Recessions and Local Labor Market Hysteresis*

Brad Hershbein
W.E. Upjohn Institute for Employment Research
hershbein@upjohn.org

Bryan A. Stuart
George Washington University
bastuart@gwu.edu

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Abstract

This paper studies the effects of recessions on U.S. local labor markets. Using variation in sudden employment losses during each recession between 1973 and 2009, we find that areas more affected by recessions experience highly persistent declines in employment and population. Most importantly, and contrary to prior work, every recession we study generates local labor market hysteresis in the form of persistent decreases in the employment-population ratio and earnings per capita. Our results imply that recessions induce persistent reallocation of employment across space, and that limited population responses result in longer-lasting disruptions to local labor markets than previously thought.

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*Hershbein: hershbein@upjohn.org; Stuart: bastuart@gwu.edu, 2115 G Street NW, Room 302, Washington, DC 20052. We gratefully acknowledge funding from the 2018–2019 DOL Scholars Program (Contract # DOL-OPS-15-C-0060). We thank Steve Yesiltepe for excellent research assistance. For helpful feedback and conversations, we thank Tim Bartik, Leah Boustan, Katherine Eriksson, Andy Garin, Harry Holzer, Larry Katz, Lutz Kilian, Fabian Lange, Matt Notowidigdo, Chris Severen, Jay Shambaugh, Tara Sinclair, Anthony Yezer, and numerous seminar audiences.

1 Introduction

Recessions receive enormous attention from researchers, policymakers, and the public. Most of this attention focuses on short-run changes in nationwide measures like the unemployment rate and 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. The value of understanding how recessions shape local areas is underscored by growing evidence that place-specific factors shape intergenerational mobility (Chetty and Hendren, 2018*a*,*b*), health (Finkelstein, Gentzkow and Williams, 2019), voting (Charles and Stephens, 2013; Autor et al., 2016), and many other outcomes.

This paper studies the impacts of every U.S. recession between 1973 and 2009 on local labor markets.[1] Specifically, we study how employment, population, and earnings evolve in local areas (metropolitan areas and commuting zones) where national recessions are more versus less severe. We draw upon multiple data sources, including those from the Bureau of Economic Analysis and 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 sudden employment changes that arise during recessions, while controlling for secular trends in population growth. This empirical strategy allows us to examine whether recessions have temporary or persistent impacts on local labor markets.

We find that declines in employment which emerge during recessions are extremely persistent. 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 7–9 years after the recession trough. The sudden decreases in employment that occur during recessions are not associated with differential pre-trends beforehand. These results suggest that areas which suffer a more severe recession experience a *persistent* relative decrease in labor demand.

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

The consequences of these local employment declines depend on the extent of population adjustment. We find evidence of population declines that begin during recessions and continue to grow for several years after the recession trough. The post-recession decrease in population is persistent, but smaller than the decrease in employment. Due to this limited population response, each recession leads to local labor market hysteresis in the form of persistently lower employment rates. On average, a 5 percent decrease in employment during a recession leads to a 3.2 percent (2 percentage point) decrease in the employment-population ratio. This effect accounts for about 55 percent of the decline in local area employment 7–9 years after trough, with the decline in population explaining the remaining 45 percent. Moreover, local labor market hysteresis persists for several decades. As of 2017, we continue to find reduced local employment rates for every recession we study. Each recession also leads to local hysteresis through lasting decreases in earnings per worker. On average, a 5 percent decrease in employment during a recession leads to a 1.7 percent decrease in earnings per worker 7–9 years after the recession trough. Over the same horizon, the decline in earnings per capita—a summary measure of hysteresis that reflects decreases in the employment-population ratio and earnings per worker—is 5.2 percent.

The key mechanism underlying local labor market hysteresis is incomplete adjustment of population after reductions in employment. Additional results shed further light on the operative mechanisms. We examine the nature of the shift in labor demand by examining which industries experience declines in employment. 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 proportion across all industries—including services, trade, and government—suggesting a persistent, broad-based decline in labor demand. To study the nature of the labor supply response, we use IRS data to examine in- and out-migration after the 2001 and 2007–2009 recessions. We find that the population decline stems entirely from reduced in-migration to severely hit areas, with out-migration actually falling after these recessions. To study the nature of the impacts on workers, we use individual-level data from the decennial Census and American Community Survey. Recessions decrease earnings throughout the distribution, but effects tend to be more severe at the bottom and middle. On average, about 80 percent of the medium-term decline in annual earnings—for those who remain employed—arises from a reduction in hourly wages. In contrast, decreases in work attachment at the intensive margin explain little of the decline in average earnings for this group.

A possible concern is that our estimates simply reflect the effects of secular changes in the economy, such as the decline in manufacturing. Several factors point against this interpretation. Our regressions use division-by-year fixed effects to absorb broader secular changes as well as metro-level pre-recession population growth to adjust for pre-existing trends at the local level. We find little evidence of pre-trends, and instead see declines in economic activity that emerge during recessions. To address the role of secular economic changes more directly, we estimate regressions that control for interactions between year indicators and the pre-recession share of employment in each sector. These regressions could over-control for the effects of recessions, which partly arise from local areas' exposure to industry-level employment shifts. However, we continue to find persistent declines in the employment-population ratio in these specifications, which indicates that local labor market hysteresis does not simply reflect secular declines in manufacturing or other sectors.

One potential explanation for local labor market hysteresis is a change in the composition of residents following a recession, especially in light of changed migration patterns. To examine the importance of this mechanism, we use individual-level Census data and residualize earnings with respect to education, age, race/ethnicity, and sex. The composition-adjusted impacts are about 75 percent as large as our baseline estimates on average, implying that hysteresis is not driven by changes in worker characteristics correlated with these variables. Instead, local labor market hysteresis appears to stem mainly from lasting impacts on individuals, consistent with evidence on the effects of job displacement (e.g., Jacobson, LaLonde and Sullivan, 1993; Lachowska, Mas and Woodbury, 2020).

Our results differ from a series of influential papers that suggest that most recessions do not lead to local labor market hysteresis. In particular, Blanchard and Katz (1992) estimate vector autoregressions (VARs) and find that the unemployment rate, labor force participation rate, and wages return to trend within ten years of a decline in local labor demand. Dao, Furceri and Loungani (2017) estimate a similar degree of medium-run convergence in more recent data.[2] Using empirically-relevant Monte Carlo simulations, we show that finite sample bias arising from a limited number of time series observations leads VARs estimated in prior work to incorrectly imply convergence in the presence of hysteresis. This finite sample bias, which would be of first-order importance even if researchers had access to 100 years of data, explains the difference in our results from those based on the Blanchard and Katz (1992) VAR model. Evidence of finite sample bias also helps clarify a longstanding debate initiated by the landmark studies of Bartik (1991) and Blanchard and Katz (1992) on whether shifts in labor demand lead to local labor market hysteresis.

Thus, the key contribution of this paper is new evidence on how recessions have affected local labor markets over the past 50 years. Our results show that recessions not only generate lasting shifts in the spatial distribution of employment and population, but that relative reductions in employment rates and earnings also last longer than previously thought. Moreover, the impacts of recessions on local labor markets have changed little over the past five decades. This similarity is remarkable, given the different macroeconomic drivers of the recessions and 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, have lasting effects on local areas. These results underscore the extent to which local hysteresis is a general feature of the U.S. economy.

Our work complements Yagan (2019), who uses tax data to provide evidence of individual-level hysteresis: people living in areas severely affected by the Great Recession experienced enduring employment and earnings losses. We differ from Yagan (2019) by focusing on how recessions affect local labor markets, as opposed to individuals, and by examining a larger number of re-

[2]Dao, Furceri and Loungani (2017) use a different source of identification and find that population is less responsive in the short run. Yagan (2019) calculates employment growth forecast errors from the Blanchard and Katz (1992) VAR and divides states into those with larger or smaller shocks. He finds rapid recovery following the 1980–1982 and 1990–1991 recessions in line with Blanchard and Katz (1992), but slower recovery from the Great Recession.

cessions. Our results suggest that the individual-level hysteresis documented by Yagan (2019) following the Great Recession may be the norm, and not the exception. Moreover, we show that this hysteresis is associated with persistent spatial reallocation of earnings, employment, and—to a lesser degree—people. Monras (2020) provides empirical evidence that reduced in-migration accounts for essentially all of the population decline in areas hit harder by the Great Recession and develops a structural model to rationalize this fact. Our findings on in-migration are qualitatively similar. We differ from Monras (2020) in our empirical strategy and examination of more recessions and more outcomes.

Our work also complements several other studies that examine how local labor demand shifts, such as a change in manufacturing jobs, affect earnings, employment, and population (e.g., Bound and Holzer, 2000; Freedman, 2017; Amior and Manning, 2018; Beaudry, Green and Sand, 2018; Garin, 2019; Gathmann, Helm and Schönberg, 2020; Notowidigdo, 2020). We provide new evidence by combining annual data—which directly reveal local labor market dynamics—and a research design that studies local labor demand shifts over a 50-year period.[3] Additional evidence is particularly valuable because of the disagreement in the literature over whether shifts in local labor demand have persistent effects on wages and employment, and 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 per-capita earnings at the county level; our analysis goes considerably further, by examining a larger range of outcomes, other levels of geography, and additional business cycles.

We emphasize that our finding of local labor market hysteresis is not inconsistent with aggregate economic recovery (e.g., Dupraz, Nakamura and Steinsson, 2020; Hall and Kudlyak, 2020). The cross-sectional identifying variation we use permits an implicit counterfactual of how a local

[3]Amior and Manning (2018) also show that population adjusts incompletely to local labor demand shifts, leading to persistent gaps in employment rates. We differ in our use of sudden shifts in local labor demand that arise during recessions and our use of annual data, as compared to their analysis of predicted employment changes based on industrial structure (Bartik, 1991) using decadal data. A key benefit of our empirical setting and flexible regression models is that we can provide direct evidence that the severity of different recessions is not strongly correlated over time—it is not the case that on average the same areas experience particularly large employment losses during each recession—even though the *effects* of recessions are persistent.

labor market that experienced a severe employment loss during a recession would have evolved had it experienced a less severe employment loss.[4] A persistent relative decline does not imply that an area fails to recover in an absolute sense, but rather that a gap remains between that area and one that experienced a less severe recession. These relative impacts most directly shed light on the distributional consequences of recessions and the efficiency costs associated with incomplete local labor market adjustments.

2 Conceptual Framework

To guide our empirical analysis, we offer a stylized framework to discuss how recessions might affect local labor markets. The simple framework highlights the fundamental demand and supply adjustments that govern the evolution of local economic activity, as emphasized by Blanchard and Katz (1992). We do not develop a fully micro-founded model because our data do not allow us to isolate adjustments made by specific workers and firms. Instead, the basic unit of analysis is a local labor market. We thus make several simplifying assumptions to focus on the most relevant channels. All variables are expressed in logarithms.

2.1 Set-Up

We begin with a simple labor demand curve:

$$l_{i,t}^d = a_{i,t} - \eta^d w_{i,t}, \tag{1}$$

where $l_{i,t}^d$ is labor demanded by employers in local labor market i at time t, $a_{i,t}$ is a labor demand shifter, $\eta^d \geq 0$ is the absolute value of the labor demand elasticity, and $w_{i,t}$ is the wage. A downward sloping labor demand curve naturally arises in a situation where firms pay workers their marginal product and the marginal product of labor is diminishing or firms face downward sloping demand curves for their output.

[4]Other papers studying local labor markets also identify relative effects (e.g., Blanchard and Katz, 1992; Autor, Dorn and Hanson, 2013; Amior and Manning, 2018).

Labor supply is

$$l_{i,t}^{s} = z_{i,t} + \eta^{s} w_{i,t}, \tag{2}$$

where $z_{i,t}$ is a labor supply shifter and $\eta^s \geq 0$ is the labor supply elasticity. The available workforce in an area might respond to higher wages through two channels. First, workers might migrate to an area in pursuit of higher wages. Second, conditional on living in an area, individuals might enter the labor force when wages are higher (e.g., because individuals have heterogeneous reservation wages). The labor supply elasticity η^s captures both responses. The labor supply shifter reflects changes in non-wage amenities that affect migration (e.g., quality of life) and other factors that affect labor supply (e.g., transit or certain taxes).

Finally, we allow population to depend on a population shifter and the wage:

$$n_{i,t} = q_{i,t} + \eta^{n} w_{i,t}, \tag{3}$$

where $\eta^n \geq 0$ is the elasticity of population with respect to the wage. We include separate equations for labor supply and population for several reasons. First, different factors could attract workers versus the general population so that $z_{i,t}$ need not be identical to $q_{i,t}$.[5] Second, the general population might be attracted by higher wages (i.e., η^n might be positive), possibly because higher wages either lead to or are associated with natural and endogenous amenities. It is likely that the population response to higher wages is smaller, in proportional terms, than the labor supply response, because workers receive both direct and indirect benefits from higher wages.

We consider an equilibrium in which wages adjust in each period so that the labor market clears. The exogenous variables in this model are the labor demand shifter $a_{i,t}$, the labor supply shifter $z_{i,t}$, and the population shifter $q_{i,t}$. It is straightforward to describe employment, population, the employment-population ratio, and wages as functions of these exogenous variables.

[5]For example, better weather could attract more workers, which would mechanically increase population. However, better weather also could attract residents to an area independently of any effect on the available workforce.

2.2 Short-Run Responses to a Labor Demand Shift

Our starting point is that labor demand falls during recessions. This decrease could stem from many 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. Labor markets that experience a more severe recession have a larger decrease in $a_{i,t}$.

We begin by considering responses to a change in the labor demand shifter. The response of employment is:

$$\Delta l_i = \frac{\eta^s}{\eta^d + \eta^s} \Delta a_i, \tag{4}$$

where Δl_i is the change in log employment in local labor market i over some time horizon and Δa_i is the change in the demand shifter over the same time period. If the supply of labor is not perfectly inelastic, so that $\eta^s > 0$, local employment will fall during the recession.

The response of population is:

$$\Delta n_i = \frac{\eta^n}{\eta^d + \eta^s} \Delta a_i. \tag{5}$$

As seen by comparing equations (4) and (5), employment will decrease by more than population in response to a negative labor demand shift if $\eta^s > \eta^n$. Because workers face both direct and indirect effects of wage changes—for example, a lower wage could lead to reduced earnings and worse amenities—this situation seems likely.

The response of the log employment-population ratio is:

$$\Delta l_i - \Delta n_i = \frac{\eta^s - \eta^n}{\eta^d + \eta^s} \Delta a_i. \tag{6}$$

If employment is more responsive to Δa_i than is population, then the employment-population ratio will fall during a recession. Finally, the response of wages is:

$$\Delta w_i = \frac{1}{\eta^d + \eta^s} \Delta a_i. \tag{7}$$

In this simple framework, without frictions to wage adjustment, a recession can affect wages even if the employment-population ratio does not change. Consequently, examining both outcomes is valuable.

In sum, a recession-induced decrease in the labor demand shifter $a_{i,t}$ will tend to decrease employment, population, and wages. If employment falls by more than population, which will naturally happen if recessions lower the value of working in a place by more than the value of living in a place, the employment-population ratio will fall, too. The magnitude of the responses depends on the size of the shift in labor demand, as well as the elasticities of labor demand, labor supply, and population.

2.3 Longer-Run Responses to a Labor Demand Shift

This framework is static, which simplifies the analysis greatly. Nonetheless, we can consider longer-run effects of recessions in two ways. First, behavior might be more elastic in the longer run. Most notably, the elasticity of population with respect to the wage, η^n, might increase as longer time horizons are considered.[6] Second, we can study dynamics by considering the time path of the demand shifter, $a_{i,t}$. If recessions lead to only temporary reductions in the labor demand shifter, Δa_i would return to zero at longer time horizons. On the other hand, the decline in local labor demand could persist, possibly because employers change their production process (Jaimovich and Siu, 2015; Hershbein and Kahn, 2018) or shut down (Foster, Grim and Haltiwanger, 2016). This would lead to a persistent fall in Δa_i in local labor markets that experience a severe recession.

The longer-run comparative statics depend critically on whether the decrease in the labor demand shifter is temporary or permanent. If the decline in $a_{i,t}$ is temporary, then equations (4)–(7)

[6]For example, moving costs or idiosyncratic preferences for locations may be less relevant over a longer horizon (e.g., Kennan and Walker, 2011), or expectations may adjust gradually over time in response to shifts in labor demand.

imply that all variables will return to their pre-recession level. This pattern would arise if firms temporarily laid off workers or reduced their hours, and individuals did not move across labor markets in the short run. At the other extreme—where the decline in $a_{i,t}$ is permanent—the framework could imply lasting decreases in employment, population, the employment-population ratio, and wages. More elastic population responses over time would lead to greater reductions in employment and population, and smaller decreases in the employment-population ratio and wages. A noteworthy special case that is commonly used in spatial equilibrium models in the spirit of Rosen (1979) and Roback (1982) is where all individuals work (i.e., $l_{i,t} = n_{i,t}$) and population and labor supply are each perfectly elastic (i.e., $\eta^s = \eta^n = \infty$). In this situation, a permanent decline in $a_{i,t}$ leads to a permanent decline in employment and population, but no lasting change in the employment-population ratio or wages.[7]

We define *local labor market hysteresis* as the scenario in which a recession-induced decrease in labor demand leads to a lasting reduction in the employment-population ratio or wages. In this simple framework, two necessary and sufficient conditions for such hysteresis are (1) a persistent decrease in the labor demand shifter and (2) less-than-perfectly-elastic labor supply responses.

The framework is flexible enough to permit several extensions, such as worker heterogeneity, housing markets, or firm spillovers, and we discuss these when interpreting our empirical results below.

[7]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).

3 Data and Empirical Strategy

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), Census County Business Patterns (CBP), and Quarterly Census of Employment and Wages (QCEW).[8] BEAR and CBP data are available starting in 1969, while QCEW data are available from 1975-onward. BEAR data also contain aggregate earnings.[9] 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.[10] Finally, we use tabulations and microdata from the decennial Census and the American Community Survey (ACS) to examine the earnings distribution and composition changes.[11]

With the exceptions of the decennial Census and ACS microdata, all of the data sets are available at the county level. The Census and ACS are available at the Public Use Microdata Area (PUMA) level, 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 re-

[8]Because employment counts are often suppressed for small counties and industries in CBP data, we adopt the imputation procedure of Holmes and Stevens (2002) when necessary. Details are in the Data Appendix. Results from this approach agree closely with WholeData, which uses a linear programming algorithm to recover suppressed employment estimates (Bartik et al., 2019).

[9]More specifically, BEAR data contain earnings by both place of residence and place of work. Since wage and salary employment is available only by place of work, we use the corresponding earnings measure and define earnings to be wages, salaries, and supplements (benefits). As discussed more below, our results are similar when measuring earnings by place of work or place of residence.

[10]SOI data are available starting in the 1990s. Although they capture moves only for tax filers, SOI data 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, Forthcoming). We use a version of these data compiled by Janine Billadello of Baruch College's Geospatial Data Lab (Billadello, 2018).

[11]We use versions of these tabular 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.

cessions at multiple levels of geography: metropolitan area and commuting zone.[12] 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).[13] Both types of areas are composed of counties, so it is straightforward to map our county-level data into metro areas or commuting zones. A slight complication is that definitions of metropolitan areas and commuting zones change 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 main results are robust to using commuting zones, which unlike metro areas cover the entire United States.[14]

3.2 Empirical Strategy

Our empirical strategy relies on cross-sectional variation in sudden employment changes that occur during nationwide recessions. We use this variation to estimate the impacts of a decline in labor demand on local labor market outcomes, separately for each recession.

One natural approach is to estimate the event study regression

$$y_{i,t} = s_i \delta_t + x_{i,t}\beta + \mu_i + \varepsilon_{i,t}, \tag{8}$$

where $y_{i,t}$ is a measure of local economic activity in location i and year t; s_i is the severity of the recession, measured as 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

[12] We do not examine counties because these are often too small to constitute local labor markets, our area of focus.

[13] 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 (Tolbert and Sizer, 1996).

[14] Metro areas, consistently defined, cover 80–90 percent of people and jobs throughout our sample, with this share growing over time.

describes the relationship between the change in employment during the recession 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.[15] This specification allows the sudden decline in employment during the recession to have impacts which vary flexibly across years, transparently showing both pre-trends and dynamic effects.

An important issue with estimating equation (8) in our setting is that log employment is both an outcome of interest and used to construct the key independent variable, s_i. This can introduce a mechanical correlation between $y_{i,t}$ and s_i, so that estimates of δ_t for all years are inconsistent.[16] Instead, we estimate

$$y_{i,t} = s_i\delta_t + x_{i,t}\beta + y_{i,t_0-2}\gamma_t + \varepsilon_{i,t}. \tag{9}$$

Equation (9) does not include location fixed effects, but instead controls for time-invariant cross-sectional differences using the dependent variable two years before the nationwide business cycle 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 (8), estimates of δ_t from equation (9) generally are consistent under the null hypothesis of a random walk process.

We measure local recession severity using annual employment data from BEAR.[17] We modify NBER recession peak and trough dates to account for our use of annual data. Specifically, we con-

[15] 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.

[16] To see this problem, consider normalizing $\delta_t = 0$ for the peak year, t_0. Equation (8) then can be rewritten

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

where $s_i \equiv -(y_{i,t_1} - y_{i,t_0})$ is -1 times the change in log employment from recession peak to trough. 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 still prefer equation (9) because it has better properties for any choice of normalization year and can be extended to control for a vector of lagged dependent variables.

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

struct s_i using the log employment change for each geography between 1973–1975, 1979–1982, 1989–1991, 2000–2002, and 2007–2009.[18] 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 measure recession severity, as coverage of the self-employed is incomplete and varies over time.

The specification in equation (9) captures the initial effect of the decline in labor demand, along with subsequent demand and supply responses. The key identifying assumption is that local recession severity, s_i, is exogenous to unobserved changes in local labor market outcomes, $\varepsilon_{i,t}$, conditional on the controls in the regression. In addition to controlling for time-invariant differences across local areas, we include several variables in $x_{i,t}$ to bolster the credibility of this assumption. 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 pre-recession population growth and year indicators to adjust for secular changes in population and demographics.[19] A key possible violation of our identifying assumption is the presence of pre-trends in local economic activity that are correlated with recession severity. Fortunately, estimates of δ_t for pre-recession years allow us to directly examine the presence of such pre-trends. We cluster our standard errors at the metro or commuting zone level to allow for arbitrary autocorrelation in the error term $\varepsilon_{i,t}$.

The parameter vector $\{\delta_t\}$ describes the time-varying relative effects of recessions on local labor markets. A negative value of δ_t in post-recession years implies that economic activity falls in areas that experience a more severe recession, relative to what would have happened if they experienced a less severe recession. 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.

[18] 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 to November 2001, and December 2007 to June 2009.

[19] We control for the log change in population age 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 pre-recession 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).

3.3 The Severity of Recessions Across Time and Space

Before moving to estimates of equation (9), 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 2017. 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 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.[20] The recessions vary in overall magnitude, from a 3 percent employment decline during the Great Recession to a 1 percent increase from 1989–1991, with the others falling 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 patterns of employment changes for other industries differ across recessions. The early 1990s downturn and the Great Recession were broad in scope, with most major industries experiencing 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 services. 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 or construction were more likely to suffer severe recessions, although the variation across recessions in other

[20]We use BEAR data rather than national Current Employment Statistics data to be consistent with our subsequent analysis, but the patterns are qualitatively similar.

industries implies that it is not necessarily the same areas being hit each time. Figure 2 shows the log employment change across metropolitan areas during each recession. 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, with several areas actually gaining employment in each episode.[21]

Figure 3 displays the frequency with which each area experienced a severe recession over the 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 metros, for example, experienced downturns worse than the median for all five recessions, while the Houston metro did so only in 2001. The distribution in severity frequency is roughly symmetric, with a similar number of metros 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, the serial correlation is positive, but moderate. 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 (i.e., after partialing out division fixed effects), and in Panel C the correlations after additionally controlling for pre-recession 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

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

an additional robustness check and show that these controls do not appreciably change the results.

Table 3 describes the characteristics of metro areas that experience a more versus 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 pre-recession 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 versus 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 reductions in labor demand. We estimate impacts directly on some of these variables, while also examining effects on worker composition to better understand related mechanisms.[22]

4 The Impacts of Recessions on Local Labor Markets

4.1 Employment

We begin with estimates of equation (9) for log employment in metro areas. Each panel in Figure 4 shows separate estimates for each recession. We include four years before the start of the recession to capture any pre-trends, 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 pre-recession, age group-specific population growth, as described above. Specification 3 (green squares) adds interactions between year indicators and the severity of the previous recession, which is possible for all but the mid-1970s recession. Finally, specification 4 (orange triangles) further includes interactions between year indicators and the severity of *all* previous recessions since the mid-1970s.

[22] We examined whether impacts of recessions were heterogeneous across these factors but found little evidence of such heterogeneity.

Overall, there is little evidence of pre-trends from specification 1. The exceptions are negative pre-trends 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. Controlling for pre-recession population growth eliminates these pre-trends. Since population growth is calculated over the decade before the recession, it is likely we eliminate secular trends (such as growing migration to certain metros in the South and West).[23]

The recession severity variable s_i is mechanically correlated with a large drop in log employment during the recession. 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 pre-trends.[24] Much more interesting is that after each recession, the decline in employment shows little to no recovery over the subsequent 10 years. 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 persistent decline in employment is not affected by whether we control for the severity of previous recessions. 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).

Figure 5 illustrates how the relative effects identified by equation (9) translate into aggregate outcomes. Panel A shows the event study coefficients for the 1980–1982 recession from our preferred specification, and Panel B displays the implied evolution of mean log employment in metro areas with a more versus less severe recession.[25] Employment grows after 1982 in both areas, regardless of recession severity. However, the *level* of employment is persistently lower in ar-

[23]It is also possible that we remove previous recession-induced changes to population growth. However, the correlations in Table 2 between each of the 1980–1982 and 2001 recessions and their immediately-preceding recessions are small. Since our objective is to estimate the impacts on a local area of a given recession, net of previous ones, whether the pre-trends are driven by secular or long-lasting cyclical effects is not paramount; it is sufficient that we can adequately control for them.

