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THE ENDURING EMPLOYMENT IMPACT OF YOUR GREAT RECESSION LOCATION*

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Abstract

This paper asks whether Americans were jobless in 2014 because of where they were living in 2007. In the cross section, employment rates diverged across U.S. local areas 2007-2009 and—in contrast to history—have barely converged. This "great divergence" could reflect spatial differences in human capital, rather than causal location effects. I therefore use administrative data to compare two million workers with very similar pre-2007 human capital: those who in 2006 earned the same amount from the same retail firm, at establishments located in different local areas. I find that conditional on 2006 firm-x-wages fixed effects, living in 2007 in a below-median 2007-2009-fluctuation area caused those workers to have a 1.3%-lower 2014 employment rate. Hence, U.S. local labor markets are limitedly integrated: location has caused long-term joblessness and exacerbated within-skill inequality. The enduring impact is not explained by enduringly high unemployment, more layoffs, more disability enrollment, or reduced migration. Instead, the employment outcomes of cross-area movers are consistent with severe-fluctuation areas continuing to depress residents' labor force participation. Impacts are correlated with housing busts but not manufacturing busts, possibly reconciling current experience with history. If recent trends continue, employment rates are estimated to remain diverged into the 2020s—adding up to over a relative lost decade for half the country. Employment models should allow market-wide shocks to cause persistent labor force exit, leaving employment depressed even after unemployment recovers.

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1 Introduction

Does your U.S. location affect your long-term employment? I study the aftermath of local variation in the 2007-2009 recession. Like all aggregate fluctuations, the 2007-2009 recession was a collection of subnational fluctuations that varied in their intensity: some places like Phoenix, Arizona, suffered a severe 2007-2009 employment contraction while other similar places like San Antonio, Texas, did not. Consider two identical workers in 2007—one living in Phoenix and the other living in San Antonio. This paper asks whether those workers now have similar likelihoods of being employed.

Beyond testing an integrated labor markets hypothesis, this paper—under a plausible assumption—also addresses a major macroeconomic question: is the 2007-2009 recession continuing to depress U.S. employment even though unemployment has returned to normal? The U.S. unemployment rate has now returned to its pre-recession level, yet the U.S. employment rate (employment-population ratio) remains three percentage points below its 2007 level as U.S. labor force participation has plummeted.[1] Secular nationwide forces like skill-biased technical change and population aging could explain the country's enduring participation decline. Alternatively and in contrast to standard models,[2] the recession and its underlying causes may have induced persistent labor force exit. Local variation in the recession's intensity provides a laboratory for testing between these views, if one assumes that the local variation was caused by the same underlying causes as the aggregate recession.[3]

[1] As shown in Figure 1, the February 2016 unemployment rate was 4.9%, compared to 4.7% in November 2007; the February 2016 employment rate was 59.8%, compared to 62.9% in November 2007; and the February 2016 labor force participation rate was 62.9%, compared to 66.0% in November 2007. These rates apply to the 16+ civilian non-institutional population.

[2] For example: "Despite a flurry of activity since [the first papers on job search equilibrium]...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...Given this restriction, we can interchangeably talk either about employment equilibrium or about unemployment equilibrium" (Mortensen and Pissarides 1999).

[3] Enduring effects of a recession and of a recession's underlying causes are indistinguishable. Local variation in the 2007-2009 recession could reflect the same causes as the aggregate recession (e.g. geographically concentrated spending contractions as in Mian and Sufi (2014)) or different causes (e.g. geographically concentrated spending contractions layered on top of an aggregate response to higher national policy uncertainty as in Baker, Bloom and Davis (2016)).

This paper's null hypothesis—that ex ante identical workers have identical outcomes across space five years after 2007-2009 local fluctuations—has roots in two literatures. In regional economics, state-level employment rates have historically returned to parity five years after state-level fluctuations, as local populations rapidly expanded or contracted in response (Blanchard and Katz 1992) unlike within Europe (Eichengreen 1993).[4] Even without labor mobility, standard macroeconomic models imagine that wage flexibility and job search restore pre-fluctuation employment (Barro and Grossman 1971, Mortensen and Pissarides 1999). Yet despite adjustment mechanisms, long-term inequality between similar workers has been documented across industries and layoff status. This paper tests for long-term inequality between similar workers across U.S. local areas since 2007.

I begin by estimating whether cross-sectional employment rates have converged across space after 2007-2009 local fluctuations. I use Blanchard-Katz's autoregressive system of state adult civilian noninstitutional employment, population, unemployment, and labor force participation to compute a 2007-2009 employment fluctuation for each U.S. state and then compare post-2009 employment rate convergence across states to historical convergence speeds. I find that the conventional convergence mechanism—population decelerations in severe-fluctuation states and population accelerations elsewhere—occurred after 2007-2009 fluctuations by exactly as much as history would predict. Unemployment rates have converged across space, also in line with history. Yet in contrast to history—including the six-year aftermaths of the early-1980s and early-1990s recessions—employment rates have barely converged across space as participation rates have steadily diverged. The employment gap is large: relative to full employment rate convergence to pre-recession differences across states, a 2.2-million-person (1.5%) employment gap remained in 2015 between the below-median-fluctuation and above-median-fluctuation halves of the country.

[4]For example: "When some parts of the country are booming while others are slumping, Americans move. Blanchard and Katz famously found that such labor mobility is the prime way we deal with regional shocks. But Europeans are divided by language, culture, even food. They tend to be reluctant to move even within nations...So how can the euro work?" (Paul Krugman http://krugman.blogs.nytimes.com/2008/06/05/will-the-polish-plumber-save-the-euro)

Of course, America's "great divergence" in state employment rates since the onset of the great recession could merely reflect worker selection rather than an enduring causal impact of workers' 2007 locations. First, severe-fluctuation areas may have simply been disproportionately populated in 2007 by workers who suffered large nationwide contractions in demand for their types of human capital—e.g. construction workers or routine laborers—and would be non-employed anywhere now (Katz and Murphy 1992, Autor, Levy and Murnane 2003, Acemoglu and Autor 2011, Jaimovich and Siu 2012).[5] Second, retirees and others secularly out of the workforce may have disproportionately stayed in or moved to severe-fluctuation areas in order to enjoy low costs of living while foregoing employment (Bound and Holzer 2000, Notowidigdo 2011).[6]

I therefore turn to a quasi-experiment that provides a narrow but unique opportunity to control for pre-2007 selection on human capital as well as for post-2007 selection on labor supply. Unlike other major firm types, retail chain firms like Walmart and Starbucks employ workers to perform identical tasks in many different local areas. I build on this observation by using linked employer-employee panel data to compare the long-term employment rates of workers who in 2006 earned the same amount from the same retail firm at establishments located in different local areas. By controlling for pre-2007 retail firm and wages, I hold constant an unusually fine measure of initial human capital. And by tracking workers over time no matter where they move, I directly control for all post-2007 composition changes. This design follows a large literature tracking laid-off and trade-competition-exposed workers over time (e.g. Jacobson, LaLonde and Sullivan 1993, Davis and Von Wachter 2011, Autor, Dorn, Hanson and Song 2014). I deviate from this earlier work by focusing on area-level fluctuations that are by construction orthogonal to worker industry and firm, as well as by

[5]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).

[6]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/).

not conditioning on layoff.

I implement this design using de-identified data from federal income tax records spanning 1999-2014 (all available years). The main analysis sample comprises the 2,238,187 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 distinct local labor markets and are similar to metropolitan statistical areas but span the entire United States. I define severe-fluctuation (mild-fluctuation) CZs as CZs that experienced a below-median (above-median) 2006-2009 log employment change relative to its 2000-2003 trend, similar to Blanchard-Katz. I define severe-fluctuation (mild-fluctuation) natives as workers who were living in a severe-fluctuation (mild-fluctuation) CZ in January 2007, regardless of where they lived afterward. The paper's main outcome is employment 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-x-wages fixed effects and other demographic controls, severe-fluctuation natives were 0.98-percentage-points (1.3%) less likely to be employed in 2014 than mild-fluctuation natives. The estimate is very statistically significant, with a standard error of 0.24 clustered at the 2007 state level. As falsification tests, I find statistically zero difference in employment rates between these two groups of workers in every year 1999-2007—before 2007 location would be expected to have affected employment.

The finding of a large effect of 2007 location on 2014 employment is robust to CZ-level controls (pre-2007 size, pre-2007 size growth, commuting rates, post-2007 maximum unemployment insurance duration), alternative CZ fluctuation measures (linear level, alternative detrending, and employment rate denomination), instrumenting with birth-state fluctuation, and an alternative sample comprising year-2000 retail workers. The effect is small and insignificant for high-earners and attenuated for the married but is otherwise nearly identical across subgroups defined by gender, age, 2006 number of children, and 2006 mortgage holding. Hence, I find robust evidence of an enduring employment impact of one's 2007 location.

Probing mechanisms, I do not find evidence that severe-fluctuation natives are now "scarred" with higher reservation wages or decayed human capital that would depress their 2014 employment anywhere. I first find that the enduring impact of 2007 location is insignificantly attenuated by 7.8% when defining the outcome as employed in 2014 *or* on Social Security Disability Insurance (DI) in 2014. This indicates that higher reservation wages after transition to DI (Autor and Duggan 2003) can explain only 0%-7.8% of the enduring impact of 2007 location, under the mild monotonicity assumption that severe-fluctuation areas did not make anyone less likely to go on DI. I next find that severe-fluctuation natives were insignificantly more likely to have received unemployment insurance (UI) at some point 2007-2014 and that controlling for having received UI leaves the enduring impact of 2007 location unchanged. This indicates that greater layoffs also do not explain the effect of 2007 location, under the assumption that laid-off severe-fluctuation natives were no worse on unobservables than laid-off mild-fluctuation natives as in layoffs-and-lemons models (Gibbons and Katz 1991).

Instead, I find suggestive evidence that the enduring impact of 2007 location is driven by severe-fluctuation areas continuing to depress their current residents' employment, in conjunction with most workers not having moved. Leveraging the employment outcomes of those who did move, I find that conditional on one's origin, movers to severe-fluctuation CZs were significantly 1.07 percentage-points less likely to be employed in 2014 than movers to mild-fluctuation CZs. In contrast and conditional on one's destination, I find that movers from severe-fluctuation CZs were insignificantly 0.12 percentage-points more likely to be employed than movers from mild-fluctuation CZs. Hence, the employment effect loads entirely on one's 2014 CZ rather than on one's 2007 CZ. Naturally, these cross-sectional patterns are purely suggestive because moving is endogenous. Instrumenting for moving and for movers' destinations using pre-2007 age-gender-CZ-specific moving patterns as in Card (2001) delivers noisy estimates but similarly suggests a leading role for 2007-2009 severe-fluctuation areas depressing their 2014 residents' employment.

What could explain different employment rates among identical workers across areas after populations reallocated and unemployment rates converged? A natural explanation outside of multiple equilibria (e.g. Diamond 1982, Benhabib and Farmer 1994, Kaplan and Menzio 2014) would involve serially correlated area-level demand shocks (Hall 1992, Amior and Manning 2014) that induce non-participation. In this vein, I informally suggest that local population reallocations can fail to generate employment rate convergence when local fluctuations are locally-driven demand shocks (e.g. due to housing-based net worth changes as in Mian and Sufi 2014) instead of conventional globally-driven demand shocks (i.e. for locally produced traded goods as in Blanchard and Katz 1992)—potentially reconciling the current divergence with historical convergence. Consistent with this distinction, enduring employment impacts of 2007 location are large in CZs with relatively high 2006-2009 house-price-driven net worth declines and with high pre-2007 construction employment shares, but are statistically zero in CZs with relatively large pre-2007 shares of negatively affected manufacturing industries.

This paper is organized as follows. Section 2 documents cross-sectional divergence in employment rates, benchmarked to history. Section 3 details the empirical design for isolating the effects of 2007 location using worker-level data. Section 4 presents worker-level results. Section 5 investigates mechanisms. Section 6 concludes.

2 Employment Rate Divergence across U.S. States

This section uses the empirical framework of Blanchard and Katz (1992, "BK") to evaluate the extent to which employment rates (employment-to-population ratios) have converged across U.S. states following 2007-2009 state-level employment fluctuations. I first detail the state-year data (used only for this section) and the empirical specifications for computing state-level fluctuations, historical-benchmark convergence, and mean actual convergence. I then present the section's main result: in spite of typical population reallocation and in contrast to the aftermath of previous recessions, state employment rates have barely converged since 2009.

2.1 State-Year Data

I estimate 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).[7] Variable definitions are standard and pertain to the age-16-and-over civilian noninstitutional population.[8] 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.[9] Table 1 displays summary statistics. Similar to well-known national aggregates, the table's state-weighted figures show that the mean employment rate fell from 64.1% in 2007 to 60.7% in 2009 without recovering by 2015. The mean unemployment rate fell 2009-2015 from 8.5% to 5.1% (below the full-sample average of 6.1%) as the participation rate fell from 66.3% to 63.6%. The question of the section is whether employment rates converged across states 2009-2015

[7]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.

[8]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.

[9]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 (see Online Appendix Figure A.2, introduced below).

to their pre-recession deviations from the national aggregate, following 2007-2009 state-level employment fluctuations.

2.2 Computing 2007-2009 State-Level Fluctuations

I use BK's canonical empirical model of state labor market outcomes to compute 2007-2009 employment fluctuations 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. 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, unemployment, and participation rates (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:

$$\widetilde{\Delta \ln E}_{st} = \alpha_{s10} + \alpha_{11}(2)\,\widetilde{\Delta \ln E}_{s,t-1} + \alpha_{12}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{13}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon_{st}^{E} \quad (2.1)$$

$$\widetilde{\ln E/L}_{st} = \alpha_{s20} + \alpha_{21}(2)\,\widetilde{\Delta \ln E}_{st} + \alpha_{22}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{23}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon_{st}^{E/L} \quad (2.2)$$

$$\widetilde{\ln L/P}_{st} = \alpha_{s30} + \alpha_{31}(2)\,\widetilde{\Delta \ln E}_{st} + \alpha_{32}(2)\,\widetilde{\ln E/L}_{s,t-1} + \alpha_{33}(2)\,\widetilde{\ln L/P}_{s,t-1} + \varepsilon_{st}^{L/P} \quad (2.3)$$

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 $\sim$ denotes a difference relative to the year's national aggregate value; and where (2) denotes a vector of two lags. Thus the first dependent variable ("relative state employment") is the first difference of log state employment minus the first difference of log aggregate employment. The second ("relative state unemployment") is the log of one minus the state unemployment rate minus the log of one minus the aggregate unemployment rate. The third ("relative state participation") is the log of the state participation rate minus the log of the aggregate participation rate. Relative state population is the implied residual. Each equation includes a state fixed effect. I follow BK in weighting states equally (e.g. not by population).

The BK system embodies four substantive assumptions. First, unforecasted changes in relative state employment growth ε_{st}^E 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.[10] This reflects the assumption that ε_{st}^E primarily reflects changes in labor demand rather than supply—supported by negative values of ε_{st}^E 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.[11] 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.[12] 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.

[10] This assumption allows each equation 2.1-2.3 to be estimated independently via ordinary least squares.

[11] This limits the estimation sample to years 1978 and beyond. Three and four lags deliver similar results.

[12] State fixed effects are motivated by cross-decadal persistence in the outcomes. Formal stationarity tests are underpowered and inconclusive in short time series (see BK and Online Appendix Table 1). Stationarity here is best motivated by spatial arbitrage priors and the empirical fact that the standard deviation of each of the outcomes has stayed constant or fallen slightly over time instead of rising (Online Appendix Table 1C), at least prior to the 2007-2009 recession. Figure 3D below depicts employment rate stationarity after previous recessions.

To estimate 2007-2009 state-level employment fluctuations, I first estimate the BK system coefficients using sample years 1978-2007. I then compute each state's 2008 employment fluctuation $\widehat{\varepsilon^E_{s,2008}}$, 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 observed independent variable values 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 fluctuation equal to zero. I similarly compute each state's 2009 employment fluctuation $\widehat{\varepsilon^E_{s,2009}}$, 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 observed independent variable values through 2008 and the estimated coefficients. Note that the 2009 fluctuation is net of the expected response to the 2008 fluctuation. I refer to each state's vector $\{\widehat{\varepsilon^E_{s,2008}}, \widehat{\varepsilon^E_{s,2009}}\}$ as the state's 2007-2009 employment fluctuations and (for simplicity and compactness in some graphs) the sum of the vector's elements as the state's 2007-2009 employment fluctuation.[13]

To understand these fluctuations, Figure 2 plots each state's 2007-2009 employment fluctuation. Darker shading denotes more negative fluctuations, with the shading dividing the sample into unweighted quartiles. The standard deviation of state-level fluctuations over the 2007-2009 recession (2.74) was similar to the standard deviation of state-level fluctuations over the early-1980s (1980-1982) recession (2.73) computed similarly (detailed below).[14] Recall that fluctuations 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 fluctuation 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 fluctuation, since fluctuations are measured relative to the aggregate. The figure displays patterns famil-

[13]The 2007-2009 recession began in December 2007; I ignore 2007 fluctuations for simplicity.

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

iar 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 fluctuations relative to other states.[15] As two focal examples, Arizona's fluctuation equals −2.24% while Texas's fluctuation equals +1.30%.[16]

2.3 Results: Actual Convergence vs. Historical Benchmark

Figures 3A-B plot actual mean responses (solid lines) of state labor market outcomes to 2007-2009 fluctuations versus historical benchmark responses (dotted lines) to a −1% fluctuation, following BK. Forty-one percent of the average state's 2007-2009 fluctuation 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.[17]

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 (employment divided by population) 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

[15]The South exhibits such negative fluctuations in part because the system assumes relatively strong employment growth trends based on the South's relatively strong employment growth early in the 1978-2007 sample range. The Commuting-Zone-level fluctuations introduced and plotted below in Figure 4 use only post-2000 employment growth for detrending, when Southern employment growth was weaker.

[16]For additional reference, Online Appendix Table 2 lists 2007-2009 employment fluctuations and 2007-2015 employment rate changes for each state. Online Appendix Table 3 lists LAUS-based 2007-2014 employment rate changes for the fifty largest Commuting Zones, the local area concept used in subsequent sections.

[17]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.

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.

To plot mean actual responses to 2007-2009 fluctuations, I first compute baseline predictions for how each state's outcomes would have evolved in the absence of 2007-2009 fluctuations—using actual state values as of 2007 and the estimated coefficients from the 1978-2007-estimated system. I then compute actual-minus-baseline deviations for each variable-state-year $\{\eta_{st}^{E}, \eta_{st}^{E/L}, \eta_{st}^{L/P}\}$ equal to the actual value minus the baseline predicted value. I then estimate mean actual effects of 2007-2009 fluctuations on 2008-2015 outcomes by regressing actual-minus-baseline deviations on 2007-2009 fluctuations in year-by-year regressions:[18]

$$\eta_{st}^{E} = \widehat{\varepsilon_{s,2008}^{E}}\gamma_t^{E} + \widehat{\varepsilon_{s,2009}^{E}}\theta_t^{E},\ \forall t \tag{2.4}$$

$$\eta_{st}^{E/L} = \widehat{\varepsilon_{s,2008}^{E}}\gamma_t^{E/L} + \widehat{\varepsilon_{s,2009}^{E}}\theta_t^{E/L},\ \forall t \tag{2.5}$$

$$\eta_{st}^{L/P} = \widehat{\varepsilon_{s,2008}^{E}}\gamma_t^{L/P} + \widehat{\varepsilon_{s,2009}^{E}}\theta_t^{L/P},\ \forall t \tag{2.6}$$

This specification is flexible in that it allows for the 2008 and 2009 employment fluctuations to have arbitrary additive effects on each subsequent year's outcomes. I then use the estimated coefficients from equations 2.4-2.6 to plot mean actual responses (solid lines in Figures 3A-B) to the -1% 2007-2009 fluctuation $\{\widehat{\varepsilon_{s,2008}^{E}}, \widehat{\varepsilon_{s,2009}^{E}}\} = \{-.41, -.59\}$.

Figure 3A shows that on a slight lag, mean actual relative population responded identically to 2007-2009 fluctuations 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

[18] For 2008, only the 2008 employment fluctuation is included as a regressor.

