Is the Great Recession Really Over? Longitudinal Evidence of Enduring Employment Impacts*

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July 2016

Abstract

The severity of the Great Recession varied across U.S. local areas. Comparing two million workers within firms across space, I find that starting the recession in a below-median 2007-2009-employment-shock area caused workers to be 1.0 percentage points less likely to be employed in 2014, relative to starting the recession elsewhere. This enduring impact holds even when controlling for current local unemployment rates, which have converged across space. The results demonstrate limits to U.S. local labor market integration and suggest hysteresis effects culminating in labor force exit.

*Email: yagan@berkeley.edu. I thank David Card, Raj Chetty, Robert Hall, Erik Hurst, Lawrence Katz, Amir Kermani, Patrick Kline, Emi Nakamura, Matthew Notowidigdo, Emmanuel Saez, Benjamin Schoefer, Till Von Wachter, Chris Walters, Owen Zidar, Eric Zwick, and seminar participants for helpful comments. Evan K. Rose provided outstanding research assistance. I acknowledge financial support from The Laura and John Arnold Foundation and the Berkeley Institute for Research on Labor and Employment. This paper subsumes earlier findings through 2011 from a different empirical design circulated under the title Moving to Opportunity? Migratory Insurance over the Great Recession. The opinions expressed in this paper are those of the author alone and do not necessarily reflect the views of the Internal Revenue Service or the U.S. Treasury Department. This work is a component of a larger project examining the effects of tax expenditures on the budget deficit and economic activity; all results based on tax data in this paper are constructed using statistics in the updated November 2013 SOI Working Paper The Home Mortgage Interest Deduction and Migratory Insurance over the Great Recession, approved under IRS contract TIRNO-12-P-00374 and presented at the November 2013 National Tax Association Annual Conference on Taxation.

1 Introduction

The 2007-2009 employment contraction varied substantially across U.S. local areas: some places like Phoenix, Arizona, suffered a severe contraction while other similar places like San Antonio, Texas, did not. Consider two identically skilled workers in 2007—one living in Phoenix and the other living in San Antonio. I ask whether these two workers now have similar likelihoods of being employed. The answer tests an integrated labor markets hypothesis: that ex ante identical workers experience identical long-term outcomes across space.[1]

The answer also informs whether the Great Recession and its underlying causes have continued to depress employment even after the unemployment rate (the standard adjustment measure) signaled full recovery—a definition of hysteresis via labor force exit.[2] Figure 1A illustrates that by the end of 2015, the U.S. unemployment rate had returned to its 2007 level. Yet the U.S. labor force participation rate and thus the U.S. employment rate (employment-population ratio) remained three percentage points below their 2007 levels—only half or less of which is explained by demographic changes (Aaronson, Cajner, Fallick, Galbis-Reig, Smith and Wascher 2014, CEA 2014, Shimer 2014). These enduring declines were concentrated among the low-skilled (Charles, Hurst and Notowidigdo 2016). Two natural potential causes are (a) nationwide skill-biased technical and trade changes that would have occurred even without the Great Recession, or (b) the Great Recession driving process (i.e. the recession and the underlying shocks that gave rise to it). When applied to Great Recession local variation, the former channel predicts that the identical Phoenix and San Antonio workers have identical long-term employment likelihoods, while the latter channel predicts lower employment for the Phoenix worker if local labor markets are limitedly integrated.

In the cross section, the evolution of unemployment, participation, and employment rates in severely shocked areas relative to mildly shocked areas mirrored the aggregate. Defining state-level shocks as employment growth forecast errors in an autoregressive system (Blanchard

[1] A related hypothesis pertains to ex *post* identical workers. My stricter form is more useful for assessing area-specific incidence because it requires no area-specific worker scarring.

[2] This definition includes both worker-level (scarring) and area-level (e.g. multiple equilibrium) mechanisms and does not necessarily imply inefficiency.

and Katz 1992), I find that employment rates remain low in the U.S. states that experienced relatively severe 2007-2009 shocks—even though between-state differences in unemployment rates have returned to normal and despite typical between-state population reallocation. The enduring employment gap is large: relative to full convergence to pre-recession between-state employment rate differences, a 2.2-million-person (1.8pp) employment gap remained in 2015 between the severely shocked and mildly shocked halves of the country.

Yet without finer evidence, the enduring cross-area employment gap that originated during the Great Recession could merely reflect local differences in worker types rather than enduring causal impacts of having started the recession in a severely shocked area. Two cross-area selection threats loom large: pre-2007 sorting on human capital (hereafter "skill") and post-2007 sorting on labor supply. First, severely shocked areas may have been disproportionately populated in 2007 by worker types (e.g. low- and middle-skill laborers) who subsequently suffered large nationwide contractions in demand for their skills such as via skill-biased technical or trade change (Katz and Murphy 1992, Acemoglu and Autor 2011, Pierce and Schott 2016).[3] Second, severely shocked areas may have disproportionately attracted or retained those secularly out of the workforce (e.g. the disabled and retired) after 2007, such as via low living costs after local house price declines (Notowidigdo 2011).[4] In either case, severely shocked areas would not have caused their residents' non-employment.

I therefore turn to a quasi-experiment that provides a unique opportunity to isolate causal effects of workers' Great Recession locations. Retail firms employ 24% of U.S. private sector workers, and unlike other major firm types, retail chain firms like Walmart and Starbucks employ workers with similar skills to perform similar tasks at similar wages in many different local areas. I isolate causal effects of Great Recession location by using administrative data to compare workers who in 2006 earned the same amount from the same retail firm in different local

[3]For example: "You can't change the carpenter into a nurse easily...monetary policy can't retrain people" (Charles Plosser, http://www.wsj.com/articles/SB10001424052748704709304576124132413782592) Also: "the available data suggest that the Great Recession has reinforced these trends [of polarization of job growth across high- and low-skill occupations] rather than reversing or redirecting them" (Autor 2010).

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

areas. The within-firm comparisons control unusually finely for pre-2007 cross-area sorting on skill, and tracking workers over time no matter where they move directly controls for post-2007 cross-area sorting on labor supply.

My within-firm cross-area research design differs from earlier designs by isolating area-specific impacts from skill-specific impacts. Earlier papers have found long-term earnings losses after industry-based or firm-based shocks (Jacobson, LaLonde and Sullivan 1993, Davis and Von Wachter 2011, Autor, Dorn and Hanson 2013) though only sometimes have found long-term employment losses (Black, Daniel and Sanders 2002, Autor and Duggan 2003 vs. Walker 2013, Autor, Dorn, Hanson and Song 2014). Parallel work since the Great Recession has analyzed employment losses either during (Mian and Sufi 2014) or after (Krueger, Cramer and Cho 2014, Foote, Grosz and Stevens 2015, Acemoglu, Autor, Dorn, Hanson and Price 2016, Charles, Hurst and Notowidigdo 2016) the recession. In all cases, local long-term impacts may have been caused by nationwide skill-specific shocks interacted with local concentration of affected skill types, rather than area-specific shocks.[5] My within-firm design isolates area-specific impacts by controlling for skill.

I implement the design using longitudinal de-identified data from federal income tax records spanning 1999-2014. The main analysis sample comprises the 2,238,310 people aged 25-75 in 2006 in the continental United States who can be matched to 2006 W-2 employer information, who did not live near their employer's headquarters in January 2007, and who worked for one of the 816 retail firms with substantial operations in at least five local areas. I allow for within-state variation by using the local area concept of the Commuting Zone (CZ): 741 county groupings that approximate local labor markets and are similar to metropolitan statistical areas but span the entire United States. I define severely shocked (mildly shocked) CZs as CZs that experienced below-median (above-median) 2006-2009 log employment growth relative to their own 2000-2003 trend. I define severely shocked (mildly shocked) workers as workers who were

[5]For example after the 1980s coal price collapse, coal mining areas experienced long-term employment rate declines (Black, Daniel, and Sanders). This would be evidence of long-term impacts of area-specific shocks, if all workers had identical skills. But since miners had mining-specific skills, the coal price collapse may have been a nationwide mining-skill-specific shock, lowering miners' employment rates equally everywhere.

living in a severely shocked (mildly shocked) CZ in January 2007, regardless of where they lived afterward. The main outcome of interest is employment at any point in 2014, equal to an indicator for whether the worker had any W-2 wage earnings or any 1099-MISC independent contractor earnings in 2014.

I find that conditional on 2006 firm-by-wages fixed effects and on age and other demographic controls, living in 2007 in a severely shocked CZ caused workers to be 1.0 percentage points less likely to be employed in 2014, relative to living in 2007 in a mildly shocked CZ. The estimate is very statistically significant and rises to 1.8 percentage points when comparing only top- and bottom-shock-decile CZs' workers. These effect sizes are large: they do not condition on layoff and are comparable to the nation's demographics-adjusted point-in-time employment rate decline of approximately 1.5 percentage points. My estimated annual employment impacts are statistically equivalent in each year 2009-2014, indicating little attenuation since 2009.

The 2014 employment impact of Great Recession location is robust to placebo tests (zero effects on pre-2007 employment), CZ-level controls (pre-2007 size, pre-2007 size growth, cross-CZ commuting rate, post-2007 maximum unemployment insurance duration), alternative CZ shock measures (linear level, alternative detrending, and employment rate denomination), instrumenting with birth-state shock, and using an alternative sample comprising year-2000 retail workers. The effect is largest for 2006-low-earners and statistically zero for 2006-high-earners—indicating a causal increase in employment inequality across initial earnings level, a proxy for skill level. The effect is also attenuated for the married but is otherwise nearly identical across subgroups defined by gender, age, 2006 number of children, and 2006 mortgage holding.

The 2014 employment impact could have been caused by 2010-2014 local labor demand shocks that were independent of but happened to correlate with the Great Recession driving process. Local labor demand shocks typically induce layoffs and elevate local unemployment for approximately five years (Blanchard and Katz 1992). Yet unemployment rates steadily and nearly completely converged 2010-2014 between severely shocked and mildly shocked areas, and the 2014 employment impact holds even conditional on local 2014 unemployment. Hence unless there were independent 2010-2014 shocks in severely shocked areas that depressed participation

without elevating unemployment, the evidence indicates that local Great Recession driving processes caused enduring local non-employment via labor force exit.

I rule out quantitatively large effects of two worker-scarring channels: higher disability insurance enrollment and higher layoff rates in severely shocked areas. Related to previous work (e.g. Davis and Von Wachter 2011, Diamond 1982), I find evidence consistent with a third worker-scarring channel—especially-costly layoffs in severely shocked areas—as well as with area-level channels like multiple local equilibria. By whichever mechanism, the paper's within-skill impacts demonstrate the empirical relevance of models that allow cyclical shocks to depress employment via labor force exit even after unemployment has returned to normal.

The rest of the paper is organized as follows. Section 2 documents the persistence of 2007-2009 local area employment rate contractions. Section 3 details the within-firm cross-area empirical design. Section 4 presents the main results. Section 5 investigates mechanisms. Section 6 concludes.

2 Employment across Space since 2007

I begin by documenting a basic correlation in publicly available data: employment rates remain low in the U.S. states that experienced relatively severe employment shocks 2007-2009—even though unemployment rates have converged across states and state populations have adjusted in line with history. This cross-sectional fact serves three purposes. First, it motivates the quasi-experimental analysis in administrative data that follows: Great Recession location has affected long-term employment, unless the persistent spatial employment differences reflect spatial differences in worker types. Second, it shows that reduced population reallocation is not likely to be the source of any quasi-experimental impacts I estimate below. Third, it suggests that U.S. local areas may constitute a suitable laboratory for understanding whether the Great Recession and its underlying causes continue to depress U.S. aggregate employment: mirroring aggregate changes since 2007, severely shocked states have experienced long-term employment and participation declines even though unemployment has returned to normal. See Online

Appendix: State-Level Shocks for full details and historical comparisons.

For comparability to previous work, I choose in this section to analyze cross-area employment outcomes at the state level, and I categorize states into severely shocked states and mildly shocked states using unforecasted state-level changes in 2007-2009 employment derived from the autoregressive system of Blanchard and Katz (1992). The data are the annual Bureau of Labor Statistics 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).[6] I estimate Blanchard and Katz's log-linear autoregressive system (reprinted fully in Online Appendix: State-Level Shocks) in state employment growth, state unemployment rates, and state participation rates in LAUS 1976-2007. I then compute 2008 and 2009 employment growth forecast errors for each state—equal to each state's actual employment growth minus the system's prediction for that state—and sum the two to obtain each state's 2007-2009 employment shock. Roughly speaking, each state's shock equals the state's 2007-2009 log employment change minus the state's long-run trend. Then for simplicity, I group the 26 states with the most negative shocks (e.g. Arizona) into a severely shocked category and the remaining states (e.g. Texas) into a mildly shocked category.

Figure 1B displays the 2002-2015 time series of unemployment, participation, and employment (employment-population) 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 when weighting by population). Within each series, I subtract the mean pre-2008 severe-minus-mild difference from each data point before plotting, so each plotted series has a mean of zero before 2008.

The figure's flat pre-recession trends indicate that unemployment, participation, and employment rates trended similarly for severely shocked and mildly shocked states before the recession. Then the unemployment rate in severely shocked states relative to mildly shocked

[6]Variable definitions are standard and pertain to the age-16-and-over civilian noninstitutional population. Unemployment denotes non-employment conditional on participation. Participation denotes either employment or searching for and being available for employment. Annual counts are calendar-year averages across months.

states spiked in 2008, peaked in 2009-2010, and returned by 2015 to its mean pre-recession severe-mild difference. Yet the 2015 employment and participation rates in severely shocked states remained over 1.8 percentage points below the corresponding rates in mildly shocked states, relative to mean pre-recession severe-mild differences. The implied 2015 employment gap is large: 2.23 million fewer adults were employed in severely shocked states than in mildly shocked states, relative to full recovery to the pre-recession severe-mild employment rate difference (see Online Appendix: Cross-State Employment Gap for the underlying computation). Figure 1B's time series of severe-minus-mild state differences closely mirror Figure 1A's time series of current-minus-2007 aggregate differences.

The aftermath of 2007-2009 state-level shocks is unusual relative to the historical norm documented by Blanchard and Katz (1992) in which state unemployment, participation, and employment rates return to parity five years (2014 in the present case) after a state-level shock. Indeed, Online Appendix Figure A.1D repeats Figure 1B for the aftermath of the 1980s and 1990s recessions and finds approximately five-year cross-state employment rate convergence. Blanchard and Katz suggest that the historical convergence mechanism is rapid population reallocation: a -1% state population change relative to trend follows every -1% employment shock within five years.[7] Therefore a natural possible explanation for Figure 1B's employment rate persistence is that population reallocation has slowed. Online Appendix Figure A.2 investigates this possibility by plotting de-trended 2007-2014 population changes—equal to each state's 2007-2014 percent change in population minus the 2000-2007 percent change in the state's population—versus the state's 2007-2009 employment shock. The graph shows that population reallocation was exactly as large as in the historical benchmark: each -1% 2007-2009 employment shock was on average accompanied by a -1.014% (robust standard error 0.258) de-trended population change.[8]

[7]The unit elastic population response holds when reestimating the Blanchard and Katz system on updated data 1978-2015. The suggested causal chain is: 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.

[8]When not de-trending, state population changes were largely uncorrelated with 2007-2009 employment shocks, also shown in Mian and Sufi (2014) for the 2007-2009 period only. Blanchard and Katz find adjustment

To sum up, this section has documented a simple correlation: employment rates remain low in the U.S. states that experienced relatively severe employment shocks 2007-2009, even though unemployment has returned to normal—similar to aggregate changes relative to 2007 and in spite of typical post-shock population reallocation between states. I now turn to identifying whether there is a causal relationship between a worker's location at the start of the Great Recession and her long-term employment rate.

3 Isolating Great Recession Location Effects

The previous section demonstrated that employment rates remain unusually low in U.S. local areas that experienced an especially severe 2007-2009 employment shock. However, that fact does not alone imply that workers are now non-employed because of where they were living during the Great Recession. First, severely shocked areas may have been disproportionately populated before the recession by workers who subsequently suffered large nationwide contractions for their skill types, like construction workers or routine laborers. Second, 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 foregoing employment. In either case, such workers might be non-employed now regardless of where they were living when the recession hit.

This section details my quasi-experimental strategy for using longitudinal (worker-level panel) data to isolate causal effects of 2007 location on 2014 employment. 2014 is the most recent year available. For clarity, note that I seek to estimate the all-in causal effect of 2007 location on 2014 employment regardless of mechanism; subsequent analyses probe mechanisms.

via de-trended population changes. Though gross (out-) migration rates have declined 15% since 1980 (Molloy, Smith and Wozniak 2011), the gross flows are still an order of magnitude larger than the net flows (population reallocations) predicted by history in response to 2007-2009 shocks, and most population reallocation occurs via reduced in-migration rather than increased out-migration (Monras 2015).

3.1 Definitions

Consider identical local areas c that experienced in 2007-2009 a (for simplicity) binary local Great Recession 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 a worker i lived in 2007 in a severely shocked area. $EMPLOYED_{i2014}(1) \in \{0, 1\}$ indicates i's potential 2014 employment if her 2007 local area was severely shocked, and $EMPLOYED_{i2014}(0) \in \{0, 1\}$ indicates i's potential 2014 employment if her 2007 local area was mildly shocked. Define the causal effect β_i of living in 2007 in a severely shocked area on i's 2014 employment as the difference in the worker's potential employment: $\beta_i \equiv EMPLOYED_{i2014}(1) - EMPLOYED_{i2014}(0)$. The mean $E[\beta_i] \equiv \beta$ in a relevant sample of workers is my causal effect of interest.[9] I refer to it as the enduring employment impact of Great Recession location and as an area-specific effect.

Further notation distinguishes my causal effect of interest from previous analyses'. Let each worker be endowed in 2007 with a skill type $s(i)$ and suppose that skill type, shock severity, and white noise fully determine 2014 employment. Then one can write the 2014 employment rate of a given skill type that lived in 2007 in a given local area as:

$$EMPLOYED_{i2014} = \theta_{s(i)} + \beta_{s(i)} SEVERE_{c(i2007)} + \epsilon_i \tag{3.1}$$

where ϵ_i is an i.i.d. mean-zero error. The nationwide effect $\theta_{s(i)}$ reflects any nationwide skill-neutral shocks as well as any nationwide skill-specific shocks, such as exogenous skill-biased technical progress that diffuses everywhere and changes marginal labor productivities everywhere, possibly below reservation wages. The area-specific effect $\beta_{s(i)}$ may also be skill-specific, for example if only construction or routine workers are affected by a severe local shock. I aim to estimate the mean area-specific effect across skill types $E[\beta_{s(i)}] = \beta$ in a relevant sample of workers. Previous papers that have tracked workers over time have estimated causal effects that are an unknown combination of nationwide skill-specific effects and of area-specific effects.[10]

[9] Strictly speaking, regressions estimate a variance-weighted mean of heterogeneous effects.

[10] For example, Autor, Dorn, Hanson and Song (2014) find no long-term employment effect but a negative long-term earnings effect of industry-level trade competition. The earnings effect is an unknown combination

Empirically below, I isolate a mean area-specific effect by differencing 2014 outcomes between severely shocked workers and mildly shocked workers of the same 2007 skill type.

