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The Emergence of a Uniform Business Cycle in the United States: Evidence from New Claims-Based Unemployment Data*

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Abstract

Using newly digitized unemployment insurance claims data we construct historical monthly unemployment series for U.S. states going back to January 1947. We validate our series, showing that they are highly correlated with the Bureau of Labor Statistics' state-level unemployment data, which are only available since January 1976, and capture consistent business cycle dynamics. We use our claims-based unemployment rates to study the post-war evolution of labor market adjustments to local demand shocks and state unemployment fluctuations around national recessions. We document 1) a trend decrease in the dispersion of relative employment growth and unemployment across states; 2) a marked attenuation of relative employment and relative population responses to state-specific demand shocks, whereas relative unemployment responses are more stable; and 3) a convergence across states in both the speed and degree to which unemployment recovers after recessions. These trends show the emergence of a national business cycle experienced more uniformly across U.S. states, particularly since the 1960s. Convergence in states' industrial composition helps explain why a more uniform business cycle emerged when it did. And states' increasingly similar experience in recessions helps explain why interstate migration became less of an important adjustment mechanism.

Keywords: State-Level Unemployment Rates, Unemployment Insurance, Business Cycles, Economic Recoveries.

JEL Codes: C82, E24, E32, J64, J65, R11

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Macroeconomists are increasingly leveraging panel datasets and regional heterogeneity to identify economic relationships.[1] There is also an increasing awareness that the unemployment rate is one of the best indicators of economic slack, particularly for business cycle analysis (Romer and Romer, 2019). Unfortunately, official unemployment rate data for U.S. states are only available from the Bureau of Labor Statistics (BLS) starting in 1976, greatly hampering historical state-level analyses. For instance, a rich literature on state-level labor market recoveries, regional business cycles, and state coincident economic indexes has largely been limited to starting around 1976.[2]

In this paper we present a newly developed monthly unemployment dataset for U.S. states that greatly expands the time horizon for work with state-level panel data. Our novel unemployment series are constructed from a large dataset of newly digitized monthly unemployment insurance (UI) claims, pieced together from various historical reports published by the Department of Labor (DOL) and Social Security Administration (SSA). Together with available monthly data on nonfarm payroll employment we compute an alternative claims-based unemployment rate that can be consistently constructed for U.S. states from January 1947 to present day. We validate our new dataset by showing that our claims-based unemployment rates are highly correlated with official measures of unemployment, both state and national, in available overlapping samples.[3] We also use our claims-based unemployment rates to identify post-war peaks and troughs in state and national business cycles, and document that our new measures capture consistent business cycle patterns as official measures of unemployment, such as inflection points and amplitude dynamics.

We revisit the classic question of how labor markets respond to local demand shocks, using our longer historical sample of claims-based unemployment rates to study how these responses have evolved over the full post-war sample. Building on Blanchard and Katz (1992) and Dao, Furceri, and Loungani (2017), among others, we estimate relative employment, population, and unemployment responses to a relative industry mix Bartik (1991) instrument in a panel local projections-instrumental variable (LP-IV) framework over the full post-war sample and subsamples. Our analysis shows that the response of relative population, proxying for interstate migration, has greatly diminished since the start of the Great Moderation. Our results are more in line with those of Dao, Furceri, and Loungani (2017) than Blanchard and Katz (1992), who found that population bore the brunt of local labor market adjustments. Similarly, we find that the response of relative employment has diminished and become far less persistent in recent decades. Conversely, the peak

[1]For instance, Nakamura and Steinsson (2014) and Chodorow-Reich (2019) exploit regional heterogeneity to identify cross-sectional fiscal multipliers and Hazell, Herreño, Nakamura, and Steinsson (2022) exploit regional heterogeneity to study the slope of the Phillips curve. See Glandon, Kuttner, Mazumder, and Stroup (2023) for an overview of the recent shift in empirical macro toward panel data and microdata.

[2]See, e.g., Blanchard and Katz (1992); Crone and Clayton-Matthews (2005); Owyang, Piger, and Wall (2005); Brown (2017), Dao, Furceri, and Loungani (2017), and Tasci and Zevanove (2019).

[3]We use "official measures of unemployment" to refer to data that are produced and presently made available by federal statistical agencies. We discuss historical data availability in Data Appendix A.1.

response of unemployment has been fairly stable, but unemployment rises more gradually and remains elevated for longer in more recent decades. The attenuation of relative employment and population responses coupled with the roughly unchanged peak response of relative unemployment suggests that labor force participation has become more of an important adjustment margin in recent decades. Lastly, we document that larger Bartik shocks drive these responses, but these larger relative shocks are less frequent and smaller in magnitude in more recent decades, which helps explain the attenuation of relative population responses.

In addition to studying local labor market responses to demand shocks, we also use our novel dataset to study patterns between state and national business cycles to better understand their co-evolution. We use our claims-based unemployment series to study the evolving pace and nature of labor market recoveries following all postwar U.S. recessions. Our analysis of unemployment recoveries follows the recent work of Hall and Kudlyak (2020), but does so at the state level, which was previously precluded by data limitations. Hall and Kudlyak (2020) document that recoveries in the U.S. unemployment rate have been quite stable since the early 1960s, but the pace of recovery has decelerated markedly since the recoveries from earlier post-war recessions.[4] We corroborate this stylized fact with our new dataset, and find that the faster, early post-war recoveries are associated with greater heterogeneity in recovery rates across states, whereas states tend to experience more uniform recovery rates in more recent, slower national recoveries. We show that this deceleration and convergence in states' recovery rates is robust to indexing to state-specific business cycle troughs around national business cycle troughs. We also document a convergence across states in the degree to which unemployment recovers after recessions since the late 1970s.

The evidence from our historical claims-based unemployment rates points toward the emergence of a national business cycle experienced more uniformly across U.S. states, particularly since the 1960s–70s. We show that the industrial composition of states' economies became increasingly similar to one another—with much of this convergence transpiring in the 1940s–60s—which helps explain why a more uniform business cycle emerged across states when it did. States' increasingly common experience in recessions and recoveries, in turn, helps explain why interstate migration is becoming less of an important margin for adjustment following local demand shocks.

Section I. Dataset Construction

In this section we first overview the digitization and data cleaning process for historical state-level unemployment insurance claims. We discuss the construction of a novel claims-based unemployment series from this newly digitized data. To validate our dataset, we analyze the relation-

[4] Hall and Kudlyak (2020) find that, on average, the U.S. unemployment rate falls by 0.1 log points—or one-tenth of the peak unemployment rate—per year after recovery begins, until this relatively stable recovery rate is upended by the next recession, consistent with "plucking models" of the business cycle (Dupraz, Nakamura, and Steinsson, 2023).

ship between our claims-based unemployment series and official unemployment measures during available overlapping samples. Lastly, we model and present an alternative “fitted" claims-based unemployment series, which some practitioners might find more appropriate for their purposes.

Section I.A. Digitizing Historical Unemployment Claims

Monthly state-level UI claims are presently available in digital form dating back to January 1971 from the Department of Labor’s website; see Data Appendix A.1. Using scanned versions of printed reports previously published by the DOL and SSA, we backdated the publicly available data by digitizing monthly data on Initial Claims (IC) and Continued Claims (CC) back to December 1946 for all 50 states and the District of Columbia.[5] The historical claims data originate from one of a series of periodical reports: *Employment Security Activities*, *The Labor Market and Employment Security*, *Unemployment Insurance Statistics*, and the *Unemployment Insurance Review*. We were able to access most of these primary sources via HathiTrust or Google Books, and supplemented missing publications with Interlibrary Loan requests or scans from the Department of Labor’s internal library. We were almost always able to track down high-quality scans that were easily legible, but used data on changes in claims to guide digitization when merited, and we always used reported data on national aggregates as a crosscheck with the sum of state claims; see Data Appendix A.2 for details. In total, just over 36,000 monthly observations were digitized.

Newly digitized UI claims data were merged with the DOL’s publicly available state-level IC and CC data for regular state programs only, to be consistent with the historical claims data. After merging the series, we seasonally adjusted the full backdated IC and CC series using the Census Bureau’s Win X-13 seasonal adjustment program. We also used Win X-13 to run a series of outlier tests, which identified roughly 200 potential outliers from roughly 91,000 observations (newly digitized and existing data combined). We manually checked each potential outlier to assess whether it represented a legitimate change in claims (e.g., a surge in Louisiana following Hurricane Katrina) or a “fat thumb” data coding issue (e.g., an implausible spike in Missouri exceeding the state’s population); see Appendix A.2 for details on data cleaning and seasonal adjustment.

The monthly claims data we digitized is aggregated by the DOL from weekly claims data collected by state UI offices, and we first convert these monthly claims to average weekly claims; this approach mimics the DOL’s conversion of weekly data to average weekly data for calculating insured unemployment in a given month.[6]

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[5]The sample start is chosen so a three-month centered moving average of claims is available back to January 1947.

[6]In keeping with the DOL data for average weekly insured unemployment in a given month, monthly data are weighted by the split number of five-day workweeks in the month. We calculate the weights as the sum of workdays in each given month divided by five days for the workweek, ignoring the distinction of holidays.

Section I.B. Claims-Based Unemployment Rates for U.S. States

Using these UI claims data, we construct monthly claims-based unemployment rates for all 50 states and the District of Columbia. Our claims-based unemployment rate draws conceptually on both the official unemployment rate estimated by the BLS—the ratio of unemployed workers to the labor force—as well as the insured unemployment rate (IUR) produced by the DOL Employment and Training Administration (ETA)—the ratio of average weekly continued claims divided by covered employment, i.e., workers eligible for state or federal unemployment programs. We use initial and continued claims as an alternate measure of unemployed workers (the subset receiving regular state benefits) and measure this as a ratio to employed workers plus these UI claimants, a related proxy for the labor force influenced by data limitations. Specifically our claims-based unemployment rate for state i in month t is computed as

$$UR_{i,t}^{Claims} = \frac{IC_{i,t} + CC_{i,t}}{NP_{i,t} + IC_{i,t} + CC_{i,t}} \tag{1}$$

where $IC_{i,t}$ and $CC_{i,t}$, are average weekly claims for the month and $NP_{i,t}$ is nonfarm payroll employment from the Current Employment Statistics (CES)—the only state-level employment series presently available at a monthly frequency back to 1947.[7] The seasonally adjusted claims data can be rather noisy, particularly for initial claims, so we smooth the $IC_{i,t}$ and $CC_{i,t}$ series using a three-month centered moving average in constructing (1). We analogously construct a claims-based unemployment rate for the United States, aggregating seasonally adjusted average weekly claims and nonfarm payroll employment for all 50 states and Washington, D.C.

The left panel of Figure 1 plots our claims-based unemployment rate (red), the BLS unemployment rate (green), and the DOL IUR (blue) for Ohio, meant as an illustrative, representative large state; like all states, official data for Ohio start in January 1976 for the unemployment rate and February 1986 for the IUR.[8] The three unemployment series capture similar features of Ohio's business cycle in overlapping samples, such as identifying similarly timed local peaks and troughs. Figure 1(a) also underscores the practical benefit of our claims-based unemployment rates: Relative to the official BLS data, our historical series offer nearly three additional decades of monthly state-level data, spanning six post-war national recessions as identified by the National Bureau of Economic Research (NBER) Business Cycle Dating Committee (gray bars).

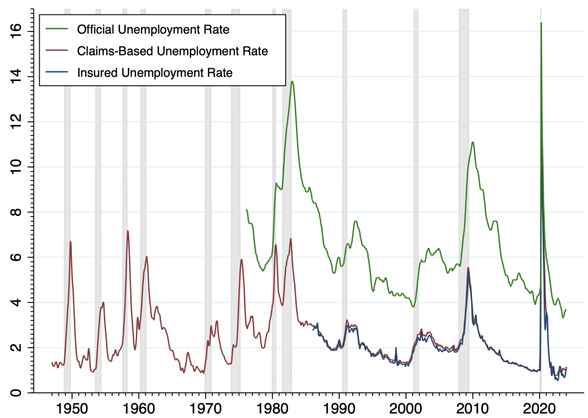
The right panel of Figure 1 plots our aggregated U.S. claims-based unemployment rate (red),

[7] Historical state-level data on covered employment is not consistently available at a monthly frequency from our primary sources. We seasonally adjust nonfarm payroll employment for each state using the Win X-13 program, as seasonally adjusted nonfarm payroll employment data for states are only available from the BLS starting in 1990. Several states do not have nonfarm employment data available since January 1947: Data begin in January 1950 for Minnesota, in January 1956 for Michigan, in January 1958 for Hawaii, and in January 1960 for Alaska. Our claims-based unemployment rates are constrained to these later start dates for these four states.

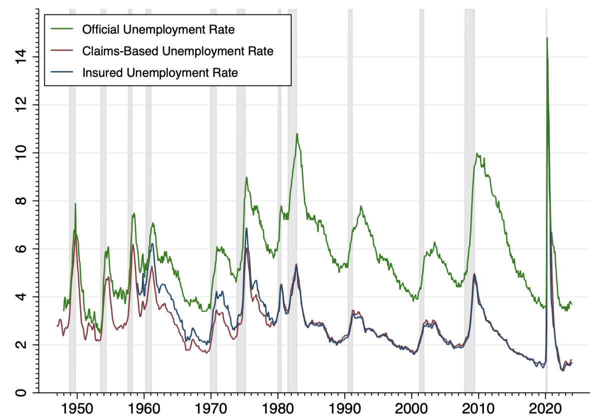
[8] The monthly IUR series is aggregated from weekly data that is not seasonally adjusted. We seasonally adjust the monthly IUR for Ohio using the Census Win-X 13 program.

Figure 1: Comparison of Official and Claims-based Unemployment Rates for Ohio and the U.S.

(a) Ohio



(b) United States



Notes: The left (right) panel plots a comparison of unemployment data for Ohio (the U.S.). Gray bars denote NBER recession dates. Sample: January 1948–December 2023 or when available. The backdated U.S. IUR data for January 1959–December 1970 is digitized from primary sources and seasonally adjusted; see Data Appendix A.3 for details.

along with the U.S. unemployment rate (green) and IUR (blue), when available. The U.S. unemployment rate starts in January 1948 and official monthly IUR data starts in January 1971, but we digitized and backdate this monthly U.S. IUR data to January 1959; see Data Appendix A.3 for details. Figure 1(b) shows that our U.S. claims-based unemployment rates are highly correlated with these official U.S. measures over long overlapping samples and identify broadly consistent features of the aggregate business cycle. We discuss these relationships in more depth below.

Section I.C. Comparisons with Official Unemployment Measures

It must be emphasized that our claims-based unemployment rates measure labor market slack differently than either the official unemployment rate or IUR.[9] Readers should not view our claims-based unemployment rates as an attempt to displace or backdate any other official measure; the motivation behind this new dataset is simply to expand our ability to study the U.S. economy and labor markets, across states and over a longer time horizon. But as shown in this section, official measures and our claims-based measures of unemployment contain similar informational content about the degree and timing of labor market slack—the series are highly correlated at both the state and national level and identify comparable inflection points in economic activity.

The BLS calculates the U.S. unemployment rate from the Current Population Survey (CPS), a monthly survey of roughly 60,000 households inquiring about their employment status; to be

[9]See Appendix A.3 for more detailed discussion of conceptual similarities, differences, and robustness checks.

counted as unemployed, a worker must have been available for work but not been employed during the surveyed week, and must have actively searched for work in the last four weeks or been expecting to be recalled following a temporary layoff. Official measures of state-level unemployment rates are intended to reflect the same definition of unemployment but are instead a statistical construct, derived in part from unemployment claims data.[10] And the IUR is calculated, for the U.S. as well as states, strictly from reported UI claims and coverage data.

It is immediately clear from Figure 1 that our claims-based unemployment rates almost perfectly align with the IUR both in terms of levels and inflection points; the correlation between Ohio's claims-based unemployment rate and IUR is 0.98 in the overlapping sample. The key advantage of our state-level claims-based unemployment rates relative to IUR data is simply data availability: At a monthly frequency, digital state-level IUR data are only presently available from ETA back to February 1986, and data limitations appear to preclude digitizing and backdating consistent monthly state-level IUR series to the 1940s or 1950s; see Data Appendix A.1.

On the other hand, there is a level difference between our series and the official unemployment rates. More similar to the IUR, our claims-based unemployment rates are restricted to individuals qualifying for and claiming regular state UI benefits as reported weekly by state unemployment offices to the ETA or preceding agencies. This is a subset of the population surveyed by the CPS: State UI programs have typically excluded certain workers from benefit eligibility, notably agricultural and self-employed workers, while federal employees and veterans have usually been covered by separate federal UI programs.[11] Consequently, our claims-based unemployment rates should be strictly lower than the BLS unemployment rates because of the narrower pool of benefit-eligible workers and because anyone unemployed beyond the maximum duration for regular state UI benefits will drop out of our measure.

Figure 1(a) shows such an expected level difference between the official unemployment rate and our claims-based unemployment rate for Ohio, one that is quite stable. A stable level difference poses no impediment to business cycle analysis so long as the series are highly correlated (they are, with a correlation coefficient of 0.81) and identify comparable inflection points (they do); moreover, it could be differenced out or removed by detrending the series if desired.[12]

The level difference between the U.S. series, however, shrinks moving back into the 1950s and 1940s, which is partly driven by our use of *nonfarm* payroll employment in the denominator of (1); when a larger share of workers are employed in agriculture and appear in the CPS survey measure

[10]The BLS Local Area Unemployment Statistics (LAUS) program uses data from the CES, CPS, and state UI programs to estimate state unemployment rates, but the methodology is something of a black box to the public; see the BLS LAUS program webpage.

[11]The larger pool of state and local government workers have been eligible for state UI programs for most of our sample and thus appear in our *IC* and *CC* measures. Official IUR series also typically focus on regular state UI programs and exclude federal UI programs, helping to explain the close match between the series seen in Figure 1.

[12]The correlation between the annual percentage point change in these two unemployment rates for Ohio is 0.82.

of employment but not the CES measure we use, it mechanically pushes up our claims-based unemployment rates relative to the CPS unemployment rate.[13] As would be expected, substituting the CPS measure of employment into (1) would hardly have any effect in recent decades but would gradually start pulling down our U.S. claims-based unemployment rate moving back in time into the early post-war era, as seen in Appendix Figure B.1. While using CPS employment would reduce the convergence in levels between the two U.S. unemployment series in the 1940s–60s, state-level CPS employment data are not available from the BLS until January 1976—precisely why we use the CES employment data.

Despite the time-varying level difference, our U.S claims-based unemployment rate identifies similar peaks and troughs as the BLS unemployment rate, as seen in Figure 1(b) and quantified in Section III.A, and the secular decline in the share of workers employed in agriculture would be absorbed by most detrending exercises. To illustrate this point, Appendix Figure B.2(a) depicts the cyclical versus trend components of the official U.S. unemployment rate (blue) and our U.S. claims-based unemployment rate (red), both extracted using a Hodrick and Prescott (1997) filter (HP, hereafter). The detrended data underscore that the time-varying level difference between the series does not impede business cycle analysis: The inflection points between positive and negative cyclical unemployment line up nearly perfectly between the two detrended series, particularly so in the earlier decades when there was a greater divergence between total employment and non-farm payroll employment. And the two HP-filtered series are highly correlated, with a correlation coefficient of 0.89 for the full sample and 0.94 for the pre-1976 sample.[14]

One possible concern about our claims-based unemployment rate is that the maximum duration of benefits have, to a degree, changed over time; procyclical changes in benefit duration would be particularly problematic and would not be absorbed by detrending exercises. Our construction of claims-based unemployment rates from only regular state UI programs is partly intended to avoid such a confounding influence from standing or ad hoc benefit extensions during recessions. We also examine how the maximum benefit duration for regular state programs evolved using the State Unemployment Insurance Laws dataset compiled by Massenkoff (2021) for 1970–2018, which we extend back to 1947 from scanned DOL reports. Appendix Figure A.1 shows that the average maximum duration of benefits is quite stable throughout our sample of interest and Appendix Figure A.2 shows that the average duration of unemployment is almost always well below typical maximum benefit durations; as such, legislative changes to the maximum duration of regular

[13] The ratio of total farm employment (Historical Statistics of the United States, K-179) to nonfarm payroll employment steadily fell from 23.6% in 1947 to 13.0% in 1960 and 6.4% in 1970. The CPS/CES employment ratio has been much more stable since 1970 than before 1970.

[14] Hamilton (2018) raises compelling concerns about the HP filter and proposes an alternative linear forecasting method for detrending data, but the implied trend in unemployment is highly sensitive to any recent recession, quickly rising and thus generating rapid declines in cyclical unemployment. While we prefer the HP filter in this context, the Hamilton (2018) method generates similar inflection points and correlations in cyclical unemployment measures.

benefits should have a minimal influence over time variation in our digitized UI claims.[15]

Another possible concern is that expansion of UI coverage in the early post-war era might be driving cyclical variation in our claims-based unemployment rate. Our use of nonfarm payroll employment in the denominator of (1) is always broader than covered employment used in IUR calculations, but the share of workers covered by UI programs rose sharply in the early post-war era (McMurrer and Chasanov, 1995), partly because of UI policy expansions and partly because of the shift from (mostly uncovered) farm to (mostly covered) nonfarm labor.

We digitize monthly U.S. IUR data back to January 1959 and annual data on covered employment back to 1945 to examine any concerning influence of expanding UI coverage for our claims-based unemployment rates. Appendix Figure A.4 shows that the ratio of U.S. covered employment to nonfarm payroll employment was quite stable at roughly 72-75% from the 1940s through early 1970s, then—driven by two federal policy changes—jumps to roughly 95-97% by the late 1970s; see Data Appendix A.3. Congruently, Figure 1(b) shows that our U.S. claims-based unemployment rate and the backdated IUR line up almost seamlessly since the late 1970s, but diverge slightly in earlier years, before this coverage expansion. But the two series are consistently capturing the magnitude and timing of business cycles throughout the sample, which is reassuring. Moreover, the last (and largest) UI coverage expansion occurs in the late 1970s, when the BLS state-level unemployment rates exist (these should be unaffected by coverage expansions); reassuringly, we do not observe a systematic change in our claims-based unemployment series relative to the official data around this period. But if desired, this federal expansion of UI coverage could be absorbed by detrending exercises; the HP-filtered cyclical components of the U.S. claims-based unemployment rate and IUR line up nearly identically (correlation of 0.98) throughout the sample and the trend component alone diverges between the series before the late 1970s; see Appendix Figure B.2(b).

Previewing some things that follow, much of our analysis below studies relative state-level variables that difference out national labor market averages, absorbing any common effect from federal policy changes. Moreover, the important changes we document regarding the convergence of unemployment dynamics across states and the emergence of a more uniform business cycle occur in the early post-war era, before the UI coverage expansions of the 1970s, meaning that these results are not being driven or biased by any spurious variation from those policy changes.

[15] McMurrer and Chasanov (1995) similarly document stability in max benefit durations for regular state programs over much of our historical sample of study. As an additional robustness check we also compute an alternative variant of our claims-based unemployment rate only using IC data, which will not be impacted by changes in maximum duration policies. Appendix Figure B.3 plots the (IC+CC) claims-based unemployment rate along with the IC-only variant; the two series track each other quite closely. This strong correlation highlights the fact that even after the trough of a business cycle, new separations from employment remain elevated for a significant period of time.

Section I.D. Fitted Claims-Based Unemployment Rates

Given the distinctions between the BLS state-level unemployment rates and our (unfitted) claims-based unemployment rates, we also estimate an alternative “fitted" measure of state unemployment rates using a statistical model of the relationship between the two series since January 1976. Since the BLS uses UI claims as an input into their (not publicly known) statistical model, our fitting exercise explores how much informational content UI claims alone have for official state-level unemployment rates, since this is effectively the data world that exists pre-1976, before CPS microdata are available.[16] If a good fit to official unemployment rates is achieved with UI claims, that helps build confidence that the claims data capture consistent features of state-level labor markets, or even more mechanically, that claims are a key input to the BLS's statistical model. The regression framework we choose captures the idea that a state's unemployment rate is likely higher than the national rate when that state is experiencing a higher claims-based unemployment rate relative to the national claims-based rate; it also reflects that the national unemployment rate has predictive power for state unemployment rates, particularly as pertains to long-term unemployment, exhaustion of state benefits, and UI eligibility. We then use the fitted model to back-cast predicted state unemployment rates before 1976.

To construct our fitted state-level series, we first estimate the relationship between the official and claims-based unemployment rates for each state i in month t with the following specification:

$$UR^{Official}_{i,t} = \beta_{0,i} + \beta_{1,i}(UR^{Claims}_{i,t} - UR^{Claims}_{US,t}) + \beta_{2,i}UR^{Official}_{US,t} + \varepsilon_{i,t} \quad (2)$$

where $UR^{Claims}_{i,t} - UR^{Claims}_{US,t}$ measures the difference between the state and national claims-based unemployment rates and $UR^{Official}_{US,t}$ is the national unemployment rate. Equation (2) is estimated on data spanning January 1976–December 2023 for each state, and we use these fitted models to generate predicted unemployment rates for January 1948–December 1975, which are merged with model estimates since January 1976.

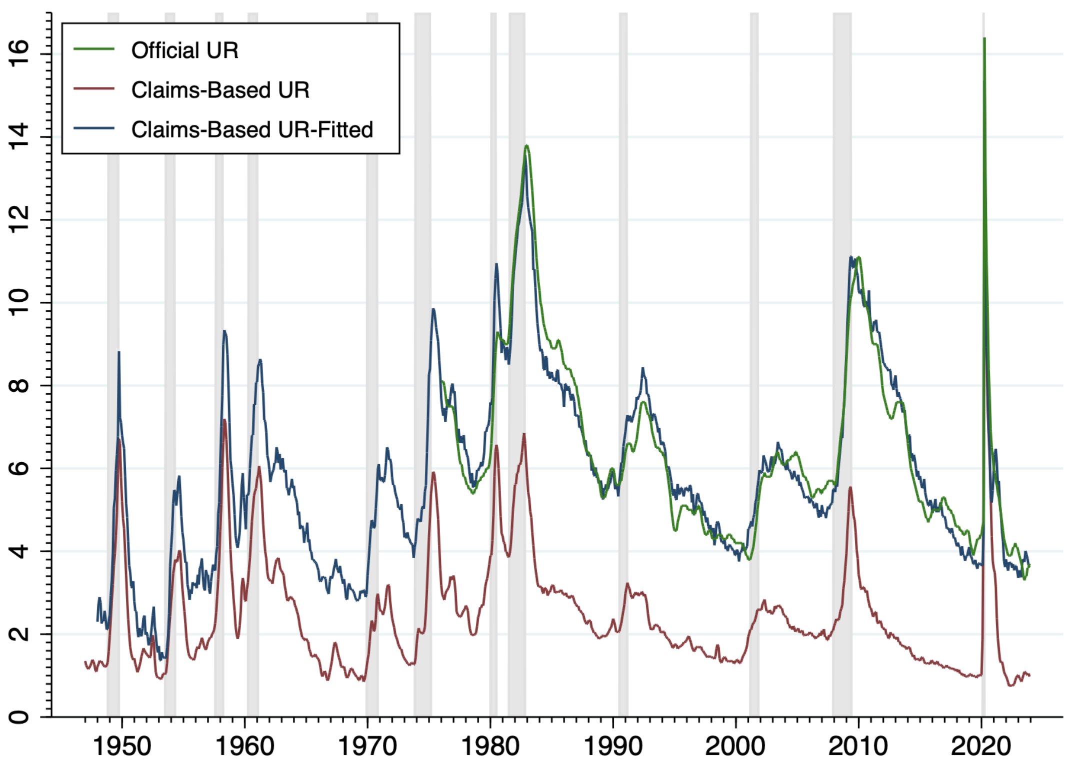
This simple statistical model fits the state-level data extremely well, and the predicted unemployment rates capture state business cycle features that are entirely consistent with the two unemployment measures used in estimating (2).[17] Both correlates are highly significant predictors of a state's official unemployment rate and the average R^2 is 0.84. The average correlation coefficient of the official and predicted unemployment rates is 0.91, with a maximum of 0.98 (Indiana) and a minimum of 0.80 (Nebraska and New Mexico). Revisiting our earlier illustrative example, Figure 2 plots our fitted claims-based unemployment rate for Ohio (blue) along with the BLS un-

[16] Similarly, more detailed CES data are not available until 1990.

[17] Reassuringly, adding a covariate for the ratio of U.S. covered unemployment to total unemployment—which, if serious, would help address the concern about UI coverage expansion discussed above—does not meaningfully improve the model fit or change the predicted series. Similarly, adding state-level controls for changes in UI policy parameters have a negligible effect on the fitted claims-based unemployment rates; see Appendix Figure A.3.

employment rate (green) and our unfitted claims-based unemployment rate (red) that were plotted in Figure 1(a).[18] The fitted claims-based unemployment rate picks up on inflection points in Ohio's business cycle that are nearly identical to those of the official unemployment rate over 1976–2023 and to our unfitted series over the full 1948-2023 sample.

