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Recessions and Local Labor Market Hysteresis

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ABSTRACT

This paper studies the effects of each U.S. recession since 1973 on local labor markets. We find that recession-induced declines in employment are permanent, suggesting that local areas experience permanent declines in labor demand relative to less-affected areas. Population also falls, primarily due to reduced in-migration, but by less than employment. As a result, recessions generate long-lasting hysteresis: persistent decreases in the employment-to-population ratio and earnings per capita. Changes in the composition of workers explain less than half of local hysteresis. We further show that finite sample bias in vector autoregressions leads to artificial convergence, which can explain why some previous work finds no evidence of hysteresis in employment rates.

JEL Classification Codes: I24, I26, J24, J31

Key Words: recessions, hysteresis, demand shocks, local labor markets, event study

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1 Introduction

Recessions receive enormous attention from researchers, policymakers, and the public. Most of this attention focuses on short-run, nationwide measures like the unemployment rate and gross domestic product (GDP). These outcomes are clearly important, but many of the broader consequences of recessions remain uncertain. One topic that has received comparatively little attention is how recessions affect local labor markets.

Previous research suggests that most recessions have only temporary impacts on local labor markets. Influential work by Blanchard and Katz (1992) finds that population adjusts quickly to labor demand shocks, generating complete recovery of employment rates within 10 years. Using the same methodology, Yagan (2019) finds similarly rapid recovery following the 1980–1982 and 1990–1991 recessions, but slower recovery from the Great Recession. Other influential work finds lasting impacts of Chinese import competition, which had large effects on some local labor markets (Autor, Dorn and Hanson, 2013). One reasonable interpretation of this evidence is that sufficiently large shocks might have lasting impacts on local labor markets, but smaller shocks do not. The accuracy of this interpretation has broad implications for our understanding of labor markets, economic opportunities available to workers and their children, and appropriate policy responses.

This paper examines how every recession in the United States since 1973 has affected local economic activity.[1] Specifically, we study how employment, population, and earnings evolve in local areas (metropolitan areas and commuting zones) where national recessions vary in severity. We draw upon multiple data sources, including those from the U.S. Bureau of Economic Analysis and the Census Bureau, to create annual panels of longitudinally harmonized geographic areas stretching over five decades. We estimate event study models that relate the evolution of local economic activity to sharp employment changes during recessions, while controlling for secular trends in population growth. This empirical strategy allows us to examine whether recessions have

[1] These recessions took place from 1973 to 1975, 1980 to 1982 (we pool the very short recession in 1980 with the longer one in 1981–1982), 1990 to 1991, 2001, and 2007 to 2009.

temporary or persistent impacts on local labor markets.

We find that employment declines during recessions generate permanent relative reductions in local employment. Moreover, these relative employment losses tend to grow over time. Across the five recessions that we study, a 5 percent decrease in metro-area employment during the recession (about the median for the Great Recession) on average leads to a 6.2 percent decrease in employment seven to nine years after the recession trough. During and immediately after recessions, the employment decline is driven by manufacturing and construction, two procyclical sectors. In the longer term, employment falls relative to less-affected areas by a similar amount across all industries, including services, trade, and government. Moreover, the sharp decreases in employment that occur during recessions are not associated with differential pretrends before the recession. These results suggest that areas that suffer a more severe recession experience a *permanent* relative decrease in labor demand.

The consequences of this employment decline depend on the extent of population adjustment. We see relative population declines that begin during the recession and continue for several years after the recession trough. This is consistent with the emphasis of Blanchard and Katz (1992) on the role of population adjustments as an important channel through which local labor markets respond to shocks. However, we do not find evidence that population declines because residents leave negatively impacted areas. Instead, after the 2001 and 2007–2009 recessions, for which we can use IRS data to measure in- and out-migration, the population decline stems entirely from reduced in-migration to severely hit areas. In fact, out-migration *falls* after each recession, and the net decline in population is too small to offset the decline in employment.

We thus find that each recession leads to long-lasting local hysteresis: areas hit by a more severe recession experience persistent relative declines in the employment-to-population ratio. These areas also experience a relative decline in earnings per capita. Averaging across recessions, a 5 percent recession-induced employment loss leads to a 3.2 percent (2 percentage point) decrease in the employment-to-population ratio and a 3.2 percent decrease in earnings per capita seven to nine years after the recession trough.

One possible explanation for the persistent decrease in local economic activity is a change in the composition of residents or jobs following a recession. We see a persistent increase in the share of residents aged 65 and above and a decrease in the share of residents aged 15–39, but the size of these impacts is modest. To examine other compositional shifts, we turn to individual-level data from the decennial census and the American Community Survey (ACS). Following the 1973–1975, 1990–1991, and 2007–2009 recessions, we see a decrease in the share of workers employed in managerial, professional, and technical occupations and an increase in the share employed in manual and service jobs. For these same recessions, we also see a decrease in the share of residents with a college degree and an increase in the share with no more than a high school degree. For the 1980–1982 and 2001 recessions, there is less evidence of a shift in occupational or educational composition. The fact that we see hysteresis for all recessions, but a change in education and occupation shares for only three, suggests that compositional changes are not the key drivers. Indeed, when we estimate recessionary impacts on demographically adjusted local labor market aggregates, we conclude that changes in demographics (education, age, sex, and race/ethnicity) explain less than half of the overall impacts on average earnings and income.

The impacts of recessions on local labor markets have changed little over the past 50 years. This similarity is remarkable, given the different macroeconomic drivers of the recessions and the secular changes in business dynamics (Haltiwanger, 2012; Decker et al., 2016), mobility (Molloy, Smith and Wozniak, 2011, 2014), and demographics (Shrestha and Heisler, 2011). Even recessions that are less severe in aggregate terms, such as those in 1990–1991 or 2001, lead to sizable and persistent shifts in the distribution of economic activity across space.

We also help resolve a longstanding debate—initiated by the landmark studies of Bartik (1991) and Blanchard and Katz (1992)—on whether demand shocks lead to local labor market hysteresis. Bartik (1991) estimates distributed lag regression models on metropolitan-level data and finds evidence of hysteresis. In contrast, Blanchard and Katz (1992) estimate vector autoregressions (VARs) on state-level data and find that the unemployment rate, labor force participation rate, and wages return to trend within ten years after negative labor demand shocks. Dao, Furceri and

Loungani (2017) and Yagan (2019) use the Blanchard and Katz (1992) methodology and find a similar degree of medium-run convergence.[2] We show how finite sample bias can lead to artificial convergence in VAR impulse response functions. Although this bias is well-known (e.g., see the discussion in Kilian and Lütkepohl, 2017), its importance for studies of hysteresis has been underappreciated. Using empirically relevant Monte Carlo simulations, we show that VARs can incorrectly imply convergence after a permanent demand shock. This bias would be of first-order importance even if researchers had access to 100 years of data. As a result, the convergence found by Blanchard and Katz (1992) and other authors using the same methodology appears to stem from finite sample bias.

This paper has two key contributions. First, we demonstrate a general and persistent relative decline in economic activity in local areas that experience a more severe recession. These results show that the consequences of recessions last longer—and that labor market adjustments to shocks occur more slowly—than previously thought. Second, we show that finite sample bias casts doubt on the common practice of using VARs to study local labor market hysteresis and related phenomena. We also show that event study regressions do not suffer from this finite sample bias, which makes them well suited for future research on this question.

Our work complements several other studies that examine how labor demand shocks (such as a change in manufacturing jobs) affect earnings, employment, and population in local areas (e.g., Bound and Holzer, 2000; Notowidigdo, 2013; Freedman, 2017; Amior and Manning, 2018; Beaudry, Green and Sand, 2018; Garin, 2019). These papers do not study recessions but instead focus on changes in jobs over 1- or 10-year horizons across all phases of the business cycle. As a result, these studies provide limited guidance on the short- and long-run effects of recessions on local areas. Additional evidence is particularly valuable because of the disagreement in the literature over whether labor demand shocks have persistent effects on wages and employment, and

[2]Dao, Furceri and Loungani (2017) use a different source of identification and find that population is less responsive in the short run. In addition to examining the implications of the Blanchard and Katz (1992) model for the recovery of states following recessions, Yagan (2019) uses tax data to show that individuals living in areas severely affected by the Great Recession suffered enduring employment and earnings losses regardless of whether they stayed in the same location or moved away.

how, when, and why these relationships may have changed (Bartik, 1993, 2015; Austin, Glaeser and Summers, 2018). Greenstone and Looney (2010) and Stuart (2018) provide evidence that recessions lead to persistent declines in earnings per capita at the county level; our analysis goes considerably further, by examining a larger range of outcomes and results at other levels of geography.

2 Conceptual Framework

To guide our empirical analysis, we describe how recessions might affect local labor markets. Our starting point is that labor demand falls during recessions. This decrease could stem from many possible sources, such as an increase in interest rates or oil prices, or a consumption decline driven by expectations or animal spirits. The decline in labor demand generally differs across local labor markets, possibly because of differences in industrial specialization or the types of tasks performed.

A local recession shock—i.e., a decline in labor demand during the recession—may or may not catalyze a persistent decline in labor demand. If the shock leads only to a temporary decline in labor demand, then employment, employment rates, and wages would fall during the recession and return to their previous trend afterward. This pattern would arise if firms temporarily laid off workers or reduced their hours, and if individuals did not move across labor markets in the short run.

On the other hand, a recession shock could catalyze a persistent decline in local labor demand, possibly because employers change their production process (Jaimovich and Siu, 2015; Hershbein and Kahn, 2018) or shut down (Foster, Grim and Haltiwanger, 2016).[3] Although the short-term dynamics are similar whether the decline in labor demand is temporary or persistent, the latter

[3]The possibility of a persistent decline in local labor demand relates to the relative importance of agglomeration and locational fundamentals as determinants of economic geography. Davis and Weinstein (2002, 2008) find striking evidence of a recovery in Japanese city population and manufacturing employment following Allied bombings in World War II. These results suggest that rationalizing a persistent decline in local labor demand would require that fundamentals change during recessions. This might seem surprising, but the presence of adjustment costs could diminish firms' responses to secular changes, and firms might pay these adjustment costs during recessions (Foote, 1998). Moreover, there is some disagreement about the relative importance of fundamentals and agglomeration (e.g., Bosker et al., 2007; Miguel and Roland, 2011; Michaels and Rauch, 2018).

generates a lasting decline in employment. The response of other variables depends on the elasticities of labor supply within and across local labor markets. If labor supply is perfectly elastic, then wages and employment rates return to their prior trend (Blanchard and Katz, 1992). If labor supply is less than perfectly elastic, then declines in wages and employment rates are persistent.

This framework implicitly assumes there is only one type of worker. However, worker heterogeneity can also generate persistent declines in economic activity. For example, if high-income workers are more likely to leave an area in response to a recessionary shock (Bound and Holzer, 2000; Wozniak, 2010; Notowidigdo, 2013), then average wages might fall simply because of a change in worker composition. If younger workers are more likely to leave an area in response to a recessionary shock (Molloy, Smith and Wozniak, 2011)—or are less likely to move in—then the average employment rate might fall. Firm heterogeneity also could generate persistent declines in economic activity (e.g., if large, high-paying firms are more likely to relocate or shut down).

This framework points to several takeaways. First, we expect to see temporary declines in employment, employment rates, and wages following recessionary shocks. Second, a persistent decline in employment indicates a persistent decline in local labor demand. Third, the responsiveness of population influences whether employment rates and wages recover or decline persistently. Finally, changes in worker composition could partly explain any persistent changes in employment rates and wages. Guided by these implications, we next describe our strategy for estimating how recessions affect local labor markets.

3 Estimating the Impacts of Recessions on Local Labor Markets

3.1 Data

We compile several public-use data sets to measure local economic activity. These data sets are constructed by government agencies using administrative data. Employment is available from the Bureau of Economic Analysis Regional Economic Accounts (BEAR), the Census County Business

Patterns (CBP), and the Quarterly Census of Employment and Wages (QCEW).[4] BEAR and CBP data are available starting in 1969, while QCEW data are available from 1975 onward. BEAR data also contain aggregate earnings. We use the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) data for annual population estimates, which are available by sex, race, and age. To measure in- and out-migration, we use the Internal Revenue Service Statistics of Income (SOI) data.[5] Finally, we use tabulations and microdata from the decennial census and the American Community Survey (ACS) to examine the earnings distribution and compositional changes.[6]

With the exceptions of the decennial census and the ACS microdata, all of the data sets are available at the county level. The census and the ACS are available at the Public Use Microdata Area (PUMA), which we map to other geographies using crosswalks available from the Geocorr program of the Missouri Census Data Center. Consequently, we can examine the effects of recessions at multiple levels of geography, including metropolitan area and commuting zone.[7] Metropolitan areas and commuting zones are commonly used to approximate local labor markets, although there is some disagreement as to which provides the better approximation (Foote, Kutzbach and Vilhuber, 2017).[8] Both types of areas are composed of counties, so it is straightfor-

[4]Because employment counts are often suppressed in CBP data for small counties and industries, we adopt the imputation procedure of Holmes and Stevens (2002) and Stuart (2018) when necessary; we also supplement the CBP files released by the Census Bureau with WholeData, an industry-harmonized version of the data, available from 1998 through 2016, that uses a linear programming algorithm to recover suppressed employment estimates (Bartik et al., 2019). For periods when they overlap, the results using the imputation procedure in Holmes and Stevens (2002) and Stuart (2018) agree closely with those using Wholedata.

[5]Although SOI data capture moves only for tax filers and are available only from the 1990s, they are considered a high-quality source for point-to-point migration flows and have been used in several papers (e.g., Kaplan and Schulhofer-Wohl, 2012, 2017; Wilson, 2018). We use a version of these data compiled by Janine Billadello of Baruch College's Geospatial Data Lab (Billadello, 2018).

[6]We use versions of these tabular data and microdata from NHGIS and IPUMS, respectively (Manson et al., 2019; Ruggles et al., 2019). The Data Appendix describes the processing of these data and how we link individuals to our geographies of interest. We also explored the possibility of using the Current Population Survey, which contains many of the same demographic items as the census and the ACS and, starting in 1989, provides meaningful substate geographic indicators in the basic monthly version of the data. However, while we have harmonized these substate geographies over time (see Data Appendix), changes in sampling result in relatively few areas with sufficient sample sizes to offer meaningful analysis.

[7]We can also examine counties, but these are often too small to constitute local labor markets, our area of focus.

[8]Metropolitan statistical areas are defined by the Office of Management and Budget (OMB) as having "at least one urbanized area of 50,000 or more population, plus adjacent territory that has a high degree of social and economic integration with the core as measured by commuting ties" (Office of Management and Budget, 2003). Commuting zones are defined based on commuting patterns and do not have a minimum population threshold or urban requirement

ward to map our county-level data into metro areas or commuting zones. A slight complication is that neither definition is fixed over time; we use core-based statistical areas (CBSAs) as defined by OMB in 2003 (reflecting the 2000 census) and commuting zones also based on the 2000 census. Although we focus on metro areas because of their greater size and thicker labor markets, we show that our core results are robust to using commuting zones, which unlike metro areas cover the entire United States.[9]

3.2 Empirical Strategy

Our empirical strategy relies on cross-sectional variation in sharp employment changes that occur during nationwide recessions. Separately for each recession, we use this variation to estimate the impacts of local recessionary shocks on labor market outcomes.

One natural approach is to estimate the event study regression

$$y_{i,t} = \text{shock}_i \delta_t + x_{i,t}\beta + \mu_i + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is a measure of local economic activity in location i and year t, shock_i is the log employment change in location i from the nationwide peak to trough (multiplied by -1), $x_{i,t}$ is a vector of control variables, and μ_i is a location fixed effect that absorbs time-invariant differences across locations. The key parameter of interest is δ_t, which describes the relationship between the recession shock and local economic activity in year t. The inclusion of location fixed effects means that one of the δ_t coefficients must be normalized; we do this two years before the nationwide peak because the exact timing of recessions is uncertain and there is variation in when aggregate economic indicators decline.[10] This specification allows the impact of the recession shock to vary flexibly across years, transparently showing both pretrends and dynamic impacts.

An important issue with estimating Equation (1) in our setting is that log employment is both an

(Tolbert and Sizer, 1996).

[9] Metro areas cover between two-thirds and five-sixths of both people and jobs throughout our sample, and this share has grown over time.

[10] Because we show the entire range of estimates of δ_t, it is straightforward to see how our estimates would change with a different normalization year.

outcome of interest and used to construct the recessionary shock. This can introduce a mechanical correlation between the dependent variable and the shock variable, so that estimates of δ_t for all years are inconsistent.[11] Instead, we estimate

$$y_{i,t} = \text{shock}_i \delta_t + x_{i,t}\beta + y_{i,t_0-2}\gamma_t + \varepsilon_{i,t}. \tag{2}$$

Equation (2) does not include location fixed effects, but instead controls for time-invariant cross-sectional differences using the dependent variable two years before the peak, y_{i,t_0-2}. We allow the coefficient γ_t to vary by year to increase the flexibility of this control. Unlike Equation (1), estimates of δ_t from Equation (2) generally are consistent under the null hypothesis of a random walk process.

We construct the recession shock using annual employment data from BEAR.[12] We modify NBER recession peak and trough dates to account for our use of annual data. Specifically, we define *shock*$_i$ to be the log employment change for each geography between 1973 and 1975, 1979 and 1982, 1989 and 1991, 2000 and 2002, and 2007 and 2009.[13] Using fixed national timings for each recession, rather than location-specific peak-to-trough periods, introduces some measurement error but minimizes the risk of endogeneity. We use wage and salary employment (private and public) to define the recession shock, as coverage of the self-employed is incomplete and varies over time.

We include several control variables in $x_{i,t}$ to bolster our interpretation of estimates of δ_t as

[11] To see this problem, consider normalizing $\delta_t = 0$ for the peak year $t = t_0$. Equation (1) then can be rewritten as

$$y_{i,t} - y_{i,t_0} = (y_{i,t_1} - y_{i,t_0})\delta_t + (x_{i,t} - x_{i,t_0})\beta + (\varepsilon_{i,t} - \varepsilon_{i,t_0}),$$

where shock$_i \equiv -(y_{i,t_1} - y_{i,t_0})$. It is straightforward to show that, if $y_{i,t}$ follows a stationary random walk, the probability limit of $\hat{\delta}_t$ equals -0.5 for all years except the trough year, when the coefficient equals -1 mechanically. We mitigate this problem by normalizing δ_t two years before the peak, but we still prefer Equation (2), because it has better properties for any choice of normalization year and can be extended to control for a vector of lagged dependent variables.

[12] QCEW is an alternative. While quarterly data would allow us to use the NBER recession quarters to define the shock, they would also require a seasonal adjustment. In practice, as we show below, results are robust to using either source to define the shock.

[13] The NBER recession dates are November 1973 to March 1975, January 1980 to July 1980, July 1981 to November 1982, July 1990 to March 1991, March 2001 to November 2001, and December 2007 to June 2009.

reflecting the effect of local recession shocks. First, we include census division-by-year fixed effects to flexibly capture broader changes in economic conditions and demographics. Second, we control for interactions between prerecession population growth and year to adjust for slow-moving changes in population and demographics.[14]

Estimates of δ_t capture the relationship between local recession shocks and *relative* changes in economic activity before and after recessions. For example, although aggregate employment trended upward throughout our sample period, estimates of δ_t do not reflect this aggregate movement, as changes in economic activity at the division-year level are absorbed by fixed effects. A negative value of δ_t implies that a more severe shock reduces economic activity relative to a less severe shock. Although for simplicity's sake we do not always frame our discussion in relative terms explicitly, all our findings should be interpreted in this manner. Our estimates reflect both demand and supply adjustments after the initial labor demand shock during the recession. We cluster our standard errors at the metro or community-zone (CZ) level to allow for arbitrary autocorrelation in the error term $\varepsilon_{i,t}$.

3.3 The Severity of Recessions across Time and Space

Before moving to estimates of Equation (2), we describe the characteristics of the five recessions that are our focus. Figure 1 displays aggregate seasonally adjusted nonfarm employment from the Current Employment Statistics from 1969 to 2016. Nationwide employment more than doubled over this period. This growth was interrupted by five recessions (combining the two in the early 1980s), as indicated by the vertical shaded bars in the graph. While there is little consensus on the macroeconomic causes of each recession, the drivers almost certainly differ (Temin, 1998). The 1973–1975 and 1980–1982 recessions followed increases in the price of oil and subsequent increases in interest rates by the Federal Reserve. There is less agreement on the causes of the 1990–1991 recession (Temin, 1998) or the 2001 recession. The 2007–2009 recession followed

[14]We control for the log change in population aged 0–14, 15–39, 40–64, and 65 and above. We construct these population variables using SEER data, which are available starting in 1969. The prerecession population growth years are 1969–1973 (for the 1973–1975 recession), 1969–1979 (for the 1980–1982 recession), 1979–1989 (for the 1990–1992 recession), 1990–2000 (for the 2001 recession), and 1997–2007 (for the 2007–2009 recession).

tumult in housing and financial markets.

Using annual data from BEAR, Table 1 shows the national changes in employment from peak to trough for each recession, both overall and for major industrial sectors. (We use BEAR data rather than national Current Employment Statistics data to be consistent with our subsequent analysis, but the patterns are qualitatively similar.) The recessions vary greatly in overall magnitude, from a 3 percent employment decline during the Great Recession to a 1 percent increase from 1989 to 1991, with the others falling somewhere in between. Manufacturing and construction usually experience the largest employment decline, with the exception of construction during the 2001 recession, which was accompanied by a housing boom. The impact on other industries varies widely across recessions. The early 1990s downturn and the Great Recession were broad in scope, as most major industries experienced an employment decline. The early 1980s recession was heavily concentrated in certain industries, including manufacturing and construction. Similarly, the mid-1970s recession and the one in 2001 saw flat or rising employment in several industries, including the relatively large services sector. Our use of annual BEAR data masks some of the severe employment losses that are evident in monthly data.

These patterns suggest that areas with employment bases reliant on manufacturing and/or construction were more likely to suffer severe recessions, although the variation across recessions in other industries implies that it is not necessarily the same areas being hit each time. Figure 2 shows the severity of each recession (as captured by log employment change) across metropolitan areas. While many areas in the Midwest Rust Belt fare poorly in each recession, there is considerable heterogeneity for other areas. The Northeast, for example, is severely affected in the 1970s, 1990s, and 2001, but only modestly in the early 1980s and late 2000s. The Pacific Northwest fares relatively well in the 1970s and 1990s but is hit harder in the other three recessions. There is also ample variation across areas in severity within a given recession, as several areas actually gained employment in each case.[15]

Figure 3 displays the frequency with which a given area experienced a severe recession over the

[15]Panels A and B of Appendix Figure A.1 present kernel densities of the demeaned and unadjusted log employment changes across metro areas for each recession.

sample horizon. We define a metropolitan area as having a severe recession if it experienced a log employment change worse than the median area for a given recession. The Detroit and Chicago metro areas, for example, experienced downturns worse than the median for all five recessions, while the Houston metro area did so only in 2001. The distribution in severity frequency is roughly symmetric, with a similar number of metro areas experiencing zero or one severe recession (109) as those experiencing four or five (103).

We show the serial correlation in recession severity in Table 2. Panel A shows the raw correlations across metros in log employment changes for each pair of recessions. As suggested by Figures 2 and 3, there is moderate positive serial correlation generally, although, consistent with the different origins of the recessions as well as temporal changes in industrial mix, the pattern is not monotonic across time. Notably, the Great Recession is basically uncorrelated with the previous two recessions, and the early 1990s recession is uncorrelated with the early 1980s recession. We also show in Panel B the correlations within each of the nine census divisions (that is, after partialing out division fixed effects), and in Panel C the correlations after additionally controlling for prerecession population growth. These controls tend to slightly reduce the magnitudes of the correlations, but positive serial correlation remains in a few cases. Our event study approach will reveal whether this serial correlation affects the estimates. We also control for the severity of previous recessions as an additional robustness check and show that these additional controls do not appreciably change the results.

Table 3 describes the characteristics of metro areas that experience a more severe recession and those that experience a less severe recession (defined as whether the log employment change is above or below the median). We measure these characteristics using the closest decennial census to the recession start year, except for the 2007–2009 recession, which is measured using the 2005–2009 ACS. Recessions tend to be more severe in places with higher population but slower prerecession population growth, higher employment rates and earnings per capita, a higher manufacturing employment share, and a less educated workforce. The largest difference between areas that experience a more severe recession versus those that experience a less severe recession is the manufacturing employment share, though this difference has decreased considerably over time. Moreover, many of the differences are quite small. The variables in Table 3 include both sources of recession severity and factors that might influence the response of local areas to demand shocks. As a result, we do not control for these variables in our regressions, but we do examine their association with postrecession dynamics.

4 Results: Local Hysteresis Effects

4.1 Employment

We begin with estimates of Equation (2) for log employment in metro areas. Each of the five panels in Figure 4 shows separate estimates for each recession. We include four or five years (as data permit) before the employment peak to capture any pretrends, and we follow areas for up to 10 years after the trough. Specification 1, shown in red circles, includes only census division-by-year fixed effects in $x_{i,t}$. Our preferred Specification 2 (solid blue line) also controls for prerecession age-group-specific population growth, as described above. Specification 3 (green squares) further adds the employment shock from the previous recession, which is possible for all but the mid-1970s recession. Finally, Specification 4 (black triangles) further includes employment shocks for *all* previous recessions since the mid-1970s.