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

[25]We construct these conditional means using estimates of equation (9), holding all covariates besides recession severity at their mean value, and defining the gap between a more and less severe recession as a log employment change difference of -0.12 (equal to the difference in mean recession severity for areas with a log employment change below or above the median).

eas where the recession was more severe; this is the relative effect identified with cross-sectional variation.

Panel A of Table 4 summarizes the (preferred) specification 2 results seven to nine years after the recession trough.[26] The equally-weighted average of elasticities across recessions is -1.2, which indicates that a 10 percent decrease in employment during the recession leads to a 12 percent decrease in employment 7–9 years later. Because recession severity varies both across recessions and across areas within a given recession (Appendix Figure A.1), we also report standardized effects. On average, a one-standard deviation employment decline leads to a six percent decrease in employment. The standardized effects from the 2001 and 2007–2009 recessions are somewhat smaller, mainly because these recessions exhibit less variation across areas in severity.

The consequences of these decreases in employment depend on the degree of population response. We examine this next.

4.2 Population

In Figure 6 we present estimates of equation (9) where the dependent variable is the log of the total working-age population (15+). For brevity, we show only the results from specification 2, although the patterns are robust to specifications 3 and 4. We see no evidence of pre-trends and find negative, sustained impacts of the recession-induced decline in employment.[27] Population continues to decline long after each recession ends, implying that harder-hit areas remain on a lower population-growth trajectory. The elasticities at recession trough are modest, between -0.2 and -0.3, but then double or even close to triple over the next decade.

Panel B of Table 4 presents summaries of these results. On average, a 10 percent decrease in employment during the recession leads to a 5.5 percent decrease in population 7–9 years after the trough. The average effect of a one-standard deviation employment decrease is a 2.8 percent

[26]We generate the results in this table by restricting the pre-recession coefficients to be zero and pooling the coefficients in equation (9) for post-trough years 1–3, 4–6, 7–9, and (for the first four recessions) 10, in accord with the event study in Figure 4. The coefficient for post-trough years 7–9 summarizes the medium-term impacts while also increasing precision.

[27]The lack of pre-trends for the population results is not surprising, as we control for pre-recession population growth.

population decrease. Consistent with the previously-documented decline in migration (Molloy, Smith and Wozniak, 2014; Dao, Furceri and Loungani, 2017), we find that the effects of recessions on population have fallen over time.

4.3 Employment-to-Population Ratio

Population declines by less than employment in areas that experience more severe recessions. This implies that employment-to-population ratios fall after each recession. To examine this local labor market hysteresis more directly, we use the log of the ratio of employment to working-age population as the outcome in Figure 7. These ratios remain below their pre-recession peaks, even a decade after recession's end.

The elasticities at trough are about -0.75 for the 1973–1975, 1980–1982, and 2001 recessions and 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 declines, the employment-to-population trajectories generally show a slight recovery over time. The medium-term elasticity remains below -0.3 (and statistically different from 0) in each case, implying a 10 percent decrease in employment during a recession 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. Hence, recessions lead to local labor market hysteresis in the form of persistently depressed employment rates. Panel C of Table 4 reports summaries of these estimates seven to nine years post trough. The average elasticity is -0.7, with a one-standard deviation employment decline leading to a 3.1 percent decrease in the employment-population ratio on average.[28]

The estimates in Table 4 facilitate a simple decomposition of the post-recession decline in employment, namely that the effect of recession severity on log employment equals the effect on log population plus the effect on the log employment-population ratio.[29] On average, the decline in

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

[29]The estimates for log employment, log population, and log employment-to-population are approximately, but not exactly, additive due to 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

the log employment-population ratio accounts for about 55 percent of the decline in employment seven to nine years after recession trough, with the remaining 45 percent explained by the decline in population.[30]

4.4 Earnings per worker

As discussed in Section 2, the damaging effects of local recessions need not manifest only through employment losses, but can also affect wages (and, implicitly, other dimensions of job quality, including hours worked). We thus next examine annual earnings per worker, which encapsulates both the quantity and quality of employment.

Figure 8 shows estimates of equation (9) for the log of real earnings per worker, where we define earnings as wages and benefits and use the PCE deflator to adjust for inflation. There is again evidence of local labor market hysteresis, with per-worker earnings generally remaining below their pre-recession peak, especially after the 1980–1982, 2001, and 2007–2009 recessions. Panel D of Table 4 shows that the average medium-term elasticity is roughly -0.3, with a one-standard-deviation greater employment decline resulting in earnings per worker that are about 1.7 percent lower than they otherwise would have been nearly a decade later. We explore wages in greater detail below using individual-level from the Census/ACS.

4.5 Earnings per capita

So far, we have shown evidence of local labor market hysteresis in the forms of persistent declines in employment-population ratios and earnings per worker. We combine these variables into a single outcome, the log of earnings per capita, which summarizes both dimensions of local labor market hysteresis. Figure 9 shows evidence of persistent reductions in earnings per capita following each recession. The average medium-term elasticity in Panel E of Table 4 is roughly -1. A one-standard-deviation greater employment decline reduces earnings per capita by about 5 percent

of jobs to the number of working-age people; because of multiple job-holding, it is not strictly comparable to official employment-population ratios, which represent the share of the population that is employed.

[30]The equally-weighted average coefficient in Table 4 is -1.23 for log employment and -0.55 for log population, so the recession-induced decrease in population explains 45 percent ($=0.55/1.23$) of the decline in employment.

relative to what they otherwise would have been 7–9 years after the recession trough.[31,32]

The estimates in Panels C–E of Table 4 facilitate a decomposition of the relative importance of declines in the employment-population ratio and earnings per worker in explaining the overall decline in earnings per capita. In particular, the effects on log earnings per capita approximately equal the sum of the effects on the log employment-population ratio and the effects on log earnings per worker.[33] We find that about two-thirds of the decrease in earnings per capita is explained by the decline in the employment-population ratio, with the remaining one-third explained by the decrease in earnings per worker. Consequently, extensive margin employment adjustments are particularly important in driving local labor market hysteresis.[34]

4.6 Long-Run Results

Our main results focus on a ten-year post-recession window. There is evidence of a partial recovery in employment-population ratios for the 1973–1975, 1980–1982, and 2007–2009 recessions, so a natural question is whether local areas eventually recovered. Appendix Figures A.5 and A.6 show that employment and employment-population ratios had not recovered by 2017 for any recession. The partial recovery from the 1973–1975 recession reversed itself in the mid-1980s, after which employment rates declined for the next 20 years. A similar pattern exists for the 1980–1982 recession: starting in the mid-1990s, the partial recovery reverses itself and employment rates fall for several decades. The declines in employment rates following the 1990–1991 and 2001 recessions were extremely stable over time. In sum, local labor market hysteresis persists for several decades

[31]Our preferred earnings measure includes wages, salaries, and supplements (benefits), which are only available by place of work. We show in Appendix Figure A.4 that our findings are not sensitive to the place of residence versus place of work distinction.

[32]Recessions also could lower housing prices. Using average rent prices by metro area from the Census and ACS, we estimate a long-run elasticity to employment changes across recessions of about -0.6. Assuming 30 percent of income is spent on housing, a one-standard deviation decrease in employment during a recession translates into roughly a 0.8 percent long-term decrease in housing costs. This could offset about 15 percent of the decrease in earnings per capita (0.15 = 0.8/5.2). However, this interpretation is complicated in that homeowners facing a similar housing price loss suffer a negative wealth effect (Campbell and Cocco, 2007; Mian, Rao and Sufi, 2013; Guren et al., 2020).

[33]The relationship is approximate, and not exact, because the lagged dependent variables included in the regressions differ.

[34]Using the simple framework in Section 2, we can divide the changes in equations (6) and (7) to conclude that the elasticity of labor supply, ϵ^s, is approximately equal to the elasticity of population, ϵ^n, plus two. Consistent with our expectations, labor supply is considerably more responsive than population.

after each recession.

Moreover, there is little evidence that the persistent decline in local economic activity is driven by subsequent, *independent* shocks that occur after recessions. Instead, our results indicate that local economic activity tends to evolve smoothly after recessions.[35] If areas faced a severe recession and then an independent shock a few years later, we would expect to see post-recession years with sharp decreases in employment, which are not evident in Figure 4 or Appendix Figure A.5.

4.7 Robustness

Our results are robust to different measures of recession severity and different definitions of 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 data to measure recession severity. Appendix B.2 discusses results when measuring recession severity with the log employment change predicted by an area's industry mix (Bartik, 1991). While there are several reasons to prefer the log employment change over the predicted log employment change, 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 Mechanisms

The key mechanism underlying local labor market hysteresis is incomplete adjustment of population after reductions in employment, as shown above. In this section, we provide several pieces of evidence that deepen our understanding of how local labor markets respond to recessions. We show that local labor market hysteresis results from a decline in employment across all industries, accompanied by a reduction in population through lower in-migration. Out-migration accounts for very little of the population decline. Moreover, the decrease in earnings among individuals who remain employed is explained by a reduction in hourly wages, as opposed to hours of work. Finally,

[35]The exception is that areas hit harder by the 1973–1975 recession were also hit harder by the Great Recession, 35 years later (consistent with the correlations in Table 2).

we show that local labor market hysteresis does not simply reflect secular changes in economic conditions or changes in the composition of residents.

5.1 Employment Declines across All Sectors

Are the employment losses shown in Figure 4 broad-based or concentrated in certain industries? Figure 10 shows estimates of equation (9), where the dependent variable is log employment in each sector. 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, while government employment generally shows the smallest declines. 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.[36] As noted above (see Figure 5), these relative employment losses need not reflect absolute employment losses. Nonetheless, the main takeaway is that recessions lead to shifts in labor demand across all sectors.

5.2 Population Declines through Lower In-Migration

What explains the decline in population, and why does population not respond more completely? We use the SOI data to examine these questions for the two most recent recessions. Panels A and B of Figure 11 replicate the event study analysis of population for the 2001 and 2007–2009 recessions using the total number of exemptions in the tax data to proxy for population. The patterns are similar to those in Figure 6.

We decompose the net change in population into changes in in-migration, out-migration, and

[36]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, and 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.

residual net births. This starts with the identity,

$$\text{pop}_{i,t} = \text{pop}_{i,t-1} + \text{inmig}_{i,t} - \text{outmig}_{i,t} + \text{netbirths}_{i,t}, \tag{10}$$

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 (10) forward and normalizing by a baseline population level, we have

$$\frac{\text{pop}_{i,t}}{\text{pop}_{i,0}} - 1 = \sum_{j=0}^{t-1} \frac{\text{inmig}_{i,j}}{\text{pop}_{i,0}} - \sum_{j=0}^{t-1} \frac{\text{outmig}_{i,j}}{\text{pop}_{i,0}} + \sum_{j=0}^{t-1} \frac{\text{netbirths}_{i,j}}{\text{pop}_{i,0}}. \tag{11}$$

We estimate versions of equation (9), where the dependent variables are in-migration, out-migration, and net births, relative to the baseline population level. This provides an exact decomposition of the population change.[37]

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, with a 10 percent decrease in employment during the recession reducing in-migration by about 1 percent of pre-recession population. Over the subsequent decade, these rates recover only slightly, and by the end of the horizon they remain between 0.6 and 0.8 percentage points below pre-recession values. Out-migration shows little response until after the recession has ended, although there is a slight upward pre-trend for the 2001 recession. Beginning in the year after the recession trough, however, out-migration rates steadily *decline*, with similar medium-term magnitudes as for in-migration.

To understand how these components contribute to the change in population, 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 three transformed coefficients—in-migration,

[37]The exact decomposition requires that we include the same covariates in all regressions. We construct net births as a residual using equation (10).

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. In both cases we find that lower in-migration accounts for more than 100 percent of the medium-run decrease in population after recessions. Lower out-migration partly offsets falling in-migration, especially for the Great Recession.[38] The lack of out-migration is a natural explanation for why the population response is incomplete.

5.3 Earnings Decline throughout the Distribution, via Lower Wages

We use the Census/ACS to examine distributional impacts on the earnings of prime-age workers. Specifically, we estimate a variant of equation (9) in which dependent variables are drawn from the Census or 3-year ACS period following the recession.[39] We look at the mean and the 10th, 50th, and 90th percentiles of the log annual earnings distribution. By examining log earnings, we abstract away from extensive margin employment changes. The first row of Panel A of Table 5 shows that estimates for mean log earnings are similar to those from the BEAR data on earnings per worker (Figure 8). The percentile estimates in the next three rows indicate that recessions generally decrease earnings throughout the distribution, with longer-term earnings impacts tending to be less severe at the top of the distribution. These results are consistent with the finding that job losses are more concentrated among lower parts of the earnings distribution (Hoynes, Miller and Schaller, 2012), but we find that medium-term impacts have reached farther up the distribution more recently.

[38] Monras (2020) also finds this pattern of relative population decline due to falling in-migration for the Great Recession, using variation in recession severity based on pre-recession per capita debt and the share of employment in non-tradable industries (see also Mian, Rao and Sufi, 2013). His calibrated general equilibrium model predicts that migration dissipates about 60 percent of the long-term impact on wages following the Great Recession. See also Coen-Pirani (2010).

[39] 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 1980 for the one in 1980–1982, in 1990 for the one in 1990–1991, in 2000 for the one in 2001, and in 2005–2007 for the one in 2007–2009. These controls capture the pre-recession period, again because outcomes are based on the previous calendar year or 12 months. Results are essentially unchanged if we include an additional lagged dependent variable that goes back another ten years.

Does the reduction in earnings stem from a reduction in hours worked, a reduction in earnings per hour, or both? To answer this question, we use the Census/ACS data to estimate regressions where the dependent variable is average log annual, weekly, and hourly earnings. If the earnings losses are driven by a reduction in hours, 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, 2020), hourly wage losses may explain more of the annual earnings declines. The results in Panel B of Table 5 indicate that the latter story better fits the data, as the estimated effects on log hourly wages generally explain 80 percent of the effects on log annual earnings. Decreases in work attachment at the intensive margin therefore explain relatively little of the persistent reduction of annual earnings among individuals who remain employed.[40]

5.4 Hysteresis is Distinct from Secular Decline

A possible concern is that our estimates simply reflect the effects of secular changes in the economy, such as the decline in manufacturing. Several factors point against this interpretation. Our regressions use pre-recession population growth to adjust for pre-existing trends at the local level.[41] We find little evidence of pre-trends in any of the outcomes of interest, and instead see declines in economic activity that emerge during recessions.

To explore this issue further, we estimate regressions that control for interactions between year indicators and the pre-recession share of employment in each of ten sectors: agriculture, construction, finance, government, manufacturing, mining, retail trade, services, utilities, and wholesale trade. These controls absorb changes in economic activity that are associated with industrial specialization. For example, areas that specialize in manufacturing might have experienced reductions in employment rates for the past 50 years. Industrial specialization is correlated with recession severity, so these controls could attenuate the effects of recessions on local areas. The results in

[40]These results do not conflict with our finding that the reduction in the employment-population ratio explains most of the decline in earnings per capita, because our analysis of Census/ACS data conditions on earnings being positive.

[41]Results are extremely similar if we replace linear controls for population growth with more flexible specifications.

Figure 12 show that the estimated effects on the employment-population ratio are very similar when including these controls. There is ample variation in recession severity even when controlling for pre-recession sectoral specialization, and our estimates of local labor market hysteresis do not simply reflect secular declines in manufacturing or other sectors. We conclude that, instead, our results reflect the effects of distinct changes in local economic activity that emerge during recessions.[42]

5.5 Hysteresis is Not Driven by Changes in the Composition of Residents

A remaining explanation for why recessions lead to persistent declines in the employment-population ratio and earnings per capita is a change in worker composition due to differential migration responses. For example, if highly educated workers are more likely to leave an area in response to a decline in labor demand (Bound and Holzer, 2000; Wozniak, 2010; Notowidigdo, 2020), then average wages might fall because of a change in worker composition. Composition shifts are not a threat to our identification strategy, because local labor market hysteresis is defined from the standpoint of an area, but they are an interesting mechanism to understand.

To 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 (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 C of Table 5 presents results for composition-adjusted wage and salary earnings (Panel A, already discussed, shows non-adjusted results). The composition-adjusted results tend to be somewhat smaller in magnitude, which indicates that the age and education shifts identified above partly contribute to the persistent decline in earnings. However, the composition-adjusted impacts are still about 75 percent as large as the unadjusted

[42] We also have estimated regressions that control for interactions between year indicators and the *pre-recession* log employment change predicted by pre-recession industrial structure (Bartik, 1991). Results are extremely similar when including this control, which further supports the conclusion that our estimates do not simply reflect secular decline.

impacts on average. This result implies that hysteresis is not driven by changes in worker characteristics correlated with these variables.

We use a complementary approach to explore the role of composition adjustments in explaining the change in employment-population ratios. In particular, we predict the average change in the employment-population ratio due to changes in the age structure by combining the estimated effects of recessions on the share of the population age 15–39, 40–64, and over 65 with the cross-sectional, pre-recession relationship between the age structure and the employment-population ratio. The results in Appendix Figure A.19 show that, while changes in the age structure do predict a decrease in the employment-population ratio, the predicted effect is much smaller than the actual decrease in the employment-population ratio. We conclude that shifts in the age of residents explain only a small amount of local labor market hysteresis.[43]

5.6 Hysteresis Is Not Unique to Recessions

We focus on local labor market hysteresis following recessions for two main reasons. First, the recessions we study generate substantial variation in local employment changes that are plausibly unrelated to changes in labor supply (i.e., recessions are useful for identification). Second, recessions attract a large amount of attention from policymakers and researchers (i.e., recessions are important). Nonetheless, local labor markets experience shifts in labor demand outside of recessions, and it is natural to wonder whether recessions are unique in generating hysteresis. To examine this question, we estimate versions of equation (9) where the key independent variable s_i is the change in log employment during non-recession periods from 1976–1978, 1983–1985, 1986–1988, 1992–1994, 1995–1997, 1998–1999, and 2003–2006.[44] We adjust all other aspects of the regression so that recession and non-recession periods are treated symmetrically. Importantly, we control for population growth in the pre-period, so the coefficients of interest are identified off of sudden changes in employment that diverge from existing trends in population growth. We view

[43]This conclusion is very similar if we use the same approach to examine the degree to which the decrease in log earnings per capita is explained by changes in the age structure.

[44]The recession periods are 1973–1975, 1979–1982, 1989–1991, 2000–2002, and 2007–2009. Panel D of Appendix Figure A.1 displays the density of log employment changes in recession and non-recession periods.

these short-run employment changes as reflecting shifts in labor demand, especially when arising from recessions.

Appendix Figure A.20 displays the estimated effects of changes in employment on the employment-population ratio, similar to Figure 7. To facilitate comparisons, we display coefficients relative to the first year of the employment change being studied. Point estimates from recession periods are in black, and non-recession periods are in gray. The results show that employment losses similarly depress employment-population ratios after recession and non-recession periods. This finding is consistent with the basic mechanism underlying our results—population responding more slowly than employment to labor demand shifts—also operating in non-recession periods.

6 A Comparison to Results from the Blanchard and Katz (1992) Model

The widespread evidence of local labor market hysteresis that we present above differs from the well-known results of Blanchard and Katz (1992)—hereafter BK—who find that the unemployment rate, labor force participation rate, and wages return to trend within ten years after state-level employment declines. At a basic level, our empirical strategy is similar to BK, in that we both rely on cross-sectional variation in how local areas respond to employment changes. The key difference is that BK, and the many papers which follow their approach, estimate vector autoregressions (VARs) while we estimate event study models. 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 spurious recovery of impulse response functions in the BK VAR.

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 recursive VAR using data from 1976–1990:

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

$$el_{i,t} = \alpha_{i20} + \alpha_{21}(L)\Delta e_{i,t} + \alpha_{22}(L)el_{i,t-1} + \alpha_{23}(L)lp_{i,t-1} + \epsilon_{i,el,t}, \tag{13}$$

$$lp_{i,t} = \alpha_{i30} + \alpha_{31}(L)\Delta e_{i,t} + \alpha_{32}(L)el_{i,t-1} + \alpha_{33}(L)lp_{i,t-1} + \epsilon_{i,lp,t}. \tag{14}$$

BK include two lags of each explanatory 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 decrease in employment (i.e., a reduction in $\epsilon_{i,e,t}$ of 0.01).[45] Primary interest lies in these IRFs, which are constructed using only the coefficients in equations (12)–(14).

Figure 13 shows IRFs of log employment, the "unemployment rate" (one minus the log employment-labor force ratio), and the log 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.[46] Consequently, the most comparable outcome is the employment-population ratio. The IRF of the log employment-population ratio can be constructed as the sum of the IRFs of the log employment-labor force ratio and the log labor force-population ratio. Panel B of Figure 13 shows this IRF from the BK model. As expected given the results in Panel A, the IRF shows complete recovery of the employment rate.

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-

[45]Because this is a recursive VAR, there is a natural unit of measurement for $\epsilon_{i,e,t}$. In contrast, a structural VAR does not feature this property (see, e.g., Stock and Watson, 2001).

[46]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.

population ratio, $ep_{i,t}$. Second, we include only one lag of each variable. The resulting recursive VAR is:

$$\Delta e_{i,t} = \tilde{\alpha}_{i10} + \tilde{\alpha}_{11}\Delta e_{i,t-1} + \tilde{\alpha}_{12} ep_{i,t-1} + \tilde{\epsilon}_{i,e,t}, \tag{15}$$

$$ep_{i,t} = \tilde{\alpha}_{i20} + \tilde{\alpha}_{21}\Delta e_{i,t} + \tilde{\alpha}_{22} ep_{i,t-1} + \tilde{\epsilon}_{i,ep,t}. \tag{16}$$

These simplifying assumptions have little impact on the estimated IRF of the log employment-population ratio, as shown in Panel B of Figure 13.

Equations (15) and (16) permit simpler expressions of the IRF in terms of the underlying parameters. Consider a one-time change in log employment in period t through $\tilde{\epsilon}_{i,e,t}$. The subsequent impacts on the log employment-population ratio are:

$$\frac{dep_{i,t}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21}, \tag{17}$$

$$\frac{dep_{i,t+1}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21}^2\tilde{\alpha}_{12} + \tilde{\alpha}_{21}\tilde{\alpha}_{11} + \tilde{\alpha}_{21}\tilde{\alpha}_{22}, \tag{18}$$

$$\frac{dep_{i,t+2}}{d\tilde{\epsilon}_{i,e,t}} = \tilde{\alpha}_{21}^3\tilde{\alpha}_{12}^2 + 2\tilde{\alpha}_{21}^2\tilde{\alpha}_{11}\tilde{\alpha}_{12} + 2\tilde{\alpha}_{21}^2\tilde{\alpha}_{22}\tilde{\alpha}_{12} + \tilde{\alpha}_{21}\tilde{\alpha}_{11}^2 + \tilde{\alpha}_{21}\tilde{\alpha}_{22}^2 + \tilde{\alpha}_{21}\tilde{\alpha}_{11}\tilde{\alpha}_{22}. \tag{19}$$

Similar expressions exist for the IRF at later horizons, but these first few periods are adequate to highlight some important takeaways. First, bias in the OLS estimates of equations (15) and (16) can generate bias in the IRF, because the IRF is a function of the coefficients in these equations. Second, bias in the IRF can be a nonlinear function of bias in the coefficients, because the IRF is a nonlinear function of these coefficients. 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 can converge towards zero even if the true IRF does not. This arises because the exponents in the IRF increase with time, magnifying attenuation bias.[47]

The potential for finite sample attenuation bias in autoregressive models, including VARs, has long been recognized (e.g., Hurwicz, 1950; Shaman and Stine, 1988; Stine and Shaman, 1989;

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

Pope, 1990; Lucas, 1992; Kilian, 1998, 1999; Kilian and Lütkepohl, 2017).[48] 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:

$$e_{i,t} = e_{i,t-1} + \varepsilon_{i,e,t}, \tag{20}$$

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

$$p_{i,t} = p_{i,t-1} + (1-\phi)\Delta e_{i,t} + \varepsilon_{i,p,t}. \tag{21}$$

This implies that the log employment-population ratio is:

$$ep_{i,t} = ep_{i,t-1} + \phi\Delta e_{i,t} - \varepsilon_{i,p,t}. \tag{22}$$

In terms of equations (15) and (16), 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), $\tilde{\alpha}_{21} = \phi$, and $\tilde{\alpha}_{22} = 1$. Changes in log employment have a permanent effect on the log employment-population ratio, with the true IRF equal to ϕ at all horizons. We study DGPs with this feature to examine whether the BK VAR accurately estimates persistent effects of declines in local area employment 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 variances of $\varepsilon_{i,e,t}$ and $\varepsilon_{i,p,t}$ approximate the variance of log employment and

[48] In his discussion of Blanchard and Katz (1992), Lucas (1992) raises a concern about small sample bias, but speculates that such bias does not drive their conclusions. Kilian (1998, 1999) specifically addresses bias in impulse responses. The methods discussed in these papers allow for bias-corrected confidence intervals of impulse responses, but we focus on point estimates here for simplicity. 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).

population in subsequent years.[49] We focus on the case where $\phi = 0.75$, with 50 cross-sectional observations and different time-series lengths, T.

Panel A of Figure 14 plots the true IRF along with average estimates across 499 Monte Carlo simulations. The true IRF reveals a persistent decrease in the employment-population ratio. 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-population ratio. Ten years after the shock, the IRF estimate is downward-biased (in absolute value) by 89 percent. This bias remains very large for $T = 25$ and $T = 50$. Because previous work on local labor market hysteresis uses annual data, the relevant value of T ranges from 15 to 50. The bias remains sizable for $T = 100$, for which the bias ten years after the shock equals 25 percent. Even for $T = 500$, finite sample bias incorrectly implies slow, but steady recovery.[50] The bias stems from an insufficient number of time series observations, so instrumental variables do not solve this problem in general. Indeed, we find that a sufficiently strong instrumental variable (as has been used in previous work) generates nearly identical results in our DGP (in which an instrument is not needed to obtain consistent estimates).

Event study estimates do not suffer from finite sample bias due to small T in this setting. To show this, we use the same DGP and estimate the following event study regression:

$$ep_{i,t} = \Delta e_i \delta_t + \beta_t + ep_{i,0}\gamma_t + \varepsilon_{i,t}, \tag{23}$$

where the shock Δe_i occurs between year 0 and 1, and, to be consistent with the VAR IRFs, we normalize the coefficient $\delta_0 = 0$. This is the direct analog of equation (9). Under this DGP, we have $\delta_t = -0.75$ for all years $t \geq 1$. Hence, the event study coefficient δ_t (which is akin to an empirical impulse response function) and the IRF coincide in population for all post-shock years. Panel B of Figure 14 shows that there is no systematic bias in estimates of δ_t, regardless of T.

