MOVING TO OPPORTUNITY? MIGRATORY INSURANCE OVER THE GREAT RECESSION*

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ABSTRACT

Over the Great Recession, the employment rate in some U.S. cities declined by more than twice the aggregate decline. To what extent did the ability to migrate insure workers against these idiosyncratic local shocks? I answer this question using geo-coded administrative panel data on the universe of U.S. males from years 2000-2011. I find that despite migration flows that were in principle large enough to provide full insurance, migration has provided only 7% insurance: the *2006* residents of the average local area have borne 93% of the area's idiosyncratic Great Recession labor demand shock. I find similarly small degrees of insurance across specifications, demographic groups, and labor market outcomes. Insurance was three times greater over the 2001 recession, driven entirely by greater insurance for above-average earners. The relative failure of migratory insurance over the Great Recession is attributable to unusually small employment gains for those migrating from heavily-shocked areas to lightly-shocked areas, rather than to a decline in migration rates. The results imply large spatial adjustment frictions in the U.S. labor market and indicate that past location may be a powerful tag for directing social insurance.

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I Introduction

Americans are unusually mobile—20% change counties every five years—and migration is the primary mechanism by which the U.S. labor market adjusts to local employment shocks (Blanchard and Katz 1992; Eichengreen 1993; Borjas 2001; Glaeser and Gottlieb 2009; Kennan and Walker 2011). The Great Recession generated extraordinarily large variation in local employment shocks. For example, America's sixth largest city (Phoenix, Arizona) lost 10% of its per-capita employment between 2006 and 2011 while America's seventh largest city (San Antonio, Texas) lost 4%, relative to a mean decline of 6%. This paper asks to what extent have the pre-existing residents of places like Phoenix escaped the incidence of their idiosyncratic local shocks by migrating to places like San Antonio. That is, to what degree has migration insured Americans against the spatial variation in the effects of the Great Recession?

Standard assumptions—often geared toward the long run—imply that the incidence of local labor demand shocks is diffused across workers nationwide, a condition that I refer to as full insurance. Spatial equilibrium models typically start by assuming that migration equalizes utility levels across space (Mills 1967; Rosen 1979; Roback 1982). Standard models in local public finance also assume frictionless mobility (Tiebout 1956; Gordon 1983). Business cycle models stress European migration frictions but usually abstract from U.S. migration frictions (Eichengreen 1993). Evidence from repeated cross sections suggests that workers are indeed fully insured in the long run: net out-migration from adversely shocked places like Phoenix restores employment rate parity across space approximately seven years after the initial shock (Blanchard and Katz 1992).

Yet over the horizon relevant for cyclical analysis and policy, local employment rates can remain very depressed, and any degree of insurance is consistent with the cross-sectional evidence. The reason is that migration is selective—some people migrate to stronger labor markets to find employment (Blanchard and Katz 1992; Kennan and Walker 2011) while others migrate to weaker labor markets to enjoy lower costs of living (Glaeser 2008; Notowidigdo 2013)—and migration rates are quantitatively large enough for pre-existing residents to have entirely escaped the incidence of idiosyncratic local shocks since the Great Recession via selective migration. This paper uses geo-coded individual-level panel data to directly estimate the incidence of local Great Recession shocks on pre-existing residents.

I begin by formalizing the indeterminacy of insurance in a spatial equilibrium model in the Rosen-Roback tradition—augmented to allow for worker heterogeneity (Kline 2010; Moretti 2011) applied to place of origin, as well as for job rationing that approximates actual employment rate declines (Rogerson 1988; Michaillat 2012). Insurance against a local labor demand shock hinges on who has low moving costs and who has access to rationed jobs. Full insurance may be provided through selective migration or through local cost-of-living changes (rent and government transfers) that equalize average real income declines across space. Yet large migration rates can also be consistent with no insurance. The actual degree of insurance is an empirical question.

I first test whether cost-of-living changes equalized real income declines across space. This test requires only repeated cross sections, so I use the 2006 and 2011 American Community Surveys. The difference between the 90th percentile change across U.S. states and the 10th percentile change ("90-10 spread") in mean wage earnings was equal to 12.4% of the 2006 mean. After including government transfers and deflating by Moretti's (2012) local consumer price index, the 90-10 spread in mean income changes remains large at 8.7% of the 2006 mean.[1] Cost-of-living changes therefore provided up to 30% insurance four years after the onset of the Great Recession, an estimate similar to that of Blanchard and Katz (1992) for the average state-level shock in the 1978-1990 period.[2] Hence, high degrees of insurance required migration.

Estimating the degree of migratory insurance requires geo-coded individual-level panel data. I draw such data from the universe of U.S. tax returns. I use 2006 ZIP code of residence from the universe of third-party-reported information returns to map 60.3 million U.S. males aged 25-59 to one of 722 continental U.S. Commuting Zones (CZ's)—geographic units designed by Tolbert and Sizer (1996) to approximate U.S. local labor markets (Autor and Dorn 2013). I then observe these individuals' 2011 outcomes, no matter where in the United States they have moved.

I define the degree of migratory insurance as the share of the average idiosyncratic local employment demand shock that was not borne by pre-existing residents. Under two identifying assumptions, I estimate the degree of migratory insurance as follows. I define a CZ's employment shock as the CZ's 2011 employment rate minus the CZ's 2006 employment rate. I then estimate the share of the average CZ's employment shock that was borne by the CZ's *2006* residents (the CZ's "cohort") rather than by the cohorts of other CZ's, in the form of reduced employment. Migratory insurance equals 100% minus this estimated incidence rate: 0% incidence implies that selective migration provided full insurance, while 100% incidence implies that selective migration provided no insurance. The average 2006-2011 inter-CZ migration rate (13%) was five times higher than the minimum rate (2.4%) necessary for 100% migratory insurance across the bulk of CZ's.

[1] The 90-10 spread is larger at lower levels of geographic aggregation.

[2] Cost-of-living changes provided less insurance to the extent that adverse local shocks reduced local amenities, such as through lower funding of public goods (Suarez Serrato and Wingender 2011). Population-dependent amenities likely changed little because absolute population changes were orthogonal to local shocks, even though de-trended changes were highly correlated as in Blanchard and Katz (1992); see Appendix Figure 1.

I find that the average CZ-cohort bore 93% of the incidence of its CZ's idiosyncratic employment shock: for every percentage point that the employment rate declined in a given CZ relative to the mean decline, the CZ-cohort's employment rate declined by .93 percentage points. This implies 7% migratory insurance: selective migration diffused 7% of the incidence of local employment shocks across workers nationwide. I find similarly small degrees of migratory insurance when defining shocks in terms of continuous wage earnings and across subgroups defined by age, marital status, number of children, and homeownership.

Consistent estimation of the degree of migratory insurance requires that local employment rate declines were caused by place-specific labor demand shocks—which selective migration can in principle diffuse across workers nationwide—and that these employment declines equal the counterfactual declines that would have prevailed under no migration. I evaluate three potential sources of bias.

First, local employment rate declines may have been caused by nationwide-skill-specific labor demand shocks. For example, perhaps the Phoenix cohort disproportionately comprises manual laborers who would have become non-employed anywhere. To evaluate this threat, I first hold constant rich measures of skill—the finest of which are employer fixed effects interacted with 2006 wage earnings. These specifications estimate the degree of migratory insurance off of a subset of very similar workers: those who in 2006 were paid the same amount by the same employer (e.g. Walmart), just in different CZ's (e.g. Phoenix vs. San Antonio).[3] In a separate strategy, I instrument for individuals' CZ employment shocks using Great Recession employment shocks to the individual's state of birth, thereby accounting for pre-recession sorting on skill across space. Neither strategy alters the 7% migratory insurance estimate.

Second, local employment rate declines may have been caused by labor supply contractions induced by unemployment insurance (UI) duration extensions. I exploit within-state variation in CZ employment shocks and cross-state variation in UI extensions to flexibly control for UI-induced contractions in labor supply. The migratory insurance estimate remains unchanged.[4]

Finally, endogenous migration may have compressed the variance of local employment rate declines relative to the no-migration counterfactual, biasing the migratory insurance estimate downward. To address this, I instrument for CZ employment shocks using two instruments used in earlier work that do not rely on local post-shock outcomes: 2006-2011 CZ employment changes predicted by nationwide changes in each CZ's 2006 industries as in Bartik (1991), and 2006 CZ household leverage as in Mian

[3] Unmasked firm identifiers and names were not accessed for any part of this research.

[4] Note that this analysis of *local variation* in employment declines does not directly bear on sources of the aggregate decline (cf. Ohanian 2010; Hall 2011; Bloom, Floetotto, Jaimovich, Saporta-Eksten, and Terry 2012; Lazear and Spletzer 2012; Mulligan 2012; Baker, Bloom, and Davis 2013; Mian, Rao and Sufi 2013; Mian and Sufi 2013).

and Sufi (2013). I continue to obtain similar estimates. I conclude that migration has provided little insurance against Great Recession local employment shocks that in principle could have been diffused across workers nationwide.

Why has migration diffused so little incidence? I conduct two tests to investigate the relative importance of three potential frictions: moving costs, weak information on the strength of far-away labor markets, and weak access to employment upon moving to a stronger labor market. First, individuals are likely better informed about and able to move cheaply to nearby areas. I therefore test for evidence of the joint importance of moving costs and weak information by estimating whether migratory insurance was significantly greater among cohorts whose CZ's experienced shocks substantially different from those experienced by nearby CZ's. I do not find such evidence.

Second, I report that migratory insurance was three times greater over the 2001 recession than over the Great Recession, driven entirely by greater selective migration among those with above-average pre-recession wage earnings. Each of the three candidate frictions could *a priori* have been responsible for the decline in migratory insurance: moving costs may have been higher over the Great Recession (e.g. due to underwater mortgages as in Ferreira, Gyourko, and Tracy 2010), information may have been weaker (e.g. few places were booming), and in-migrants to stronger labor markets may have had weaker employment access (e.g. because incumbents had priority for rationed jobs).

To test among these possibilities, I decompose migratory insurance into the product of three estimable components: (a) the migration rate, (b) the degree to which cross-CZ migration was "directed" from heavily-depressed CZ's to less-depressed CZ's rather than to other heavily-depressed CZ's, and (c) the cross-sectional benefit to moving to a less-depressed CZ. Higher moving costs should have reduced migration rates. Weaker information should have reduced directedness. And weaker employment access for in-migrants should have reduced the cross-sectional benefit to moving.

I find that neither migration rates nor directedness was lower among above-average earners over the Great Recession relative to the 2001 recession and to below-average earners. Instead, the cross-sectional benefit to moving was uniquely and substantially lower among above-average earners over the Great Recession. This suggests that migratory insurance was lower over the Great Recession because of weaker employment access among migrants from heavily-shocked areas—rather than due to higher moving costs or to worse information about where to move.

The results have implications for literatures in four fields. In public economics, a long tradition in optimal taxation emphasizes the benefits of conditioning taxes and transfers on fixed characteristics ("tags") such as age that strongly predict labor income (Akerlof 1978; Weinzierl 2011). Social insurance models have rarely featured tags, but this paper's results suggest that past location may be a powerful tag for directing social insurance. For example, the federal government distributed special tax credits after Hurricane Katrina to those who were living in the storm's path on the day it struck (Gregory 2013). The government could similarly have distributed transfers in recent years to those who were living in places like Phoenix when the Great Recession struck. Note that transfers based on *past* location echo but are fundamentally different from transfers based on *current* location (e.g. state-based UI extensions and intra-city Empowerment Zones). The reason is that current location is not a fixed characteristic, so transfers based on current location may simply increase local rent levels rather than increase recipients' real incomes (cf. Glaeser 2008; Busso, Gregory, and Kline 2013).

For labor economics, the results combine with existing empirical work to suggest the following path for workers' insurance against Great Recession local shocks. Workers will likely be insured in the long run: net out-migration from adversely shocked areas will gradually restore employment rate parity across space (Blanchard and Katz 1992), with lower wages in adversely shocked locales and compensatingly lower costs of living (Notowidigdo 2013). But along that transition path and in spite of high gross migration rates, locally depressed employment rates may be borne almost entirely by the locale's original residents, with lower costs of living mitigating only a minority of the associated nominal income declines.

On migration specifically, a large recent literature has focused on whether migration rates have slowed over the Great Recession (Frey 2009; Kaplan and Schulhofer-Wohl 2010; Molloy, Smith, and Wozniak 2011) and whether underwater mortgages are the cause (Ferreira, Gyourko, and Tracy 2010; Aaronson and Davis 2011; Modestino and Dennett 2011; Schulhofer-Wohl 2011; Farber 2012; Valetta 2013). This paper's results suggest a focus for future work on why post-migration reemployment rates were not higher among migrants from heavily-shocked areas, such as a relative decline in local vacancies or matching efficiency (Sahin, Song, Topa, and Violante 2012).

