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

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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

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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.[1] 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.

[1] Data available here: https://oui.doleta.gov/unemploy/DataDownloads.asp

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.[2] 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.[3] 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.[4] 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.[5]

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.[6] Data were always digitized from the most

[2]Data available here: https://oui.doleta.gov/unemploy/claimssum.asp.

[3]Fortunately data for Alaska and Hawaii are consistently available before they become states.

[4]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.

[5]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.

[6]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

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 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.[7] 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).[8] 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.[9] 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,

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.

[7]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%.

[8]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.

[9]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.

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 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.[10] 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,

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

if necessary, used a linear interpolation between CC data for the prior and subsequent month.[11] 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)
- 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

[11] 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.

in-sample insofar as they are erroneous data. When estimating equation (2) from the main paper 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

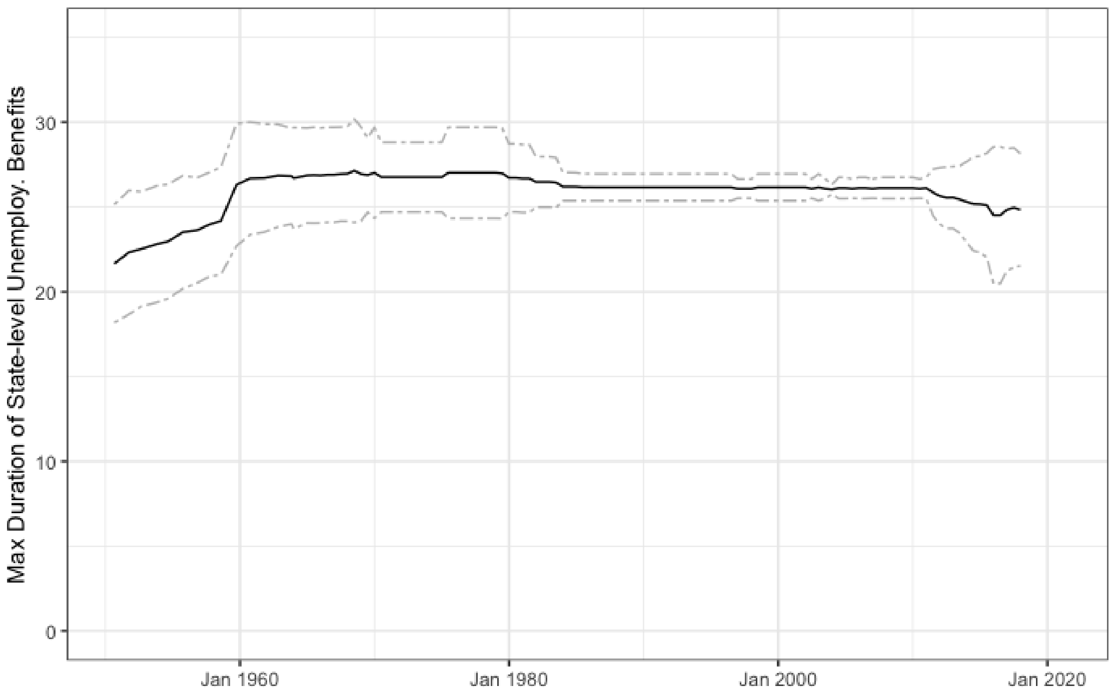
The advantage to using unemployment claims as a proxy for the level of unemployment in constructing our claims-based unemployment rates simply boils down to historical data availability. But conceptual differences between the surveyed level of unemployment and the number of unemployment insurance claimants 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.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.

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 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

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.

phased out by the late 1940s or early 1950s.[12] Twenty four states never changed their waiting period policies throughout our sample, with a plurality of states consistently imposing a one-week waiting period.[13] 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.

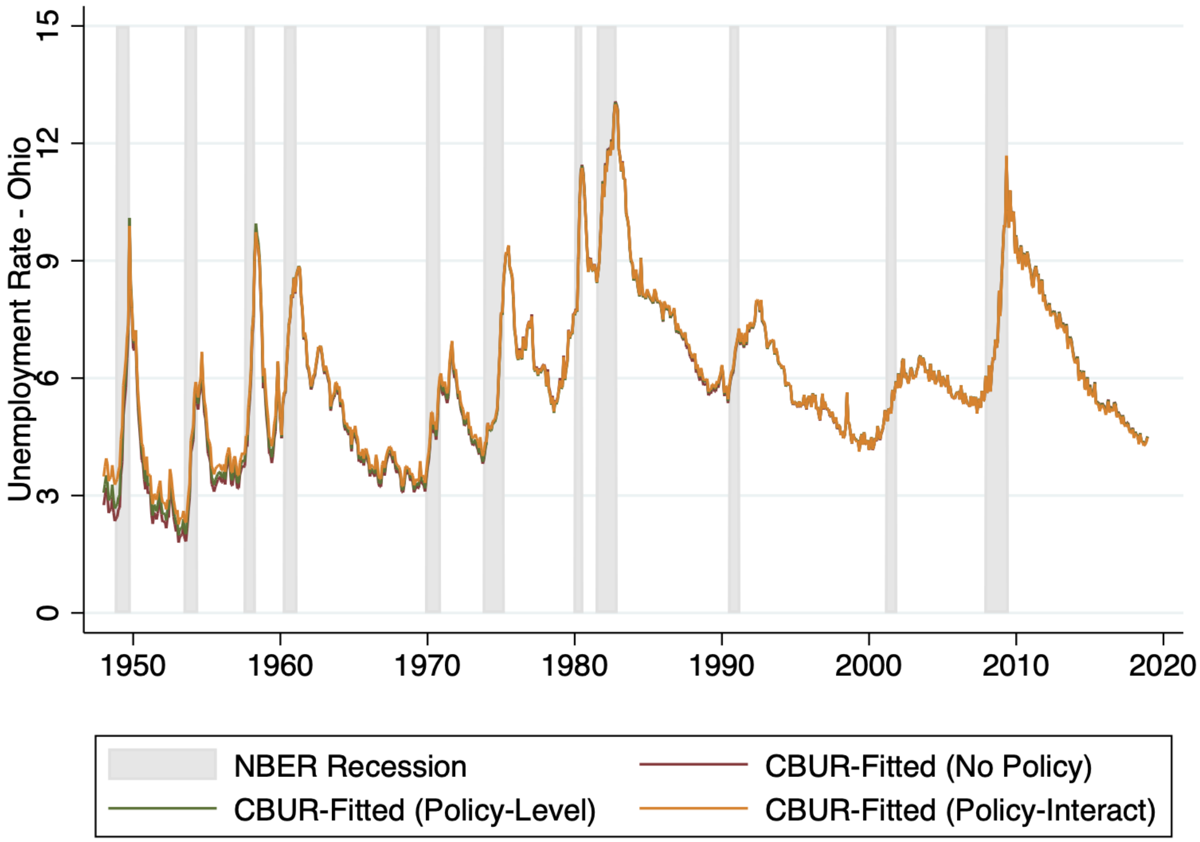
We also explore the influence of these state-level UI policy changes on our claims-based unemployment rate by directly controlling for them in the fitting regressions modified from equation (2) from the main paper. We separately add these UI policy controls for maximum benefit duration and waiting periods in either levels or as interaction terms with the other regressors in equation (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

[12]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.

[13]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.

specification for simplicity.

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

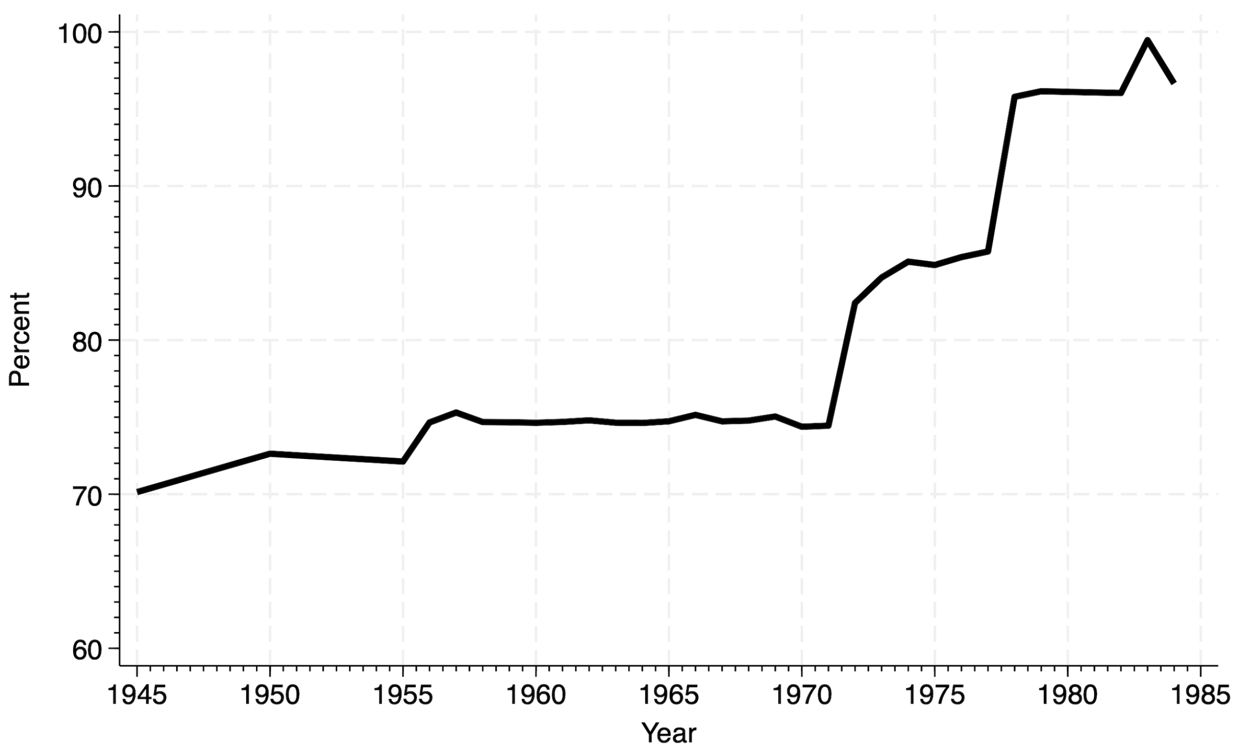


Notes: CBUR-Fitted (Policy-Level) denotes the fitted CBUR with UI policy regressors in levels and CBUR-Fitted (Policy-Interact) denotes the fitted CBUR with UI policy variables interacted with the other regressors in equation (2) from the main paper.

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 employment 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).[14] 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.

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.

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 increased 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 Administra-

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

tions [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.[15]

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.[16] But the legislation was much more a reaction to budgetary pressures from the 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. And as a related robustness check, in Figure B.4 we also compute an alternative annual version of our claims-based unemployment rates using state-level covered employment instead of nonfarm payroll employment; the HP-filtered series are nearly a perfect match with that of our claims-based unemployment rates for every state, underscoring that the degree of labor market slack is not particularly sensitive to the choice of employment data. This evidence is perhaps not

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

[16]“Congress Revises Jobless Benefits System," *CQ Almanac 1976*, 32nd ed., 359-64. Washington, DC: Congressional Quarterly, 1971.

surprising given that the UI coverage 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). And, as noted in the main paper, 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) from the main paper, and any residual level effect would be corrected for with the inclusion of the national unemployment rate on the right-hand side.