In sum, finite sample bias can lead the BK VAR to find evidence of recovery when there is

[49]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 $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$.

[50]Appendix Table A.3 reports the underlying bias in estimates of the parameters of equations (15) and (16) for various values of T. All parameters are biased. While this bias is modest in many cases, it is amplified in the IRF. The IRF bias is of primary interest, because the IRF is used to quantify the extent of hysteresis.

none. The event study regressions that we estimate are not subject to this finite sample bias in empirically relevant DGPs. We believe that finite sample bias is the main explanation for why we find widespread evidence of local hysteresis, while papers estimating the BK VAR do not.[51] To be clear, we do not claim that all VARs are incapable of identifying persistent effects. However, finite bias is evident in DGPs that are relevant for VARs estimated in previous work on local labor market hysteresis.

7 Conclusion

This paper examines the effects of recessions on U.S. local labor markets. Studying five recessions over the course of 50 years, we find that employment losses which emerge during recessions are long-lasting, implying a persistent relative decline in local labor demand. Population falls during recessions and for several years afterwards, but by less than employment. Most importantly, recessions lead to local labor market hysteresis: persistent declines in employment-to-population ratios, earnings per worker, and earnings per capita for over a decade after recession's end.

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 effects, 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 as in Blanchard and Katz (1992)—incorrectly finds convergence after a persistent decline in local labor demand because of finite sample bias. In contrast, the event study models that we estimate do not suffer from this bias.

Cross-sectional variation in recession severity allows us to estimate relative effects by comparing local labor markets that experience a more versus less severe recession. This variation, however, does not allow us to identify the absolute effects of recessions on local economic activ-

[51] The literature estimating BK 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.

ity (e.g., Nakamura and Steinsson, 2014). Nonetheless, local labor market hysteresis raises the concern that the capabilities of workers in some areas remain underutilized. This "direct effect" could lower aggregate output. At the same time, there could be an offsetting indirect effect if recessions reallocate employment to more productive areas. We examine this possibility through simple back-of-the-envelope calculations, described in Appendix B.4, and find no evidence of such productivity-enhancing reallocation. Fully assessing the impacts of local labor market hysteresis on aggregate output requires additional assumptions about the counterfactual evolution of economic activity in the absence of recessions, which we leave for future work.

Irrespective of the aggregate consequences of local labor market hysteresis, our findings have important implications for labor market dynamism, the economic opportunities of workers and their children, and optimal policy responses. Our results show that recessions lead to a sizable reallocation of employment across space. Local areas that experience more severe recessions see a persistent decline in employment across all sectors. 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 shifts in labor demand. Moreover, the persistent decrease in local economic activity limits the opportunities available to both adults and children in these places. The potential importance of investments in job creation is underscored by the fact that employment rate declines drive the persistent losses in earnings per capita; the fact that decreases in hourly wages drive the reductions in earnings among those who remain employed points to the potential value of policies centered on skill development. Such policies could also potentially forestall the reduction in economic mobility for children caused by long-run decline in local economic activity (Stuart, 2018). Currently, the vast majority of policy responses to recessions focus on short-term conditions. Our results imply that additional consideration should be paid to recessions' long-term effects.

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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. (4)	Log emp. change (5)	Emp. change (6)	Share of peak year emp. (7)	Log emp. change (8)	Emp. change (9)
	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 due to rounding.
Source: Authors' calculations using Bureau of Economic Analysis Regional Economic Accounts (BEAR) data.

Table 2: Correlation of Metropolitan Area Recession Severity

	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 pre-recession population growth					
1973–75	1.000				
1980–82	0.259	1.000			
1990–91	0.167	0.017	1.000		
2000–02	0.140	0.082	0.100	1.000	
2007–09	0.392	0.276	0.047	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 pre-recession 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 population 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)	18.9	20.2	20.7	22.2	22.5	25.4	27.2	31.4	32.9	32.2
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 ten years for the other recessions. We define an area as experiencing a more severe recession if its log employment change for a given recession is less than the median across CBSAs for that recession.

Source: Authors' calculations of data from BEAR, decennial Censuses and American Community Surveys (via IPUMS and NHGIS), and Surveillance, Epidemiology, and End Results (SEER).

Table 4: Summary of Impacts of Log Employment Decreases During Recessions on Metropolitan Area 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	−0.871	−1.656	−1.543	−0.790
	(0.184)	(0.138)	(0.153)	(0.131)	(0.123)
Implied effect of 1 SD log employment decrease	−0.072	−0.069	−0.075	−0.053	−0.031
Panel B: Dependent Variable: Log Population Age 15+					
Coefficient, 7–9 years after trough	−0.648	−0.557	−0.627	−0.548	−0.371
	(0.113)	(0.078)	(0.127)	(0.099)	(0.061)
Implied effect of 1 SD log employment decrease	−0.036	−0.044	−0.029	−0.019	−0.014
Panel C: Dependent Variable: Log Employment-Population Ratio					
Coefficient, 7–9 years after trough	−0.600	−0.360	−0.924	−0.992	−0.424
	(0.100)	(0.101)	(0.123)	(0.133)	(0.101)
Implied effect of 1 SD log employment decrease	−0.033	−0.028	−0.042	−0.034	−0.017
Panel D: Dependent Variable: Log Earnings per Worker					
Coefficient, 7–9 years after trough	−0.175	−0.403	−0.096	−0.639	−0.407
	(0.067)	(0.064)	(0.100)	(0.127)	(0.113)
Implied effect of 1 SD log employment decrease	−0.010	−0.032	−0.004	−0.022	−0.016
Panel E: Dependent Variable: Log Earnings per Capita					
Coefficient, 7–9 years after trough	−0.776	−0.774	−1.160	−1.708	−0.804
	(0.117)	(0.151)	(0.150)	(0.216)	(0.167)
Implied effect of 1 SD log employment decrease	−0.043	−0.061	−0.053	−0.058	−0.031
SD of log employment change	0.056	0.079	0.045	0.034	0.039

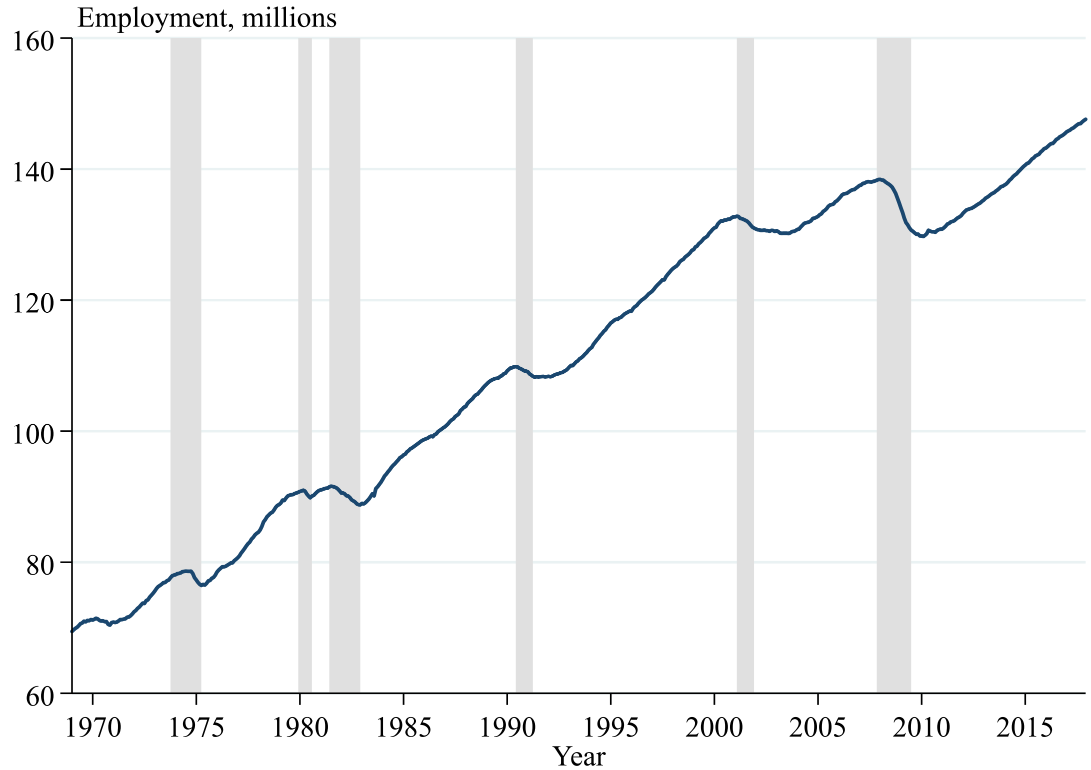
Notes: Table reports estimates of equation (9), separately for each recession. We impose the constraint that pre-recession coefficients equal zero and group post-recession coefficients across years 1–3, 4–6, 7–9, and (for the earliest four recessions) 10. Dependent variables are indicated in the panel titles, and the key independent variable is the change in log wage and salary employment during the recession from BEAR data. All regressions control for division-year fixed effects and interactions between pre-recession population growth and year indicators. There are 363 metropolitan areas in the sample. Standard errors are clustered by metropolitan area.
Source: Authors' calculations using BEAR and SEER data.

Table 5: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Wage Earnings, Census/ACS

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Log Annual Earnings, Without Composition Adjustment					
Average log earnings	−0.203	−0.435	−0.157	−0.592	−0.492
	(0.095)	(0.089)	(0.100)	(0.103)	(0.125)
10th percentile, log earnings	−0.024	−0.568	−0.219	−0.836	−0.303
	(0.168)	(0.155)	(0.165)	(0.250)	(0.243)
50th percentile, log earnings	−0.212	−0.410	−0.025	−0.385	−0.598
	(0.105)	(0.089)	(0.085)	(0.099)	(0.126)
90th percentile, log earnings	−0.103	−0.299	−0.077	−0.406	−0.423
	(0.085)	(0.068)	(0.086)	(0.090)	(0.142)
Panel B: Weekly and Hourly Earnings					
Average log weekly earnings	−0.192	−0.417	−0.107	−0.485	−0.445
	(0.082)	(0.075)	(0.081)	(0.084)	(0.109)
Average log hourly earnings	−0.171	−0.375	−0.131	−0.391	−0.401
	(0.071)	(0.067)	(0.071)	(0.075)	(0.097)
Panel C: Log Annual Earnings, With Composition Adjustment					
Average log earnings	−0.155	−0.315	−0.085	−0.673	−0.329
	(0.086)	(0.077)	(0.083)	(0.088)	(0.113)
10th percentile, log earnings	−0.023	−0.330	−0.139	−1.135	−0.124
	(0.160)	(0.149)	(0.130)	(0.246)	(0.248)
50th percentile, log earnings	−0.190	−0.305	−0.040	−0.519	−0.337
	(0.077)	(0.071)	(0.074)	(0.069)	(0.090)
90th percentile, log earnings	−0.125	−0.247	−0.073	−0.496	−0.288
	(0.083)	(0.061)	(0.062)	(0.079)	(0.123)

Notes: Table reports estimates of separate regressions for each recession. The dependent variable is indicated in the row titles and taken from the post-recession Census year (1980, 1990, 2000, 2005–2007, and 2015–2017, respectively). The key independent variable is the change in log wage and salary employment during the recession from BEAR data. Regressions include one lagged dependent variable for the pre-recession period. Sample limited to individuals age 25–54. All regressions control for division-year fixed effects and interactions between pre-recession population growth and year indicators. The dependent variables in Panel C are constructed using residuals from regressing log earnings on indicators for education, indicators for age, an indicator for sex, and 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.
Source: Authors' calculations using BEAR, decennial Census, and ACS data.

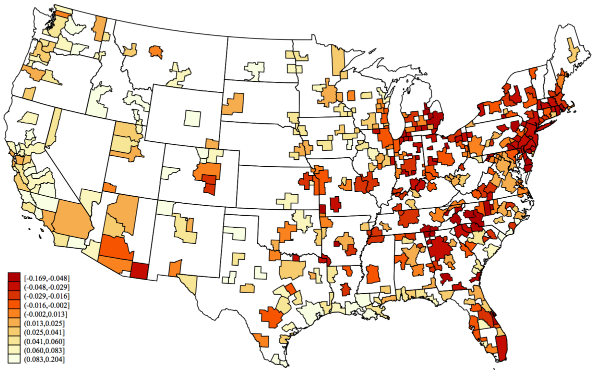
Figure 1: Aggregate Employment and Recessions, 1969–2017



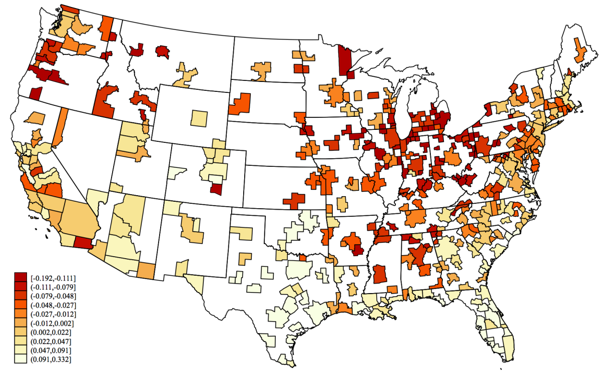
Notes: Figure shows seasonally adjusted national nonfarm employment. The shading indicates NBER national recession dates.
Source: Authors' calculations using Bureau of Labor Statistics Current Employment Statistics.

Figure 2: Log Employment Changes During Recessions in Metropolitan Areas

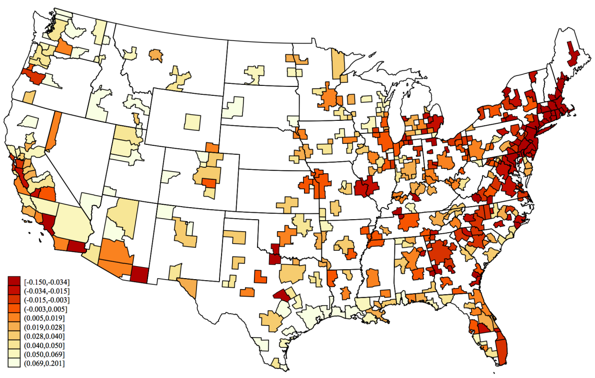
(a) 1973–1975 Recession



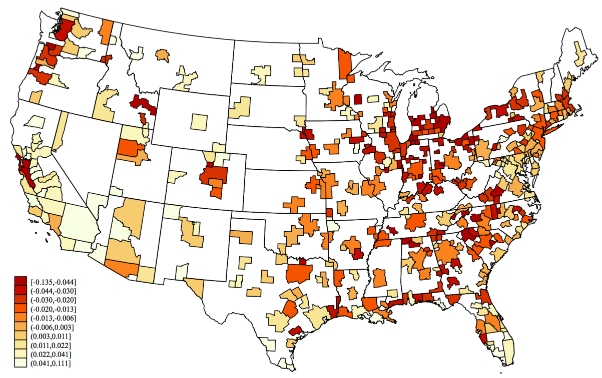
(b) 1980–1982 Recession



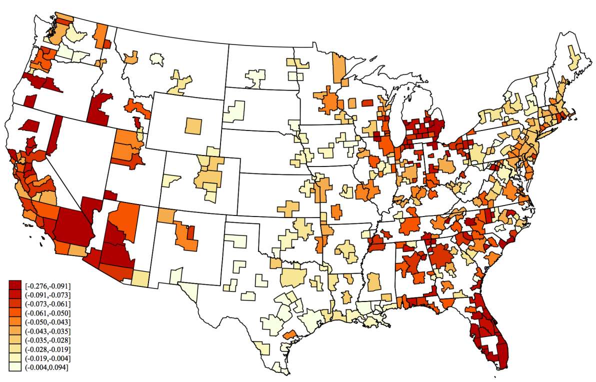
(c) 1990–1991 Recession



(d) 2001 Recession

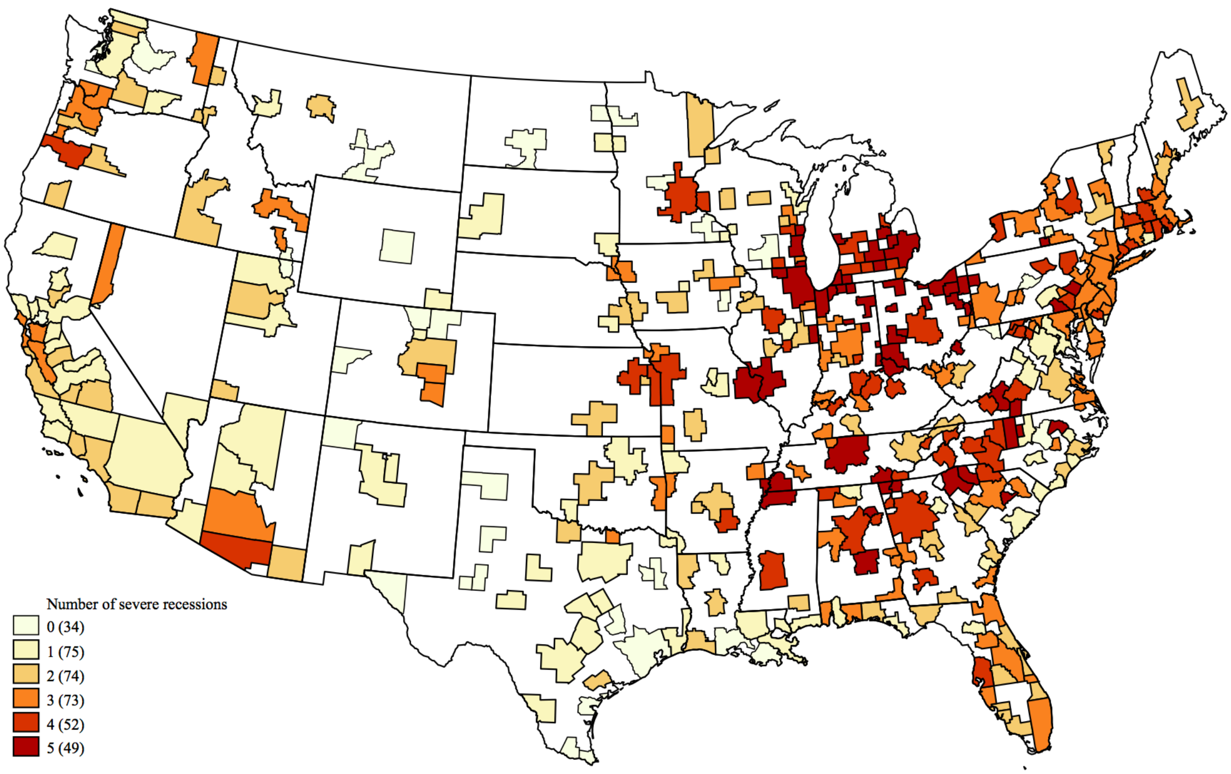


(e) 2007–2009 Recession



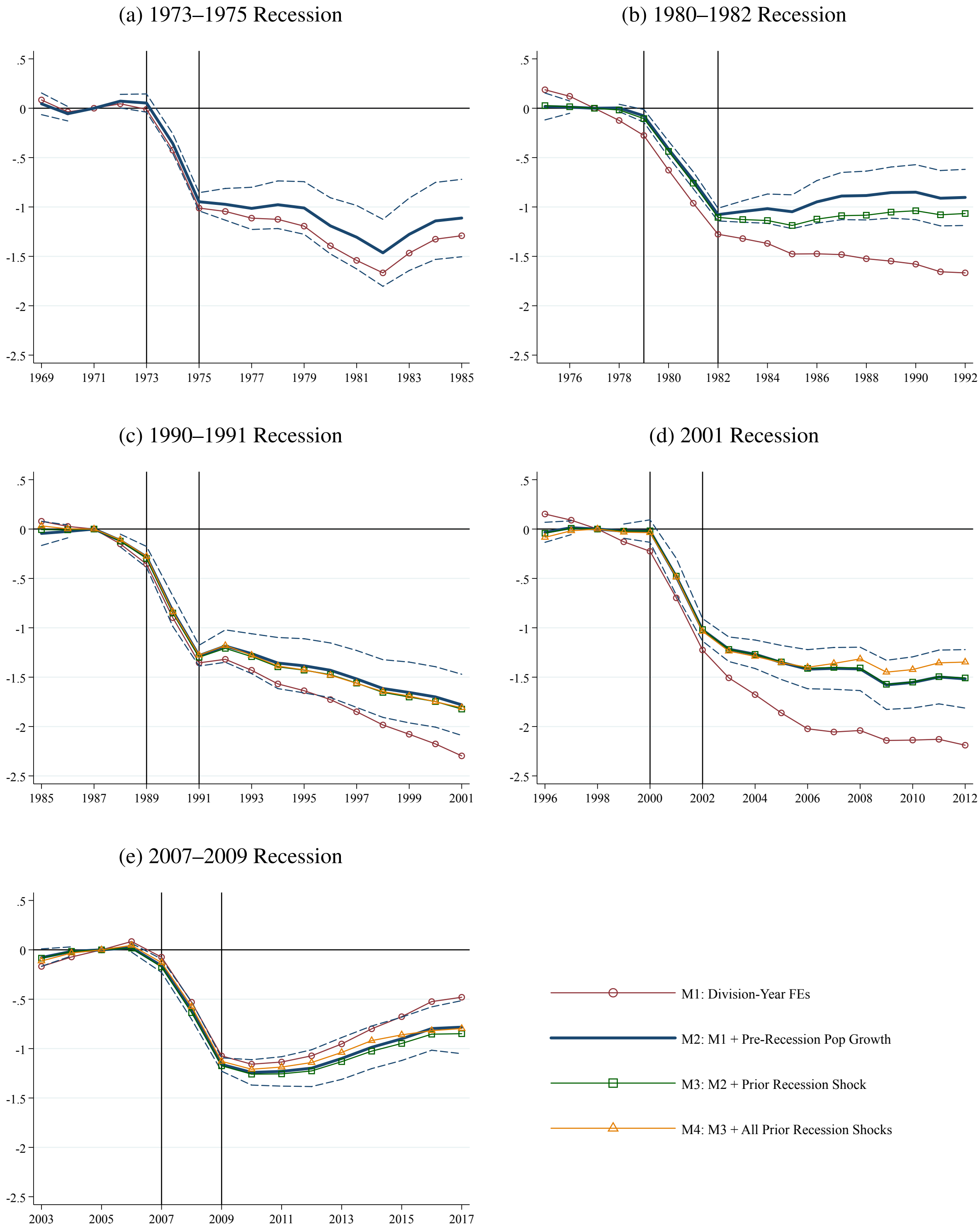
Notes: Each map shows the change in log employment from national peak to trough for 363 CBSAs (OMB vintage 2003 definitions) as described in the text. Areas in darker colors experienced larger employment losses.
Source: Authors' calculations from BEAR.

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



Notes: We define an area as experiencing 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.

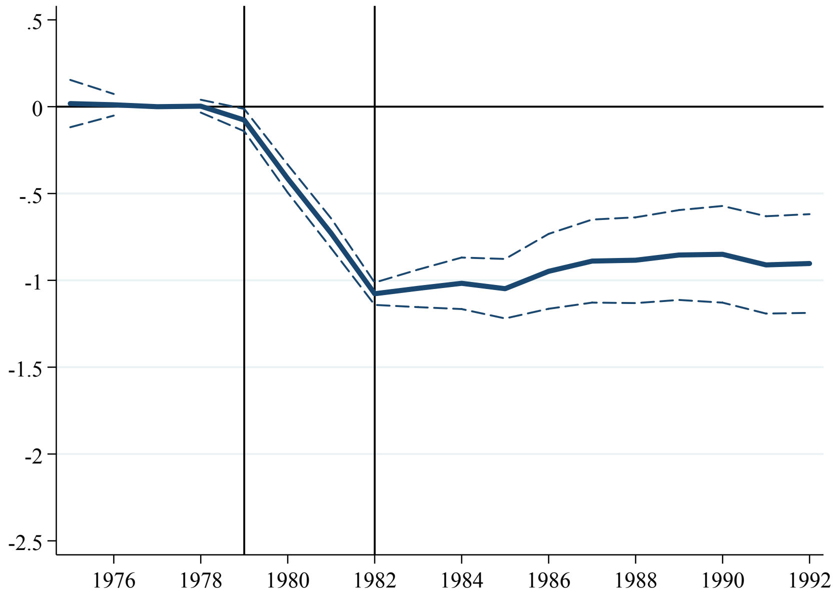
Figure 4: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment



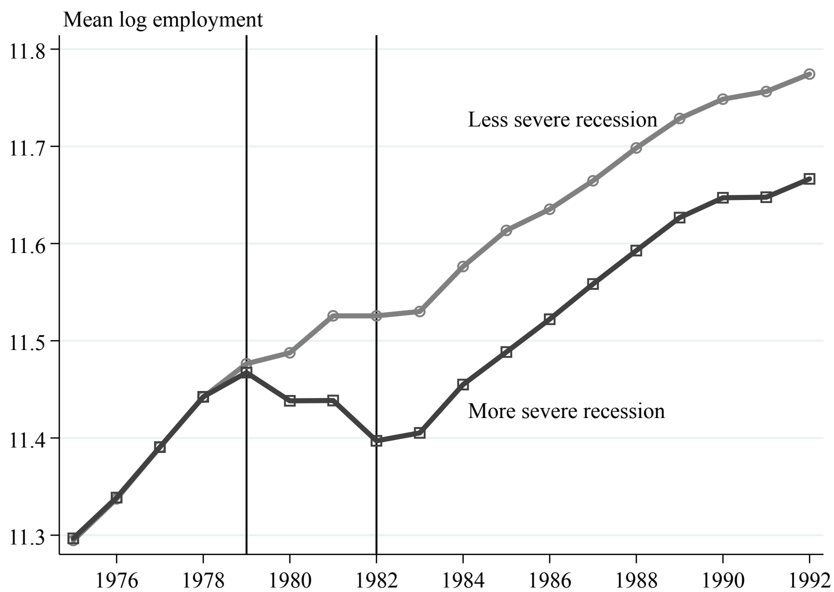
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the change in log wage and salary employment during the recession from BEAR data. Specifications are indicated by the legend. There are 363 metropolitan areas in the sample. Standard errors are clustered by metropolitan area. Source: Authors' calculations using BEAR and SEER data.

