Recessions and Local Labor Markets*

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August 19, 2019

Abstract

This paper studies the short- and long-run effects of each U.S. recession since 1973 on local economic activity. We analyze how economic activity evolves across local areas that are differentially affected by national recessions. For each recession, we find that employment, population, employment-to-population ratios, and earnings per capita experience persistent declines for at least a decade after recession's end. While transfers also remain elevated for a decade or more, they are insufficient to fully offset earnings losses, leading to long-term declines in per-capita income as well. Changes in the composition of workers explain less than half of these persistent effects.

JEL Classification Codes: I24, I26, J24, J31

Keywords: recessions, hysteresis, local demand shocks, MSAs, commuting zones, event study

*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 with cleaning and harmonizing the CPS.

1 Introduction

Recessions have received enormous attention from researchers, policymakers, and the public.[1] Most of this attention focuses on short-run, 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 relatively little attention is how recessions affect local labor markets. The size and persistence of these impacts inform labor market dynamism, the economic opportunities of workers and their children, and optimal policy responses.

This paper examines how every recession in the U.S. since 1973 has affected local economic activity.[2] Specifically, we study how earnings, employment, government transfers, and population 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 U.S. Bureau of Economic Analysis, the Census Bureau, and the Bureau of Labor Statistics, to create annual panels of longitudinally-harmonized geographic areas stretching over five decades. We estimate event study models that relate the evolution of local economic activity to sharp employment changes during recessions, while controlling for secular trends in population growth. This empirical strategy allows us to examine whether recessions have temporary or persistent impacts on local labor markets and the extent to which existing transfer programs offset any economic decline.

For every national recession, we find that areas that experience a larger decrease in jobs suffer persistent relative declines in economic activity, with little evidence of a recovery in employment even ten years afterward. Moreover, the sharp decreases in employment that occur during recessions are not associated with differential pre-trends in economic activity beforehand. During and immediately following recessions, the employment decline is driven by manufacturing and construction, two highly pro-cyclical sectors. In the longer-term, relative to less-affected areas,

[1] For example, economists have studied the welfare costs of recessions (see the review in Lucas, 2003), how recessions affect international trade (e.g., Eaton et al., 2016), how recessions affect wages (e.g., Solon, Barsky and Parker, 1994) and labor market outcomes more generally (e.g., Elsby, Hobijn and Sahin, 2010).

[2] These recessions include those 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.

employment falls by a similar amount across all industries, including services, trade, and government.

The consequences of this employment decline depend critically on the extent of population adjustment. If population falls by the same amount as employment, then local areas might recover to their pre-recession employment rates and earnings per capita (Blanchard and Katz, 1992). We find that the persistent employment decline is followed by a lasting, but more gradual and smaller decline in population. The smaller population adjustment leads to a persistent decline in the employment-population ratio. A 5-percent recession-induced employment loss, about the median for the Great Recession, inflicts about a 2–4 percent (1–2 percentage point) drop in the employment-population ratio even seven to nine years after the recession ends. After the 2001 and 2007–2009 recessions, for which we can use IRS data to decompose population changes into components due to in- and out-migration, we find that the population decline stems entirely from reduced in-migration to severely hit areas. Moreover, out-migration actually falls after the recession, so this does not account for the reduction in population.

We also find that recessions lead to persistent declines in earnings per capita and persistent increases in transfers per capita. Nearly a decade after recession's end, per-capita earnings are approximately 2–6 percent lower for a 5-percent initial employment decline. The increase in transfers mitigates less than 30 percent of the longer-term decrease in earnings, implying that recessions lead to lasting reductions in income per capita. The most responsive transfer categories in the long-run are not unemployment insurance (which is designed to be temporary) or income maintenance programs, but instead medical transfers (both Medicaid and Medicare) and Social Security, including disability insurance. Education and training transfers are essentially unresponsive.

One possible explanation for the persistent decrease in local economic activity is a change in the composition of residents or jobs following a recession. We see a persistent increase in the share of residents age 65 and above and a decrease in the share of residents age 15–39, but the size of these impacts are modest. To examine other composition shifts, we turn to individual-level data from the decennial Census and American Community Survey (ACS). Following the 1973–1975, 1990–1991, and 2007–2009 recessions, we see a decrease in the share of workers employed in managerial, professional, and technical occupations and an increase in the share in manual and service jobs. For these same recessions, we also see a decrease in the share of residents with a college degree and an increase in the share with a high school degree or less. For the 1980–1982 and 2001 recessions, there is less evidence of a shift in occupation or education composition. To quantify the importance of composition adjustments, we estimate the impacts of recessions on demographic-adjusted local labor market aggregates. Changes in demographics (education, age, sex, and race/ethnicity) explain less than half of the overall impacts on average earnings and income.

Finally, we use the ACS to study how the Great Recession impacted earnings inequality and poverty. In places where the Great Recession was more severe, earnings decreased throughout the distribution. The short-term impacts are larger at the 10th percentile than the 50th or 90th percentiles, but the longer-term impacts are similar. Lower work hours explain about half of the short-term decrease in average earnings, but play less of a role in the persistent longer-run decrease in earnings. We also see a persistent increase in poverty rates following the Great Recession.

By estimating the impacts of every recession since 1973, we can examine whether the impacts of recessions have changed over the last 50 years. In general, we find that the impacts of recessions on local labor markets are quite similar across time. 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). A related point is that even recessions which are less severe in aggregate terms, such as those in 1990–1991 or 2001, can lead to sizable and persistent shifts in the distribution of economic activity across space.

The key contribution of this paper is new evidence of a general and persistent reduction in economic activity in local areas that experience a more severe recession. We also show that existing transfer programs offset a relatively small share of earnings decline. Our work complements several previous studies. Many of these studies examine how labor demand shocks, such as a decrease in manufacturing jobs, affect earnings, employment, and population in local areas (Bartik, 1991; Blanchard and Katz, 1992; Bound and Holzer, 2000; Notowidigdo, 2013; Dao, Furceri and Loungani, 2017). These papers do not study recessions, per se, but instead focus on changes in jobs over one- or ten-year horizons across all phases of the business cycle. As a result, these studies provide limited guidance on the short- and long-run effects of recessions on local areas. Additional evidence is particularly valuable because of the disagreement in the literature over whether labor demand shocks have persistent effects on wages and employment, and how, when, and why these relationships may have changed (Blanchard and Katz, 1992; Bartik, 1993, 2015; Austin, Glaeser and Summers, 2018). Yagan (Forthcoming) uses tax data to show that individuals living in areas severely affected by the Great Recession suffered enduring employment and earnings losses regardless of whether they stayed or moved locations. In comparison, we focus on how recessions impact local labor markets, and we examine more recessions. Greenstone and Looney (2010) and Stuart (2018) provide evidence that recessions lead to persistent declines in earnings per capita at the county-level; our analysis goes considerably further, by examining a much larger range of outcomes and results at other levels of geography.

2 Conceptual Framework

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

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

On the other hand, a recession shock could catalyze a persistent decline in local labor demand, possibly because employers change their production process (Jaimovich and Siu, 2015; Hershbein and Kahn, 2018) or shut down (Foster, Grim and Haltiwanger, 2016).[3] Although the short-term dynamics are similar under either a temporary or persistent decline in labor demand, the latter generates a lasting decline in employment. The response of other variables depends on the elasticity of labor supply. If labor supply is perfectly elastic, then wages, employment rates, and transfers return to their prior trend (Blanchard and Katz, 1992). If labor supply is less than perfectly elastic, then wages and employment rates persistently fall while means-tested transfers rise.

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

The long-run change in economic conditions reflects both demand and supply adjustments. For example, a decline in labor demand might reduce the attractiveness of living in an area, leading to a reinforcing decline in labor supply. Our empirical strategy identifies the net effect of recession shocks on local economic activity. We do not attempt to disentangle the contribution of demand and

[3]The possibility of a persistent decline in local labor demand relates to the relative importance of agglomeration and locational fundamentals as determinants of economic geography. Davis and Weinstein (2002, 2008) find striking evidence of a recovery in Japanese city population and manufacturing employment following Allied bombings in World War II. These results suggest that rationalizing a persistent decline in local labor demand would require that fundamentals change during recessions. This would be very 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).

supply shifts to the long-run change in economic activity; we discuss below why we are confident that the initial recession shock represents a decline in local labor demand.

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

3 Estimating the Impacts of Recessions on Local Labor Markets

3.1 Data

We compile several public-use data sets that together provide a wealth of information on local economic activity and government transfers. The first source is the Regional Economic Accounts from the Bureau of Economic Analysis (BEAR), which measures income, earnings, and government transfers for multiple geographies from 1969 forward. The second source is the Census County Business Patterns (CBP), which tracks for each county the number of establishments and employment, by industry.[4] The third source is the Quarterly Census of Employment and Wages (QCEW), produced by the Bureau of Labor Statistics. These data, used as inputs to construct both the BEAR and CBP series, capture the universe of unemployment-insurance eligible jobs and, as implied by the name, are available quarterly (and even monthly). The first two sources are available from 1969 through 2016, while the QCEW starts in 1975. All three data sources allow us to measure local employment, even at the industry level, although exact coverage varies slightly. We

[4]Because employment counts are often suppressed for small counties and industries in County Business Patterns, we use the imputation procedure adopted in Holmes and Stevens (2002) and Stuart (2018) when necessary; we also supplement the CBP files released by the Census Bureau with WholeData, an industry-harmonized version of the data, available from 1998 through 2017, that uses a linear programming algorithm to recover suppressed employment estimates (Bartik et al., 2019). For periods where they overlap, the results using the imputation procedure in Holmes and Stevens (2002) and Stuart (2018) agree closely with those using Wholedata.

view them as complements, and they allow us to check the robustness of some results to different employment measures. As we describe below, we also use these data to define our key local labor demand shock of employment change. Although the BEAR data are used for our preferred measure, owing to their longer availability and design as a time series, the higher frequency of the QCEW data provide greater flexibility in cyclical timing than the annual series in BEAR or CBP.

We supplement the above sources with population estimates from the Census Bureau and the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. Although both Census and SEER provide annual population counts, allowing economic outcomes to be standardized by population, SEER uses other Census data and modeling to estimate population by sex, race, and age, as well.

These four data sets allow us to measure aggregate economic outcomes at geographies resembling local labor markets. However, given recent research that documents heterogeneous impacts of recessions across different types of individuals (Hoynes, Miller and Schaller, 2012), we are also interested in distributional outcomes, such as measures of wage inequality, poverty, and part-time employment. Furthermore, as described above, recessions may catalyze lasting compositional change in an area's population and workforce, either through differential migration (Blanchard and Katz, 1992), change in skill and technology demand by employers (Hershbein and Kahn, 2018), or other forces. To capture distributional and compositional measures, we supplement the datasets described above with additional sources.

The decennial Census and the American Community Survey (ACS), the successor to the long-form of the Census, offer large sample sizes available at relatively detailed geographies, and the microdata allow us to investigate distributional and compositional outcomes for important subgroups, such as the prime-age population of 25–54-year-olds.[5] Unfortunately, only the ACS is annual, and substate geographic identifiers are available starting only in 2005. Thus, while we can track the evolution of outcomes year by year for the Great Recession, we use changes over longer

[5]We use versions of these microdata from IPUMS (Ruggles et al., 2019). The Data Appendix describes the processing of these data and how we link individuals to our geographies of interest.

intervals for earlier recessions.[6]

To address the role of differential migration, we draw upon the Statistics of Income (SOI) from the Internal Revenue Service. Compiled from federal tax filings, these data capture migration from one county to another each year. Although they capture moves only for tax filers (with the number of exemptions being used to proxy for the total number of individual movers), they are considered a high-quality source for point-to-point migration flows and have been used in several papers (e.g., Kaplan and Schulhofer-Wohl, 2012, 2017; Wilson, 2018).[7]

With the exceptions of the decennial Census and ACS (and CPS) 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), which we can stochastically map to other geographies using crosswalks available from the Geocorr program of the Missouri Census Data Center.[8] Consequently, we can examine the effects of recessions at multiple levels of geography: metropolitan area and commuting zone.[9] 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).[10] 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 neither definition is fixed over time; we consistently 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 the metro areas because these are likely to have thicker labor markets, we show that our core results are all robust to using commuting zones, which unlike

[6]We have also explored using the Current Population Survey, which contains many of the same demographic items as the Census and ACS and provides meaningful substate geography indicators starting in 1989 in the basic monthly version of the data. While we have harmonized these substate geographies over time (see Data Appendix), changes in sampling result in relatively few areas with sufficient sample sizes to offer meaningful analysis.

[7]We use a version of these data compiled by Janine Billadello of Baruch College's Geospatial Data Lab (Billadello, 2018).

[8]The CPS provides county identifiers in only some cases but metropolitan areas are available from 1989.

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

[10]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).

metro areas cover the entire United States.

3.2 Empirical Strategy

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

One natural approach is to estimate the event study regression

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

where $y_{i,t}$ is a measure of local economic activity or government transfers in location i and year t, shock_i is the log employment change in location i from the nationwide peak to trough (multiplied by -1), $x_{i,t}$ is a vector of control variables, and μ_i is a location fixed effect that absorbs time-invariant differences across locations. The key parameter of interest is δ_t, which describes the relationship between the recession shock and local economic activity in year t. The inclusion of location fixed effects means that one of the δ_t coefficients must be normalized; we do this three years before the nationwide peak because the exact timing of recessions is uncertain and there is variation in when aggregate economic indicators decline.[11] This specification allows the impact of the recession shock to vary flexibly across years, transparently showing both pretrends and dynamic impacts.

An important issue with estimating equation (1) in our setting is that log employment is both an outcome of interest and used to construct the recession shock. This can introduce a mechanical correlation between the dependent variable and the shock variable, so that estimates of δ_t are inconsistent under a model in which log employment evolves as a stationary random walk.[12] Instead,

[11] 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.

[12] To see this problem, consider normalizing $\delta_t = 0$ for the peak year $t = t_0$. Equation (1) 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 the recession shock is $\text{shock}_i \equiv y_{i,t_1} - y_{i,t_0}$. It is straightforward to show that, if $y_{i,t}$ follows a stationary random we estimate

$$y_{i,t} = \text{shock}_i \delta_t + x_{i,t}\beta + y_{i,t_0-3}\gamma_t + \varepsilon_{i,t}. \tag{2}$$

Equation (2) does not include location fixed effects, but instead controls for cross-sectional differences using the dependent variable three years before the peak, y_{i,t_0-3}. We allow the coefficient γ_t to vary by year to increase the flexibility of this control. Unlike equation (1), estimates of δ_t from equation (2) generally are consistent under the null hypothesis of a random walk process.

Our outcomes $y_{i,t}$ include log employment (as measured in BEAR, CBP, or QCEW), log population age 15 and above (as measured in SEER), the log employment-population ratio for individuals age 15 and above, as well as earnings and total government transfers per capita (where the numerators are from BEAR and the denominators from SEER).[13] We also examine overall income per capita (equal to earnings plus transfers) and specific types of transfers, again drawing from BEAR. To examine earnings and income percentiles (inequality) and compositional changes in the population (such as the share with a college degree), we draw on decennial Census and ACS data.[14]

We construct the recession shock using annual employment data from BEAR.[15] We modify NBER recession peak and trough dates to account for our use of annual data. Specifically, we define shock_i to be the log employment change for each geography between 1973–1975, 1979–1982, 1989–1991, 2000–2002, and 2007–2009.[16] Using fixed national timings for each recession, rather than location-specific peak-to-trough periods, introduces some measurement error but minimizes

walk, the probability limit of δ_t is equal to 1/2 for all years (besides the trough year, where the coefficient equals 1 mechanically). We mitigate this problem by normalizing δ_t three years before the peak, but still prefer equation (2) because it has better properties for any choice of normalization year and can be extended to control for a vector of lagged dependent variables.

[13]The local area data sets measure the number of jobs, as opposed to the number of workers. As a result, our employment-population ratio does not precisely measure the share of people who are employed (e.g., the two might differ because of multiple job holding). When using the decennial Census and ACS data, we can construct the share of individuals who are employed.

[14]We continue to explore using the CPS, which offers annual data, but much smaller samples.

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

[16]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. We treat the double-dip recessions of the early 1980s as a single recession.

the risk of endogeneity. We use wage and salary employment (private and public) to define the recession shock, as coverage of the self-employed is incomplete and varies over time.

We include several control variables in $x_{i,t}$ to bolster our interpretation of estimates of δ_t as reflecting the effect of local recession shocks. First, we include Census division-by-year fixed effects to flexibly capture broader changes in economic conditions and demographics. Second, we control for interactions between pre-recession population growth and year to adjust for slow-moving changes in population and demographics.[17] In certain specifications, we also control for the severity of prior recession, although this ultimately has little impact on our results.

Estimates of δ_t capture the relationship between local recession shocks and *relative* changes in economic activity before and after recessions. For example, although aggregate employment trended upward throughout our sample period, estimates of δ_t do not reflect this aggregate movement, as changes in economic activity at the division-year level are absorbed by fixed effects. A negative value of δ_t implies that a more severe shock reduces economic activity relative to a less severe shock; the estimates seek to compare outcomes to what would have existed in the absence of the recession. Although for simplicity we do not always frame our discussion in relative terms explicitly, all our findings should be interpreted in this manner.

Our primary results focus on metropolitan areas, which cover between two-thirds and five-sixths of workers over our sample period. However, we also examine the effects on commuting zones.[18] We cluster our standard errors at the level of the geography used to allow for arbitrary autocorrelation in the error term $\varepsilon_{i,t}$.

3.3 The Severity of Recessions Across Time and Space

Before moving to estimates of equation (2), we describe the characteristics of the five recessions that are our focus. Figure 1 displays aggregate seasonally adjusted, nonfarm employment from the

[17]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).

[18]In the future, we intend to replicate our main results at the state and county level.

Current Employment Statistics from 1969 to 2016. Nationwide employment more than doubled over this period. This growth was interrupted by five recessions (combining the two in the early 1980s), as indicated by the vertical shaded bars in the graph. While there is little consensus on the macroeconomic causes of each recession, the drivers almost certainly differ across recessions (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 the housing and financial markets.

Using annual data from BEAR, Table 1 shows the national changes in employment from peak to trough for each of these recessions, both overall and for major industrial sectors. (We use BEAR data rather than monthly national CES aggregates to be consistent with our subsequent analysis, but the patterns are qualitatively similar.) The recessions vary greatly in overall magnitude, from a 3 percent decline during the Great Recession to a 1 percent increase from 1989–1991, with the others falling in between. Manufacturing and construction, among the more cyclical industries, are usually the most negatively affected, with the exception of construction during the 2001 recession, which was buoyed by the concomitant housing boom. However, the impact on other industries varies widely across recessions. The early 1990s downturn and the Great Recession were broad in scope, with most major industries experiencing a decline in employment. The early 1980s recession, despite its perceived severity, was heavily concentrated in just a few industries, including the aforementioned manufacturing and construction. Similarly, the mid 1970s recession and the one in 2001 were characterized by several industries that went relatively unscathed or even grew, including the relatively large services sector. Our use of annual BEAR data masks some of the severe employment losses that are evident in monthly CES or QCEW data.

These patterns suggest that areas with employment bases reliant on manufacturing and/or construction were more likely to experience severe recessions, although the variation across recessions in other industries implies that it is not necessarily the same areas being hit each time. This is illustrated in Figure 2, which shows the severity of each recession (as captured by log employment change) across metropolitan areas. While many of the 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 case.[19]

Figure 3 displays the variation in recession intensity in another way, by showing the frequency with which a given 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. Thus, the Detroit and Chicago metros, for example, experienced downturns worse than the median for all five recessions, while the Houston metro did so in only a single recession (2001). More generally, 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, there is moderate positive serial correlation generally, although, consistent with the different origins of the recessions as well as temporal changes in industrial mix, the pattern is not monotonic across time. Notably, the Great Recession is basically uncorrelated with the previous two recessions, and the early 1990s recession is uncorrelated with the early 1980s recession. Because of the apparent spatial correlation in Figure 2, we also show in Panel B the correlations within each of the nine Census divisions (that is, after partialing out division fixed effects), and in Panel C the correlations after additionally controlling for pre-recession population growth. These controls tend to slightly reduce the magnitudes of the correlations, but positive serial correlation remains in a few cases. Although our event study approach will capture this serial correlation to the extent that it affects the estimates, we also control in some specifications for the severity of pre-

[19] Panel A of Appendix Figure A.1 presents kernel densities of the (demeaned) log employment change across metros for each recession.

vious recessions as an additional robustness check. As we show below, these additional controls do not appreciably change the results.

4 Results

4.1 Employment

We begin with estimates of equation (2) for log employment in metro areas, as shown in Figure 4. Each of the five panels shows separate estimates for each recession. We include four or five years (as data permit) before the employment peak to capture any pretrends, and we follow areas for up to 10 years after the trough. For each panel, we also show the event study estimates under multiple specifications. Specification M1, shown in red (circles), includes only Census division-by-year fixed effects in $x_{i,t}$. Our preferred specification, M2 (solid blue line), adds controls for pre-recession, age group-specific population growth, as described above. The third specification, M3 (green squares), is implemented for all but the mid 1970s recession and further adds the employment shock from the previous recession. Finally, the fourth specification, M4 (black triangles), implemented for the latter three recessions, includes employment shocks for *all* previous recessions since the mid 1970s.

Specification M1 shows some evidence of negative pretrends, particularly 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 the national recession struck. Adding controls for pre-recession population growth essentially eliminates the pretrends. Since the population growth is calculated over the previous decade, it is possible we eliminate secular trends (such as the growing migration to warmer Southern and Western areas over time), but as we show below, it is also possible that we remove previous recession-induced changes to population growth. Fortunately, since our objective is to estimate the long-term employment effects for an area of a given recession, net of previous ones, whether the pretrends are driven by secular or long-lasting cyclical effects is not paramount; it is sufficient that we can adequately control for it. Thus, we henceforth focus on M2 and higher specifications.

In each recession, the employment shock is mechanically correlated with a large, immediate drop in log employment. Because we normalize the base period to $t_0 - 3$ (three years before the peak), the coefficient at the trough need not be exactly -1, although the estimate is generally quite close to this number, reflecting flat pretrends.[20] Much more salient is that in each recession, the decline in employment shows little to no recovery over the subsequent 10 years, and in the case of the 1990–1991 and 2001 recessions (which have been noted as having "jobless" and "jobloss" national recoveries, respectively), employment continued to fall over this period. Moreover, the confidence intervals imply that we can reject a return to initial peak employment in every subsequent time period. These results show that employment hysteresis to localized recessions is not a new phenomenon from the Great Recession (Yagan, Forthcoming), but has been ongoing for at least the last 50 years. The graphs also show that the durability of the negative shock is not affected by whether shocks from the *previous* recession(s) are included as controls, which supports our identification strategy. We obtain similar results when examining employment from County Business Patterns data (Appendix Figure A.2), where we also see a persistent decline in the number of establishments (Appendix Figure A.3).

Table 3 summarizes the results of specification M2, presenting three-year averages for both estimated elasticities and normalized magnitudes for a one-standard-deviation-unit shock for each recession. The elasticities in Panel A show that the long-term responsiveness to the employment shock (7–9 years after trough) are substantial, and if anything actually grow relative to the short-term elasticity for three of the five recessions. Interestingly, the long-term elasticity is smaller in magnitude—and statistically different—for the two most severe recessions nationally, that in 1980–1982 and the Great Recession, compared to the others. While several factors could explain this difference, one possibility is that the *relative* impacts we estimate may be nonlinear in absolute recession severity. Put differently, when even the typical labor market suffers a severe shock, the long-term differences across areas could be less than when relatively few areas suffer a severe

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

shock.[21] Because the severity of the employment shock varies both across recessions and across areas within a given recession (Appendix Figure A.1), it is also helpful to transform the elasticities into standardized effects. Panel B shows that a one-standard deviation increase in the (absolute value) of the employment shock in the 1973–1975 and 1980–1982 recessions reduced employment by about 7 percent seven to nine years after recession trough, even though variation in severity across areas was one-third greater for the 1980–1982 recession than for the 1973–1975 recession.[22] The two most recent recessions exhibit less variation across areas in severity, so the long-term impacts of a one-standard-deviation shock are smaller, although still sizable, between about 3 and 5 percent. For the Great Recession, for example, a metro area that suffered a one-standard-deviation greater negative employment shock would have total employment seven to nine years after the recession about 3 percent less than it would be expected to otherwise.

In Figure 5, we examine whether these relative employment losses are broad-based or concentrated in certain industries. For simplicity, we present estimates for specification M2 only and suppress confidence intervals. We find that, across recessions, the negative impacts are generally quite pervasive across sectors, as nearly every point estimate is below zero. Government, perhaps not surprisingly, tends to show among the smallest losses, although it fared worse during the 1990–1991 and 2007–2009 recessions. In contrast, construction and manufacturing, the most cyclical industries, experience the largest impacts in the short and medium terms, although construction recovers somewhat in the earliest two recessions (but not so much since), and manufacturing—in line with aggregate trends—has recovered somewhat from the Great Recession (but not so much in earlier recessions). The remaining industries tend to move similarly and fall in between, with no clear evidence in any case of an upward slope to suggest an eventual recovery.[23]

Given these long-term negative impacts on employment, a natural question is whether there is

[21] We are continuing to examine this issue.

[22] Indeed, this is consistent with the elasticity of the earlier recession at this time horizon being approximately one-third greater than for that of the later recession.

[23] 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.

a fully-offsetting population response (e.g., migration) or the share of the population in the area that is employed experiences a long-lasting decline.

4.2 Population and Migration

Thus, in Figure 6 we present estimates of equation (2) where the log of the total working-age population (taken from SEER) is the outcome. For brevity, we show only the results from specification M2, although the patterns are robust to the M3 and M4 specifications, as well. Once again, we are able to rule out pretrends, and once again, we find substantial negative, sustained impacts of the initial employment shock.[24]

For each recession, log population continues to decline long after the recession has ended, implying that harder-hit areas are on a long-lasting, if not permanent, 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. The most severe response comes from the 1990–1991 recession, with a long-run elasticity of roughly -0.7, implying that a 10 percent greater employment shock leads to a relative population loss of 7 percent a decade later.