Figure 2: Comparison of the Official and Fitted Claims-based Unemployment Rates for Ohio



Notes: The claims-based unemployment rate is smoothed with a three-month centered moving average. Shaded bars are NBER recession dates. Sample: January 1948–December 2023.

The fitted and unfitted series both have their advantages and drawbacks. One advantage of the fitted unemployment rates is that the official U.S. unemployment rate helps anchor them, removing the level differences, but the inflection points remain virtually an identical match. The inclusion of the U.S. unemployment rate as a regressor in (2) also helps to smooth the fitted claims-based unemployment data, as unemployment data are less noisy than claims data. A minor related drawback of the fitted series is that using the U.S. unemployment rate in (2) modestly limits the availability of our fitted claims-based unemployment rate to January 1948, a year later than our unfitted series.

Another drawback of the fitted unemployment rates is the fact that out-of-sample observations

[18] Appendix Figure C.1 plots our fitted claims-based unemployment rates for all 50 states along with state recession dates (gray bars) derived from those unemployment rates and the BLS state unemployment rates when available; see Section III.B. for details on identifying state recession dates.

are constructed on the assumption of a stable empirical relationship. To gauge this potential threat, we leverage state-level data available at a lower frequency to test the out-of-sample forecast of our fitted claims-based unemployment rates when feasible: In Appendix A.4, we use the CPS Annual Social and Economic Supplement (ASEC) to construct annual snapshots of state-level “unemployment rates" back to 1962 for larger states with more observations. Encouragingly, the fitted claims-based unemployment rates track the alternative ASEC-based unemployment rates quite well, both out-of-sample (1962-75) and in-sample (1976–89); see Appendix A.4.

Reassuringly, the unfitted and fitted claims-based unemployment rate series also generate similar results when examining the timing of recessions and pace of economic recoveries, as discussed in Section III and various robustness checks in Appendix B.3. We include both the unfitted and fitted series in our dataset and let researchers determine which is more appropriate for their uses.

Section II. Evolving Regional Adjustments Revisited

With our claims-based unemployment rates in hand, we first use our historical dataset to revisit the question of how labor markets adjust to local demand shocks, contributing new causal evidence on how those adjustments have evolved since WWII. This application builds on the seminal work of Blanchard and Katz (1992) and related work by Dao, Furceri, and Loungani (2017), among others.[19] Blanchard and Katz (1992) estimate relative employment, unemployment, and participation responses to innovations from VAR residuals over a 1978-1990 sample; they find employment responds strongly and remains persistently depressed following adverse shocks, whereas unemployment and participation see more muted, transitory responses, and thus conclude that interstate migration accounts for most of the adjustment to local demand shocks. Dao, Furceri, and Loungani (2017) revisit this question over a longer 1978-2013 sample, identifying local demand shocks using a Bartik (1991) instrument in a VAR framework; they find that labor mobility is less of an important short-run macroeconomic adjustment mechanism and state unemployment rates instead bear the brunt of the short-run adjustment. Like much of the related literature, both studies are constrained by the availability of official state unemployment, employment, and participation rates. To better understand the post-war evolution of local labor market adjustments, we estimate responses of relative employment, population, and claims-based unemployment rates to local labor demand shocks in an LP-IV framework over a much longer 1950-2019 sample and staggered subsamples.

[19] Many other related papers look at more specific, shorter shock episodes; see, e.g., Davis, Loungani, and Mahidhara (1997) on the 1970s oil shocks and post-Cold War military base closures, Autor, Dorn, and Hanson (2013) on the China trade shock, and Yagan (2019) on the Great Recession.

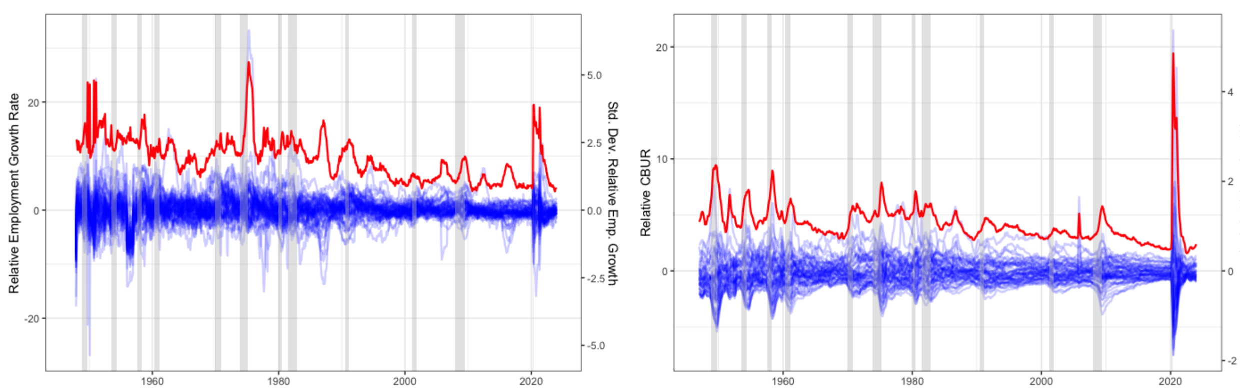
Section II.A Dispersion and Persistence of Local Adjustments

We first revisit two overarching empirical observations motivating this literature, reexamined over a longer postwar horizon. Blanchard and Katz (1992) document a wide dispersion in employment growth rates across U.S. states; moreover, they find a high degree of persistence in states' average employment growth rates between a 1950–70 sample and a 1970–90 sample. In a similar spirit, Dao, Furceri, and Loungani (2017) analyze the dispersion of growth in annual employment across states over 1977–2015, finding that the standard deviation has fallen since the early 1990s; they also confirm that a high degree of persistence in employment growth and unemployment rates still holds when comparing more recent subsamples (1977–94 and 1995–2013). The significant degree of heterogeneity and persistence in state labor market conditions would motivate interstate migration as a potentially important adjustment mechanism following local labor demand shocks.

Figure 3: Dispersion of Employment Growth and Unemployment Across U.S. States

(a) Relative Employment Growth Rates

(b) Claims-Based Unemployment Rates



Notes: Relative employment growth shows each state's annual nonfarm payroll employment growth less U.S. nonfarm payroll employment growth. Relative unemployment rates show each state's unfitted claims-based unemployment rate less the U.S. claims-based unemployment rate. Relative employment growth and unemployment rates are plotted in blue for each state and scaled on the left axis (in percentage points). The standard deviations of each series are plotted in red and scaled on the right axis. Shaded gray bars are NBER recession dates.

We, in turn, look at the dispersion of relative employment growth and unemployment rates across U.S. states over a longer January 1948–December 2023 sample. Relative employment growth (unemployment) measures each state's year-over-year growth in nonfarm payroll employment (monthly unfitted claims-based unemployment rate) less that of the national rate. Figure 3 plots states' relative employment growth rates (left panel) and relative unemployment rates (right panel) with a blue line for each state (left axes) along with the respective standard deviation across states (red lines, plotted on the right axes). The dispersion in relative employment growth spikes during recessions for the entire postwar sample, as Dao, Furceri, and Loungani (2017) observe post-1977, but the dispersion in relative unemployment is far more procyclical than that of employment growth. And up until the COVID pandemic, the degree of dispersion across states has generally trended downward throughout the postwar era, both for employment and unemployment.

To quantify this trend more clearly, Table 1 reports the maximum standard deviation of relative employment and unemployment from Figure 3 during or within six months of each national recession, as identified by NBER. Table 1 broadly underscore a long-run trend of states increasingly experiencing more similar labor market dynamics in recessions and recoveries, particularly since the early 1980s. State-level variation also notably tends to gradually decrease during relatively longer periods of economic tranquility: Dispersion in both relative employment growth and unemployment declines throughout the unusually long business cycle expansion in the 1960s, rises again during the turbulent 1970s and early 1980s, and then declines again during the Great Moderation. Overall, the results in Figure 3 and Table 1 document the increasing similarities in state-level labor markets over the post-war era.

Table 1: Maximum Labor Market Dispersion During National Recessions

Recession	’49	’54	’58	’61	’70	’75	’80	’82	’91	’01	’09	’20	Avg.*
SD Emp.	4.7	3.1	3.4	2.6	2.8	5.5	2.9	2.9	2.6	1.4	2.0	3.9	3.1
SD CBUR	2.4	1.8	2.2	1.6	1.4	2.0	1.8	1.5	1.2	1.0	1.4	4.9	1.7

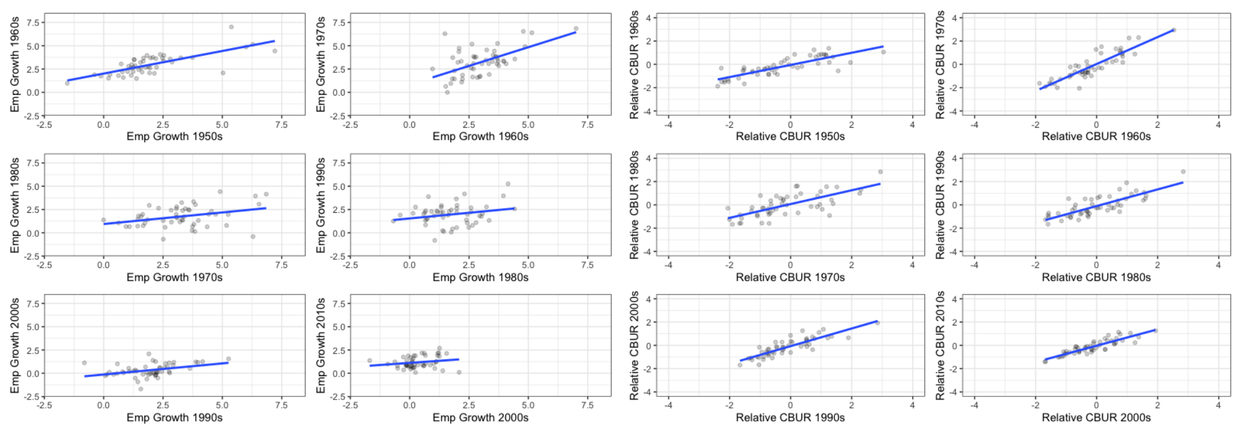
Notes: Table reports the maximum standard deviation of relative employment growth (SD Emp.) and relative claims-based unemployment rates (SD CBUR) across states during or within six months of each national recession, as plotted in Figure 3. The final column reports an unweighted average across recessions excluding the pandemic.

We also examine the persistence of labor market outcomes across states. The left panel of Figure 4 compares average employment growth rates for each state in one decade relative to their growth rate in the subsequent decade, all the way from a comparison of the 1950s against the 1960s (top left) through a comparison of the 2000s against the 2010s (bottom right). A strong positive correlation is found in the early post-war decades, but since the 1970s, the relationship between average employment growth in one decade and the next has weakened considerably, highlighting less persistent differences in states’ employment conditions in recent decades. Conversely, the right panel of Figure 4 documents a much more persistently positive relationship between states’ relative claims-based unemployment rates across decades.[20] Both the employment and unemployment persistence figures also show less dispersion in labor market conditions in recent decades, consistent with the results in Figure 3. Broadly speaking, our evidence of a diminishing persistence in employment growth and less disperse labor market outcomes suggests less scope for migration as an adjustment to local labor market shocks in more recent decades.

[20] Neumann and Topel (1991) document a similar persistence in annual relative state IURs over intervals spanning 1950–85, consistent with our longer analysis of monthly data, contrary to the evidence of Blanchard and Katz (1992).

Figure 4: Changes in Log Employment and Unemployment by Decade Across U.S. States

(a) Employment Growth Rates (b) Relative Claims-Based Unemployment Rates



Notes: The left panel plots each state's average annualized log employment growth in one decade against that of the next decade. The right panel plots each state's average relative (unfitted) claims-based unemployment rate in one decade against that of the next decade.

Section II.B Adjustments to Bartik Industry Share Shocks

In this section, we more formally analyze the local labor market responses to labor demand shocks and assess how these responses have changed over the post-war era. For our local labor market demand shock, we take a similar Bartik (1991) instrument approach to identification as used by Dao, Furceri, and Loungani (2017).[21] To capture each state's exposure to national labor demand shocks, we construct a relative industry mix variable for each state's annual personal income growth weighted by industrial composition, measured relative to the national average for industry-weighted personal income growth. For for state i, the industry mix variable in year t, $imix_{i,t}$, is constructed as a weighted share of personal income growth across J industries:

$$imix_{i,t} = \sum_{j=1}^{J} \left[\bar{\theta}_{i,j,t} \Delta \ln(\bar{i}_{-i,j,t}) \right] \tag{3}$$

The weights $\bar{\theta}_{i,j,t}$ in (3) reflect state i's share of personal income growth in industry j in year t, taken as a five-year moving average, which are used to scale $\Delta \ln(\bar{i}_{-i,j,t,})$, the annual growth of log personal income in industry j for all states excluding i. Our relative industry income mix variable, $rimix_{i,t}$, subtracts the national average from each state's industrial mix of personal income growth.

Dao, Furceri, and Loungani (2017) construct a version of this state-level industry mix variable from (3) using nonfarm private sector employment by industry from the Bureau of Economic Analysis (BEA) Regional Economic Accounts (REA), based on 20 industries at the two-digit

[21] Davis, Loungani, and Mahidhara (1997) take a similar approach to constructing state industry mixes of employment interacted with national employment growth rates as one of their studied local demand shocks.

SIC/NAICS industry level. The REA data on employment by industry, however, are only available starting in 1969. We instead use REA data on annual personal income by major component and earnings by industry because they are available for most states back to 1929, allowing us to construct a Bartik instrument for the full sample of our claims-based unemployment rates.[22] Our relative industry income mix variable is based nine industry groups that can be consistently constructed across the NAICS, SIC, and BEA's historical industry classifications. While less refined in terms of industry exposure, our use of earnings as opposed to employment is advantageous for capturing labor demand shocks through both the extensive and intensive margins; changes in hours worked might be more relevant than changes in employment following certain national shocks.

Our choice of labor market outcome variables is also motivated by historical data availability. Blanchard and Katz (1992) and Dao, Furceri, and Loungani (2017) estimate VAR systems using changes in relative (log) employment growth, the relative (log) employment rate, and the relative (log) participation rate, and then back out the implied responses of unemployment and population growth.[23] But like the official state unemployment rates, state-level participation and employment rates are only available back to 1976. We analyze dynamics of our claims-based unemployment rate, nonfarm payroll employment, and total civilian population because they are all available at the state level back to the 1940s. Dao, Furceri, and Loungani (2017) argue that, in response to local demand shocks, changes in the civilian working-age population—which they use as a proxy for migration—should primarily be driven by net migration, as adult mortality, incarceration, and immigration from abroad are unlikely to respond quickly to local demand shocks; in the same vein, we expect that the response of total population should also largely reflect net migration, as births and child mortality are also unlikely to respond strongly or quickly to local labor demand shocks.

We estimate state labor market adjustments to local demand shocks in the following reduced form LP-IV panel regression framework:

$$\Delta Y_{i,t+h} = \alpha_i + \gamma_t + \beta_h rimix_{i,t} + \varphi_h(L)\mathbf{Z}_{i,t-1} + \varepsilon_{i,t+h} \tag{4}$$

where α_i and γ_t are state and year fixed effects, respectively, and $rimix_{i,t}$ is the relative industry income mix Bartik instrument for state i in year t. For dependent variable of interest $\Delta Y_{i,t+h}$, we rotate in the cumulative change in relative unemployment ($\Delta Y_{i,t+h} = \tilde{Y}_{i,t+h} - \tilde{Y}_{i,t-1}$), the cumulative log point change in relative employment ($\Delta Y_{i,t+h} = \ln(\tilde{Y}_{i,t+h}) - \ln(\tilde{Y}_{i,t-1})$), and the cumulative log

[22] Data for Alaska and Hawaii start in 1950 and data are available back to 1929 for all other states.

[23] While we do not study employment rates due to historical data limitations, several other papers have found persistent local employment rate responses more in keeping with our results below, contrary to the transitory response of relative employment rates or participation rates documented by Blanchard and Katz (1992) and Dao, Furceri, and Loungani (2017). For instance, Autor, Dorn, and Hanson (2013) find persistent local labor market responses in areas more exposed to increased Chinese import demand mover 1990–2007. And studying state-level labor market hysteresis following the Great Recession, Yagan (2019) finds a high degree of persistence in both employment and employment rates for states more exposed to the recession.

point change in relative population. The respective relative variables are constructed by subtracting the claims-based unemployment rate for the U.S. from the claims-based unemployment rate for state i, subtracting log employment for the U.S. from log employment for state i, and subtracting log population for the U.S. from log population for state i. Regardless of which variable is being rotated in for $\Delta Y_{i,t+h}$, $\mathbf{Z}_{i,t-1}$ is a vector of lagged controls containing first differences of the relative claims-based unemployment rate and log first differences of relative employment and population for each state, to mop up any state-specific labor market trends not absorbed by the state and time fixed effects. In keeping with the two-lag annual VAR specifications of Blanchard and Katz (1992) and Dao, Furceri, and Loungani (2017), $\varphi_h(L)$ is a lag polynomial of order two.

For our benchmark analysis, we first estimate the local projections in (4) over an annual sample of 1950-2019, with separate regressions for each forecast horizon $h \in \{0, 1, ..., 11\}$.[24] The estimated sequence $\{\hat{\beta}_h\}_{h=0}^{11}$ traces out the dynamic impulse response function for the cumulative changes in relative labor market outcomes over the 12-year forecast horizon in response to a -1 percentage point shock to state i's personal income growth, given its industrial composition, relative to the national average growth rate. The cumulative log point responses of relative employment and population reflect growth in state i less average national growth over the same horizon, so impulse responses for all dependent variables are measured in percentage points.

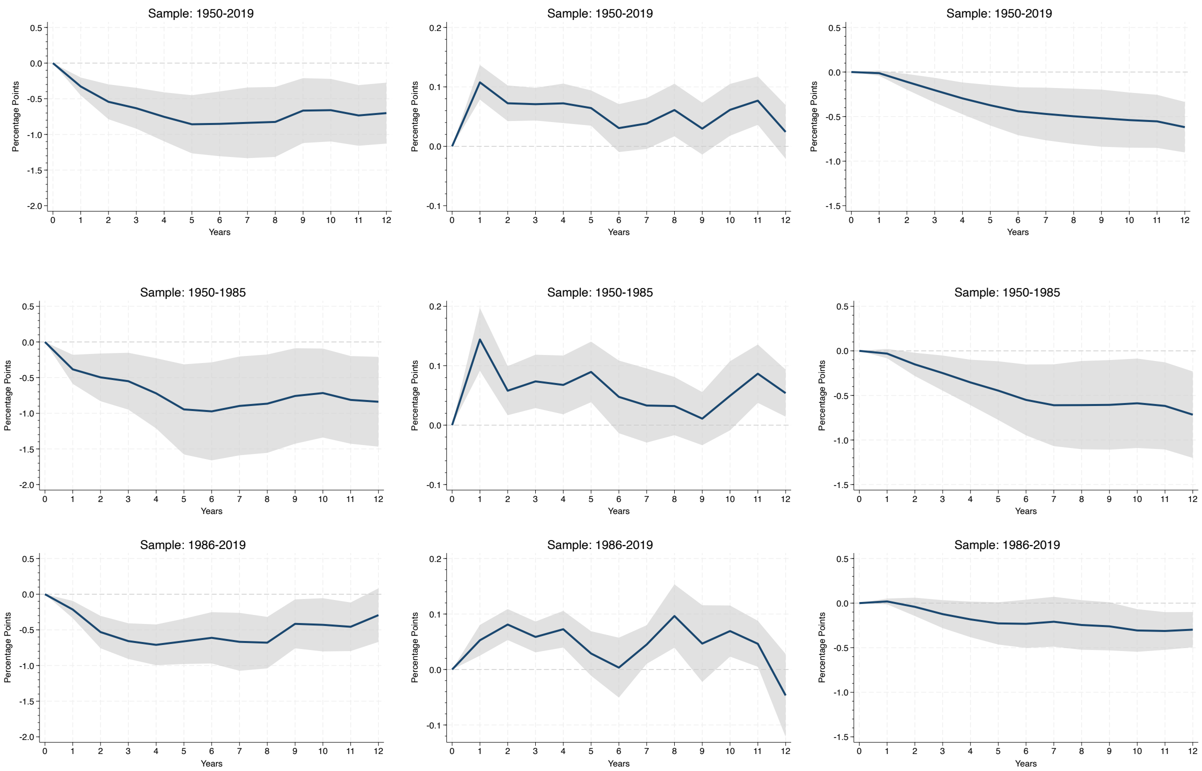
Because the persistence of local labor market outcomes can be quite sensitive to sample selection, we also estimate the same regressions over two evenly split subsamples: 1950–1985, reflecting the early post-war era through the oil shocks, and 1986–2019, capturing the "Great Moderation" through recovery from the Great Recession (deliberately excluding the pandemic). The top row of Figure 5 depicts the impulse responses of relative (log) employment (left), relative unemployment (middle), and relative (log) population (right) in response to the Bartik demand shocks over the full sample, along with shaded 95% confidence bands constructed from robust standard errors, clustered at the state level. The middle and bottom rows of Figure 5 depict the responses of the same relative variables estimated over the earlier and the more recent subsamples, respectively.

Over the full postwar sample, the estimated response of relative employment growth to an adverse local labor market demand shock shows a gradual but persistent decline, peaking at -0.9 percentage points below national growth after six years; the decrease is significant at the 95% confidence level throughout the forecast horizon. The response of relative unemployment is much more immediate, with a peak increase of 0.11 percentage points after one year; the jump in unemployment is less persistent than the decline in employment, and the null hypothesis of no effect on unemployment cannot be rejected at conventional levels of significance for much of the second

[24] Our unfitted claims-based unemployment rate data begin in 1947, but we lose three burn-in years in our sample due to the first-differenced lagged controls. The choice of a 12-year impulse response horizon is in keeping with Blanchard and Katz (1992) and Dao, Furceri, and Loungani (2017).

Figure 5: Local Labor Market Responses to Bartik Demand Shocks

(a) Relative Employment (b) Relative Unemployment (c) Relative Population



Notes: Figures depict the impulse responses of relative labor market variables estimated by the local projections in equation (4), with the $rimix_{i,t}$ instrument scaled to reflect a 1 percentage point decrease in state i's personal income growth relative to national average growth. Impulse responses are estimated over the full 1950-2019 sample (top), 1950-1985 subsample (middle), and 1985-2019 subsample (bottom). Shaded bands denote 95% confidence intervals.

half of the impulse response horizon. Lastly, the negative response of relative population is even more gradual and persistent than that of employment, declining as much as -0.65 percentage points by the end of the twelve-year forecast horizon; save the first year, when there is no response on impact, the decline in population is consistently significant at the 95% confidence level.

Our LP-IV impulse responses for the full sample qualitatively resemble the main Bartik IV estimates of Dao, Furceri, and Loungani (2017), despite varying regression frameworks and samples.[25] Across both, unemployment sees the greatest response on impact, with a peak effect after one year, employment sees a more gradual but fairly persistent decline, and population see a more gradual and more persistent decline than employment. Moreover, our benchmark impulse response estimates are far more similar to the Dao, Furceri, and Loungani (2017) IV estimates than their OLS estimates, intended as more in keeping with the earlier Blanchard and Katz (1992) estimates.

[25] Dao, Furceri, and Loungani (2017) do not plot confidence intervals in their figures, hence our inability to speak to comparisons of the statistical significance of our two sets of impulse responses.

Looking to the bottom two rows of Figure 5, it is clear that state labor market adjustments to local shocks have markedly changed over time. The response of relative employment growth to an adverse local labor demand shock is somewhat stronger in the earlier post-war sample, falling a little more than -1.0 percentage points after six years, whereas the drop in employment is only half as large in the recent sample, with peak decline of -0.55 percentage points after four years; and the decline in employment remains highly persistent and significant at the 95% level throughout the forecast horizon when estimated over 1950-1986, whereas the decline is transitory when estimated over 1986-2019, with point estimates reverting to roughly zero by the end of the forecast horizon.

Similarly, an immediate jump in unemployment is even more pronounced when estimated over 1950-1986, rising by 0.15 percentage points after one year, than when estimated over the entire sample, but again generally follows a qualitatively similar path as the estimates for 1950-2019. Conversely, the rise in unemployment estimated over 1986-2019 is much more gradual, with a peak increase of 0.11 percentage points a full eight years after the demand shock, although the effect again dissipates at longer horizons. The peak response of unemployment is, however, relatively more stable across samples than those of employment.

But the starkest difference between labor market adjustment margins between the two subsamples are those of population. Estimated over 1950-1985, relative population sees a gradual and highly persistent decline quite similar to the impulse response estimated over the full sample, but that decline all but disappears when estimating (4) over the 1986-2019 sample. For the latter, the point estimates are much smaller and only statistically significant at horizons of ten years or more; by the end of the forecast horizon, relative population drops by -0.14 percentage points in the more recent sample, versus -0.73 percentage points in the earlier sample. Dao, Furceri, and Loungani (2017) find a similar sharp attenuation of relative population responses when comparing a 1978-1990 subsample with post-1990 subsamples, but our results place this trend in much longer historical context. Both sets of results mirror a broader trend of decreasing internal migration rates in recent decades, particularly since the early 1980s (Molloy, Smith, and Wozniak, 2011).

Given the sensitivity of local labor market adjustments to choices about sample, in Appendix B.2 we also estimate the local projections in (4) as rolling regressions over staggered 25-year estimation samples. Appendix Figure B.4 shows that the headline results in Figure 5 for our post-war subsample split are broadly robust to more refined, staggered subsamples: Relative employment and population responses are fairly stable, both in magnitude and persistence, for most of the early post-war decades, but the magnitude (and, for employment, persistence) of responses starts falling sharply for samples estimated starting in 1980 and beyond. And the rolling regressions again show much less of a secular post-war trend in the impulse responses of relative unemployment than the relative employment and population margins of adjustment following a local labor demand shock; the peak response is reached more gradually, but the impulse responses of relative unemployment remain transient throughout the staggered subsamples.

In recent decades, the stark attenuation of relative employment and population responses coupled with the fairly steady peak response of relative unemployment seems to suggest that labor force participation has become relatively more important as an adjustment margin than interstate migration—consistent with the findings of Yagan (2019) for the Great Recession and broader evidence that non-participation is a key adjustment margin (Elsby, Hobijn, and Şahin (2015)).

Section II.C Non-Linear Local Labor Market Adjustments to Shocks

So far we have documented two interesting facts about labor markets: 1) state-level labor market conditions have become more similar over time and 2) there appears to be a much weaker migration response to local labor market shocks in recent decades, far more so than the corresponding diminished response of employment. To tie these two empirical results together, we explore whether there is a non-linear effect of local shocks, not just sign but in magnitude.[26] If labor market adjustments are particularly responsive to larger relative shocks, e.g. because of a high fixed cost of migration, then more similar labor market conditions across states (empirical regularity #1) could help rationalize the weaker migration response since the 1980s (empirical regularity #2).

