	14	59	5	33	70	7	179
TN	7	24	14	8	47	7	36	30	7	78
TX	7	27	9	9	32	7	41	23	10	74
UT	7	17	17	9	55	6	48	17	18	64
VA	5	34	11	14	38	5	59	31	18	102
VT	5	37	14	14	48	5	63	40	6	105
WA	4	36	16	30	64	4	67	19	46	92
WI	4	33	18	11	49	4	80	19	56	98
WV	5	23	17	12	57	4	74	28	49	113
WY	6	21	9	12	35	6	50	39	6	114

Notes: Table shows characteristics of completed state-level business cycle phases from Bry-Boschan algorithm, January 1976–December 2015. Median, standard deviation, minimum and maximum indicate phase duration in months. See text for details.

itive duration dependence in economic expansions, especially in post World War II data.[12] Our national-level estimates indicated some degree of positive duration dependence but lacked statistical significance. The evidence presented in table 5 confirms the positive duration dependence of business cycles expansions. The baseline state-level estimates are similar to the estimates from national data. For expansions, the shape parameter is smaller in absolute value, around 1.3, but much more precisely estimated. The null hypothesis of no duration dependence ($\alpha = 1$) is strongly rejected in favor of positive duration dependence. An estimated value of 1.3 implies that an expansion of 120 months is about 35 percent more likely to end than an expansion of median duration, 42 months. The estimated probability that the expansion will end after 120 months is about 4.4 percent, while the estimated probability that an expansion ends after 42 months is about 3.2 percent.[13]

An important advantage of having a panel of business cycle chronologies is the ability to control for factors that may impact business cycle hazard rates. We control for a number of state-specific socioeconomic factors that may influence the length of unemployment spells and the hazard rate: education level is measured by the share of the population with a bachelor's degree or more; local labor market flexibility is measured using a dummy variable that indicates whether a state has enacted right-to-work legislation. We also include the average firm size within each state since large and small firms may have differing employment responses to economic shocks. Since the housing market is known to be an important barometer of household balance sheets and therefore the business cycle, we control for state-level housing conditions with house price indexes from the Federal Housing Finance Agency, as well as regional 30-year mortgage interest rates published by Freddie Mac. To adjust for the economic conditions of nearby geographic areas, we include a dummy variable that indicates whether a state within the same census division is currently in recession. We also include measures of financial conditions prevalent at the national level. Since oil price changes may be an important driver of the business cycle, we adjust for the net oil price increase (Hamilton 1996). We also include in the regressions variables that have been identified as promising recession indicators: the slope of the yield curve, lagged by 12 months; recent equity price index changes (measured by the Wilshire 5000); realized volatility; and the level of the federal

[12] See, for example: Kim & Nelson (1998), Filardo & Gordon (1998), Zuehlke (2003) and Castro (2010).

[13] The Weibull parametric assumption implies non-linear time dependence. However, comparing the duration of an expansion of 120 months to one of 42 months: $\left(\frac{120}{42}\right)^{\alpha-1} \approx 1.37$. The predicted probability of failure after 120 months is calculated $\alpha\lambda(x)120^{\alpha-1} \approx 4$ percent, assuming that the elements of x are at their mean.

funds rate. See table A2 for details on data construction and sources.

The results for business cycle expansions when we include state-specific controls are in columns 3 of table 5; column 4 adds national controls as well. The estimate of the shape parameter α is robust across all regressions. Of state-specific controls, both the average firm size and the growth rate of housing prices enter the regression in a statistically significant manner: on average, expansions that occur in states with larger firms and that concur with high 30-year interest rates are more likely to end. As in Francis et al. (2018), we find that the state of the business cycle in nearby states importantly affects the probability of switching from one economic business cycle phase to another. The hazard rate for an economic expansion of a state nearly triples when a state in the same census division is currently in recession. Conversely, if a state is currently in recession, and a nearby state is expanding instead, the hazard of the recession ending increases (table 6). These strong effects reflect the close economic ties of local economies. Expansions that are concurrent to increases in local house prices are less likely to end, possibly reflecting momentum in local housing markets that affects household spending patterns. Of national controls, the yield curve and level of the federal funds rates are statistically significant at the 95 percent level. Net oil increase is statistically significant at the 90 percent level, and its sign is as expected.