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.

Before proceeding, some readers may find it surprising that mean population reallocation after 2007-2009 state-level fluctuations was exactly in line with historical experience, in light of the long-term decline in inter-state migration rates (Molloy, Smith and Wozniak 2011) and fears of migration-inhibiting underwater mortgages (Ferreira, Gyourko and Tracy 2010).[19] I therefore non-parametrically demonstrate the population result in Online Appendix Figure A.1. The figure plots de-trended 2007-2014 population changes—equal to each state's percent change in population 2007-2014 minus the percent change in the state's population 2000-2007—versus the state's 2007-2009 employment fluctuation. The relationship is unit-elastic (exactly as depicted in Figure 3A in both the historical benchmark and actuality) with a slope of 1.014 and a standard error of 0.258. The population result is a testament to the enduring validity of BK's core finding of large and rapid population reallocation in response to state-level fluctuations and is consistent with other recent results.[20] Continued population reallocation in spite of slightly reduced gross migration rates is easy to digest when one realizes that gross migration flows are still an order of magnitude larger than the net flows (population changes) predicted in the historical benchmark.[21]

[19]Kaplan and Schulhofer-Wohl (2012*a*) show that migration rate declines have been overstated by imputed CPS one-year migration rates; five-year inter-state migration rates have in fact exhibited a relatively modest decline from approximately 15% in 1945-1950 to 13% in 2005-2010 (Molloy, Smith and Wozniak 2011).

[20]Mian and Sufi (2014) find that *absolute* (not de-trended) 2007-2009 population changes were unrelated to their measure of 2007-2009 fluctuations; though appropriate for their island-model context, absolute population changes do not capture the halt to decades-long fast population growth in severe-fluctuation states like Arizona. Relatedly, Monras (2015) estimates that two-thirds of the historical population response to adverse state-level fluctuations are attributable to reduced in-migration and one-third to increased out-migration.

[21]For example, the data are consistent with a large share of people out-migrating from all locales for

Returning to Figure 3, panel C 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 fluctuation, 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.[22] 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.[23] In contrast, the mean actual employment rate series (reprinted in solid black from Figures 3A-B) exhibited only 2% convergence 2009-2015. Hence, the 2007-2015 employment rate divergence is exceptional even relative to fully-updated convergence predictions.

Finally, Figure 3D shows that the slow convergence after 2007-2009 fluctuations 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. The figure plots the mean employment rate of severe-fluctuation states minus the mean employment rate of mild-fluctuation states, by year around the early-1980s recession, the early 1990s recession, and the 2007-2009 recession. I construct the graph as follows. For each aggregate recession, I estimate a fluctuation value for each state in each year of the aggregate reces-

idiosyncratic reasons, and a small share of those out-migrants choosing where to settle based on local labor market conditions. Gross flows frequently swamp net flows in labor markets (e.g. Davis and Haltiwanger 1992).

[22] 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.

[23] 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). For example, Online Appendix Figure A.2 and Online Appendix Table 4B show nine-year employment rate convergence to within .02 percentage points on sample range 1978-2007 and twelve-year convergence on the same-length but post-2007-inclusive sample range 1986-2015. 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.

sion as documented in Section 2.2 for the 2007-2009 recession: years 1980-1982 for the two early 1980s recession (treated here as a single recession) and years 1990-1991 for the early 1990s recession. Then for each aggregate recession, I sum employment fluctuations across recession years within states, compute the unweighted median value across states, and divide states into two groups: those with a below-median-fluctuation ("severe-fluctuation states") and those with an above-median fluctuation ("mild-fluctuation states"). Then for each year before and after the recession, I compute the unweighted mean employment rate within each group and plot the severe-fluctuation-group-minus-mild-fluctuation-group difference.[24] For comparability across recessions, I subtract from each series the series's average pre-recession value so that the pre-recession values average zero within each series. 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.[25]

Similar to panels A-C, the 2007-2009 series of Figure 3D shows an immediate employment rate deficit in 2009 (year 0) in severe-fluctuation states relative to mild-fluctuation states that closes only modestly to -1.89 percentage points by 2015 (year 6). The 2015 employment gap is large: 2.24 million (1.50%) fewer adults were employed in severe-fluctuation states than in mild-fluctuation states, relative to full convergence to pre-recession differences.[26] I refer to this quantitatively large and persistent employment rate gap between severe-fluctuation states and mild-fluctuation states after the great recession as a "great divergence." In contrast, the 1980-1982 and 1990-1991 series exhibit majority or complete convergence within

[24]I weight by states for comparability to the other panels. When weighting by population, one observes slower but still-complete convergence after the early-1980s recession and similar convergence after early-1990s and 2007-2009 recessions (see Online Appendix Figure A.3).

[25]See Online Appendix Figure A.3 for analogous figures for participation and unemployment rates, for population-weighted versions, and for the post-2001-recession experience which demonstrated approximately 50% convergence (substantially more complete than post-2009 convergence) before being interrupted by positively correlated 2007-2009 fluctuations.

[26]That is, the counterfactual is one in which 2015 state populations had returned to pre-2007 employment rate differences at the 2015 national employment rate. $2.24 \approx .0181/2 \times 250.8$, where 1.81 percentage points is the population-weighted equivalent to the 1.89 severe-mild 2015 employment rate deficit (see Online Appendix Figure A.3) and where 250.8 million was the 2015 adult civilian noninstitutional population of the fifty-one states. (The formula is approximate because population is not exactly evenly divided between the two state groups; see the Employment Gap Appendix for the precise formula yielding 2.24 million.)

six years after each recession.

To sum up, this section has found that employment rates diverged dramatically across U.S. states 2007-2009 and have remained diverged in spite of the typical population reallocation response and in spite of unemployment rate convergence. There are generally two ways to interpret this fact with respect to whether Americans are jobless now because of where they were living in 2007: causality or selection. First, enduringly depressed local employment rates may reflect causal impacts of 2007 location: residents would have higher employment rates now if they had been living elsewhere in 2007. Alternatively, enduringly depressed local employment rates may reflect local concentrations of workers who would be non-employed now regardless of where they were living in 2007. I now isolate causal impacts.

3 Isolating Location Effects with Worker-Level Data

The previous section found that employment rates remain unusually low in U.S. areas that experienced an especially severe 2007-2009 employment fluctuation. This fact is limitedly relevant if it reflects only selection rather than causal location effects. Two selection threats loom large. First, severe-fluctuation areas may have been disproportionately populated before the recession by workers who subsequently suffered large nationwide contractions for the types of human capital, like construction workers or routine laborers. Second, retirees and others secularly out of the labor force may have disproportionately stayed in or moved into severe-fluctuation 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 worker-level 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. Living in 2007 in a severe-fluctuation area could reduce 2014 employment by "scarring" workers through layoffs, or by making workers more likely to live in 2014 in an area being hit by new labor demand shocks, or something else. I estimate the combined effect.[27] Section 4 executes the strategy detailed in this section; Section 5 probes mechanisms.

3.1 Empirical Design

Consider local areas c that experience a (for simplicity) binary 2007-2009 employment fluctuation: severe or mild. Workers i live in different local areas in 2007. Denote i's observed 2014 binary employment status as $EMPLOYED_{i2014}$. Let $EMPLOYED_{i2014}(1)$ denote i's potential 2014 employment if her 2007 local area had experienced a severe fluctuation, and let $EMPLOYED_{i2014}(0)$ denote i's potential 2014 employment if her 2007 local area had experienced a mild fluctuation. Define the causal effect of living in 2007 in a severe-fluctuation area on 2014 employment in a given sample as the average difference between the workers' potential outcomes: $E[EMPLOYED_{i2014}(1) - EMPLOYED_{i2014}(0)]$.

If workers were randomly assigned in 2007 across local areas, one could estimate the causal effect of living in 2007 in a severe-fluctuation area using unconditional differences in worker-level panel data as $E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 1] - E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 0]$, where $SEVERE_{c(i2007)}$ is an indicator for whether i was living in a severe-fluctuation area in 2007. Lacking random assignment, I assume that workers are as good as randomly assigned conditional on a rich observed vector of pre-2007-determined worker-level and area-level characteristics $\mathbf{X}_{i2007c(i,2007)}$:

$$\Big(EMPLOYED_{i2014}(0), EMPLOYED_{i2014}(1)\Big) \perp SEVERE_{c(i2007)} \mid \mathbf{X}_{i2007c(i2007)} \quad (3.1)$$

Then the casual effect of living in 2007 in a severe-fluctuation area equals the conditional

[27] Similarly, consider the finding that students randomly assigned to high-quality kindergarten classrooms subsequently attend college at higher rates and earn higher adult earnings (Chetty, Friedman, Hilger, Saez, Schanzenbach and Yagan 2011). One concludes that high-quality kindergarten classrooms cause higher adult earnings. This is true regardless of whether the earnings effect operates through college attendance.

observed employment rate difference:

$$\begin{aligned} &E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 1, \mathbf{X}_{i2007c(i2007)}] - \\ &E[EMPLOYED_{i2014}|SEVERE_{c(i2007)} = 0, \mathbf{X}_{i2007c(i2007)}] - \\ &\quad E[EMPLOYED_{i2014}(1) - EMPLOYED_{i2014}(0)]. \end{aligned} \tag{3.2}$$

With respect to the two selection threats listed in this section's introduction, the vector $\mathbf{X}_{i2007c(i2007)}$ controls for pre-2007 selection across local areas, and following workers over time in worker-level panel data controls for post-2007 selection 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 workers with similar typically observed variables (e.g. income and industry) often perform different tasks and possess different human capital across local areas.[28] However, I observe that—unlike firms in manufacturing and other industries—retail firms like Walmart and Starbucks employ workers to perform identical tasks in many local areas. I therefore assume that after excluding firm headquarters workers, workers with similar demographics 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. Restricting the analysis to people working in retail in 2006 sacrifices external validity (by analyzing only a subset of workers) for internal validity: the purpose of the worker-level exercise is to address selection threats, and retail workers provide the available setting.[29]

This paper's empirical design builds on previous worker-level designs. First, earlier work studies 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

[28] 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.

[29] To the extent that non-retail workers have relatively location-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 more discussion.

causal effect of interest is location-specific, independent of a worker's industry and 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), this paper's causal effect of interest does not condition on layoff. This is valuable both for identification since layoffs can be endogenous (Gibbons and Katz 1991) as well as for estimating effects unrelated to layoff.

3.2 Worker-Level 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 the Online Data Appendix.

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.[30] 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 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 Census-defined two-digit-NAICS retail industries: 44 or 45 (retail trade, e.g. Walmart) or 72 (accommodation and food services, e.g. Starbucks).[31] 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

[30] 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.

[31] 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.

least five CZs and restrict to the firms' employees living in those CZs.[32] This leaves me with a sample of 2,238,187 people working in 2006 at 816 retail firms across 659 CZs.[33]

3.3 Variable Definitions

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

1. Outcomes. *Employment* in a given year is an indicator for whether a worker has 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 although self-employment is not observed systematically in federal tax data, transition of affected workers to self-employment likely does not affect 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. 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 Social Security 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

[32]Specific establishments are not identified in federal tax data. Workers can move and receive a W-2 at their new residence, so I infer each firm's CZ 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.

[33]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 the Online Data Appendix for more details.

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 and because local price deflation remains contentious.

2. CZ and CZ Fluctuation. To allow 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: 92.5% of U.S. workers live in the CZ in which they work.[34] Urban CZs are similar to metropolitan statistical areas (MSAs), but unlike MSAs which exclude rural areas, every spot in the United States lies in exactly one of the 741 CZs.[35]

2007 CZ is the CZ corresponding the payee ZIP code that appears most frequently for the worker in 2006 among the approximately thirty types of information returns (filed mandatorily typically by an institution on behalf of an individual, including W-2s)—almost always equal to the W-2's payee ZIP code.[36] 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.[37] 2014 CZ is used only for secondary analyses, in Section

[34] I compute this statistic based on the 2006-2010 American Community Surveys which provide county-to-county commuting flows, restricted to people living and working in the fifty states or the District of Columbia. Monte, Redding and Rossi-Hansberg (2015) report a similar figure ($91.4\% = 1 - 0.22 \times 0.39$, from their Table 1) based on a smaller sample of CZs.

[35] Rural CZs often comprise one or two counties while urban CZs typically comprise many. For example, the Chicago CZ comprises Cook County (the county in which the city of Chicago lies) and seven surrounding counties.

[36] Numerous activities trigger an information return including employment; Social Security 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. Comparison to external data suggested that 98.2% of the U.S. population appeared on some form submitted to the IRS in 2003 (Mortenson, Cilke, Udell and Zytnick 2009).

[37] 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.

5.3. *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 fluctuation* equals the worker's 2007 CZ's detrended log employment change 2006-2009 relative to the aggregate: 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 the difference between the log 2006-2009 aggregate employment change and the log 2000-2003 aggregate employment change.[38] Detrending is important 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 fluctuations using a comprehensive tax data sample—all workers aged 25-75 in the current year with a 2007 CZ and no other restrictions—that demographically parallels the main analysis sample. I test robustness to CZ fluctuation definitions below. CZ fluctuations are in log-point units, but for simplicity I refer to them as being in percentage-point units.

Severe-fluctuation CZ is an indicator for whether a CZ's 2007 fluctuation lies in the bottom half of 2007 CZ fluctuations weighted by workers in the main analysis sample; *mild-fluctuation CZ's* are all other CZs. Coarsening CZ fluctuations into the binary severe-fluctuation indicator for the main analyses has attractive properties: the units are easy to understand, it can avoid misspecification error in linear regressions (e.g. related to extreme fluctuation values from imperfect detrending-based counterfactuals), and it ensures covariate overlap with a limited number of CZs.[39] I also present results under linear specifications below. *Severe-fluctuation natives* are workers who were living in a severe-fluctuation CZ in

[38] That is, a worker's CZ fluctuation equals $[\ln(E_{c(i2007)2009}/E_{c(i2007)2006}) - \ln(E_{c(i2007)2003}/E_{c(i2007)2000})]$ $-[\ln(E_{agg2009}/E_{agg2006}) - \ln(E_{agg2003}/E_{agg2000})]$, using the notation of the previous section and where E_{ct} denotes the number of employed workers in CZ c in year t using this paper's employment definition and with both c and t defined using year-t information returns. Note that subtracting the aggregate 2006-2009 employment change (a constant) is irrelevant to the analysis.

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

January 2007; *mild-fluctuation natives* are defined analogously.

3. Covariates. *Age* is defined as of December 31 of the year, using date of birth from Social Security Administration (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 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.[40] *Birth state* is derived from SSA records and, for immigrants, equals the state of naturalization.[41]

2006 firm equals the masked employer identification number on the worker's highest-paying 2006 W-2. *2006 firm-x-wages fixed effects* are interactions between 2006 firm and sixteen bins of the worker's 2006 W-2 wage earnings as listed on her highest-paying 2006 W-2.[42] *2006 wages* equals the worker's 2006 W-2 wage earnings (summed across the W-2s from each of her 2006 employers, covering formal employment) plus the worker's 2006 1099-MISC non-employee compensation (summed across the 1099-MISCs from each of her 2006 employers, covering independent contractor work). I deflate the wages variables to 2010 dollars using the CPI-U and, heeding a large 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.[43] I winsorize (top-code) both 2006 wages at $500,000. Since education level and race are not observed at the worker-level, *high-school dropout share*,

[40] 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.

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

[42] 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.

[43] 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.

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 in the text.

3.4 Summary Statistics

Table 2 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 CZs covered by the main analysis sample. 74.6% of the main analysis sample was employed in 2014 while 5.7% received SSDI 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.[44] 83.7% of the sample worked in 2006 in retail trade while the remainder worked in accommodation and food service. The three 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).[45] Workers in 2007 lived across 659 CZs which together account for 99.8% of continental U.S. population and employment.

Figure 4 displays a color-coded map of CZ fluctuations across the continental United States. Fluctuation 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 fluctuations in California's Central Valley but not along

[44]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.

[45]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.

California's coast. The map communicates the kinds of comparisons that I make in this paper. Phoenix—America's sixth largest city and shown in the dark red CZ in the middle of Arizona—experienced a 16th percentile fluctuation (−4.34 percentage points) 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 fluctuation (+1.25 percentage points). 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 Worker-Level Results

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

4.1 Main Estimates

Figure 5A plots the time series of estimated effects of living in 2007 in a severe-fluctuation CZ, conditional on this paper's main controls. The plotted 2014 data point is the paper's main result and equals $\hat{\beta}$ estimated on the main analysis sample in:

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

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 severe-fluctuation CZ, and $\mathbf{X}_{i2007c(i,2007)}$ is this paper's main vector of covariates. The main vector of covariates comprises worker-level demographics (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), 2007-CZ-level demographics (quartics in high-school dropout share, college graduate share, black share, and Hispanic share), and 2006 firm-x-wages fixed effects. For other years t, plotted data points equal the same coefficient from a regression of $EMPLOYED_{it}$ on the exact same right-hand-side values in the exact same sample. 95% confidence intervals are plotted in vertical lines unadjusted for multiple hypotheses, based on standard errors clustered at the 2007 state level.

The 2014 data point shows the main result: living in 2007 in a severe-fluctuation CZ is estimated to have caused the average worker to be 0.980 percentage points less likely to be employed in 2014, relative to living in 2007 in a mild-fluctuation CZ. 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, severe-fluctuation CZ status is as good as randomly assigned. Confidence intervals are substantial 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 severe-fluctuation and mild-fluctuation CZs: estimated effects are nearly zero in every year 1999-2005, before 2007 CZ fluctuations could affect employment. Hence, severe-fluctuation natives and mild-fluctuation natives exhibited similar employment attachment in the several years leading up to the 2007-2009 recession. Moreover and because employment is measured as being employed anytime during the calendar year, the estimated zero effect visible in 2007 is also consistent with unconfoundedness. The 1999-2007 time series suggests that estimated effects would have continued to be zero after 2007 in lieu of 2007 CZ fluctuations and their underlying causes. Instead, employment rates fell substantially in 2009 for severe-fluctuation natives relative to mild-fluctuation natives. Severe-fluctuation natives' employment rates continued to fall slightly 2010-2012 and then recovered a minority of the lost ground 2012-2014, landing at the −0.980 percentage-point estimate in 2014.

Table 3 columns 4-8 display coefficient estimates from equation 4.1 in the main analysis sample under various sets of controls. Column 8 corresponds to Figure 5A's 2014 data point, my preferred estimate. Column 4 shows that with no controls, living in 2007 in a severe-fluctuation area is estimated to have reduced 2014 employment by 1.843 percentage points. However, there is substantial selection across CZs on age: column 5 shows that once one includes age fixed effects, the estimated effect falls to −0.997 percentage points. Column 6-8 show that when including the other demographic controls, 2006 firm fixed effects, and 2006 firm-x-wages fixed effects, the estimate barely changes while shrinking the standard error.[46] This indicates that once one limits the population of workers to those working in retail and controls for age, there turns out to be little systematic selection across space.[47] 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 industry is observed. Columns 9-10 report placebo tests for effects of living in 2007 in a severe-fluctuation area on the worker's mean employment status across years 1999-2005 (column 9) and on 2007 employment (column 10, also displayed in Figure 5A). Both estimates are nearly zero, supporting the paper's identifying assumption.

As a 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.[48] Column 3 shows that conditional on the main controls excluding the firm-based fixed effects, the estimated effect of living in 2007 in a severe-fluctuation CZ is somewhat though insignificantly larger in magnitude than the analogous estimate in the main analysis sample in column 6: −1.235, corresponding to

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

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

[48] The outcome 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.

−1.95% of mean 2014 employment in the random sample.[49]

4.2 Robustness

I present several robustness checks in Table 4. 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). 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). 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 fluctuation intensity. Column 9 replaces the severe-fluctuation indicator with the worker's 2007 CZ fluctuation, a continuous measure of fluctuation intensity, in the main specification and with CZ fluctuations winsorized (bottom-coded and top-coded) at the sample-weighted fifth and ninety-fifth percentiles.[50] The coefficient of 0.148 (t-statistic of 4.5) indicates that a one-percentage-point more severe CZ fluctuation reduced the 2014 employment rate of its 2007 residents by 0.148 percentage points (the standard deviation of CZ fluctuations is 4.0, see Table 2).[51]

[49] The higher r-squared in column 6 relative to column 3 suggests that the main analysis sample's industry restriction removes substantial noise.

