Employment Hysteresis from the Great Recession

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This paper uses US local areas as a laboratory to test for long-term impacts of the Great Recession. In administrative longitudinal data, I estimate that exposure to a 1 percentage point larger 2007–9 local unemployment shock reduced 2015 working-age employment rates by over 0.3 percentage points. Rescaled, this long-term recession impact accounts for over half of the 2007–15 US age-adjusted employment decline. Impacts were larger among older and lower-earning individuals and typically involved a layoff but are present even in a mass-layoffs sample. Disability insurance and out-migration yielded little income replacement. These findings reveal that the Great Recession imposed employment and income losses even after unemployment rates signaled recovery.

I. Introduction

The US unemployment rate spiked from 5.0 percent to 10.0 percent over the course of the Great Recession and then returned to 5.0 percent in 2015. However, the US employment rate (employment-population ratio)

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did not exhibit similar recovery. Figure 1*A* shows that the employment rate declined 3.6 percentage points between 2007 and 2015, as millions of adults exited the labor force.[1] Population aging explains a minority of the employment rate decline: weighting 2015 ages by the 2007 age distribution reduces the decline to 2.0 percentage points, and the unadjusted age 25–54 employment rate declined by 2.6 percentage points. The decline was concentrated among the low skilled (Charles, Hurst, and Notowidigdo 2016). The decline's persistence contrasts with employment rate recovery after earlier recessions—leading history-based analyses such as Fernald et al. (2017) to doubt the possibility of employment "hysteresis": the Great Recession having depressed long-term employment despite the unemployment recovery.[2]

This paper tests whether the Great Recession and its underlying sources caused part of the 2007–15 age-adjusted decline in US employment or whether that decline would have prevailed even in the absence of the Great Recession. It is typically difficult to test for long-term employment impacts of recessions, for the simple reason that recession-independent ("secular") forces may also affect employment over long horizons (Ramey 2012). For example, the US employment rate *rose* by 2 percentage points from the start of the 1981–82 recession to the late 1980s, as women continued to enter the labor force. In the context of the Great Recession, secular nationwide skill-biased shocks such as technical or trade changes could have caused the entire 2007–15 employment decline, rather than the recession.

I attempt to overcome this challenge by leveraging spatial variation in Great Recession severity, along with data that minimize selection threats. All US local areas by definition experienced the same secular nationwide shocks, but some local areas experienced more severe Great Recession shocks than other local areas. For example: Phoenix, Arizona—America's sixth-largest city—experienced a relatively large unemployment spike during the Great Recession, while San Antonio, Texas—America's seventh-largest city—did not. A cross-area research design has the potential to distinguish recession impacts from secular nationwide shock impacts.

In the first part of the paper, I show, using public state-year aggregates, that a cross-area research design is indeed fertile ground for studying the labor market consequences of the Great Recession. Defining state-level

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[1] Variable definitions are standard and pertain to the age-16-and-over civilian noninstitutional population.

[2] This definition of hysterisis encompasses permanent impacts as well as transitory impacts that last longer than elevated unemployment.

shocks as 2007–9 employment growth forecast errors in an autoregressive system (Blanchard and Katz 1992), I find that 2015 employment rates remained low in the US states that experienced relatively severe Great Recession shocks—even though between-state differences in unemployment rates had returned to normal (and despite normal between-state population reallocation). Hence, the cross-area patterns of employment, unemployment, and labor force participation closely mirrored the aggregate cross-time patterns of figure 1*A*. The persistent cross-area employment rate difference departs from the Blanchard-Katz finding of rapid regional convergence.

The state-year evidence does not imply persistent employment impacts of the Great Recession, because of two potential forms of cross-area composition bias: post-2007 sorting on labor supply and pre-2007 sorting on human capital. First, severe Great Recession local shocks caused long-term declines in local costs of living (Beraja, Hurst, and Ospina 2016), which may have disproportionately attracted or retained those secularly out of the workforce, such as the disabled and the retired (Notowidigdo 2011).[3] Even without such post-2007 sorting on labor supply, severe Great Recession local shocks may have happened to hit areas with particularly large preexisting concentrations of individuals affected by secular nationwide shocks. For example, the Great Recession disproportionately struck local areas that had experienced housing booms (Mian and Sufi 2014) attracting low- and middle-skill construction labor, and low- and middle-skill laborers have been relatively adversely affected by secular nationwide shocks in recent decades (e.g., Katz and Murphy 1992).[4] Under either type of cross-area sorting, severe Great Recession local shocks may not have caused local residents' 2015 nonemployment.

I therefore turn for the second part of the paper to longitudinal linked employer-employee data in order to control for prominent dimensions of cross-area sorting. The longitudinal component allows one to measure individuals' employment over time regardless of whether and where in the United States they migrated—directly controlling for post-2007 sorting on labor supply. The linked employer-employee component allows one to control for fine interactions of age, 2006 earnings, and 2006 industry—proxies for pre-2007 human capital.

Specifically, I draw a 2 percent random sample of individuals from deidentified federal income tax records spanning 1999–2015. The main

[3] For example, "Warren Buffett's Advice to a Boomer: Buy Your Sunbelt Retirement Home Now" (*Forbes*, January 27, 2012; http://www.forbes.com/sites/janetnovack/2012/01/27/warren-buffetts-advice-to-a-boomer-buy-your-sunbelt-retirement-home-now/).

[4] For example, "You can't change the carpenter into a nurse easily . . . monetary policy can't retrain people" (Charles Plosser in "The Fed's Easy Money Skeptic," *Wall Street Journal*, February 12, 2011; http://www.wsj.com/articles/SB10001424052748704709304576124132413782592).

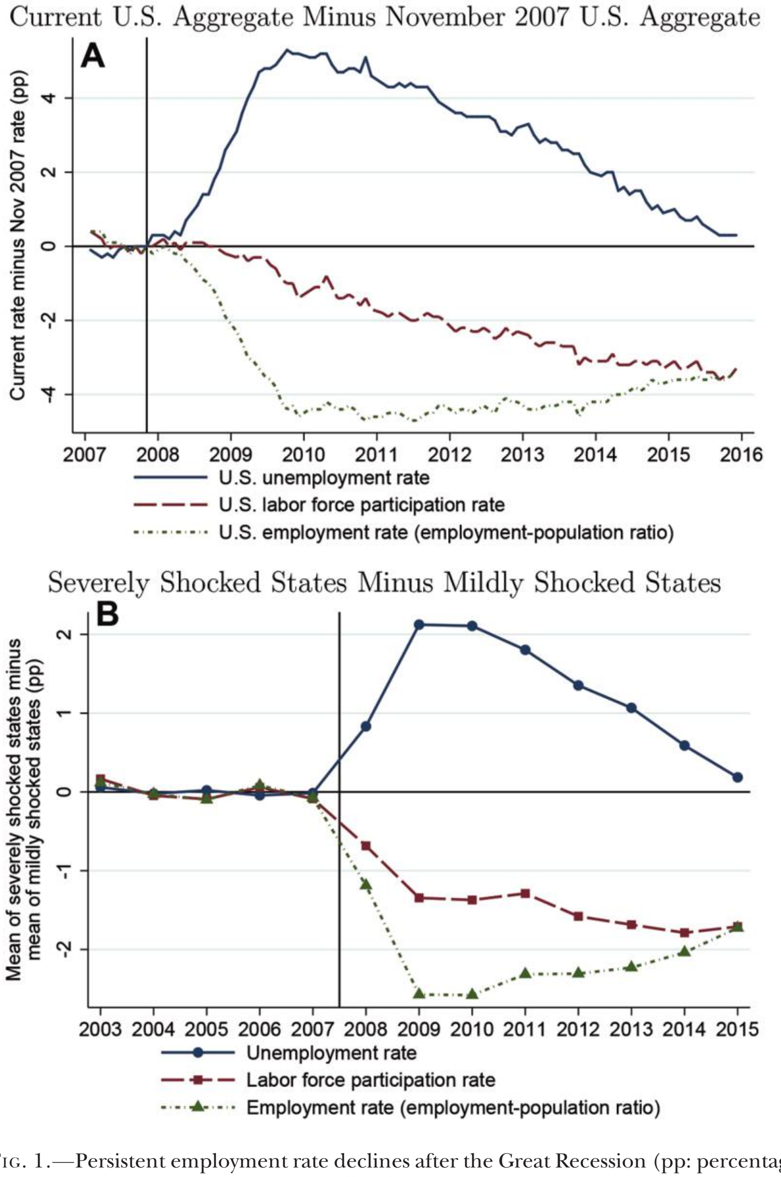

FIG. 1.—Persistent employment rate declines after the Great Recession (pp: percentage points). *A*, Data are from official seasonally adjusted BLS US labor force statistics from January 2007 through December 2015. The data are monthly and refer to the adult (16+) civilian noninstitutional population. The vertical line denotes November 2007, the last month before the Great Recession. For each outcome and month, the graph plots the current value minus the November 2007 value, so that each data point in these series denotes a percentage point change relative to November 2007. See online figure A.1 for age-adjusted versions and versions restricted to 25–54-year-olds. *B*, US states are divided into severely (below-median) and mildly (above-median) shocked states, based on 2007–9 state-level employment growth forecast errors in the autoregressive system of Blanchard and Katz

outcome of interest is employment at any point in 2015, equal to an indicator for whether the individual had any W-2 earnings or any 1099-MISC independent contractor earnings in 2015. The main sample is restricted to those aged 30–49 ("working age") in 2007 in order to confine the 1999–2015 employment analysis to those between typical schooling and retirement ages, and it is restricted to American citizens in order to minimize unobserved employment in foreign countries. The analysis allows for within-state variation by using the local-area concept of the commuting zone (CZ): 722 county groupings that approximate local labor markets and are similar to metropolitan statistical areas (MSAs) but span the entire continental United States. I use the universe of information returns to assign individuals to their January 2007 CZ. Each individual's Great Recession local shock equals the percentage point change in her 2007 CZ's unemployment rate between 2007 and 2009 as recorded in the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS). I obtain 2006 four-digit North American Industry Classification System (NAICS) industry codes for half of 2006 W-2 earners by linking W-2s to employers' tax returns.

I find that, conditional on 2006 age-earnings-industry fixed effects, a 1 percentage point higher Great Recession local shock caused the average working-age American to be 0.39 percentage points less likely to be employed in 2015. The estimate is very statistically significant, approximately linear in shock intensity, robust across numerous specifications, and large: those living in 2007 in largest-shock-quintile CZs were 1.7 percentage points less likely to be employed in 2015 than initially similar individuals living in 2007 in smallest-shock-quintile CZs. Placebo tests indicate no relative downward employment trend in severely shocked areas before the recession, corroborating identification. Controlling for 2015 local unemployment rates suggests that the incremental 2015 nonemployment took the form of labor force exit rather than long-term unemployment. I similarly find impacts on 2015 wage and contractor earnings: −3.6 percent (−$997) of the individual's preperiod earnings for every 1 percentage point higher shock.

One could be concerned that the foregoing within-industry analysis fails to sufficiently control for pre-2007 sorting on human capital, as jobs and therefore skill types in some industries are geographically differentiated. Thus, as a novel robustness check, I approximate a within-job analysis, using a sample of 2006 workers at retail chain firms such as Walmart

(1992) estimated on 1976–2007 BLS LAUS state-year labor force statistics. For each outcome and year, the graph uses LAUS to plot the unweighted mean in severely shocked states, minus the same mean in mildly shocked states. Each series is demeaned relative to its pre-2008 mean. See online appendixes A and B for more details.

and Safeway that employ workers with similar skills to perform similar tasks at similar salaries in many different local areas. Adding firm fixed effects in the retail sample attenuates the retail-sample employment effect by 0.05 percentage points, or one-half of one standard error, to −0.36 percentage points, similar to the main estimate of −0.39 percentage points.

Certain comparisons suggest that the true average employment impact could be closer to −0.30 percentage points per percentage point unemployment rate shock. Naive extrapolation of the −0.30-for-1 and −0.39-for-1 magnitudes would explain 58–76 percent of the US 2007–15 working-age annual employment rate decline as a long-term impact of the Great Recession. The actual implied aggregate impact depends on general equilibrium amplification or dampening.

The recession's impacts were highly uneven within local areas. The employment and earnings impacts were most negative for older individuals and those with low 2006 earnings. The latter pattern indicates that the Great Recession induced a long-term increase in employment and earnings inequality across skill levels, not merely within skill levels across space. Impacts do not appear smaller among mobile subgroups such as renters or the childless.

Adjustment via migration and social insurance appears highly incomplete in replacing lost income. I find no statistically significant impact of Great Recession local shocks on out-migration from one's 2007 CZ, and earnings in other CZs did not replace any lost 2015 earnings relative to those exposed to smaller Great Recession local shocks. Great Recession local shocks caused significantly higher unemployment insurance (UI) benefits 2008–10 before UI benefits expired and insignificantly higher Social Security Disability Insurance (SSDI) benefits in all years. I estimate that 2015 SSDI replaced 2 percent of lost 2015 earnings, or up to 6 percent at the 95 percent confidence upper bound.

Finally, I find that most of the 2015 incrementally nonemployed in severely shocked areas had been laid off at some point during 2007–14 and were nonemployed in the entire 2013–15 period. However, higher layoff rates do not appear to explain the results, as the impacts hold within a sample of laid-off individuals. In particular, I find equally large impacts when comparing workers who were displaced in a 2008–9 mass layoff. Hence, interactions with area-level economic conditions appear key to any full explanation of the long-term impacts—such as human capital decay during extended nonemployment or persistently low local labor demand.

The paper's findings constitute evidence of long-term employment impacts of the Great Recession (cf. Fernald et al. 2017) and add to a large literature on the incidence of labor market shocks. Earlier work had found long-term impacts on individuals' earnings (Topel 1990; Jacobson, LaLonde, and Sullivan 1993; Neal 1995; Kahn 2010; Davis and von Wachter 2011) and sometimes on local areas' employment rates (Blanchard and

Katz [1992] vs. Black, Daniel, and Sanders [2002] and Autor and Duggan [2003]) and individuals' employment rates (Ruhm [1991] and Walker [2013] vs. Autor et al. [2014] and Jarosch [2015]). I provide evidence of long-term impacts of Great Recession local shocks on individuals' employment rates, likely via labor force exit. This evidence reinforces the view of Autor and Duggan and a large literature dating back at least to Bowen and Finegan (1969) and Phelps (1972) that transitory adverse aggregate shocks can have persistent negative employment impacts even after unemployment recovers.

The rest of the paper is organized as follows. Section II uses state-year data to show that cross-area employment patterns mirrored aggregate cross-time employment patterns 2007–15. Section III details the empirical design and longitudinal linked employer-employee data. Section IV estimates overall impacts. Section V estimates impact heterogeneity. Section VI investigates adjustment margins. Section VII documents layoff and nonemployment trajectories. Section VIII discusses candidate mechanisms. Section IX concludes the paper.

II. Local Labor Markets Mirrored the Aggregate

This paper uses cross-area variation in Great Recession severity to test whether the recession and its underlying sources caused part of the 2007–15 decline in the US employment rate displayed in figure 1*A*. I begin by testing whether figure 1*A*'s aggregate cross-time employment patterns have been mirrored across US local areas: did the local areas that experienced severe Great Recession shocks also experience persistent declines in employment and participation rates but not unemployment rates, relative to mildly shocked areas? If so, then local labor markets may indeed serve as a fruitful laboratory for understanding sources of aggregate employment patterns.

A large literature has studied local labor market dynamics after local employment shocks. The canonical analysis of Blanchard and Katz (1992) found, in state-year data for 1976–90, that when a state experiences an adverse employment shock, its population falls relative to trend but its unemployment, participation, and employment rates return to parity with those of other states in 5–6 years. That is, local shocks leave local areas smaller but no less employed. This conclusion has been replicated in European data (Decressin and Fatas 1995) and in a longer US state-year time series (Dao, Furceri, and Loungani 2017). However, other papers have found long-term participation and employment impacts of local shocks: Black et al. (2002), Autor and Duggan (2003), and Autor, Dorn, and Hanson (2013) found long-term impacts of specific types of US local shocks on local SSDI enrollment, participation, and/or employment rates. Hence, the existing literature presents a mixed picture.

This section documents local labor market dynamics after Great Recession local employment shocks, with greater detail presented in online appendixes B and C. For comparability to the broadest line of previous work, I conduct this analysis at the state level and categorize states into severely shocked states and mildly shocked states, using unforecasted state-level changes in 2007–9 employment, derived from the autoregressive system of Blanchard and Katz (1992). I estimate Blanchard and Katz's log-linear autoregressive system in state employment growth, state unemployment rates, and state participation rates in LAUS data for 1976–2007. The LAUS data are the annual BLS LAUS series of employment, population, unemployment, and labor force participation counts in 1976–2015 for 51 states (the 50 states plus the District of Columbia). Annual counts are calendar year averages across months. I then compute 2008 and 2009 employment growth forecast errors for each state—equal to each state's actual log employment growth minus the system's prediction for that state—and sum the two to obtain each state's 2007–9 employment shock. Roughly speaking, each state's shock equals the state's 2007–9 log employment change minus the state's long-run trend. Then, for expositional simplicity, I group the 26 states with the most-negative shocks (e.g., Arizona) into the severely shocked category and the remaining states (e.g., Texas) into the mildly shocked category.

Figure 1*B* displays the 2003–15 time series of unemployment, participation, and employment rate differences between severely shocked states and mildly shocked states. For each outcome and year, the graph plots the unweighted mean among severely shocked states minus the unweighted mean among mildly shocked states (the graph looks nearly identical weighting by population). Within each series, I subtract the mean pre-2008 severe-minus-mild difference from each data point, so each plotted series has a mean of zero before 2008.

The figure shows that the unemployment rate in severely shocked states relative to that in mildly shocked states spiked in 2008, peaked in 2009–10, and returned by 2015 to its mean prerecession severe-mild difference. Yet the 2015 employment and participation rates in severely shocked states remained 1.74 percentage points below the corresponding rates in mildly shocked states, relative to the mean prerecession severe-mild differences. The implied 2015 cross-area employment gap is large: 2.01 million fewer adults were employed in severely shocked states than in mildly shocked states, relative to full recovery to the prerecession severe-mild employment rate difference.

Hence, the cross-area (severe-minus-mild) patterns of unemployment, participation, and employment of figure 1*B* do indeed broadly mirror the aggregate cross-time (current-minus-2007) patterns of figure 1*A*. Moreover, in the same sense that the aggregate employment aftermath of the Great Recession appears to contrast with the aftermath of the early-1980s and early-1990s recessions, so too does the cross-area aftermath of the Great Recession appear to contrast with the cross-area aftermath of those earlier recessions. Figure 2*A* repeats the employment rate series of figure 1*B* for the aftermath of the 1980s and 1990s recessions, treating the early-1980s recessions as a single recession. The figure shows that cross-state employment rates fully converged 4 years after the early-1990s recession and had converged by 1.65 percentage points (78 percent of the $t = 0$ divergence) 6 years after the early-1980s recession. Six-year convergence after the Great Recession was smaller in both absolute terms (0.85 percentage points) and relative to the $t = 0$ divergence (33 percent).

Blanchard and Katz (1992) suggest that the historical convergence mechanism is rapid population reallocation: a -1 percent state population change relative to trend follows every -1 percent employment shock within 5–6 years.[5] Therefore, a natural possible explanation for local employment rate persistence after the Great Recession is that population reallocation has slowed. Figure 2*B* investigates this possibility by plotting detrended 2007–14 population changes—equal to each state's 2007–14 percent change in population minus the 2000–7 percent change in the state's population—versus the state's 2007–9 employment shock. The graph shows that population reallocation after the Great Recession was similar to the historical benchmark: each -1 percent 2007–9 employment shock was on average accompanied by a -1.016 percent (robust standard error: 0.260) detrended population change.[6] Figure 2*C* shows the same conclusion when the original Blanchard-Katz time range—1976–90, well before the recent housing boom—is used to estimate state-specific population trends in the Blanchard-Katz system. Population in severely shocked states fell in 2007–15 relative to trend and relative to that of mildly shocked states, as much as in the Blanchard-Katz benchmark.

To sum up, this section has found that aggregate 2007–15 cross-time unemployment, participation, and employment rate patterns have been mirrored in cross-area unemployment, participation, and employment rate patterns over the same time period. Participation and employment rates remained persistently low in the US states that experienced relatively

[5] The unit elastic population response holds when reestimating the Blanchard and Katz system on updated data for 1976–2015. The suggested causal chain is as follows: a state (e.g., Michigan) experiences a one-time random-walk contraction in global consumer demand for its locally produced traded good (e.g., cars), which induces a local labor demand contraction and wage decline, which in turn induces a local labor supply (population) contraction, which then restores the original local wage and employment rate.

[6] When not detrended, state population changes were largely uncorrelated with 2007–9 employment shocks, also shown in Mian and Sufi (2014) for the 2007–9 period only. Blanchard and Katz (1992) find adjustment via population changes relative to trend. Gross (out-)migration rates have declined modestly since 1980 (Molloy, Smith, and Wozniak 2011), but gross flows are still an order of magnitude larger than the net flows (population reallocations) predicted by history in response to 2007–9 shocks.