Looking more broadly in labor economics, a series of influential papers has demonstrated lasting effects of concentrated labor demand shocks in other contexts: firm-level mass layoffs (Jacobson, LaLonde, and Sullivan 1993; Wachter, Song, and Manchester 2009), entering labor markets during a recession (Beaudry and DiNardo 1991; Baker, Gibbs, and Holmstrom 1994; Kahn 2010), and trade competition (Autor, Dorn, Hanson, and Song 2013). This paper constitutes a spatial analogue of these earlier papers by showing that Great Recession local shocks substantially and lastingly affected specific people, not just places, conditional on fine proxies for general skill.

A related literature has estimated the degree to which and via which mechanisms workers are insured against various types of labor income shocks (Hall and Mishkin 1982; Auerbach and Feenberg 2000; Blundell, Pistaferri, and Preston 2008; Blundell, Pistaferri, and Saporta-Eksten 2012; Kaplan

2012). This paper shows that in spite of large U.S. migration flows that are to some extent related to local labor demand conditions and costs of living (Eichengreen 1993; Kennan and Walker 2011; Notwidigdo 2013), the option to migrate has enabled pre-existing residents to escape only a limited share of the incidence of Great Recession local shocks over the medium run.

In urban economics, a standard assumption has been that migration equalizes real incomes across space (Mills 1967; Rosen 1979; Roback 1982). While this assumption has proven powerful in explaining long run trends and steady states, this paper suggests that it poorly characterizes the spatial equilibrium that has prevailed since the Great Recession. The results underscore the quantitative importance of urban economics work emphasizing adjustment frictions and dynamics (Topel 1986; Bound and Holzer 2000; Glaeser 2008; Kline 2010; Moretti 2011).

Finally, the dominant tradition in macroeconomic modeling of the U.S. business cycle is to consider a single spatially unified labor market. Yet this paper's results imply large spatial adjustment frictions (Lucas and Prescott 1974) and raise the possibility that the U.S. business cycle may be usefully modeled more like the Eurozone's: a collection of regional economies subject to idiosyncratic shocks under a common currency (Mundell 1961; Asdrubali, Sorensen, and Yosha 1990; Benigno and Benigno 2003; Adao, Correia, and Teles 2009; Farhi and Werning 2013; Farhi, Gopinath, and Itskhoki forthcoming). This modeling choice can fundamentally change policy considerations. For example, the Federal Reserve cannot lower the interest rate in Phoenix relative to San Antonio, but the federal government can concentrate stimulus spending in Phoenix.

The remainder of this paper is organized as follows. Section II uses a spatial equilibrium model to demonstrate the range of possible insurance and to motivate the paper's empirical strategy. Section III tests whether rent changes and government transfers equalized real income declines across space. Section IV introduces the geo-coded administrative panel data that makes possible an analysis of migratory insurance. Section V specifies the estimating equation for the degree of migratory insurance. Section VI presents estimates. Section VII evaluates robustness. Section VIII investigates mechanisms. Section IX concludes.

II Theoretical Indeterminacy and Empirical Strategy

I use a simple model to demonstrate the range of insurance that spatial equilibrium can provide workers against local labor demand shocks and to motivate the empirical research design to follow. Kline (2010) presents a tractable extension of the classic static framework of Rosen (1979) and Roback (1982) that incorporates agent heterogeneity, similar to Moretti (2011). I apply Kline's setup to heterogeneity in city of origin with positive moving costs, extend it to allow for local technology shocks and price rigidities that yield rationed jobs (Rogerson 1988; Michaillat 2012), and consider different sets of primitives that may intuitively correspond to different time horizons.

Both heterogeneity in city of origin and heterogeneity in employment outcomes are substantively important because homogeneity in either dimension generates full insurance by construction. Agent heterogeneity in city of origin allows for the incidence of local labor demand shocks to vary by city of origin. And rationed jobs—allocated by lottery under particular assumptions—allow expected income to vary across agents within cities and across cities and thus allow for the possibility that rent levels do not automatically adjust to equalize expected real incomes across space. Job rationing carries the added benefit of enabling the model to match this paper's motivating empirical fact: dramatic local variation in employment rate declines over the Great Recession. Finally, different time horizons clarify the relationship between the present model and the Rosen-Roback benchmark. I stress that I investigate insurance against heterogeneity in idiosyncratic local labor demand shocks, not insurance against heterogeneity in idiosyncratic individual-level employment lottery outcomes. The notation and setup exposition closely follow Kline.

The conclusion of this section can be stated simply. Two forces of spatial equilibrium—cost-of-living changes and selective migration—can provide full insurance against idiosyncratic local labor demand shocks. Yet depending on who has low moving costs and who has access to rationed jobs, spatial equilibrium can provide arbitrary levels of insurance. When cost-of-living changes do not equalize real incomes across space, panel data are necessary to estimate the degree of insurance against idiosyncratic local labor demand shocks.

II.A Setup

Assume two cities $c \in \{P, S\}$ ("Phoenix" and "San Antonio") and a continuum of agents of measure one. Each agent inelastically demands a single unit of housing that he rents at a city-specific market rate. Agents who are employed must live in the city in which they are employed. I consider a pre-shock equilibrium and a post-shock equilibrium. Let $t = 0$ denote the pre-shock period, and $t = 1$ denote the post-shock period. Let $C(i, t)$ denote the city in which an agent i lives in time t. I refer to $c(i, 0)$ as the agent's origin city and $c(i, 1)$ as the agent's destination city. I refer to the set of agents originating in a city c as the c-cohort.

Agents have quasi-linear utility over consumption (wages and government transfers minus rent and moving costs) and the local amenities in their destination city.[5] Write utility as:

$$\begin{aligned} u_i &= \tilde{w}_{ic(i,1)} + G_{c(i,1)} + A_{c(i,1)} - r_{c(i,1)} - m_i I[c\,(i,0) \neq c\,(i,1)] \\ &\equiv v_{c(i,1)} + \tilde{w}_{ic(i,1)} - m_i I[c\,(i,0) \neq c\,(i,1)] \end{aligned}$$

where $\tilde{w}_{ic(i,1)}$ is the possibly stochastic wage income (referred to as "wages" or "nominal wages") the agent earns in his destination city, $G_{c(i,1)}$ is a place-specific government transfer (Suarez Serrato and Wingender 2011; Notowidigdo 2013), $A_{c(i,1)}$ is the dollar value of local amenities in the agent's destination city, $r_{c(i,1)}$ is the local rent level in the agent's destination city, and m_i is the moving cost that the agent pays if and only if his destination city is different from his origin city. Moving costs here comprise both monetary costs to moving (e.g. the cost of a moving van) and non-monetary costs (e.g. dollar-denominated costs of effort expended in moving, or the cost of no longer living near friends or family). Stochastic wages allow for the possibility of employment lotteries, specified below. The term $v_{c(i,1)}$ denotes the systematic indirect utility of residing in city $c\,(i,1)$ and equals $G_{c(i,1)}+A_{c(i,1)}-r_{c(i,1)}$.

Define agent i's idiosyncratic willingness to pay to reside in city S as:

$$\xi_{ic(i,0)} \equiv \begin{cases} E\left[\tilde{w}_{iS}\right] - E\left[\tilde{w}_{iP}\right] - m_i & \text{if} \quad c\,(i,0) = P \\ E\left[\tilde{w}_{iS}\right] - E\left[\tilde{w}_{iP}\right] + m_i & \text{if} \quad c\,(i,0) = S \end{cases}$$

where the expectations are taken before the agent chooses his destination city. Agents will choose to move if and only if moving delivers higher expected utility. Thus agents choosing city P as their destination will have $\xi_{ic(i,0)} < v_P - v_S$. Denote the cumulative distribution of $\xi_{ic(i,0)}$ by $F(.)$. The measure of agents who choose city P as their destination city equals $F(\xi_{ic(i,0)})$, equivalently $F[(A_P + G_P - r_P) - (A_S + G_S - r_S)]$.

Goods are produced in each city by Cobb-Douglas production function $Y_c = B_c L_c^{\alpha} K_c^{1-\alpha}$ with $\alpha \in (0,1)$, where B_c is the city's productivity level, L_c is the measure of employed workers in the city, and K_c is the stock of capital in the city. Depending on the time horizon, each city's capital stock is either fixed or supplied at a constant marginal cost κ. I assume that producers make zero profits and that output is sold on an international market at a fixed price equal to one. Denote the housing stock (the number of housing units rented out) in city c by H_c. Depending on the time horizon, each city's number of housing units is either fixed or is supplied at strictly-increasing marginal cost $\psi\,(H_c)$. Define an equilibrium as a pair of rental prices that clears the housing market, which can be represented as

[5] With quasi-linear utility, agents are risk-neutral and insurance has no ex ante value. I assume quasi-linear utility for presentational simplicity. All qualitative results hold when indirect utility is quasi-linear but utility is not.

the relative supply of housing in city P equaling the relative demand for residing in city P:

$$H_P - H_S = 2F^{-1}\left[(A_P + G_P - r_P) - (A_S + G_S - r_S)\right] - 1$$

where F^{-1} denotes the inverse CDF of $\xi_{ic(i,0)}$.

I consider the long-run, short-run, and medium-run incidence of city-specific labor demand shocks on city cohorts. I emphasize that the model is static, so time horizons refer simply to different sets of potentially appropriate primitives. For simplicity, I assume that before the shocks, the economy is in an equilibrium in which both cities are identical with production technology equal to 1, equal government transfers, equal amenities, equal populations $H_P = H_S = 0.5$, full employment $L_P = L_S = 0.5$ at equal non-stochastic wages $\bar{w}$, equal capital stocks $\bar{K}$, and equal rent. This pre-shock equilibrium can be thought of as a long-run equilibrium in which all stocks and prices are flexible and each city's residents have moving costs that are distributed such that $F(0) = 0.5$.

I consider shocks to production technology, though shocks to product demand have identical implications. Specifically, each city experiences an exogenous permanent technology shock $\lambda_c \in (0, 1)$ such that the city's productivity level becomes λ_c, with city P experiencing a worse shock than city S: $\lambda_P < \lambda_S$. I assume that shocks do not affect amenity levels; I discuss the implications of shock-dependent amenities at the end of this section.

Define the idiosyncratic local labor demand shock to city c as c's deviation from the mean employment change across cities between the pre-shock equilibrium and the post-shock equilibrium: $(L_c - L_{c'})/2$, where $c \neq c'$. Idiosyncratic local labor demand shocks may yield idiosyncratic changes in mean utility across cities. Define "no insurance against idiosyncratic local labor demand shocks" as a post-shock equilibrium in which each city-cohort experiences a mean utility change equal to its city's mean utility change. Conversely, define "full insurance against idiosyncratic local labor demand shocks" as a post-shock equilibrium in which both city-cohorts experience the same mean utility change. I now evaluate the degree of insurance against idiosyncratic local labor demand shocks over different horizons.

II.B Full Insurance in the Long Run

As a benchmark, I first consider a set of primitives that reduces this model to a version of the Rosen (1979)-Roback (1982) canon. Define the long run as a time horizon in which stocks and prices are flexible and in which mobility costs are continuously distributed over a trivially small interval: $m_i \sim [0, \bar{m}]$ for some arbitrarily small $\bar{m} > 0$. This corresponds to models in the classic Rosen-Roback tradition because there are arbitrarily small frictions to sorting across space and no frictions to price changes. Trivial moving costs over the long run can be understood as amortization of one-time moving costs over a long horizon.[6]

Equilibrium involves full employment in each city at local wages $w_c = \lambda_c^{1/\alpha}\kappa^{-(1-\alpha)/\alpha}$. Local wages are invariant to local population (labor demand is infinitely elastic) because production technology exhibits constant returns to scale and because capital supply is infinitely elastic. Lower technology λ_c in city P than in city S implies that residents of city P earn lower wages than residents of city S. In equilibrium, the difference in local wages plus any difference in government transfers is exactly offset by a difference in rents such that the housing market clears:

$$\psi\left(H_P\right) - \psi\left(1 - H_P\right) = \lambda_P^{\frac{1}{\alpha}}\kappa^{-\frac{1-\alpha}{\alpha}} - \lambda_S^{\frac{1}{\alpha}}\kappa^{-\frac{1-\alpha}{\alpha}} + G_P - G_S - F^{-1}\left(H_P\right)$$

where rent $r_P = \psi\left(H_P\right)$ and $r_S = \psi\left(H_S\right) = \psi\left(1 - H_P\right)$.