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

[51] Under an alternative fluctuation 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.

Figure 5B non-parametrically depicts this dose-response relationship between workers' 2014 employment and their CZ fluctuation by regressing 2014 employment and CZ fluctuation on the main controls, computing residuals, adding back their means for interpretation, and plotting means of the 2014 employment residuals withing twenty equal-sized bins of the CZ fluctuation residuals. Overlaid is the best-fit line estimated by regressing the 2014 employment residuals on the CZ fluctuation residuals, whose slope of course equals the 0.148 reported in Table 4 column 9. The dose-response relationship is relatively linear.

Columns 10 and 11 replicate columns 1 and 9 using an alternative employment-*rate*-based definition of CZ fluctuations—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 fluctuation measure. Column 10 reports that when using this alternative definition, living in 2007 in a severe-fluctuation CZ is estimated to have caused a 1.309 percentage-point decline in employment rates relative to living in a mild-fluctuation area, with a confidence interval that includes the main estimate. Column 11 includes the alternatively defined CZ fluctuation 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 native retail chain workers to have a nearly one-percentage-point (0.969) lower employment rate five years later in 2014. Figure 5C replicates Figure 5B to reveal the tight linear relationship underlying column 11. Although employment-rate-based fluctuation measures are more strongly related to 2014 employment than the main employment-level-based fluctuation measure, the employment-level-based measure is my preferred measure in order to more closely align with earlier work that conceives of employment-*level* impulses and endogenous employment *rate* outcomes (e.g. Blanchard and Katz 1992).

Additionally, severe-fluctuation CZs like Phoenix had attracted many in-migrants in the decades leading up to 2007; if those in-migrants were somehow negatively selected on future human capital or other employment determinant changes conditional on the main controls, the main estimate could be confounded. Column 12 addresses this concern by instrumenting for living in 2007 in a severe-fluctuation CZ using the mean of the severe-fluctuation variable in the worker's birth state. The instrument makes the point estimate insignificantly larger in magnitude (more negative).

Finally, one may be concerned that conditional on employment history, severe-fluctuation natives were simply less attached to the labor force than mild-fluctuation natives and would have withdrawn from the labor force even in the absence of 2007-2009 fluctuations. If so, one might expect to see pre-2007 employment declines among severe-fluctuation natives 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 severe-fluctuation CZ in Online Appendix Figure A.5, replicating Figure 5A on this alternative sample.[52] The independent variables are defined as of 2000 (worker-level demographics) and 2001 (CZ) rather than 2006 and 2007. The severe-fluctuation indicator continues to be defined using 2007-2009 CZ fluctuations, but it now corresponds to whether the worker was living in *2001* in a severe-2007-2009 fluctuation CZ. The graph therefore plots estimated effects of living in 2001 in a 2007-2009-defined severe-fluctuation area.

Contrary to the motivating concern, Online Appendix Figure A.5 reveals no pre-2007 employment declines in the alternative year-2000-based sample. Instead, the graph shows statistically insignificant effects of 2001 location on pre-2007 employment, with year-2001 severe-fluctuation natives exhibiting insignificantly slightly elevated employment rates 2001-2004 that then returned nearly to parity with mild-fluctuation natives by 2006. Naturally, this placebo test cannot eliminate concerns of selection in the main analysis sample because there could be additional treatment effects occurring 2001-2006 as suggested by Charles, Hurst and Notowidigdo (2015*b*), but the evidence is comforting.

Separately, it is interesting to note that the estimated post-2007 effects in the year-2000-

[52]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.

based sample (Online Appendix Figure A.5) are similar and slightly larger in magnitude than those shown in the main analysis sample (Figure 5A).[53] Effects of 2001 location on post-2007 employment correspond to a mixture of year-2007 location effects (among workers who stay in a severe-fluctuation CZ through 2007) and any pre-2007 treatment effects (e.g. differential human capital accumulation). The similarity in the results across the two samples likely reflects the fact that the vast majority of workers remained in their 2001 CZ through 2007. The slightly more negative magnitudes in Online Appendix Figure A.5 could reflect the 2001-2006 accumulation of construction-industry human capital as year-2000 retail workers disproportionately transitioned into construction in severe-fluctuation areas and reduced 2001-2006 schooling (Charles, Hurst and Notowidigdo 2015*a*), although the sample omits workers below age 25.

Hence, the main result is robust to the inclusion of several additional controls, alternative fluctuation measures, and defining the quasi-experiment using retail employment and location information from well before 2007.

4.3 Cyclical versus Persistent Effects

If the effect of 2007 location on 2014 employment simply reflects persistently elevated unemployment rates in severe-fluctuation areas, one may expect that the enduring effect of 2007 location will not endure much longer—as unemployment rates are less persistent than labor force participation rates and had converged across states in 2015 (see Figure 3B). Unemployment and participation are not observable in the worker-level data. Table 4 column 7 therefore controls for a quartic in the worker's 2007 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

[53]The estimated effect on 2014 employment in the year-2000-based sample is -1.123 with 2000-state-clustered standard error 0.260. Mean 2014 employment in the year-2000-based sample (63.4%) is naturally smaller than in the main analysis sample (74.6%) which conditions on employment closer to 2014, so the year-2000-based sample's point estimate is larger in percentage terms (-1.77% versus -1.31%).

sample's 2014 age minimum).[54] This control attenuates the main estimate (column 1) by only about 25.9%. Male unemployment rates had already nearly completely converged across space by the end of 2014 (see Online Appendix Figure A.6). Column 8 controls for the same quartic above except that the unemployment rate is averaged across the last two months of 2014; this control attenuates the main estimate by only 16.3%. These figures suggest that the closing of the unemployment rate gap beyond 2014 is unlikely to eliminate the enduring employment impact of 2007 location.

4.4 Heterogeneity

Figure 6 plots point estimates and 95% confidence intervals for several worker subgroups defined by pre-2007-determined characteristics. Each row reports results from estimating equation 4.1 on a different subsample: the full main analysis sample (reprinting the Table 3 column 8 estimate), 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.[55] The results by 2006 wage earnings bin are consistent with earlier work on the 2007-2009 recession (Hoynes, Miller and Schaller 2012) and on trade competition Autor, Dorn, Hanson and Song (2014): low-earners (defined here as those who earned less than \$15,000 in 2006) experienced larger employment losses from living in 2007 in a severe-fluctuation CZ than did high-earners (those who earned more than \$45,000 in 2006), for whom the effect of 2007 location is not statistically significant. I also find attenuated effects among the married. But there are no significant differences by gender, age, number of children, or mortgage holding.[56] This underscores that reduced migration due to dual-earner frictions (e.g. not wanting to out-migrate because a spouse is still employed locally) or underwater mortgages do not explain

[54]Using gender-unspecific and age-unspecific CZ-level unemployment rates based on LAUS county-level estimates yields a similar result: -0.728 with a standard error of 0.203.

[55]Note that the Table 3 column 8 estimate is already net of all these subgroup controls and is effectively a weighted average of the subgroup-specific effects reported in this figure.

[56]Hoynes, Miller and Schaller find that those aged 33-60 (the age range as of 2014 studied here) are similarly likely to be unemployed or nonemployed in states with different unemployment rates, in contrast to the higher unemployment rates for those younger than 25.

the results.[57]

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 2007 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. This helps to clarify a common misconception of how migration has been understood to make initial location irrelevant for long-term employment, already reviewed in Section 2. When a locale experiences an idiosyncratic contraction in global demand for its traded good (e.g. a 5% contraction in demand for cars from Detroit), the initial residents should not all need to out-migrate in order for the initial residents to avoid idiosyncratic incidence. Instead, only a small number of them need to migrate to other states (e.g. perhaps 5%)—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). Hence given the typical population reallocation documented in Section 2, the question can be said to be less "why didn't more people out-migrate?" than "why didn't typical migration generate convergence?" The next section probes and discusses potential mechanisms.

5 Mechanisms

The previous section answered this paper's question: 2007 location indeed had a large causal effect on worker's 2014 employment. This section sheds light on channels by testing between *worker-specific* mechanisms and *current-location-specific* mechanisms to the extent possible

[57] The similarity among mortgage holders is consistent with a large recent literature (Farber 2012, Schulhofer-Wohl 2012, Valletta 2013, Sahin, Song, Topa and Violante 2014) arguing that negative housing equity for mortgage holders in severe-fluctuation areas did not limit spatial adjustment, in contrast to earlier suggestions (Ferreira, Gyourko and Tracy 2010).

in the data. Define worker-specific mechanisms as those that do not depend on the worker's 2014 location, such as higher reservation wages after transitioning to disability insurance (a form of "discouragement") or the loss of firm-specific human capital after job displacement (a form of "scarring"). Define current-location-specific mechanisms as those that depend on the worker's 2014 location, such as the worker living in a local area with persistently depressed labor demand. The implication of worker-specific mechanisms is that exogenously moving the worker to another location would not affect her employment status, whereas the implication of current-location-specific mechanisms is that doing so would.[58]

5.1 Transition to Disability Insurance

A first potential worker-specific channel is disability insurance. Severe 2007-2009 fluctuations may have induced workers to supplement their income with Social Security Disability Insurance ("DI")—a usually permanent location-independent income stream—thereby permanently raising their reservation wages and reducing their employment independent of current location (Autor and Duggan 2003, Maestas, Mullen and Strand 2013).[59] One can estimate an upper bound on the contribution of DI receipt to the main employment result, under the mild monotonicity assumption that the treatment (living in 2007 in a severe-fluctuation area) did not make anyone in the analysis sample less likely to go on DI. The estimated upper bound on the DI mechanism equals the estimated treatment effect on an indicator for 2014 employment (the paper's main estimate) minus the estimated treatment effect on an indicator for whether the worker was employed in 2014 *or* was on DI in 2014. This quantity is an upper bound because it represents the contribution of DI to non-employment only if every non-employed DI recipient would have been employed without DI. The lower bound on DI's contribution equals zero, since DI recipients may have been non-employed even without

[58]This refers to an individual worker, not all workers at once, and does not capture externalities on other workers. Thus the analysis is silent on whether aggregate employment would be higher if workers were exogenously moved—the subject of Sahin, Song, Topa and Violante (2014).

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

DI.[60]

Table 5 column 2 displays the result: living in 2007 in a severe-fluctuation 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 −0.980 percentage-point effect on employment, only 7.8% of the incrementally non-employed severe-fluctuation natives were on DI by 2014, and 7.8% is the estimated upper-bound contribution of DI transition to the enduring employment impact. To the extent that any incremental DI transition was a *response* to a lack of employment rather than a *cause* of it, DI transition explains less than 7.8% of the employment impact and potentially 0%.[61] The tight upper-bound on the DI contribution is reflected in the statistically zero impact of living in 2007 in a severe-fluctuation area on 2014 DI receipt (column 3).

In contrast and of separate interest, Table 5 column 4 shows that affected elderly are estimated to have transitioned entirely to Social Security retirement benefits. Using as the dependent variable an indicator for whether the worker was employed or was collecting any SSA income (typically DI or retirement benefits), the top row shows that the effect is moderately less negative than the employment-only effect (−0.664 versus −0.980). The bottom row shows that this attenuation is driven by the elderly, for whom the employed-or-on-DI effect is nearly zero (0.040) compared to their large negative employment-only effect (−1.098). Retirement benefit receipt likely does not raise reservation wages (Coile and Levine 2007) since labor earnings do not reduce retirement benefits.[62] Nevertheless treating all SSA

[60] 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 variables identification. Lee requires that treatment affects sample attrition only "in one direction" whereas I require that treatment affects DI receipt only in one direction.

[61] DI can be awarded on a lag, so the upper bound could expand in subsequent years. However, 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).

[62] For retirees aged 66 and older, labor earnings do not affect retirement benefits in any way. For early retirees (those aged 62-65), withheld benefits due to high labor earnings (above $15,480 in 2014) generate higher future benefits approximately actuarially fairly on average—as if the worker had delayed receiving retirement benefits in the first place (Diamond and Gruber 1999).

programs as potential causes of non-employment, the second-to-last row of column 4 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.

5.2 More Layoffs

A second potential worker-specific channel is firm-specific human capital destruction or related "scarring" via more workers being laid off—following a long line of work documenting long-term earnings losses after layoff (Ruhm 1991, Jacobson, LaLonde and Sullivan 1993, Neal 1995, Couch and Placzek 2010).[63] I proxy for layoff using unemployment insurance (UI) receipt.[64] Table 5 column 7 shows that living in 2007 in a severe-fluctuation area caused workers to be 1.513 percentage points more likely to have received UI by 2014 (i.e. at any point 2007-2014), but this effect is insignificant and small relative to the sample-wide mean of 26.9 percentage points.[65] This suggests that higher rates of layoff cannot explain severe-fluctuation natives' lower 2014 employment rates. Column 9 shows that controlling for UI receipt by 2014 barely changes the employment effect estimate.[66] Column 9 is of course not quasi-experimental since layoff is endogenous. But if one assumes that the laid-off severe-fluctuation natives were on average equal or stronger on unobservables than laid-off mild-fluctuation natives—as would be expected of incremental layoffs in a layoffs-and-lemons model (Gibbons and Katz 1991)—then Column 9 indicates that higher layoffs do not explain the results since laid-off severe-fluctuation natives would be weakly stronger

[63] Displacement is typically intended to represent layoff but often must be measured as separation from a firm during a large downsizing.

[64] 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).

[65] UI receipt rates are generally high: I calculate using full-population records that 24.8% (55,883,216 million people) of all people who received a W-2 at some point 2000-2010 also received UI at some point 2000-2010. Churn is generally high: 73.5% of workers in the main analysis sample had separated from their 2006 firms by 2014.

[66] UI receipt by 2014 is insignificant even though UI receipt by 2008, 2010, and 2012 are each negative and very significant, because most 2014 UI recipients were employed in 2014 before being laid off. See Online Appendix Table 6 for these additional permutations.

on unobservables than laid-off mild-fluctuation natives.[67]

Differences in the number of layoffs is not the only channel through which layoffs could explain the enduring employment impact of 2007 location: a given layoff could be worse for workers in severe-fluctuation areas, consistent with our evolving understanding of general layoff effects being larger during weak aggregate labor markets than during strong aggregate labor markets (Davis and Von Wachter 2011). Such a mechanism could be worker-specific: laid-off workers in severe-fluctuation areas could be less likely to become re-employed and thus experience human capital decay or discouragement, depressing their employment anywhere in 2014. Alternatively the mechanism could be current-location-specific: laid-off workers stay in severe-fluctuation areas could be at the end of job queues in areas with persistently low equilibrium employment (e.g. Michaillat 2012), implying that layoffs determine which severe-fluctuation natives are non-employed but not how many.

One can place an upper bound on these combined layoff effects by analyzing 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 severe-fluctuation CZ) did not make anyone in the sample less likely to be laid off.[68] Table 5 column 11 shows that the effect of living in 2007 in a severe-fluctuation CZ on employed-in-2014-or-UI-receipt-by-2014 is -0.278 and a standard error of 0.255. Subtracting this effect from the main -0.980 percentage-point effect on employment, 71.7% of the incrementally laid off severe-fluctuation natives had collected UI by 2014, and 71.7% is the estimated upper-bound contribution of layoff to the enduring employment impact with a standard error that does not reject 100%. By construction the lower bound equals 0%, corresponding to the case in which the 71.7% incrementally non-employed would have been non-employed even without having been laid off. Hence, one concludes that layoff is predictive of which severe-fluctuation natives were non-

[67] That is, this assumption is satisfied if layoffs are random, or if severe-fluctuation-CZ establishments had higher layoff rates (column 7) and if the first workers to get laid off from each establishment are the worst workers who would be least likely to still be employed in 2014 even without layoff.

[68] For example, this exercise requires that workers who would have retired anywhere would not have been more likely to be laid off before retirement in mild-fluctuation areas than in severe-fluctuation areas.

employed. However, we cannot know whether layoffs caused low overall 2014 employment rates among severe-fluctuation natives and whether any such effects are worker-specific or current-location-specific.

5.3 Analysis of Movers

The ideal experiment for distinguishing worker-specific mechanisms from current-location-specific mechanisms would be to exogenously move 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 and instrumental variables estimates of the effect of living in *2014* in a severe-fluctuation CZ and compare it to the effect of living in 2007 in a severe-fluctuation CZ. Throughout this subsection, CZs continue to be defined as severe-fluctuation CZs or mild-fluctuation CZs using 2007-2009 CZ fluctuations. I define movers as workers who lived in 2014 in a CZ that was different from their 2007 CZ.

The starting point of this analysis is the observation that among the natives of every CZ, many workers moved to a mild-fluctuation CZ while many others moved to a severe-fluctuation CZ. Online Appendix Figure A.7 displays this fact visually by plotting the share of movers who lived in 2014 in a mild-fluctuation CZ versus the fluctuation of their 2007 CZ. Rather than nearly 100% of workers moving to mild-fluctuation CZs, approximately 40-65% of movers from every CZ moved to mild-fluctuation CZs while the remaining 35-60% moved to severe-fluctuation CZs. This follows naturally from the well-known fact that gross migration flows swamp net migration flows—due to some combination of idiosyncratic preferences, family ties, professional connections, and information and other frictions. Overall, 18.6% of workers moved across CZs, and 42.7% of (cross-CZ) movers moved from a severe-fluctuation CZ to a mild-fluctuation CZ or vice versa. Severe-fluctuation natives were slightly more likely to move: 19.2%, relative to 18.0% of mild-fluctuation workers. 44.8% of severe-fluctuation-native movers moved to a mild-fluctuation CZ while 40.5% of mild-fluctuation natives moved to a severe-fluctuation CZ.

Table 6 column 4 extends the main quasi-experimental specification of Table 3 column 8 to include in the regression an indicator for whether the worker was living in 2014 in a severe-fluctuation CZ. 2014 location is endogenous so this cross-sectional regression is purely suggestive. I include indicators for moving because moving is negative correlated with employment—perhaps because only desperate workers move or because moving is disruptive (Chetty, Hendren and Katz forthcoming)—and severe-fluctuation natives were more likely to move. In this cross-sectional regression, the effect of living in 2007 in a severe-fluctuation CZ is small and insignificantly positive (0.120) while the cross-sectional effect of living in 2014 in a severe-fluctuation CZ is -1.074. In words: among movers out of a given CZ type, movers to severe-fluctuation CZs were 1.074 percentage-points less likely to be employed in 2014 than movers to mild-fluctuation CZs.[69] The p-value on whether the 2014 CZ effect is smaller (more negative) than the 2007 CZ effect is 0.016, suggestive of a leading role for current-location-specific mechanisms.

Of course, workers moving decisions may have been correlated with unobserved employment determinants. I therefore follow the immigration literature (e.g. Card 2001) in constructing instruments based on pre-2007 age-gender-CZ-specific moving patterns. The idea is that if, say, Phoenix workers of a certain age and gender had a tendency before 2007 to move to San Antonio before 2007, that age-gender-group in Phoenix may have moved disproportionately to San Antonio after 2007 for reasons other than unobserved employment determinants. Empirically, I use the full population of people aged 25-75 in 2000; construct the mean value of people's 2007 CZ fluctuation severity (averaged over movers and non-movers) within each age-x-gender-x-2000-CZ bin; merge those age-x-gender-x-2000-CZ values to the main analysis sample on age-x-gender-x-2007-CZ; and use that mean value as an instrument for main analysis sample workers' 2014 CZ fluctuation severity. I similarly construct a second instrument for whether workers moved and interact that with the 2007

[69] Moving is correlated with non-employment, so movers from severe-fluctuation CZs to mild-fluctuation CZs have lower employment rates than non-movers even though they have higher employment rates than movers to severe-fluctuation CZs. This fact relates to the evidence from Figure 6 showing that highly-mobile subgroups did not experience attenuated effects of 2007 location.

CZ severity indicator to create a third instrument, covering the three endogenous covariates of column 4.