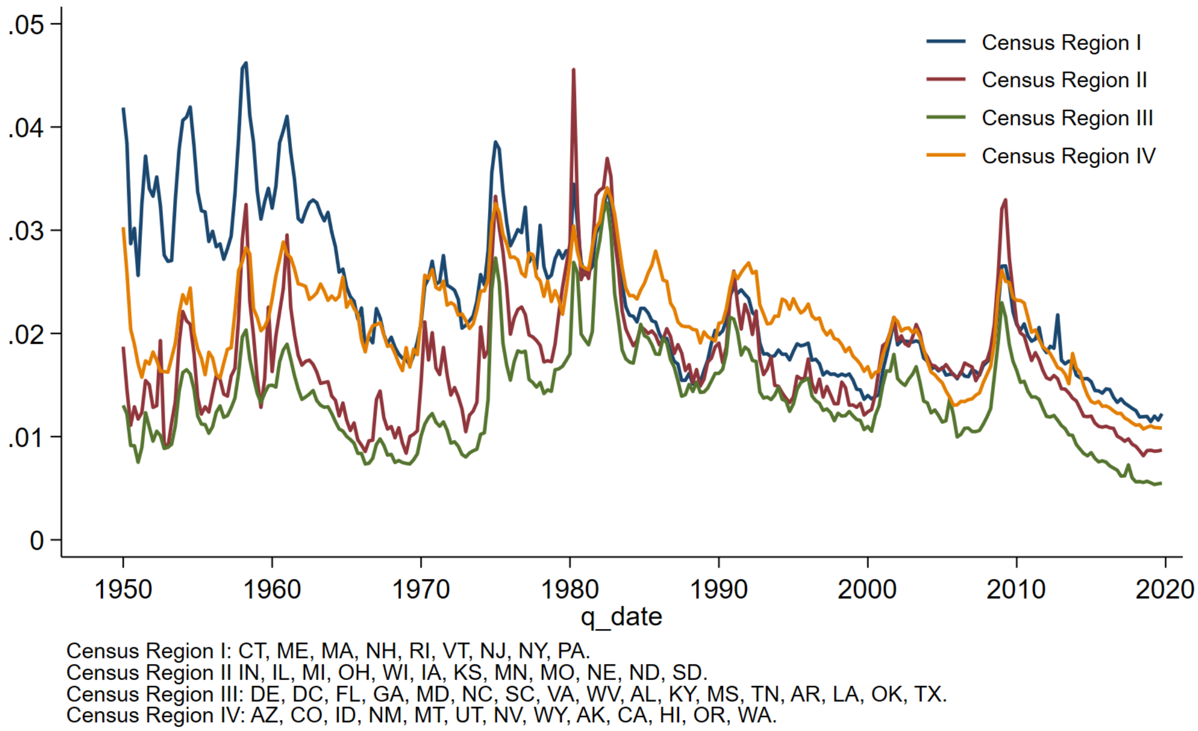
More abrupt changes in take-up rates and/or denial rates in only a subset of states would, however, potentially undermine inference from our claims-based unemployment rates. For instance, to the extent that racial discrimination affected take-up rates or denial rates differentially across regions, the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

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 pe-

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.

riod. 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.[17] 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

[17] 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.

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.

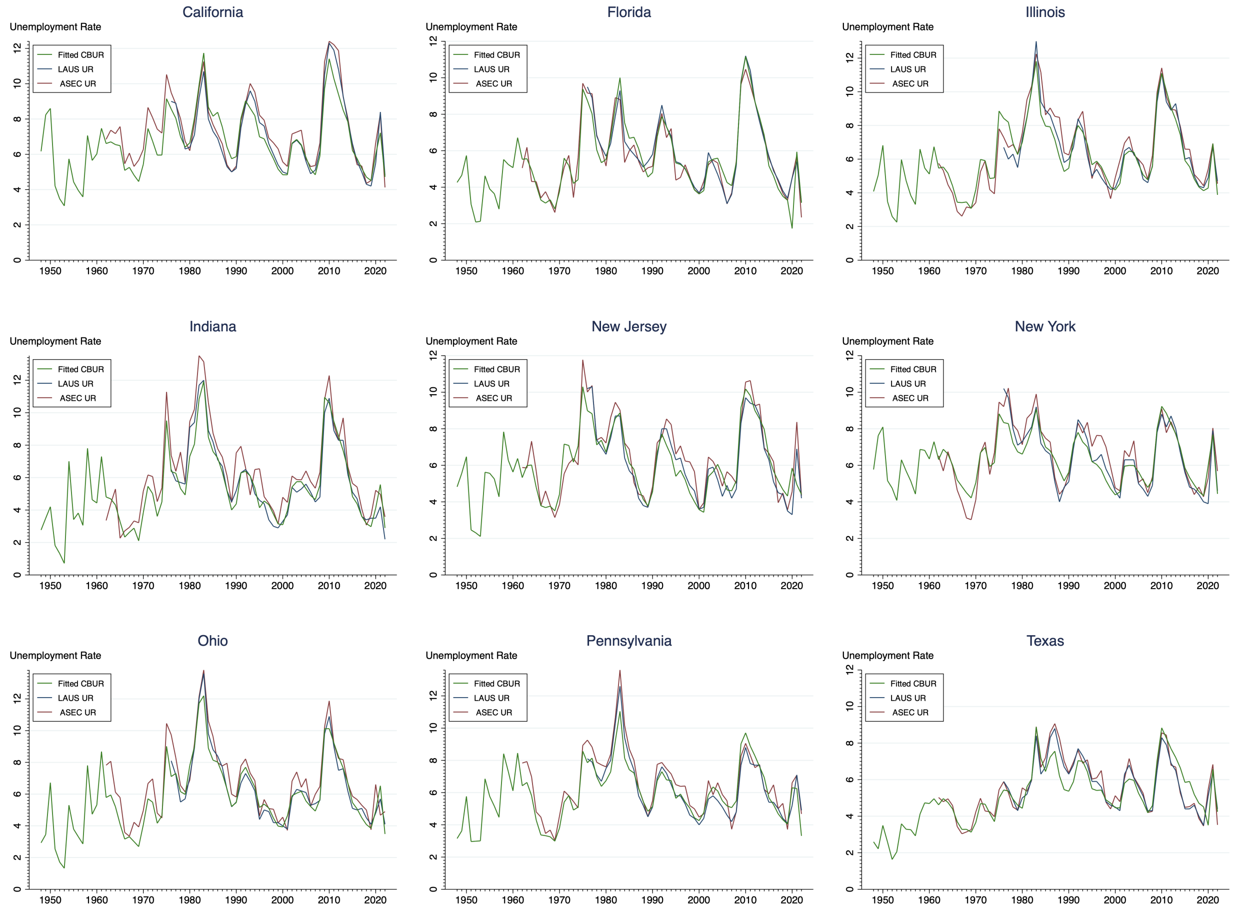
Leveraging the ASEC data, we compute alternative state-level unemployment rates for this set of nine larger states, which we plot in Figure A.6, along with the official (LAUS) unemployment rates and our fitted claims-based unemployment rates.[18] These ASEC-based unemployment rates should not match either 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.

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.1. Reassuringly, Table A.1 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.[19]

[18] 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.

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

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.

Table A.1: 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)...

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 of the main paper. 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 of the main paper, 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.

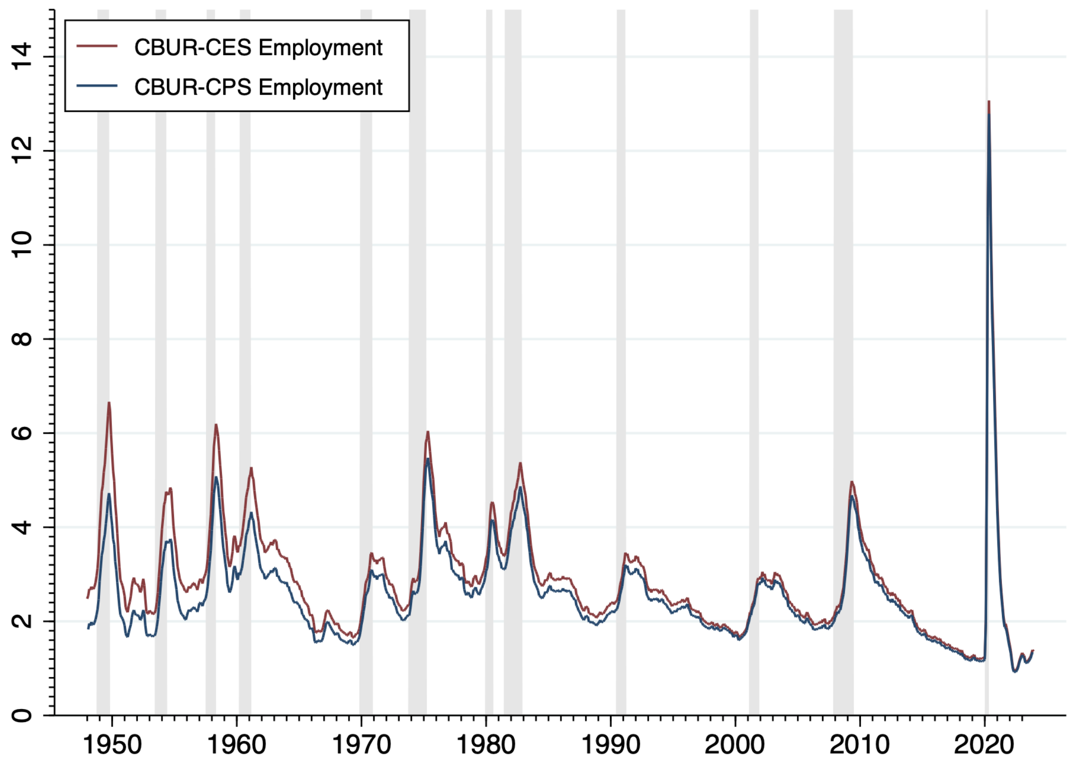
As an additional robustness check for our state recession dates, we estimate state recession peaks and troughs using the Bry and Boschan (1971) algorithm (B-B, henceforth), another approach to estimating inflection points used in the literature on state and regional business cycles that is more similar to the DNS algorithm than the Markov regime-switching approach; see, e.g., Brown (2017).[20] 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.