Relative to the pre-shock equilibrium, city P has lower population, lower employment, and lower rent, while city S has higher population, higher employment, and higher rent. Unless government transfers are large, real wages—defined as nominal wages plus government transfers minus rent—are lower for all agents than in the pre-shock equilibrium but are equal across the two cities and across all agents as in Roback. Rent changes, government transfers, and migration thus combine to diffuse the incidence of idiosyncratic local labor demand shocks evenly across agents, regardless of city of origin. Hence, agents are insured in the long run against idiosyncratic local labor demand shocks.

II.C No Insurance in the Short Run

In contrast to the long-run extreme of full flexibility of stocks and prices and trivial moving costs across all agents, consider an alternative "short run" extreme in which stocks and prices are fixed at pre-shock levels and mobility costs are infinite for all agents. This economy is in spatial disequilibrium because no one can move. With migration and rent changes shut down, agents obviously have no insurance against idiosyncratic local labor demand shocks.

It is instructive for the next subsection to detail the nature of the idiosyncratic local labor demand shock. With fixed capital, derived labor demand slopes downward: $w_c = \lambda_c \alpha \bar{K} L_c^{\alpha-1}$. Thus with fixed local wages $\bar{w}$, employment and employment rates decline by the factor $(1-\lambda_c)^{1/(\alpha-1)}$ and employment must be rationed, with fraction $\lambda_c^{1/(\alpha-1)}$ remaining employed. Employment, employment rates, mean wages, and mean real wages fall in city P relative to city S, and each city's residents bear the full

[6] In reality, moving costs can be large even when amortized over a long horizon if moving costs comprise ongoing costs (e.g. living far from friends and family) rather than one-time costs (e.g. the cost of a moving van and the effort to physically move). In such a departure from the Rosen-Roback benchmark (Kline 2010; Moretti 2011; Busso, Gregory, and Kline 2013), there can be incomplete insurance.

incidence of these idiosyncratic local labor demand shocks. Thus agents enjoy no insurance in the short run.

II.D Indeterminate Insurance in the Medium Run

Define the medium run as a time horizon in which stocks and wages are fixed, rent is flexible, and moving costs are zero for some share of agents.[7] The medium run is meant to refer to a multiple-year horizon—such as year 2011 after the Great Recession—in which city populations have changed little, local employment rates remain depressed in places like Phoenix relative to places like San Antonio, and most agents have not moved. I now consider three possible medium-run equilibria, each corresponding to a different distribution of mobility costs and employment lottery priorities.

II.D.1 Full Insurance Via Cost-of-Living Changes

As a first set of primitives that may apply to the medium run, assume that a non-zero share of original residents in each city have zero moving costs, all other agents have positive moving costs, agents enter the employment lottery only of their destination city, and all employment lottery entrants have an equal probability of obtaining employment. The set of perfectly mobile agents can be thought of as workers who value variety as much as familiarity or who otherwise have little attachment to their origin city such as recent immigrants or those without children. The employment lottery assumptions can be thought of as reflecting homogenous workers who must be physically present in the local labor market before being considered for a job because of physical search requirements (though search is not directly modeled here).

For the housing market to clear in this setup, the cross-city difference in rents must equal the cross-city difference in expected wages and government transfers so that the agents with zero moving costs enjoy equal expected utility in each city before the lottery:

$$\begin{aligned} r_P - r_S &= E\left[\tilde{w}_{iP}\right] - E\left[\tilde{w}_{iS}\right] + G_P - G_S \\ &= \bar{w}\left(\lambda_P^{\frac{1}{\alpha-1}} - \lambda_S^{\frac{1}{\alpha-1}}\right) + G_P - G_S \end{aligned}$$

In words, the housing market clears with any pair of rents (r_P, r_S) in which the difference $r_P - r_S$ equals the cross-city difference in mean wages and government transfers. This implies that expected real wages are equal across cohorts as well as across destination cities, even though realized employment

[7] Appendix Figure 1a shows that in the American Community Survey, state-level population changes over years 2006-2011 among working-age males averaged zero (reflecting little net migration into or out of the United States) and were orthogonal to state-level 2006-2011 employment declines. This latter fact is unsurprising because housing is durable and because new housing can be unprofitable when house prices fall (Glaeser and Gyourko 2005; Notowidigdo 2013).

and mean nominal wages are lower in P than in S.

Thus frictionless mobility and the ability to move to and compete in out-of-city employment lotteries of an arbitrarily small share of agents diffuses the incidence of idiosyncratic local labor demand shocks evenly across all agents, regardless of city of origin. The key to full insurance via rent changes is that those on the margin between moving and not moving experience the same utility in each location as inframarginal agents, so the rental difference that clears the housing market also equalizes expected utility for all agents.

II.D.2 Full Insurance Via Selective Migration

As a second set of primitives that may apply to the medium run, assume that all agents have zero moving costs and have access to both cities' employment lotteries without having to move first and that government transfers are not city-specific. Specifically, all agents apply to both cities' employment lotteries, receive employment offers, and then decide which to accept and whether to move. The mobility assumption can be thought of as approximating the amortization of positive moving costs over a long horizon. The employment lottery assumption can be thought of as agents applying for out-of-origin-city jobs online or during an in-person visit to the other city. The government transfers assumption reflects the federal nature of many transfers (e.g. disability insurance) and is present only to isolate the migratory insurance channel in this exposition.

For many agents in this setup, realized nominal income is independent of the destination city choice because, at the time of destination city choice, they have either zero employment offers or an employment offer in each city. Thus for the housing market to clear, rent must be equal in each city: $r_P - r_S = 0$. Equilibrium involves members of the P-cohort who won only the S employment lottery moving to city S in order to accept employment there, and members of the S-cohort who lost both employment lotteries moving to city P. This migration is selective in the sense that P-to-S movers are employed at a strictly higher rate than S-to-P movers.

Unlike in the previous equilibrium, mean real wages—not just employment and mean nominal wages—are lower in city P than in city S. But employment, nominal wages, and real wages are orthogonal to city of origin, so the incidence of idiosyncratic local labor demand shocks is diffused across cohorts. The key to full insurance via selective migration is that agents can frictionlessly compete for and accept out-of-city employment offers, so the P-cohort can on average arbitrage spatial differences in real wages.

II.D.3 No Insurance

As a final set of primitives that may apply to the medium run, assume that government transfers are not city-specific and a small positive share of original residents in each city have zero moving costs and can find employment in either city with certainty, while all other agents either have infinite moving costs or cannot compete in out-of-city employment lotteries. The set of agents who can find employment in either city with certainty can be thought either as representing a fraction of agents dying and being replaced by college graduates or foreign immigrants who face the same cost to settling in either city and who secure employment before moving, or as representing a fraction of agents working at multi-city firms that were not affected by technology shocks, retain all existing employees to avoid hiring costs, and allow employees to change offices.

As in the previous equilibrium, equilibrium here requires rent to be equal in each city ($r_P - r_S = 0$), and employment, mean nominal wages, and mean real wages are lower in P than in S. But unlike the previous equilibrium, the only agents who could arbitrage spatial differences in real wages are those who enjoy equal nominal wages across space—i.e. the people who are unaffected by idiosyncratic local labor demand shocks. Thus on average, each cohort bears the full incidence of its original city's idiosyncratic labor demand shock. Rent changes provide no insurance in this equilibrium because the marginal agents receive the same nominal wages in each city while the inframarginal agents do not. Migration provides no insurance in this equilibrium because the original residents who stand to be affected by idiosyncratic local labor demand shocks cannot compete for employment in the other city.

II.E Empirical Implications

The preceding exposition used extreme examples to illustrate that over the medium run, insurance against local labor demand shocks is theoretically indeterminate and hinges on the unobservable joint distribution of moving costs and priority for rationed jobs. Testing for medium-run insurance against Great Recession local shocks is therefore an empirical exercise. My approach is guided by the theoretical cases considered above.

In the first leading case of full insurance discussed above, cost-of-living changes (rent changes and government transfers) equalize mean real wages across cities and across city-cohorts. In the second leading case, cost-of-living changes do not equalize mean real wages across space, but selective migration across cities equalizes mean real wages across city-cohorts. Measuring changes in mean real incomes across space requires only publicly-available repeated cross sections, so I begin by testing for equal changes in real incomes across space between 2006 and 2011. If I find that real income changes are unequal across space, I turn to panel data to directly estimate the relationship between the outcomes of cities and the outcomes of cohorts between 2006 and 2011. Under no insurance, city-cohorts bear the full incidence of idiosyncratic shocks to their original cities. Under intermediate insurance, they bear only part of the incidence. Under full insurance, they bear none of the incidence.

The strength of panel data is that panel-based incidence estimates do not require inference from equilibrium relationships; the econometrician can estimate incidence directly. Yet no large dataset permits direct measurement of moving costs incurred in reaching the 2011 equilibrium or changes in the utility value of local amenities in response to local shocks (Diamond 2012). In particular, amenity values may be decreasing in the severity of local shocks, such as by reducing funding for local public goods.[8] Because utility changes are not measured perfectly, I present estimates of the incidence of Great Recession local shocks on the well-defined measures of employment, wage income, and real income.

III Real Income Changes Across Space

The preceding section's theoretical exploration illustrated that the forces of spatial equilibrium—changes in the local cost of living and selective migration—can insure people against local labor demand shocks over the medium run. In this section, I use publicly available data from the American Community Survey (ACS) to test for insurance via the former channel: did changes in local rent levels and government transfers between 2006 and 2011 equalize mean real income changes across space? If they did, that may suggest that spatial equilibrium insured workers against local labor market shocks, as in the specific case considered in Section II.D.1. I begin by analyzing mean employment and income declines across states. I then compute locally deflated income declines using the more aggressive of the two local price deflators in Moretti (2012).

Every year, the U.S. Census Bureau surveys 1% of the U.S. population for the ACS. Each randomly selected household is required by law to complete the survey, which collects information on all members of the household. Individuals, families, and households in the ACS are not linked over time. Consistent with the sample frame applied in the rest of the paper, I confine the samples to male U.S. citizens in the continental United States who were between 25 and 59 years old in 2006 (30 and 64 in 2011). Aggregating individuals in each year to their local areas of residence, I compute 2006-2011 changes in area-level employment and income outcomes and report statistics on the variance of the distribution of these changes.

[8] It is also traditional to assume that amenities are decreasing in city population. However as I show below, population changes were largely orthogonal to Great Recession local shocks.

Figure 1a uses these samples to display state-level changes in mean personal wage income from 2006 to 2011. The graph color-codes states according to unweighted deciles of personal wage income changes. The difference between the weighted 90^{th} percentile change and the 10^{th} percentile change ("90-10 spread") across states was 12.4% of the 2006 mean personal wage income level.

One way in which males could have been insured against local shocks is through compensating increases in government transfers and spousal income. Figure 1b displays spatial variation in changes in post-transfer family income in 2010 dollars, deflated by the nationwide CPI-U. The 90-10 spread in these changes across states was 9.8% of the 2006 mean. So these margins of adjustment did not equalize mean income declines across space.

Figure 1c deflates post-transfer household incomes using the Local CPI 2 deflator developed in Moretti (2012). Local CPI 2 is created using a method that closely follows the Bureau of Labor Statistics's (BLS) method of computing nationwide price deflators. Specifically, after deflating mean pre-tax post-transfer household income in state s in year t using the nationwide CPI-U, I deflate it further by dividing by:

$$1 + h_t \left(\frac{RENT_{st}}{\overline{RENT_t}} - 1 \right) \tag{1}$$

where h_t is a housing weight, $RENT_{st}$ is the annual all-in cost of renting a 2-3 bedroom apartment in state s in year t as measured in the ACS, and $\overline{RENT_t}$ is the population-weighted average of $RENT_{st}$.[9] For Moretti's Local CPI 1—not used here—one uses for h_t BLS's housing weight used for the CPI-U in year t. Figure 1c deflates by the more aggressive and consequential Local CPI 2, which uses for h_t approximately 130% of BLS's annual housing weight to account for the dependency of local non-housing costs on local real estate costs.[10] Figure 1c shows that when deflating by Local CPI 2, large spatial variation in 2006-2011 income changes remains: the 90-10 spread equals 9.0% of the 2006 mean. This means that local rent declines in states like Arizona were much smaller than their local income declines.

Finally, the ACS records government cash transfers received by respondents but not the in-kind transfer of food stamps (vouchers for food purchases under the Supplemental Nutrition Assistance

[9] This assumes that local rental costs do not simply reflect local amenities (cf. Rosen 1979; Roback 1982; Diamond 2012). BLS uses rental costs and not house prices in computing the national CPI-U because house prices reflect both the user cost of housing and expected price changes, while rental costs reflect only the user cost (Poole, Ptacek, and Verbugge 2006; BLS 2007). All-in rental cost is reported in the ACS as "gross monthly rental cost" and includes contract rent, utilities, and heating fuels. This is the same variable that the Department of Housing and Urban Development uses for rent regulation.