For recessions, table 6, the estimated hazard is steeper, with α estimated to be about 1.75 across all regression specifications. This value implies that a recession of 60 months is twice as likely to end relative to a recession of duration 24 months (roughly 14 percent versus 7 percent). Interestingly, a number of state-specific controls affect the probability of recessions ending in a statistically significant manner. States that have enacted right-to-work legislation, a proxy for a flexible labor markets, have recessions that shorter average duration. This result suggests that states with flexible labor markets may allow the business cycle to adjust more quickly, although we note that we do not know whether the severity of the downturns differ across these states. Finally, we find that housing prices, mortgage rates, realized volatility, and the slope of the yield curve affect the probability of recessions ending. As an example, when the standard deviation of daily returns within a month increases by 1 percentage point—a two standard deviation increase in volatility—recessions are 68 percent less likely to end.

A natural question is whether the initial conditions of a phase have a significant impact on that phase's subsequent duration. We may expect that an expansion that follows a particularly severe

Table 5: Estimated hazard rates for expansions and peak-to-peak cycles.

	Expansions				Peak-to-peak			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education			0.78	1.03			1.33	1.78
			(0.37)	(0.42)			(0.52)	(0.91)
Right-to-work			0.52	0.67			1.88	0.80
			(0.44)	(0.62)			(0.19)	(0.82)
Avg firm size			1.67**	1.29**			1.03	1.39**
			(0.21)	(0.20)			(0.22)	(0.19)
30-yr mortgage			1.32**	0.95			1.10**	0.88
			(0.08)	(0.10)			(0.03)	(0.08)
Housing price			0.94*	0.96*			1.01	0.98
			(0.02)	(0.02)			(0.01)	(0.02)
Nearby state in recession			3.18**	2.46**			2.39**	2.09**
			(0.66)	(0.60)			(0.45)	(0.42)
Net oil increase				1.12*				1.12*
				(0.08)				(0.07)
Wilshire 5000				0.87				0.90
				(0.33)				(0.25)
Realized volatility				1.19				1.09
				(0.24)				(0.20)
Slope of yield curve				0.76**				0.88*
				(0.07)				(0.07)
Federal funds rate				1.35**				1.36**
				(0.11)				(0.07)
α	1.21**	1.34**	1.37**	1.34**	1.91**	2.20**	2.47**	2.52**
	(0.06)	(0.08)	(0.12)	(0.14)	(0.08)	(0.10)	(0.14)	(0.14)
State fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
N phase	286	286	232	231	298	298	288	287
N mo.	12,504	12,504	7,866	7,863	20,699	20,699	14,079	13,913
AIC	802.2	572.8	452.7	404.1	580.0	510.0	256.1	218.4

Notes: Estimated hazard rates for state-level expansions and peak-to-peak cycles, 1976–2015. Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test the null that they differ from one. See text for details.

recession may have a longer duration, all else equal, since the local economy has more 'room to run.' Conversely, recessions may be longer-lasting when they follow particularly tight labor conditions, perhaps because factors of production become inefficiently utilized when labor markets are tight. In contrast, if recessions are truly exogenous, random events, then the hazard should not depend on the previous phase.

To explore the effects of previous phases, we add two covariates to the analysis from the previous

Table 6: Estimated hazard rates for recessions and trough-to-trough cycles.

	Recessions				Trough-to-trough			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education			3.81**	3.16**			1.65	1.12
			(1.77)	(1.61)			(0.74)	(0.66)
Right-to-work			2.93**	3.77**			3.45	2.88
			(0.51)	(0.61)			(8.58)	(3.46)
Avg firm size			0.75**	0.83**			0.48**	0.66**
			(0.05)	(0.07)			(0.07)	(0.10)
30-yr mortgage			0.92**	0.93**			0.83**	0.98
			(0.03)	(0.03)			(0.06)	(0.06)
Housing price			1.02	1.03**			0.95**	0.95**
			(0.02)	(0.02)			(0.02)	(0.02)
Nearby state in expansion			4.91**	3.66**			2.28**	1.96**
			(0.99)	(0.76)			(0.61)	(0.60)
Net oil increase				0.74*				0.84
				(0.12)				(0.14)
Wilshire 5000				1.17				1.07
				(0.29)				(0.25)
Realized volatility				0.32**				0.46**
				(0.05)				(0.08)
Slope of yield curve				1.24**				1.25**
				(0.08)				(0.10)
Federal funds rate				1.21*				0.53**
				(0.13)				(0.21)
α	1.68**	1.86**	1.81**	1.66**	2.00**	2.32**	2.73**	2.57**
	(0.08)	(0.10)	(0.12)	(0.10)	(0.10)	(0.12)	(0.21)	(0.20)
State fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
N phase	298	298	276	273	292	292	244	244
N mo.	6,443	6,443	4,882	4,738	19,061	19,061	12,912	12,909
AIC	632.1	574.9	436.4	350.3	517.0	449.4	226.7	146.6