Overall, there is little evidence of pretrends from Specification 1. The exceptions are negative pretrends in the 1980–1982 and 2001 recessions, suggesting that serial correlation from the previous recession or some other factor causing an employment slowdown was already at work before these recessions struck. Adding controls for prerecession population growth eliminates these pretrends. Since the population growth is calculated over the previous decade, it is likely that we eliminate secular trends (such as the growing migration to warmer southern and western areas over time).[16]

[16] It is also possible that we remove previous recession-induced changes to population growth. However, the correlations in Table 2 between the 1980–1982 and 2001 recessions and the recessions that immediately preceded them are small. Moreover, since our objective is to estimate the long-term employment effects for an area of a given recession, net of previous ones, whether the pretrends are driven by secular or long-lasting cyclical effects is not paramount; it is

In each recession, the employment shock is mechanically correlated with a large, immediate drop in log employment. Because we normalize the base period to $t_0 - 2$ (two years before the peak), the coefficient at the trough need not be exactly -1, although the estimate is generally close to this number, reflecting flat pretrends.[17] Much more interesting is that in each recession, the decline in employment shows little to no recovery over the subsequent 10 years. In the case of the 1990–1991 and 2001 recessions (which have been noted as having "jobless" and "jobloss" national recoveries), employment continued to fall over this period. Moreover, the confidence intervals imply that we can reject a return to initial peak employment in every subsequent time period shown. The graphs also show that the durability of the negative shock is not affected by whether shocks from the *previous* recession(s) are included as controls, which supports our identification strategy. We obtain similar results when examining employment from County Business Patterns data (Appendix Figure A.2), where we also see a persistent decline in the number of establishments (Appendix Figure A.3).

It is important to keep in mind that the event study coefficients capture relative changes. To highlight this point, Appendix Figure A.4 shows the event study coefficients from our main specification in Panel A of Figure 4 alongside the implied evolution of mean log employment in areas with a more versus a less severe recession. Employment grows after 1975 for areas with a more severe recession, but at a permanently lower level.

Panel A of Table 4 summarizes the (preferred) Specification 2 results seven to nine years after the recession trough.[18] The medium-run employment elasticities range from -0.8 to -1.7. Interestingly, the elasticity is smaller in magnitude—and statistically different—for the two most severe recessions nationally, that in 1980–1982 and the Great Recession. Because the severity of the employment shock varies both across recessions and across areas within a given recession (Appendix Figure A.1), we also report standardized effects. A one-standard-deviation increase in the (absolute

sufficient that we can adequately control for it.

[17]The difference between coefficients from peak-to-trough mechanically equals -1 for the employment regressions because the shock variable is constructed as the difference in log employment.

[18]We generate the results in this table by restricting the prerecession coefficients to be zero and pooling the coefficients in Equation (2) for years 1–3, 4–6, and 7–9 after trough. This yields a simple and efficient summary.

value) of the employment shock in the 1973–1975 and 1980–1982 recessions reduced employment by about 7 percent seven to nine years after the recession trough. The two most recent recessions exhibit less variation across areas in severity, so the long-term impacts of a one-standard-deviation shock are smaller, although still sizable—between about 3 and 5 percent.

In Figure 5, we examine whether these relative employment losses are broad-based or concentrated in certain industries. For simplicity and ease of presentation, we present estimates for Specification 2 only and suppress confidence intervals. We find that, across recessions, the negative impacts are pervasive across sectors, as nearly every point estimate is below zero. Construction and manufacturing experience the largest short-term impacts. Construction recovers somewhat in the earliest two recessions (but has done so little since), and manufacturing—in line with aggregate trends—has recovered partially from the Great Recession (but not so much in earlier recessions). Not surprisingly, declines in the government sector tend to be among the smallest, although that sector fared worse during the 1990–1991 and 2007–2009 recessions. The remaining industries tend to move similarly and fall in between, with no clear evidence, in any case, of an upward slope to suggest an eventual recovery.[19] These results show that recessions lead to slower relative growth in employment across many industries.

The consequences of these relative employment declines depend on the degree to which population also responds. We examine this next.

4.2 Population and Migration

In Figure 6 we present estimates of Equation (2), where the log of the total working-age population is the outcome. For brevity's sake, we show only the results from Specification 2, although the patterns are robust to Specifications 3 and 4. We see no evidence of pretrends and find negative, sustained impacts of the recessionary shock.[20] For each recession, log population continues to

[19]We exclude agriculture and mining, which are small (especially in metro areas) and highly spatially concentrated. We note the unusual positive pattern for utilities and transportation following the Great Recession. The confidence intervals for this series are wider than in previous recessions, so we are hesitant to read much into these results, but it is possible that recent growth in freight transportation stemming from e-commerce has mitigated employment losses in this sector.

[20]The lack of pretrends for the population results is not surprising, as we control for prerecession population growth.

decline long after the recession has ended, implying that harder-hit areas are on a long-lasting, lower-population-growth trajectory. The elasticities at recession trough are modest, between -0.2 and -0.3, but then they double or even nearly triple over the next decade. The most severe response comes from the 1990–1991 recession, which has a long-run elasticity of roughly -0.7, implying that a 10 percent greater employment shock leads to a relative population loss of 7 percent a decade later.

Panel B of Table 4 presents summaries of these results. In terms of relative magnitudes, a one-standard-deviation employment shock has the largest population impact for the 1980–1982 recession, with an effect of -4.3 percent. Consistent with the decline in migration documented previously (Molloy, Smith and Wozniak, 2014), including that specifically due to labor market shocks (Dao, Furceri and Loungani, 2017), we find that responsiveness of population to employment shocks has fallen over time: the long-term impacts per standard deviation of shock of the two most recent recessions lie between -1.5 and -2 percent, approximately half the magnitude of the earlier recessions.

We use the SOI data to investigate migration responses more directly for the two most recent recessions. Panels A and B of Figure 7 replicate the event study analysis of population for the 2001 and 2007–2009 recessions in Figure 6, using the total number of exemptions in the tax data to proxy for population. The patterns are quite similar and, if anything, the long-term elasticities are slightly greater in magnitude in the SOI data.

We decompose the change in net population into changes in in-migration, out-migration, and residual net births. This starts with the identity

$$\text{pop}_{i,t} = \text{pop}_{i,t-1} + \text{inmig}_{i,t} - \text{outmig}_{i,t} + \text{netbirths}_{i,t}, \tag{3}$$

where $\text{inmig}_{i,t}$ is the number of in-migrants between period $t-1$ and t, $\text{outmig}_{i,t}$ is the number of out-migrants, and $\text{netbirths}_{i,t}$ is the number of births minus deaths. Iterating Equation (3) forward and normalizing by a baseline population level, we have

$$\frac{\text{pop}_{i,t}}{\text{pop}_{i,0}} - 1 = \sum_{j=0}^{t-1} \frac{\text{inmig}_{i,j}}{\text{pop}_{i,0}} - \sum_{j=0}^{t-1} \frac{\text{outmig}_{i,j}}{\text{pop}_{i,0}} + \sum_{j=0}^{t-1} \frac{\text{netbirths}_{i,j}}{\text{pop}_{i,0}}. \tag{4}$$

We estimate versions of Equation (2), where the dependent variables are each term of the right-hand side of Equation (4). This provides an exact decomposition of the population change.[21]

Panels C and D present the results of this decomposition analysis. We normalize migration inflows and outflows, as well as residual net births, by the total number of exemptions in year $t_0 - 2$, so the estimates capture changes in rates. By recession trough, in-migration rates have fallen sharply, by about 10 percent in both recessions. Over the subsequent decade, these rates recover only slightly, and by the end of the horizon they remain between 6 and 8 percent below prerecession values. Out-migration shows little response until after the recession has ended, although there is a slight upward pretrend for the 2001 recession. Beginning in the year after the recession trough, however, out-migration rates steadily *decline*, with similar long-term magnitudes as for in-migration.

To understand how these components contribute to the change in population, as well as the role of net births, which also show a slight reduction (especially for the Great Recession), we divide the coefficient estimates in Panels C and D by the respective estimates in Panels A and B. When we also multiply the out-migration estimates by -1, the sum of the three transformed coefficients—in-migration, out-migration, and net births—sum to 1 and fully decompose the population effects found in the first two panels. These estimates are shown in Panels E and F. We find that in-migration accounts for more than 100 percent of the medium-run decrease in population. The short-term results differ between the recessions, reflecting the fact that in-migration declines for several years after the 2001 recession trough. The decline in out-migration is a counterbalance to population decline, especially for the Great Recession.

[21] The exact decomposition requires that we include the same covariates in all regressions. We construct net births as a residual using Equation (3).

4.3 Employment-to-Population

The population response is less than the employment response in each recession. This implies that employment-to-population ratios also fall in each recession. To examine this more directly, we use the log of the ratio of employment to working-age population (15+) as the outcome in Figure 8. These ratios remain lower than their prerecession peaks, even a decade after recession's end.[22]

The elasticities at trough vary somewhat. For the 1973–1975, 1980–1982, and 2001 recessions, these initial elasticities are about -0.75, but they are slightly larger—closer to -1—for the 1990–1991 and 2007–2009 recessions. As a consequence of the relatively flat employment trajectories and steady population decline, the employment-to-population trajectories generally show a slight recovery over time, although this is less true for the 1990–1991 and 2001 recessions. The long-term elasticity remains below -0.3 (and statistically different from 0) in each case, implying that a severe employment shock of 10 percent suppresses the employment-to-population ratio a decade later by at least 3 percent, or about 2 percentage points, given a national mean of about 60 percent.

Panel C of Table 4 reports summaries of these estimates seven to nine years post trough. Whereas a standard deviation employment shock leads to a long-term reduction in the employment-to-population ratio of about 3–4 percent (1.5–2.5 percentage points) for the four earlier recessions, the relative effect size is only half as large for the Great Recession. Nonetheless, in no case is the population response sufficient to fully counteract long-term employment losses.[23]

4.4 Earnings per capita

The damaging effects of local recessions need not manifest only through extensive-margin employment losses; they may also affect wages, hours worked, and other dimensions of job quality.

[22] The estimates for log employment, log population, and log employment-to-population are approximately, but not exactly, additive, because of slightly different controls (in particular, the different lagged dependent variables) included across each specification. We also note that our employment-to-population measure is the ratio of the count of jobs to the number of working-age people; because of multiple job-holding, it is not strictly comparable to official employment-to-population ratios, which represent the share of the population that is employed.

[23] These extensive-margin estimates do not preclude the possibility of impacts at the intensive margin. Census and ACS microdata reveal declines in both full-year and full-time, full-year employment rates, with somewhat imprecise but larger magnitudes for these outcomes than for overall employment rates.

We thus next examine the summary measure of annual earnings per capita (which encapsulates both the quantity and quality of employment).

Figure 9 shows estimates of Equation (2) for the log of real earnings per capita, for which we use the PCE deflator to adjust for inflation. There is again evidence for hysteresis, with per-capita earnings below their prerecession peak for each recession over the entire horizon, although the confidence interval just barely excludes zero for the 1973–1975 recession. Trough elasticities are typically between -0.5 and -0.75, though slightly larger for the 2007–2009 recession. Long-term elasticities show little improvement, with that for the 2001 recession doubling from its trough. As shown in Panel D of Table 4, a one-standard-deviation-greater shock results in earnings per capita between 2 percent (Great Recession) and 4 percent (2001 recession) lower than they otherwise would have been nearly a decade later.

We use the census/ACS to examine distributional impacts on the earnings of prime-age, employed workers. Specifically, we estimate a variation of Equation (2) in which dependent variables are drawn from the census (or 3-year ACS period) following the recession, rather than annually as in the event study.[24] We look at the mean and the 10th, 50th, and 90th percentiles of the log annual earnings distribution. The first row of Panel A of Table 5 shows that estimates for mean log earnings are generally similar to those from the BEAR data presented above, although magnitudes are somewhat smaller, especially for the 1990–1991 recession. The percentile estimates in the next three rows, moreover, indicate that recessions generally decrease earnings throughout the distribution. Longer-term earnings impacts tend to be less severe at the top of the distribution; for the middle three recessions, the brunt is borne at the bottom, although impacts at the middle are more severe for the 1973–1975 and 2007–2009 recessions. These results are consistent with the finding that job losses are more concentrated among lower parts of the earnings distribution

[24]We use the 1980 census for the 1973–1975 recession, the 1990 census for the 1980–1982 recession, the 2000 census for the 1990–1991 recession, the 2005–2007 ACS for the 2001 recession, and the 2015–2017 ACS for the 2007–2009 recession. Because the variables used are based on the previous calendar year (census) or preceding 12 months (ACS), these outcomes are generally measured before subsequent recessions begin. In these regressions, we control for lagged dependent variables in 1970 for the recession in 1973–1975, in 1970 and 1980 for the one in 1980–1982, in 1980 and 1990 for the one in 1990–1991, in 1990 and 2000 for the one in 2001, and in 2000 and 2005–2007 for the one in 2007–2009. These controls generally capture the prerecession period, again because outcomes are based on the previous calendar year or 12 months.

(Hoynes, Miller and Schaller, 2012), but we find that long-term impacts have had farther reach up the distribution more recently.

The long-term relative earnings decline could stem from either a reduction in hours or weeks worked, a reduction in earnings per hour or week, or both. Thus, in Appendix Table A.1 we show additional census/ACS estimates for (mean) log weekly and log hourly earnings (with those for log annual earnings repeated from Table 5 for convenience). If the earnings losses are driven by weeks or hours reductions, hourly wages could be relatively unaffected several years later. On the other hand, if the recession slows wage growth or displaced workers are less likely to find good employer matches (Lachowska, Mas and Woodbury, 2018), hourly wage losses may explain more of the per-capita earnings declines. The results indicate that the latter story better fits the data, as estimates for log hourly wages are at least two-thirds, and generally closer to four-fifths, of those for log annual wages. Recession-induced decreases in long-term work attachment at the intensive margin therefore explain relatively little of the persistent reduction of annual earnings per capita.

4.5 Robustness

Our results are robust to different measures of recession shocks or local labor markets. In particular, Appendix B.1 shows that our results are very similar when using private wage and salary employment from BEAR or QCEW to construct recession shocks. Appendix B.2 discusses results when replacing the recession shock with the log employment change predicted by an area's industry mix (Bartik, 1991). While there are several reasons to prefer the recessionary shock over the Bartik shock, the results are generally similar. Finally, Appendix B.3 shows that our results are nearly identical when examining commuting zones instead of metropolitan areas.

5 The Role of Composition Changes

Thus far, we have shown that recessions lead to persistent relative declines in local economic activity. One explanation for these persistent effects is a change in worker composition due to differential migration responses. We next examine these composition changes and explore whether they explain local hysteresis. While we find some evidence of composition changes, the qualitative and quantitative pattern of results suggests that composition changes are not the key mechanism.

5.1 Examining Composition Changes

First, we use Equation (2) to directly estimate the effects of recessions on the composition of individuals in a metro area. We focus on age, education, and occupation, as these directly relate to an area's earnings capacity. Figure 10 plots the effects of recessionary shocks on the share of population aged 0–14, 15–39, 40–64, and 65 and above.[25] Across all recessions, we see a persistent increase in the share aged 65 or above and a decrease in the share aged 15–39. This is consistent with the fact that early career workers are more mobile than older individuals (e.g., Molloy, Smith and Wozniak, 2011). The response of other age groups varies more: the 0–14 share declines following the 1973–1975, 1980–1982, and 2007–2009 recessions, but rises after the 1990–1991 recession and does not change after the 2001 recession. The 40–64 share generally increases, with the exception of 1990–1991. Most of these point estimates are statistically significant (filled-in markers indicate significance at the 0.05 level). These estimates imply that a one-standard-deviation increase in recession severity leads to a long-term 0.2–0.6 percentage-point (0.5–1.6 percent) decrease in the 15–39 share and a 0.1–0.6 percentage-point (0.8–5.0 percent) increase in the share aged 65 and above (Appendix Table A.4).

Table 6 reports estimates of recessionary shocks on occupational structure and educational composition, using decennial census and ACS data. Panel A examines the share of employed individuals aged 25–54 in three occupation groups: 1) managerial, professional, and technical; 2) administrative, office, production, and sales; and 3) manual and service. We follow Autor (2019) in using these classifications, which correspond to high-, medium-, and low-paid occupations. The 1973–1975, 1990–1991, and 2007–2009 recessions decreased the share of workers in managerial, professional, and technical jobs while they increased the share in manual and service occupations. There is less evidence of an impact on occupational structure following the 1980–1982 and 2001

[25] In the age, education, and occupation composition regressions, we control for all covariates in each regression. Including the same explanatory variables in all regressions ensures that the coefficients add up to zero across groups.

recessions. Panel B examines the share of individuals aged 25–54 with a high school degree or less; those with some college (but less than a four-year degree); and those with a four-year degree or more. The results mirror those in Panel A: the 1973–1975, 1990–1991, and 2007–2009 recessions increased the share of individuals with a high school degree or less and decreased the college share.[26] The coefficients for the Great Recession imply that a one-standard-deviation recession shock decreases the share of workers employed in managerial, professional, and technical occupations by 0.4 percentage points (1 percent). The same shock also increases the share of individuals with no more than a high school degree by 0.8 percentage points (2 percent) and decreases the share of individuals with a bachelor's degree by 0.6 percentage points (2 percent).

In sum, all recessions led to a modest shift in the population away from early career workers and toward the elderly. Some recessions decreased the share of workers employed in high-wage occupations, and the same recessions decreased the share of individuals with a college degree.[27] The changes in age, occupation, and education are modest in size, which suggests that these composition shifts likely cannot explain all of the persistent impacts on local labor markets. Furthermore, the fact that we find hysteresis even in recessions for which education and occupation are stable suggests that composition shifts along these dimensions are not driving the persistent effects of recessions.

5.2 The Role of Composition Changes in Aggregate Patterns

To more directly quantify the role of composition changes, we estimate the effects of recessions on residualized earnings. We regress log annual earnings of prime-age workers from the census and ACS against indicators for education (of which there are 11), age (30), sex (2), and race/ethnicity

[26] Many papers suggest that a recession-induced decrease in the opportunity cost of schooling should increase educational attainment for individuals of high school and college ages (e.g., Black, McKinnish and Sanders, 2005; Cascio and Narayan, 2015; Charles, Hurst and Notowidigdo, 2018). Our results do not contradict this possibility, but they show that any increase in schooling (which would take several years to appear, given our focus on 25-to-54-year-olds) is offset by shifting migration patterns. Nonetheless, because recessions reduce income, the negative income effect could offset the opportunity-cost effect. Stuart (2018) finds that the 1980–1982 recession reduced educational attainment for individuals who were even younger when the recession began, but this effect is unlikely to appear during our 10-year postrecession window.

[27] Understanding this heterogeneity across recessions is the subject of ongoing research.

(4), plus interactions between the education indicators and a quartic in age. We estimate these regressions separately for each year and use metro-area averages and percentiles of the residuals as dependent variables in our regressions.

Panel B of Table 5 presents results for composition-adjusted wage and salary earnings (Panel A, already discussed, shows nonadjusted results). The composition-adjusted results tend to be somewhat smaller in magnitude, which indicates that the age and education shifts identified above contribute to the persistent decline in earnings. At the same time, the composition-adjusted impacts remain sizable, and for the 2001 recession they actually increase in magnitude. Overall, composition changes along observed dimensions explain less than half of the overall effects.

6 A Comparison to Results Based on Vector Autoregressions

We have shown that every recession since 1973 has led to a persistent, relative decline in local economic activity, including employment-to-population ratios and earnings per capita. Our finding of widespread local hysteresis differs from the well-known results of Blanchard and Katz (1992)—hereafter BK. BK estimate vector autoregressions (VARs) on state-level data and find that the unemployment rate, labor force participation rate, and wages return to trend within 10 years after negative labor demand shocks. Dao, Furceri and Loungani (2017) use a different source of identification and find a similar degree of convergence, although population is less responsive in their short-run results. Moreover, Yagan (2019) shows that the BK model implies complete recovery of the employment-to-population ratio within eight years following the 1980–1982 and 1990–1991 recessions, and slower but steady convergence following the 2007–2009 recession. This section explores why our results differ. We show that finite sample bias, stemming from the relatively short time series that researchers must rely on, leads to artificial recovery of VAR impulse response functions.

To facilitate discussion, we first introduce the BK VAR. The key variables are the annual change in log employment, $\Delta e_{i,t}$, the level of the log employment–labor force ratio, $el_{i,t}$, and the level of the log labor force–working-age population ratio, $lp_{i,t}$. BK account for aggregate trends by differencing out the same variables for the aggregate U.S. economy. They estimate the following VAR using data from 1976–1990:

$$\Delta e_{i,t} = \alpha_{i10} + \alpha_{11}(L)\Delta e_{i,t-1} + \alpha_{12}(L)el_{i,t-1} + \alpha_{13}(L)lp_{i,t-1} + \epsilon_{i,e,t} \tag{5}$$

$$el_{i,t} = \alpha_{i20} + \alpha_{21}(L)\Delta e_{i,t} + \alpha_{22}(L)el_{i,t-1} + \alpha_{23}(L)lp_{i,t-1} + \epsilon_{i,el,t} \tag{6}$$

$$lp_{i,t} = \alpha_{i30} + \alpha_{31}(L)\Delta e_{i,t} + \alpha_{32}(L)el_{i,t-1} + \alpha_{33}(L)lp_{i,t-1} + \epsilon_{i,lp,t}. \tag{7}$$

BK include two lags of each variable, along with state fixed effects $\alpha_{i10}, \alpha_{i20}$, and α_{i30}. After estimating these equations (which can be done using three separate OLS regressions), BK construct the impulse response functions (IRFs) of each variable with respect to a one percent shock to employment (i.e., a reduction in $\epsilon_{i,e,t}$ of 0.01). Primary interest lies in these IRFs.

Figure 11 shows IRFs of employment, the "unemployment rate" (one minus the log employment–labor force ratio), and the participation rate. We use BLS LAUS data from 1976–1990 to generate these results, which are extremely similar to Figure 7 of BK. Notably, the unemployment rate and participation rate completely recover within eight years.

Our preferred unit of geography is a metropolitan area or commuting zone. When using sub-state areas, reliable data on labor force participation are available for a limited time period at best.[28] Consequently, the most comparable outcome is the employment-to-population ratio. The IRF of the log employment-to-population ratio can be constructed as the sum of the IRF of the log employment–labor force ratio (Equation (6)) and the IRF of the log labor force–population ratio (Equation (7)). Panel B of Figure 11 shows this IRF from the BK model. As expected, the IRF shows complete recovery.

To facilitate the analysis below, we simplify the BK model in two ways. First, we estimate a two-equation VAR in first differences of log employment and levels of the log employment-to-population ratio, $ep_{i,t}$. Second, we include only one lag of each variable. The resulting VAR is as

[28]The BLS provides county-level labor force estimates from 1990 onward. A separate series contains county-level labor force estimates from 1976–1989, but BLS stresses that this series is not comparable to the 1990-forward series. Both data sets rely substantially on extrapolations from statistical models, as household surveys are not large enough to reliably measure unemployment and labor force for most counties.

follows:

$$\Delta e_{i,t} = \tilde{\alpha}_{i10} + \tilde{\alpha}_{11}\Delta e_{i,t-1} + \tilde{\alpha}_{12} ep_{i,t-1} + \tilde{\epsilon}_{i,e,t} \tag{8}$$

$$ep_{i,t} = \tilde{\alpha}_{i20} + \tilde{\alpha}_{21}\Delta e_{i,t} + \tilde{\alpha}_{22} ep_{i,t-1} + \tilde{\epsilon}_{i,ep,t} \tag{9}$$

These simplifying assumptions have little impact on the resulting estimates. For example, Panel B of Figure 11 shows that the IRF of the log employment-to-population ratio is nearly identical after making these changes.

Equations (8) and (9) facilitate simpler expressions of the IRF in terms of the underlying parameters. Consider a one-time decrease in labor demand in period t, $\tilde{\epsilon}_{i,e,t}$. The subsequent impacts on the log employment-to-population ratio are:

$$\frac{dep_{i,t}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21} \tag{10}$$

$$\frac{dep_{i,t+1}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21}^2\tilde{\alpha}_{12} + \tilde{\alpha}_{21}\tilde{\alpha}_{11} + \tilde{\alpha}_{21}\tilde{\alpha}_{22} \tag{11}$$

$$\frac{dep_{i,t+2}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21}^3\tilde{\alpha}_{12}^2 + 2\tilde{\alpha}_{21}^2\tilde{\alpha}_{11}\tilde{\alpha}_{12} + 2\tilde{\alpha}_{21}^2\tilde{\alpha}_{22}\tilde{\alpha}_{12} + \tilde{\alpha}_{21}\tilde{\alpha}_{11}^2 + \tilde{\alpha}_{21}\tilde{\alpha}_{22}^2 + \tilde{\alpha}_{21}\tilde{\alpha}_{11}\tilde{\alpha}_{22} \tag{12}$$

Similar expressions exist for the IRF at later horizons, but these first few periods are adequate to highlight some important facts. First, bias in the OLS estimates of Equations (8) and (9) generally leads to bias in the IRF. Second, bias in the IRF after the period of the shock is a nonlinear function of bias in the underlying parameters. Third, bias in the IRF can increase in importance over time. For example, if the OLS estimates are attenuated, this bias generates an IRF that converges toward zero even if the true IRF does not. This arises because the exponents in the IRF increase with time, magnifying attenuation bias.[29]

The potential for bias in estimating autoregressive models, including VARs, has long been recognized. In particular, there can be severe attenuation bias in finite samples, and this attenuation bias becomes more severe as processes approach random walks (e.g., Hurwicz, 1950; Shaman and

[29] More generally, if $a \in (0, 1)$ is an attenuation factor, then $(ax)^t$ converges to zero faster than x^t.

Stine, 1988; Stine and Shaman, 1989; Pope, 1990; Kilian and Lütkepohl, 2017).[30] This bias arises because residuals are not independent of all regressors in an autoregression, since regressors are lagged dependent variables.