[10] The BLS weights for 2006 and 2011 figures are $w_{2006} = .427$ and $w_{2011} = .410$, respectively. For Local CPI 2, the exact weights h_t equal $w_t(1 + (.588 - w_t)/(1 - w_t))$, where .588 is the non-housing-costs adjustment term from Moretti and equals the coefficient from a regression of overall CPI changes on housing CPI changes among the twenty-three metropolitan areas for which the BLS has historically produced local CPI measures.

Program). The U.S. Department of Agriculture publishes total food stamp transfers by state and year. When including in annual post-transfer state income totals the total cash value of food stamp transfers and then deflating by Local CPI 2, the 90-10 spread in 2006-2011 income changes remains large at 8.7%.

Table 1 panel A presents these and alternative statistics for state-level changes in mean income. The alternative statistics are the 75-25 spread (the difference between the weighted 75^{th} percentile change and the 25^{th} percentile change), the 95-5 spread (the difference between the weighted 95^{th} percentile change and the 5^{th} percentile change), and the standard deviations. These alternative statistics exhibit qualitatively similar reductions in the variance of mean income declines across space as successive income adjustments are added.

In order to parallel the rest of the paper, panel B repeats panel A using the Commuting Zone (CZ) local area concept, used recently in economics by Dorn (2009), Autor and Dorn (2013), Autor, Dorn, and Hanson (2013), and Autor, Dorn, Hanson, and Song (2013). CZ's are collections of counties designed by Tolbert and Sizer (1996) to approximate U.S. local labor markets based on commuting patterns in the 1990 Census. The continental United States comprises 722 CZ's, and every spot in the United States lies in exactly one CZ. The ACS does not contain a CZ variable; I therefore assign ACS observations to CZ's based on each observation's Public Use Micro Area (PUMA, the finest geographic unit in the ACS) as in Autor and Dorn (2013).[11] The CZ-based analysis of panel B reveals larger spatial variation in real income declines than the state-based analysis of panel A.[12]

With large spatial variation in real income declines, only selective migration could have insured individuals against Great Recession local shocks. I now introduce individual-level panel data that will allow me to analyze such migration.

IV Panel Data

IV.A Data Source

Estimating the effect of local labor demand shocks on the employment outcomes of original residents requires geo-coded panel data. This paper uses selected de-identified data from the Internal Revenue Service Statistics of Income Databank, a population panel of U.S. tax returns covering years 1996-2011 (Chetty, Friedman, Hilger, Saez, and Yagan 2011). The Databank compiles annual data from

[11] When a PUMA straddles a county border, observations in the PUMA are apportioned to the straddling counties by population shares.

[12] Food stamp transfers are available only at the state-year level. To allocate food stamp transfers to CZ's within each year, I regress state-level per-capita food stamp transfers on state-level per-capita cash transfers from the ACS and use the resulting coefficients to impute total food stamp transfers to CZ's by CZ-level per-capita cash transfers.

information returns (third-party reports like Form W-2) and Form 1040 individual income tax returns. Approximately 10% of adults do not file Form 1040's in a given year, but information returns are filed on behalf of all covered individuals, regardless of Form 1040 filing status. Data on information returns are complete for years 2000-2011 and on Form 1040's for years 1996-2011.

IV.B Variable Definitions

The IRS Databank contains the variables necessary for this paper's analysis: measures of income, employment, location, population, and several covariates. All analyses are conducted on datasets stripped of unmasked personal identifiers. All monetary variables are deflated to 2010 dollars using the national Consumer Price Index for all urban consumers (CPI-U) and monetary thresholds described here refer to CPI-U 2010 dollars; see Section III for the location-specific deflation used in a subset of analyses.

Income and Employment. Wage earnings in a given year equals the sum of wages on all Form W-2's reported on the individual's behalf by the individual's employer(s). Similar to Chetty, Friedman, Hilger, Saez, Schanzenbach, and Yagan (2011), I cap wage earnings at $200,000 to reduce the influence of outliers.

In most analyses, I classify an individual as employed in a given year if he has wage earnings greater than $15,000, approximately the full time salary of an individual employed at the federal minimum wage. Employment rate equals employment divided by population; see below for the definition of population. When specified, I define employment in either of two alternative ways: having locally deflated wage earnings greater than $15,000 ("locally deflated employment"), or having any positive wage earnings ("any employment"). For the first alternative definition, wage earnings are locally deflated using the CZ's value of Moretti's (2012) Local CPI 2, specified in Section III.[13]

Location and Population. Already detailed in the American Community Survey analysis above, the Commuting Zone (CZ) is the local labor market definition used in this paper. See Section III for details on the CZ concept. CZ is derived from the ZIP code field on information returns. If the individual was issued a Form W-2 with a valid payee ZIP code (i.e. the individual's ZIP code, not the employer's ZIP code), I use the ZIP code on the W-2 with the highest wage earnings. If not, I use the payee ZIP code that appears most frequently across the twenty-eight other kinds of information returns that may have been filed on the individual's behalf in the given year. See the next subsection for information on coverage rates of this ZIP code variable. ZIP code is mapped to the CZ level for all analyses (Autor and Dorn 2013).

[13] That is, I compute Local CPI 2 for each CZ as specified in equation (1) for the state level.

A CZ's population in a given year equals the number of people in the analysis sample (defined in the next subsection) with a ZIP code that lies in the CZ.

Covariates. Age, gender, and state of birth are derived from SSA data. For immigrants, state of birth is defined as the state of naturalization: the state in which the individual was issued a social security number. An individual is classified as married and assigned a spouse in a given year if and only if he filed a Form 1040 with status married filing jointly or married filing separately. An individual's number of kids in a given year equals the number of child exemptions claimed on a Form 1040 in the given year, with the number of children capped at two. An individual is classified as a homeowner in a given year if a Form 1098 mortgage interest information return was filed on his behalf or his spouse's behalf with a positive amount of mortgage interest or points paid, or if the individual's Form 1040 claimed itemized deductions for either mortgage interest or real estate taxes. Firm ID in a given year equals the masked employer identification number (EIN) on the individual's W-2 with the highest wage earnings. Note that corporate subsidiaries sometimes use an EIN that is different from the corporate parent's EIN, so firm ID in these cases refers to the subsidiary.

Finally, I use publicly-available data from the 2000 Census to construct the following CZ level aggregates: the share of CZ residents that are black, the share that are Hispanic, the share that have less education than a high school degree, and the share that have a bachelors degree or higher level of education.

IV.C Analysis Sample

This paper's analysis sample comprises all males aged 25-59 in 2006 who were born or naturalized in the continental United States, possess continental U.S. ZIP codes in 2006 and 2011, and were alive through the end of 2011.[14] The requirement of possessing a ZIP code excludes under 10% of middle-aged men, based on comparison to the American Community Survey.

Age is defined as of December 31, 2006. The continental United States comprises the District of Columbia and all 50 states excluding Alaska and Hawaii. I restrict the sample to working-age males for computational tractability and to focus on a subpopulation with high formal employment rates that is relatively unlikely to shift to equally valuable home production. I restrict the sample to those located in the continental United States at the onset of the Great Recession because migration may be expected to be more limited for residents of Alaska and Hawaii; I restrict the sample to those located in the continental United States in 2011 so the panel is balanced, ruling out compositional confounds. Finally, I restrict the sample to those born or naturalized in the continental United States in order to

[14] Results are nearly identical when confining the analysis to U.S. citizens.

keep samples stable when I use an instrument based on state of birth.

IV.D Summary Statistics

Table 2 displays summary statistics for the analysis sample. The sample comprises 60,312,920 men. The overall employment rate in this sample fell from 73.0% to 65.9% between 2006 and 2011. Mean wage earnings (including those earning zero dollars) fell from $\$45,438$ to $\$42,469$. The sample is approximately evenly distributed across age except slightly higher representation of 40-49-year-olds, reflecting both the baby boom and a higher likelihood of locating a ZIP code for prime-aged men. Fifty-five percent claimed no children on a Form 1040 in 2006, 58.5% were married in 2006, and 58.9% owned a home in 2006. Below in Figure 2, I plot the spatial distribution of 2006-2011 employment declines.

V Estimating Equation

I use the panel data introduced in the previous section to estimate the degree to which migration insured Americans against the spatial variation in the effects of the Great Recession. This section uses an empirical model of the causal impact of place-specific labor demand shocks in order to define the degree of migratory insurance and to specify an estimating equation and consistency conditions. The empirical model nests the theoretical model of Section II. For simplicity, this exposition focuses on the binary individual-level outcome of employment, but the exposition holds analogously for the alternative outcome of individual-level wage earnings. Recall from the previous two sections that the local area concept implemented here is the Commuting Zone (CZ).

In words, I define the degree of migratory insurance as the share of the average idiosyncratic CZ-specific labor demand shock that was not borne by pre-existing residents but that would have been borne by them in a counterfactual economy without migration. This definition has a straightforward interpretation. For example, suppose that in the no-migration counterfactual, a Phoenix-specific labor demand shock would have caused the Phoenix employment rate to fall by four percentage points relative to the aggregate decline. I ask: in the actual economy in which many people migrate, by how many percentage points did the employment rate of Phoenix's pre-existing residents fall relative to the aggregate decline? A relative decline of four percentage points implies under the above definition that migration provided no insurance against Phoenix's idiosyncratic labor demand shock; a relative decline of zero percentage points implies full insurance; an intermediate decline implies intermediate insurance.

I use a general empirical model to specify conditions under which the degree of migratory insurance can be estimated consistently by regressing individual-level changes in employment on the observed employment rate change of the individual's 2006 CZ. Section VI uses the estimating equation to estimate the degree of migratory insurance. Section VII evaluates identification threats.

V.A General Empirical Model

Let $e_{it} \in \{0,1\}$ denote individual i's employment status in year t. Let Δe_i denote the change in the individual's employment status since the onset of the Great Recession:

$$\Delta e_i \equiv e_{i2011} - e_{i2006}$$

Let c denote a CZ and let $c(i,t)$ denote the CZ in which individual i lived in year t. Let the cohort of a given CZ c (the "c-cohort") denote the set of individuals who lived in c in 2006: $\{i : c(i,2006) = c\}$. An individual who lived in c in 2006 is a member of the c-cohort no matter where he lived in other years. Let $g(i)$ denote an arbitrarily fine grouping of individuals along dimensions of skill and all labor supply determinants.

Let $\bar{e}_{ct} \equiv E[e_{it}|c(i,t) = c]$ denote the employment rate in CZ c in year t. Note that while each CZ's 2006 employment rate equals its cohort's 2006 employment rate by construction ($\bar{e}_{c2006} \equiv E[e_{i2006}|c(i,2006) = c], \forall c$), migration leads each CZ's 2011 employment rate to differ from its cohort's 2011 employment rate ($\bar{e}_{c2011} \neq E[e_{i2011}|c(i,2006) = c], \forall c$). Let the employment shock to a CZ c equal the CZ's 2011 employment rate minus the CZ's 2006 employment rate:

$$\Delta\bar{e}_c \equiv \bar{e}_{c2011} - \bar{e}_{c2006}$$

When not otherwise specified, "CZ shock" refers to the CZ employment shock $\Delta\bar{e}_c$.

Finally, let e^N_{i2011} denote individual i's employment status in year 2011 under the no-migration counterfactual: a counterfactual economy in which no one had been allowed to migrate across CZ's between 2006 and 2011 such that $c(i,2011) = c(i,2006), \forall i$.[15] Let $\Delta e^N_i \equiv e^N_{i2011} - e_{i2006}$ denote the change in the individual's employment status since the onset of the Great Recession under the no-migration counterfactual.

Assume that under the no-migration counterfactual, each individual's employment change is determined additively separably by a demographic-CZ-specific shock to labor demand $\xi^N_{g(i)c(i,2006)}$ and by a vector $\mathbf{w}_i$ of all independent determinants of changes in labor supply and non-CZ-specific changes in

[15] The term e^N_{i2011} is a kind of potential outcome. However unlike in Angrist, Imbens, and Rubin (1996), the potential outcome here is an individual's outcome when *all* agents experience an alternative environment, not just when individual i does.

labor demand as:

$$\begin{aligned}(2)\qquad \Delta e_i^N &= \mathbf{w}_i\mathbf{a}^N + \xi^N_{g(i)c(i,2006)} \\ &\equiv \mathbf{w}_i\mathbf{a}^N + \eta^N_{c(i,2006)} + \varphi^N_{g(i)c(i,2006)}\end{aligned}$$

where $\eta^N_{c(i,2006)}$ is defined as the weighted average of $\xi^N_{g(i)c(i,2006)}$ across individuals within each CZ and $\varphi^N_{g(i)c(i,2006)}$ equals the resulting residual.[16] The term $\eta^N_{c(i,2006)}$ ("the CZ no-migration shock") equals the change in the CZ's employment rate due to CZ-specific labor demand shocks $\xi^N_{g(i)c(i,2006)}$. Vector $\mathbf{w}_i$ could include arbitrarily high-dimension polynomials and interactions between characteristics like age, skill, and changes in local unemployment insurance policies in the individual's 2006 CZ.