Figure 5: Comparison of Relative Effects from Event Study Regressions and Absolute Effects, Log Employment and the 1980–1982 Recession

(a) Relative Effects: Event Study Coefficients



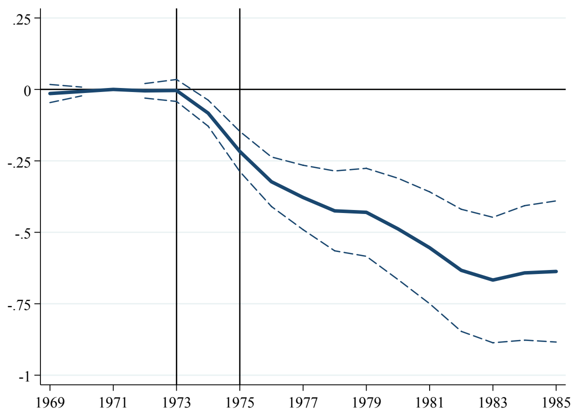
(b) Absolute Effects: Implied Conditional Means



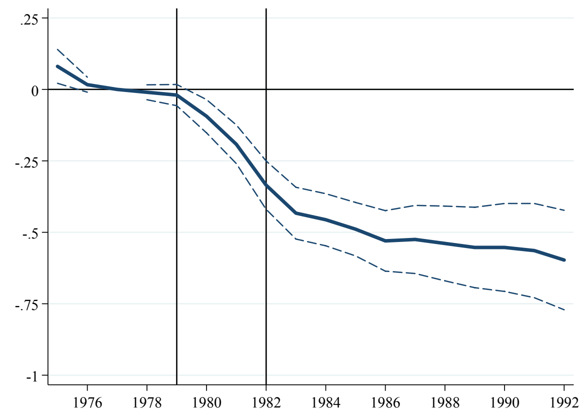
Notes: Panel A shows estimates of event study coefficients from our main specification, as in Panel B of Figure 4. In Panel B, we use estimates of equation (9) to construct mean log employment for metro areas with a more versus less severe recession (based on whether the log employment change is greater than or less than the median log employment change during the recession), holding all other covariates in the regression at their mean value. We do this for the 1980–1982 recession for purposes of illustration.
Source: Authors' calculations from BEAR data.

Figure 6: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Population Age 15+

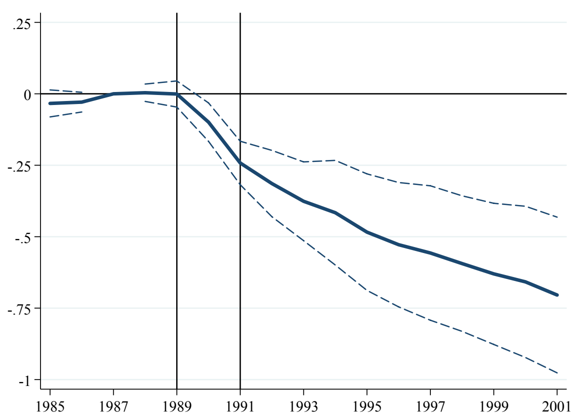
(a) 1973–1975 Recession



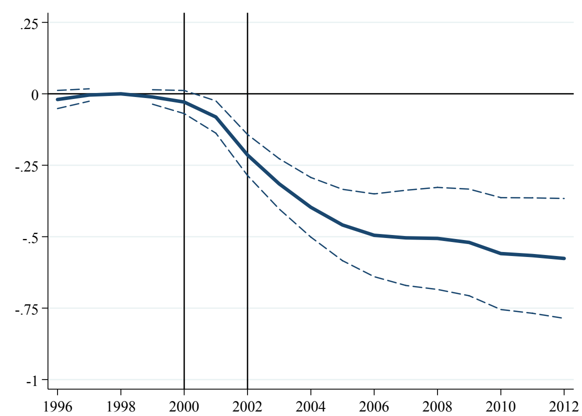
(b) 1980–1982 Recession



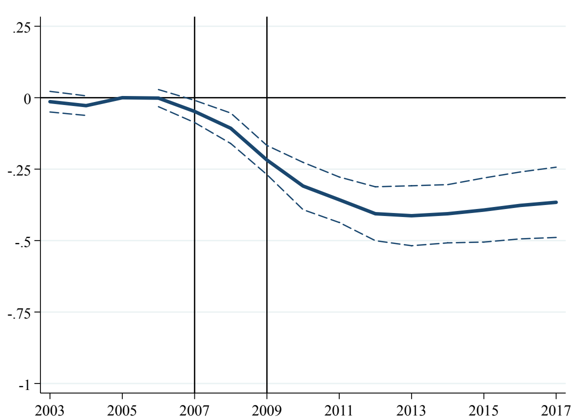
(c) 1990–1991 Recession



(d) 2001 Recession



(e) 2007–2009 Recession



Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure 7: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment-Population Ratio

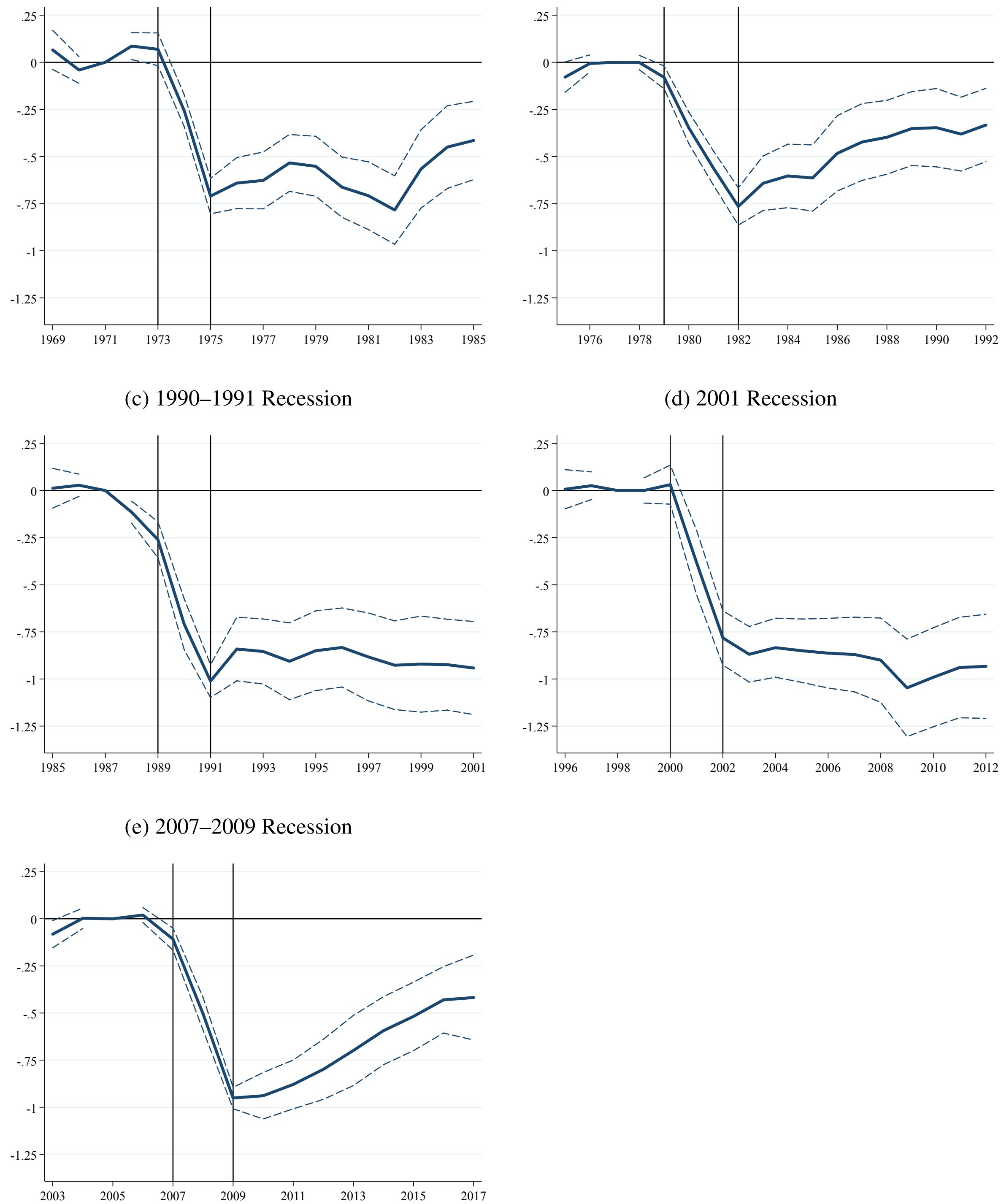
(a) 1973–1975 Recession

(b) 1980–1982 Recession

(c) 1990–1991 Recession

(d) 2001 Recession

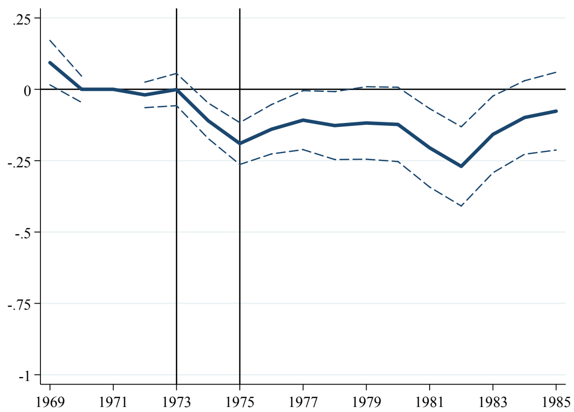
(e) 2007–2009 Recession



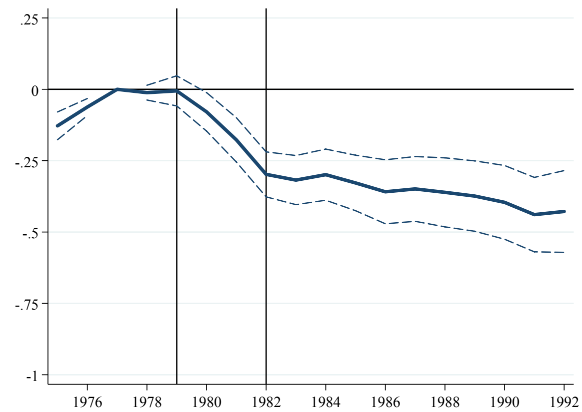
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log ratio of wage and salary employment to population age 15 and above. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure 8: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Real Earnings per Worker

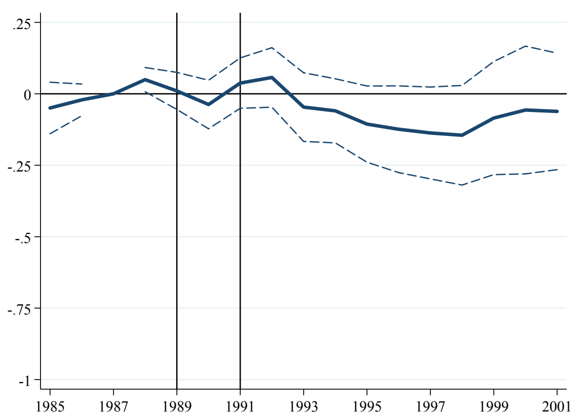
(a) 1973–1975 Recession



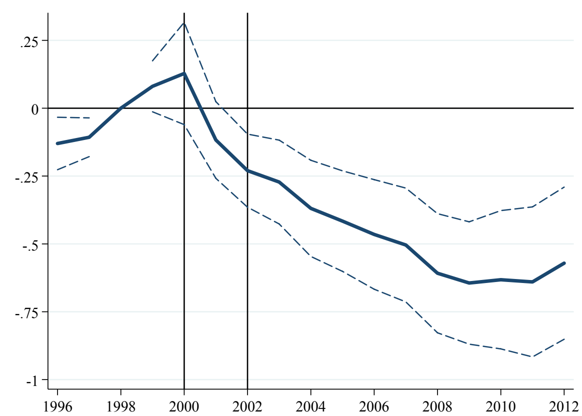
(b) 1980–1982 Recession



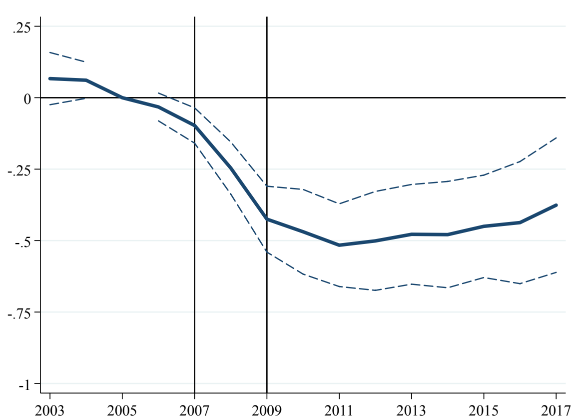
(c) 1990–1991 Recession



(d) 2001 Recession

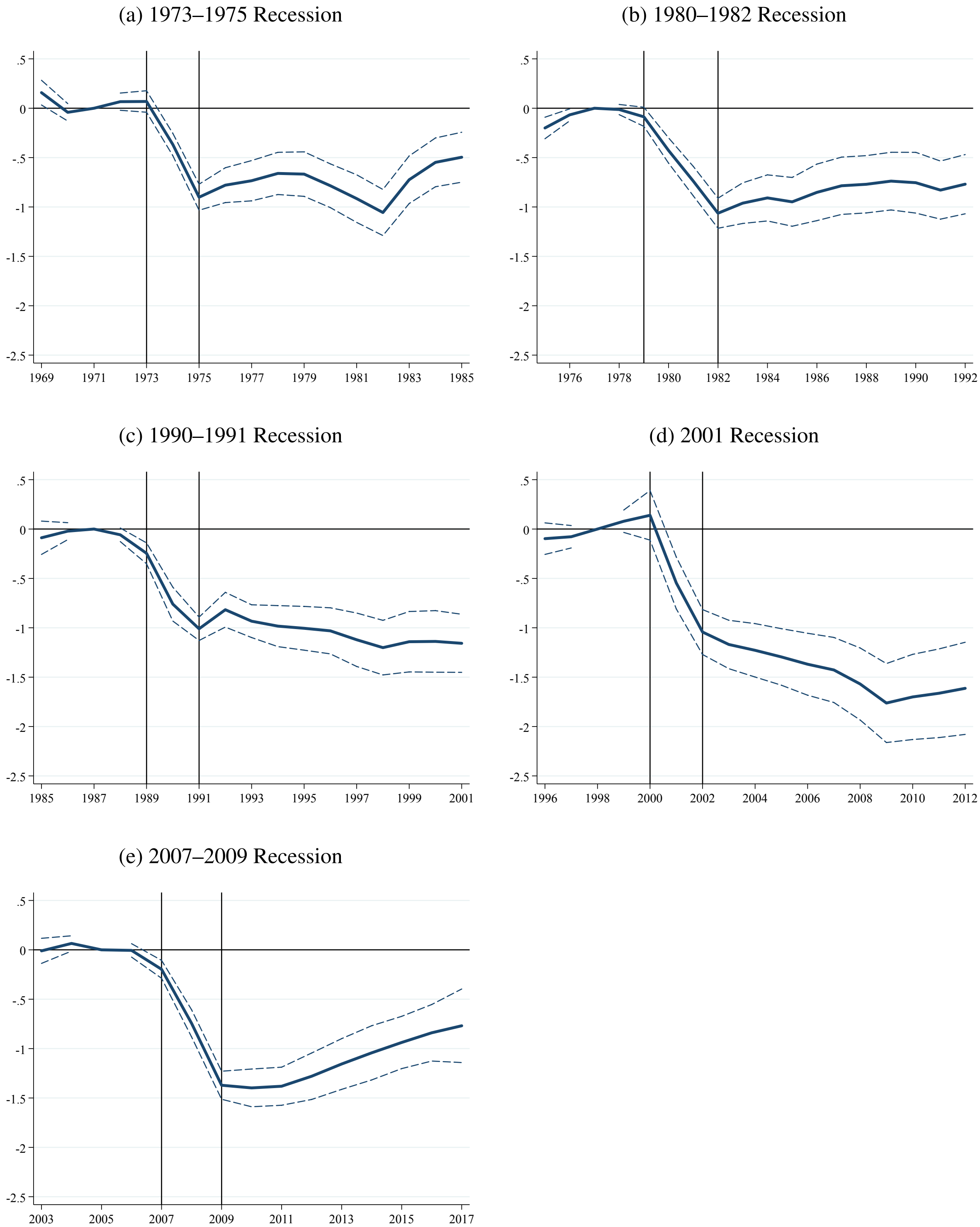


(e) 2007–2009 Recession



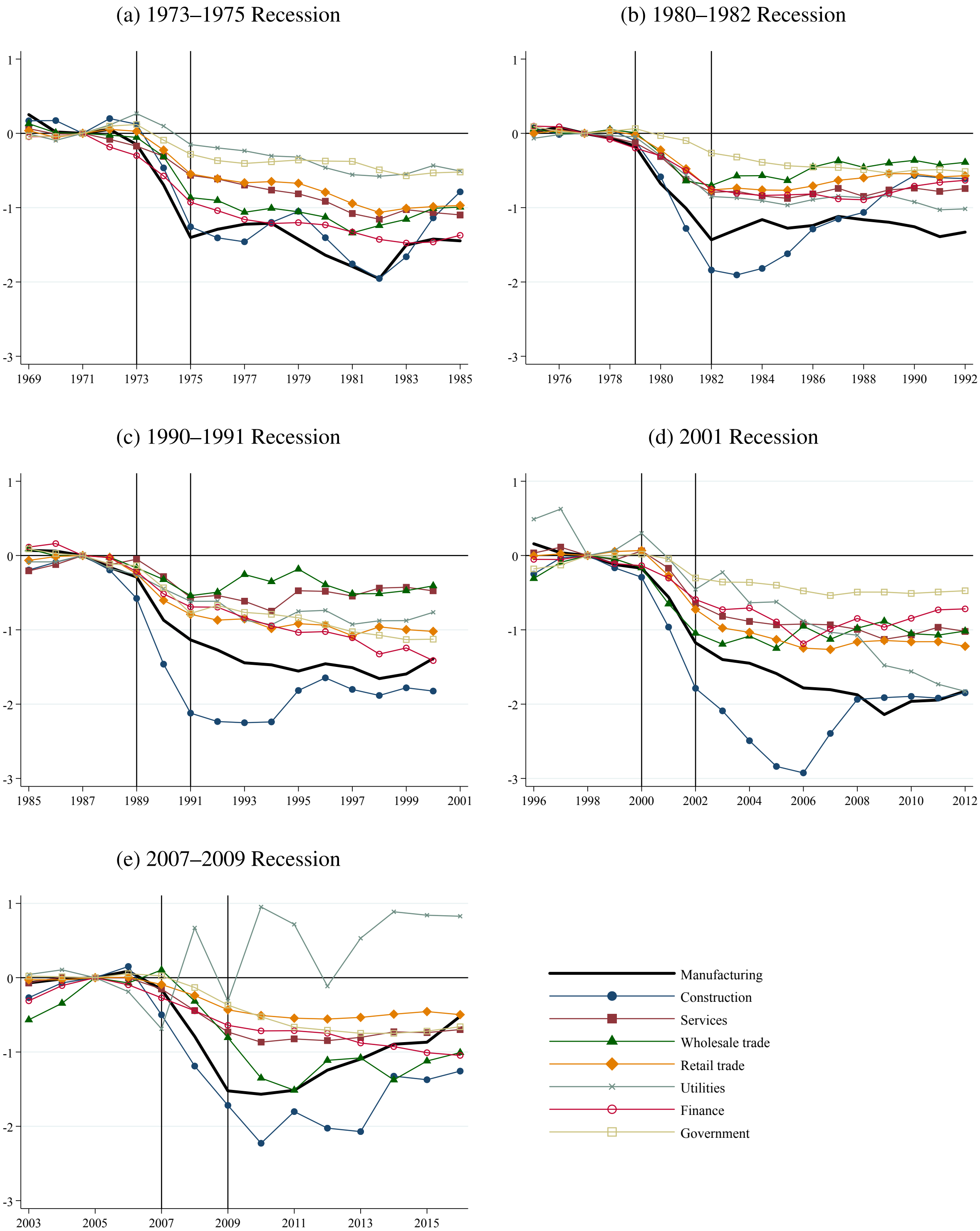
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log real earnings per wage and salary worker. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure 9: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Real Earnings per Capita



Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

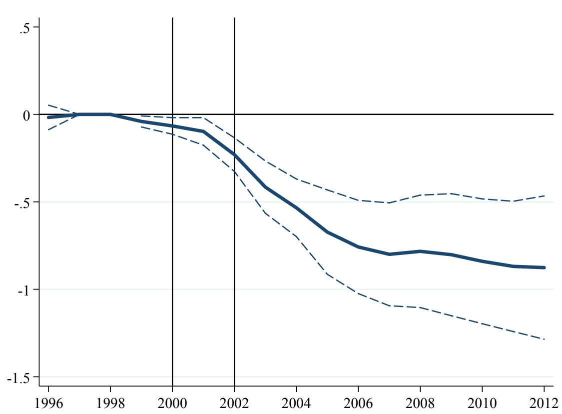
Figure 10: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment, by Sector



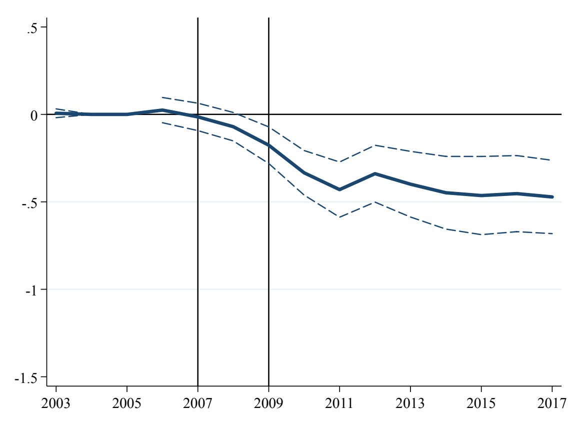
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log employment from the indicated sector. We use BEAR data for the 1973–75, 1980–82, 1990–91, and 2007–09 recessions. We use QCEW data for the 2001 recession (due to SIC-NAICS industry seaming issues), except for government, which comes from BEAR. See notes to Figure 4. Source: Authors' calculations using BEAR, SEER, and QCEW data.

Figure 11: Impacts of Log Employment Decreases During Recessions on Metropolitan Area In-Migration and Out-Migration

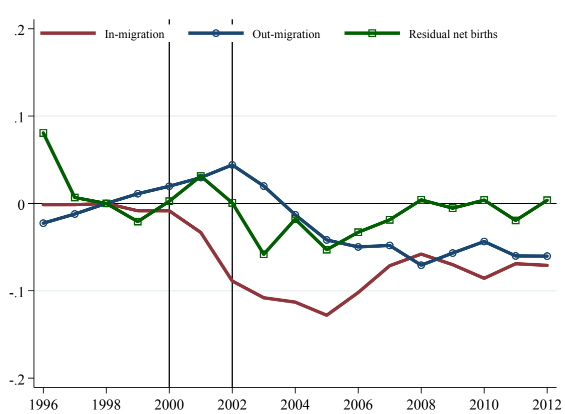
(a) 2001, DV: Log Population



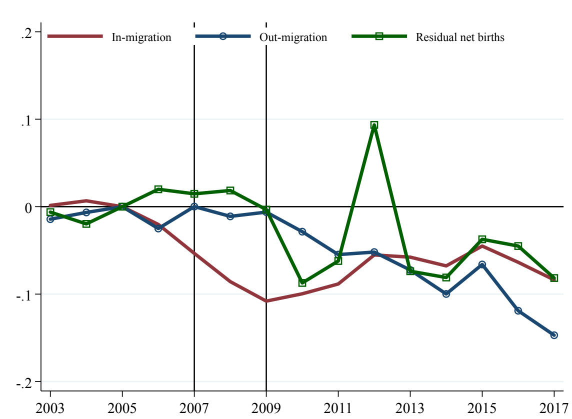
(b) 2007–2009, DV: Log Population



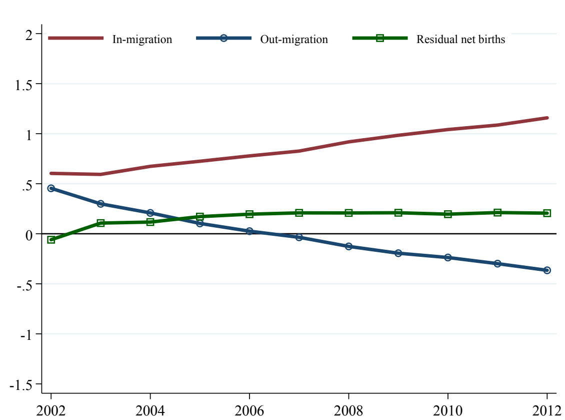
(c) 2001, DV: Population Component



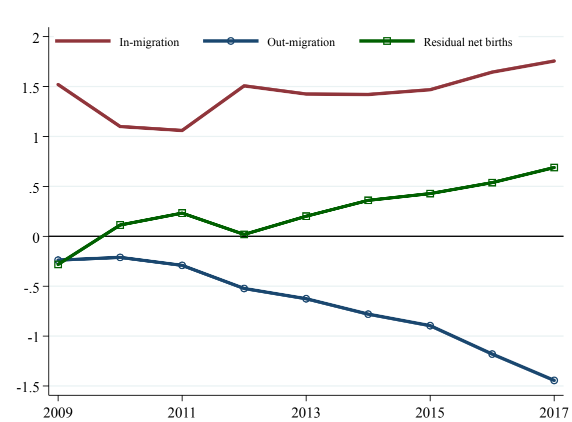
(d) 2007–09, DV: Population Component



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

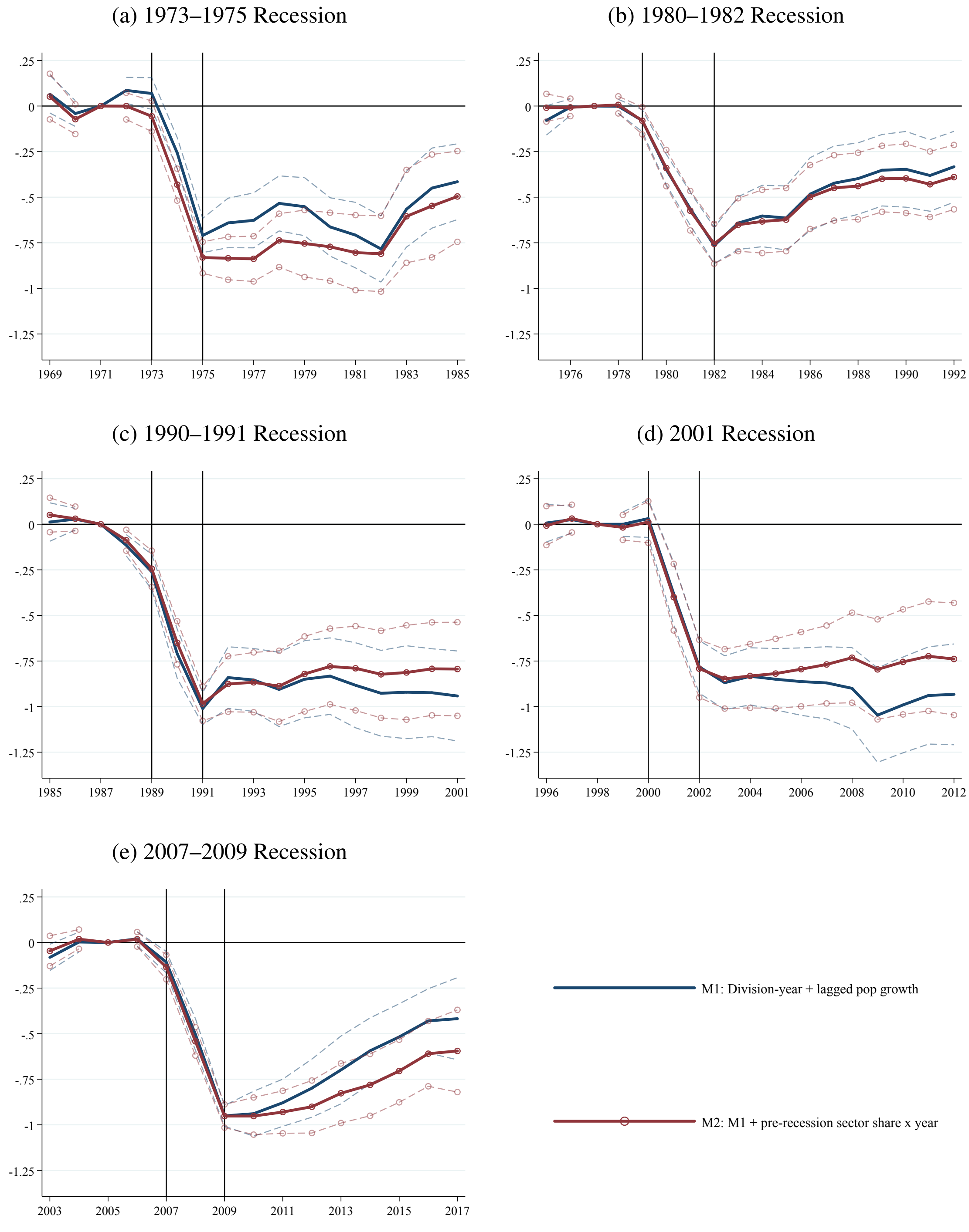


(f) 2007–09, Share of Total Effect due to Components



Notes: Figure reports estimates of equation (9), 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 level of exemptions, in-migration, out-migration, and residual net births in the normalization year and year indicators, in addition to the baseline controls described in the notes to Figure 4. Source: Authors' calculations using CBP, BEAR, and SOI data.