To test this hypothesis, we construct "very positive" and "very negative" Bartik shocks, defined as above-average positive and below-average negative shocks, respectively, with these average thresholds, $\overline{rimix^{+}_{i,t}}$ and $\overline{rimix^{-}_{i,t}}$, defined over the full 1950-2019 sample.[27] We define "smaller shocks" as those remaining in between these thresholds:

$$\begin{aligned}
rimix^{++}_{i,t} &= rimix_{i,t} \;\text{ if }\; rimix_{i,t} \geq \overline{rimix^{+}_{i,t}},\; 0 \;\text{ otherwise;}\\
rimix^{--}_{i,t} &= rimix_{i,t} \;\text{ if }\; rimix_{i,t} \leq \overline{rimix^{-}_{i,t}},\; 0 \;\text{ otherwise;}\\
rimix^{s}_{i,t} &= rimix_{i,t} \;\text{ if }\; \overline{rimix^{+}_{i,t}} > rimix_{i,t} > \overline{rimix^{-}_{i,t}},\; 0 \;\text{ otherwise.}
\end{aligned}$$

We use these disaggregated Bartik instruments to estimate the following modified LP-IV regression, where the objects of interest are coefficients β^{++}_{h} and β^{--}_{h}, which trace out impulse response functions for "very positive" and "very negative" shocks, respectively:

$$\Delta Y_{i,t+h} = \alpha_i + \gamma_t + \beta^{++}_{h} rimix^{++}_{i,t} + \beta^{--}_{h} rimix^{--}_{i,t} + \beta^{s}_{h} rimix^{s}_{i,t} + \varphi_h(L)\mathbf{Z}_{i,t-1} + \varepsilon_{i,t+h} \tag{5}$$

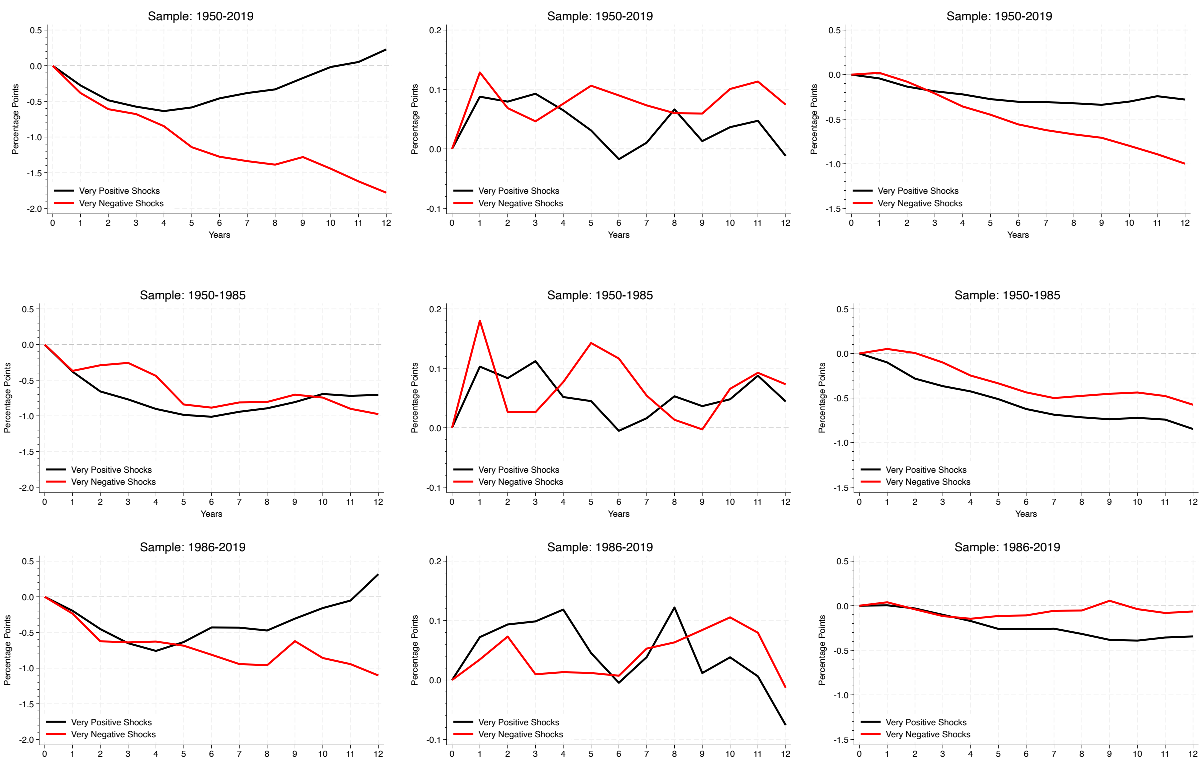
[26] Asymmetric local labor market responses to positive versus negative shocks have been documented in the literature, e.g., Davis, Loungani, and Mahidhara (1997), Dao, Furceri, and Loungani (2017), and Notowidigdo (2020).

[27] The cutoff for "very positive" labor shocks is above 0.55 percentage points and the cutoff for "very negative" labor shocks is -0.52 percentage points. The states with the most frequent "very negative" demand shocks are South Dakota (32), North Dakota (26), Iowa (26), Nebraska (25), Arizona (22), Wyoming (22), West Virginia (21), and Idaho (21), while the "states" with the most frequent "very positive" demand shocks are Washington, D.C. (34), Nevada (32), New York (29), Wyoming (23), Alaska (23), Hawaii (22), Maryland (22), and Massachusetts (21).

Results from this modified LP-IV regression are reported in Figure 6. As conjectured, there is a strong non-linear response from the Bartik instruments: Responses to positive versus negative shocks often tell a different story, particularly for relative employment and population, but more importantly the “very positive" and “very negative" shocks are driving these non-linear effects. A modified regression instead using “all positive" and “all negative" shocks produces virtually identical impulse response functions as those shown in Figure 6, highlighting that these very large shocks are driving all the results.[28]

Figure 6: Relative Labor Market Responses to Very Positive vs. Very Negative Bartik Shocks

(a) Relative Employment (b) Relative Unemployment (c) Relative Population



Notes: Figures depict the impulse responses of relative labor market variables estimated by the local projections in equation (5), with the $rimix_{i,t}^{++}$ and $rimix_{i,t}^{--}$ instruments each scaled to induce a 1 percentage point change in state i's personal income growth relative to national average growth. Impulse responses are estimated over the full 1950-2019 sample (top), 1950-1985 subsample (middle), and 1985-2019 subsample (bottom).

In other words, large *relative* labor market shocks are needed to trigger a migration response (and large negative shocks are needed for an employment response), but in recent decades fewer states experience local labor market conditions that are significantly different than the national average. Our disaggregation of the $rimix_{i,t}$ Bartik instrument reflects as much: There are fewer “very positive" and “very negative" shocks in the 1986–2019 sample than the 1950–85 sample,

[28]These results are omitted for brevity—they look almost identical to Figure 6—but are available upon request.

and the mean (absolute) values are smaller in the later sample than the earlier sample.[29] Fewer and relatively smaller "large" relative labor market shocks in more recent decades corresponds with the declining dispersion in states' relative employment growth and unemployment rates seen in Figure 3, all wholly consistent with a smaller migration response in recent decades: The improvements in labor market conditions that could be achieved by migration appear to have diminished in more recent decades as states look increasingly similar across the national business cycle.

Section III. Unemployment Recoveries from Recessions

Studies of state or regional business cycles often difference out the national business cycle—as our analysis above did—for good reasons, e.g., stationarity of outcome variables, identifying variation from Bartik (1991) instruments. But the U.S. business cycle is surely influencing the evolution of states' labor market adjustments analyzed in Section II. Moreover, evidence from states can help shed light on the evolution of the aggregate U.S. business cycle. In this section, we use our historical measures of unemployment to examine various features of postwar U.S. business cycles and the evolution of unemployment dynamics at both the state and national level.

We first use our claims-based unemployment rates to identify peaks and troughs in the U.S. business cycle, which produces similar business cycle inflection points as those estimated from the official U.S. unemployment rate. We also use our dataset to identify peaks and troughs for all 50 states and document that these state-level recession dates line up reasonably well with existing estimates of state recession probabilities in overlapping samples since the late 1970s.

We use these recession dates and our new dataset to study the rate and degree to which unemployment recovers following recessions. Studying unemployment dynamics remains an active research agenda in macroeconomics, particularly as relates to the existence of a natural rate of unemployment (e.g., Dupraz, Nakamura, and Steinsson (2023), Hall and Kudlyak (2020, 2022)) or "jobless recoveries" from recent recessions (e.g., Galí, Smets, and Wouters (2012)). Most of this research agenda focuses on the national business cycle, but our historical claims-based unemployment rates allow us to study heterogeneity across states exploiting a much larger post-war sample of observations. In particular, we explore the evolving dispersion of states' unemployment recovery rates as well as changes in unemployment relative to state-specific business cycle troughs.

[29] For $rimix_{i,t}^{++}$, there are 341 observations (mean of 1.3 percentage points) in the early sample and 235 observations (mean of 1.2 percentage points) in the later sample. For $rimix_{i,t}^{--}$, there are 312 observations (mean of -1.4 percentage points) in the early sample and 193 observations (mean of -1.2 percentage points) in the later sample.

Section III.A. Recession Dating for the U.S. Business Cycle

To analyze the speed and dispersion of unemployment recoveries, we must first must choose a chronology of business cycle inflection points.[30] There are various approaches to identifying peaks and troughs in the business cycle; see Romer and Romer (2019) for an overview. We adopt the relatively simple, unemployment-based recession dating algorithm proposed in Dupraz, Nakamura, and Steinsson (2023) (DNS, henceforth), which generates a close match to the NBER recession dates.[31] The DNS recession dating algorithm identifies local minima and maxima of the unemployment rate, ignoring low frequency variation, similar in spirit to the Bry and Boschan (1971) algorithm or the unemployment-based Sahm (2019) Rule; see Appendix A.5 for an overview of the DNS algorithm.[32] Table 2 reports national business cycle peak and trough dates identified by the DNS algorithm from our U.S. claims-based unemployment rate as well as those from the BLS unemployment rate, along with NBER recession dates as a benchmark.

The two unemployment-based chronologies of recession dates generate a relatively consistent match with one another. The peaks and troughs identified from the claims-based unemployment rate generally occur earlier than those generated from the official unemployment rate, with an average absolute discrepancy of 3.8 months for troughs versus 5.8 months for peaks. The UI claims we use in constructing our claims-based unemployment rates are faster to pick up changes in the labor market—in particular, IC are a leading economic indicator—than the official unemployment rate, a lagging economic indicator. Appendix Figure B.5 plots cross-correlograms for the official U.S. unemployment rate versus our claims-based unemployment rate as well as the IUR. These figures highlight that, in addition to being highly correlated with the official unemployment rate, both claims-based indicators tend to slightly lead the official unemployment rate—consistent with business cycle peaks and troughs being identified slightly earlier when using our claims-based unemployment rate instead of the official unemployment rate, as seen in Table 2.

Unsurprisingly, the unemployment-based recession dates align better for troughs than peaks. A challenge with the DNS algorithm is a sensitivity to "flat peaks" in economic activity, i.e., trying to identify a local minima around low and relatively stable unemployment rates late into business cycle expansions, which is never an issue for troughs. The worst peak match is the end of the recovery

[30] The common alternative to using chronologies is estimating a Markov regime-switching model, first popularized by Hamilton (1989), in which turning points are unobserved latent variables; the model produces posterior probabilities that a given period is an inflection point, and hence recession probabilities. A chronology of inflection points is far more tractable for estimating recovery speeds and comparisons with the recent literature on national recoveries.

[31] The DNS algorithm also identifies peaks and troughs in U.S. business cycles that are also nearly identical to the Hall and Kudlyak (2020, 2022) chronology based on observed peaks and troughs in the U.S. unemployment rate.

[32] As a robustness check, we also estimate state recession peaks and troughs using the Bry and Boschan (1971) algorithm, which generates similar results. The Sahm Rule heuristic for identifying recessions is based on the 3-month moving average of the U.S. unemployment rate rising at least 0.5 percentage points above its preceding 12-month low, which could also be adapted to state unemployment rates.

Table 2: Business Cycle Peaks and Troughs

	NBER		DNS Dating Algorithm			
			Claims-based UR		Official UR	
	Peak	Trough	Peak	Trough	Peak	Trough
1	Nov. 1948	Oct. 1949	[Dec. 1947]	Oct. 1949	[Jan. 1948]	Oct. 1949
2	[July 1953]	May 1954	Apr. 1953	Sep. 1954	May 1953	Sep. 1954
3	Aug. 1957	Apr. 1958	Dec. 1955	May 1958	Mar. 1957	July 1958
4	Apr. 1960	Feb. 1961	June 1959	Mar. 1961	Feb. 1960	May 1961
5	Dec. 1969	Nov. 1970	June 1969	Nov. 1970	Sep. 1968	Dec. 1970
6	Nov. 1973	Mar. 1975	Apr. 1973	May 1975	Oct. 1973	May 1975
7a	Jan. 1980	July 1980	Nov. 1978	July 1980	May 1979	
7b	July 1981	Nov. 1982	June 1981	Oct. 1982		Nov. 1982
8	July 1990	Mar. 1991	Nov. 1988	Mar. 1991	Mar. 1989	June 1992
9	Mar. 2001	Nov. 2001	Apr. 2000	Mar. 2002	Apr. 2000	June 2003
10	Dec. 2007	June 2009	Apr. 2006	May 2009	Oct. 2006	Oct. 2009
11	[Feb. 2020]	Apr. 2020	June 2019	May 2020	Sep. 2019	Apr. 2020

Notes: Recession dates for the claims-based unemployment rate (CBUR) and official unemployment rate (UR) are generated by applying the DNS algorithm on these two series, setting DNS parameter $X = 1.5$ for the official UR and $X = 1.0$ for the CBUR; see Appendix A.5. Recession dates in brackets denote some uncertainty about the precise timing of those inflection points. For the NBER recession dates, the peaks in July 1953 and February 2020 are bracketed to note that the identified *quarterly* peak occurred earlier, in 1953Q2 and 2019Q4, respectively. For the DNS algorithm, the peaks in December 1948 and January 1948 are in brackets because the DNS algorithm cannot identify those peaks due to data limitations; both dates are hard-coded based on minima during available samples.

from the 1953-54 recession, where the peak dates from the unemployment-based chronologies are 15 months apart—a dating discrepancy easily understood by looking at Figure 1, which shows an unusually “flat peak" in U.S. unemployment. The official unemployment rate (green line) reaches close to its local minimum in 1955, but jumps around before reaching a minimum in March 1957, whereas the claims-based unemployment rate (red line) hits its local minimum in December 1955, and is slightly trending upwards into 1957. If we discard this extreme case, the average discrepancy between the peak dates is 4.8 months, roughly in line with the average absolute discrepancy in troughs, implying comparable recession recovery durations on average.

Overall, the unemployment-based recession dates also generate a relatively consistent match with the NBER business cycle dates. One notable difference between the two unemployment-based recession dates is that the claims-based unemployment rate series identifies a double-dip recession in the early 1980s, spot on with the July 1980–July 1981 recovery identified by NBER, but only a single, longer recession is identified from the official unemployment rate.[33] Looking back to Figure 1, this divergence is again easily understood: There is only a modest decline in the official unemployment series in late 1980 and early 1981 but a much more pronounced dip in our claims-based unemployment series. Our U.S. claims-based unemployment rate also generates a closer match to the NBER troughs dates than does the official unemployment rate: Ignoring the initial 1980 recession, the average absolute difference between the claims-based and NBER trough dates is 1.4 months, versus 4.6 months between the official unemployment rate and NBER trough dates. Both the claims-based and official unemployment rate series do a worse job matching the NBER peaks than matching the NBER troughs, again reflecting the “flat peak" challenge with the DNS algorithm; the claims-based and official unemployment rate recession dates have an average absolute discrepancy of 11.9 months and 7.9 months, respectively, from the NBER peaks.

In our baseline analysis below we employ the recession dates inferred from the official U.S. unemployment rate as a better cross-walk with the existing literature. Appendix B.3 provides additional results using recession dates inferred from our claims-based unemployment rates; our headline results are robust to choices about chronologies of national recession dates.

Section III.B. Recession Dating for U.S. States

To understand the evolution of state-level business cycles, we next need business cycle peaks and troughs at the state level, and we construct them by applying the DNS recession dating algorithm to our fitted claims-based unemployment rate for each state.[34] Appendix Figure C.1 depicts our claims-based unemployment rates (blue lines) and the state recession dates derived from them (gray bars) for every state. As a validation exercise, we compare our claims-based peak and trough dates for U.S. states with the state recession probabilities estimated by Owyang, Piger, and Wall (2005) using a Markov regime-switching model; they produce estimates of state recession probabilities for February 1979–June 2002, a sample limited by the availability of the state coincident indexes, which in turn are limited by the unavailability of the BLS state unemployment rates before 1976.[35] Appendix Figure C.1 also depicts the Owyang, Piger, and Wall (2005) state recession probabilities for this subsample (red lines).

[33]The chronology of recession dates identified by Hall and Kudlyak (2020, 2022) from the U.S. unemployment rate similarly does not identify a double-dip recession in the early 1980s.

[34]There is more of an open question about how to appropriately set the parameter “X" for the DNS algorithm for states, which have more varied amplitudes of unemployment than the nation. For the national (unfitted) claims-based unemployment rate we set X=1.0, which generates a good match with NBER recession dates (reported in Table 2). For states, we compute the ratio of the state-level and national claims-based unemployment rates and scale each state's “X" parameter accordingly, and then rescale these down by 25% to be conservative; see Appendix A.5. for details.

[35]The Federal Reserve Bank of Philadelphia produces up-to-date monthly state coincident indexes using the model of Crone and Clayton-Matthews (2005), but data are similarly only available starting in January 1979 or later. The coincident indexes are estimated from four state-level variables: Nonfarm payroll employment, average hours worked of production workers in in manufacturing, the official state unemployment rate, and real wage and salary disbursements.

Broadly speaking, the crosswalk suggests that our claims-based unemployment rates identify similar business cycle dynamics for most states, particularly larger ones, during the overlapping 1979–2002 sample; the similarities and differences between our state-level recession dates and the Owyang, Piger, and Wall (2005) recession probabilities are discussed in more detail in Appendix C. Some differences are to be expected. Markov-switching models and the DNS algorithm identify fundamentally different objects, and state-level coincident indexes are a related but broader measure of economic activity than state unemployment rates, our exclusive focus in identifying recession dates. Neither approach is right or wrong per se. But Appendix Figure C.1 underscores a drawback of using the Markov regime-switching approach for studying recovery rates: Our recession dates exhibit fewer erratic, short-lived recessionary spikes or dubiously long recessionary periods, and no judgement is required regarding a cutoff for recession probabilities to identify recovery dates and durations. The principal advantage to our approach, however, is the ability to identify inflection points in state business cycles for more than 30 additional years when using our claims-based unemployment rate series instead of existing off-the-shelf state coincident indexes. It would be possible to construct backdated coincident indexes using our new dataset and estimate state recession probabilities over a longer horizon, but we leave that for future research.

Section III.C. National and State Unemployment Recovery Rates

With these recession dates in hand, we use our new claims-based unemployment rates to examine the evolving pace of economic recoveries at both the state and national level as well as the dispersion of recovery rates across states. Following the general approach in Hall and Kudlyak (2020), we compute the pace of recovery in unemployment as the mean decline in the log unemployment rate, UR_t, over the recovery period, defined as:

$$\text{Recovery Pace} = -12 \cdot (\log UR_0 - \log UR_T)/T \tag{6}$$

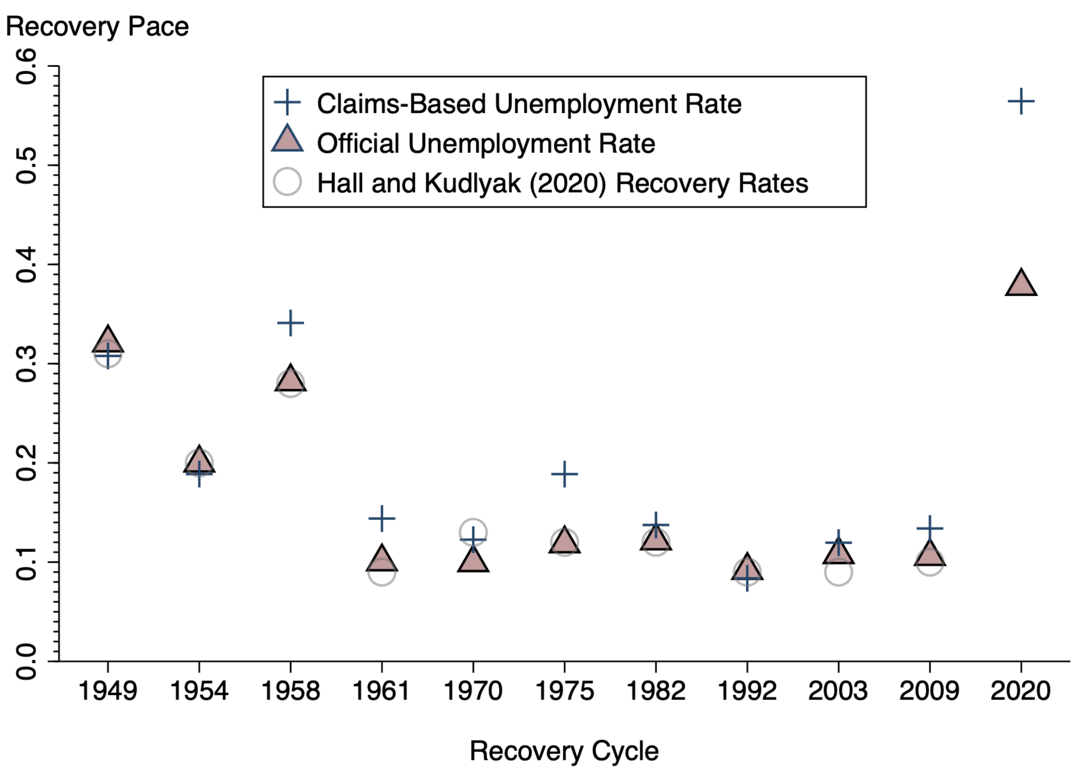
Equation (6) calculates the average annualized percentage decline in the unemployment rate from its maxima at the end of a recession (recovery starting at month 0) to its minima at the end of the ensuing expansion (recovery ending at month T).

Figure 7 depicts the national recovery rates for the official U.S. unemployment rate (red triangles) and our claims-based unemployment rate (blue crosses) for eleven post-war exapansions, calculated from the recession dates in Table 2 derived from the U.S. unemployment rate. As a benchmark, Figure 7 also replicates the national unemployment recovery rates from Figure 3 of Hall and Kudlyak (2020) for the first ten recoveries (circles), constructed from their unemployment-based chronology of recession dates.[36] Figure 7 shows that national unemployment recovery rates

[36] In a subsequent version of their paper, Hall and Kudlyak (2022) revise their methodology for estimating recovery rates, such that equation (6) is a nested case when log unemployment is a random walk—a reasonable approximation of

have decelerated markedly since the 1950s and roughly stabilized starting in the 1960s, at least up until the pandemic. Encouragingly, our claims-based unemployment rate generates very similar recovery rates as the official unemployment rate—using either our recession dates or the Hall and Kudlyak (2020) chronology—for the first ten post-war recessions; our series also identifies the same structural break in recovery rates between the 1957-58 and 1960-61 recessions. It is important to emphasize that our U.S. claims-based unemployment rate is not fitted using the official unemployment rate as in equation (2)—it is computed from unfitted claims data, see equation (1).

Figure 7: National Recovery Rates of Official and Claims-based Unemployment Rates



Notes: Recovery dates are estimated from the official U.S. unemployment rate using the DNS algorithm, see Table 2 for dates. The recovery from the pandemic recession is dated from the trough in April 2020 (see Table 2) to a peak in December 2023 (the end of our sample). Recovery from the 1980 recession is excluded because that expansion is only identified from the claims-based unemployment rate series and recovery is cut short by the more severe 1981-82 double-dip recession.

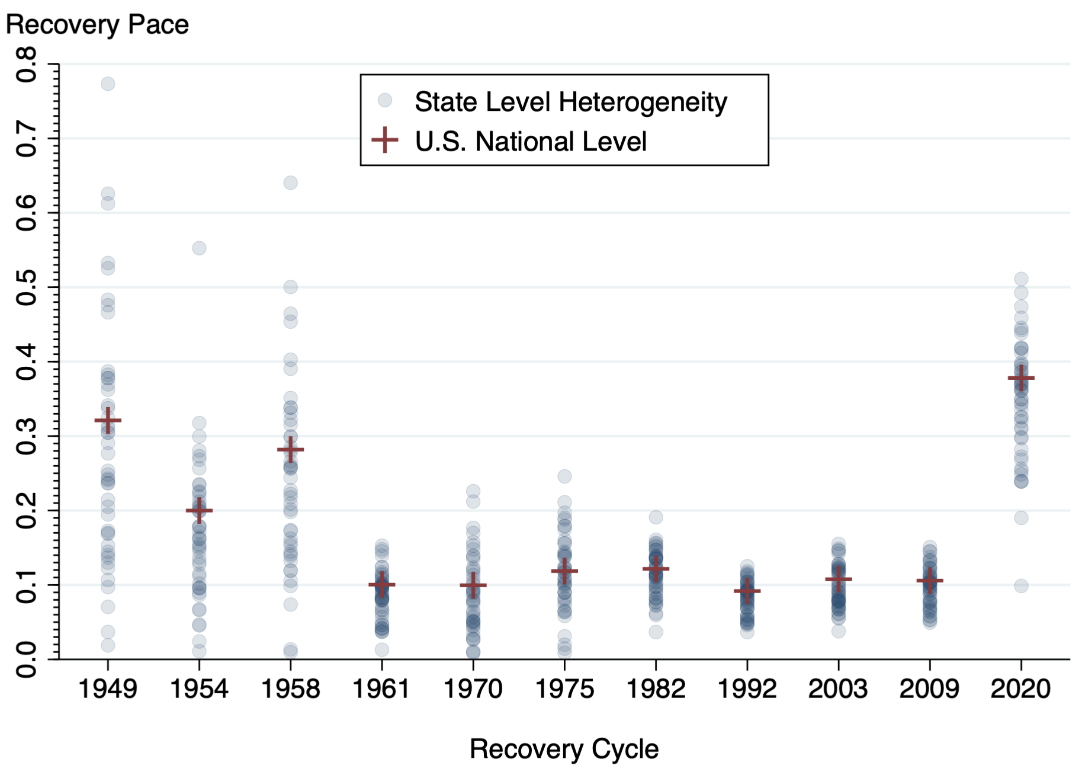
The choice of recession dates influences the calculation of recovery speeds, both in terms of the log point change in the unemployment rate and potentially in the duration of the recession, as underscored by several slight differences in the U.S. unemployment recovery rates when using the

reality, as the autocorrelation of the log U.S. unemployment rate and U.S. claims-based unemployment rate both exceed 0.98. We view equation (6) as the appropriate descriptive statistic for average recovery rates of unemployment, and a far more tractable approach for calculating 500+ recovery rates than their bootstrapped estimation of 10 recoveries.

DNS-based recession dates versus the Hall and Kudlyak (2020, 2022) dates. As a robustness check, Appendix Figure B.6 replicates Figure 7 using recession dates estimated from our U.S. claims-based unemployment rate (also in Table 2) instead of those derived from the BLS unemployment rate. The broad trends of a marked deceleration in unemployment recoveries since the 1950s and more stable, uniform recovery rates over the last 60 years hold using either set of recession dates.[37]

The only major divergence between the two recovery rates comes after the pandemic, when the claims-based unemployment rate shows a much faster "recovery rate" than the official unemployment rate, as would be expected from Figure 1. Both series see a comparable spike in March–May 2020, but during the ensuing recovery the claims-based unemployment rate quickly falls below pre-pandemic levels to record lows, whereas the official unemployment rate did not recover to its pre-pandemic rate until July 2022. The differential degrees of recovery are amplified into fast, more divergent recovery rates by the historically short time to recovery.

Figure 8: State-level Recovery Rates of Claims-based Unemployment Rates

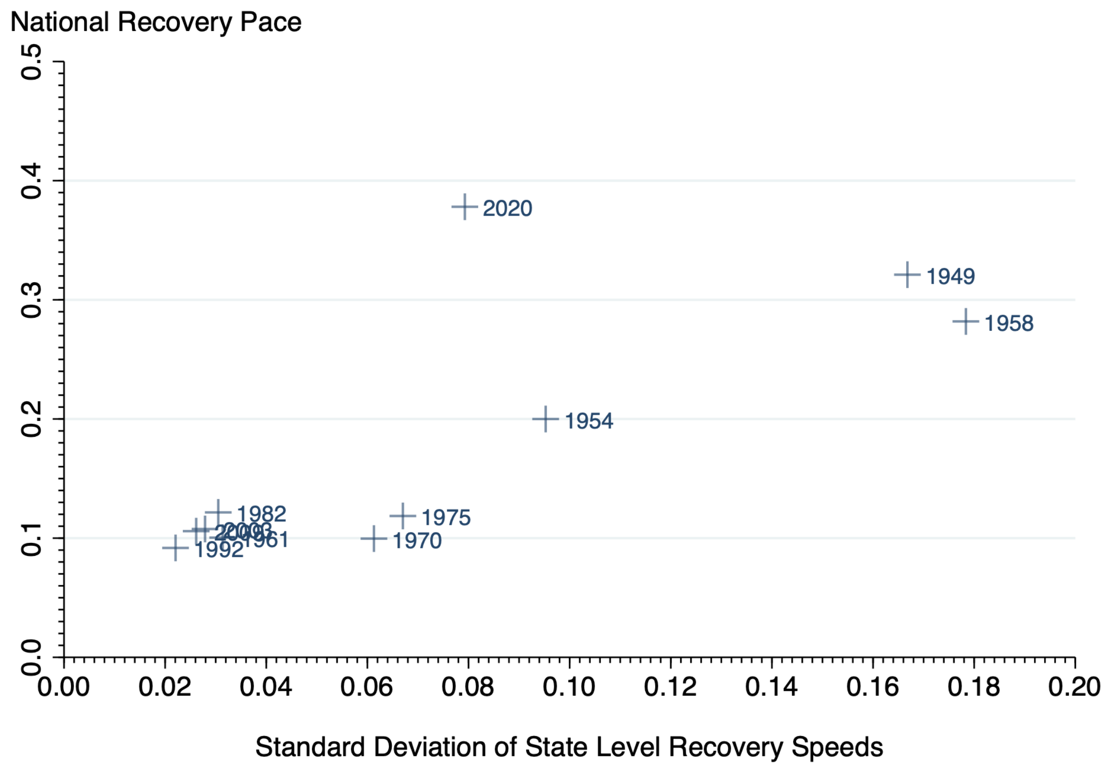


Notes: Recovery dates are estimated from the official U.S. unemployment rate using the DNS algorithm, see Table 2. Recovery from the 1980 recession is again excluded and recovery from the pandemic is hard-coded to a peak in December 2023, see notes to Figure 7. Recovery rates are negative for a few states, i.e., their unemployment rate rose during the national recovery, but only nonnegative recovery rates are plotted.