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

Using the SOI data, we can investigate migration responses more directly, at least for the two most recent recessions. Panels A and B of Figure 7 replicate the event study analysis of population for the 2001 and 2007-2009 recessions in Figure 6 but instead using the total number of exemptions

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

in the tax data to proxy for population. The patterns are quite similar and, if anything, the long-term elasticities are slightly greater in magnitude in the SOI data.[25] In Panels C and D, we use the decomposition of population change described in Appendix B.1 to examine the impacts of the recession shock on migration inflows and outflows, as well as the residual net births. To aid interpretation, we normalize these measures by the total number of exemptions in year $t_0 - 2$, so the estimates capture changes in rates. By recession trough, in-migration rates have fallen sharply, by about 10 percent in both recessions. Over the subsequent decade, these rates recover only slightly, and by the end of the horizon they remain between 6 and 8 percent below pre-recession values. Out-migration shows little response until after the recession has ended, although there is a slight upward pretrend for the 2001 recession. Beginning in the year after the recession trough, however, out-migration rates steadily *decline*, with similar long-term magnitudes as for in-migration. These patterns generally accord with Dao, Furceri and Loungani (2017), who find both declining out-migration over time from states with worse employment shocks and shifts of in-migration toward less affected states. However, these patterns also imply that reduced out-migration partially offsets the expected population decline from reduced in-migration.

To understand how these components contribute to the change in population, as well as the role of net births, which also show a slight reduction (especially for the Great Recession), we divide the coefficient estimates in Panels C and D by the respective estimates in Panels A and B. When we also multiply the out-migration estimates by -1, the sum of the three transformed coefficients—in-migration, out-migration, and net births—sum to 1 and fully decompose the population effects found in the first two panels. These estimates are shown in Panels E and F. For the 2001 recession, the slight upward pretrend for out-migration means that this component explains just under half of the population loss at trough, but its subsequent reversal implies that a decade later the drop in in-migration explains more than the full population growth decline. For the 2007–2009 recession, the pattern is slightly different. The decline in in-migration explains more than the full population growth decline for the entire period since the trough, with a reduction in net births gradually

[25] Note that the SEER data used in Figure 6 is for the population age 15+, while the SOI data implicitly includes all ages.

adding to the effect. The reduction in out-migration provides a growing counterbalance, and by the end of the period both in-migration and out-migration contributions to population growth exceed 1. Consequently, gross migration flows in both directions plummeted among areas more severely affected by the Great Recession. In gauging why relative population growth declined, the predominant driver is the long-lasting contraction of in-migration. We return below to how these changes in migration patterns affected the composition of residents.

4.3 Employment-to-Population

The astute reader may have noticed that the population response is less than the employment response in each of the five recessions. This implies that employment-to-population ratios also fall in each recession. To examine this more directly, we use the log of the ratio of employment to working age population (15+) as the outcome in Figure 8. As expected, across every recession these ratios remain lower than their pre-recession peaks, even a decade after recession's end.[26]

The elasticities at trough do vary somewhat, primarily reflecting the variation in initial employment response. For the 1973–1975, 1980–1982, and 2001 recessions, these initial elasticities are about -0.75, but they are slightly larger, closer to -1, for the 1990–1991 and 2007–2009 recessions. As a consequence of the relatively flat employment trajectories and steady population decline, the employment-to-population trajectories generally show a slight recovery over time, although this is less true for the 1990–1991 and 2001 recessions. The long-term elasticity remains below -0.3 (and statistically different from 0) in each case, implying a severe employment shock of 10 percent suppresses the employment-to-population ratio a decade later by *at least* 3 percent, or about 2 percentage points, given a national mean of about 60 percent.[27]

[26]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.

[27]Our finding of a persistent decrease in the employment-to-population ratio constrasts somewhat with the well-known results of Blanchard and Katz (1992), who find that the population response is large enough to return unemployment and participation rates to trend within 5–7 years of a negative employment shock. They use a different methodology (vector autoregression), different identification (annual rather than cyclical employment changes), different geography (states), and different time period (1960s through 1980s). However, other studies, from Bartik (1993) to Dao, Furceri and Loungani (2017), have questioned some of the assumptions made in Blanchard and Katz (1992), finding their results are sensitive to them. Unlike many other papers, our methodology does not rely on assumptions

Table 5 shows these estimates numerically, with partial recoveries most prevalent for the 1980–1982 and 2007–2009 recessions. Whereas a standard deviation employment shock leads to a long-term reduction in the employment-to-population ratio of about 3–4 percent (1.5–2.5 percentage points) for the four earlier recessions, the relative effect size is only half as large for the Great Recession. Nonetheless, in no case was the population response sufficient to fully counteract long-term employment losses.

These reductions in employment rates at the extensive margin may mask additional impacts at the intensive margin. We examine this possibility using decennial Census and ACS data, as shown in Appendix Table A.1. Specifically, we estimate a variation of equation (2) where dependent variables are drawn from the Census (or 3-year ACS period) following the recession, rather than annually as in the event study.[28] We focus on the (logs of the) employment rate (defined to be the fraction of the area's working age population with a positive number of weeks worked in the last year or 12 months), the full-year employment rate (the fraction with 50 or more weeks worked last year or 12 months), and the full-time, full-year employment rate (the fraction with 50 or more weeks worked last year or 12 months and who usually worked 35 or more hours per week). The employment measure here is conceptually different than the employment-population ratio (more accurately, jobs-population ratio) from the BEAR/SEER data, and we focus on the prime-age population of 25–54 year-olds for consistency with later analyses. We find much smaller—though still negative—employment rate elasticities that are statistically significant only for the latter two recessions. For the full-year and full-year–full-time employment rates, the elasticities are generally larger in magnitude. While we hesitate to draw strong conclusions from these patterns, given the limitations in the data and somewhat mixed results, they certainly suggest that the long-term effects

about the autocorrelation structure of variables or the nature of the correlations between them.

[28]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 1973–1975 recession, 1970 and 1980 for 1980–1982, 1980 and 1990 for 1990–1991, 1990 and 2000 for 2001, and 2000 and 2005–2007 for the 2007–2009 recession. These controls generally capture the pre-recession period, again because outcomes are based on the previous calender year or 12 months.

of recessions may not only decrease an area's employment and employment rate, they may also *increase underemployment* as well. Indeed, for the Great Recession, we use annual ACS data to estimate event study models, and we find strong evidence of a lasting increase in underemployment (Appendix Figure A.16).

4.4 Earnings per capita

Of course, the damaging effects of local recessions need not manifest only through long-term employment losses (and even as a share of the working-age population); they may also affect the quality of employment, most notably through earnings and wages. We thus next examine the summary measure of annual earnings per capita (which encapsulates both the quantity and quality of employment).

Figure 9 and Table 6 show estimates of equation (2) on the log of real earnings per capita, where we have used the CPI-U-RS to adjust for inflation. In a pattern that is becoming familiar, there is again evidence for hysteresis, with per capita earnings below their pre-recession peak for each recession for the entire horizon, although the confidence interval just barely excludes zero for the 1973–1975 recession. Trough elasticities are typically between -0.5 and -0.75, though slightly larger for the 2007–2009 recession. As can be seen in Table 6, long-term elasticities show little improvement, with that for the 2001 recession doubling from its trough. When scaling effects in Panel B, a one-standard-deviation greater shock results in earnings per capita 2 percent (Great Recession) to 4 percent (2001 recession) lower than they otherwise would have been nearly a decade later.

We can again use the Census/ACS to examine impacts on earnings of workers who are employed, both on average and across the earnings distribution. Using the same specification and setup as described above, we look at different moments and percentiles of the log annual earnings distribution: the mean, and the 10th, 50th, and 90th percentiles.[29] The first row of Panel A of Table 7 shows that estimates for the mean log earnings are generally similar to those from the BEAR data

[29]Because of Jensen's inequality, the mean of the log estimated here is not the same as the log of the mean estimated in Table 6.

presented above, although magnitudes are somewhat smaller, especially for the 1990–1991 recession. Looking at the percentile estimates in the next three rows indicates that recessions generally decrease earnings throughout the distribution. Longer-term earnings impacts tend to be less severe for the top of the distribution; for the middle three recessions, the brunt is borne at the bottom, although impacts at the middle are more severe for the 1973–1975 and 2007–2009 recessions. These findings are consistent with the argument that job losses were more concentrated among lower parts of the earnings distribution in past recessions, but that long-term impacts have had farther reach up the distribution more recently.

The long-term relative earnings decline could stem from either a reduction in hours or weeks worked, a reduction in earnings per hour or week, or both. Thus, in Appendix Table A.2 we show additional Census/ACS estimates for (mean) log weekly and log hourly earnings (with those for log annual earnings repeated from Table 7 for convenience). If the earnings losses are driven by the weeks or hours reductions found in Appendix Table A.1, hourly wages could be relatively unaffected several years later. On the other hand, if the recession slows wage growth or displaced workers are less likely to find good employer matches (Lachowska, Mas and Woodbury, 2018), hourly wage losses may explain more of the per-capita earnings declines. The results indicate that the latter story better fits the data, as estimates for log hourly wages are at least two-thirds, and generally closer to four-fifths, of those for log annual wages. Recession-induced decreases in long-term work attachment at the intensive margin therefore explain relatively little of the persistent reduction of annual earnings per capita.

4.5 Government Transfers

Given the persistent decrease in earnings per capita, it is important to understand how well the social safety net responds in replacing these lost earnings, and whether this efficacy has changed over time. In Figure 10 we plot estimates of the employment shock on the log of per-capita real personal current transfers, which are almost entirely from the federal and state governments.[30] Un-

[30]More than 95 percent of transfers come from government, with the remainder coming from businesses in the form of liability payments for personal injury claims or direct money transfers through nonprofits (some of which are

surprisingly, transfers rise at the trough of each recession, although elasticities vary greatly, from 0.75 during the 1973–1975 recession to about 0.3 during the 1990–1991. Interestingly, despite the historically large fiscal stimulus during the Great Recession (the American Recovery and Reinvestment Act of 2009, which included many additional government transfers), the estimate of the trough transfers elasticity for the 2007–2009 recession is only about 0.6, less than that of the two earliest recessions. Of perhaps greater importance, transfers in each case remain elevated throughout the entire post-recession period, even through the next business cycle peak, with almost no tail-off from the respective trough elasticities.

In relative magnitudes, a one-standard-deviation employment shock raises long-term per-capita transfers by 3–4 percent for the two earliest recessions and closer to 2 percent for the three more recent recessions (Table 8). Of course, these magnitudes do not make clear how large transfers are relative to the drop in earnings, and so in Figure 11, we present per-capita transfers estimates divided by per-capita earnings estimates (both in levels, not logs) to illustrate effective replacement rates. At recession trough, these replacement rates (thick black lines) generally cluster around 15 percent, although the rate for the 1990–1991 recession isn't quite half that.[31] As each recovery progresses, replacement rates tend to rise to between 20 and 30 percent for the 1973–1975, 1980–1982, and 2007–2009 recessions, but are relatively flat for the 1990–1991 and 2001 recessions. These patterns suggest that, despite expansions in the social safety net over the past 40-plus years, including food stamps/SNAP and the EITC, among others, replacement rates are no higher today than they were at the start of our analysis period. The similarity of the results in 1990–1991 and 2001 also suggest that welfare reform in 1996 did not dramatically change the responsiveness of the social safety net to the recession shocks we study. Because recessions reduce earnings throughout the distribution, this is perhaps unsurprising.

Of course, the roles of different types of transfers may have varied over this time, as well as within the recovery period for each recession. Figure 11 displays also the component replacement

indirectly funded by governments). Government transfers include retirement and disability (Social Security), medical (Medicare and Medicaid), income maintenance and welfare (SSI, EITC, AFDC/TANF, etc.), unemployment insurance benefits, veterans benefits, education and training assistance, and miscellaneous other benefits.

[31] Figure 11 shows replacement rates only post-recession, because the impact on earnings is near zero before.

rates for each major type of transfer program. The most important transfer types are retirement and disability (e.g., Social Security OASDI) and—especially in the Great Recession—medical (Medicare and Medicaid).[32] These transfers typically constitute over half the overall replacement rate in the early recovery, with this share rising through the later recovery period. Since earnings per capita have minimal recovery (Figure 9), areas that experienced more severe recessions have faster and more durable growth in these types of transfers. Income maintenance transfers (e.g., AFDC/TANF, SSI, SNAP, and EITC) play a smaller role, except in the 1990–1991 recession, when they compose a relatively large share of (the relatively small) replacement rate. As expected, unemployment insurance transfers (which have a fixed duration per unemployment spell) spike shortly after trough but fade away within a few years thereafter. Other types of transfers have minimal response to the recession shock.

4.6 Income per capita

A related way to think about the importance of transfers is the degree of insurance they provide for lost earnings. If transfers fully insured local areas, for example, the increase in transfers would exactly offset the decrease in earnings, although the results above indicate this did not happen in any of the recessions.[33] However, because transfers occur independent of response to recessions and may constitute significant shares of income even in cyclically normal periods, it is worth investigating the impact of employment shocks on real income per capita, where income equals earnings plus transfers by construction. Figure 12 shows these estimates for each recession. The patterns here are similar to those for earnings per capita in Figure 9, but with slightly smaller magnitudes. Put differently, there are persistent reductions in income per capita after each recession, and only in the case of the 1973–1975 recession do we (barely) fail to reject the null of a full recovery after 10 years. For the other recessions, long-term elasticities range between -0.25 (1980–1982 and

[32] Appendix Tables A.3 and A.4 show estimates for detailed types of transfer programs at the 1–3 year and 7–9 year horizons, respectively. These tables show that the increase in retirement and disability transfers stem almost entirely from Social Security transfers (for both retirement and disability insurance). They also show that medical transfers are generally balanced between Medicare and Medicaid.

[33] Transfers could fully insure individuals, but not areas, against the earnings loss if higher-earning individuals were more likely to out-migrate following the recession (e.g., Bound and Holzer, 2000; Notowidigdo, 2013).

2007–2009) and -0.75 (2001). Table 6 provides elasticities and relative magnitudes numerically. A one-standard-deviation larger shock translates to per-capita income growth between 1.1 percent (for the Great Recession) and 2.7 percent (for the 2001 recession) lower than it would be otherwise seven to nine years after trough. This is roughly equivalent to one year's worth of growth nationally, averaged over the past decade.

As with earnings, these income losses may occur at different parts of the distribution, and so we conduct Census/ACS analysis analogous to that in Table 7, substituting log income for log earnings for the dependent variables. As a summary measure, we also examine poverty rates. These estimates are shown in Table 10. The first row of Panel A shows the effect of the recession shock on average (real) log income in the Census data. Although magnitudes vary slightly from the long-term estimates in Table 9, they are qualitatively similar and also indicate hysteresis in average income. The next three rows provide estimates at different percentiles of the log income distribution. As with the effects on earnings in Table 7, the results generally indicate larger losses in the lower half of the distribution, although this pattern seems to have moderated for the more recent recessions. Nevertheless, we find that the impact of recession shocks on long-term poverty rates has grown over time, with semi-elasticities rising from the 0.04–0.08 range for the first three recessions to the 0.13–0.15 range for the recessions this century.[34] In practical terms, these effects are substantive: a 10 percent employment shock increases the long-term poverty rate by between 0.4 and 1.5 percentage points.

4.7 Robustness

We present several robustness tests in the appendix. In particular, Appendix B.2 shows that our results are very similar when using private wage and salary employment from BEAR or QCEW to construct recession shocks. Appendix B.3 discusses results when replacing the recession shock with the predicted log employment change, as in Bartik (1991). While there are several reasons to prefer the recession shock over the Bartik shock, the results are generally similar. Finally,

[34] It is possible that these increases reflect the 1996 welfare reform; we leave this investigation to future research.

Appendix B.4 shows that our results are nearly identical when examining commuting zones instead of metropolitan areas.

5 Recession Shocks and Composition Changes

So far, we have shown that recessions lead to persistent relative declines in local economic activity and persistent increases in transfers per capita. One explanation for these persistent effects is a change in worker composition due to differential migration responses. We next examine these composition changes—which are of independent interest—and explore whether they explain the persistent effects of recessions. While we find some evidence of composition changes, they are not large enough to explain the majority of the effects we uncover.

5.1 Examining Composition Changes

First, we use equation (2) to directly estimate the effects of recessions on the composition of individuals in a metro area. We focus on age, education, and occupation distributions, as these directly relate to an area's earnings capacity. Figure 13 plots the effects of recession shocks on the share of population ages 0–14, 15–39, 40–64, and 65 and above.[35] Across all recessions, we see a persistent increase in the share age 65 or above and a decrease in the share age 15–39. This is consistent with the fact that early career workers are more mobile than older individuals (e.g., Molloy, Smith and Wozniak, 2011). The response of other age groups varies more: the 0–14 share declines following the 1973–1975, 1980–1982, and 2007–09 recessions, but rises after 1990–1991 and does not change after the 2001 recession. The 40–64 share generally increases, with the exception of 1990–1991. Most of these point estimates are statistically significant (filled-in markers indicate significance at the five-percent level).

To further describe the magnitude of these effects, Appendix Table A.7 reports the coefficients and one-standard-deviation effects 7–9 years after the recession trough. The bottom panel shows

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

that a one-standard-deviation increase in recession severity leads to a 0.2–0.6 percentage point (0.5–1.6 percent) decrease in the 15–39 share and a 0.1–0.6 percentage point (0.8–5.0 percent) increase in the share age 65 and above.[36] Consequently, metro areas suffering greater recession severity grow older, with smaller shares in traditional working ages.

Table 11 reports estimates of recession shocks on occupational structure and educational composition, using decennial Census and ACS data. Panel A examines the share of employed individuals age 25–54 in three occupation groups: managerial, professional, and technical; administrative, office, production, and sales; and manual and service. We follow Autor (2019) in using these classifications, which correspond to high, medium, and low paid occupations. The 1973–1975, 1990–1991, and 2007–2009 recessions decreased the share of workers in managerial, professional, and technical jobs, while increasing the share in manual and service occupations. There is less evidence of an impact on occupational structure following the 1980–1982 and 2001 recessions. The coefficients for the Great Recession imply that a one-standard-deviation recession shock decreases the share of workers employed in managerial, professional, and technical occupations by 0.4 percentage points (1 percent). Thus, metro areas harder hit also tended to shift toward lower-paying occupations.

Panel B examines the share of individuals age 25–54 with a high school degree or less, some college (but less than a four-year degree), and a four-year degree or more. The results mirror those in Panel A: the 1973–1975, 1990–1991, and 2007–2009 recessions increased the share of individuals with a high school degree or less and decreased the college share.[37] A one standard deviation Great Recession shock increases the share of individuals with no more than a high school degree by 0.8 percentage points (2 percent) and decreases the share of individuals with a bachelor's

[36] The average share age 15–39 is 0.38, and the average share age 65+ is 0.12.

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

degree by 0.6 percentage points (2 percent).

In sum, all recessions led to a modest shift in the population away from early career workers and towards the elderly. Some recessions decreased the share of workers employed in high wage occupations, and the same recessions decreased the share of individuals with a college degree.[38] The changes in age, occupation, and education are modest in size, which suggests that these composition shifts likely cannot explain all of the persistent impacts on local labor markets. We further explore this issue next.

5.2 The Role of Composition Changes in Aggregate Patterns

To more directly quantify the role of composition changes, we estimate the effects of recessions on residual measures of earnings, income, and poverty. We use the Census and ACS data to regress outcome variables 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 B of Table 7 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 contribute to the persistent decline in earnings. At the same time, the composition-adjusted impacts remain sizable, and for the 2001 recession, they actually increase in magnitude slightly. In general, composition changes along observed dimensions explain less than half of the overall effects. Panel B of Table 10 presents similar results for total income and poverty.

Decennial Census data allow us to examine the five most recent recessions, but they are available for limited years. We complement this analysis by studying the effects of the Great Recession using the ACS, which provides annual data starting in 2005. Appendix B.5 discusses the results. We continue to find a modest role for composition changes in explaining the persistent effects

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

on local labor markets. Consistent with the results in Table 8, we find sizable effects on annual, weekly, *and* hourly earnings, which indicates that the decrease in annual earnings stems from changes in both hours worked and the amount earned per hour. Finally, we find a greater increase in poverty following the Great Recession for individuals age 25–54 than for individuals of all ages. We plan to further characterize heterogeneity in the effects of the Great Recession on workers in future research.

6 Conclusion

This paper examines the short- and longer-term effects of recessions on local areas. We find consistent and robust evidence that, for each of the past five national recessions, local areas that suffered larger employment losses experience persistent relative declines in employment, population, employment-to-population ratios, and earnings per capita for at least a decade after recession's end. While government transfers also remain elevated for a decade or more after the trough of a recession, they are insufficient to fully offset earnings losses, leading to long-term declines in per-capita income, as well. Both earnings and income decline throughout the distribution, but effects tend to be more severe at the middle and bottom, and poverty rates also rise. Recessions also lead to an increase in the share of population age 65 and above and a decrease in the share age 15–39. In three out of five recessions, we observe a decrease in the share of workers employed in managerial, professional, or technical occupations, and a decrease in the share of residents with a bachelor's degree. These composition shifts, however, explain less than half of the persistent effects we uncover.

In short, recessions produce enduring economic disruptions to local labor markets, and this pattern has existed for at least the past five decades. While there are some differences across recessions, more striking is the similarity of the responses, especially in light of different macroeconomic drivers and secular changes in the economy over time.

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

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

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Table 1: Aggregate Employment Changes, by Recession

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

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

Table 2: Correlation of Metropolitan Area Recession Shocks

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

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

Table 3: Impacts of Metropolitan Area Recession Shocks on Log Employment

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	−0.653	−0.740	−1.069	−0.751	−0.888
	(0.047)	(0.038)	(0.065)	(0.074)	(0.039)
1–3 years after trough	−0.988	−1.037	−1.288	−1.279	−1.223
	(0.102)	(0.072)	(0.106)	(0.072)	(0.078)
4–6 years after trough	−1.169	−0.908	−1.478	−1.415	−0.996
	(0.146)	(0.118)	(0.147)	(0.103)	(0.109)
7–9 years after trough	−1.294	−0.873	−1.703	−1.543	−0.797
	(0.183)	(0.138)	(0.161)	(0.131)	(0.112)
Panel B: Implied impact of a 1 SD recession shock					
During recession	−0.037	−0.058	−0.048	−0.026	−0.035
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
1–3 years after trough	−0.055	−0.082	−0.058	−0.043	−0.048
	(0.006)	(0.006)	(0.005)	(0.002)	(0.003)
4–6 years after trough	−0.065	−0.072	−0.067	−0.048	−0.039
	(0.008)	(0.009)	(0.007)	(0.004)	(0.004)
7–9 years after trough	−0.072	−0.069	−0.077	−0.052	−0.031
	(0.010)	(0.011)	(0.007)	(0.004)	(0.004)
SD of recession shock	0.056	0.079	0.045	0.034	0.039

Notes: Table reports estimates of equation (2), separately for each recession. We impose the constraint that pre-recession coefficients equal zero and group post-recession coefficients across years. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the log wage and salary employment change from BEAR data. All regressions control for division-year fixed effects and interactions between pre-recession population growth and year. There are 363 metropolitan areas in the sample. Standard errors are clustered by metropolitan area.
Sources: Authors' calculations using BEAR and SEER data.

Table 4: Impacts of Metropolitan Area Recession Shocks on Log Population Age 15+

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	−0.150	−0.211	−0.199	−0.148	−0.162
	(0.026)	(0.038)	(0.044)	(0.028)	(0.025)
1–3 years after trough	−0.375	−0.464	−0.407	−0.390	−0.357
	(0.057)	(0.048)	(0.078)	(0.053)	(0.043)
4–6 years after trough	−0.487	−0.536	−0.574	−0.502	−0.404
	(0.090)	(0.061)	(0.119)	(0.083)	(0.054)
7–9 years after trough	−0.642	−0.562	−0.692	−0.548	−0.377
	(0.114)	(0.079)	(0.136)	(0.099)	(0.060)
Panel B: Implied impact of a 1 SD recession shock					
During recession	−0.008	−0.017	−0.009	−0.005	−0.006
	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)
1–3 years after trough	−0.021	−0.037	−0.018	−0.013	−0.014
	(0.003)	(0.004)	(0.004)	(0.002)	(0.002)
4–6 years after trough	−0.027	−0.042	−0.026	−0.017	−0.016
	(0.005)	(0.005)	(0.005)	(0.003)	(0.002)
7–9 years after trough	−0.036	−0.044	−0.031	−0.019	−0.015
	(0.006)	(0.006)	(0.006)	(0.003)	(0.002)

Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log population age 15 and above. See notes to Table 3.
Sources: Authors' calculations using BEAR and SEER data.

Table 5: Impacts of Metropolitan Area Recession Shocks on Log Employment-Population Ratio

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	−0.483	−0.547	−0.841	−0.582	−0.727
	(0.041)	(0.049)	(0.055)	(0.079)	(0.034)
1–3 years after trough	−0.600	−0.608	−0.847	−0.851	−0.872
	(0.071)	(0.084)	(0.088)	(0.079)	(0.068)
4–6 years after trough	−0.646	−0.424	−0.835	−0.877	−0.604
	(0.083)	(0.103)	(0.107)	(0.100)	(0.092)
7–9 years after trough	−0.608	−0.351	−0.902	−0.992	−0.430
	(0.101)	(0.102)	(0.120)	(0.133)	(0.090)
Panel B: Implied impact of a 1 SD recession shock					
During recession	−0.027	−0.043	−0.038	−0.020	−0.028
	(0.002)	(0.004)	(0.002)	(0.003)	(0.001)
1–3 years after trough	−0.034	−0.048	−0.038	−0.029	−0.034
	(0.004)	(0.007)	(0.004)	(0.003)	(0.003)
4–6 years after trough	−0.036	−0.033	−0.038	−0.030	−0.024
	(0.005)	(0.008)	(0.005)	(0.003)	(0.004)
7–9 years after trough	−0.034	−0.028	−0.041	−0.034	−0.017
	(0.006)	(0.008)	(0.005)	(0.005)	(0.004)

Notes: Table reports estimates of equation (2), 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 Table 3.

Sources: Authors' calculations using BEAR and SEER data.

Table 6: Impacts of Metropolitan Area Recession Shocks on Log Real Earnings per Capita

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	−0.520	−0.492	−0.677	−0.501	−0.795
	(0.046)	(0.043)	(0.066)	(0.113)	(0.090)
1–3 years after trough	−0.462	−0.534	−0.607	−0.840	−0.984
	(0.061)	(0.068)	(0.074)	(0.125)	(0.127)
4–6 years after trough	−0.510	−0.430	−0.611	−1.002	−0.726
	(0.077)	(0.089)	(0.094)	(0.157)	(0.139)
7–9 years after trough	−0.436	−0.388	−0.679	−1.182	−0.513
	(0.078)	(0.092)	(0.115)	(0.183)	(0.137)
Panel B: Implied impact of a 1 SD recession shock					
During recession	−0.029	−0.039	−0.030	−0.017	−0.031
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
1–3 years after trough	−0.026	−0.042	−0.027	−0.029	−0.038
	(0.003)	(0.005)	(0.003)	(0.004)	(0.005)
4–6 years after trough	−0.029	−0.034	−0.027	−0.034	−0.028
	(0.004)	(0.007)	(0.004)	(0.005)	(0.005)
7–9 years after trough	−0.024	−0.031	−0.031	−0.040	−0.020
	(0.004)	(0.007)	(0.005)	(0.006)	(0.005)

Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Table 3.
Sources: Authors' calculations using BEAR and SEER data.