A non-zero enduring impact of Great Recession location β must be interpreted carefully. Such an impact rejects the integrated labor markets hypothesis defined in the introduction, where I consider 2014 to be the long term relative to 2007. The implication of a non-zero β for enduring impacts of the Great Recession requires a caveat. Define the enduring impact of local Great Recession driving processes as the component of β that was caused by severe Great Recession local shocks as well as by any subsequent shocks that share the same underlying causes. This excludes the component of β caused only by independent 2010-2014 local shocks—defined as those that do not share the same underlying causes as Great Recession local shocks—that may nevertheless have been positively correlated with Great Recession local shocks.[11]

If severely shocked areas experienced independent 2010-2014 local labor demand shocks, such as to at least some local firms not particularly affected by the severe Great Recession shock, one might expect that local unemployment rates spiked after 2010 and remained elevated in 2014. Instead, unemployment rates in severely shocked areas relative to mildly shocked areas peaked in 2010 before steadily and nearly fully converging by 2014 (Figure 1B across states and Online Appendix Figure A.3A across Commuting Zones, the local area concept used below). This pattern is exactly as history would predict after Great Recession local shocks with no independent correlated local shocks 2010-2014 (Blanchard and Katz 1992, Online Appendix Figure A.1B). Given this fact and related evidence below in Section 4.3 that the estimate of β largely holds when controlling for local 2014 unemployment, a non-zero β suggests an enduring impact of local Great Recession driving processes. However unlike location, local shocks are not directly measured, so it is always possible that a non-zero β instead reflects independent but correlated 2010-2014 shocks that depressed 2014 participation without elevating unemployment.

of skill-specific effects (e.g. via reduced productivity of one's industry-specific human capital) and area-specific effects (e.g. via reduced productivity of one's area's firms and one's unwillingness to move).

[11]For example if local house price declines caused Great Recession local shocks, then the enduring impact of local Great Recession driving processes would include both the persistent effect of Great Recession local shocks as well as effects of any 2010-2014 local shocks also caused by the house price declines. Excluded would for example be effects of a 2014 natural disaster that happened to disproportionately strike severely shocked areas.

3.2 Empirical Design

If workers were randomly assigned in 2007 across local areas, then one could estimate the enduring employment impact of Great Recession location β as the unconditional observed employment rate difference in longitudinal data as: $E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 1] - E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 0]$. Lacking random assignment, I assume empirically that workers were as good as randomly assigned conditional on a rich observed vector of pre-2007-determined worker-level and area-level characteristics $\mathbf{X}_{i2007c(i,2007)}$:

$$\Big(EMPLOYED_{i2014}(0), EMPLOYED_{i2014}(1)\Big) \perp SEVERE_{c(i2007)} \mid \mathbf{X}_{i2007c(i2007)} \quad (3.2)$$

Then β equals the conditional observed employment rate difference in longitudinal data:

$$\begin{aligned} &E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 1, \mathbf{X}_{i2007c(i2007)}] - \\ &E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 0, \mathbf{X}_{i2007c(i2007)}] - \\ &\quad = E[EMPLOYED_{i2014}(1) - EMPLOYED_{i2014}(0)]. \end{aligned} \quad (3.3)$$

With respect to the two selection threats listed in this section's introduction, the vector $\mathbf{X}_{i2007c(i2007)}$ controls for pre-2007 sorting across local areas, and comparing workers based on where they were living in 2007 controls for post-2007 sorting across local areas.

Identification hinges on possessing controls $\mathbf{X}_{i2007c(i2007)}$ that credibly provide conditional as-good-as-random assignment across 2007 local areas. Such controls are difficult to find because in typical datasets, workers with similar observables (e.g. income and industry) often perform different tasks and possess different human capital across local areas. However, I observe that—unlike firms in manufacturing and other industries—retail chain firms like Walmart and Starbucks employ workers to perform identical tasks in many local areas.[12] I therefore assume that conditional on age and other demographics related to labor supply trajectories and once

[12]For example according to its website, the manufacturing firm Boeing operates in 29 states, e.g. bidding for government contracts in Virginia and making airplanes in Washington State. Boeing's Virginia workers have different human capital from Boeing's Washington workers.

I exclude firm headquarters workers, workers were as good as randomly assigned across 2007 local areas conditional on their 2006 retail firm and the amount they earned at their 2006 firm. This assumption is satisfied if workers are paid their marginal products and if establishments are homogenous enough across space that workers within firm-by-wages bins possess the same human capital on average across local areas.[13] I conduct placebo tests for this assumption below.

Restricting the analysis to people working at retail chain firms in 2006 sacrifices external validity (by analyzing only a subset of workers) for internal validity: the purpose of the longitudinal data exercise is to overcome selection threats, and retail chain workers provide the available setting.[14] However, retail work is very common: the Bureau of Labor Statistics reports that 23.9% of U.S. private sector workers worked in retail industries (defined in the next subsection) in March 2016.

This within-firm cross-area empirical design builds on previous longitudinal designs. First, earlier work studied industry-specific or firm-specific shocks such as trade competition (Autor, Dorn, Hanson and Song 2014) or environmental regulation (Walker 2013). In contrast, this paper's causal effect of interest is area-specific by construction—independent of a worker's industry and firm since the analysis is entirely within-firm. Second and deviating from the job displacement literature (Ruhm 1991, Jacobson, LaLonde and Sullivan 1993, Neal 1995, Couch and Placzek 2010, Davis and Von Wachter 2011), my design does not condition on layoff. This is valuable both for identification since unobservably low-skill-workers may be disproportionately selected for layoffs (Gibbons and Katz 1991) as well as for estimating effects unrelated to layoff, such as via an inability to find stable reemployment after a voluntary separation (most separations are not layoffs). Relative to these earlier papers, my within-skill cross-area design controls for skill and isolates area-specific impacts without potential layoff-based endogeneity.

[13]Empirically below, the wage earnings bins are irrelevant: firm fixed effects and firm-by-wages fixed effects yield the same results. I deflate wage earnings using a local price index.

[14]To the extent that non-retail workers have relatively area-specific human capital—due to local industry agglomerations (e.g. auto workers), local client bases (e.g. plumbers), or local licensing (e.g. lawyers)—retail workers might be relatively mobile. See Section 4.4 for results by subgroup mobility.

3.3 Longitudinal Data

I implement this paper's empirical design using selected de-identified data from federal income tax records spanning 1999-2014. The sample construction is summarized as follows; additional details are listed in Online Appendix: Longitudinal Data.

I attempt to link the universe of 2006 W-2 forms that were issued to workers aged 25-75 (as of December 31, 2006) to at least one business return in the universe of business income tax returns 1999-2007 using the masked employer identification number (EIN) on both forms.[15] 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 Commuting Zones (CZs, the local area concept defined in the next subsection), I exclude employees living outside the continental United States or in the CZ of their employer's headquarters. I then use the North American Industry Classification System (NAICS) code on the business income tax return to restrict to workers whose 2006 firms operated in the two-digit-NAICS retail industries as defined by the Census Bureau: 44 or 45 (retail trade, e.g. Walmart) or 72 (accommodation and food services, e.g. Starbucks).[16] Then to identify CZs in which the 2006 firms operated, I further restrict to firms with at least ten stably located 2006 employees living in each of at least five CZs and restrict to the firms' employees living in those CZs.[17] This leaves me with a sample of 2,238,310 people working in 2006 at 816 retail firms across 659 CZs.[18]

[15]The age minimum allows 1999 employment—the earliest year of observed employment—to provide a meaningful placebo test. Few Americans are employed above the age maximum. I restrict to workers alive through 2015. Birth and death data are drawn from Social Security Administration records housed alongside tax records. For workers with multiple 2006 W-2's, I restrict attention to each worker's highest-paying W-2.

[16]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.

[17]As in other U.S. administrative data (e.g. Census's Longitudinal Employer Household Dynamics, see Walker 2013), specific establishments of multi-establishment firms are not directly identified in federal tax data. My process infers firms' CZ-level operations from workers' residential locations. Workers can move and receive a W-2 from an old employer at their new residence, so I infer each firm's CZ-level operations using the residential location of workers who do not move in adjacent years. The analysis sample of workers does not condition on no-adjacent-year moves.

[18]The sample is smaller than the universe of retail chain store workers for three main reasons: the 25-year-old age minimum, mismatches between W-2 EIN and business return EIN, and conservative removal of workers at firm headquarters; see Online Appendix: Longitudinal Data for more details.

3.4 Variable Definitions

I now define variables. Year refers to calendar year unless otherwise specified. Variables are available 1999-2014.

1. Outcomes. Following Autor, Dorn, Hanson and Song (2014) and others, *employment* in a given year is an indicator for whether a worker has positive Form W-2 wage earnings or Form 1099-MISC independent contractor earnings (both filed mandatorily by the employer) in the year and is thus a measure of having been employed at any time during the year. Note that this annual employment measure differs from the typical point-in-time (survey reference week) measure. Note that 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 (see Online Appendix Figure A.4).

DI receipt is an indicator for whether the worker has positive Social Security Disability Insurance income in the year as recorded on 1099-SSA information returns filed by the Social Security Administration (SSA). Social Security Disability Insurance is the main disability insurance program in the United States. *Any SSA receipt* is an indicator for whether the worker has any type of positive SSA income in the year as recorded on 1099-SSA information returns. SSA receipt without DI receipt typically reflects receipt of Social Security retirement benefits, which can be claimed by eligible retirees beginning at age 62, but occasionally reflects survivors benefits receipt or receipt of Social Security Income (a smaller cash transfer program for individuals of any age with extremely low income and wealth). *UI receipt* is an indicator for whether the worker has positive unemployment insurance benefit income in the year as recorded on 1099-G information returns filed by state governments. I do not study continuous monetary outcomes in order to focus on employment; local price deflation also remains contentious.

2. CZ and CZ shock. Allowing for within-state variation, a worker's *CZ* is defined as her residential Commuting Zone, a local area concept used in much recent work (Dorn 2009, Autor, Dorn, Hanson and Song 2014, Chetty, Hendren, Kline and Saez 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 based on the 2006-2010 American Community Surveys: 92.5% of U.S. workers live in the CZ in which they work. Urban CZs are similar to metropolitan statistical areas (MSAs), but whereas MSAs exclude rural areas, every spot in the United States lies in exactly one of the 741 CZs.

2007 CZ is the CZ corresponding to the payee (residential) ZIP code that appears most frequently for the worker in 2006 among the approximately thirty types of information returns (filed mandatorily typically by institutions on behalf of an individual, including W-2s)—almost always equal to the W-2's payee ZIP code.[19] Information returns are typically issued in January of the following year, so the ZIP code on a worker's 2006 information return typically refers to the worker's location as of January 2007. *2014 CZ* is defined analogously to 2007 CZ, except that if an individual lacks an information return in 2013, I impute CZ using information return ZIP code from the most recently preceding year in which the worker received an information return.[20] *2007 state* denotes the state with most or all of the 2007 CZ's population, as computed by Chetty, Hendren, Kline and Saez (2014).

In the spirit of Blanchard and Katz (1992), each worker's *2007 CZ shock* equals the worker's 2007 CZ's detrended log employment change 2006-2009, normalized around zero by the detrended aggregate change. Specifically it equals the log 2006-2009 employment change in the worker's 2007 CZ minus the log 2000-2003 employment change in the worker's 2007 CZ, minus a normalizing constant—the difference between the log 2006-2009 aggregate employment change and the log 2000-2003 aggregate employment change—that is irrelevant to the analysis but conforms shock levels to earlier work.[21] Some form of detrending is important (Blanchard

[19]Numerous activities trigger an information return 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% of the U.S. population appeared on some form submitted to the IRS in 2003 (Mortenson, Cilke, Udell and Zytnick 2009).

[20]It is possible that some workers without an information return have left the country, for example temporary immigrants or guest workers. I find nearly identical results when limiting the sample to U.S. citizens.

[21]Using earlier notation, a worker's CZ shock equals $[\ln(E_{c(i2007)2009}/E_{c(i2007)2006}) - \ln(E_{c(i2007)2003}/E_{c(i2007)2000})] - [\ln(E_{agg2009}/E_{agg2006}) - \ln(E_{agg2003}/E_{agg2000})]$, where E_{ct} denotes the number of employed workers in CZ c in year t using this paper's employment definition with both c and t defined using year-t information returns and where agg denotes totals across continental U.S. CZs.

and Katz) because CZs vary in long-run growth rates. Year 2000 is the first year that comprehensive location data are available, and the 2000-2003 time period lies before the peak years of the mid-decade housing boom. For congruence with the main analysis sample, I define CZ shocks using a comprehensive tax data sample that demographically parallels the main analysis sample: all workers aged 25-75 in the current year and no other restrictions. CZ shocks are in log-point units, but for simplicity I refer to them as being in percentage units.

Severely shocked CZ is an indicator for whether a CZ's 2007 shock lies in the bottom half of 2007 CZ shocks weighted by workers in the main analysis sample; *mildly shocked CZs* are all other CZs. Coarsening CZ shocks into the binary severely shocked indicator for the main analyses has attractive properties: the units are especially easy to understand, it can avoid misspecification error in linear regressions (e.g. related to extreme shock values from imperfect detrending-based counterfactuals), and it ensures covariate overlap with a limited number of CZs.[22] I also present results under linear specifications below. *Severely shocked workers* are workers who were living in a severely shocked CZ in January 2007; *mildly shocked workers* are defined analogously.

3. Covariates. *Age* is defined as of December 31 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. *1040 filer* is an indicator for whether the worker 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 worker was either the primary or secondary filer on a married-filing-jointly or married-filing-separately 1040 return in tax year 2006. *Number of current dependent kids* equals the number of children living with the worker as recorded on the worker's 2006 1040 if the worker was a 1040 filer and zero otherwise. *Mortgage holder* is an indicator for whether a Form 1098 information return was issued on the worker's behalf by a mortgage servicer in 2006.[23] *Birth state* is derived from SSA records and, for immigrants, equals the state of naturalization.[24]

[22]To the extent that classical measurement error and this discrete shock categorization causes some borderline CZs to be miscategorized, the estimates below will be somewhat attenuated.

[23]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.

[24]Results are nearly identical when the sample is limited to U.S. citizens.

2006 firm equals the masked employer identification number on the worker's highest-paying 2006 W-2. *2006 firm-by-wages fixed effects* are interactions between 2006 firm and sixteen bins of the worker's deflated 2006 W-2 wage earnings as listed on her highest-paying 2006 W-2.[25] *2006 wages* equals the worker's deflated 2006 W-2 wage earnings (summed across the W-2s from each of her 2006 formal employers) plus the worker's 2006 1099-MISC non-employee compensation (summed across the 1099-MISCs from each of her 2006 independent-contractor employers). I deflate the wages variables to 2010 dollars using the CPI-U and, heeding the urban economics literature on compensating wage differentials, I further deflate by Local CPI 2 (Moretti 2013) to account for local differences in the cost of living.[26] I winsorize (top-code) 2006 wages at $500,000. Since education level and race are not observed at the worker-level, *high-school dropout share*, *college graduate share*, *black share*, and *Hispanic share* equal the respective share of the CZ's adults falling into these respective categories as defined in the 2000 Census. Other controls are used only for robustness checks and are defined when used.

3.5 Summary Statistics

Table 1 reports summary statistics in the main analysis sample and, for comparison, also a 1% random sample of the full population who satisfy the main analysis sample restrictions excluding the firm-based restrictions and who lived in 2007 in the 659 CZs covered by the main analysis sample. 74.6% of the main analysis sample was employed in 2014 while 5.7% received DI in 2014, 20.0% received any SSA income in 2014, and 26.9% received UI in at least one year 2007-2014. 60.2% of the sample is female. The average worker earned $27,425 in 2006. The average 2006 age is 41.2 years and the age distribution is right-skewed. The sample is on average more female, poorer, younger, less likely to be married, less likely to have children, and less likely to hold a mortgage than the full population. 83.7% of the sample worked in 2006 in retail trade while the remainder worked in accommodation and food service. The three

[25] Chosen to create roughly even-sized bins, the bin minimums are: $1, $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.

[26] Results are more negative and pre-2007 trends are less parallel when not locally deflating. Local CPI 2 is the more aggressive of Moretti's two deflators.

most common three-digit NAICS categories are general merchandise (comprising 29.8% of this sample's workers, e.g. Walmart), grocery (comprising 19.1% of workers, e.g. Safeway), and restaurants (comprising 13.4% of workers, e.g. Starbucks).[27] Workers in 2007 lived across 659 CZs which together account for 99.8% of continental U.S. population and employment.

Figure 2 displays a color-coded map of CZ shocks across the continental United States. Shock values are top-coded at the sample-weighted 95th percentile and bottom-coded at the sample-weighted 5th percentile. Familiar patterns are apparent, including within-state patterns such as severe shocks in California's Central Valley but not along California's coast. Recalling the introduction's example, Phoenix—America's sixth largest city and shown in the dark red CZ in the middle of Arizona—experienced a 16th percentile shock (−4.34%) while San Antonio—America's seventh largest city and shown in the large yellow CZ in the middle-bottom of Texas—experienced a 69th percentile shock (+1.25%). The empirical analysis compares the 2014 outcomes of retail workers who were living in 2007 in places like Phoenix to workers at the same 2006 firm who were living in 2007 in places like San Antonio.

4 Results

This section presents the paper's main result: the estimated effect on 2014 employment of living in 2007 in a severely shocked CZ. I begin by presenting the main regression estimate, first visually and then in table form. I then present robustness checks and heterogeneity analyses.

4.1 Main Estimates

Figure 3A plots the time series of estimated effects of living in 2007 in a severely shocked CZ, conditional on this paper's main controls. The plotted 2014 data point is the paper's main

[27] 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.

result and equals $\hat{\beta}$ estimated on the main analysis sample in the regression:

$$EMPLOYED_{i2014} = \mathbf{X}_{i2007c(i2007)}\hat{\boldsymbol{\gamma}} + \hat{\beta} SEVERE_{c(i2007)}, \tag{4.1}$$

where $EMPLOYED_{i2014}$ is an indicator for whether worker i was employed in 2014, $SEVERE_{c(i2007)}$ is an indicator for whether i was living in 2007 in a severely shocked CZ, and $\mathbf{X}_{i2007c(i,2007)}$ is this paper's main vector of controls. The main vector of controls comprises 2006 firm-by-wages fixed effects to control for skill, as well as demographics at the worker-level (age fixed effects, a quartic in 2006 wage earnings, and indicators for being female, being a 2006 mortgage holder, being a 2006 1040-filer, being married in 2006, and having zero, one, or two-or-more dependent kids in 2006) or at the 2007-CZ-level (quartics in high-school dropout share, college graduate share, black share, and Hispanic share) that may relate to labor supply trajectories. For other years t, plotted data points equal the same coefficient from a regression of $EMPLOYED_{it}$ on the exact same right-hand-side variable values in the exact same sample. 95% confidence intervals are plotted in vertical lines unadjusted for multiple hypotheses, based on standard errors clustered on 2007 state.