To explore this issue further, we conduct a Monte Carlo study of finite sample bias in empirically relevant scenarios. We assume that log employment is a random walk:

$$e_{i,t} = e_{i,t-1} + \varepsilon_{i,e,t}, \tag{13}$$

and that log population depends on changes in log employment as follows:

$$p_{i,t} = p_{i,t-1} + (1-\beta)\Delta e_{i,t} + \varepsilon_{i,p,t}. \tag{14}$$

This implies that the log employment-to-population ratio is

$$ep_{i,t} = ep_{i,t-1} + \beta\Delta e_{i,t} - \varepsilon_{i,p,t}. \tag{15}$$

In terms of Equations (8) and (9), this data-generating process (DGP) sets $\tilde{\alpha}_{i10} = \tilde{\alpha}_{i20} = 0$ (state fixed effects do not matter), $\tilde{\alpha}_{11} = \tilde{\alpha}_{12} = 0$ (log employment is a random walk), $\tilde{\alpha}_{21} = \beta$, and $\tilde{\alpha}_{22} = 1$. Changes in log employment have a permanent effect on the log employment-to-population ratio, with the true IRF equal to β. We study DGPs with this feature to examine whether VARs are capable of identifying persistent effects in finite samples.

We calibrate the DGP using state-level LAUS data. We assume that all variables are distributed normally. The first period mean and variance of $e_{i,t}$ and $p_{i,t}$ equal those observed in the 1976 LAUS data, and the variance of shocks approximates the cross-sectional variance of log employment and population in subsequent years.[31] We focus on the case where $\beta = 0.75$, with 50 cross-sectional

[30] There are various approaches to addressing bias and inaccurate coverage rates (e.g., Andrews, 1993; Kilian, 1998, 1999), not all of which are applicable to the Blanchard and Katz (1992) VAR. In general, "there is no consensus in the literature that impulse responses should be estimated based on bias-adjusted slope parameters rather than the original [least squares] estimates" (Kilian and Lütkepohl, 2017, p. 37).

[31] In particular, we set $e_{i,0} \sim \mathcal{N}(13.94, 1.00^2)$, $p_{i,0} \sim \mathcal{N}(14.49, 1.02^2)$, $\varepsilon_{i,e,t} \sim \mathcal{N}(0, 0.015^2)$, and $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$.

observations and different time-series lengths, T.

Panel A of Figure 12 plots the true IRF along with average estimates across 499 Monte Carlo simulations. For $T = 15$, which is approximately the number of years available to BK when they wrote their paper, finite sample bias leads to rapid recovery of the employment-to-population ratio. Ten years after the shock, the IRF estimate is downward-biased by 89 percent. This bias remains very large for $T = 25$, $T = 50$, and even $T = 100$, where the bias 10 years after the shock equals 25 percent. Even for $T = 500$, finite sample bias incorrectly implies slow but steady recovery.[32] Because this bias stems from an insufficient number of time series observations, instrumental variables do not solve this problem in general. Not surprisingly, we find a sufficiently strong instrumental variable generates near-identical results in our DGP (in which an instrument is not necessary).

In sum, finite sample bias can lead VARs to find evidence of recovery when there is none. We believe that this is the main explanation for why we find widespread evidence of local hysteresis, while the literature estimating VARs does not.[33] Our approach does not suffer from this same bias because we estimate a separate event study coefficient for each year and hold fixed the lagged dependent variable two years before the recession peak. Indeed, when we adopt the same Monte Carlo approach for estimating the event study, we find no systematic bias, regardless of T, as shown in Panel B of Figure 12.

7 Conclusion

This paper examines the short- and medium-term effects of recessions on local areas. We find consistent and robust evidence that, for each of the past five national recessions, local areas that suffered larger employment losses experience permanent reductions in employment, suggesting a permanent decline in labor demand. Population falls during recessions and for several years

[32] Appendix Table A.5 reports the underlying bias in estimates of the parameters of Equations (8) and (9) for various values of T. All parameters are biased. While this bias is modest in many cases, it is amplified in the IRF.

[33] The literature estimating VARs uses state-level data. Estimating our event study models on state-level data also yields widespread hysteresis, so this does not explain the difference.

afterwards, primarily because of lower in-migration, and in spite of reduced out-migration. However, population responds by less than employment, which leads to persistent relative declines in employment-to-population ratios and earnings per capita for at least a decade after the recession's end. Earnings decline throughout the distribution, but effects tend to be more severe at the middle and bottom. Recessions also lead to an increase in the share of the population over age 64 and a decrease in the share aged 15–39. In three out of the five recessions, we observe a decrease in the share of workers employed in managerial, professional, or technical occupations, and a decrease in the share of residents with a bachelor's degree. Composition shifts explain less than half of the persistent effects we uncover.

In short, recessions produce enduring economic disruptions to local labor markets, and this pattern has existed for at least the past five decades. While there are some differences across recessions, more striking is the similarity of the responses, especially in light of different macroeconomic drivers and secular changes in the economy over time. One explanation for why these results have not been shown before is that an influential approach in the literature—estimating vector autoregressions and calculating impulse response functions—incorrectly finds convergence because of finite sample bias. This finite sample bias is likely of first-order importance for all settings in which researchers study hysteresis. In contrast, the event study models that we estimate do not suffer from this bias.

Our findings have important implications for labor market dynamism, the economic opportunities of workers and their children, and optimal policy responses. They show that recessions lead to a sizable reallocation of economic activity across space. At the same time, we find that recessions reduce both in-migration and out-migration, which indicates limited ability or willingness of households to move across areas to equilibrate these shocks. Moreover, the persistent decrease in local economic activity limits the opportunities available to both adults and children. For workers, most of the decrease in earnings is due to a decrease in hourly wages, which indicates that offsetting these long-run effects might require investments in worker human capital, labor demand, or both. For children, the long-run reduction in income and increase in poverty likely reduce their economic mobility (Stuart, 2018).

Our results inform optimal policy responses in other ways. Approximately $5 billion per year is spent on employment services and job training by the Department of Labor's Employment and Training Administration alone, through the Workforce Investment Opportunity Act. These and other government funds are often allocated based on current (or very recent) economic conditions. This study shows that recessions have long-lasting impacts, which suggests that there may be scope for improvements in targeting aid based on a longer economic history (see also Yagan, 2019). Additionally, as we find declining population mobility in response to recessions, to the extent that selective migration is an important factor, policies to encourage greater mobility may be worth considering (Moretti, 2012), although recent evidence has called into question whether relocation to areas with greater job growth would benefit all workers (Autor, 2019). If firm behavior plays an important role, especially to the extent that recessions accelerate the adoption of routine-labor–saving technology and the demand for abstract skills (Hershbein and Kahn, 2018), then policies that aim to counteract skill depreciation may be applicable (Fitzpayne and Pollack, 2018; Warner, 2018).

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Table 1: Aggregate Employment Changes, by Recession

	Share of peak year emp. (1)	Log emp. change (2)	Emp. change (3)	Share of peak year emp. (1)	Log emp. change (2)	Emp. change (3)	Share of peak year emp. (1)	Log emp. change (2)	Emp. change (3)
	1973–1975 Recession			1980–1982 Recession			1990–1991 Recession		
Total	1.000	0.004	421,100	1.000	0.010	1,123,200	1.000	0.011	1,531,000
Manufacturing	0.216	−0.090	−1,758,600	0.196	−0.110	−2,230,100	0.150	−0.049	−962,800
Services	0.203	0.053	1,041,400	0.220	0.103	2,606,900	0.276	0.060	2,264,500
Government	0.177	0.046	792,000	0.168	0.008	149,000	0.156	0.023	493,000
Retail Trade	0.159	0.010	153,300	0.161	0.020	359,600	0.168	0.005	110,800
Finance, Insurance, Real estate	0.076	0.027	192,700	0.079	0.037	322,200	0.080	−0.014	−146,000
Transportation and Public Utilities	0.054	−0.018	−91,400	0.052	0.003	17,400	0.048	0.034	220,600
Construction	0.054	−0.084	−410,000	0.054	−0.096	−536,900	0.054	−0.065	−451,500
Wholesale Trade	0.048	0.073	341,800	0.052	0.008	44,900	0.050	−0.012	−76,200
Mining	0.008	0.140	114,100	0.011	0.264	350,800	0.008	−0.025	−26,000
Agriculture, Forestry, Fisheries	0.006	0.073	45,800	0.008	0.043	39,400	0.010	0.077	104,600
	2001 Recession			2007–2009 Recession					
Total	1.000	−0.000	−62,700	1.000	−0.034	−5,866,000			
Manufacturing	0.109	−0.120	−2,004,900	0.082	−0.147	−1,982,600			
Services	0.409	0.022	1,504,500	0.432	−0.012	−886,900			
Government	0.141	0.027	638,000	0.137	0.018	452,000			
Retail Trade	0.114	−0.015	−268,300	0.107	−0.064	−1,171,600			
Finance, Insurance, Real estate	0.082	0.019	260,100	0.094	0.025	426,900			
Construction	0.059	0.013	128,500	0.064	−0.190	−1,975,100			
Transportation and Public Utilities	0.038	−0.022	−133,000	0.037	−0.061	−385,500			
Wholesale Trade	0.039	−0.027	−169,900	0.037	−0.070	−443,300			
Mining	0.005	−0.012	−9,000	0.006	0.107	114,300			
Agriculture, Forestry, Fisheries	0.005	−0.010	−8,700	0.005	−0.017	−14,200			

Notes: Table reports nationwide wage and salary employment changes during recessions. Employment changes are from 1973–1975, 1979–1982, 1989–1991, 2000–2002, and 2007–2009. The 1973–1991 data are based on SIC industries, and the 2000–2009 data are based on NAICS industries. Industry changes may not sum to total changes because of rounding.
Sources: Authors' calculations using BEAR data.

Table 2: Correlation of Metropolitan-Area Recessionary Shocks

	Change in Log Employment during Recession Years				
	1973–75	1979–82	1989–91	2000–02	2007–09
Panel A: Unadjusted					
1973–75	1.000				
1980–82	0.386	1.000			
1989–91	0.462	0.156	1.000		
2000–02	0.442	0.412	0.280	1.000	
2007–09	0.346	0.206	−0.008	0.154	1.000
Panel B: Adjusted for Census Division					
1973–75	1.000				
1980–82	0.326	1.000			
1989–91	0.291	0.174	1.000		
2000–02	0.290	0.308	0.236	1.000	
2007–09	0.354	0.064	−0.054	0.089	1.000
Panel C: Adjusted for Census Division and Prerecession Population Growth					
1973–75	1.000				
1980–82	0.260	1.000			
1989–91	0.171	0.021	1.000		
2000–02	0.140	0.082	0.101	1.000	
2007–09	0.391	0.278	0.035	0.210	1.000

Notes: Table reports correlations of log wage and salary employment changes across recessions for 363 metropolitan areas. Panel B reports correlations after partialling out census division fixed effects, and Panel C partials out census division fixed effects and prerecession population growth.
Source: Authors' calculations using BEAR data.

Table 3: Characteristics of Metro Areas with More versus Less Severe Recessions

	Recession									
	1973–75		1980–82		1990–91		2001		2007–09	
	Less Severe	More Severe	Less Severe	More Severe	Less Severe	More Severe	Less Severe	More Severe	Less Severe	More Severe
Population (000s)	328.6	589.4	545.1	426.3	325.9	760.2	524.3	725.3	609.0	738.7
Log pop. growth	0.090	0.067	0.247	0.108	0.136	0.079	0.162	0.096	0.091	0.117
Employment rate	0.517	0.537	0.532	0.547	0.545	0.579	0.590	0.632	0.611	0.583
Manufacturing share	0.141	0.253	0.140	0.236	0.132	0.178	0.095	0.163	0.081	0.110
Real earnings per capita (000s)	25.4	25.2	27.4	27.3	30.8	32.4	37.9	39.4	41.4	42.2
HS degree+ share	0.559	0.505	0.676	0.655	0.763	0.746	0.808	0.814	0.855	0.847
BA+ share	0.119	0.096	0.172	0.141	0.194	0.182	0.229	0.219	0.259	0.240
Nonwhite share	0.146	0.134	0.210	0.121	0.189	0.188	0.257	0.203	0.275	0.275
Foreign-born share	0.028	0.027	0.048	0.028	0.045	0.043	0.081	0.047	0.068	0.080

Notes: Population, employment rate, manufacturing share of employment, and real earnings per capita are measured two years before the recession start year. The last four rows are measured as of the closest decennial census to the recession start year, except for the 2007–2009 recession, which is measured from the 2005–2009 ACS. Population growth is from 1969 to 1973 for the 1973-1975 recession and over the previous 10 years for the other recessions.

Sources: Authors' calculations of data from Bureau of Economic Analysis, decennial censuses, and American Community Surveys (via IPUMS and NHGIS), and Surveillance, Epidemiology, and End Results (SEER).

Table 4: Summary of Impacts of Metropolitan-Area Recessionary Shocks on Local Economic Activity

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Dependent Variable: Log Employment					
Coefficient, 7–9 years after trough	−1.294	−0.873	−1.703	−1.543	−0.797
	(0.183)	(0.138)	(0.161)	(0.131)	(0.112)
Implied effect of 1 SD recessionary shock	−0.072	−0.069	−0.077	−0.052	−0.031
Panel B: Dependent Variable: Log Population Age 15+					
Coefficient, 7–9 years after trough	−0.642	−0.562	−0.692	−0.548	−0.377
	(0.114)	(0.079)	(0.136)	(0.099)	(0.060)
Implied effect of 1 SD recessionary shock	−0.036	−0.044	−0.031	−0.019	−0.015
Panel C: Dependent Variable: Log Employment-to-Population Ratio					
Coefficient, 7–9 years after trough	−0.608	−0.351	−0.902	−0.992	−0.430
	(0.101)	(0.102)	(0.120)	(0.133)	(0.090)
Implied effect of 1 SD recessionary shock	−0.034	−0.028	−0.041	−0.034	−0.017
Panel D: Dependent Variable: Log Earnings per Capita					
Coefficient, 7–9 years after trough	−0.436	−0.388	−0.679	−1.182	−0.513
	(0.078)	(0.092)	(0.115)	(0.183)	(0.137)
Implied effect of 1 SD recessionary shock	−0.024	−0.031	−0.031	−0.040	−0.020
SD of recessionary shock	0.056	0.079	0.045	0.034	0.039

Notes: Table reports estimates of Equation (2), separately for each recession. We impose the constraint that prerecession coefficients equal zero and group postrecession coefficients across years 1–3, 4–6, and 7–9. Dependent variables are indicated in the panel titles, and the key independent variable is the log wage and salary employment change from BEAR data. All regressions control for division-year fixed effects and interactions between prerecession population growth and year. There are 363 metropolitan areas in the sample. Standard errors are clustered by metropolitan area. Sources: Authors' calculations using BEAR and SEER data.

Table 5: Impacts of Metropolitan-Area Recessionary Shocks on Annual Wage Earnings, Census/ACS

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Without Composition Adjustment					
Average log earnings	−0.203	−0.503	−0.126	−0.547	−0.549
	(0.095)	(0.092)	(0.099)	(0.104)	(0.127)
10th percentile, log earnings	−0.023	−0.694	−0.177	−0.760	−0.339
	(0.168)	(0.161)	(0.167)	(0.247)	(0.230)
50th percentile, log earnings	−0.211	−0.474	0.008	−0.375	−0.677
	(0.105)	(0.091)	(0.082)	(0.098)	(0.127)
90th percentile, log earnings	−0.103	−0.291	−0.056	−0.371	−0.441
	(0.085)	(0.065)	(0.089)	(0.093)	(0.145)
Panel B: With Composition Adjustment					
Average log earnings	−0.155	−0.331	−0.060	−0.627	−0.359
	(0.086)	(0.076)	(0.080)	(0.090)	(0.112)
10th percentile, log earnings	−0.022	−0.443	−0.072	−1.082	−0.267
	(0.160)	(0.153)	(0.128)	(0.243)	(0.219)
50th percentile, log earnings	−0.189	−0.312	−0.028	−0.490	−0.358
	(0.077)	(0.071)	(0.074)	(0.070)	(0.093)
90th percentile, log earnings	−0.124	−0.215	−0.056	−0.437	−0.294
	(0.083)	(0.048)	(0.059)	(0.081)	(0.125)

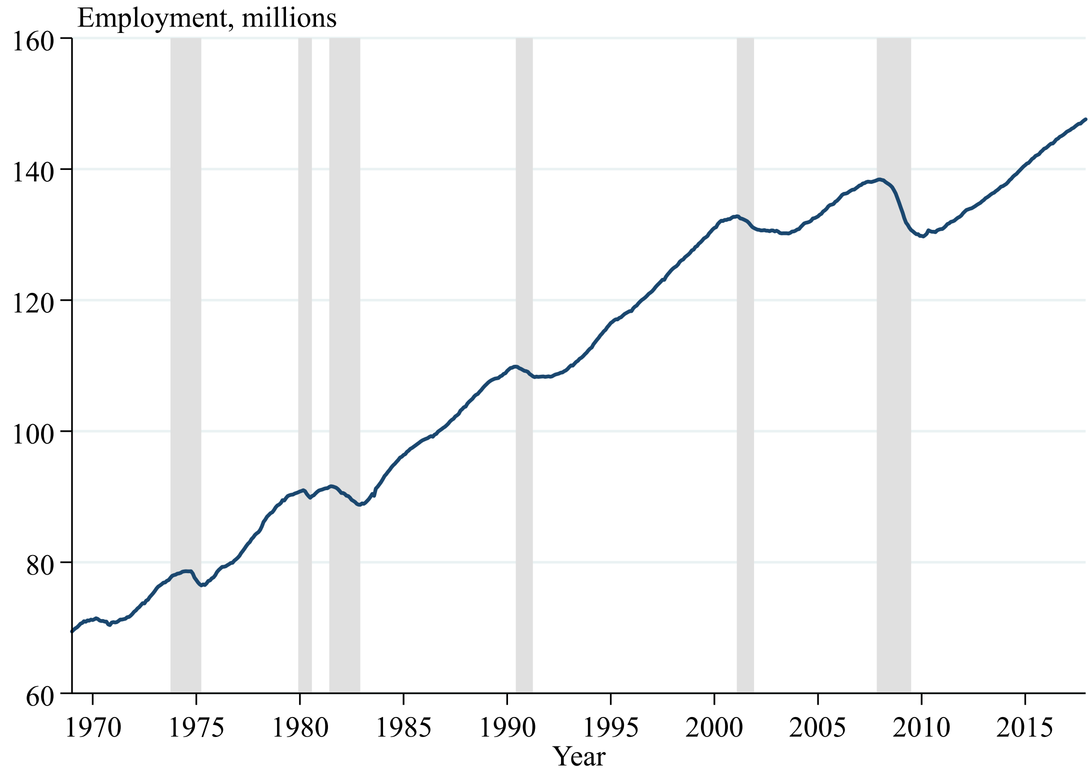
Notes: Table reports estimates of separate regressions for each recession. The dependent variable is taken from the postrecession census year (1980, 1990, 2000, 2005–2007, and 2015–17, respectively). The 1973–75 regression controls for the 1970 value of the dependent variable, and other regressions control for two lagged/contemporaneous values. Sample limited to individuals aged 25–54. All regressions control for division-year fixed effects and interactions between prerecession population growth and year. The dependent variables in Panel B are constructed using residuals from regressing log earnings on indicators for education, indicators for age, an indicator for sex, an indicator for race/ethnicity (white/black/Hispanic/other), plus interactions between the education indicators and a quartic in age. Standard errors are robust to heteroskedasticity.
Sources: Authors' calculations using BEAR, decennial census, and ACS data.

Table 6: Impacts of Metropolitan-Area Recessionary Shocks on Occupational Structure and Education Composition

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Share of Employed Workers by Occupation Group					
Managerial, professional, technical	−0.106	−0.025	−0.059	0.002	−0.093
	(0.033)	(0.028)	(0.031)	(0.036)	(0.040)
Administrative, office, production, sales	−0.054	−0.001	−0.048	−0.010	0.014
	(0.028)	(0.021)	(0.027)	(0.032)	(0.034)
Manual and service	0.160	0.026	0.107	0.008	0.079
	(0.038)	(0.033)	(0.033)	(0.038)	(0.045)
Panel B: Share of Individuals by Educational Attainment					
HS degree or less	0.130	0.001	0.108	0.037	0.209
	(0.052)	(0.040)	(0.042)	(0.038)	(0.044)
Some college	−0.027	0.027	−0.059	0.001	−0.064
	(0.027)	(0.024)	(0.029)	(0.032)	(0.033)
Four-year degree or more	−0.103	−0.028	−0.049	−0.038	−0.145
	(0.038)	(0.026)	(0.031)	(0.030)	(0.040)

Notes: Table reports estimates of separate regressions for each recession. We control for all occupation or education shares (which are mutually exclusive). Sample limited to individuals aged 25–54. See notes to Table 5.
Sources: Authors' calculations using BEAR, decennial census, and ACS data.

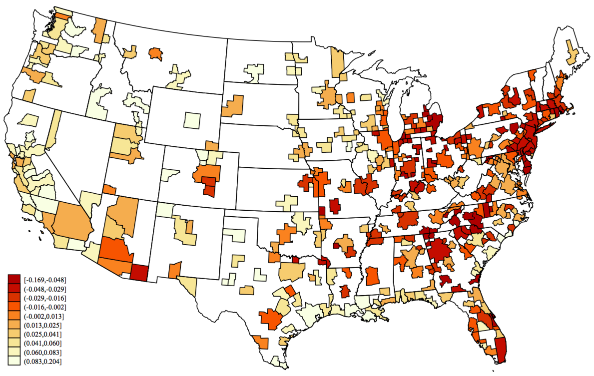
Figure 1: Aggregate Employment and Recessions, 1969–2016



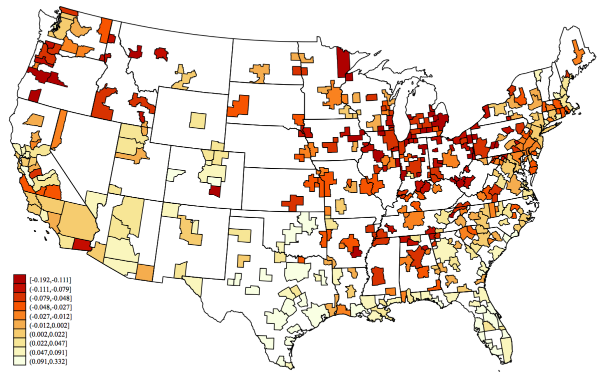
Notes: Figure shows total, seasonally adjusted national nonfarm employment. The shading indicates NBER national recession dates.
Source: Current Employment Statistics, Bureau of Labor Statistics.