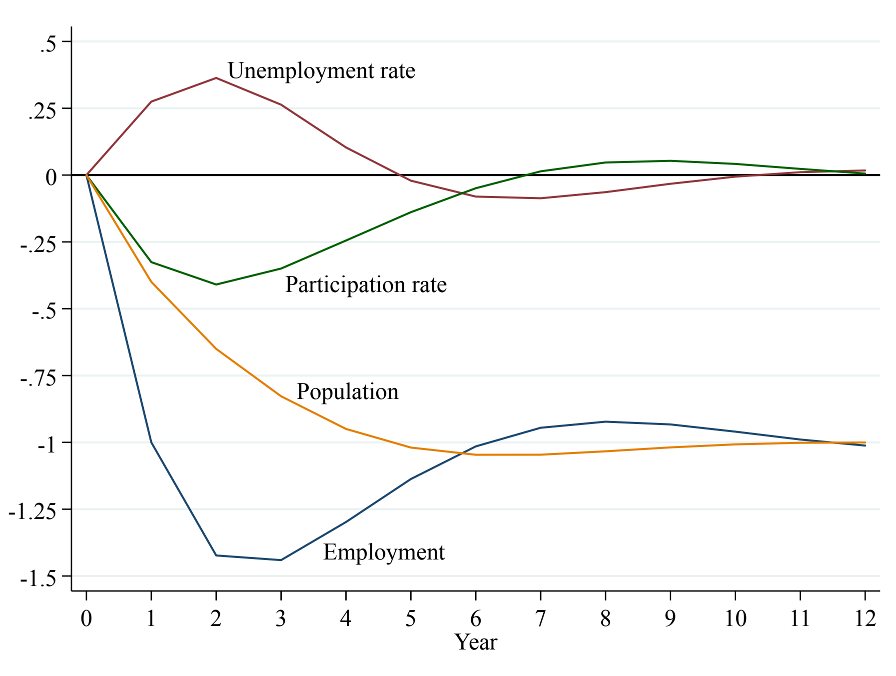
Figure 12: Impacts on Log Employment-Population Ratio, Robustness to Controlling for Pre-Recession Industrial Specialization



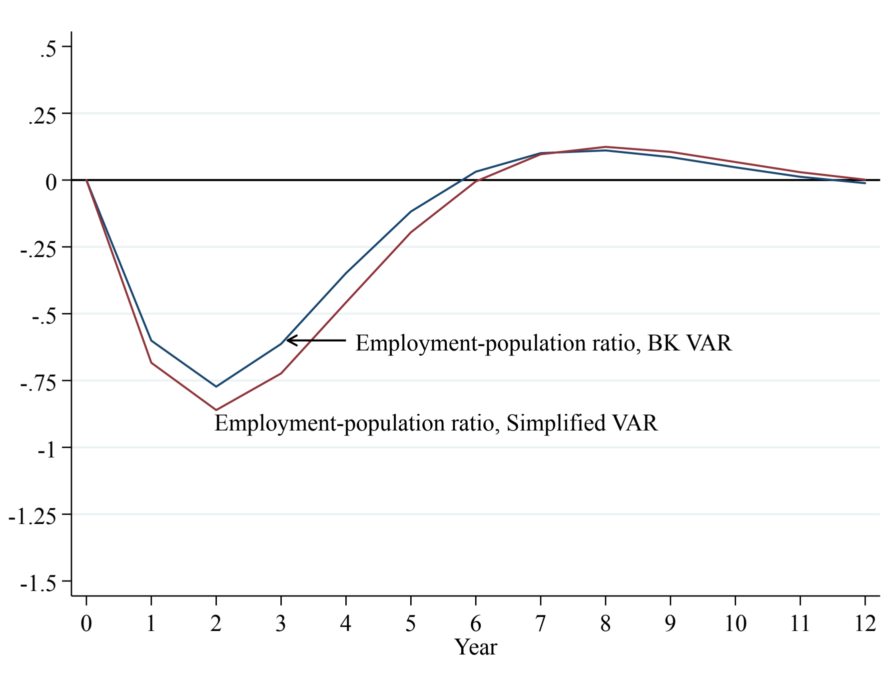
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log ratio of wage and salary employment to population age 15 and above. Model 1 is our preferred specification. Model 2 further controls for the pre-recession share of employment in agriculture, construction, finance, manufacturing, mining, retail trade, services, utilities, and wholesale trade (government is the omitted sector). Pre-recession employment is measured in 1973, 1979, 1989, 2000, and 2007. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure 13: Impulse Response Functions to Negative Log Employment Shock from Vector Autoregressions

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



(b) Employment-Population Ratio

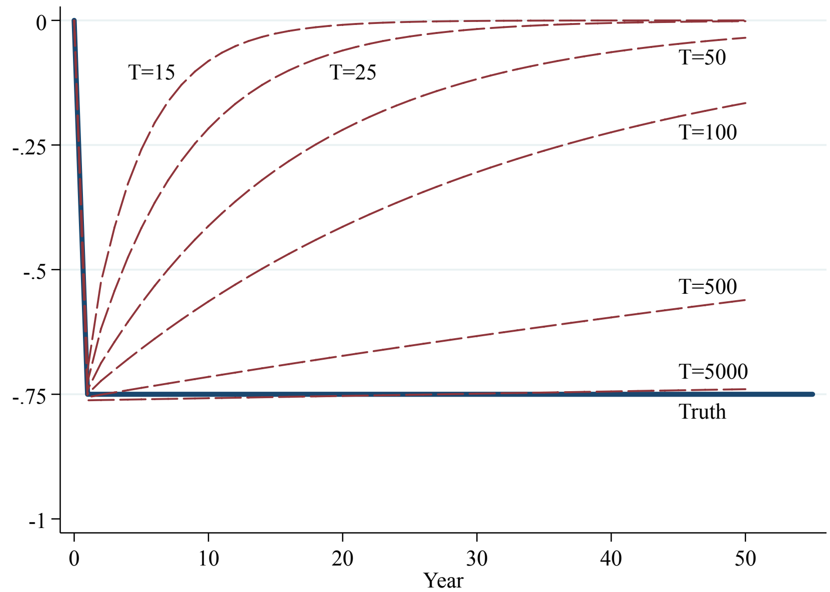


Notes: Figure shows impulse response functions of indicated variables with respect to a negative log employment shock. We construct impulse response functions for the BK VAR using estimates of equations (12)–(14). For the simplified VAR in Panel B, we use equations (15)–(16). Sample contains 48 continental states plus Washington, D.C. from 1976–1990.

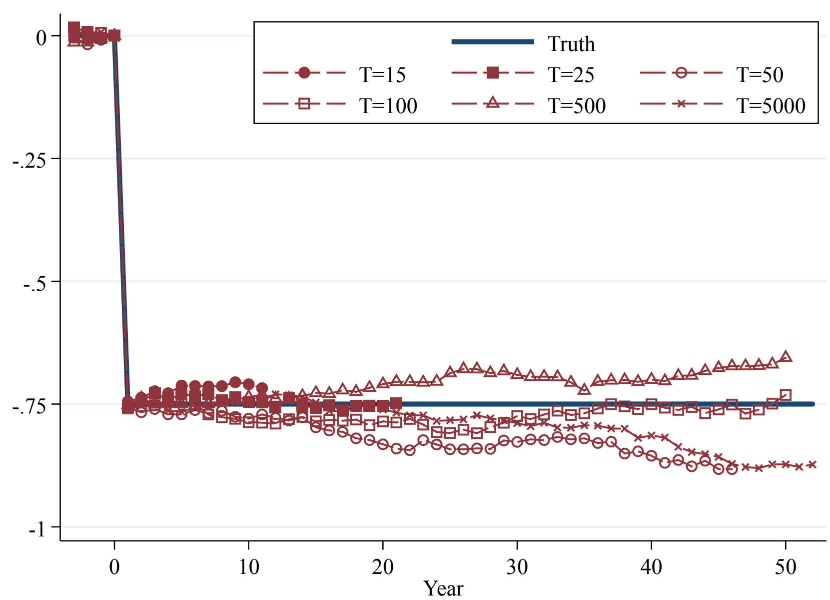
Source: Authors' calculations using BLS LAUS data.

Figure 14: Comparison of Finite Sample Bias from Vector Autoregression Impulse Response Functions and Event Study Regressions

(a) Vector Autoregression Impulse Response Functions



(b) Event Study Regression Coefficients



Notes: Panel A displays impulse response functions of the log employment-population ratio with respect to a negative log employment shock based on estimates of equations (15)–(16). Panel B displays estimates of δ_t from the event study regression in equation (23). For both panels, we simulate data following equations (20)–(22). We set $e_{i,0} \sim \mathcal{N}(13.94, 1.00^2)$, $p_{i,0} \sim \mathcal{N}(14.49, 1.02^2)$, $\varepsilon_{i,e,t} \sim \mathcal{N}(0, 0.015^2)$, $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$, $\phi = -0.75$, and $N = 50$. Results are based on 499 Monte Carlo simulations.

Online Appendices

A Data Appendix

A.1 Creating Consistent Geography Definitions over Time

We examine the impacts of recessions for 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.[52] Metropolitan areas are periodically redefined by the Office of Management and Budget (OMB), and commuting zones are redefined decadally by the Department of Agriculture based on commuting questions in the Census (in 1990 and 2000) or 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.[53] 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 Census Bureau) or the Department of Agriculture.

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.[54] 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 county group-county crosswalks available from IPUMS to map county groups to CBSAs.[55] 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.

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.

[52] Counties are the most stable, but occasionally change due to state legislative action or boundary disputes.

[53] 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.

[54] 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.

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

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.[56] However, data suppressions are common, especially earlier in the period. At the county level, data for small or highly concentrated industries (e.g., agriculture and 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 predicted log employment changes as in Bartik (1991), 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;[57] 2) If either total *or* private employment is missing 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, such 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.

A.3 Imputing Employment in County Business Patterns

When constructing the predicted log employment change as in Bartik (1991), we use County Business Patterns (CBP) data to measure local industry employment shares. In the relevant years, 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 1000 employees, we impute employment as the number of establishments times average pre-recession employment in the establishment size group, where the average comes from nationwide data across all industries. We use 1999 data to construct these imputation adjustments, but the results are very similar when using other years.

[56] 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.

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

Nationwide CBP data report total employment among establishments with at least 1000 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 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 1000 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 1000 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, 0.62), and (7.51, 0.67). We use 1999 parameters throughout for simplicity. Standard facts about the log-normal distribution imply that the imputed means for the four establishment size groups are (1249, 1950, 3373, 6679).[58]

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 Measures of Log Employment Changes

Our baseline specification uses the change in log total wage and salary employment from BEAR to measure recession severity. We believe 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

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

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

$$E(y|y > a) = E(y)\frac{\Phi(\sigma - a_0)}{\Phi(-a_0)}.$$

alternatives are private wage and salary employment from BEAR and private wage and salary employment from QCEW.[59] Figures A.7–A.10 show that the estimated effects on employment, population, the employment-population ratio, and earnings per capita are quite similar when using these other measures to define recession severity. 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 Predicted Log Employment Changes

We estimate equation (9) 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 predicted log employment change is

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

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.[60]

We do not use the predicted log employment change in our preferred specification, because our focus on a shorter window during recessions and our controls for pre-recession 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 (Adão, Kolesár and Morales, 2018; Kirill, Hull and Jaravel, 2018; Goldsmith-Pinkham, Sorkin and Swift, 2018). Nonetheless, given the ubiquity of the Bartik (1991) approach, we report results from using it here.

Appendix Table A.1 describes the relationship between the actual log employment change and the predicted log employment change. The first column includes no other controls. For every recession besides 1990–1991, the predicted log employment change explains 33–36 percent of the cross-metro variation in the actual log employment change. For 1990–1991, the predicted log employment change explains only six percent of the actual 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

[59] CBP data represent another alternative, although its 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 private households.

[60] QCEW data have the advantage of being available at a quarterly frequency, which we could (but do not) use in constructing the predicted log employment change; our results are 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 the pre-recession employment share.

statistically significant even with the additional controls.

Appendix Table A.2 shows that predicted log employment changes are more highly correlated across time than actual log employment changes. This is not surprising, as the shift-share variable primarily reflects local industry employment shares, which are relatively stable. These high correlations raise the concern that the coefficients on the predicted log employment change might not isolate the impact of a given recession. Instead, the predicted log employment change could pick up the effects of earlier or later recessions, in addition to secular changes in industry-level employment.

Appendix Figure A.11 displays estimates of the effect of the predicted log employment change on log employment. The results are qualitatively similar to those using log employment changes in Figure 4 for the 1980–1982, 2001, and 2007–2009 recessions.[61] There is less evidence of a persistent employment decline for the 1973–1975 and 1990–1991 recessions; for these recessions, there is clear evidence of an employment decline during the subsequent recession, consistent with the high cross-recession correlations. Figures A.12 through A.14 display results for population, the employment-population ratio, and earnings per capita. The patterns largely mirror those 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, which span the entire (continental) United States, including rural areas. Appendix Figures A.15 through A.18 show that results are very similar when using commuting zones (specifically, the 2000 definition).

B.4 Back of Envelope Calculations on the Role for Productivity-Enhancing Reallocation

This appendix reports the results of simple calculations that assess whether recessions are likely to increase aggregate earnings per worker by reallocating employment to more productive areas. We refer to these calculations in the conclusion.

The change in aggregate earnings per worker due to recession-induced cross-area reallocation is

$$Y_{t+k}^C - Y_t = \sum_i (\theta_{i,t+k}^C - \theta_{i,t}) Y_{i,t}, \tag{A.5}$$

where Y_t is aggregate earnings per worker in pre-recession year t, and Y_{t+k}^C is the counterfactual level of earnings per worker in year $t+k$ reflecting recession-induced employment reallocation

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

across local labor markets. These aggregate earnings per worker terms are defined as:

$$Y_t \coloneqq \sum_i \theta_{i,t} Y_{i,t} \tag{A.6}$$

$$Y^C_{t+k} \coloneqq \sum_i \theta^C_{i,t+k} Y_{i,t}, \tag{A.7}$$

where $Y_{i,t}$ is earnings per worker in metro i in year t, $\theta_{i,t} \equiv E_{i,t}/E_t$ is the employment share of metro i in year t, and $\theta^C_{i,t+k}$ is the counterfactual employment share in year $t+k$. We construct this counterfactual employment share as

$$\theta^C_{i,t+k} = \frac{E_{i,t} \times \exp(s_i \hat{\delta}_{t+k})}{\sum_j E_{j,t} \times \exp(s_j \hat{\delta}_{t+k})}. \tag{A.8}$$

The numerator of this expression is the pre-recession employment level multiplied by the percent change in employment predicted by recession severity from equation (9). Using only the employment change that is explained by recession severity ensures that we do not attribute secular changes (absorbed by our controls) to the effect of the recession.

Column 1 of Appendix Table A.4 reports the unweighted standard deviation (SD) of the difference between the counterfactual employment share and the observed pre-recession employment share, $(\theta^C_{i,t+k} - \theta_{i,t})$. We construct this counterfactual 7–9 years after the recession trough, using the estimates in Panel A of Table 4. We set t as the peak recession year. Column 2 reports the unweighted SD of the relative employment share difference, $(\theta^C_{i,t+k} - \theta_{i,t})/\theta_{i,t}$. There is a fair amount of reallocation, with the standard deviation ranging from 3.5 to 7.5 percent of baseline employment. Column 3 reports the nationwide average of mean annual earnings per worker in the peak year, expressed in constant 2017 dollars. Column 4 reports the change in aggregate earnings per worker, $Y^C_{t+k} - Y_t$. In four out of five recessions, cross-area reallocation lowers earnings per worker. However, the aggregate changes are extremely small, ranging from a reduction of \$213 (1990–1991) to an increase of \$21 (1980–1982). This is underscored in column 5, which divides column 4 by column 3 and then multiplies by 100 to express percent changes. The largest change is only 0.3 percent of peak year earnings per worker.

To shed further light on these results, Appendix Figure A.21 displays the cross-metro correlations between the employment share change $(\theta^C_{i,t+k} - \theta_{i,t})$ and peak-year earnings per worker $(Y_{i,t})$. The marker symbols are proportional to the peak year employment share. High-earning metro areas regularly lose and gain employment. On average, there is no net shift towards higher or lower earning metro areas, as seen in Table A.4.

In sum, these calculations suggest that recessions do not meaningfully reallocate employment towards more productive metro areas.

Table A.1: Cross-Sectional Relationship between Metropolitan Area Log Employment Change and Predicted Log Employment Change

	Dependent variable: Log employment change during recession		
	(1)	(2)	(3)
Panel A: 1973–1975 Recession			
Predicted log employment change	1.813	1.217	1.161
	(0.180)	(0.201)	(0.210)
R^2	0.338	0.449	0.485
Panel B: 1980–1982 Recession			
Predicted log employment change	1.951	1.779	1.544
	(0.162)	(0.141)	(0.156)
R^2	0.358	0.591	0.665
Panel C: 1990–1991 Recession			
Predicted log employment change	1.342	0.728	1.028
	(0.233)	(0.230)	(0.237)
R^2	0.062	0.415	0.484
Panel D: 2001 Recession			
Predicted log employment change	1.517	1.261	1.260
	(0.114)	(0.133)	(0.137)
R^2	0.344	0.407	0.539
Panel E: 2007–2009 Recession			
Predicted log employment change	1.799	1.537	1.599
	(0.173)	(0.191)	(0.205)
R^2	0.331	0.453	0.512
Division fixed effects		x	x
Pre-recession population growth			x

Notes: Table reports estimates of regressing the log employment change during recessions against the predicted log employment change during recessions, as in Bartik (1991). 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.2: Correlation of Metropolitan Area Predicted Log Employment Changes

	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	0.808	1.000			
1990–91	0.719	0.725	1.000		
2000-02	0.722	0.695	0.808	1.000	
2007–09	0.476	0.525	0.723	0.667	1.000
Panel B: Adjusted for Census division					
1973–75	1.000				
1980–82	0.753	1.000			
1990–91	0.663	0.662	1.000		
2000-02	0.661	0.628	0.809	1.000	
2007–09	0.497	0.495	0.735	0.682	1.000
Panel C: Adjusted for Census division and pre-recession population growth					
1973–75	1.000				
1980–82	0.736	1.000			
1990–91	0.592	0.577	1.000		
2000-02	0.552	0.534	0.717	1.000	
2007–09	0.434	0.452	0.673	0.608	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 pre-recession population growth.
Source: Authors' calculations using BEAR, CBP, and QCEW data.

Table A.3: Bias in Vector Autoregression 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	−0.038	−0.101	0.701	0.855
25	−0.022	−0.060	0.725	0.918
50	−0.010	−0.030	0.741	0.960
100	−0.004	−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 (15)–(16). We simulate data following equations (20)–(22). We set $e_{i,0} \sim \mathcal{N}(13.94, 1.00^2)$, $p_{i,0} \sim \mathcal{N}(14.49, 1.02^2)$, $\varepsilon_{i,e,t} \sim \mathcal{N}(0, 0.015^2)$, $\varepsilon_{i,p,t} \sim \mathcal{N}(0, 0.015^2)$, $\phi = 0.75$, and $N = 50$. Results are based on 499 Monte Carlo simulations.

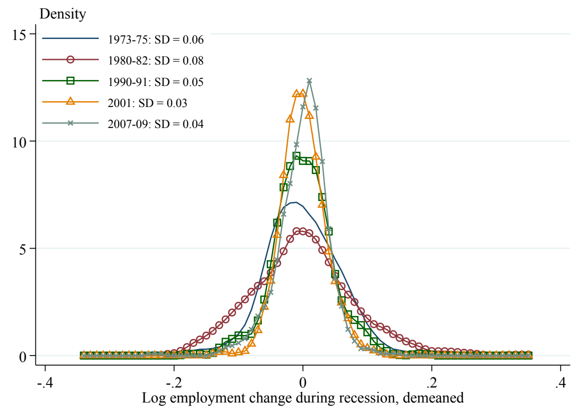
Table A.4: Changes in Earnings per Worker due to Recession-Induced Reallocation

Recession	SD, emp. share change (1)	SD, rel. emp. share change (2)	Mean earnings per worker, peak year (3)	Change in mean earnings per worker (4)	Percent change in mean earnings per worker ($\times$ 100) (5)
1973–1975	0.00039	0.075	54,060	-12	-0.022
1979–1982	0.00032	0.071	54,339	21	0.038
1990–1991	0.00049	0.072	62,974	-213	-0.339
2001	0.00020	0.049	76,888	-70	-0.091
2007–2009	0.00016	0.035	85,751	-1	-0.001

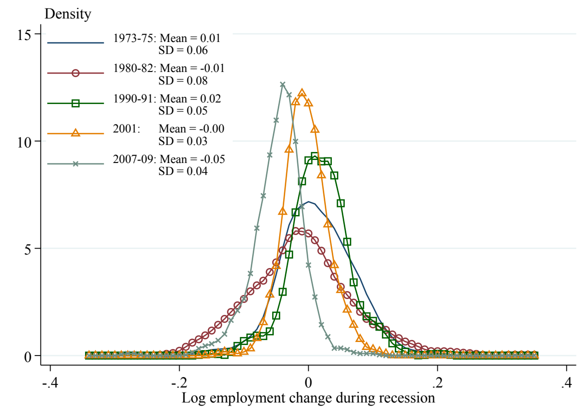
Notes: Column 1 reports the unweighted standard deviation of the difference between the counterfactual employment share (reflecting recession-induced employment reallocation) and the observed pre-recession employment share, $(\theta^C_{i,t+k} - \theta_{i,t})$. We construct this counterfactual 7–9 years after the recession trough, using the estimates in Panel A of Table 4. Column 2 reports the unweighted SD of the relative employment share change, $(\theta^C_{i,t+k} - \theta_{i,t})/\theta_{i,t}$. Column 4 reports the change in aggregate earnings per worker, $Y^C_{t+k} - Y_t = \sum_i(\theta^C_{i,t+k} - \theta_{i,t})Y_{i,t}$. Column 5 divides column 4 by column 3 and then multiplies by 100 to express percent changes.
Source: Authors' calculations using BEAR, decennial Census, and ACS data.