Table 7: Impacts of Metropolitan Area Recession Shocks on Annual Wage Earnings, Census/ACS

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

Notes: Table reports estimates of separate regressions for each recession. The dependent variable is taken from the post-recession Census year (1980, 1990, 2000, 2005–2007, and 2015–17, respectively). The 1973–75 regression controls for the 1970 value of the dependent variable, and other regressions control for two lagged/contemporaneous values. Sample limited to individuals age 25–54. All regressions control for division-year fixed effects and interactions between pre-recession population growth and year. The dependent variables in Panel B are constructed using residuals from regressing log earnings on indicators for education (11), 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.
Sources: Authors' calculations using BEAR, decennial Census, and ACS data.

Table 8: Impacts of Metropolitan Area Recession Shocks on Log Real Transfers per Capita

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	0.518	0.532	0.249	0.331	0.501
	(0.063)	(0.075)	(0.061)	(0.073)	(0.104)
1–3 years after trough	0.503	0.671	0.356	0.540	0.574
	(0.100)	(0.086)	(0.091)	(0.081)	(0.106)
4–6 years after trough	0.626	0.564	0.356	0.556	0.535
	(0.121)	(0.109)	(0.128)	(0.115)	(0.112)
7–9 years after trough	0.585	0.493	0.392	0.727	0.471
	(0.146)	(0.106)	(0.139)	(0.125)	(0.121)
Panel B: Implied impact of a 1 SD recession shock					
During recession	0.029	0.042	0.011	0.011	0.020
	(0.004)	(0.006)	(0.003)	(0.002)	(0.004)
1–3 years after trough	0.028	0.053	0.016	0.018	0.022
	(0.006)	(0.007)	(0.004)	(0.003)	(0.004)
4–6 years after trough	0.035	0.045	0.016	0.019	0.021
	(0.007)	(0.009)	(0.006)	(0.004)	(0.004)
7–9 years after trough	0.033	0.039	0.018	0.025	0.018
	(0.008)	(0.008)	(0.006)	(0.004)	(0.005)

Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log real transfers per capita (age 15+). See notes to Table 3.
Sources: Authors' calculations using BEAR, SEER, and QCEW data.

Table 9: Impacts of Metropolitan Area Recession Shocks on Log Real Income per Capita

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Coefficients on recession shock					
During recession	−0.397	−0.370	−0.565	−0.359	−0.535
	(0.041)	(0.040)	(0.064)	(0.096)	(0.075)
1–3 years after trough	−0.346	−0.380	−0.481	−0.606	−0.650
	(0.054)	(0.057)	(0.060)	(0.103)	(0.100)
4–6 years after trough	−0.373	−0.295	−0.478	−0.704	−0.456
	(0.068)	(0.075)	(0.080)	(0.127)	(0.108)
7–9 years after trough	−0.297	−0.259	−0.515	−0.787	−0.290
	(0.065)	(0.078)	(0.100)	(0.141)	(0.107)
Panel B: Implied impact of a 1 SD recession shock					
During recession	−0.022	−0.029	−0.025	−0.012	−0.021
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
1–3 years after trough	−0.019	−0.030	−0.022	−0.021	−0.025
	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)
4–6 years after trough	−0.021	−0.023	−0.022	−0.024	−0.018
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)
7–9 years after trough	−0.017	−0.020	−0.023	−0.027	−0.011
	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)

Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log real income per capita (age 15+). See notes to Table 3.
Sources: Authors' calculations using BEAR and SEER data.

Table 10: Impacts of Metropolitan Area Recession Shocks on Annual Income and Poverty, Census/ACS

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Panel A: Without Composition Adjustment					
Average log income	−0.225	−0.507	−0.113	−0.575	−0.718
	(0.083)	(0.095)	(0.101)	(0.103)	(0.126)
10th percentile, log income	−0.083	−0.579	−0.121	−0.692	−0.325
	(0.144)	(0.139)	(0.153)	(0.193)	(0.238)
50th percentile, log income	−0.234	−0.522	−0.047	−0.488	−0.769
	(0.099)	(0.096)	(0.089)	(0.102)	(0.131)
90th percentile, log income	−0.142	−0.252	−0.043	−0.433	−0.535
	(0.081)	(0.069)	(0.089)	(0.089)	(0.142)
Poverty rate	0.035	0.077	0.049	0.150	0.131
	(0.025)	(0.024)	(0.026)	(0.031)	(0.028)
Panel B: With Composition Adjustment					
Average log income	−0.173	−0.314	−0.050	−0.609	−0.493
	(0.076)	(0.074)	(0.080)	(0.092)	(0.115)
10th percentile, log income	−0.017	−0.426	0.007	−0.815	−0.425
	(0.126)	(0.133)	(0.138)	(0.191)	(0.207)
50th percentile, log income	−0.203	−0.294	−0.008	−0.482	−0.464
	(0.074)	(0.073)	(0.075)	(0.075)	(0.097)
90th percentile, log income	−0.106	−0.182	−0.070	−0.438	−0.357
	(0.081)	(0.053)	(0.062)	(0.079)	(0.119)
Poverty rate	0.009	0.066	0.016	0.158	0.087
	(0.020)	(0.022)	(0.021)	(0.028)	(0.026)

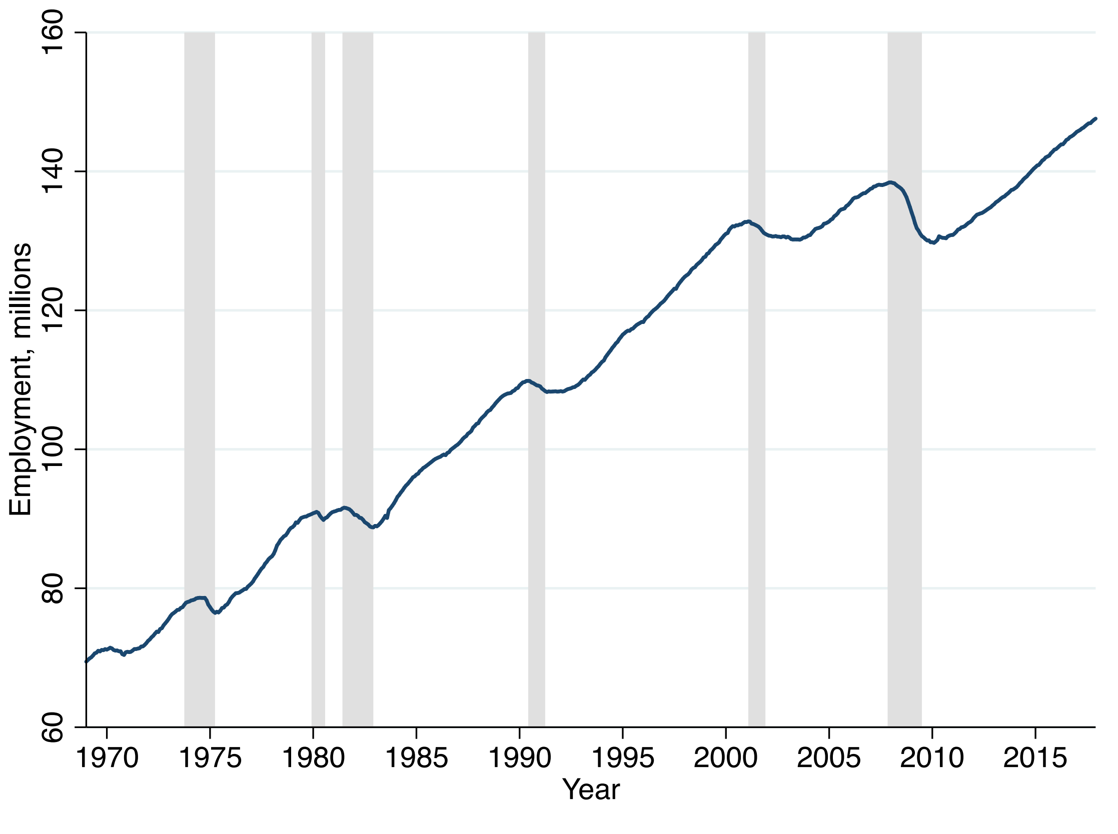
Notes: Table reports estimates of separate regressions for each recession. Sample limited to individuals age 25–54. See notes to Table 7.
Sources: Authors' calculations using BEAR, decennial Census, and ACS data.

Table 11: Impacts of Metropolitan Area Recession Shocks on Occupational Structure and Education Composition

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

Notes: Table reports estimates of separate regressions for each recession. We control for all occupation or education shares (which are mutually exclusive). Sample limited to individuals age 25–54. See notes to Table 7.
Sources: Authors' calculations using BEAR, decennial Census, and ACS data.

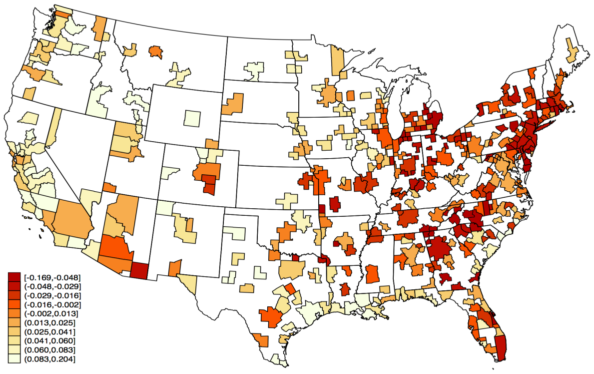
Figure 1: Aggregate Employment and Recessions, 1969–2016



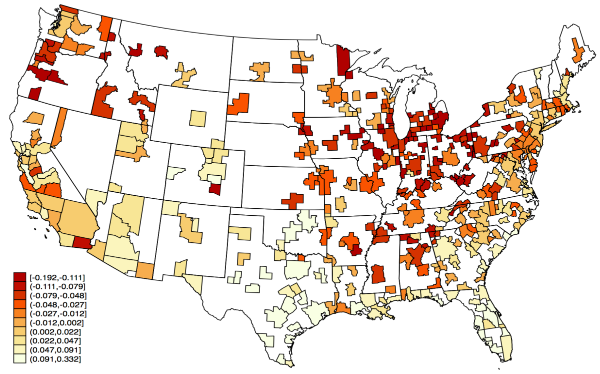
Notes: Figure shows total, seasonally adjusted national nonfarm employment. The shading indicates NBER national recession dates.
Source: Current Employment Statistics, Bureau of Labor Statistics.

Figure 2: Metropolitan Area Recession Shocks

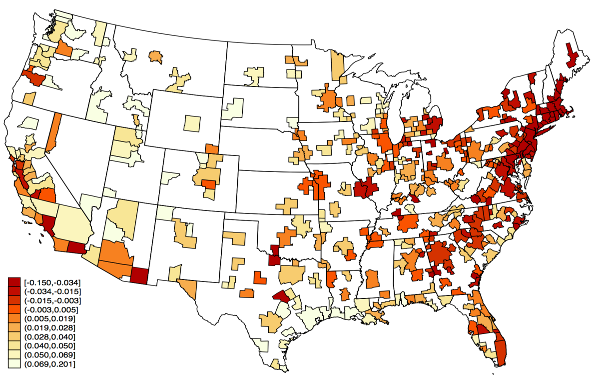
(a) 1973–1975 Recession



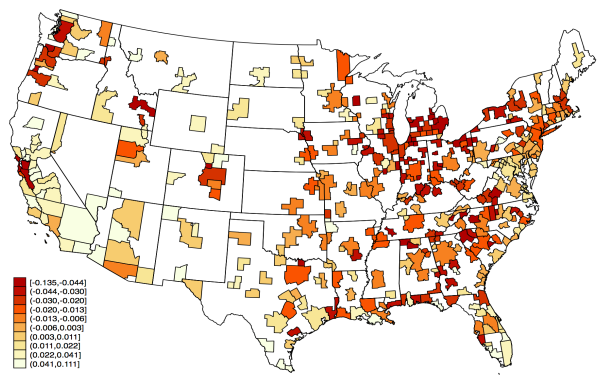
(b) 1980–1982 Recession



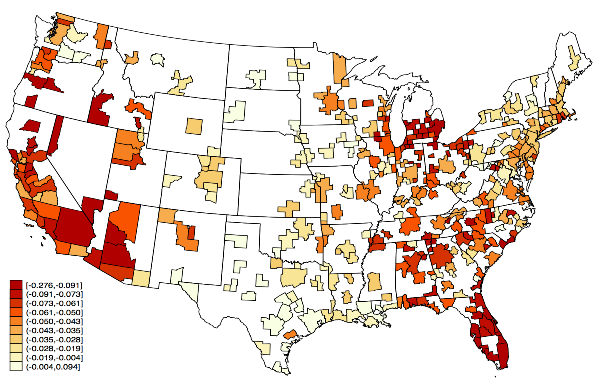
(c) 1990–1991 Recession



(d) 2001 Recession

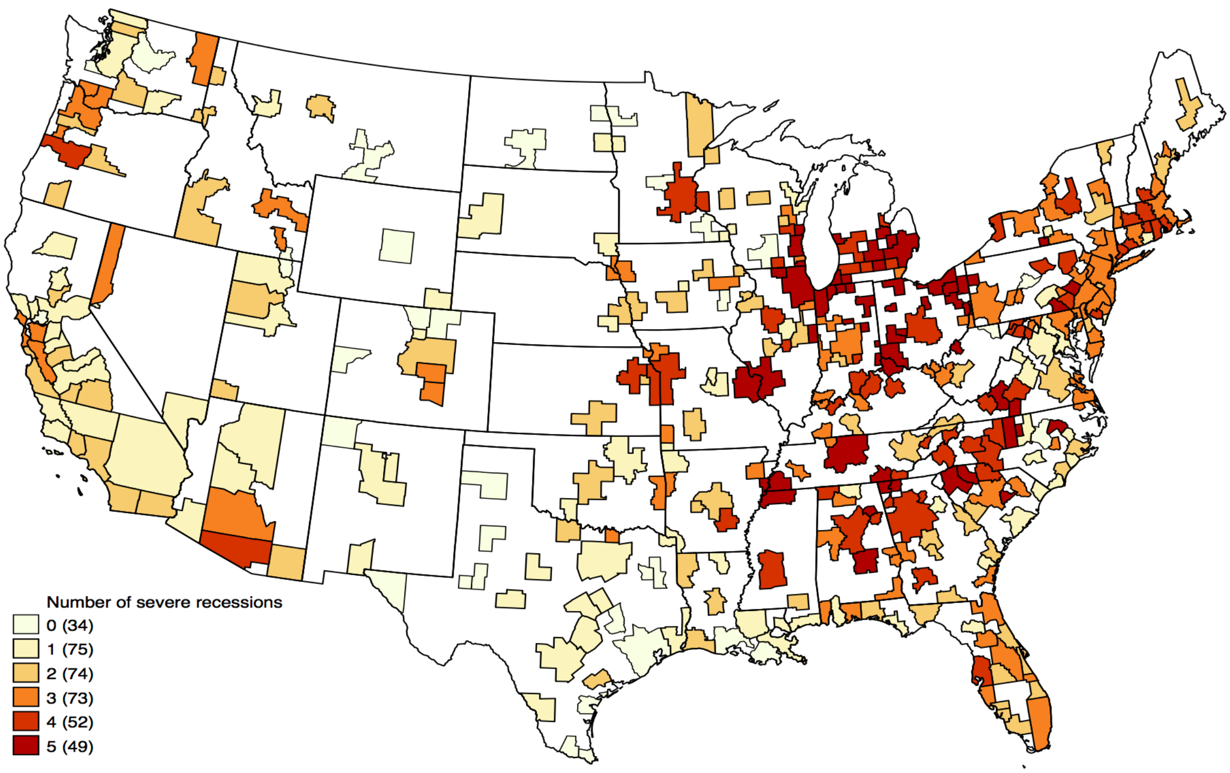


(e) 2007–2009 Recession



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

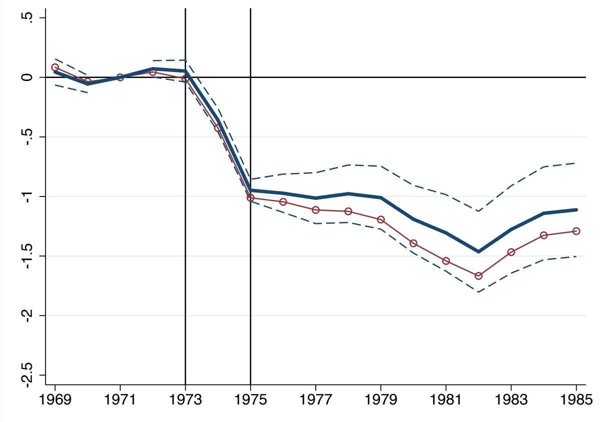
Figure 3: Frequency of Severe Recessions, by Metropolitan Area, from 1973–2009



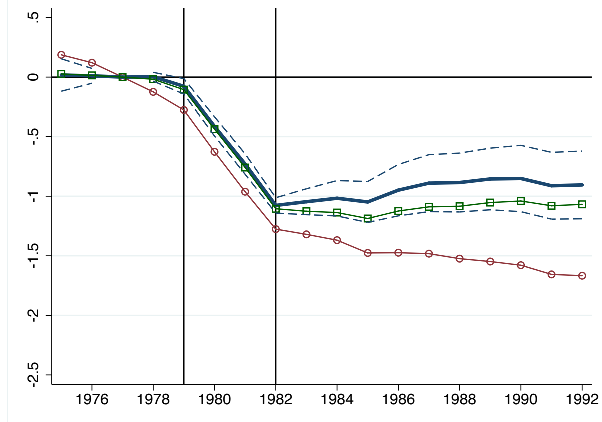
Notes: We denote an area as suffering a severe recession if its log employment change for a given recession is less than the median across CBSAs for that recession.
Source: Authors' calculations from BEAR.

Figure 4: Impacts of Metropolitan Area Recession Shocks on Log Employment

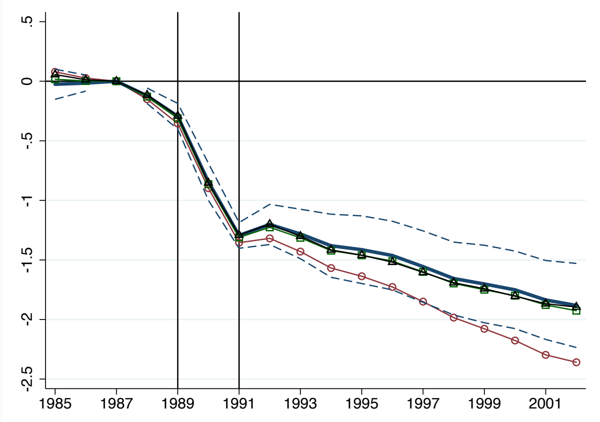
(a) 1973–1975 Recession



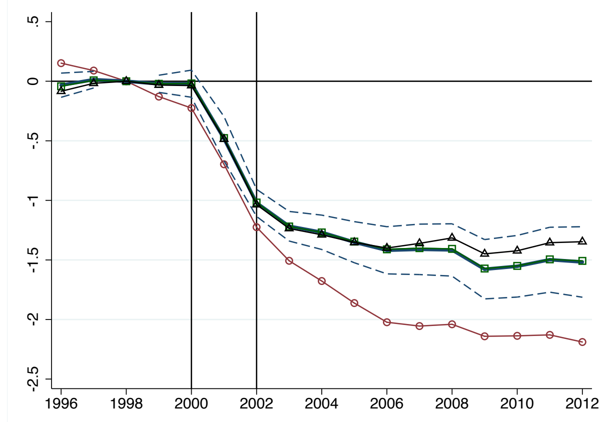
(b) 1980–1982 Recession



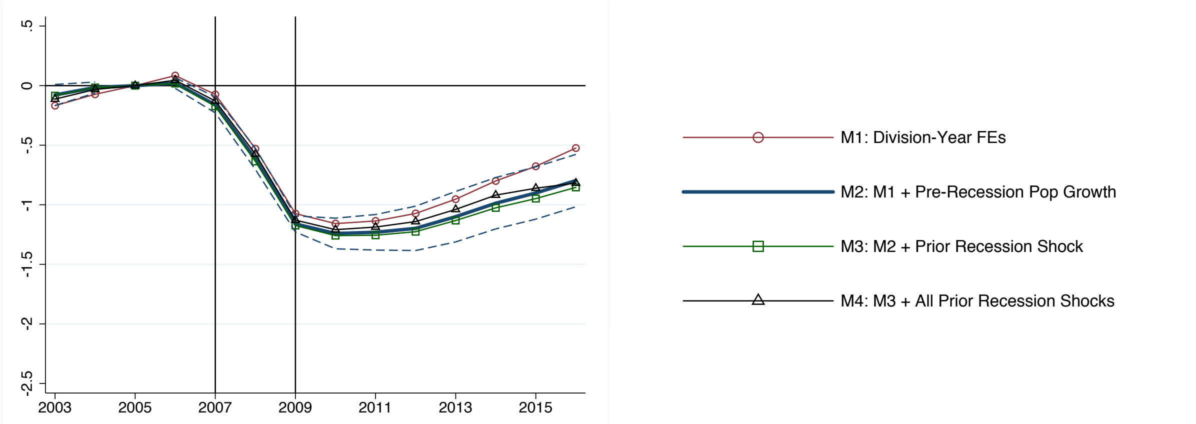
(c) 1990–1991 Recession



(d) 2001 Recession

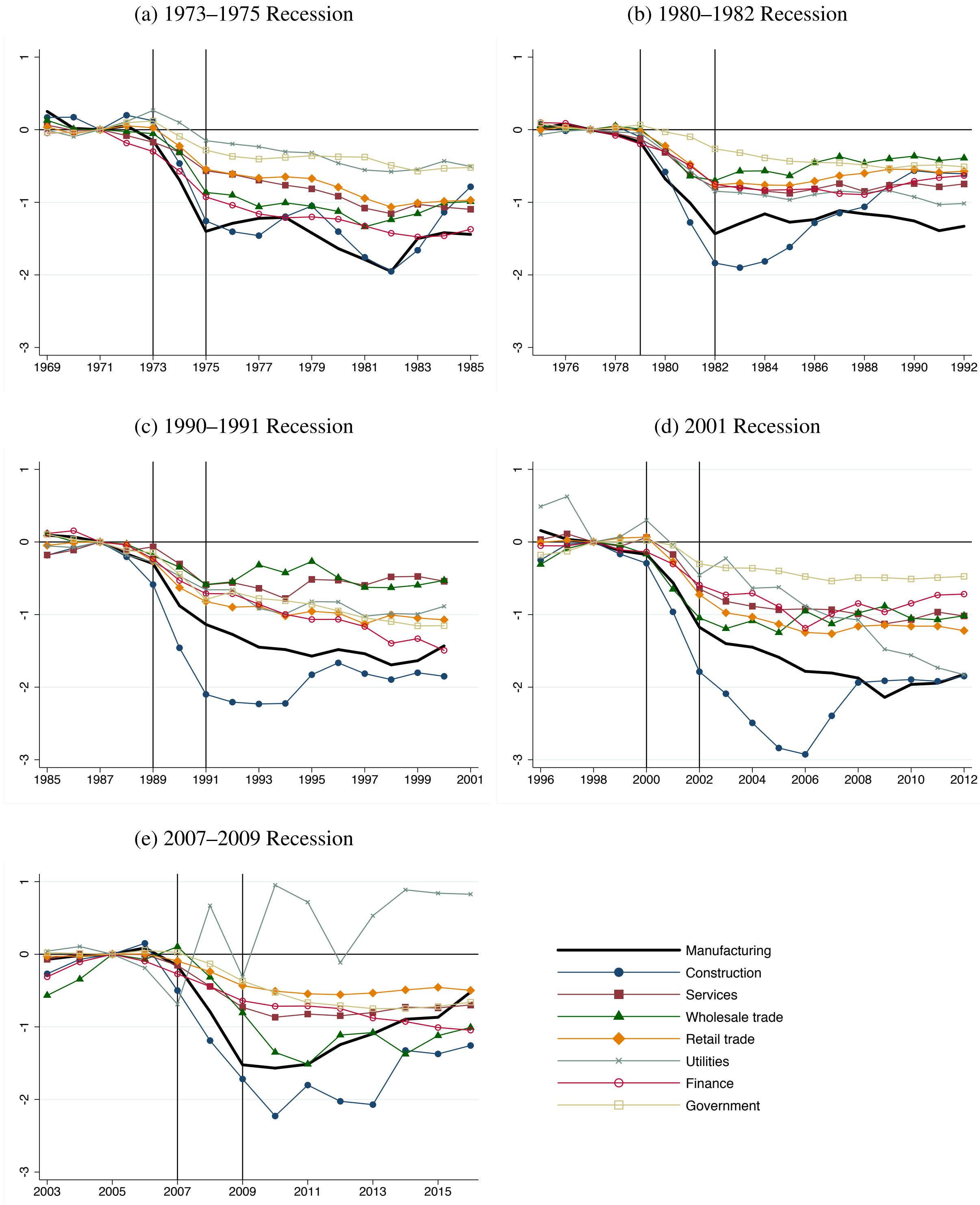


(e) 2007–2009 Recession



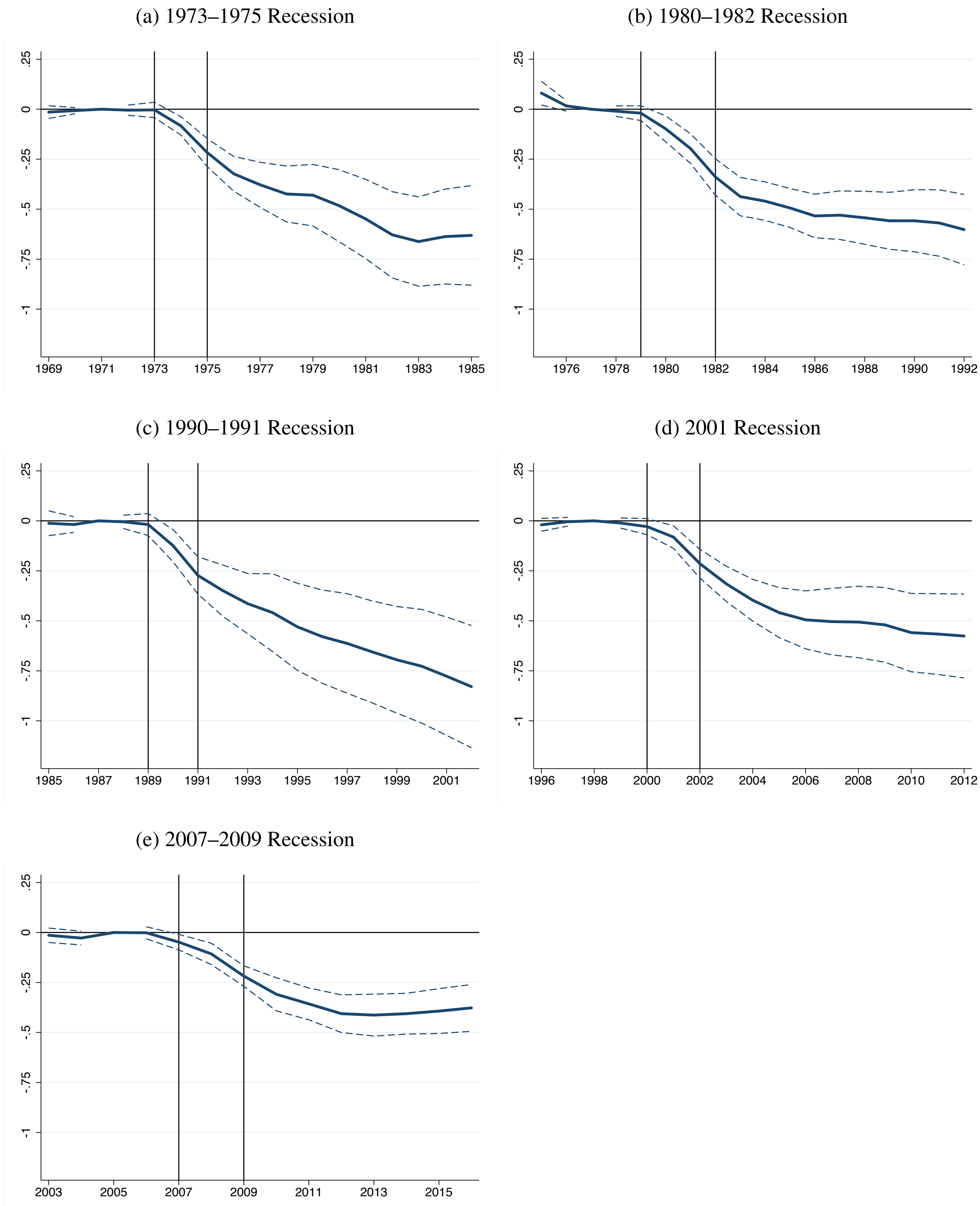
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the log wage and salary employment change from BEAR data. Specifications are indicated by the legend. There are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