Column 7 displays the instrumented results. The results are very noisy but continue to suggest a leading role for current-location-specific mechanisms relative to worker-specific mechanisms (p-value 0.218) rather than the opposite.[70] Given column 7's low power and the relative power of employment-rate-defined fluctuations (Table 4 and Figure 5), columns 11 and 14 repeat columns 4 and 7 using 2007-2009 CZ employment rate declines to divide CZs into severe-fluctuation and mild-fluctuation CZs. Individual instrumented coefficient estimates remain noisy, but the results continue to point more negative effects 2014 location than 2007 location (respective p-values of 0.002 and 0.025). Hence, cross-sectional and instrumental-variables evidence from cross-CZ movers is suggestive of a large and perhaps leading role for current-location-specific mechanisms—i.e. with the enduring impact of great recession location being dependent on workers remaining in severe-fluctuation areas.

5.4 Discussion

Worker-specific effects are easy to conceptualize: identical workers as of 2007 become different workers as of 2014 via different 2007-2014 experiences that leave them with different levels of human capital (scarring) or reservation wages (discouragement). Current-location-specific effects are harder to contemplate. In standard models without scarring or discouragement, wage flexibility delivers a unique long-term employment rate for a given market area (Kydland and Prescott 1982, King, Plosser and Rebelo 1988, Mortensen and Pissarides 1994), especially under spatial arbitrage from mobile workers (Roback 1982, Blanchard and Katz 1992). But given this section's lack of evidence of worker-specific mechanisms and suggestive evidence of current-location-specific mechanisms, I close with a discussion and analysis of current-location-specific mechanisms—those in which local areas that experienced severe 2007-2009 fluctuations cause their 2014 residents to have low 2014 employment rates.

[70] The instruments exhibit a reasonable first-stage F-statistic but the first stage effect is small.

There are at least two prominent classes of current-location-specific mechanisms consistent with little employment rate convergence in spite of large population reallocation. First, 2007-2009 local fluctuations could have shifted local areas across multiple equilibria (e.g. Diamond 1982, Benhabib and Farmer 1994, christiano-harrison-1999, Eggertsson and Krugman 2012, Kaplan and Menzio 2014). This class of mechanisms is difficult to test and distinguish in available data. Second, local fluctuations may have exhibited positive serial correlation 2007-2014 (Hall 1992, Amior and Manning 2014). This class is consistent with the results, as long as repeated severe fluctuations need not generate persistently high unemployment rates since unemployment rates have converged across space. Both classes require moving frictions like idiosyncratic location preferences that permit local incumbents to bear long-term incidence of local shocks despite the option to move (Kline 2010, Moretti 2011, Busso, Gregory and Kline 2013)—consistent with 70% of adults living in their birth state (Molloy, Smith and Wozniak 2011) and 50% living within eighteen miles of their mothers.[71] I now speculate on a version of the serially-correlated-fluctuations class in which population reallocation drives the serial correlation, and then test a key prediction.

The conventional dynamic that is supposed to generate spatial employment rate convergence is simple (e.g. Blanchard and Katz 1992). A state (e.g. Michigan) experiences a one-time random-walk contraction in global consumer demand for its traded good (e.g. cars). This induces a local labor demand contraction and wage decline, which in turn induces a local labor supply (population) contraction, which then restores the original per-capita local labor demand, wage, and employment rate at lower employment and population levels. A key ingredient to this dynamic is that the local population contraction in response to a local demand contraction does not cause a further local demand contraction.

Suppose instead that during the 2007-2009 recession, a state (e.g. Arizona) experienced a local demand contraction that derived from *local* residents permanently reducing their will-

[71] See http://www.nytimes.com/interactive/2015/12/24/upshot/24up-family.html for proximity to mothers. Other papers have stressed inefficiencies of spatial frictions (e.g. Albouy 2009, Desmet and Rossi-Hansberg 2013), but distributional consequences are typically ruled out by assuming agent homogeneity.

ingness to pay for local nontraded goods—for example after a local housing bust destroyed a share of local wealth (Mian and Sufi 2014) and residents shifted to home production (Benhabib, Rogerson and Wright 1991, Aguiar and Hurst 2005).[72] Like a demand contraction for a locally produced traded good, a demand contraction for a locally produced nontraded good induces a local wage decline that in turn induces a local population contraction. But unique to the nontraded good case, the local population contraction induces a further (serially correlated) local demand contraction: each out-migrant reduces demand for the locally produced nontraded good. Thus the local population contraction leaves *per-capita* local labor demand and the depressed local wage unchanged. If some residents have idiosyncratic location preferences and if substitution effects dominate income effects (i.e. preferences are not balanced-growth), many workers will stay in spite of permanently lower wages, and those with high reservation wages will permanently withdraw from the labor force rather than work at low wages. Colloquially, Arizonans become poorer and shift to making coffee at home, which lowers wages at Arizona Starbucks even as people leave; residents with local ties stay in Arizona, and stayers with high reservation wages stop working and produce at home.[73]

A formal version of the local demand contraction explanation could require complex interactions between local traded and nontraded sectors and is beyond the scope of this paper. However, a testable prediction is that effects of 2007 location on 2014 employment should correlate more strongly with 2007-2009 local *nontraded*-sector employment contractions than with 2007-2009 local *traded*-sector employment contractions. Table 7 presents such tests: correlations between enduring location effects and 2007-2009 local measures of either nontraded employment contractions or traded employment contractions.

To construct the table, I first estimate equation 4.1 with the main controls and with 2007-CZ fixed effects instead of the severe-fluctuation indicator.[74] Then with no controls,

[72] Alternatively, local production could have suffered a permanent productivity shock.

[73] Note that this sketch predicts that local wages and employment rates exhibit unit roots; employment and population levels could stabilize depending on location preferences. Beraja, Hurst and Ospina (2015) report that wages fell in severe-fluctuation areas relative to mild-fluctuation areas at least through 2012.

[74] 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

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 net worth (Mian, Rao and Sufi 2013, Mian and Sufi 2014).[75] The first measure is intended to reflect conventional shocks to local traded demand. The second measure is intended to reflect a shock to nontraded demand particular to the 2007-2009 housing bust, as buildings are not traded. The third measure is intended to reflect deleveraging-induced contractions in local spending, as households reduce spending following wealth and collateral declines. Both the dependent and independent variables are standardized to have mean zero and standard deviation one across CZs weighted by main analysis sample size and the CZ-level regressions are also weighted by main analysis sample size, so the reported coefficients are weighted correlation coefficients.[76] 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 7 column 2 shows that the manufacturing-only Bartik shock correlates weakly and insignificantly with effects of 2007 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 2007 location on 2014 employment. This means that 2007-CZ effects on 2014 employment are more related to measures of nontraded good demand contraction that traded good demand contraction. Columns 2-4 replicate columns 6-8 for the outcome of 2007-CZ fixed effects on *2009* employment. Although noisy, the

and San Antonio CZs are −1.35 and 1.85 percentage points respectively relative to the mean.

[75]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-310-339) or non-construction (non-230-239)—interacted with the CZ's 2007 industry concentration. 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; this variable is aggregated using county population weights from Mian and Sufi's county-level measures.

[76]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.

employment effect of manufacturing-only shocks appear, if anything, to have declined between 2009 and 2014 (column 6 vs. column 2), consistent with the conventional traded good demand convergence dynamic. 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).

Hence, the results of Table 7 are consistent with a central role for nontraded good demand contractions in the enduring impact of 2007 location.[77] Moreover, the evidence suggests that the unusually persistent employment rate divergence documented in Section 2 may be explained by 2007-2009 local fluctuations having a large nontraded good demand component, relative to historical local fluctuations. The long-term decline in manufacturing employment and the rise in service employment suggests that future local fluctuations may also have large nontraded demand components and thus may also have persistent employment rate effects. Nevertheless, uncertainty in the identification of current-location-specific effects and the informality of the nontraded demand contraction explanation recommend caution in drawing strong conclusions and invite future work.

6 Conclusion

Employment rates diverged across U.S. local areas during the 2007-2009 recession, as typically happens during U.S. recessions. But atypically, employment rates since 2009 have barely converged between the severe-fluctuation and mild-fluctuation halves of the country, with a 2.2-million-person employment gap remaining in 2015. Does this employment gap reflect mere selection of different worker types across space, or were Americans jobless in 2014 because of where they were living in 2007? To isolate effects of 2007 location, I compared two million workers with unusually similar levels of pre-2007 human capital: those working in 2006 at the same retail chain firm at establishments located in different Commuting Zones. I

[77] For clarity, note that local fluctuations always depress contemporaneous local employment rates (Blanchard and Katz, Mian and Sufi 2014) while Table 7 shows that recent nontraded-good-led fluctuations have had persistent employment rate effects.

estimated that the average retail chain worker who was living in 2007 in a severe-fluctuation Commuting Zone would have been 1.0 percentage points (1.3%) more likely to be employed in 2014 if she had instead been living elsewhere in 2007. Hence, great recession location has affected residents' employment even after several years of adjustment.

The enduring employment impact of great recession location builds on the growing literature identifying long-term limits to U.S. labor market integration, defined as ex ante similar workers enjoying dissimilar long-term outcomes.[78] Previous work has found long-term impacts of layoffs (Ruhm 1991, Jacobson, LaLonde and Sullivan 1993, Neal 1995, Couch and Placzek 2010, Davis and Von Wachter 2011) and industry trade exposure (Autor, Dorn, Hanson and Song 2014). I find long-term impacts of location. Previous work has documented a long-term decline in worker migration across jobs (Hyatt and Spletzer 2013, Davis and Haltiwanger 2014) and locations (Molloy, Smith and Wozniak 2011, Kaplan and Schulhofer-Wohl 2012*b*). I find enduring cross-location employment differences in spite of normal cross-location migration after 2007-2009 fluctuations. Recent work has highlighted employment losses among the low-skilled (Jaimovich and Siu 2012, Charles, Hurst and Notowidigdo 2015*b*, Acemoglu, Autor, Dorn, Hanson and Price 2016), driven in part by disability insurance enrollment (Autor and Duggan 2003). I too find enduring employment losses concentrated among the low-skilled (2006 low-earners), but these losses were not driven by disability enrollment or ex ante skill differences—deepening the puzzle of why wage declines have not restored ex ante employment.

For modeling aggregate market-area employment, the results build on older arguments (e.g. Clark and Summers 1979, Blanchard and Diamond 1989) to show that when a market area suffers an adverse employment fluctuation, a worker's employment can remain depressed even after unemployment returns to normal. Workhorse models of employment fluctuations abstract from labor force participation and thus feature a unique employment rate at each unemployment rate (e.g. Mortensen and Pissarides 1999). Such models can successfully char-

[78] This is a useful definition even if local fluctuations made ex ante similar workers different ex post, since then labor markets were not sufficiently integrated to prevent such scarring or discouragement.

acterize post-fluctuation employment (and not just unemployment) if participation does not depend on fluctuations, instead depending only on secular forces like aging and skill-biased technical and trade change.[79] Yet I found that among very similarly aged and skilled workers, those whose 2007 local areas experienced the most negative 2007-2009 employment fluctuations also experienced the lowest 2014 employment rates. This result holds even though unemployment rates had largely converged across areas and even after directly controlling for 2014 local unemployment rates. Hence, models of area-level employment dynamics should allow for participation and employment to remain depressed after a severe fluctuation (relative to a mild-fluctuation or no-fluctuation counterfactual) even once unemployment returns to normal.[80] And to the extent that local areas are a suitable laboratory, the results suggest that the persistent decline in U.S. aggregate participation and employment following the 2007-2009 aggregate fluctuation cannot be explained simply by aging and technical and trade change.[81]

Participation could depend on past area-level fluctuations because severe-fluctuation-area residents became scarred or discouraged and thus would be non-employed anywhere. However, the employment success of cross-area movers is consistent with the areas themselves continuing to depress their residents' employment. Such a phenomenon could be explained by area-level state-dependence (area-level "hysteresis") in which a one-time fundamental shock moves the area to a less efficient equilibrium (e.g. Diamond 1982, Benhabib and Farmer 1994, DeLong and Summers 2012, Kaplan and Menzio 2014). Related but without inefficiency, Section 5.4 discussed that substitution effects between market production and home production may outweigh income effects (Benhabib, Rogerson and Wright 1991, Aguiar and Hurst 2005, cf. King, Plosser and Rebelo 1988)—implying that participation rises with wages and thus that wage declines do not restore ex ante employment.

[79] Unemployment and employment can each be worthy of focus, depending on the context.

[80] This recommendation applies to the ubiquitous models that characterize endogenous responses to one-time fluctuations. An alternative and observationally equivalent approach would be modeling responses to serially correlated fluctuations.

[81] Aaronson, Cajner, Fallick, Galbis-Reig, Smith and Wascher (2014) and CEA (2014) already argued that aging can explain only some of the decline but could not test nationwide skill-biased change explanations.

For local shock adjustment, the results suggest that a generalization of traditional models may reconcile rapid historical employment rate convergence across space (Blanchard and Katz 1992) with the current persistence. I found that enduring impacts of 2007 location are large in housing bust areas but statistically zero in manufacturing bust areas. This fact raises the possibility that local population reductions—the traditional adjustment mechanism which has also occurred since 2007—may restore original per-capita employment rates only after traded-good (*globally* driven) demand contractions but not after nontraded-good (*locally* driven) demand contractions.[82] This is a particularly useful area for future work, as dwindled employment in local manufacturing clusters means that future local demand shocks may also be relatively locally driven.

Future employment rate convergence is naturally uncertain, but I close in the vein of Blanchard-Katz's motivating question—"when will things return to normal in [severe-fluctuation areas]?"—by illustrating how long post-2009 convergence will take at its current pace. Figure 7 reprints Figure 5A's time series of the estimated causal employment effect of living in 2007 in a severe-fluctuation 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 pace, convergence will occur in the year 2023—fourteen years after 2009.[83] The graph also plots the analogous extensions of the last-available (2014-2015) cross-*state* percentage-point convergence speeds; states are on pace for convergence in 2021 or 2026, depending on whether one weights by population.[84] Hence across samples and measures, estimated post-2009 employment rate convergence is

[82] That is: local fluctuations always depress contemporaneous local employment rates (Blanchard and Katz, Mian and Sufi 2014), but perhaps only locally-driven fluctuations persistently depress local employment rates. The mechanism could be that net out-migration does not raise per-capita local labor demand when the local fluctuation is locally driven, since each out-migrant further reduces local labor demand (see Section 5.4).

[83] For context on whether fourteen-year convergence is fast or slow, even five-year convergence can be considered slow: "My first comment [on Blanchard-Katz] 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). As a more recent benchmark, Walker (2013) highlights large transitional costs to environmental regulation when earnings convergence between affected and unaffected workers took five years.

[84] Convergence here means convergence to states' pre-recession employment rate differences.

on pace to stretch into the 2020s—amounting to more than a relative "lost decade" of depressed employment for severe-fluctuation areas and their pre-recession residents.[85] Note that an autoregressive model with constant-*percentage* convergence would of course exhibit even slower convergence to any given threshold than the plotted constant-percentage-point convergence.

Regardless of future dynamics, 2007 location has substantially affected workers' long-term employment and thus exacerbated within-skill income inequality. Such limits to local labor market integration suggest a view 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 more than mere economic geography, instead reflecting causal effects of location on economic outcomes and distribution.

[85]The lost-decade reference pertains to average outcomes in severe-fluctuation areas relative to mild-fluctuation areas—similar to references to Japan's purported lost decade of real income growth during the 1990s relative to peer countries and benchmark forecasts.

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Employment Gap Appendix

This appendix documents the computation of the 2.24 million employment gap statistic reported in Section 2.3. On average 2003-2007, the population-weighted employment rate in severe-fluctuation states minus that in mild-fluctuation states equaled -0.880 percentage points. In 2015, severe-fluctuation states had an adult civilian noninstitutional population of 142.1 million with a 58.18% population-weighted employment rate while mild-fluctuation states had an adult civilian noninstitutional population of 108.7 million with a 60.87% population-weighted employment rate.[86] Then the full-convergence employment rate in severe-fluctuation states (e_S^*) and in mild-fluctuation 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 severe-fluctuation and mild-fluctuation states to the pre-2007 difference and the second equation imposes equality between the full-convergence aggregate employment level (rate) and the actual 2015 aggregate employment level (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 severe-fluctuation 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. Likewise, 1.116 ($= 108.71 \times (.60870 - .59843)$) million more residents of mild-fluctuation 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 around the actual 2015 aggregate employment rate, a 2.2-million-person employment gap between severe-fluctuation and mild-fluctuation states remained in 2015.

Online Data Appendix

This appendix provides additional details on two components of the sample frame documented in Section 3.2. 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 conditional 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

[86] To align these figures with the -1.81 severe-mild percentage-point gap reported in the text, note $58.18 - 60.87 + 0.88 = -1.81$. Populations differ between the two fluctuation groups because the unweighted median was used to define the groups.

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.

TABLE 1
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.

TABLE 2
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 filers (%)	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		
Severe CZ fluctuation (%)	49.9	50.0	53.9	49.8
CZ fluctuation (pp)	-0.8	4.0	-0.3	3.7
Number of people	2,238,187		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. 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. 2006 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). 2006 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. 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 and state derive from the worker's January 2007 location as reflected most commonly on her 2006 information returns. CZ fluctuation equals the worker's 2007 CZ's log 2006-2009 employment level change, minus the worker's 2007 CZ's log 2000-2003 employment level change, winsorized (bottom-coded and top-coded) at the sample-weighted 5th and 95th percentiles. Severe CZ fluctuation is an indicator for the worker having a below-median CZ fluctuation. 2007 state the state with most or all of the 2007 CZ's population.

TABLE 3
Employment Effects of Great Recession Location

Outcome:	Employed in 2014 minus employed in 2006			Employed in 2014					Employed 1999-2005	Employed in 2007
	(pp)	(pp)	(pp)	(pp)	(pp)	(pp)	(pp)	(pp)	(pp)	(pp)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Severe-fluctuation CZ in 2007	-1.043	-1.080	-1.235	-1.843	-0.998	-0.951	-1.044	-0.980	0.029	0.012
	(0.233)	(0.236)	(0.274)	(0.693)	(0.415)	(0.291)	(0.237)	(0.239)	(0.233)	(0.057)
Age FEs		X	X		X	X	X	X	X	X
Other demographics			X			X	X	X	X	X
2006 firm FEs							X			
2006 firm FEs × 2006 wages FEs								X	X	X
Main analysis sample				X	X	X	X	X	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	2,238,310	2,238,310
R^2	0.00	0.04	0.08	0.00	0.12	0.19	0.20	0.20	0.19	0.16
Employment mean (%)	63.3	63.3	63.3	74.6	74.6	74.6	74.6	74.6	89.5	93.8
Estimate divided by emp. mean (%)	-1.65	-1.70	-1.95	-2.47	-1.34	-1.27	-1.40	-1.31	0.03	0.01

Notes – This table displays estimates of the effect of living in 2007 in a severe-fluctuation CZ on 2014 employment in the 1% population sample (columns 1-3) and in the main analysis sample (columns 4-10) conditional on the listed controls. 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. The last two columns constitute placebo tests. Employed 1999-2005 equals the mean of the worker's employment indicators 1999-2005. Employment mean denotes the given sample's 2014 employment rate (columns 1-8), 1999-2005 mean employment rate (column 9), or 2007 employment rate (column 10). Standard errors are clustered by 2007 state.

TABLE 4
Robustness of Employment Effects 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)
Severe-fluctuation CZ in 2007	-0.980 (0.239)	-0.984 (0.225)	-0.932 (0.230)	-0.938 (0.245)	-1.072 (0.215)	-0.925 (0.253)	-0.727 (0.199)	-0.820 (0.193)				-1.542 (0.440)
2007 CZ's fluctuation									0.148 (0.033)			
Severe-fluctuation CZ in 2007, emp. rate defn.										-1.309 (0.294)		
2007 CZ's fluctuation, emp. rate defn.											0.969 (0.167)	
Main controls	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 unemployment rate							X					
End-of-2014 unemployment rate								X				
Instrumented with birth state fluctuation												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
R^2	0.20	0.21	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20
Employment 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
Estimate divided by emp. mean (%)	-1.31	-1.32	-1.25	-1.26	-1.44	-1.24	-0.97	-1.10	0.20	-1.75	1.30	-2.07

Notes – This table presents estimates of the effect of living in 2007 in a severe-fluctuation CZ on 2014 employment in the main analysis sample, with various additional controls or alternative fluctuation measures. Column 1 replicates the main specification (Table 3 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 2007 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 the same quartic as in column 7 except that the unemployment rate is averaged across the last two months of 2014. Column 9 replaces the severe-fluctuation indicator with the worker's 2007 CZ fluctuation, a continuous measure of fluctuation intensity, where CZ fluctuations are winsorized (bottom-coded and top-coded) at the sample-weighted 5th and 95th percentiles. Columns 10 and 11 replicate columns 1 and 9 using an alternative employment-rate-based definition of CZ fluctuations, 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 fluctuation measure. Column 12 instruments for living in 2007 in a severe-fluctuation CZ using the mean of the binary CZ severity variable in the worker's birth state. Employment mean denotes the sample's 2014 employment rate. Standard errors are clustered by 2007 state.