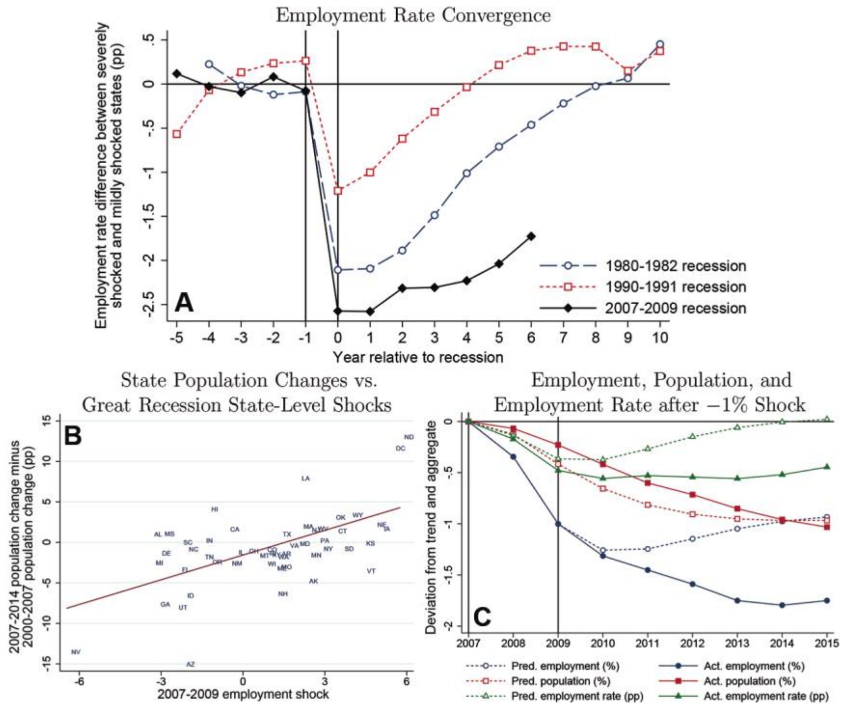

FIG. 2.—Great Recession local adjustment in comparison to history. *A*, States are divided into severely (below-median) and mildly (above-median) shocked states based on the sum of 2008 and 2009 employment growth forecast errors, as described in figure 1*B*, repeating the process for the early-1980s recessions (1980–82, treated as a single recession) and the early-1990s (1990–91) recession. Then, for each recession and year relative to the recession, the graph plots the unweighted mean LAUS employment rate in severely shocked states, minus the same mean in mildly shocked states. Each series is demeaned relative to its prerecession mean. For comparability across recessions, year 0 denotes the last recession year (1982, 1991, or 2009), while year −1 denotes the last prerecession year (1979, 1989, or 2007); intervening years are not plotted. Online figure A.5 plots analogous graphs for labor force participation, unemployment, employment growth, and population growth. The post-2001-recession experience exhibited incomplete convergence before being interrupted by positively correlated 2007–9 shocks (not shown). *B*, This graph uses LAUS data to plot detrended 2007–14 population changes—equal to each state's 2007–14 percent change in population minus its 2000–7 percent change in population—versus the state's 2007–9 employment shock. Overlaid is the unweighted best-fit line, with a slope of 1.016 (robust standard error of 0.260). *C*, The dotted lines plot Blanchard-Katz (1992) history-based predictions (Pred.) for state-level responses to a −1 percent 2007–9 state-level employment shock, based on feeding the 1976–90-estimated Blanchard-Katz system a −.41 percent employment shock followed by a −.59 percent employment shock. The solid lines plot mean actual (Act.) state-level responses based on reduced-form regressions of 2008–14 state-level outcomes on 2007–9 state-level shocks. See online appendix B for more details.

severe employment shocks 2007–9, even though unemployment rates converged across space. Reduced population reallocation does not explain the local persistence. The cross-area aftermath of the Great Recession departs from the broad historical findings of Blanchard and Katz (1992), but they accord with findings of Black et al. (2002), Autor and Duggan (2003), and Autor et al. (2013) from specific contexts. I now turn to identifying whether Great Recession local shocks caused individuals to have lower 2015 employment.

III. Isolating Impacts of Great Recession Local Shocks

The previous section showed that local labor markets were microcosms of aggregate employment patterns in 2007–15: 2015 employment rates remained unusually low in US local areas that experienced an especially severe 2007–9 employment shock. However, that cross-sectional fact does not imply that individuals were nonemployed in 2015 because of where they were living during the Great Recession, in light of two selection threats. First, the disabled, retirees, and others secularly out of the labor force may have disproportionately stayed in or moved to severely shocked areas in order to enjoy low living costs while forgoing employment. Even without selective migration on post-2007 labor supply, severely shocked areas may have been disproportionately populated before the recession by individuals who subsequently suffered large nationwide contractions for their skill types, such as construction workers or routine laborers, that would have occurred even in the absence of the recession. Under either selection threat, the 2015 residents of severely shocked areas might be nonemployed now regardless of geography.

This section specifies my empirical strategy for using longitudinal linked employer-employee data to isolate causal effects of Great Recession local shocks on individuals' 2015 employment; 2015 is the most recent year of data available. Additional details are listed in online appendixes D and E, including design foundations in potential outcomes.

A. *Empirical Design*

I adopt an empirical design that closely follows earlier work using longitudinal individual-level data to estimate long-term impacts of labor market shocks (e.g., Jacobson et al. 1993; Davis and von Wachter 2011; Autor et al. 2014). I estimate regressions of the form

$$y_{i2015} = \beta \text{SHOCK}_{c(i2007)} + \theta_{g(i2006)} + \epsilon_{i2015}, \qquad (1)$$

where y denotes an employment or related outcome, i denotes an individual, $\text{SHOCK}_{c(i2007)}$ is the Great Recession shock to the individual's 2007 local area c, $\theta_{g(i2006)}$ denotes fixed effects for groups g of individuals defined using individual pre-2007-determined characteristics, and ϵ_{i2015} is a disturbance term. Here, β is the coefficient of interest: the causal effect on one's 2015 outcomes of living in 2007 in a local area that experienced a one-unit-larger Great Recession shock.

I interpret β as the causal effect of Great Recession local shocks *and* their underlying sources, which are empirically indistinguishable,[7] and I refer to this effect as the causal effect of Great Recession local shocks. Alternative interpretations include β reflecting differential prerecession trends (e.g., a downward pre-2007 employment trend in severely shocked areas) or independent correlated local shocks (e.g., post-2009 floods in severely shocked areas). However, my interpretation of β is sensible because severely shocked and mildly shocked areas exhibited relatively similar prerecession trends in the outcomes of interest (shown below in Sec. IV.A) and because post-2010 unemployment rates converged monotonically across severely shocked and mildly shocked areas. Moreover, adverse 2010–15 industry-based shift-share shocks are not positively correlated with adverse Great Recession local shocks.

As in earlier work, the identifying assumption is selection on observables: individuals were as good as randomly assigned across local areas within groups g. Also as in earlier work, I aim to satisfy this assumption, using rich longitudinal data to define groups finely along dimensions (e.g., age, prerecession earnings, and prerecession industry) that could be correlated with both Great Recession local shocks and omitted secular nationwide shocks, and to restrict attention to subsamples in which the identifying assumption is particularly likely to hold. I use event study graphs to evaluate potentially confounding prerecession trends.

B. *Samples*

I implement the paper's empirical design by using selected deidentified data from federal income tax records spanning 1999–2015. I construct three samples as follows. All three samples are balanced panels of individuals.

Main sample.—The main sample comprises a 2 percent random sample from what I call the *full sample.* The full sample comprises all American citizens aged 30–49 ("working age") on January 1, 2007, who had not died by December 31, 2015, and who had a valid payee ZIP code on at least one information return that indicates continental US residence in January

[7] For example, if the underlying source of Great Recession local shocks was persistent local spending contractions (Mian, Rao, and Sufi 2013), then 2015 employment could in principle be depressed because of layoffs during the Great Recession or because local spending remained depressed through 2015, among other possibilities.

2007.[8] The age restriction confines the 1999–2015 employment analysis to those older than schooling age and younger than retirement age. Birth, death, and citizenship data are drawn from Social Security Administration (SSA) records housed alongside tax records.[9] Restricting attention to those alive in 2015 excludes analysis of mortality effects, likely a conservative choice (Sullivan and von Wachter 2009). I describe geocoded information returns in the next subsection. I randomly sample individuals from the full sample by using the last two digits of the individual's masked identification number, yielding a main sample comprising 1,357,974 individuals.

Retail chain sample.—The retail chain sample comprises individuals in the full sample whose main employer in 2006 was a retail chain firm and who lived outside the local area of the retail chain firm's headquarters. It is constructed as follows. For every individual in the full sample with a 2006 W-2 form, I attempt to link the masked employer identification number (EIN) on the individual's highest-paying 2006 W-2 to at least one business return in the universe of business income tax returns 1999–2007.[10] I use the NAICS code on the business income tax return to restrict attention to workers whose 2006 firms operated in the two-digit NAICS retail trade industries (44 or 45), for example, Walmart and Safeway.[11] I further exclude employees living in 2007 in the CZ of their employer's headquarters, using the workers' payee ZIP codes across their information returns (see the next subsection) and the filing ZIP code on business income tax returns and mapping these ZIP codes to CZs (the local-area concept defined in the next subsection). Then, to identify CZs in which the 2006 firms operated, I further restrict the sample to firms with at least 10 2006 employees living in each of at least five CZs and then to the firms' employees living in 2007 in those CZs. This procedure yields a retail chain sample of 865,954 individuals at 524 retail firms.[12]

[8] In other contexts, "working age" sometimes refers to the age-15–65 population. My sample lies within ages 15 and 65 in all years 1999–2015. I refer to the sample as working age mainly to communicate that it omits individuals beyond normal retirement age.

[9] Citizenship is recorded as of December 2016. Results are very similar when not conditioned on citizenship status. Conditioning on citizenship reduces the possibility that 2007 residents are employed in other countries but appear nonemployed in US tax data.

[10] Many firms' workers cannot be linked to a business income tax return; see the next subsection.

[11] Accessed data lacked firm names. I do not know which specific firms survived the sample restrictions. These example firms and their industry codes were found on Yahoo Finance.

[12] As in other US administrative data (e.g., the Census Bureau's Longitudinal Employer Household Dynamics; see Walker 2013), specific establishments of multiestablishment firms are not directly identified in federal tax data. My process infers firms' CZ-level operations from workers' residential locations. The retail chain sample is smaller than the universe of retail chain workers for four main reasons: the age restriction, the de facto exclusion of workers at independently owned franchises, mismatches between W-2 EIN and business return EIN (see Sec. III.C.3 below), and removal of workers at firm headquarters.

Mass-layoffs sample.—The mass-layoffs sample comprises all individuals in the full sample who separated from an employer during a mass-layoff event in either 2008 or 2009, after having worked for the employer during the prior three calendar years inclusive of the separation year. It is constructed as follows, closely adhering to the sampling frame of Davis and von Wachter (2011), except that I define an employer as an EIN-CZ pair rather than an EIN; an EIN may represent a firm or a division of a firm. Using the universe of W-2s and linking W-2 payee (residential) ZIP codes to CZs, I compute annual employment counts at the EIN-CZ level. For an employer to qualify as having a mass-layoff event in year $t \in \{2008, 2009\}$, the employer must satisfy the following conditions: it had at least 50 employees in $t - 1$; employment contracted by between 30 and 99 percent from $t - 1$ to $t + 1$; employment in $t - 1$ was no greater than 130 percent of $t - 2$ employment; and $t + 2$ employment was less than 90 percent of $t - 1$ employment.[13] The mass-layoffs sample comprises all 1,001,543 individuals in the full sample who received a W-2 with positive earnings from a mass-layoff employer in years $t - 2$ through year t but not in $t + 1$.

C. *Variable Definitions*

I now define variables. Year always refers to calendar year. Variables are available for 1999–2015.

1. Outcomes

Similar to usage by Davis and von Wachter (2011) and Autor et al. (2014), *employment* in a given year is an indicator for whether an individual has positive Form W-2 earnings or Form 1099-MISC independent contractor earnings (both filed mandatorily by the employer) in the year. Employment is thus a measure of having been employed at any time during the year. Note that this annual employment measure differs from the conventional point-in-time (survey reference week) measure used by the BLS. Although not all self-employment is reported on 1099-MISCs, transition of affected workers to self-employment likely does not explain the results: Current Population Survey data indicate that changes in state self-employment rates since 2007 were unrelated to changes in state formal employment rates.

Earnings in a given year represents labor income and equals the sum of an individual's Form W-2 earnings and Form 1099-MISC independent

[13] The 99 percent threshold protects against EIN changes yielding erroneous mass-layoff events. The last two criteria exclude temporary employment fluctuations. A firm that initially qualifies as having mass-layoff events in both 2008 and 2009 is assigned a 2008 event only.

contractor earnings. All dollar values are measured in 2015 dollars, adjusted for inflation using the headline consumer price index (CPI-U), and are top-coded at $500,000 after inflation. *DI receipt* is an indicator for whether the individual has positive SSDI income in the year as recorded on Form 1099-SSA information returns filed mandatorily by the SSA. SSDI is the main US disability insurance program. *UI receipt* is an indicator for whether the individual has positive UI benefit income in the year as recorded on Form 1099-G information returns filed mandatorily by state governments.

2. CZ and Great Recession Local Shock

Allowing for within-state variation, an individual's *CZ* is defined as her residential commuting zone, a local-area concept used in much recent work (Dorn 2009; Autor et al. 2014; Chetty et al. 2014). CZs are collections of adjacent counties, grouped by Tolbert and Sizer (1996) using commuting patterns in the 1990 census to approximate local labor markets. I calculate, based on the 2006–10 American Community Surveys, that 92.5 percent of US workers live in the CZ in which they work. Urban CZs are similar to MSAs, but whereas MSAs exclude rural areas, every spot in the continental United States lies in exactly one of 722 CZs.

The *2007 CZ* is the CZ corresponding to the payee (residential) ZIP code that appears most frequently for the individual in 2006 among the approximately 30 types of information returns (filed mandatorily by institutions on behalf of an individual, including W-2s).[14] Information returns are typically issued in January of the following year, so the ZIP code on an individual's 2006 information return typically refers to the individual's location as of January 2007. The *2015 CZ* is defined analogously to the 2007 CZ, except that if an individual lacks an information return in 2014, I impute CZ using information return ZIP code from the most recently preceding year in which the individual received an information return. The *2007 state* denotes the state with most or all of the 2007 CZ's population.

Each individual's *Great Recession local shock* equals the percentage point change in the individual's 2007 CZ's unemployment rate from 2007 to 2009. Annual CZ unemployment rates are computed by aggregating monthly population-weighted county-level unemployment rates from the monthly BLS LAUS series to the CZ-month level, then averaging evenly within CZ-years across months. Measuring local shocks in units of the unemployment

[14] Numerous activities trigger information returns, including formal and independent contractor employment, SSA or UI benefit receipt, mortgage interest payment, business or other capital income, retirement account distribution, education and health savings account distribution, debt forgiveness, lottery winning, and college attendance. A comparison to external data suggests that 98.2 percent of the US population appeared on some income tax or information return submitted to the IRS in 2003 (Mortenson et al. 2009).

rate change permits Section IV.D's comparison to the aggregate shock, which can be measured in the same units. Great Recession local shocks are available on the author's website, along with data that can be used to approximate the paper's main result using publicly available data, as demonstrated in online appendix table 1.

3. Covariates

Age is defined as of January 1 of the year, using date of birth from SSA records housed alongside tax records. *Female* is an indicator for being recorded as female in SSA records. Following Autor et al. (2014), an individual had *high labor force attachment* if she earned at least $10,382 in 2015 dollars—the compensation for 1,600 hours of work at the 2004 federal minimum wage in 2015 dollars—in each of the four years 2003–6. An individual had *no labor force attachment* if she had zero earnings in any year 2003–6. All other individuals had *low labor force attachment*. The term *1040 filer* is an indicator for whether the individual appeared as either a primary or secondary filer on a Form 1040 tax return in tax year 2006. *Married* is an indicator for whether the individual was either the primary or secondary filer on a married-filing-jointly or married-filing-separately 1040 return in tax year 2006. *Number of kids* equals the number of children (zero, one, or two or more) living with the individual as recorded on the individual's 2006 Form 1040 if the individual was a 1040 filer and zero otherwise. *Mortgage holder* is an indicator for whether a Form 1098 information return was issued on the individual's behalf by a mortgage servicer in 2006.[15] *Birth state* is derived from SSA records and, for immigrants, equals the state of naturalization.

The *2006 industry* equals the four-digit NAICS industry code on the business income tax return of an individual's highest-paying 2006 Form W-2, whenever a match can be made between the masked EIN on the W-2 and the masked EIN on the business income tax return. Four-digit NAICS codes are quite narrow, distinguishing, for example, between restaurants and bars. As displayed below in summary statistics and similar to recent work (Mogstad, Lamadon, and Setzler 2017; Kline et al. 2018), almost half of all W-2 earners could not be matched—likely because the employer is a government entity (which does not file an income tax return, covering 15–20 percent of employment) or because the firm uses a different EIN (e.g., a non-tax-filing subsidiary) to pay workers from the one that appears on the firm's tax return. For the construction of fixed effects, I assign individuals with a missing industry code to their own exclusive industry;

[15] A mortgage servicer is required to file a Form 1098 on behalf of any individual from whom the servicer receives at least $600 in mortgage interest on any one mortgage during the calendar year.

I assign non-W-2-earning contractors to their own exclusive industry; and I assign the nonemployed to their own exclusive industry. I show below that results are nearly unchanged when the sample is restricted to the nonemployed and those with a valid W-2 industry, for whom the correct industry is universally observed.

The *2006 age-earnings-industry fixed effects* are interactions between age (measured in one-year increments), 2006 industry, and 16 bins of the individual's 2006 earnings (in 2015 dollars inflated by the CPI-U) from the individual's highest-paying employer.[16] The *2006 firm* equals the masked EIN on the individual's highest-paying 2006 W-2. The *2006 age-earnings-firm fixed effects* are constructed analogously to the 2006 age-earnings-industry fixed effects. Other controls are used only for robustness checks and are defined when used.

D. Summary Statistics

Table 1 reports summary statistics across the three samples. Of the main sample, 79.1 percent were employed in 2015, with mean 2015 earnings (including zeros and top-coded at $500,000) of $47,587; 6.2 percent received SSDI income in 2015, and 25.6 percent received UI benefit income in at least one of the years 2007–14. The sample is 49.3 percent female; 62.4 percent had high labor force attachment in 2003–6. The average 2006 age is 39.9 years. The retail chain sample is on average more female, less attached to the labor force, and less likely to be married. The mass-layoffs sample is on average less female, more attached to the labor force, less likely to be married, and more likely to have worked in construction or manufacturing in 2006 than the main sample. Industry in the main sample is observed for 51.1 percent of W-2 earners. The average Great Recession local shock was a 2007–9 increase in the local unemployment rate of 4.6 percentage points, with a standard deviation of 1.5 percentage points. Each of the three samples comprises roughly one million individuals.

Figure 3 displays a heat map of Great Recession local shocks. Familiar patterns are apparent, such as severe shocks in certain manufacturing areas and California's Central Valley but not along California's coast. Of the variation in Great Recession local shocks, 30.0 percent is statistically explained by the house price–driven percent change in household net worth in 2006–9 (Mian and Sufi 2009; correlation: 0.547). Recalling Section I's example, Phoenix—America's sixth-largest city, shown in the medium-dark-shaded CZ in the middle of Arizona—experienced a 77th percentile

[16] The main result below is nearly identical when using Local CPI (consumer price index) 2—the more aggressive of the Moretti (2013) local price deflators—to locally deflate 2006 earnings before binning. Chosen to create roughly even-sized bins, the bin minimums are $0, $2,000, $4,000, $6,000, $8,000, $10,000, $15,000, $20,000, $25,000, $30,000, $35,000, $40,000, $45,000, $50,000, $75,000, and $100,000.

TABLE 1
SUMMARY STATISTICS

	Main Sample		Retail Chain Sample		Mass-Layoffs Sample	
	Mean (1)	Standard Deviation (2)	Mean (3)	Standard Deviation (4)	Mean (5)	Standard Deviation (6)
Outcomes (in 2015):						
Employed (%)	79.1	40.7	81.8	38.5	84.1	36.5
Earnings (2015 $)	47,587	63,784	33,381	44,557	48,204	62,830
UI receipt sometime 2007–14 (%)	25.6	43.6	28.3	45.0	52.2	50.0
DI receipt (%)	6.2	24.2	6.9	25.3	6.0	23.8
Personal characteristics (in 2006, 2007):						
Female (%)	49.3	50.0	60.8	48.8	44.5	49.7
Age (years)	39.9	5.7	39.2	5.8	39.7	5.7
Aged 30–34 (%)	22.2	41.5	27.0	44.4	23.8	42.6
Aged 35–39 (%)	24.5	43.0	25.0	43.3	25.0	43.3
Aged 40–44 (%)	26.0	43.9	24.3	42.9	25.5	43.6
Aged 45–49 (%)	27.3	44.6	23.6	42.5	25.7	43.7
Earnings (2015 $)	45,652	55,122	33,424	36,708	52,511	55,336
No labor force attachment (%)	22.7	41.9	15.1	35.8	10.0	29.9
Low labor force attachment (%)	14.9	35.6	24.3	42.9	16.4	37.1
High labor force attachment (%)	62.4	48.4	60.6	48.9	73.6	44.1
Married (%)	62.8	48.3	52.2	50.0	52.9	49.9
0 children (%)	36.2	48.0	41.8	49.3	40.6	49.1
1 children (%)	22.6	41.8	22.9	42.0	23.4	42.3
2+ children (%)	41.2	49.2	35.4	47.8	36.0	48.0

Mortgage holder (%)	38.3	48.6	25.9	43.8	38.4	48.6
Retail trade (NAICS 44,45; %)	5.2	22.3	100.0	.0	4.7	21.2
Construction/manufacturing (NAICS 23, 31–33; %)	11.9	32.4	.0	.0	17.7	38.2
Other observed industry (%)	25.9	43.8	.0	.0	34.9	47.7
Contractor (%)	4.2	20.1	.0	.0	.0	.0
Nonemployed (%)	11.5	31.9	.0	.0	.0	.0
Great Recession local shock (pp)	4.6	1.5	4.8	1.5	5.0	1.5
No. of individuals	1,357,974		865,954		1,001,543	
No. of 2007 CZs	722		655		667	

Note.—This table lists summary statistics for the paper's three tax-data-based samples: the main sample (a 2 percent random sample), the retail chain sample (all year-2006 nonheadquarters workers for identifiable retail chain firms), and the mass-layoffs sample (all workers who separated from a firm in a 2008 or 2009 mass layoff). All samples are restricted to American citizens aged 30–49 on January 1, 2007, who had not died by December 31, 2015 and who had a continental United States ZIP code in January 2007. Earnings is the sum of W-2 wage earnings and 1099-MISC independent contractor earnings in the calendar year, in 2015 dollars and top-coded at $500,000. "Employed" is an indicator for having positive earnings. "UI receipt sometime 2007–14" is an indicator for having positive 1099-G unemployment insurance benefit income at some point in 2007–14. "DI receipt" is an indicator for having positive 1099-SSA disability insurance income in the calendar year. Age is measured on January 1, 2007. Labor force attachment is high if the individual had, in every year 2003–6, earnings above 1,600 hours times the federal minimum wage, zero if the individual was nonemployed in any year 2003–6, and low otherwise. "Married" is an indicator for filing a married-filing-jointly or married-filing-separately Form 1040 for tax year 2006. "Children" are dependent children currently living with the individual as listed on the filed 1040 form. A 1040 filer is a primary or secondary filer on a 1040 form for tax year 2006. Displayed marriage and number-of-children statistics are restricted to 1040 filers; in regressions controlling for marriage or number-of-children fixed effects, non-1040-filers are included as a separate group. "Mortgage holder" is an indicator for having positive mortgage payment listed on a Form 1098 in 2006 (mortgages held only in the name of a worker's spouse or other third party are not included here). Industry categories are based on the NAICS code on the business income tax return matched to the individual's highest-paying 2006 W-2 form. Nearly half of W-2 earners could not be matched, and individuals who had only 1099-MISC independent contractor earnings are not matched; in fixed-effect regressions, unmatched 2006 W-2 earners, contractors, and the nonemployed are assigned to three separate industries. The 2007 CZ derives from the individual's January 2007 residential ZIP code, as reflected most commonly on her 2006 information returns. Great Recession local shock (pp: percentage points) equals the 2009 unemployment rate in the individual's 2007 CZ minus the 2007 unemployment rate in that CZ as reported in the BLS LAUS.