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



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

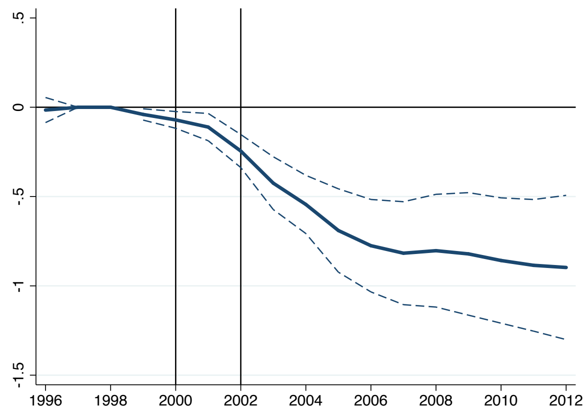
Figure 6: Impacts of Metropolitan Area Recession Shocks on Log Population Age 15+



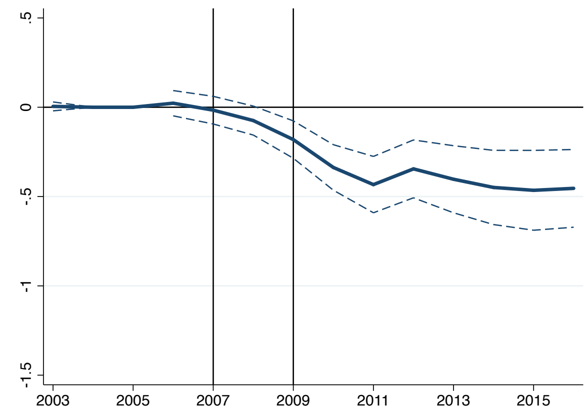
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

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

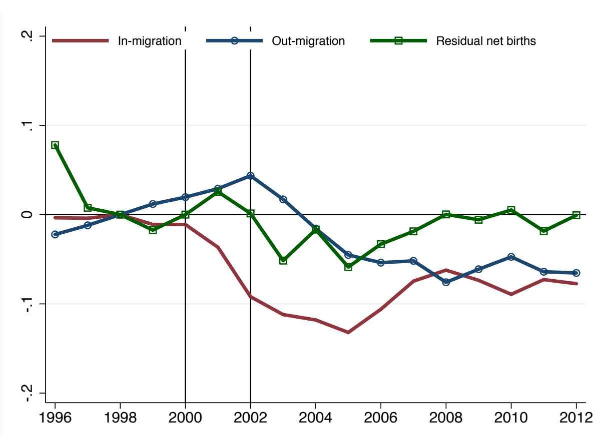
(a) 2001, DV: Log Population



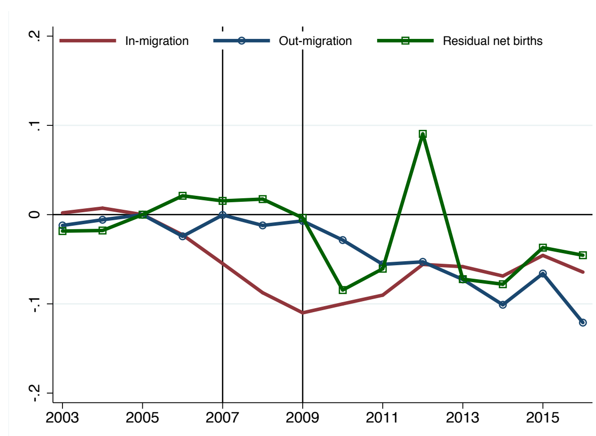
(b) 2007–2009, DV: Log Population



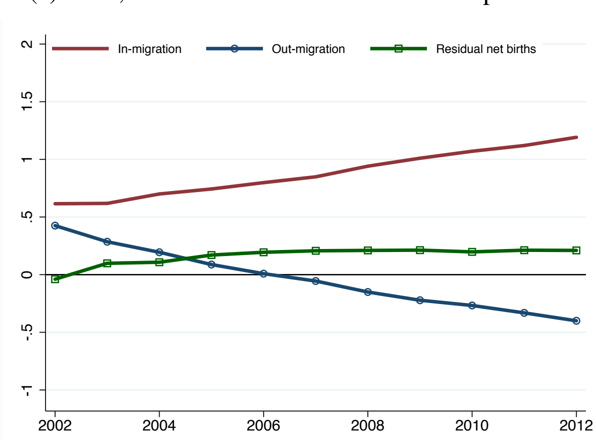
(c) 2001, DV: Population Components



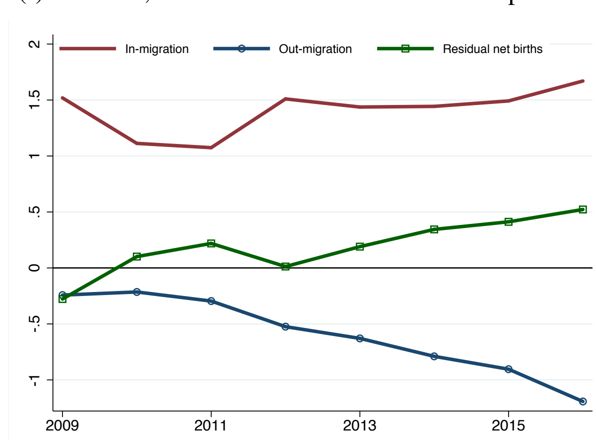
(d) 2007–09, DV: Population Components



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



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



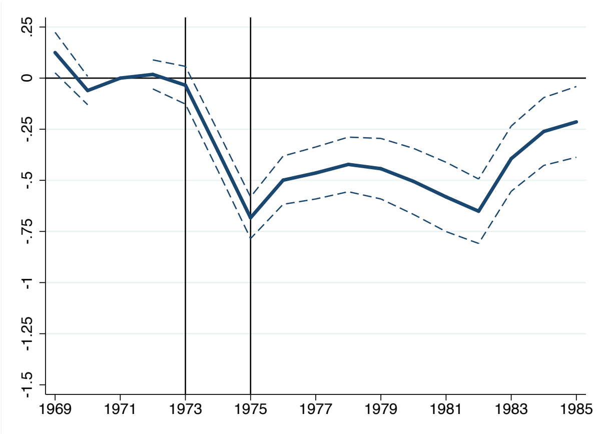
Notes: Figure reports estimates of equation (2), separately for each recession. In Panels A and B, the dependent variable is the number of exemptions relative to the normalization year (1998 or 2005). In Panels C and D, the dependent variables are in-migration, out-migration, and residual net births,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

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

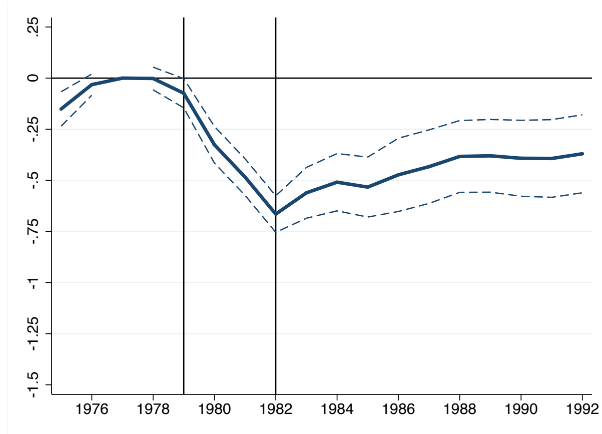
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is the log ratio of wage and salary employment to population age 15 and above. See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

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

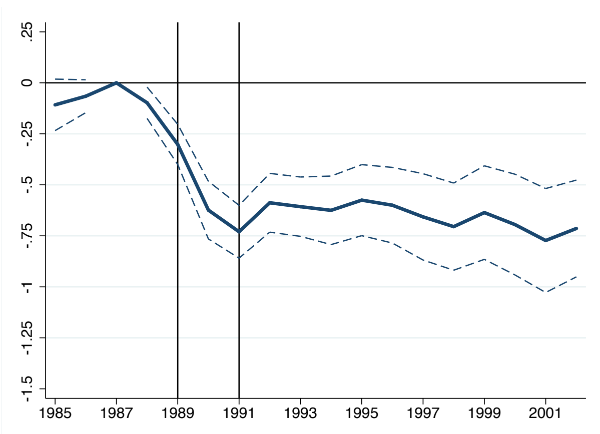
(a) 1973–1975 Recession



(b) 1980–1982 Recession

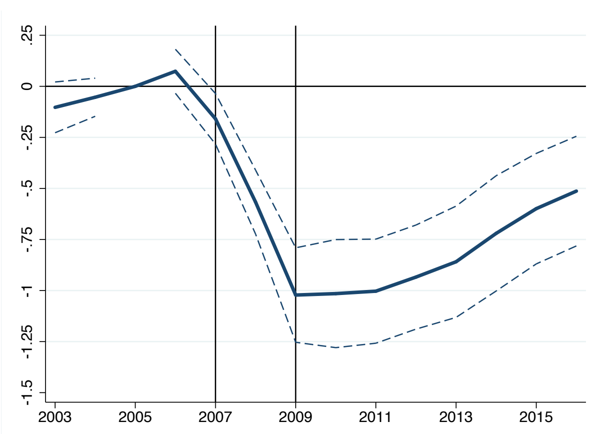


(c) 1990–1991 Recession



(d) 2001 Recession

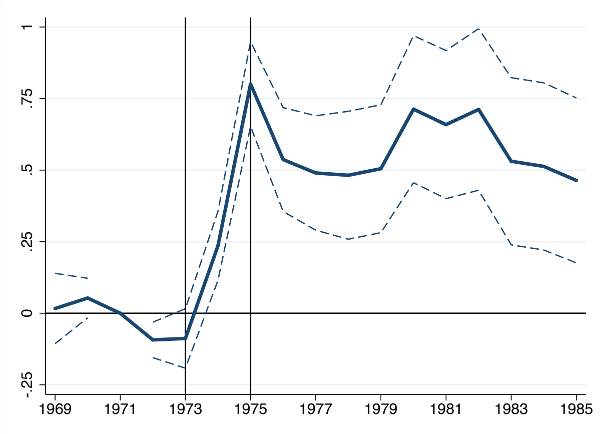
(e) 2007–2009 Recession



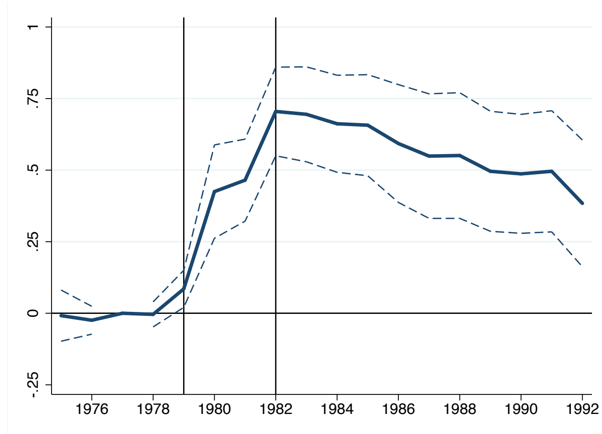
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

Figure 10: Impacts of Metropolitan Area Recession Shocks on Log Real Transfers per Capita

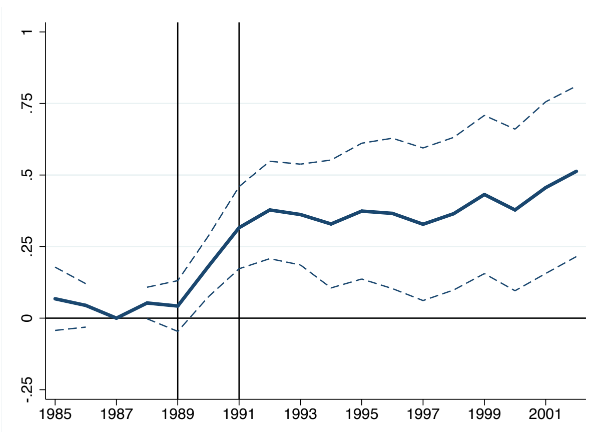
(a) 1973–1975 Recession



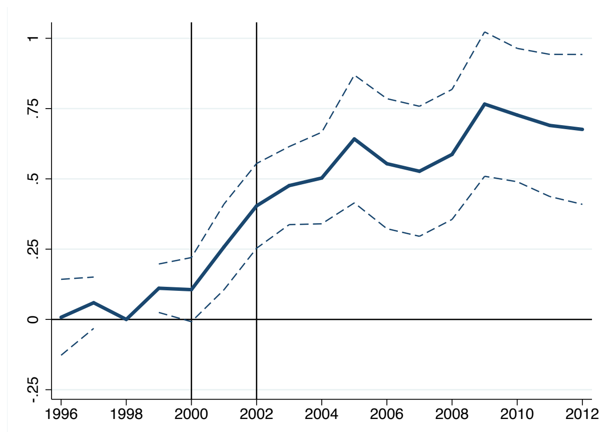
(b) 1980–1982 Recession



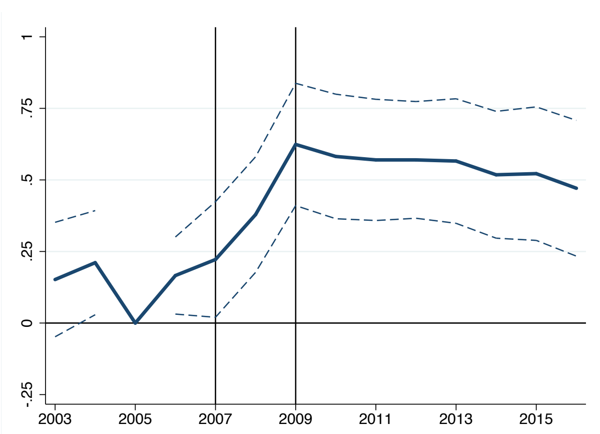
(c) 1990–1991 Recession



(d) 2001 Recession

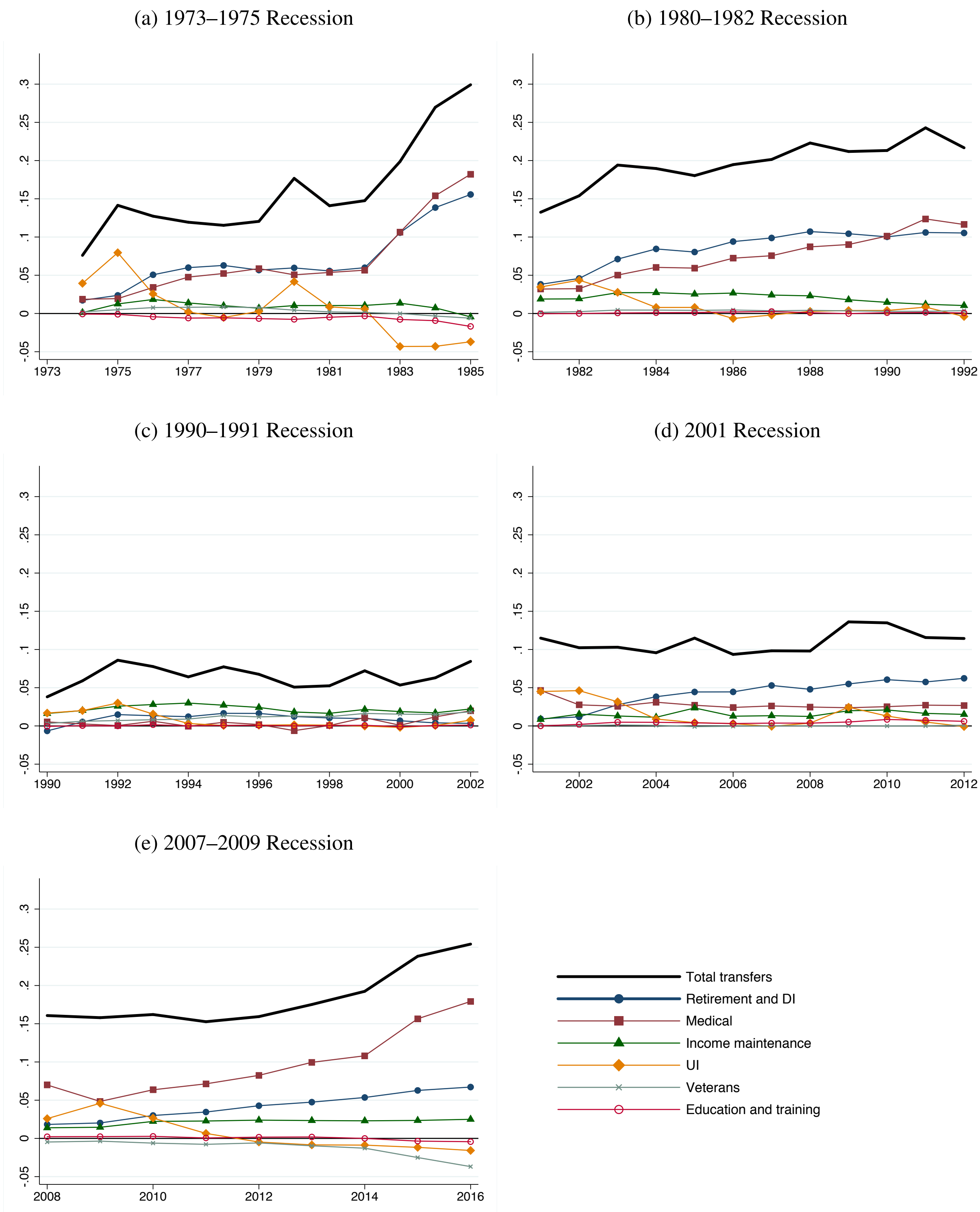


(e) 2007–2009 Recession



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

Figure 11: Impacts of Metropolitan Area Recession Shocks on Real Transfers per Capita Relative to Impact on Real Earnings per Capita

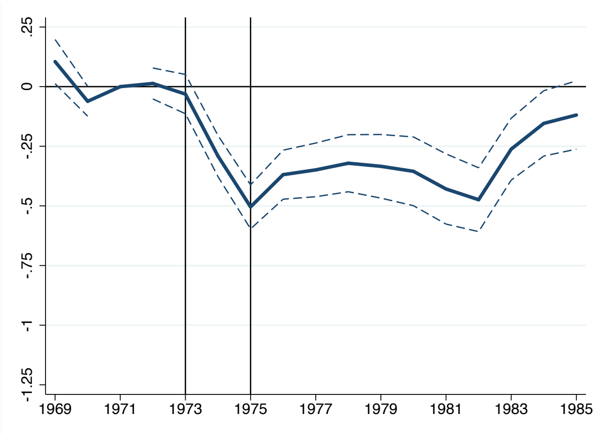


Notes: Figure reports estimates of equation (2), separately for each recession. We display coefficients for transfers in the indicated category per capita (age 15+) divided by coefficients for earnings per capita (age 15+). See notes to Figure 4.

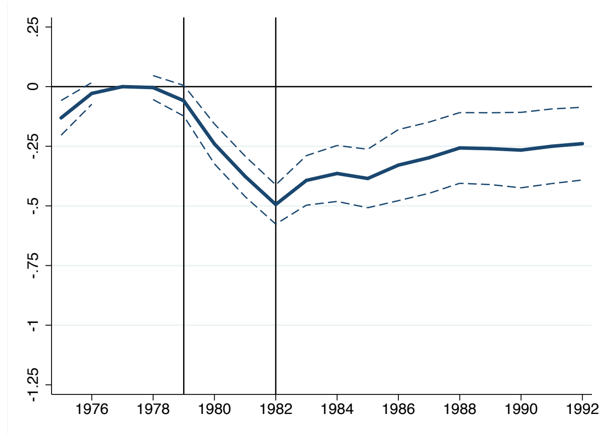
Sources: Authors' calculations using BEAR and SEER data.

Figure 12: Impacts of Metropolitan Area Recession Shocks on Log Real Income per Capita

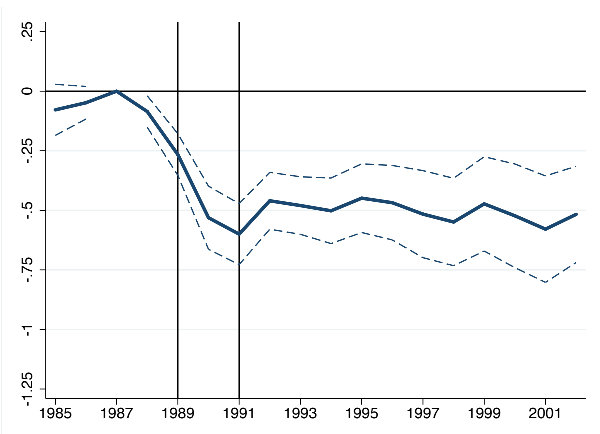
(a) 1973–1975 Recession



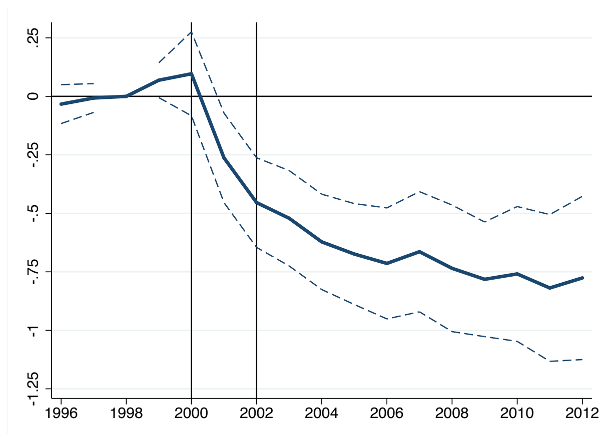
(b) 1980–1982 Recession



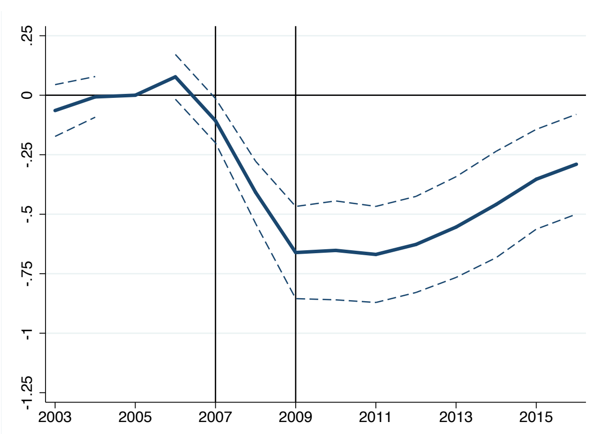
(c) 1990–1991 Recession



(d) 2001 Recession

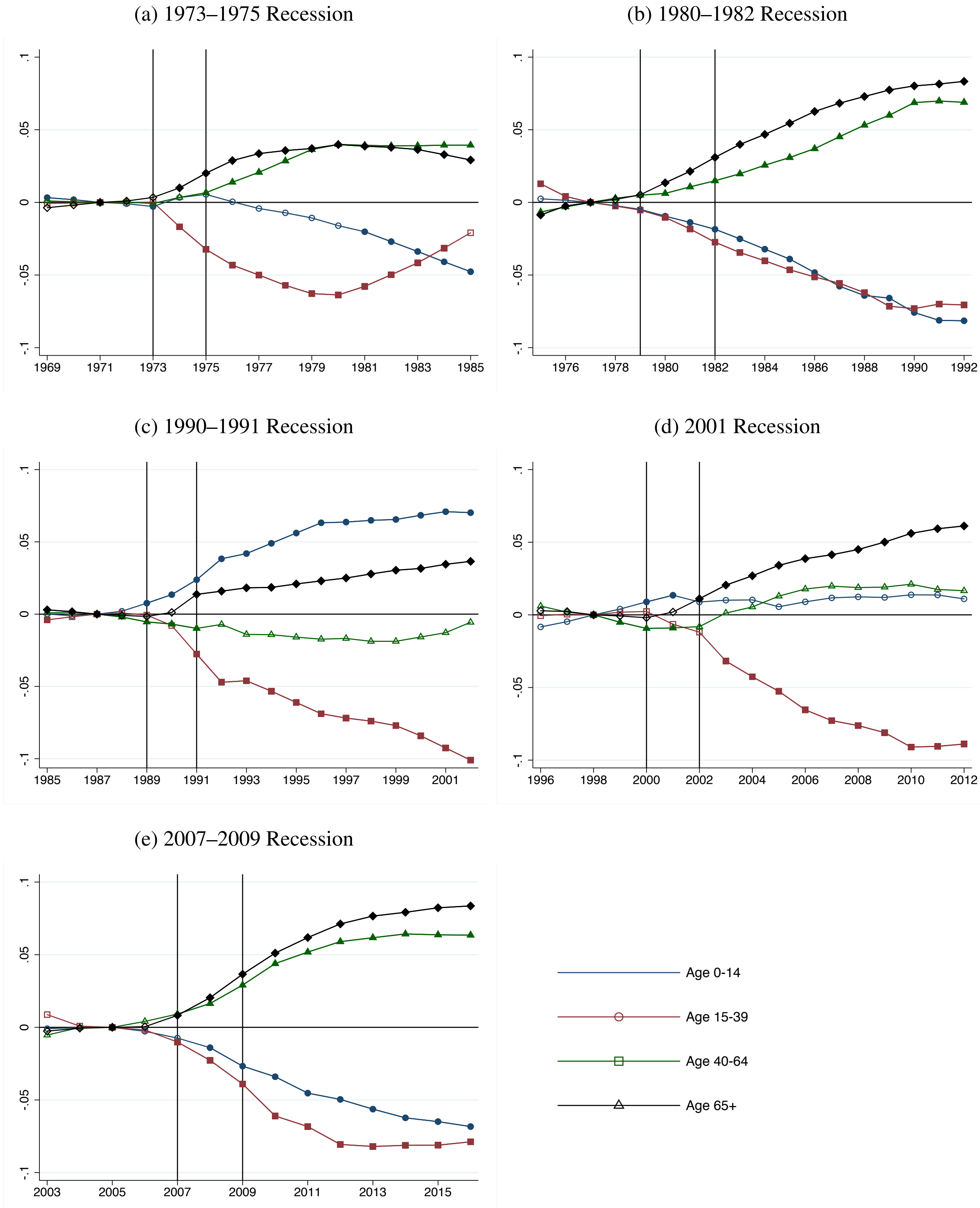


(e) 2007–2009 Recession



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

Figure 13: Impacts of Metropolitan Area Recession Shocks on Age Structure



Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is the share of population in the indicated age range. All regressions control for age shares in the normalization year; for other specification details, see notes to Figure 4. Filled-in markers indicate that the point estimate is significant at the 5-percent level.