[37] If anything, the deceleration in recovery rates since the 1950s is even more pronounced when using recovery dates estimated from our claims-based unemployment rates.

We next explore the pace of economic recoveries across states for the same eleven recessions, using our fitted claims-based unemployment rates. Figure 8 plots the state-level recovery rates as circles along with red crosses depicting the national claims-based recovery rates (previously plotted in Figure 7). One striking feature of this data is that faster recoveries tend to be associated with much more dispersion in the pace of states' recoveries: This was true during the faster recoveries from the early post-war recessions of 1948-49, 1953-54, and 1957-58, and this dynamic remerged in the rapid pandemic recovery, albeit likely for different reasons discussed below.

Figure 9: Dispersion in State-level Unemployment Recovery Rates by U.S. Recovery Rates



Notes: Recovery dates are estimated from the official unemployment rate using the DNS algorithm, see Table 2 for dates. Recovery from the pandemic recession is dated from trough to December 2023 based on present data availability. Recovery from the 1980 recession is again excluded, see notes to Figure 7.

To display this association more clearly, we plot the national recovery pace against the standard deviation of state-level recovery rates in Figure 9, which displays a clear increasing relationship. Again, faster national recoveries tend to be ones where states experience very different outcomes, and states experience rather similar unemployment recoveries during slower national recoveries throughout the 1960s–2010s. The deceleration and convergence in states' unemployment recovery rates depicted in Figure 8 and Figure 9, most prominent since the 1982 recession, corresponds with the evidence from Figure 5 showing a shift from more immediate jumps in relative unemployment and faster ensuing recoveries early in the post-war era to more gradual, persistent increases in relative unemployment since the start of the Great Moderation.

Echoing the evidence in Figure 3, these results also underscore that labor market conditions are increasingly similar across states in more recent recessions, possibly reducing the job prospects workers can achieve via migration. The rapidness of the early post-war recoveries may be driven, at least in part, by a migration response that subsequently weakened: Larger differences across local labor markets may induce greater labor mobility, quickening the national adjustment to recessionary shocks. Saks and Wozniak (2011) document that U.S. migration rates are generally procyclical but note that the recessions of 1957–58 and 1960–61 are the only postwar exceptions in which inter-state migration rates instead rose during a recession; the 1948–49 recession was also soon followed by the largest one-year jump in inter-state migration rates in their post-war sample. Prior to pandemic, the 1949 and 1958 recessions also saw the fastest national unemployment recovery rates and the greatest dispersion of recovery rates across states, as seen in Figure 9.

Section III.D State vs. National Recessions and Recoveries

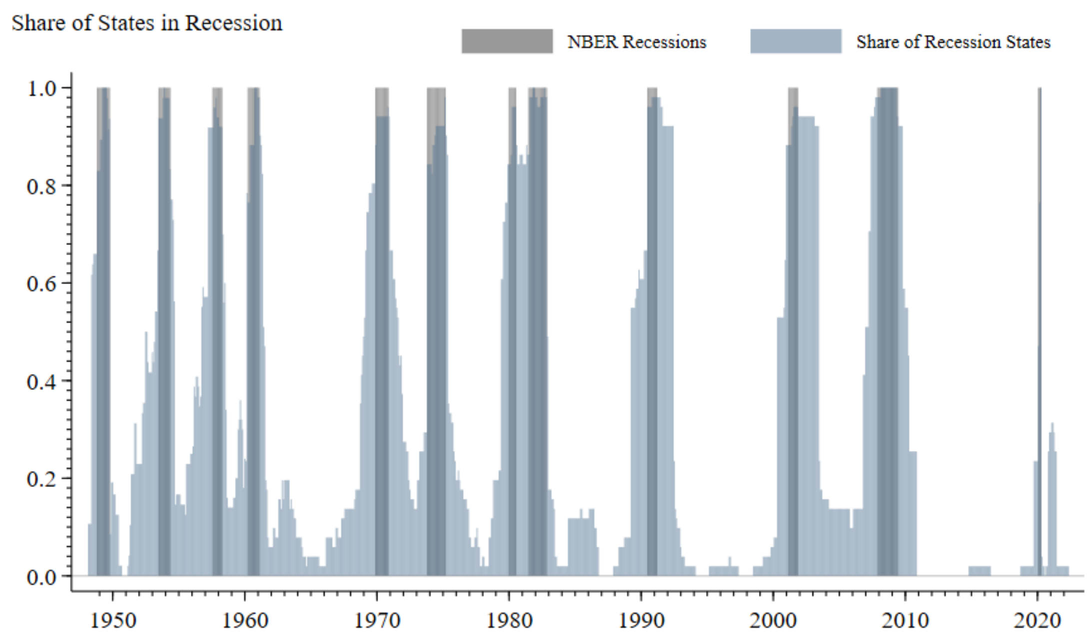
One possible explanation behind the more disparate pace of recovery across states in the 1940s and 1950s might simply be that some states never entered a recession and, as a result, their unemployment rates remained relatively flat during national recoveries, or even began rising. With a flat unemployment rate, equation (6) would estimate a very slow "recovery rate" during the national recovery, while a rising unemployment rate would generate a negative "recovery rate." A greater share of states being relatively out of sync with the national business cycle in the 1940s and 1950s could thus generate the earlier dispersion of recovery rates depicted in Figure 8. To explore this question, we first use our state recession dates to compute the share of states determined to be in a recession in each month and study how this share varies across the national business cycle.

Figure 10 plots the share of states identified as currently in a recession every month along with national NBER recession dates (gray bars).[38] The peak share of states experiencing a recession is roughly the same across the pre-1960s recessions and subsequent recessions, underscoring that the early post-war dispersion of recovery rates seen in Figure 8 is not simply a product of many states not experiencing recessions during the national recessions of the 1940s–50s. Another notable feature of Figure 10 is that a number of national "recovery" periods show numerous states have yet to see unemployment recover. In particular, a sizable share of states remain coded as still experiencing a recession throughout the national business cycle expansions of 1954-57, 1958-60, 1970-73, and 2001-07. And a far greater share of states remain coded as in a recession in the two

[38]Though it is hard to see visually in the figure, 100% of states are being coded as "in recession" during the COVID-19 pandemic recession. The secondary rise in the share of states being coded as "in recession" after the initial onset of the pandemic occurs during the severe third national COVID wave during November 2020–February 2021.

years following the 1990-91 and 2001 recessions than any of the other NBER recessions, consistent with the particularly slow, uniformly paced state-level recoveries following these more recent downturns, as seen in Figure 8. Broadly speaking, national recessions appear to be experienced more uniformly across states as recessions than do U.S. business cycle expansions—particularly so in the first half of the post-war sample.[39]

Figure 10: Share of U.S. States in Recession, 1948-2022



Notes: State-level recession dates are estimated from the fitted claims-based unemployment rate for each state using the DNS algorithm. The DNS algorithm parameter is adjusted for each state proportionate to its average level of unemployment over the entire time period; see Appendix A.5 for details. Due to data limitations in nonfarm payroll employment, not all states are included early in this sample but are added when feasible; see footnote 6 for details.

A final concern we investigate is whether the early post-war dispersion in recovery rates depicted in Figure 8 is simply an artifact of using uniform U.S. recession dates to calculate state recovery rates, high and stable share of states in recession during U.S. recessions notwithstanding. We instead analyze recovery paces normalized to state-specific business cycle troughs that occur near the end of national recessions. We match state business cycles to national business cycles by limiting our focus to state trough dates identified by the DNS algorithm within a +/-12-month

[39]As a robustness check, we also construct a version of Figure 10 that instead uses our unfitted claims-based unemployment rates. Using our fitted series for state-level recession dating might be misleading if the inclusion of the U.S. unemployment rate as a regressor in (2) causes state unemployment rates to track the national rate too closely in the pre-1976 out-of-sample predictions. These alternative state recession shares, which are plotted in Appendix Figure B.7, are broadly consistent with those in Figure 10.

window around the national trough dates identified from the official U.S. unemployment rate, as reported in Table 2.[40] In the case of double-dip recessions identified within each 12-month window, we only measure the change in unemployment from the second trough.

Figure 11 depicts the cumulative log point change in each state's fitted claims-based unemployment rate relative to that state's business cycle trough date, when its unemployment rate almost always hits a local maximum. Figure 11 shows that the headline results depicted in Figure 8 are robust to the choice of state or national recession dates: Indexing to state-specific trough dates, there is again far more dispersion in the pace of states' unemployment recoveries following the 1948-49, 1953-54, and 1957-58 recessions than during the recoveries from subsequent recessions—until the heterogeneity in recovery rates comes roaring back during the pandemic. And as in Figure 8, albeit more visible here, Figure 11 shows a more moderate degree of dispersion in state recovery rates following the 1960-61, 1969-70, 1973-75, and 1981-82 recessions, followed by much more consistent, slower paces of recovery following the 1990-91, 2001, and 2007-09 recessions. Pandemic recession aside, Figure 11 again appears to reflect the emergence of a national business cycle that is increasingly experienced uniformly across almost all U.S. states in the late 20th century, mirroring the convergence of relative unemployment rates in Figure 3 and states' unemployment recovery rates in Figure 8.

Figure 11 also visually highlights the asymmetric speed with which our claims-based unemployment rates rise and fall, which is again consistent with dynamics of official unemployment rates. The bold black lines show the (unweighted) averages of the state-level changes in log unemployment, which underscore that unemployment tends to rise much faster during recessions than it falls during ensuing expansions, as seen for the U.S. unemployment rate and claims-based unemployment rate in Figure 1.[41] This is an important empirical regularity at both the state and national level that should be reflected in models of the business cycle, as Dupraz, Nakamura, and Steinsson (2023) emphasize at the national level; in particular, these asymmetries favor unemployment models where recessionary shocks have a lasting impact on workers' employment outcomes.[42]

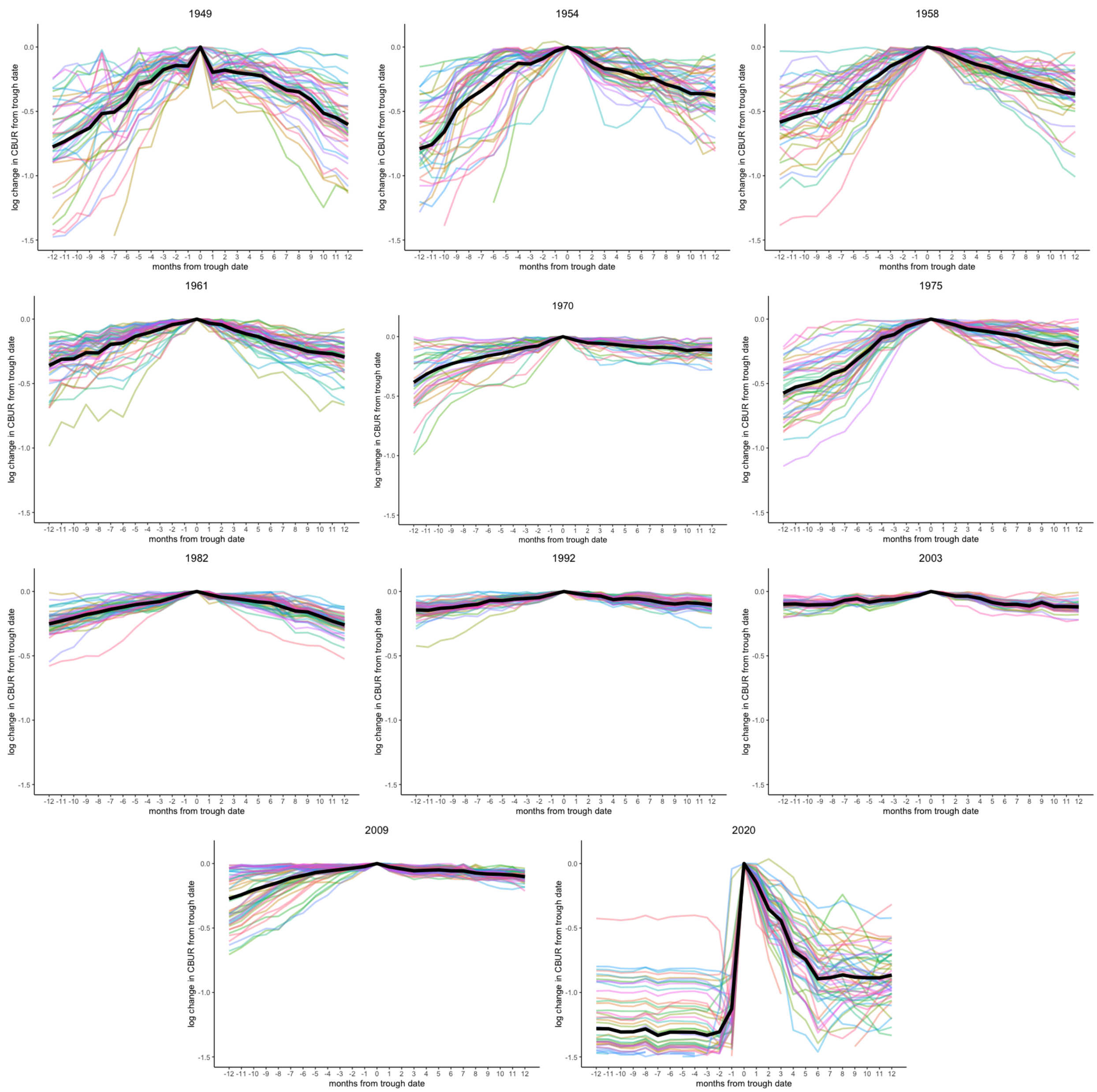
Lastly, Figure 11 shows that recessions with greater dispersion in recovery rates are also clearly led by more varied rates of unemployment rising across states; the early post-war heterogeneity in recovery rates seen in Figure 8 may thus partly be driven by greater variation in the magnitudes of shocks to unemployment across states. Consistent with this observation, the (pre-pandemic) standard deviation of relative unemployment rates reported in Table 1 are highest around the U.S. recessions of 1948-49 and 1957-58, which were followed by unusually fast, varied recovery rates.

[40] We marginally relax this window to 13 months for the Great Recession, as an unusually large share of states (13 of 50) are identified as experiencing troughs exactly 13 months from the national trough of October 2009; the latter is an unusual case of a relatively "flat trough" and we thought it imprudent to throw away a quarter of those observations.

[41] This tendency did, however, dissipate during the Great Moderation before remerging in the Great Recession.

[42] Gorry, Munro, and vom Lehn (2020) develop an alternative labor search model that yields such shock propagation.

Figure 11: Unemployment Recoveries from States' Peak Unemployment Rate

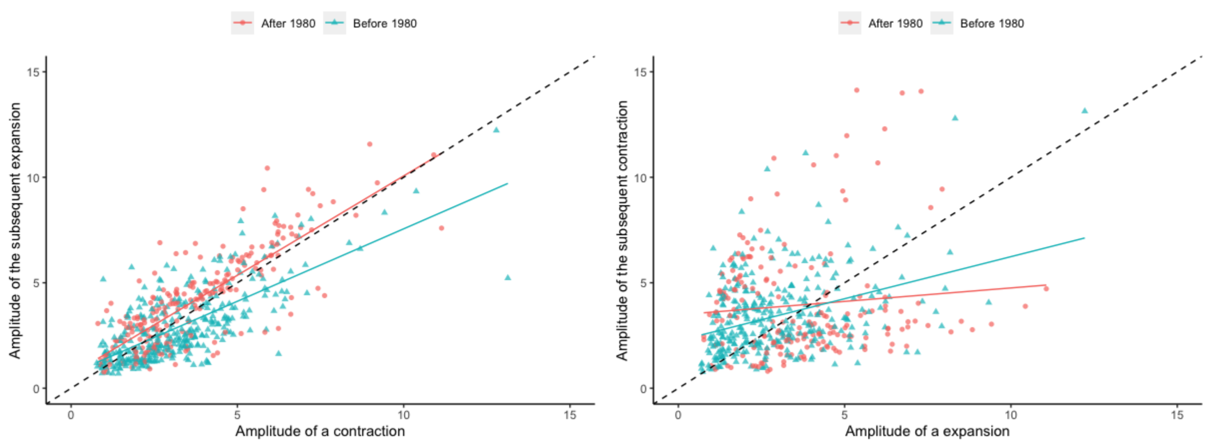


Notes: Unemployment recoveries are measured as the cumulative log point change in each state's fitted claims-based unemployment rate relative to that state's peak unemployment rate (normalized to $t = 0$). Peaks in the fitted claims-based unemployment rates are identified within +/-12 months of the peak U.S. unemployment rate for each recession. In the case of double-dip recessions identified within the +/-12-month window, we measure the cumulative change from the second peak in unemployment. Bold black lines are unweighted averages across states for each recession.

The cross-state convergence in the amplitude of unemployment rising during recessions and then falling during expansions, particularly since the late 1950s but also since the late 1970s, seems potentially important to the emergence of a more uniformly experienced post-war business cycle.

To cleanly study this evolving dynamic, we calculate state-specific cumulative changes in unemployment across postwar recessions and expansions, following Dupraz, Nakamura, and Steinsson (2023) but at the state level, using our state business cycle dates for the analysis.[43] Dupraz, Nakamura, and Steinsson (2023) document that the rise in the U.S. unemployment rate during recessions is highly correlated with the decrease during the subsequent expansion, whereas the decrease in unemployment during expansions is uncorrelated with the rise in unemployment during the following recession; this asymmetry in the amplitude of unemployment supports the "plucking model" of business cycles of Friedman (1993), in which cyclical shocks pull output below potential, but the magnitude of shocks is unrelated to the strength of preceding expansions.

Figure 12: Amplitude of State-Level Claims-Based Unemployment Rates



Notes: The amplitude of contractions and expansions are measured as the percentage point change in our fitted claims-based unemployment rates between state peak and trough dates. We identify state-level recession dates using the DNS dating algorithm on state-level CBURs as in Figure 10. Data points before 1980 are plotted in blue triangles and post-1979 data are plotted in red circles. OLS regression lines are plotted separately for each sample in both panels (solid lines). Dashed lines are 45-degree lines.

The left panel of Figure 12 plots the amplitude of states' claims-based unemployment rates rising in contractions (x-axis) against the fall in unemployment during the ensuing expansion (y-axis); the right panel plots the amplitude of states' unemployment falling during expansions against the rise during the ensuing contraction. Based on the evidence on the convergence in amplitude dynamics from Figure 11, we differentiate between data before and after January 1980; data points up through 1979 are plotted as blue triangles and data for 1980 onward are plotted in red circles.

The left panel of Figure 12 shows that our state-level claims-based unemployment rates also exhibit a strong positive correlation between the amplitude of unemployment rising during contractions and falling in subsequent recoveries; the association is significant at the 1% level for both

[43] We document that our claims-based unemployment rates emit the same amplitude dynamics at the national level; see Appendix B.4. Tasci and Zevanove (2019) also study these dynamics at the state level for 1976–2018, a sample limited by unemployment data availability; we find similar results over this more recent sample.

the pre- and post-1980 samples. But this relationship has strengthened across states since 1980, with the correlation coefficient approaching unity: States are increasingly experiencing more uniform degrees of recovery in recent decades. In other words, they completely recover regardless of how much unemployment rises.[44] The flatter amplitude relationship in the pre-1980 data shows that unemployment not only tended to rise faster than it recovered, as seen in Figure 11, but that more states had not fully recovered when their next recession hit; Figure 10 similarly showed a higher average share of states coded as in recession during U.S. expansions before 1980 than after. The increasingly uniform degree, not just speed, of states' unemployment recoveries also points to the emergence of a national business cycle experienced more uniformly across state, and may also help explain the diminishing role of interstate migration as a margin of adjustment to adverse shocks since the early 1980s.

The right panel of Figure 12 shows that our claims-based unemployment rates exhibit an insignificant correlation between the amplitude of unemployment in expansions and subsequent contractions for the post-1980 sample, but a positive correlation in the pre-1980 sample—one that is significant at the 1% level. Put differently, our historical state-level data show that the evidence in favor of the plucking model has actually strengthened since the late 1970s. Through the lens of microfounded plucking model of Dupraz, Nakamura, and Steinsson (2023), which incorporates downward nominal wage rigidities into a search model to generate comparable amplitude dynamics, this implies that welfare gains from stabilization policy have, if anything, risen since the 1970s.

Section IV. The Emergence of a Uniform National Business Cycle

We have documented, over the post-war era: 1) a trend decrease in the standard deviation of relative employment growth and unemployment across states, both during recessions and expansions; 2) a marked attenuation of relative employment and relative population responses to state-specific demand shocks, whereas relative unemployment responses have only become more gradual and persistent; and 3) convergence across states in both the speed and degree to which unemployment recovers after recessions. What can explain these related trends all pointing toward the emergence of a national business cycle experienced more uniformly across U.S. states?

Our state unemployment recovery rates enable us to explore other state-level features that might be influencing the emergence of a more uniform business cycle, particularly the deceleration and convergence in unemployment recoveries after the late 1950s. To understand the convergence in unemployment recoveries first requires finding state-level features that are correlated with recovery paces in the early post-war recessions. Second, to account for the convergence over time, it

[44] The slope of the regression fit increases from 0.69 in the pre-1980 sample to 0.94 in the post-1980 sample; these correlation coefficients reflect the degree to which states' unemployment rates had, on average, recovered from the previous recession when the next recession hit.

must also be the case that those features have become more similar across states. We start by examining three key features that might be responsible for the convergence of business cycles across states over the post-war era: The convergence in industrial composition across states, the rise and convergence in female labor force participation rates (LFPR) across states, and the convergence in income per capita across states, as poorer regions caught up to richer ones.

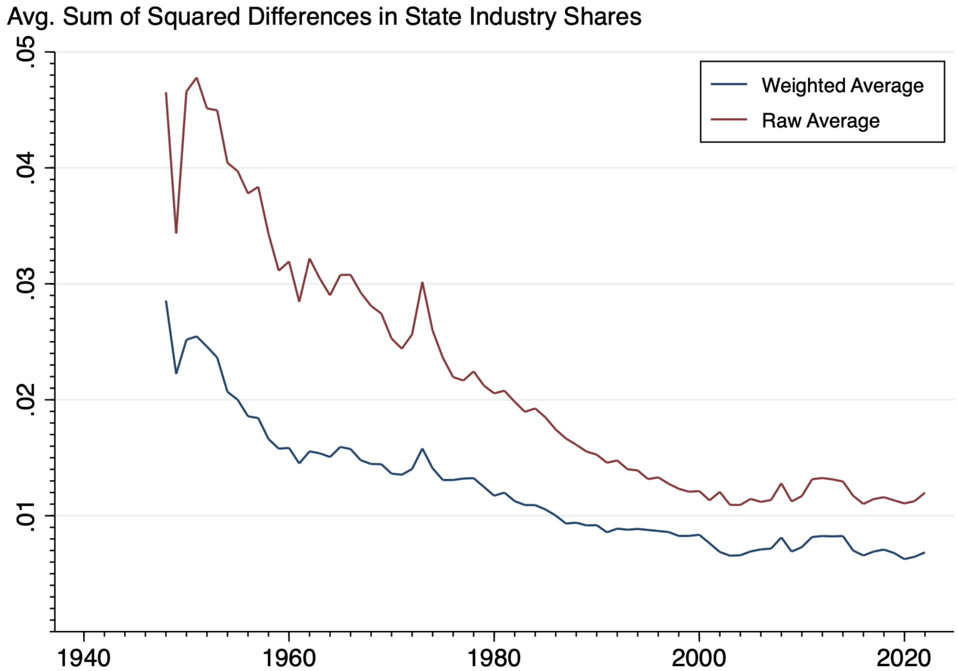
We first document that states' industrial composition have indeed become more similar over the post-war era, which could explain a convergence in both the speed and degree to which unemployment recovers after recessions, if it means states experience shocks more similarly. Moreover, states sharing a more common industrial composition in more recent decades could mean fewer better economic opportunities in other industries elsewhere, inviting less out-of-state migration and explaining the diminished response of relative population to local shocks in recent decades. Similar to the construction of our Bartik instrument above, we construct annual national industry shares of earnings for the nine industries consistently available in the REA and compute the same industry shares for every state in each year. We calculate the sum of squared difference in these state-year industry shares relative to the nation-year industry shares, and then average these differences across states. Figure 13 plots these annual averages—both unweighted and weighted by total earnings in each state—showing the evolving dispersion in industrial composition across states.

Figure 13 depicts a relatively rapid convergence in industrial composition from the start of our sample in the late 1940s to 1960, followed by a more gradual convergence from around 1960 to 2000; the degree of dispersion has been relatively narrow and stable since 2000. Notably, there was much greater dispersion in industrial composition across states during the faster, disparate recoveries from the early post-war recessions in the 1940s–50s. Over our sample of study, a big part of this industrial convergence was the transition of the U.S. economy from manufacturing toward services and a knowledge economy. As a simple proxy for exposure to the post-war convergence in industrial composition, we study states' manufacturing share of output.[45]

States with particularly low female LFPR at the start of the Gender Revolution saw on average faster growth in female employment rates in subsequent decades (Fukui, Nakamura, and Steinsson, 2023); this dynamic could be contributing to faster and more varied recovery rates earlier in our sample and more uniform, slower recovery rates after female LFPR stabilized around higher, more similar rates across states. To gauge any relationship between the convergences in female LFPR and unemployment recovery rates, we compute state-level female LFPR in 1950 using data from Fogli and Veldkamp (2011) as a proxy for differential exposure to this labor market shock, similar to the identification strategy used by Fukui, Nakamura, and Steinsson (2023). If there is an association between state-level recovery rates and female LFPR in the early post-war recessions, and these differences have diminished over time, this could be a mechanism driving more uniform

[45] Manufacturing accounts for roughly 8% of employment since the Great Recession, down from about 25% in 1948.

Figure 13: Differences in State-level Industrial Composition



Notes: Plots the sum of squared differences in state-level industry shares relative to national industry shares. Sample: 1948–2021.

unemployment dynamics across states in more recent decades.

Lastly, if poorer regions of the country had faster growth rates coming out of WWII, as they caught up to richer regions, this dynamic could also be contributing to faster and more varied unemployment recovery rates earlier in our sample of study; the gradually diminished persistence of employment growth seen in Figure 4 could reflect such “catch up." To gauge any relationship between convergence in regional income and unemployment recovery rates, we construct measures of relative income for each state—computed as a state’s per capita income divided by national per capita income—using REA data. Relative incomes have indeed converged across states: Computing state-level Gini coefficients using per capita earnings (weighted by state population) we see a downward trend, with Gini coefficients falling from 0.12 in 1950 to 0.09 in 2000. If early postwar recovery rates are correlated with states’ relative income, and relative incomes have converged over time, this could again be contributing to increasingly uniform unemployment dynamics.

To explore any relationship between these trends of convergence, we run a simple multivariate regression, regressing states’ average recovery pace in the first three “rapid" recoveries (1948–1958) on their manufacturing share of output (averaged over 1948-1957), female LFPR (in 1950), and relative income (averaged over 1950-1960). This empirical exercise is a simple way of gauging which state-level factors seem relevant to changing unemployment recovery rates, and thus the emergence of more uniform business cycle across states. Regression results are reported in Table 3.

Table 3: Recovery Rate Regressions

Recovery Pace	Coeff.	Std. Err.	p-value
Manufacturing Share	0.745***	0.132	0.000
Female LFPR	-0.004	0.005	0.374
Relative Income	0.232**	0.088	0.012

Notes: The dependent variable is the average state-level recovery rate over the national recoveries of 1949, 1954, and 1958. Stars *, **, and *** denote statistical significance at the 90%, 95%, and 99% levels, respectively.

Of these trends of convergence we examine, Table 3 shows that states' manufacturing share of output accounts for the largest share of the variance in unemployment recovery rates.[46] We find that female LFPR has no significant correlation with recovery paces in these early recessions.[47] Lastly, we find states' relative income to be significantly correlated with recovery paces, though contrary to the "catch up" dynamic we had in mind, relatively poorer states tended to recover more slowly after WWII. The explanatory power of relative incomes is considerably smaller than manufacturing shares, but the relationship is statistically significant at the 95% level. Thus, our results here suggest two partial explanations for states starting to experience recessions and recoveries more similarly: Industrial composition is an important determinant of recovery rates and state's industrial compositions have become more uniform over time, while states with higher relative incomes tended to recover faster, but such income disparities across states have also diminished.