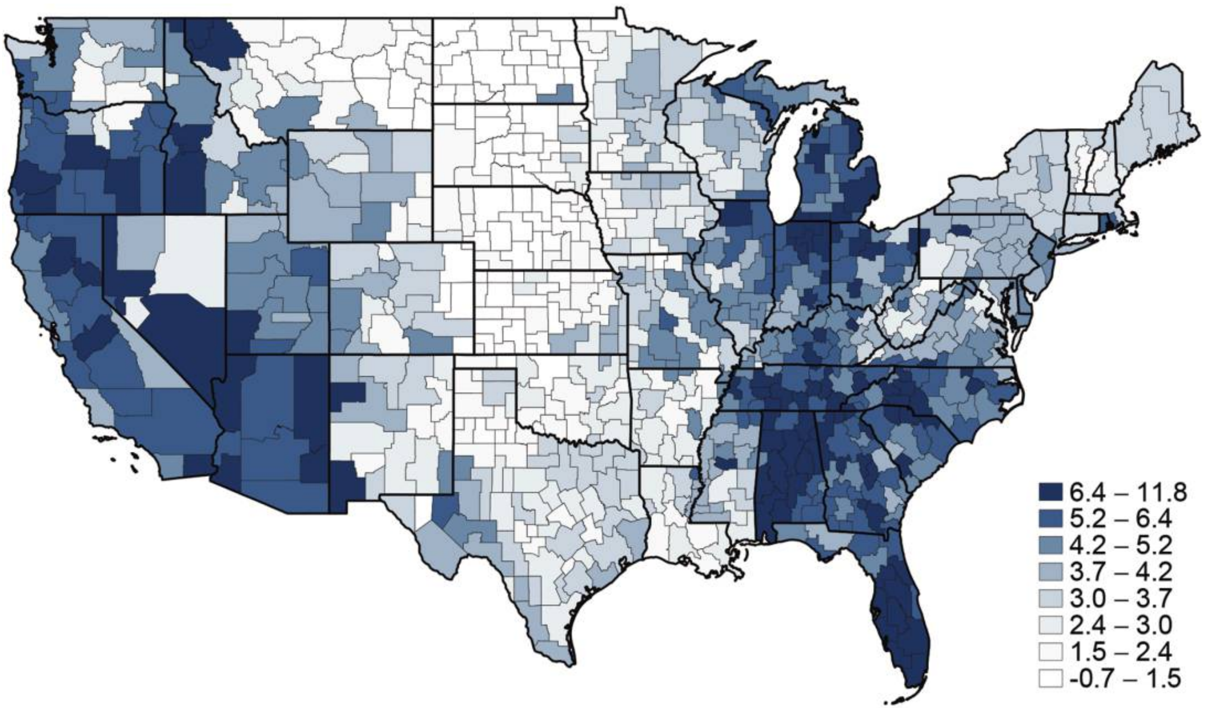

FIG. 3.—Great Recession local shocks. This map depicts unweighted octiles (divisions by increments of 12.5 percentiles) of Great Recession local shocks across commuting zones (CZs). CZs span the entire United States and are collections of counties that share strong commuting ties. Each CZ's shock equals the CZ's 2009 LAUS unemployment rate minus the CZ's 2007 LAUS unemployment rate. In the individual-level analysis, I assign each individual to the Great Recession local shock of the individual's January 2007 CZ.

shock (6.0 percentage points), while San Antonio—America's seventh-largest city, shown in the large, faintly shaded, landlocked CZ in the middle-bottom of Texas—experienced a 7th percentile shock (2.6 percentage points). The empirical analysis compares the 2015 outcomes of individuals who were living in 2007 in places like Phoenix to initially similar individuals who were living in 2007 in places like San Antonio.

IV. Overall Impacts

This section presents the paper's main result: the estimated effect on 2015 employment of Great Recession local shocks. I begin by presenting the main regression estimate visually and in table form, followed by robustness and extrapolation exercises.

A. *Preferred Estimates*

Figure 4*A* plots the time series of estimated effects of Great Recession local shocks on employment. Each year *t*'s data point equals $\hat{\beta}$ from the following version of equation (1) estimated on the main sample:

$$e_{it} = \beta \text{SHOCK}_{c(i2007)} + \theta_{g(i2006)} + \epsilon_{it}, \tag{2}$$

where relative employment $e_{it} \equiv \text{EMPLOYED}_{it} - (1/8)\Sigma_{s=1999}^{2006} \text{EMPLOYED}_{is}$ is *i*'s change in mean binary employment status from prerecession years to year *t*, $\text{SHOCK}_{c(i2007)}$ denotes the Great Recession shock to *i*'s 2007 CZ, and $\theta_{g(i2006)}$ denotes 2006 age-earnings-industry fixed effects. Measuring employment outcomes relative to each individual's prerecession mean transparently allows for baseline employment rate differences, similar to the relative cumulative earnings outcome of Autor et al. (2014). The identifying assumption is that Great Recession local shocks are as good as randomly assigned, conditional on age, initial earnings, and initial industry. The sample and independent variable values are fixed across figure 4*A*'s annual regressions; only the outcome varies from year to year. The 95 percent confidence intervals are plotted in vertical lines unadjusted for multiple hypotheses, based on standard errors clustered by 2007 state.

The 2015 data point shows the paper's main result: a 1 percentage point higher Great Recession local shock (2007–9 spike in the CZ unemployment rate) caused the average working-age American to be an estimated 0.393 percentage points less likely to be employed in 2015. The 2015 impact of Great Recession local shocks is very significantly different from zero, with a *t*-statistic of 4.1.

Figure 4*B* supports the linear specification of equation (2) by plotting the underlying conditional expectation. It is constructed by regressing each

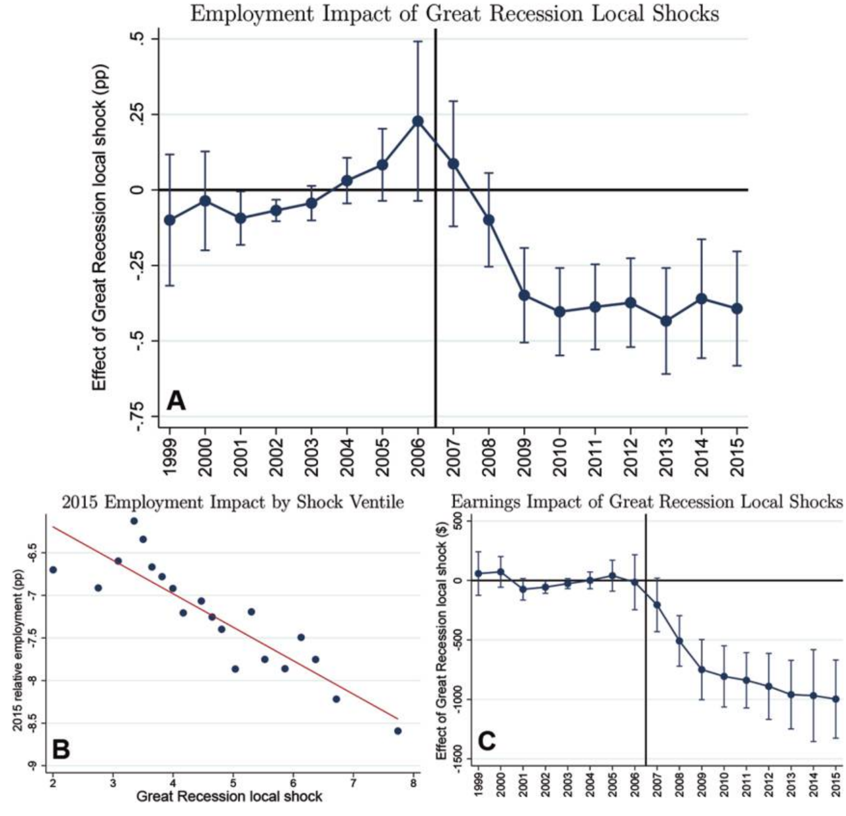

FIG. 4.—Employment and earnings impacts of Great Recession local shocks. *A*, Regression estimates of the effect of Great Recession local shocks on relative employment, controlling for 2006 age-earnings-industry fixed effects in the main sample. Each year *t*'s outcome is year *t* relative employment: the individual's year *t* employment (indicator for any employment in *t*) minus the individual's mean 1999–2006 employment. The 95 percent confidence intervals are plotted around estimates, clustering on 2007 state. For reference, the 2015 data point (the paper's main estimate) implies that a 1 percentage point higher Great Recession local shock caused individuals to be 0.393 percentage points less likely to be employed in 2015. *B*, This graph nonparametrically depicts the relationship underlying the main estimate. It is produced by regressing Great Recession local shocks on 2006 age-earnings-industry fixed effects, computing residuals, adding back the mean shock level for interpretation, and plotting means of 2015 relative employment within 20 equal-sized bins of the shock residuals. Overlaid is the best-fit line, whose slope equals −0.393. *C*, This graph replicates *A* for the outcome of relative earnings: the individual's year-*t* earnings minus the individual's mean 1999–2006 earnings.

individual's Great Recession local shock on the age-earnings-industry fixed effects, computing residuals, adding the mean shock to the residuals, ordering and binning the residuals into 20 evenly sized bins, and plotting mean 2015 relative employment within each bin versus the bin's mean residual. The displayed nonparametric relationship between 2015 relative employment and Great Recession local shocks is largely linear.

Returning to figure 4*A*, the prerecession time series of estimated effects constitute placebo tests supporting the identifying assumption that, conditional on controls, Great Recession local shocks were as good as randomly assigned. In particular, the 1999–2006 point estimates do not display a downward pretrend that would suggest a negative 2015 point estimate even in the absence of Great Recession local shocks. The graph makes other statistics possible. For example, the 1999–2006 point estimates average zero by construction, but one could prefer to benchmark the 2015 estimate to a subset of prerecession estimates such as those for 1999–2001; when subtracting the 1999–2001 mean estimate from the 2015 estimate, one obtains the still-large effect of −0.316. When subtracting the 2004–6 mean estimate, one obtains −0.507.

Table 2 displays coefficient estimates from equation (2) in the main sample under various specifications. Column 4 corresponds to figure 4*A*'s 2015 data point, my preferred estimate. Columns 1–3 replicate the analysis with coarser fixed effects and yield similar results, indicating relatively little selection on the controlled dimensions among working-age Americans. Column 5 displays ordered effect sizes by shock quintile, indicating, for example, that living in 2007 in the most shocked quintile of CZs resulted in the average individual being 1.75 percentage points less likely to be employed in 2015 relative to living in 2007 in the least shocked quintile. These effects are large, in that they are similar in magnitude to the age-adjusted US employment rate decline 2007–15.

Full-year nonemployment indicates either long-term unemployment or labor force nonparticipation ("exit"). Unemployment and participation are not observed in tax data. To provide an indication of whether the nonemployment effects of Great Recession local shocks reflect labor force exit, I test whether controlling for local unemployment persistence—equal to the CZ's 2015 unemployment rate minus its 2007 unemployment rate—in the individual's 2007 CZ (col. 6) or 2015 CZ (col. 7) attenuates the main estimate. This test can be viewed as conservative: controlling for epsilon-higher unemployment persistence in relatively severely shocked areas could fully attenuate the main estimate without that unemployment persistence being able to explain it quantitatively. In practice, the controls in columns 6 and 7 slightly and insignificantly attenuate the main estimate from −0.393 to −0.366 and −0.364, respectively. This suggests that most and possibly all of the 2015 nonemployment impact of Great Recession local shocks took the form of labor force exit, consistent with cross-state patterns in figure 1*B*.[17]

[17] Local unemployment rates converged throughout 2015. When only the July–December period was used to define local unemployment persistence, the controls in cols. 6 and 7 leave the estimate unchanged, at −0.392 and −0.393, respectively.

TABLE 2
2015 Impacts of Great Recession (GR) Local Shocks: Outcomes Relative to Pre-2007 Mean

	N = 1,357,974						
	A. 2015 Employment (pp)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GR local shock	−.412	−.425	−.417	−.393		−.366	−.364
	(.112)	(.112)	(.099)	(.097)		(.089)	(.089)
Most severely shocked quintile					−1.746		
					(.471)		
Fourth shock quintile					−1.144		
					(.434)		
Third shock quintile					−.793		
					(.356)		
Second shock quintile					−.181		
					(.320)		
Age FEs		X					
Age-earnings FEs			X				
Age-earnings-industry FEs				X	X	X	X
Unemployment persistence in 2007 CZ						X	
Unemployment persistence in 2015 CZ							X
R^2	.00	.00	.01	.07	.07	.07	.07
Outcome mean	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23
Absolute outcome mean	79.1	79.1	79.1	79.1	79.1	79.1	79.1
Standard deviation of GR local shocks	1.49	1.49	1.49	1.49	1.49	1.49	1.49
Interquartile range of GR local shocks	2.31	2.31	2.31	2.31	2.31	2.31	2.31

	B. Additional Outcomes and Controls					
	Cumulative Employment 2009–15 (pp)	Earnings in 2015 ($)	Cumulative Earnings 2009–15 ($)	Employed in 2015 (pp)		
	(8)	(9)	(10)	(11)	(12)	(13)
GR local shock	−2.700	−997	−6,212	−.364	−.480	−.378
	(.516)	(168)	(919)	(.100)	(.133)	(.112)
Rust CZ × GR local shock					.067	−.035
					(.192)	(.148)
Other CZ × GR local shock					.094	
					(.250)	
Age-earnings-industry FEs	X	X	X	X	X	X
Manufacturing share				X		
R^2	.07	.11	.13	.07	.07	.07
Outcome mean	−40.5	6,249	27,646	−7.23	−7.23	−7.23
Absolute outcome mean	563.9	47,587	317,011	79.1	79.1	79.1
Standard deviation of GR local shocks	1.49	1.49	1.49	1.49	1.49	1.49
Interquartile range of GR local shocks	2.31	2.31	2.31	2.31	2.31	2.31

NOTE.—All columns except col. 5 report coefficient estimates of the effect of Great Recession local shocks on postrecession outcomes in the main sample. Column 5 divides individuals into quintiles based on their Great Recession local shocks and reports coefficients on indicators of shock quintiles, relative to the least shocked quintile. Age fixed effects (FEs) are birth year indicators. Earnings FEs are indicators for 16 bins in the individual's 2006 earnings. Industry FEs are indicators for the individual's 2006 four-digit NAICS industry. Local unemployment persistence equals the 2015 LAUS unemployment rate minus the 2007 LAUS unemployment rate in either the individual's 2007 CZ or the individual's 2015 CZ. The outcome in cols. 1–7 is 2015 relative employment: the individual's 2015 employment (indicator for any employment in 2015) minus the individual's mean 1999–2006 employment. The col. 8 outcome equals the sum of the individual's 2009–15 employment minus seven times the individual's mean 1999–2006 employment. The col. 9 outcome equals the individual's 2015 earnings minus the individual's mean 1999–2006 earnings. The col. 10 outcome equals the sum of the individual's 2009–15 earnings minus seven times the individual's mean 1999–2006 earnings. Column 11 controls for the 2000 manufacturing share of employment in the individual's 2007 CZ, computed in County Business Patterns. Columns 11 and 12 control for indicators (not shown) and interactions of a Rust-CZ indicator and an Other-CZ indicator, based on the individual's 2007 CZ. A Rust CZ is a CZ with an above-median manufacturing share; an Other CZ is a CZ with a below-median manufacturing share and an above median 2006–9 change in housing net worth from Mian and Sufi (2014). The absolute outcome mean equals the outcome mean before subtraction of the prerecession mean. Standard errors (in parentheses) are clustered by 2007 state. For reference, col. 4 (the paper's main specification) indicates that a 1 percentage point higher Great Recession local shock caused individuals to be 0.393 percentage points less likely to be employed in 2015.

Figure 4*C* repeats figure 4*A* for the alternative outcome of relative earnings: $\text{EARNINGS}_{it} - (1/8)\Sigma_{s=1999}^{2006}\text{EARNINGS}_{is}$. Similar to figure 4*A*'s, figure 4*C*'s coefficient estimates exhibit no consistent trend in the prerecession period and then fall persistently after the recession. Table 2, column 9, presents the graph's 2015 data point, which indicates that living in 2007 in a CZ that experienced a 1 percentage point higher unemployment shock caused the average working-age American to earn 997 fewer dollars in 2015.[18] Column 10 indicates that when cumulative relative earnings $\Sigma_{t=2009}^{2015}[\text{EARNINGS}_{it} - (1/8)\Sigma_{s=1999}^{2006}\text{EARNINGS}_{is}]$ are considered, the effect size cumulates to −$6,212 over the 7-year period 2009–15. Multiplied by the interquartile range of Great Recession shocks, this last point estimate implies an average cumulative earnings loss of $14,352, unconditional on layoff or nonemployment.

Finally, traditional analyses conceive of the Great Recession as a fluctuation that interrupted a long-run trend. However, it is possible that the mid-2000s period was a positive "masking" fluctuation (Charles et al. 2016) around a long-run secular decline in manufacturing employment (Charles, Hurst, and Schwartz 2018) without extremely low unemployment rates. In that case, this section would still identify 2015 employment impacts of the masking-ending recession, and one may then interpret 2015 employment as being in line with a premasking trend.

I test for the manufacturing-driven unmasking interpretation of this section's results in two ways. First, I use the Census Bureau's County Business Patterns (CBP) for 2000 to compute each CZ's manufacturing share of employment. Column 11 repeats column 4, except that it controls for the individual's 2007 CZ's manufacturing share. The coefficient falls by less than one-half of one standard error, to −0.364, and is still very statistically significant.[19] Thus, the effect of Great Recession local shocks holds within CZs with similar manufacturing shares. Second, for columns 12–13, I use the Mian and Sufi (2014) county-level measure of the 2006–9 change in housing net worth to compute each CZ's 2006–9 change in housing net worth. I then group individuals into three bins according to their 2007 CZ: CZs with an above-median manufacturing share ("Rust," since many are in Rust Belt states), CZs with a below-median manufacturing share and a below-median (i.e., especially adverse) change in housing net worth ("Sun," since many are in Sun Belt states), and "Other" CZs. Column 12 repeats column 4, except that it includes an interaction of the

[18] Fig. 6*B* scales this estimate by the individual's prerecession earnings. The earnings analysis makes no correction for local cost-of-living changes. Measuring changes in local living costs remains contentious, given the difficulty of measuring changes in local amenities (Moretti 2013) and a historical presumption that local amenities exactly offset apparent cross-area real income differences (Rosen 1979; Roback 1982).

[19] The coefficient is −0.358 when a quartic in the manufacturing share is controlled for.

Rust indicator and the Great Recession local-shock variable, an interaction of the Other indicator and the shock variable, and the indicators separately. Column 13 repeats column 12, except without the Other indicator and interaction. Both columns exhibit large and significant main effects of the shock variable, indicating that the effect holds within Sun CZs and is not driven only by Rust CZs. However, much room remains for manufacturing-driven masking effects, including effects across industries and effects that predated the recession or were otherwise not mediated by Great Recession unemployment shocks.

B. *Basic Robustness*

Table 3 presents several robustness checks. Column 1 replicates the main estimate, from table 2, column 4. Columns 2–5 control, respectively, for individual-level characteristics that could independently determine labor supply: gender, 2006 number of children, 2006 marital status, and 2006 home ownership fixed effects. In case residents of large or growing CZs had different employment trajectories, column 6 controls for the individual's 2007 CZ size, equal to the CZ's total employment in 2006 as reported in the CBP, while column 7 controls for the individual's 2007 CZ's size growth, equal to the CZ's log change in CBP employment from 2000 to 2006. Column 8 controls for the individual's 2007 CZ's share of workers who work outside the CZ, computed from the 2006–10 American Community Surveys and motivated by recent work suggesting that commuting options can attenuate local-shock incidence (Monte, Redding, and Rossi-Hansberg 2015). As an early check of a policy mechanism, column 9 controls for the individual's 2007 state's maximum UI duration over years 2007–15, derived from Mueller, Rothstein, and von Wachter (2015). Column 10 similarly controls for the individual's 2007 state's minimum wage change 2007–15, using data provided by Vaghul and Zipperer (2016) and used in Clemens and Wither (2014). The number of children, marriage, and pre-2007 CZ size growth controls somewhat attenuate the main estimate while others amplify it, and all estimates remain within one-half of one standard error of the main estimate.