TABLE 5

Disability Insurance, Social Security, Layoffs, and Employment Effects of Great Recession Location

A. Disability Insurance and Any SSA Receipt

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

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)
Severe-fluctuation CZ in 2007	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 severe-fluctuation CZ on various employment and social insurance outcomes. All outcomes are binary indicators. See Table 2 for variable definitions. Each cell of Panel A represents a separate regression and reports the coefficient and standard error on the severe-fluctuation indicator. Panel A column 1 replicates the specification underlying Table 3 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 4 for more permutations. Standard errors are clustered by 2007 state.

TABLE 6
Movers-Based Anaylsis of Worker-Specific vs. Current-Location-Specific Mechanisms

A. Using Main Fluctuation Definition

Outcome:	Employed in 2014						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Severe-fluctuation CZ in 2007	-0.980 (0.239)		-0.514 (0.484)	0.120 (0.311)	-0.286 (0.282)	-0.573 (0.502)	4.582 (6.588)
Severe-fluctuation CZ in 2014		-0.949 (0.275)	-0.556 (0.528)	-1.072 (0.288)	-0.764 (0.252)	-0.511 (0.526)	-5.968 (6.841)
Moved across CZ's 2007-2014				-2.359 (0.288)			5.537 (3.698)
Moved across CZ's 2007-2014 × Severe-fluctuation CZ in 2007				-0.790 (0.481)			-3.358 (5.600)
Main controls	X	X	X	X	X	X	X
Movers only					X		
Instrument destination using pre-2007 patterns						X	X
Instrument moving using pre-2007 moving rates							X
N	2,238,310	2,238,310	2,238,310	2,238,310	416,291	2,238,310	2,238,310
R^2	0.20	0.20	0.20	0.21	0.21	0.20	0.20
p-value: 2014 CZ effect < 2007 CZ effect			0.483	0.016	0.095	0.475	0.218

B. Using Employment-Rate Fluctuation Definition

Outcome:	Employed in 2014						
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Severe-fluctuation CZ in 2007, emp. rate defn.	-1.309 (0.294)		-0.137 (0.530)	0.082 (0.323)	-0.133 (0.356)	-0.242 (0.538)	8.123 (4.470)
Severe-fluctuation CZ in 2014, emp. rate defn.		-1.500 (0.321)	-1.394 (0.573)	-1.580 (0.308)	-1.303 (0.262)	-1.312 (0.568)	-10.270 (4.702)
Moved across CZ's 2007-2014				-2.647 (0.303)			5.377 (3.708)
Moved across CZ's 2007-2014 × Severe-fluctuation CZ in 2007				-0.223 (0.460)			-4.291 (3.976)
Main controls	X	X	X	X	X	X	X
Movers only					X		
Instrument destination using pre-2007 patterns						X	X
Instrument moving using pre-2007 moving rates							X
N	2,238,310	2,238,310	2,238,310	2,238,310	416,291	2,238,310	2,238,310
R^2	0.20	0.20	0.20	0.21	0.21	0.20	0.20
p-value: 2014 CZ effect < 2007 CZ effect			0.120	0.002	0.007	0.159	0.025

Notes – Column 1 reproduces the paper's main specification (Table 3 column 8). As in other tables, severe-fluctuation CZ in 2007 is an indicator for whether the worker lived in January 2007 in a CZ that experienced a severe 2007-2009 fluctuation. Likewise, severe-fluctuation CZ in 2014 is an indicator for whether the worker lived in January 2014 in a CZ that experienced a severe 2007-2009 fluctuation. Moved 2007-2014 is an indicator for the worker's January 2014 CZ being different from her January 2007 CZ. Column 6 instruments for 2014 CZ fluctuation severity using an instrument constructed as follows: using the full population of people aged 25-75 in 2000, I construct the mean value of people's 2007 CZ fluctuation severity (averaged over movers and non-movers) within each age-x-gender-x-2000-CZ bin and merge those age-x-gender-x-2000-CZ values (the instrument) to the main analysis sample on age-x-gender-x-2007-CZ. I similarly construct a second instrument for whether workers moved and interact that with the 2007 CZ severity indicator to create a third instrument, covering the three endogenous covariates of column 7. Panel B replicates Panel A except using employment rate changes to define CZ fluctuation severity, as in Table 4 column 10. Standard errors are clustered by 2007 state.

TABLE 7
Correlates of Great Recession Location Effects

Outcome:	2007-CZ Effect on 2014 Employment				2007-CZ Effect on 2009 Employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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 simply 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 3 column 8) except with the severe-fluctuation 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-310-339) or non-construction (non-230-239) as indicated—interacted with the CZ's 2007 industry concentration. 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 housing net worth declines are available only for 363 CZs; these CZ cover 95% of the main analysis sample.

ONLINE APPENDIX TABLE 1
Unit Root Tests of State-Level Labor Market Outcomes

A. Panel Unit Root Tests

Displayed statistic:	p-value that All States Have Unit Roots		
Sample years:	1978-1990	1978-2007	1978-2015
	(1)	(2)	(3)
Outcome			
$\ln(E_{st})$	1.00	1.00	0.17
$\ln(E_{st}) - \ln(E_{agg,t})$	1.00	0.95	0.25
$\Delta\ln(E_{st}) - \Delta\ln(E_{agg,t})$	**0.21**	**0.00**	**0.00**
$\ln(E/L_{st})$	0.25	0.03	0.00
$\ln(E/L_{st}) - \ln(E/L_{agg,t})$	**0.10**	**0.00**	**0.00**
$\Delta\ln(E/L_{st}) - \Delta\ln(E/L_{agg,t})$	0.93	0.00	0.00
$\ln(L/P_{st})$	1.00	0.00	0.97
$\ln(L/P_{st}) - \ln(L/P_{agg,t})$	**0.16**	**0.04**	**0.26**
$\Delta\ln(L/P_{st}) - \Delta\ln(L/P_{agg,t})$	0.00	0.00	0.00

B. State-Specific Unit Root Tests

Displayed statistic:	Number of States for which a Unit Root is Rejected (Augmented Dickey-Fuller Tests)								
Sample years:	1978-1990			1978-2007			1978-2015		
p-value rejection threshold:	5%	10%	25%	5%	10%	25%	5%	10%	25%
	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome									
$\ln(E_{st})$	1	1	7	0	3	7	2	6	13
$\ln(E_{st}) - \ln(E_{agg,t})$	5	6	8	1	3	10	4	8	13
$\Delta\ln(E_{st}) - \Delta\ln(E_{agg,t})$	**4**	**5**	**12**	**12**	**21**	**42**	**26**	**39**	**47**
$\ln(E/L_{st})$	0	0	9	2	3	11	5	19	43
$\ln(E/L_{st}) - \ln(E/L_{agg,t})$	**3**	**6**	**14**	**7**	**12**	**32**	**9**	**20**	**42**
$\Delta\ln(E/L_{st}) - \Delta\ln(E/L_{agg,t})$	2	3	6	25	33	49	40	44	51
$\ln(L/P_{st})$	0	0	7	7	11	19	3	5	10
$\ln(L/P_{st}) - \ln(L/P_{agg,t})$	**2**	**7**	**18**	**4**	**7**	**19**	**4**	**6**	**13**
$\Delta\ln(L/P_{st}) - \Delta\ln(L/P_{agg,t})$	7	8	17	39	45	49	45	49	51

C. Standard Deviation of Outcomes over Time

Displayed statistic:	Standard Deviation								
Outcome:	$\ln(E_{st})$	$\ln(E_{st}) - \ln(E_{agg,t})$	**$\Delta\ln(E_{st}) - \Delta\ln(E_{agg,t})$**	$\ln(E/L_{st})$	**$\ln(E/L_{st}) - \ln(E/L_{agg,t})$**	$\Delta\ln(E/L_{st}) - \Delta\ln(E/L_{agg,t})$	$\ln(L/P_{st})$	**$\ln(L/P_{st}) - \ln(L/P_{agg,t})$**	$\Delta\ln(L/P_{st}) - \Delta\ln(L/P_{agg,t})$
	(13)	(14)	**(15)**	(16)	**(17)**	(18)	(19)	**(20)**	(21)
Sample years									
1978-1987	1.012	1.011	**0.018**	0.025	**0.022**	0.008	0.060	**0.059**	0.010
1988-1997	1.016	1.015	**0.017**	0.017	**0.016**	0.006	0.058	**0.058**	0.010
1998-2007	1.016	1.016	**0.012**	0.012	**0.011**	0.004	0.057	**0.057**	0.009
2008-2015	1.007	1.007	**0.013**	0.023	**0.018**	0.007	0.066	**0.064**	0.009
1978-2015	1.021	1.012	**0.016**	0.022	**0.017**	0.006	0.063	**0.059**	0.009

Notes – This table analyzes possible unit roots in LAUS state labor market outcomes. Panel A lists Fisher tests of the hypothesis that all states have unit roots; small p-values suggest stationarity. Panel B lists the number of states for which a unit root is rejected using augmented Dickey-Fuller tests; large numbers suggest stationarity. Panel C lists simple standard deviations in various sample ranges; stability or declines over time suggest stationarity. Bolded rows and columns indicate the three measures actually used the autoregressive system of Blanchard and Katz (1992) and Section 2.

ONLINE APPENDIX TABLE 2
2007-2009 State Fluctuations and 2007-2015 State Employment Rate Changes

2007-2009 Fluctuation Rank	State	2007-2009 Fluctuation	Change in Employment Rate 2007-2015	2007-2009 Fluctuation Rank	State	2007-2009 Fluctuation	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 fluctuations and 2007-2015 percentage-point changes in state-level employment rates that underlie Figures 2-3. See the notes to those figures for details.

ONLINE APPENDIX TABLE 3
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.

ONLINE APPENDIX TABLE 4A
Updated Estimates of State Labor Market Adjustment
Two Estimation Sample Ranges and Actual 2009-2015 Estimates

Impulse:	–1% State-Level Employment Fluctuation														
Labor market outcome:	Employment (%)			Population (%)			Employment Rate (pp)			Participation Rate (pp)			Unemployment Rate (pp)		
Sample range:	'78-'90	'78-'15	'09-'15	'78-'90	'78-'15	'09-'15	'78-'90	'78-'15	'09-'15	'78-'90	'78-'15	'09-'15	'78-'90	'78-'15	'09-'15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Years after fluctuation (actual year):															
0 (2009)	-1.000	-1.000	-1.000	-0.363	-0.325	-0.229	-0.388	-0.419	-0.479	-0.229	-0.301	-0.253	0.267	0.206	0.365
				(0.050)	(0.027)	(0.107)	(0.030)	(0.017)	(0.067)	(0.022)	(0.015)	(0.050)	(0.032)	(0.017)	(0.069)
1 (2010)	-1.484	-1.506	-1.314	-0.691	-0.589	-0.418	-0.482	-0.570	-0.556	-0.287	-0.411	-0.306	0.329	0.277	0.407
	(0.084)	(0.034)	(0.117)	(0.090)	(0.052)	(0.167)	(0.038)	(0.021)	(0.127)	(0.027)	(0.019)	(0.055)	(0.037)	(0.024)	(0.080)
2 (2011)	-1.537	-1.617	-1.452	-0.918	-0.755	-0.599	-0.376	-0.535	-0.530	-0.244	-0.403	-0.323	0.227	0.237	0.343
	(0.146)	(0.069)	(0.148)	(0.123)	(0.075)	(0.206)	(0.046)	(0.023)	(0.157)	(0.030)	(0.018)	(0.068)	(0.040)	(0.025)	(0.092)
3 (2012)	-1.426	-1.564	-1.585	-1.056	-0.856	-0.712	-0.225	-0.439	-0.542	-0.176	-0.354	-0.398	0.093	0.161	0.255
	(0.172)	(0.097)	(0.181)	(0.136)	(0.087)	(0.235)	(0.045)	(0.030)	(0.184)	(0.029)	(0.021)	(0.072)	(0.040)	(0.025)	(0.097)
4 (2013)	-1.297	-1.468	-1.750	-1.127	-0.918	-0.852	-0.103	-0.341	-0.557	-0.112	-0.299	-0.459	-0.003	0.092	0.191
	(0.173)	(0.110)	(0.209)	(0.139)	(0.092)	(0.264)	(0.037)	(0.033)	(0.209)	(0.025)	(0.024)	(0.087)	(0.035)	(0.025)	(0.086)
5 (2014)	-1.196	-1.375	-1.841	-1.156	-0.957	-0.962	-0.024	-0.259	-0.546	-0.060	-0.246	-0.504	-0.050	0.043	0.110
	(0.167)	(0.114)	(0.236)	(0.141)	(0.095)	(0.294)	(0.029)	(0.032)	(0.234)	(0.021)	(0.023)	(0.104)	(0.029)	(0.023)	(0.072)
6 (2015)	-1.131	-1.298	-1.826	-1.161	-0.983	-1.068	0.018	-0.195	-0.471	-0.022	-0.199	-0.470	-0.060	0.012	0.044
	(0.162)	(0.115)	(0.259)	(0.143)	(0.097)	(0.297)	(0.025)	(0.030)	(0.245)	(0.016)	(0.022)	(0.087)	(0.024)	(0.021)	(0.058)
7	-1.097	-1.238		-1.155	-1.001		0.035	-0.147		0.001	-0.159		-0.051	-0.004	
	(0.157)	(0.116)		(0.145)	(0.099)		(0.022)	(0.027)		(0.013)	(0.021)		(0.019)	(0.018)	
8	-1.087	-1.191		-1.145	-1.014		0.036	-0.110		0.013	-0.126		-0.036	-0.012	
	(0.153)	(0.118)		(0.147)	(0.102)		(0.018)	(0.025)		(0.011)	(0.019)		(0.013)	(0.016)	
9	-1.090	-1.157		-1.137	-1.023		0.028	-0.083		0.016	-0.098		-0.020	-0.014	
	(0.148)	(0.119)		(0.147)	(0.104)		(0.013)	(0.022)		(0.010)	(0.017)		(0.009)	(0.013)	
10	-1.101	-1.131		-1.131	-1.030		0.018	-0.062		0.014	-0.076		-0.007	-0.014	
	(0.145)	(0.120)		(0.146)	(0.106)		(0.010)	(0.019)		(0.008)	(0.016)		(0.007)	(0.011)	
11	-1.113	-1.111		-1.128	-1.035		0.009	-0.047		0.010	-0.059		0.000	-0.012	
	(0.143)	(0.120)		(0.146)	(0.108)		(0.007)	(0.017)		(0.006)	(0.014)		(0.007)	(0.009)	
12	-1.123	-1.097		-1.127	-1.039		0.002	-0.036		0.005	-0.045		0.004	-0.010	
	(0.143)	(0.120)		(0.145)	(0.110)		(0.006)	(0.014)		(0.004)	(0.012)		(0.006)	(0.007)	
13	-1.130	-1.086		-1.128	-1.043		-0.002	-0.027		0.002	-0.035		0.005	-0.008	
	(0.144)	(0.120)		(0.145)	(0.111)		(0.006)	(0.012)		(0.003)	(0.010)		(0.005)	(0.005)	
14	-1.134	-1.078		-1.129	-1.045		-0.003	-0.021		0.000	-0.027		0.005	-0.007	
	(0.145)	(0.119)		(0.145)	(0.112)		(0.005)	(0.010)		(0.003)	(0.009)		(0.004)	(0.004)	
15	-1.136	-1.072		-1.130	-1.047		-0.003	-0.016		-0.001	-0.020		0.003	-0.005	
	(0.145)	(0.119)		(0.145)	(0.113)		(0.004)	(0.008)		(0.003)	(0.007)		(0.003)	(0.003)	

Notes – This table lists estimated responses of relative state employment, population, employment rate, labor force participation rate, and unemployment rate to a –1% employment fluctuation based on coefficient estimates from the Blanchard-Katz system estimated separately on each of two sample ranges: 1978-1990 (as in Blanchard-Katz) and 1978-2015. See Appendix Figure 2 for visual representations. Each standard error equals the standard deviation of the corresponding estimate across 500 bootstrapped samples. Block-bootstrapped samples are formed by sampling error vectors by state with replacement and computing new dependent variable values based the original independent variables, the coefficients obtained from estimating equations 2.1-2.3 on the original data, and the sampled error vector. See Online Appendix Table 5 for the underlying system coefficient estimates. Also listed for comparison are reduced form estimates of actual 2009-2015 adjustments as graphed in Figures 3A-B with robust standard errors from the underlying reduced form regressions and calculated via delta method; see the notes to Online Appendix Figure 2 for guidance on interpreting comparisons between these predicted and actual values.