Figure 2: Metropolitan-Area Recession Shocks

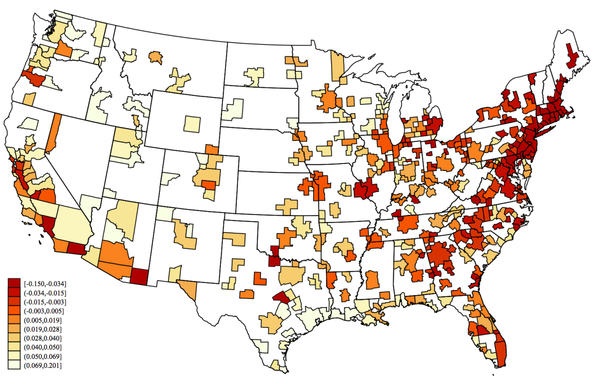
(a) 1973–1975 Recession



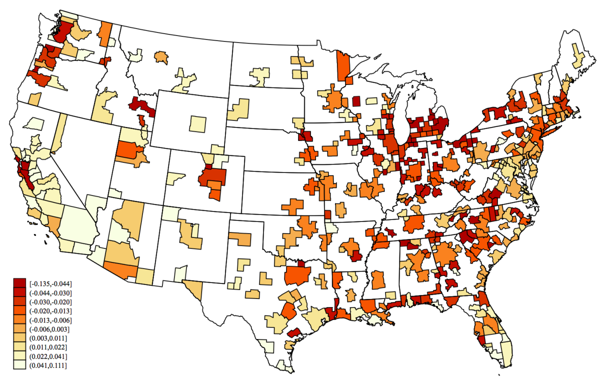
(b) 1980–1982 Recession



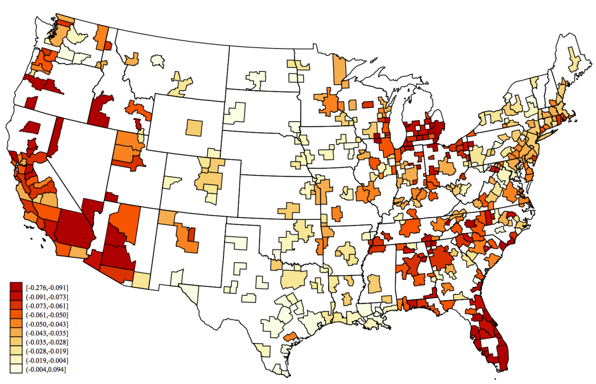
(c) 1990–1991 Recession



(d) 2001 Recession

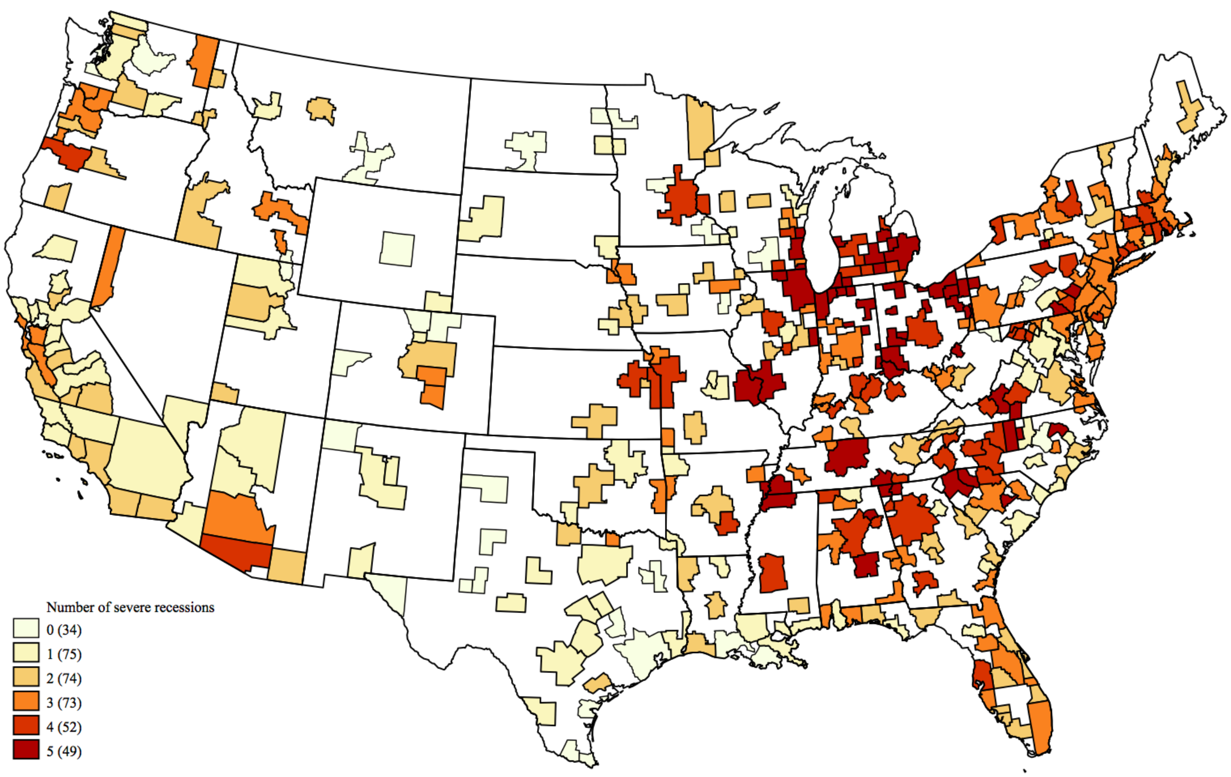


(e) 2007–2009 Recession



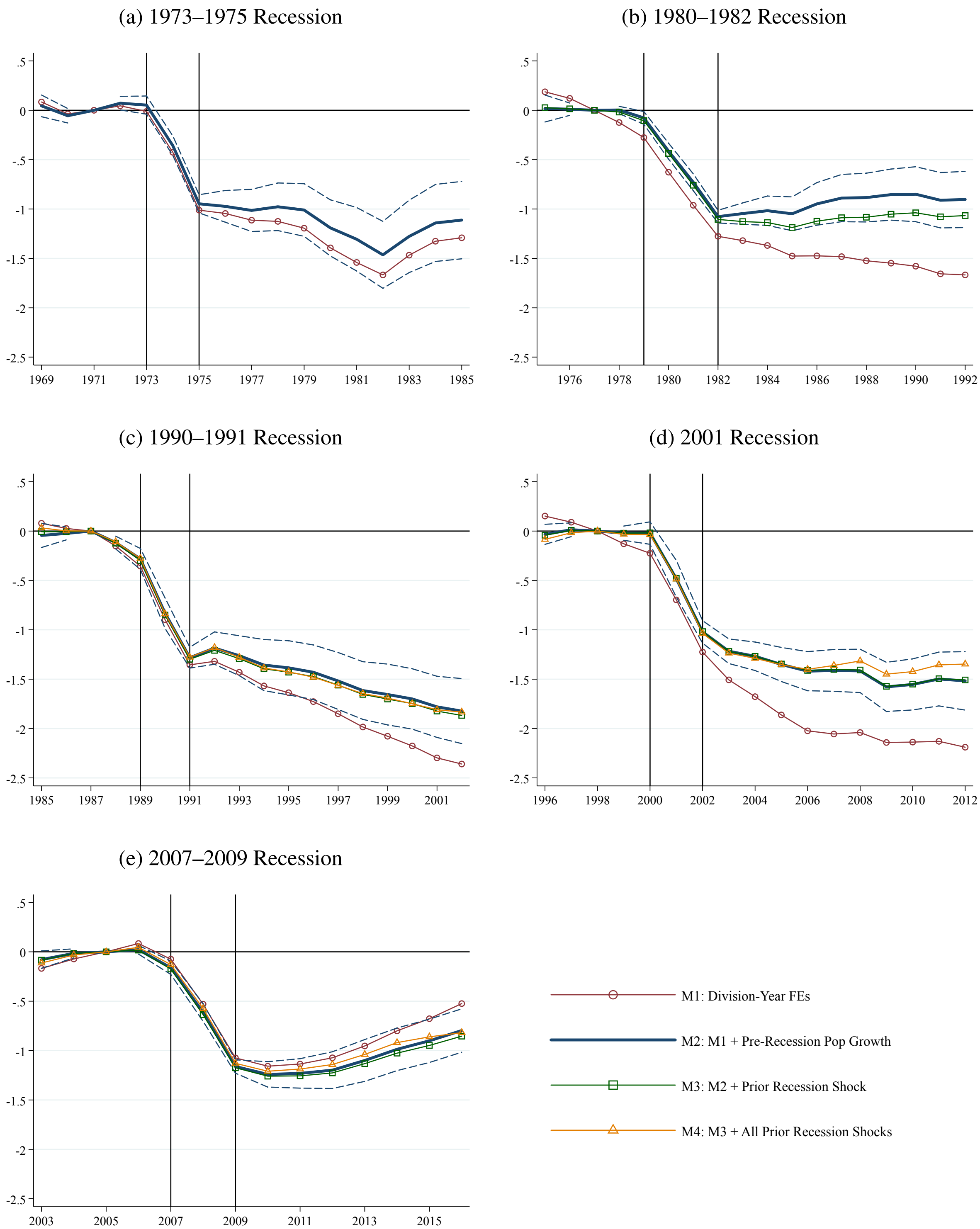
Notes: Each map shows the change in log employment from national peak to trough for 363 CBSAs (OMB vintage 2003 definitions) as described in the text. Areas in darker colors experienced larger employment losses.
Source: Authors' calculations from BEAR.

Figure 3: Frequency of Severe Recessions, by Metropolitan Area, 1973–2009



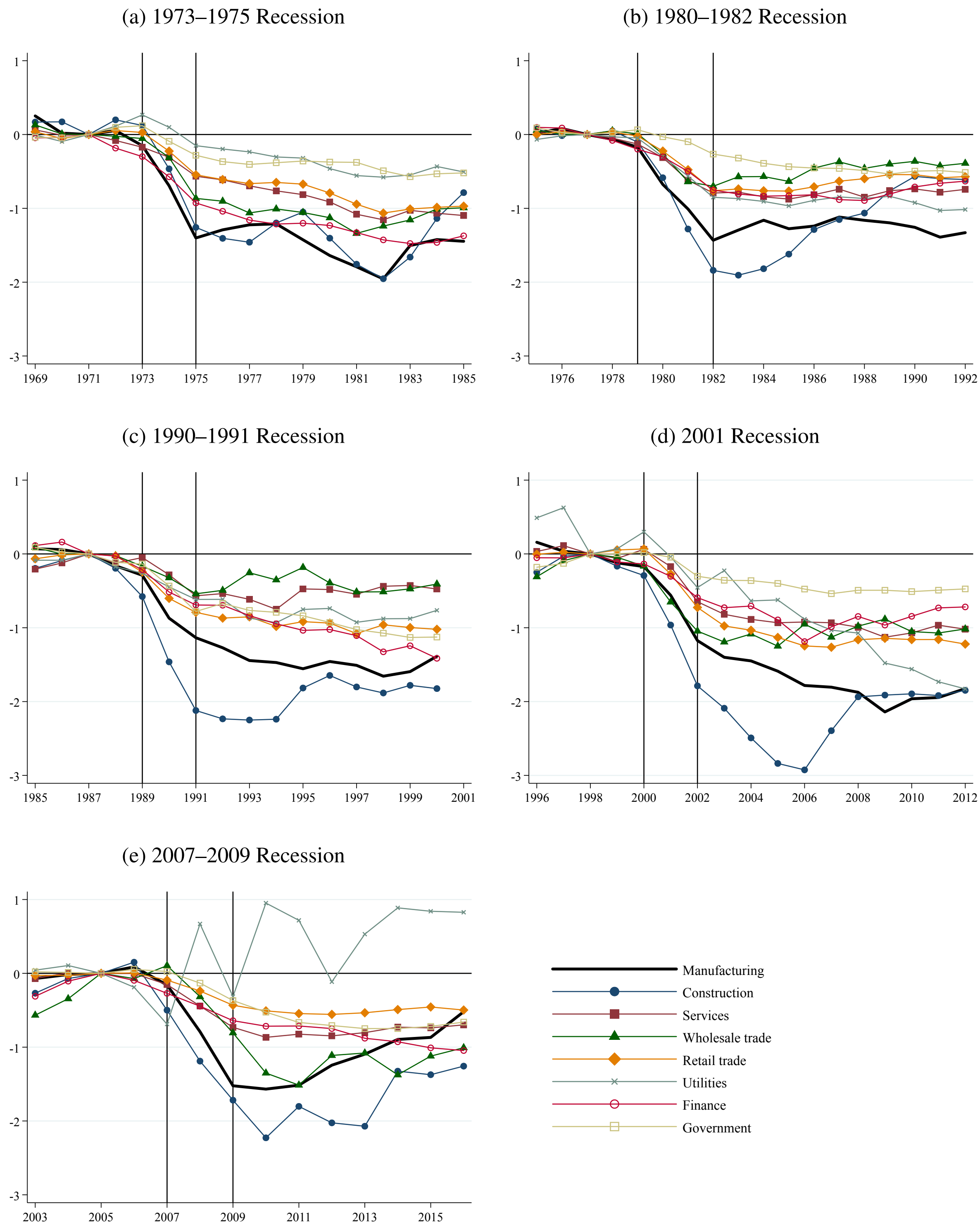
Notes: We denote an area as suffering a severe recession if its log employment change for a given recession is less than the median across CBSAs for that recession.
Source: Authors' calculations from BEAR.

Figure 4: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment



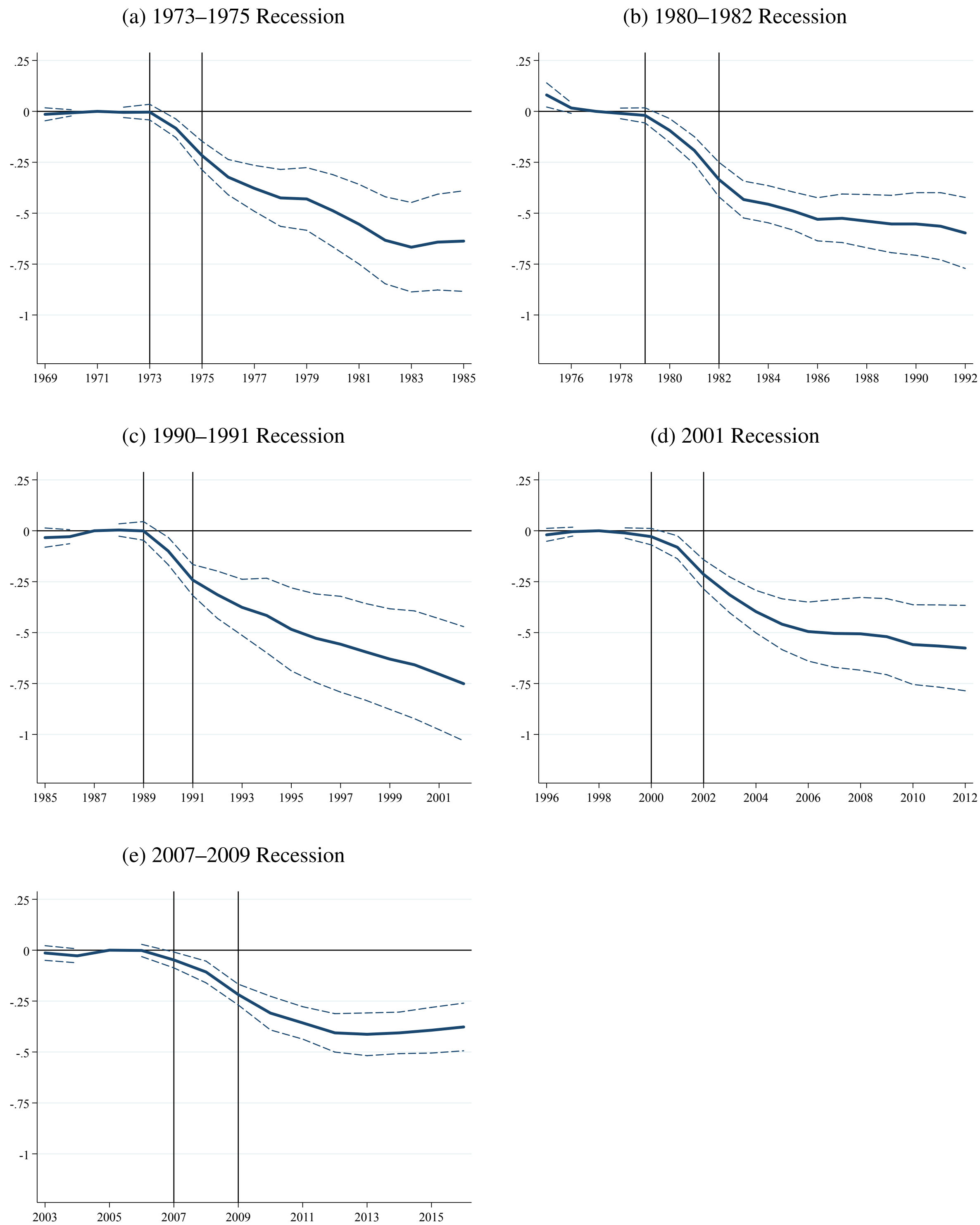
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the log wage and salary employment change from BEAR data. Specifications are indicated by the legend. There are 363 metropolitan areas in the sample. Standard errors are clustered by metropolitan area.
Sources: Authors' calculations using BEAR and SEER data.

Figure 5: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment, by Sector



Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log employment from the indicated sector. We use BEAR data for the 1973–75, 1980–82, 1990–91, and 2007–09 recessions. We use QCEW data for the 2001 recession (because of SIC-NAICS industry seaming issues), except for government, which comes from BEAR. See notes to Figure 4.
Sources: Authors' calculations using BEAR, SEER, and QCEW data.

Figure 6: Impacts of Metropolitan-Area Recessionary Shocks on Log Population Ages 15+

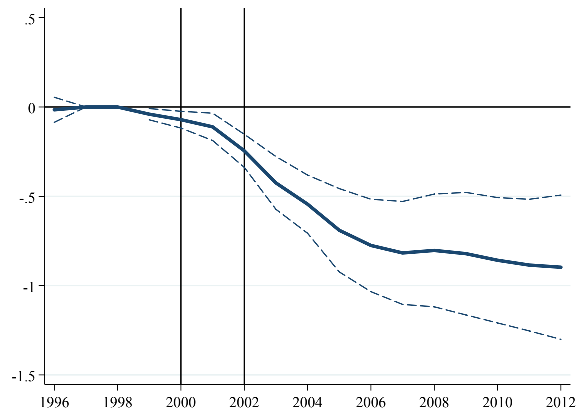


Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure 4.

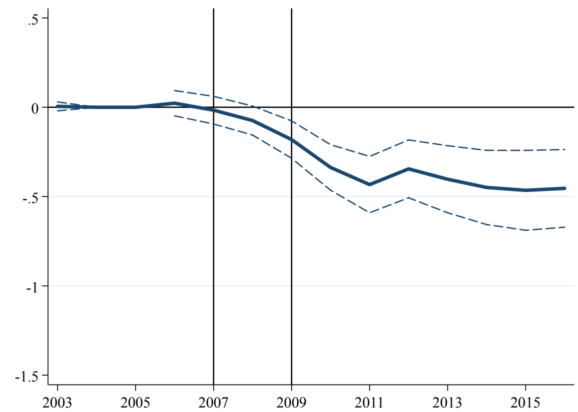
Sources: Authors' calculations using BEAR and SEER data.

Figure 7: Impacts of Metropolitan-Area Recessionary Shocks on In-Migration and Out-Migration

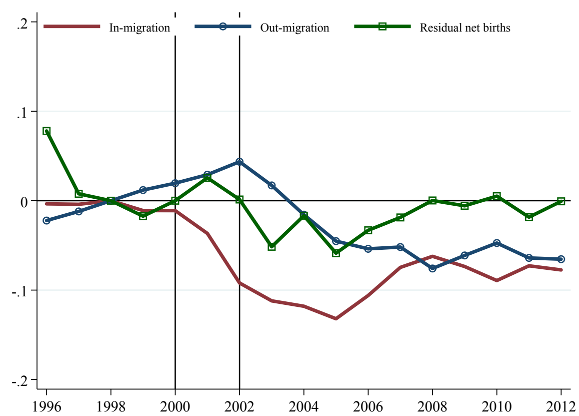
(a) 2001, DV: Log Population



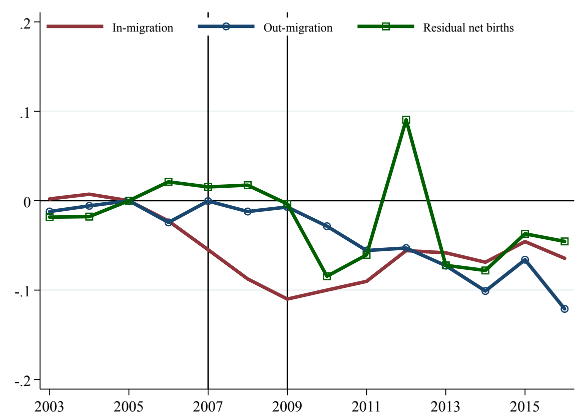
(b) 2007–2009, DV: Log Population



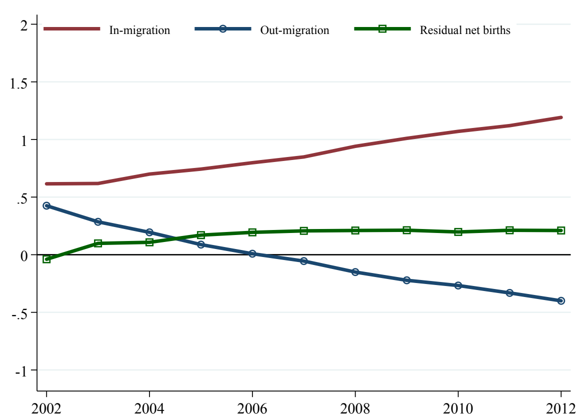
(c) 2001, DV: Population Components



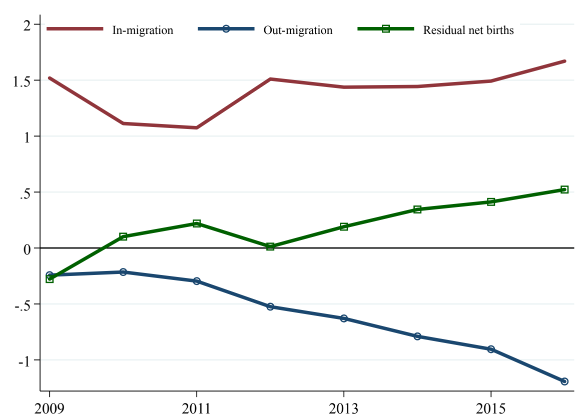
(d) 2007–09, DV: Population Components



(e) 2001, Share of Total Effect due to Components



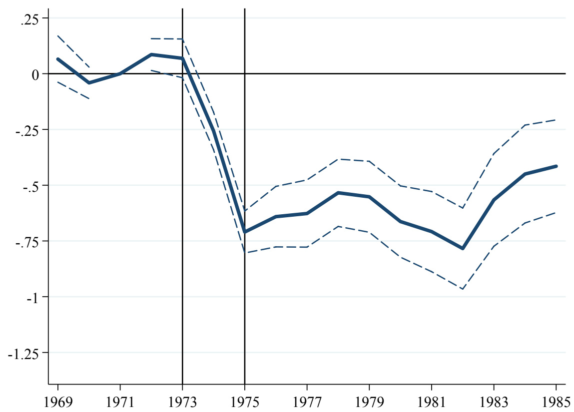
(f) 2007–09, Share of Total Effect due to Components



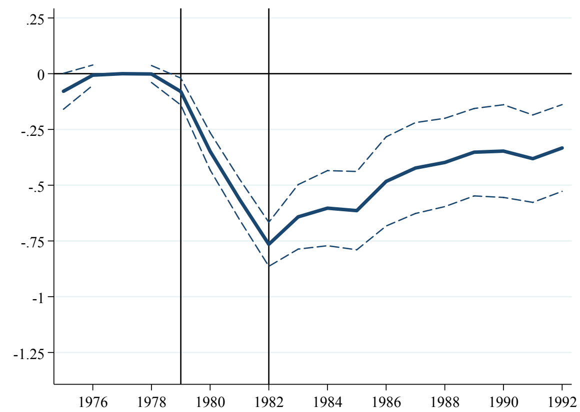
Notes: Figure reports estimates of Equation (2), separately for each recession. In Panels A and B, the dependent variable is the number of exemptions relative to the normalization year (1998 or 2005). In Panels C and D, the dependent variables are in-migration, out-migration, and residual net births, all relative to the number of exemptions in the normalization year. In Panels E and F, we divide the coefficients from Panels C and D by the coefficients in Panels A and B; we multiply the out-migration coefficient by -1 so that the shares in Panels E and F add up to one. All regressions control for interactions between the levels of exemptions, in-migration, out-migration, and residual net births in the normalization year with indicators for the current year, in addition to the baseline controls described in the notes to Figure 4.
Sources: Authors' calculations using CBP, BEAR, and SOI data.

Figure 8: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment-to-Population Ratio