Assume that in the actual economy in which some people migrate, each individual's employment change is determined similarly as:

$$\begin{aligned}(3)\qquad \Delta e_i &= \mathbf{w}_i\mathbf{a} + \theta_{g(i)}\xi^N_{g(i)c(i,2006)} \\ &\equiv \mathbf{w}_i\mathbf{a} + b\eta^N_{c(i,2006)} + \varphi_{g(i)c(i,2006)}\end{aligned}$$

where b is defined as the weighted average of $\theta_{g(i)}$ across individuals and $\varphi_{g(i)c(i,2006)}$ equals the resulting residual.[17] This equation allows the effect of CZ no-migration shocks to vary arbitrarily across demographic subgroups by multiplicative factors $\theta_{g(i)}$, encompassing the effects of two channels by which migration may affect individual outcomes. First, demographic groups may differ in their abilities to escape the incidence of their CZ-specific demand shocks via migration out of their 2006 CZ.[18] Second, CZ-specific demand shocks in the actual economy may differ from $\xi^N_{g(i)c(i,2006)}$.[19] I refer to b as the incidence rate of the average CZ no-migration shock on the CZ's cohort.

Migratory insurance—the share of the average CZ no-migration shock not borne by the CZ's cohort—equals $1 - b$. If b equals one, then the average CZ-cohort bore the full incidence of its CZ's no-migration shock as in the no-migration counterfactual, and migration provided individuals 0% insurance against CZ no-migration shocks. If b equals zero, then the average CZ-cohort bore no more of the incidence of its CZ's no-migration shock than other cohorts, and migration provided individuals 100% insurance against CZ no-migration shocks. Intermediate values of b indicate intermediate

[16] For simplicity of exposition, I exclude individual-level randomness, for example due to employment lotteries.

[17] As above, I abstract from any individual-level randomness for simplicity.

[18] Empirically in multivariate regressions, young men, single men, childless men, renters, especially-low-income men, and especially-high-income men have relatively high migration rates.

[19] For example, suppose as in Mian and Sufi (2013) that local labor demand shocks over the Great Recession were proximately caused by local variation in the concentration of strongly deleveraging households. Then cross-CZ migration (e.g. migration from Phoenix to San Antonio by strongly deleveraging households and from San Antonio to Phoenix by less deleveraging households) could have compressed the variance of demand-driven local employment rate declines.

incidence burdens and thus intermediate insurance.[20]

V.B Estimating Equation

I estimate migratory insurance as follows. I observe a subset $\mathbf{x}_i$ of characteristics $\mathbf{w}_i$, and I measure CZ no-migration shocks η_c^N as CZ shocks $\Delta\bar{e}_c$ potentially with error $v_c \equiv \eta_c^N - \Delta\bar{e}_c$. Rewrite equation (3) as:

$$\Delta e_i = \mathbf{x}_i \mathbf{a}_{\mathbf{x}} + b\Delta\bar{e}_{c(i,2006)} + \varphi_{g(i)c(i,2006)} + u_i + bv_{c(i,2006)}$$

where $\mathbf{a}_{\mathbf{x}}$ is the corresponding subvector of $\mathbf{a}$ and u_i is an index of the effect of the unobserved components of $\mathbf{w}_i$, leading to the estimating equation:[21]

$$\Delta e_i = \mathbf{x}_i \boldsymbol{\alpha} + \beta\Delta\bar{e}_{c(i,2006)} + \varepsilon_i \ . \tag{4}$$

I cluster standard errors at the cohort-level.[22] Empirically estimated incidence rate $\hat{\beta}$ consistently estimates true incidence rate b under the identifying assumption of conditional exogeneity of CZ shocks:

$$cov\left(\widetilde{\Delta\bar{e}_{c(i,2006)}}, u_i + bv_{c(i,2006)}\right) = 0 \tag{5}$$

where $\widetilde{\Delta\bar{e}_c}$ equals residuals from the linear projection of $\Delta\bar{e}_{c(i,2006)}$ on $\mathbf{x}_i$.[23]

Except for knife-edge cases, consistent estimation requires two economically interpretable conditions. Assumption A1 ("no omitted variables bias") is that CZ shocks are conditionally uncorrelated with the unobserved determinants of individual employment changes:

$$cov\left(\widetilde{\Delta\bar{e}_{c(i,2006)}}, u_i\right) = 0 \tag{A1}$$

Assumption A1 would be violated if, for example, CZ shocks reflect nationwide-skill-specific labor demand shocks or CZ-specific labor supply contractions. Assumption A2 ("uncorrelated measurement error") is that CZ shocks are conditionally uncorrelated with measurement error v_c:

$$cov\left(\widetilde{\Delta\bar{e}_c}, v_c\right) = 0 \tag{A2}$$

[20] In principle, b is not restricted to the $[0, 1]$ range.

[21] Note that without controls, this individual-level regression on millions of observations is identical to the 722-observation cohort-level regression $\Delta\bar{e}^c = \alpha + \beta\Delta\bar{e}_c + \varepsilon_c$ weighted by each cohort's size and with robust standard errors, where $\Delta\bar{e}^c \equiv E\left[e_{i2011} - e_{i2006}|c(i, 2006) = c\right]$ denotes the change in the cohort's employment rate. Further note that although an individual who does not move generates a mechanical relationship between Δe_i and $\Delta\bar{e}_{c(i,2006)}$, the resulting bias is vanishingly small in the very large sample used here (60.3 million observations divided among 722 cohorts) so I do not resort to a leave-one-out-mean specification.

[22] The standard error on this paper's main migratory insurance estimate is approximately 20% larger when clustered at the state level. The empirical results below are estimated using OLS on the full sample of 60.3 million observations; I obtain similar results when using non-linear models in random subsamples.

[23] Note that by construction $E[\varphi_{g(i)c(i,2006)}|\mathbf{w}_i, \eta^N_{c(i,2006)}] = 0$, so condition (5) is sufficient for consistency.

Assumption A2 would be violated if, for example, endogenous migration compressed the variance of local employment rate declines. Assumptions A1-A2 together imply conditional exogeneity of CZ shocks (5) and thus that the quantity $1 - \hat{\beta}$ estimated using equation (4) consistently estimates the degree of migratory insurance. Section VII conducts several tests for quantitatively important violations of identifying assumptions A1-A2.

V.C Algebraic Interpretation

Without controls $\mathbf{x}_i$, a non-zero estimate of migratory insurance clearly requires CZ-cohort employment rates to deviate from CZ shocks. Recognizing this, Appendix A provides an equivalent estimating equation for migratory insurance expressed as a function of the full matrix of migrant and non-migrant 2011 employment rates.

Appendix B extends the analysis of Appendix A to demonstrate that, algebraically in a stylized economy that approximates the actual economy over the Great Recession, migratory insurance requires that migration be large, directed, and selective. That is, a sufficiently large share of people must migrate out of their 2006 CZ (large migration rate), a sufficiently large share of out-migrants from heavily-depressed CZ's must migrate to less-depressed CZ's rather than to other heavily-depressed CZ's (large directedness), and migrants from heavily-depressed CZ's to less-depressed CZ's must be employed at especially high rates relative to those migrating in the opposite direction (large selectivity). Migratory insurance can fail via any or all of these channels. Section VIII.B investigates mechanisms by decomposing migratory insurance into these three channels.

V.D Theoretical Special Case

The medium-run spatial equilibrium model analyzed in Section II.D is a special case of the above empirical model in which identifying assumptions A1-A2 are satisfied. In the spatial equilibrium model, idiosyncratic exogenous adverse shocks to CZ production technology (or equivalently to CZ product demand) under fixed wages and fixed housing supplies cause CZ employment rates to fall to new levels that are independent of post-shock migration. Hence, CZ shocks equal CZ no-migration shocks, satisfying assumption A2. Further, there are no other shocks to employment, satisfying assumption A1.

In the spatial equilibrium model, the degree of migratory insurance hinges on the unobserved joint distribution of moving costs and priority for rationed jobs: migratory insurance is complete when the demographic subgroups with cross-CZ variance in demand shocks $\xi^N_{g(i)c(i,2006)}$ have trivial moving costs and equal priority for rationed jobs in all CZ's, and it is incomplete when their moving costs are high or when they have low priority for out-of-origin-city jobs. Regardless of the mix of underlying frictions, equation (4) consistently estimates migratory insurance under the assumptions of the spatial equilibrium model.

V.E Discussion

Though equation (4) consistently estimates migratory insurance under the assumptions of the spatial equilibrium model of Section II.D, the actual U.S. economy over years 2006-2011 may have deviated from that stylized ideal. Section VII conducts several tests for robustness against potential violations of the identifying assumptions. Here, I discuss an obvious potential violation, comment on my definition of migratory insurance, and detail two attractive properties of the estimating equation.

First, one might be concerned that the Great Recession caused population to shrink in lightly-shocked areas and to grow in heavily-shocked areas, potentially compressing the variance of local employment rate declines relative to the no-migration counterfactual.[24] This would violate assumption A2 and bias migratory insurance estimates downward. However, 2006-2011 population growth was in fact unrelated to CZ shocks throughout the bulk of the CZ-shock distribution, suggesting that any such bias may be small.

Figure 2b plots CZ population growth versus CZ shocks in the paper's main sample. To construct the graph, I bin CZ's into twenty equal-sized groups (five-percentile-point bins) of employment rate declines weighted by 2006 population and then plot the 2006-population-weighted mean percentage change in population within each bin.[25] The figure shows that CZ population changes were unrelated to CZ shocks throughout the bulk of the CZ shock distribution.[26] Economically, this is unsurprising because housing is durable and because new housing can be unprofitable to build when house prices fall (Glaeser and Gyourko 2005; Notowidigdo 2013) as they did over the Great Recession.

Second, note that I analyze outcomes relative to the no-migration counterfactual rather than to the potentially different trend-migration counterfactual. Blanchard and Katz (1992) found that the average adverse local labor demand shock over the 1978-1990 period caused local population to decline relative to trend, and I find the same pattern over the Great Recession. Appendix Figure 1c repeats Figure 2b except that it plots on the y-axis the difference between 2006-2011 population growth and 2001-

[24] For example, suppose that members of the Phoenix cohort who would have been non-employed under the no-migration counterfactual had moved out of Phoenix and were not replaced by in-migrants. Such net out-migration by the non-employed could imply that Phoenix's shock as measured in the data is less negative than Phoenix's no-migration shock.

[25] Note that such binned scatterplots non-parametrically portray the conditional expectation function but do not portray the underlying variance.

[26] The main sample is a balanced panel, so the economy-wide population growth rate in this sample is zero. There is positive growth in approximately absolute-least-shocked CZ's; as Figure 3a below indicates, migratory insurance estimates are nearly identical when dropping the least-shocked CZ's.

2006 population growth. The graph shows that de-trended population growth was highly positively correlated with CZ shocks as in Blanchard and Katz, even though absolute population growth was not (Figure 2b).[27] Outcomes for cohorts of heavily-shocked CZ's may have been worse under the trend-migration counterfactual in which pre-2006 population growth had continued on trend.

Finally, estimating equation (4) has two yet-unmentioned properties that would likely be attractive in any empirical analysis of the incidence of idiosyncratic local labor demand shocks over the 2006-2011 period. First, it denominates CZ shocks in the same units as individual employment changes, permitting estimation of an incidence rate (expected to lie between zero and one) while controlling for individual-level characteristics. Some leading alternatives do not exhibit this property. For example, although one could denominate CZ shocks (the independent variable) in terms of the log or percentage change in CZ employment levels, one cannot denominate individual employment changes (the dependent variable) in similar units because individuals can be non-employed ($e_{it} = 0$). One could instead estimate cohort-level regressions, but such specifications do not permit the individual-level controls that figure prominently in the robustness checks of Section VII. Second, local employment declines occurred largely over the 2007-2009 period and had run their course by 2010, implying that CZ shocks were not contemporaneous to 2011 location decisions. The timing of CZ shocks is illustrated below in Figure 3b. I now turn to the empirical analysis.

VI Results

The preceding section used a general empirical model to specify an estimating equation for the degree of migratory insurance. This section presents my main estimate of the degree of migratory insurance over the Great Recession. I first report that the average inter-CZ migration rate (13%) over the Great Recession was five times higher than the minimum rate (2.4%) necessary for 100% migratory insurance across the bulk of CZ's. Yet I find that migration in fact provided only 6.6% migratory insurance. I find similarly small degrees of migratory insurance across labor market outcomes and subgroups defined by age, marital status, number of children, and homeownership. The next section investigates robustness.