Nearly half of 2006 employees could not be matched to an industry code (Sec. III.B). Column 11 confines the sample to the 2006 employees who could be matched to an industry code and to the 2006 nonemployed. Column 12 further omits 2006 employees in construction or manufacturing—two industries that could have disproportionately attracted workers (e.g., non-college-educated men) in severely shocked areas who might have experienced large employment declines even in the absence of the recession due to secular nationwide skill-biased change. Finally, severely shocked CZs such as Phoenix had attracted many in-migrants in the decades leading up to 2007; if those in-migrants had somehow been negatively selected

TABLE 3
ROBUSTNESS OF THE 2015 EMPLOYMENT IMPACTS

	Employed in 2015: Outcome Relative to Pre-2007 Mean (pp)												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
GR local shock	−.393	−.394	−.344	−.344	−.397	−.439	−.412	−.381	−.404	−.399	−.400	−.397	−.477
	(.097)	(.096)	(.090)	(.093)	(.098)	(.093)	(.098)	(.095)	(.095)	(.096)	(.115)	(.129)	(.125)
Main controls	X	X	X	X	X	X	X	X	X	X	X	X	X
Gender		X											
No. of children			X										
Married				X									
Home ownership					X								
CZ size						X							
CZ pre-2007 size growth							X						
Cross-CZ commuting								X					
Max UI duration 2007–15									X				
Minimum wage change 2007–15										X			
Exclude if invalid industry code											X	X	
Exclude if construction/ manufacturing												X	
Instrumented with birth state shock													X
Observations					1,357,974						741,165	579,553	1,357,974

R^2	.07	.08	.09	.08	.07	.07	.07	.07	.07	.07	.11	.10	.07
Outcome mean	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.23	−7.20	−6.69	−7.23
Absolute outcome mean	79.1	79.1	79.1	79.1	79.1	79.1	79.1	79.1	79.1	79.1	73.9	70.8	79.1
Standard deviation of GR local shocks	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49
Interquartile range of GR local shocks	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31

NOTE.—This table adds controls, sample restrictions, or instruments to the specification underlying table 2, col. 4, reprinted here in col. 1. Column 2 controls for the individual's gender. Column 3 controls for the individual's 2006 number of children (fixed effects for 0, 1, or 2+ children). Column 4 controls for the individual's 2006 marital status. Column 5 controls for the individual's 2006 home ownership status. Columns 6–10 control for CZ-level characteristics. Column 6 controls for the individual's 2007 CZ size, equal to the CZ's total employment in 2006 as reported in the Census Bureau's County Business Patterns (CBP). Column 7 controls for the individual's 2007 CZ size growth, equal to the CZ's log change in CBP employment from 2000 to 2006. Column 8 controls for the individual's 2007 CZ's share of workers who work outside the CZ, computed from the 2006–10 American Community Surveys. Column 9 controls for the individual's 2007 state's maximum unemployment insurance duration over years 2007–15. Column 10 controls for the individual's 2007 state's 2015 minimum wage minus that state's 2007 minimum wage. Column 11 excludes 2006 W-2 earners without an industry code and 2006 contractors and thus restricts the sample to those for whom the 2006 industry is correctly measured: 2006 W-2 earners with a valid industry code and the 2006 nonemployed. Column 12 further excludes individuals employed in construction or manufacturing in 2006. Column 13 instruments the individual's Great Recession local shock, using the mean of the Great Recession local shock in the individual's birth state. Standard errors (in parentheses) are clustered by 2007 state.

on future labor productivity or other employment determinants, conditional on the main controls, the main estimate could be confounded. Column 13 addresses this concern by instrumenting one's Great Recession local shock using the mean Great Recession local shock in the individual's birth state. None of these specifications attenuates the main point estimate.

C. *Within-Job Robustness*

The above estimates of the 2015 impact of Great Recession local shocks control for age-earnings-industry fixed effects. Those estimates will be biased if there was secular nationwide skill-biased change in 2007–15 and if skill differed across space within age-earnings-industry bins in a correlated way with Great Recession local shocks. To address this possibility, figure 5*A* and table 4 attempt to better approximate within-skill estimates by controlling for age-earnings-*firm* fixed effects in the retail chain sample. The motivation is that—unlike firms in manufacturing and other industries—retail chain firms such as Walmart and Starbucks employ workers with similar skills to perform the same job at similar earnings in many local areas.[20] The retail chain sample comprises working-age workers who in 2006 worked at a retail chain firm in a local area outside the firm's corporate headquarters. To the extent that age-earnings-firm bins proxy for jobs across space and that skill selection into jobs is similar across space, estimates controlling for age-earnings-firm fixed effects in the retail chain sample will mitigate skill selection threats. I refer to such estimates as within-job estimates.

Figure 5*A* repeats figure 4*A* in the retail chain sample and with 2006 age-earnings-firm fixed effects.[21] Figures 5*A* and 4*A* show broadly similar time series patterns and point estimates. In the retail chain sample, I estimate that a 1 percentage point higher Great Recession local shock resulted in the average working-age American being 0.359 percentage points less likely to be employed in 2015—similar to the 0.393 percentage point estimate in the main sample. Thus, the main result is robust to the within-job specification.

Table 4 repeats table 2 for the retail chain sample and with specifications using 2006 age-earnings-firm fixed effects. Column 5 displays the

[20] In contrast, e.g., Boeing employs workers with strong writing skills in Virginia in order to manage government contracts and employs workers—possibly of the same age and at the same annual earnings—with strong manufacturing skills in Washington State in order to build airplanes.

[21] Table 1 showed that the main and retail chain samples differ demographically, and the next section finds impact heterogeneity across demographic groups. I therefore reweight the retail chain sample to match the main sample, as in DiNardo, Fortin, and Lemieux (1996), along 2007 CZ, gender, 5-year age bin, and 2006 earnings bins, as defined below in fig. 6.

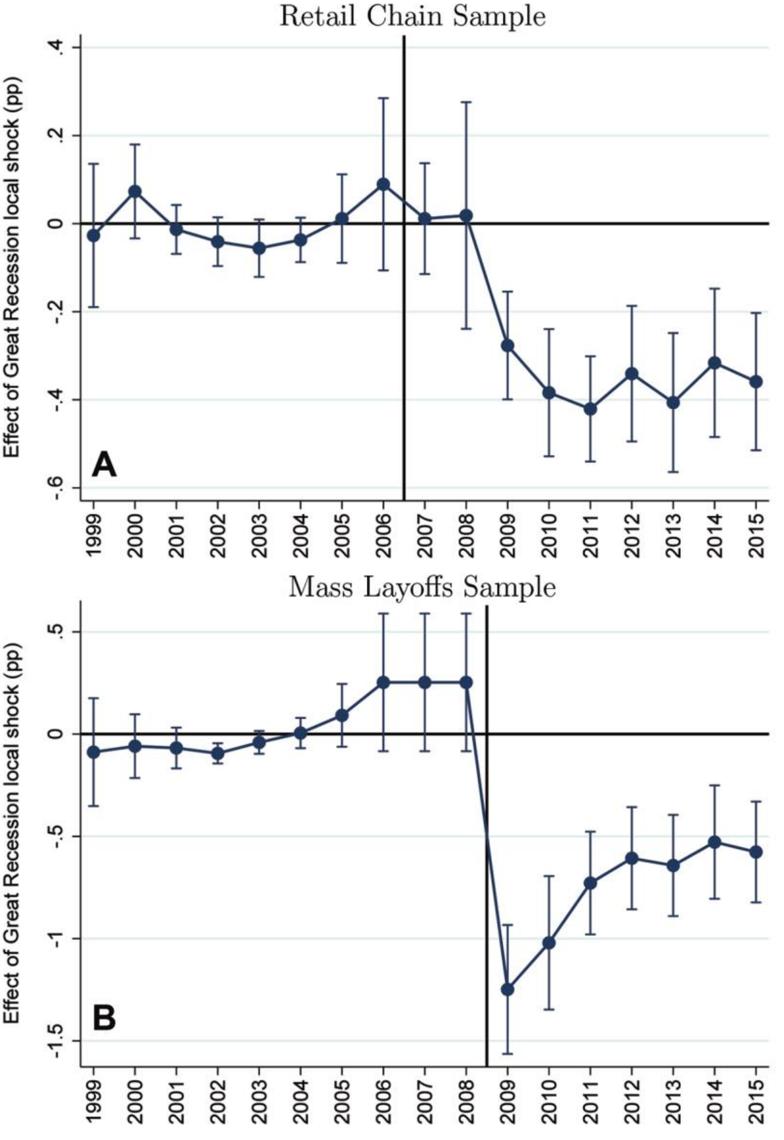

FIG. 5.—Employment impacts in special samples. *A*, This graph replicates figure 4*A* in the retail chain sample (all year-2006 nonheadquarters workers for identifiable retail chain firms), with 2006 age-earnings-firm fixed effects instead of 2006 age-earnings-industry fixed effects. The retail chain sample is reweighted to match the main sample as in DiNardo et al. (1996), along 2007 CZ, gender, 5-year age bin, and 2006 earnings bins as defined in figure 6. Each fully interacted cell with overlap in both samples receives the same total weight in the retail sample as in the main sample; the 0.02 percent of retail chain sample observations with no overlap receive zero weight. Reweighting nearly halves the impact of firm fixed effects (the difference between cols. 4 and 5 of table 4) and negligibly affects the final point estimate. *B*, This graph replicates figure 4*A* in the mass-layoffs sample (all workers who separated from a firm in a 2008 or 2009 mass layoff), also reweighted to match the main sample, as above, with a negligible effect on the final point estimate. See the figure 4*A* legend for specification details.

TABLE 4
2015 Impacts of Great Recession (GR) Local Shocks, Retail Chain Sample: Outcome Relative to Pre-2007 Mean

	$N = 865{,}954$							
	A. Main Specifications: Employed in 2015 (pp)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GR local shock	−.414	−.426	−.398	−.407	−.359		−.340	−.339
	(.118)	(.119)	(.102)	(.094)	(.080)		(.063)	(.061)
Most severely shocked quintile						−1.496		
						(.308)		
Fourth shock quintile						−1.416		
						(.298)		
Third shock quintile						−.968		
						(.288)		
Second shock quintile						−.366		
						(.298)		
Age FEs		X						
Age-earnings FEs			X					
Age-earnings-industry FEs				X				
Age-earnings-firm FEs					X	X	X	X
Unemployment persistence in 2007 CZ							X	
Unemployment persistence in 2015 CZ								X
R^2	.00	.00	.02	.03	.07	.15	.15	.15
Outcome mean	−9.80	−9.80	−9.80	−9.80	−9.80	−9.80	−9.80	−9.80
Absolute outcome mean	81.8	81.8	81.8	81.8	81.8	81.8	81.8	81.8
Standard deviation of GR local shocks	1.49	1.49	1.49	1.49	1.49	1.49	1.49	1.49
Interquartile range of GR local shocks	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31

	B. Additional Outcomes and Controls					
	Cumulative Employment 2009–15 (pp)	Earnings in 2015 ($)	Cumulative Earnings 2009–15 ($)	Employed in 2015 (pp)		
	(9)	(10)	(11)	(12)	(13)	(14)
GR local shock	−2.504	−422	−2,777	−.365	−.589	−.405
	(.430)	(134)	(606)	(.081)	(.111)	(.095)
Rust CZ × GR local shock					.258	.069
					(.144)	(.115)
Other CZ × GR local shock					.414	
					(.251)	
Age-earnings-firm FEs	X	X	X	X	X	X
Manufacturing share				X		
R^2	.15	.21	.23	.15	.15	.15
Outcome mean	−51.5	2,356	8,381	−9.80	−9.80	−9.80
Absolute outcome mean	590.0	33,381	225,554	81.8	81.8	81.8
Standard deviation of GR local shocks	1.49	1.49	1.49	1.49	1.49	1.49
Interquartile range of GR local shocks	2.31	2.31	2.31	2.31	2.31	2.31

NOTE.—This table replicates table 2 in the retail chain sample. See the note to that table for details. "Firm" is an indicator for the individual's 2006 firm (a retail chain firm). The retail chain sample is reweighted to match the main sample, as described in the fig. 5 legend.

2015 point estimate from figure 5*A*. Relative to column 4's estimate controlling only for age-earnings-industry fixed effects, one sees that the firm fixed effects attenuate the point estimate by 11.8 percent, or one-half of one standard error.[22] If the paper's main estimate of −0.393 is overstated by 11.8 percent, then the true main-sample effect size would be −0.347. The retail chain sample's earnings effects are smaller than those in the main sample, though 2015 mean earnings are also smaller in the retail chain sample. Overall, results are similar across the main and retail chain samples.

D. Extrapolation

I close the section with a simple extrapolation of the 2015 employment impact of Great Recession local shocks to the 2015 employment impact of the Great Recession aggregate shock. The exercise adopts the strong "naive" assumption that the impact of the Great Recession aggregate shock on national residents is identical to the impact of a proportionally sized Great Recession local shock on initial local residents, as in similar work (e.g., Charles, Hurst, and Notowidigdo 2015). I find that simple extrapolation suggests that the Great Recession caused 76 percent of the postrecession age-adjusted decline in the working-age US employment rate as measured in this paper (any annual employment of the birth cohorts aged 30–49 in 2007).

The extrapolation estimate of 76 percent derives from three inputs. First, the aggregate US unemployment rate increased by 4.63 percentage points from 2007 to 2009. Second, table 2, column 4, reported that exposure to a 1 percentage point higher local unemployment spike 2007–9 induced a 0.393 percentage point decline in any 2015 employment. Based on these two inputs, simple extrapolation suggests a 1.82 (4.63×0.393) percentage point decline in the US working-age employment rate because of the Great Recession.

Third, the employment rate decline of these birth cohorts through 2015, 7.23 percentage points (table 2), was 2.40 percentage points larger than the decline that would have been expected because of aging, based on analogous earlier cohorts through years 2003–7. Specifically, recall that $-7.23 = \Delta\bar{e}_{2015}$, where $\Delta\bar{e}_t \equiv E[\text{EMPLOYED}_{it}|c(i) \in C_t] - (1/8)\Sigma_{s=t-16}^{t-9} E[\text{EMPLOYED}_{is}|c(i) \in C_t]$ and that C_t is the set of working-age birth cohorts $\{t-58, \ldots, t-39\}$. These cohorts would have experienced an employment rate decline due to aging even in the absence of the recession. I quantify the aging effect by using employment rates of analogous earlier working-age cohorts through years 2003–7 as $(1/5)\Sigma_{t=2003}^{2007}\Delta\bar{e}_t = -4.83$,

[22] Introducing firm fixed effects increased the effect magnitude in an earlier version's specifications.

based on the Current Population Survey's Annual Social and Economic Supplement (ASEC) in lieu of tax data availability before 1999. The age-adjusted working-age employment rate decline was therefore 2.40 (7.23−4.83) percentage points. Hence, simple extrapolation suggests that the Great Recession caused 76 percent (1.82/2.40) of the age-adjusted decline.[23]

It is important to note that the actual impact may be more or less than 76 percent. First, there is statistical and specification uncertainty in the 2015 impact of Great Recession local shocks. Sections IV.A and IV.C described scenarios in which one could believe that the true effect size was closer to 0.3. When 0.300 is used instead of 0.393, the share explained by the recession is 58 percent. Second, there is extrapolative uncertainty because of general equilibrium considerations (Nakamura and Steinsson 2014; Beraja et al. 2016). A shock to one local area can have a larger local impact than a proportionately sized aggregate shock, for example, because production can more easily shift across local areas than across countries. Alternatively, the impact of an aggregate shock may exceed the impact of a proportionately sized local shock on initial local residents, for example, to the extent that initial local residents escaped or dampened local-shock impacts by migrating to other local areas (Blanchard and Katz 1992) more than to other countries.

V. Impact Heterogeneity

This section analyzes impact heterogeneity across individuals. An active literature in labor economics studies determinants of wage earnings inequality within (e.g., Card, Heining, and Kline 2013) and across (e.g., Autor, Katz, and Kearney 2008) worker types. The previous section found that Great Recession local shocks caused 2015 wage earnings inequality *within* worker types: initially similar workers experienced different 2015 employment and earnings outcomes after exposure to different Great Recession local shocks. Figure 6 explores effects of Great Recession local shocks on inequality *across* worker types.

Figure 6*A* displays employment impact heterogeneity. The figure's first five rows plot point estimates and 95 percent confidence intervals of the 2015 employment impact of Great Recession local shocks overall in the main sample (reprinting the main estimate from table 2, col. 4) and in each of four 2006 earnings bins, a common proxy for broad initial skill level (e.g., Autor et al. 2014). I find that low initial earners bore more of the employment incidence of Great Recession local shocks, suggesting

[23] Note that the age-adjusted decline of 2.40 percentage points is similar to the age-adjusted declines in headline BLS point-in-time employment rates reported in the introduction. To account for modest age distribution differences related to the baby boom, cohorts underlying the computation of $(1/5)\Sigma_{t=2003}^{2007}\Delta\bar{e}_t$ are reweighted to match the working-age distribution for t = 2015, as written in the appendix below.

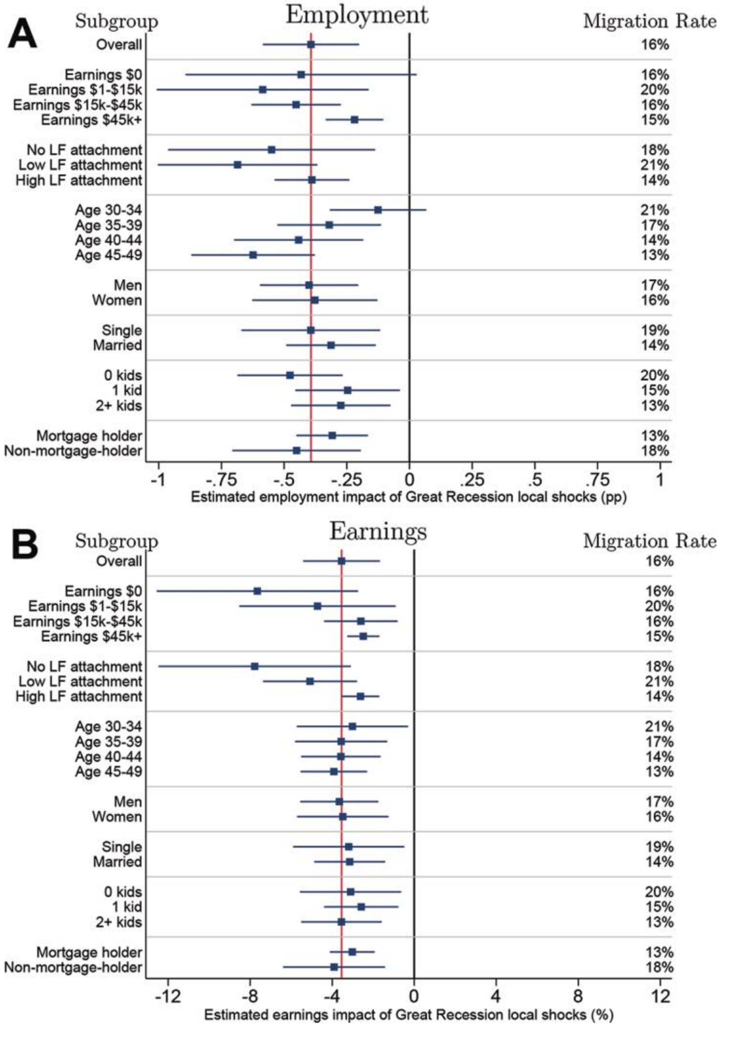

FIG. 6.—Impact heterogeneity. *A*, Coefficients and 95 percent confidence intervals (clustering by 2007 state) of the impact of Great Recession local shocks on 2015 relative employment—overall and by subgroup. All estimates derive from the specification underlying figure 4*A*'s 2015 data point, corresponding here to the overall row. Subgroup estimates restrict the sample to the specified subgroup defined by 2006 earnings, 2003–6 labor force (LF) attachment, 2007 age, gender, 2006 marital status, 2006 number of children, or 2006 mortgage holding. Non-1040-filers are classified here as single and childless. Subgroup migration rates are superimposed on the right. Migration is defined as one's 2015 CZ being different from one's 2007 CZ. *B*, Replication of *A* for 2015 earnings, expressed in multiples of mean annual earnings 1999–2006: 2015 earnings divided by mean annual 1999–2006 earnings. This quantity is top-coded at the 99th percentile: individuals with zero 1999–2006 earnings are assigned the top code if 2015 earnings were positive and assigned 0 otherwise. The overall estimate is −3.55 (standard error: 0.94), implying that a 1 percentage point higher Great Recession local shock reduced the average individual's 2015 earnings by 3.55 percent of her prerecession earnings.

that those shocks increased employment inequality across workers of different initial skill levels. Low initial earners (defined as those who earned less than \$15,000 in 2006, approximately the 33rd percentile in this sample) experienced a worse than average impact, while high initial earners (defined as those who earned more than \$45,000 in 2006, approximately the 67th percentile) experienced a better-than-average impact. This cross-area finding mirrors the earlier cross-time finding that aggregate employment declines since 2007 were concentrated among the least skilled (Hoynes, Miller, and Schaller 2012; Charles et al. 2016). The subsequent three rows suggest worse-than-average impacts among individuals with less labor force attachment (Autor et al. 2014).

Figure 6*B* displays similar patterns for earnings. I analyze proportional earnings changes in order to parallel earlier work on earnings inequality studying earnings ratios, such as the ratio of the 90th and 50th percentiles (Autor et al. 2008). Analogous to that of Autor et al. (2014), the outcome in each regression is the ratio of 2015 earnings to mean annual pre-2007 earnings with no local cost-of-living adjustments: $\text{EARNINGS}_{i2015}/(1/8)\Sigma_{s=1999}^{2006}\,\text{EARNINGS}_{is}$.[24] The overall estimate indicates a large impact of Great Recession local shocks on proportional earnings: a 1 percentage point higher Great Recession local shock reduced the average individual's 2015 earnings by 3.55 percent of her prerecession earnings. The subgroup analysis reveals relatively similar proportional earnings declines across subgroups, except by initial earnings and labor force attachment subgroups, where low initial earners and less-attached individuals experienced larger declines. Hence, both employment and earnings analyses suggest that Great Recession local shocks increased inequality across workers of different initial skill levels.

The remaining rows of figure 6 display impact heterogeneity by gender, 2007 age group, 2006 marital status, 2006 number of children, and 2006 mortgage holding status. I find larger employment effects among older individuals than among younger individuals—consistent with previous work suggesting that older workers are less resilient to labor market shocks (Jacobson et al. 1993).

Finally, one may expect that migration rate heterogeneity explains impact heterogeneity, in light of the lesson from Blanchard and Katz (1992) that population reallocation equilibrates US local labor markets. Figure 6 lists migration rates—defined as the share of individuals with a 2015 CZ different from their 2007 CZ—for each subgroup of individuals to the right of the subgroup-specific impacts. There is no consistent correlation between migration rates and estimated Great Recession local-shock

[24] This quantity is very right skewed and therefore top coded at the 99th percentile. Individuals with zero 1999–2006 earnings are assigned the top code if 2015 earnings were positive and are assigned 0 otherwise.

impacts. For example, non–mortgage holders had an 18 percent migration rate, while mortgage holders had a 13 percent migration rate; yet, if anything, non–mortgage holders appear to have experienced larger impacts. The next section further explores adjustment via out-migration.