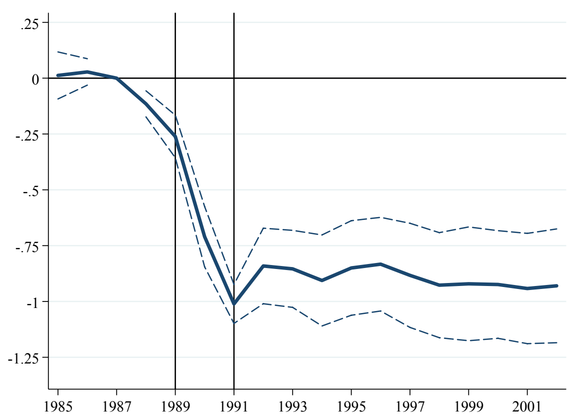
(a) 1973–1975 Recession



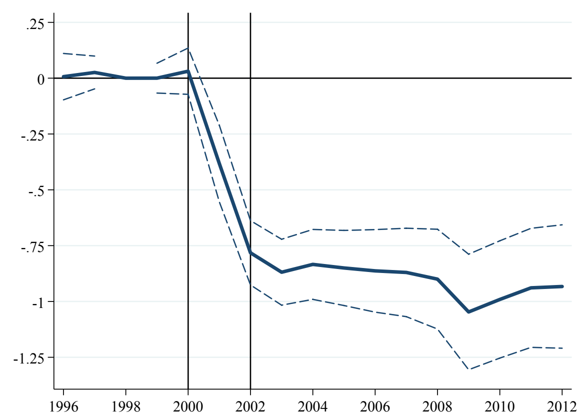
(b) 1980–1982 Recession



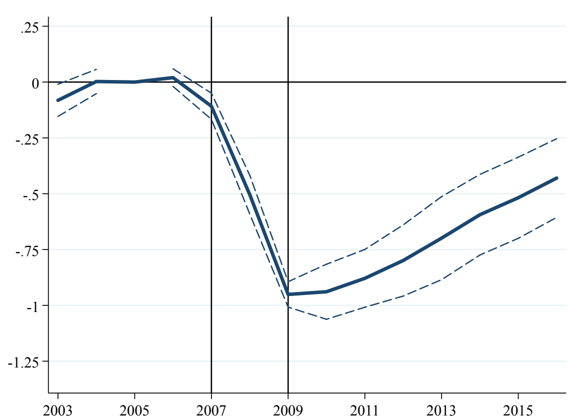
(c) 1990–1991 Recession



(d) 2001 Recession

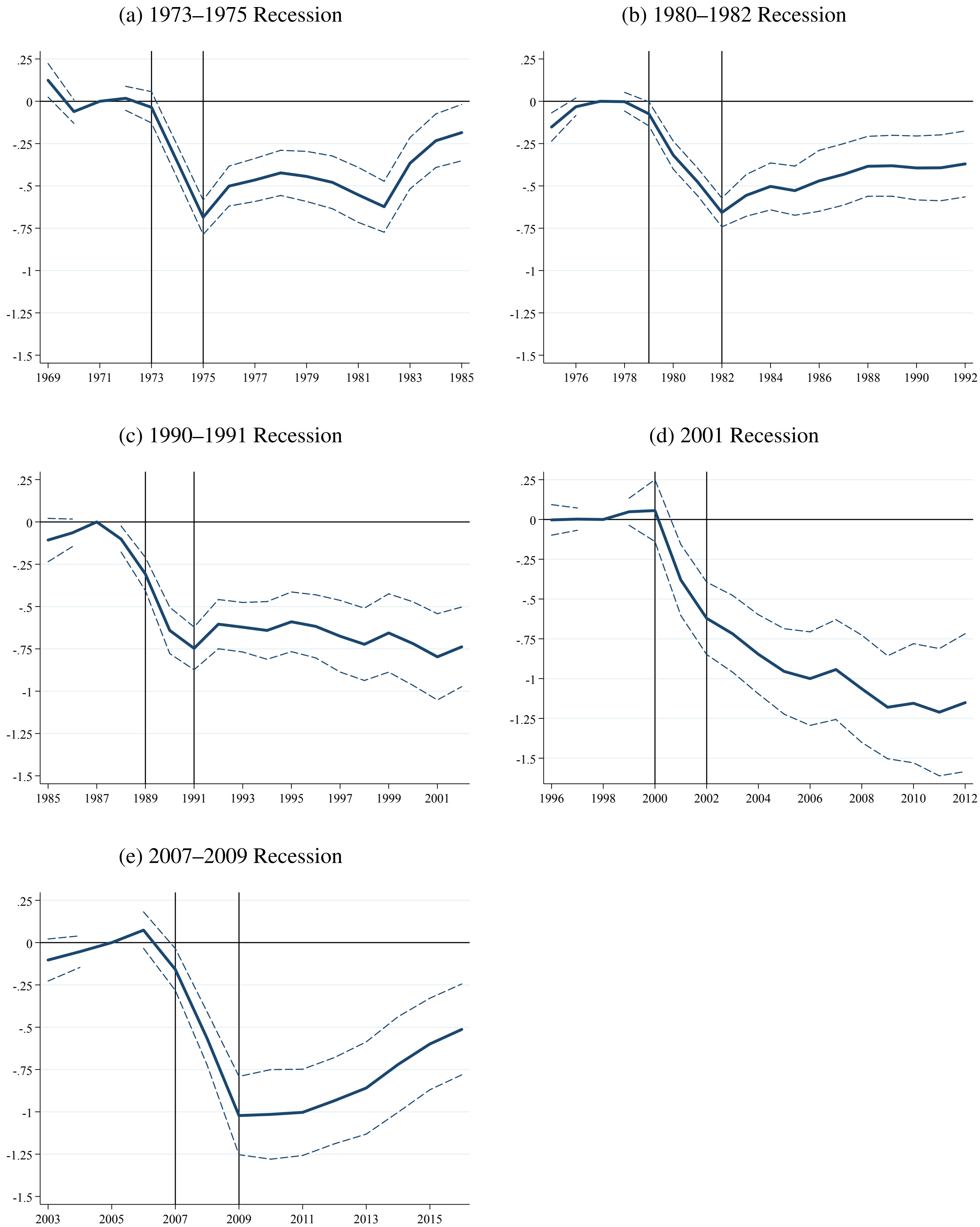


(e) 2007–2009 Recession



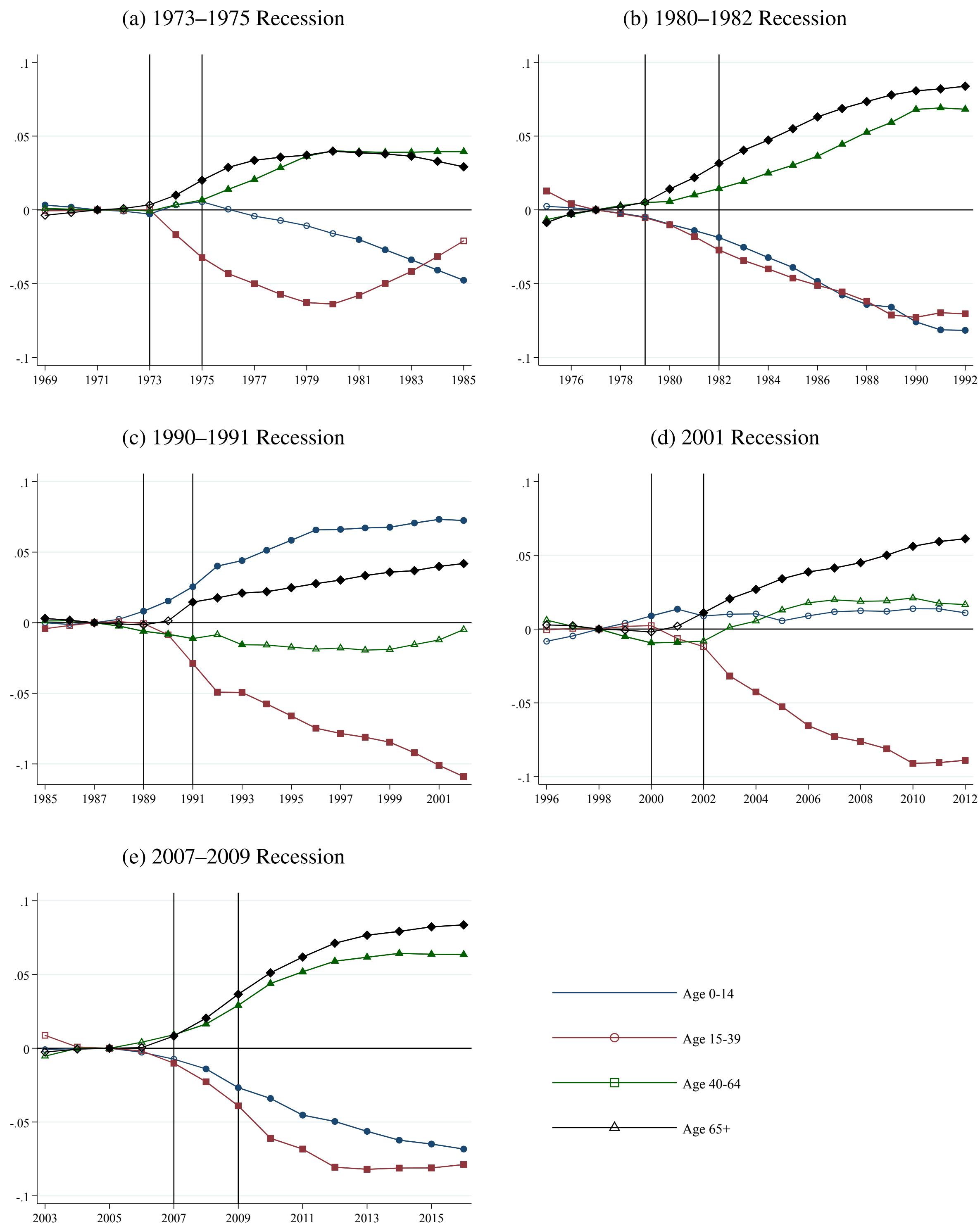
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is the log ratio of wage and salary employment to population aged 15 and above. See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

Figure 9: Impacts of Metropolitan-Area Recessionary Shocks on Log Real Earnings per Capita



Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log real earnings per capita (ages 15+). See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

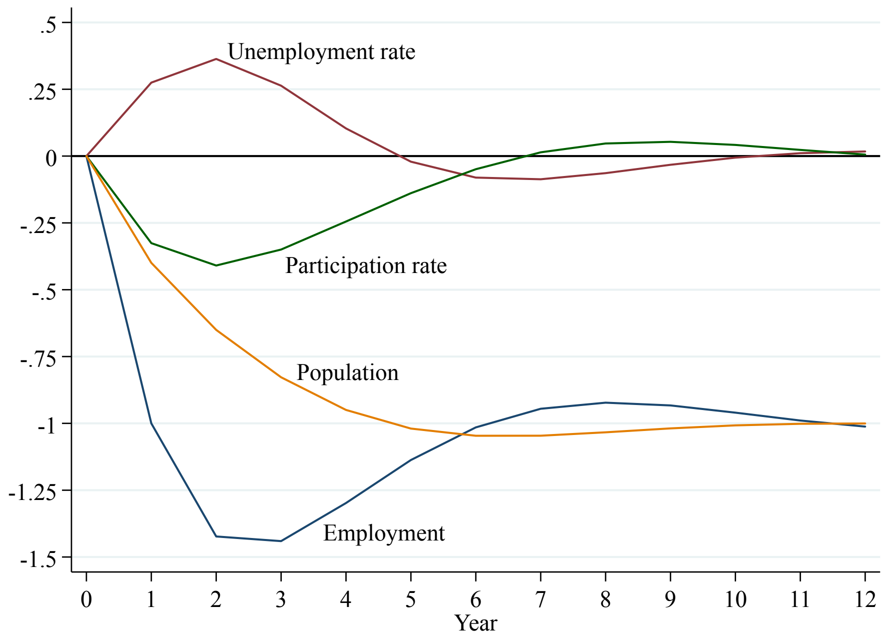
Figure 10: Impacts of Metropolitan-Area Recessionary Shocks on Age Structure



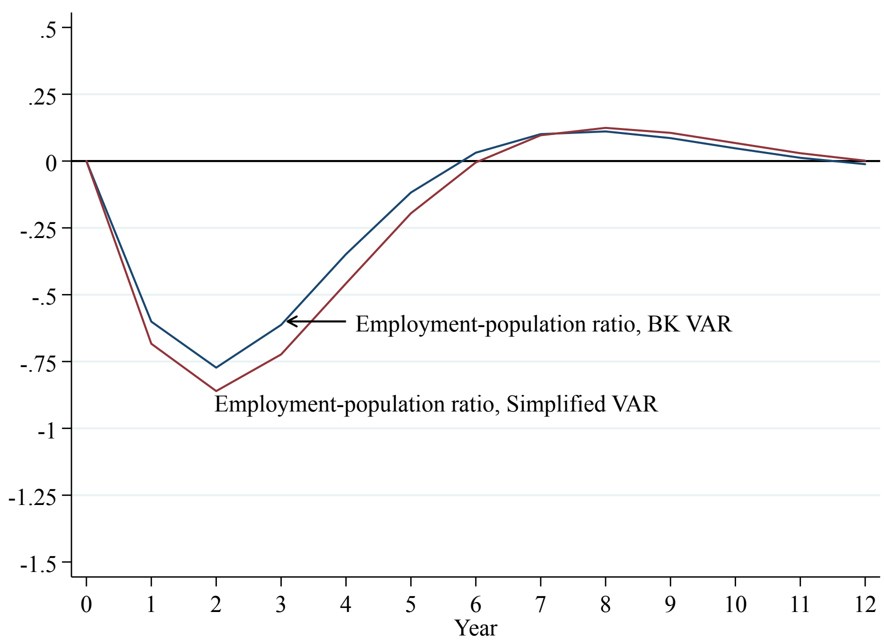
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is the share of population in the indicated age range. All regressions control for age shares in the normalization year; for other specification details, see notes to Figure 4. Filled-in markers indicate that the point estimate is significant at the 0.05 level.
Sources: Authors' calculations using BEAR and SEER data.

Figure 11: Impulse Response Functions to Negative Labor Demand Shock from Vector Autoregressions

(a) Results from Blanchard and Katz (1992) Model



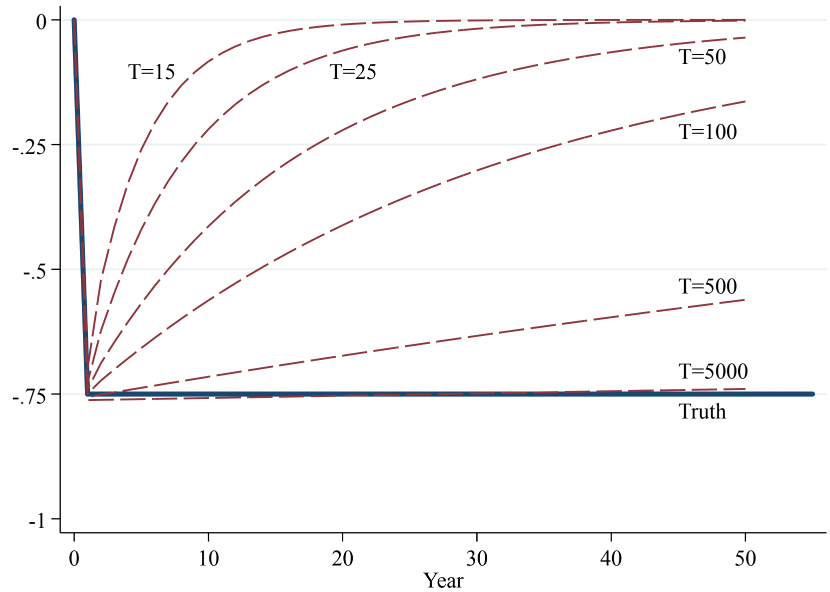
(b) Employment-Population Ratio



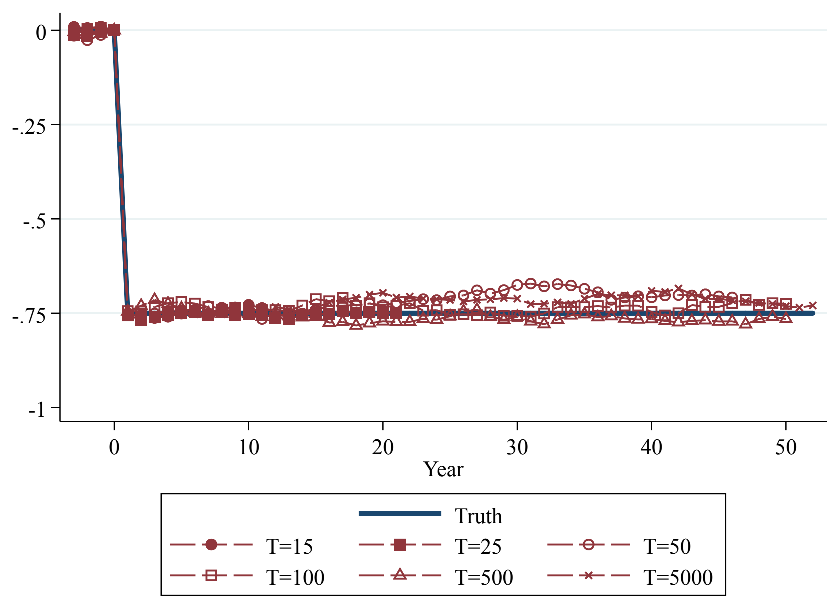
Notes: Figure shows impulse response functions of indicated variables with respect to a negative labor demand shock. We construct impulse response functions for the BK VAR using estimates of Equations (5)–(7). For the simplified VAR in Panel B, we use Equations (8)–(9)
Sources: BLS LAUS data, 1976–1990

Figure 12: Comparison of Finite Sample Bias from Vector Autoregression Impulse Response Functions and Event Study Regressions

(a) Vector Autoregression Impulse Response Functions



(b) Event Study Regressions



Notes: Panel A displays impulse response functions of the log employment-to-population ratio with respect to a negative labor demand shock based on estimates of Equations (8)–(9). Panel B displays estimates of θ_t from the following event study regression: $ep_{it} = \Delta e_i \theta_t + ep_{i,-3}\gamma_t + \mu_i + \delta_t + \epsilon_{it}$, where the shock Δe_i occurs between year 0 and 1. We normalize the coefficient $\theta_0 = 0$ and estimate the event study regression on up to 60 years of data. For both panels, we simulate data following Equations (13)–(15). We set $e_{i,0} \sim \mathcal{N}(13.94, 1.00^2)$, $p_{i,0} \sim \mathcal{N}(14.49, 1.02^2)$, $\varepsilon_{i,e,t} \sim \mathcal{N}(0, 0.015^2)$, $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$, $\beta = -0.75$, and $N = 50$. Results are based on 499 Monte Carlo simulations.

Appendices

A Data Appendix

A.1 Creating Consistent Geography Definitions over Time

We examine the impacts of recessions for two different definitions of local areas: metropolitan areas and commuting zones. Each of these geography definitions changes over time. Moreover, each geography is composed of counties, and these, too, change over time.[34] Metropolitan areas are periodically redefined by the Office of Management and Budget (OMB), and commuting zones are redefined decadally by the U.S. Department of Agriculture based on commuting questions in the census (in 1990 and 2000) or the American Community Survey (2010). For ease of interpretation, we work with temporally fixed definitions of metro areas and commuting zones throughout our analyses. Specifically, we use core-based statistical areas (CBSAs) based on OMB definitions from June 2003 (drawn based on the 2000 census), and commuting zones based on the 2000 census.[35] Since both these geographies are composed of counties, it is straightforward to aggregate county-level data using crosswalks released by the Office of Management and Budget (via the U.S. Census Bureau) or the Department of Agriculture, and we provide these crosswalks as part of our public data files.

To ensure we work with consistently defined counties, we use the Census Bureau's county change database to recode county and county equivalents in the source data (BEAR, CBP, QCEW, SEER) to consistent definitions.[36] We also restrict our analytic samples to the continental United States, excluding Alaska and Hawaii. Finally, we combine the independent cities in Virginia with their surrounding counties.

For analysis using microdata from the decennial census and ACS, counties are generally not observable. Rather, the ACS, 1990 census, and 2000 census contain indicators for the Public Use Microdata Area (PUMA), time-varying areas of at least 100,000 individuals. The 1970 and 1980 censuses instead contain county-group identifiers, which are conceptually similar but based on municipal and county units rather than census tracts. We use population-weighted crosswalks available from the Missouri Census Data Center's Geocorr application to map PUMAs to counties, and we use crosswalks that link counties to county groups, available from IPUMS, to map county groups to CBSAs.[37] As described in the main text, for many of the analyses we first process the microdata and then collapse the relevant measures to our analytic geographies using the crosswalks.

Finally, because the census/ACS do not provide annual data prior to 2005, we attempted to use the Current Population Survey (CPS) to conduct our event study analyses for compositional and

[34] Counties are the most stable, but they occasionally change because of state legislative action or boundary disputes.

[35] See `https://www.census.gov/geographies/reference-files/time-series/demo/metro-micro/historical-delineation-files.html` and `https://www.ers.usda.gov/data-products/commuting-zones-and-labor-market-areas/`, respectively.

[36] See `https://www.census.gov/programs-surveys/geography/technical-documentation/county-changes.html`. For counties that change only names or codes, we use the modern versions, and we combine counties that either merge or split.

[37] See `https://usa.ipums.org/usa/volii/t1970maps.shtml` and `https://usa.ipums.org/usa/volii/ctygrp.shtml`.

distributional outcomes for some of the earlier recessions. Substate geography indicators become available in the (basic monthly) CPS beginning in 1989, but unlike the census/ACS, these are not PUMAs but metropolitan areas. As noted above, the definitions of these metro areas change relatively frequently, and sometimes they do so in the data in ways that are not well documented.[38] We have used the appendices in the CPS technical documentation and multiple OMB crosswalks to create harmonized metropolitan areas covering the same geography, when possible, in the CPS from 1989 through 2018. Over this period, we can identify more than 100 distinct areas; over shorter and more recent intervals (such as from 1995 to 2018, which covers both the 2001 and 2007–2009 recessions), the number rises to 145, accounting for about 63 percent of the U.S. population and 74 percent of the population living in metropolitan areas. Around only the period of the Great Recession, from 2004 to 2018, we can identify 221 metropolitan areas.[39]

We provide, upon request, a public-use data file that synthesizes and harmonizes the data for the various analyses in the paper and allows replication of our work.

A.2 Imputing Employment in Quarterly Census of Employment and Wages

For some robustness checks, we use the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) as an alternative measure to the BEAR data for local area employment. QCEW data are based on unemployment insurance records from each state, are one of the inputs used by BEA to construct its employment data, and constitute the data source used to benchmark the Current Employment Statistics for monthly jobs reports. Data are available starting in 1975 from the BLS website and provide employment and establishment counts, as well as aggregate and average weekly wages, for each county and industry, at annual, quarterly, and (for employment counts) monthly frequencies.[40] However, data suppressions are common, especially earlier in the period. At the county level, data for small or highly concentrated industries (e.g., agriculture, mining) are often suppressed, although very small counties may even have total or total private employment suppressed. When these suppressions occur, *all* data for the county-industry quarter are suppressed, unlike in County Business Patterns, described below. (For national series, used for constructing the "shifts" in the creation of Bartik shocks, suppression is not an issue.)

For total and total private (excluding government) employment, we impute missing employment counts at the county level through the following ordered process: 1) If total and government employment are reported but private employment is suppressed, we impute private employment as the difference between total and government.[41] 2) If either total *or* private employment is missing

[38]The codes used to identify metro areas change for major revisions, such as the switch from primary metropolitan statistical areas (PMSAs) in the 1990s to core-based statistical areas in the 2000s, but in other cases the same codes are used even if the OMB definitions changed by adding (or removing) a county from a given metro area. Which vintage of metropolitan-area definition is in effect for any given month in the CPS appears in an appendix to the technical documentation, but we are unaware of any effort to systematically track these changes. Furthermore, the CPS periodically changes which subset of metro areas are identified in the data, and these changes are not clearly documented. Starting in 1995, a limited number of counties (usually larger ones) are identified in the CPS.

[39]An additional complication is periodic sampling changes in the CPS that affect, sometimes dramatically, the number of included households and individuals in a given metro area. When we eliminate areas that experienced severe sampling changes, we observe approximately 70 areas spanning the period around the 1990–1991 recession and approximately 75 areas spanning the period around the 2001 recession.

[40]Aggregate employment for each geography is available from 1975; industry-level measures are available under NAICS coding from 1990 forward and SIC coding from 1975 through 2000.

[41]We follow this rule for 1978 forward, when local and state government reporting was near universal; prior to

in a given quarter, but not for all quarters in the year, we impute the one that is missing based on the average ratio (private share of total) for the year. 3) If either total *or* private employment is missing for an entire year, so that the private share for that year is unavailable, we impute the missing values based on the average share over the rolling window from two years prior to two years after the current year. This process imputes aggregate employment counts for nearly every case from 1978 onward. For the few remaining cases, mostly before 1978, we impute values by running a county-specific regression of the log of the employment measure (either total or total private) on year and quarter dummies from 1978 forward and replacing the missing values (including those from before 1978) with their predicted values from the regression.

We also attempted to impute industry-level employment through regression-based means, as above. This worked reasonably well for most industries (excluding agriculture and mining) if missing values occurred in interior points of the series and were relatively sparse; however, the procedure performed poorly when missing values occurred near the beginning or end of the horizon or were sequentially dense. Most of these cases occurred in smaller counties, few of which were in CBSAs. Nonetheless, we do not use the regression-based imputations in our industry analysis in Figure 5. For this analysis, we use QCEW data only for the 2001 recession, as the BEAR data show obvious seaming issues around the SIC-NAICS industry transition that occurred during the middle part of this recession; QCEW data are available under NAICS for the full analysis horizon and thus suffer no seaming issues.

A.3 Imputing Employment in County Business Patterns

When constructing the Bartik (1991) shock, we use County Business Patterns (CBP) data to measure local industry employment shares. CBP data always report establishment counts by county, industry, and establishment size, but frequently suppress employment at the county by industry level. From 1974 forward, the establishment size groups are 1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1000–1499, 1500–2499, 2500–4999, and 5000 or more employees.

We impute employment at the county by industry level using establishment counts and nationwide information on employment by establishment size. For establishments with fewer than 1,000 employees, we impute employment as the number of establishments times average prerecession employment in the establishment size group, where the average comes from nationwide data across all industries. We use years 1978, 1988, 1999, and 2006 for the 1980–1982, 1990–1991, 2001, and 2007–2009 recessions.[42]

Nationwide CBP data report total employment among establishments with at least 1,000 employees, but not by establishment size group. To impute employment for these large establishments, we assume that employment follows a log normal distribution, with mean μ and standard deviation σ, and estimate (μ, σ) using the generalized method of moments (GMM), as in Holmes

this year, many jobs in local and state governments were not in the reporting universe and available counts, when not suppressed, vastly underestimated government employment. See P.L. 94-566.

[42]For the 1980–1982 and 1990–1991 recessions, we use approximately 70 two-digit SIC industries. For the 2001 and 2007–2009 recessions, we use approximately 85 three-digit NAICS industries. For the 1973–1975 recession, imputation isn't possible.

and Stevens (2002) and Stuart (2018). We estimate (μ, σ) using the following four moments:

$$p_1 = \Phi\left(\frac{\ln(1499) - \mu}{\sigma}\right) - \Phi\left(\frac{\ln(1000) - \mu}{\sigma}\right) \tag{A.1}$$

$$p_2 = \Phi\left(\frac{\ln(2499) - \mu}{\sigma}\right) - \Phi\left(\frac{\ln(1500) - \mu}{\sigma}\right) \tag{A.2}$$

$$p_3 = \Phi\left(\frac{\ln(4999) - \mu}{\sigma}\right) - \Phi\left(\frac{\ln(2500) - \mu}{\sigma}\right) \tag{A.3}$$

$$E[y] = \exp(\mu + \sigma^2/2), \tag{A.4}$$

where p_1 is the share of establishments of at least 1,000 employees with 1000–1499 employees, p_2 is the share with 1500–2499 employees, p_3 is the share with 2500–4999 employees, $\Phi(\cdot)$ is the standard normal CDF, and $E[y]$ is average employment among establishments with at least 1,000 employees.

We use Equations (A.1)–(A.4) to estimate (μ, σ) with GMM, using the identity matrix as the weighting matrix. For years 1978, 1988, 1999, and 2006, the estimates of (μ, σ) are (7.50, 0.67), (7.49, 0.63), (7.50, 0.61), and (7.51, 0.67). Standard facts about the log-normal distribution imply that the imputed means for the four establishment-size groups are (1247, 1951, 3406, 6980) for 1978, (1248, 1949, 3379, 6745) for 1988, (1250, 1949, 3363, 6610) for 1999, and (1248, 1951, 3405, 6956) for 2006.[43]

For 1999 and 2006, we can compare the county-industry employment imputations from this procedure (normalized by overall county employment to make industry shares) with those from the Upjohn Institute's WholeData series (Bartik et al., 2019), which provides desuppressed employment counts in the NAICS period. The correlations are very high, in excess of 0.99, suggesting the imputation procedure is quite accurate.