VI.A Was Migration Large Enough for Full Insurance?

Before reporting estimates of equation (4), I ask whether migration rates—observable in public data at various levels of aggregation—were large enough to provide full insurance. Figure 2a displays the variation in CZ employment shocks across the United States. The variation is quantitatively large:

[27] Hence, 2001-2006 population growth was negatively correlated with CZ shocks.

the difference between the population-weighted 90^{th} and 10^{th} percentiles of CZ shocks ("90-10 spread") was 4.8 percentage points.

The minimum migration rate necessary for full insurance depends on CZ population changes. Figure 2b showed that population changes averaged zero across the bulk of the CZ shock distribution. As shown in Appendix B, with fixed populations and a 90-10 spread in CZ shocks of 4.8 percentage points, full migratory insurance across the bulk of CZ-cohorts required migration rates of at least 2.4%: at least 2.4% of heavily-shocked CZ-cohorts had to move to and find employment in lightly-shocked CZ's, and at least 2.4% of lightly-shocked CZ-cohorts had to move to and not be employed in heavily-shocked CZ's (e.g. in order to enjoy lower costs of living when leaving the labor force).

Figure 2c plots CZ-cohort migration rates versus CZ shocks, where migration is defined as living outside one's 2006 CZ in 2011 and where data points are binned as in Figure 2b. The figure shows that the mean out-migration rate was 13.2% and varied little by CZ shock. Hence, out-migration rates were over five times larger than the minimum necessary for full migratory insurance.[28]

VI.B Main Estimates

Figure 3a illustrates the paper's main estimate of the incidence rate of CZ shocks on CZ-cohorts. The figure displays the full non-parametric relationship between CZ-cohort employment changes and CZ employment shocks for the full main sample. Data points are binned identically as in Figure 2b except that the underlying observations are reweighted on age as in DiNardo, Fortin, and Lemieux (1996) to flexibly control for minor differences in age distributions across CZ's. The regression coefficient and standard error displayed on all binned scatter plots in this paper are estimated on the underlying (non-binned) data with standard errors clustered at the 2006 CZ level. The zero-slope dotted line displays the hypothetical full-insurance extreme in which migration causes there to be no relationship between employment shocks to CZ's and employment changes of the CZ's cohorts. The unity-slope dotted line displays the no-insurance extreme in which CZ employment shocks are borne fully by the CZ's cohorts.

The figure shows that CZ-cohorts bore nearly the full incidence of CZ shocks and thus that migration has provided only modest insurance since the onset of the Great Recession. The slope of the best-fit line estimated on the underlying microdata is .934 with a standard error of .026 clustered at the CZ-level; clustering at the state level increases the standard error to .031. This is an estimate of β in equation (4) with no controls. This estimate implies that CZ-cohorts bore 93.4% of their CZ's

[28] The difference between the minimum CZ shock and the maximum CZ shock was 22.9 percentage points, so the mean migration rate was also larger than the minimum rate necessary (22.9%/2) for full migratory insurace across the entire CZ distribution. The difference between the 5^{th} and the 95^{th} percentiles was 6.0 percentage points.

employment shocks and that migration provided 6.6% insurance against Great Recession local shocks.

Figure 3b displays the finding of modest migratory insurance in a way that also facilitates evaluation of pre-recession trends. The solid lines of Figure 3b display the 2000-2011 time series of employment rates across CZ's. These series are constructed as follows. I confine attention to two groups of CZ's: the least-shocked CZ's (the CZ's that experienced 2006-2011 employment rate changes in the top-quartile of the population-weighted nationwide distribution) plotted in solid blue, and the most-shocked CZ's (the CZ's in the bottom quartile) plotted in solid red. Restricting the main sample to those aged 35-49 in 2006 (so that they may be expected to be employed 2000-2011) with a valid ZIP code in all years 2000-2011, each plotted point equals the number of employed people in the CZ group in that year, divided by the number of people living in the CZ group in that year. For example, the 2006 bottom-quartile data point indicates that 79.4% of men who lived in a bottom-quartile CZ in 2006 were employed in 2006, and the 2011 bottom-quartile data point indicates that 71.7% of men who lived in a bottom-quartile CZ in 2011 were employed in 2011.

The figure shows that employment rates in the two CZ groups tracked each other closely 2000-2006 but then diverged sharply beginning in 2007. By 2011, the most-shocked CZ's had employment rates 4.2 percentage points (5.5%) lower than the least-shocked CZ's. This is a simple representation of the wide variance of local labor demand shocks over the Great Recession displayed in Figure 2a, but also demonstrates parallel average pre-2006 trends across CZ's of different shock intensities. The parallel trends imply that CZ shocks were innovations in employment rates, rather than continuations of employment rate trends.

The dashed lines of Figure 3b display the 2000-2011 time series of employment rates across CZ-cohorts. These CZ-cohort series are constructed exactly as the CZ series, except that each series follows the same people—the 2006 residents of top- and bottom-quartile CZ's—over time. For example, the 2011 bottom-quartile data point indicates that 71.9% of men who lived in a bottom-quartile CZ *in 2006* were employed in 2011—similar to the 71.7% employment rate among 2011 residents reported above. The figure shows that employment rates of CZ-cohort groups were nearly identical to the employment rates of their respective CZ groups. This is simple evidence that migration diffused only a small fraction of CZ employment shocks across CZ-cohorts.

Table 3 row 1 reports estimates of equation (4) with no controls, for three employment outcomes: employment using the wage earnings threshold of $15,000 (as in Figures 2-3), employment using the wage earnings threshold of $0, employment using the locally deflated wage earnings threshold of $15,000, and wage earnings. The CZ shock regressor in each column is defined using that column's outcome of interest, as specified in equation (4). The table shows relative stability in incidence rates of CZ shocks on CZ-cohorts across these alternative outcomes.

VI.C Heterogeneity

Some groups (e.g. middle-aged men) are relatively attached to the labor market, and some groups (e.g. single men) may be relatively more mobile. Did any major demographic subgroups avoid bearing most of the incidence of Great Recession local shocks? Incidence of CZ shocks on subgroups reflects two forces: the effect of the CZ shock on the subgroup's employment in the CZ ("subgroup-specific attachment"), and diffusion by that subgroup via migration ("subgroup-specific migration").[29]

Figure 4 reports regression estimates that isolate the diffusion effects of subgroup-specific migration for the employment outcome. Every row reports a coefficient estimate and clustered 95% confidence interval from equation (4) with no controls. Each row's regression is restricted to the subgroup (defined as of 2006) specified in the row, and the CZ shock regressor is constructed using only that subgroup. For example, the coefficient estimate and confidence interval displayed in the "Age 25-29" row are derived from a regression on the subsample of individuals i who were aged 25-29 in 2006 and in which the CZ shock regressor $\Delta\bar{e}_{c(i,2006)}$ equals the 2011 employment rate in CZ $c\,(i, 2006)$ among residents who were aged 30-34 in 2011, minus the 2006 employment rate in CZ $c\,(i, 2006)$ among residents who were aged 25-29 in 2006.

The figure reports estimates for subgroups based on age, marital status, number of children, homeownership, and wage earnings; see Appendix Figure 2a for illustrations of the non-parametric relationships underlying selected estimates. The incidence estimates are nearly all within the confidence interval of the overall incidence estimate of .934, and no incidence estimate is lower than .8. Thus migratory insurance diffused the majority of local employment shocks for no demographic subgroup.

Appendix Figure 2b repeats Figure 4 except that the CZ shock regressor in every equation is the same CZ shock constructed using the full sample. This means that the estimates in Appendix Figure 2b report the combined effect of subgroup-specific attachment and subgroup-specific migration, rather than just the effect of subgroup-specific migration. The results are noisier, possibly reflecting the fact that demographic subgroup employment trends differ across space; for example, older workers in Florida may be less attached to the labor market than older workers elsewhere. However, the estimates are similar in magnitude to those in Figure 4, reinforcing the conclusion that no demographic subgroup studied here avoided the bulk of the incidence of their CZ shock.

[29] Consider the example of Phoenix. The large negative employment shock to Phoenix lowered employment rates of young men living in Phoenix relative to the average man living in Phoenix; then conditional on that relative decline, young men living in Phoenix in 2006 may have been relatively able to avoid its incidence.

VII Robustness

The preceding section estimated that the average CZ-cohort bore 93% of the CZ's idiosyncratic employment shock. This implies 7% migratory insurance under identifying assumptions A1 (no omitted variables bias) and A2 (uncorrelated measurement error) specified in Section V.B. I now test the robustness of the finding of modest migratory insurance using strategies that address three potential violations of the identifying assumptions: endogeneity of CZ shocks due to nationwide-skill-specific labor demand shocks, endogeneity of CZ shocks due to policy-induced labor supply shocks, and attenuation in the measurement of CZ shocks.

First, it is possible that employment declines across CZ's reflect nationwide-skill-specific labor demand shocks that affected workers identically across space, combined with selection across space on skill. For example, perhaps manual laborers like construction workers experienced a very negative labor demand shock over the Great Recession and perhaps places like Phoenix were disproportionately populated by manual laborers. In this case, the relatively large number of non-employed workers in places like Phoenix would have been non-employed no matter where they had lived in 2006. Second, employment declines across CZ's could reflect place-specific contractions in labor supply due to the place-specific government policy of extended unemployment insurance (UI) (Mulligan 2010). In this case, non-employed workers in places like Phoenix may simply prefer non-employment in Phoenix to employment elsewhere.

Third, CZ shocks could underestimate the counterfactual employment rate decline in heavily-shocked CZ's that would have prevailed under no migration, and likewise overestimate the counterfactual in lightly-shocked CZ's. For example, if CZ shocks were caused by household deleveraging (Mian, Rao, and Sufi 2013; Mian and Sufi 2013) and if many deleveraging Phoenix households moved to places like San Antonio and vice versa by 2011, such migration would attenuate the observed CZ shock difference between Phoenix and San Antonio, relative to the no-migration counterfactual. In each of these three cases, the incidence estimate would be biased upward and thus the migratory insurance estimate would be biased downward.

I use four empirical strategies to investigate the quantitative importance of these potential confounds. First, I use rich controls to control for individual skill. Second, I instrument CZ shocks using Great Recession employment shocks to the individual's state of birth in order to remove variation in CZ shocks generated by selective pre-recession in-migration based on skill. Third, I use within-state variation in CZ shocks and cross-state variation in UI extensions to test whether the above incidence estimates hold when controlling flexibly for UI durations. Finally, I instrument CZ shocks using proxies for labor demand shocks that use local information only from year 2006.

VII.A Employer Fixed Effects and Other Controls

As a first test for bias from violations of assumption A1, columns 1-5 of Table 4 report employment incidence estimates from equation (4) while controlling sequentially for a basic set of controls. Column 2 reports the same coefficient displayed in Figure 3b. Column 1 displays the coefficient without age reweighting. Columns 3-6 display estimates with the sequential addition of 2006 personal demographics (indicators for 2006 marital status and for number of kids in 2006), 2006 CZ demographics (quartics in the share of residents who are black, share that are Hispanic, share with less than high-school education, and share with a college degree), 2006 wage earnings (indicators for the individual having zero wage earnings, $1-15k, $15k-$30k, $30k-$45k, $45k-$60k, $60k-$75k, $75k-$90k, and $90k or above).[30] Coefficient estimates are stable across these specifications.

One may yet be concerned that basic controls insufficiently control for cross-cohort skill differences. I therefore further control for firm fixed effects. Multi-CZ firms like Walmart employ similarly-skilled workers across space, so controlling for firm fixed effects may hold constant many dimensions of unobserved skill.[31]

I carefully restrict the sample for the 2006 firm fixed effects analysis to maximize comparability across workers within firms and to minimize computational burden. I first link firm ID from Form W-2's to business income tax returns in order to restrict the main sample to employees who reside in a CZ other than the CZ of their firm's headquarters.[32] This accounts for the reasonable possibility that employees at a firm's headquarters are not comparable to employees at the firm's other establishments. I then further restrict the sample to employees at firms that employed at least ten workers in at least ten CZ's in 2006. This yields a sample of 3,097,279 employees of 1,266 multi-CZ firms.

Column 6 uses this subsample to repeat the specification of Column 5 with 2006 firm fixed effects. The confidence interval widens relative to the previous specifications, but the point estimate remains nearly unchanged. Column 7 further interacts firm fixed effects with each of the six wage earnings bins ($15k-$30k, $30k-$45k, $45k-$60k, $60k-$75k, $75k-$90k, and $90k or above) that apply to 2006 employed workers. The point estimate remains nearly unchanged. Appendix Table 1 replicates Table 4 for the alternative outcomes of locally-deflated employment (employment defined as having locally

[30] I use binned measures of wage earnings rather than continuous measures for computational tractability. Regressions with 60 million observations are computationally intractable. However, regressions on binned data with weights equal to the underlying number of observations replicate the coefficients and standard errors from microdata, so long as clustering is done at a weakly-higher level than the binning.