[20] 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

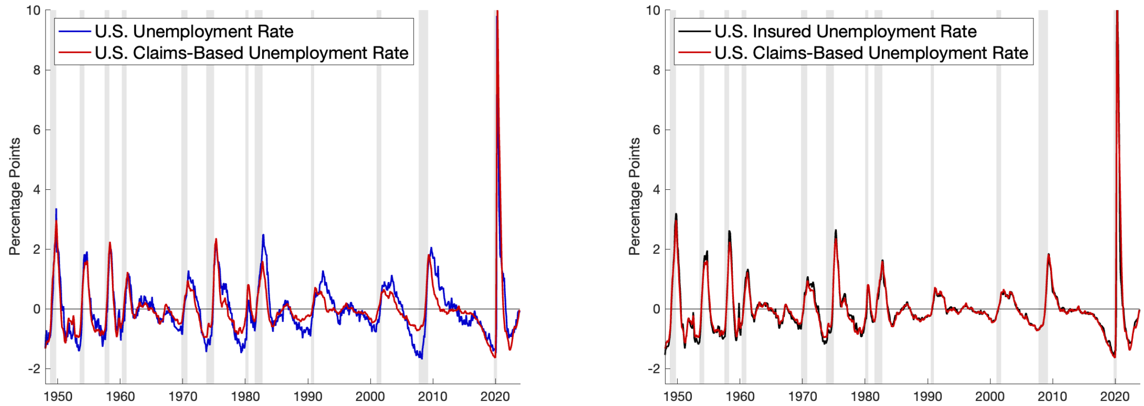
Figure B.1: Influence of Employment on U.S. Claims-based Unemployment Rate



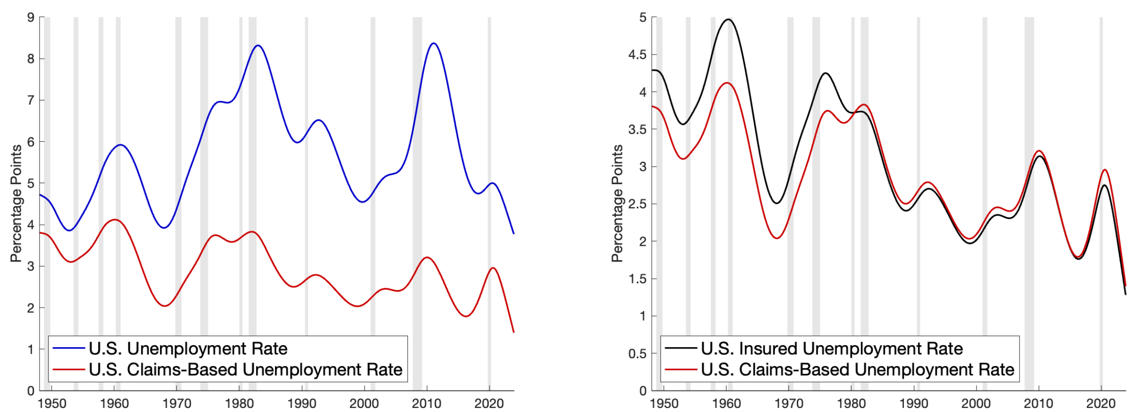
Notes: An alternative U.S. claims-based unemployment rate (blue) is computed from equation (1) from the main paper using total employment (CPS) instead of nonfarm payroll employment (CES), which is plotted against our U.S. claims-based unemployment rate (red) that uses the CES measure. Sample: January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

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

(a) Cyclical

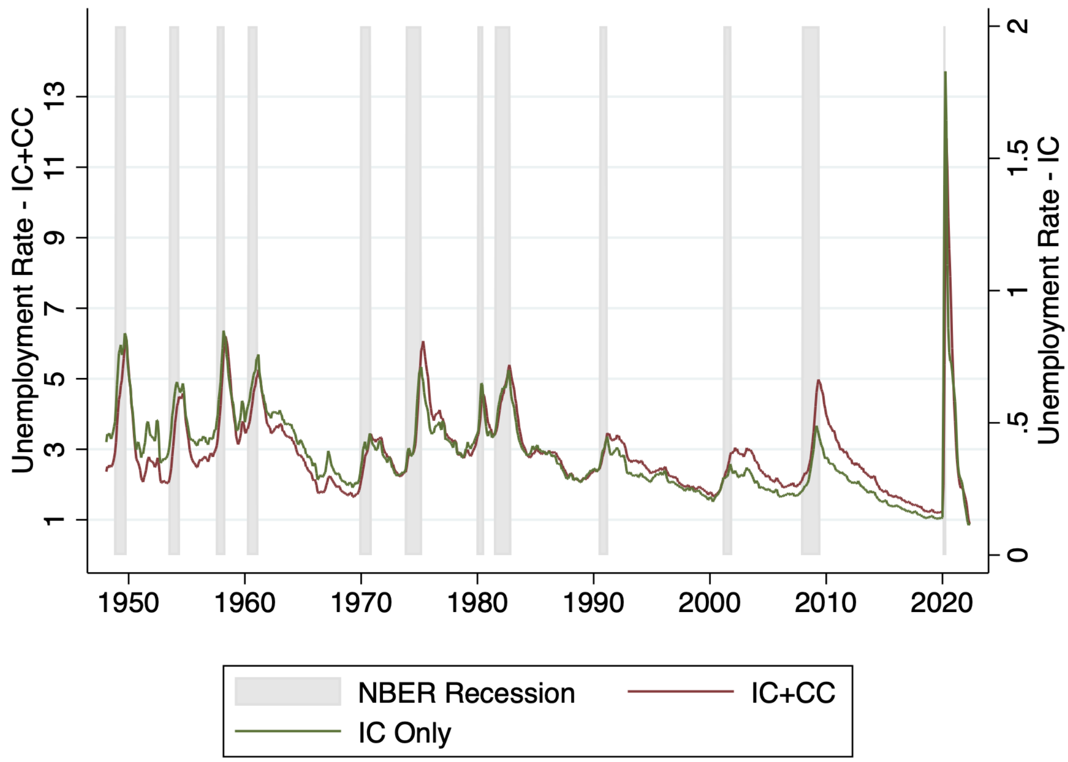


(b) Trend



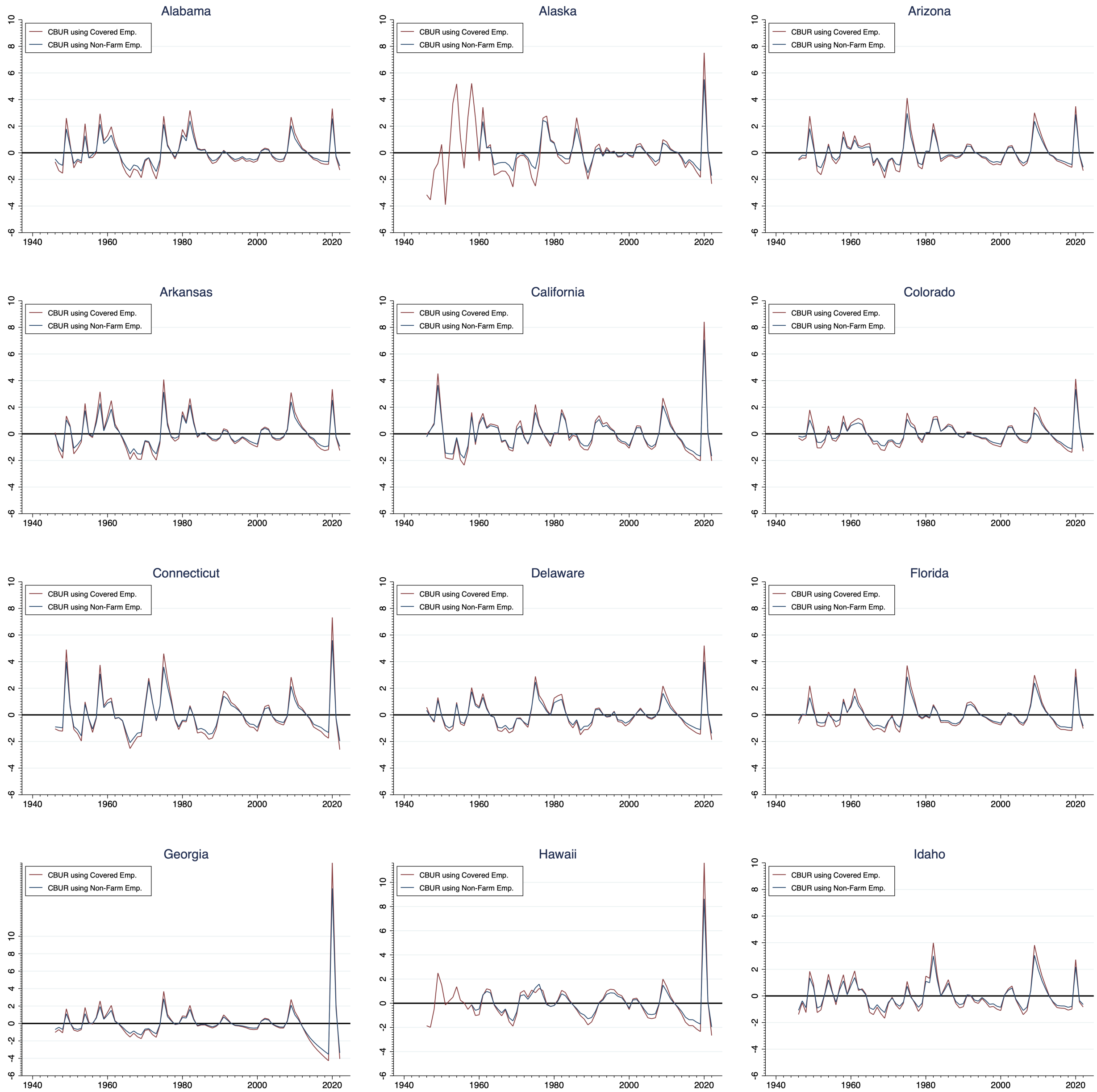
Notes: The left panels show a comparison of the HP-filtered cyclical (top) and trend (bottom) components of the U.S. unemployment rate (blue) and U.S. claims-based unemployment rate (red). The right panels show a comparison of the HP-filtered cyclical (top) and trend (bottom) components of the backdated 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). Sample: January 1948–December 2023.

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.

Figure B.4: Influence of Covered Employment versus Nonfarm Payroll Employment



Notes: We compute an alternative annual version of our claims-based unemployment rate using covered employment (only available at an annual frequency for states) instead of nonfarm payroll employment in equation (1) from the main paper. This figure plots a comparison of the HP-filtered cyclical components of the annual version of our claims-based unemployment rate (blue) and the alternative version using covered employment (red) for every state. The annual smoothing parameter for the HP filter is set to $\lambda = 6.5$ per Ravn and Uhlig (2002). The y-axis measures the cyclical component of the unemployment rates in percentage points. Sample: 1948–2022.

Figure B.4: Influence of Covered Employment versus Nonfarm Payroll Employment (Continued...)