Because the convergence of industrial composition has by far the most explanatory power for early post-war unemployment recovery rates, we explore how this relationship in particular has evolved. Figure 14 plots states' average manufacturing share against their pace of economic recovery for two time periods: The first three "rapid" recoveries (1948–1958) and the subsequent seven "slower" recoveries (1961–2020) in the left and right panels, respectively. Both samples show a positive, statistically significant correlation: States with larger manufacturing industries tend to experience more rapid recoveries in unemployment throughout the post-war era. The strength of this relationship diminishes substantially in the latter period, but remains positive and significant.[48]

There could be multiple mechanisms behind states with larger manufacturing industries experiencing faster recoveries. Manufacturing-intensive states might be more adversely impacted by recessions, generating a higher UR_0 in equation (6) and thus faster recoveries. Nearly every

[46]The Adj. R^2 of the regression in Table 3 is 0.523 versus an Adj. R^2 of 0.49 in a univariate regression with only average manufacturing shares on the right-hand side.

[47]Alternatively using the change in female LFPR from 1950 to 2000 also results in an insignificant correlation.

[48]The economic significance is somewhat weaker in the latter sample, but the linear relationships for both time periods are significant at the 1% level.

Figure 14: State-level Recovery Rates by Manufacturing Share of Output

(a) 1948–1958 Recoveries (b) 1961–2020 Recoveries

Notes: Recovery dates are estimated from the official U.S. unemployment rate using the DNS algorithm, see Table 2. Recovery from the pandemic recession and the 1980 recession are excluded in the right panel, see notes to Figure 7.

state is in a recession during most downturns, so for this explanation to hold, it must be that recessions in manufacturing-intensive states are more severe. Owyang, Piger, and Wall (2005) find that states with a higher manufacturing share of employment, particularly those in the Great Lakes region, contract relatively *faster* during recessions in their sample of study over 1979-2002, but our "very negative" Bartik shocks constructed over the full post-war sample show no such evidence of manufacturing-intensive states accounting for a disproportionate share of large negative shocks; see footnote 28. Another possible mechanism is that the pace of recovery is impacted by unique features of the manufacturing industry, for example, higher rates of unionization and/or the more intensive use of temporary layoffs; a national shift away from manufacturing could thus partly explain a slowdown in U.S. recoveries.[49] Regarding the convergence in states' recovery rates, the right panel of Figure 14 also highlights that being a manufacturing-intensive state confers less benefit in terms of faster recoveries in the post-1960 era; this could be a function of changes in within manufacturing, such as deunionization, decreased reliance on temporary layoffs, and more permanent displacements during downturns. These questions are worthy of further examination

[49] See Lilien (1980) for evidence of high temporary layoff rates in the manufacturing sector, see Nekoei and Weber (2015) for evidence that temporary layoffs experience shorter unemployment spells, and see Gorry, Munro, and vom Lehn (2020) for a theoretical discussion about the importance of permanent displacements for the propagation of unemployment shocks.

using our newly constructed dataset, but such an analysis is beyond the scope of this paper.

Many factors beyond the convergence in industrial composition and income per capita could also be contributing to the post-war emergence of a U.S. business cycle experienced more uniformly across states. For one, significant changes in the degree of interstate economic integration since World War II could partly explain the deceleration and convergence in unemployment recoveries documented above. The abrupt decrease in both the dispersion and average pace of unemployment recoveries after the 1950s depicted in Figure 8 occurs shortly after construction began on the Dwight D. Eisenhower Interstate Highway System, following the Federal Aid Highway Act of 1956. The U.S. has also become far more integrated financially, particularly following widespread interstate and intrastate banking deregulation in the late 1970s through the early 1990s (Mian, Sufi, and Verner, 2020); the timing of this shift toward a more deregulated, centralized, and nationwide banking system also lines up with the second convergence and deceleration in recovery rates between the 1973–75 and 1990–91 recessions, as seen in Figure 11. Fiscal federalism has also shifted toward greater economic integration across states, particularly since the late 1960s.

Related to such increases in integration, our findings are consistent with general features of economic network models: Shocks spread through the system more readily if the nodes of the network have higher connectedness (Kali and Reyes, 2010; Giroud and Mueller, 2019) and shocks with a more severe impact to more nodes in a network tend to be more severe for the network as a whole (Jackson, 2010). Historical data availability make it more challenging to quantify the potential relevance of these alternative mechanisms, but examining how the evolving network structure of the U.S. economy relates to the convergence of state-level recovery rates is an exciting direction for future research.

While tradable goods and services markets have become far more integrated, it is well documented that interstate migration has fallen in recent decades. But U.S. migration dynamics cannot seem to explain the timing of the sharp deceleration in recovery rates between the 1950s and 1960s; interstate migration rose rapidly following the Great Depression and then plateaued throughout the 1950s–70s before starting to fall in the 1980s (Rosenbloom and Sundstrom, 2004; Molloy, Smith, and Wozniak, 2011). If anything, the decreased persistence of employment following local shocks and convergence of state recovery rates—most pronounced since the Great Moderation—instead likely helps explain the decrease in interstate migration: If local job prospects gradually recover and economic prospects are not much better elsewhere, why incur the costs of relocating to a new state for economic reasons after a bad local shock?[50]

Lastly, while we have documented numerous trends pointing toward the emergence of a na-

[50] Saks and Wozniak (2011) find that procyclical inter-county migration patterns are fairly stable pre- and post-1980 samples, but they study national measures of labor market slack. Other factors are also surely contributing to falling migration rates e.g., home prices (Olney and Thompson, 2024).

tional business cycle experienced more uniformly across U.S. states, greater dispersion in state recovery rates abruptly reemerged following the pandemic. Does that undermine our narrative?

We think not. There was unquestionably far more variance in the magnitude of the pandemic shock across states than any other post-war recession, as underscored by the record spike in the dispersion of relative unemployment rates seen in Figure 3 and the wide-ranging increases in log unemployment seen in Figure 11. Some of that variation appears to again reflect states' differential exposure from industrial composition, this time via the collapse of in-person services. For instance, Nevada was poised for a particularly bad shock, given its heavy reliance on tourism and leisure and hospitality services; Las Vegas casinos were shut down between March and June 2020, and Nevada saw the highest spike in either its claims-based unemployment rate or official unemployment rate of any state, followed by one of the fastest recovery rates. The variable timing of states experiencing waves of COVID-19 cases and when (or if) states introduced lockdowns is also surely contributing to some of the heterogeneity in unemployment dynamics. Many post-war shocks should impact most states at roughly the same time, e.g., the 1973 oil embargo or the 2008 financial crisis, but the pandemic spread more slowly and variably throughout the country, partly influenced by prevailing temperatures; for instance, Figure 10 shows that roughly one-third of U.S. states are identified as experiencing a double-dip recession during the third wave of late 2020 and early 2021, which had a differential and staggered degree of regional spread. An outlier in so many respects, it is interesting but perhaps unsurprising that post-pandemic data show much more of a divergence between state and national business cycles than the otherwise clear pre-pandemic, post-1950s trend.

Section V. Concluding Remarks

In this paper we introduce a new state-level unemployment dataset spanning 1947–2023, a dataset constructed from historical UI claims data that we digitized from a series of primary sources and then merged with existing state-level data for 1971 onwards. We construct an (unfitted) claims-based unemployment rate series going back to January 1947 and an alternative “fitted" unemployment series going back to January 1948, estimating state unemployment rates from a statistical model of the dynamics between our claims-based unemployment rates and official measures of unemployment. We show that both claims-based unemployment rate series capture similar state and national business cycle dynamics as official data sources throughout overlapping samples. As the official BLS state unemployment rates only begin in January 1976, our dataset represents a sizable expansion of panel data availability for measuring labor market slack, offering practitioners nearly three additional decades of seasonally adjusted monthly state-level data.

Our claims-based unemployment series significantly expand the scope for studying the evolution of local labor market adjustments to state-specific demand shocks as well as the evolving relationship between state and national business cycles. With our dataset in hand, we document, over the post-war era, 1) a downward trend in the dispersion of relative employment growth and unemployment across states, during recessions and expansions; 2) a sharp attenuation of relative employment and relative population responses to state-specific demand shocks, whereas relative unemployment responses have been more stable; and 3) a convergence in both the speed and degree to which state unemployment rates recover after recessions. We argue that these are all related dynamics pointing toward the evolution of a national business cycle experienced more uniformly across states, especially since the late 1950s and also since the late 1970s.

We contribute some preliminary evidence on *why* a more uniform national business cycle emerged when it did. In particular, we provide some suggestive evidence that the convergence in industrial composition across states—particularly as relates to manufacturing shares of output—and the convergence in relative income per capita across states are related to the deceleration and convergence in unemployment recovery rates that we document above. By no means is our analysis exhaustive. Using our dataset to study the role of increased interconnectedness—be it transportation, financial, or fiscal—would be an exciting direction for future research. Similarly, our dataset would allow for a more comprehensive analysis of the evolving network structure of labor markets across U.S. states, which is beyond the scope of this paper. A related avenue for future research would be constructing backdated state coincident indexes using our historical claims-based unemployment rates.[51] More broadly, we hope our historical dataset of claims-based unemployment rates, derivative state recession dates, and the underlying digitized unemployment claims data prove useful for a wide range of empirical macroeconomic work using state-level panel data.

Lastly, this evolution from more disparate state business cycles into a more uniform national business cycle in the late 20th century has important implications for macroeconomic stabilization policy. Both the increased persistence of relative unemployment and the decrease in relative population responses to bad local shocks make a stronger case for macroeconomic stabilization policy at the federal level to help accelerate recoveries, as does the slowdown and convergence in states' unemployment recovery rates. The evolution of regional business cycles, however, has more nuanced implications for more regionally targeted stabilization policies. On one hand, if out-state migration is now a weaker adjustment mechanism, that might call for more targeted regional policies to help depressed labor markets in which non-participation is now bearing more of the adjustment. However, if states are now experiencing business cycles that are more aligned with the national cycle, that implies that federal-level policies might be sufficient to spur faster recoveries across most or all states. Thus, the trend towards weaker out-state migration and more uniform business cycles seem to push in opposite directions regarding the merits of regionally targeted fiscal policies.

[51] Nonfarm payroll employment and wage and salary disbursements, two of the three other inputs used in the latent factor model of Crone and Clayton-Matthews (2005), are also available at the state level back to 1948Q1.

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A New Claims-Based Unemployment Dataset: Application to Postwar Business Cycles for U.S. States

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ONLINE APPENDIX

Appendix A: Data Appendix

Appendix A.1. Historical Data Availability

Before we explain our digitization and data construction below, a brief word is merited on why we undertake this effort. The premise of our data contribution is that "official" state-level measures of unemployment—by which we mean produced and presently made available in digital form by federal statistical agencies—are not available for much of the early post-war era, particularly at a monthly frequency: The BLS LAUS unemployment rates start in January 1976 and the DOL state-level IUR data start in January 1986 (weekly) or February 1986 (monthly aggregation of weekly data). Some state-level data are available at annual frequencies for longer horizons, but annual unemployment data are not particularly well suited to business cycle analysis, such as our identification of inflection points and estimation of unemployment recovery rates (precision matters for both the amplitude change and recovery time) in Section III of the main paper. Our objective is to contribute state-level measures of unemployment that can consistently be constructed and made accessible at a (seasonally adjusted) monthly frequency back to the 1940s.

Even if it not presently available in digital form via federal statistics agencies, older historical ("pre-official") state-level data on UI claims and IURs were produced and published by federal statistics agencies: Our dataset is built from digitizing lots of such historical data from primary sources produced by federal agencies (see Appendix A.2). There were also older various attempts to produce "pre-official" state-level unemployment rates more in line with the CPS concept of unemployment, in part using UI claims data to try to work around the low sampling frequency of small states (or lack of state identifiers) in survey data—similar in spirit to the current BLS LAUS program's official state-level unemployment rates. But to the extent that long monthly samples of consistently constructed (or defined) historical data still exist but are not digitally accessible to practitioners (or were produced but have subsequently been lost on microfiche, floppy disks, or due to library downsizing), we think our digitization efforts and dataset are valuable contributions that will facilitate historical work with state-level panel data.

Historical availability for state-level UI claims and IUR data are much better at an annual frequency than monthly or weekly frequencies. For instance, annual state-level IUR data are presently available since 1947 from the ETA 394, and related data have been widely used in the related literature; for instance, Neumann and Topel (1991), Blank and Card (1991), and Davis, Loungani, and

Mahidhara (1997) use annual state-level IUR data dating back to 1948, 1977, and 1949, respectively.[52] But in the primary sources we collect, weekly or monthly data series are often reported inconsistently, either because data tables or data definitions change; see Blaustein (1980) for an overview of UI claims and IUR measurement and historical data availability. It would be possible to somewhat backdate monthly state-level IURs from the early 1960s through the 1970s using the primary sources we collected (principally those cited by Blaustein (1980)) but we are not aware of any primary source or presently available digitized data with *monthly* state-level data for the early 1980s. Even if the 1980s could be fully backdated, such a dataset would nonetheless significantly truncate the scope of our analysis of state-level unemployment dynamics in postwar recessions; to the best of our knowledge no monthly state-level IUR data were consistently reported in published primary sources during the 1948-49, 1953-54, or 1957-58 recessions and ensuing recoveries.

The story is a bit different with historical CPS-style measures of state-level unemployment, but the punchline is similar: Most of the “pre-official" data can either be manually constructed at an annual frequency or was produced (again at an annual frequency) but does not seem accessible in digital form today. As Blank and Card (1991) explain, the “CPS did not report state of residence for individuals in most states prior to 1977,” but it is possible to construct annual unemployment rates for a handful of larger states for which CPS state identifiers start in 1968. Similarly, Blanchard and Katz (1992) use annual unemployment rates from the CPS for a handful of larger states starting in 1970, before data are available for all states in 1976; for other smaller states, they use annual data starting in 1970 “constructed from the BLS unemployment rates for Labor Market Areas (LMAs) and were provided by Hugh Courtney" (p. 57). We have not been able to track down Hugh Courtney's data or similar “pre-official" state-level data. But following the approach in much of this literature, our out-of-sample test of our fitted claims-based unemployment rates constructs annual CPS-style measures of unemployment from the ASEC for a handful of larger states, whereas sampling frequency precludes constructing reliable unemployment rates for smaller states; see Appendix A.4 below. Moreover, this annual data approach for larger states is not feasible for the 1940s and 1950s. And for our purposes, annual data are better suited for data validation checks of our monthly claims-based unemployment rates than for business cycle analysis.

Lastly, if you are reading this and have monthly or weekly historical state-level unemployment data on hand (or on an old thumb drive) that you think other practitioners would benefit from, please send us an email; we would happily add it to our public dataset with proper attribution.

Appendix A.2. Data Construction

While unemployment claims data are collected and reported on a weekly basis, state-level unemployment claims aggregated to a monthly frequency are available in digital form through the U.S. Department of Labor (DOL) Employment and Training Administration's website beginning in January 1971.[53] We backdate this dataset from scanned versions of a series of earlier periodical government agency reports, digitizing monthly data for regular state program initial claims (IC) and continued claims (CC) back to December 1946 for all 50 states and Washington, D.C.[54]

[52]Data available here: https://oui.doleta.gov/unemploy/DataDownloads.asp
[53]Data available here: https://oui.doleta.gov/unemploy/claimssum.asp.
[54]Fortunately data for Alaska and Hawaii are consistently available before they become states.

Our preferred specification for the claims-based unemployment series uses a three-month centered moving average of the *IC* and *CC* data, so the start date for our digitization project was chosen so those moving averages are available starting in January 1947, in line with the availability of quarterly National Income and Product Account data. Digitizing data at a monthly frequency is obviously preferable to digitizing data at a weekly frequency, and monthly data are more easily seasonally adjusted with state-of-the-art methods like the Census Bureau's Win-X13 program.

Historical claims data for December 1946–October 1949 are digitized from the *Employment Securities Activities* (ESA) report published monthly by the Social Security Board's Bureau of Employment Security.[55] Data for November 1949–October 1963 are digitized from the *Labor Market and Employment Security* (LMES) report published monthly by the Bureau of Employment Security after it was transferred to the DOL. And data for November 1963 onward are digitized from the *Unemployment Insurance Statistics* (UIS) report published by the Bureau of Employment Security after it was transferred to the DOL Manpower Administration. Digitized scans of all issues of the ESA reports and most issues of the LMES and UIS reports were available through HathiTrust. We supplemented missing monthly tables with Interlibrary Loan Request or Google Book scans and data from the Unemployment Insurance Review (UIR) reports published by the Bureau of Employment Security.[56]

Overall, the image quality of the scans we were able to locate was generally quite good and data revisions appeared to be a minimal complication. In some cases we retrieved alternative copies of reports scanned by a different library to resolve uncertainties relating to legibility. We always used reported data on national aggregates to cross-check the sum of state and territory claims against total U.S. claims. The LMES and UIS data tables typically report the percentage change of IC and CC from the prior month alongside the reported number of claims, in which case we also calculated the corresponding percentage changes based on our digitized data as another cross-check. In cases where image quality presented serious legibility issues or a handful of observations for which data were missing, we used reported data on monthly or annual changes in claims to guide our digitization or impute missing observations.[57] Data were always digitized from the most recently published report available if multiple sources reported claims data for a certain month; data revisions seemed to be more of an issue for the earliest ESA reports than the LMES and UIS reports, but luckily later reports had multiyear tables with revised claims data for most of the observations we digitized from the ESA reports (for September 1947–October 1949).

To construct a complete time series for December 1946–January 2024, the data we digitized from these primary sources were merged with monthly data already digitized and available online from the DOL. To be as consistent as possible with data definitions, the more recent data pulled

[55]Federal unemployment insurance programs and the Bureau of Employment Security were transferred from the SSA–at the time part of the Federal Security Agency–to the DOL in 1949, and with it the publication of the *Economic Security Activities* report. ESA reports with tables of IC and CC by state are available back to February 1943, so it would be feasible to backdate these historical claims series slightly further.

[56]We are also especially grateful to Department of Labor librarian Erica Cooper for her assistance in providing digital images of tables from UIS reports for months not available through HathiTrust.

[57]For instance, claims data are missing for Rhode Island in September 1971, and a footnote in the UIS reports flagged "Data not available" for that state. But RI claims data are reported in the subsequent report for October 1971 along with the percentage change from September 1971, enabling us to impute the missing observation for September 1971 fairly accurately. Similarly, claims data are missing for a handful of states from the ESA reports for 1947, but we were able to fill in all missing observations using data on actual claims and the year-over-year change in the number of claims reported in ESA reports for 1948.

from the DOL were always restricted to IC and CC data from regular state programs only, excluding the federal Extended Benefits (EB) program, which was enacted in August 1970; state-level EB data are only available from the DOL for 1986 onwards and almost all of the newly digitized data predates the permanent federal EB program, rendering ours the most consistent data definition. We digitized claims data from the UIS reports through December 1972 to investigate how well our newly digitized data lined up with the existing DOL data, which starts in January 1971. Encouragingly, initial claims data for 1971–72 line up almost perfectly between the DOL data and that of the UIS reports: Only two of the more than 1,200 observations showed any discrepancy, and both were minor.[58] Given the seamless integration of the IC series, we merged our IC data digitized from the various primary sources for December 1946–December 1970 into the DOL data for January 1971–January 2024.

In a potential complication with this merge, the continuing claims data digitized from the UIS reports line up perfectly with the DOL data over 1971–72 for some states (e.g., CT, DE, MS, MT, PA, and OH) but are significantly higher in the UIS reports than the DOL data for certain other states (e.g., CA, KY, MI, MN, NE, NJ, OK, and VA) and are just slightly (less than 2%) off for many other states (e.g., AL, DE, KS, LA, MA, ME, ND, NH, NM, NV, NY, SD, SC, UT, VT, WI).[59] The state-specific discrepancies between the UIS reports and the DOL data could not be explained by certain states triggering EB, geographical regions, or political orientation. But encouragingly, all state-specific discrepancies between the two CC series disappear over a slightly longer horizon, by mid-1977 if not earlier.[60] As such, we extended our digitization of CC from the UIS reports through June 1977 and compared the two series. In almost all cases the two CC series seem to be off by a fairly stable level effect—perhaps suggesting a persistent misunderstanding of data reporting requirements at certain state UI offices—but capture similar business cycle fluctuations. For most states with discrepancies between the two continuing claims series, the CC data for 1971–77 digitized from the UIS reports looks less disjoint than the data from the DOL (e.g., AZ, CA, CT, DC, FL, KY, MN, NE, NJ, VA, and WA). And in a few states the level of continuing claims in the DOL data seems suspiciously lower than all other observations in surrounding decades (e.g., CA, KY, and WV). Outliers were also a more frequent cause for concern in the existing DOL data than the newly digitized CC data for 1971–77 (discussed below). As such, we use the CC data digitized from the UIS reports and preceding primary sources for December 1946–June 1977 as our preferred data specification, which is then merged into the DOL data for July 1977–January 2024.

Neither the newly digitized historical claims data nor the existing DOL claims data were seasonally adjusted. As such, we seasonally adjusted the monthly IC and CC data for regular state programs for the full 1946–2024 sample using the U.S. Census Bureau's X-13 ARIMA-SEATS

[58]For Louisiana in April 1971, the UIS reports reported 17,289 claims whereas the DOL data online showed 17,290 claims. And for Utah in August 1971, the UIS reports reported 6,026 claims whereas the DOL data online showed 6,006 claims. Both data discrepancies were off by less than 0.5%.

[59]Save the following three exceptions, UIS data for CC were consistently greater than or equal to the DOL data available online: The UIS reports showed 3,907 (-3.2%) fewer claims for FL in May 1972, 48 (-0.05%) fewer claims for LA in June 1972, and 33 (-0.03%) fewer claims for LA in July 1972 than the DOL data available online.

[60]There is one later CC discrepancy between the UIS reports and the DOL data available online for RI in April-June 1978. Rhode Island exhibited frequent reporting problems in the UIS reports during the 1970s, and the percentage change from June 1978 to July 1978 suggests that the previously reported UIS data are incorrect and the DOL data available online is accurate. Merging the UIS data into the DOL data online in mid-1977 obviates this particular data issue with the UIS reports for RI.

seasonal adjustment software. The unprecedented spike in initial claims starting in March 2020, however, throws off the seasonal adjustment factors in the lead up to the pandemic. We separately seasonally adjust data for December 1946–February 2020 to avoid this confounding influence, and then splice in data for March 2020–January 2024 from a separate seasonal adjustment of all data for December 1946–January 2024. We also ran tests for outliers using the Win-X13 program, which identified roughly 200 potential additive outliers and temporary change outliers from approximately 91,000 observations (newly digitized historical claims data and existing data combined). These flagged observations were roughly evenly distributed between our newly digitized data and the existing DOL data. We manually checked each potential outlier to determine if it seemed to represent a legitimate change in claims due to plausible or exigent economic circumstances (e.g. a surge in IC in Louisiana and Mississippi in September 2005 as a result of Hurricane Katrina) or a "fat thumb" data coding issue. We used several verification processes. The first was to double check the digitized data against primary source reports when available.[61] The second was to leverage the relationship between IC and CC, which should move in the same direction contemporaneously or with a one-month lag. For example, a spike in CC, without a concurrent or preceding spike in IC would suggest a data coding issue. And finally, we also examined nonfarm employment data to determine if there was a contemporaneous change in another labor market indicator, reflecting a legitimate change in labor market conditions.

Fat thumb coding issues were relatively rare but could be quite striking and misleading. As an extreme example of an obvious data coding issue identified in the DOL's online data, CC in Missouri in June of 1974 surged 4700% from 147,351 to 7,132,843, then collapsed again the following month to 145,365. There is no contemporaneous or lagged surge in IC. And this particular outlier is entirely implausible, as the population of Missouri was less than 5 million in 1974. This is a case in which we believe the first '7' is a typo and the observation should read '132,843,' which is in line with continuing claims data for the prior and subsequent months (147,351 and 145,365, respectively). It is worth noting that the U.S. total for CC in June of 1974 in the DOL's online data appears to be calculated as the sum of claims for states and territories, and was also flagged as a likely outlier. The U.S. total for CC of 12,910,365 is similarly well above CC data for the prior and subsequent months (roughly 82% higher than 7,110,210 and 7,222,162, respectively), and this is surely a related fat thumb error by aggregation. In the handful of cases thought to reflect such fat thumb coding errors, we replaced the seemingly spurious data with data observations from primary sources, adjusted the first digit when a monthly observation was off by an order of magnitude, or, if necessary, used a linear interpolation between CC data for the prior and subsequent month.[62] Only the following "fat thumb" outliers were identified and manually adjusted:

- DE: May 1974 CC: 42,850 to 24,850 (UIS report reads "24,850" not "42,850")
- DE: June 1981 CC: 6,433 to 36,433 (off by an order of magnitude)
- FL: March 1972 CC: 74,478 to 143,979 (UIS report reads "148,845" not "74,478')
- KY: February 1974 CC: 1,218,070 to 121,807 (off by an order of magnitude)

[61] To the best of our knowledge, undigitized historical claims data are only available through March 1980, when the UIS reports stopped being published.

[62] Linear interpolation was only needed for adjusting CC in the DOL data when the related UIS primary sources reflected regular state program claims as well as EB, and the UIS dynamics across the current, preceding, and subsequent month were mapped into the DOL data (state programs only, excluding EB) using observations for the preceding and subsequent month.

- MA: January 1978 IC: 53,954 to 50,829 (UIS report reads “50,829” not “53,954”)
- MA: February 1978 IC: 90,507 to 86,580 (UIS report reads “86,580” not “90,507”)
- MI: February 1973 CC: 546,984 to 255,264 (UIS report reads “413,526” not “546,984”)
- MO: February 1974 CC: 31,088 to 201,743 (off by an order of magnitude)
- MO: June 1974 CC: 7,132,843 to 132,843 (off by an order of magnitude)
- NY: September 1973 CC: 76,674 to 642,675 (UIS report reads “671,981” not “76,674”)
- NY: August 1977 CC: 1,762,353 to 1,162,353 UIS report (reads “1,162,353” not “1,762,353”)
- RI: May 1984 CC: 6,796 to 56,796 (off by an order of magnitude)

In addition to adjusting these fat thumb issues, we use monthly data on average weekly insured unemployment (AWIU) to interpolate CC data for Illinois in March–April 1977 and for Michigan in April–May 1977, around the merge of the digitized UIS data into the DOL's digitally available data. The UIS data for IL in April 1977 was flagged as an outlier, just as the UIS data series start lining up with the DOL's digitally available data in April–May 1977. The UIS data for IL is consistently higher than the DOL's digitally available data before the merge, but then spike erratically in March 1977 and crater implausibly in April 1977, before aligning at reasonable levels in May 1977. These movements in CC for March–April 1977 do not align with movements in the corresponding IC or AWIU data for IL. The UIS data for MI in April 1977 was also flagged as an outlier before the two series align perfectly starting in June 1977. Unlike the rest of of the UIS data for MI, which are consistently higher than the DOL data available online, the April and May readings in the UIS reports are much lower. The June 1977 report shows the reading of 498,892 (the same as the DOL data online) having fallen 10.8%, which would put the May reading around 559,296, instead of 317,385, as reported in the UIS reports. Again, these movements in CC for April–May 1977 do not align with movements in the corresponding IC or AWIU data for MI. To interpolate CC in these two cases, we use the UIS reports to calculate the average ratio of CC to AWIU across the two previous and two subsequent months relative to the two months in question, and then multiply the average ratio by AWIU in each of the months in question to back out an estimate of CC. The interpolated CC data for IL and MI are far more consistent with IC and AWIU dynamics throughout 1977.

Our judgement calls about data adjustments will modestly affect the unfitted claims-based unemployment series. However, data adjustments for 1976 onwards—after the BLS state unemployment rates are available—will have a negligible effect on our fitted out-of-sample state unemployment rates for January 1948–December 1975. Our fitting exercise will wash these things out in-sample insofar as they are erroneous data. When estimating equation (2) over January 1976–December 2023, erroneous data entering the claims-based unemployment rates on the right-hand side will only show up in the error term. Moreover, the official state unemployment rate being estimated on the left-hand side is also constructed in part from state unemployment insurance claims data subject to similar or identical fat thumb data coding issues.

After manually correcting these handful of fat thumb outliers we re-ran the seasonal adjustment (without hard coding for outliers) for the monthly IC and CC data over December 1946–January 2024, again separately seasonally adjusting data for December 1946–February 2020 to avoid the confounding influence of the pandemic spike in claims on seasonal factors. The seasonally adjusted time series for total U.S. IC and CC are constructed by summing the respective seasonally adjusted series for all 50 states plus Washington, D.C., as opposed to seasonally adjusting total U.S. claims.