Figure A.1: Density of Log Employment Changes and Predicted Log Employment Changes During Recessions Across Metros

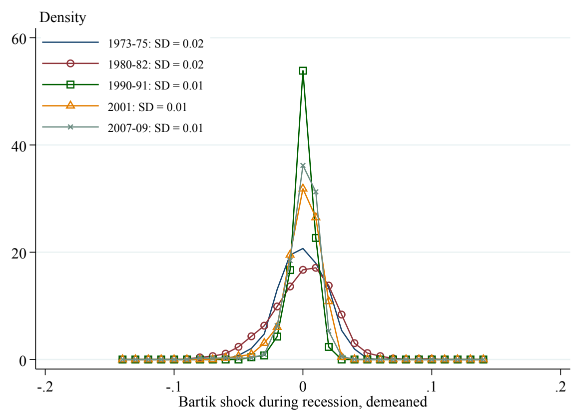
(a) Log Employment Change, Demeaned



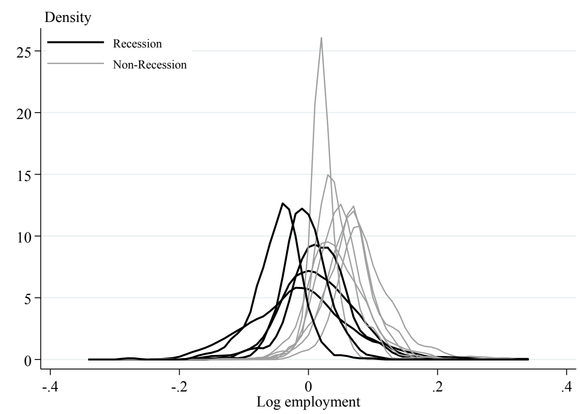
(b) Log Employment Change



(c) Predicted Log Employment Change, Demeaned



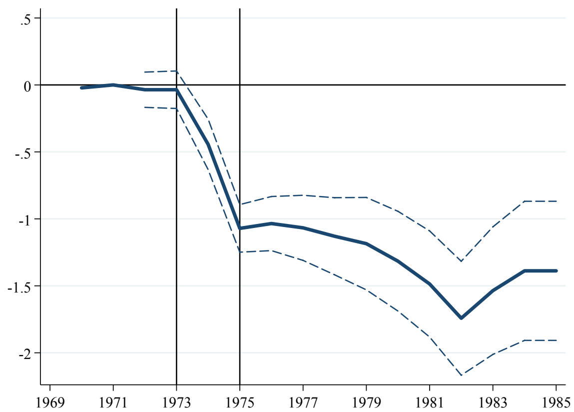
(d) Log Emp. Change, Recessions v. Other Periods



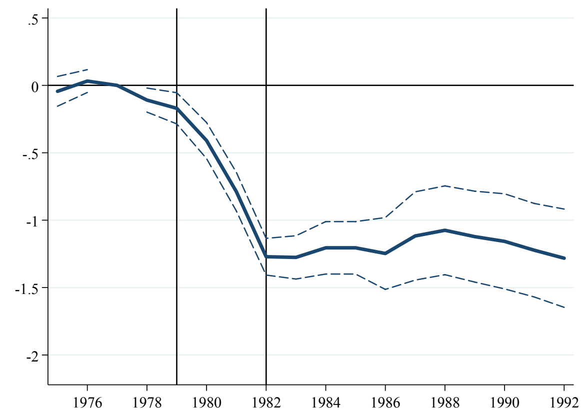
Notes: Figure shows estimated kernel densities of the log wage and salary employment change (Panels A, B, and D) and predicted log employment change based on pre-recession industrial structure (as in Bartik (1991); Panel C) across metros for each of the five recessions since the mid 1970s. In Panels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

Figure A.2: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment from CBP

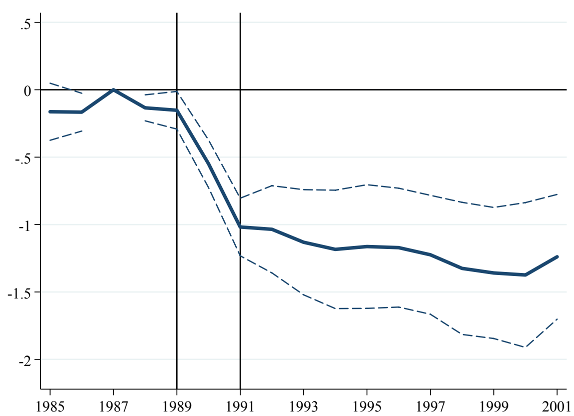
(a) 1973–1975 Recession



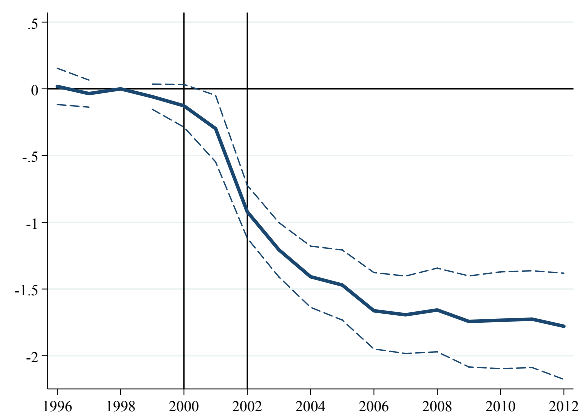
(b) 1980–1982 Recession



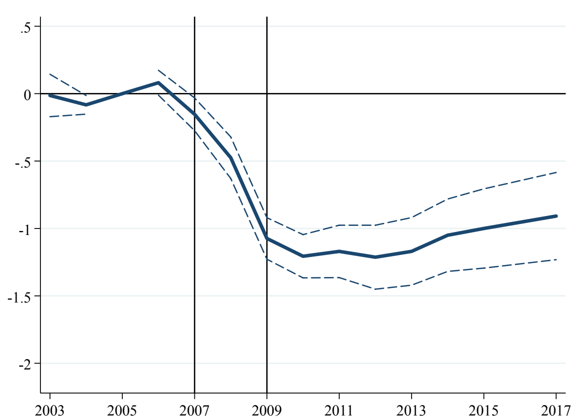
(c) 1990–1991 Recession



(d) 2001 Recession



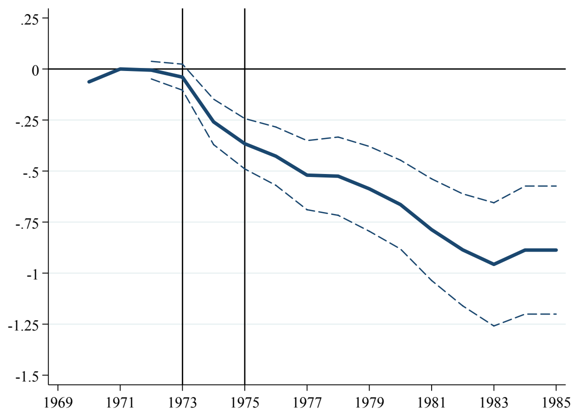
(e) 2007–2009 Recession



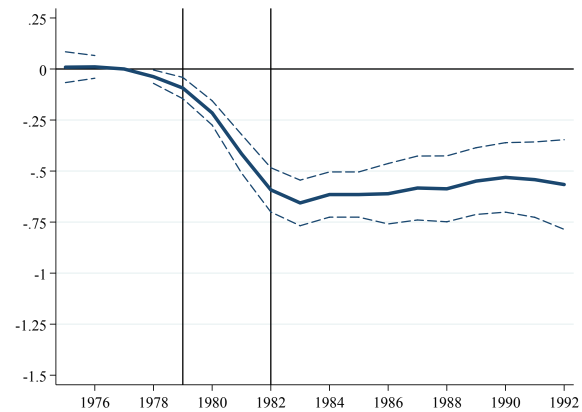
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log employment from CBP data. See notes to Figure 4.
Source: Authors' calculations using CBP, BEAR, and SEER data.

Figure A.3: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Establishments from CBP

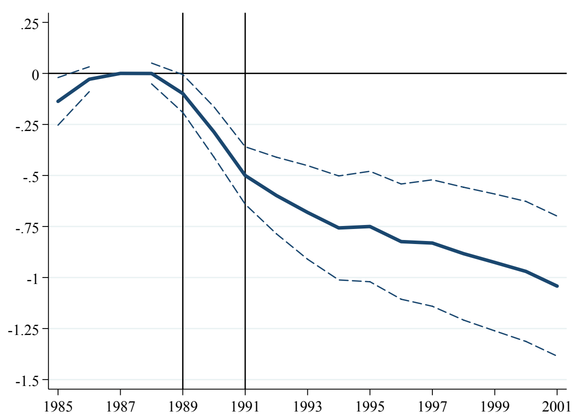
(a) 1973–1975 Recession



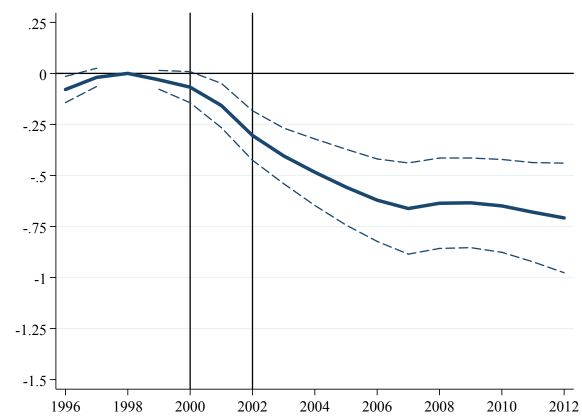
(b) 1980–1982 Recession



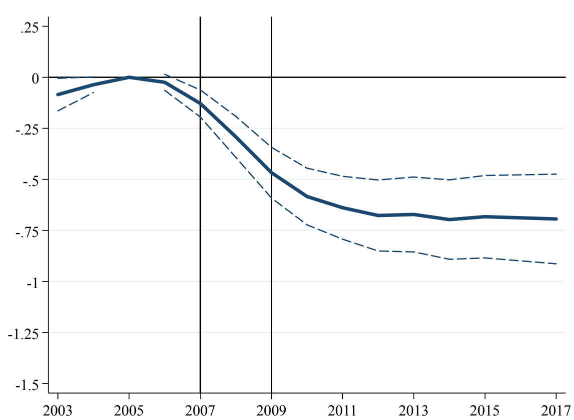
(c) 1990–1991 Recession



(d) 2001 Recession

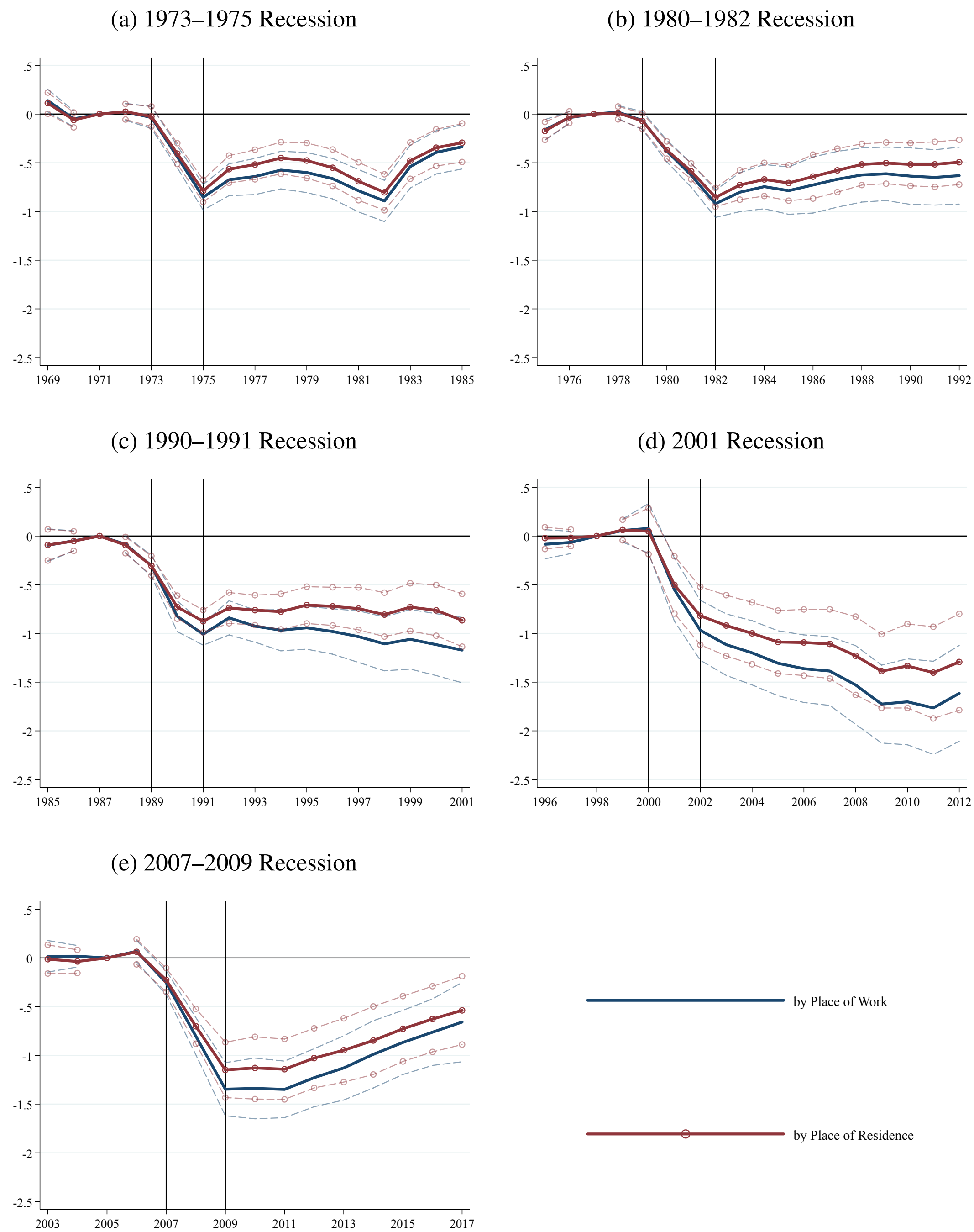


(e) 2007–2009 Recession



Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log establishments from CBP data. See notes to Figure 4.
Source: Authors' calculations using CBP, BEAR, and SEER data.

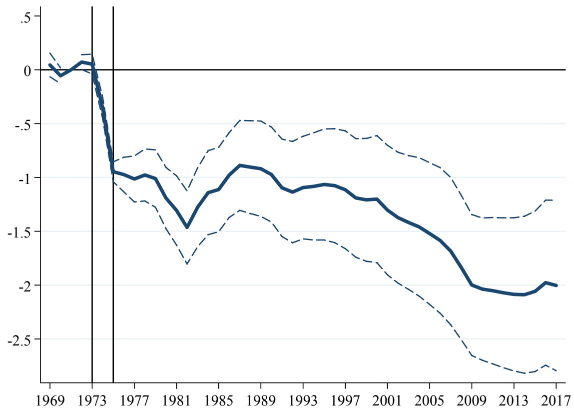
Figure A.4: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Real Earnings per Capita, Robustness to Different Earnings Measures



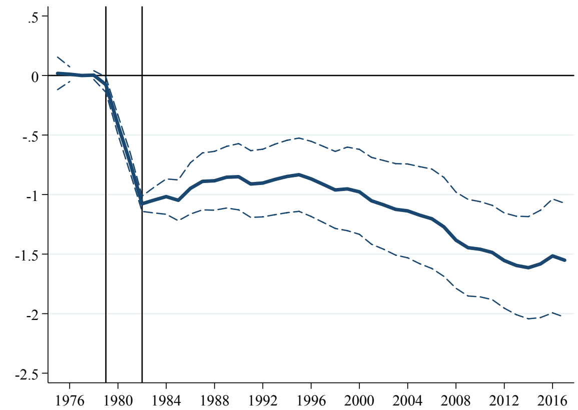
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variables are log real earnings per capita (age 15+), either by place of work or place of residence, as indicated in the legend. For comparability, both measures exclude contributions to government social insurance but include proprietors' income; this is distinct from the earnings measure in Figure 9, which excludes proprietors' income. (Proprietors' income is separable from earnings by place of work but not place of residence).
Source: Authors' calculations using BEAR and SEER data.

Figure A.5: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment, Longer Horizon

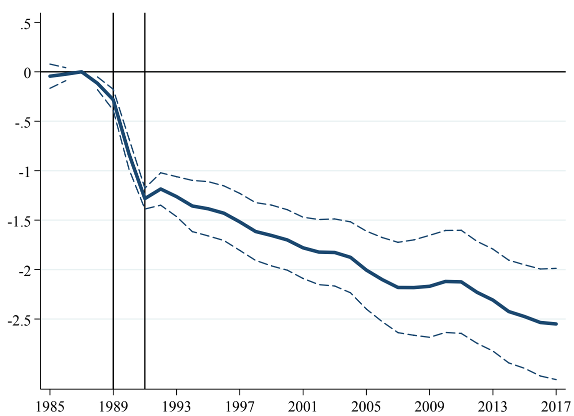
(a) 1973–1975 Recession



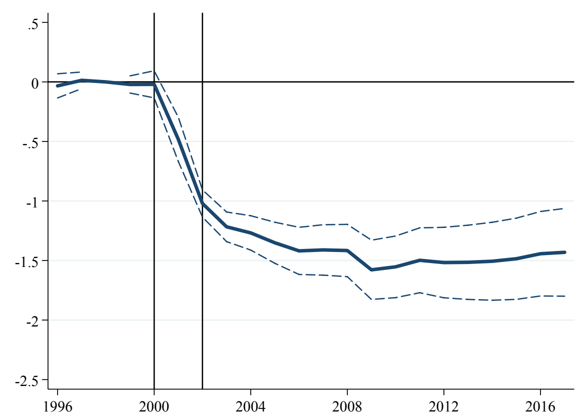
(b) 1980–1982 Recession



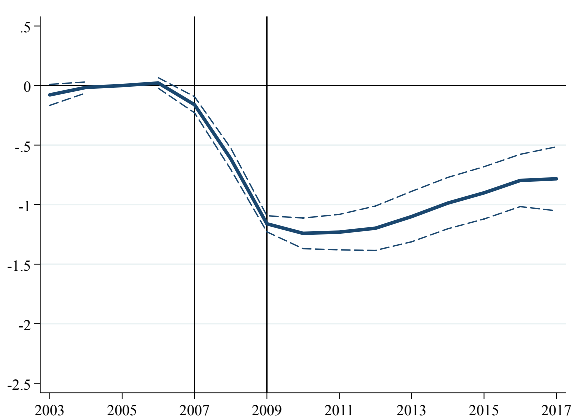
(c) 1990–1991 Recession



(d) 2001 Recession



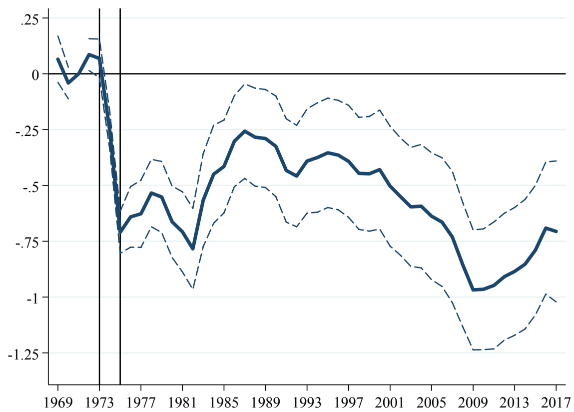
(e) 2007–2009 Recession



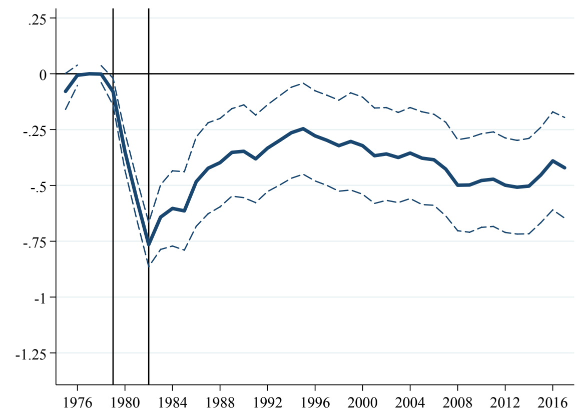
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log wage and salary employment from BEAR data. See notes to Figure 4, which reports estimates over a shorter time horizon. Source: Authors' calculations using BEAR and SEER data.

Figure A.6: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment-Population Ratio, Longer Horizon

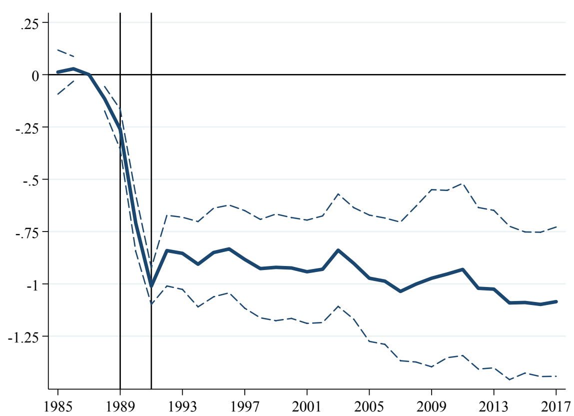
(a) 1973–1975 Recession



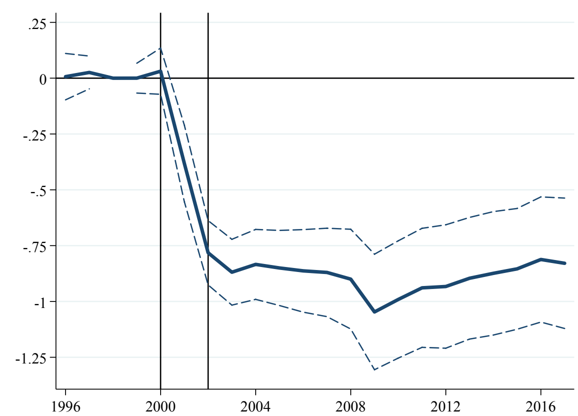
(b) 1980–1982 Recession



(c) 1990–1991 Recession



(d) 2001 Recession

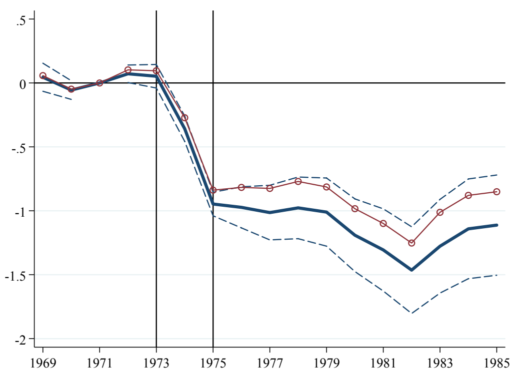


(e) 2007–2009 Recession

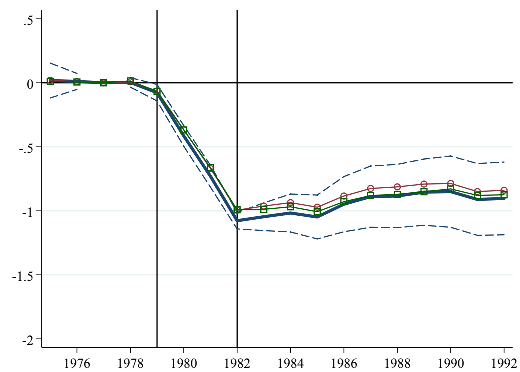
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log ratio of wage and salary employment to population age 15 and above. See notes to Figure 7, which reports estimates over a shorter time horizon.
Source: Authors' calculations using BEAR and SEER data.

Figure A.7: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment, Robustness to Different Log Employment Change Measures

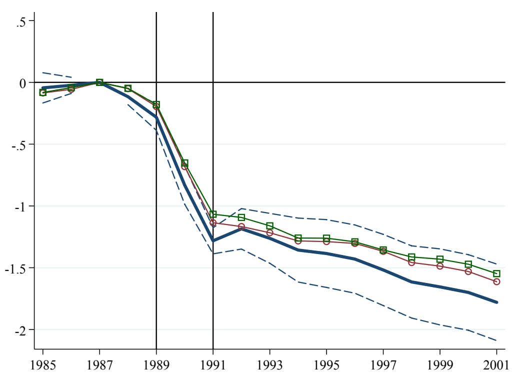
(a) 1973–1975 Recession



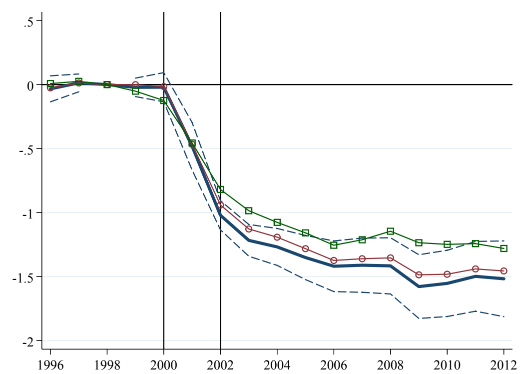
(b) 1980–1982 Recession



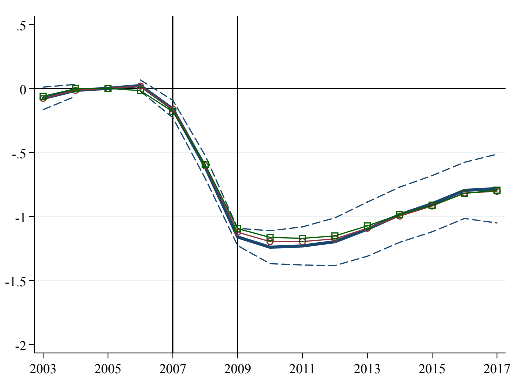
(c) 1990–1991 Recession

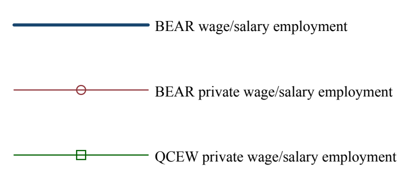


(d) 2001 Recession



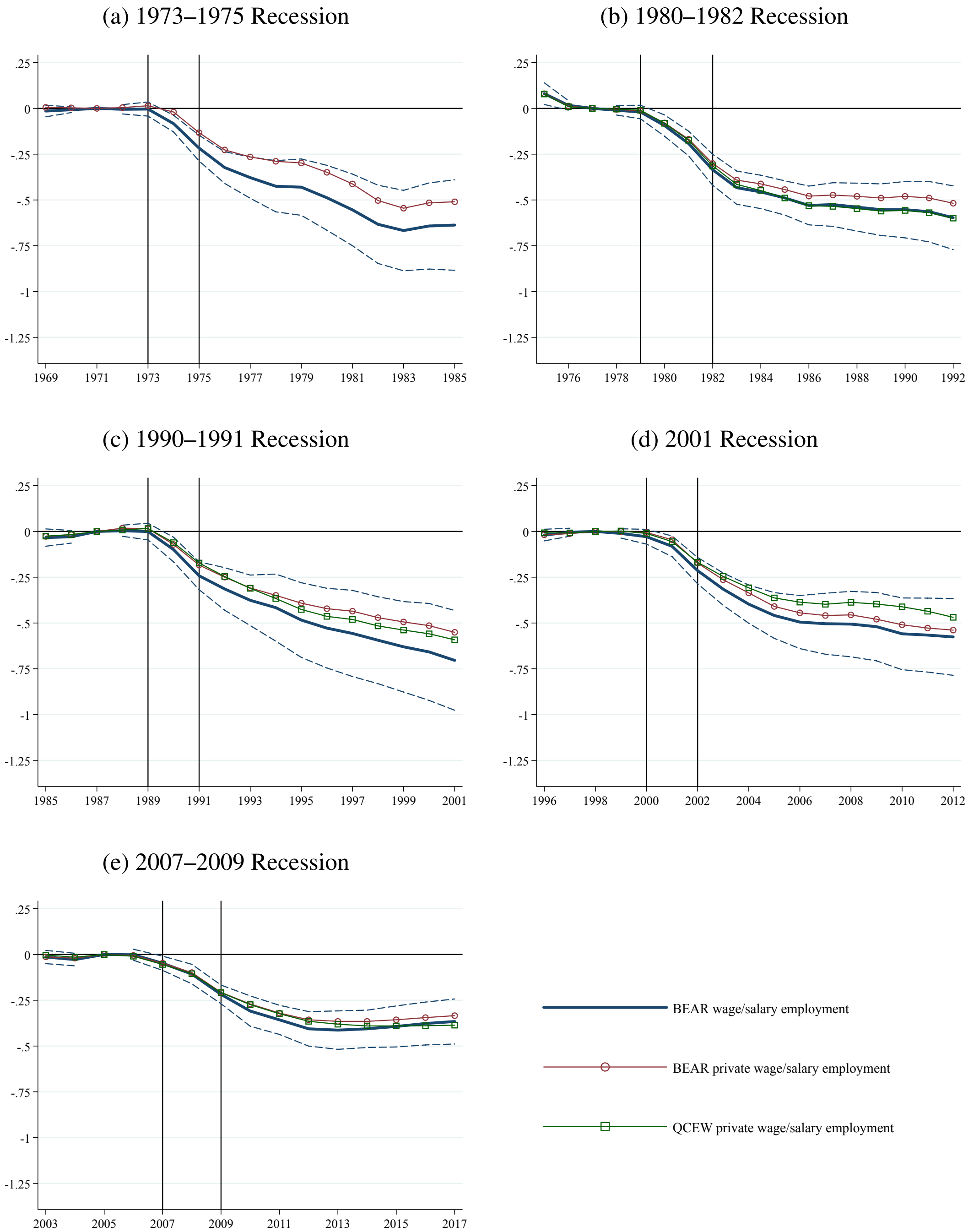
(e) 2007–2009 Recession





Notes: Figure reports estimates of equation (9), 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 log employment change.
Source: Authors' calculations using BEAR, QCEW, and SEER data.