Sources: Authors' calculations using BEAR and SEER data.

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.[39] 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.[40] 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, and we provide these crosswalks as part of our public data files.

To ensure we work with consistently defined counties, we use the Census Bureau's county change database, available at `https://www.census.gov/programs-surveys/geography/technical-documentation/county-changes.html` to recode county and county equivalents in the source data (BEAR, CBP, QCEW, SEER) to consistent definitions.[41] 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.[42] As described in the main text, for many of the analyses we first process the microdata and then collapse the relevant measures to our analytic geographies using the crosswalks.

Finally, because the Census/ACS do not provide annual data prior to 2005, we attempted to use the Current Population Survey (CPS) to conduct our event study analyses for compositional and distributional outcomes for some of the earlier recessions. Substate geography indicators become available in the (basic monthly) CPS beginning in 1989, but unlike the Census/ACS, these are not

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

[40] 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.

[41] We use modern names and codes for counties that change only these, and we combine counties that either merge or split.

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

PUMAs but metropolitan areas. As noted above, the definitions of these metro areas change relatively frequently, and sometimes they do so in the data in ways that are not well documented.[43] We have used the appendices in the CPS technical documentation and multiple OMB crosswalks to create harmonized metropolitan areas covering the same geography, when possible, in the CPS from 1989 through 2018. Over this period, we can identify more than 100 distinct areas; over shorter and more recent intervals (such as from 1995 to 2018, which covers both the 2001 and 2007-2009 recessions), the number rises to 145, accounting for about 63 percent of the U.S. population and 74 percent of the population living in metropolitan areas. Around the period of just the Great Recession, from 2004 to 2018, we can identify 221 metropolitan areas.[44]

We provide a public-use data file that synthesizes and harmonizes the data for the various measures and allows replication of our work. Moreover, the consistent coding of metro areas in the CPS should facilitate future work on this topic and others that rely on substate temporal variation.

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. The QCEW 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.[45] However, data suppressions are common, especially earlier in the period. At the county level, data for small or highly concentrated industries (e.g., agriculture, mining) are often suppressed, although very small counties may even have total or total private employment suppressed. When these suppressions occur, *all* data for the county-industry-quarter are suppressed, unlike in County Business Patterns, described below. (For national series, used for constructing the "shifts" in the creation of Bartik shocks, suppression is not an issue.)

For total and total private (excluding government) employment, we impute missing employment counts at the county level through the following ordered process: 1) If total and government employment are reported but private employment is suppressed, we impute private employment as the difference between total and government[46]; 2) If either total *or* private employment is missing

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

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

[45]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.

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

in a given quarter, but not for all quarters in the year, we impute the one that is missing based on the average ratio (private share of total) for the year; 3) If either total *or* private employment is missing for an entire year, 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.

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

A.3 Imputing Employment in County Business Patterns

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

We impute employment at the county by industry level using establishment counts and nationwide information on employment by establishment size. For establishments with fewer than 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 years 1978, 1988, 1999, and 2006 for the 1980–1982, 1990–1991, 2001, and 2007–2009 recessions.[47]

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

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.

[47]For the 1980–1982 and 1990–1991 recessions, we use approximately 70 2-digit SIC industries. For the 2001 and 2007–2009 recessions, we use approximately 85 3-digit NAICS industries. For the 1973–1975 recession, imputation isn't possible.

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

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

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

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

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

where p_1 is the share of establishments of at least 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, 0.61), and (7.51, 0.67). Standard facts about the log-normal distribution imply that the imputed means for the four establishment size groups are (1247, 1951, 3406, 6980) for 1978, (1248, 1949, 3379, 6745) for 1988, (1250, 1949, 3363, 6610) for 1999, and (1248, 1951, 3405, 6956) for 2006.[48]

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 Decomposing Changes in Migration

This section describes our method of decomposing the effects of recessions on population into components due to in-migration, out-migration, and net births.

First consider the identity:

$$\text{pop}_t = \text{pop}_{t-1} + \text{inmig}_{t-1,t} - \text{outmig}_{t-1,t} + \text{netbirths}_{t-1,t} \tag{A.5}$$

where $\text{inmig}_{t-1,t}$ are in-migrants between period $t-1$ and t, $\text{outmig}_{t-1,t}$ are out-migrants between period $t-1$ and t, and $\text{netbirths}_{t-1,t}$ is the count of births minus deaths. Note that $\text{netbirths}_{t-1,t}$ can

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

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

be positive or negative, while other variables are weakly positive. We suppress location subscripts for simplicity.

Take population in period 0 as given. Iterating the above expression, we can write population in period $t > 0$ as

$$\text{pop}_t = \text{pop}_0 + \sum_{j=0}^{t-1} \text{inmig}_{j,j+1} - \sum_{j=0}^{t-1} \text{outmig}_{j,j+1} + \sum_{j=0}^{t-1} \text{netbirths}_{j,j+1}. \tag{A.6}$$

Equation (A.6) allows an exact decomposition of the effect of recessions on population into components due to in-migration, out-migration, and net births. Estimating this equation in levels is unattractive because population and the other variables are highly skewed, leading results to depend heavily on the largest areas. One alternative is to take logs of each side and then derive a decomposition in logs. However, this approach provides only an approximation, and we cannot take the log of netbirths when it is non-positive.

A more useful approach is to normalize equation (A.6) by the baseline population:

$$\frac{\text{pop}_t}{\text{pop}_0} - 1 = \sum_{j=0}^{t-1} \frac{\text{inmig}_{j,j+1}}{\text{pop}_0} - \sum_{j=0}^{t-1} \frac{\text{outmig}_{j,j+1}}{\text{pop}_0} + \sum_{j=0}^{t-1} \frac{\text{netbirths}_{j,j+1}}{\text{pop}_0}. \tag{A.7}$$

We can then estimate versions of equation (2) where the dependent variables are each term of equation (A.7), measured in the SOI migration data. (We construct net births as a residual using equation (A.5).) This transformation leads to better-behaved dependent variables and allows for an exact decomposition if we include the same covariates in all regressions. Since we want to include, at a minimum, the lagged dependent variable as a control, we include all lagged dependent variables—that is, for the normalization year, we control for the transformed in-migration, out-migration, and net births. In practice, the results are not very sensitive to these additional controls.

B.2 Robustness to Different Employment Shocks

Our baseline specification uses public and private wage and salary employment from BEAR to construct recession shocks. 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 alternatives are private wage and salary employment from BEAR and private wage and salary employment from QCEW.[49] Figures A.4–A.7 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 the employment shock. The similarity of the results is not surprising, as the public sector accounts 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.

[49] 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.

B.3 Results Using Bartik Shocks

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

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

where $\eta_{i,j}$ is the share of employment in local area i in industry j in a base year, and the term in parentheses equals the nationwide log employment change in industry j from recession peak to trough. We use CBP data to construct $\eta_{i,j}$ (see Appendix A.3) and QCEW data to construct the nationwide log employment change.[50]

We do not use the Bartik shock 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 like the Bartik shock (Adão, Kolesár and Morales, 2018; Kirill, Hull and Jaravel, 2018; Goldsmith-Pinkham, Sorkin and Swift, 2018). Nonetheless, given the ubiquity of the Bartik shock, we report results from using it here.

Appendix Table A.5 describes the relationship between our preferred recession shock (actual log employment change) and the Bartik shock (predicted log employment change). The first column includes no other controls. For every recession besides 1990–1991, the Bartik shock explains 33–36 percent of the cross-metro variation in the recession shock. For 1990–1991, the Bartik shock explains only six percent of the variation. Columns 2 and 3 add in division fixed effects and controls for lagged population growth. The coefficients—which are all positive, as expected—are reasonably stable across specifications, especially after 1973–1975 when greater industry-level detail is available. Moreover, the coefficient estimates remain highly statistically significant even with the additional controls.

Appendix Table A.6 shows that Bartik shocks are more highly correlated across time than our preferred recession shocks. This is not surprising, as Bartik shocks primarily reflect local industry employment shares, which are relatively stable. These high correlations raise the concern that the coefficients on the Bartik shock variable might not isolate the impact of a given recession.

Appendix Figure A.8 displays estimates of the effect of the Bartik shock on log employment. The results are qualitatively similar to those using recession shocks in Figure 4 for the 1980–1982, 2001, and 2007–2009 recessions.[51] There is less evidence of a persistent employment decline

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

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

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.9 through A.11 display results for population, the employment-population ratio, and earnings per capita. The patterns largely mirror those already discussed for employment.

B.4 The Effects of Recessions on Commuting Zones

Our main approach defines local labor markets as metropolitan areas. Another reasonable approach is to use commuting zones as the unit of the geography, which span the entire (continental) United States, including rural areas. Appendix Figures A.12 through A.15 show that results are very similar when using commuting zones (specifically, the 2000 definition).

B.5 The Great Recession and Local Labor Markets: Evidence from the American Community Survey

This section uses the ACS to estimate the impacts of the Great Recession on local labor markets. The ACS provides annual data for substate geographies starting in 2005, and these individual-level data allow us to estimate results not possible with aggregate data.

Appendix Figure A.16 describes the impacts of the Great Recession on employment. Panel A reports estimates of equation (2) where the dependent variable is the log of the share of individuals age 25–54 who are employed.[52] The results are qualitatively similar to those for the log employment-population ratio from BEAR and SEER data (which is the ratio of the number of jobs to individuals age 15+), but smaller in magnitude. Possible explanations for this difference include (a) the impacts on employment are more severe for individuals age 15–24 or over 54, (b) individuals shift into self-employment, (c) the recession reduces the rate of multiple job-holding, or (d) the ACS employment measure better captures informal or temporary work. We will examine possibilities (a), (b), and (d) in the future. Unfortunately, multiple job-holding is not measured in the ACS, so we cannot directly examine (c); however, multiple job-holding as measured in the CPS is relatively rare (5–6 percent) and has been falling secularly even before the Great Recession (Lalé, 2015), so we believe this channel is unlikely to be a driver of the divergence between the ACS and BEAR estimates. Panel B reports results for the share of individuals employed full-time full-year (at least 35 hours per week and 50 weeks per year). The recession leads to a greater decrease in full-time full-year employment than employment overall, consistent with some individuals working only part-time or part-year after the recession.

Appendix Figure A.17 displays results for annual, weekly, and hourly wage and salary earnings. Examining these different outcomes provides evidence on the sources of the decline in annual earnings. The effect on average weekly earnings is about three-fourths as large as the effect on annual earnings, and the effect on hourly earnings is over half as large as the effect on annual earnings. Consistent with the pattern in Appendix Figure A.16, these results imply that impacts of the recession on weeks worked per year and usual hours worked per week can account for some, but less than half, of the impact on annual earnings. Results are very similar for the 50th percentile.

[52] We define employment to be a positive number of weeks worked in the preceding 12 months. When we define employment based on activity in the prior week, the estimates are larger in magnitude, but still smaller than for full-time, full-year employment.

For the 10th percentile, the weekly and hours impacts are considerably smaller than the annual impacts, suggesting that much of the earnings decline is due to a reduction in hours worked. For the 90th percentile, all of the impacts are extremely similar, indicating that changes in realized labor supply played little role in this earnings decline.

Appendix Figure A.18 displays results with and without adjusting for the composition of the workforce in each metro area. Our baseline composition adjustment controls for 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. Our second adjustment further adds 322 occupation fixed effects and 225 industry fixed effects to these demographic variables. To perform these adjustments, we run separate regressions on individual-level data for each year, with the specified controls, and use the residuals to construct the dependent variables for estimation of equation (2).

Panel A shows results for average log annual earnings. The results are quite similar to those for earnings per capita (using BEAR and SEER data) in Figure 9. During the recession, the results with and without demographic adjustments are very similar. Following the recession, the demographic-adjusted impacts are slightly attenuated, suggesting that changes in composition can explain some of the long-term earnings decline. However, the demographic-adjusted line always lies within the unadjusted confidence interval. Controlling also for occupation and industry leads to more attenuation, which implies that the shift towards lower-paying industries and occupations explains part of the long-term earnings decline. Nonetheless, the point estimates imply that demographics, industry, and occupation can explain no more than 50 percent of the decline in mean earnings. Panels B, C, and D present results for the 50th, 10th, and 90th percentiles of the earnings distribution. The Great Recession reduced earnings across the distribution, although the decline is more severe at the 10th percentile, especially in the short-run.[53] This leads to an increase in earnings inequality.

Understanding the underlying composition shifts is of independent interest. Appendix Figure A.19 plots estimates of the effect of the Great Recession on the share of individuals age 25–54 with a high school degree or less, some college (but less than a bachelor's degree), or at least a bachelor's degree. The share of individuals with a high school degree or less increases after the recession, and the share of individuals with at least a bachelor's degree falls. These composition changes largely occurred by 2011.

Appendix Figure A.20 displays changes in the share of workers in managerial, professional, and technical; administrative, production, office, and sales; and manual and service occupations. The share of workers in the higher-paid occupations falls, while the share in lower-paid occupations rises. The changes in occupational structure are relatively stable after 2011, coinciding with the change in educational composition.

To summarize, we find systematic evidence that the Great Recession durably shifted hours and weeks downward at the intensive margin (as well as the extensive margin noted earlier), with concomitant durable shifts in the composition of area residents toward the less educated and lower-earning occupations. Although these compositional changes account for less than half of the overall decline in earnings, they are associated with widening dispersion in earnings and, as Appendix Figure A.21 shows, an increase in poverty.

[53]One limitation of studying the distribution of log earnings is that individuals with zero earnings are excluded. If individuals who are no longer employed tend to come from the bottom of the earnings distribution, this would attenuate the negative effects of the recession on the 10th percentile. In the future, we plan on exploring this issue further.

These results focus on individuals age 25–54, who tend to be more attached to the labor force. Because we have also found age composition changes stemming from recessions (Figure 13), we plan to further examine related impacts for different age groups.

Table A.1: Impacts of Metropolitan Area Recession Shocks on Employment, Census/ACS

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Coefficient: Interaction between recession shock and post-recession year					
Log employment rate	−0.044	−0.021	−0.023	−0.134	−0.107
	(0.038)	(0.020)	(0.030)	(0.030)	(0.042)
Log share working 50+ weeks	−0.007	−0.013	−0.118	−0.310	−0.181
	(0.066)	(0.052)	(0.059)	(0.063)	(0.075)
Log share working 50+ weeks and 35+ hours/week	0.077	−0.069	−0.085	−0.401	−0.286
	(0.073)	(0.056)	(0.063)	(0.073)	(0.081)

Notes: Table reports estimates of separate regressions for each recession. The dependent variable is taken from the post-recession Census year (1980, 1990, 2000, 2005–2007, and 2015–17, respectively) for individuals age 25–54. The 1973–75 regression controls for the 1970 value of the dependent variable, and other regressions control for two pre-recession values. See notes to Table 3.

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

Table A.2: Impacts of Metropolitan Area Recession Shocks on Annual, Weekly, and Hourly Wage Earnings, Census/ACS

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

Notes: See notes to Table 7.
Sources: Authors' calculations using BEAR, decennial Census, and ACS data.

Table A.3: Impacts of Metropolitan Area Recession Shocks on Detailed Transfer Categories, Relative to Effect on Earnings, 1–3 Years after Recession Trough

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Impact on transfers divided by impact on earnings, ($\times -100$)					
Total transfers	12.08	18.80	7.57	10.51	15.79
	(2.23)	(3.45)	(2.38)	(1.73)	(3.30)
Retirement and DI	5.77	7.86	1.34	3.77	3.57
	(0.73)	(1.19)	(0.84)	(0.53)	(0.53)
Social Security	5.97	7.62	1.16	3.63	3.34
	(0.67)	(1.07)	(0.79)	(0.58)	(0.51)
Non-SS Retirement and DI	-0.18	0.21	0.30	0.09	−0.17
	(0.19)	(0.14)	(0.22)	(0.28)	(0.10)
Medical	4.45	5.67	0.21	2.81	7.24
	(0.76)	(0.73)	(1.74)	(1.13)	(1.49)
Medicare	2.02	2.71	0.33	1.80	3.72
	(0.41)	(0.48)	(0.58)	(0.62)	(0.75)
Public assistance medical care	1.67	2.54	−0.12	0.59	2.64
	(0.54)	(0.48)	(1.36)	(0.85)	(0.78)
Military Medical Care	−0.04	0.04	0.14	0.08	0.02
	(0.03)	(0.03)	(0.09)	(0.05)	(0.06)
Income maintenance	1.42	2.66	2.80	1.65	2.30
	(0.81)	(0.49)	(0.54)	(0.44)	(0.81)
SSI	0.21	0.45	0.07	0.00	0.27
	(0.27)	(0.08)	(0.16)	(0.06)	(0.09)
Other income maintenance	0.84	0.78	1.41	0.27	0.06
	(0.43)	(0.42)	(0.27)	(0.29)	(0.44)
EITC		0.07	0.15	0.22	0.22
		(0.01)	(0.09)	(0.09)	(0.10)
SNAP		1.24	1.36	1.08	1.28
		(0.18)	(0.24)	(0.19)	(0.29)
UI	0.78	1.46	1.59	1.35	0.96
	(0.72)	(0.49)	(0.57)	(0.24)	(0.39)
State UI	0.56	1.44	1.31	1.24	1.08
	(0.69)	(0.48)	(0.49)	(0.23)	(0.36)
Federal UI	0.14	0.07	0.38	0.06	−0.15
	(0.12)	(0.04)	(0.20)	(0.03)	(0.06)
Veterans	0.81	0.43	0.81	0.03	−0.66
	(0.25)	(0.17)	(0.25)	(0.16)	(0.27)
Education	−0.53	0.10	0.06	0.45	0.16
	(0.25)	(0.10)	(0.15)	(0.18)	(0.26)

Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is transfers in the indicated category per capita (age 15+). We normalize the impacts by dividing the coefficients for transfers by the coefficients from a regression where the dependent variable is earnings per capita. Coefficients do not add up perfectly because the lagged dependent variable differs for each outcome. See notes to Figure 4.
Sources: Authors' calculations using BEAR and SEER data.

Table A.4: Impacts of Metropolitan Area Recession Shocks on Detailed Transfer Categories, Relative to Effect on Earnings, 7–9 Years after Recession Trough

	Recession				
	1973–75	1980–82	1990–91	2001	2007–09
Impact on transfers divided by impact on earnings, (× −100)					
Total transfers	18.88	22.25	5.90	12.85	25.42
	(4.45)	(4.98)	(2.57)	(2.47)	(6.78)
Retirement and DI	9.03	10.35	0.89	5.77	6.71
	(1.44)	(2.03)	(1.00)	(0.91)	(1.37)
Social Security	9.56	10.58	1.10	5.44	5.68
	(1.40)	(1.96)	(0.94)	(0.89)	(1.27)
Non-SS Retirement and DI	−0.44	−0.18	−0.24	0.17	−0.39
	(0.35)	(0.59)	(0.28)	(0.17)	(0.31)
Medical	9.21	10.50	0.39	2.55	17.91
	(1.54)	(1.61)	(1.61)	(1.22)	(3.14)
Medicare	4.58	5.33	1.09	2.12	10.40
	(0.92)	(1.01)	(0.67)	(0.79)	(1.86)
Public assistance medical care	3.28	5.02	−0.44	0.29	8.13
	(0.93)	(1.06)	(1.22)	(0.77)	(2.27)
Military Medical Care	−0.17	−0.10	0.21	0.16	−0.15
	(0.07)	(0.09)	(0.09)	(0.08)	(0.11)
Income maintenance	1.08	1.49	1.89	1.90	2.50
	(1.01)	(0.96)	(0.63)	(0.70)	(1.25)
SSI	0.49	0.80	0.14	0.14	0.60
	(0.27)	(0.18)	(0.16)	(0.09)	(0.22)
Other income maintenance	0.28	0.07	0.88	0.37	-0.51
	(0.51)	(0.65)	(0.27)	(0.34)	(0.67)
EITC		0.13	0.14	0.24	0.46
		(0.06)	(0.23)	(0.11)	(0.19)
SNAP		0.49	0.61	1.08	1.04
		(0.32)	(0.15)	(0.27)	(0.41)
UI	−1.91	0.54	−0.04	1.40	−1.58
	(0.98)	(0.40)	(0.26)	(0.46)	(0.35)
State UI	−2.09	0.53	−0.06	1.34	−1.54
	(0.97)	(0.39)	(0.25)	(0.45)	(0.33)
Federal UI	0.06	0.05	0.06	0.07	−0.06
	(0.06)	(0.03)	(0.04)	(0.03)	(0.03)
Veterans	0.00	0.31	1.46	0.01	−3.69
	(0.28)	(0.22)	(0.48)	(0.29)	(1.42)
Education	−0.59	0.08	0.02	0.70	−0.43
	(0.28)	(0.17)	(0.15)	(0.30)	(0.42)

Notes: See notes to Table A.3.

Sources: Authors' calculations using BEAR and SEER data.

Table A.5: Cross-Sectional Relationship between Metropolitan Level Log Employment Change and Bartik Shock

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

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

Table A.6: Correlation of Metropolitan Area Bartik Shocks

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

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

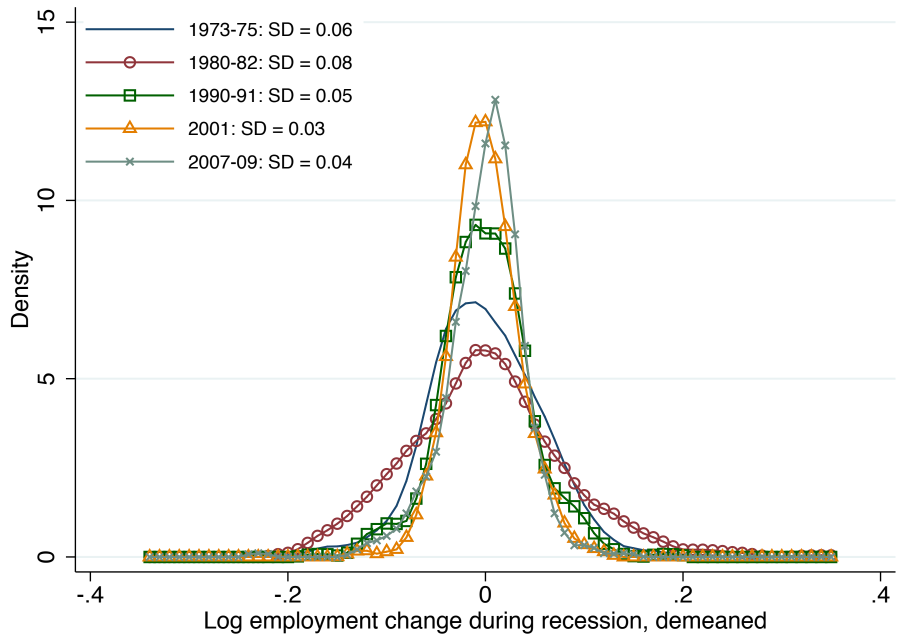
Table A.7: Impacts of Metropolitan Area Recession Shocks on Age Structure, 7–9 Years after Recession Trough

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

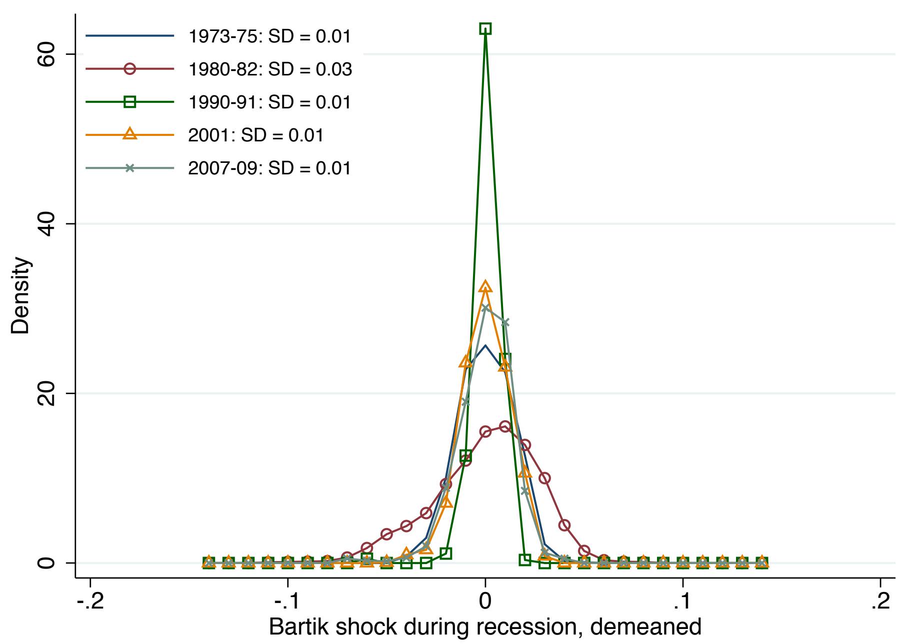
Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is the share of population in the indicated category. All regressions control for all age shares in the normalization year, plus the covariates described in Table 3.
Sources: Authors' calculations using BEAR and SEER data.