The 2014 data point shows the main result: I estimate that living in 2007 in a severely shocked CZ caused the average worker to be 0.980 percentage points less likely to be employed in 2014, relative to living in 2007 in a mildly shocked CZ. I refer to this estimate as the enduring employment impact of Great Recession location. The estimate is very significantly different from zero, with a t-statistic of 4.1. The mean 2014 employment rate in this sample is 74.6%, so this estimated effect equals a 1.31% difference in employment rates.

The plotted time series of estimated effects 1999-2007 constitute placebo tests corroborating the identifying assumption that conditional on controls, severely shocked CZ status is as good as randomly assigned. Confidence intervals grow larger as one moves away from 2006 (when everyone was employed), partly reflecting different pre-2007 trends across individual CZs. But on average, those differences cancel out across severely shocked and mildly shocked workers: estimated effects are nearly zero in every year 1999-2005, before Great Recession location could

affect employment. Moreover and because employment is measured as being employed anytime during the calendar year, the estimated zero effect visible in 2007 is similarly consistent with unconfoundedness. These placebo tests suggest that employment rates of severely shocked workers and mildly shocked workers would have continued to be similar beyond 2007 in lieu of shocks.

Instead, employment rates fell substantially 2007-2009 for severely shocked workers relative to mildly shocked workers and then exhibited no substantial or statistically significant recovery 2009-2014. Future recovery speed is uncertain. If the last estimated percentage-point convergence speed (from 2013 to 2014) were to continue, employment losses among severely shocked workers relative to mildly shocked workers would continue until year 2024—fifteen years after the official end of the Great Recession (see Online Appendix: Recent Trends).

Table 2 displays coefficient estimates from equation 4.1 in the main analysis sample under various specifications. Column 8 corresponds to Figure 3A's 2014 data point, my preferred estimate. This -0.980 percentage-point employment estimate equals the 2014 employment effect of living in 2007 in a below-median shock area instead of an above-median-shock area. Column 9 replaces this above-below-median comparison with a top-bottom-decile comparison by restricting the sample to the twenty percent of workers living in 2007 in either a top-decile-shock area or a bottom-decile-shock area. I find that living in 2007 in a bottom-decile-shock area instead of a top-bottom-decile-shock area caused workers to be -1.78 percentage points less likely to be employed in 2014. Column 11 repeats column 9 for the outcome of cumulative employment 2009-2014, equal to the number of years 2009-2014 that the worker was employed. The -13.3 percentage-point-years estimate implies that bottom-decile-shock workers accumulated a six-year employment deficit relative to top-decile-shock workers equivalent to 13.3% of bottom-decile-shock workers losing a year of any employment.

These effect sizes are large. Unlike previous papers that condition on individual-level layoff or establishment-level mass layoff, I have considered a large population without conditioning on an individual-level or establishment-level negative event. Recall from the introduction that in the full population, the U.S. point-in-time employment rate remained in 2014 approximately

1.5 percentage points below its 2007 level after adjusting for demographic changes (Aaronson, Cajner, Fallick, Galbis-Reig, Smith and Wascher 2014, CEA 2014, Shimer 2014). Thus with the caveat that the employment definitions differ, the causal 1.0- and 1.8-percentage-point employment losses I estimate across workers from different U.S. local areas are similar in magnitude to the aggregate employment losses since 2007 that are not explained by demographic controls.

Columns 4-8 explore the influence of the controls in my design. Column 4 shows that with no controls, living in 2007 in a severely shocked area is associated with a 1.843 percentage-point reduction in 2014 employment. However, there is quantitatively relevant selection across CZs on age, as the average severely shocked worker was 0.8 years older than the average mildly shocked worker: column 5 shows that once one includes age fixed effects, the coefficient falls to -0.997 percentage points. Columns 6-8 show that when including the other demographic controls, 2006 firm fixed effects, and 2006 firm-by-wages fixed effects, the estimate barely changes while shrinking the standard error.[28] This indicates that once one limits the population to those initially working in retail and controls for age, there turns out to be little systematic selection across space.[29] Thus we learn that subsequent papers may be able to perform similar analyses even when linked employer-employee data are unavailable, as long as initial retail industry is observed. Columns 12-13 report placebo tests for effects of living in 2007 in a severely shocked area on the worker's mean employment status across years 1999-2005 (column 12) and on 2007 employment (column 13, also displayed in Figure 3A). Both estimates are nearly zero, supporting the paper's identifying assumption.

As a final benchmark, columns 1-3 repeat the regressions underlying columns 4-6 on the 1% random sample of the full population of workers who satisfy the main analysis sample restrictions excluding the 2006-firm-based restrictions.[30] Column 3 shows that conditional on

[28]Dividing the column 8 estimate by the column 4 estimate, 53.2% of the cross-sectional effect is estimated to be causal.

[29]Economically, this is consistent with retail workers being similar across firms (e.g. across Walmart and Starbucks) or with firm shares not varying systematically across shock severity (e.g. Phoenix and San Antonio having similar shares of retail workers working at Walmart and Starbucks).

[30]The outcome variable in columns 1-3 equals the worker's 2014 binary employment status minus the worker's 2006 binary employment status; this is comparable to the main analysis because main analysis sample workers were by construction 100% employed in 2006.

the main controls excluding the firm-based fixed effects, the estimated effect of living in 2007 in a severely shocked CZ is somewhat though insignificantly larger in magnitude than the analogous estimate in the main analysis sample in column 6: -1.235 relative to -0.951.

4.2 Robustness

I present several robustness checks in Table 3. In case there was pre-2007 sorting across space on employment attachment, column 2 replicates the main specification (reprinted in column 1) while controlling for each worker's employment history (indicators for employment in each year 1999-2005). In case workers of large or growing CZs had different employment trajectories, column 3 controls for a quartic in the worker's 2007 CZ size, equal to the CZ's total employment in 2006 as reported in Census's County Business Patterns (CBP). Column 4 controls for a quartic in the worker's 2007 CZ's size growth, equal to the CZ's log change in CBP employment from 2000 to 2006. Column 5 controls for a quartic in the worker's 2007 CZ's share of workers who work outside of the CZ, computed from the 2006-2010 American Community Surveys and motivated by recent work suggesting that commuting options can attenuate local shock incidence (Monte, Redding and Rossi-Hansberg 2015). As a check of a policy mechanism, column 6 controls for a quartic in the worker's 2007 CZ's state's maximum unemployment insurance duration over years 2007-2014, derived from Mueller, Rothstein and Von Wachter (2015). All estimates remain close to and insignificantly different from the main estimate of -0.980.

The preceding specifications use a binary measure of 2007 CZ shock intensity. Column 9 replaces the severely shocked indicator with the worker's 2007 CZ shock, a continuous measure of shock intensity, in the main specification and with CZ shocks winsorized (bottom-coded and top-coded) at the sample-weighted fifth and ninety-fifth percentiles.[31] The coefficient of 0.148 (t-statistic of 4.5, similar to the main estimate's) indicates that a one-percentage-point more severe CZ shock reduced the 2014 employment rate of its 2007 residents by 0.148 percentage

[31]The conditional expectation function of 2014 employment and 2007 CZ shock is more linear when winsorizing tails than when not.

points (the standard deviation of CZ shocks is 4.0, see Table 1).[32] Figure 3B non-parametrically depicts this dose-response relationship between workers' 2014 employment and their CZ shock by regressing 2014 employment and CZ shock on the main controls, computing residuals, adding back their means for interpretation, and plotting means of the 2014 employment residuals within twenty equal-sized bins of the CZ shock residuals. Overlaid is the best-fit line estimated by regressing the 2014 employment residuals on the CZ shock residuals, whose slope of course equals the 0.148 estimate reported in Table 3 column 10. The dose-response relationship is relatively linear.

Columns 11 and 12 replicate columns 1 and 10 using an alternative employment-*rate*-based definition of CZ shocks—equal to the 2006-2009 percentage-point change in the CZ's employment rate (residents employed divided by total residents) minus the 2000-2003 percentage-point change in the CZ's employment rate in the same data used to compute the main CZ shock measure. Column 11 reports that when using this alternative definition, living in 2007 in a severely shocked CZ is estimated to have caused a 1.309 percentage-point decline in employment rates relative to living in a mildly shocked area, with a confidence interval that includes the main estimate. Column 12 includes this alternatively defined CZ shock linearly and indicates that living in a CZ in which the local employment rate fell 2007-2009 relative to the CZ's trend by one percentage point caused the incumbent retail chain workers to have a nearly one-percentage-point (0.969) lower employment rate five years later in 2014.

Additionally, severely shocked CZs like Phoenix had attracted many in-migrants in the decades leading up to 2007; if those in-migrants had somehow been negatively selected on future labor productivity or other employment determinant changes conditional on the main controls, the main estimate could be confounded. Column 13 addresses this concern by instrumenting for living in 2007 in a severely shocked CZ using the mean of the severely shocked variable in the worker's birth state. The instrument makes the point estimate insignificantly larger.

Finally, one may be concerned that conditional on employment history, severely shocked

[32]Under an alternative shock definition in which I de-trend 2006-2009 CZ employment growth using 2003-2006 rather than 2000-2003 CZ employment growth, the analogous coefficient is 0.241 with a standard error of 0.039.

workers were simply less attached to the labor force than mildly shocked workers and would have withdrawn from the labor force even in the absence of 2007-2009 shocks. If so, one might expect to see 2001-2006 employment declines among severely shocked workers when the sample is defined according to *year-2000* retail employment. I therefore construct an alternative sample of year-2000 retail workers and plot estimated annual effects of living in a severely shocked CZ in Figure 3C, replicating Figure 3A on this alternative sample.[33] The independent variables are defined as of 2000 (worker-level demographics) and 2001 (CZ) rather than 2006 and 2007. The severely shocked indicator continues to be defined using 2007-2009 CZ shocks, but it now corresponds to whether the worker was living in *2001* in a severe-2007-2009-shock CZ. The graph therefore plots estimated effects of living in 2001 in a 2007-2009-defined severely shocked area. The estimated effects are insignificantly different from zero 2001-2006 with no net trend, passing this placebo test.[34]

4.3 Unemployment versus Participation Margins

A natural question is whether the incrementally non-employed severely shocked workers were unemployed in 2014 or had exited the labor force. Participation is not observed in the longitudinal data. Instead, I control flexibly for a quartic in each worker's 2014 local unemployment rate and will conclude that most of the incrementally non-employed had likely exited the labor force if the controls fail to attenuate most of the estimated 2014 employment impact of Great Recession location. Note that the exercise is conservative in the sense that I would abstain from the labor force exit conclusion if severely shocked workers' 2014 areas (mostly still severely shocked areas) systematically had only epsilon-higher 2014 local unemployment rates than mildly shocked workers' 2014 areas which attenuates the estimated impact to zero.

[33]The alternative sample applies the same sample restrictions as in the main analysis sample except that all year-based restrictions use a year subtracted by six; for example, age is defined as of December 31, 2000, for the 25-75 age restriction. The sample comprises 1,605,539 workers at 709 firms. 2001 is the first year with comprehensive location information because some 1999 information returns are incomplete.

[34]The estimated post-2007 effects in the year-2000-based sample are slightly larger than those in the main analysis sample, especially in percentage terms: -1.77% versus -1.31%. This is consistent with some share of year-2000 (but not year-2006) retail workers in severely shocked areas having transitioned into construction (Charles, Hurst and Notowidigdo 2016) and possibly foregone education by 2007 (Charles, Hurst and Notowidigdo 2015) (although I omit workers below age 25)—endowing them with worse human capital by 2007.

Table 3 column 7 replicates the main specification while controlling for a quartic in the worker's 2014 state's 2014 unemployment rate (averaged across all twelve months of 2014 based on the Current Population Survey), specific to the worker's gender and people 33 years and older (corresponding to the main analysis sample's 2014 age minimum). Column 8 controls instead for a quartic in the LAUS-based gender-and-age-unspecific CZ-level unemployment rate.[35] Since local unemployment rates converged through months of 2014 (Online Appendix Figure A.3B-D) and since I study a measure of any calendar-year employment, column 9 controls for the same quartic as in column 7 except that the unemployment rate is averaged across the last two months of 2014. In all cases, the unemployment control attenuates less than half of the estimated impact of Great Recession location and does not reject zero attenuation. I therefore conclude that most of the incrementally non-employed severely shocked workers have likely left the labor force.[36] The relatively low explanatory power of 2014 local unemployment is consistent with the near convergence of local unemployment rates across space by 2014 (see Online Appendix Figure A.3) and an enduring impact of local Great Recession driving processes rather than independent 2010-2014 local labor demand shocks in severely shocked areas (Section 3.1).

4.4 Worker-Level Heterogeneity

Shock adjustment is believed to be easier for some subgroups than for others. Figure 4 plots point estimates and 95% confidence intervals of the enduring employment impact of Great Recession location for several worker subgroups defined by pre-2007-determined characteristics. Each row reports results from estimating equation 4.1 on a different subsample: the overall main analysis sample (reprinting the main estimate from Table 2 column 8), by gender, by 2006 wage earnings bin, by 2014 age group, by 2006 marital status, by 2006 number of kids, and by 2006 mortgage holding status. Recall that the overall estimate is already net of all of these subgroup controls, so this figure illustrates impact heterogeneity, not the influence of

[35] LAUS does not contain unemployment by age and gender, so the CZ-level unemployment rates are more specific to workers' CZs but less specific to workers' genders and incompatibly include youth unemployment. I aggregate LAUS county-level unemployment rates to the CZ level using population weights.

[36] It is conceivable that the incrementally non-employed severely shocked workers I study remained unemployed while other local residents exited the labor force.

demographic controls.

Proxying for skill by education level, previous work showed that aggregate employment rate declines since 2007 were concentrated among the least-skilled (Hoynes, Miller and Schaller 2012, Charles, Hurst and Notowidigdo 2016). The figure displays similar results for the enduring impact of Great Recession location, proxying for skill by initial earnings level (Autor, Dorn, Hanson and Song 2014). Low initial earners (defined here as those who earned less than $15,000 in 2006, approximately the 30th percentile among wage earners) experienced a worse than average effect of living in 2007 in a severely shocked area, while high initial earners (defined here as those who more than $45,000 in 2006, approximately the 70th percentile among wage earners) experienced no significant effect. This implies a causal increase in employment inequality across workers of different initial earnings levels.

I find attenuated effects among the married but no significant differences by gender, age, number of children, or mortgage holding.[37] This is consistent with reduced migration due to dual-earner frictions (e.g. not wanting to out-migrate because a spouse is still employed locally) or underwater mortgages not explaining the results—a conclusion already foreshadowed by the typical population reallocation found in Section 2 and by earlier work (Farber 2012, Schulhofer-Wohl 2012, Valletta 2013, Sahin, Song, Topa and Violante 2014). For comparison, rates of 2007-2014 migration—defined as the worker's 2014 CZ being different from her 2007 CZ—of the analyzed subsample are listed in the far right of each row. Comparison of subgroup migration rates to subgroup differences presents an at-first surprising fact: the effect of Great Recession location is not smaller for more mobile subgroups. Youth, singles, the childless, and those without a mortgage were all substantially more likely to have migrated but experienced similar or larger effects than the old, married, parents, and mortgage-holders.

The zero correlation between subgroup incidence and subgroup migration rates is consistent with how migration is typically understood to make initial location irrelevant for long-term employment, already reviewed in Section 2. When a locale experiences an idiosyncratic con-

[37]The age invariance accords with Hoynes, Miller and Schaller who find that those aged 33-60 (the age range as of 2014 studied here) are similarly likely to be non-employed in states with different unemployment rates, in contrast to the higher non-employment rates among those younger than 25.

traction in global demand for its traded good (e.g. a 5% contraction in demand for cars from Detroit), the initial residents do not all need to out-migrate in order for the initial residents to avoid idiosyncratic incidence. Instead, population needs to decline relative to trend only modestly—matching each locale's idiosyncratic local labor demand shift with a local labor supply (population) shift that restores initial relative wages and employment rates across all locales and across stayers and leavers (Blanchard and Katz 1992). Most of the population decline happens via reduced in-migration rather than increased out-migration (Monras 2015), and Section 2 found typical population reallocation after Great Recession local shocks. For these reasons, the next section probes mechanisms other than out-migration frictions.

4.5 CZ Heterogeneity

Previous work has emphasized two types of local shocks. Typical work on U.S. local labor markets has imagined that local employment shocks reflect shifts in global demand for locally produced traded goods (e.g. cars produced in Detroit). Recent work on local variation in the Great Recession has emphasized not only contractions in locally produced traded good demand but also contractions in locally produced non-traded good demand (e.g. haircuts in Phoenix) associated with local declines in household net worth (e.g. driven by Phoenix's house price decline). To characterize heterogeneity in the enduring impact of Great Recession location, I therefore estimate simple correlations between enduring location effects and 2007-2009 local measures of either nontraded employment contractions or traded employment contractions.

To do so, I first estimate equation 4.1 with the main controls and with 2007-CZ fixed effects instead of the severely shocked indicator.[38] Then with no controls, I regress the estimated 2007-CZ fixed effects on measures of different types of CZ-level demand changes: the CZ's 2007-2010 manufacturing shift-share (Bartik-1991) shock, the CZ's 2007-2010 construction shift-share (Bartik-1991) shock, and the CZ's 2006-2009 house-price-driven percent change in household

[38]This regression naturally omits the colinear CZ-level demographic controls of high-school dropout share, college graduate share, black share, and Hispanic share. As examples, the point estimates for the Phoenix and San Antonio CZs are -1.35 and 1.85 percentage points respectively relative to the mean.

net worth (Mian, Rao and Sufi 2013, Mian and Sufi 2014).[39] The first measure is intended to reflect conventional shocks to local traded good demand. The second measure is intended to reflect a shock to nontraded good demand particular to the 2007-2009 housing bust. The third measure is intended to reflect deleveraging-induced contractions in local spending. Both the dependent and independent variables are standardized to have mean zero and standard deviation one across CZs weighted by main analysis sample size; the CZ-level regressions are also weighted by main analysis sample size, so the reported coefficients are weighted correlation coefficients.[40] A positive coefficient indicates that adverse shocks are correlated with large employment declines. Standard errors are clustered at the state level with no adjustment for the error in dependent variable construction.

Table 4 column 2 shows that the manufacturing-only Bartik shock correlates weakly and insignificantly with effects of Great Recession location on 2014 employment. In contrast, columns 3-4 show that the construction-only Bartik shock and the housing-net-worth shock correlate strongly and significantly with effects of Great Recession location on 2014 employment. This means that 2007-CZ effects on 2014 employment are more related to measures of nontraded good demand contraction than traded good demand contraction. Columns 6-8 replicate columns 2-4 for the outcome of 2007-CZ fixed effects on *2009* employment. Although noisy, the employment effect of manufacturing-only shocks appear, if anything, to have declined between 2009 and 2014 (column 6 vs. column 2). In contrast, employment effects of construction-only and housing-net-worth shocks appear, if anything, to have increased between 2009 and 2014 (columns 7-8 vs. columns 3-4).