Notes: Estimated hazard rates for state-level expansions and peak-to-peak cycles, 1976–2015. Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test the null that they differ from one. See text for details.

section: the change in the unemployment rate in the previous phase and the previous phase's duration. The results are presented in table 7. Expansions that follow recessions with steep increases in the unemployment rate are longer, all else equal. Specifically, the regression coefficient on the previous phase's increase in the unemployment rate is about 0.8—for every 1 percentage point increase in the previous phase's unemployment rate change lowers the expansion's hazard rate by 20 percent. Expansions that follow long-lasting recessions are shorter, all else equal, although the effect is very small. The impact of the previous phase on the duration of recessions and trough-to-trough cycles is less clear cut. The duration of recessions does not depend on the previous phase, although trough-to-trough cycles that follow large increases in the unemployment rate are somewhat longer, on average.

Table 7: Estimated effects of previous phase on hazard rate.

	Expansions		Peak-to-peak		Recessions		Trough-to-trough	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δu_{i-1}	0.75**	0.85**	0.75**	0.90	0.96	1.07	0.85**	0.78**
	(0.03)	(0.07)	(0.04)	(0.07)	(0.04)	(0.08)	(0.04)	(0.05)
τ_{i-1}	1.02**	1.02**	1.02**	1.02**	0.99**	0.99	1.01**	1.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
α	1.40**	1.38**	2.46**	2.47**	2.02**	1.81**	1.77**	2.18**
	(0.08)	(0.12)	(0.14)	(0.17)	(0.11)	(0.12)	(0.10)	(0.16)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls?	No	Yes	No	Yes	No	Yes	No	Yes
N phase	286	231	298	287	247	225	292	244
N obs.	12,504	7,863	20,699	13,913	5,075	3,885	19,061	12,909
AIC	720.0	353.5	476.5	194.5	440.9	231.0	406.9	111.8

Notes: Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test the null that they differ from one. 'Other controls?' line indicates whether regression includes all controls from tables 5 and 6. Sample period 1976–2015. See text for details.

4 Monetary policy as a risk factor

Finally, we consider the effect of monetary policy shocks on the hazard function for expansions and recessions.[14] Our measure of monetary policy shocks are those developed by Romer & Romer (2004). Romer & Romer estimate monetary policy shocks in two steps. First, they identify the intended change in the federal funds rate using the narrative records of the Federal Open Market Committee (FOMC). They then regress the intended change of the policy rate onto the Federal Reserve's Greenbook forecasts; deviations from the predicted values of this regressions are the policy shocks. We have extended the series through 2010.[15] One of the advantages of using Romer &

[14] Francis et al. (2018) study the effects of fiscal and monetary policy on the duration of recession. They measure the phase-specific monetary policy response as the cumulative change in the policy rate between the first and last month of each recession.

[15] We approximate the intended change in the federal funds rate for the zero bound period; see, for example, the December 12 2008 memoranda to the Federal Open Market Committee "Notes on issues related to the zero lower

Romer shocks is that they are available from the start of our sample, although Greenbook forecasts are released with a five year lag, limiting the more recent period. Summary statistics for the measure of monetary policy shocks are presented in table 8.

Table 8: Summary statistics of Romer-Romer monetary policy shocks.

	Romer-Romer
Mean	0.00
Std dev.	0.34
N.obs	462
Period	Feb 1972–Dec 2010

Notes: See text for details.

Tables 9–10 present the results.[16] Because the effects of monetary policy shocks on hazard rates are very likely asymmetric, for each phase we run five specifications. The first includes only the linear effect of the monetary policy shock, while the next two (columns 2 and 3 of the tables) interact the measure of monetary policy with a dummy that indicates the sign of the change. All regressions include state-level fixed effects and the controls presented in the previous section. In expansions, we expect contractionary monetary policy shocks will increase the hazard rate, while expansionary shocks lower it. In recessions we expect contractionary shocks to prolong the recession, all else equal, so the hazard rate ought to decline. Another asymmetry identified by the literature is the differential effect of small versus large shocks. Columns 4 and 5 of the tables interact the monetary policy shocks with a dummy variable that identifies when the shocks are below or above 1 standard deviation.