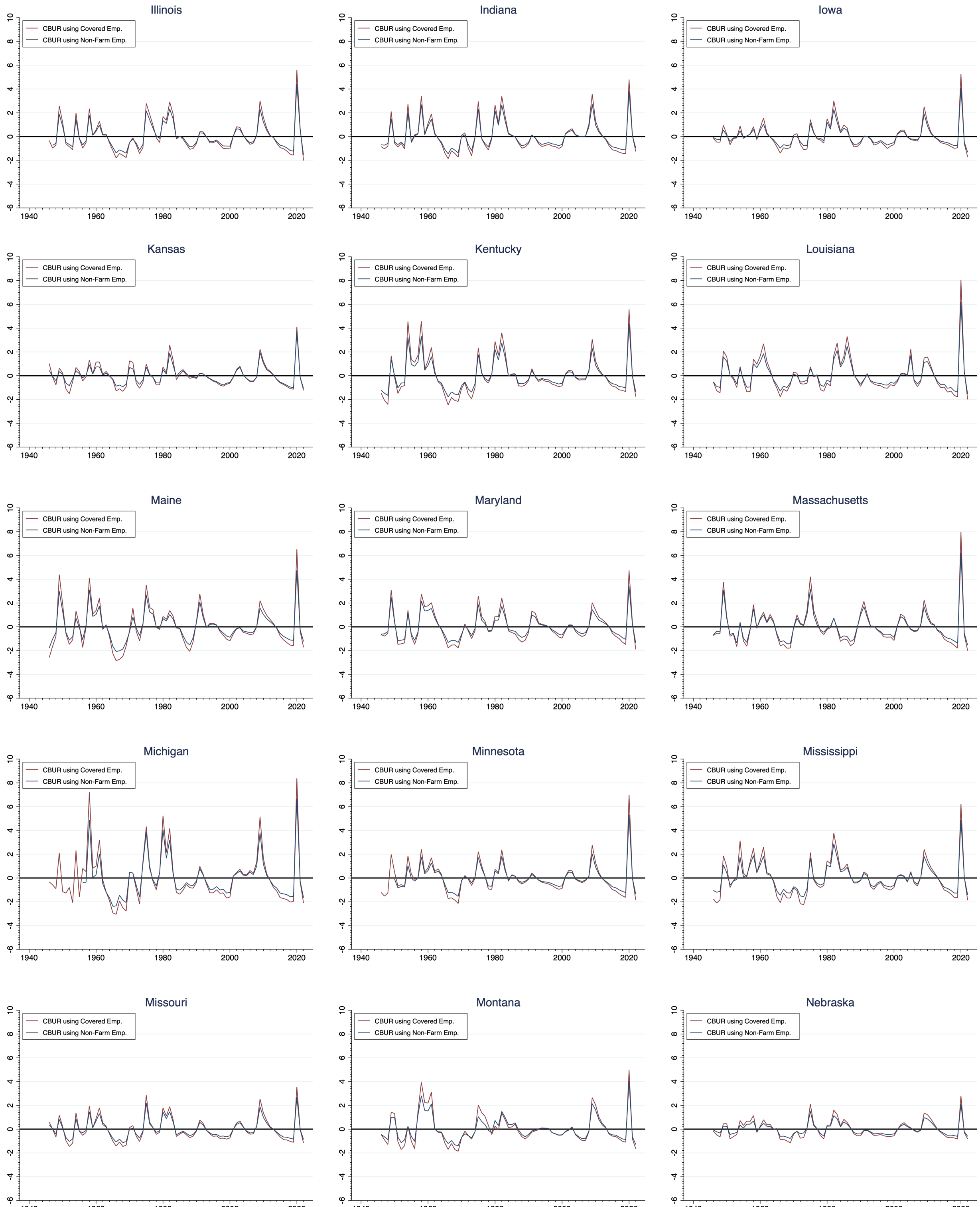


Figure B.4: Influence of Covered Employment versus Nonfarm Payroll Employment (Continued...)

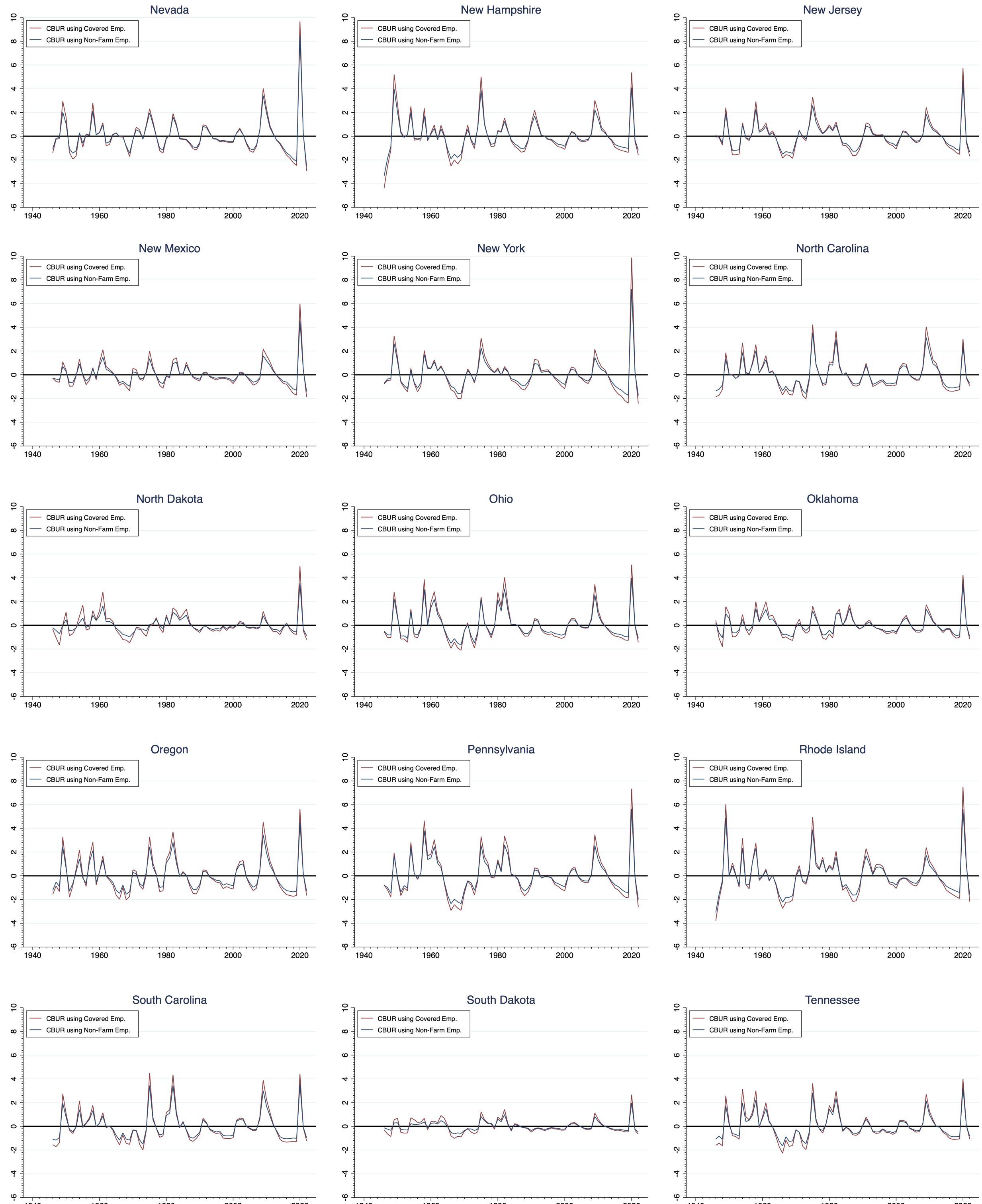
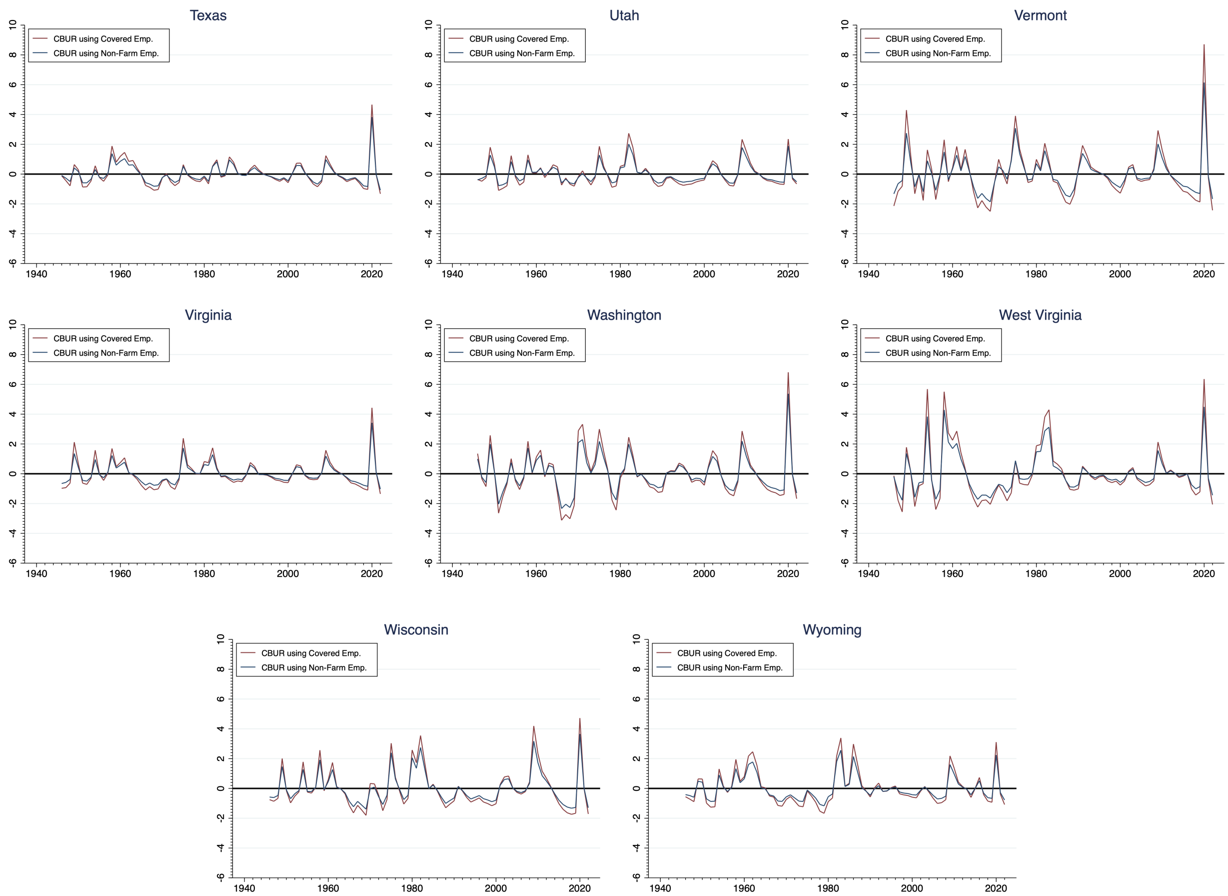


Figure B.4: Influence of Covered Employment versus Nonfarm Payroll Employment (Continued...)