Figure A.1: Density of Recession Severity and Bartik Shock Across Metros

(a) Recession Severity



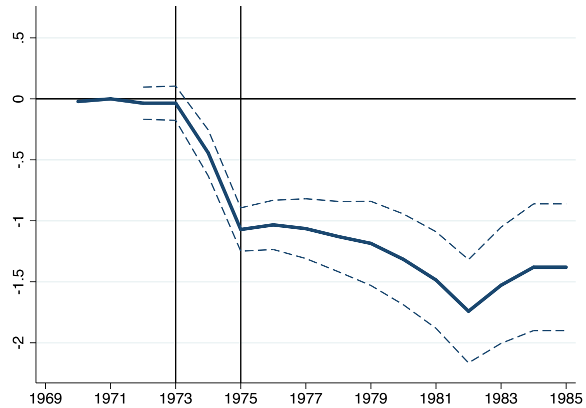
(b) Bartik Shock



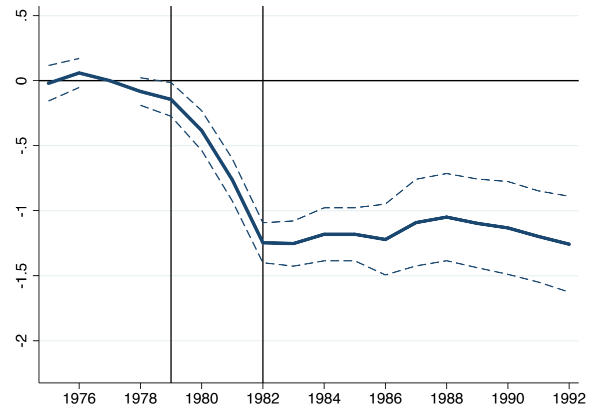
Notes: The figure shows estimated kernel densities of the log wage and salary employment change (Panel A) and predicted log employment change based on pre-recession industrial structure (as in Bartik (1991); Panel B) across metros for each of the five recessions since the mid 1970s. For each recession,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

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

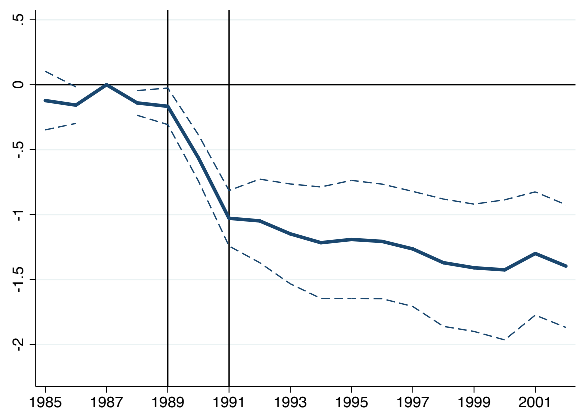
(a) 1973–1975 Recession



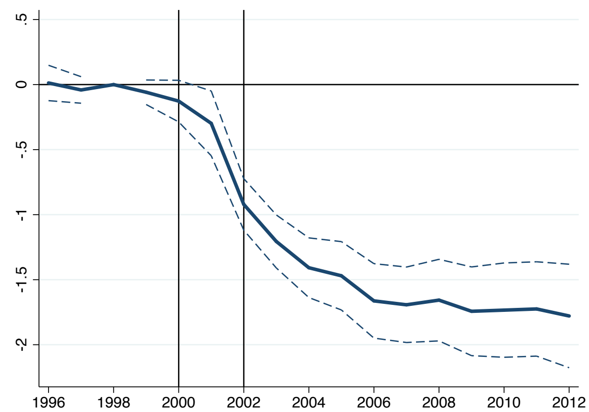
(b) 1980–1982 Recession



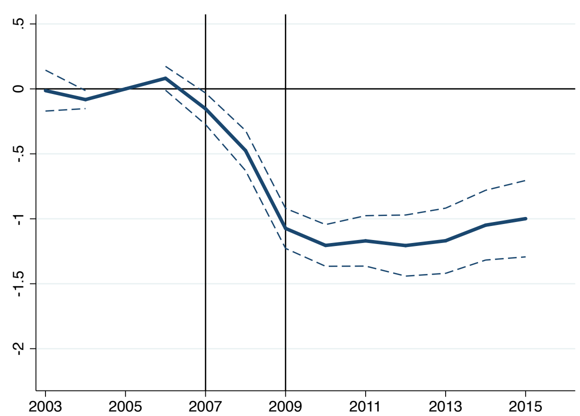
(c) 1990–1991 Recession



(d) 2001 Recession

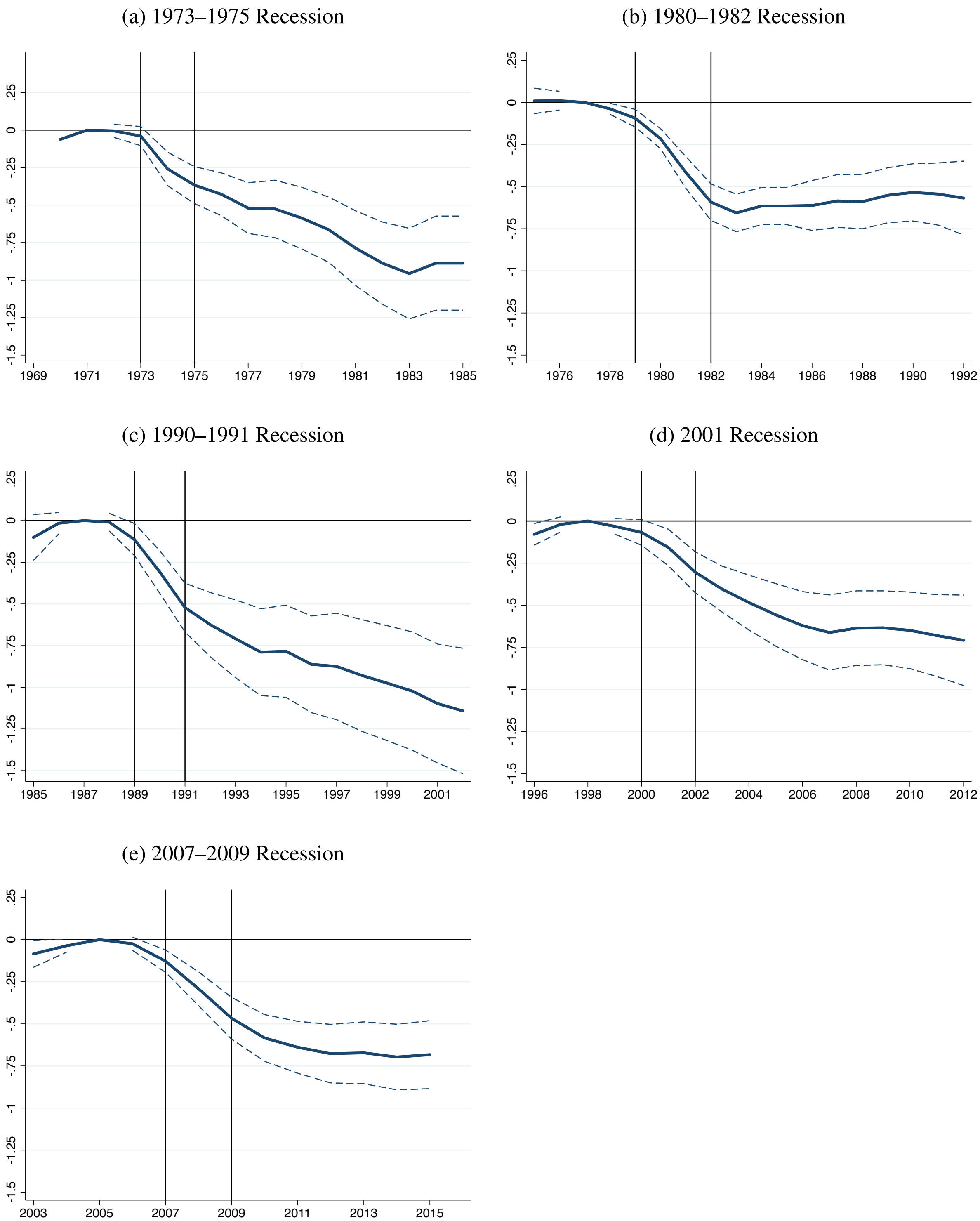


(e) 2007–2009 Recession



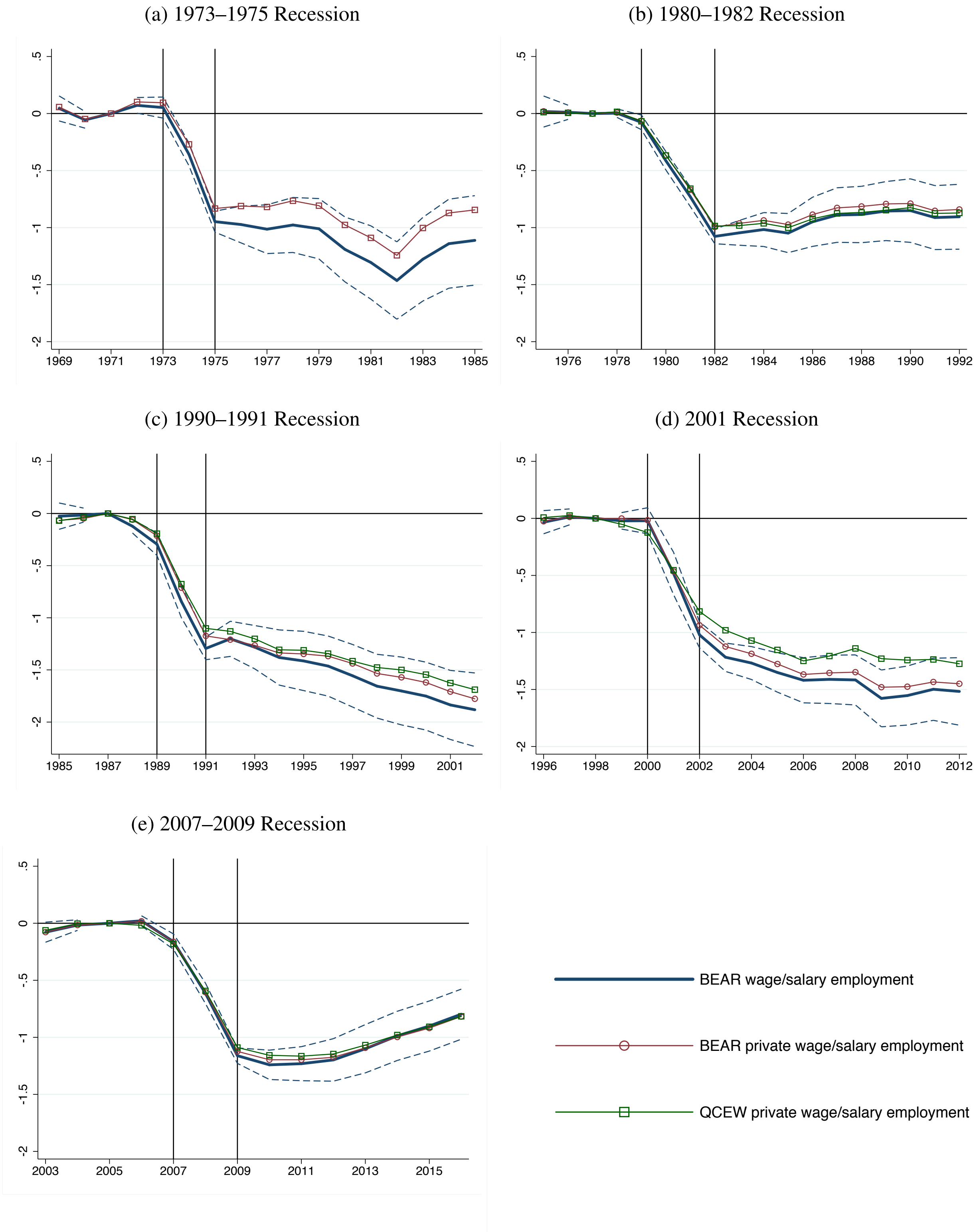
Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log employment from CBP data. See notes to Figure 4.
Sources: Authors' calculations using CBP, BEAR, and SEER data.

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



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

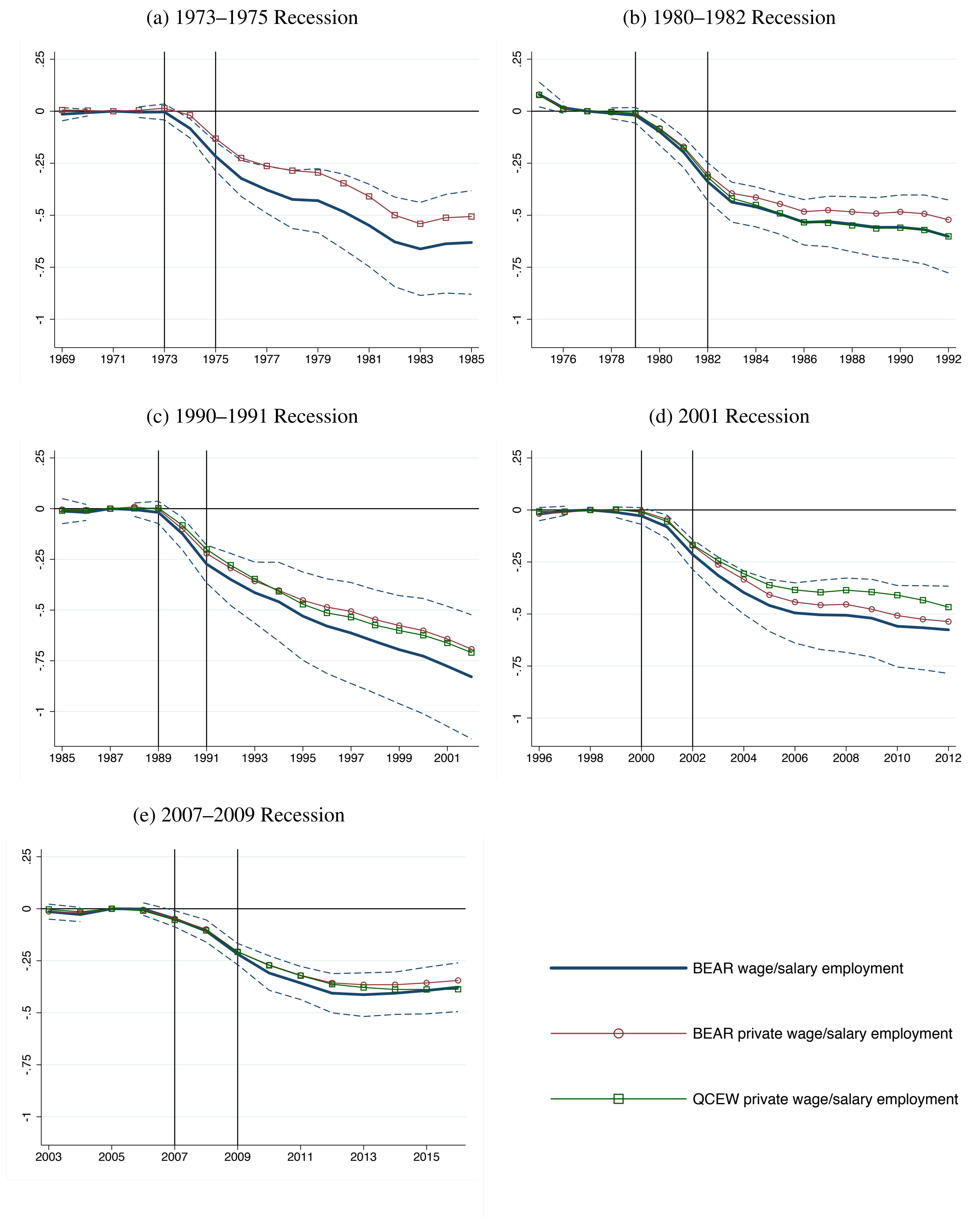
Figure A.4: Impacts of Metropolitan Area Recession Shocks on Log Employment, Robustness to Different Employment Shocks



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

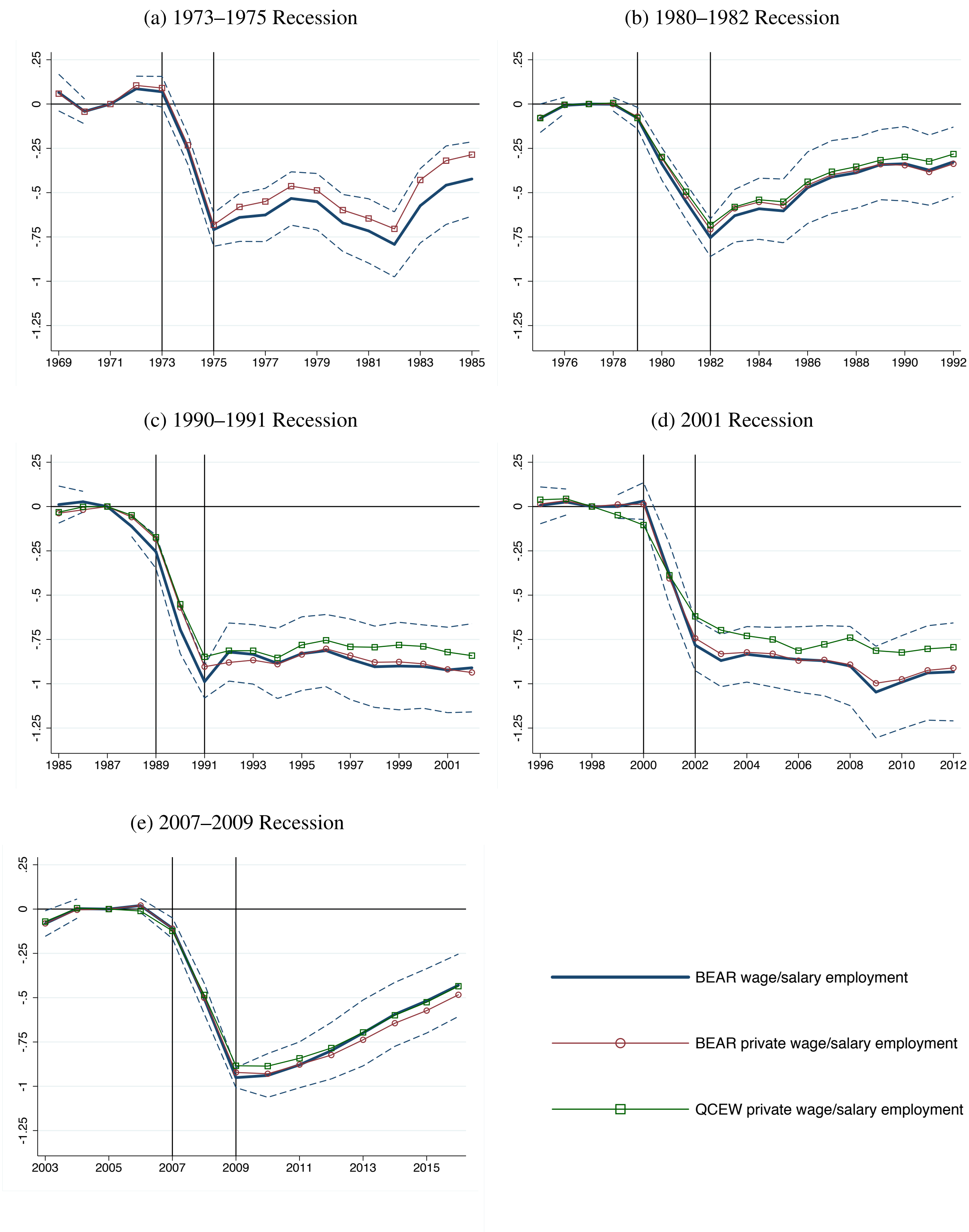
Sources: Authors' calculations using BEAR, QCEW, and SEER data.

Figure A.5: Impacts of Metropolitan Area Recession Shocks on Log Population Age 15+, Robustness to Different Employment Shocks



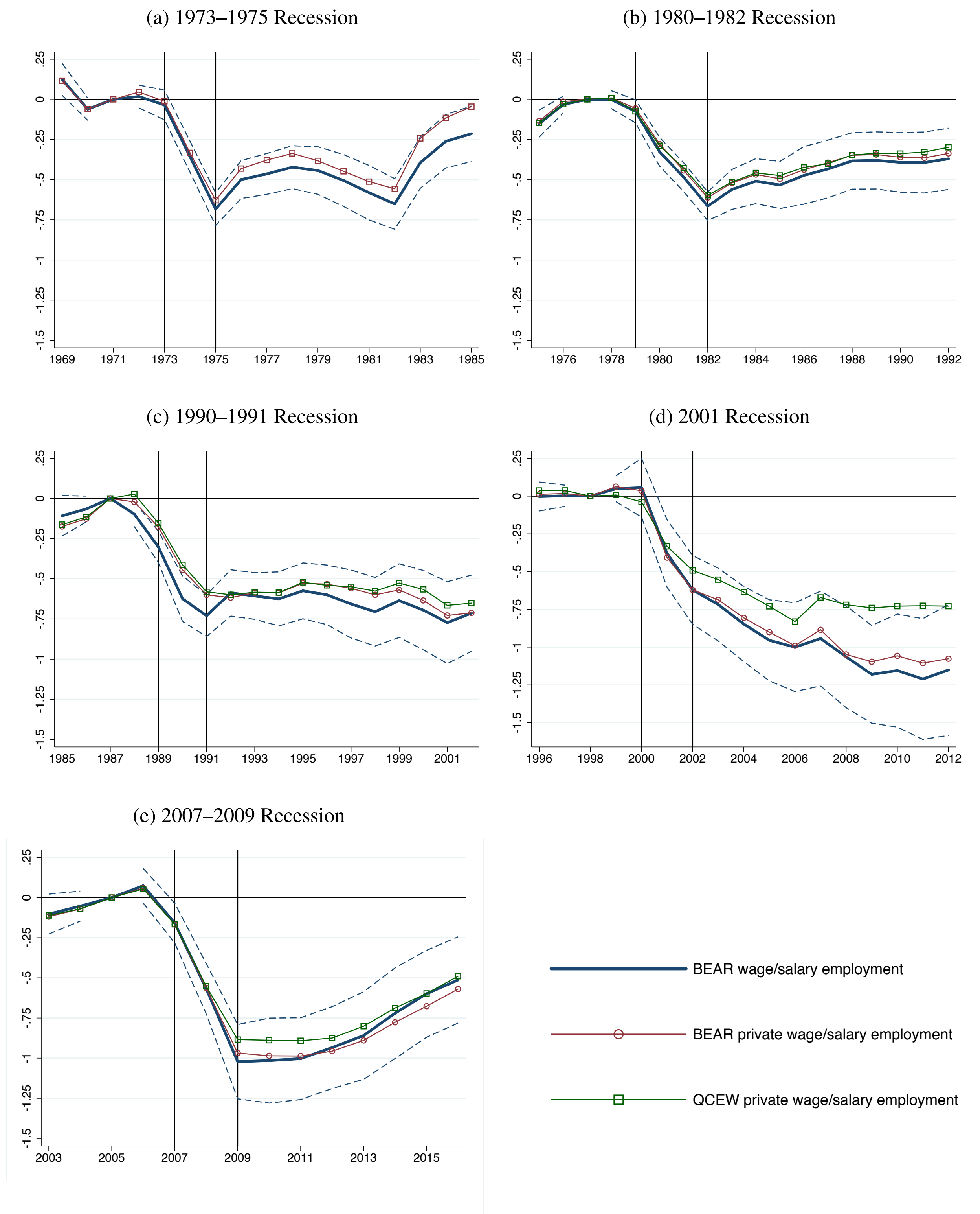
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log population age 15 and above, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment, we normalize the coefficients by multiplying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

Figure A.6: Impacts of Metropolitan Area Recession Shocks on Log Employment-Population Ratio, Robustness to Different Employment Shocks