[39]Each CZ's shift-share shock is computed using County Business Pattern data as the projected 2007-2010 change in the worker's 2007 CZ, based on leave-one-CZ-out nationwide changes in employment by three-digit NAICS industry categories—with changes respectively zeroed-out for either non-manufacturing (non-NAICS-310-339) or non-construction (non-NAICS-230-239). See Online Appendix: Shift-Share Shocks. A CZ's housing net worth decline equals a CZ's 2006-2009 log change in median house price times the 2006 value of the CZ's housing stock, divided by the CZ's 2006 household net worth, drawn from Mian and Sufi.

[40]The housing net worth data are missing for several small CZs, reducing total usable CZs to 363 which together cover 95% of the main analysis sample. Standardization is done on the 363-CZ subset.

5 Mechanisms

The previous section established this paper's main result: Great Recession location had a large causal effect on workers' 2014 employment, even conditional on local 2014 unemployment. As discussed in Section 3.1, it is always possible that this enduring employment impact was not caused by local Great Recession driving processes and was instead caused by independent post-2009 shocks that happened to strike severe-2007-2009-shock areas and depressed local participation without elevating unemployment. With that caveat in mind, I now probe four potential channels by which local Great Recession driving processes could have caused the enduring impact of Great Recession location.

First, severely shocked workers may have transitioned to disability insurance, persistently raising their reservation wages. Second, severely shocked workers may have suffered more layoffs, destroying firm-specific human capital. Third, a given layoff may have been especially costly in severely shocked areas, such as due to skill decay, discouragement, or loss of one's taste for work during especially long non-employment spells. Fourth, most severely shocked workers may have stayed in severely shocked areas, and severely shocked areas may still be depressing their *current* (2014) residents' employment, such as via a persistent driving process or via one-time shocks moving areas to persistent low-employment equilibria without scarring workers.

The first three channels—transition to disability insurance, more layoffs, and costlier layoffs in severely shocked areas—can be classified as forms of worker scarring: ex ante identical workers become different ex post (Phelps 1972, Clark and Summers 1979, Blanchard and Diamond 1989, DeLong and Summers 2012). In the fourth channel—continued area-specific depression—ex ante identical workers remain identical ex post, but ex ante identical local areas become different ex post.[41] To preview the results, I do not find support for the first two channels, but I do find evidence consistent with the latter two. I then discuss implications.

[41]This channel implies that exogenously moving a worker across locations in 2014 would have affected her employment. That implication refers to an individual worker, not all workers at once, and does not capture externalities on other workers and is therefore silent on whether aggregate employment would be higher via spatial reallocation—the subject of the spatial mismatch literature (e.g. Sahin, Song, Topa and Violante 2014).

5.1 Transition to Disability Insurance

Severe 2007-2009 shocks may have induced workers to supplement their income with Social Security Disability Insurance (DI)—a costly-to-obtain but typically-permanent location-independent income stream—thereby permanently raising their reservation wages and reducing their employment regardless of current location (Autor and Duggan 2003, Maestas, Mullen and Strand 2013).[42] Table 5 column 2 replicates the paper's main specification (Table 2 column 8) for the binary outcome of DI receipt in 2014. I find no statistically significant impact of living in 2007 in a severely shocked area on 2014 DI receipt: a point estimate of -0.038 with a standard error of 0.095. This suggests that transition to DI does not explain the enduring employment impact of Great Recession location.[43]

More rigorously, I estimate an upper bound on the contribution of DI enrollment to the main employment result, under the mild monotonicity assumption that the treatment (living in 2007 in a severely shocked area) did not make anyone in the analysis sample less likely to go on DI. To do so, I estimate the causal effect of living in 2007 on a new outcome: employed-or-on-DI, equal to an indicator for the whether the worker was employed in 2014 *or* was on DI in 2014. One minus the ratio of the employed-or-on-DI effect to the main employment effect (-0.980 percentage points) equals the share of the incrementally non-employed severely shocked workers who were on DI in 2014. This quantity equals the share of the employment effect caused by DI enrollment if every incrementally non-employed DI recipient would have been employed without DI. If on the other hand every incrementally non-employed DI recipient would have been non-employed even without DI, then DI enrollment caused 0% of the employment effect. These two numbers equal upper and lower bounds, respectively, on DI's contribution to the employment effect, under the monotonicity assumption.[44]

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

[43] DI can be awarded on a lag but DI rolls have plateaued in aggregate. After rising annually for thirty years (Autor 2011, Mueller, Rothstein and Von Wachter 2015), the share of working-age adults on disability insurance decelerated after 2010 and declined absolutely in 2014: the working-age population rose 0.6% in 2014 (https://research.stlouisfed.org/fred2/series/LFWA64TTUSM647S) while disability recipients rose by only 0.1% (http://www.ssa.gov/oact/STATS/dibStat.html).

[44] Lee (2009) makes a similar monotonicity assumption for computing treatment effect bounds under sample attrition, building on Angrist, Imbens and Rubin's (1996) no-defiers monotonicity requirement for instrumental

Table 5 column 3 displays the result: living in 2007 in a severely shocked area is estimated to have caused workers to be −0.903 percentage-points less likely to be employed or on DI in 2014. Dividing this effect by the main employment effect, only 7.8% of the incrementally non-employed severely shocked workers were on DI by 2014, and thus 7.8% is the estimated upper-bound contribution of DI transition to the enduring employment impact. The lower bound is 0%, corresponding to the case in which incremental DI transition among severely shocked workers was a response to a lack of employment rather than a cause of it.[45]

5.2 More Layoffs

A second potential channel is firm-specific human capital destruction via more workers being laid off in severely shocked areas. I proxy for layoff using unemployment insurance (UI) receipt.[46] Though imprecisely estimated, Table 5 column 6 shows that living in 2007 in a severely shocked area caused workers to be 1.194 percentage points more likely to have received UI by 2010 (i.e. at any point 2007-2010).[47] The effect size is somewhat small relative to the sample-wide rate of UI receipt by 2010 of 18.3% but large relative to the enduring employment impact of −0.980 percentage points.[48] Hence at first glance, this could suggest that more layoffs among severely shocked workers explains the enduring employment impact of Great Recession location.

Column 8 tests this possibility by controlling for UI-receipt-by-2010 in the main specification. Layoff is endogenous so this specification is not quasi-experimental. However, if one assumes

variables identification. Lee requires that treatment affects sample attrition only "in one direction"; I require that treatment affects DI receipt only in one direction.

[45]Of separate interest, Table 5 column 5 (bottom row) shows that affected elderly are estimated to have transitioned entirely to Social Security retirement benefits. Retirement benefit receipt likely does not raise reservation wages (Coile and Levine 2007) since labor earnings do not reduce net retirement benefits on average. Even if one treats all SSA programs as potential causes of non-employment, the second-to-last row of column 5 implies that transition to SSA programs explains at most 10.8% (= 1−.827/.926) of the effect for the non-elderly (aged 61 and younger in 2014) bulk of the sample.

[46]Earlier papers typically proxy for layoff using firm separation during a firm's large downsizing; my measure does not condition on large downsizing. Kawano and LaLumia (forthcoming) show that UI-tax-data-based unemployment rates are close in both level and trend to official Bureau-of-Labor-Statistics unemployment rates 1999-2011 (correlation 0.94).

[47]Analysis of UI receipt by 2008, 2012, or 2014 yields qualitatively similar results (Table 5 and Online Appendix Table 1). 2014 is most comprehensive but 2014 UI receipt is mechanically very *positively* correlated with 2014 employment, obscuring understanding of the next paragraph's analysis.

[48]UI receipt rates are generally high: in the full population, the number of people who received UI at some point 2000-2010 equals 24.8% of number of people who received a W-2 at some point 2000-2010.

that the laid-off severely shocked workers were on average equal or stronger on unobservables than laid-off mildly shocked workers—as would be expected in a layoffs-and-lemons model (Gibbons and Katz 1991)—then one can rule out large effects of the more-layoffs channel if controlling for UI-receipt-by-2010 does not attenuate the estimated effect of Great Recession location, assuming homogenous layoff effects.[49]

Column 8 shows that controlling for UI-receipt-by-2010 attenuates the employment effect of living in 2007 in a severely shocked CZ by only 0.02 percentage points (−0.960 versus −0.980). This indicates that the higher layoff rate among severely shocked workers does not explain the employment effect, assuming homogenous layoff effects. To understand this result, note that multiplying the coefficient on UI receipt in column 8 (−1.751) by the higher rate of UI receipt in column 6 (−1.751) yields the 0.02-percentage-point contribution of the higher layoff rate to the employment rate deficit. In words, layoff is negatively correlated with employment but not nearly enormously enough for a two-percentage-point-higher layoff rate to explain a one-percentage-point employment rate deficit.[50]

5.3 Costlier Layoffs

Differences in the number of layoffs is not the only channel through which layoffs could explain the enduring employment impact of Great Recession location: a given layoff could be costlier for workers in severely shocked areas. Analogous to the DI analysis above, one can place an upper bound on combined layoff effects (both more layoffs and costlier layoffs) by analyzing the outcome of whether a worker was employed in 2014 or was laid off by 2014, under the mild monotonicity assumption that the treatment (living in 2007 in a severely shocked CZ) did not

[49]That is, I assume that layoffs are either random, or that the first workers to get laid off from each establishment had the worst unobservables and that severely-shocked-CZ establishments had weakly higher layoff rates (column 6). Then laid-off severely shocked workers had weakly stronger unobservables than laid-off mildly shocked workers. Thus if layoffs are equally costly everywhere, the UI receipt coefficient weakly overstates the layoff effect for severely shocked workers, implying that the severely shocked location coefficient weakly understates its non-layoff component. This analysis cannot rule out heterogeneous effects: that the incrementally laid-off severely shocked workers would have experienced *larger*-than-average layoff effects anywhere.

[50]Economically, note that layoff is not the primary way for someone to become non-employed (voluntary churn is large). For example, 73.5% of workers in this sample had separated from their 2006 firms by 2014 (defined as not have a 2014 W2 from their 2006 employer) while only 26.9% had received UI by 2014.

make anyone in the sample less likely to be laid off. Table 5 column 11 shows that the effect of living in 2007 in a severely shocked CZ on the indicator employed-in-2014-or-UI-receipt-by-2014 is −0.278 with a standard error of 0.255. Dividing this effect by the main −0.980 percentage-point effect on employment, 71.7% of the incrementally non-employed severely shocked workers had collected UI by 2014, and thus 71.7% is the estimated upper-bound contribution of layoff to the enduring employment impact with a standard error that does not reject 100%. Hence, it is possible that most or even all of the enduring employment impact of Great Recession location is due to costlier layoffs in severely shocked areas.

However, the lower bound on the contribution of the costlier layoff channel naturally equals 0%, corresponding to the case where layoff did not cause net non-employment. For example, suppose that frictions and persistently low labor demand in severely shocked areas (see the next subsection) induced a fixed level of rationed employment (e.g. Michaillat 2012) in severely shocked areas in 2014. Then the 71.7% figure is consistent with layoffs simply determining which severely shocked workers were non-employed, but not how many were non-employed.

5.4 Continued Area-Specific Depression

Instead of the Great Recession driving process scarring workers, it is possible that most severely shocked workers have stayed in severely shocked areas and that severely shocked areas are continuing to depress their current (2014) residents' employment. The ideal experiment for identifying continued area-specific depression would be exogenously moving workers across CZs after 2007. Lacking that experiment but leveraging the numerous endogenous cross-CZ moves observed in the data, I now present cross-sectional estimates of the effect of living in *2014* in a severely shocked CZ and compare it to the effect of living in *2007* in a severely shocked CZ. Throughout this subsection, CZs continue to be defined as severely shocked CZs or mildly shocked CZs using 2007-2009 CZ shocks. I define movers as workers who lived in 2014 in a CZ that was different from their 2007 CZ. All told, 18.6% of workers moved across CZs, and 42.7%

of movers moved from a severely shocked CZ to a mildly shocked CZ or vice versa.[51]

Table 6 columns 3-6 extend the main quasi-experimental specification of Table 2 column 8 to include in the regression an indicator for whether the worker was living in 2014 in a severely shocked CZ. The specifications also either control for whether the worker moved or limit the sample to movers only. In each of these cross-sectional regressions, the 2014 employment effect of living in 2007 in a severely shocked CZ is similarly sized or insignificantly smaller than the effect of living in 2014 in a severely shocked CZ. For example, column 6 shows that when limiting the sample to movers only, living in a severely shocked CZ in 2007 is associated with a 0.286-percentage-point lower 2014 employment rate, while living in a severely shocked CZ in 2014 is associated with a 0.764-percentage-point-lower 2014 employment rate. Hence, most of the 2014 employment impact statistically loads on where a worker was currently living (in 2014) rather than where she used to live (in 2007).

If moving decisions were random, the coefficients in Table 6 would reflect causal effects and would indicate that persistence in area-specific depression is likely equally or more important than worker scarring. However, moving decisions may have been correlated with unobserved employment determinants. For example, housing costs tend to decline after adverse local shocks (Blanchard and Katz 1992), and retirees and others secularly out of the workforce may have disproportionately stayed in or moved to severely shocked areas in order to enjoy low costs of living while secularly foregoing employment (Notowidigdo 2011). Such selective post-2007 migration could generate the results displayed in Table 6 even if the enduring employment impact of Great Recession location were entirely due to worker scarring.[52]

[51]Severely shocked workers were slightly more likely to move: 19.2%, relative to 18.0% of mildly shocked workers. 44.8% of severely shocked movers moved to a mildly shocked CZ while 40.5% of mildly shocked movers moved to a severely shocked CZ. See Online Appendix Figure A.5 for a visualization.

[52]In unreported results, I find that instruments for moving and for moving destination—based on pre-2007 age-gender-CZ-specific moving patterns as in Card (2001)—deliver extremely noisy estimates with a qualitative pattern similar to those listed in Table 6.

5.5 Discussion

This section has found evidence consistent with two distinct channels for the enduring employment impact of Great Recession location: costlier layoffs in severely shocked areas and continued area-specific employment depression in severely shocked areas. I close this section by discussing these results with reference to previous work.

The costlier layoffs channel is consistent with recent evidence (Davis and Von Wachter 2011) arguing that long-term earnings losses following layoffs (Ruhm 1991, Jacobson, LaLonde and Sullivan 1993, Neal 1995, Couch and Placzek 2010) are larger when the layoffs occur during aggregate recessions. The same could be true of employment losses following layoffs that occur during severe local recessions, although Song and Von Wachter (2014) do not find differential employment effects across aggregate recessions in the same data as Davis and Von Wachter. To the extent that costlier layoffs is the channel, this paper advances the layoff literature by countering the central confoundedness critique leveled against it: that laid-off workers are unobservably worse than other workers (Gibbons and Katz 1991), so low earnings among laid-off workers may merely reflect selection (Hall 2011).[53] My analysis did not condition on layoff in any way but still finds long-term losses.

Qualitatively, this paper augments the layoff literature by finding long-term *employment* losses. Autor, Dorn, Hanson and Song (2014) find no long-term employment losses from Chinese trade competition. Walker (2013) finds no long-term employment losses from environmental-regulation-driven plant downsizings. Jacobson, LaLonde and Sullivan (1993) and Couch and Placzek (2010) rule out employment effects by conditioning on post-layoff employment. Ruhm (1991) and Neal (1995) find no enduring employment losses in small samples. Recently, an unpublished working paper by Von Wachter, Song and Manchester (2009) finds enduring employment losses after early-1980s mass layoffs. This paper bolsters that rare finding, without the endogenous-layoffs concern.

However, the precise mechanism underlying a costlier layoffs channel is yet unclear. General

[53] Autor, Dorn, Hanson and Song (2014) and Hilger (2014) find negative selection into mass-layoff firms, so layoff-based shock definitions at both the individual level and the firm level may face endogeneity concerns.

human capital decay (Acemoglu 1995, Ljungqvist and Sargent 1998) may be relatively unlikely in this setting. Effects are largest among initially low-earners in this retail worker sample, who were likely performing tasks like making coffee and serving customers (e.g. Starbucks baristas), and the human capital required to perform such work may not decay. Alternatively, employers may infer that long non-employment spells signal low unobserved productivity (Vishwanath 1989, Lockwood 1991), though that effect is smallest in slack labor markets (Kroft, Lange and Notowidigdo 2013). Perhaps most plausibly, laid-off workers in severely shocked areas may have become discouraged (Bowen and Finegan 1969 Part III), lost their taste for work or their habits conducive to work (Phelps 1972 p.79), or otherwise raised their reservation wages (Krueger, Cramer and Cho 2014).

The continued area-specific depression channel is consistent with two types of mechanisms.[54] First, local Great Recession driving processes could be persistent in that the underlying causes that gave rise to severe 2007-2009 local shocks subsequently gave rise to severe shocks 2010-2014 in the same local areas (Hall 1992) without raising local unemployment. Second, severe 2007-2009 shocks may have moved local areas to low-employment equilibria as in multiple equilibria models (Diamond 1982, Blanchard and Summers 1986, Benhabib and Farmer 1994, Christiano and Harrison 1999, Eggertsson and Krugman 2012, Jaimovich and Siu 2013, Kaplan and Menzio 2014). Multiple equilibrium mechanisms are difficult to test, though some predictions are consistent with cross-sectional data since the Great Recession (e.g. Jaimovich and Siu, Kaplan and Menzio). My results advance that work by rejecting the natural alternative explanation of nationwide skill-specific shocks unrelated to the Great Recession. The continued area-specific depression channel may offer an avenue for reconciling local employment rate persistence following the Great Recession with rapid local employment rate recovery following earlier local shocks (Blanchard and Katz 1992)—if Great Recession local shocks were shocks to non-traded-good demand (see Section 4.5) while historical local shocks were shocks to local traded-good

[54]These mechanisms require persistence in workers' locations, which has always held. Every decennial census 1900-2010 shows that over two-thirds of adults live in their birth state (Molloy, Smith and Wozniak 2011). The Health and Retirement Study shows that half of adults live within eighteen miles of their mothers (http://www.nytimes.com/interactive/2015/12/24/upshot/24up-family.html).

demand, with different effects.

Further work distinguishing between the costlier layoffs channel and the continued area specific depression channel would be valuable. For example, future work could develop valid instruments for 2013 migration from severely shocked areas to mildly shocked areas, thereby quasi-experimentally varying 2014 location across otherwise identical severely shocked workers. The costlier layoffs channel predicts that the impact of Great Recession location on 2014 employment would be independent of quasi-experimentally-varied 2014 location, while the continued area-specific depression channel predicts that the impact would be dependent.

6 Conclusion

This paper has found in a sample of two million retail chain workers that starting the Great Recession in a below-median 2007-2009-employment-shock area caused workers to be 1.0 percentage points less likely to be employed in 2014, relative to starting the recession elsewhere. The effect is similar in magnitude to the aggregate demographics-adjusted point-in-time employment rate decline, is concentrated among the low-skilled just like the aggregate decline, and likely reflects labor force exit just like the aggregate decline. These findings have two implications for understanding the U.S. labor market.

First, they demonstrate long-term limits to U.S. local labor market integration: ex ante similar individuals experiencing dissimilar long-term outcomes across space (cf. Rosen 1979, Roback 1982, Blanchard and Katz 1992).[55] In contrast to earlier work on long-term shock adjustment, my within-skill cross-area design isolated area-specific impacts and thus could reject full spatial integration. Limited integration recommends the perspective that "In the same sense that all politics is local, it might be said that all macroeconomics is regional" (Eichengreen 1992): local employment rate differences can reflect not just economic geography but also enduring causal impacts of location on employment and distribution.