[31] Unmasked firm identifiers were not accessed for this paper.

[32] Firms report the location of their headquarters on their income tax returns, regardless of the firms' states of incorporation.

deflated wages earnings greater than $15,000) and wage earnings, yielding similar results. Hence, the introduction of a rich set of controls yields no evidence that the incidence estimates of the previous section are biased by selection on skill across CZ-cohorts.

VII.B Instrumenting Using State of Birth

As a second test for substantial bias from violations of assumption A1, Table 3 row 2 reports incidence estimates when instrumenting CZ shocks using the 2006-2011 employment shock to the individual's state of birth ("state-of-birth shock"). This instrumental variable (IV) strategy directly addresses the identification threat of selective in-migration: that the most-shocked CZ's may have by 2006 accumulated men whose skills would become obsolete over the Great Recession, such as displaced manufacturing workers from the Rust Belt consistent with Charles, Hurst, and Notowidigdo (2013). By instrumenting individuals' CZ shock using the 2006-2011 employment shock to their states of birth, I identify a treatment effect local to the subpopulation of individuals who experienced a CZ shock similar to their state-of-birth's shock. The identifying assumption is that the 2006-2011 shock to one's state of birth affects one's 2006-2011 employment change only through its influence on one's CZ shock; in particular, it assumes that people born in states like Arizona were not intrinsically likely to experience 2006-2011 employment declines relative to people born in states like Texas.

Each cell of Table 3 displays a coefficient estimate from a separate regression. Recall that row 1 displays coefficient estimates with no instruments. Row 2 displays the results when instrumenting one's 2006 CZ shock with one's state-of-birth shock. Coefficient estimates remain near unity across all employment and income outcomes. Hence, pre-recession sorting on skill is unlikely to be driving the large incidence estimate reported in Section VI.

VII.C UI Extensions

To test for substantial bias from violations of assumption A1 due to unemployment insurance (UI) benefit duration extensions, column 8 of Table 4 controls for place-specific extensions in maximum unemployment insurance (UI) benefit durations. In 2006, no state offered UI benefits for longer than 26 weeks after layoff; between 2009 and 2011, maximum UI durations were extended to 99 weeks. These extensions varied considerably across states. Some of this variation was directly related to state-level unemployment rates, but other variation was related instead to political decisions. For example, Alabama and Mississippi both had unemployment rates of 10.4% in early 2010 but state-level laws made Alabama workers eligible for 20 more weeks of unemployment insurance than Mississippi workers (Rothstein 2011). UI benefits subsidize non-employment, so CZ shocks could in theory have been driven by place-based variation in UI durations. Here, I test whether controlling flexibly for UI durations attenuates the main incidence estimates.

Using publicly available data from the Current Population Survey and the Social Security Administration, Farber and Valetta (2011), Rothstein (2011), and Rothstein (2013) find little to no effect of UI extensions and expiration on employment since the Great Recession. I nevertheless investigate whether UI extensions may be confounding the present analysis. I use Rothstein's (2011) state-level panel of maximum UI durations to compute each state's maximum duration during years 2008-2011 (there was no variation in UI durations in 2007). Specifically, I compute for each state-year the average maximum UI duration available, averaged over days in the calendar year.

Column 8 of Table 4 repeats the specification underlying column 5 except controlling additionally for a quartic in the individual's 2006 CZ's maximum UI duration in each year 2008-2011. For example, if an individual lived in the Phoenix CZ in 2006, I include a quartic in Arizona's 2008 average maximum UI duration, a quartic in Arizona's 2009 average maximum UI duration, etc., through 2011.[33] The incidence estimate is therefore estimated off within-state variation in CZ shocks and cross-state variation in UI extensions. The incidence estimate remains unchanged.

VII.D Instrumenting Using Observable Proxies for Labor Demand Shocks

Finally, to test for substantial bias from violations of assumption A2 due to endogenous 2006-2011 migration, I instrument CZ shocks using proxies for labor demand shocks that rely on local information only from year 2006. Table 3 row 3 follows Blanchard and Katz (1992) and numerous others in using a Bartik (1991) instrument for local labor demand shocks, equal to the projected change in a CZ's employment based on leave-one-CZ-out nationwide changes in employment by industry, interacted with each CZ's industry concentration. For example, the Detroit CZ had a large concentration of automobile workers in 2006. Automobile manufacturing outside the Detroit CZ experienced a larger decline than the average industry, so Detroit would be projected to have experienced a larger employment decline than the average CZ and thus would be assigned a relatively negative Bartik instrument value.

Row 4 follows Mian and Sufi (2013) in using CZ household leverage—defined as the ratio of total household debt to total household income—to instrument for CZ shocks. Mian and Sufi and the earlier paper Mian, Rao, and Sufi (2013) present two facts that suggest that CZ household leverage isolates place-specific labor demand shocks. First, Mian et al. show that household spending declined most in areas of the United States that were most highly leveraged, consistent with households deleveraging

[33] For the 122 CZ's that straddle state borders, I use an average among the states' maximums, weighted by 2006 within-CZ employment shares across states based on County Business Patterns data. UI is based on state of employment, not state of residence; County Business Patterns data reflect state of employment.

in the wake of the housing market collapse. Second, Mian and Sufi show county-level household leverage strongly negatively predicts county-level changes in employment in non-tradable industries that rely on local demand, but not county-level changes in employment in tradable industries that rely on nationwide demand. Hence, CZ household leverage may isolate place-specific labor demand shocks.

Rows 3-4 present coefficient estimates for each employment outcome using the Bartik instrument and household debt-to-income ratio instrument, respectively. The estimates using the Bartik instrument are somewhat smaller than the raw (non-instrumented) estimates, while the estimates using the debt-to-income ratio instrument are somewhat larger, perhaps because they are estimating different local average treatment effects. Row 5 combines all three instruments—state-of-birth shock, Bartik, and debt-to-income ratio—into a single specification. These estimates are of equal or larger magnitude than the raw estimates. All told, the evidence supports Section VI's finding of modest migratory insurance since the onset of the Great Recession.

VIII Why Has Migration Insured So Little?

The previous two sections presented evidence that migration has diffused little of the incidence of Great Recession local shocks, even though migration could in principle have diffused the incidence entirely. This section investigates the relative importance of three candidate frictions in hindering migratory insurance: moving costs, weak information on far-away employment conditions, and weaker employment access upon moving to a stronger labor market.

I first test for quantitative importance of moving costs and weak information by testing for greater migratory insurance among the cohorts of CZ's with large nearby concentrations of differently-shocked CZ's. I then document substantially greater migratory insurance over the 2001 recession and use a cross-sectional decomposition of migratory insurance into three components—each primarily related to one the three candidate frictions—to test which friction is likely responsible for the reduction in migratory insurance between the two recessions.

VIII.A Incidence Rates by Geographic Shock Concentration

Individuals are likely better informed about and able to cheaply move to nearby areas. I therefore test for evidence of the quantitative importance of moving costs and weak information by estimating whether migratory insurance was significantly larger among cohorts of CZ's that experienced shocks substantially different from shocks to nearby CZ's, relative to migratory insurance among cohorts of other CZ's. Figure 2a showed that there is substantial heterogeneity in geographic shock concentration. For example, the Phoenix CZ is surrounded by other heavily-shocked CZ's, while the Fort Smith CZ (the heavily-shocked CZ on the border between Oklahoma and Arkansas) was surrounded by lightly-shocked CZ's. I test whether migratory insurance was higher among cohorts of CZ's like Fort Smith, relative to cohorts of CZ's like Phoenix.

I implement this test as follows. For each CZ c, I compute what I refer to as the "neighbor shock", equal to the population-weighted mean CZ shock among all other CZ's whose geographic centroid lies within 250 miles of the geographic centroid of CZ c. For example, the Phoenix CZ has a very negative value of neighbor shock, while the Fort Smith CZ does not. I then restrict the sample to CZ's in the population-weighted top quartile (e.g. San Antonio) or bottom quartile (e.g. Phoenix and Fort Smith) of CZ shocks. I define a top-quartile (bottom-quartile) CZ as having been surrounded by differently shocked CZ's ("isolated") if and only if its neighbor shock was below (above) the population-weighted median neighbor shock. Finally, I test for a significant difference in incidence rates between the group of isolated CZ's and the group of non-isolated CZ's. If the estimated incidence rate is lower among isolated CZ's, then I conclude that there is suggestive evidence that moving costs or information frictions are quantitatively important.

Figure 5a displays the results. The data are binned as in Figure 3a. As the graph illustrates, incidence rates in the two groups of CZ's are quite similar and close to the overall incidence rate of 93%. If moving costs or weak information had been quantitatively important, one would expect that the dotted red best-fit line would be flatter: for example, one would expect that the left-most red data point (which includes the Fort Smith cohort) would lie significantly above the left-most blue data point (which includes the Phoenix cohort). The graph does not display such a difference.

Table 5 and Appendix Table 2 report the results of formal tests for significantly different incidence rates between isolated CZ cohorts and non-isolated CZ cohorts, varying the definition of isolation for robustness. Table 5 reports estimates of coefficients from estimation of the following extension of equation (4):

$$\Delta e_i = \alpha + \beta \Delta \bar{e}_{c(i,2006)} + \delta \Delta \bar{e}_{c(i,2006)} \times ISOLATED_{c(i,2006)} + \varepsilon_i \qquad (6)$$

where $\Delta e_i \in \{-1, 0, 1\}$ equals individual i's 2006-2011 change in employment status and where $ISOLATED_{c(i,2006)}$ is a binary variable equal to one if and only if CZ $c\,(i, 2006)$ was surrounded by differently-shocked CZ's ("isolated"). Appendix Table 2 replicates Table 5 while including $ISOLATED_{c(i,2006)}$ as a separate regressor, allowing isolated cohorts to have a different intercept from non-isolated cohorts. The precise definition of $ISOLATED_{c(i,2006)}$ varies across specifications. If the coefficient δ is significantly different from zero, then I conclude that isolated cohorts had different incidence rates than non-isolated cohorts.

The various definitions of $ISOLATED_{c(i,2006)}$ are the following. Panel A column 1 uses the definition of $ISOLATED_{c(i,2006)}$ used in Figure 5a. For column 2, I use the same definition except that I define a top-quartile (bottom-quartile) CZ as having been isolated if and only if its neighbor shock was below (above) the population-weighted 25^{th} percentile (75^{th} percentile) neighbor shock. For columns 3-4, I use the same definitions as in columns 1-2, respectively, except that the median, 25^{th} percentile, and 75^{th} percentile thresholds refer to the CZ shock distribution, rather than the neighbor shock distribution. Finally, panel B replicates panel A using a 500-mile radius instead of a 250-mile radius to compute neighbor shocks.

As suggested by Figure 5a, none of the sixteen specifications in Table 5 and Appendix Table 2 reveal significant differences in incidence rates between isolated and non-isolated CZ-cohorts. Thus I conclude that this test does not provide evidence in favor of the quantitative importance of moving costs or information frictions.

VIII.B Comparison to the 2001 Recession

Relative to the 2001 recession, migratory insurance may have been particularly low over the Great Recession via any of the three candidate frictions. First, moving costs may have been higher. For example, house price declines over the Great Recession were much larger than over the 2001 recession, potentially increasing moving costs by pushing mortgages underwater as in Ferreira, Gyourko, and Tracy (2010).[34]

Second, information on employment conditions in far-away labor markets may have been weaker. For example, it may have been obvious to potential migrants in the early 2000's that employment conditions in the Sun Belt were excellent because both employment and population were growing briskly there, while over the Great Recession, absolute employment declines across the United States may have masked the fact that employment conditions were nevertheless much stronger in places like San Antonio than places like Phoenix.

Finally, employment access among in-migrants to stronger labor markets may have been weaker over the Great Recession than over the 2001 recession. For example, suppose that employment is rationed in downturns (e.g. Michaillat 2012) and that locally incumbent residents have priority for rationed jobs, perhaps because employers strictly prefer hiring people who appear committed to living

[34] A mortgage is "underwater" (equivalent to the home having "negative equity") when the balance on the mortgage is greater than the market price of the home. In that case, the proceeds from home sale are insufficient to pay off the mortgage, and a homeowner wishing to move without defaulting must use personal savings or other funds to cover the difference between the mortgage balance and the sale price. Liquidity-constrained households may have difficulty doing so.

in the local area. The relatively strong local labor markets over the 2001 recession experienced very mild employment declines and were unlikely to have exhibited substantial employment rationing, while the relatively strong local labor markets over the Great Recession (e.g. San Antonio) experienced substantial employment declines and thus may have exhibited employment rationing that placed in-migrants at a relative disadvantage in obtaining local employment.