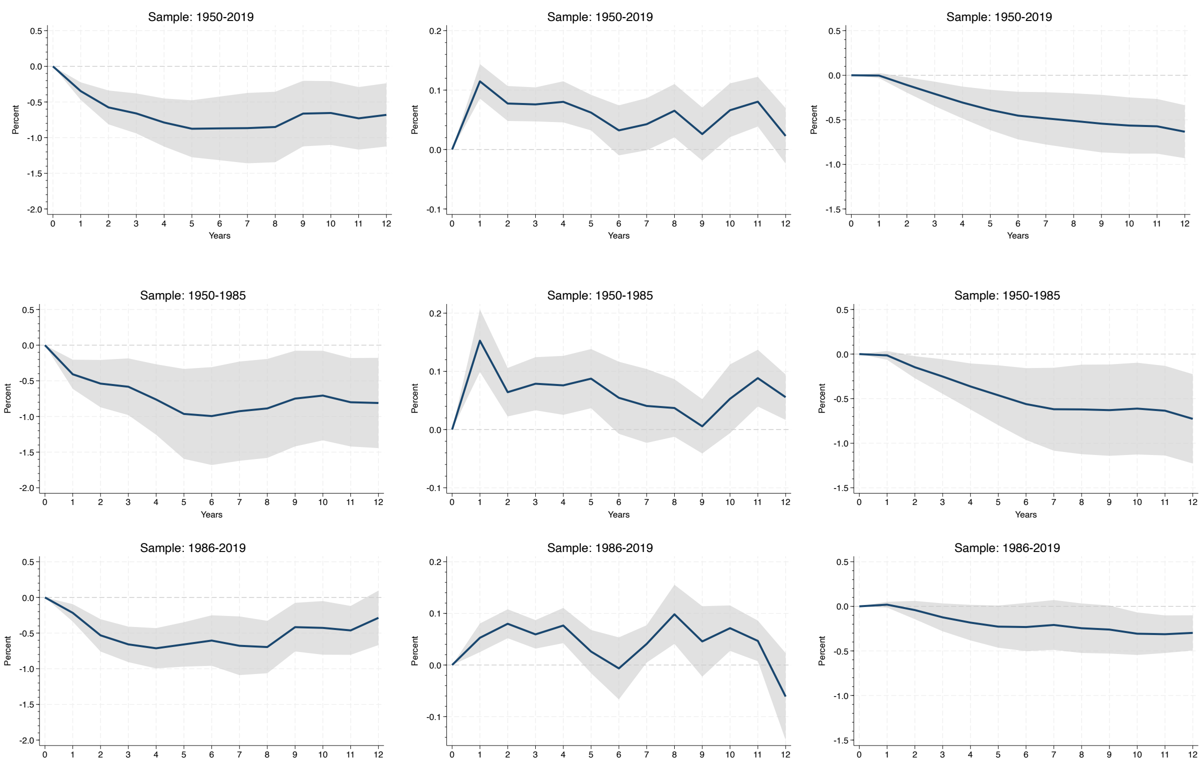


Appendix B.2. Robustness Checks for Labor Market Adjustments to Bartik IV

Given concerns about the reclassification of workers from manufacturing to services employment during the transition from SIC to NAICS industry codes, our analysis in the main paper drops all observations for our $rimix_{i,t}$ Bartik instrument over 2000–03; this approach is equivalent to constructing separate instruments predating the NAICS classification era (up to 1999) and for the NAICS era (2004 onward), avoiding any potential industry contamination from the five-year centered moving average construction of $rimix_{i,t}$ when switching from the SIC in 2001 to NAICS in 2002.[21] To examine the influence of any potential measurement error stemming from employment reclassification around the SIC/NAICS transition, Figure B.5 replicates Figure 5 of the main paper *without* dropping observations of our Bartik instrument for 2000–03, instead following the approach of Dao, Furceri, and Loungani (2017).

Figure B.5: Local Labor Market Responses to Bartik Instrument Without Dropping 2000–03

(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 a 1 percentage point decrease in state i's personal income growth relative to national average growth but without dropping observations for 2000-2003. Shaded bands denote 95% confidence intervals. Sample: 1950-2019 and subsamples.

The impulse responses estimate over the full sample, shown in the top row of Figure B.5, are

[21] We thank Christopher House, Andrea Foschi, Linda Tesar, and Christian Pröbsting for flagging this concern about potential measurement error stemming from the SIC/NAICS transition in the construction of our $rimix_{i,t}$ Bartik instrument.

negligibly affected by including the Bartik instrument for 2000–03. However, we see a much starker attenuation of the relative population response estimated over the more recent 1986–2019 subsample when we do not drop the Bartik instrument for 2000–03—much more consistent with the attenuation of relative population reported by Dao, Furceri, and Loungani (2017) for their 1991-2007 and 1991–2013 subsamples of study. The Dao, Furceri, and Loungani (2017) construction of a similar Bartik instrument mixes employment across the two industry classifications around the SIC/NAICS transition, which may be biasing their population response toward zero, similar to the influence of $rimix_{i,t}$ observations for 2000–03 on the relative population response shown in the bottom row of Figure B.5. We also see a bit less attenuation of relative employment and unemployment responses (and less transitory responses) in the more recent subsample when we do not drop the Bartik instrument for 2000–03. But while the magnitude and persistence of labor market adjustments to our Bartik instrument estimated over the SIC/NAICS transition are somewhat sensitive to the inclusion or exclusion of $rimix_{i,t}$ observations for 2000–03, we see an attenuation of relative employment, population, and unemployment responses in both specifications.

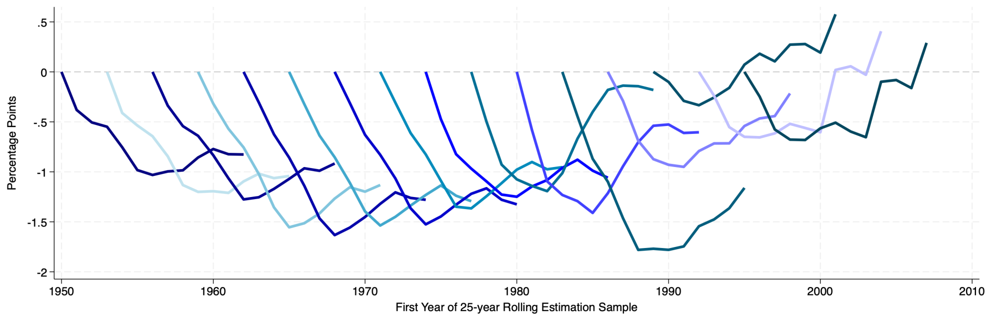
Given the sample selection sensitivity of local labor market responses to Bartik shocks, as a robustness test we also estimate the local projections in equation (4) from the main paper as rolling regressions over staggered 25-year estimation samples. Figure B.6 plots the rolling regression impulse responses 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.6 shows that the employment response to local labor demand shocks intensified over the late 20th century, with a peak effect rising from roughly -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 of the main paper, 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 of the main paper.

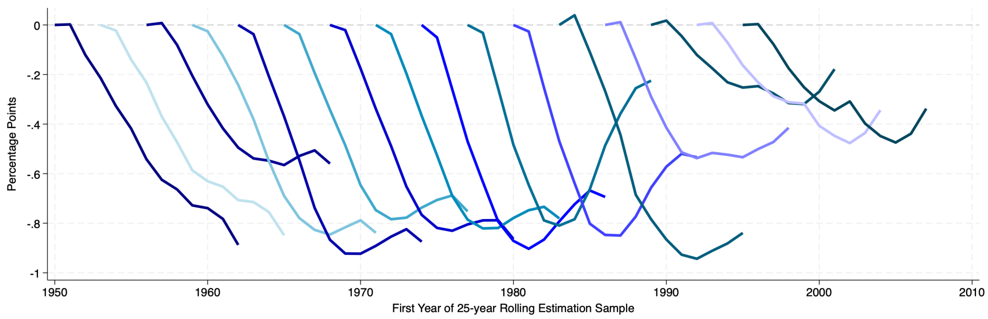
Like the employment responses, the rolling regression impulse responses of relative population responses to the Bartik instrument also diminish in more recent decades, as seen in the middle panel of Figure B.6. For the first half of the post-war estimation samples, relative population tends to see a steady and highly persistent decline in response to an adverse local labor market demand shock, with the peak effect often around -0.9 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 roughly -0.5 percentage points or less in samples spanning the late 1980s to present day. The peak effects of relative employment and relative population both drop roughly 45-65% in more recent decades, relative to the peak effects estimated over earlier post-war subsamples. But while the decline in employment shifts from persistent to transitory, the response of relative population remains far more persistent in more recent estimation samples.

Figure B.6: Rolling Regressions of Local Labor Market Responses to Bartik Demand Shocks

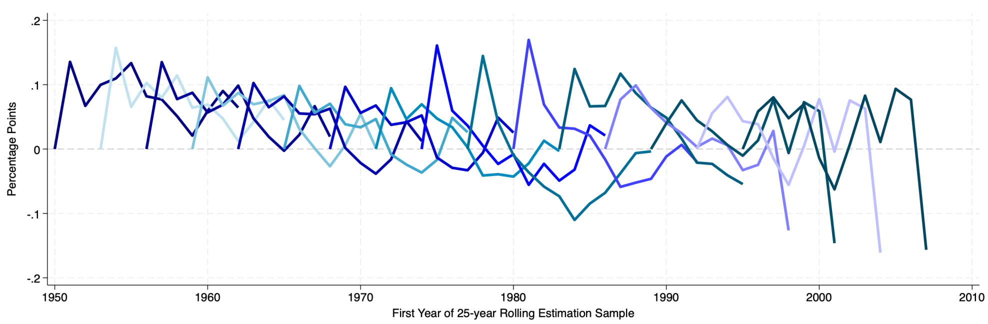
(a) Relative Employment



(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) from the main paper, using $rimix_{i,t}$ as the instrument but dropping observations for 2000-2003. 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.

In the bottom panel of Figure B.6, we see a similar trend toward moderating impulse responses of relative unemployment in more recent decades. Like the peak response of relative employment rising in the middle of our rolling regression subsamples, we see higher relative unemployment responses and more 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. For the first half of the post-war estimation samples, relative unemployment also sees the largest spike immediately on impact, but the peak effect is realized more gradually in more recent samples, in addition to the peak effect diminishing somewhat.

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) local labor market adjustments to local demand shocks appear relatively smaller in more recent decades. 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.[22] 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 attenuated labor market responses we see 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 1950-2019 sample (excluding 2000-03).[23] We define "smaller shocks" as those remaining in between these thresholds, and again drop all observations between 2000-03 around the SIC/NAICS transition:

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

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:

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

Results from this modified LP-IV regression are reported in Figure B.7. As conjectured, there is a strong non-linear response to the Bartik instruments: Responses to "very positive" (solid black lines) and "very negative" (dashed) shocks often tell a different story, most notably in divergent employment responses at longer horizons. Moreover, these largest relative shocks are driving the non-linear effects. A modified regression instead using "all positive" and "all negative" shocks produces very similar impulse response functions as those shown in Figure B.7, highlighting that these very large shocks are driving all the results.[24]

In other words, large *relative* labor market shocks are needed to trigger more substantial relative labor market adjustments, but in recent decades fewer states experience local labor market conditions that are significantly different than the national average. Our disaggregation of the $rimix_{i,t}$