B Results Appendix

B.1 Robustness to Different Employment Shocks

Our baseline specification uses public and private wage and salary employment from BEAR to construct recession shocks. We believe that this variable is best because the BEA makes considerable efforts to construct data that are consistent over time, although this is more difficult for the self-employed (whose employment can vary over time in response to tax incentives). The two leading alternatives are private wage and salary employment from BEAR and private wage and salary employment from QCEW.[44] Figures A.5–A.8 show that the estimated effects on employ-

[43] In particular, if $\ln(y) \sim \mathcal{N}(\mu, \sigma^2)$, then

$$E(y|a < y \leq b) = E(y)\frac{\Phi(\sigma - a_0) - \Phi(\sigma - b_0)}{\Phi(b_0) - \Phi(a_0)}, \quad a_0 \equiv (\ln a - \mu)/\sigma, \quad b_0 \equiv (\ln b - \mu)/\sigma$$
$$E(y|y > a) = E(y)\frac{\Phi(\sigma - a_0)}{\Phi(-a_0)}.$$

[44] CBP data represent another alternative, although the CBP coverage is not quite as complete as BEAR or QCEW; notably, CBP excludes most public-sector employment, as well as agricultural services, railroads, postal workers, and

ment, population, the employment-to-population ratio, and earnings per capita are quite similar when using these other measures to define the employment shock. The similarity of the results is not surprising, as the public sector accounts for less than 25 percent of wage and salary employment on average, and BEAR data rely on QCEW data as an input. Still, it is reassuring that our results are not sensitive to this choice.

B.2 Results Using Bartik Shocks

We estimate Equation (2) using OLS. A potential concern with this approach is that employment changes in local areas might stem from factors besides recessions, such as changes in labor supply. A common approach in the literature—much of which examines ten-year employment changes rather than business-cycle peak-to-troughs—is to instead use variation in log employment changes predicted by a location's baseline industrial structure, following Bartik (1991). In our setting, the Bartik shock is

$$\text{BartikShock}_i = \sum_j \eta_{i,j}(\ln(E_{j,t_1}) - \ln(E_{j,t_0})),$$

where $\eta_{i,j}$ is the share of employment in local area i in industry j in a base year, and the term in parentheses equals the nationwide log employment change in industry j from recession peak to trough. We use CBP data to construct $\eta_{i,j}$ (see Appendix A.3) and QCEW data to construct the nationwide log employment change.[45]

We do not use the Bartik shock in our preferred specification, because our focus on a shorter window during recessions and our controls for prerecession population growth mitigate concerns about labor supply driving the sharp employment changes that we see. Furthermore, recent work highlights issues that arise in using industry shift-share methods like the Bartik shock (Adão, Kolesár and Morales, 2018; Kirill, Hull and Jaravel, 2018; Goldsmith-Pinkham, Sorkin and Swift, 2018). Nonetheless, given the ubiquity of the Bartik shock, we report results from using it here.

Appendix Table A.2 describes the relationship between our preferred recessionary shock (actual log employment change) and the Bartik shock (predicted log employment change). The first column includes no other controls. For every recession besides 1990–1991, the Bartik shock explains 33–36 percent of the cross-metro variation in the recessionary shock. For 1990–1991, the Bartik shock explains only 6 percent of the variation. Columns 2 and 3 add in division fixed effects and controls for lagged population growth. The coefficients—which are all positive, as expected—are reasonably stable across specifications, especially after 1973–1975, when greater industry-level detail is available. Moreover, the coefficient estimates remain highly statistically significant even with the additional controls.

Appendix Table A.3 shows that Bartik shocks are more highly correlated across time than our preferred recessionary shocks. This is not surprising, as Bartik shocks primarily reflect local industry employment shares, which are relatively stable. These high correlations raise the concern that the coefficients on the Bartik shock variable might not isolate the impact of a given recession.

private households.

[45]QCEW data have the advantage of being available at a quarterly frequency, which we could (but do not) use in constructing the Bartik shock; in earlier versions of the paper, we found our results were not sensitive to this choice. Because detailed county-by-industry employment counts in the QCEW are commonly suppressed, with less information with which to make imputations, we use the CBP to construct prerecession employment share.

Appendix Figure A.9 displays estimates of the effect of the Bartik shock on log employment. The results are qualitatively similar to those using recessionary shocks in Figure 4 for the 1980–1982, 2001, and 2007–2009 recessions.[46] There is less evidence of a persistent employment decline for the 1973–1975 and 1990–1991 recessions; however, for these recessions, there *is* clear evidence of an employment decline during the subsequent recession, consistent with the high cross-recession correlations. Figures A.10 through A.12 display results for population, the employment-to-population ratio, and earnings per capita. The patterns largely mirror those already discussed for employment.

B.3 The Effects of Recessions on Commuting Zones

Our main approach defines local labor markets as metropolitan areas. Another reasonable approach is to use commuting zones as the unit of the geography, as these zones span the entire (continental) United States, including rural areas. Appendix Figures A.13 through A.16 show that results are very similar when using commuting zones (specifically, the 2000 definition).

[46]There is much less cross-sectional variation in the Bartik shocks than in the actual employment shocks (Appendix Figure A.1); all else being equal, this would cause the coefficients on the Bartik shock to be larger than those on the recession shock. However, the Bartik shock captures only a fraction of the total variation in the recessionary shock, so we would not necessarily expect the magnitudes to be identical even if we normalized by the standard deviations of the shocks.

Table A.1: Impacts of Metropolitan-Area Recessionary Shocks on Annual, Weekly, and Hourly Wage Earnings, Census/ACS

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Without Composition Adjustment					
Log annual earnings	−0.203	−0.503	−0.126	−0.547	−0.549
	(0.095)	(0.092)	(0.099)	(0.104)	(0.127)
Log weekly earnings	−0.192	−0.453	−0.107	−0.441	−0.489
	(0.082)	(0.076)	(0.085)	(0.087)	(0.111)
Log hourly earnings	−0.170	−0.416	−0.116	−0.356	−0.428
	(0.071)	(0.069)	(0.074)	(0.078)	(0.097)
Panel B: With Composition Adjustment					
Log annual earnings	−0.155	−0.331	−0.060	−0.627	−0.359
	(0.086)	(0.076)	(0.080)	(0.090)	(0.112)
Log weekly earnings	−0.142	−0.305	−0.050	−0.517	−0.338
	(0.076)	(0.064)	(0.068)	(0.077)	(0.098)
Log hourly earnings	−0.126	−0.312	−0.057	−0.423	−0.296
	(0.064)	(0.062)	(0.061)	(0.070)	(0.084)

Notes: See notes to Table 5.

Sources: Authors' calculations using BEAR, decennial census, and ACS data.

Table A.2: Cross-Sectional Relationship between Metropolitan-Level Log Employment Change and Bartik Shock

	Dependent variable: Log employment change During recession		
	(1)	(2)	(3)
Panel A: 1973–1975 Recession			
Bartik shock	2.388	1.602	1.561
	(0.232)	(0.261)	(0.280)
R^2	0.344	0.450	0.489
Panel B: 1980–1982 Recession			
Bartik shock	1.983	1.805	1.565
	(0.164)	(0.143)	(0.159)
R^2	0.360	0.591	0.666
Panel C: 1990–1991 Recession			
Bartik shock	1.341	0.727	0.977
	(0.233)	(0.229)	(0.243)
R^2	0.062	0.415	0.473
Panel D: 2001 Recession			
Bartik shock	1.517	1.261	1.260
	(0.114)	(0.133)	(0.138)
R^2	0.344	0.407	0.538
Panel E: 2007–2009 Recession			
Bartik shock	1.789	1.528	1.590
	(0.173)	(0.191)	(0.205)
R^2	0.330	0.452	0.512
Division fixed effects		x	x
Pre-recession population growth			x

Notes: Table reports estimates of the log employment change during recessions against the Bartik (1991) shock. There are 363 metropolitan areas in the sample. Heteroskedastic-robust standard errors are in parentheses.
Source: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Table A.3: Correlation of Metropolitan-Area Bartik Shocks

	Predicted Change in Log Employment during Recession Years				
	1973–75	1979–82	1989–91	2000–02	2007–09
Panel A: Unadjusted					
1973–75	1.000				
1980–82	0.815	1.000			
1989–91	0.723	0.726	1.000		
2000–02	0.741	0.696	0.808	1.000	
2007–09	0.478	0.527	0.723	0.667	1.000
Panel B: Adjusted for Census Division					
1973–75	1.000				
1980–82	0.768	1.000			
1989–91	0.677	0.664	1.000		
2000–02	0.686	0.629	0.809	1.000	
2007–09	0.509	0.497	0.735	0.682	1.000
Panel C: Adjusted for Census Division and Prerecession Population Growth					
1973–75	1.000				
1980–82	0.750	1.000			
1989-91	0.606	0.578	1.000		
2000–02	0.572	0.534	0.716	1.000	
2007–09	0.449	0.453	0.675	0.607	1.000

Notes: Table reports correlations of predicted log employment changes (Bartik, 1991) across recessions for 363 metropolitan areas. Panel B reports correlations after partialling out census division fixed effects, and Panel C partials out census division fixed effects and prerecession population growth.
Source: Authors' calculations using CBP and QCEW data.

Table A.4: Impacts of Metropolitan-Area Recessionary Shocks on Age Structure, 7–9 Years after Recession Trough

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on Recessionary Shock					
Share age 0–14	−0.034	−0.074	0.066	0.013	−0.068
	(0.010)	(0.011)	(0.016)	(0.015)	(0.018)
Share age 15–39	−0.041	−0.072	−0.078	−0.088	−0.079
	(0.014)	(0.015)	(0.022)	(0.024)	(0.015)
Share age 40–64	0.039	0.066	−0.018	0.019	0.064
	(0.009)	(0.012)	(0.016)	(0.019)	(0.020)
Share age 65+	0.036	0.080	0.030	0.055	0.084
	(0.010)	(0.011)	(0.012)	(0.014)	(0.013)
Panel B: Implied Effect of a 1 SD Recessionary Shock					
Share age 0–14	−0.002	−0.006	0.003	0.000	−0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Share age 15–39	−0.002	−0.006	−0.004	−0.003	−0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Share age 40–64	0.002	0.005	−0.001	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Share age 65+	0.002	0.006	0.001	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.0005)	(0.001)

Notes: Table reports estimates of Equation (2), separately for each recession. The dependent variable is the share of population in the indicated category. All regressions control for all age shares in the normalization year, plus the covariates described in Table 4.
Sources: Authors' calculations using BEAR and SEER data.

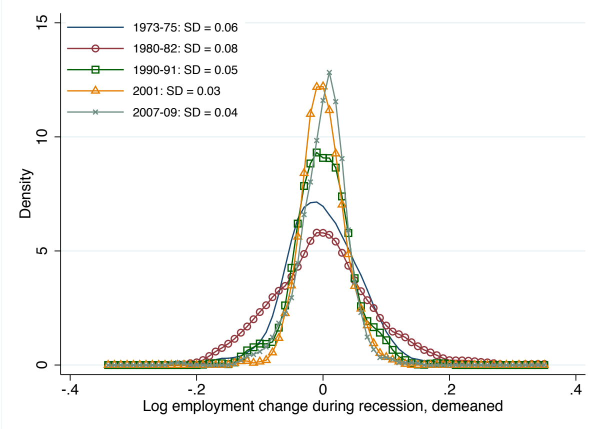
Table A.5: Bias in VAR Parameters

	Parameter			
	$\tilde{\alpha}_{11}$	$\tilde{\alpha}_{12}$	$\tilde{\alpha}_{21}$	$\tilde{\alpha}_{22}$
Truth	0.000	0.000	0.750	1.000
Time series obs. (T)	Average estimate			
15	−0.042	−0.099	0.699	0.856
25	−0.019	−0.060	0.727	0.919
50	−0.010	−0.030	0.741	0.960
100	−0.005	−0.015	0.749	0.980
500	−0.001	−0.003	0.756	0.996
5000	0.000	0.000	0.762	1.000

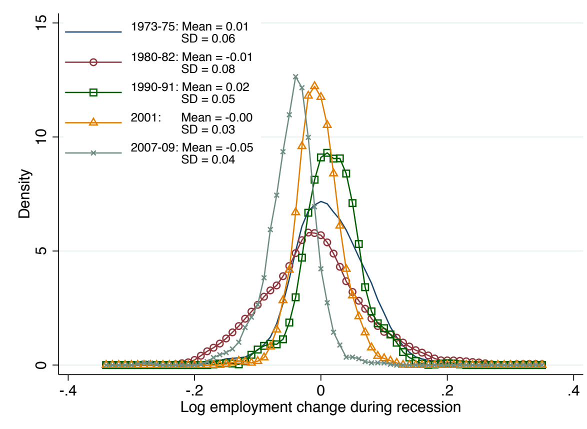
Notes: Table displays average estimates of parameters in Equations (8)–(9). We simulate data following Equations (13)–(15). We set $e_{i,0} \sim \mathcal{N}(13.94, 1.00^2)$, $p_{i,0} \sim \mathcal{N}(14.49, 1.02^2)$, $\varepsilon_{i,e,t} \sim \mathcal{N}(0, 0.015^2)$, $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$, $\beta = 0.75$, and $N = 50$. Results are based on 499 Monte Carlo simulations.

Figure A.1: Density of Recession Severity and Bartik Shock across Metro Areas

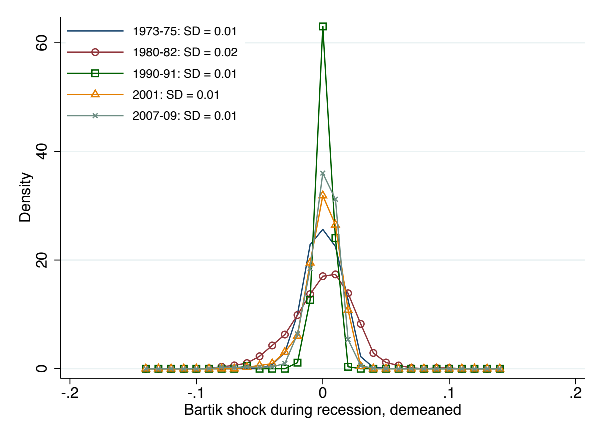
(a) Recession Severity, Demeaned



(b) Recession Severity



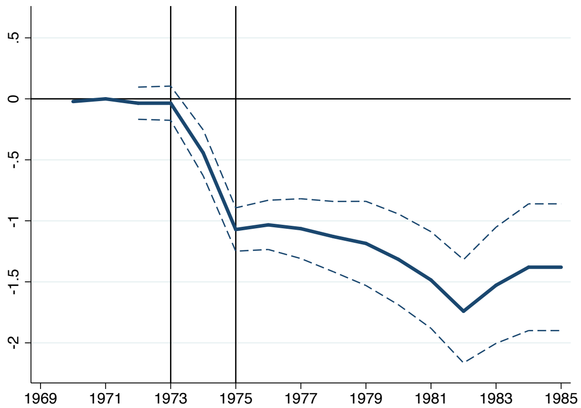
(c) Bartik Shock, Demeaned



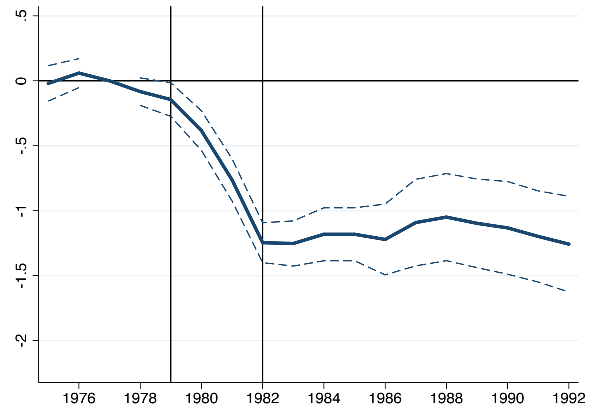
Notes: The figure shows estimated kernel densities of the log wage and salary employment change (Panels A and B) and predicted log employment change based on prerecessionary industrial structure (as in Bartik (1991); Panel C) across metro areas for each of the five recessions since the mid-1970s. In Panels A and C, log employment changes are demeaned for each recession using the unweighted average across metro areas.
Source: Authors' calculations from BEAR, CBP, and QCEW data.

Figure A.2: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment from CBP

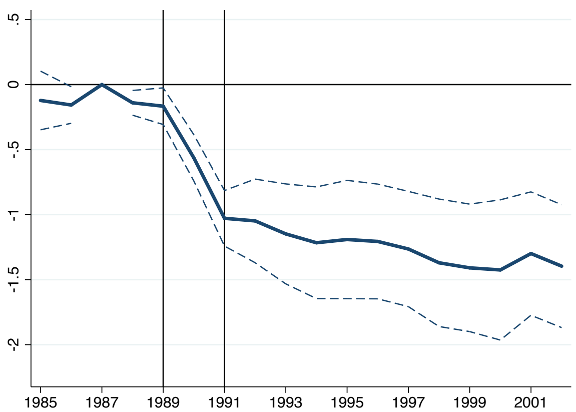
(a) 1973–1975 Recession



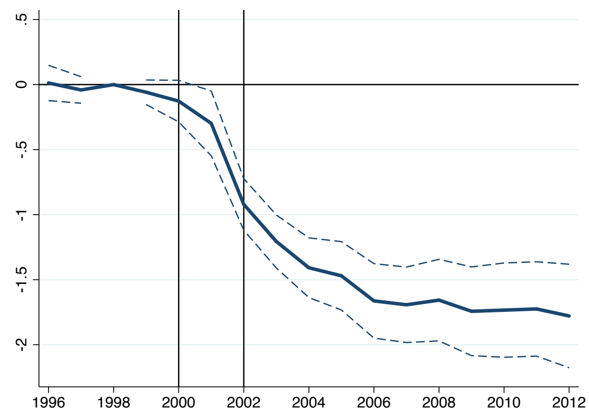
(b) 1980–1982 Recession



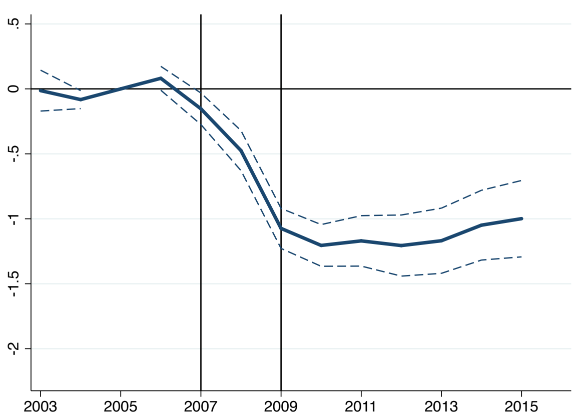
(c) 1990–1991 Recession



(d) 2001 Recession

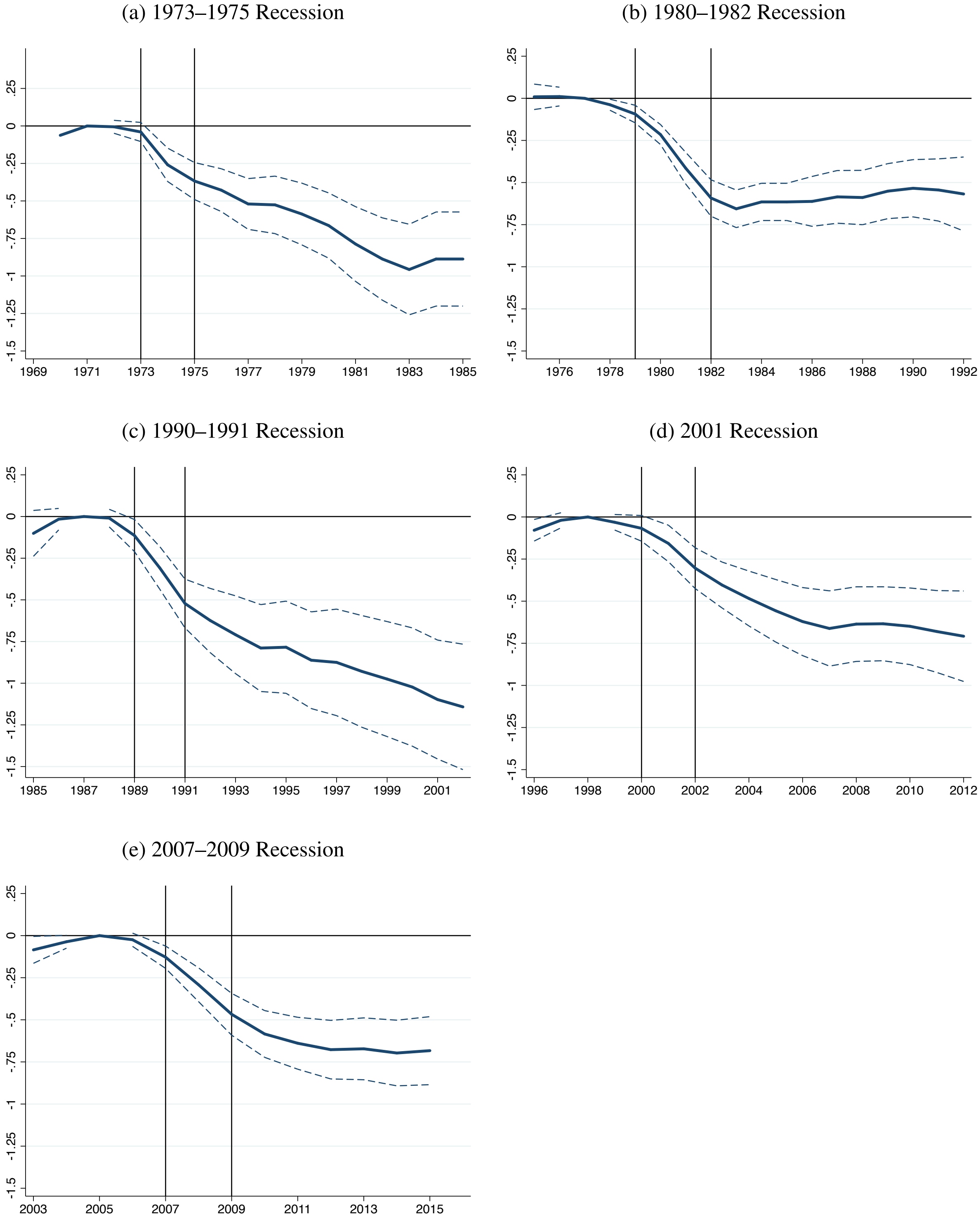


(e) 2007–2009 Recession



Notes: Table reports estimates of Equation (2), separately for each recession. The dependent variable is log employment from CBP data. See notes to Figure 4.
Sources: Authors' calculations using CBP, BEAR, and SEER data.

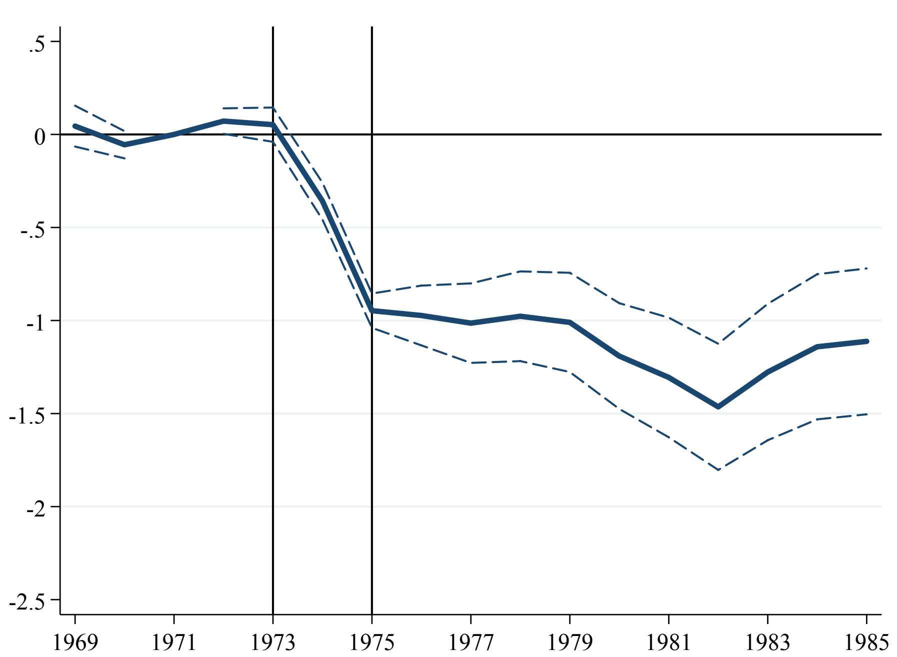
Figure A.3: Impacts of Metropolitan-Area Recessionary Shocks on Log Establishments from CBP



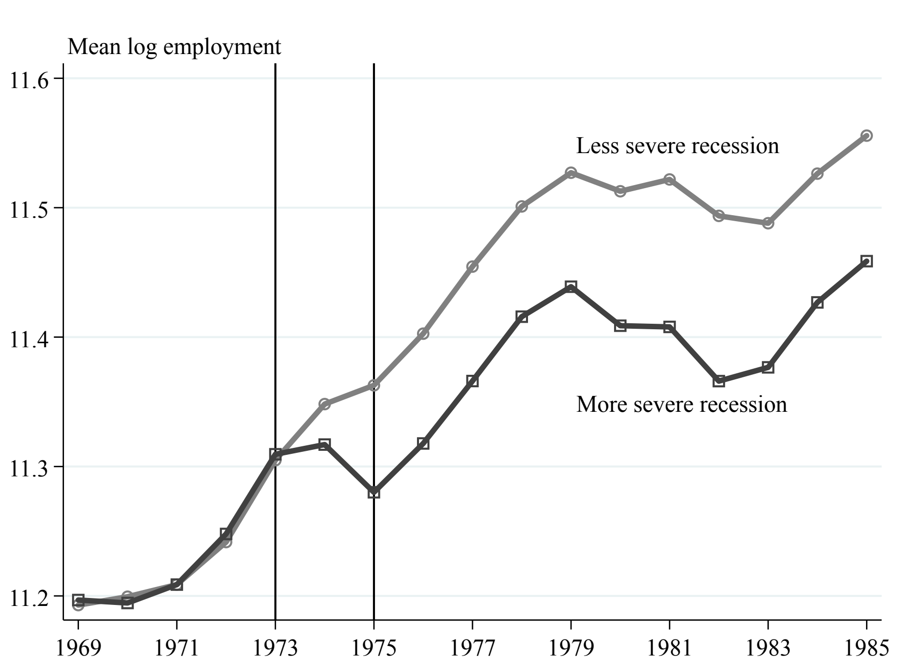
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log establishments from CBP data. See notes to Figure 4.
Sources: Authors' calculations using CBP, BEAR, and SEER data.