ONLINE APPENDIX TABLE 4B
Updated Estimates of State Labor Market Adjustment
Additional Sample Ranges

Impulse:	–1% State-Level Employment Fluctuation														
Labor market outcome:	Employment (%)			Population (%)			Employment Rate (pp)			Participation Rate (pp)			Unemployment Rate (pp)		
Sample range:	'91-'07	'78-'07	'86-'15	'91-'07	'78-'07	'86-'15	'91-'07	'78-'07	'86-'15	'91-'07	'78-'07	'86-'15	'91-'07	'78-'07	'86-'15
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Years after fluctuation:															
0	-1.000	-1.000	-1.000	-0.306	-0.342	-0.365	-0.444	-0.411	-0.398	-0.358	-0.291	-0.305	0.155	0.206	0.166
				(0.024)	(0.027)	(0.027)	(0.016)	(0.017)	(0.017)	(0.016)	(0.014)	(0.017)	(0.013)	(0.020)	(0.012)
1	-1.274	-1.448	-1.476	-0.439	-0.591	-0.595	-0.535	-0.536	-0.553	-0.431	-0.386	-0.420	0.186	0.259	0.236
	(0.032)	(0.043)	(0.031)	(0.037)	(0.054)	(0.045)	(0.024)	(0.022)	(0.023)	(0.022)	(0.016)	(0.025)	(0.019)	(0.025)	(0.017)
2	-1.116	-1.496	-1.542	-0.471	-0.733	-0.724	-0.413	-0.477	-0.513	-0.341	-0.362	-0.397	0.132	0.205	0.209
	(0.068)	(0.075)	(0.075)	(0.048)	(0.078)	(0.067)	(0.034)	(0.025)	(0.028)	(0.029)	(0.016)	(0.028)	(0.023)	(0.026)	(0.019)
3	-0.843	-1.387	-1.457	-0.461	-0.807	-0.808	-0.244	-0.363	-0.408	-0.215	-0.299	-0.331	0.060	0.122	0.145
	(0.086)	(0.092)	(0.112)	(0.056)	(0.087)	(0.082)	(0.036)	(0.028)	(0.037)	(0.031)	(0.019)	(0.032)	(0.023)	(0.026)	(0.021)
4	-0.606	-1.247	-1.351	-0.438	-0.843	-0.868	-0.108	-0.253	-0.303	-0.110	-0.232	-0.263	0.005	0.053	0.083
	(0.085)	(0.097)	(0.129)	(0.061)	(0.088)	(0.093)	(0.031)	(0.028)	(0.038)	(0.026)	(0.021)	(0.032)	(0.022)	(0.025)	(0.021)
5	-0.449	-1.121	-1.261	-0.414	-0.856	-0.912	-0.022	-0.166	-0.219	-0.040	-0.171	-0.205	-0.023	0.007	0.038
	(0.083)	(0.098)	(0.132)	(0.064)	(0.087)	(0.100)	(0.026)	(0.027)	(0.034)	(0.021)	(0.022)	(0.029)	(0.019)	(0.023)	(0.019)
6	-0.365	-1.021	-1.191	-0.396	-0.857	-0.943	0.020	-0.102	-0.155	-0.001	-0.121	-0.158	-0.030	-0.018	0.010
	(0.083)	(0.099)	(0.130)	(0.065)	(0.085)	(0.106)	(0.025)	(0.026)	(0.029)	(0.019)	(0.021)	(0.025)	(0.015)	(0.020)	(0.016)
7	-0.333	-0.947	-1.139	-0.385	-0.853	-0.965	0.033	-0.059	-0.110	0.016	-0.082	-0.120	-0.026	-0.028	-0.005
	(0.080)	(0.100)	(0.127)	(0.066)	(0.085)	(0.110)	(0.022)	(0.025)	(0.025)	(0.016)	(0.020)	(0.022)	(0.011)	(0.018)	(0.014)
8	-0.331	-0.895	-1.101	-0.379	-0.846	-0.979	0.031	-0.030	-0.077	0.020	-0.053	-0.089	-0.018	-0.029	-0.011
	(0.075)	(0.100)	(0.125)	(0.066)	(0.085)	(0.113)	(0.016)	(0.022)	(0.021)	(0.013)	(0.017)	(0.018)	(0.007)	(0.014)	(0.011)
9	-0.341	-0.860	-1.074	-0.377	-0.839	-0.989	0.023	-0.013	-0.053	0.017	-0.032	-0.066	-0.010	-0.026	-0.013
	(0.069)	(0.099)	(0.123)	(0.065)	(0.086)	(0.115)	(0.010)	(0.018)	(0.017)	(0.008)	(0.015)	(0.016)	(0.004)	(0.011)	(0.009)
10	-0.356	-0.838	-1.055	-0.377	-0.833	-0.995	0.014	-0.003	-0.037	0.011	-0.018	-0.048	-0.004	-0.021	-0.012
	(0.066)	(0.098)	(0.122)	(0.065)	(0.087)	(0.117)	(0.006)	(0.014)	(0.014)	(0.005)	(0.012)	(0.013)	(0.003)	(0.008)	(0.007)
11	-0.368	-0.825	-1.041	-0.378	-0.828	-1.000	0.007	0.002	-0.026	0.006	-0.009	-0.035	-0.001	-0.016	-0.010
	(0.064)	(0.096)	(0.121)	(0.064)	(0.087)	(0.118)	(0.004)	(0.011)	(0.012)	(0.003)	(0.009)	(0.011)	(0.002)	(0.006)	(0.005)
12	-0.376	-0.818	-1.032	-0.379	-0.825	-1.003	0.002	0.004	-0.018	0.003	-0.004	-0.025	0.001	-0.012	-0.008
	(0.063)	(0.094)	(0.121)	(0.064)	(0.088)	(0.118)	(0.003)	(0.008)	(0.009)	(0.003)	(0.007)	(0.009)	(0.002)	(0.004)	(0.004)
13	-0.381	-0.815	-1.025	-0.380	-0.822	-1.005	-0.001	0.005	-0.013	0.000	-0.001	-0.018	0.002	-0.008	-0.006
	(0.063)	(0.092)	(0.121)	(0.064)	(0.088)	(0.119)	(0.003)	(0.006)	(0.007)	(0.002)	(0.005)	(0.007)	(0.001)	(0.003)	(0.003)
14	-0.383	-0.814	-1.021	-0.381	-0.820	-1.007	-0.002	0.004	-0.009	-0.001	0.001	-0.013	0.002	-0.005	-0.004
	(0.064)	(0.090)	(0.121)	(0.064)	(0.088)	(0.119)	(0.002)	(0.004)	(0.006)	(0.002)	(0.004)	(0.005)	(0.001)	(0.002)	(0.002)
15	-0.384	-0.814	-1.018	-0.381	-0.819	-1.008	-0.002	0.003	-0.006	-0.001	0.001	-0.009	0.001	-0.003	-0.003
	(0.064)	(0.089)	(0.121)	(0.064)	(0.088)	(0.120)	(0.001)	(0.003)	(0.004)	(0.001)	(0.003)	(0.004)	(0.001)	(0.002)	(0.001)

Notes – This table replicates Online Appendix Table 4a under alternative sample ranges; see the notes to that table for details.

ONLINE APPENDIX TABLE 5
System Coefficient Estimates of State Labor Market Adjustment

Sample range:	1978-1990	1978-2015	1991-2007	1978-2007	1986-2015
	(1)	(2)	(3)	(4)	(5)
Equation 1: $\Delta\ln(E_{st})$					
$\Delta\ln(E_{s,t-1})$	0.854	0.617	0.316	0.501	0.508
	(0.092)	(0.038)	(0.045)	(0.043)	(0.041)
$\Delta\ln(E_{s,t-2})$	-0.118	-0.111	-0.059	-0.091	-0.125
	(0.050)	(0.025)	(0.032)	(0.028)	(0.028)
$\ln(E/L_{s,t-1})$	-0.362	0.071	0.242	0.197	0.262
	(0.123)	(0.060)	(0.088)	(0.067)	(0.070)
$\ln(E/L_{s,t-2})$	0.354	-0.064	-0.184	-0.129	-0.204
	(0.107)	(0.056)	(0.079)	(0.062)	(0.064)
$\ln(L/P_{s,t-1})$	-0.758	-0.278	-0.153	-0.220	-0.170
	(0.106)	(0.048)	(0.060)	(0.053)	(0.052)
$\ln(L/P_{s,t-2})$	0.336	0.168	-0.198	0.037	0.058
	(0.109)	(0.047)	(0.055)	(0.052)	(0.050)
Equation 2: $\ln(E/L_{st})$					
$\Delta\ln(E_{st})$	0.286	0.220	0.163	0.219	0.177
	(0.019)	(0.010)	(0.014)	(0.011)	(0.010)
$\Delta\ln(E_{s,t-1})$	-0.135	-0.114	-0.054	-0.105	-0.064
	(0.037)	(0.016)	(0.019)	(0.018)	(0.017)
$\ln(E/L_{s,t-1})$	1.024	1.109	0.970	1.065	1.085
	(0.053)	(0.025)	(0.036)	(0.028)	(0.028)
$\ln(E/L_{s,t-2})$	-0.332	-0.330	-0.269	-0.307	-0.316
	(0.043)	(0.023)	(0.033)	(0.025)	(0.026)
$\ln(L/P_{s,t-1})$	0.161	0.120	0.088	0.111	0.084
	(0.047)	(0.020)	(0.025)	(0.022)	(0.021)
$\ln(L/P_{s,t-2})$	-0.122	-0.096	-0.043	-0.085	-0.062
	(0.045)	(0.020)	(0.023)	(0.021)	(0.020)
Equation 3: $\ln(L/P_{st})$					
$\Delta\ln(E_{st})$	0.351	0.455	0.531	0.439	0.459
	(0.024)	(0.013)	(0.020)	(0.014)	(0.014)
$\Delta\ln(E_{s,t-1})$	-0.167	-0.126	-0.045	-0.104	-0.073
	(0.048)	(0.021)	(0.026)	(0.023)	(0.023)
$\ln(E/L_{s,t-1})$	0.166	0.047	0.025	0.059	-0.001
	(0.068)	(0.032)	(0.049)	(0.037)	(0.039)
$\ln(E/L_{s,t-2})$	-0.003	0.062	0.136	0.052	0.098
	(0.056)	(0.029)	(0.045)	(0.033)	(0.035)
$\ln(L/P_{s,t-1})$	1.111	1.117	1.007	1.087	1.061
	(0.061)	(0.026)	(0.034)	(0.029)	(0.029)
$\ln(L/P_{s,t-2})$	-0.312	-0.220	-0.129	-0.201	-0.185
	(0.058)	(0.025)	(0.031)	(0.028)	(0.028)

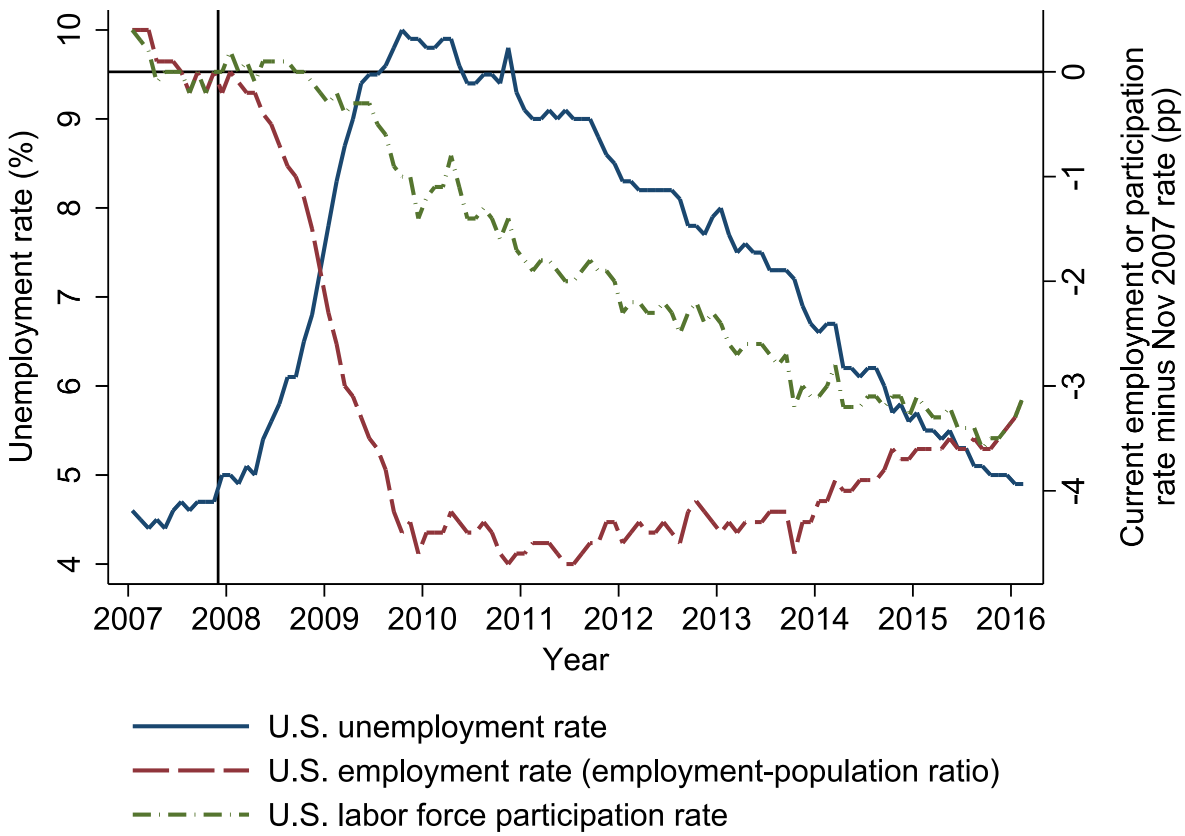
Notes – This table lists the coefficients underlying the estimates of state labor market adjustment listed in Appendix Table 4; see the notes to that table for details.

ONLINE APPENDIX TABLE 6
Layoffs and Employment Effects 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)
Severe-fluctuation CZ in 2007	-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

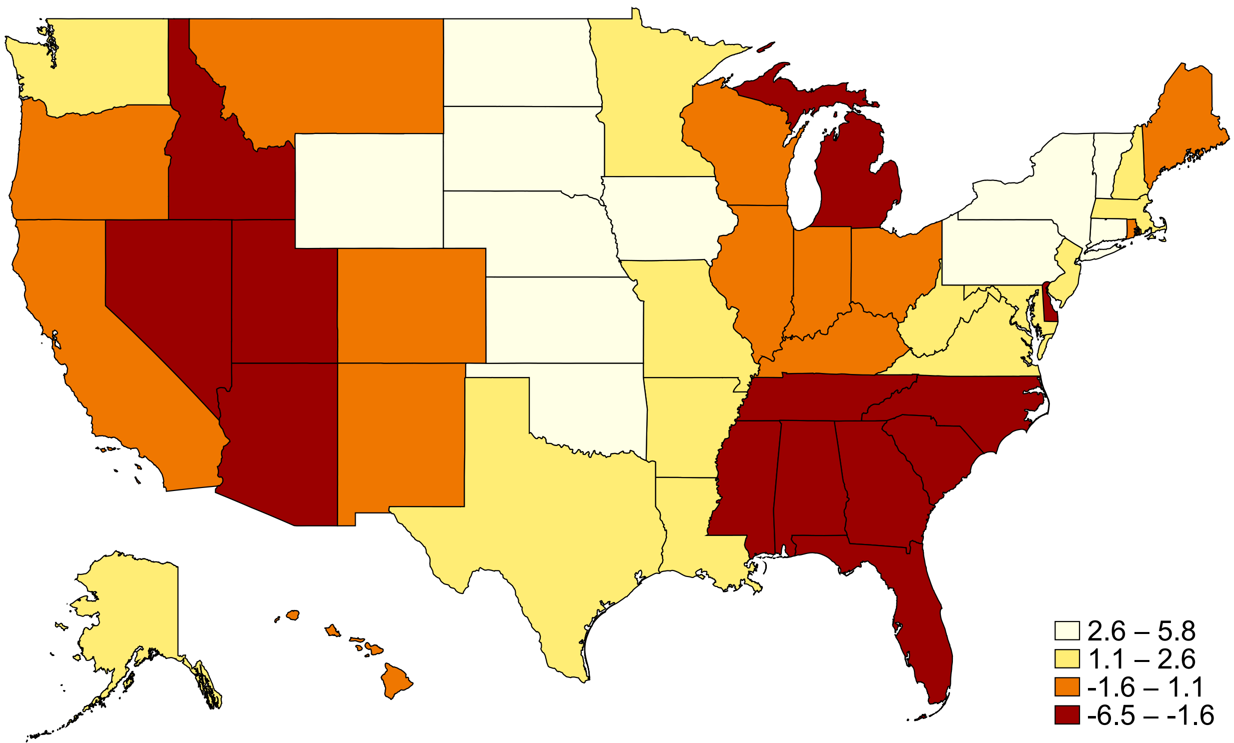
Notes – This table lists more permutations of Table 5B; see the notes to that table for details.

Figure 1: **The Puzzle of Persistently Low U.S. Employment**



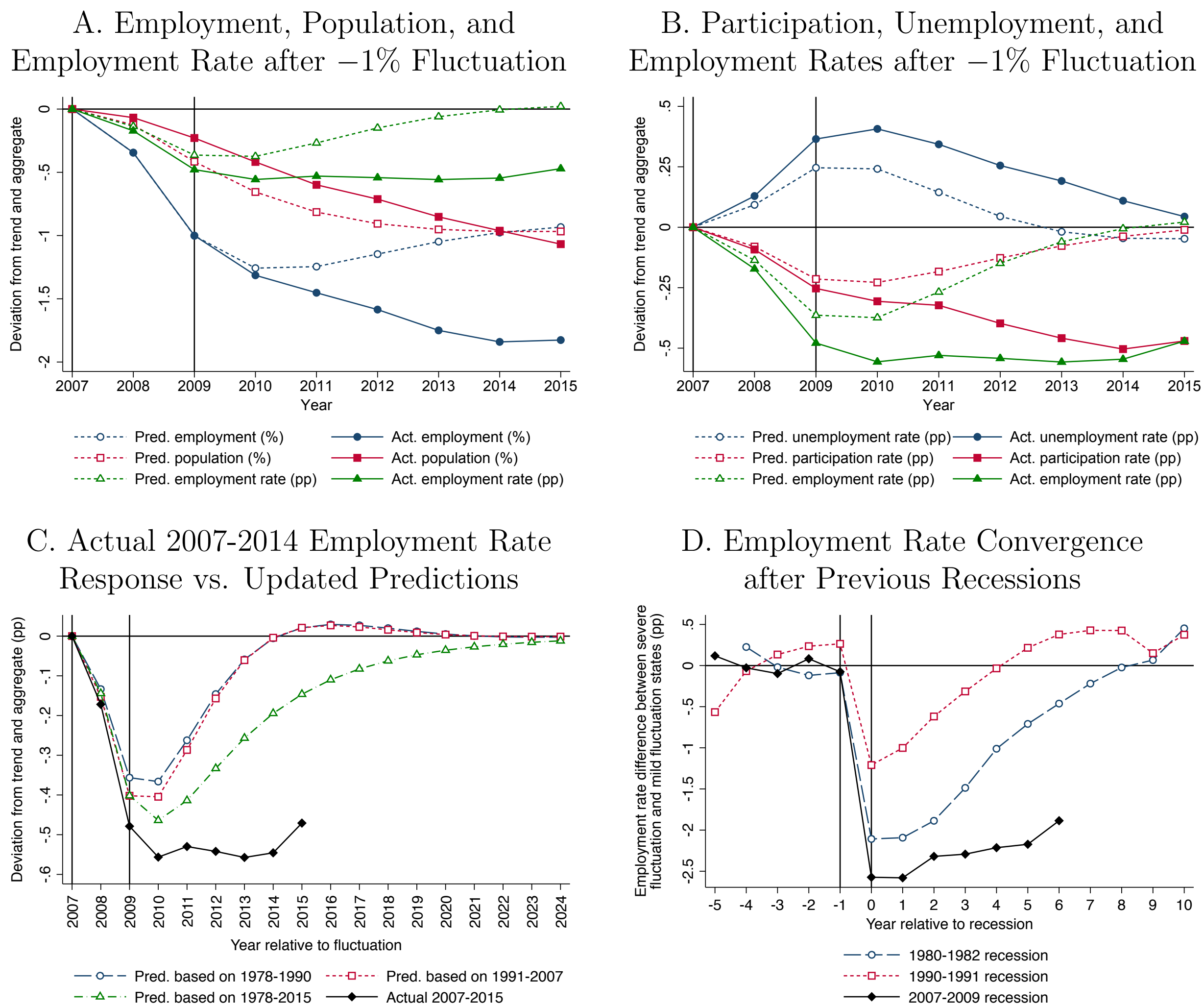
Notes: This graph plots the official seasonally adjusted Bureau of Labor Statistics U.S. labor force statistics from January 2007 through February 2016. The data are monthly and refer to the adult (16+) civilian non-institutional population. The vertical black line denotes December 2007, the first month of the 2007-2009 recession. The unemployment rate series is plotted on the left axis. The employment rate (employment-population ratio) and participation rate series are plotted on the right axis and equal the current month's value minus the November 2007 value; hence, each data point in these series denotes a percentage-point change relative to November 2007. The graph shows that the unemployment rate has returned to its pre-recession level, yet the employment rate remains three percentage points below its November 2007 level as workers have stopped participating in the labor force. Without more evidence, the persistent decline in participation and employment could reflect effects of secular nationwide forces like skill-biased technical and trade change, or enduring effects of the recession and its underlying causes. Local variation in the recession's intensity can provide a laboratory for testing between these views, if one assumes that the local variation was caused by the same underlying causes as the aggregate recession.