[55]Rosen-Roback models assume frictionless mobility and full employment and thus rule out area-specific effects. Kline (2010) and Moretti (2011) allow mobility frictions and thus area-specific effects on real income (Busso, Gregory and Kline 2013) while still assuming full employment.

Second, the results suggest that the Great Recession and its underlying causes have continued to depress employment rates via labor force exit even after unemployment rates had returned to normal. An alternative explanation for post-2007 employment rate declines is that nationwide skill-specific shocks independent of the recession, such as secular skill-biased technical and trade changes, have induced labor force exit. Yet my within-skill cross-area design rejects independent nationwide shocks as a full explanation of persistent local employment rate declines. Furthermore, local labor demand shocks typically elevate local unemployment, but local unemployment rates steadily and nearly completely converged between severely and mildly shocked areas 2010-2014. Hence unless severely shocked areas experienced independent local shocks 2010-2014 that depressed participation without elevating unemployment, the results indicate that local Great Recession driving processes caused enduring local non-employment via labor force exit. Extrapolation of this qualitative conclusion to the aggregate Great Recession driving process and aggregate non-employment is plausible but uncertain.[56]

Economists have long recognized that local or national cyclical shocks may cause labor force exit.[57] However, a wide class of cyclical employment models assume an exogenous labor force and restrict attention to unemployment (Barro and Grossman 1971, Mortensen and Pissarides 1999). These models naturally treat shock adjustment as complete once unemployment recovers, abstracting from whether coincident labor force changes would have occurred in the shock's absence. My within-skill results provide new empirical support to augmented models in which worker scarring, multiple equilibria, or some other mechanism allows cyclical shocks to persistently depress employment via labor force exit, even after unemployment has recovered.

[56]See Nakamura and Steinsson (2014), Mian and Sufi (2014), and Beraja, Hurst and Ospina (2016) for general equilibrium examples of aggregate shock effects possessing the same sign as local shock effects. Aggregate effects may be smaller or larger than local effects. For example if out-migration enables local workers to diffuse the incidence of local shocks, then the aggregate impact of an aggregate shock may be larger than the local impact of a proportionately sized local shock. The aggregate recession may reflect a different source (e.g. Baker, Bloom and Davis 2016) from its local variation (e.g. Mian and Sufi), undermining qualitative extrapolation.

[57]For example: "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" (Mortensen and Pissarides 1999). Exceptions include Burdett, Kiefer, Mortensen and Neumann (1984) and Pissarides (2000).

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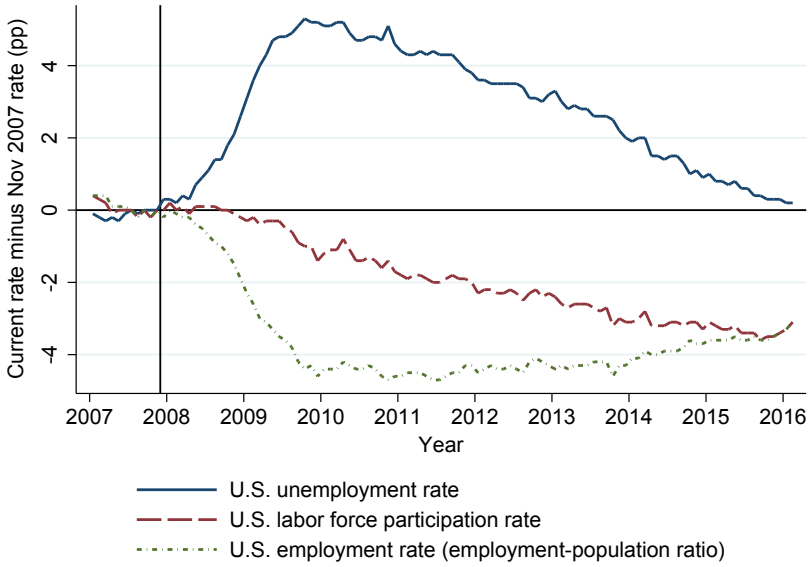
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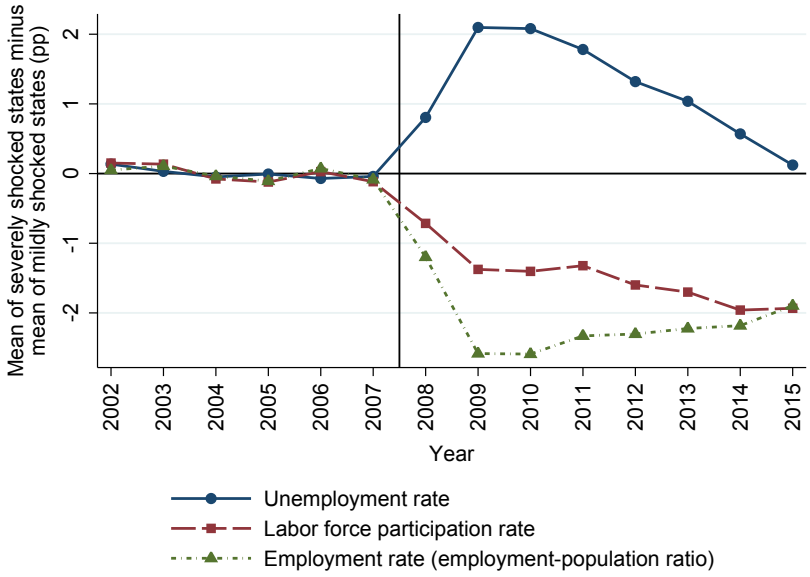
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Figure 1: Persistent Employment Rates Declines after the Great Recession

A. Current U.S. Aggregate Minus November 2007 U.S. Aggregate

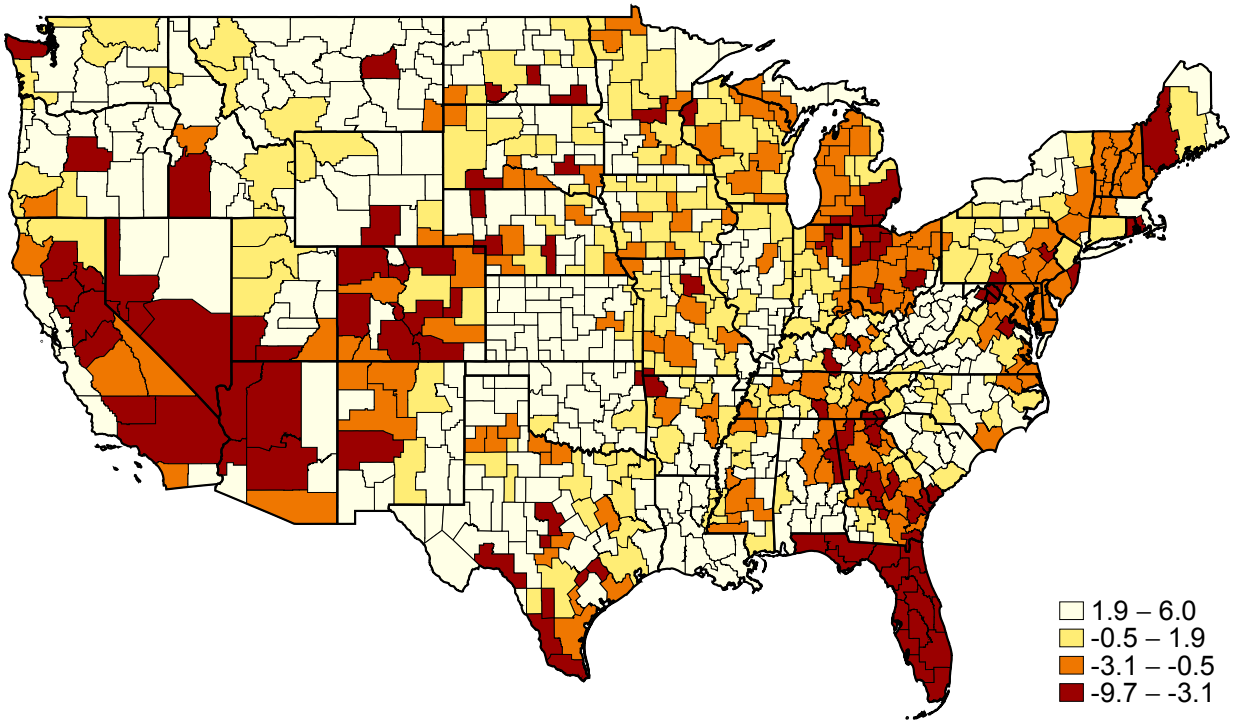


B. Severely Shocked States Minus Mildly Shocked States



Notes: Panel A plots the official seasonally adjusted Bureau of Labor Statistics U.S. labor force statistics from January 2007 through February 2016. The data are monthly and refer to the adult (16+) civilian non-institutional population. The vertical black line denotes December 2007, the first month of the Great Recession. For each outcome and month, the graph plots the current value minus the November 2007 value, so each data point in these series denotes a percentage-point change relative to November 2007. For specific numbers underlying the end points: the unemployment rate was 4.7% in November 2007 and 4.9% in February 2016; the labor force participation rate was 66.0% in November 2007 and 62.9% in February 2016; and the employment rate (employment-population ratio) was 62.9% in November 2007 and 59.8% in February 2016. Panel B divides U.S. states into severely (below-unweighted-median) and mildly (above-unweighted-median) shocked states based on 2007-2009 state-level employment shocks. Each state's shock equals the sum of 2008 and 2009 employment growth forecast errors derived from the autoregressive system of Blanchard and Katz (1992). For each outcome and year, the graph plots 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. The underlying data are the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) annual state-level counts of employment, unemployment, labor force, and population.

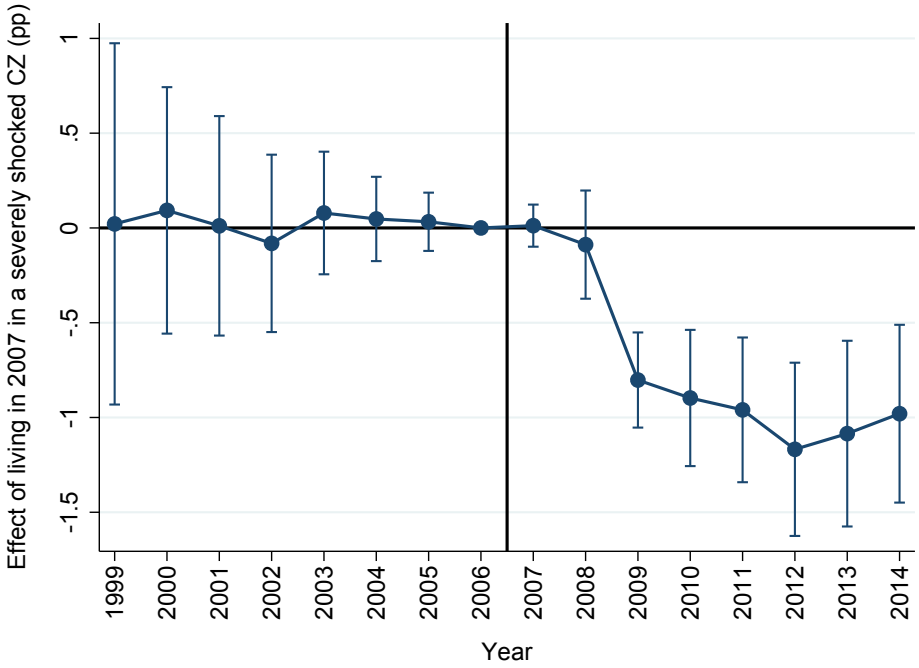
Figure 2: 2007-2009 CZ Shocks



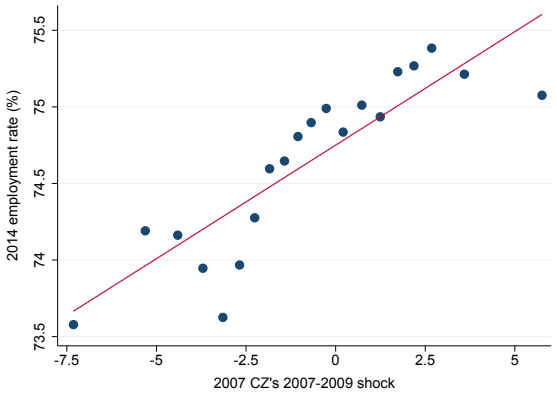
Notes: This color-coded map depicts quartiles (weighted by main analysis sample counts) of 2007-2009 Commuting Zone (CZ) level shocks. Each CZ's shock equals the worker's 2007 CZ's detrended log employment change 2006-2009, normalized around zero by the detrended aggregate change. Specifically it equals the log 2006-2009 employment change in the worker's 2007 CZ minus the log 2000-2003 employment change in the worker's 2007 CZ, minus a normalizing constant: the difference between the log 2006-2009 aggregate employment change and the log 2000-2003 aggregate employment change. The underlying data used to compute CZ shocks comprise all individuals in the longitudinal data aged 25-75 in the current year with a continental U.S. ZIP code from information returns in the current year. Employment is defined as the number of workers with positive W-2 wage earnings or positive 1099-MISC independent contractor earnings. Plotted shock values are top-coded at the main-analysis-sample-weighted 95th percentile and bottom-coded at the main-analysis-sample-weighted 5th percentile, corresponding to their use in Figure 3B.

Figure 3: Employment Impacts of Great Recession Location

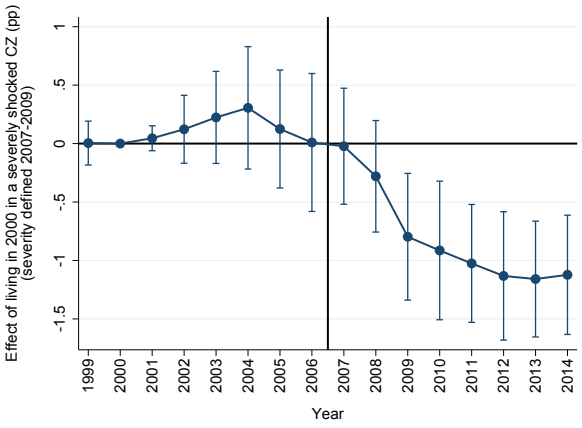
A. Employment Effect of Living in 2007 in a Severely Shocked CZ



B. Mean 2014 Employment Versus 2007 CZ Shock

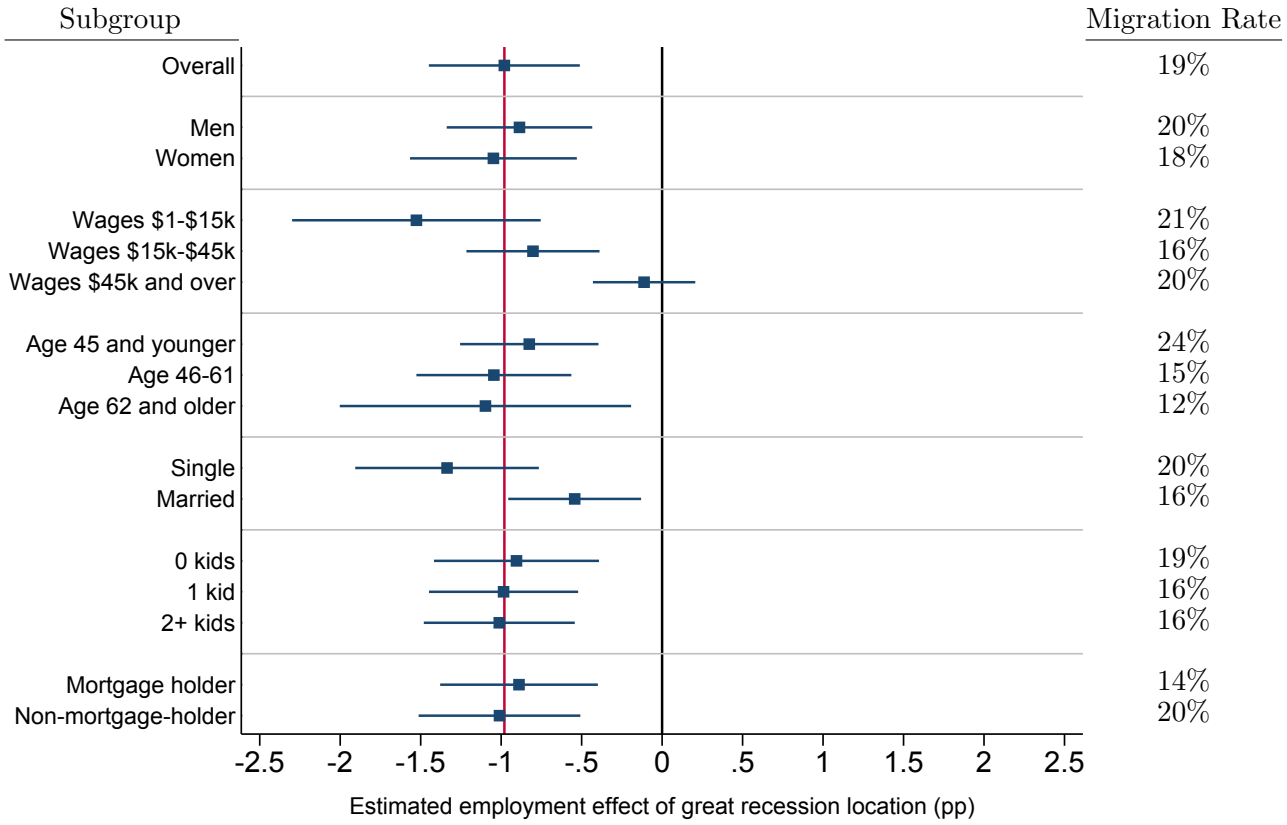


C. Employment Impact of Living in 2001 in a Severely Shocked CZ



Notes: Panel A plots regression estimates of the effect of living in 2007 in a severely shocked (below-median-shock) CZ on annual employment in the main analysis sample conditional on firm-by-wages fixed effects and demographic controls (age fixed effects; a quartic in 2006 wage earnings; indicators for being female, a 2006 mortgage holder, a 2006 1040-filer, married in 2006, and having zero, one, or two-or-more dependent kids in 2006; and quartics in the worker's 2007 CZ's high-school dropout share, college graduate share, black share, and Hispanic share). The dependent variable is an indicator for whether the worker had positive W-2 wage earnings or positive 1099-MISC independent contractor earnings in the calendar year. 95% confidence intervals are plotted around estimates, clustering on 2007 state. Panel B non-parametrically depicts the relationship between workers' 2014 employment and their 2007 CZ shock by regressing 2014 employment and 2007 CZ shock on the controls listed above, computing residuals, adding back their means for interpretation, and plotting means of the 2014 employment residuals within twenty equal-sized bins of the shock residuals. Overlaid is the best-fit line (slope 0.148, standard error 0.033 clustered on 2007 state). Panel C replicates panel A on a sample identical to the main analysis sample except that it is constructed using year-*2000* retail chain workers. Specifically, I apply the same restrictions except that all year-based restrictions use a year subtracted by six; for example, age is defined as of December 31, 2000, for the 25-75 age restriction. Severely shocked CZ still refers to severe-2007-2009-shock CZ.