VI. Adjustment Margins

This paper's rich administrative data allow me not only to estimate long-term employment and earnings impacts but also to estimate year-by-year adjustments across space and onto social-insurance programs. Table 5 analyzes year-by-year adjustment margins. Each cell lists the coefficient and standard error on the Great Recession local-shock variable from a separate regression in the main analysis sample with the main controls (2006 age-earnings-industry fixed effects), varying only the outcome. Column 1 reproduces the 2007–15 employment estimates plotted in figure 4*A*. Column 2 analyzes migration, using a binary outcome equal to one if and only if an individual's residential CZ in the year was different from her 2007 CZ. The estimates indicate that individuals who experienced more severe Great Recession local shocks were insignificantly (0.073 percentage points) more likely to have moved after 2007 relative to the overall migration rate of 16 percent, suggesting that out-migration was not a major adjustment margin.

Columns 3 and 4 further support that conclusion by disaggregating the annual employment results of column 1 into two types of annual employment: employment inside the individual's 2007 CZ and employment outside the individual's 2007 CZ, similar to Autor et al. (2014).[25] Unsurprisingly, given the main effect of column 1, column 3 shows that individuals subject to more severe Great Recession local shocks were significantly less likely to be employed in their 2007 CZs in 2015. But column 4 shows that these individuals were insignificantly less likely to be employed outside their 2007 CZs as well. Columns 5–7 show analogous results for earnings. Column 7 shows a marginally significant negative effect of Great Recession local shocks on out-of-2007-CZ earnings in 2015, implying no replacement of lost 2015 within-initial-CZ earnings with earnings in other CZs at the 95 percent confidence upper bound.

The lack of an out-migration response may be surprising, given the finding of Section II that population fell relative to trend in severely shocked states and by the same large magnitude predicted by Blanchard and Katz (1992). However, population reallocation is consistent with substantially

[25] That is, the col. 3 outcome for a year t equals $(1 - \text{MOVED}_{it}) \times \text{EMPLOYED}_{it} - (1/8)\Sigma_{s=1999}^{2006}\,\text{EMPLOYED}_{is}$, where MOVED_{it} (the col. 2 outcome) equals one if $c(it) \neq c(i2007)$ and zero otherwise. The col. 4 outcome for a year t equals $\text{MOVED}_{it} \times \text{EMPLOYED}_{it} - (1/8)\Sigma_{s=1999}^{2006}\,\text{EMPLOYED}_{is}$.

TABLE 5
TIME SERIES OF ADJUSTMENT MARGINS

	Outcome (Relative or Absolute)								
	Employed (pp) (1)	Migrated outside 2007 CZ (pp) (2)	Employed in 2007 CZ (pp) (3)	Employed outside 2007 CZ (pp) (4)	Earnings ($) (5)	Earnings in 2007 CZ ($) (6)	Earnings outside 2007 CZ ($) (7)	UI Income ($) (8)	SSDI Income ($) (9)
Effect in 2007	.087	.000	.087	.000	−205	−205	0	14.3	−3.8
	(.106)		(.106)		(115)	(115)		(12.2)	(8.9)
Effect in 2008	−.099	.036	−.098	−.001	−508	−480	−28	36.3	−3.4
	(.079)	(.119)	(.077)	(.006)	(108)	(108)	(11)	(15.6)	(9.4)
Effect in 2009	−.349	.109	−.321	−.028	−750	−687	−63	94.0	.5
	(.080)	(.208)	(.077)	(.012)	(129)	(127)	(18)	(32.1)	(11.8)
Effect in 2010	−.403	.209	−.367	−.037	−807	−736	−71	83.9	2.6
	(.074)	(.272)	(.074)	(.017)	(131)	(125)	(30)	(32.1)	(13.0)
Effect in 2011	−.387	.248	−.339	−.048	−840	−745	−94	43.1	7.5
	(.072)	(.296)	(.071)	(.022)	(119)	(105)	(35)	(24.3)	(13.7)
Effect in 2012	−.373	.244	−.324	−.049	−890	−784	−106	19.9	9.8
	(.075)	(.334)	(.076)	(.026)	(141)	(117)	(45)	(20.1)	(15.2)
Effect in 2013	−.434	.180	−.365	−.069	−960	−810	−149	7.7	13.0
	(.089)	(.382)	(.091)	(.032)	(147)	(108)	(56)	(16.3)	(17.0)
Effect in 2014	−.360	.134	−.322	−.038	−968	−799	−169	−3.0	15.7
	(.100)	(.422)	(.101)	(.031)	(197)	(135)	(84)	(9.9)	(17.7)
Effect in 2015	−.393	.073	−.338	−.055	−997	−786	−211	−5.9	19.6
	(.097)	(.456)	(.100)	(.042)	(168)	(108)	(101)	(8.9)	(18.8)

NOTE.—This table expands on the specifications of table 2, cols. 4 and 9, whose results are reprinted here in the bottom rows of cols. 1 and 5, respectively. Each cell reports the coefficient on the Great Recession local-shock variable from a separate regression in which the outcome uses the postrecession year indicated in the row, instead of exclusively using 2015, as in table 2. Every regression uses the same 1,357,974 observations underlying table 2. The col. 1 outcome of relative employment is defined in table 2, varying the postrecession year between 2007 and 2015. The col. 2 outcome is an indicator for out-migration, equal to the individual's year-f CZ being different from her 2007 CZ. Columns 3 and 4 separate the col. 1 outcome for year f into two outcomes: employment in year f in the individual's 2007 CZ and employment in year f outside the individual's 2007 CZ, each minus mean 1999–2006 employment. The col. 5 outcome is defined in table 2. Columns 7 and 8 separate the col. 5 outcome analogously to cols. 3–4. The col. 8 outcome is the individual's unemployment insurance benefits in year f. The col. 9 outcome is the individual's Social Security Disability Insurance benefits in year f. Standard errors (in parentheses) are clustered by 2007 state. See online appendix table 2 for pretrends.

reduced in-migration rather than substantially increased out-migration (Monras 2015). Autor et al. (2014) similarly find that out-migration was not a major margin of adjustment to import competition. I leave to other work why more individuals did not out-migrate or where they migrated (Yagan 2014).

Columns 8 and 9 study adjustment via social-insurance transfer payments: UI benefits and SSDI benefits.[26] Unsurprisingly, individuals exposed to larger Great Recession local shocks received significantly higher mean UI benefits in 2008–10 than those exposed to smaller local shocks. However, Great Recession UI benefits soon expired, and these individuals' higher mean UI benefits declined after 2010 to insignificantly lower UI benefits by 2015. Column 9 shows that individuals subject to Great Recession local shocks accumulated rising, though insignificantly higher, SSDI benefits relative to those subject to smaller Great Recession local shocks. Comparing the sum of the 2007–15 UI and SSDI income coefficients to the sum of the column 5 earnings coefficients, one estimates that elevated UI and SSDI transfer payments replaced 5.1 percent of lost earnings: 4.2 percent from UI and 0.9 percent from SSDI. Focusing only on 2015 SSDI income, one estimates that it replaced 2.0 percent (19.6/997) of lost 2015 earnings, rising to 5.6 percent at the 95 percent confidence upper bound.[27] Thus, observed transfer payments far from fully compensated for the negative earnings effects.[28]

SSDI has been found to be an important margin of adjustment to labor market shocks (Autor and Duggan 2003; Autor et al. 2013, 2014). Expanding on table 5's null SSDI results, table 6, column 1, replicates the paper's main specification for the binary outcome of SSDI receipt in 2015. I find a moderate and insignificant estimated impact, though with substantial uncertainty: a point estimate of 0.071 percentage points and a standard error of 0.145 percentage points, relative to the employment impact of −0.393 percentage points.

More thoroughly, one can estimate the share of the incrementally nonemployed in severely shocked areas who were on SSDI in 2015. Table 6,

[26] UI provides temporary cash benefits to laid-off workers who had earned above a minimum threshold in the quarters preceding layoff. SSDI provides typically permanent (until retirement age) cash and medical benefits to individuals with at least 5 years of work history in the 10 years before the individual developed a long-lasting medical condition deemed to prevent substantial employment.

[27] To estimate the upper bound, I regress 2015 SSDI on 2015 relative earnings instrumented with Great Recession local shocks, controlling for 2006 age-earnings-industry fixed effects and clustering on 2007 state. The coefficient on 2015 relative earnings is −0.020, with a standard error of 0.018.

[28] Spousal labor supply was likely also a very incomplete adjustment margin: the shocks studied here are measured as CZ-wide shocks, both genders and marital statuses suffered large impacts (fig. 6), and earlier work in similar data showed that wives replaced only 5.6 percent of males' lost income after layoff (Hilger 2016).

column 2, estimates the effect of Great Recession local shocks on a new outcome similar to that of Lee (2009): an indicator for whether the worker was employed in 2015 *or* received SSDI in 2015, minus mean employment 1999–2006.[29] Table 6, column 2, reports that Great Recession local shocks had a significant negative effect, −0.265 percentage points, on the employed-or-SSDI outcome, suggesting that 32.6 percent (1−(−0.265/−0.393)) of the incrementally nonemployed received SSDI in 2015. However, the standard errors are substantial.

The potentially modest role of SSDI in absorbing individuals after Great Recession local shocks is consistent with aggregate patterns. The share of age-16–65 Americans on SSDI rose for three decades before decelerating after 2010 and declining absolutely in both 2014 and 2015. Aggregate SSDI applications spiked after the Great Recession, as they have after previous recessions, but Maestas, Mullen, and Strand (2015) use data through 2012 to estimate that virtually all of the Great Recession–induced applications were initially declined. SSDI application is not observed in this paper's data.

VII. Layoffs and Nonemployment Trajectories

Table 5 showed that statistically significant nonemployment effects began in 2009 and that there were statistically significant effects on UI benefits 2008–10. A large literature connects layoffs and long-term earnings losses (Topel 1990; Ruhm 1991; Jacobson et al. 1993; Neal 1995; Kahn 2010; Davis and von Wachter 2011). I therefore provide additional evidence on layoffs and long-term employment losses.

Table 6, column 6, replicates the main specification for the binary outcome of ever having received UI in 2007–14—a good proxy for ever having been laid off in 2007–14.[30] The column indicates that a 1 percentage point higher Great Recession local shock induced individuals to be 1.43 percentage points more likely to receive UI in 2007–14, with a *t*-statistic over 3 and relative to the sample mean of 25.6 percent.

The substantial 2007–14 layoff effect relative to the 2015 employment effect suggests that most of the incrementally nonemployed from severely shocked areas may have been laid off. Column 7 confirms that suggestion. Analogous to column 2's employed-or-on-SSDI analysis, column 7 replicates the main specification on a new outcome: an indicator for whether

[29] That is, the outcome equals $\max\{\text{EMPLOYED}_{i2015}, \text{SSDI}_{i2015}\} - (1/8)\Sigma_{s=1999}^{2006}$ EMPLOYED_{is}, where SSDI_{i2015} equals one if *i* received SSDI in 2015 and zero otherwise.

[30] Kawano and LaLumia (2017) show that UI-tax-data-based unemployment rates are close in both level and trend to official BLS unemployment rates for 1999–2011 (correlation: 0.94). Earlier papers typically proxy for layoff by using firm separation during a firm's large downsizing; my measure here is not limited to large downsizings.

TABLE 6
Additional Outcomes

	A. Disability Insurance Receipt in Main Sample (pp)		B. Employment Impacts in Mass Layoffs Sample (pp)		
	SSDI Receipt in 2015 (1)	2015 Relative Employment or SSDI Receipt (2)	2015 Relative Employment (3)	2015 Relative Employment (4)	2015 Relative Employment (5)
Great Recession local shock	.071	−.265	−.577	−.628	−.605
	(.145)	(.099)	(.126)	(.118)	(.125)
Main controls	X	X	X	X	X
Exclude if invalid industry code				X	X
Exclude if construction/ manufacturing					X
Observations	1,357,974	1,357,974	1,001,543	573,493	396,377
R^2	.12	.09	.11	.17	.15
Outcome mean	6.22	−2.28	−10.12	−10.45	−9.85
Absolute outcome mean	6.22	84.06	84.12	83.11	83.20

	C. Layoffs and Nonemployment in Main Sample					
	UI Receipt Sometime 2007–14 (6)	2015 Relative Employment or UI Receipt Sometime 2007–14 (7)	2015 Relative Employment (8)	Relative Nonemployment 2007–14 (9)	2015 Relative Employment (10)	2013–15Relative Employment (11)
Great Recession local shock	1.431	−.019	−.354	.487	−.057	−.285
	(.418)	(.121)	(.099)	(.122)	(.111)	(.101)
UI receipt sometime 2007–14			−2.734			
			(.142)			
Main controls	X	X	X	X	X	X
Employment 2007–12					X	
Observations	1,357,974	1,357,974	1,357,974	1,357,974	1,357,974	1,357,974
R^2	.16	.06	.08	.08	.26	.07
Outcome mean	25.6	−2.8	−7.2	3.0	−7.2	−1.2
Absolute outcome mean	25.6	83.6	79.1	3.0	79.1	85.2

Note.—The table reports estimates of the specification in table 2, col. 4, with alternative outcomes, samples, and/or controls. Column 1 replicates the main specification, using the outcome of an indicator for 2015 receipt of Social Security Disability Insurance. Column 2 replicates the main specification, using the outcome of an indicator for 2015 employment or 2015 SSDI receipt, minus the individual's mean employment 1999–2006. Column 3 replicates the main specification, and cols. 4–5 replicate table 3, cols. 11–12, in the mass-layoffs sample. Column 6 replicates the main specification, using the outcome of an indicator for unemployment insurance benefit receipt at some point 2007–14. Column 7 replicates col. 2 but uses UI receipt in 2007–14 in place of 2015 SSDI receipt. Column 8 replicates the main specification, controlling for UI receipt sometime in 2007–14. Column 9 replicates the main specification, using the outcome of an indicator for having any year of nonemployment during 2007–14, minus an indicator for having any year of nonemployment 1999–2006. Column 10 replicates the main specification while controlling for indicators of employment in each year 2007–12. Column 11 replicates the main specification, using the outcome of an indicator for employment in any year 2013–15, minus the individual's mean employment 1999–2006. Standard errors (in parentheses) are clustered by 2007 state.

the individual was employed in 2015 *or* received UI at some point in 2007–14, minus the individual's mean employment status in 1999–2006. The column 7 estimate is nearly zero and indicates that Great Recession local shocks had no statistically significant impact on the employed-or-UI outcome. Hence, most of the incrementally nonemployed from severely shocked areas had been laid off.

The preceding findings may suggest that the initial residents of severely shocked areas suffered higher rates of 2015 nonemployment purely because those residents were laid off from their initial jobs at higher rates. Two pieces of evidence suggest that that is not the case. First, column 8 replicates the main specification while controlling for the indicator of ever receiving UI 2007–14. The Great Recession local-shock coefficient is barely attenuated, indicating that effects hold within layoff status. It is easy to see why quantitatively: multiplying the coefficient on UI receipt (−2.73) by the higher rate of UI receipt (1.43) yields the 0.04 percentage point attenuation in the main estimate. Layoff status is endogenous, but with homogeneous layoff effects and under the mild monotonicity condition that the marginally laid off in severely shocked areas had weakly better unobservables (Gibbons and Katz 1991), this result suggests that layoff effects are not nearly large enough for a 1.43 percentage point higher layoff rate to explain the 2015 employment effect.[31]

More simply, figure 5*B* replicates figure 4*A* in the mass-layoffs sample—thereby comparing the employment rates of workers who were displaced in 2008–9 mass-layoff events at the same age, in the same initial earnings bin, and in the same initial industry but in different local areas. If layoff effects were homogenous across space and severely shocked areas simply experienced more layoffs—and if the incrementally displaced in severely shocked areas were no worse on unobservables (Gibbons and Katz)—then one would not find a negative 2015 employment effect in the mass-layoffs sample. Yet figure 5*B* shows that individuals in the mass-layoffs sample were 0.577 percentage points less likely to be employed in 2015 for every 1 percentage point higher Great Recession local shock. This result (also reported in table 6, col. 3) is robust to restricting the sample to mass-layoff firms with valid industry codes, including those outside manufacturing and construction (cols. 4–5). This evidence suggests that this paper's 2015 nonemployment effects are not explained purely by higher layoff rates and parallels work on larger earnings impacts in weak local (Jacobson et al. 1993) and aggregate (Davis and von Wachter 2011) labor markets.

[31] Under homogenous layoff effects and the monotonicity condition, the UI receipt coefficient weakly overstates the layoff effect for workers from severely shocked areas, suggesting that the magnitude of the shock coefficient is a lower bound on the true effect net of the layoff effect.

Columns 9–11 of table 6 return to the main sample to document continuous nonemployment trajectories. Column 9 replicates the main specification for the outcome of an indicator for whether the individual was nonemployed for any year 2007–14, minus an indicator for whether the individual was nonemployed for any year 1999–2006. The estimated coefficient implies that Great Recession local shocks resulted in the average working-age American being 0.487 percentage points more likely to experience a full year of nonemployment in 2007–14. Column 10 shows that pre-2015 nonemployment statistically explains 2015 nonemployment: there is no statistically significant correlation between Great Recession local shocks and 2015 relative employment once one controls additively for indicators of employment in each year 2007–12. Column 11 replicates the main specification for the outcome of an indicator for whether the individual was employed in any year 2013–15, minus the individual's mean employment in 1999–2006. The estimate is −0.285, with a *t*-statistic of 2.8. Thus, the point estimates in columns 9–11 suggest that 2015 employment impacts typically followed an annual nonemployment impact in 2007–12 and constituted a third consecutive year of nonemployment impacts.

VIII. Discussion of Mechanisms

The previous section found that one candidate channel—higher layoff rates with homogenous layoff effects—likely does not explain the paper's results. I close with brief further discussion of six candidate mechanisms: reduced migration, higher reservation wages on SSDI, lost job-specific rents, lost firm-specific human capital, general human capital decay, and persistently low labor demand. Higher reservation wages on SSDI could explain a sizable portion of the impact. General human capital decay and persistently low labor demand could explain the full impact.

Reduced migration could in principle explain long-term impacts of Great Recession local shocks, relative to other local shocks, but does not appear to do so. First, and despite the possibility of negative-equity mortgages impeding homeowner out-migration (Ferreira, Gyourko, and Tracy 2010, 2011), several papers have argued that the Great Recession did not impede migration (Farber 2012; Schulhofer-Wohl 2012; Valletta 2013; Şahin et al. 2014). Second, Section II found that 2007–15 population reallocation was in line with historical responses. Third, figure 6 found that impact heterogeneity across demographic groups was not systematically correlated with group migration rates—consistent with, though not dispositive of, a hypothetically higher out-migration rate attenuating little incidence.

The Great Recession could have induced individuals to supplement their income with SSDI—a costly-to-obtain but typically permanent location-independent income stream—thereby raising their reservation wages and potentially reducing their employment (Autor and Duggan 2003; Maestas, Mullen, and Strand 2013).[32] Reservation wages are not observed, but the SSDI channel appears to explain a minority of the results. Column 1 of table 6 estimated that Great Recession local shocks caused insignificantly higher 2015 SSDI enrollment, with a point estimate equal to one-fifth of the main employment effect, while column 2 estimated that one-third of the incrementally nonemployed were on SSDI in 2015, though with substantial standard errors. An important caveat is that applying for SSDI effectively requires nonemployment, so the data are consistent with strategic labor force exit in order to apply for benefits, even if strategic application was unsuccessful through 2015.

Laid-off workers will choose to remain nonemployed after losing a high-paying rent-sharing job if subsequent wage offers lie below the worker's reservation wage.[33] Lost job-specific rents do not appear to explain the results. First, the 2015 impact of Great Recession local shocks is large not only in the main sample but also in the retail chain sample—even though retail is a canonical low-rent industry (Krueger and Summers 1988; Katz and Summers 1989; Murphy and Topel 1990; Gibbons and Katz 1991). Second, figure 6 showed that the largest impacts were suffered by those with the lowest initial earnings, where the scope for rents was likely low. Third and most simply, the same figure shows that the estimated 2015 nonemployment impact appears large for those who had no earnings at all in 2006 and thus certainly had been earning no rents.

A worker losing a job for which she had firm-specific human capital will choose to remain nonemployed if the worker's marginal product—and thus wage at the next-best firm—lies below her reservation wage (Topel 1990; Jacobson et al. 1993). Lost firm-specific human capital at layoff does not appear to explain the results, as the quantitative results remain essentially intact within displaced individuals (table 6, cols. 5–8; fig. 5*B*). The incrementally laid off in severely shocked areas seem likely to have had weakly better unobservables, as employers may lay off the unobservably worst workers first (Gibbons and Katz 1991). Hence, heterogeneous layoff effects—with the incrementally laid off being less resilient to their loss of firm-specific human capital—appear necessary for the firm-specific human capital channel to explain the results.

The results appear consistent with general human capital decay and persistently low labor demand. Severe recessions generate relatively long

[32] Recipients forfeit their income streams if they return to substantial work.

[33] Motivated by the union wage premium (e.g., Lewis 1963; Farber 1986), some empirical commentary has interpreted postlayoff earnings losses as reflecting workers' loss of rents in an initial job that paid above the worker's marginal product (e.g., Hall 2011). Preexisting contrary evidence includes that postlayoff earnings losses are large and significant across both high-apparent-rent and low-apparent-rent industries (Jacobson et al. 1993).

nonemployment spells, and general human capital can decay during such spells (Phelps 1972), leading to persistent nonemployment (Pissarides 1992), such as via job ladders with serially correlated unemployment spells (Jarosch 2015). For example, workers may fail to keep up with new technologies or preserve good habits such as punctuality and then may choose nonparticipation over lower-wage employment (Juhn, Murphy, and Topel 1991, 2002). Consistent with general human capital decay, columns 1 and 9–11 of table 6 found that incremental 2015 nonemployment typically followed a layoff and multiple years of nonemployment in 2007–14. General human capital decay after a prolonged nonemployment spell is also consistent with earlier findings of reduced long-term earnings after mass layoffs in weak local (Jacobson et al. 1993) and aggregate (Davis and von Wachter 2011) labor markets.

Alternatively, severely shocked areas may have experienced persistently low labor demand, and mobility frictions may have resulted in initial residents bearing the incidence (Kline 2010; Moretti 2011).[34] First, local Great Recession–driving processes (e.g., spending contractions or productivity shocks) could have been persistent, causing wages to fall below reservation wages (Hall 1992; Kline and Moretti 2014).[35] Second, transitory Great Recession local shocks may have reduced the opportunity costs for agents to respond to exogenous nationwide skill-biased or routine-biased technical progress (Jaimovich and Siu [2012], with recent empirical support from Hershbein and Kahn [2016])—accelerating wage declines below reservation wages. Third, transitory Great Recession local shocks may have moved local areas to low-employment equilibria (Diamond 1982; Blanchard and Summers 1986; Benhabib and Farmer 1994; Christiano and Harrison 1999; Eggertsson and Krugman 2012; Kaplan and Menzio 2014), where neither workers nor firms search for job matches even though both would gain from trade at the prevailing wage. To the extent that the first and last cases are reversible, they are consistent with "hidden slack" among labor force nonparticipants. All three cases are consistent with the results, including the result that most of the incrementally nonemployed had been laid off. Laid-off workers may be exactly those individuals whose wages fell below reservation wages, or layoffs may have determined which specific workers lost out in the new equilibrium without determining how many did.