Figure 2: **2007-2009 State Fluctuations**



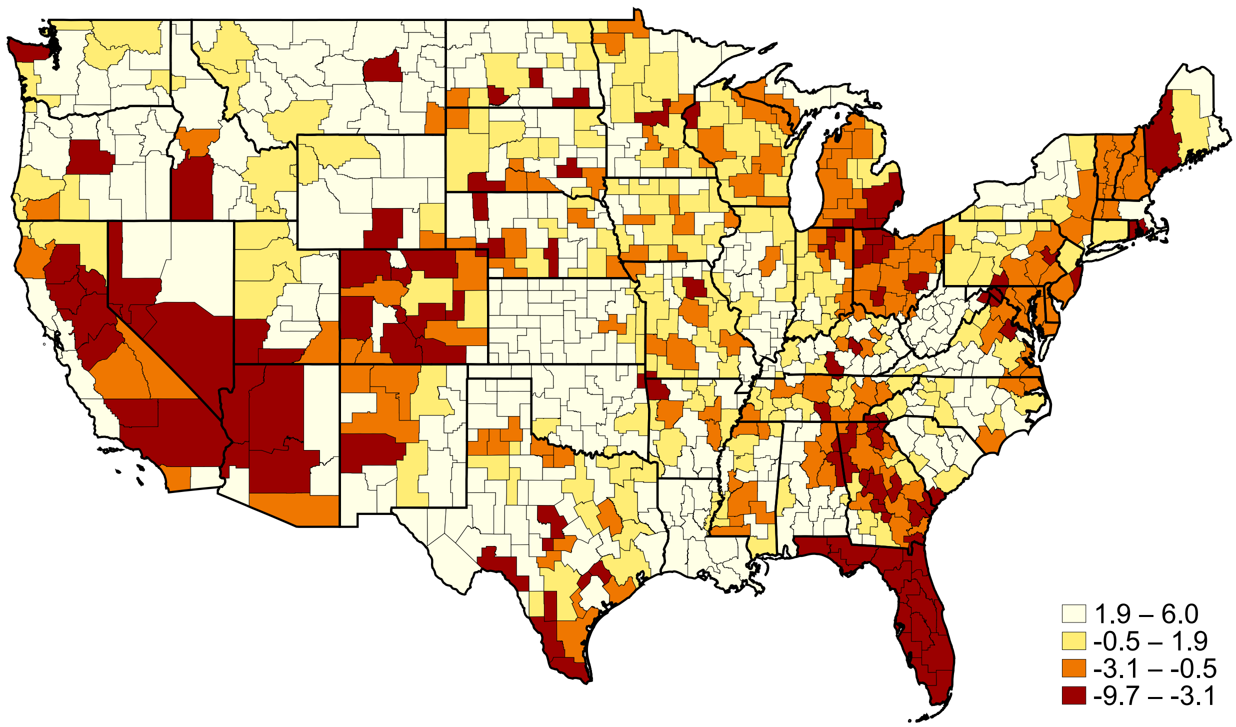
Notes: This color-coded map depicts unweighted quartiles of 2007-2009 state-level fluctuations, representing each state's unforecasted percentage-point change in its employment level relative to its own trend and the national aggregate change. Specifically, I estimate Blanchard and Katz's (1992) autoregressive system of state adult civilian noninstitutional employment, population, unemployment, and labor force participation in the Bureau of Labor Statistics's Local Area Unemployment Series (LAUS) 51-state series for years 1978-2007. I then compute each state's 2008 employment fluctuation as equal to the state's actual 2008 relative (to trend and aggregate) employment growth minus the relative employment growth predicted by the state's observed independent variable values through 2007 and the estimated coefficients. I similarly compute each state's 2009 employment fluctuation using values through 2008. Each state's fluctuation plotted in the map equals the sum of the state's 2008 and 2009 fluctuations. Units are log-point employment changes, approximately equal to percentage-point employment changes. The standard deviation of these fluctuations is 2.74; as a benchmark, the analogous standard deviation is 2.73 when computed over years 1980-1982 (corresponding to the early-1980s recessions). See Section 2.2 for further details.

Figure 3: State Employment Rate Divergence after 2007-2009 Fluctuations



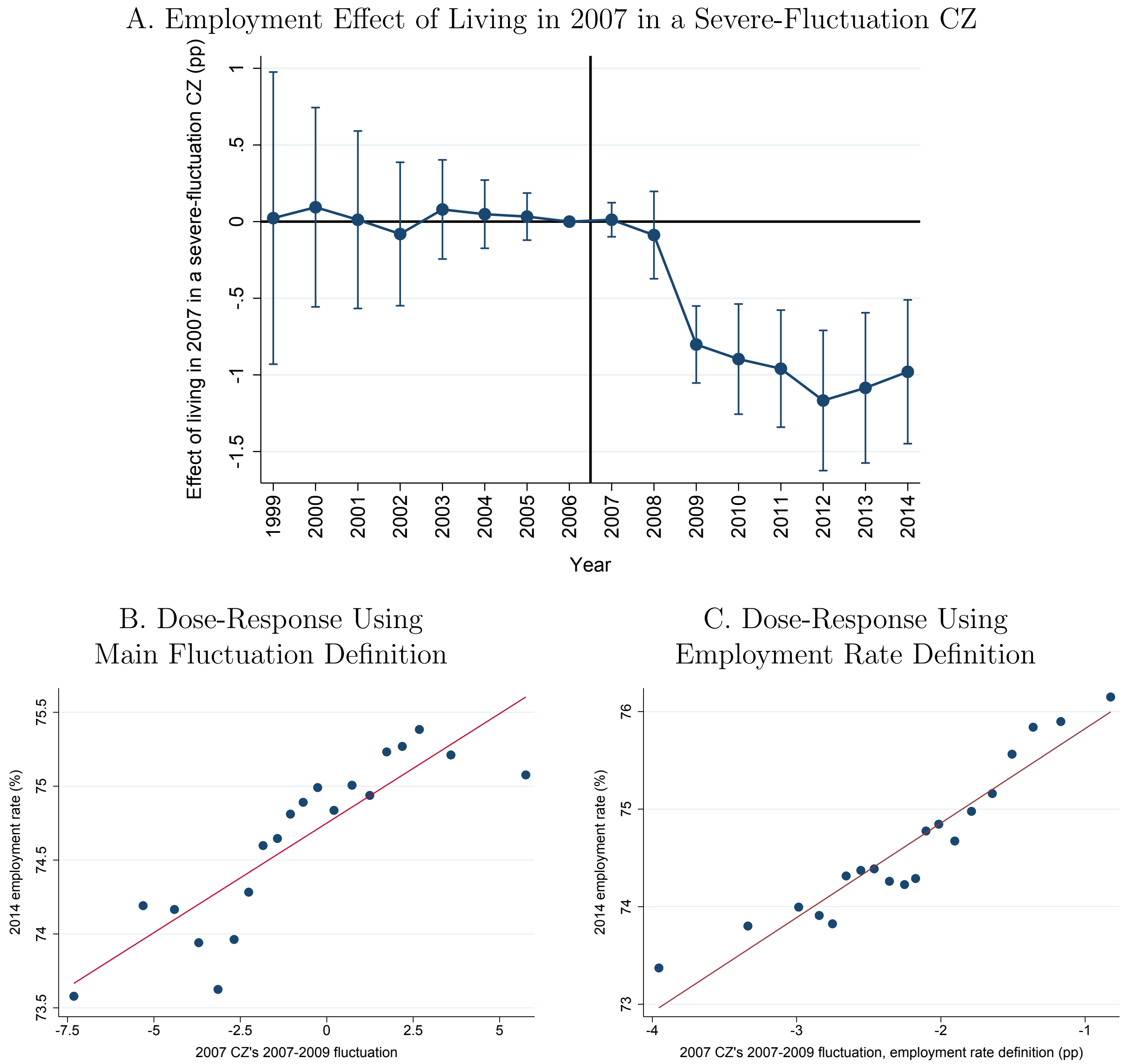
Notes: The dotted lines of panels A-B plot benchmark history-based predictions for state-level responses to a −1% 2007-2009 state-level employment fluctuation, based on estimating Blanchard-Katz's (1992) autoregressive system of state labor market outcomes using LAUS data on the original sample range 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 fluctuations (Figure 2). Panel C plots the mean actual employment rate response series from panels A-B alongside predicted employment rate series based on three estimation time ranges: 1978-1990 (as in panels A-B), 1991-2007, and 1978-2014. See Online Appendix Figure A.2 for more permutations. Panel D divides states into severe (below-unweighted-median) and mild (above-unweighted-median) fluctuation states based on 2007-2009 state-level fluctuations 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 severe-fluctuation states, minus the same mean in mild-fluctuation 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 fluctuations; 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. See Online Appendix Figure A.3 for population-weighted versions of panel D with different outcomes and for the 2001 recession, which exhibited convergence before being interrupted by correlated 2007-2009 fluctuations. See Section 2.3 for additional details.

Figure 4: 2007-2009 CZ Fluctuations



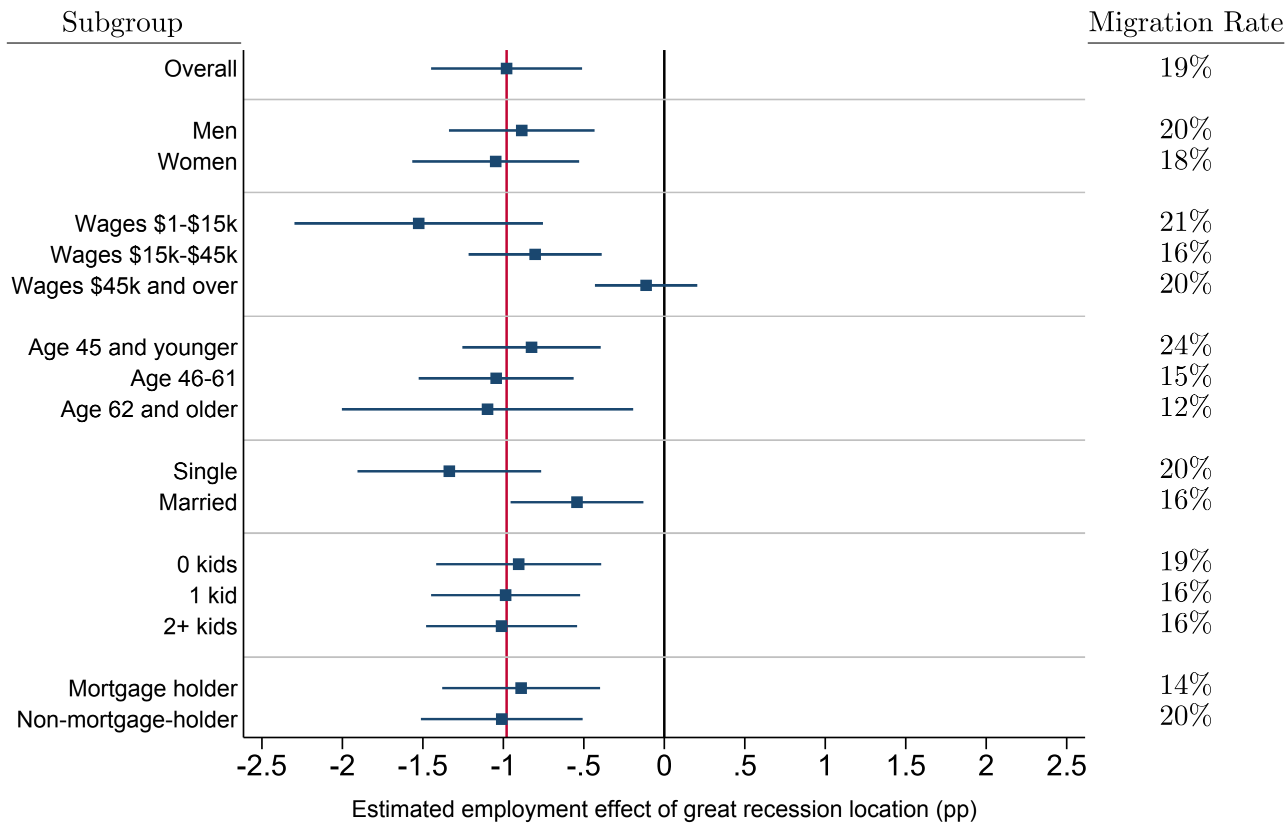
Notes: This color-coded map depicts main-analysis-sample-weighted quartiles of 2007-2009 Commuting Zone (CZ) level fluctuations. Each CZ's fluctuation equals the CZ's detrended log employment change 2006-2009 relative to the aggregate: the CZ's log 2006-2009 employment change minus the CZ's log 2000-2003 employment change, minus 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 fluctuations comprise all individuals in the tax 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 fluctuation 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 5B.

Figure 5: Employment Effects of Great Recession Location



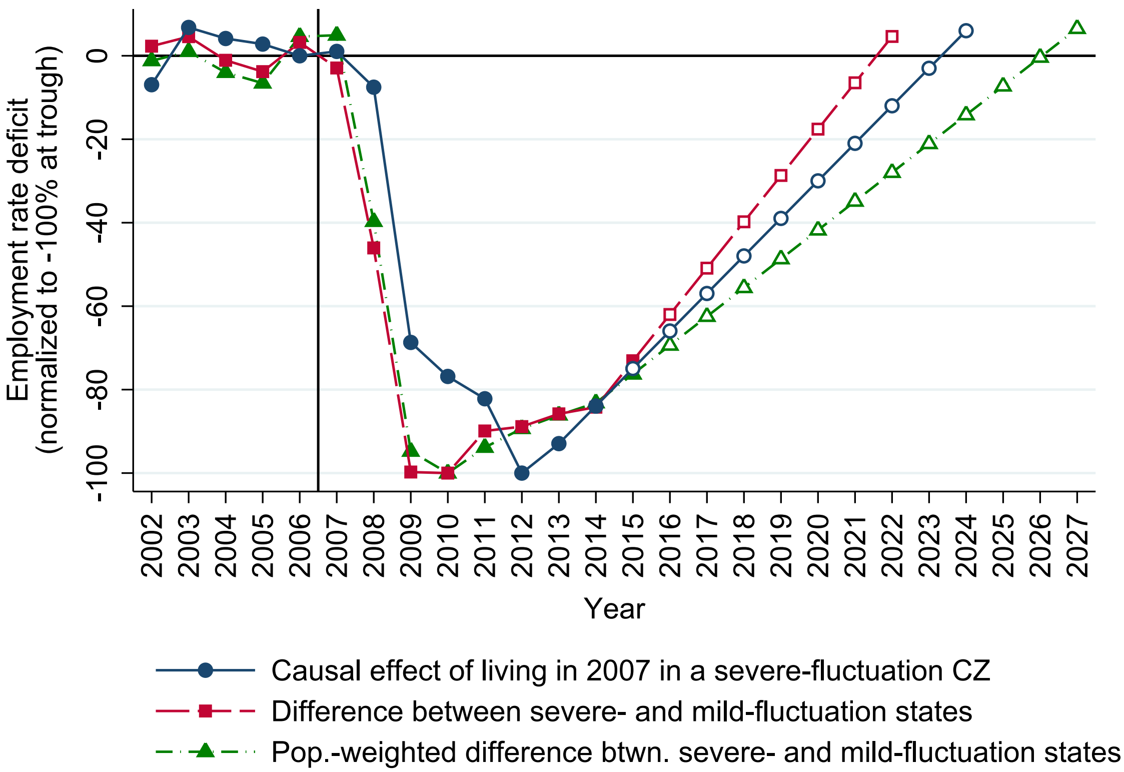
Notes: Panel A plots regression estimates of the effect of living in 2007 in a severe-fluctuation (below-median-fluctuation) CZ on annual employment in the main analysis sample conditional on firm-x-wages fixed effects and demographic controls (age fixed effects; a quartic in 2006 wage earnings; indicators 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. Panel B non-parametrically depicts the relationship between workers' 2014 employment and their 2007 CZ fluctuation by regressing 2014 employment and 2007 CZ fluctuation on the controls listed above, computing residuals, added back their means for interpretation, and plotting means of the 2014 employment residuals within twenty equal-sized bins of the 2007 CZ fluctuation residuals. Overlaid is the best-fit line (slope 0.148, standard error 0.033). Panel C repeats panel B for an alternative definition of CZ fluctuations, 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 fluctuation measure. The overlaid best-fit line has slope 0.969 and standard error 0.167. Standard errors are clustered by 2007 state.

Figure 6: Employment Effect Heterogeneity



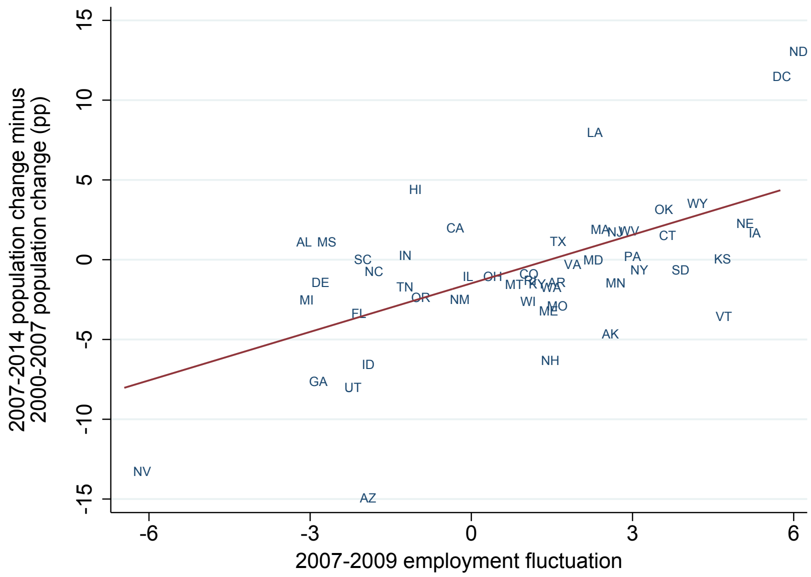
Notes: This graph plots coefficients and 95% confidence intervals of the effect of living in 2007 in a severe-fluctuation CZ on workers' 2014 employment rates—overall (equal to the 2014 data point in Figure 5A) and by subgroup. All estimates derive from the specification underlying the 2014 data point in Figure 5A. 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.

Figure 7: **Convergence Paths at Current Pace**



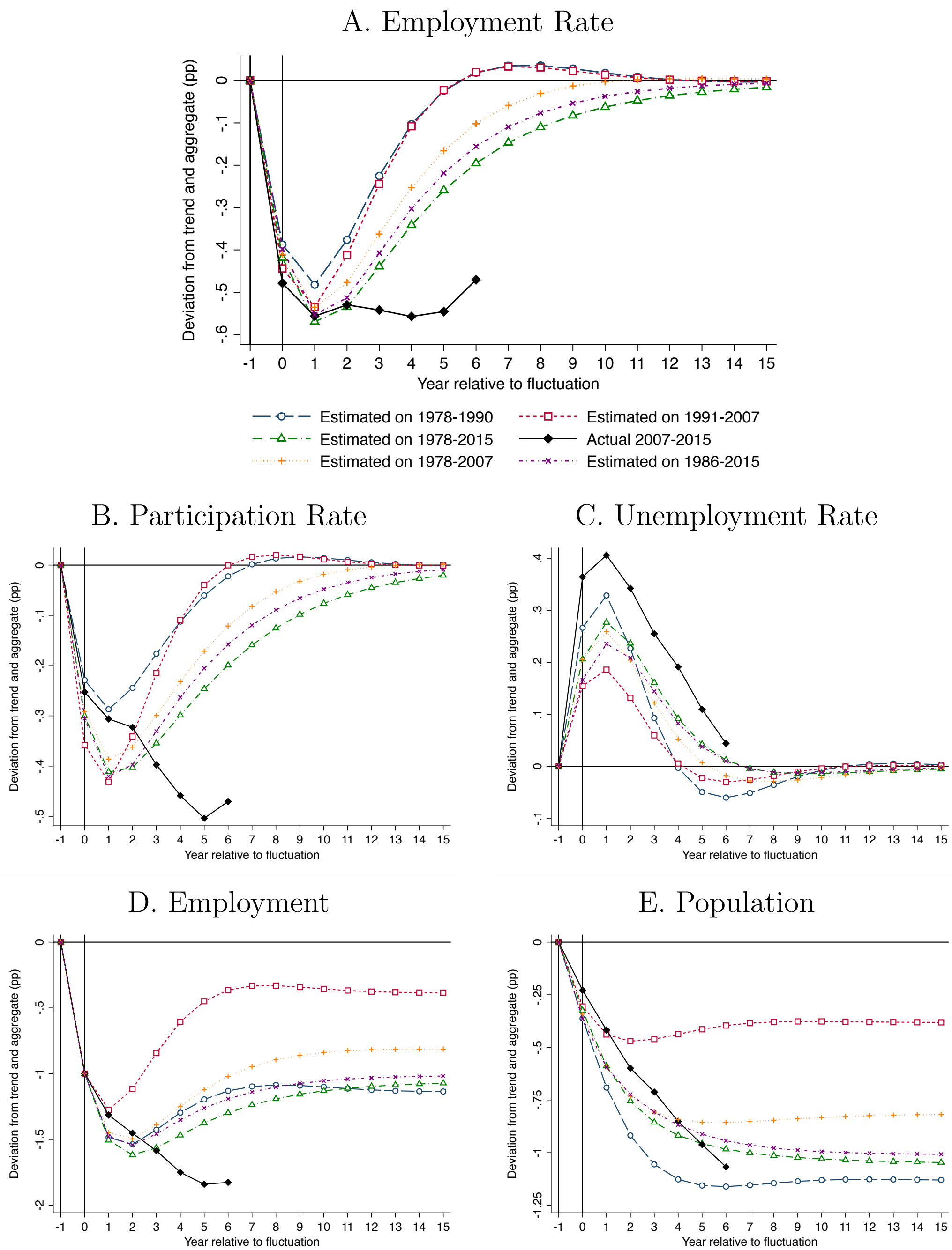
Notes: This figure plots the time path of actual and linearly extended employment differences between severe-fluctuation areas and mild-fluctuation areas 2002-2027. The first series (blue circles) reproduces the 2002-2014 time series of estimated causal effects of living in 2007 in a severe-fluctuation CZ (from Figure 5A) and extends the series forward by linearly extending the two last available data points (2013-2014). Also plotted are the analogous actual values and linear extensions (based on their last two available years, 2014-2015) for two state-level series: the unweighted 2007-2009-recession series of Figure 3D and the population-weighted 2007-2009-recession version from Online Appendix Figure A.3B, which equal the employment rate of severe-fluctuation states minus the employment rate of mild-fluctuation states. Each series's actual values are displayed using filled markers while the forward extensions are displayed using empty markers. For comparability, each series is normalized to -100% in its trough year by dividing each data point by the absolute value of its trough value; see Online Appendix Figure A.8 for non-normalized versions as well as a series corresponding to the mean actual response to a -1% state-level fluctuation plotted in Figures 3A-C. The series are plotted until they cross zero, constituting full convergence.