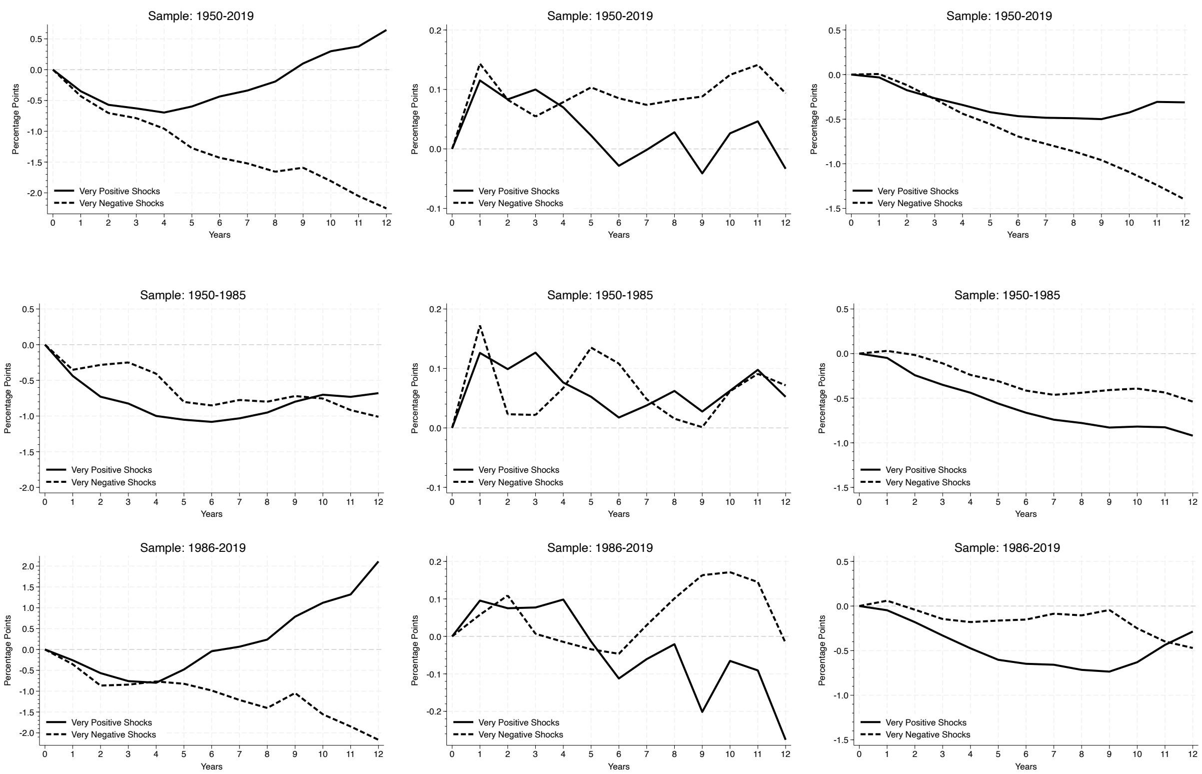
[22] Asymmetric local labor market responses to positive versus negative shocks have been documented in the literature, e.g., Davis, Loungani, and Mahidhara (1997), Dao, Furceri, and Loungani (2017), and Notowidigdo (2020).

[23] The cutoff for "very positive" labor shocks is above 0.53 percentage points and the cutoff for "very negative" labor shocks is -0.51 percentage points. The states with the most frequent "very negative" demand shocks are South Dakota (31 years), North Dakota (25), Iowa (25), Nebraska (24), Wyoming (22), West Virginia (21), Arkansas (20), and Idaho (20), while the "states" with the most frequent "very positive" demand shocks are Washington, D.C. (32), Nevada (30), New York (27), Alaska (21), Hawaii (21), Maryland (21), Massachusetts (21), and Wyoming (20).

[24] These results are omitted for brevity—they look almost identical to Figure B.7—but are available upon request.

Figure B.7: 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 (B-1), 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. Source: Authors' calculations based on digitized and publicly available data from the DOL, SSA, and BLS. Sample: 1950-2019 and subsamples.

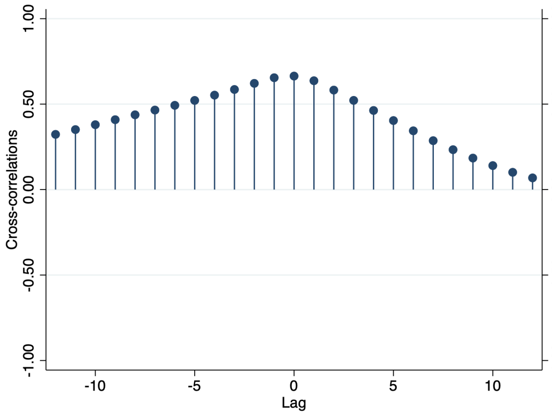
Bartik instrument reflects as much: There are far fewer “very positive" and “very negative" shocks in the 1986–2019 sample than the 1950–85 sample, and the mean (absolute) values are significantly smaller in the later sample than the earlier sample.[25] Fewer and relatively smaller “large" relative labor market shocks in more recent decades corresponds with the declining dispersion in states’ relative employment growth and unemployment rates seen in Figure 4 of the main paper, all wholly consistent with smaller relative employment, unemployment, and population responses in recent decades. Broadly speaking, 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 aggregate U.S. business cycle.

[25] See footnote 31 of the main paper.

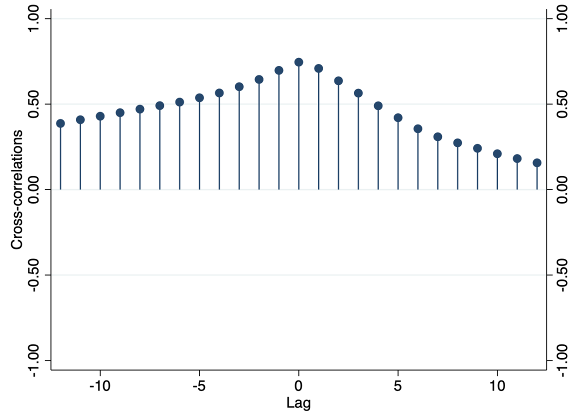
Appendix B.3. Additional Results and Robustness Checks

Figure B.8: Cross Correlations Between the U.S. Unemployment Rate, CBUR, and IUR

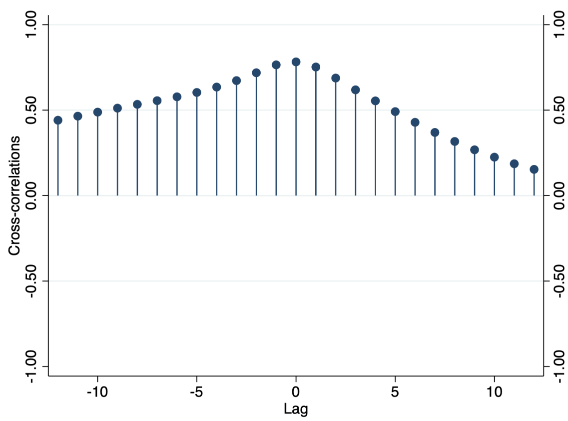
(a) U.S. Unemployment Rate and CBUR



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

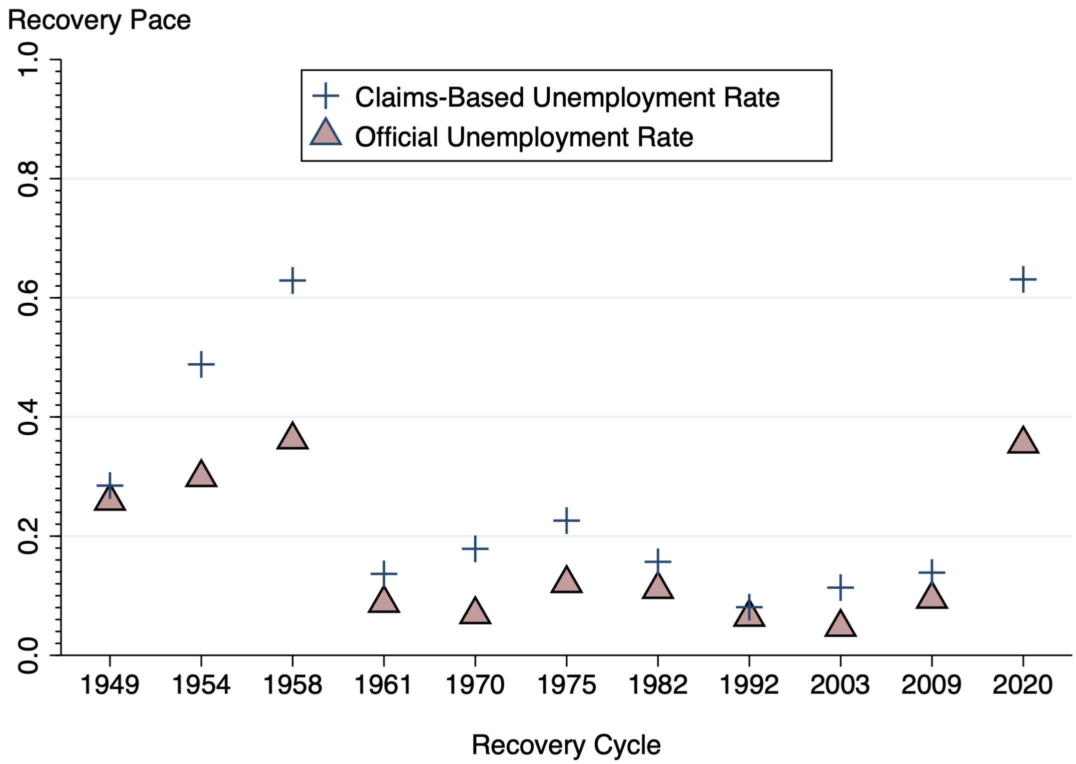


(c) U.S. Unemployment Rate and CBUR (Post-1970)



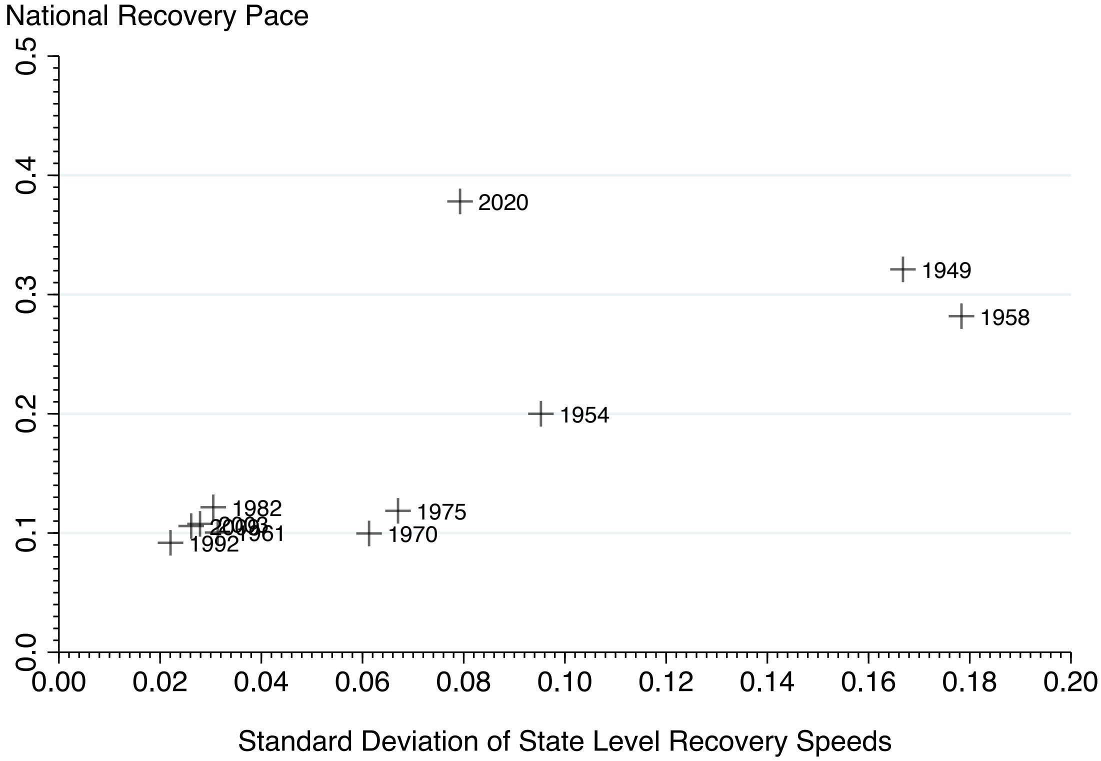
Notes: Panel (a) plots the cross correlations between the official U.S. unemployment rate (UR) versus our U.S. claims-based unemployment rate (CBUR) from 1948–2023. Panel (b) plots the cross correlations between the official U.S. unemployment rate versus the U.S. IUR, which is only available from 1971 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.9: National Unemployment Recovery Rates: Recession Dates from CBUR