Figure A.4: Translating Relative Effects from Event Study into Absolute Effects

(a) Event Study Coefficients for Log Employment, 1973–1975 Recession

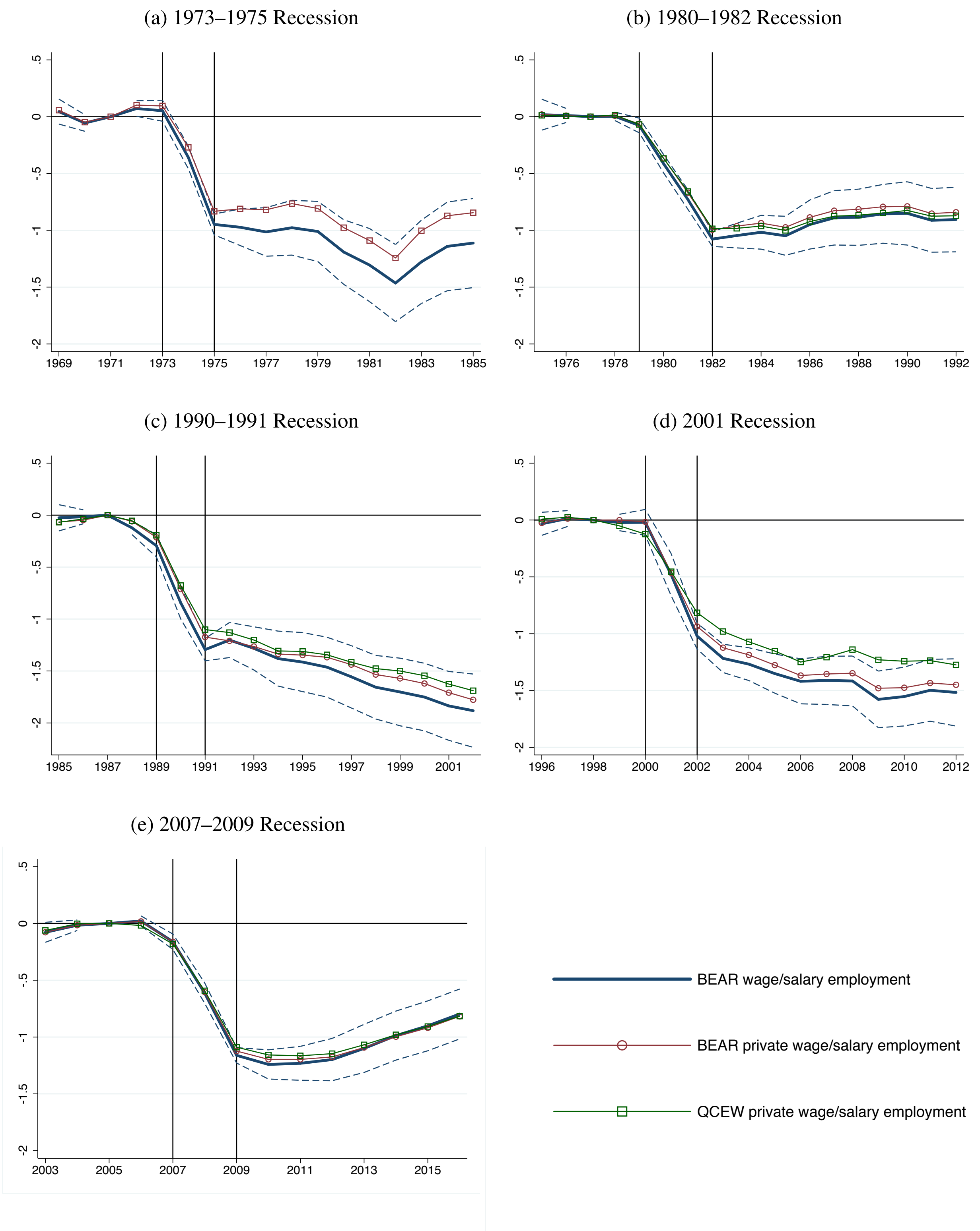


(b) Implied Effect of 1973–1975 Recession on Log Employment



Notes: Panel A shows estimates of our main specification, as in Panel A of Figure 4. In Panel B, we use these estimates to construct the implied effect on mean log employment for metro areas with a more-versus-less-severe recession, holding all other covariates in the regression at their mean value.
Source: Authors' calculations from BEAR data.

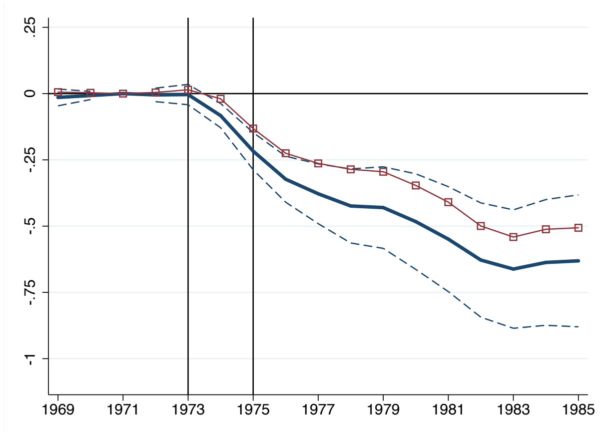
Figure A.5: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment, Robustness to Different Employment Shocks



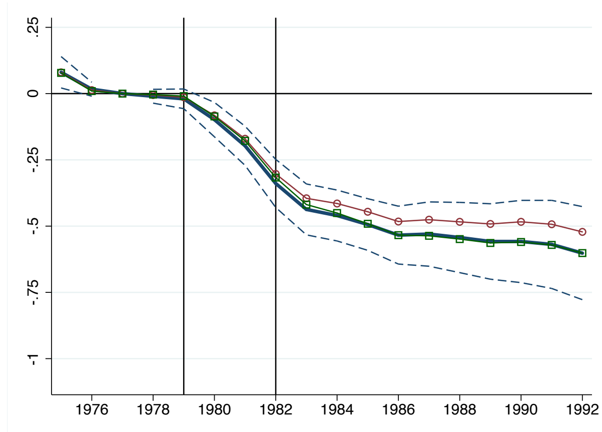
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary employment shock.
Sources: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.6: Impacts of Metropolitan-Area Recessionary Shocks on Log Population Ages 15+, Robustness to Different Employment Shocks

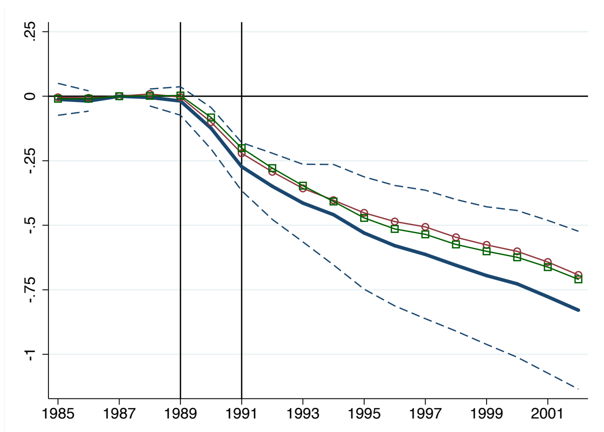
(a) 1973–1975 Recession



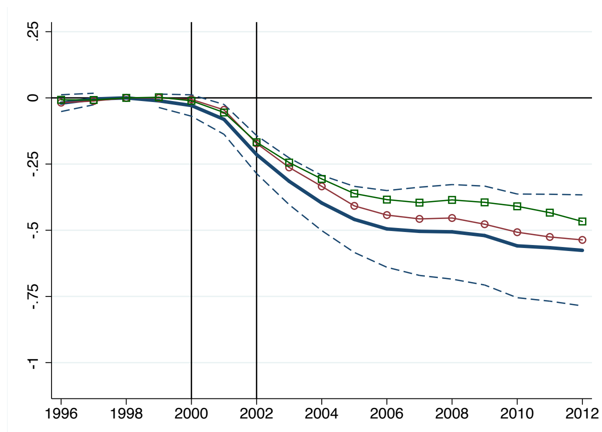
(b) 1980–1982 Recession



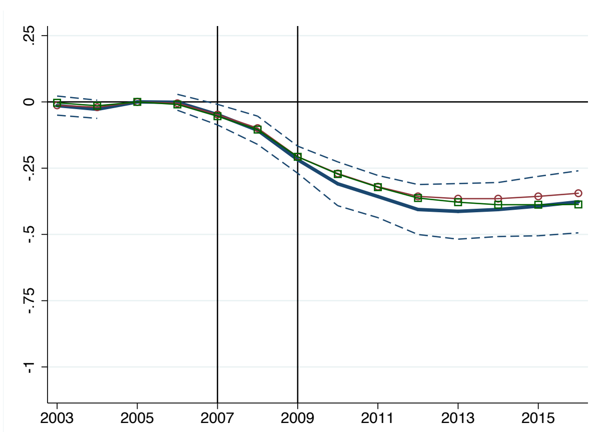
(c) 1990–1991 Recession

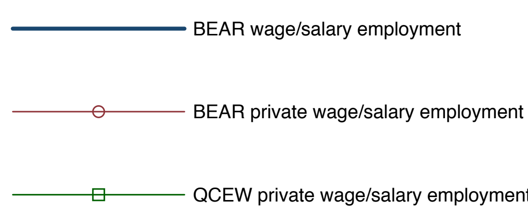


(d) 2001 Recession



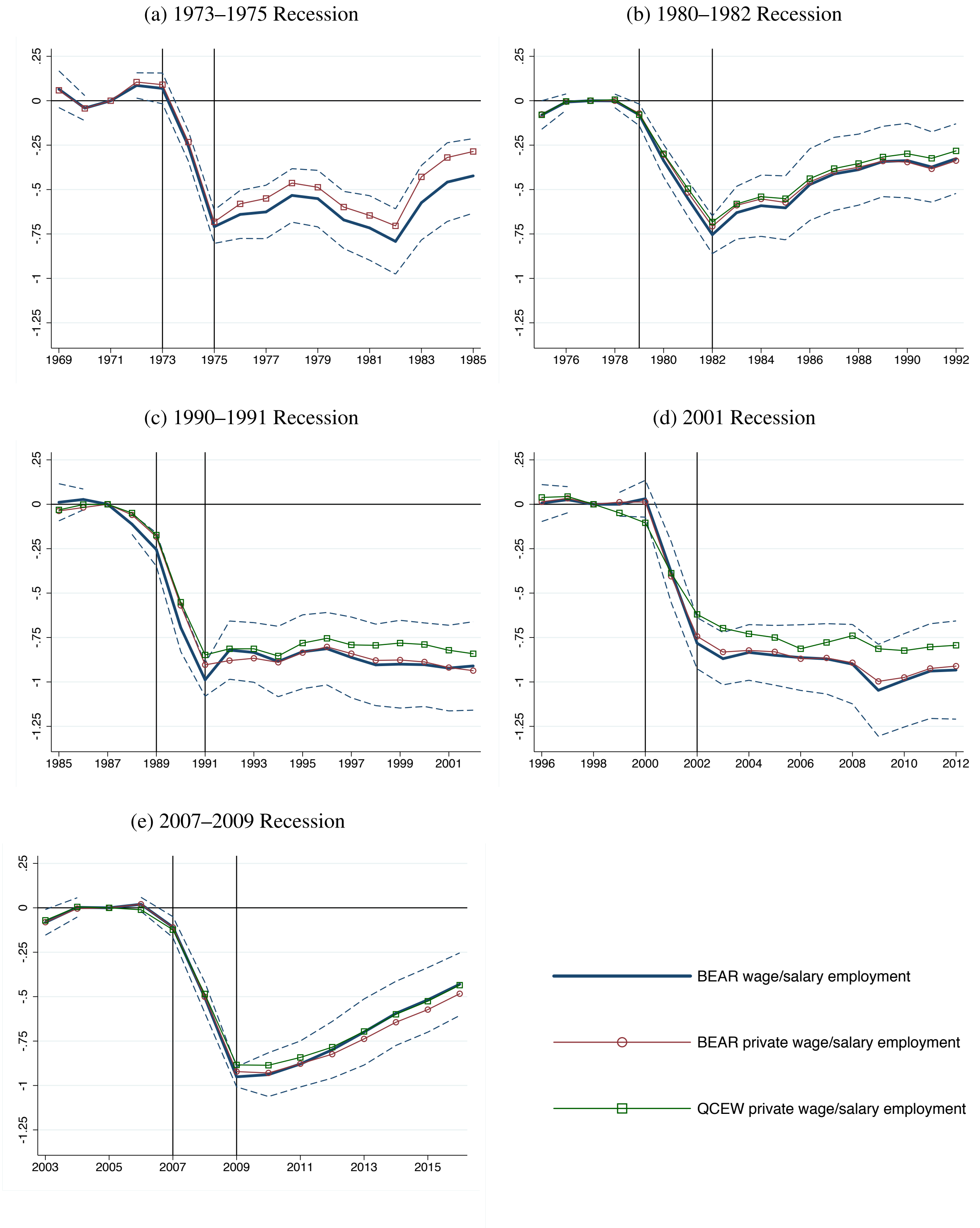
(e) 2007–2009 Recession





Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log population aged 15 and above, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary employment shock.
Sources: Authors' calculations using BEAR, QCEW, and SEER data.

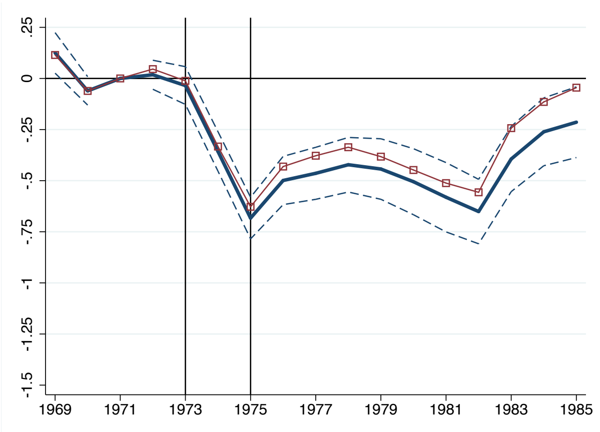
Figure A.7: Impacts of Metropolitan-Area Recessionary Shocks on Log Employment-to-Population Ratio, Robustness to Different Employment Shocks



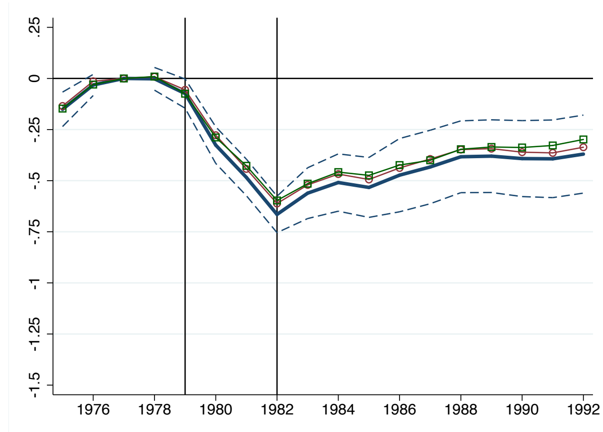
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population aged 15 and above, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary employment shock.
Sources: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.8: Impacts of Metropolitan-Area Recessionary Shocks on Log Real Earnings per Capita, Robustness to Different Employment Shocks

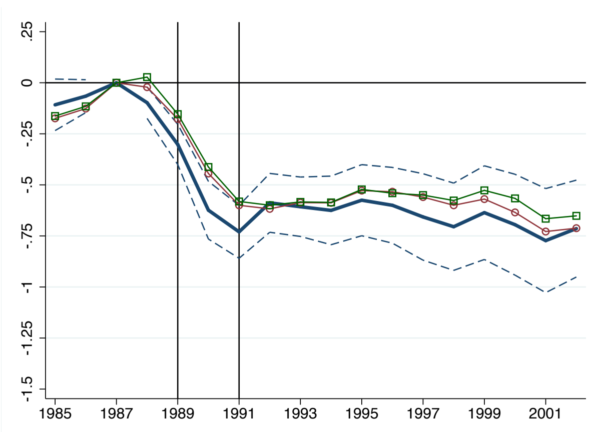
(a) 1973–1975 Recession



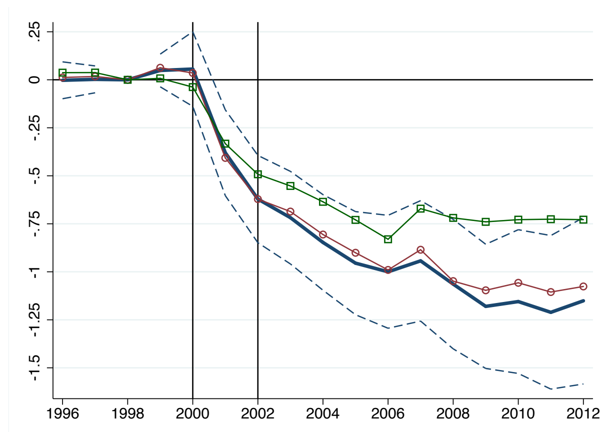
(b) 1980–1982 Recession



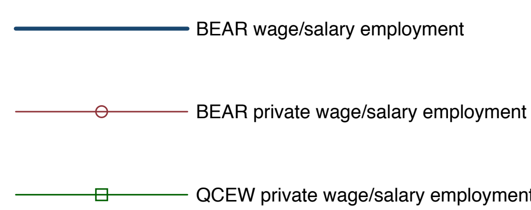
(c) 1990–1991 Recession



(d) 2001 Recession



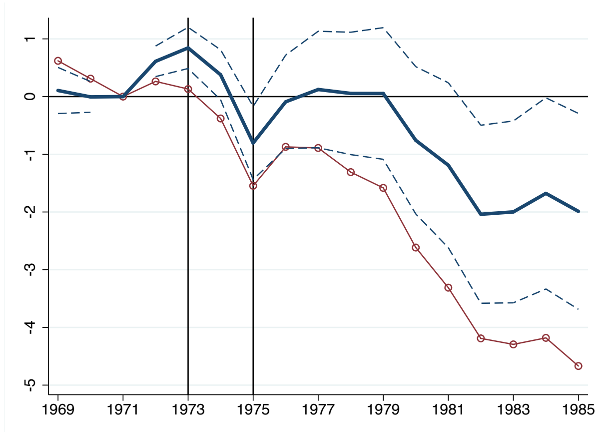
(e) 2007–2009 Recession



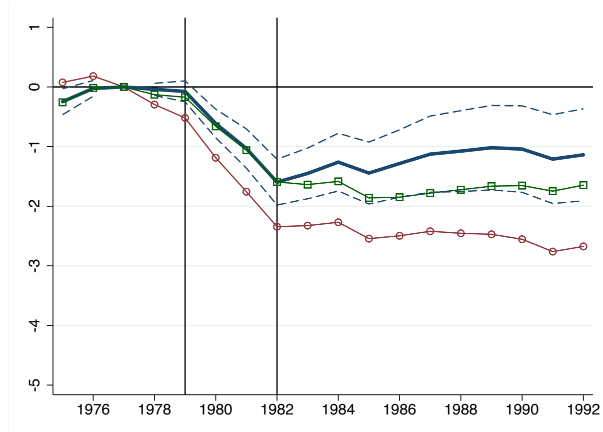
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+), and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary employment shock. Sources: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.9: Impacts of Metropolitan-Area Bartik Shocks on Log Employment

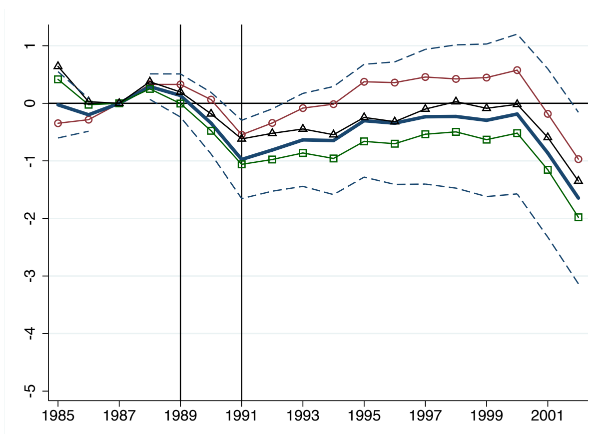
(a) 1973–1975 Recession



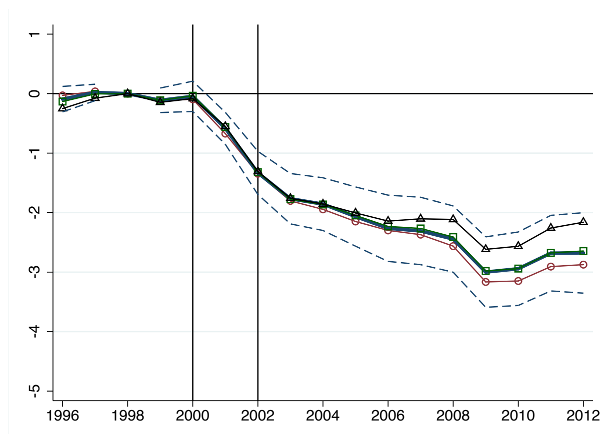
(b) 1980–1982 Recession



(c) 1990–1991 Recession

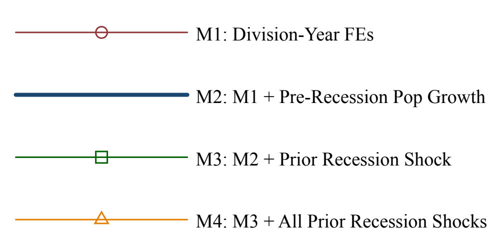


(d) 2001 Recession



(e) 2007–2009 Recession

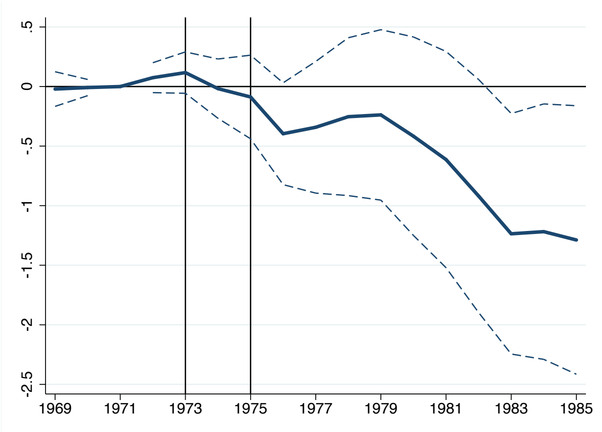
(f)



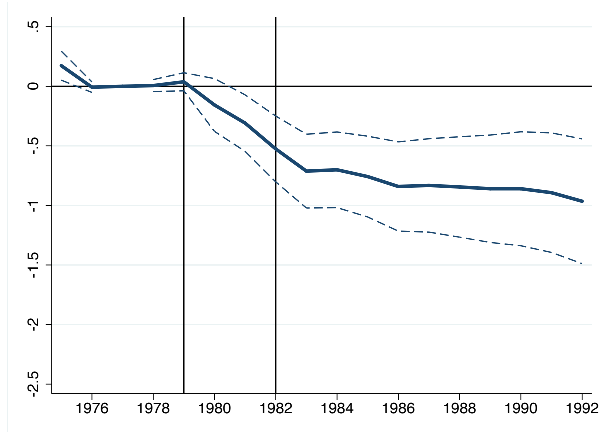
Notes: Table reports estimates of Equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the predicted log employment change as in Bartik (1991). Specifications are indicated by the legend. See notes to Figure 4.
Sources: Authors' calculations using BEAR, CBP, and QCEW data.