Figure 4: Heterogeneity in the Enduring Employment Impact



Notes: This graph plots coefficients and 95% confidence intervals of the effect of living in 2007 in a severely shocked CZ on workers' 2014 employment rates—overall (equal to the 2014 data point in Figure 3A) and by subgroup. All estimates derive from the specification underlying the 2014 data point in Figure 3A. Subgroup estimates restrict the sample to the specified subgroup defined by gender, 2006 wage earnings, 2014 age, 2006 marital status, 2006 number of dependent kids, or 2006 mortgage holding. The marital status and number of kids specifications are restricted to 1040 filers. Standard errors are clustered by 2007 state. Subgroup migration rates are superimposed on the right, where migration is defined as one's 2014 CZ being different from one's 2007 CZ.

TABLE 1
Summary Statistics: Worker-Level Data

	Main Analysis Sample		Full Population (1% Sample)	
	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)
Outcomes (in 2014)				
Employed (%)	74.6	43.5	63.3	48.2
DI receipt (%)	5.7	23.2	5.2	22.2
Any SSA receipt (%)	20.0	40.0	28.9	45.3
UI receipt sometime 2007-2014 (%)	26.9	44.4	21.0	40.8
Personal characteristics (in 2006, 2007)				
Female (%)	60.2	49.0	49.7	50.0
Wage earnings ($)	27,425	31,163	36,558	50,887
Age	41.2	12.2	46.5	13.1
Aged 25-29 (%)	22.5	41.7	11.3	31.7
Aged 30-39 (%)	27.2	44.5	22.7	41.9
Aged 40-49 (%)	24.3	42.9	25.5	43.6
Aged 50-59 (%)	16.9	37.5	21.9	41.4
Aged 60-75 (%)	9.1	28.7	18.6	38.9
Married (%)	48.4	50.0	63.3	48.2
0 current dependent kids (%)	56.1	49.6	55.6	49.7
1 current dependent kid (%)	19.6	39.7	18.5	38.9
2+ current dependent kids (%)	24.3	42.9	25.9	43.8
1040 filer (%)	92.3	26.7	89.3	30.9
Mortgage holder (%)	22.8	42.0	34.7	47.6
Firm in retail trade (NAICS 44,45) (%)	83.7	36.9		
Firm in accommodation and food (NAICS 72) (%)	16.3	36.9		
Firm in general merchandise (NAICS 452) (%)	29.8	45.8		
Firm in grocery (NAICS 445) (%)	19.1	39.3		
Firm in restaurants (NAICS 722) (%)	13.4	34.1		
2007 CZ's Great Recession shock (%)	-0.8	4.0	-0.3	3.7
Severely shocked 2007 CZ (%)	49.9	50.0	53.9	49.8
Number of people	2,238,310		1,576,940	
Number of 2006 firms	816			
Number of 2007 CZs	659		659	
Number of 2007 states	49		49	

Notes - This table lists summary statistics for the paper's main analysis sample and also for a 1% random sample of all people satisfying the main analysis sample restrictions except the firm-based ones. Employed is an indicator for having positive W-2 wage earnings or positive 1099-MISC independent contractor earnings in the calendar year. DI receipt is an indicator for having positive 1099-SSA disability insurance income in the calendar year. Any SSA receipt is an indicator for having positive 1099-SSA income in the calendar (typically reflecting retirement benefits or disability insurance). UI receipt sometime 2007-2014 is an indicator for having positive 1099-G unemployment insurance benefit income at some point 2007-2014. 2006 wage earnings equals the worker's total 2006 W-2 wages plus total 1099-MISC independent contractor earnings inflated to 2010 dollars and locally deflated using Local CPI 2 as in Moretti (2013) and then winsorized (top-coded) at $500,000. Age is age as of December 31, 2006. 2006 married is an indicator for filing a married-filing-jointly or married-filing-separately 1040 for tax year 2006 (statistics shown only for 1040-filers). Current dependent kids is the number of current dependent kids currently living with the worker as listed on the filed 1040 (statistics shown only for 1040-filers). 1040 filer is an indicator for having appeared as a primary or secondary filer on a Form 1040 for tax year 2006. Mortgage holder is an indicator for having positive mortgage payment listed on a Form 1098 in 2006. Firm refers to the masked Employer Identification Number (EIN) on the worker's highest-paying 2006 W-2, matched to industry on a business income tax return filed by the EIN. 2007 CZ derives from the worker's January 2007 residential location as reflected most commonly on her 2006 information returns. 2007 CZ's Great Recession shock equals the log 2006-2009 employment change in the worker's 2007 CZ minus the log 2000-2003 employment change in the worker's 2007 CZ, minus a normalizing constant and winsorized (bottom-coded and top-coded) at the sample-weighted 5th and 95th percentiles. Severely shocked 2007 CZ is an indicator for the worker's 2007 CZ's shock being below the sample median. 2007 state is the state with most or all of the 2007 CZ's population. The mortgage holder share is lower than the U.S. adult home ownership rate: the sample is younger and poorer than the U.S. as a whole, the mortgage holder share excludes home owners without a mortgage, and mortgages held only in the name of a worker's spouse or other third party are not included here.

TABLE 2
Enduring Employment Impacts of Great Recession Location

Outcome:	Employed in 2014 minus employed in 2006			Employed in 2014						Cumulative employment 2009-2014		Employed 1999-2005	Employed in 2007
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)	(pp) (6)	(pp) (7)	(pp) (8)	(pp) (9)	(pp-yr) (10)	(pp-yr) (11)	(pp) (12)	(pp) (13)
Severely shocked 2007 CZ	-1.043 (0.233)	-1.080 (0.236)	-1.235 (0.274)	-1.843 (0.693)	-0.998 (0.415)	-0.951 (0.291)	-1.044 (0.237)	-0.980 (0.239)	-1.784 (0.399)	-5.899 (1.140)	-13.291 (2.057)	0.029 (0.233)	0.012 (0.057)
Age FEs		X	X		X	X	X	X	X	X	X	X	X
Other demographics			X			X	X	X	X	X	X	X	X
2006 firm FEs							X						
2006 firm FEs × 2006 wages FEs								X	X	X	X	X	X
Main analysis sample				X	X	X	X	X	X	X	X	X	X
Top+bottom shock deciles only									X		X		
N	1,576,940	1,576,940	1,576,940	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	446,169	2,238,310	446,169	2,238,310	2,238,310
R^2	0.00	0.04	0.08	0.00	0.12	0.19	0.20	0.20	0.23	0.25	0.27	0.19	0.16
Outcome mean	63.3	63.3	63.3	74.6	74.6	74.6	74.6	74.6	72.9	475.8	468.0	89.5	93.8
Estimate divided by outcome mean (%)	-1.65	-1.70	-1.95	-2.47	-1.34	-1.27	-1.40	-1.31	-2.45	-1.24	-2.84	0.03	0.01

Notes – This table displays estimates of the effect of living in 2007 in a severely shocked CZ on 2014 employment in the 1% population sample (columns 1-3) and in the main analysis sample (columns 4-9) conditional on the listed controls and sample restrictions. Age fixed effects comprise birth year fixed effects. Other demographics comprise indicators for being a 2006 mortgage holder, being a 2006 1040 filer, being married in 2006, and having zero, one, or two-or-more kids in 2006; a quartic in 2006 wage earnings; and quartics in the worker's 2007 CZ's high-school dropout share, college graduate share, black share, and Hispanic share. 2006 firm FEs × 2006 wages FEs are interactions between indicators for the worker's 2006 firm and sixteen bins of the worker's 2006 wage earnings earned from her 2006 firm. See Table 1 for the definition of annual employment, which is denominated in percentage points and thus ranges from 0 to 100. Cumulative employment 2009-2014 equals the sum of the worker's 2009-2014 employment indicators in percentage points and thus ranges from 0 to 600. The last two columns constitute placebo tests. Employed 1999-2005 equals the mean of the worker's 1999-2005 employment indicators in percentage points and thus ranges from 0 to 100. Standard errors are clustered by 2007 state.

TABLE 3
Robustness of Enduring Employment Impacts of Great Recession Location

Outcome:	Employed in 2014												
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)	(pp) (6)	(pp) (7)	(pp) (8)	(pp) (9)	(pp) (10)	(pp) (11)	(pp) (12)	(pp) (13)
Severely shocked 2007 CZ	-0.980 (0.239)	-0.984 (0.225)	-0.932 (0.230)	-0.938 (0.245)	-1.072 (0.215)	-0.925 (0.253)	-0.738 (0.206)	-0.611 (0.225)	-0.860 (0.199)				-1.542 (0.440)
2007 CZ's shock										0.148 (0.033)			
Severely shocked 2007 CZ, emp. rate defn.											-1.309 (0.294)		
2007 CZ's shock, emp. rate defn.												0.969 (0.167)	
Main controls	X	X	X	X	X	X	X	X	X	X	X	X	X
Employment history		X											
CZ size			X										
CZ pre-2007 size growth				X									
Cross-CZ commuting					X								
Max UI duration 2007-2014						X							
2014 local unemployment rate (CPS)							X						
2014 local unemployment rate (LAUS)								X					
End-of-2014 local unemployment rate (CPS)									X				
Instrumented with birth state shock													X
N	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310
R^2	0.20	0.21	0.20	0.20	0.20	0.20	0.20	0.21	0.20	0.20	0.20	0.20	0.20
Outcome mean	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6	74.6
Estimate divided by outcome mean (%)	-1.31	-1.32	-1.25	-1.26	-1.44	-1.24	-0.99	-0.82	-1.15	0.20	-1.75	1.30	-2.07

Notes – This table presents estimates of the effect of living in 2007 in a severely shocked CZ on 2014 employment in the main analysis sample, with various additional controls or alternative shock measures. Column 1 replicates the main specification (Table 2 column 8). Column 2 controls for each worker's employment history (indicators for employment in each year 1999-2005). Column 3 controls for a quartic in the worker's 2007 CZ size, equal to the CZ's total employment in 2006 as reported in Census's County Business Patterns (CBP). Column 4 controls for a quartic in the worker's 2007 CZ's size growth, equal to the CZ's log change in CBP employment from 2000 to 2006. Column 5 controls for a quartic in the worker's 2007 CZ's share of workers who work outside of the CZ, computed from the 2006-2010 American Community Surveys. Column 6 controls for a quartic in the worker's 2007 CZ's state's maximum unemployment insurance duration over years 2007-2014. Column 7 controls for a quartic in the worker's 2014 state's 2014 unemployment rate (averaged across all twelve months of 2014 based on the Current Population Survey), specific to the worker's gender and people 33 years and older (corresponding to the main analysis sample's 2014 age minimum). Column 8 controls for a quartic in LAUS-based gender-and-age-unspecific CZ-level unemployment rates. Column 9 controls for the same quartic as in column 7 except that the unemployment rate is averaged across the last two months of 2014. Column 10 replaces the severe-shock indicator with the worker's 2007 CZ shock, a continuous measure of shock intensity, where CZ shocks are winsorized (bottom-coded and top-coded) at the sample-weighted 5th and 95th percentiles. Columns 11 and 12 replicate columns 1 and 10 using an alternative employment-rate-based definition of CZ shocks, equal to the 2006-2009 percentage-point change in the CZ's employment rate minus the 2000-2003 percentage-point change in the CZ's employment rate in the same data used to compute the main CZ shock measure. Column 13 instruments for living in 2007 in a severe-shock CZ using the mean of the binary CZ severity variable in the worker's birth state. Standard errors are clustered by 2007 state.

TABLE 4
Correlates of Employment Impacts of Great Recession Location

Outcome:	2007-CZ Effect on 2014 Employment				2007-CZ Effect on 2009 Employment			
	(sd) (1)	(sd) (2)	(sd) (3)	(sd) (4)	(sd) (5)	(sd) (6)	(sd) (7)	(sd) (8)
Overall shift-share (Bartik) shock	0.305 (0.076)				0.260 (0.057)			
Manufacturing-specific shift-share (Bartik) shock		0.031 (0.084)				0.071 (0.089)		
Construction-specific shift-share (Bartik) shock			0.351 (0.077)				0.236 (0.082)	
House-price-driven net worth change				0.377 (0.089)				0.253 (0.140)
N	363	363	363	363	363	363	363	363
R^2	0.09	0.00	0.12	0.14	0.07	0.01	0.06	0.06

Notes – This table presents coefficient estimates and standard errors clustered by 2007 state from univariate regressions of CZ-level employment effects on CZ-level measures of employment shocks, weighted by 2007 population in the main analysis sample. All measures are standardized to have weighted mean zero and standard deviation one, so the displayed regression coefficients are weighted correlation coefficients. A positive coefficient indicates that adverse shocks are correlated with large employment declines. For columns 1-4, the dependent variable is point estimates of 2007 CZ fixed effects on 2014 employment, using the paper's main specification (underlying Table 2 column 8) except with the severe-shock indicator replaced with 2007 CZ fixed effects. For columns 5-8, I follow the same process except that the outcome is 2009 employment. Each CZ's shift-share shock is computed using County Business Pattern data as the projected 2007-2010 change in the worker's 2007 CZ based on leave-one-CZ-out nationwide changes in employment by three-digit NAICS industry categories—with changes optionally zeroed-out for either non-manufacturing (non-NAICS-310-339) or non-construction (non-NAICS-230-239) as indicated. See Online Shocks: Shift-Share Shocks for details. A CZ's house-price-driven net worth change equals the CZ's 2006-2009 log change in median house price times the 2006 value of the CZ's housing stock, divided by the CZ's 2006 household net worth—computed from Mian and Sufi (2014). The number of CZs is only 363 because house-price-driven net worth changes are available only for 363 CZs; these CZ cover 95% of the main analysis sample.

TABLE 5
Disability Insurance, Layoffs, and Enduring Employment Impacts of Great Recession Location

A. Disability Insurance and Any SSA Receipt

Outcome:	Employed in 2014	DI receipt in 2014	Employed in 2014 or DI receipt in 2014	Any SSA receipt in 2014	Employed in 2014 or any SSA receipt in 2014
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)
All ages	-0.980	-0.038	-0.904	0.041	-0.665
	(0.239)	(0.095)	(0.233)	(0.092)	(0.178)
33-45 years old	-0.825	0.028	-0.752	0.018	-0.745
	(0.219)	(0.079)	(0.218)	(0.081)	(0.214)
45-61 years old	-1.028	-0.091	-0.934	-0.133	-0.936
	(0.248)	(0.140)	(0.229)	(0.130)	(0.220)
33-61 years old	-0.926	-0.019	-0.832	-0.039	-0.827
	(0.220)	(0.101)	(0.214)	(0.096)	(0.207)
62-83 years old	-1.097	-0.103	-1.087	0.272	0.040
	(0.462)	(0.112)	(0.442)	(0.315)	(0.155)

B. Layoffs

Outcome:	UI receipt sometime 2007-2010	UI receipt sometime 2007-2014	Employed in 2014	Employed in 2014	Employed in 2014 or UI receipt sometime 2007-2010	Employed in 2014 or UI receipt sometime 2007-2014
	(pp) (6)	(pp) (7)	(pp) (8)	(pp) (9)	(pp) (10)	(pp) (11)
Severely shocked 2007 CZ	1.194	1.513	-0.960	-0.996	-0.432	-0.278
	(1.037)	(1.342)	(0.229)	(0.247)	(0.209)	(0.255)
UI receipt sometime 2007-2010			-1.751			
			(0.263)			
UI receipt sometime 2007-2014				0.531		
				(0.283)		
Main controls	X	X	X	X	X	X
N	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310
R^2	0.07	0.08	0.20	0.20	0.20	0.20
Outcome mean	18.3	26.9	74.6	74.6	79.2	80.6

Notes – This table displays estimates of the effect of living in 2007 in a severely shocked CZ on various employment and social insurance outcomes. All outcomes are binary indicators in percentage point terms and thus equal 0 or 100. See Table 1 for definitions. SSA receipt without DI receipt typically denotes Social Security retirement benefit receipt, explaining differences between columns 2-3 and columns 4-5. Each cell of Panel A represents a separate regression and reports the coefficient and standard error on the severely shocked indicator. Panel A column 1 replicates the specification underlying Table 2 column 8. Columns 2-5 replace the employment dependent variable of column 1 with the indicator listed in the column heading. The row heading indicates the subsample used. Each column of Panel B represents a single regression; see Online Appendix Table 1 for more permutations. Standard errors are clustered by 2007 state.

TABLE 6
Movers-Based Anaylsis of Persistence in Area-Specific Effects

Outcome:	Employed in 2014					
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)	(pp) (6)
Severely shocked 2007 CZ	-0.980		-0.514	-0.322	0.120	-0.286
	(0.239)		(0.484)	(0.314)	(0.311)	(0.282)
Severely shocked 2014 CZ		-0.949	-0.556	-0.725	-1.072	-0.764
		(0.275)	(0.528)	(0.312)	(0.288)	(0.252)
Moved across CZ's 2007-2014				-2.753	-2.359	
				(0.197)	(0.288)	
Moved across CZ's 2007-2014 × Severely shocked 2007 CZ					-0.790	
					(0.481)	
Main controls	X	X	X	X	X	X
Movers only						X
N	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	416,291
R^2	0.20	0.20	0.20	0.21	0.21	0.21
p-value: 2014 CZ effect = 2007 CZ effect			0.966	0.483	0.031	0.191

Notes – Column 1 reproduces the paper's main specification (Table 2 column 8). As in other tables, severely shocked 2007 CZ is an indicator for whether the worker lived in January 2007 in a CZ that experienced a severe 2007-2009 shock. Likewise, severely shocked 2014 CZ is an indicator for whether the worker lived in January 2014 in a CZ that experienced a severe 2007-2009 shock. Moved 2007-2014 is an indicator for the worker's January 2014 CZ being different from her January 2007 CZ. Standard errors are clustered at the 2007 state level. The last row reports p-values for rejecting the null hypothesis that the column's first two coefficients are equal.

ONLINE APPENDIX

Online Appendix: Cross-State Employment Gap

This online appendix documents the computation of the 2.23 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. $2.23 \approx .01810/2 \times 250.8 = 2.27$, where 1.810 percentage points is the population-weighted equivalent (see Online Appendix Figure A.6) to the 1.896-percentage-point severe-mild 2015 employment rate deficit plotted in Figure 1B and where 250.8 million was 2015 total population. This formula is not exact because population was not exactly evenly divided between the two state groups, since the unweighted shock median was used to define the groups. For reference, the exact 2.23 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 -0.880 percentage points. In 2015, severely shocked states had an adult civilian noninstitutional population of 142.1 million with a 58.18% population-weighted employment rate while mildly shocked states had an adult civilian noninstitutional population of 108.7 million with a 60.87% population-weighted employment rate (note that $58.18 - 60.87 + 0.88 = -1.810$). Then the full-convergence employment rate in severely shocked states (e_S^*) and in mildly shocked states (e_M^*) solve:

$$e_S^* - e_M^* = -.0088$$
$$142.1 \times e_S^* + 108.7 \times e_M^* = 142.1 \times .5818 + 108.7 \times .6087$$

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 1.118 ($= 142.09 \times (.58962 - .58175)$) 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, 1.116 ($= 108.71 \times (.60870 - .59843)$) 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 2.23-million-person employment gap between severely shocked and mildly shocked states remained in 2015.