[34] Every decennial census shows that over two-thirds of adults live in their birth state (Molloy et al. 2011). The Health and Retirement Study shows that half of adults live within 18 miles of their mothers (http://www.nytimes.com/interactive/2015/12/24/upshot/24up-family.html).

[35] For example, with only nontradable production, net worth contractions (Mian and Sufi 2014) could have permanently caused residents to shift to home production (Benhabib, Rogerson, and Wright 1991; Aguiar and Hurst 2005), reducing per capita local labor demand and equilibrium wages below reservation wages.

General human capital decay leaves individuals changed ("scarred"), while persistently low labor demand leaves local areas changed. Thus, unique to the persistently-low-labor-demand mechanism, exogenously moving individuals in 2015 from severely shocked to mildly shocked areas may increase their employment rate. Future work could therefore distinguish between these mechanisms, with strong migration instruments that generate quasi-experimental variation in individuals' 2015 local areas.[36]

IX. Conclusion

This paper used local labor markets as a laboratory to test for long-term employment impacts of the Great Recession. The central finding is that exposure to a severe local Great Recession caused working-age Americans to be substantially less likely to be employed at all in 2015, despite recovery in the local unemployment rate. This finding contrasts with the conventional view that a business cycle's employment impacts cease once unemployment recovers. Instead, the Great Recession altered unemployment-constant employment.[37]

The paper highlights five areas for future work. First, the results suggest the importance of allowing for labor force exit in models of macroeconomic fluctuations (Mortensen and Pissarides 1999).[38] Such models have the potential to show countercyclical policies to be relatively fiscally inexpensive or even self-financing, to the extent that they persistently increase employment and earnings and thereby tax revenue (DeLong and Summers 2012). Second, higher layoff rates do not appear to explain the results, as the impacts hold within a sample of laid-off individuals, pointing instead to interactions with area-level economic conditions. Future analyses could further test among mechanisms. Third, new work could investigate whether previous recessions also depressed long-term employment.

Fourth, naive extrapolation of the paper's local-shock-based estimate to the aggregate would suggest that the Great Recession caused over half

[36] In addition, there are other potential mechanisms. For example, workers may have made costly investments, such as moving in with one's parents (Kaplan 2012) or honing leisure skills (Aguiar et al. 2017), during nonemployment spells that then induced labor supply contraction. However, nominal and real hourly wages declined rather than rose in severely shocked areas (Beraja et al. 2016). Employers may have inferred low unobserved productivity from long nonemployment spells, though such inference was smallest in severely shocked areas (Kroft, Lange, and Notowidigdo 2013).

[37] This conclusion includes the possibility that the recession ended a period of unsustainable unemployment-constant employment, returning the economy to its pre-2000s trend (Charles et al. 2016).

[38] From Mortensen and Pissarides (1999, 1173): "Despite a flurry of activity [in search and matching theory] since [the early 1980s], there are still many important questions that are unexplored. One such question is the dynamics of worker movement in and out of the labor force. . . . Virtually all search equilibrium models assume an exogenous labor force." Pissarides (1992) models search intensity that declines with unemployment duration.

of the 2007–15 age-adjusted decline in US working-age employment. Subsequent analysis could determine whether the true aggregate impact was less or more. Finally, employment impacts through 2015 do not imply employment impacts forever, and it will be valuable to estimate and explain subsequent dynamics. For example, the age-25–54 US headline employment rate rose 1.6 percentage points from the beginning of 2016 to the end of 2017, primarily via labor force entry. This upward 2016–17 employment trend could reflect continued cyclical recovery, with there having been "hidden slack" among labor force nonparticipants who subsequently reentered the labor force in 2016–17 (Bell and Blanchflower, forthcoming). The hidden-slack explanation is consistent with low wage growth in 2016–17 and would indicate that the Great Recession's recovery extended anomalously long relative to previous cycles (cf. Fernald et al. 2017). Alternatively, it is possible that the recent upward employment trend reflects up-skilling, a new positive aggregate shock, or another force.

Appendix

The working-age population (ages 30–49 in any given year) has grown older over time because of the baby boom. To account for this age distribution change, the extrapolation exercise of Section IV.D uses the following formula to compute each element in the summation $(1/5)\Sigma_{t=2003}^{2007}\Delta\bar{e}_t$:

$$\Delta\bar{e}_t = \sum_{c\in C_t}\lambda_{ct}\left(E[\text{EMPLOYED}_{it}|c(i) = c] - \frac{1}{8}\sum_{s=t-16}^{t-9}E[\text{EMPLOYED}_{is}|c(i) = c]\right),$$

where C_t is the set of birth cohorts $\{t-58, \ldots, t-39\}$ and the cohort-reweighting term λ_{ct} is the population share of cohort $c + 2015 - t$ in C_{2015} in the March 2016 ASEC. When computing $\Delta\bar{e}_t$ without cohort reweighting and thus simply as $E[\text{EMPLOYED}_{it}|c(i) \in C_t] - (1/8)\Sigma_{s=t-16}^{t-9}E[\text{EMPLOYED}_{is}|c(i) \in C_t]$, one obtains $(1/5)\Sigma_{t=2003}^{2007}\Delta\bar{e}_t = -4.22$ instead of the -4.83 percentage points reported in the text—which would reduce the exercise's final estimate from 76 percent to 61 percent. ASEC data are limited to the civilian noninstitutional population. The variable EMPLOYED_{it} in ASEC is defined as an affirmative answer to either of two questions asked of respondents in March of $t + 1$: "Did [person] work at a job or business at any time during [t]?" and "Did [person] do any temporary, part-time, or seasonal work even for a few days during [t]?"

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ONLINE APPENDIX

Online Appendix A: Age-Adjusted U.S. Labor Force Statistics

Online Appendix Figure A.1 uses monthly Current Population Surveys (CPS) to plot unadjusted and non-parametrically age-adjusted annual U.S. employment rates (employment-population ratios), labor force participation rates, and unemployment rates 2007-2015. The figure's unadjusted statistics are essentially equal to the official Bureau of Labor Statistics U.S. labor force statistics. They are constructed as follows using the civilian non-institutional population, first for the age-16+ population and then separately for the age-25-54 population. For each age-year and top-coding age at 79, I compute the population-weighted mean across CPS months of total unemployed, labor force, and population counts. Then to construct the unadjusted year-t data points, I sum year-t's unemployed, labor force, and population counts across ages and use those totals to compute the plotted rates. To construct each age-adjusted year-t data point, I first reweight year-t's age-specific counts by the age's 2007 population share as in DiNardo, Fortin and Lemieux (1996):

$$\hat{X}_{at} = \frac{P_{a2007}}{\sum_a P_{a2007}} \frac{\sum_a P_{at}}{P_{at}} X_{at}$$

where a denotes age, X denotes an unemployed, labor force, or population (P) count, and $\hat{X}_{at}$ denotes a reweighted count. The weight $\frac{P_{a2007}}{\sum_a P_{a2007}} \frac{\sum_a P_{at}}{P_{at}}$ ranges empirically from 0.63 to 1.25, depending on whether the age's population share rose or fell between 2007 and t. I then sum the reweighted unemployed, labor force, and population counts across ages and use those totals to compute the plotted age-adjusted rates.

The results displayed in Online Appendix Figure A.1 reveal that the demographic compositional change of population aging explains a minority (1.6 percentage points) of the overall employment rate decline (3.6 percentage points) 2007-2015. The figure also shows that population aging explains essentially none (0.1 percentage points) of the age-25-54 employment rate decline (2.6 percentage points).

These results closely match the results of Shimer (2014). Shimer analyzes data through 2014 and finds, like I do, that aging through 2014 explains a minority of the age-16+ employment rate decline and essentially none of the age-25-54 decline. He also finds that adding other demographic controls actually deepens the employment rate decline. Eppsteiner, Furman and Powell (2017) similarly find that either just under or just over half of the 2007-2015 age-16+ employment rate decline is explained by aging, depending on the measurement point in 2015.

To further discuss related literature, the recent and transparent contribution of Krueger (2017) illuminates the uncertainty in projecting pre-recession *trends* rather than just controlling for demographic compositional changes like population aging. He finds that if one projects the downward trends in labor force participation during the 1997-2006 period forward to 2017, one can explain the entire decline in labor force participation through 2017 (though 2015 participation was still unusually low). However, 1997 was the peak in the U.S. labor force participation rate, so 1997-2006 participation trends were negative. Krueger reports that if one instead uses the *1992*-2006 period to estimate pre-recession trends, those trends are mostly flat. Thus the explanatory power of pre-recession trends depends substantially on the choice of the trend estimation period.

In a simultaneous equation system with many variables, Aaronson, Cajner, Fallick, Galbis-Reig, Smith and Wascher (2014) find, like Krueger's 1997-2006-based projection, that most of the age-adjusted labor force participation rate decline can be explained by projecting pre-recession trends. However, Hall (2014) suggests that the ACFGSW specification is likely to be sensitive to reasonable amendments, and at least two projections have proven too aggressive. The earlier similar analysis of Aaronson, Fallick, Figura, Pingle and Wascher (2006) projected such a large labor force participation rate decline that the actual U.S. participation rate exceeded their projection after 2014 (ACFGSW Figure 1). Similarly, ACFGSW projected continued and substantial age-16+ labor force participation declines after the second quarter of 2014 (their Table 6) while the actual age-16+ participation rate remained constant between then and the time of this writing (the second quarter of 2017).

All told, this appendix's analysis finds that the demographic compositional change of population aging explains a minority of the overall employment rate decline 2007-2015 and essentially none of the age-25-54 decline. A review of other work suggests that the explanatory power and accuracy of pre-recession trends depend substantially on the trend estimation specification. This paper's main finding supports the view that 2015 employment rates would have been higher in lieu of the Great Recession.

Online Appendix B: State-Level Shocks

This online appendix extensively details the autoregressive system of Blanchard and Katz (1992, BK) used to define state-level Great Recession shocks in Section 2 and elaborates on Figure 2's historical comparisons of state-level shock adjustment. The online replication kit contains data and code that generate the results.

Section 2 estimates state-level adjustment using the updated data used in BK: the annual Local Area Unemployment Statistics (LAUS) series of employment, population, unemployment, and labor force participation counts 1976-2015 for 51 states (the 50 states plus the District of Columbia) produced by the Bureau of Labor Statistics (BLS).[38] Variable definitions are standard and pertain to the age-16-and-over civilian noninstitutional population.[39] BLS compiles LAUS counts from the Current Population Survey (CPS), Current Employment Statistics (CES) survey, and state administrative unemployment insurance counts—blended to filter maximal signal from noise using empirical Bayes techniques.[40] Online Appendix Table 3 displays summary statistics.

I employ BK's canonical empirical model of state labor market outcomes to compute Great

[38]LAUS are the official data used to allocate federal transfers across states. The series is limited historically by the lack of Current Population Survey participation statistics for most states prior to 1976.

[39]Age is defined at the time of survey; LAUS figures effectively evenly weight underlying monthly surveys. See http://www.bls.gov/bls/glossary.htm for full definitions of labor force status. Employment is roughly defined as working for pay or being temporarily absent from regular work at any point in the reference week. Unemployment is roughly defined as having had no employment in the reference week but being available for work and having looked for work in the preceding month. Labor force equals employment plus unemployment.

[40]Since LAUS had not yet been produced, BK effectively constructed their own version of LAUS 1976-1990 using the Geographic Profile of Employment (comprising CPS unemployment and population counts), employment counts from the CES (comprising formal employment counts), and an ad-hoc CPS-based imputation for self-employment (population was implied). LAUS-based results on the original BK time series are essentially identical to BK's published results.

Recession employment shocks for each state. BK imagine a simple spatial equilibrium in which U.S. states experience one-time random-walk shocks to global demand for their locally produced and freely traded goods. Those shocks induce endogenous migration responses of workers and firms via transitory wage changes until state employment rates return to their steady states. BK aimed to estimate the nature and speed of those responses: do workers move out or do jobs move in, and over what horizon? To guide their implementation, BK observe empirically that states differ in long-run employment and population growth rates (e.g. perhaps partly due to steady improvements in air conditioning that made the Sun Belt steadily more attractive) and in long-run unemployment rates and participation rates (e.g. due to industrial mix and retiree population differences) relative to the national aggregate. Thus an attractive model of the evolution of state labor market outcomes may feature stationary employment growth, a stationary unemployment rate, and a stationary participation rate (and thus a stationary employment rate) for each state relative to the corresponding national aggregates.

BK implement such a model. They characterize state adjustment to idiosyncratic state-level labor demand shocks by estimating the following log-linear autoregressive system in relative state employment growth, unemployment rates, and participation rates:

$$\begin{aligned}
\widetilde{\Delta \ln E}_{st} &= \alpha_{s10} + \alpha_{11}(2)\,\widetilde{\Delta \ln E}_{s,t-1} + \alpha_{12}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{13}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon^{E}_{st} \\
\widetilde{\ln E/L}_{st} &= \alpha_{s20} + \alpha_{21}(2)\,\widetilde{\Delta \ln E}_{st} + \alpha_{22}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{23}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon^{E/L}_{st} \\
\widetilde{\ln L/P}_{st} &= \alpha_{s30} + \alpha_{31}(2)\,\widetilde{\Delta \ln E}_{st} + \alpha_{32}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{33}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon^{L/P}_{st}
\end{aligned}$$

where E, L, and P denote levels of employment, the labor force, and population in state s in year t; where Δ denotes a first difference (year t's value minus year $t-1$'s value); where $\tilde{}$ denotes a difference relative to the year's national aggregate value; and where (2) denotes a vector of two lags. Thus the first dependent variable ("relative state employment") is the first difference of log state employment minus the first difference of log aggregate employment. The second ("relative state unemployment") is the log of one minus the state unemployment rate minus the log of one minus the aggregate unemployment rate. The third ("relative state participation") is the log of the state participation rate minus the log of the aggregate participation rate. Relative state population is the implied residual. Each equation includes a state fixed effect. I follow BK in weighting states equally rather than by population. Under these assumptions, the autoregressive coefficients characterize the speed of the average state's convergence to its steady state following an unforecasted change in state labor demand: coefficients close to one imply slow convergence while coefficients close to zero imply fast convergence.[41]

[41] The BK system embodies four substantive assumptions. First, unforecasted changes in relative state employment growth ε^{E}_{st} affect contemporaneous relative employment growth, relative unemployment, and relative participation, but unforecasted changes in relative state unemployment and participation do not effect contemporaneous values of the other outcomes. This feature allows the system to be estimated independently via ordinary least squares. It reflects the assumption that ε^{E}_{st} primarily reflects changes in labor demand rather than supply—supported in the data by negative values of ε^{E}_{st} typically being followed by state wage declines rather than increases. Second, each state-year outcome is differenced by the year's aggregate value, so the behavior of the system is assumed to be independent of aggregate levels. Third, serial correlation is assumed to be affine in two lags, which limits the estimation sample to years 1978 and beyond (three and four lags deliver similar results). Fourth, outcomes are assumed to be stationary, i.e. to converge in the long run to time-invariant state-specific steady-state values relative to national aggregates. State fixed effects are motivated by cross-decadal persistence in the outcomes. Formal stationarity tests are underpowered and inconclusive in

For each state, I estimate a 2008 and a 2009 employment growth forecast error within the BK system and refer to their sum as its Great Recession employment shock. Specifically, I first estimate the BK system coefficients using sample years 1978-2007 (i.e. using years 1976-2007 with two lags). I then compute each state's 2008 employment shock $\widehat{\varepsilon^E_{s,2008}}$, equal to the state's actual relative employment growth $\widetilde{\Delta \ln} E_{s,2008}$ minus the relative employment growth predicted by the state's actual data through 2007 and the estimated coefficients. For example, a state that experienced 2008 relative employment growth equal to the system forecast based on its history through 2007 would have a 2008 shock equal to zero. I similarly compute each state's 2009 employment shock $\widehat{\varepsilon^E_{s,2009}}$, equal to the state's actual relative employment growth $\widetilde{\Delta \ln} E_{s,2009}$ minus the relative employment growth predicted by the state's actual data through 2008 and the estimated coefficients. I refer to each state's vector $\{\widehat{\varepsilon^E_{s,2008}}, \widehat{\varepsilon^E_{s,2009}}\}$ as the state's Great Recession employment shocks and to the sum of the vector's elements as the state's Great Recession employment shock.

To understand these shocks empirically, Online Appendix Table 4 lists each state's Great Recession employment shock. The standard deviation of state-level shocks over the Great Recession (2.74) was similar to the standard deviation of state-level shocks over the early-1980s (1980-1982) recession (2.73) computed similarly (detailed below).[42] Recall that shocks are effectively defined as 2007-2009 employment level changes *relative to* the state's own trend and the national aggregate. Thus a state can have a negative Great Recession employment shock either because its employment growth relative to the aggregate became moderately negative after a history of fast growth (e.g. Arizona) or because employment growth became very negative after a history of slow growth (e.g. Michigan). Furthermore, just over half of states naturally experienced a positive Great Recession shock, since shocks are measured relative to the aggregate. The table displays patterns familiar from popular news accounts and earlier economics work: Sun Belt states like Arizona, California, and Florida as well as Rust Belt states like Michigan and Indiana experienced severe Great Recession shocks relative to other states. As two focal examples, Arizona's shock equals -2.24% while Texas's shock equals $+1.30\%$.

Online Appendix Figures A.4A-B plot actual mean responses (solid lines) of state labor market outcomes to 2007-2009 shocks versus mean historical benchmark responses (dotted lines) to a -1% shock, following BK's exposition. Forty-one percent of the average state's 2007-2009 shock arrived in 2008 while 59% arrived in 2009. To generate historical benchmark predicted responses 2008-2015, I therefore feed the BK system the employment residual vector $\{\widehat{\varepsilon^E_{s,2008}}, \widehat{\varepsilon^E_{s,2009}}\} = \{-.41, -.59\}$ and—for maximum comparability to BK's original benchmarks—use coefficients estimated on the original sample years 1978-1990; panel C plots updated benchmarks.[43] Note that by construction in the BK system, the predicted mean response to a -1% shock is the negative of the predicted mean response to a $+1\%$ shock.[44]

short time series (BK). Stationarity here is best motivated by spatial arbitrage priors, no rise in the standard deviation of outcomes before 2007, and employment rate stationarity after previous recessions (Figure 2A).

[42]The standard deviation of shocks is smaller outside aggregate recession years.

[43]Strictly speaking, I feed the system the vector $\{-.41, -.59\}$ shrunk multiplicatively by a constant such that the 2007-2009 change in relative employment is -1% after system feedback effects.

[44]Online Appendix Figures A.4A-B correspond closely to BK's Figure 7, with year 2009 corresponding to year negative one in BK's Figure 7. The graphs differ in that the -1% shock in Online Appendix Figures A.4A-B is spread out over two years instead of one and in that Online Appendix Figures A.4A-B use the official LAUS series that was released after BK.

Panel A's benchmark predictions depict BK's core lesson: in response to a -1% change in a state's employment relative to the state's trend and the national aggregate, the 1978-1990 experience predicts that the state's population would rapidly fall by 1% relative to the state's trend and the national aggregate—such that the state's employment rate returns to its steady-state level relative to the aggregate in five years. Colloquially, residents move out and others stop moving in, rather than jobs moving in or residents remaining nonemployed—and the adjustment completes quickly. Economically, the adjustment process has been understood to embody a simple mechanism: a state (e.g. Michigan) experiences a one-time random-walk contraction in global consumer demand for its locally produced traded good (e.g. cars), which induces a local labor demand contraction and wage decline, which in turn induces a local labor supply (population) contraction, which then restores the original local wage and employment rate.

The mean actual response series equals the estimated mean responses of outcomes across states within each year. To construct the series, I first compute forecast errors for each year 2008-2015 and for each system outcome, using actual data through 2007 and the coefficients from the 1978-2007-estimated system.[45] Denote these forecast errors for each variable-state-year $\{\eta^E_{st}, \eta^{E/L}_{st}, \eta^{L/P}_{st}\}$. I then regress these forecast errors on 2007-2009 shocks in year-by-year regressions:[46]

$$\begin{aligned}
\eta^E_{st} &= \widehat{\varepsilon^E_{s,2008}}\delta^E_t + \widehat{\varepsilon^E_{s,2009}}\zeta^E_t,\ \forall t \\
\eta^{E/L}_{st} &= \widehat{\varepsilon^E_{s,2008}}\delta^{E/L}_t + \widehat{\varepsilon^E_{s,2009}}\zeta^{E/L}_t,\ \forall t \\
\eta^{L/P}_{st} &= \widehat{\varepsilon^E_{s,2008}}\delta^{L/P}_t + \widehat{\varepsilon^E_{s,2009}}\zeta^{L/P}_t,\ \forall t
\end{aligned}$$

This specification is flexible in that it allows for the 2008 and 2009 employment shocks to have arbitrary additive effects on each subsequent year's outcomes. The δ and ζ coefficients are mean actual responses of each outcome in each year to 2007-2009 shocks. I multiply these coefficients by the -1% 2007-2009 shock $\{\widehat{\varepsilon^E_{s,2008}}, \widehat{\varepsilon^E_{s,2009}}\} = \{-.41, -.59\}$ to obtain the plotted mean actual response series.

Panel A shows that on a slight lag, mean actual relative population responded identically to Great Recession shocks as in the historical benchmark—falling by 1% between 2007 and 2014, matching the initial 1% employment decline. However, actual relative employment kept declining such that employment rates remained diverged across space at nearly their 2009 levels: for every -1% decline in relative state employment 2007-2009, the relative state employment rate was 0.45 percentage points lower in 2015 than it was in 2007. This 0.45 percentage-point employment rate deficit is similar to the 0.48 percentage-point deficit that prevailed in 2009. Hence, employment rates had far from converged across space by 2015, contrary to history-based predictions. Panel B separates the employment rate response into the unemployment rate response and the labor force participation rate response. The graph shows that actual relative unemployment rates converged across space as in the historical benchmark, while actual participation rates remained diverged in a stark departure from the historical benchmark.