Figure A.10: Impacts of Metropolitan-Area Bartik Shocks on Log Population

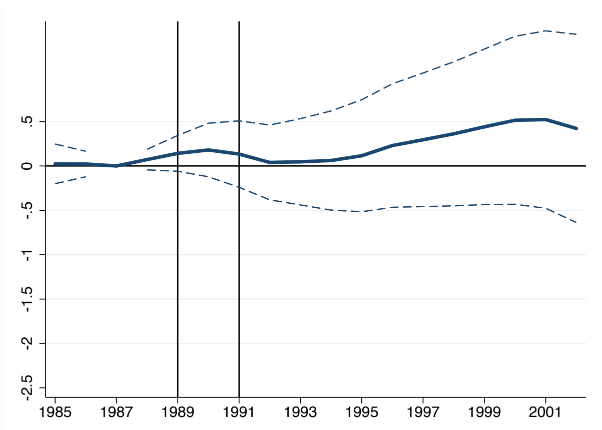
(a) 1973–1975 Recession



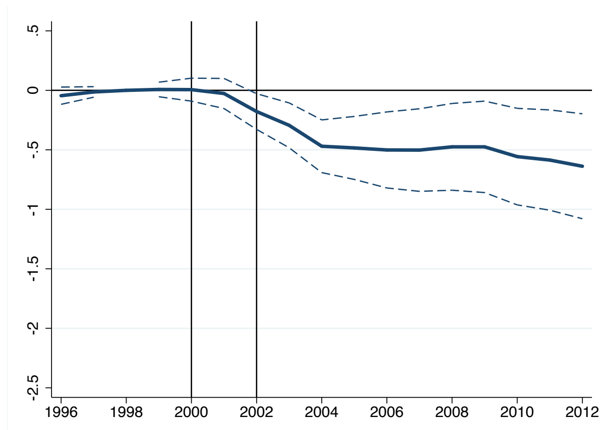
(b) 1980–1982 Recession



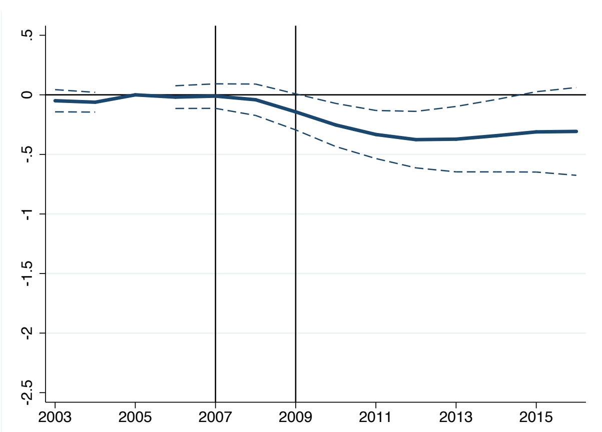
(c) 1990–1991 Recession



(d) 2001 Recession



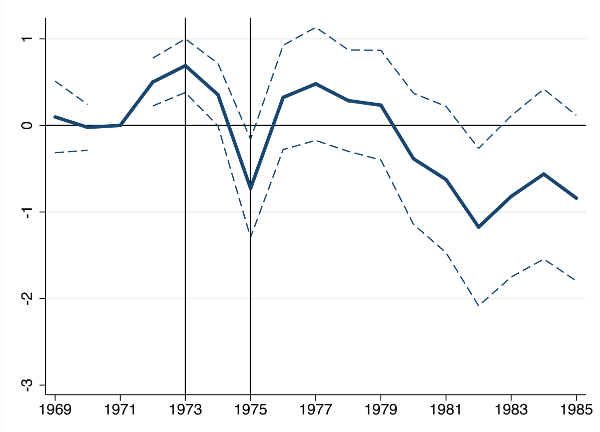
(e) 2007–2009 Recession



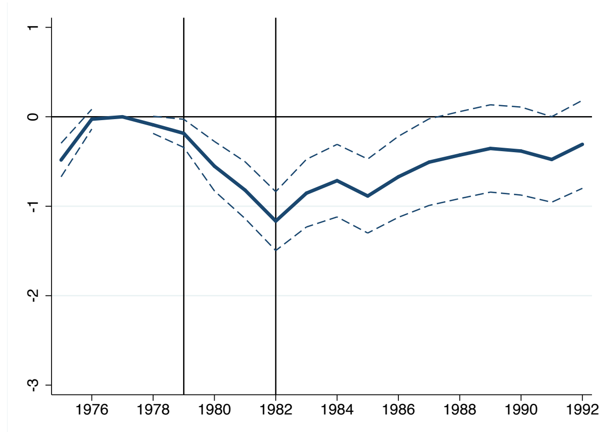
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log population aged 15 and above. See notes to Figure A.9.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.11: Impacts of Metropolitan-Area Bartik Shocks on Log Employment-to-Population Ratio

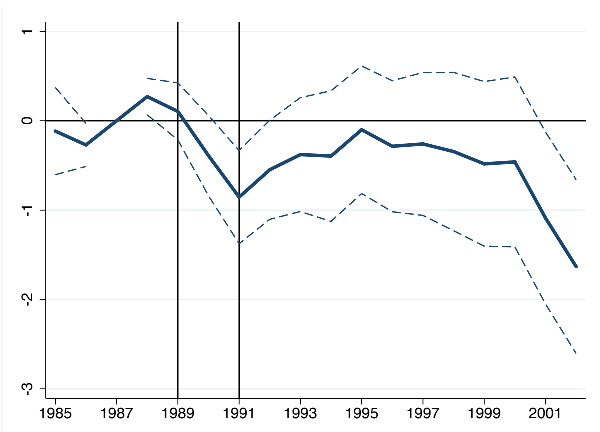
(a) 1973–1975 Recession



(b) 1980–1982 Recession

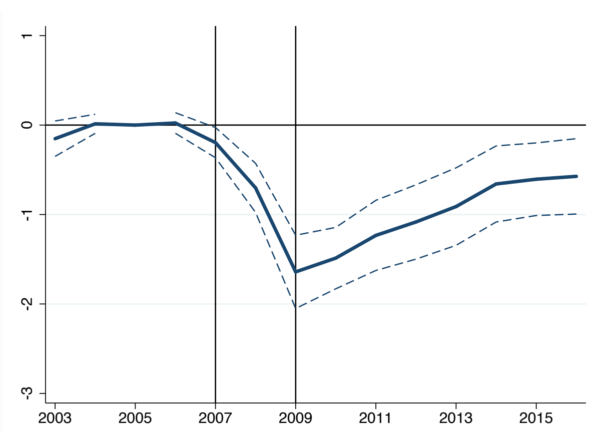


(c) 1990–1991 Recession



(d) 2001 Recession

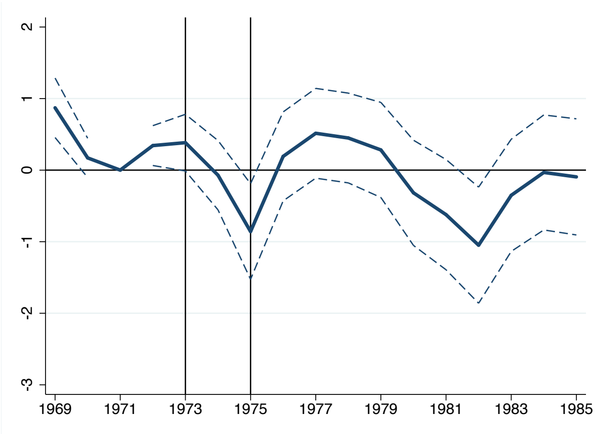
(e) 2007–2009 Recession



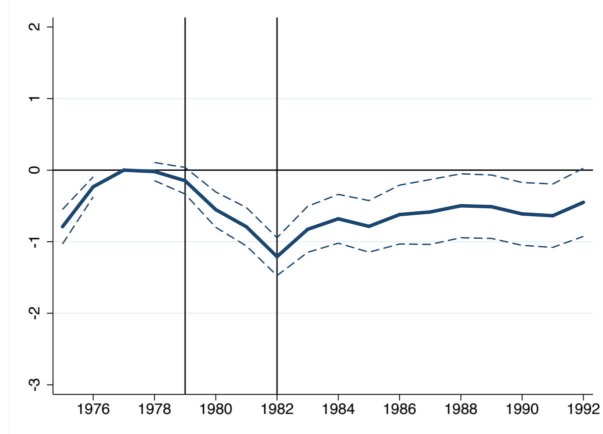
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population aged 15 and above. See notes to Figure A.9.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.12: Impacts of Metropolitan-Area Bartik Shocks on Log Real Earnings per Capita

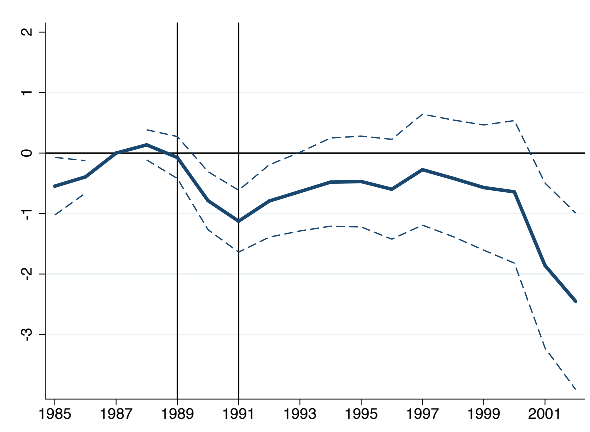
(a) 1973–1975 Recession



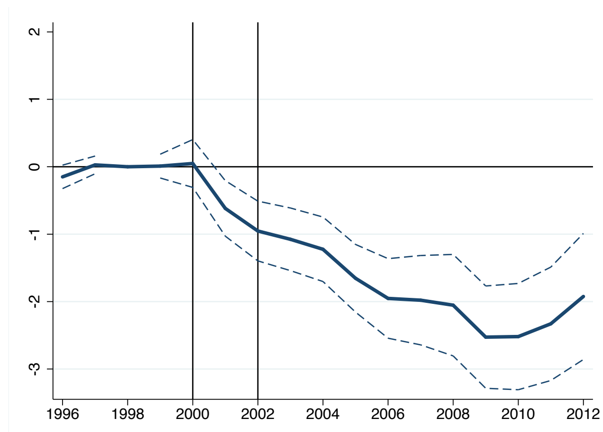
(b) 1980–1982 Recession



(c) 1990–1991 Recession



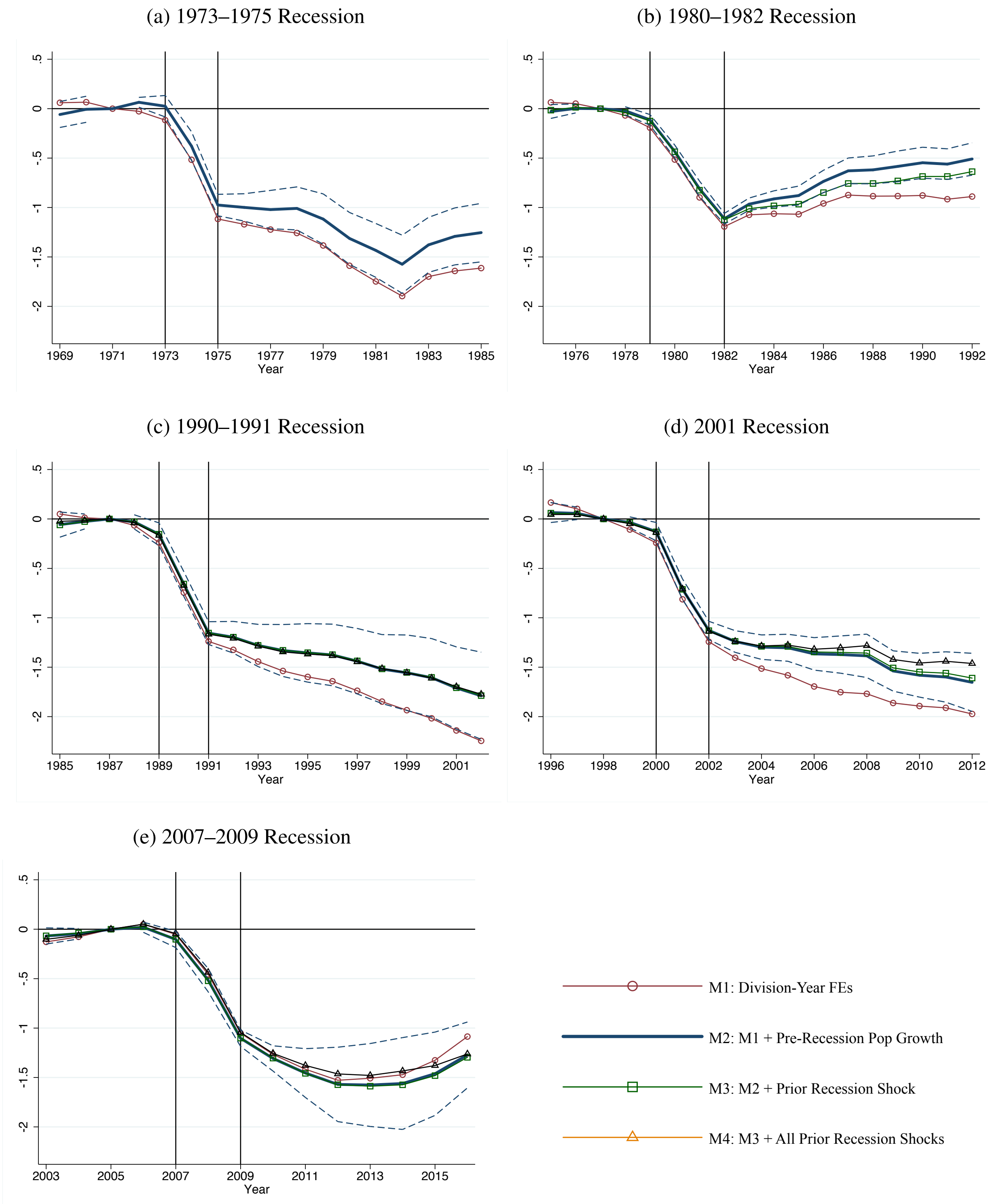
(d) 2001 Recession



(e) 2007–2009 Recession

Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log real earnings per capita (ages 15+). See notes to Figure A.9.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

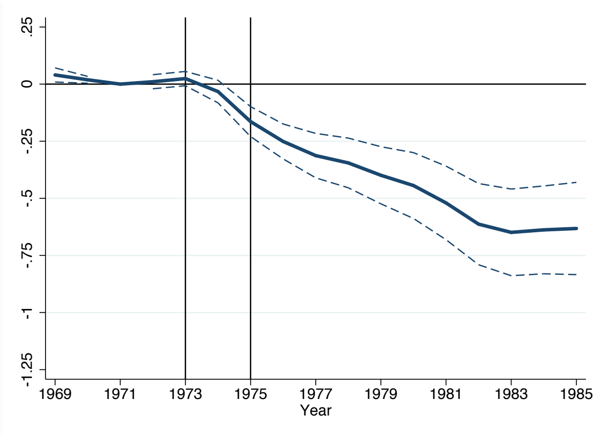
Figure A.13: Impacts of Commuting Zone Recessionary Shocks on Log Employment



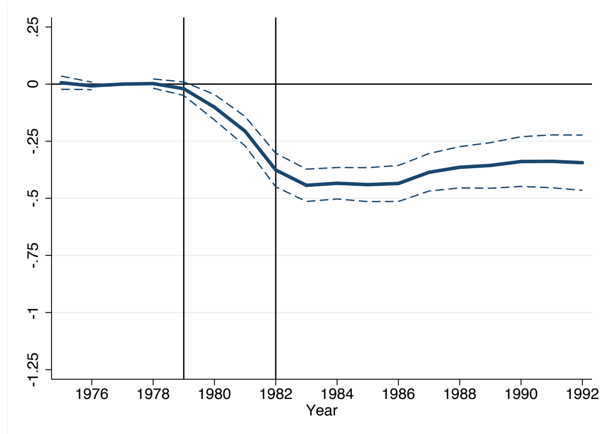
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data. There are 691 CZs in the sample. Standard errors are clustered by commuting zone. See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

Figure A.14: Impacts of Commuting Zone Recessionary Shocks on Log Population Ages 15+

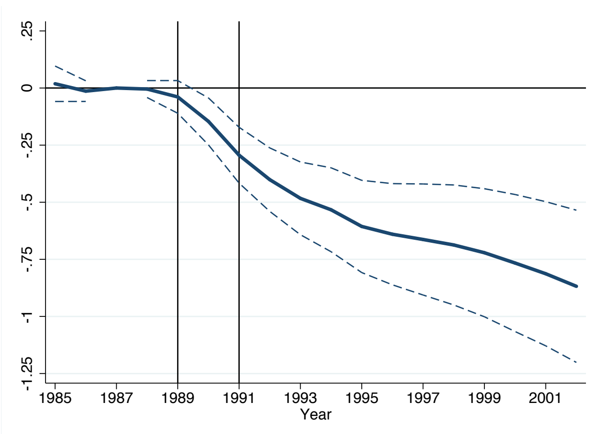
(a) 1973–1975 Recession



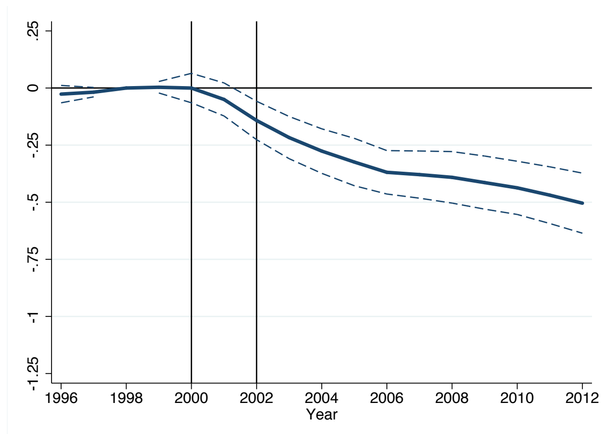
(b) 1980–1982 Recession



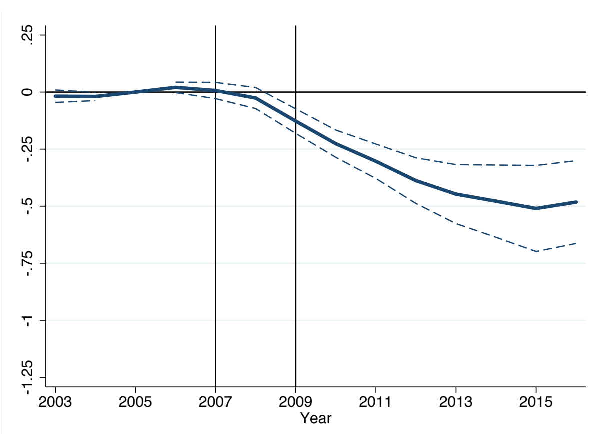
(c) 1990–1991 Recession



(d) 2001 Recession

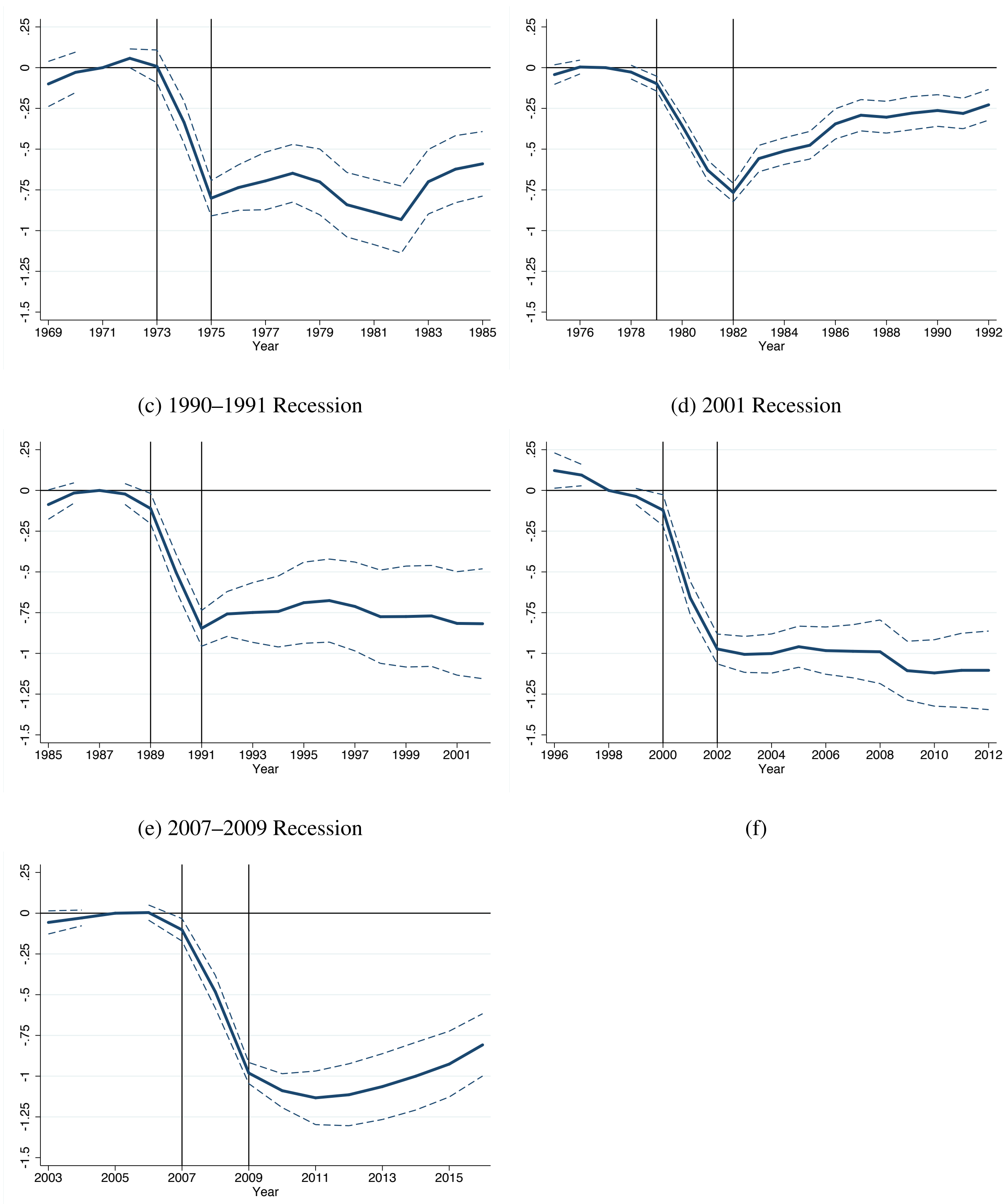


(e) 2007–2009 Recession



Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log population aged 15 and above. See notes to Figure A.13.
Sources: Authors' calculations using BEAR, SEER, and QCEW data.

Figure A.15: Impacts of Commuting Zone Recessionary Shocks on Log Employment-to-Population Ratio



(a) 1973–1975 Recession

(b) 1980–1982 Recession

(c) 1990–1991 Recession

(d) 2001 Recession

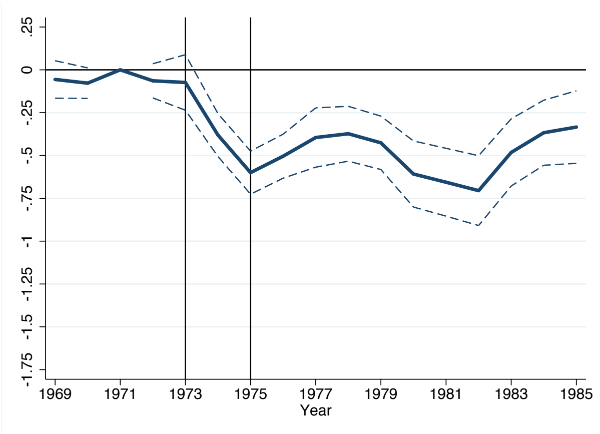
(e) 2007–2009 Recession

(f)

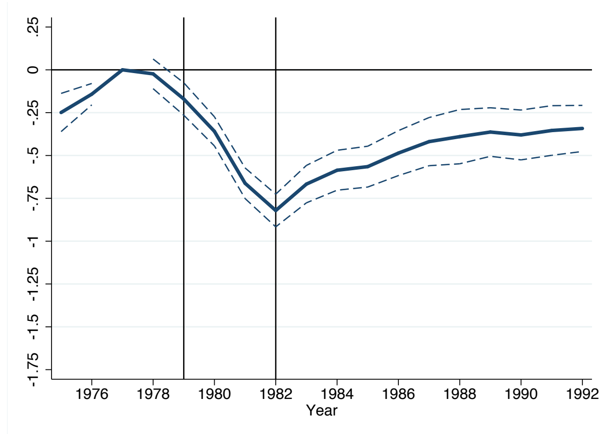
Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population aged 15 and above. See notes to Figure A.13.
Sources: Authors' calculations using BEAR and SEER data.

Figure A.16: Impacts of Commuting Zone Recessionary Shocks on Log Real Earnings per Capita

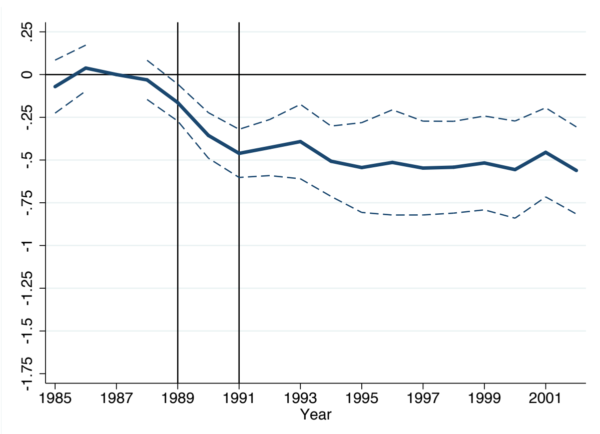
(a) 1973–1975 Recession



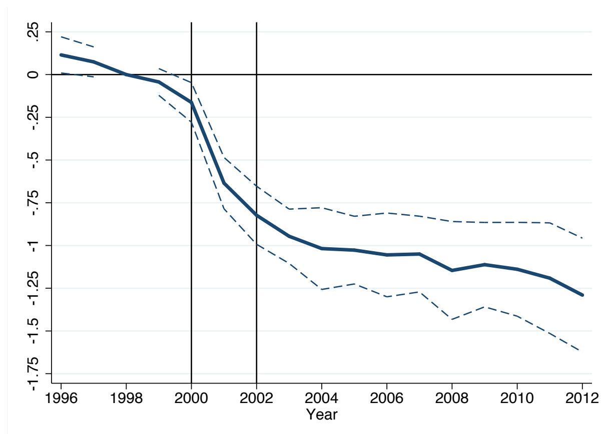
(b) 1980–1982 Recession



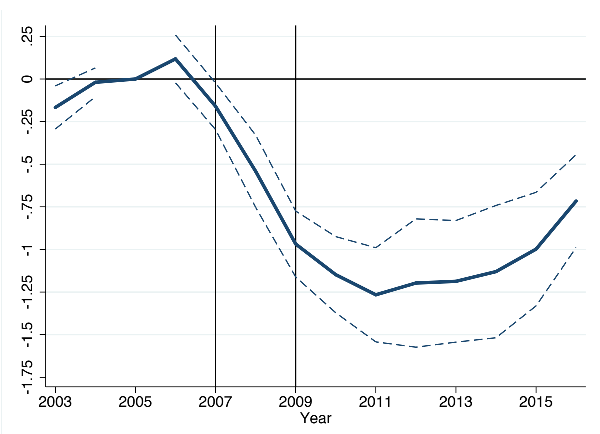
(c) 1990–1991 Recession



(d) 2001 Recession



(e) 2007–2009 Recession



Notes: Figure reports estimates of Equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure A.13.
Sources: Authors' calculations using BEAR and SEER data.