The baseline estimate for the Romer-Romer shocks is not statistically different from 1 during expansions (table 9). However, columns 2-5 show important nonlinear effects. Contractionary monetary policy shocks in expansions have a large upward effect on the hazard rate. A one standard deviation unexpected contractionary shock during an expansion shifts the hazard rate up by about 14 percent. Expansionary shocks in expansions also have a significant effect: a one standard deviation shock shifts the hazard down by about 20 percent.

There is some heterogeneity in the effects of small versus large shocks, albeit not statistically

bound on nominal interest rates," available at `https://www.federalreserve.gov/monetarypolicy/fomc-memos.htm`.

[16] Results for peak-to-peak and trough-to-trough cycles are not discussed here in the interest of concision, but can be found in the Appendix.

Table 9: Effects of monetary policy shocks to hazard rates in expansions.

	(1)	(2)	(3)	(4)	(5)
Romer-Romer shock	0.98	0.40**	1.44	0.42**	1.43
	(0.29)	(0.15)	(0.47)	(0.16)	(0.47)
$RR \times I(RR > 0)$		3.56**		1.79	
		(1.94)		(1.77)	
$RR \times I(RR < 0)$			0.28**		0.59
			(0.15)		(0.72)
$RR \times I(RR > \sigma)$				2.04	
				(2.00)	
$RR \times I(RR < -\sigma)$					0.42
					(0.48)
α	1.34**	1.36**	1.36**	1.36**	1.37**
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes
N. phase	232	232	232	232	232
N. obs	7,863	7,863	7,863	7,863	7,863
AIC	359.3	358.3	358.3	357.9	357.9

Notes: Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test the null that they differ from one. σ denotes the standard deviation of the Romer-Romer shocks. Regression includes all controls from tables 5 and 6. Sample period 1976–2010. See text for details.

significant. The marginal effect of a small contractionary shock in expansions is to shift the hazard rate down by about 8 percent, whereas the marginal effect of a large shock is to shift up the hazard by 18 percent. On net, the large shocks dominate the estimate of the average effect. One reason why small contractionary shocks lower the hazard rate in expansions is that agents perceive this information as a signal that economy is performing better than expected (Ellingsen & Soderstrom (2001), Melosi (2017), and Nakamura & Steinsson (2018)). Expansionary shocks in expansions have also an asymmetric effect, large expansionary shocks move the hazard rate down relatively more than small shocks.

Results for recessions are presented in table 10. Focusing on the specification where the Romer-Romer shock affects the hazard linearly, we see that the marginal effect of a monetary policy shock has the 'wrong' sign: a contractionary monetary policy shock is estimated to *increase* the hazard rate; that is, a contractionary shock makes the recession more likely to end. When we allow for the sign of the shock to have a differential impact on the hazard, we still find that both expansionary and contractionary shocks increase the likelihood to exit a recession, although the effect of contractionary shocks is attenuated. A one standard deviation contractionary shock increases the hazard rate by 31 percent. Although it is counterintuitive that a contractionary shock during recession raises the hazard, the signaling channel of monetary policy could account for this effect. In a recession, a contractionary shock may signal that the economy is performing better than expected. An expansionary shock of the same magnitude increases the hazard by about 240 percent. To put this in perspective, consider that the estimated hazard for a median duration recession is 15 percent. If in the next month there is a one standard deviation expansionary monetary policy shock, the hazard would move to about 35 percent.

The distinction between small and large shocks is particularly interesting during recessions, where we observe that small shocks are especially powerful. Small contractionary shocks are about 12 times as powerful as large contractionary shocks for the same sized shock. As an example, a contractionary shock of 17 basis points (one-half a standard deviation) moves the estimated hazard for a median duration recession from 15 percent to 46 percent. A shock of 68 basis points (two standard deviations) moves the hazard from 15 to 24 percent. The same holds true for expansionary shocks: an expansionary shock of 17 basis points moves the hazard from 15 to 71 percent, whereas a shock of 68 basis points moves the hazard to 44 percent. These differences are statistically significant and highlight a new sensitivity of the economy to monetary policy shocks.

During a recession, the signaling effect from monetary policy shocks always acts in the opposite direction of the policy shock itself. That is to say, whereas we would expect a contractionary shock during a recession to lower the hazard, a contractionary shock could communicate that the economy is doing better than expected. This signaling effect offsets and may even dominate the effect of higher interest rates per se. Indeed, the results suggest that for both small and large shocks this signaling effect prevails, especially for small shocks. An analogous intuition applies for expansionary shocks, although the effects of lower interest rates themselves dominate. However, the signaling effect is responsible for the difference between the small and large shocks, where the signaling effect is stronger for large shocks. Large expansionary shocks during a recession carry particularly strong signals about (deteriorating) future economic activity, attenuating the direct effect of lower interest rates.