Online Appendix Figure A.4C shows that updating the historical benchmark to more recent

[45] That is, I compute 2008-2015 baseline predictions for how each state's outcomes would have evolved in the absence of 2007-2009 shocks based on data through 2007 and the estimated coefficients, and then subtract predictions those 2008-2015 baseline predictions from actual 2008-2015 values.

[46] For 2008, only the 2008 employment shock is included as a regressor.

data does not alter the conclusion that post-2007 employment rate convergence was unusually slow and incomplete. The figure plots the estimated response of the average state's employment rate to a -1% employment shock, based on estimating the BK system on three different LAUS sample ranges: 1978-1990 (the original BK time range, reprinted from panels A-B), 1991-2007, and 1978-2015.[47] Both the 1978-1990- and 1991-2007-based predictions exhibit five-year convergence of the state's employment rate to its steady-state level relative to the the aggregate. The 1978-2015-based prediction exhibits substantially slower convergence but still exhibits majority employment rate convergence 2009-2015, in contrast to reality.[48] Hence, the 2007-2015 employment rate divergence is exceptional even relative to fully-updated convergence predictions.

Finally, Figure 2A (documented in the main text) shows that the slow convergence after Great Recession shocks was unusual not merely relative to average historical responses but also relative to the aftermath of the two previous recessions for which a long post-recession time series is available.[49] Online Appendix Figure A.5 repeats Figure 2A for the labor force participation rate, unemployment rate, employment growth, and population growth. The participation and unemployment graphs are constructed exactly as the employment rate graph in Figure 2A. To create the employment growth graph, I first compute each state's steady state value for relative employment growth by using the 1978-2007-estimated BK coefficients and solving the BK system assuming all variables are constant. Then, for each state-year around each recession, I compute actual relative employment growth minus steady state relative employment growth, cumulate this value beginning in year -5 (year -3 for the early-1980s recession, the first year available), and proceed to construct the graph exactly as done for employment rates. The population employment graph is constructed similarly. The graphs show that the unemployment rate and population growth adjustments to Great Recession employment shocks were broadly similar to those after previous recessions' shocks. However, the participation rate and employment growth in severely shocked states exhibited no recovery from 2009 levels relative to mildly shocked states, in stark contrast to the aftermath of the early-1980s and early-1990s recessions.

Online Appendix C: Cross-State Employment Gap

This online appendix documents the computation of the 2.01 million cross-state employment gap statistic reported in Section 2. The employment gap is defined as total 2015 employment in severely shocked states minus total 2015 employment in mildly shocked states—minus the difference that would have prevailed if the pre-recession severe-mild employment rate difference had prevailed in 2015 at 2015 state populations. $2.01 \approx .01635/2 \times 250.5 = 2.05$, where 1.635 percentage points is the population-weighted equivalent to the 1.736-percentage-point severe-mild 2015 employment rate deficit plotted in Figure 1B and where 250.5 million was 2015 total population. This formula is not exact because population was not exactly evenly divided

[47] See Beyer and Smets (2014) for an earlier re-estimation of the BK system augmented with multi-level factor modeling to compare U.S. and Europe population responses. See Dao, Furceri and Loungani (2017) for an earlier re-estimation augmented with instruments to find stronger population responses during aggregate recessions.

[48] Slower convergence likely derives from unique divergence after 2007 as well as from alleviated small-sample stationarity bias in a larger sample (e.g. Hurwicz 1950).

[49] The post-2001-recession experience exhibited incomplete convergence before being interrupted by positively correlated 2007-2009 shocks.

between the two state groups, since the unweighted shock median was used to define the groups. For reference, the exact 2.01 figure is computed as follows.

On average 2002-2007, the population-weighted employment rate in severely shocked states minus that in mildly shocked states equaled -0.885 percentage points. In 2015, severely shocked states had an adult civilian noninstitutional population of 142.0 million with a 58.25% population-weighted employment rate while mildly shocked states had an adult civilian noninstitutional population of 108.5 million with a 60.77% population-weighted employment rate (note that $58.25 - 60.77 + 0.885 = -1.635$). Then the full-convergence employment rate in severely shocked states (e^*_S) and in mildly shocked states (e^*_M) solve:

$$e^*_S - e^*_M = -.00885$$
$$142.0 \times e^*_S + 108.5 \times e^*_M = 142.0 \times .5825 + 108.5 \times .6077$$

where the first equation imposes full employment rate convergence between severely shocked and mildly shocked states to the pre-2007 difference and the second equation imposes equality between the full-convergence aggregate employment level (and rate) and the actual 2015 aggregate employment level (and rate).

The solution is $e^*_S = 58.96\%$ and $e^*_M = 59.84\%$. This implies that 1.005 ($= 141.99 \times (.58959 - .58251)$) million fewer residents of severely shocked states in 2015 were employed than there would have been had state employment rates returned to their pre-2007 differences at actual 2015 populations and the actual 2015 aggregate employment rate. Likewise, 1.006 ($= 108.52 \times (.60771 - .59844)$) million more residents of mildly shocked states in 2015 were employed than there would have been had state employment rates returned to their pre-2007 differences around the actual 2015 aggregate employment rate. Hence relative to the counterfactual of full convergence of state employment rates to their pre-2007 differences at actual 2015 populations, a 2.01-million-person employment gap between severely shocked and mildly shocked states remained in 2015.

Online Appendix D: Empirical Design in Potential Outcomes

This online appendix details a binary version of the paper's empirical design in potential outcomes. Consider identical local areas c that experienced in 2007-2009 a binary Great Recession local shock—severe or mild—and no other 2007-2009 shocks. Denote these areas severely shocked or mildly shocked. $SEVERE_{c(i2007)} \in \{0, 1\}$ indicates whether an individual i lived in 2007 in a severely shocked area. $EMPLOYED_{i2015}(1) \in \{0, 1\}$ indicates i's potential 2015 employment if her 2007 local area was severely shocked, and $EMPLOYED_{i2015}(0) \in \{0, 1\}$ indicates i's potential 2015 employment if her 2007 local area was mildly shocked. To align notation simply with the text's main outcome, assume that all individuals were employed in every year 1999-2006.

Define the causal effect β_i of i's 2007 CZ's Great Recession local shock on i's 2015 employment as the difference in the worker's potential employment: $\beta_i \equiv EMPLOYED_{i2015}(1) - EMPLOYED_{i2015}(0)$. Note that β_i could be zero for some skill types and negative for others, for example if only construction or routine workers are affected by a severe local shock (Jaimovich and Siu 2013, Hershbein and Kahn 2016). But β_i excludes all nationwide changes that do not vary with local shock severity. The mean $E[\beta_i] \equiv \beta$ in a relevant sample of workers is my causal effect of interest, which I refer to as the causal effect of Great Recession local shocks.

If workers were randomly assigned in 2007 across local areas, then one could consistently estimate β as the unconditional observed employment rate difference in longitudinal data as: $E[EMPLOYED_{i2015}|SEVERE_{c(i2007)} = 1] - E[EMPLOYED_{i2015}|SEVERE_{c(i2007)} = 0]$. Lacking random assignment, I assume empirically that workers were as good as randomly assigned conditional on a rich observed vector of pre-2007 characteristics $\mathbf{X}_{i2006}$:

$$\Big(EMPLOYED_{i2015}(0), EMPLOYED_{i2015}(1)\Big) \perp SEVERE_{c(i2007)} \mid \mathbf{X}_{i2007c(i2007)}$$

Then β can be consistently estimated as the conditional observed employment rate difference in longitudinal data:

$$\begin{aligned} &E[EMPLOYED_{i2015}|SEVERE_{c(i2007)} = 1, \mathbf{X}_{i2007c(i2007)}] - \\ &E[EMPLOYED_{i2015}|SEVERE_{c(i2007)} = 0, \mathbf{X}_{i2007c(i2007)}] - \\ &\quad = E[EMPLOYED_{i2015}(1) - EMPLOYED_{i2015}(0)]. \end{aligned}$$

Two appendix graphs help to justify the interpretation of β presented in Section 3.1. My interpretation of β is sensible because severely shocked and mildly shocked areas exhibited relatively similar pre-recession trends in the outcomes of interest (shown in Section 4.1) and because post-2010 unemployment rates converged monotonically across severely shocked and mildly shocked areas (Figure 2A and Online Appendix Figure A.2A). Moreover, adverse 2010-2015 industry-based shift-share shocks are not positively correlated with adverse Great Recession local shocks (Online Appendix Figure A.2B).

Online Appendix E: Longitudinal Data

This online appendix provides additional details on the longitudinal linked-employer-employee data described in Section 3.

First, the universe of business tax returns used is the universe of C-corporate (Form 1120), S-corporate (Form 1120S), and partnership (Form 1065) tax returns. Businesses that file other types of tax returns employ a small share of U.S. workers.

Second, Form 1099-MISC data on independent contractor employment are missing in 1999. Results are very similar when omitting 1999 data.

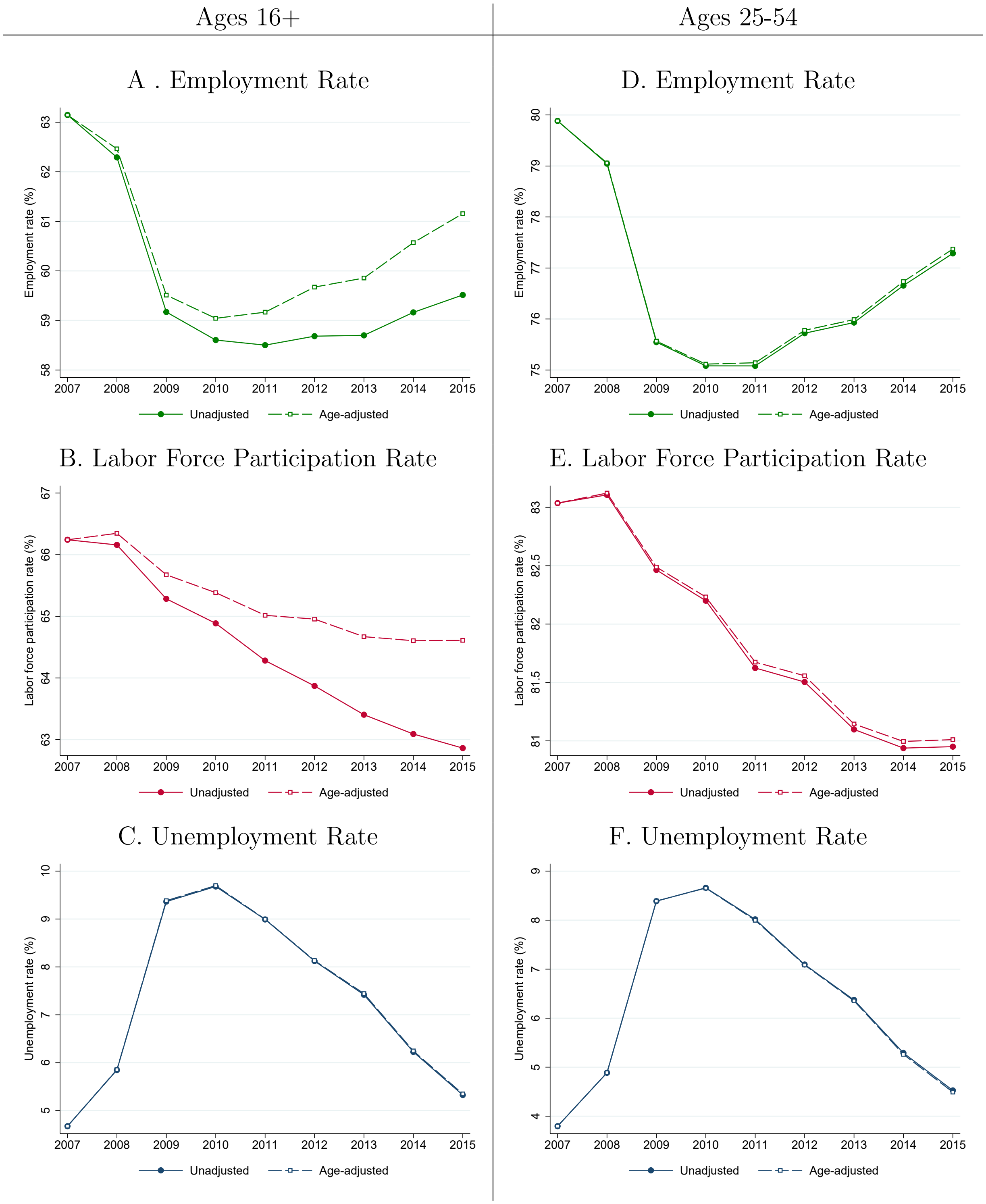
Third, many retail chain firms are missing from the retail chain sample, both because of subsidiaries and franchises as described in Section 3 and also because a (likely small) number of firms outsource their W-2 administration to third-party payroll administration firms that list their own EINs on W-2s. Nevertheless, the retail chain sample includes very large nationwide chains.

Fourth and also specific to the retail chain sample, the filing ZIP code on a firm's business income tax return typically but not always refer to the business's headquarters ZIP code. Excluding workers at the business's headquarters is useful because headquarters workers may perform systematically different tasks than workers at other establishments and thus may possess different human capital even conditional on baseline earnings. I therefore conservatively exclude firms' workers living in the CZ with the largest number of the firm's workers living there, as well as the CZ with the largest number of the firm's workers living there as a share of the total number of workers living there.

Fifth and also specific to the retail chain sample, I consider a firm to have operated in a CZ in 2006 if it employed at least ten stably located workers who lived in the CZ—defined as individuals of any age and citizenship with a W-2 from the firm in all years 2005-2007 and the same residential CZ in all years 2005-2007 based on those W-2s' payee (residential) ZIP codes. It is necessary to define CZ operations using more than one year of W-2 data because W-2 payee ZIP code refers to the worker's ZIP code in January of the year after employment. That feature implies that almost all firms would appear to have operations in every large CZ if one were to use only 2006 W-2s to identify CZ operations, since many workers move to large cities.

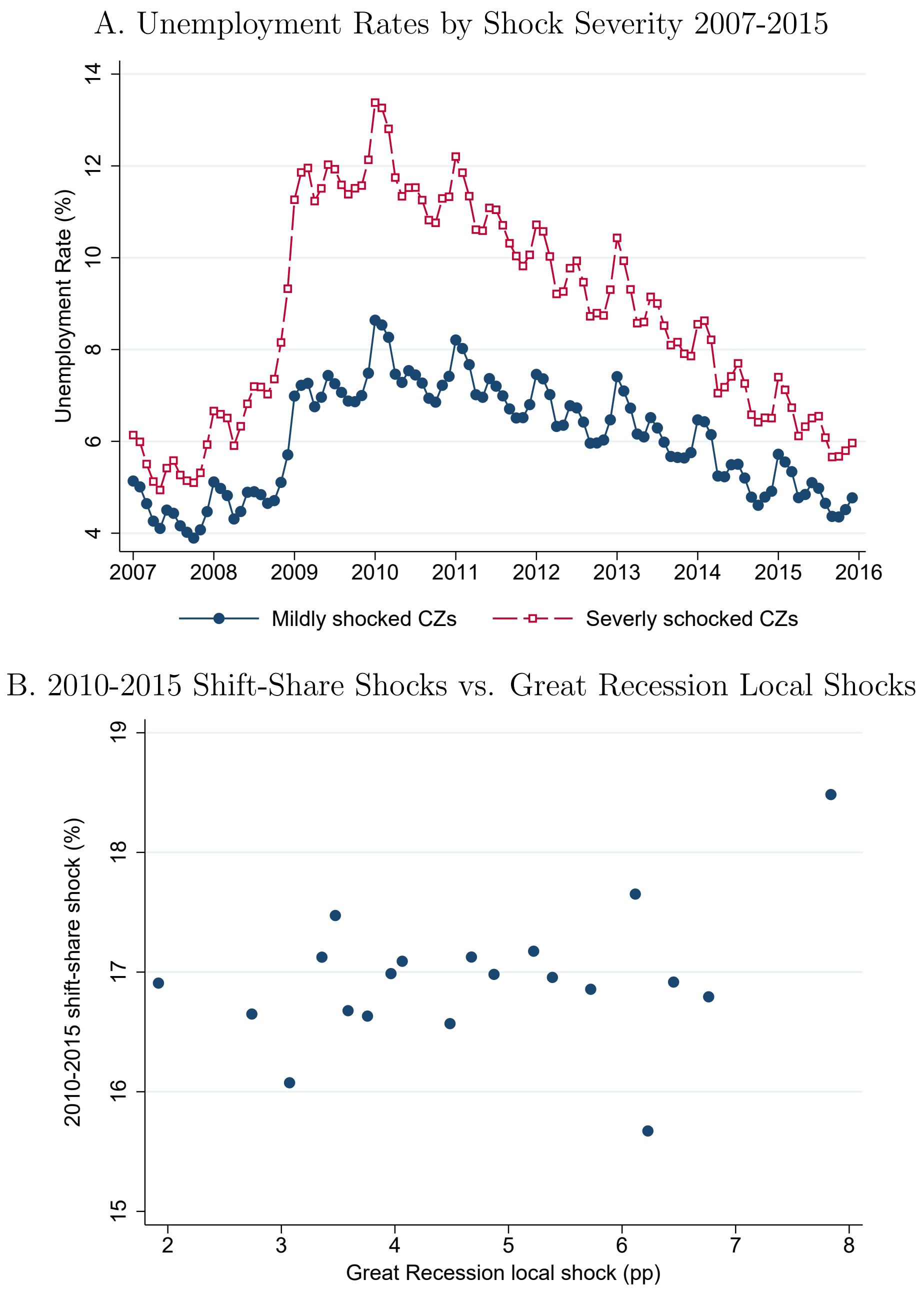
In Section 3.3, I noted that transition of affected workers to self-employment likely does not explain the results. An appendix graph helps to justify that conclusion: Current Population Survey data indicate that changes in state self-employment rates since 2007 were unrelated to changes in state formal employment rates (Online Appendix Figure A.3).

Figure A.1: The Age-Adjusted U.S. Employment Rate Decline



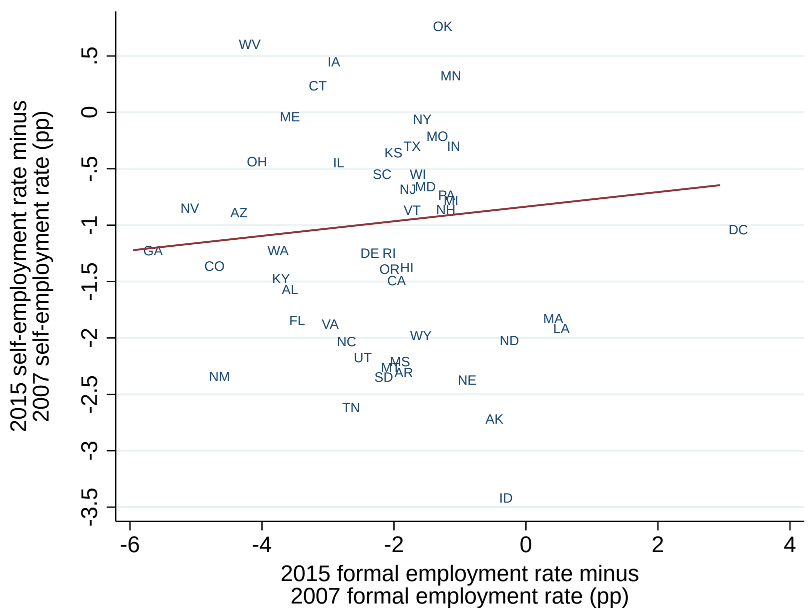
Notes: This figure uses monthly Current Population Surveys (CPS) to plot unadjusted and non-parametrically age-adjusted annual U.S. employment rates (employment-population ratios), labor force participation rates, and unemployment rates 2007-2015 for the civilian non-institutional population. The unadjusted statistics are essentially equal to the official Bureau of Labor Statistics U.S. labor force statistics. The age-adjusted statistics reweight each age-year's annual total unemployment, labor force, and population by the age group's 2007 population share as in DiNardo, Fortin and Lemieux (1996) before summing across ages and computing the displayed rates. See Online Appendix A for details.