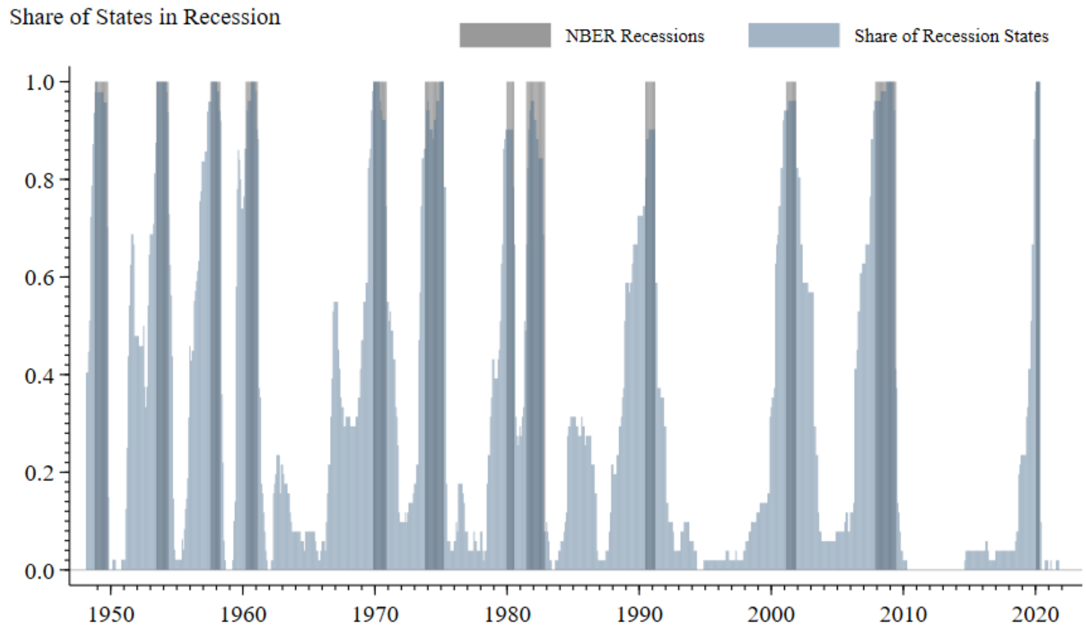
Notes: Recovery dates from DNS algorithm with recovery dates generated from the claims-based unemployment rate. Recovery from the 1980 recession is again excluded, see notes to Figure 7 of the main paper.

Figure B.10: 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 of the main paper for dates. 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 of the main paper.

Figure B.11: 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

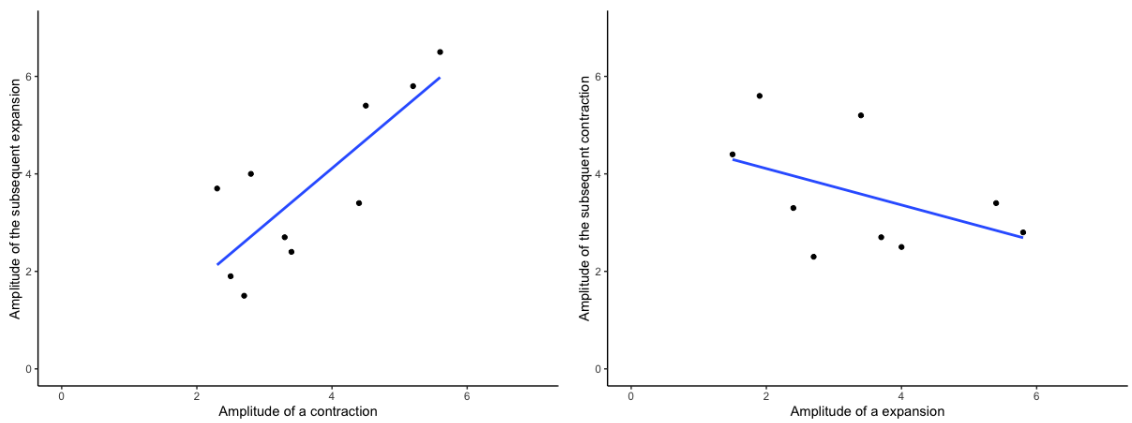
Dupraz, Nakamura, and Steinsson (2023) document an important asymmetry in U.S. unemployment dynamics throughout postwar recessions: Increases in the national unemployment rate during recessions are followed by decreases of a similar magnitude during the subsequent expansion, whereas the decrease in unemployment 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.

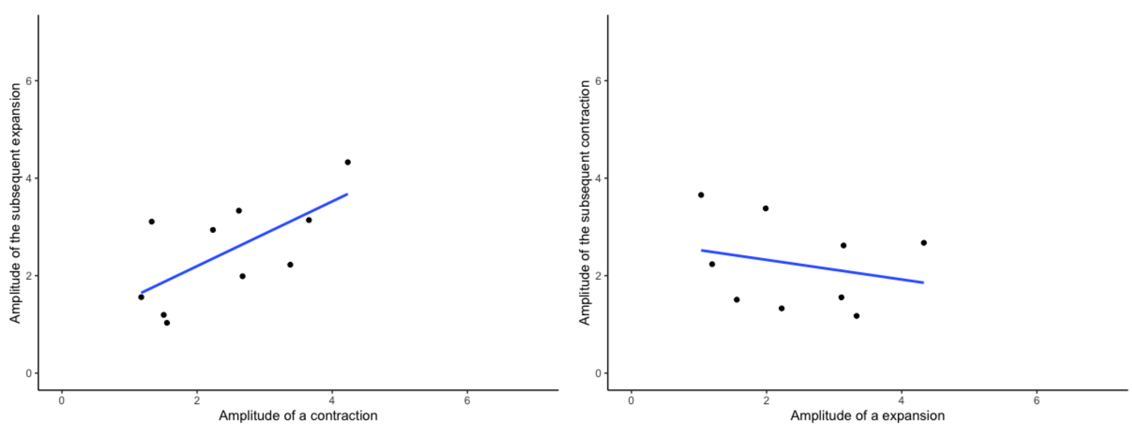
We first use our U.S. claims-based unemployment rate to try to replicate this plucking property at the national level, testing whether our series omits similar amplitude dynamics across the U.S. business cycle as documented by Dupraz, Nakamura, and Steinsson (2023). The amplitude of unemployment is measured 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 of the main paper. Figure B.12 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.[26] 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.12: 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 of the main paper. 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.12 underscores that our national claims-based unemployment rate exhibits very similar amplitude dynamics as Dupraz, Nakamura, and Steinsson (2023) document with the official

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

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 unemployment 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 of the main paper; 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 of the main paper 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.

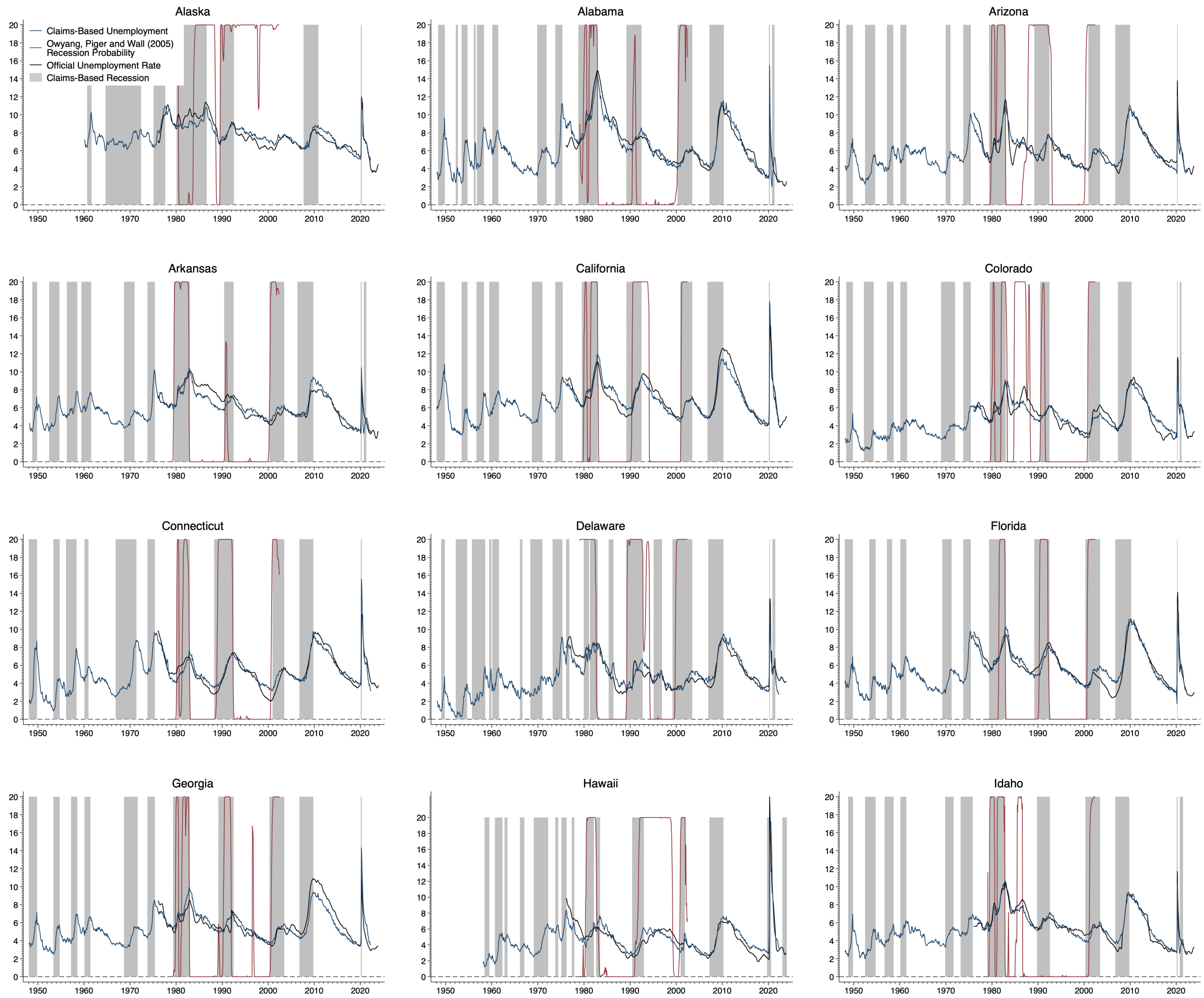
Appendix C. State Recession Dates and Unemployment

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 of the main paper 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...)