Figure A.1: **State Population Changes vs. 2007-2009 State-Level Fluctuations**



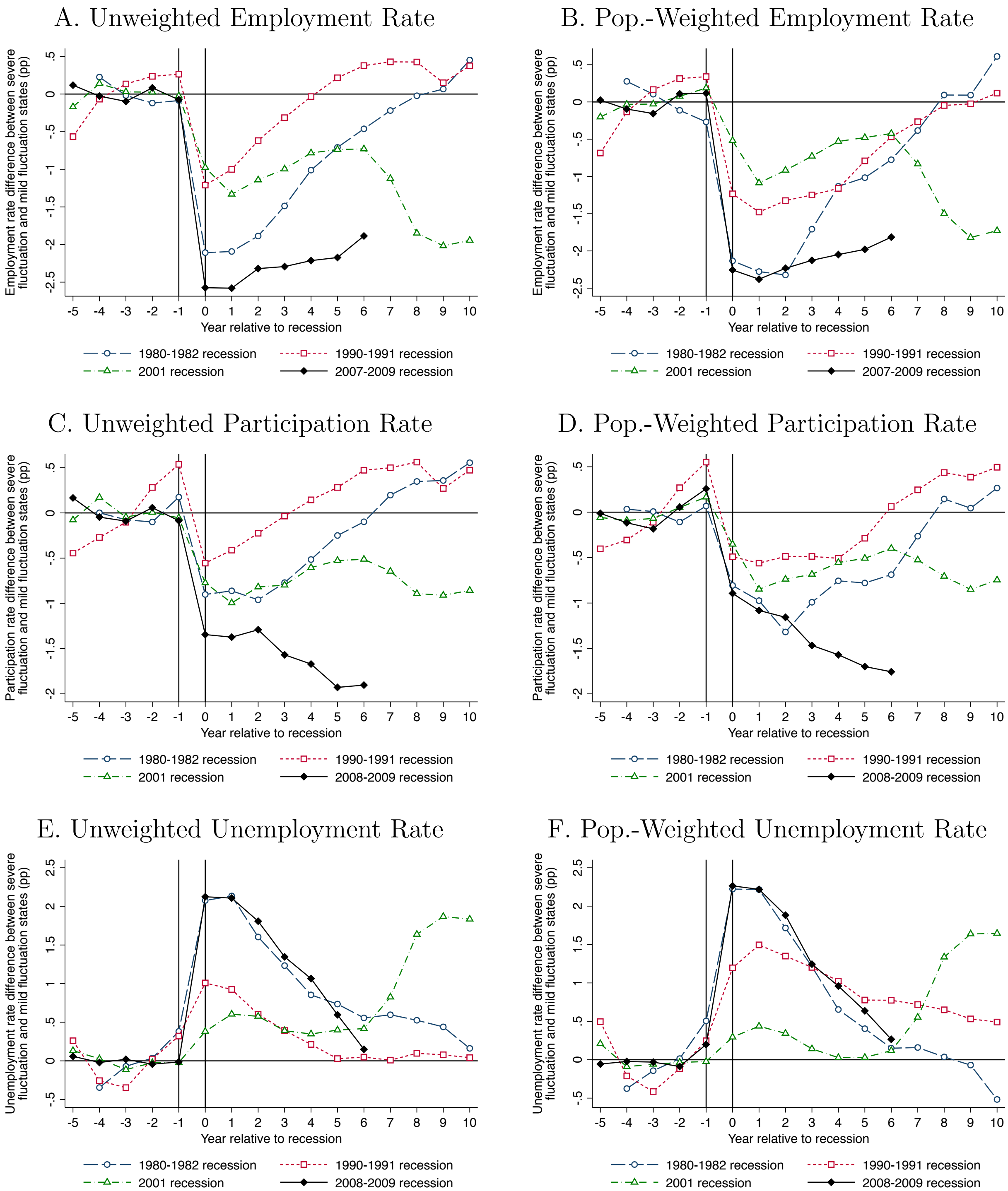
Notes: The figure plots de-trended 2007-2014 population changes—equal to each state's percent change in population 2007-2014 minus the percent change in the state's population 2000-2007—versus the state's 2007-2009 employment fluctuation (see Figure 2). Overlaid is the unweighted best-fit line with a slope of 1.014 and a standard error of 0.258.

Figure A.2: Estimated Responses to State-Level Fluctuations 1978-2015



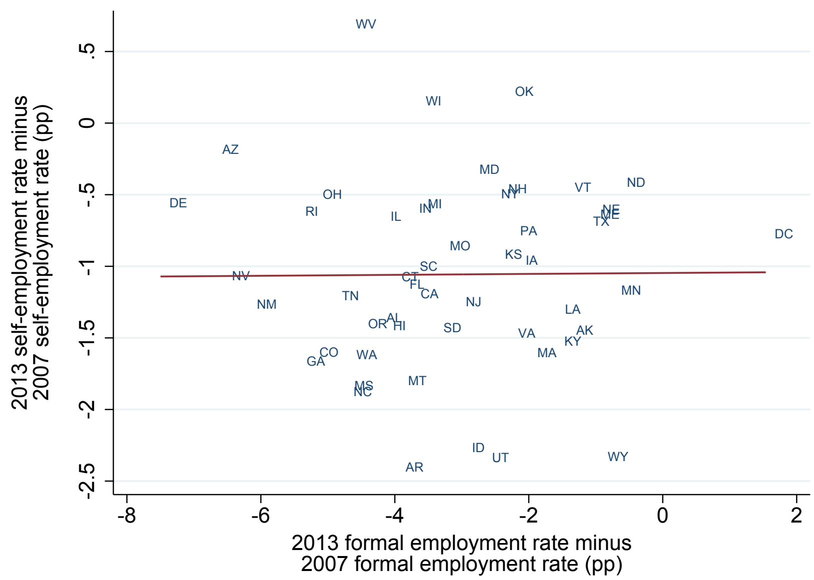
Notes: This figure plots estimated mean responses of state labor market outcomes to a −1% state-level fluctuation, over various estimation ranges. The values are also listed in Online Appendix Table 4. The 1978-1990 series are nearly identical to Blanchard-Katz's Figure 7 (note that my year zero is BK's year 1). This appendix figure approximately extends Figure 3C, though this figure's predictions are responses to a *one*-year −1% fluctuation in order to allow for exact comparability to BK, rather than responses to a −1% fluctuation spread over two years. The Actual 2007-2015 series plotted here for reference are the same as those plotted in Figure 3 which correspond to a −1% fluctuation spread over two years, introducing minor discrepancies when comparing predicted and actual series. For example, note that actual population series in panel E no longer dips below the 1978-1990-based prediction, in contrast to Figure 3A; this minor discrepancy is purely due to the mismatch between one-year and two-year impulses. Figure 3 permits direct comparability between predicted and actual.

Figure A.3: Spatial Convergence after the Last Four Recessions



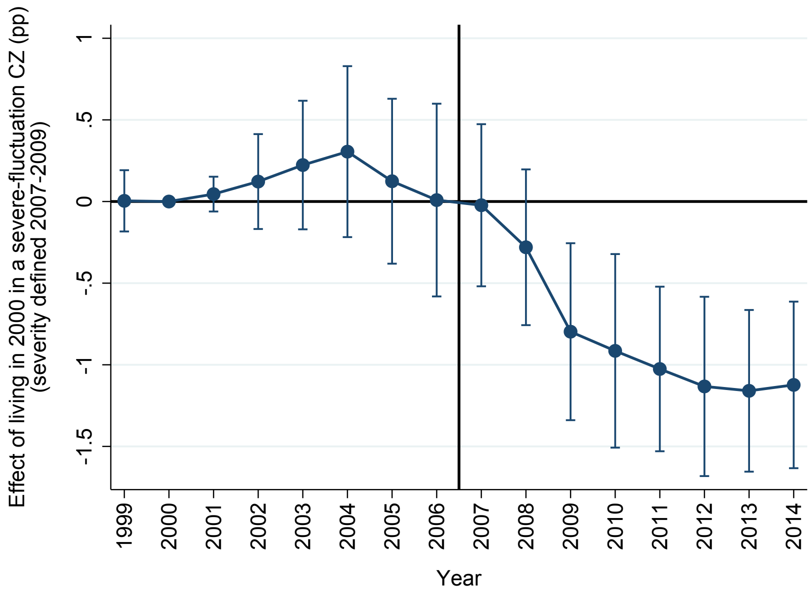
Notes: This figure extends Figure 3D to plot more outcomes and series. The left panels plot unweighted (state-weighted) outcomes among severe-fluctuation states, minus unweighted outcomes among mild-fluctuation states. The right panels use population weights among the same state groupings. Only year-2001 fluctuations are used to group states into severe-fluctuation and mild-fluctuation states for the 2001 recession; thus year -1 denotes 2000 and year-0 denotes 2001 for these series. See the notes to Figure 3D for additional details.

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.

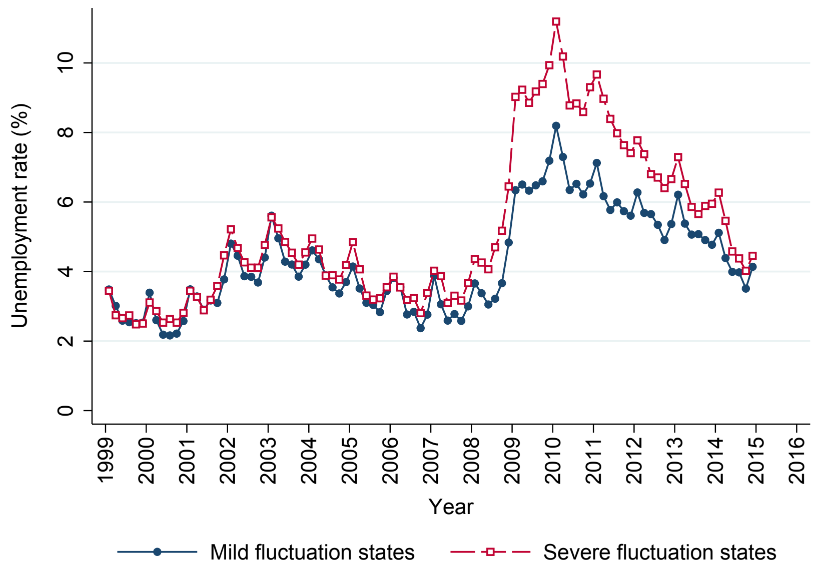
Figure A.5: Employment Effects of Living in 2001 in a Severe-2007-2009-Fluctuation CZ



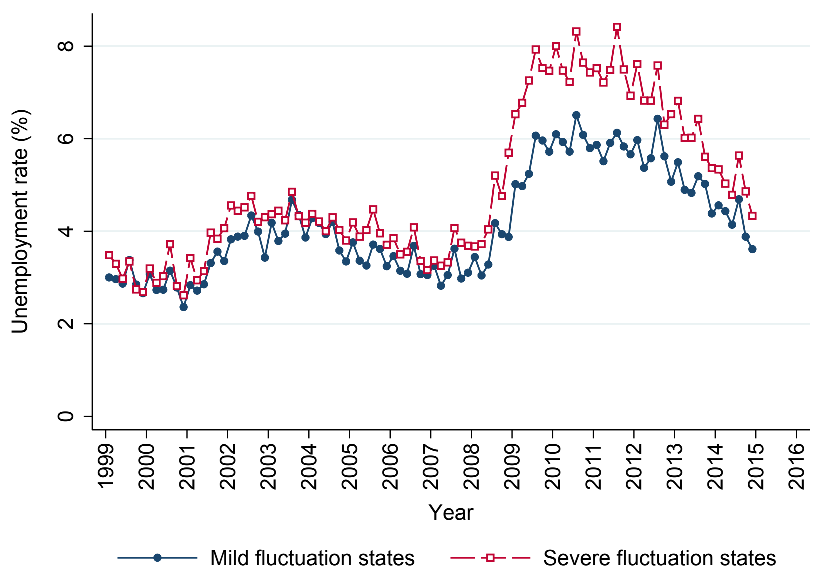
Notes: This graph replicates Figure 5A on a sample identical to the main identical sample (constructed using year-2006 retail chain workers) except that the sample is constructed using year-2000 retail chain workers. Specifically, I apply the same restrictions except that all year-based restrictions use a year subtracted by six; for example, age is defined as of December 31, 2000, for the 25-75 age restriction. This alternative year-2000-based sample comprises 1,605,539 workers at 709 firms with mean 2014 employment of 63.4%.

Figure A.6: Unemployment Rates by Fluctuation Severity and Gender

A. Males

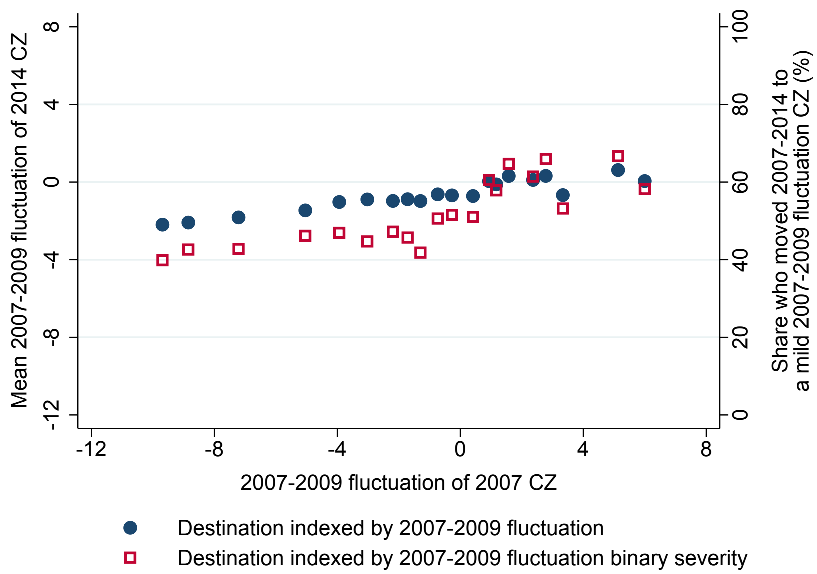


B. Females



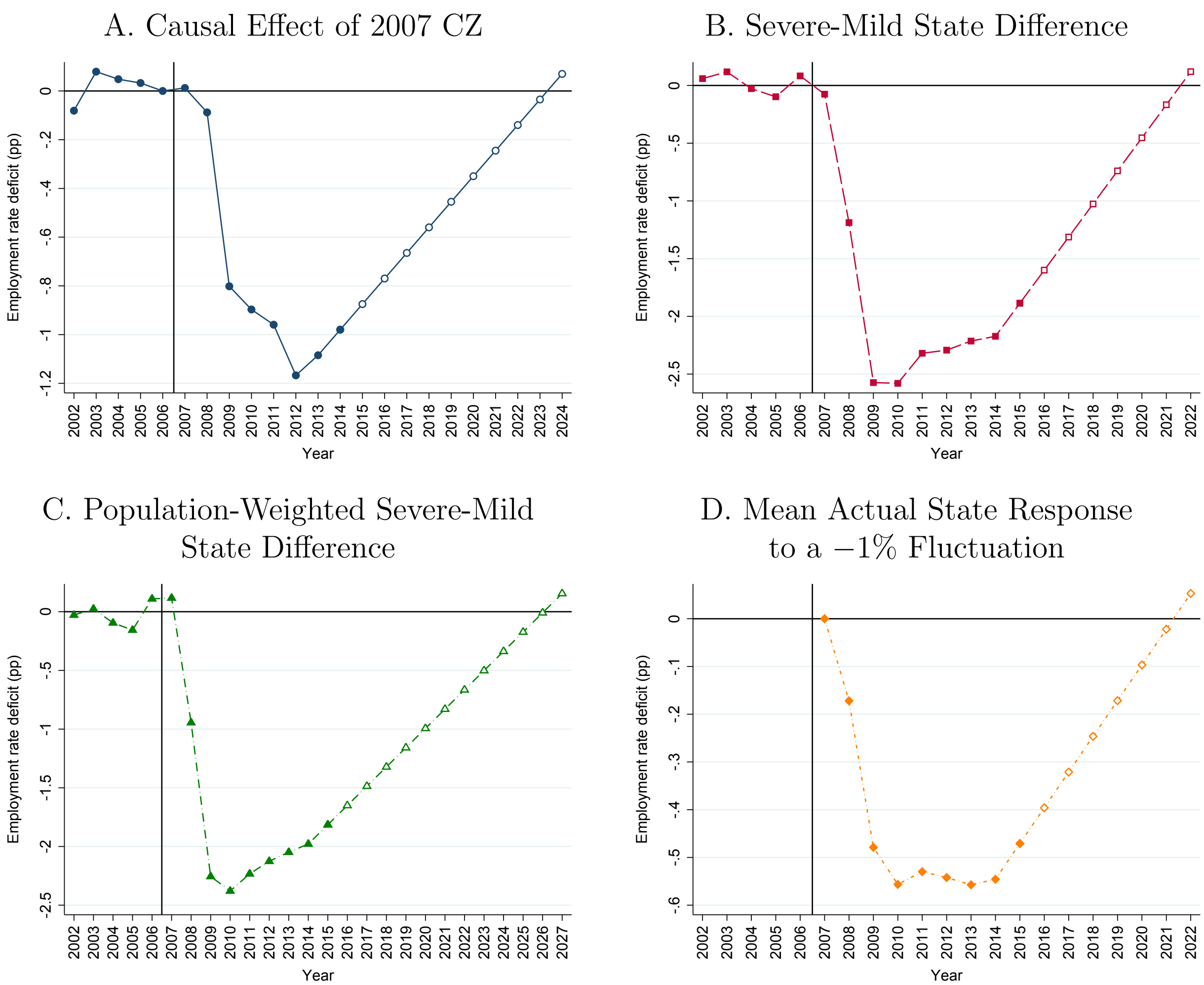
Notes: This figure uses the monthly Current Population Surveys (CPS) to plot unemployment rates 1999-2014, separately for males and females. The CPS data are restricted to the civilian non-institutional population who are 33 years or older at the time of the survey in order to parallel the minimum 2014 age of my worker-level data sample. Plotted data points are means across two-month bins.

Figure A.7: **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 fluctuation of workers' 2014 CZ (left axis) versus the 2007-2009 fluctuation 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 fluctuation, 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 a mild-fluctuation CZ as defined by 2007-2009 fluctuations (right axis).

Figure A.8: Non-normalized Convergence Paths at Current Pace



Notes: Panels A-C plot the three series of Figure 7 without normalizing each series to −100% at its trough. For additional reference, panel D plots a fourth series: actual and linearly extended mean actual response to a −1% state-level fluctuation, corresponding to the employment rate series in Figures 3A-C; panels B and D are unweighted across states and exhibit similar linearly extended convergence.