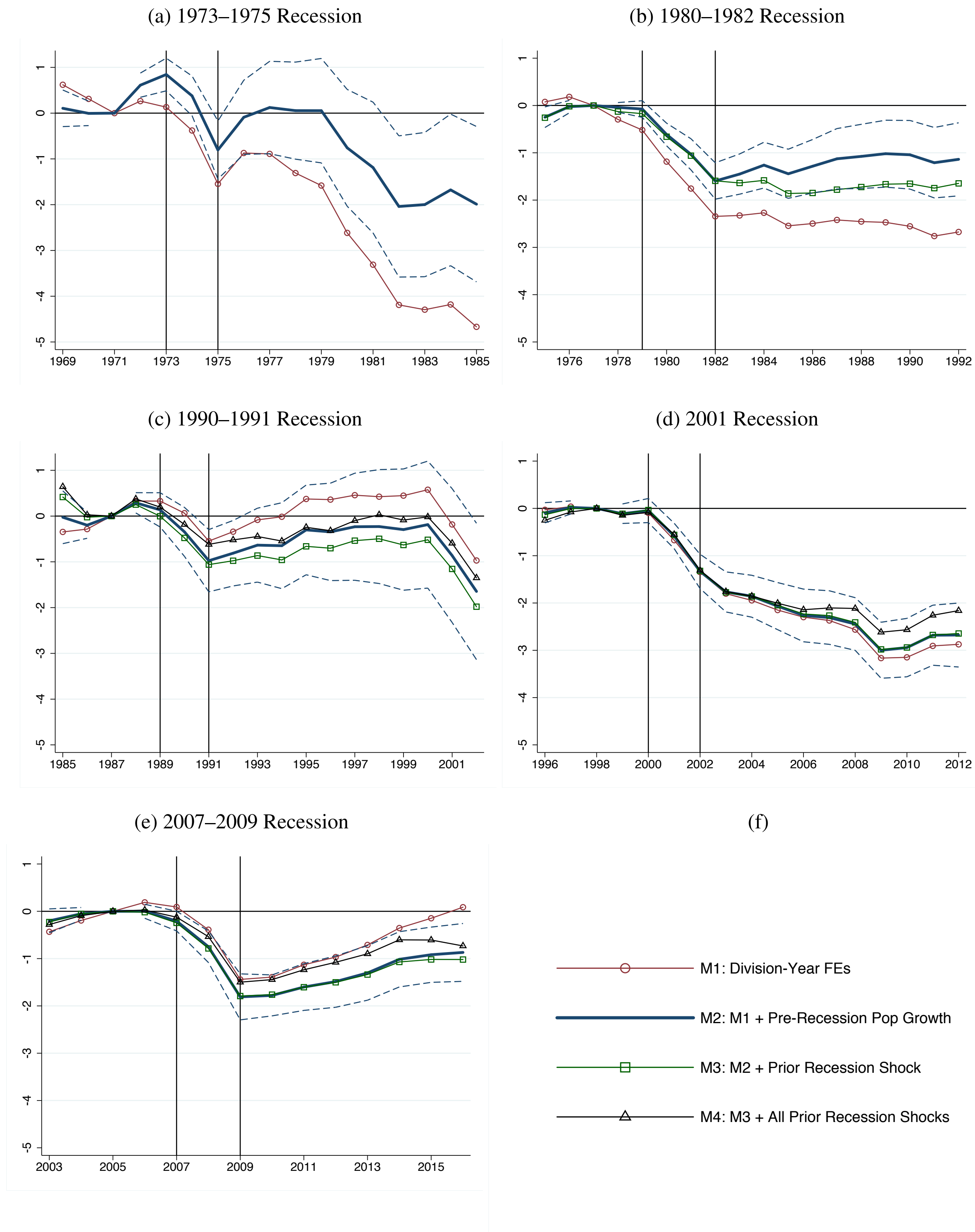
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 15 and above, and the key independent variable is indicated in the legend. For independent variables besides BEA wage/salary employment,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

Figure A.7: Impacts of Metropolitan Area Recession Shocks on Log Real Earnings per Capita, Robustness to Different Employment Shocks



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

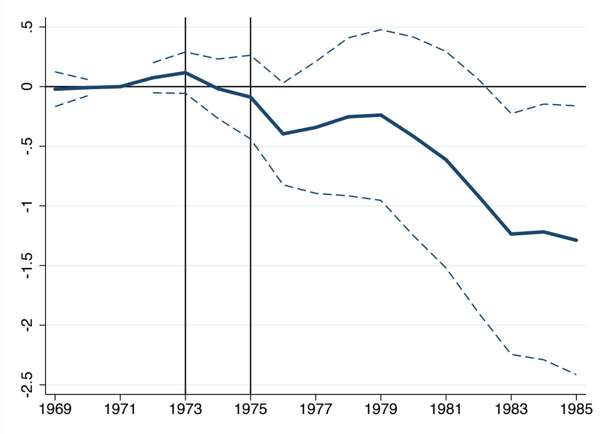
Figure A.8: Impacts of Metropolitan Area Bartik Shocks on Log Employment



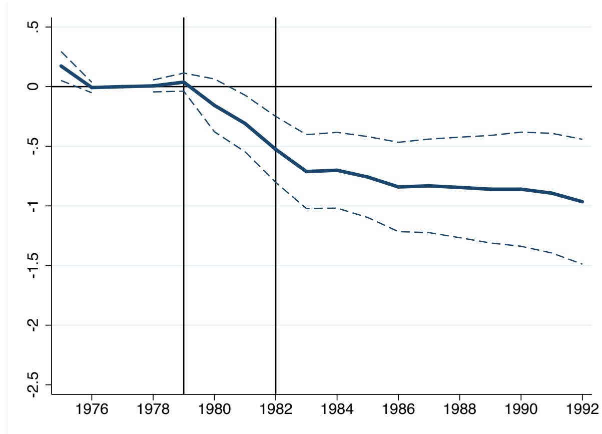
Notes: Table reports estimates of equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data, and the key independent variable is the predicted log employment change as in Bartik (1991). Specifications are indicated by the legend. See notes to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

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

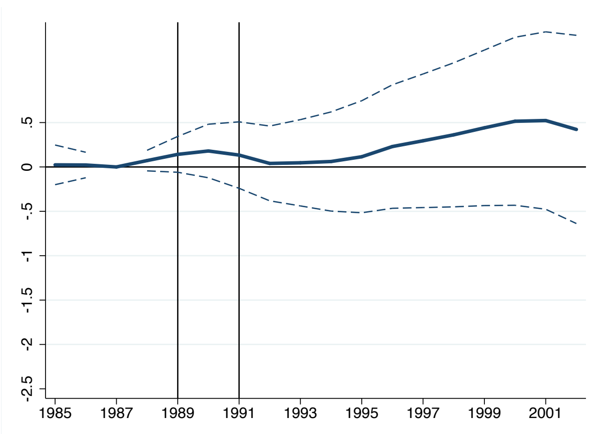
(a) 1973–1975 Recession



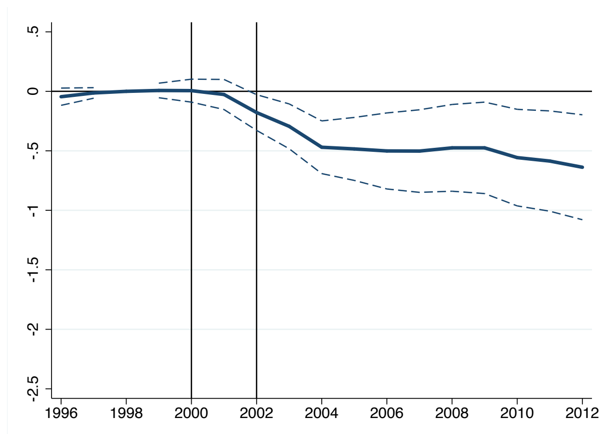
(b) 1980–1982 Recession



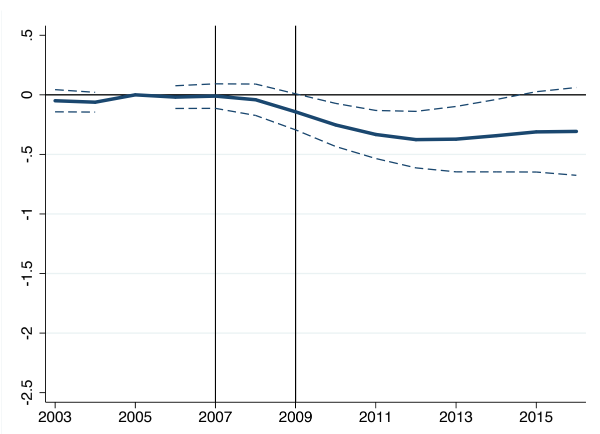
(c) 1990–1991 Recession



(d) 2001 Recession



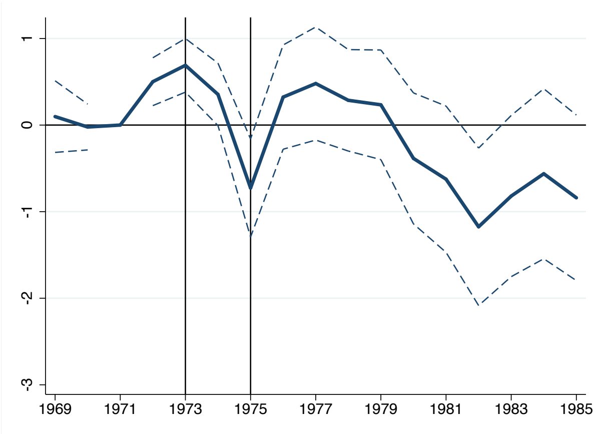
(e) 2007–2009 Recession



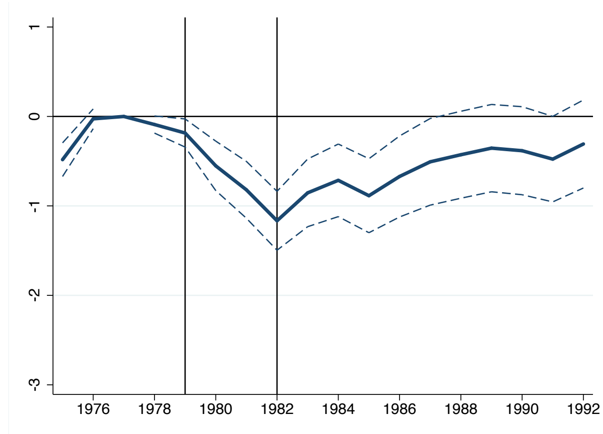
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure A.8.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

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

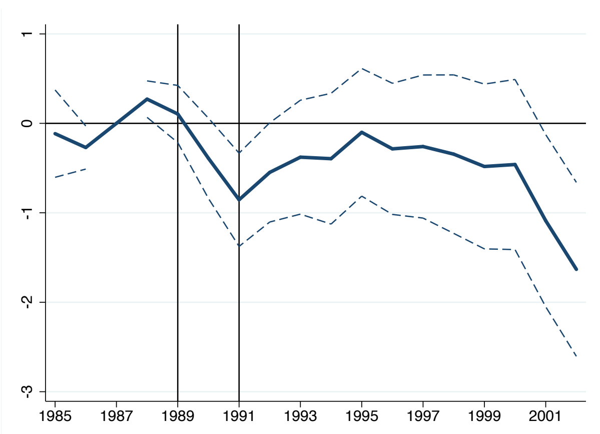
(a) 1973–1975 Recession



(b) 1980–1982 Recession

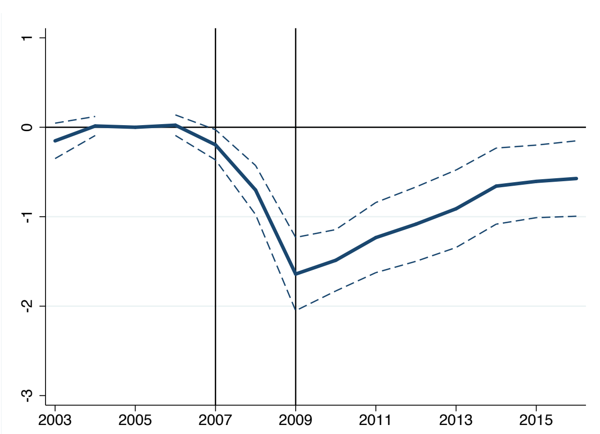


(c) 1990–1991 Recession



(d) 2001 Recession

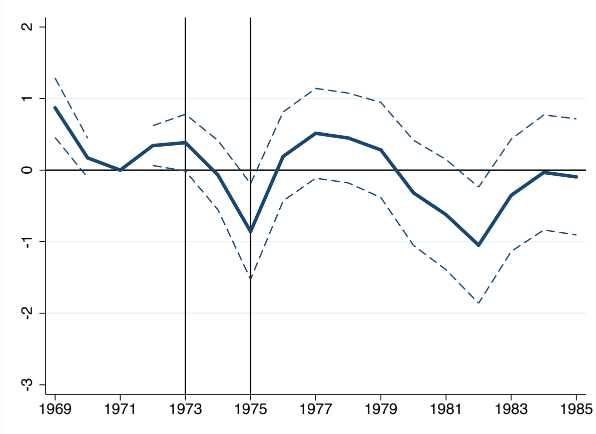
(e) 2007–2009 Recession



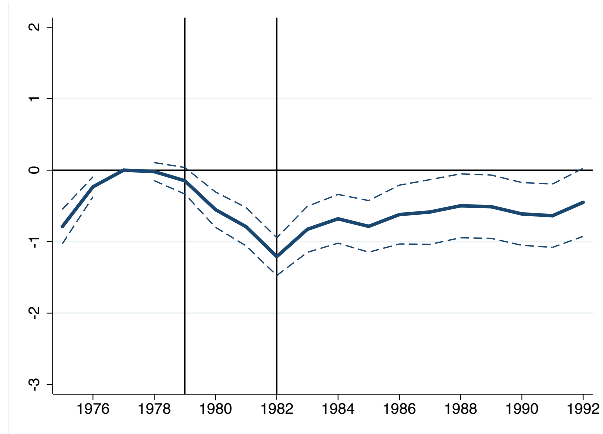
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 15 and above. See notes to Figure A.8.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

Figure A.11: Impacts of Metropolitan Area Bartik Shocks on Log Real Earnings per Capita

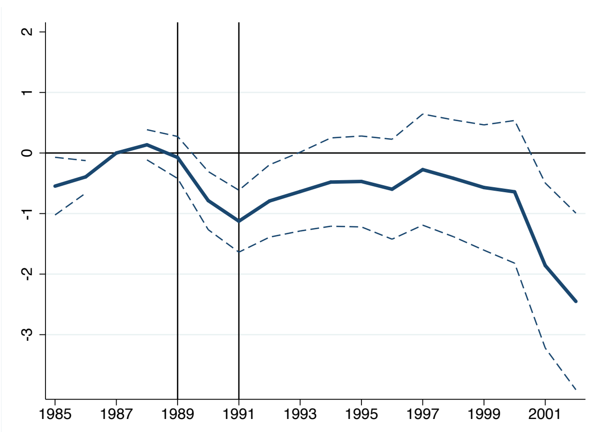
(a) 1973–1975 Recession



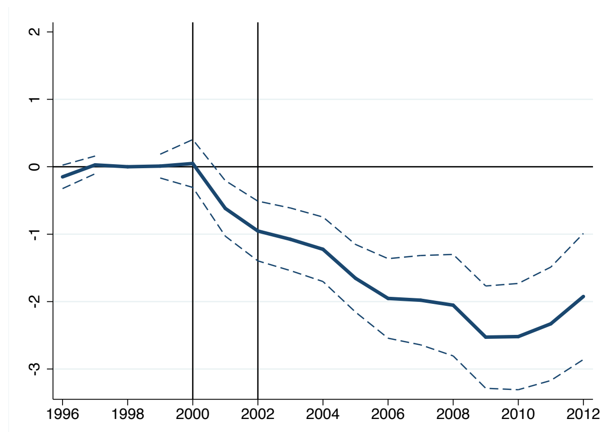
(b) 1980–1982 Recession



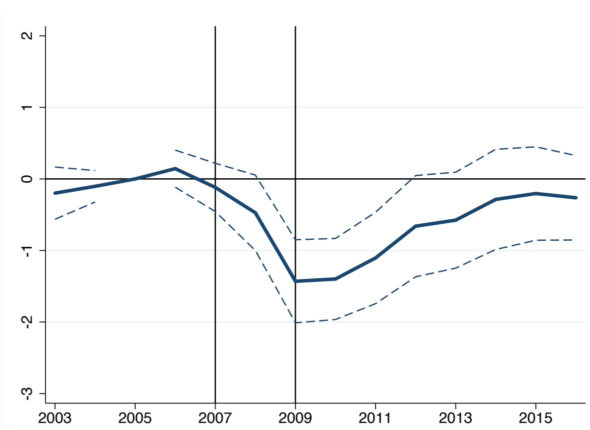
(c) 1990–1991 Recession



(d) 2001 Recession



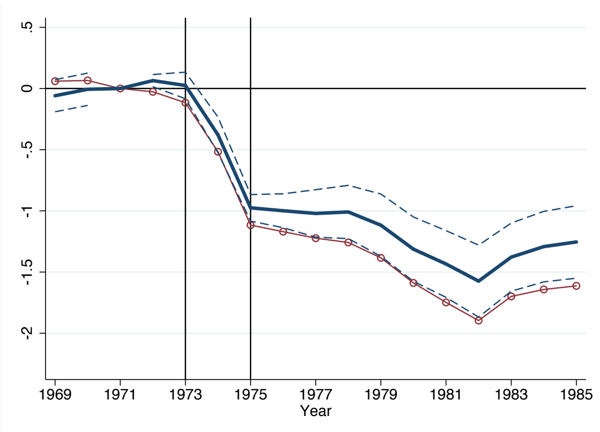
(e) 2007–2009 Recession



Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure A.8.
Sources: Authors' calculations using BEAR, CBP, QCEW, and SEER data.

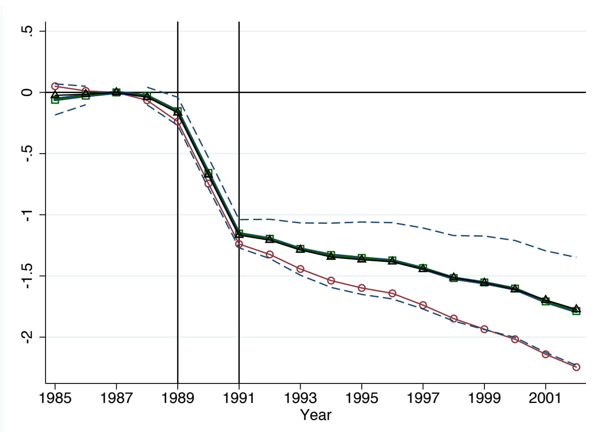
Figure A.12: Impacts of Commuting Zone Recession Shocks on Log Employment

(a) 1973–1975 Recession

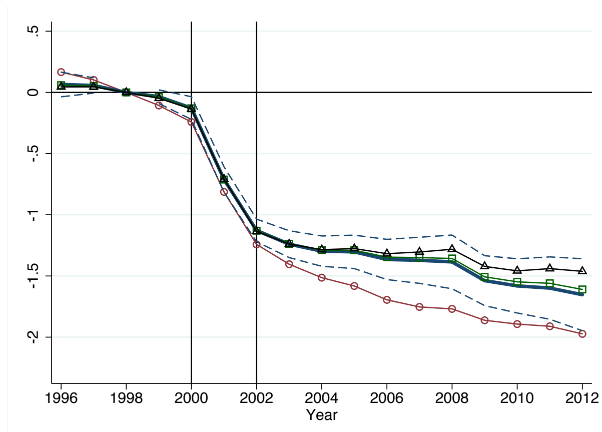


(b) 1980–1982 Recession

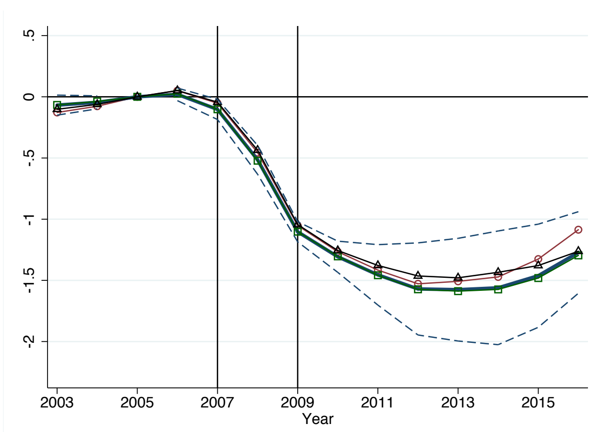
(c) 1990–1991 Recession

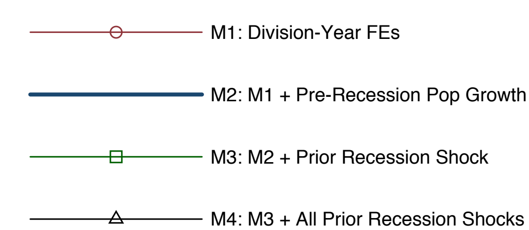


(d) 2001 Recession



(e) 2007–2009 Recession

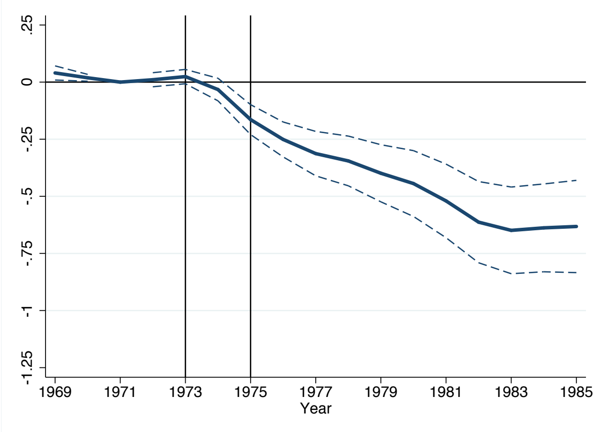




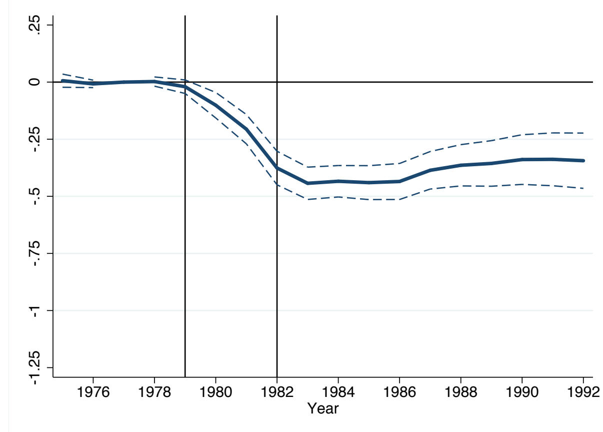
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log wage and salary employment from BEAR data. There are 691 CZs in the sample. Standard errors are clustered by commuting zone. See notes to Figure 4.
Sources: Authors' calculations using BEAR and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

Figure A.13: Impacts of Commuting Zone Recession Shocks on Log Population Age 15+

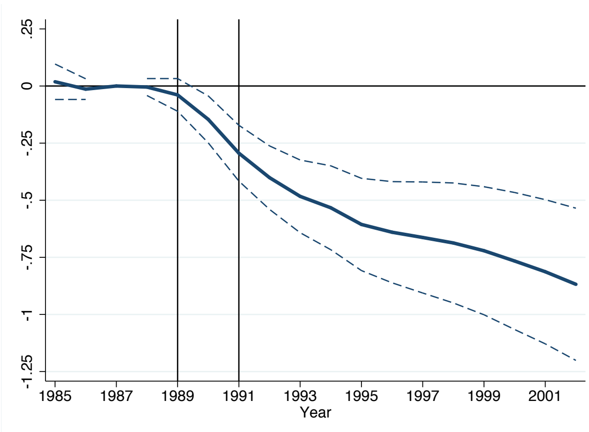
(a) 1973–1975 Recession



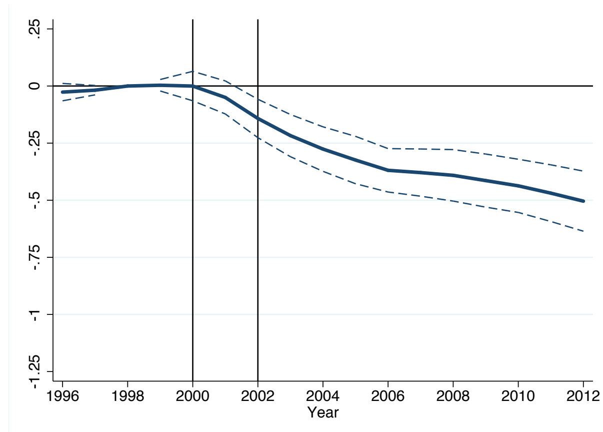
(b) 1980–1982 Recession



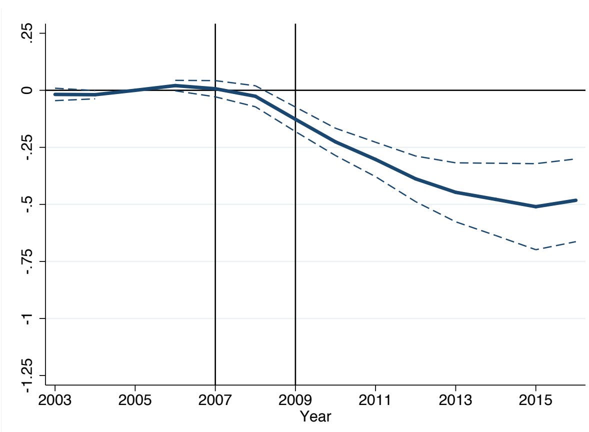
(c) 1990–1991 Recession



(d) 2001 Recession



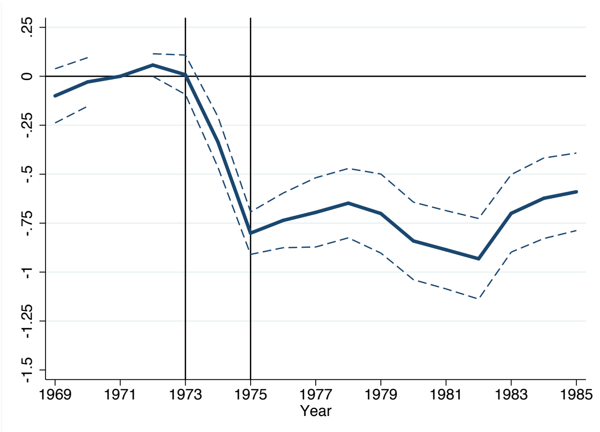
(e) 2007–2009 Recession



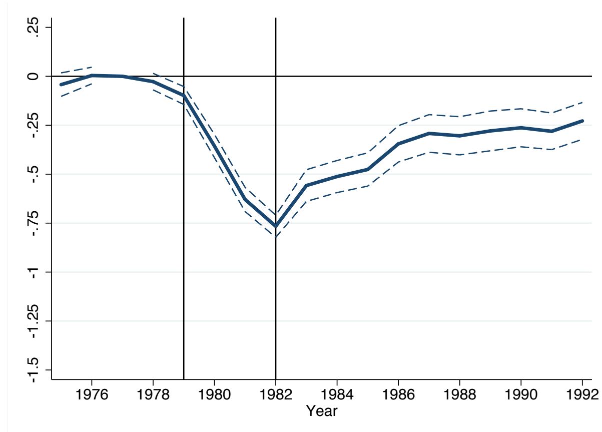
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log population age 15 and above. See notes to Figure A.12.
Sources: Authors' calculations using BEAR, SEER, and QCEW data.