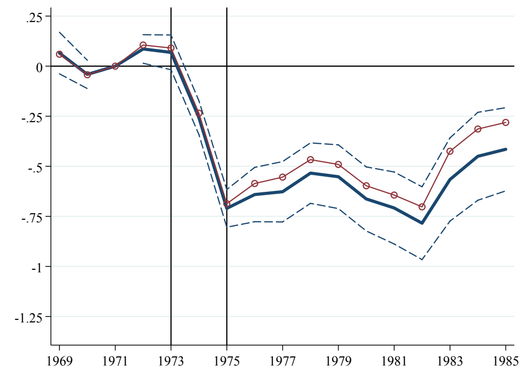
Figure A.8: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Population Age 15+, Robustness to Different Log Employment Change Measures



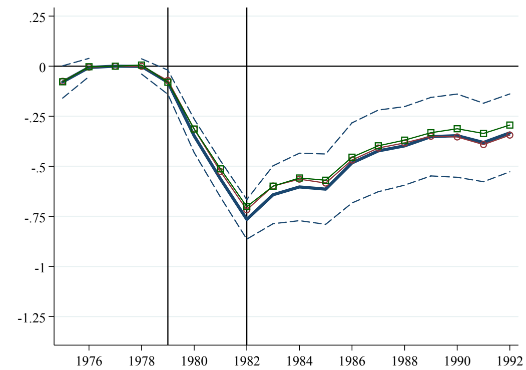
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log population age 15 and above, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary log employment change.
Source: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.9: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment-Population Ratio, Robustness to Different Log Employment Change Measures

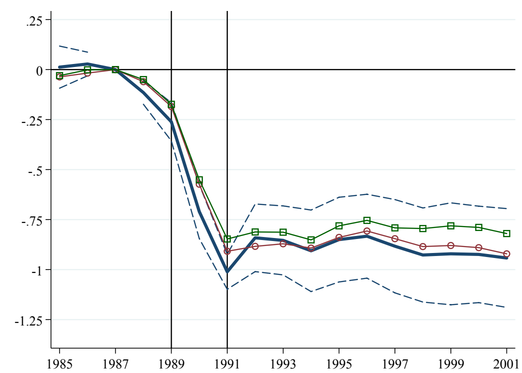
(a) 1973–1975 Recession



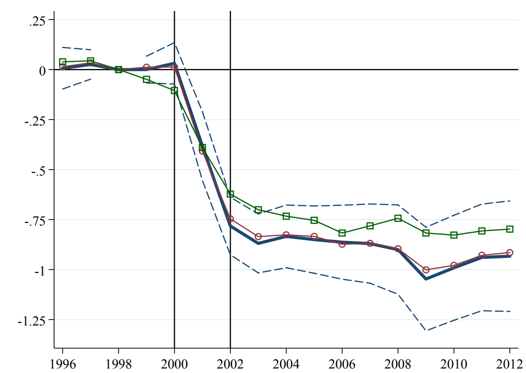
(b) 1980–1982 Recession



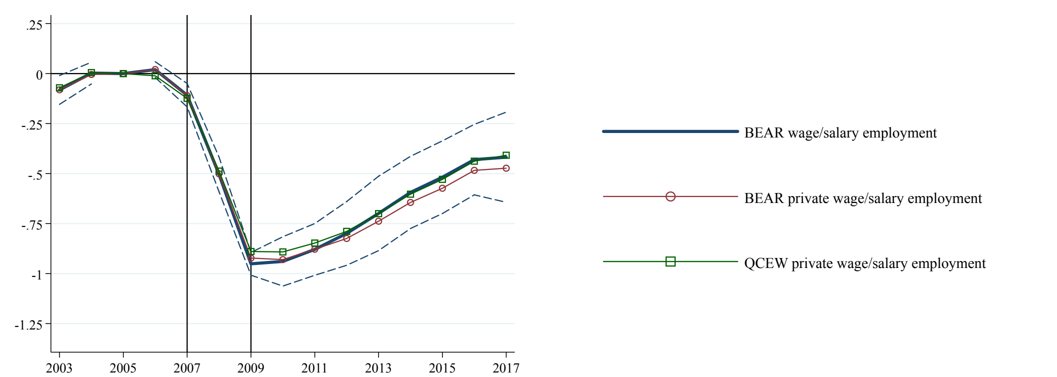
(c) 1990–1991 Recession



(d) 2001 Recession

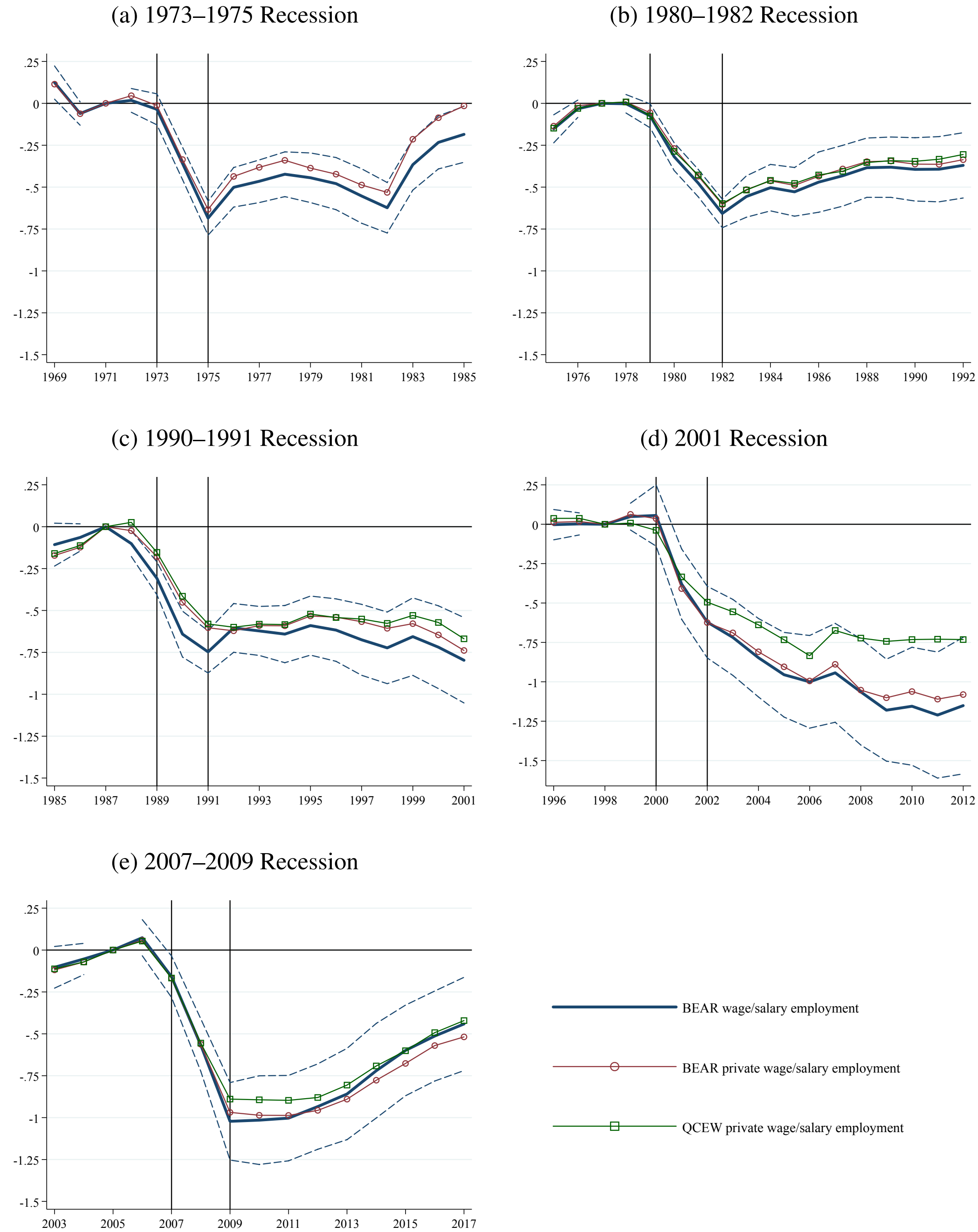


(e) 2007–2009 Recession



Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 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 log employment change.
Source: Authors' calculations using BEAR, QCEW, and SEER data.

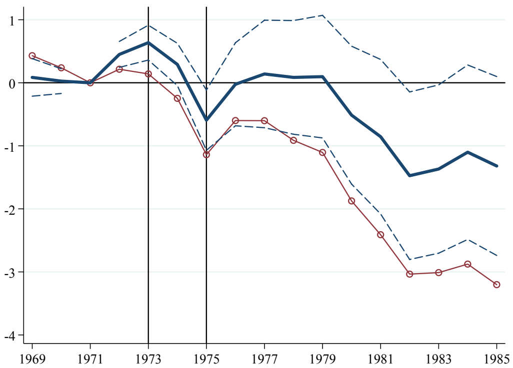
Figure A.10: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Real Earnings per Capita, Robustness to Different Log Employment Change Measures



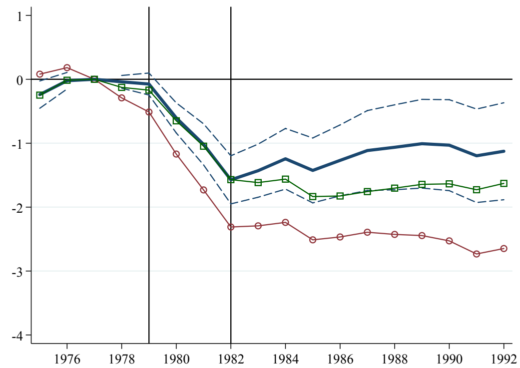
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log real earnings per capita (age 15+), and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying point estimates by the ratio of the standard deviation of the independent variable to the standard deviation of the BEA wage/salary log employment change.
Source: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.11: Impacts of Predicted Log Employment Decreases During Recessions on Metropolitan Area Log Employment

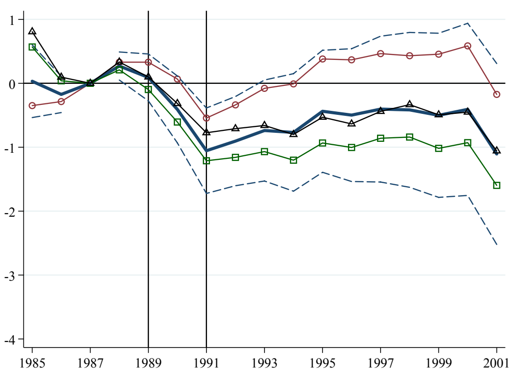
(a) 1973–1975 Recession



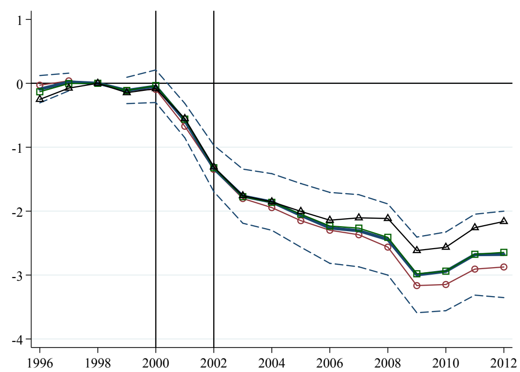
(b) 1980–1982 Recession



(c) 1990–1991 Recession

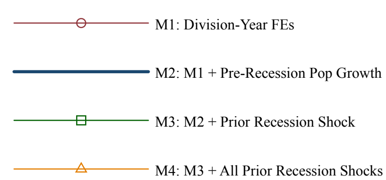


(d) 2001 Recession



(e) 2007–2009 Recession

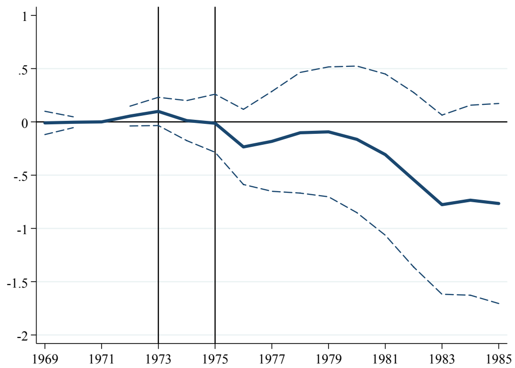
(f)



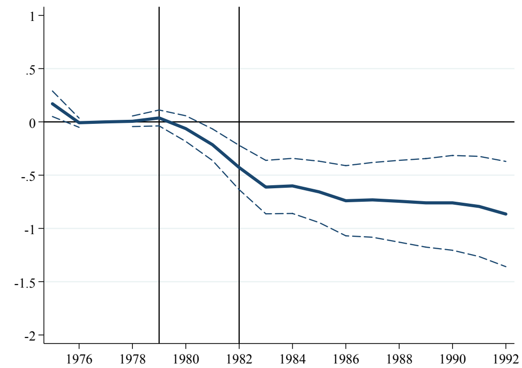
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the predicted log employment change as in Bartik (1991). Specifications are indicated by the legend. See notes to Figure 4.
Source: Authors' calculations using BEAR, CBP, and QCEW data.

Figure A.12: Impacts of Predicted Log Employment Decreases During Recessions on Metropolitan Area Log Population

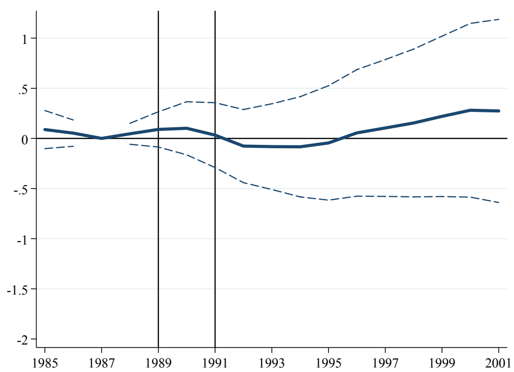
(a) 1973–1975 Recession



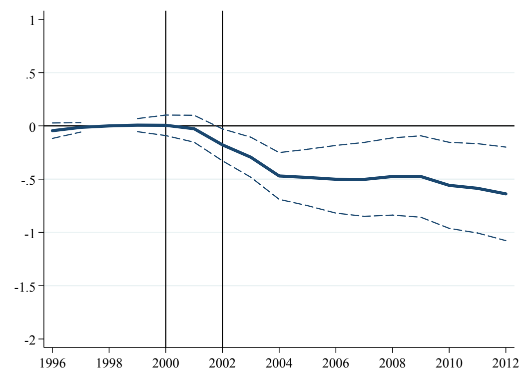
(b) 1980–1982 Recession



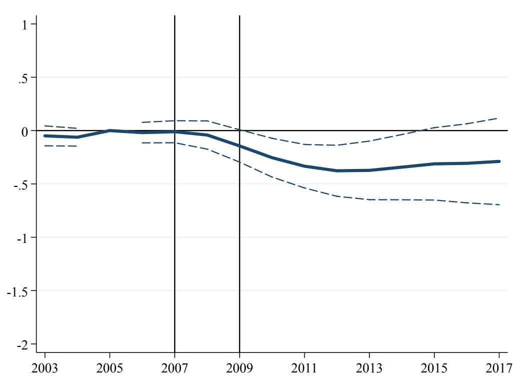
(c) 1990–1991 Recession



(d) 2001 Recession



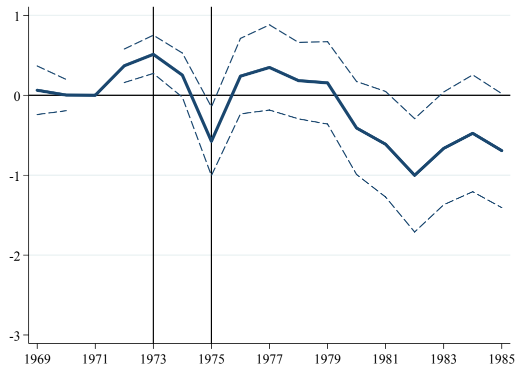
(e) 2007–2009 Recession



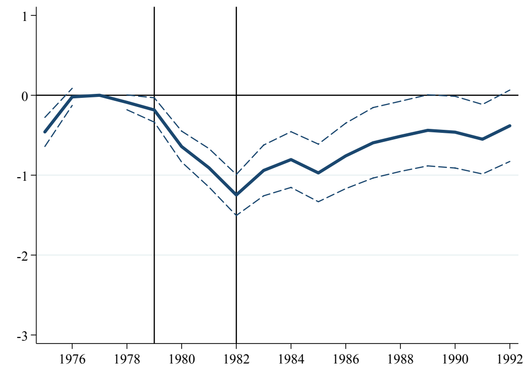
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure A.11.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.13: Impacts of Predicted Log Employment Decreases During Recessions on Metropolitan Area Log Employment-Population Ratio

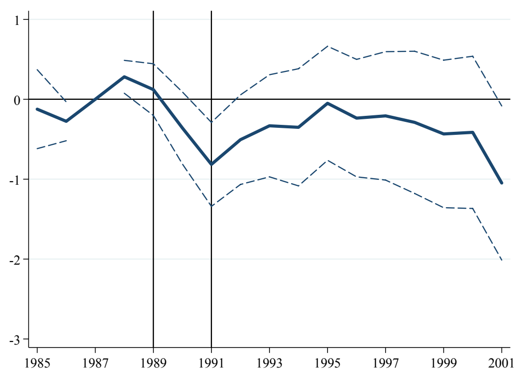
(a) 1973–1975 Recession



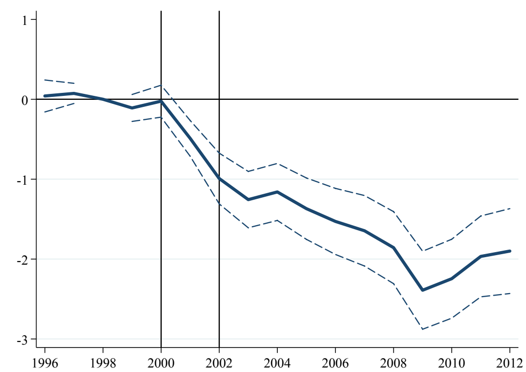
(b) 1980–1982 Recession



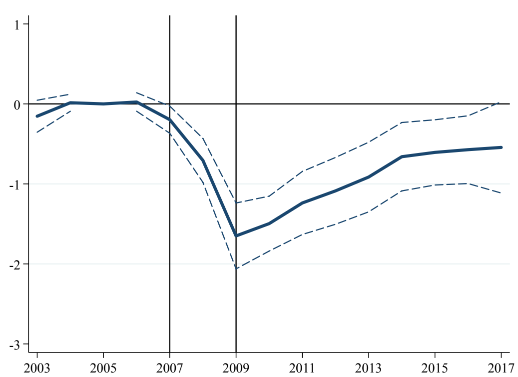
(c) 1990–1991 Recession



(d) 2001 Recession



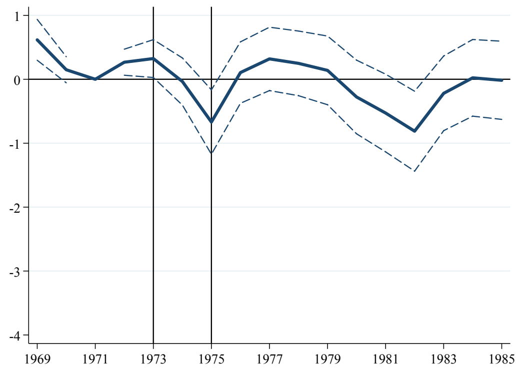
(e) 2007–2009 Recession



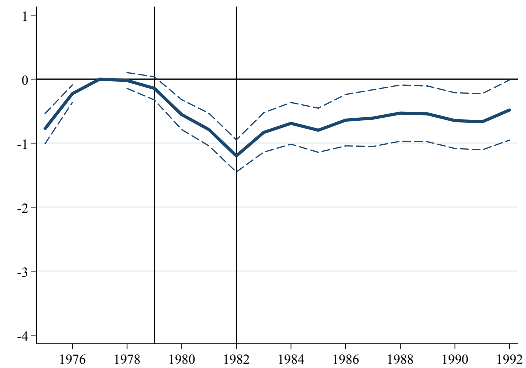
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 15 and above. See notes to Figure A.11.
Source: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.14: Impacts of Predicted Log Employment Decreases During Recessions on Metropolitan Area Log Real Earnings per Capita