Online Appendix: Longitudinal Data

This online appendix provides additional details on the main analysis sample frame described in Section 3.3. First, the filing ZIP code on a firm's business income tax return typically but not always refers 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 condi-

tional on baseline wages. 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. 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, 2006 W-2 payee ZIP code refers to the worker's ZIP code in January 2007. Because many workers move to large cities, almost all firms would appear to have operations in every large CZ if one were to simply use 2006 W-2 payee ZIP codes to identify CZ operations. I therefore conservatively use a separate sample stably-located workers—those with the same residential CZ in all years 2005-2007 based on the worker's 2005-2007 W-2s from the firm—to identify the regions in which each firm operated in 2006.

There are two main reasons that retail chain firms can be missing from my sample. First, many parent companies pay their workers through employer identification numbers (EINs) that are different from the parent's. If those non-parent EINs do not correspond to a legally separate subsidiary (which must file a business income tax return) or if the legally separate subsidiary does not operate in multiple CZs (e.g. if each establishment is a free-standing subsidiary), then that parent firm's workers will be excluded from the main analysis sample. Second, some firms outsource their W-2 administration to third-party payroll administration firms that list their own EINs on W-2s; because those payroll administration firms do not operate in retail, such workers will also be excluded from the main analysis sample. Nevertheless, the sample includes very large nationwide chains.

Online Appendix: Shift-Share Shocks

Table 4 uses three shift-share shocks, constructed along the lines of Bartik (1991) as follows using County Business Pattern data.

The overall shift-share shock is computed as the projected 2007-2010 change in the worker's 2007 CZ based on leave-one-CZ-out nationwide changes in employment by three-digit NAICS industry categories. That is, a CZ c's overall shift-share shock equals:

$$OVERALLSHOCK_c = \sum_j \left(\frac{E_{jc2007}}{\sum_{j'} E_{j'c2007}} \times \frac{\sum_{c' \neq c} E_{jc'2010} - \sum_{c' \neq c} E_{jc'2007}}{\sum_{c' \neq c} E_{jc'2007}} \right)$$

where j denotes a three-digit industry and E_{jct} denotes total employment in industry j in CZ c in year t.

The manufacturing-specific shift-share shock projects only leave-one-CZ-out nationwide changes in manufacturing employment:

$$MANUSHOCK_c = \sum_j \left(\frac{E_{jc2007}}{\sum_{j'} E_{j'c2007}} \times \frac{\mathbf{1}_{MANU}(j) \times \left(\sum_{c' \neq c} E_{jc'2010} - \sum_{c' \neq c} E_{jc'2007} \right)}{\sum_{c' \neq c} E_{jc'2007}} \right)$$

where $\mathbf{1}_{MANU}(j)$ is an indicator equal to one if j is a manufacturing industry (NAICS three-digit industries 310-339) and zero if not.

The construction-specific shift-share shock projects only leave-one-CZ-out nationwide changes in manufacturing employment:

$$CONSHOCK_c = \sum_j \left(\frac{E_{jc2007}}{\sum_{j'} E_{j'c2007}} \times \frac{\mathbf{1}_{CON}(j) \times \left(\sum_{c' \neq c} E_{jc'2010} - \sum_{c' \neq c} E_{jc'2007} \right)}{\sum_{c' \neq c} E_{jc'2007}} \right)$$

where $\mathbf{1}_{CON}(j)$ is an indicator equal to one if j is a construction industry (NAICS three-digit industries 230-239) and zero if not.

Each shift-share shock is demeaned and divided by its standard deviation (weighted by main analysis sample counts), so the regression coefficients in Table 4 equal correlation coefficients.

Online Appendix: Recent Trends

Online Appendix Figure A.6 reprints Figure 3A's time series of the estimated causal employment effect of living in 2007 in a severely shocked Commuting Zone (solid circles) and extends the last available (2013-2014) estimated percentage-point convergence speed forward through time (empty circles). The graph shows that at estimated current percentage-point convergence pace, convergence will occur in the year 2024—fifteen years after 2009.[58] As alternatives, the graph also plots the analogous extensions of the last-available (2014-2015) cross-*state* percentage-point convergence speeds from the employment rate series of Figure 1B; states are on pace for convergence in 2021 or 2026, depending on whether one weights by population.[59] Convergence in the 2020s would constitute more than a relative "lost decade" of depressed employment for severely shocked areas and their pre-recession residents. Linearly extending the latest percentage-point convergence speed yields faster convergence than under asymptotic convergence at the latest *percentage* convergence speed akin to autoregressive models (Blanchard and Katz 1992). However, predictions are outside the scope of this paper, and it is possible that convergence will arrive sooner or later than the 2020s.

Online Appendix: State-Level Shocks

This online appendix details the autoregressive system of Blanchard and Katz (1992, BK) used to define state-level 2007-2009 shocks in Section 2 and compares state-level adjustment to 2007-2009 shocks to historical shock adjustment.

Section 2 estimates state-level convergence 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).[60] Variable defini-

[58] For context on whether fifteen-year convergence is fast or slow, even five-year convergence can be considered slow: "My first comment [on Blanchard and Katz (1992)] is that the longer run is pretty long. The responses at the state level are at considerably lower frequencies than the national business cycle. A state or regional cycle at roughly the five-year frequency is superimposed on the generally faster national cycle." (Hall 1992).

[59] Cross-state convergence means convergence to states' pre-recession employment rate differences.

[60] 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.

tions are standard and pertain to the age-16-and-over civilian noninstitutional population.[61] 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.[62] Online Appendix Table 2 displays summary statistics.

I employ BK's canonical empirical model of state labor market outcomes to compute 2007-2009 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:

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

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 $\widetilde{\ }$ 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

[61] Age is defined at the time of survey; LAUS figures effectively evenly weight underlying monthly surveys. Employment is roughly defined as working for pay or being temporarily absent from regular work at any point in the reference week, and 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. See full definitions at http://www.bls.gov/bls/glossary.htm.

[62] 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.

follow BK in weighting states equally (e.g. not by population). Under these assumptions, the autoregressive coefficients characterize the speed of the average state's convergence to its steady state following unforecasted changes in state labor demand: coefficients close to one imply slow convergence while coefficients close to zero imply fast convergence.[63]

For each state, I estimate a 2008 and a 2009 employment growth forecast error within the BK system and refer to them as 2007-2009 state-level employment shocks. Specifically, I first estimate the BK system coefficients using sample years 1978-2007. 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 2007-2009 employment shocks and, when useful, the sum of the vector's elements as the state's 2007-2009 employment shock.[64]

To understand these shocks empirically, Online Appendix Table 3 lists each state's 2007-2009 employment shock. The standard deviation of state-level shocks over the 2007-2009 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).[65] 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 2007-2009 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 2007-2009 shock, since shocks are measured relative to the aggregate. The figure 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 2007-2009 shocks relative to other states.[66] As two focal examples, Arizona's

[63]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 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 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 (Online Appendix Figure A.1D below).

[64]The Great Recession began in December 2007; I ignore 2007 shocks for simplicity.

[65]The standard deviation of shocks is smaller outside aggregate recession years, motivating this paper's analysis of 2007-2009 shocks.

[66]The South exhibits such negative shocks in part because the system assumes relatively strong employment

shock equals -2.24% while Texas's shock equals $+1.30\%$.[67]

Online Appendix Figures A.1A-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. 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.[68] 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.

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 rather than jobs moving in or residents remaining non-employed, 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.[69] 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:[70]

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

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

growth trends based on the South's relatively strong employment growth early in the 1978-2007 sample range. The Commuting-Zone-level shocks introduced and plotted in Figure 2 use only post-2000 employment growth for detrending, when Southern employment growth was weaker.

[67]For additional reference, Online Appendix Table 4 lists LAUS-based 2007-2014 employment rate changes for the fifty largest Commuting Zones.

[68]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.

[69]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.

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

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. Note that by construction, the estimated mean actual response to a -1% shock is the negative of the predicted response to a $+1\%$ shock.

Online Appendix Figure A.1A shows that on a slight lag, mean actual relative population responded identically to 2007-2009 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 remain diverged across space at nearly their 2009 levels: for every -1% decline in relative state employment 2007-2009, the relative state employment rate was 0.47 percentage points lower in 2015 than it was in 2007. This 0.47 percentage-point employment rate deficit is nearly unchanged from the 0.48 percentage-point deficit that prevailed in 2009. Hence, employment rates have barely converged across space since 2009, 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 have converged across space as in the historical benchmark, while actual participation rates remain diverged in a stark departure from the historical benchmark.

Online Appendix Figure A.1C shows that updating the historical benchmark to more recent data does not alter the conclusion that post-2007 employment rate convergence is 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.[71] 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 64% employment rate convergence 2009-2015.[72] In contrast, the mean actual employment rate series (reprinted in solid black from Online Appendix Figures A.1A-B) exhibited only 2% convergence 2009-2015. Hence, the 2007-2015 employment rate divergence is exceptional even relative to fully-updated convergence predictions.

Finally, Online Appendix Figure A.1D shows that the slow convergence after 2007-2009 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.[73] The figure replicates the employment rate series of Figure 1B while adding employment rate series for the aftermaths of the early-1980s and early-1990s recessions. See the notes to Figure 1B for details. To categorize states into severe and mild groups for the early-1980s recession (two recessions treated as one), I construct a shock value for each state in each year 1980-1982, exactly as documented above for 2008-2009. Likewise for the early 1990s recession, I do so for

[71] Beyer and Smets (2014) re-estimate the BK system augmented with multi-level factor modeling to compare U.S. and Europe population responses, and Dao, Furceri and Loungani (2014) re-estimate the system augmented with instruments to find stronger population responses during aggregate recessions. My exercise documents the exceptional lack of post-2007 employment rate convergence through 2015 even relative to fully updated BK estimates and without altering the BK system.

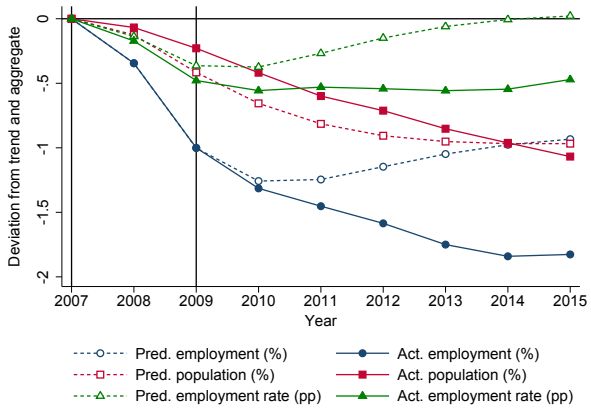
[72] Slower convergence likely derives from unique divergence after 2007 as well as from alleviated small-sample stationarity bias in a larger sample (e.g. Hurwicz 1950). Estimated U.S. persistence mirrors persistent effects of Brazilian regional trade shocks on formal, though not total, employment estimated in contemporaneous work (Dix-Carneiro and Kovak 2015); these U.S. statistics include self-employment.

[73] The post-2001-recession experience demonstrated approximately 50% convergence (substantially more complete than post-2009 convergence) before being interrupted by positively correlated 2007-2009 shocks.

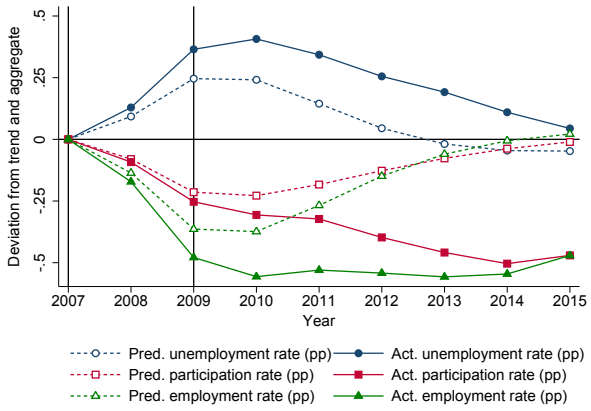
each state and each year 1990-1991. For comparability across recessions of different lengths, year zero refers to the last year of each recession, while year negative one refers to the year immediately before the recession; intervening years (1980-1981, 1990, and 2008) are omitted. The 2007-2009 series shows quantitatively large and persistent employment rate gap between severely shocked states and mildly shocked states, while the 1980-1982 and 1990-1991 series do not.

Figure A.1: State Employment Rate Persistence after 2007-2009 Shocks

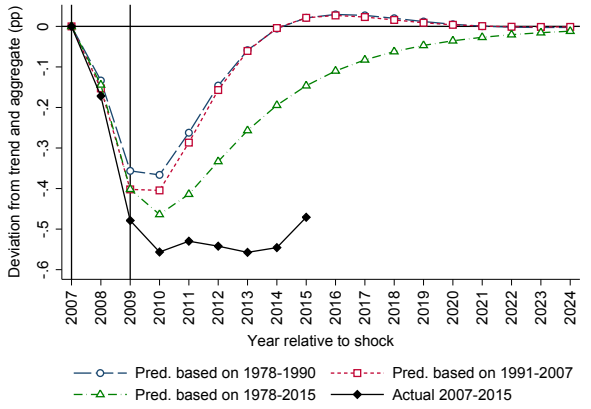
A. Employment, Population, and Employment Rate after −1% Shock



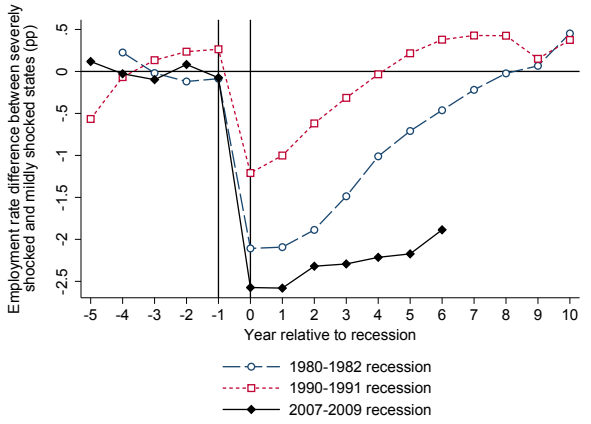
B. Participation, Unemployment, and Employment Rates after −1% Shock



C. Actual 2007-2014 Employment Rate Response vs. Updated Predictions

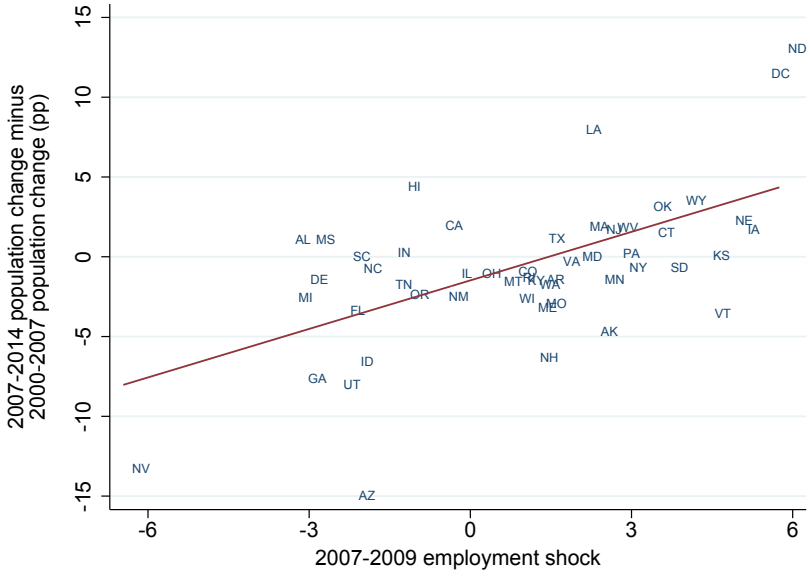


D. Employment Rate Convergence after Previous Recessions



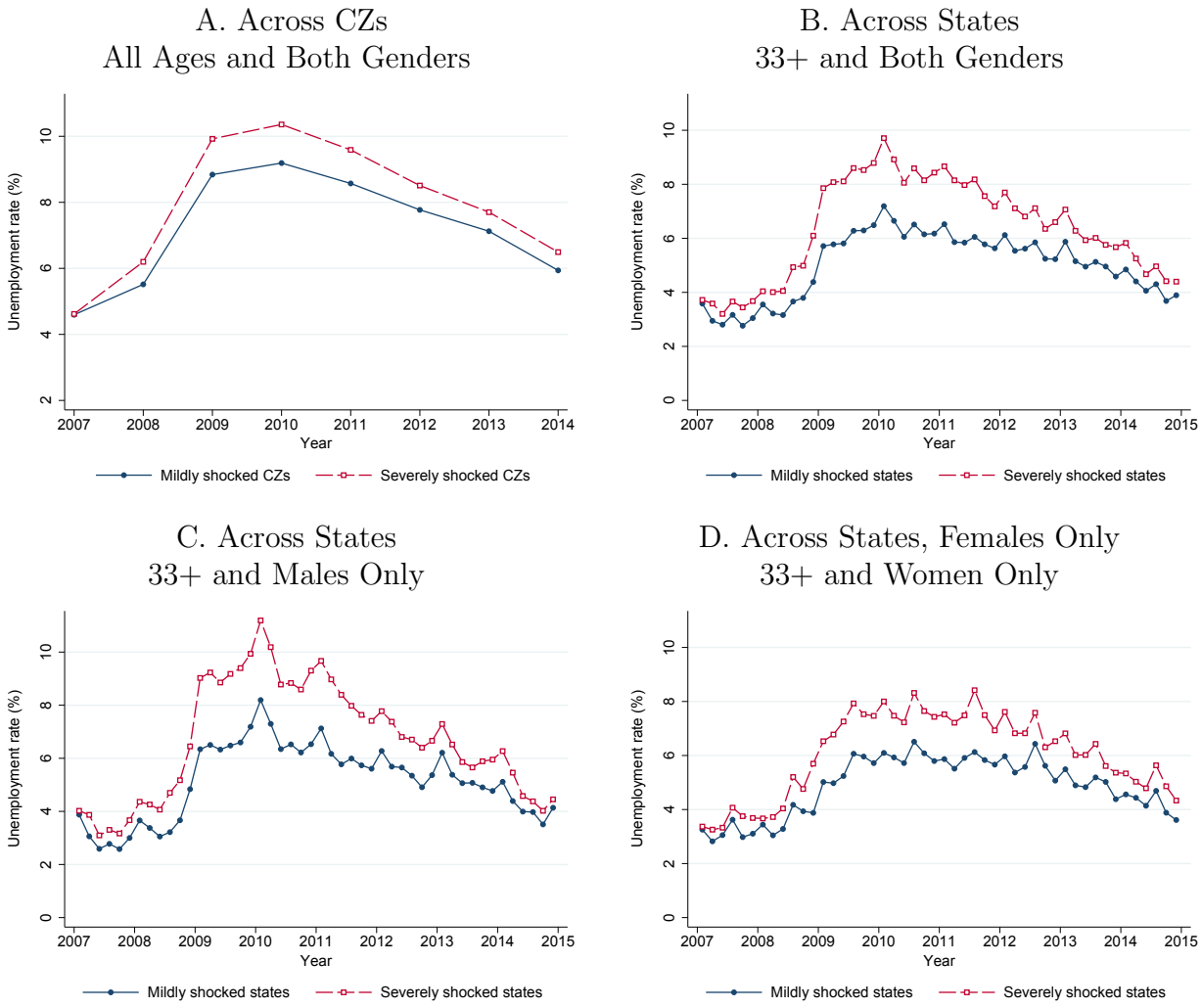
Notes: The dotted lines of panels A-B plot benchmark history-based predictions for state-level responses to a −1% 2007-2009 state-level employment shock, based on estimating Blanchard-Katz's (1992) autoregressive system of state labor market outcomes using LAUS data on the original sample range 1978-1990. 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. Panel D divides states into severe (below-unweighted-median) and mild (above-unweighted-median) shock states based on 2007-2009 state-level shocks and repeats the process for the early-1980s recessions (treated as a single recession) and the early-1990s recession. Then for each recession and year relative to the recession, it 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 pre-recession mean. For comparability across recessions, year 0 denotes the last recession year (1982, 1991, or 2009) while year -1 denotes the last pre-recession year (1979, 1989, or 2007) as defined for computing the shocks; intervening years are not plotted. Thus just as in the other panels, panel D's two interior vertical bars denote 2007 and 2009 for the 2007-2009 series.