These results give us a new perspective on the asymmetric effects of monetary policy. Previ-

Table 10: Effect of monetary policy shocks to hazard rates in recession.

	(1)	(2)	(3)	(4)	(5)
Romer-Romer shock	2.60**	8.27**	1.92*	6.39**	1.94*
	(0.75)	(6.55)	(0.64)	(5.32)	(0.65)
$RR \times I(RR > 0)$		0.23*		2.06	
		(0.19)		(3.22)	
$RR \times I(RR < 0)$			4.31*		11.75**
			(3.51)		(11.03)
$RR \times I(RR > \sigma)$				0.14*	
				(0.14)	
$RR \times I(RR < -\sigma)$					0.17*
					(0.14)
α	1.65**	1.67**	1.67**	1.68**	1.67**
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes
N. phase	287	287	287	287	287
N. obs	4,811	4,811	4,811	2,411	2,411
AIC	329.9	327.6	327.6	325.7	327.1

Notes: Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test whether they differ from one. σ denotes the standard deviation of the Romer-Romer shocks. Regression includes all controls from tables 5 and 6. Sample period 1976–2010. See text for details.

ously, asymmetries have been studied using the aggregate data and mostly looking at the effects of monetary policy on output and inflation. These results provide a richer perspective of these asymmetries, due to the use of state-level data and that we study the effects on the likelihood to exit a particular stage of the business cycles. The analysis identified several dimensions of asymmetries for the monetary policy effects, including significant differences in the effects of expansionary and contractionary shocks during expansions and recessions and significant differences in the effects of small and large shocks in recessions.

5 Conclusion

In this paper we show that the analysis of business cycle duration can be enriched by extending it to the state-level data. This allows us to more precisely estimate the duration dependence of U.S. business cycles. We indeed find positive duration dependence for all stages of the business cycles. In addition, we are able to include covariates in our regressions to examine more closely what influences the likelihood of exiting a particular stage of the business cycle. Our results indicate that national-level macroeconomic and financial variables, state-level business and institutional characteristics, and the state of the business cycle in neighboring states significantly affect the hazard rate. We also evaluated monetary policy shocks as a risk factor for transitions from one stage of the business cycle to another. We find that policy has highly asymmetric effects on phase duration, depending on the stage of the business cycle and the direction and size of the shock.

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Table A1: Summary statistics of state-level unemployment rate data, January 1976 – December 2015.

State	N obs	Mean	Std dev	Min	Max
AK	480	8.0	1.4	6.3	11.2
AL	480	7.3	2.5	3.8	15.5
AR	480	6.7	1.5	4.2	10.3
AZ	480	6.4	1.8	3.7	11.5
CA	480	7.4	1.9	4.7	12.2
CO	480	5.5	1.5	2.7	8.9
CT	480	5.6	1.8	2.2	10.0
DC	480	7.8	1.5	4.8	11.3
DE	480	5.5	1.8	3.0	9.8
FL	480	6.3	1.9	3.1	11.2
GA	480	6.1	1.7	3.4	10.5
HI	480	5.0	1.6	2.4	10.4
IA	480	4.7	1.5	2.4	9.1
ID	480	6.1	1.6	2.9	10.2
IL	480	7.0	2.0	4.1	13.1
IN	480	6.3	2.3	2.9	12.6
KS	480	4.7	0.9	2.9	7.3
KY	480	6.9	2.0	4.0	12.1
LA	480	7.4	2.1	3.9	13.1
MA	480	5.6	1.7	2.6	10.7
MD	480	5.4	1.3	3.3	8.5
ME	480	6.0	1.6	3.2	9.0
MI	480	8.1	2.9	3.2	16.5
MN	480	4.9	1.3	2.5	8.9
MO	480	6.1	1.6	3.1	10.6
MS	480	7.7	1.9	5.0	12.8
MT	480	5.9	1.3	2.9	8.8
NC	480	5.9	2.0	3.0	11.3
ND	480	3.9	0.9	2.5	6.2
NE	480	3.6	0.9	2.3	6.3
NH	480	4.4	1.4	2.2	7.4
NJ	480	6.4	1.9	3.5	10.7
NM	480	6.8	1.4	3.7	10.5
NV	480	6.6	2.5	3.7	13.7
NY	480	6.7	1.5	4.0	10.4
OH	480	6.8	2.2	3.8	14.0
OK	480	5.2	1.4	2.9	8.9
OR	480	7.3	1.9	4.7	11.9
PA	480	6.5	1.8	4.0	12.7
RI	480	6.6	2.2	2.9	11.3
SC	480	6.7	2.0	3.5	11.8
SD	480	3.7	0.8	2.4	5.9
TN	480	6.6	2.0	3.7	12.9
TX	480	6.1	1.3	4.0	9.2
UT	480	5.0	1.5	2.3	9.6
VA	480	4.8	1.3	2.1	7.9
VT	480	4.8	1.3	2.6	8.8
WA	480	7.1	1.8	4.6	12.2
WI	480	5.7	1.8	3.0	11.9
WV	480	8.3	3.0	4.1	18.8
WY	480	4.9	1.5	2.5	9.4