Figure A.14: Impacts of Commuting Zone Recession Shocks on Log Employment-Population Ratio

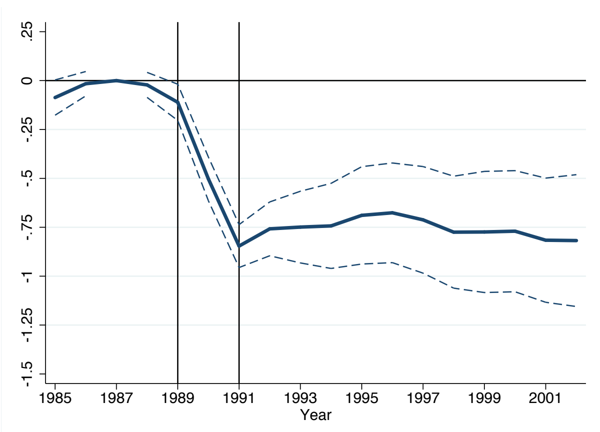
(a) 1973–1975 Recession



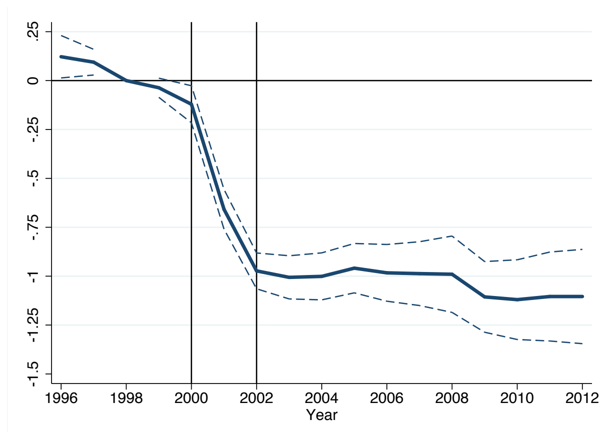
(b) 1980–1982 Recession



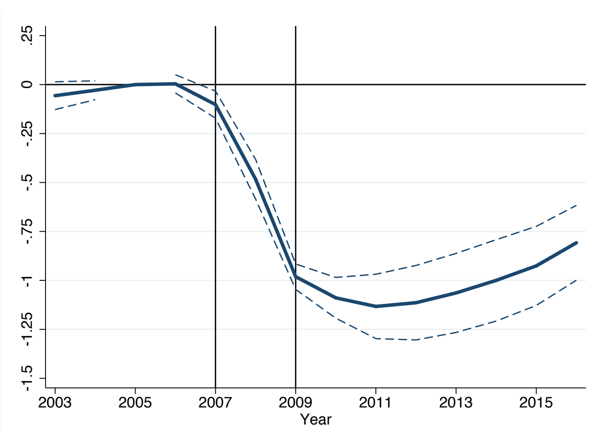
(c) 1990–1991 Recession



(d) 2001 Recession



(e) 2007–2009 Recession

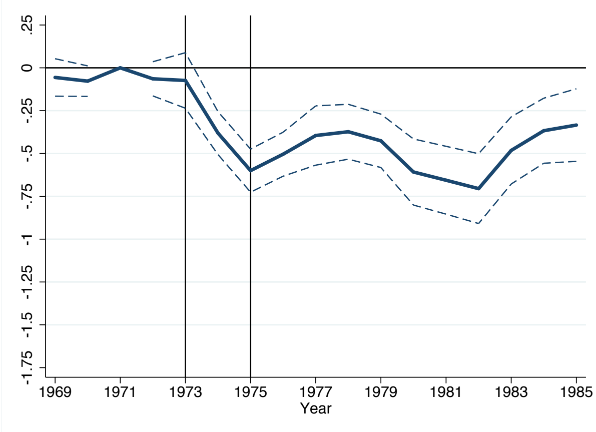


(f)

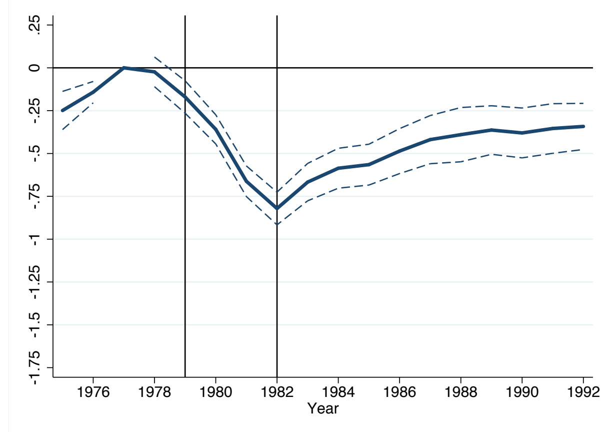
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is the log of the ratio of wage and salary employment to population age 15 and above. See notes to Figure A.12.
Sources: Authors' calculations using BEAR and SEER data.

Figure A.15: Impacts of Commuting Zone Recession Shocks on Log Real Earnings per Capita

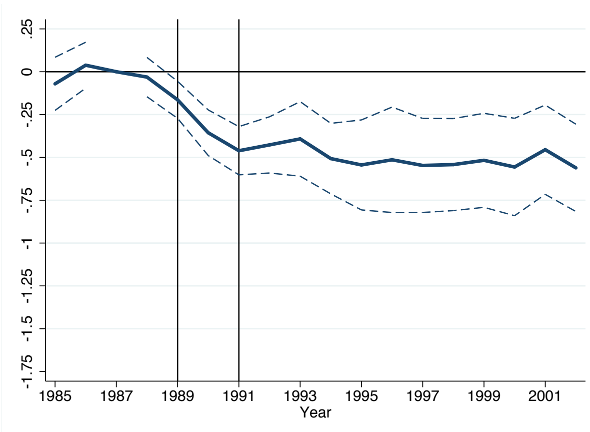
(a) 1973–1975 Recession



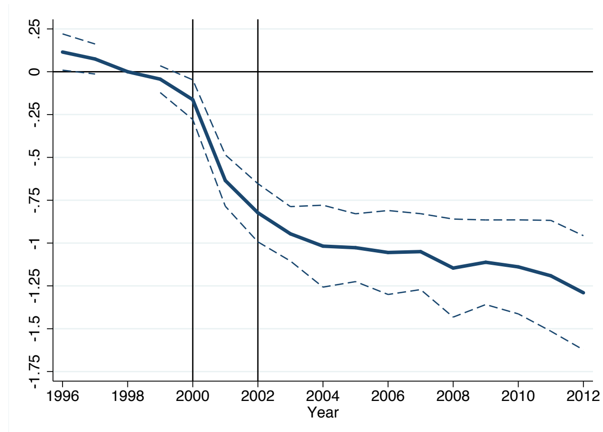
(b) 1980–1982 Recession



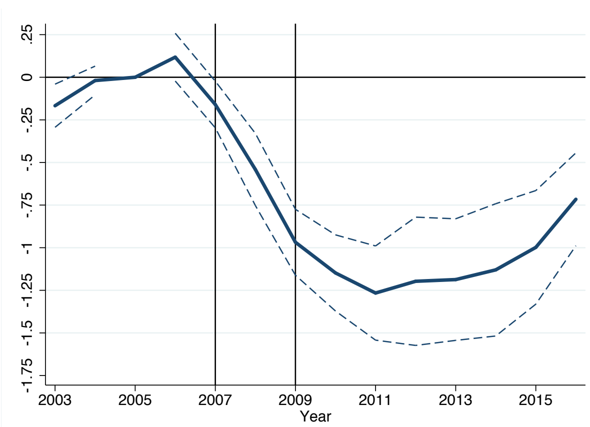
(c) 1990–1991 Recession



(d) 2001 Recession



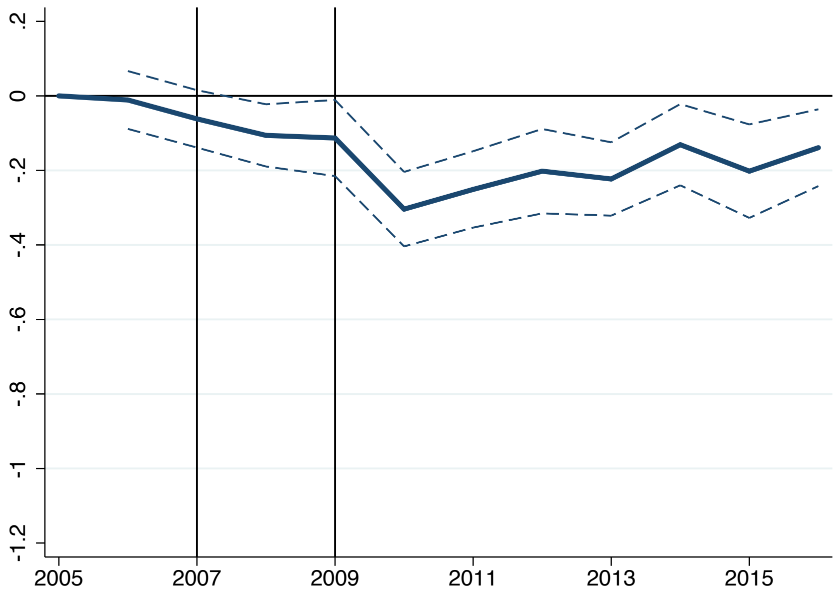
(e) 2007–2009 Recession



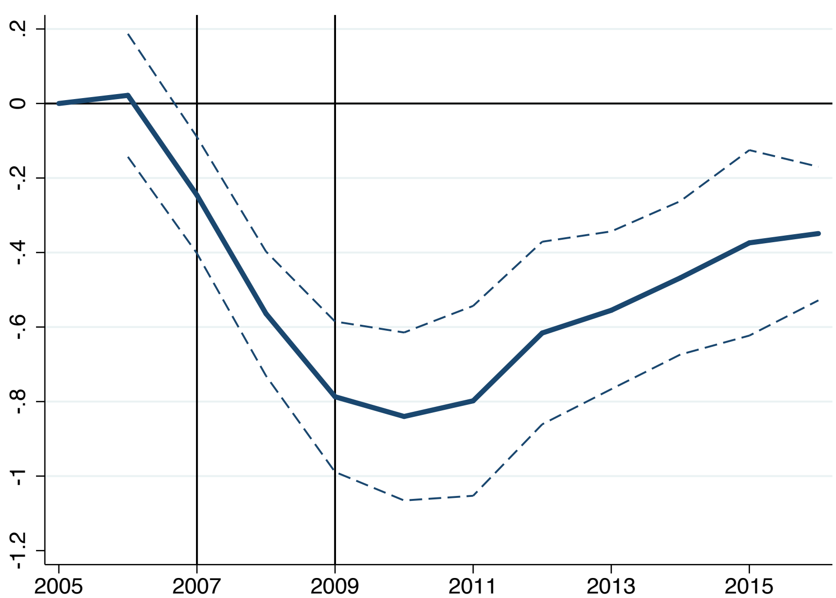
Notes: Figure reports estimates of equation (2), separately for each recession. The dependent variable is log real earnings per capita (age 15+). See notes to Figure A.12.
Sources: Authors' calculations using BEAR and SEER data.

Figure A.16: Impacts of Metropolitan Area Great Recession Shock on Employment, ACS

(a) Log share employed



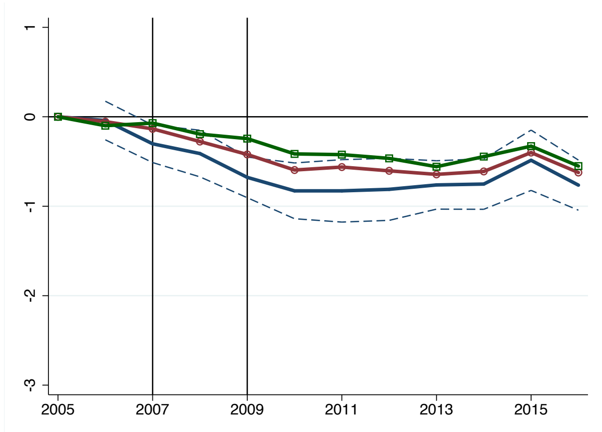
(b) Log share employed full-time full-year



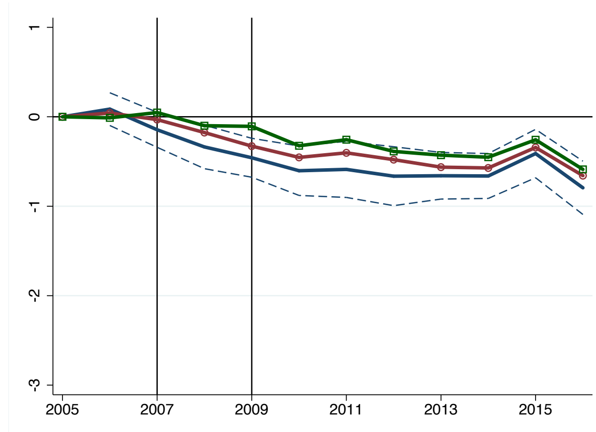
Notes: Figure reports estimates of equation (2) for the Great Recession. The dependent variable is the log of the share of 25–54 year olds who worked a positive number of weeks in the prior 12 months (Panel A) or the log of the share who worked at least 50 weeks and 35 hours per week (Panel B). See notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.

Figure A.17: Impacts of Metropolitan Area Great Recession Shock on Log Wage and Salary Earnings: Annual vs Weekly vs Hourly

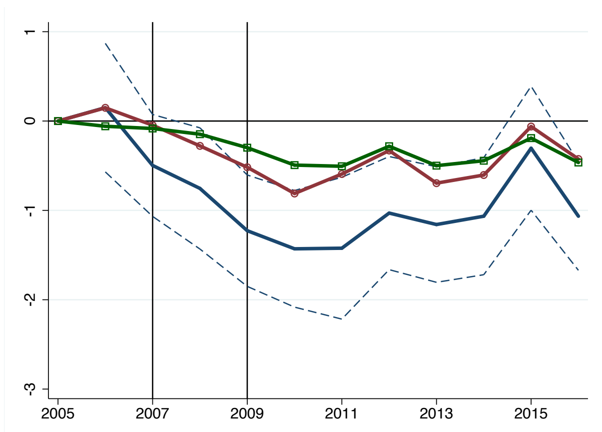
(a) Average



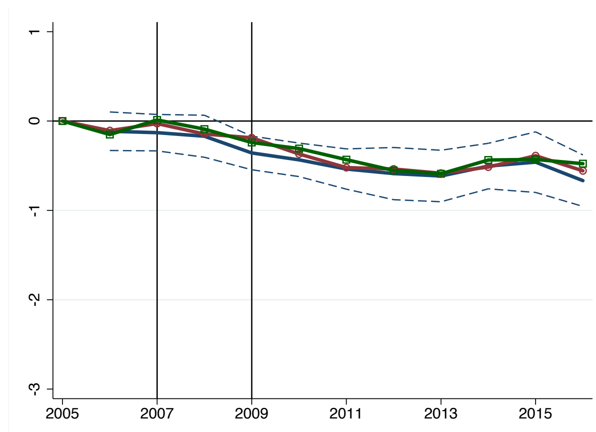
(b) 50th percentile

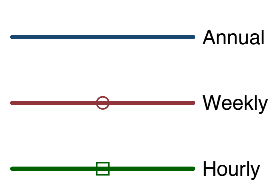


(c) 10th percentile



(d) 90th percentile

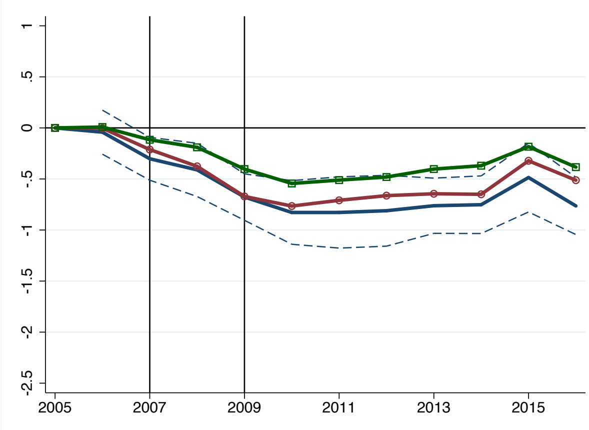




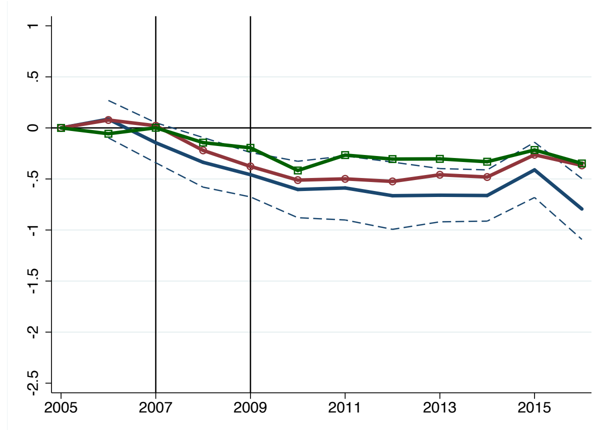
Notes: Figure reports estimates of equation (2) for the Great Recession. Sample contains individuals age 25–54. The dependent variables are log annual, weekly, and hourly earnings. See notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.

Figure A.18: Impacts of Metropolitan Area Great Recession Shock on Log Wage and Salary Earnings, Role of Changes in Composition

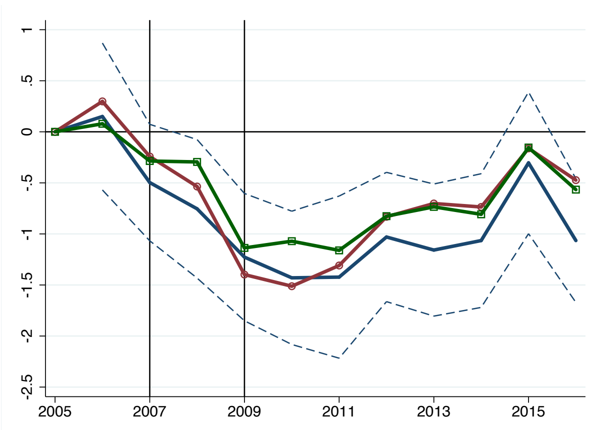
(a) Average log earnings



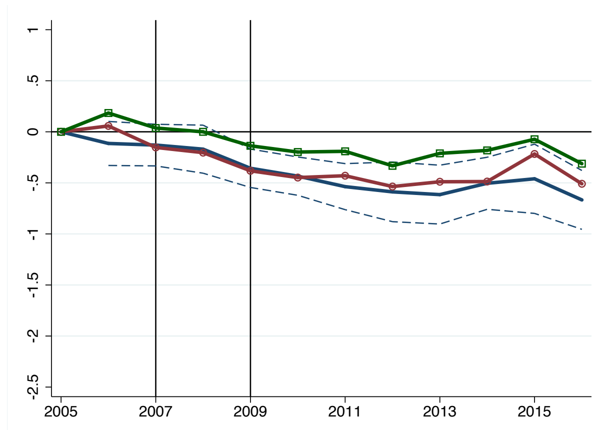
(b) 50th percentile, log earnings

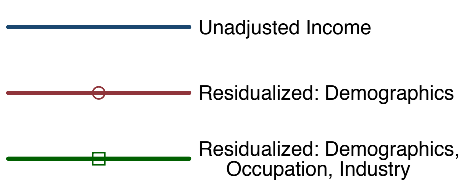


(c) 10th percentile, log earnings



(d) 90th percentile, log earnings

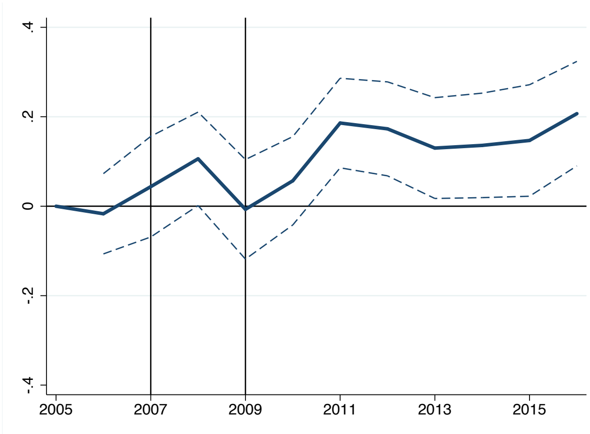




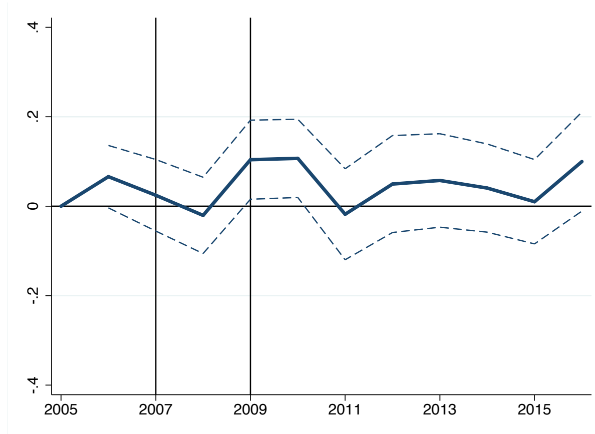
Notes: Figure reports estimates of equation (2) for the Great Recession. Sample contains individuals age 25–54. The red line shows estimates where the dependent variable is constructed using log earnings residualized against indicators for education (11), indicators for age, an indicator for sex, and indicator for race/ethnicity (white/black/Hispanic/other), plus interactions between the education indicators and a quartic in age. The green line also controls for 322 occupation fixed effects and 225 industry fixed effects. See notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.

Figure A.19: Impacts of Metropolitan Area Great Recession Shock on Education Composition

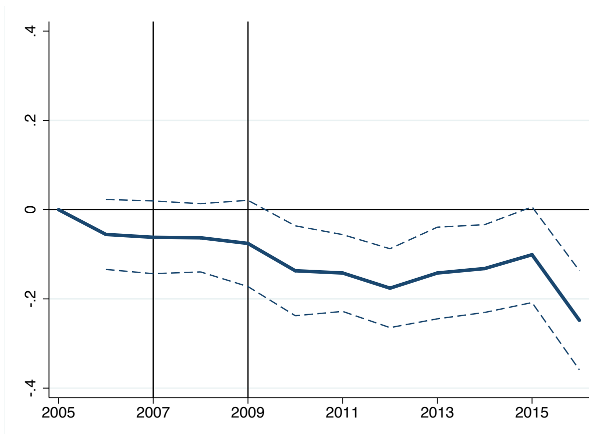
(a) Share with HS Degree or Less



(b) Share with Some College



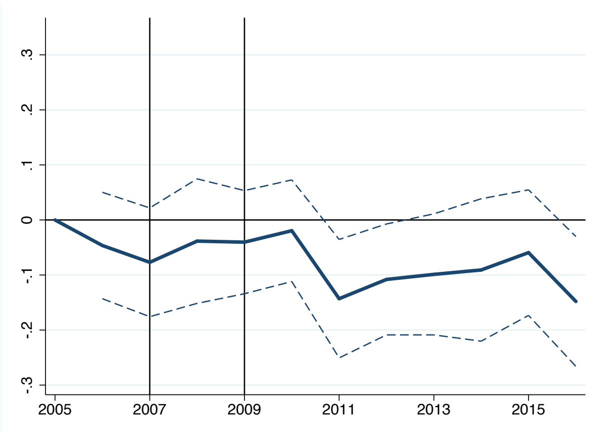
(c) Share with Bachelor's Degree or More



Notes: Figure reports estimates of equation (2) for the Great Recession. Sample contains individuals age 25–54. Regressions control for the lagged share of individuals in all three education groups (to ensure that coefficients add up to one), plus the controls described in the notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.

Figure A.20: Impacts of Metropolitan Area Great Recession Shock on Occupation Structure

(a) Share managerial, professional, technical

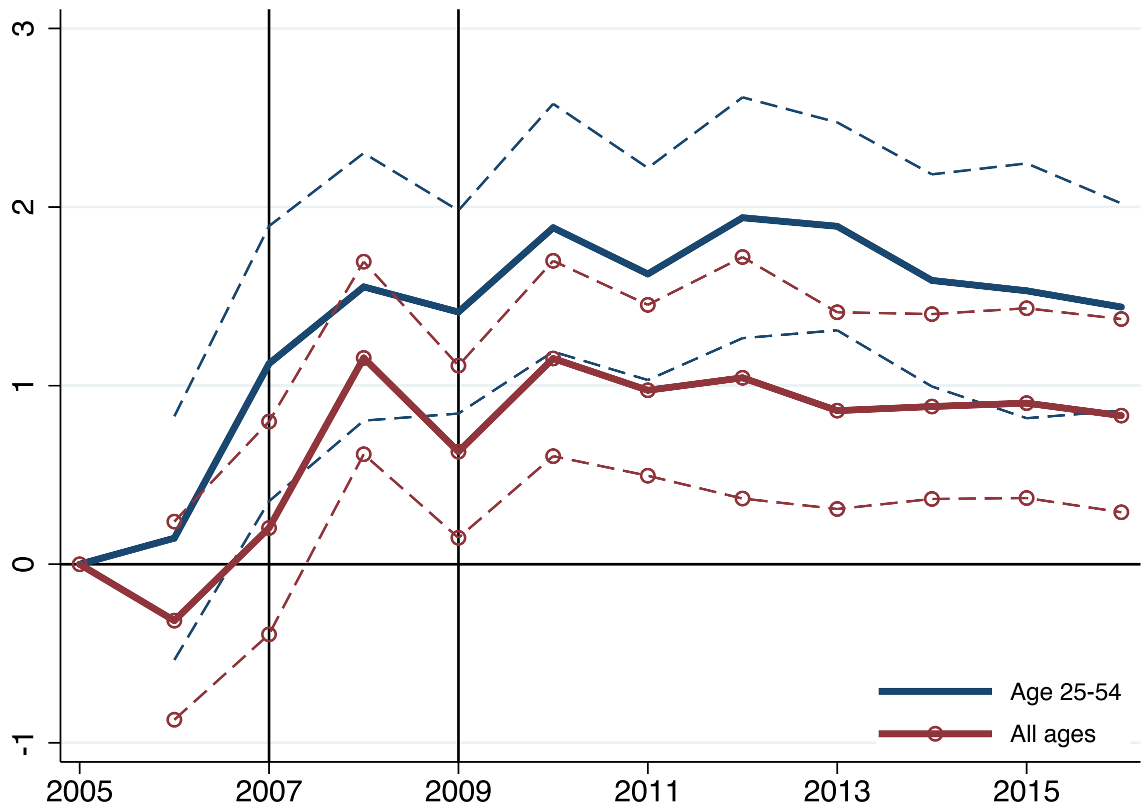


(b) Share administrative, office, production, sales

(c) Share manual and service

Notes: Figure reports estimates of equation (2) for the Great Recession. Sample contains individuals age 25–54 who are currently employed. Regressions control for the lagged share of individuals in all three occupation groups (to ensure that coefficients add up to one), plus the controls described in the notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.

Figure A.21: Impacts of Metropolitan Area Great Recession Shocks on Log Poverty Rate, ACS



Notes: Figure reports estimates of equation (2) for the Great Recession. The dependent variables are the log poverty rates for 25–54 year olds and for all individuals. See notes to Figure 4.
Sources: Authors' calculations using BEAR, IPUMS, and SEER data.