Appendix A.3. Data Validation and Robustness Checks

The advantage to using unemployment claims as a proxy for the level of unemployment in constructing our claims-based unemployment rates simply boils down to historical data availability. But conceptual differences between the surveyed level of unemployment and the number of unemployment insurance claimants do raise several potential concerns, discussed in greater detail here. It should also be noted that there is no single objective measure of unemployment. Even the official unemployment rate must take stand on job search activity requirements for individuals to be counted as unemployed, which can be an important source of bias when measuring slack in the labor market from the headline unemployment rate, e.g., the unemployment rate being pushed down by discouraged workers dropping out of the labor force after the Great Recession.

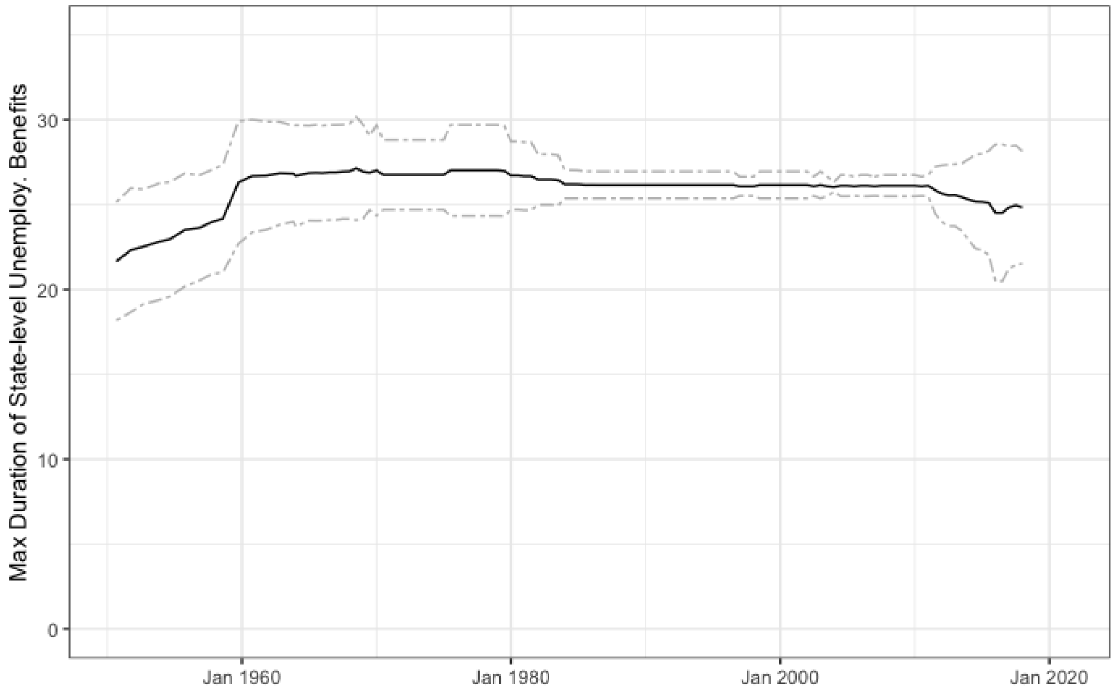
There are conceptual advantages and drawbacks alike to our approach relative to the BLS household survey methodology. Using actual claims as a proxy for unemployment leaves no margin for bias from respondents misunderstanding definitions or misreporting their circumstances, or from time-varying non-response rates to surveys, a growing concern with the CPS of late (Bernhardt, Munro, and Wolcott, 2023). On the other hand, state unemployment offices could misunderstand ETA's data definitions or incorrectly transcribe data. Another key difference arises from benefit duration limits: Unemployed workers who exhaust regular state benefits will drop from our measure of unemployment, whereas they would continue to be counted as unemployed by the BLS, provided they meet active search requirements. Official unemployment rates instead see workers drop from their headline measure if they do not report having searched for work in the previous four weeks.

As noted in Section I.C, our claims-based unemployment rate omits long-term unemployed workers who have exhausted benefits, just as the official IUR does. Such benefit exhaustion would only pose a serious challenge to our use of claims-based unemployment rates in studying state business cycles if there was considerable policy variation in the maximum duration of benefits influencing the number of UI recipients, which is not the case. We examine how the maximum duration of benefits for regular state programs have evolved over time using the State Unemployment Insurance Laws dataset compiled by Massenkoff (2021) for 1970–2018, which we extend back to 1947 from the DOL reports. Figure A.1 depicts the average maximum duration of regular state benefits for all 50 states with one standard deviation bands; there is relatively little variation in maximum durations across states in a given year or across years. The average maximum duration of benefits begins at approximately 22 weeks in 1950, and rises to approximately 26 weeks by 1960. From 1960 to 2011 it remains quite stable around 26 weeks and declines slightly to 25 weeks when a handful states began to reduce benefits during the recovery from the Great Recession.

Figure A.2 depicts the share of unemployed workers who have been out of work for 27 weeks or longer, and would thus have exhausted regular state benefits for most of our data sample. With the notable exception of the Great Recession, the long-term unemployed typically only account for 5% to 25% of unemployed workers. Moreover, excluding the long-term unemployed has very little effect on unemployment dynamics and inflection points at the national level. Over January 1948–December 2019, the correlation between the log level of unemployed workers and the log level of unemployed workers who have been out of work for 26 weeks or fewer is 0.98.

Given the relative stability of the maximum duration of benefits for regular state programs and the typically small share of long-term unemployed workers who would be affected by benefit duration limits, legislative changes to maximum duration should have a limited influence over time

Figure A.1: Maximum Duration of Regular State Unemployment Benefits, 1950–2018



Notes: This figure reports the unweighted average of each state's maximum benefit duration for regular UI programs (solid black line) along with one-standard-deviation bands (gray dashed lines). Sample: January 1950–December 2018. Data sources: Massenkoff (2021) and the DOL ETA.

variation in continued claims.

While less of a concern than benefit extensions or exhaustion influencing the volume of continued claims, the extended Massenkoff (2021) dataset also reassuringly shows minimal policy variation in "waiting periods" or a "waiting week" between job loss and eligibility for unemployment benefits, which could modesty affect timing. Since the mid-1950s, all U.S. states have implemented either a one-week waiting period or no waiting period requirement. A handful of states implemented a two-week waiting period at the start of our sample, but these were universally phased out by the late 1940s or early 1950s.[63] Twenty four states never changed their waiting period policies throughout our sample, with a plurality of states consistently imposing a one-week waiting period.[64] Eight states changed their waiting period policy once, eleven states changed their policy twice, and five states changed their policy three times. Only North Carolina and Wisconsin changed waiting period requirements more than three times over this sample. There were only 58 waiting period policy changes over 1948–2018, just 1.6% of the 3,550 state-year observations.

We also explore the influence of these state-level UI policy changes on our claims-based un-

[63]The following states had a two-week waiting periods in the late 1940s: CO, GA, MN, MS, MT, NE, OH, WI, and WY. Colorado and Montana were the last states to still require a two-week waiting period, both of which were reduced to a one-week requirement between 1954 and 1955.

[64]The following states had a one-week waiting periods throughout the entire 1947–2018 sample: AK, AR, AZ, CA, FL, HI, ID, IL, IN, KS, LA, MO, ND, NM, NY, OK, OR, RI, SD, TN, UT, WA, and WV. Maryland never had a waiting period requirement over this sample.

Figure A.2: Long-term Unemployment as a Share of Total Unemployment

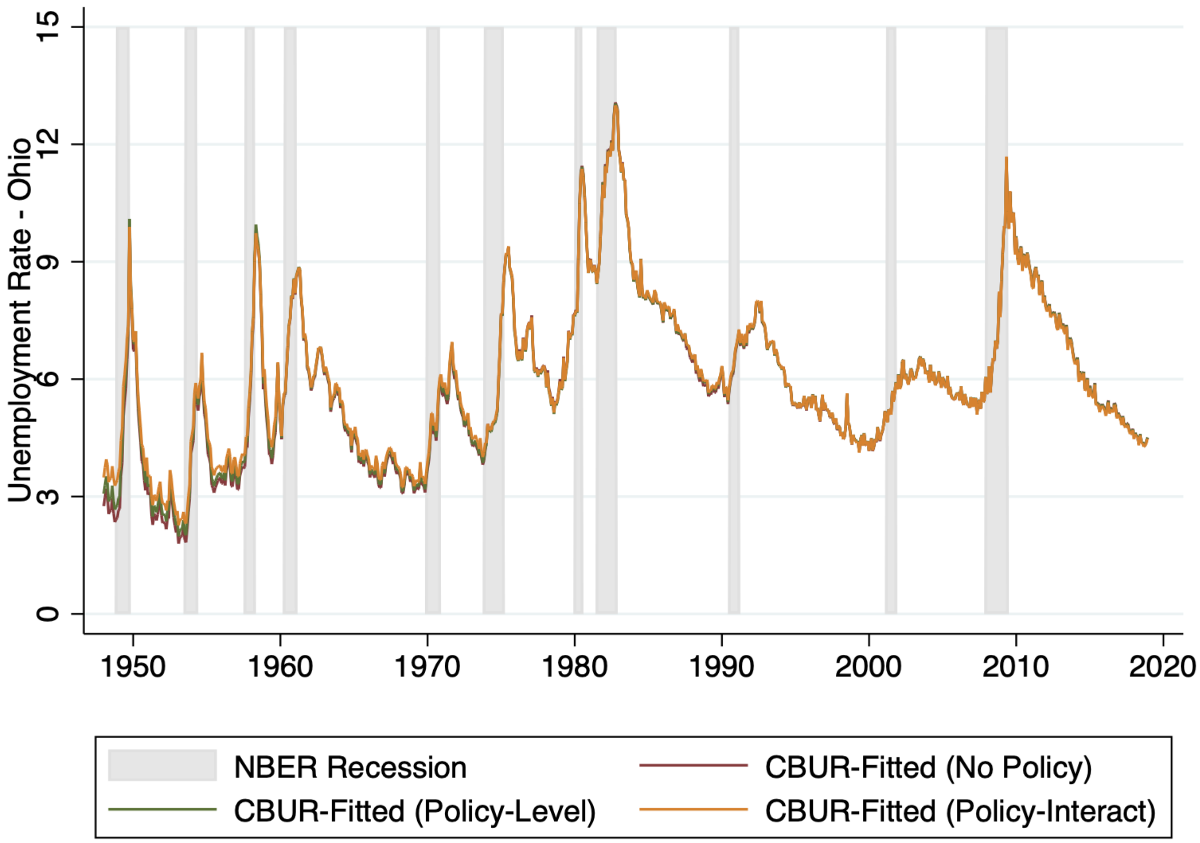
Notes: This figure reports the share of unemployed workers who have been unemployed for 27 weeks or longer relative to all unemployed workers. Sample: January 1948–December 2019. Data source: BLS.

employment rate by directly controlling for them in the fitting regressions modified from (2). We separately add these UI policy controls for maximum benefit duration and waiting periods in either levels or as interaction terms with the other regressors in (2). Continuing with Ohio as our representative example, Figure A.3 plots our fitted claims-based unemployment rate (seen in Figure 2 of the main paper) along with alternative fitted claims-based unemployment rates estimated with these additional UI policy controls. The policy controls do not generate meaningful differences in the fitted series, so we leave them out of our preferred specification for simplicity.

As noted in Section I.C, the expansion of state UI programs raises another potential concern with using unemployment claims as a proxy for unemployment. To examine any potential concerning influence of expansions in UI coverage during the early post-war era for our claims-based unemployment rate, we first digitize monthly U.S. IUR data back to January 1959 from the LMES and UIS reports (1963–74). To ensure a good merge with the existing data for the U.S. available for January 1971 onward, we digitize data through December 1973; the merge is nearly seamless in the two years of overlapping (not seasonally adjusted) data (correlation of 99.8%). We then seasonally adjust the digitized data for January 1959–December 1973 using the Census Win-X 13 program and merge this seasonally adjusted backdated data with the official seasonally adjusted data starting in 1971; this backdated series is plotted in Figure 1(b) of the main paper.

We also digitize annual data on covered employment for regular state programs back to 1945 to examine whether UI program expansions induce policy variation in the ratio of covered employ-

Figure A.3: Ohio Fitted CBUR with and without UI policy controls



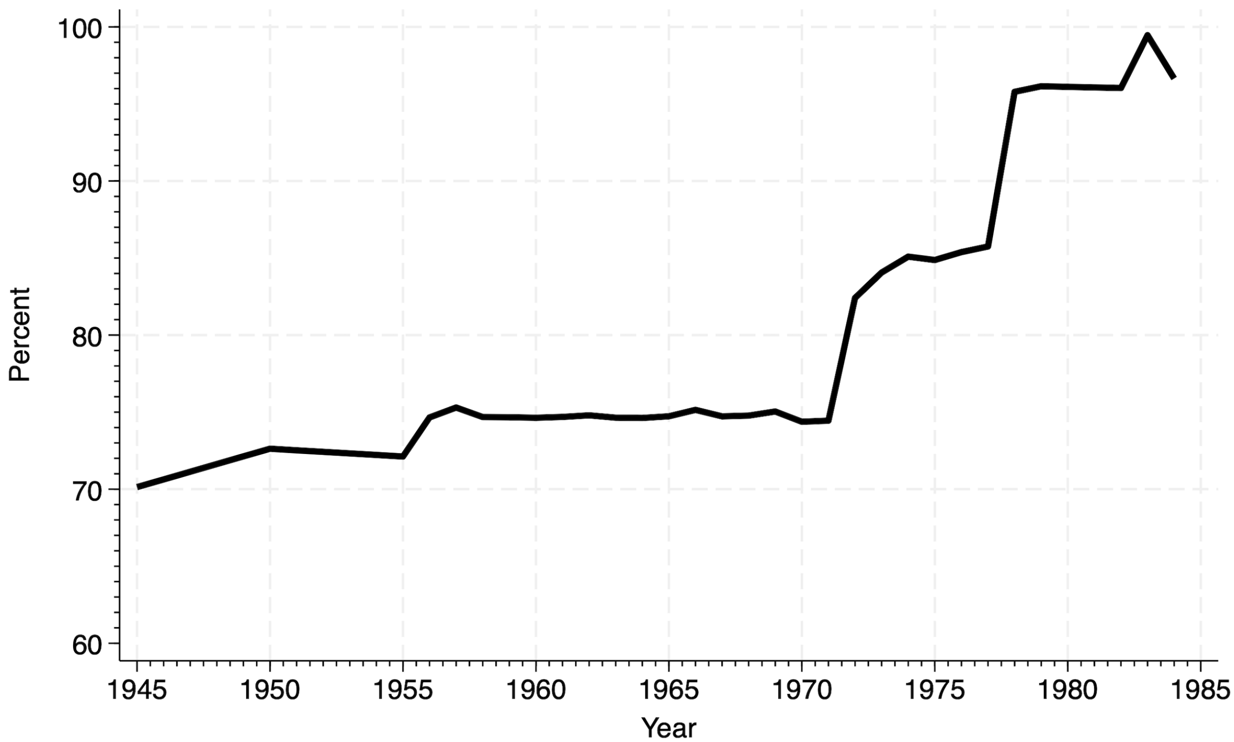
Notes: CBUR-Fitted (Policy-Level) denotes the fitted CBUR with UI policy regressors in levels and CBUR-Fitted (Policy-Interact) denotes the fitted CBUR with UI policy variables interacted with the other regressors in (2).

ment to nonfarm payroll employment of a concerning cyclical nature; the data come from various issues of the *Social Security Bulletin Annual Statistical Supplement* (1963–86).[65] Figure A.4 plots the ratio of U.S. covered employment to nonfarm payroll employment, which was relatively stable at roughly 72-75% over 1950–1971, then abruptly jumped to 84-85% over 1973-1977 and again jumps to 96-99% over 1978–84. These two level shifts in 1972–73 and 1977-88 were driven by federal legislation, as was a much smaller increase around 1954-55; and none seem particularly threatening for the construction of our claims-based unemployment rates, as explained below.

The Employment Security Amendments of 1970 (PL 91-373), enacted August 10, 1970, coerced states into extending coverage to “State hospitals and State institutions of higher learning and to certain nonprofit organizations," which was estimated to expand covered employment by roughly 3 million workers (less than 4% of the civilian labor force in 1970) (*Social Security Bulletin*, November 1970, p. 30). The legislation was part of a slow-moving attempt to update and reform social insurance programs, and established a permeant extended benefits program and in-

[65]We only digitize data up through 1984 because of a definitional change in subsequent reports, which start including federal programs.

Figure A.4: Covered Employment Relative to Nonfarm Payroll Employment



Notes: Covered employment is the annual average monthly number of employees covered by regular state programs, excluding federal programs but including state and local government workers where covered by state law. Sample: 1945, 1950, and 1955–1984; values for 1946–1949 are linearly interpolated from observations for 1945 and 1950 and values for 1951–54 are linearly interpolated from observations for 1950 and 1955. Source: Social Security Bulletin Annual Statistical Supplement, various issues.

creased the federal unemployment tax rate to shore up the financing of the program, in addition to expanding coverage. According to *CQ Almanac*, “both the Kennedy and Johnson Administrations [had] sought to broaden the unemployment compensation system," but this legislation died in congress, and the enacted reforms resembled earlier proposals from the Johnson Administration in 1968 budget request.[66]

And the Unemployment Compensation Amendments of 1976 (PL 94-566), enacted October 20, 1976, coerced states into extending coverage to “State and local government employees and to nonprofit elementary and secondary schools employing four or more persons," which was estimated to expand coverage to roughly 8.3 million state and local government workers (less than 9% of the civilian labor force in 1976) (*Social Security Bulletin*, February 1977, p. 24). According to *CQ Almanac*, the legislation was in part aimed at addressing “recession-induced deficits in the federal and state unemployment trust funds" by expanding coverage and increasing federal unemployment taxes.[67] But the legislation was much more a reaction to budgetary pressures from the

[66]“Unemployment Compensation And Benefits Extended," *CQ Almanac 1970*, 26th ed., 10-289-10-293. Washington, DC: Congressional Quarterly, 1971.

[67]“Congress Revises Jobless Benefits System," *CQ Almanac 1976*, 32nd ed., 359-64. Washington, DC: Congres-

previous recession than to current cyclical unemployment.

There were three federal UI reforms enacted in 1954, one of which created the Unemployment Compensation for Federal Employees program and also had a significant effect on coverage through regular state UI programs. President Eisenhower enacted "An Act to extend and improve the unemployment compensation program" (PL 83-767) on September 1, 1954, which was estimated to expand coverage to roughly 2.5 million federal workers (which would not impact IUR) and 1.4 million private-sector employees (less than 3% of the civilian labor force in 1954) (*Social Security Bulletin*, November 1954, p. 18). The bill changed the Federal Unemployment Tax Act tax base by lowering the firm-size threshold for eligibility to four or more employees (down form eight or more), thus expanding private-sector coverage in regular state UI programs. The reforms of 1954 again appear to have largely been motivated by reforming and shoring up the financing of UI programs, not a cyclical response to unemployment.

These federal policy changes do not seem to be driven by contemporaneous state-level cyclical concerns, but rather longer-run improvements to UI programs and fiscal sustainability concerns. And none of these policy changes appear to have been explicitly targeted toward changing UI programs of certain states. The stable ratio of covered employment to nonfarm payroll employment, save the three federally induced level shifts around 1955, 1972, and 1977, reassures us that expansions of UI coverage are not introducing spurious cyclical variation in our claims-based unemployment rates, particularly in the early post-war era.

A good way to assess the extent to which these expansions in coverage are impacting our CBUR is to compare it against the IUR. Encouragingly, Figure 1(b) in the main paper shows that the U.S. IUR and CBUR series are highly correlated over these expansions in the 1970s and capture consistent timing and magnitude of business cycles. Similarly, Figure B.2 plots the HP-filtered cyclical components of the CBUR and IUR, and the series look almost identical—strong evidence that these expansions in coverage have minimal influence over cyclical fluctuations of our CBURs. This is perhaps not surprising given that the expansions represent small shares of the employment pool, and given the large cyclical fluctuations in the unemployment rate (e.g., the CBUR increased by about 110% during the 1970 recession). As noted elsewhere, it is also worth emphasizing that the construction of our fitted CBURs (or empirical exercises using relative CBURs) would difference out any common national expansions of coverage driven by federal policy.

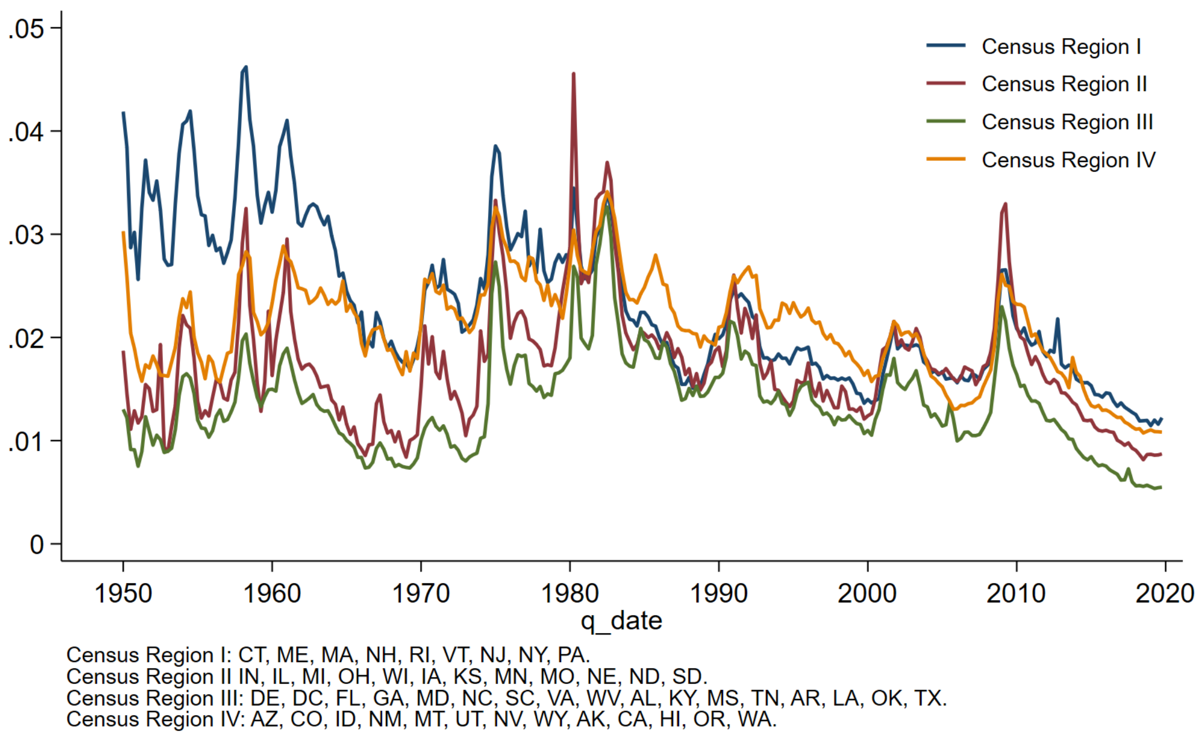
A final potential concern with using unemployment claims as a proxy for unemployment that we examine relates to time-varying take-up rates in state unemployment programs or denials of unemployment claims. Slow-moving changes in take-up rates and/or denial rates that are uniform across the country pose little threat to our empirical exercise, as they would resemble secular drift in trend unemployment without a first-order effect on unemployment recovery speeds or peaks and troughs identified by the DNS algorithm. Again, in the fitted claims-based unemployment rates, any uniform national effect will be differenced out in the term $(UR_{i,t}^{Claims} - UR_{US,t}^{Claims})$ of equation (2), and any residual level effect would be corrected for with the inclusion of the national unemployment rate on the right-hand side.

More abrupt changes in take-up rates and/or denial rates in only a subset of states would, however, potentially undermine inference from our claims-based unemployment rates. For instance, to the extent that racial discrimination affected take-up rates or denial rates differentially across regions, the Civil Rights Act of 1964 and federally enforced desegregation in the southern United

sional Quarterly, 1971.

States could have induced divergent trends in take-up and denial rates across states. Unfortunately, the LMES and UIS reports rarely report claims by race, and even data on claims or denials by race could fail to capture the effects of racial discrimination dissuading applications in the first place. Differential trends in unemployment insurance take-up and denials across race do not, however, appear pronounced in recent decades. Kuka and Stuart (2021) find that racial take-up gaps in unemployment insurance are relatively stable over 1986-2015, which the authors interpret as suggesting that take-up gaps "are explained by persistent economic or social factors." While there is a significant gap between UI take-up and receipt for white and black workers, Kuka and Stuart (2021) find that observed characteristics can explain 66% of the gap in take up and 81% of the gap in benefit receipt. They also find that fixed effects for the South have considerable predictive power for explaining racial UI gaps, whereas other regions don't have much explanatory power; the authors explain that "UI receipt and take-up is lower in the South, where unemployed Black individuals are much more likely to live."

Figure A.5: Initial Claims Per Capita by U.S. Census Region



Notes: Initial claims and total population are both summed across all states in each Census region and taken as a ratio. Sample: January 1948–December 2019.

As an additional empirical test predating their sample of study, we examine initial unemployment insurance claims per capita (for regular state programs) by Census region, which are plotted in Figure A.5 for 1948–2019. Reassuringly for our claims-based unemployment rates, IC per capita in the South (Region III, depicted in green) behave relatively similarly across the entire sample: They are consistently lower than IC per capita in the other three Census regions, they roughly follow the same inflection points as the other Census regions, and there is no discernible break in these dynamics following the passage of the Civil Rights Act. Interestingly, there is a great deal of co-movement in IC per capita across the four Census regions throughout this entire sample in spite of well documented differences in regional business cycles (Hamilton and Owyang, 2012).

Appendix A.4. Out-of-Sample Test of Fitted Claims-based Unemployment Rates

As noted in Section I.D, one important concern about the fitted claims-based unemployment rates is the underlying assumption of a stable empirical relationship in the pre-1976 out-of-sample period. Because the claims-based unemployment rates and official state-level unemployment rates are constructed from somewhat different data, they could respond differently to business cycle shocks. For example, it is possible that UI claims are more readily used by workers in manufacturing or unionized settings. If industrial composition or unionization rates experience noticeable changes over time, the assumption of a stable empirical relationship for out-of-sample forecasts could be problematic.[68] There could be numerous other possible mechanisms beyond this example that could result in a time-varying empirical relationship between claims-based unemployment rates and official unemployment rates. In this section we explicitly test the goodness of fit for the out-of-sample fitted claims-based unemployment rates using an alternative data source that is lower frequency, but goes back to 1962, 14 years earlier than the start of official state-level unemployment rates.

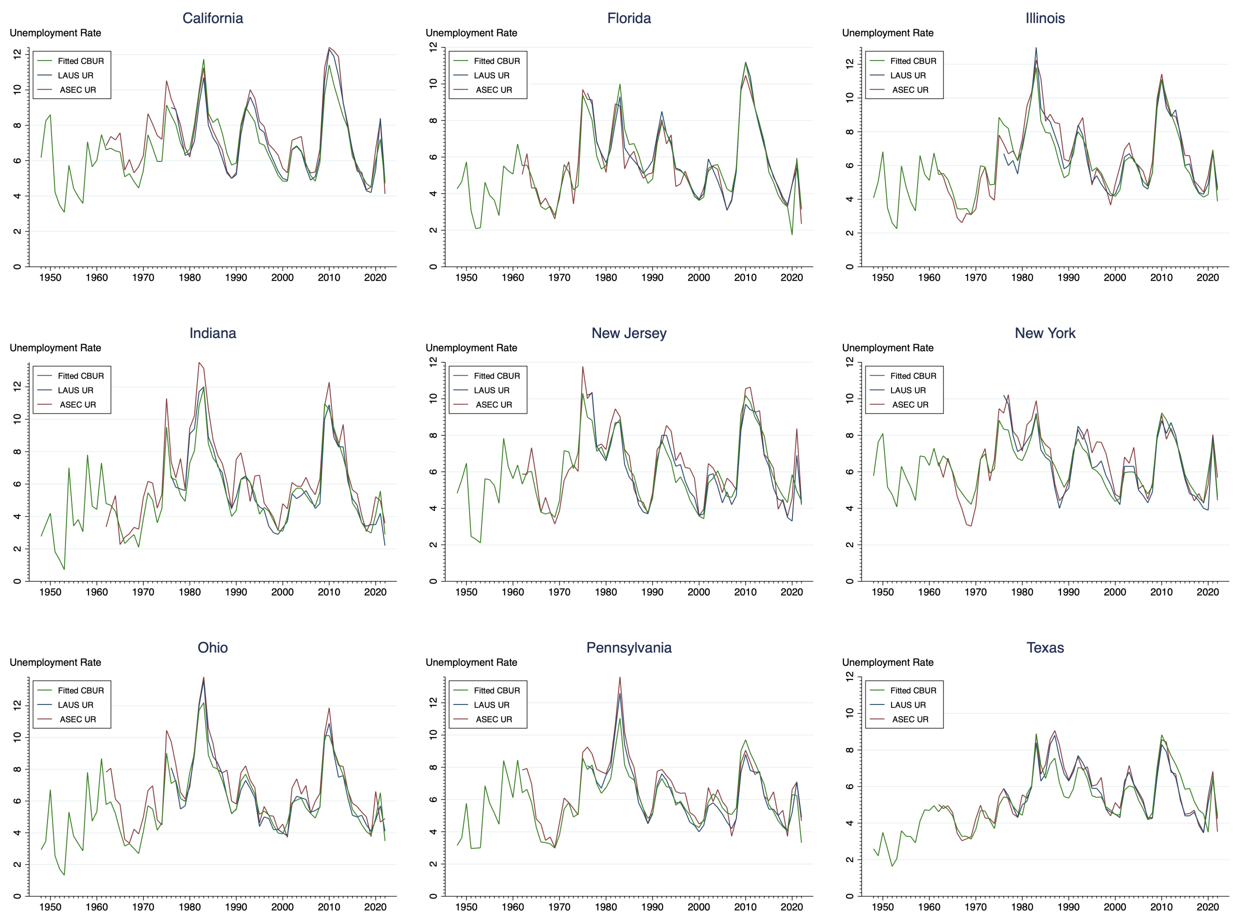
The Annual Social and Economic Supplement (ASEC) of the CPS are available on IPUMS back to 1962. These data are recorded in March each year. Using the labor force questions in the ASEC, we can compute employment status and annual snapshots of state-level "unemployment rates." It is important to emphasize that these ASEC unemployment rates are not equivalent to the official state-level unemployment rates for 1976 onwards, because the BLS uses other data beyond the CPS to compute those unemployment rates. The BLS uses multiple data sources in part because sample sizes in the CPS (and ASEC) can be quite small at the state level, leading to obvious issues when computing statistics like the unemployment rate, particularly for smaller states. These concerns should be ameliorated to some degree when focusing solely on larger states.