Figure A.2: Evidence against Positively Correlated Independent Shocks



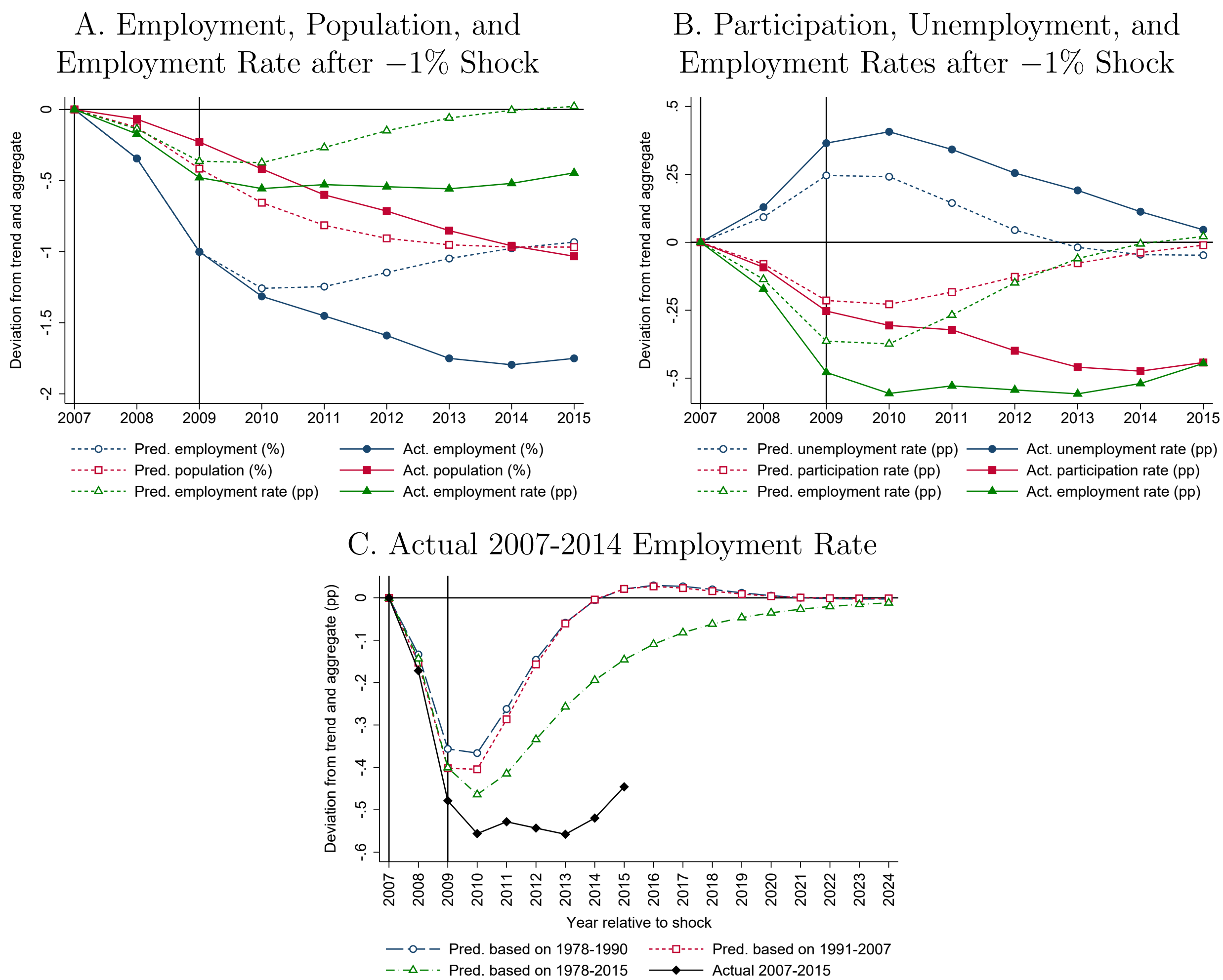
Notes: Panel A uses LAUS county-level data, aggregated to the CZ-level, to plot monthly mean unemployment rates 2007-2015 in severely shocked CZs (those with above median Great Recession local shocks) and mildly shocked CZs (all other CZs). Data points are weighted by CZ population. The post-2001-recession experience exhibited incomplete convergence before being interrupted by positively correlated 2007-2009 shocks. Panel B plots 2010-2015 CZ-level shift-share shocks versus Great Recession local shocks. The shift-share shocks are constructed analogously to Bartik (1991) using County Business Patterns data as follows. Each CZ's shift-share shock equals the projected 2010-2015 percentage change in the CZ's employment based on leave-one-CZ-out nationwide changes in employment by three-digit NAICS industry categories. That is, a CZ c's shift-share shock equals: $SHIFTSHARESHOCK_c = \sum_j \left(\frac{E_{jc2010}}{\sum_{j'} E_{j'c2010}} \times \frac{\sum_{c' \neq c} E_{jc'2015} - \sum_{c' \neq c} E_{jc'2010}}{\sum_{c' \neq c} E_{jc'2010}} \right)$ where j denotes a three-digit industry and E_{jct} denotes total employment in industry j in CZ c in year t. The graph bins CZs into ventiles (five-percentile-point bins) by their Great Recession local shock and then plots the 2007-population-weighted mean of the 2010-2015 shift share shock within each bin. If CZs that were severely shocked during the Great Recession had subsequently experienced additional adverse shift-share shocks 2010-2015 related to the CZs' industrial compositions, Panel B would have exhibited a negative relationship instead of the displayed insignificant positive relationship.

Figure A.3: Self-Employment vs. Formal Employment Rate Changes



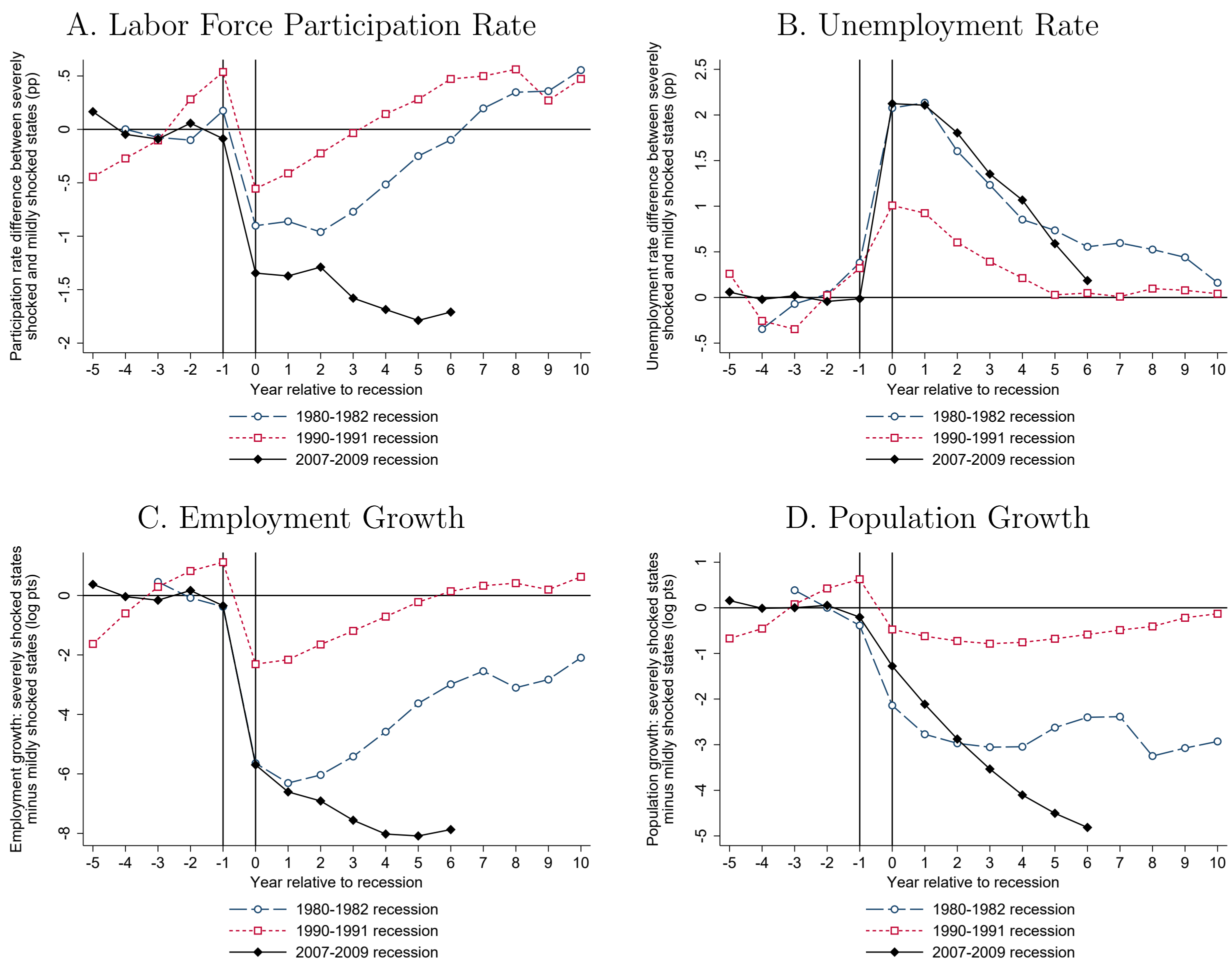
Notes: This graph uses the 2007 and 2015 monthly Current Population Surveys to plot 2007-2015 self-employment rate changes versus 2007-2015 formal employment rate changes for the adult (16+) civilian non-institutionalized population. The formal employment rate equals the number of formally employed individuals (workers for wages or salary in private or government sector) divided by the population. The self-employment rate equals the number of self-employed individuals (including independent contractors) divided by the population. Individuals are classified according to the job in which they worked the most hours. Each year's rate equals the monthly rate averaged across the year's twelve months. Overlaid is the unweighted best-fit line.

Figure A.4: State Employment Rate Persistence after 2007-2009 Shocks



Notes: The dotted lines of panels A-B plot benchmark history-based predictions for state-level responses to a −1% 2007-2009 state-level employment shock, based on estimating Blanchard-Katz's (1992) autoregressive system of state labor market outcomes using LAUS data on the original sample range 1978-1990. The solid lines plot mean actual state-level responses based on reduced-form regressions of 2008-2014 state-level outcomes on 2007-2009 state-level shocks. Panel C plots the mean actual employment rate response series from panels A-B alongside predicted employment rate series based on three estimation time ranges: 1978-1990 (as in panels A-B), 1991-2007, and 1978-2014. See Online Appendix B for more details.

Figure A.5: Great Recession Local Convergence Compared to History: Extended



Notes: Panels A-B replicate Figure 2A for the labor force participation rate and unemployment rate. Panels C-D display analogous graphs for employment growth and population growth. These latter panels require that each state's estimated steady state annual growth rate is subtracted from its actual annual growth, before constructing the graphs exactly as in Panels A-B. See Online Appendix B and the notes to Figure 2.

ONLINE APPENDIX TABLE 1
Approximating the Main Result Using Published Data

Outcome:	Individual-level 2015 relative employment (from tax data)	CZ-level estimated employment effect (from tax data)	CZ-level 2015 relative employment (from BLS LAUS and Census)
	(pp) (1)	(pp) (2)	(pp) (3)
Great Recession local shock	-0.393	-0.381	-0.490
	(0.097)	(0.100)	(0.115)
Main controls	X		
Main sample (individual-level)	X		
Published sample (CZ-level)		X	X
N	1,357,974	591	591
R^2	0.07	0.10	0.09

Notes – This table shows that the paper's main estimate of the 2015 employment impact of Great Recession local shocks—which uses the main sample's non-public individual-level data—can be well-approximated in CZ-level data published alongside this paper. Column 1 reprints the main specification and estimate from Table 2 column 4. Columns 2-3 report coefficients from univariate CZ-level regressions weighted by the CZ's 2007 population as reported in Census's Annual County Resident Population Estimates of the total adult (16+) population, aggregated to the CZ level. The column 2 outcome equals the estiamated coefficients on a vector of CZ fixed effects from estimating the main specification with Great Recession local shock variable replaced by a vector of CZ fixed effects. Online Appendix Table 2 lists these estimated coefficients for the hundred largest CZs. The column 3 outcome variable equals the CZ's 2015 adult employment rate minus the CZ's mean 1999-2007 employment rate, based on Census data and Bureau of Labor Statistics Local Area Unemployment Statistics data: LAUS county-level 16+ civilian non-institutional employment aggregated to the CZ level, divided by Census's CZ population estimate. Column 3 is restricted to the CZs available for column 2. Standard errors are clustered in column 1 by the individual's 2007 state and in columns 2-3 by the CZ's state. See the paper's Online Data Codebook and Online Data Tables for additional details and published variables on the author's website.

ONLINE APPENDIX TABLE 2
Adjustment Margins Pre-Trends

Outcome (relative or absolute):	Employed	Migrated outside 2000 CZ	Employed in 2000 CZ	Employed outside 2000 CZ	Earnings	Earnings in 2000 CZ	Earnings outside 2000 CZ	UI income	SSDI income
	(pp)	(pp)	(pp)	(pp)	($)	($)	($)	($)	($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Effect in 2000	-0.036	0.000	-0.036	0.000	72	72	0	0.6	0.4
	(0.083)		(0.083)		(66)	(66)		(6.2)	(3.9)
Effect in 2001	-0.094	-0.036	-0.093	-0.001	-74	-67	-7	10.6	0.3
	(0.045)	(0.166)	(0.044)	(0.003)	(46)	(42)	(6)	(9.2)	(4.6)
Effect in 2002	-0.068	-0.047	-0.060	-0.008	-56	-47	-9	9.7	0.7
	(0.018)	(0.280)	(0.017)	(0.004)	(26)	(23)	(5)	(15.4)	(5.4)
Effect in 2003	-0.044	-0.005	-0.036	-0.008	-26	-17	-10	10.2	0.6
	(0.029)	(0.361)	(0.031)	(0.007)	(21)	(20)	(6)	(15.9)	(5.8)
Effect in 2004	0.031	0.077	0.030	0.001	1	6	-5	11.0	0.3
	(0.039)	(0.437)	(0.036)	(0.009)	(36)	(30)	(12)	(11.9)	(6.6)
Effect in 2005	0.083	0.151	0.070	0.013	40	42	-1	6.8	-0.1
	(0.061)	(0.526)	(0.053)	(0.010)	(66)	(57)	(22)	(11.5)	(7.3)
Effect in 2006	0.228	0.252	0.189	0.039	-15	1	-16	9.1	0.4
	(0.135)	(0.612)	(0.116)	(0.020)	(118)	(94)	(36)	(13.1)	(9.1)

Notes – This table replicates Table 5 for years 2000-2006 and where each individual's Great Recession local shock equals the 2007-2009 percentage-point unemployment rate change in the individual's *2000* CZ. See the notes to Table 5 for details.

ONLINE APPENDIX TABLE 3
Summary Statistics: State-Level Data

	Mean (1)	Standard Deviation (2)
Employment rate (%)		
1978-2015	62.3	4.6
2007	64.1	3.9
2009	60.7	4.5
2015	60.4	4.4
Unemployment rate (%)		
1978-2015	6.1	2.1
2007	4.4	1.0
2009	8.5	2.0
2015	5.0	1.1
Labor force participation rate (%)		
1978-2015	66.3	4.1
2007	67.0	3.8
2009	66.3	4.0
2015	63.6	4.1
Number of states	51	
Number of years	38	
Number of observations (state-years)	1,938	

Notes – This table lists summary statistics of the Bureau of Labor Statistics's Local Area Unemployment Statistics (LAUS) state-year labor force statistics 1978-2015. The LAUS data cover the adult (16+) civilian non-institutional population of the fifty states and the District of Columbia. The employment rate is the ratio of employment to population. The unemployment rate is the ratio of unemployed to labor force. The labor force participation rate is the ratio of labor force to population.

ONLINE APPENDIX TABLE 4
State-Level Great Recession Employment Shocks and 2007-2015 Employment Rate Changes

Great Recession Employment Shock Rank	State	Great Recession Employment Shock	Change in Employment Rate 2007-2015	Great Recession Employment Shock Rank	State	Great Recession Employment Shock	Change in Employment Rate 2007-2015
		(pp)	(pp)			(pp)	(pp)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	Nevada	-6.46	-6.94	27	Washington	1.13	-5.21
2	Alabama	-3.42	-5.54	28	New Hampshire	1.13	-2.03
3	Michigan	-3.36	-2.74	29	Missouri	1.24	-1.42
4	Georgia	-3.19	-7.09	30	Arkansas	1.26	-4.79
5	Delaware	-3.14	-4.65	31	Texas	1.30	-2.06
6	Mississippi	-3.04	-4.21	32	Virginia	1.56	-4.87
7	Utah	-2.53	-4.31	33	Maryland	1.92	-2.79
8	Florida	-2.40	-5.45	34	Louisiana	1.99	-2.41
9	South Carolina	-2.35	-3.59	35	Massachusetts	2.06	-1.64
10	Arizona	-2.24	-4.82	36	Alaska	2.26	-3.67
11	Idaho	-2.20	-4.53	37	Minnesota	2.33	-2.24
12	North Carolina	-2.15	-4.79	38	New Jersey	2.37	-3.21
13	Tennessee	-1.56	-5.18	39	West Virginia	2.58	-4.18
14	Indiana	-1.52	-2.45	40	Pennsylvania	2.68	-2.15
15	Hawaii	-1.33	-4.27	41	New York	2.79	-2.28
16	Oregon	-1.29	-4.33	42	Oklahoma	3.25	-1.90
17	California	-0.63	-3.74	43	Connecticut	3.33	-3.33
18	New Mexico	-0.57	-6.80	44	South Dakota	3.56	-3.98
19	Illinois	-0.33	-4.02	45	Wyoming	3.85	-4.30
20	Ohio	0.05	-4.19	46	Kansas	4.35	-2.98
21	Montana	0.46	-3.45	47	Vermont	4.38	-2.93
22	Colorado	0.73	-5.15	48	Nebraska	4.77	-2.99
23	Wisconsin	0.75	-2.48	49	Iowa	5.00	-2.03
24	Rhode Island	0.81	-3.98	50	District of Columbia	5.44	0.94
25	Kentucky	0.90	-4.60	51	North Dakota	5.75	-2.48
26	Maine	1.09	-3.39				

Notes – This table lists the Great Recession state-level employment shocks and 2007-2015 percentage-point changes in state-level employment rates that underlie the severe-vs-mild-shock grouping of Figure 1B. See the notes to those figures and Online Appendix B for details. Severely shocked states are listed on the left; mildly shocked states are listed on the right.

LETTER TABLE 1
Specifications for Referee Responses

Outcome relative to pre-2007 mean:	Employed in 2015							
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)	(pp) (6)	(pp) (7)	(pp) (8)
Great Recession local shock	-0.393 (0.097)	-0.375 (0.107)	-0.393 (0.082)	-0.393 (0.100)	-0.393 (0.092)	-0.393 (0.094)	-0.369 (0.097)	-0.366 (0.097)
Main controls	X	X	X	X	X	X	X	X
Locally deflating earnings in fixed effects		X						
CZ has valid Hispanic share 2009-2015							X	X
Change in Hispanic share 2009-2015								X
Clustering on 2007 state and 2006 industry			X					
Clustering on 2007 state and age				X				
Clustering on 2007 state and 2006 earnings bin					X			
Clustering on 2007 state and 2006-age-earnings-industry						X		
N	1,357,974	1,357,974	1,357,974	1,357,974	1,357,974	1,357,974	1,271,391	1,271,391
R^2	0.07	0.08	0.06	0.06	0.06	0.06	0.08	0.08

Notes – Column 1 reprints the paper's main estimate from Table 2 column 4. Column 2 uses locally deflated 2006 earnings when constructing the 2006-age-earnings-industry fixed effects. Columns 3-4 two-way cluster on the listed variables. Column 7 repeats column 1 for the individuals whose 2007 CZs have a non-missing Hispanic share in 2009 and 2015 in the American Community Survey. Column 8 controls for the individual's 2007 CZ's 2015 Hispanic share minus that CZ's 2009 Hispanic share.

ONLINE APPENDIX TABLE 2
Estimated Employment Effects for the 100 Largest Commuting Zones

Effect Rank	CZ Name	Estimated Employment Effect (pp)	Effect Rank	CZ Name	Estimated Employment Effect (pp)
(1)	(2)	(3)	(5)	(6)	(7)
1	Fayetteville, NC	-4.11	51	Cincinnati, OH	0.14
2	Albuquerque, NM	-3.95	52	Portland, OR	0.15
3	Deltona, FL	-2.97	53	Grand Rapids, MI	0.19
4	Pensacola, FL	-2.88	54	Charlotte, NC	0.26
5	Bakersfield, CA	-2.46	55	San Jose, CA	0.28
6	Jacksonville, FL	-2.45	56	Columbus, OH	0.28
7	Birmingham, AL	-2.43	57	Harrisburg, PA	0.28
8	Little Rock, AR	-2.30	58	Los Angeles, CA	0.30
9	Toledo, OH	-2.26	59	Rockford, IL	0.30
10	Lakeland, FL	-2.13	60	Atlanta, GA	0.32
11	Greenville, SC	-2.12	61	Allentown, PA	0.34
12	Sarasota, FL	-1.92	62	Canton, OH	0.37
13	Las Vegas, NV	-1.86	63	Erie, PA	0.40
14	Baton Rouge, LA	-1.83	64	Indianapolis, IN	0.41
15	Palm Bay, FL	-1.77	65	Albany, NY	0.41
16	Eugene, OR	-1.62	66	Louisville, KY	0.45
17	Tampa, FL	-1.39	67	Newark, NJ	0.51
18	Knoxville, TN	-1.37	68	Toms River, NJ	0.51
19	Columbia, SC	-1.26	69	Cleveland, OH	0.52
20	Modesto, CA	-1.24	70	Santa Barbara, CA	0.52
21	Tulsa, OK	-1.23	71	Raleigh, NC	0.53
22	Fresno, CA	-1.23	72	Bridgeport, CT	0.55
23	Tucson, AZ	-1.04	73	San Diego, CA	0.58
24	Charleston, SC	-1.02	74	Washington, DC	0.58
25	New Orleans, LA	-0.98	75	Buffalo, NY	0.59
26	Sacramento, CA	-0.93	76	Kansas City, MO	0.61
27	Phoenix, AZ	-0.90	77	San Antonio, TX	0.66
28	Nashville, TN	-0.89	78	Poughkeepsie, NY	0.67
29	San Francisco, CA	-0.80	79	South Bend, IN	0.75
30	Oklahoma City, OK	-0.80	80	Syracuse, NY	0.82
31	St. Louis, MO	-0.80	81	Scranton, PA	0.83
32	Virginia Beach, VA	-0.78	82	New York, NY	0.91
33	Memphis, TN	-0.74	83	Austin, TX	1.12
34	Dayton, OH	-0.67	84	Fort Worth, TX	1.13
35	Greensboro, NC	-0.64	85	Chicago, IL	1.28
36	Detroit, MI	-0.59	86	Milwaukee, WI	1.29
37	Richmond, VA	-0.47	87	Pittsburgh, PA	1.42
38	Portland, ME	-0.41	88	Manchester, NH	1.43
39	Providence, RI	-0.26	89	Madison, WI	1.46
40	Youngstown, OH	-0.22	90	Houston, TX	1.55
41	Spokane, WA	-0.14	91	Boston, MA	1.70
42	Seattle, WA	-0.10	92	Dallas, TX	1.78
43	Gary, IN	0.02	93	Des Moines, IA	1.79
44	Cape Coral, FL	0.02	94	Reading, PA	1.93
45	Orlando, FL	0.04	95	Miami, FL	1.94
46	Baltimore, MD	0.04	96	Minneapolis, MN	2.14
47	Springfield, MA	0.06	97	El Paso, TX	2.58
48	Port St. Lucie, FL	0.09	98	Salt Lake City, UT	2.61
49	Denver, CO	0.10	99	Omaha, NE	2.98
50	Philadelphia, PA	0.13	100	Brownsville, TX	5.47

Notes - This table lists estimated effects of living in 2007 in each of the hundred largest CZs on 2015 employment, drawn from the complete data published online. The paper's main specification (Table 2 column 4) regresses 2015 relative employment (2015 employment minus mean 1999-2006 employment) on Great Recession local shocks and 2006-age-earnings-industry fixed effects. To generate this table, I repeat the main specification except that I replace the Great Recession local shock variable with a vector of CZ fixed effects. I then de-mean the 2007-population-weighted fixed effects and list the fixed effects of the hundred largest CZ's by 2007 population. 2007 population is drawn from Census's Annual County Resident Population Estimates of the total 16+ population aggregated to the CZ level. See Online Data Table 1 on the author's website for the full list of CZ-level effects and other CZ-level statistics. Online Appendix Table 1 column 2 shows that the published CZ-level data can be used to approximate the paper's main individual-level regression result.