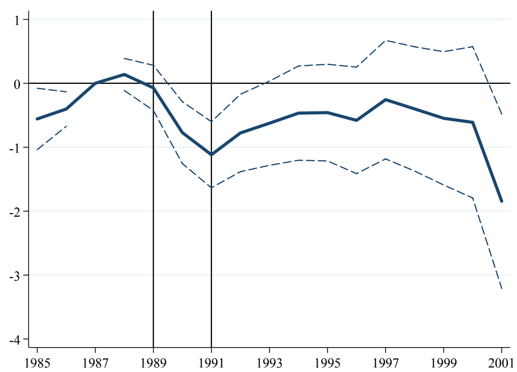
(a) 1973–1975 Recession



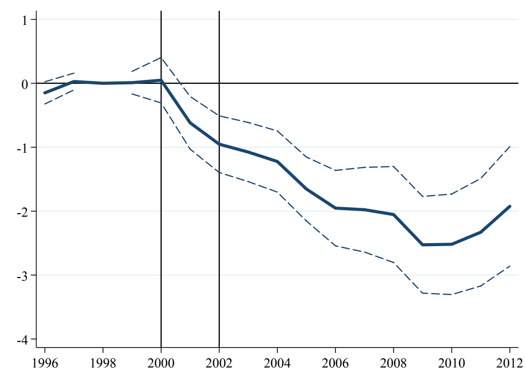
(b) 1980–1982 Recession



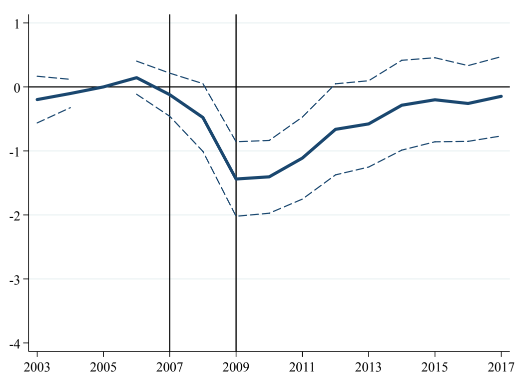
(c) 1990–1991 Recession



(d) 2001 Recession

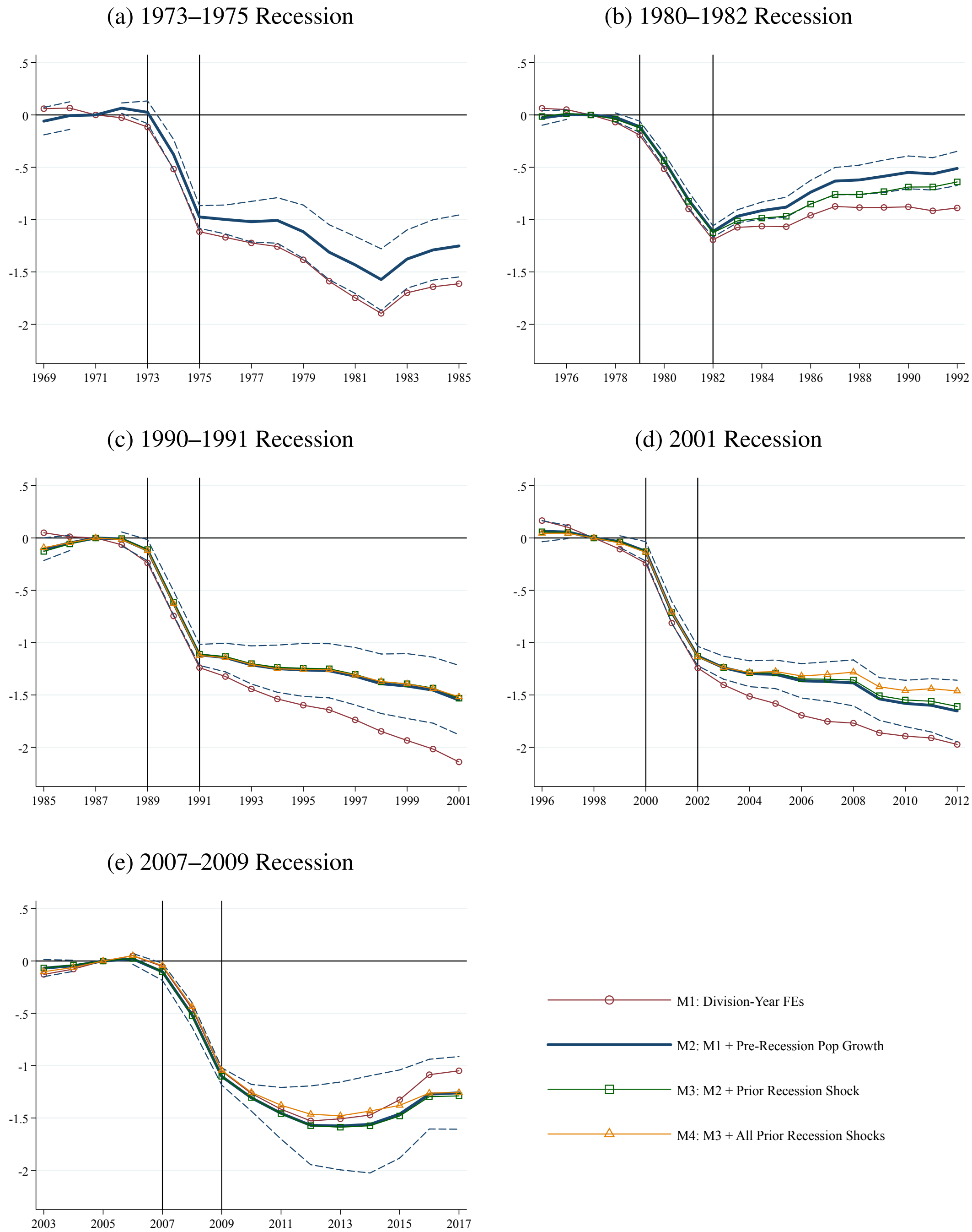


(e) 2007–2009 Recession



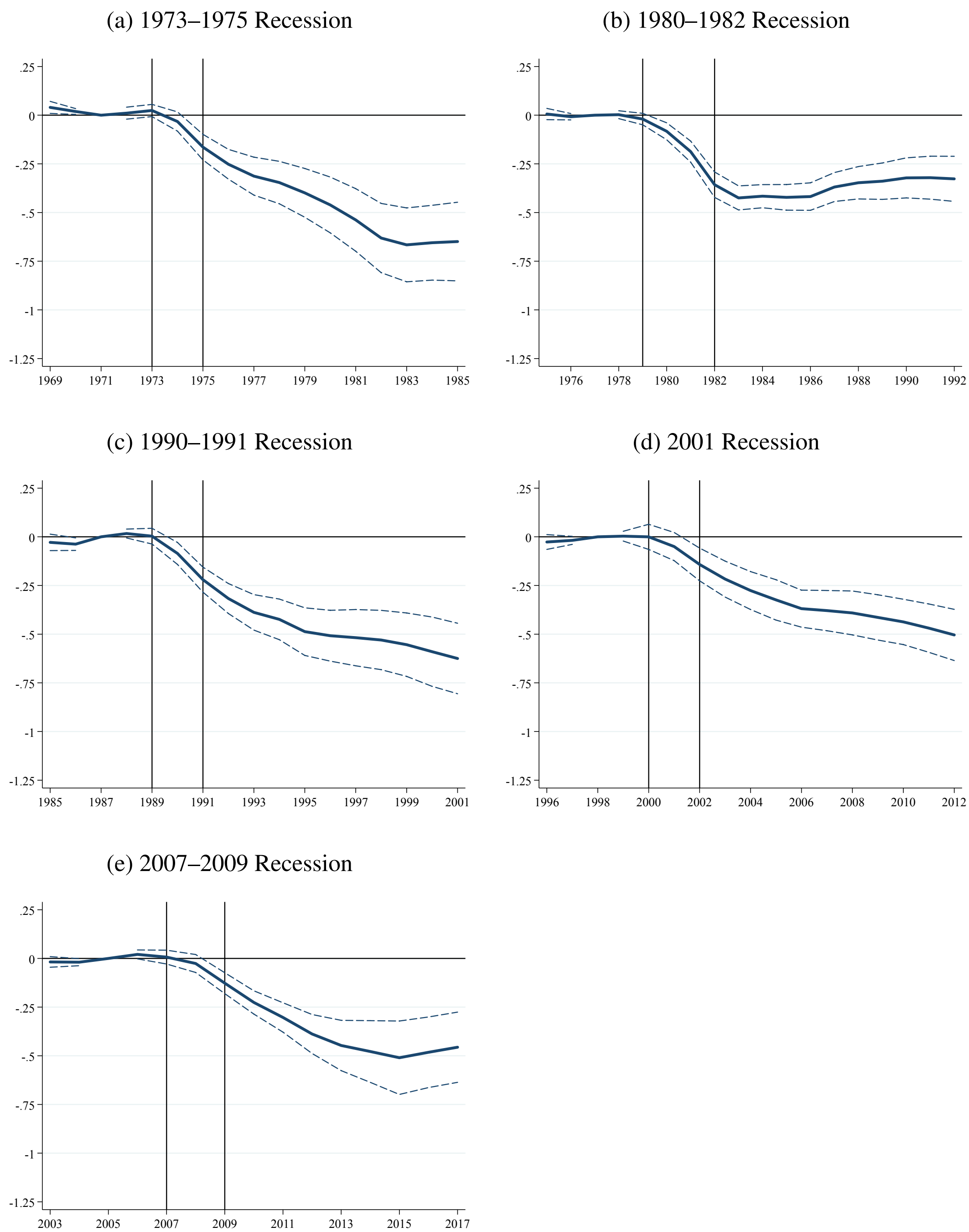
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure A.11.
Source: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.15: Impacts of Log Employment Decreases During Recessions on Commuting Zone Log Employment



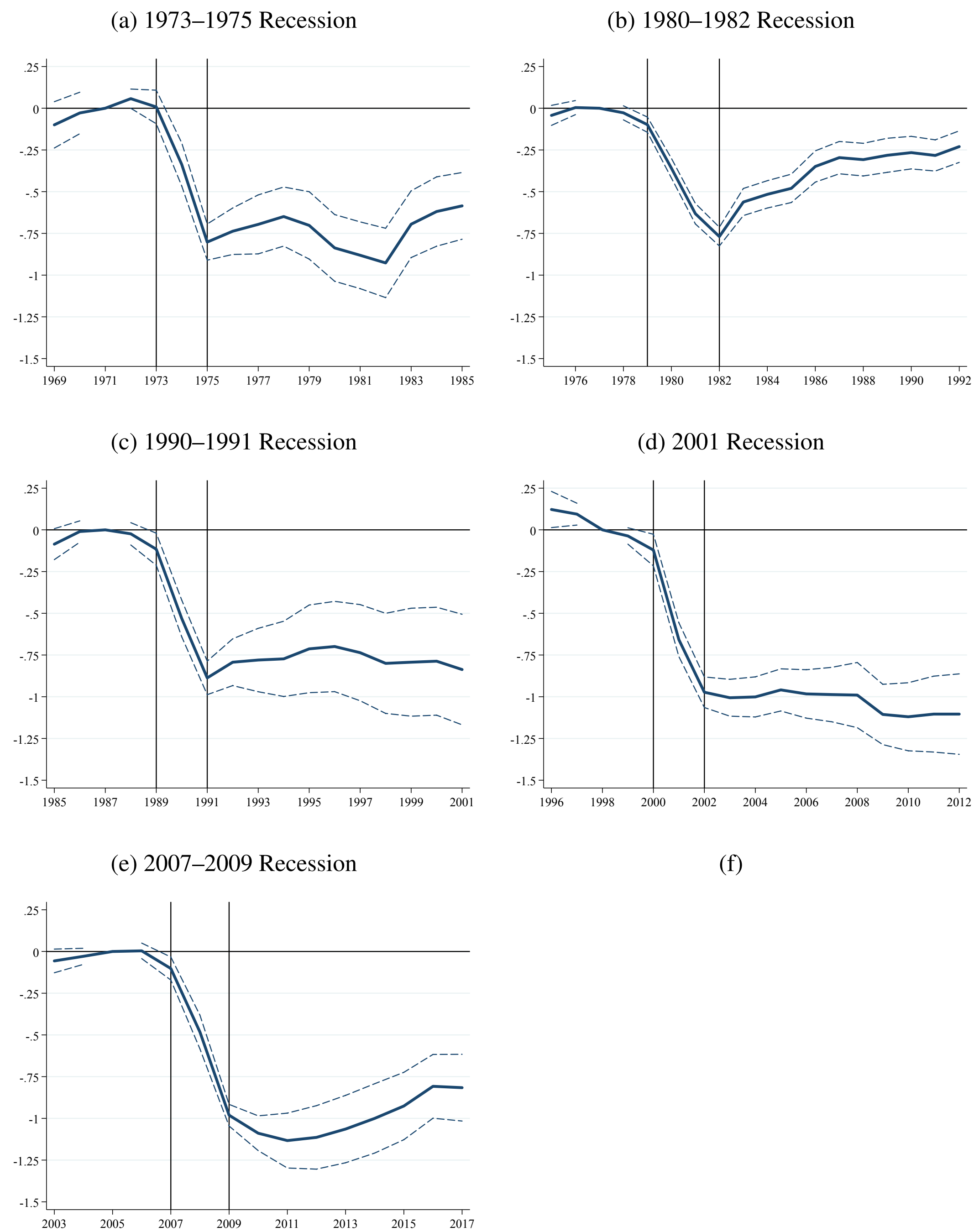
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log wage and salary employment from BEAR data. There are 691 CZs in the sample. Standard errors are clustered by commuting zone. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure A.16: Impacts of Log Employment Decreases During Recessions on Commuting Zone Log Population Age 15+



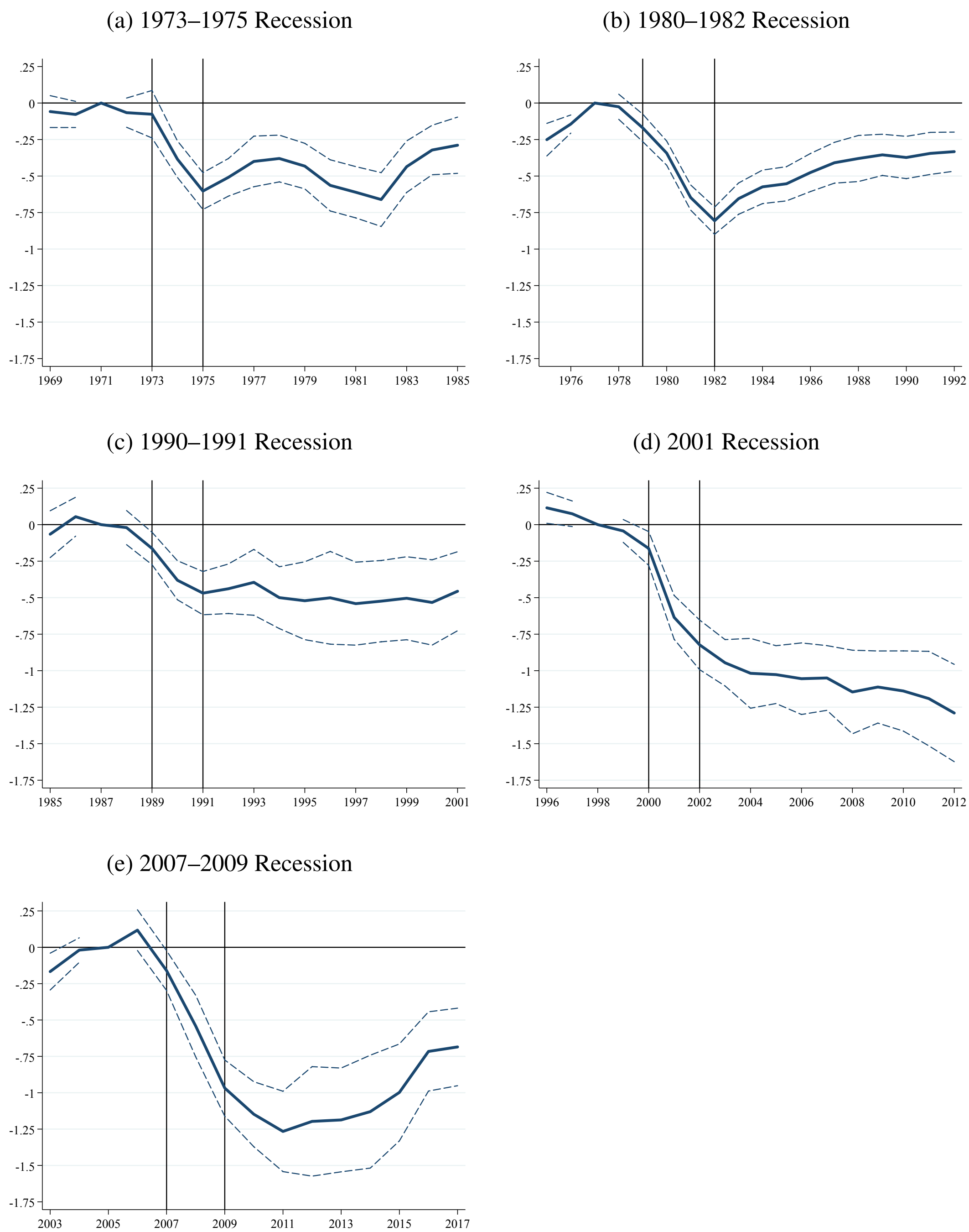
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure A.15.
Source: Authors' calculations using BEAR, SEER, and QCEW data.

Figure A.17: Impacts of Log Employment Decreases During Recessions on Commuting Zone Log Employment-Population Ratio



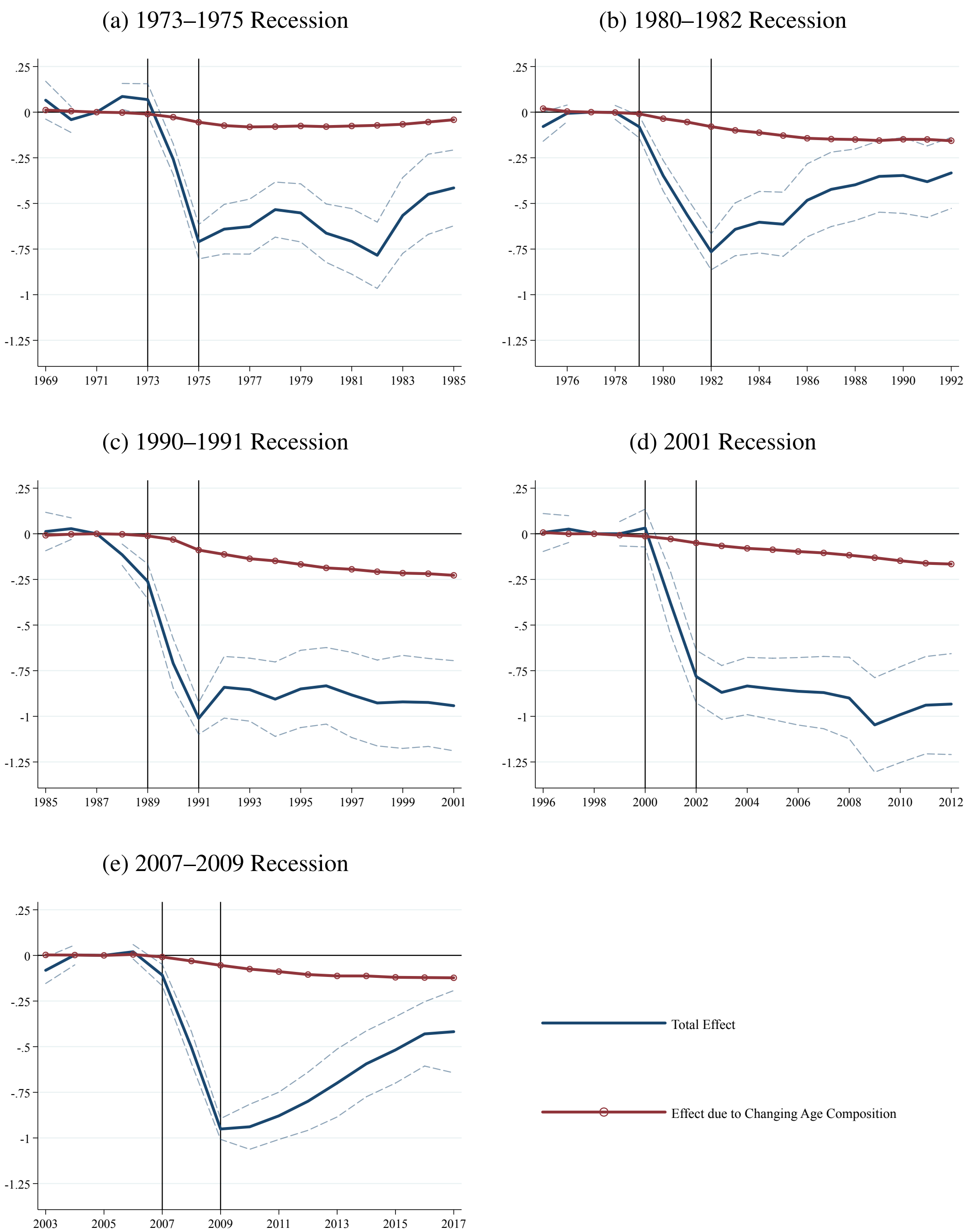
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 15 and above. See notes to Figure A.15.
Source: Authors' calculations using BEAR and SEER data.

Figure A.18: Impacts of Log Employment Decreases During Recessions on Commuting Zone Log Real Earnings per Capita



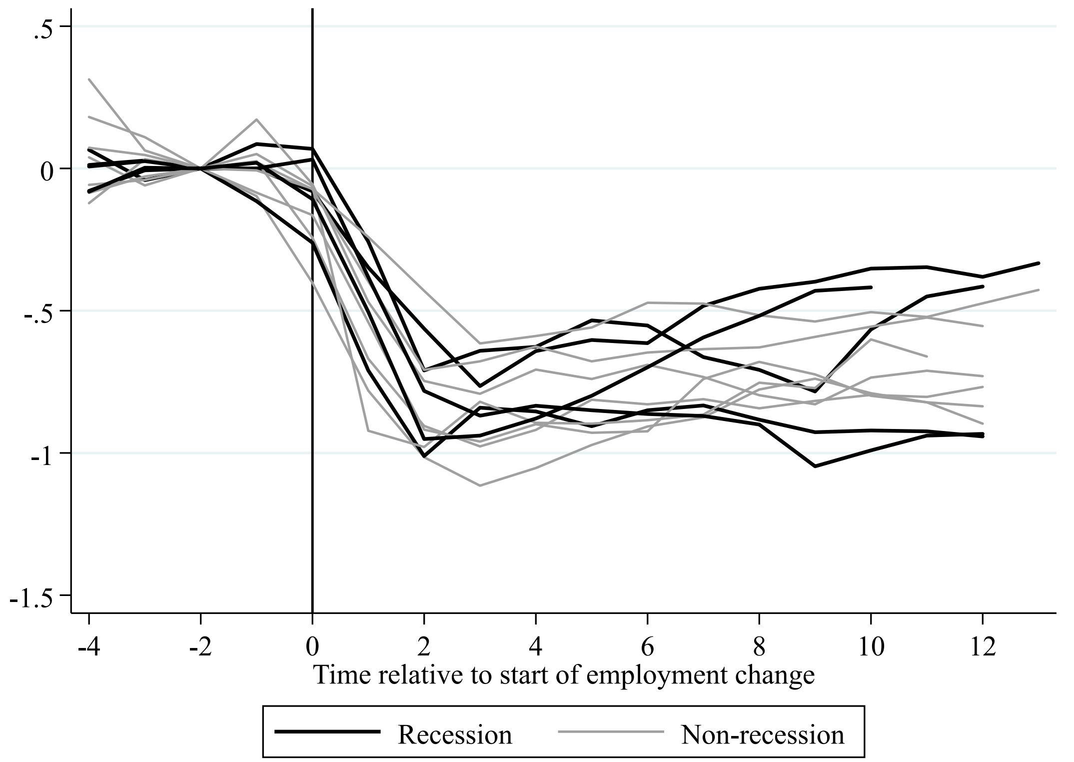
Notes: Figure reports estimates of equation (9), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure A.15.
Source: Authors' calculations using BEAR and SEER data.

Figure A.19: Impacts of Log Employment Decreases During Recessions on Metropolitan Area Log Employment-Population Ratio, Role of Shifts in Age Composition



Notes: The solid blue line displays estimates of equation (9), separately for each recession, where the dependent variable is the log of the ratio of wage and salary employment to population age 15 and above. The line in red circles is the predicted effect on the log employment-population due to the recession-induced impacts on the age structure; we estimate this predicted effect as the product of estimates of equation (9)—where the dependent variables are the share of population age 15–39, 40–64, and over 65—and estimates of the cross-sectional, pre-recession relationship between the log employment-population ratio and these age shares. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

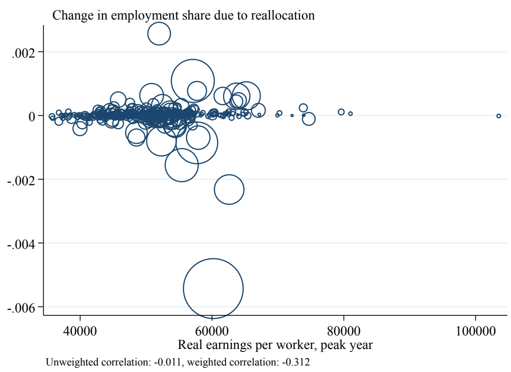
Figure A.20: Impacts of Log Employment Decreases During Recessions and Non-Recession Periods on Metropolitan Area Log Employment-Population Ratio



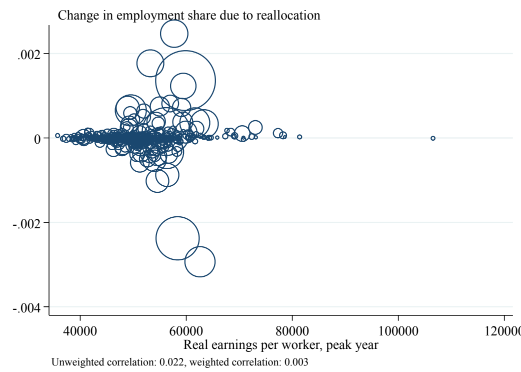
Notes: Figure reports estimates of equation (9), separately for each recession and non-recession period. Recession periods are 1973–1975, 1979–1982, 1989–1991, 2000–2002, and 2007–2009. Non-recession periods are 1976–1978, 1983–1983, 1986–1988, 1992–1994, 1995–1997, 1998–1999, and 2003–2006. We adjust the years over which population growth is measured so that the recession and non-recession periods are handled symmetrically. Event time zero is the first year of the employment change that is the key explanatory variable. The dependent variable is the log ratio of wage and salary employment to population age 15 and above. See notes to Figure 4.
Source: Authors' calculations using BEAR and SEER data.

Figure A.21: Correlation between Reallocation-Induced Change in Employment Share and Peak Year Earnings per Worker

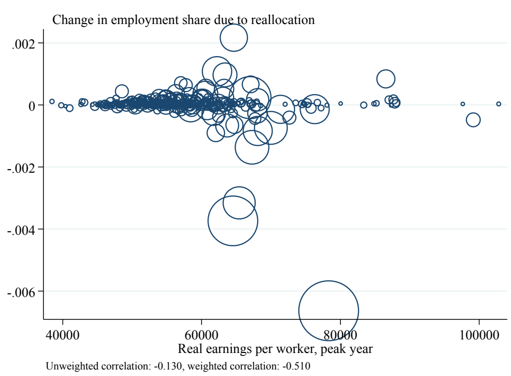
(a) 1973–1975 Recession



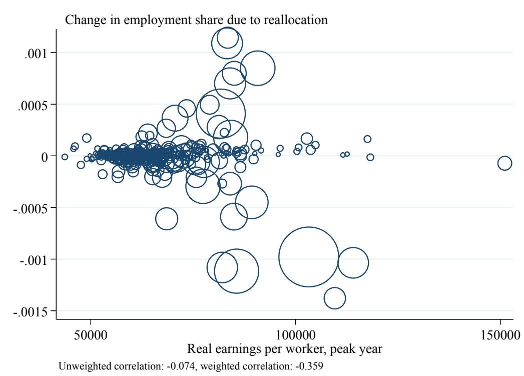
(b) 1980–1982 Recession



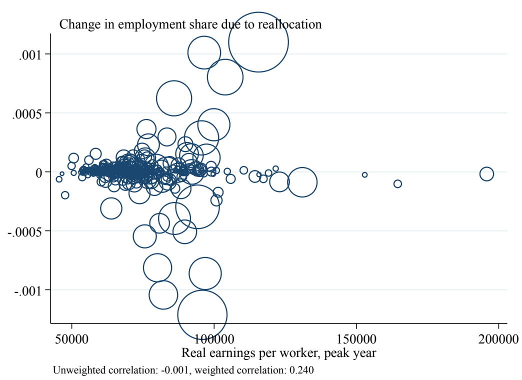
(c) 1990–1991 Recession



(d) 2001 Recession



(e) 2007–2009 Recession



Notes: Change in metro employment share is the employment share under the counterfactual minus the employment share in the peak recession year. Marker size is proportional to peak year employment share. Unweighted and peak-year-employment-share weighted correlations are reported. See notes to Appendix Table A.4.
Source: Authors' calculations using BEAR and SEER data.