For a number of states in the ASEC, geographic groupings are redefined sometime between 1962 and 1980; for example, MI is grouped by itself in the early 1960s, then is grouped with WI for a few years, then goes back to being grouped alone thereafter. However, there are 11 states—mostly large ones—that are continuously grouped individually over the life of the ASEC: CA, CT, DC, FL, IN, NJ, NY, OH, PA, and TX. From this group, we drop CT and DC because of small sample concerns: The total ASEC sample size in 1962 is 71,741, but CT and DC have samples of only 981 and 318, respectively. With labor force participation of, say, 60%, and an unemployment rate of 5%, the total number of unemployed individuals in the ASEC from CT and DC would be roughly 30 and 10, respectively; this would likely introduce a non-trivial degree of sampling noise for these smaller states. The next smallest states in terms of sample size in 1962 are IN (1,428) and FL (1,930); these sample sizes certainly are not large, but we include IN and FL nonetheless. All other continuously individually grouped states have sample sizes of over 2,000 in 1962.

[68] That said, it should also be emphasized that many states experienced dramatic changes in industrial composition across the in-sample period. Ohio is a good example of this: The manufacturing share of employment declined by roughly two-thirds over 1976–2022. In the fitting exercise, we do not control for state-specific time trends, yet the in-sample empirical fit for Ohio appears equally good across the entire in-sample horizon.

Leveraging the ASEC data, we compute alternative state-level unemployment rates for this set of nine larger states, which we plot in Figure A.6, along with the official (LAUS) unemployment rates and our fitted claims-based unemployment rates.[69] These ASEC-based unemployment rates should not match either the LAUS unemployment rates or the fitted claims-based unemployment rates, but we can use them as a benchmark to compare how well the fitted claims-based unemployment rates match during the in-sample versus out-of-sample periods, and study whether there is any important breakdown in these relationships pre-1976. Visually, the results in Figure A.6 show that the out-of-sample claims-based unemployment rates consistently track the ASEC-based unemployment rates quite well.

Figure A.6: Fitted CBUR Out-of-Sample Fit with ASEC Data



Notes: Construction of our fitted claims-based state unemployment rates is discussed in Section I.E of the main manuscript. ASEC-based unemployment rates are computed as U/L from reported employment statuses in the ASEC, where individuals are weighted by their ASEC weights.

[69]Readers might be puzzled by the relatively small spike in all the unemployment rates during the 2020 recession, but this is simply a product of the March-only data missing the peak of unemployment. This issue also applies to other sharp peaks and troughs throughout the sample, underscoring the imperative of monthly data for business cycle analysis.

To more formally assess goodness of fit, we also compute the root mean squared error (RMSE) between the fitted claims-based unemployment rates and the ASEC-based unemployment rates for each state across the 14 years available out-of-sample (1962-1975) and for the next 14 years in-sample (1976-1989), which are reported in Table A.4. Reassuringly, Table A.4 shows that for six of the nine states, the RMSE between the fitted claims-based unemployment rates and the ASEC-based unemployment rates is smaller in the out-of-sample period (1962-1975) than the in-sample period (1976-1989). And for one of the other three states (OH), the RMSE is nearly identical across the two time periods. Likewise, the unweighted averages reported in the final row show that the RMSE is smaller in the 1962-1975 period relative to the 1976-1989 period. Thus, we do not find any systematic evidence that the correlations between these two unemployment rate series breaks down in the pre-1976 out-of-sample data. It should be noted that the ASEC had substantially smaller sample sizes in the 1962-1975 period relative to the 1976-1989 period, which if anything likely biases up the RMSE in the former.[70]

Table A.4: In-Smaples vs. Out-of-Sample Error

State	RMSE 1962-1975	RMSE 1976-1989
CA	1.00	0.73
FL	0.55	0.88
IL	0.62	1.25
IN	1.00	1.53
NJ	0.89	0.69
NY	0.74	1.00
OH	1.25	1.21
PA	0.76	1.26
TX	0.30	0.98
Average	0.79	1.06

Notes: Unweighted averages are reported on the final row.

Appendix A.5. Overview of the DNS Algorithm

The gist of the recession dating algorithm proposed by Dupraz, Nakamura, and Steinsson (2023) involves identifying local minima and maxima of the unemployment rate, ignoring low frequency variation in the unemployment rate. The algorithm can be summarized in the following four steps:

- Let u_t be a candidate for a cycle peak (cp)
- If $u_{t+h} > u_{cp}$ in all subsequent months until $u_{t+h+1} > u_{cp} + X$, confirm cp
- If $u_{t+h} < u_{cp}$, new candidate for cp
- After identifying a cp, proceed analogously to identify the next cycle trough (ct)...

[70]The average sample size in the 1962-1975 ASECs was 110,718 versus 159,354 in the 1976-1989 period.

Dupraz, Nakamura, and Steinsson (2023) set the algorithm parameter $X = 1.5$, which captures a sufficient increase in the unemployment rate to trigger a recession classification. We also set parameter $X = 1.5$ for the official U.S. unemployment rate, with which the DNS algorithm identifies peaks and troughs in the U.S. business cycle that are nearly identical to the Hall and Kudlyak (2020) chronology based on observed peaks and troughs in the unemployment rate and fairly similar to the NBER recession dates, as seen in Table 2. When identifying national recession dates from the CBUR, we reduce the parameter X to 1.0, which is generates a much closer match to the NBER recession events, as seen in Table 2. A lower value of X is appropriate for the CBUR series because it is consistently lower than the UR, as seen in Figure 1, and thus the former does not rise (fall) as much in absolute percentage points during recessions (recoveries).

There is an open question as to how X should be parameterized when identifying state-level recession dates from our CBUR series. Because some states naturally have lower (higher) unemployment rates on average, $X = 1.0$ may under-count (over-count) recessions for these states. We compute the average ratio of each state's claims-based unemployment rate to that of the U.S. for our entire sample history and apply this ratio to scale the X parameter for each state, denoting these state-level parameters as Y_i. It is also possible that some states' unemployment rates have increased (or declined) relative to the national rate over our data span. Taking the average ratio of state and national unemployment rates over this entire period may result in a state-level DNS parameter that is too coarse to pick up recessions during periods when a state had a low unemployment rate relative to the nation. To be conservative we scale down all the Y_i's by 25% to reduce Type 2 errors in recession dating.

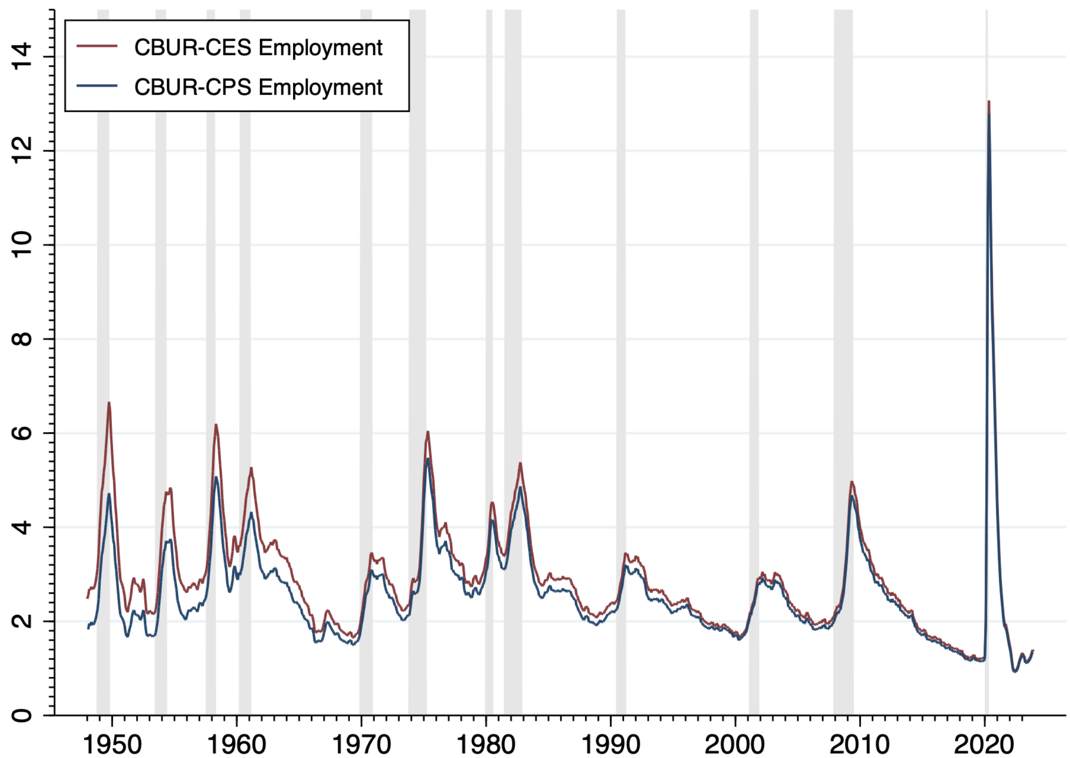
As an additional robustness check for our state recession dates, we estimate state recession peaks and troughs using the Bry and Boschan (1971) algorithm (B-B, henceforth), another approach to estimating inflection points used in the literature on state and regional business cycles that is more similar to the DNS algorithm than the Markov regime-switching approach; see, e.g., Brown (2017).[71] As with the DNS algorithm, the results of the B-B algorithm are somewhat sensitive to parameter choices, but a reasonable parameterization of the B-B algorithm generates fairly similar state-level recession dates as our preferred parameterization of the DNS algorithm.

[71] Brown (2017) compares the recession dates generated by a Markov regime-switching model and the B-B algorithm on coincident indexes for states in the Tenth Federal Reserve District, and finds the two models generally identify the same recessions, though the regime-switching model tends to identify peaks slightly later.

Appendix B. Additional Empirical Results

Appendix B.1. Claims-based Unemployment Diagnostics

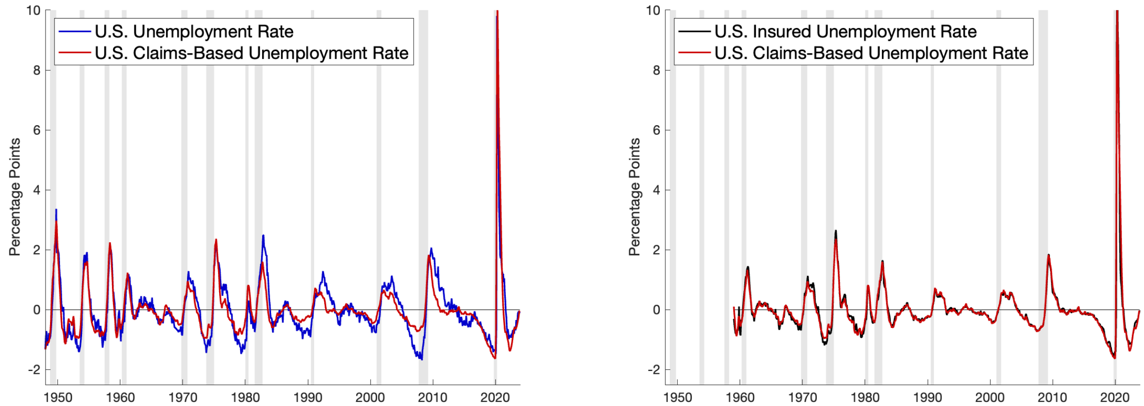
Figure B.1: Influence of Employment on U.S. Claims-based Unemployment Rate



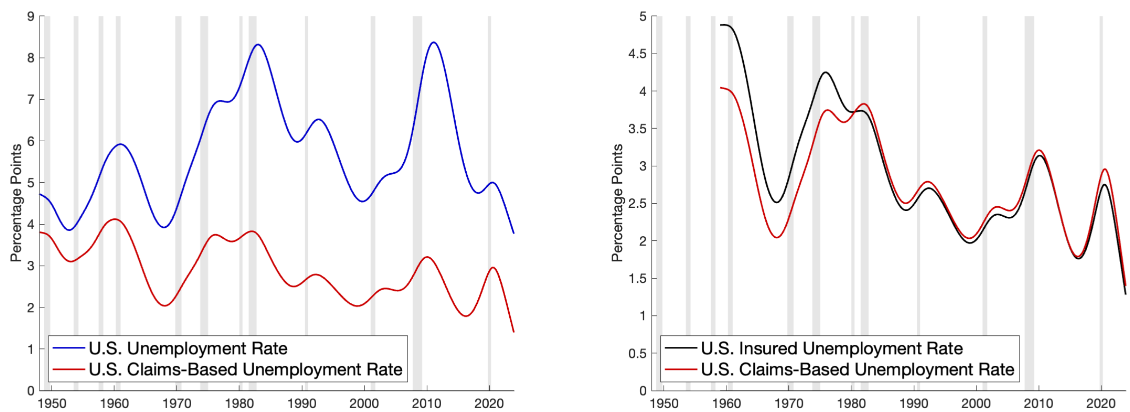
Notes: An alternative U.S. claims-based unemployment rate (blue) is computed from (1) using total employment (CPS) instead of nonfarm payroll employment (CES), which is plotted against our U.S. claims-based unemployment rate (red) that uses the CES measure. Sample: January 1948–December 2023.

Figure B.2: Comparison of Cyclical and Trend U.S. Unemployment Rates

(a) Cyclical

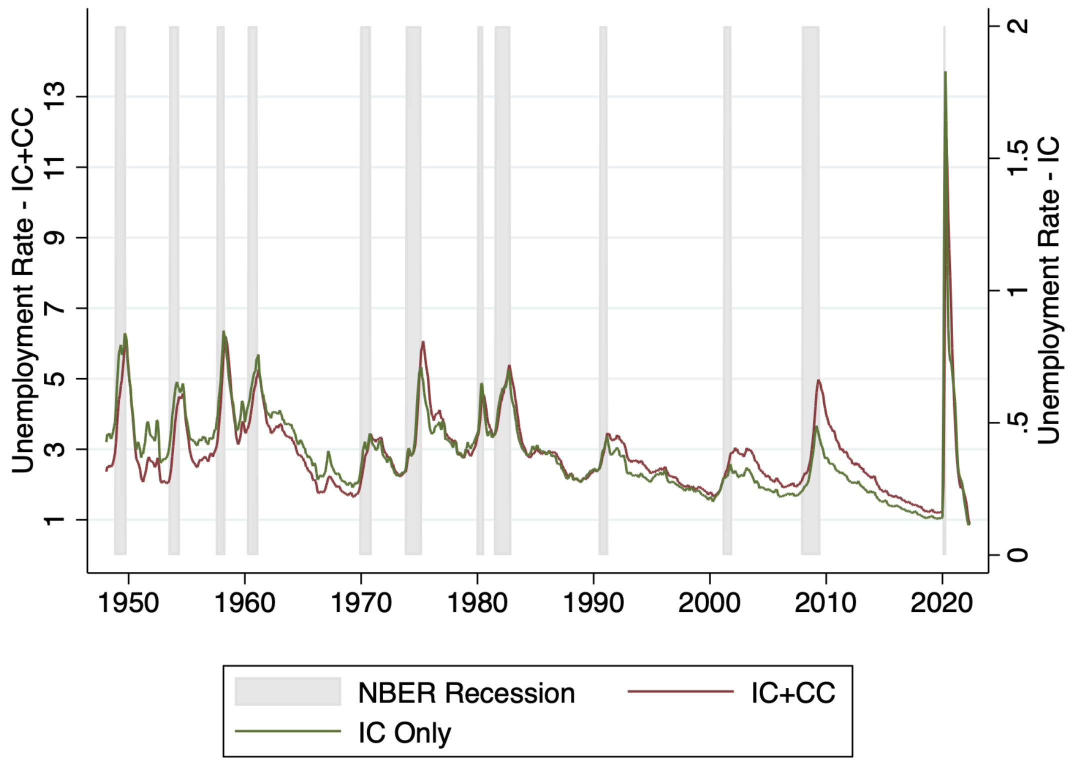


(b) Trend



Notes: The left panels show a comparison of the HP-filtered cyclical (top) and trend (bottom) components of the U.S. unemployment rate (blue) and U.S. claims-based unemployment rate (red). The right panels show a comparison of the HP-filtered cyclical (top) and trend (bottom) components of the backdated U.S. insured unemployment rate (black) and U.S. claims-based unemployment rate (red). The monthly smoothing parameter for the HP filter is set to $\lambda = 129,600$ per Ravn and Uhlig (2002). Gray bars denote NBER recessions. Sample: January 1948–December 2023 (left) or January 1959–December–2023 (right).

Figure B.3: Comparison of Claims-based Unemployment Rates Using IC+CC Versus IC



Notes: Claims-based unemployment rates computed from IC+CC data (red line) versus IC data only (green line) are plotted on the left and right axis, respectively. Sample: January 1948–December 2023.

Appendix B.2. Rolling Regressions of Labor Market Adjustments

Given the sample selection sensitivity of local labor market responses to Bartik shocks, as a robustness test we estimate the local projections in (4) as rolling regressions over staggered 25-year estimation samples. Figure B.4 plots the rolling regression impulse responses of relative (log) employment in the top panel, relative (log) population in the middle panel, and relative unemployment in the bottom panel. The years on the x-axis correspond to the first year of each estimation sample, i.e., the first impulse response is that estimated over 1950-1974 and the final impulse response is estimated over 1995-2019. For ease of interpretation, we only plot every third impulse response function, but the same trends holds when plotting every (or every other) impulse response; confidence bands are similarly omitted to reduce clutter.

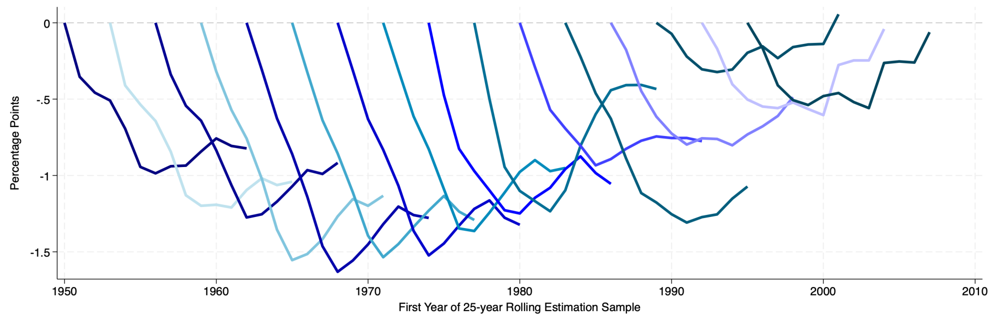
The top panel of Figure B.4 shows that the employment response to local labor demand shocks intensified over the late 20th century, with a peak effect rising from -1.0 percentage point in the earliest post-war sample to roughly -1.5 percentage points for samples spanning the 1960s through the early 1990s. But this trend hits a sharp inflection point, and the employment response moderated significantly in more recent decades; the peak employment response drops to roughly -0.5 percentage points in samples spanning the mid-1980s to present day. Consistent with the subsample estimates in Figure 5, the employment responses also gradually shift from highly persistent at the end of the 12-year impulse response horizon to entirely transitory over more recent decades; the decrease in the persistence of relative employment responses starting around the 1990s also mirrors the breakdown of the persistence in absolute employment growth seen in Figure 4.

Like the employment responses, the rolling regressions of relative population responses to the Bartik instrument also considerably diminishes in recent decades, as seen in the middle panel of Figure B.4. For the first half of the post-war estimation samples, relative population tends to see a steady and highly persistent decline in response to an adverse local labor market demand shock, with the peak effect often around -0.8 percentage points. As with employment, the relative response of population begins moderating in samples estimated over the 1980s and beyond, with the peak decline dropping off to -0.2 percentage points or less in samples spanning the late 1980s to present day. If anything, the attenuation of the population response is even starker than that of employment, with the peak response declining by roughly 75% for population versus 67% for employment. But while the decline in employment shifts from persistent to transitory, the negative response of relative population remains more persistent in more recent estimation samples. The impulse response dynamics in the most recent years are also consistent with the more limited subsample analysis of relative employment in Figure 4 of Dao, Furceri, and Loungani (2017), which shows slightly larger and more persistent responses when including recovery from the Great Recession in the post-1990 subsample, but that influence appears to be moderating in our final impulse response estimated over 1995-2019.

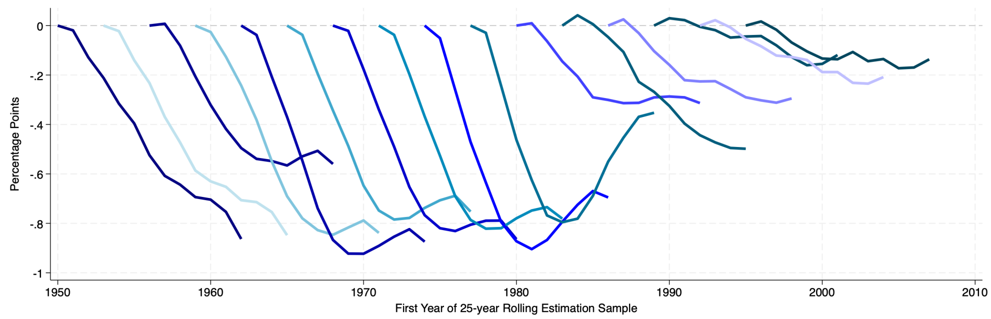
In the bottom panel of Figure B.4, we see much less of a secular post-war trend in the impulse responses of relative unemployment than the relative employment and population margins of adjustment following a local labor demand shock. For the first half of the post-war estimation samples, relative unemployment sees the largest spike immediately on impact, but the peak effect is realized more gradually in more recent samples, mirroring the more gradual peak responses of relative employment and population depicted above. But there is less of a clear trend in the magnitude of peak unemployment responses, which are roughly the same for the earliest and most recent rolling regression samples; that said, we see higher relative unemployment responses and more

Figure B.4: Rolling Regressions of Local Labor Market Responses to Bartik Demand Shocks

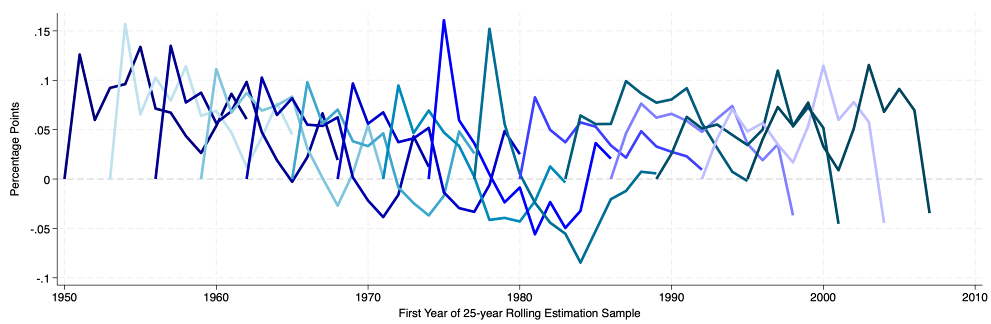
(a) Relative Employment



(b) Relative Population



(c) Relative Unemployment



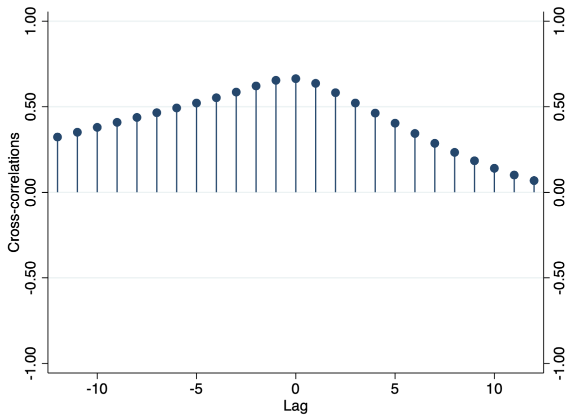
Notes: Figures depict the impulse responses of state relative labor market variables as estimated by the local projections in equation (4), using $rimix_{i,t}$ as the instrument. Impulse responses are estimated over rolling 25-year windows, with every third impulse response depicted for ease of visualization. The years on the x-axis correspond to the first year of each estimation sample. Iterative samples: 1950–1974 through 1995–2019.

rapid unemployment recoveries in rolling regressions estimated over samples starting in the mid- and late-1970s, picking up the severe recessions of the early 1980s.