Figure A.2: State Population Changes vs. 2007-2009 State-Level Shocks



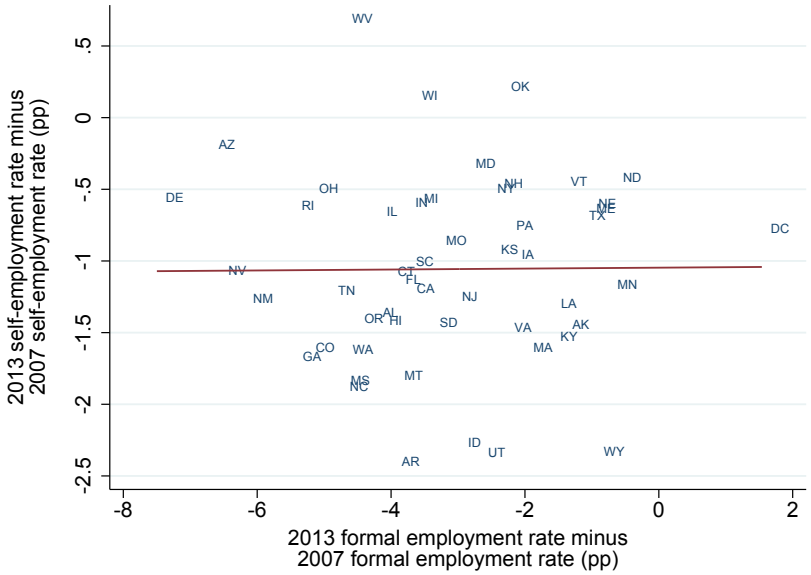
Notes: This figure uses the data underlying Figure 1B to plot de-trended 2007-2014 population changes—equal to each state's 2007-2014 percent change in population minus its 2000-2007 percent change in population—versus the state's 2007-2009 employment shock. See the notes to Figure 1B for details. Overlaid is the unweighted best-fit line with a slope of 1.014 and a robust standard error of 0.258.

Figure A.3: Unemployment Rates by Shock Severity 2007-2014



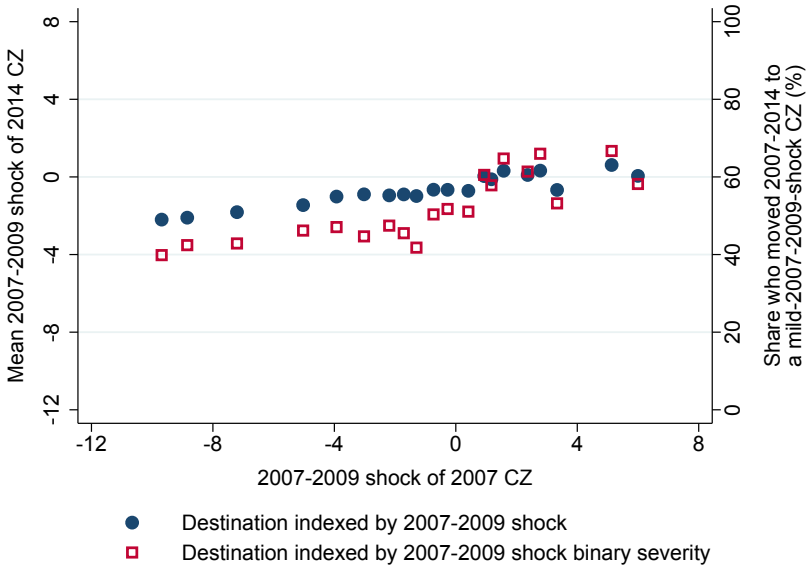
Notes: Panel A uses annual LAUS county-level data to plot annual mean unemployment rates 2007-2014 in severely shocked CZs and mildly shocked CZs. Panels B-D use the monthly Current Population Surveys (CPS) to plot mean two-month unemployment rates 2007-2014 in severely shocked states and mildly shocked states—separately for all genders, for males only, and for females only. The CPS data are restricted to the civilian non-institutional population who are 33 years or older at the time of the survey, which parallels the minimum 2014 age of my longitudinal data sample. All data points are weighted by population. The CZs comprise the 659 CZs covered by the main analysis sample. The states comprise the fifty states and the District of Columbia.

Figure A.4: Self-Employment vs. Formal Employment Rate Changes



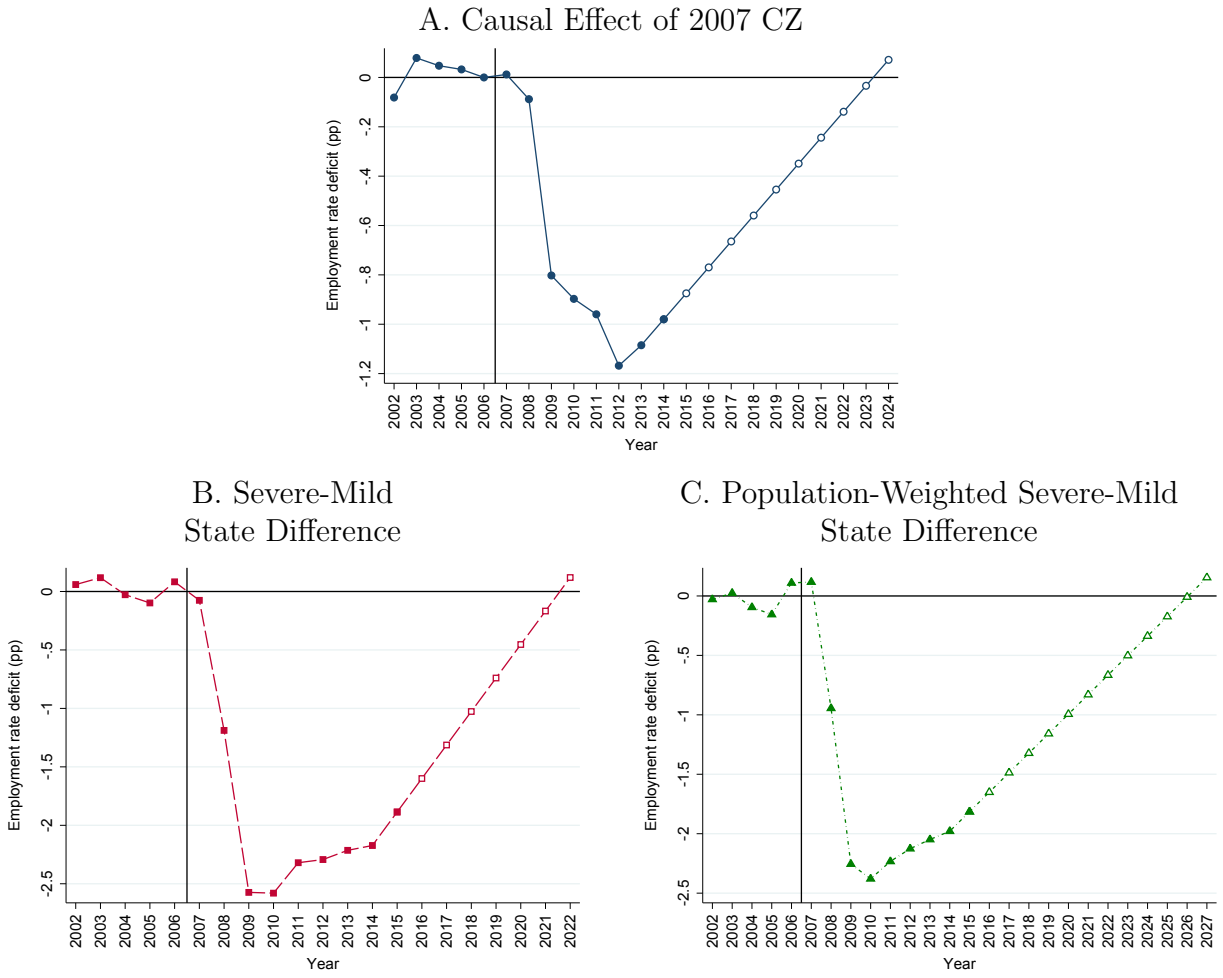
Notes: This graph uses the 2007 and 2013 monthly Current Population Surveys to plot 2007-2013 self-employment rate changes versus 2007-2013 formal employment rate changes for the adult (16+) civilian non-institutionalized population. The formal employment rate equals the number of formally employed individuals (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.5: Origin and Destination of 2007-2014 Movers



Notes: Using the 18.6% of workers who moved across CZs 2007-2014, the blue circles plot the mean 2007-2009 shock of workers' 2014 CZ (left axis) versus the 2007-2009 shock of their 2007 CZ. Moving across CZs is defined as a worker's January 2014 CZ being different from her January 2007 CZ. Workers are divided into twenty equal-sized bins (vingtiles) by their 2007 CZ's shock, and means of both the x-value and y-value are plotted within each bin. In the same subsample, the red squares plot the share of workers who moved to a mildly shocked CZ as defined by 2007-2009 shocks (right axis). The plotted gross migration flows (out-migration rates) swamp net migration flows (population changes) due to some combination of idiosyncratic preferences, family ties, professional connections, and information and other frictions.

Figure A.6: Convergence Paths at Current Percentage-Point Pace



Notes: The filled circles of Panel A reproduce the time series of estimated causal effects of living in 2007 in a severely shocked CZ (Figure 3A), and the empty circles extend that series forward by linearly extending the two last available data points (2013-2014). Panels B and C plot the analogous actual values and linear extensions (based on their last two available years, 2014-2015) respectively for two state-level series: the unweighted employment-rate series of Figure 1B and its population-weighted equivalent which equal the employment rate of severely shocked states minus the employment rate of mildly shocked states. Each series's actual values are displayed using filled markers while the linear extensions are displayed using empty markers. All three series begin in 2002 and are plotted until they cross zero, constituting full employment rate convergence between severely shocked workers/areas and mildly shocked workers/areas.

ONLINE APPENDIX TABLE 1
Layoffs and Enduring Employment Impacts of Great Recession Location

Outcome:	Employed in 2014	UI receipt sometime 2007-2008	UI receipt sometime 2007-2010	UI receipt sometime 2007-2012	UI receipt sometime 2007-2014	Employed in 2014				Employed in 2014 or UI receipt sometime 2007-2008	Employed in 2014 or UI receipt sometime 2007-2010	Employed in 2014 or UI receipt sometime 2007-2012	Employed in 2014 or UI receipt sometime 2007-2014
	(pp) (1)	(pp) (2)	(pp) (3)	(pp) (4)	(pp) (5)	(pp) (6)	(pp) (7)	(pp) (8)	(pp) (9)	(pp) (10)	(pp) (11)	(pp) (12)	(pp) (13)
Severely shocked 2007 CZ	-0.980 (0.239)	0.420 (0.659)	1.194 (1.037)	1.406 (1.224)	1.513 (1.342)	-0.968 (0.232)	-0.960 (0.229)	-0.969 (0.233)	-0.996 (0.247)	-0.741 (0.203)	-0.432 (0.209)	-0.324 (0.239)	-0.278 (0.255)
UI receipt sometime 2007-2008						-2.897 (0.237)							
UI receipt sometime 2007-2010							-1.751 (0.263)						
UI receipt sometime 2007-2012								-1.093 (0.265)					
UI receipt sometime 2007-2014									0.531 (0.283)				
Main controls	X	X	X	X	X	X	X	X	X	X	X	X	X
N	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310	2,238,310
R^2	0.20	0.06	0.07	0.08	0.08	0.21	0.20	0.20	0.20	0.20	0.20	0.20	0.20
Outcome mean	74.6	8.9	18.3	23.5	26.9	74.6	74.6	74.6	74.6	77.0	79.2	80.2	80.6

Notes – This table lists more permutations of Table 5B; see the notes to that table for details.

ONLINE APPENDIX TABLE 2
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	4.5
Unemployment rate (%)		
1978-2015	6.1	2.1
2007	4.4	1.0
2009	8.5	2.0
2015	5.1	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	4.2
Number of states	51	
Number of years	38	
Number of observations (state-years)	1,938	

Notes – This table lists summary statistics based on the Bureau of Labor Statistics's Local Area Unemployment Statistics (LAUS) dataset of state-year labor market outcomes 1978-2015 among the adult (16+) civilian non-institutional population. The employment rate is the ratio of employment to population. The unemployment rate is the ratio of the unemployed to the labor force. The labor force participation rate is the ratio of the labor force to population.

ONLINE APPENDIX TABLE 3
2007-2009 State shocks and 2007-2015 State Employment Rate Changes

2007-2009 Shock Rank	State	2007-2009 Shock	Change in Employment Rate 2007-2015	2007-2009 Shock Rank	State	2007-2009 Shock	Change in Employment Rate 2007-2015
		(pp)	(pp)			(pp)	(pp)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	Nevada	-6.459	-6.63	27	Washington	1.13	-5.08
2	Alabama	-3.422	-5.68	28	New Hampshire	1.13	-2.26
3	Michigan	-3.363	-2.92	29	Missouri	1.24	-1.79
4	Georgia	-3.187	-7.40	30	Arkansas	1.26	-4.96
5	Delaware	-3.139	-5.06	31	Texas	1.30	-1.77
6	Mississippi	-3.036	-4.50	32	Virginia	1.56	-4.61
7	Utah	-2.529	-4.11	33	Maryland	1.92	-3.06
8	Florida	-2.400	-5.38	34	Louisiana	1.99	-2.03
9	South Carolina	-2.350	-3.94	35	Massachusetts	2.06	-1.34
10	Arizona	-2.241	-4.95	36	Alaska	2.26	-3.26
11	Idaho	-2.204	-4.38	37	Minnesota	2.33	-1.52
12	North Carolina	-2.150	-4.95	38	New Jersey	2.37	-3.39
13	Tennessee	-1.561	-5.55	39	West Virginia	2.58	-4.51
14	Indiana	-1.516	-2.83	40	Pennsylvania	2.68	-2.39
15	Hawaii	-1.326	-4.44	41	New York	2.79	-2.17
16	Oregon	-1.287	-4.85	42	Oklahoma	3.25	-1.58
17	California	-0.631	-3.68	43	Connecticut	3.33	-3.09
18	New Mexico	-0.566	-6.79	44	South Dakota	3.56	-4.01
19	Illinois	-0.326	-4.23	45	Wyoming	3.85	-3.76
20	Ohio	0.054	-3.98	46	Kansas	4.35	-3.01
21	Montana	0.461	-2.94	47	Vermont	4.38	-2.80
22	Colorado	0.728	-5.28	48	Nebraska	4.77	-2.70
23	Wisconsin	0.746	-2.59	49	Iowa	5.00	-1.98
24	Rhode Island	0.813	-3.80	50	District of Columbia	5.44	0.87
25	Kentucky	0.903	-4.68	51	North Dakota	5.75	-2.77
26	Maine	1.093	-3.26				

Notes – This table lists the 2007-2009 state-level shocks and 2007-2015 percentage-point changes in state-level employment rates that underlie the severe-vs-mild-shock grouping underlying Figure 2A. See the notes to those figures for details. Severely shocked states are listed on the left; mildly shocked states are listed on the right.

ONLINE APPENDIX TABLE 4
2007-2014 Employment Rate Changes in the 50 Largest CZs

Employment Rate Change Rank	CZ Name	Change in Employment Rate 2007-2014 (pp)	Employment Rate Change Rank	CZ Name	Change in Employment Rate 2007-2014 (pp)
(1)	(2)	(3)	(4)	(5)	(6)
1	Las Vegas, NV	-7.61	26	Chicago, IL	-3.44
2	San Diego, CA	-6.73	27	Denver, CO	-3.42
3	Phoenix, AZ	-6.33	28	Minneapolis, MN	-3.15
4	Orlando, FL	-6.23	29	Port St. Lucie, FL	-2.87
5	Miami, FL	-5.88	30	Manchester, NH	-2.71
6	Sacramento, CA	-5.38	31	Charlotte, NC	-2.46
7	Atlanta, GA	-5.30	32	Philadelphia, PA	-2.43
8	Buffalo, NY	-5.25	33	Baltimore, MD	-2.33
9	Los Angeles, CA	-5.09	34	Pittsburgh, PA	-1.48
10	Fresno, CA	-5.02	35	Boston, MA	-1.18
11	Seattle, WA	-4.96	36	New York, NY	-1.17
12	Salt Lake City, UT	-4.57	37	St. Louis, MO	-1.11
13	Providence, RI	-4.46	38	Fort Worth, TX	-1.06
14	Jacksonville, FL	-4.44	39	San Antonio, TX	-0.86
15	Washington, DC	-4.36	40	Milwaukee, WI	-0.85
16	Portland, OR	-4.35	41	Grand Rapids, MI	-0.85
17	Cleveland, OH	-4.23	42	San Francisco, CA	-0.72
18	Indianapolis, IN	-4.16	43	Tampa, FL	-0.10
19	Raleigh, NC	-4.02	44	Austin, TX	0.08
20	Columbus, OH	-3.92	45	New Orleans, LA	0.13
21	Bridgeport, CT	-3.75	46	Houston, TX	0.32
22	Newark, NJ	-3.68	47	Dallas, TX	0.34
23	Detroit, MI	-3.61	48	Kansas City, MO	0.68
24	Nashville, TN	-3.52	49	San Jose, CA	0.96
25	Cincinnati, OH	-3.47	50	Oklahoma City, OK	1.07

Notes – This table lists LAUS-based 2007-2014 changes in the employment rate (employment-population ratio) for the fifty largest commuting zones (CZs) according to 2014 populations. The figures are approximate because the underlying population data reflects a slightly larger universe than the underlying employment data, though comparison to official state-level values suggests that the resulting bias is small. Employment data are Bureau of Labor Statistics's Local Area Unemployment Statistics county-level estimates of total employment for the adult (16+) civilian non-institutional population, aggregated to the CZ level. Population data are Census's Annual County Resident Population Estimates of the total adult (16+) population, aggregated to the CZ level. Displayed values equal each CZ's 2014 employment rate minus the CZ's 2007 employment rate, in percentage points.