Table A2: Data and sources.

Description	Transformation	Period	Freq	Source
Business cycle chronologies				
State unemployment rate	Bry-Boschan	1976–2015	Monthly	BLS
State coincidence index	Bry-Boschan	1976–2015	Monthly	BLS
State controls				
% pop. w/BA or more	–	1977-2013	Annual	BEA
Right-to-work legislation	Dummy variable	1977–2014	Monthly	Collins (2014)
Average firm size	–	1977-2011	Annual	Census
30-yr mortgage interest rate	–	1977-2010	Monthly	Freddie Mac
State housing index	12-mo log diff (× 100)	1978-2015	Monthly	FHFA
National macroeconomic indicators				
National unemployment rate	–	1976–2015	Monthly	BLS
Net oil price increase	WTI	1976–2015	Monthly	US EIA; Hamilton (1996)
Wilshire 5000 total index	12-mo log diff (×100)	1976–2015	Monthly	FRED
Realized volatility of Wil 5000	S.D. of daily return	1980–2015	Monthly	FRED
Federal Funds rate	–	1976–2015	Monthly	FRB H.15
3-month Treasury yield	–	1976–2015	Monthly	FRB H.15
10-year Treasury yield	–	1976–2015	Monthly	FRB H.15
Monetary policy shocks				
Romer-Romer	–	1977–2010	Monthly	R&R (2004); author's calc.

Table A3: Effects of monetary policy shocks to hazard rates of peak-to-peak business cycles.

	(1)	(2)	(3)	(4)	(5)
Romer-Romer shock	1.06	1.01	1.10	1.02	1.10
	(0.22)	(0.21)	(0.34)	(0.21)	(0.34)
$RR \times I(RR > 0)$		1.09		0.53	
		(0.42)		(0.47)	
$RR \times I(RR < 0)$			0.91		1.04
			(0.35)		(1.25)
$RR \times I(RR > 0) \times I(RR > \sigma)$				2.18	
				(1.97)	
$RR \times I(RR < 0) \times I(RR < \sigma)$					0.88
					(1.01)
α	2.59**	2.60**	2.60**	2.60**	2.60**
	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes
N. phase	238	238	238	238	238
N. obs	13,913	13,913	13,913	13,913	13,913
AIC	200.4	202.4	202.4	203.8	204.3

Notes: Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test the null that they differ from one. Regression includes all controls from tables 5 and 6. Sample period 1976–2010. See text for details.

Table A4: Effects of monetary policy shocks to hazard rates of trough-to-trough business cycles.

	(1)	(2)	(3)	(4)	(5)
Romer-Romer shock	2.84**	6.63**	1.71	6.48*	1.70
	(1.06)	(6.32)	(1.06)	(6.38)	(1.06)
$RR \times I(RR > 0)$		0.26		0.32	
		(0.33)		(0.57)	
$RR \times I(RR < 0)$			3.88		4.00
			(5.02)		(5.35)
$RR \times I(RR > 0) \times I(RR > \sigma)$				0.79	
				(1.08)	
$RR \times I(RR < 0) \times I(RR < \sigma)$					0.93
					(1.46)
α	2.57**	2.56**	2.56**	2.56**	2.56**
	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Other controls?	Yes	Yes	Yes	Yes	Yes
N. phase	238	238	238	238	238
N. obs	12,909	12,909	12,909	12,909	12,909
AIC	104.3	105.1	105.12	107.1	107.2

Notes: Standard errors clustered on state in parentheses. Asterisks denote statistical significance at 90 (*) and 95 (**) percent confidence level. β coefficients exponentiated and significance stars test whether they differ from one. Regression includes all controls from tables 5 and 6. Sample period 1976–2010. See text for details.