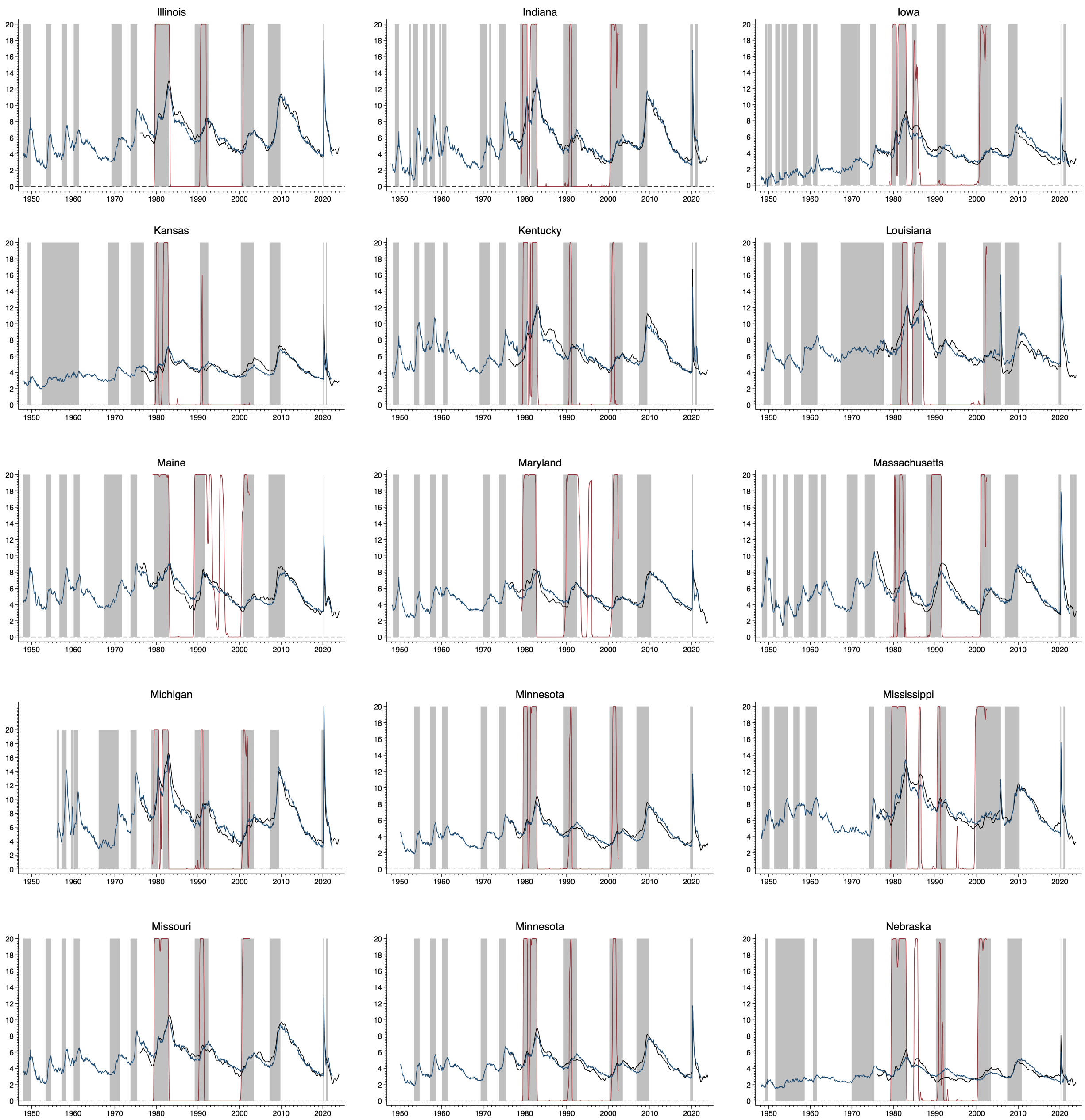


Figure C.1: State Recession Dates and Recession Probabilities (Continued...)

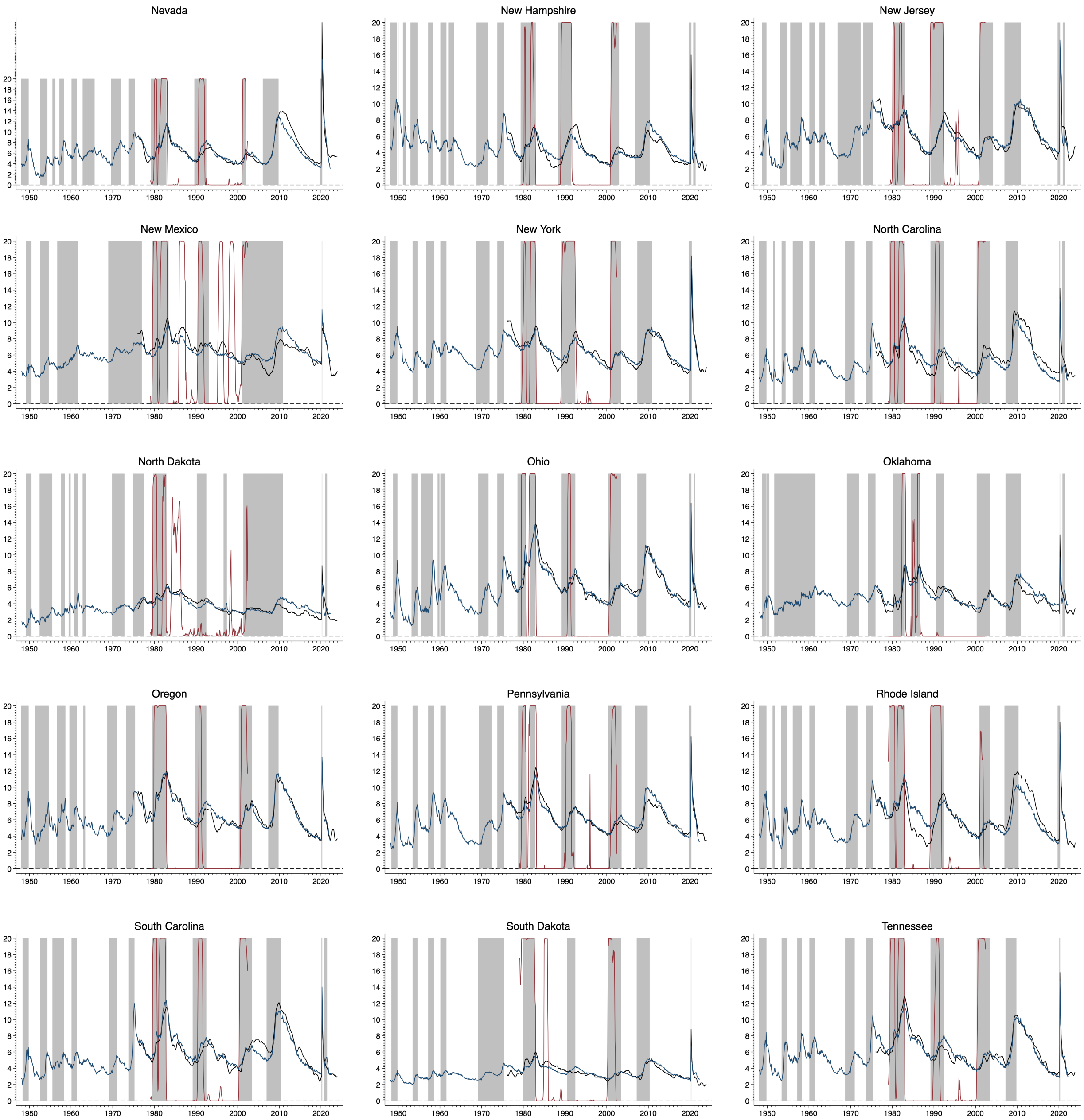
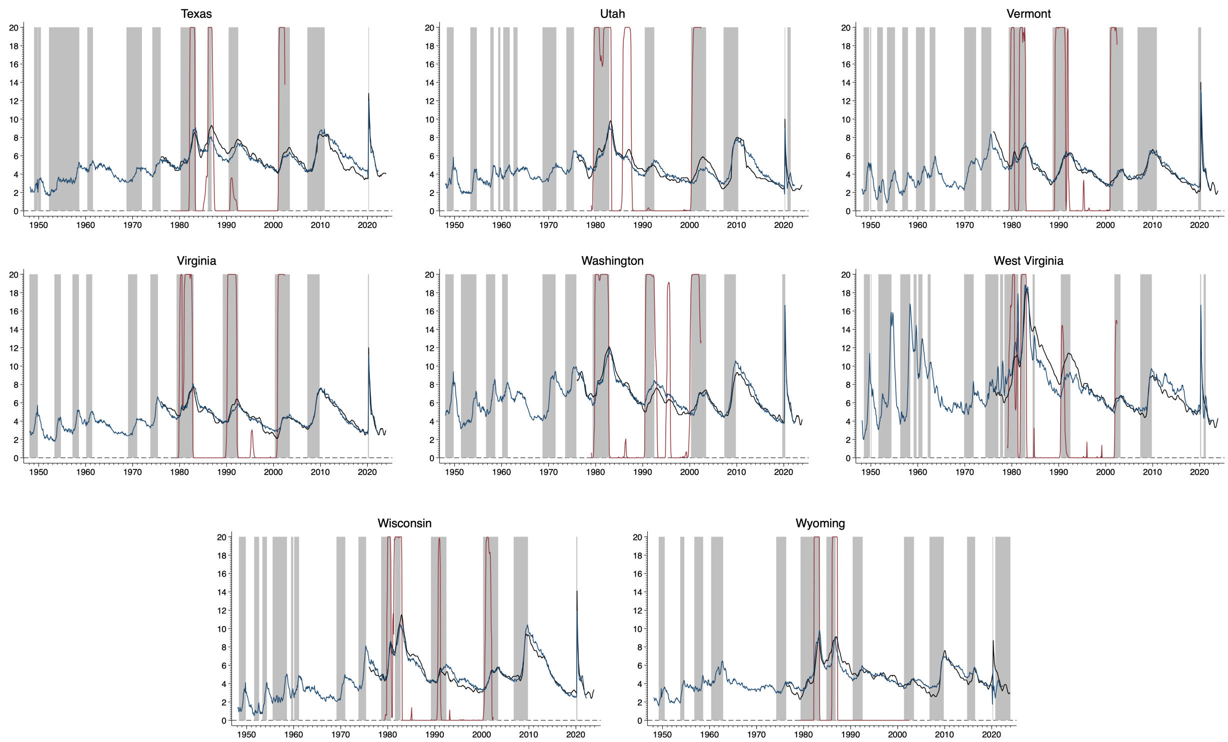


Figure C.1: State Recession Dates and Recession Probabilities (Continued...)



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