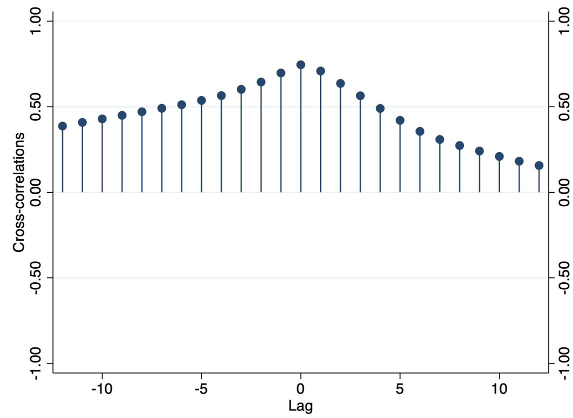
Appendix B.3. Additional Results and Robustness Checks

Figure B.5: Cross Correlations Between the U.S. Unemployment Rate, CBUR, and IUR

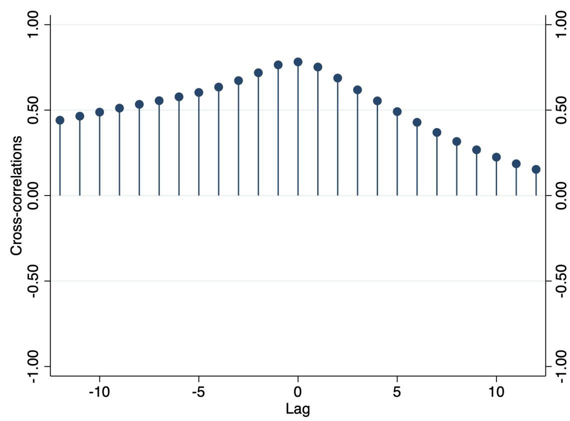
(a) U.S. Unemployment Rate and CBUR



(b) U.S. Unemployment Rate and IUR (Post-1970)

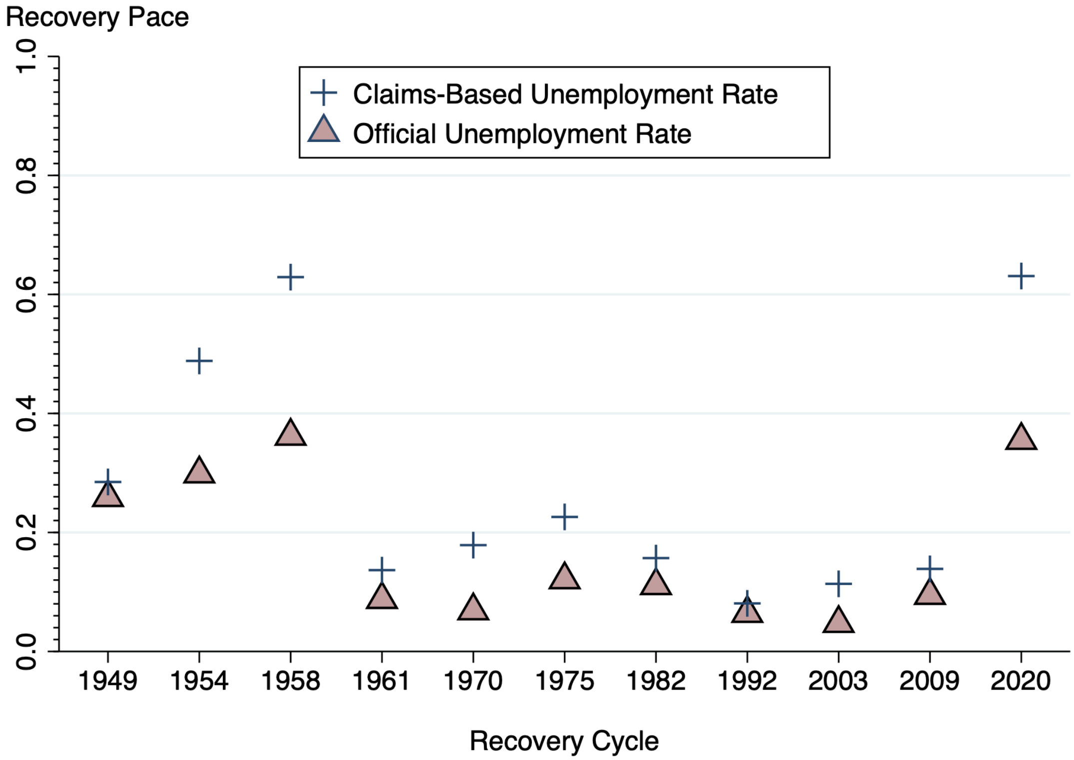


(c) U.S. Unemployment Rate and CBUR (Post-1970)



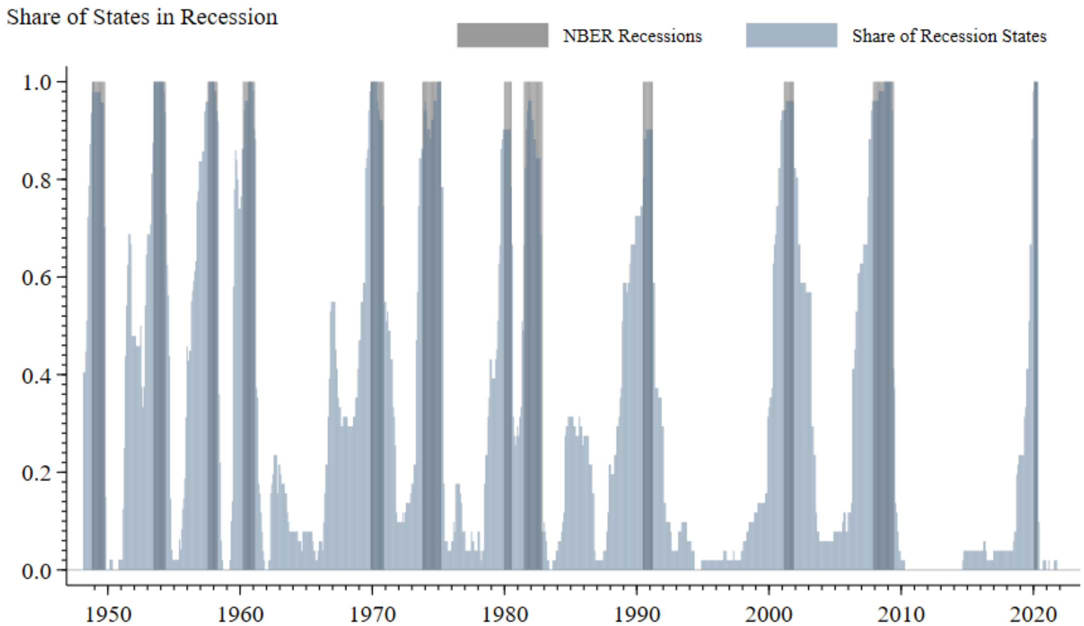
Notes: Panel (a) plots the cross correlations between the official U.S. unemployment rate (UR) versus our U.S. claims-based unemployment rate (CBUR) from 1948–2023. Panel (b) plots the cross correlations between the official U.S. unemployment rate versus the U.S. IUR, which is only available from 1971 onwards. Panel (c) plots the cross correlations between the official UR versus our CBUR, but only over 1971–2023, as a comparison with the same sample as for the IUR. The CBUR is smoothed with a three-month centered moving average.

Figure B.6: National Unemployment Recovery Rates: Recession Dates from CBUR



Notes: Recovery dates from DNS algorithm with recovery dates generated from the claims-based unemployment rate. Recovery from the 1980 recession is again excluded, see notes to Figure 7.

Figure B.7: Share of U.S. States in Recession: Recession Dates from Unfitted CBUR



Notes: State-level recession coding is constructed by applying the DNS algorithm to states' unfitted claims-based unemployment rates. The DNS algorithm parameter is adjusted for each state proportionate to its average level of unemployment over the entire time period, see Appendix A.5. Sample: January 1948–December 2023.

Appendix B.4. Amplitude of Unemployment Fluctuations

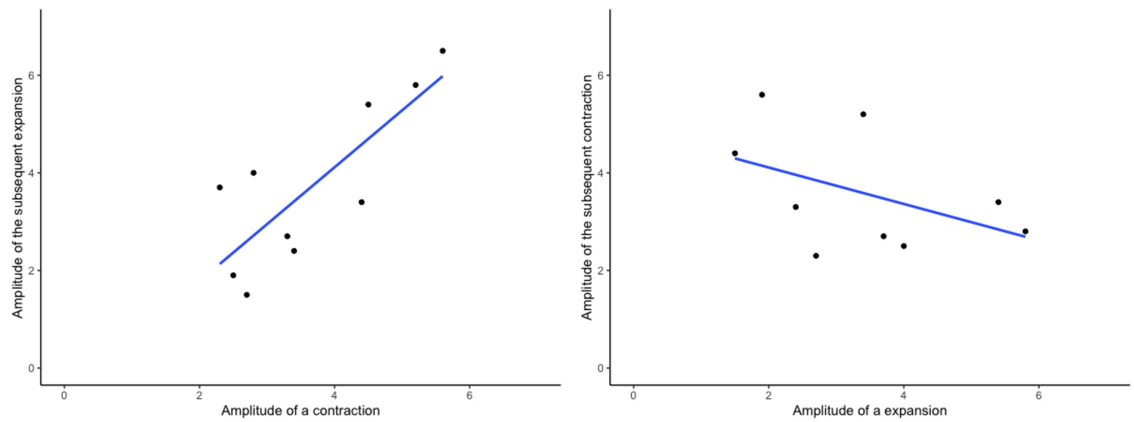
Dupraz, Nakamura, and Steinsson (2023) document an important asymmetry in U.S. unemployment dynamics throughout postwar recessions: Increases in the national unemployment rate during recessions are followed by decreases of a similar magnitude during the subsequent expansion, whereas the decrease in unemployment during expansions has no clear relationship with the rise in unemployment during the ensuing recession. Put differently, unemployment recoveries are well predicted by the severity of the prior recession, but the severity of the next recession cannot be forecast from the strength of the prior recovery. As Dupraz, Nakamura, and Steinsson (2023) explain, this asymmetric dynamic is consistent with Milton Friedman's "plucking model" of business cycles, in which cyclical shocks pull output down from operating near potential and the magnitude of these adverse shocks is not systematically correlated with the strength or duration of the previous expansion (Friedman, 1993).

In Figure 12 of the main paper we analyze this plucking property at the state level, using our state recession dates to compute the changes in states' unemployment rates across recessions and expansions. Here we discuss several related validation exercises and robustness checks.

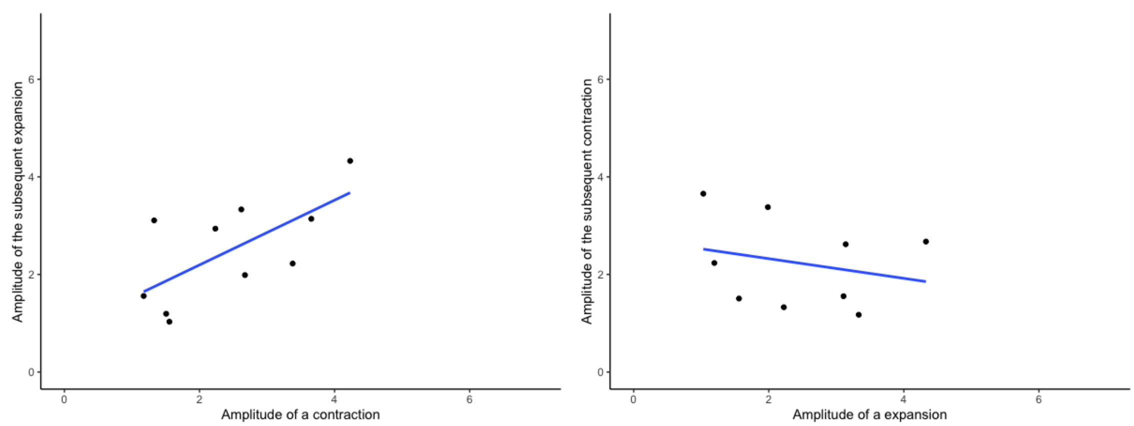
We first use our U.S. claims-based unemployment rate to try to replicate this plucking property at the national level, testing whether our series omits similar amplitude dynamics across the U.S. business cycle as documented by Dupraz, Nakamura, and Steinsson (2023). The amplitude of unemployment is measured as the percentage point increase (decrease) from peak to trough (trough to peak), using the recession dates derived from the U.S. unemployment rate using the DNS algorithm, as reported in Table 2. Figure B.8 plots amplitude dynamics for the U.S. unemployment rate (top panels) and U.S. claims-based unemployment rate (bottom panels) for national business cycle expansions and contractions since the 1948-49 recession.[72] As in Figure 2 of Dupraz, Nakamura, and Steinsson (2023), the left panels plot the amplitude of unemployment for each national recession (x-axis) against the amplitude during the ensuing expansion (y-axis), and the right panels plot the amplitude for each expansion against the amplitude during the subsequent recession.

Figure B.8: Amplitude of U.S. Unemployment in Contractions and Expansions

(a) U.S. Unemployment Rate



(b) U.S. Claims-Based Unemployment Rate



Notes: The amplitude of contractions and expansions are measured as the absolute percentage point change in the U.S. unemployment rate or claims-based unemployment rate between national business cycle peaks and troughs, as identified by the DNS algorithm using the U.S. unemployment rate, see Table 2. OLS regression lines are plotted for each panel; the fit is significant at the 5% level for both panels on the left and insignificant for both panels on the right.

Figure B.8 underscores that our national claims-based unemployment rate exhibits very similar amplitude dynamics as Dupraz, Nakamura, and Steinsson (2023) document with the official U.S. unemployment rate, replicated here in the top panels: In the bottom panels, our claims-based unemployment rate also shows a) a significant positive correlation between the amplitude of un-

[72] For a better cross-walk with the related literature, we limit the sample of study to pre-pandemic cycles.

employment rising during contractions and falling in subsequent recoveries and b) a negative, insignificant relationship between unemployment falling during expansions and rising in subsequent recoveries. On average, the amplitude of fluctuations is somewhat smaller with the U.S. claims-based unemployment rate than the official unemployment rate, as would be expected given the level difference depicted in Figure 1; the amplitude would, however, be comparable using the fitted claims-based unemployment rates at the state level, per Figure 2. But on the whole, our U.S. claims-based unemployment rate is telling an entirely consistent story that is also supportive of plucking models of (national) business cycles.

As another robustness check, we replicate Figure 12 using our unfitted claims-based unemployment rate data instead of the fitted series used in the main paper. This exercise yields very consistent results, particularly for relationship between the amplitude of unemployment during contractions and in the ensuing expansions (left panel of Figure 12). In the right panel, the flattening relationship between the amplitude in expansions and amplitude in subsequent contractions in the post-1980 data is of a smaller magnitude when we use the unfitted claims-based unemployment rates, but the results are consistent in that both the fitted and unfitted data show a positive and significant correlation pre-1980 data and no statistically significant relationship post-1980.

Lastly, we were also concerned about the possibility of “false positive" recession dating from the DNS algorithm, especially for smaller states, but the results in Figure 12 are robust to throwing out the 10th percentile of observations by trough-to-trough durations (i.e., roughly five months or less between cycle troughs).

Notably, our state-level amplitude dynamics—using either our fitted or unfitted claims-based unemployment rates—tell a different story in the earlier post-war recessions than would be inferred from official state unemployment data since 1976, e.g., Tasci and Zevanove (2019) finding a muted, slightly negative correlation between the amplitude of unemployment in expansions and subsequent contractions since 1976. Our state-level data show that the severity of the next recession could be forecast from the strength of the prior recovery throughout the first six post-war recessions (1948-49 through 1973-75), with a positive and statistically significant relationship between the amplitude of unemployment in expansions and subsequent contractions before 1980; both the fitted and unfitted claims-based unemployment series suggest that the state-level evidence for the “plucking property" has strengthened moving from pre- to post-1980 observations.

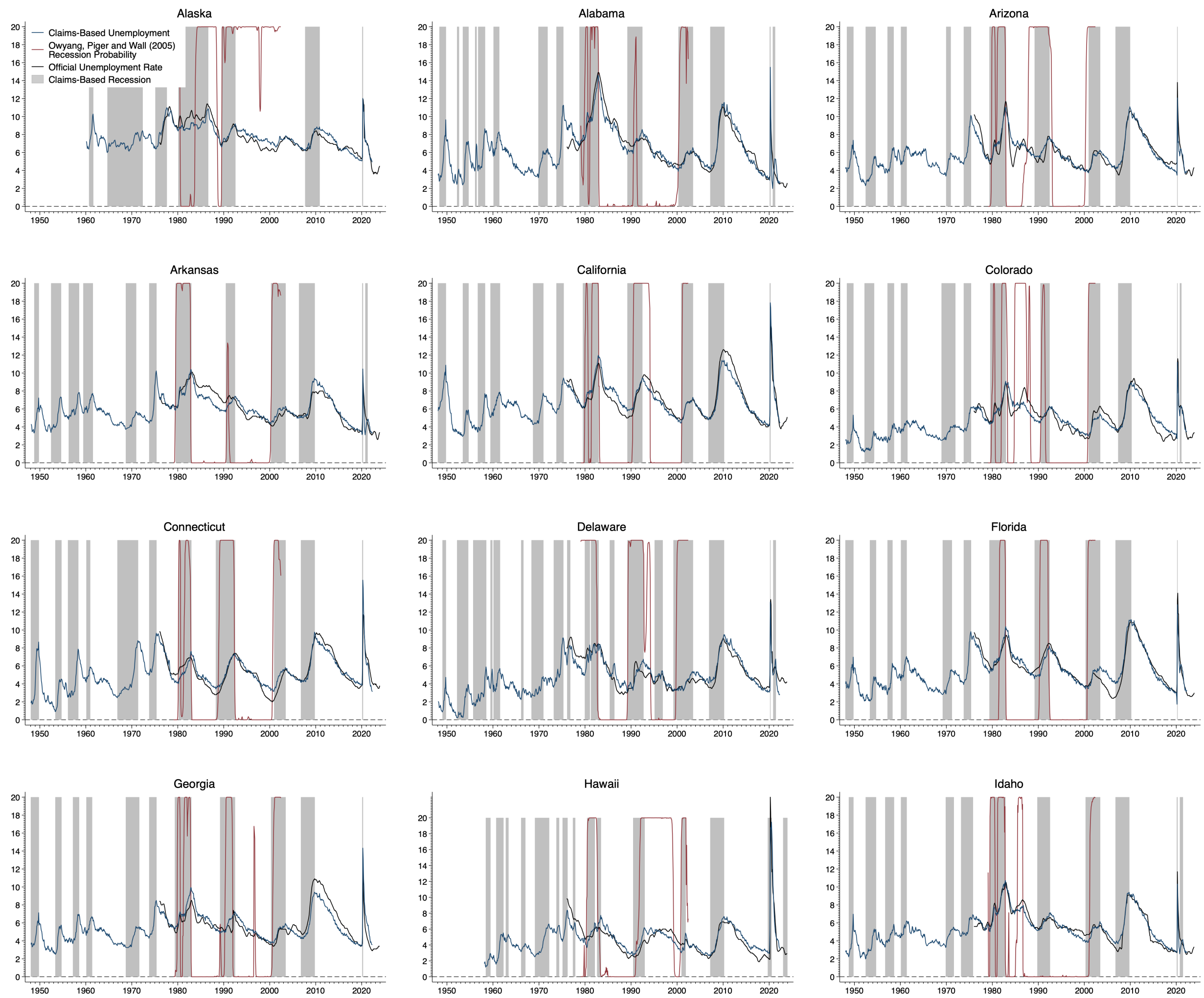
Appendix C. State Recession Dates and Unemployment

Figure C.1 depicts our claims-based unemployment rates (blue lines), state recession dates (gray bars), and the Owyang, Piger, and Wall (2005) state recession probabilities (red lines) for all 50 states. There are notable similarities for a number of states across to the two datasets when they overlap in the 1979–2002 sample. For many larger states, both the recession dates derived from our claims-based unemployment rates and the Owyang, Piger, and Wall (2005) recession probabilities only identify the same national recessions in the overlapping sample (1980, 1981-82, 1990-91, and 2001), albeit with slightly different peak and trough dates and/or ignoring the distinction between a double-dip recession versus a longer recession in the early 1980s (e.g., AZ, CA, CT, FL, IL, MA, NC, and NY). And in some cases, both datasets identify nearly identically timed recessions that were not experienced on the national level. For instance, we identify Mississippi as falling into recession over February 1986–June 1986 and Owyang, Piger, and Wall (2005) identify Mississippi as being in recession over February 1986–July 1986 with probabilities exceeding 80% for each of these months. Similarly, Owyang, Piger, and Wall (2005) identify Wyoming as falling into recession over February 1986–March 1987 with probabilities exceeding 80%, while we identify Wyoming as falling into recession over December 1984–October 1986.

There are also striking differences between the two datasets, most notably in smaller states. Out of sync with the national business cycle, Owyang, Piger, and Wall (2005) identify short-lived recessions in Idaho, New Mexico, South Dakota, and Utah in the mid-1980s, contrary to our series, whereas our dataset identifies a short-lived recession in Delaware in the mid-1980s, contrary to theirs. And Owyang, Piger, and Wall (2005) do not identify the 1990-91 recession in a number of states that are identified as being in recession by the DNS algorithm using our claims-based unemployment dates (e.g, IA, ID, LA, ND, OK, SD, TX, UT, and WY). Conversely, Owyang, Piger, and Wall (2005) identify short-lived recessions in Maine, Maryland, New Mexico, and Washington in the mid-1990s, which are not identified in our claims-based unemployment recession dates. And Owyang, Piger, and Wall (2005) do not identify the 2001 recession in Kansas, Oklahoma, or Wyoming, unlike our claims-based unemployment recession dates. In some other states where both datasets identify recessions around 1990-91 and 2001, the Owyang, Piger, and Wall (2005) recession probabilities identify considerably shorter downturns than our claims-based unemployment recession dates (e.g., KY, MN, OR, and WI). In a handful of other states, our claims-based unemployment recession dates show considerably shorter recessions than the Owyang, Piger, and Wall (2005) recession probabilities: At one extreme, the Owyang, Piger, and Wall (2005) recession probabilities show Alaska continuously in a recession from August 1989–June 2002, with recession probabilities averaging 97.5% and never falling below 50% for this sample. Similarly, their recession probabilities show Hawaii in a slump throughout almost all of the 1990s, with recession probabilities averaging 98.3% and never falling below 60% over November 1991–December 1999. In line with a clear, persistent recovery in the claims-based unemployment rates for Alaska in the early 1990s, our state recession dates show Alaska in a much shorter recession, over August 1989–July 1992. And we identify Hawaii as having experienced only a short-lived recession in the early 1990s, followed by a persistent recovery in unemployment.

Neither approach is right or wrong per se, but Figure C.1 underscores that our recession dates exhibit fewer erratic, short-lived recessionary spikes or suspiciously long recessions, and no judgement is required about how to interpret recession probabilities as recessions. The principal advantage to our approach, however, is the ability to identify inflection points in state business cycles for more than 30 additional years when using our claims-based unemployment rates, relative to official unemployment rates or off-the-shelf state coincident indexes.

Figure C.1: State Recession Dates and Recession Probabilities



Notes: Our fitted claims-based state unemployment rates (blue) are for January 1948–December 2023, save for the handful of states for which nonfarm payroll employment data is only available starting in the 1950s, see footnote 7 for details. The official BLS state unemployment rates (black) span January 1976–December 2023. State recession dates (gray bars) are estimated from our fitted claims-based unemployment rates using the Dupraz, Nakamura, and Steinsson (2023) algorithm. State recession probabilities (red) for February 1979–June 2002 are from Owyang, Piger, and Wall (2005). The y-axis measures the unemployment rate in percentage points and recession probabilities in five-percentage point increments (20=100%).

Figure C.1: State Recession Dates and Recession Probabilities (Continued...)

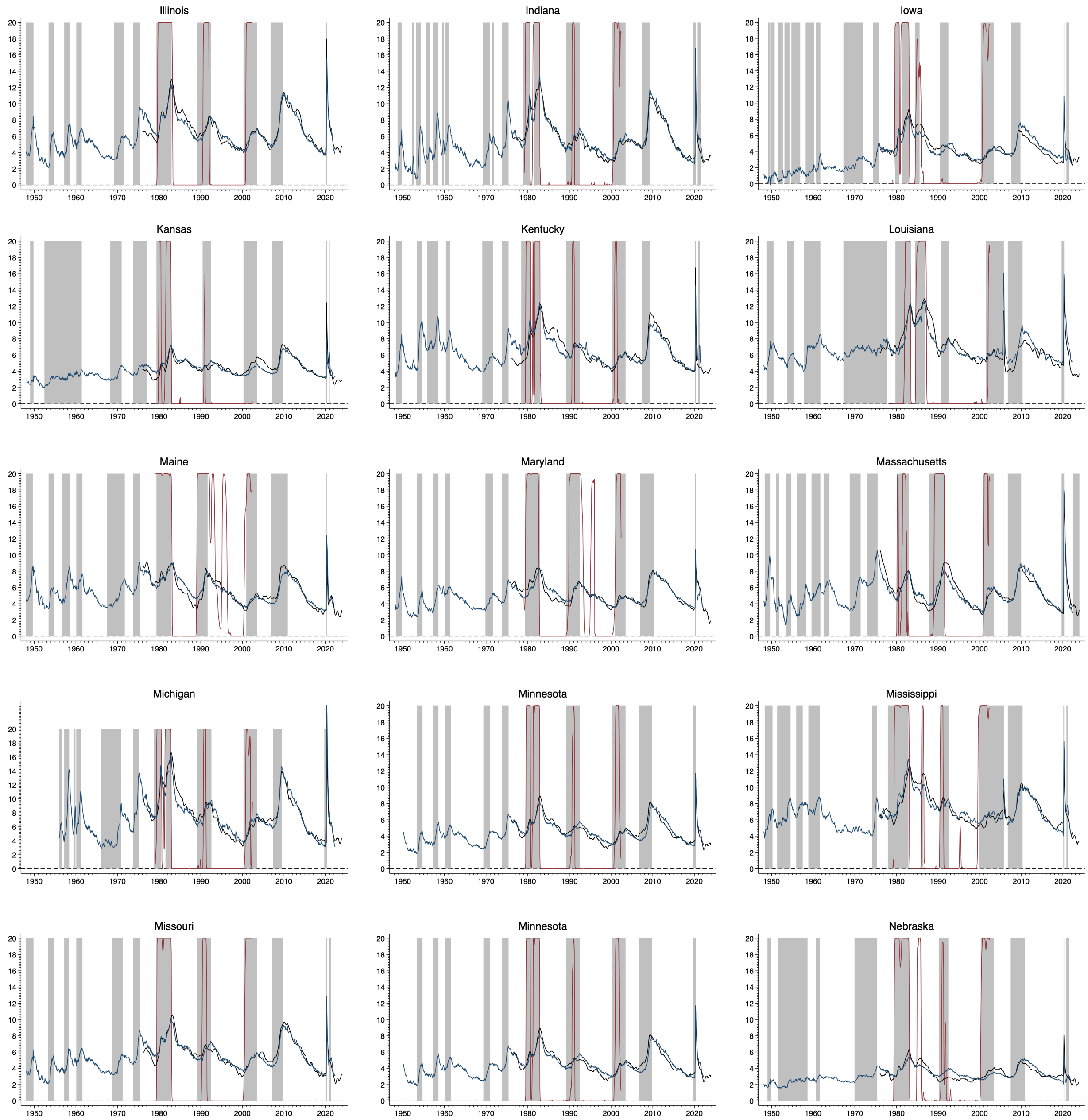


Figure C.1: State Recession Dates and Recession Probabilities (Continued...)

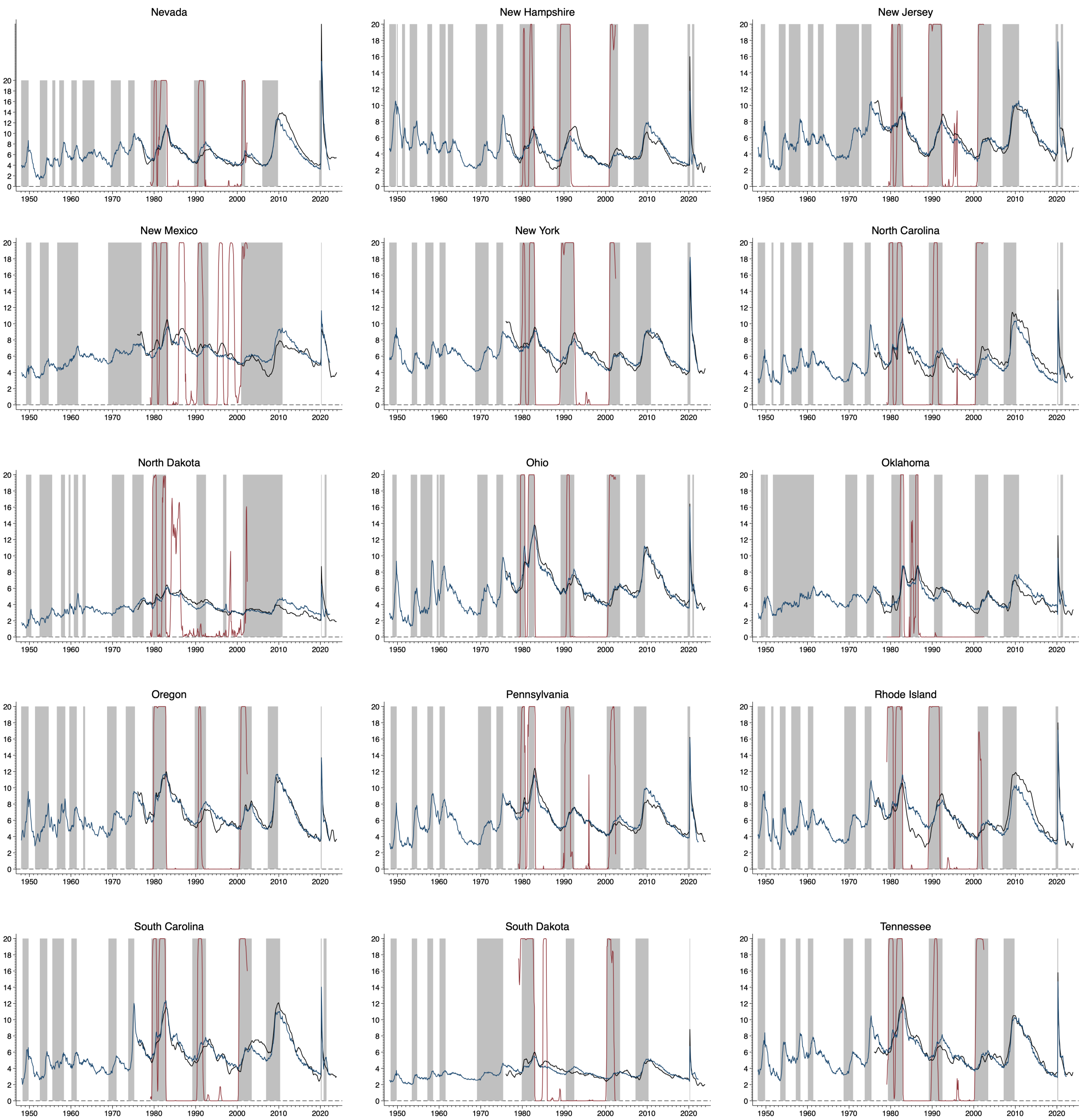


Figure C.1: State Recession Dates and Recession Probabilities (Continued...)