Thus to investigate why migration provided such little insurance over the Great Recession, I test whether migratory insurance was greater over the 2001 recession. Finding that it was, I then use a decomposition of migratory insurance to test for suggestive evidence of which candidate friction(s) explain the relatively low migratory insurance over the Great Recession.

VIII.B.1 Migratory Insurance over the 2001 Recession

I first test whether migratory insurance over the 2001 recession was substantially larger than over the Great Recession. I do so by replicating Figure 3a for the 2001 recession. The sample and shock measures are constructed exactly analogously to the main sample except that the years used are 2000 and 2005, rather than 2006 and 2011. In particular, the age group is restricted to those who were aged 25-59 in 2000. Dollar amounts are still inflated to 2010 dollars using the national CPI-U.

I then construct Figure 5b exactly as Figure 3a except that the independent variable is displayed net of the set of personal and CZ demographic controls underlying Table 4 column 5 (the most important of which is the individual's pre-recession wage earnings bin, as I illustrate next). Figure 5b shows that the incidence rate of CZ shocks on CZ-cohorts over the 2001 recession was 75.7%, implying that migratory insurance was over three times greater over the 2001 recession than over the Great Recession (24.3% versus 6.6%).

Table 6 shows this result in greater detail by replicating Table 4 for the 2001 recession. Column 2 shows that when controlling only for age, the incidence estimate is near unity, implying no migratory insurance. However, column 3 shows that pre-recession wage earnings is a quantitatively important confound: controlling for 2000 wage earnings reduces the incidence estimate to .649. Including other personal demographic and CZ demographic controls elevates the incidence estimate to .757, displayed in Figure 5b. Appendix Table 3 shows similar results for the alternative outcomes of locally-deflated employment (employment defined as having locally deflated wages earnings greater than $15,000) and wage earnings.

Note from comparison of Tables 4 and 6 that the inclusion of the pre-recession wage earnings control has a much smaller effect on the estimated incidence rate over the Great Recession than over the 2001 recession. The statistical reason for this is that although pre-recession wage earnings has a negative direct observed effect on employment changes in both recessions, pre-recession wage earnings was substantially negatively correlated with CZ shocks over the 2001 recession (i.e. richer places had larger employment declines) but not over the Great Recession.

Figure 5c shows that the much greater migratory insurance over the 2001 recession was driven by much greater diffusion of local shock incidence by above-average earners. The figure plots incidence rates for the two recessions by \$15,000 income bins, using the same method as in Figure 4. Specifically, for each recession, I bin individuals into \$15,000-wide wage earnings bin based on pre-recession (year 2000 or year 2006) earnings in 2010 dollars. Then within each bin, I compute CZ shocks using only each bin's individuals and regress individual-level employment changes on these CZ shocks.

Mean wage earnings in 2006 were approximately \$45,000 (see Table 2). The figure shows that incidence rates among those with below-average earnings were approximately equal and close to one over both recessions. However, incidence rates among those with above-average earnings were substantially smaller in the 2001 recession than in the Great Recession and well below one. For example, the estimated incidence rate for those with pre-recession earnings between \$45,000 and \$60,000 was 97.6% over the Great Recession but 79.2% over the 2001 recession. This eighteen-percentage-point difference in incidence rates across the two recessions is approximately stable across higher earnings bins.

Quantitatively, the difference in incidence rates among above-average earners explains the entire difference in mean incidence rates across the two recessions. I estimate this as follows. When weighting each wage earnings bin by its 2006 population shares, the mean incidence rate across bins in Figure 5c equals 91.5% for the Great Recession and 83.0% for the 2001 recession.[35] In a hypothetical Great Recession economy where below-average earners had Great Recession incidence rates and above-average earners had 2001-recession incidence rates, the weighted-average incidence rate would equal 82.7%.

VIII.B.2 Algebraic Decomposition of Migratory Insurance

The previous subsection revealed that migratory insurance was substantially greater over the 2001 recession than over the Great Recession, driven by substantially greater migratory insurance for above-average earners. I now conduct an algebraic decomposition of migratory insurance into three channels, each of which corresponds primarily to one of the three candidate frictions being investigated. In the next subsection, I apply this decomposition to the 2001 recession and to pre-recession wage earnings bins to test which channel(s) and thus friction(s) likely explain the decline in migratory insurance for above-average earners.

[35] These figures differ from the overall figures because each bin's incidence rate is computed using shocks and employment rates defined within each bin, rather than overall.

Appendix B demonstrated algebraically that, in a stylized economy that approximates the actual economy over the Great Recession, migratory insurance requires that migration be large, directed, and selective. That is, a sufficiently large share of people must migrate out of their 2006 CZ (large migration rate), a sufficiently large share of out-migrants from heavily-depressed CZ's must migrate to less-depressed CZ's rather than to other heavily-depressed CZ's (large directedness), and migrants from heavily-depressed CZ's to less-depressed CZ's must be employed at especially high rates relative to those migrating in the opposite direction (large selectivity). Migratory insurance can fail via any or all of these channels.

Each of these three channels is primarily related to one of the three classes of frictions investigated here. Higher moving costs should have reduced migration rates. Weaker information should have reduced directedness. And weaker employment access among in-migrants to less-depressed areas should have reduced selectivity. It is important to note that each friction could have affected the other channels as well. For example, higher moving costs may have affected directedness and selectivity by affecting the composition of migrants. Hence, findings from this subsection are ultimately suggestive and not conclusive.

To describe each of the three channels in terms of a single estimable statistic, consider an extension of the main estimating equation (4) that includes as a regressor the difference in 2011 employment rates between the individual's 2006 CZ and the individual's 2011 CZ:[36]

$$\Delta e_i = \alpha + \beta \Delta \bar{e}_{c(i,2006)} + \gamma \left(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011} \right) + \varepsilon_i \qquad (7)$$

The term $\left(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011} \right)$ equals the improvement in CZ labor market conditions that individual i achieved by moving to a new CZ, relative to staying in his 2006 CZ. If individual i did not move, then $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$ equals zero. If the person moved from a heavily-depressed CZ like Phoenix to a less-depressed CZ like San Antonio, then $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$ is large and positive. If the person moved in the opposite direction, then $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$ is large and negative. The coefficient β represents the observed effect of having lived in a lightly-shocked CZ in 2006, while the coefficient γ represents the observed benefit to moving to a less-depressed CZ.

The additive separability of equation (7) assumes that each unit improvement in CZ labor market conditions has the same observed effect γ in one's employment probability and that this effect does not

[36] This equation is meant to compactly characterize the cross-sectional relationships among employment changes, CZ shocks, and migration to stronger or weaker labor markets. This may separately represent a causal relationship for an average individual, though likely not for the population: substantial migratory insurance with low migration rates typically requires $\beta < 1$ reflecting high selectivity, but if CZ shocks equal CZ no-migration shocks as assumed in Section V, then $\beta = 1$ if the population were experimentally prevented from migrating.

vary by the intensity of the shock one was exposed to $\Delta\bar{e}_{c(i,2006)}$. Figure 6a shows that this assumption characterizes the data well. It illustrates the value of $\hat{\gamma}$ in the sample of people with a non-zero value of $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$, i.e. the 13.2% of people with a 2011 CZ different from their 2006 CZ ("2006-2011 migrants"). The figure plots the non-parametric relationship between individual 2006-2011 employment changes Δe_i and the labor market conditions improvement achieved by migrating $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$, net of the CZ shock control $\Delta\bar{e}_{c(i,2006)}$ as in equation (7).

Specifically to construct the graph, I regress $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$ on $\Delta\bar{e}_{c(i,2006)}$, compute residuals, add the mean of $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$ to the residuals to facilitate interpretation, and then construct a binned scatterplot using the same method in Figure 2b. The slope of the resulting best-fit regression line (estimated on the underlying microdata) equals $\hat{\gamma}$ in equation (7).[37] The graph shows that the non-parametric relationship is quite linear and tightly estimated. In the next subsection, I discuss in detail the implications of the empirical value of $\hat{\gamma}$.

Equation (7) allows the employment rate change of a given c-cohort to be decomposed as follows:

$$\begin{aligned}(8)\qquad \underbrace{E\left[\Delta e_i\right]-\alpha}_{\substack{\text{cohort's actual change}\\ \text{relative to mean}}} &= \underbrace{\beta\Delta\bar{e}_{c(i,2006)}}_{\substack{\text{effect of CZ shock}\\ \text{under no migration}}}\\ &+ \underbrace{\overbrace{\gamma}^{\text{benefit}} \times \overbrace{E\left[c\left(i,2011\right)\neq c\left(i,2006\right)\right]}^{\text{migration rate}} \times \overbrace{E\left[\bar{e}_{c(i,2011)2011}-\bar{e}_{c(i,2006)2011}\left|c\left(i,2011\right)\neq c\left(i,2006\right)\right.\right]}^{\text{migration directedness}}}_{\text{migratory insurance effect}}\end{aligned}$$

where the expectations are taken over the members of the given c-cohort $(\{i : c\,(i, 2006) = c\})$. The baseline cross-sectional effect of the shock $\beta\Delta\bar{e}_{c(i,2006)}$ is amended by the migratory insurance effect, equal to the estimated benefit from the whole cohort moving to a hypothetical CZ that has a 100-percentage-point-higher employment rate ("benefit") times the share of the cohort that actually moved ("migration rate") times the actual mean improvement in CZ employment conditions achieved by moving ("migration directedness"). A large observed benefit implies large selectivity.

This is an intuitive decomposition. For example, consider a heavily-shocked CZ like Phoenix. Migration helped the Phoenix cohort to the extent that moving to a place like San Antonio is associated with improved outcomes (large benefit), that many people moved (large migration rate), and that the people who moved out of Phoenix moved to places like San Antonio rather than to other places like Phoenix (large directedness). Migratory insurance could have failed via any or all of these channels.

[37] The value of $\hat{\gamma}$ estimated on the full sample is .607, slightly smaller than the .622 value reported in Figure 6a on the 2006-2011 migrants subsample.

I next parameterize these channels and explore their quantitative significance in explaining the decline of migratory insurance among above-average earners over the Great Recession relative to the 2001 recession.

To do so compactly, I represent each channel in a single estimable statistic. The migration rate is easily measured. The observed benefit γ is estimated by equation (7). I represent directedness using a "lack-of-directedness" measure equal to the coefficient on a regression of the 2011 employment rate of one's 2011 CZ $\bar{e}_{c(i,2011)2011}$ on the 2011 employment rate of one's 2006 CZ $\bar{e}_{c(i,2006)2011}$, estimated on the sample of 2006-2011 migrants. This lack-of-directedness measure maps each cohort to an average improvement in CZ labor market conditions $(\bar{e}_{c(i,2011)2011} - \bar{e}_{c(i,2006)2011})$.

The lack-of-directedness measure is an informative statistic. With uniform CZ populations, fully directed migration involves all out-migrants from the most-depressed CZ migrating to the least-depressed CZ, all out-migrants from the second-most-depressed CZ migrating to the second-least-depressed CZ, etc.—yielding a lack-of-directedness measure equal to -1. Fully undirected migration involves all out-migrants moving to a CZ with the same 2011 employment rate as their 2006 CZ—yielding a lack-of-directedness measure equal to 1.

Lack-of-directedness can be visualized in Figure 6b, which non-parametrically displays the lack-of-directedness measure over the Great Recession. It plots the 2011 employment rate of migrants' 2011 CZ versus the 2011 employment rate of migrants' 2006 CZ. The reported slope (.195) equals the lack-of-directedness measure. The positive slope implies that migration was less directed than random migration, which would have yielded a zero slope.[38] Fully directed migration would have yielded a slope of -1. Fully undirected migration would have yielded a slope of 1.

VIII.B.3 Decomposition Results

I now decompose migration rates by recession and wage earnings bins into the three migratory insurance components in order to test for suggestive evidence of the three candidate frictions—higher moving costs, weaker information on employment conditions in far-away labor markets, and weaker employment access among in-migrants to stronger labor markets—in explaining the relative decline in migratory insurance among above-average earners between the two recessions.

First, if higher moving costs were responsible for the lower migratory insurance among above-average earners over the Great Recession, one would expect that migration rates among above-average earners fell relative to below-average earners between the 2001 recession and the Great Recession.

[38] Though not directly relevant for the decomposition analysis, the lack-of-directedness measure is nearly unchanged when controlling for quartics in distance traveled and quartics in the land area of the 2006 CZ.

Figure 7a displays migration rates by recession and pre-recession wage earnings bin. Consistent with the long-term decline in U.S. migration rates observed in public data (Molloy, Smith, and Wozniak 2011), migration rates declined across all wage earnings bins between the 2001 recession and the Great Recession. The key question is whether migration rates fell among above-average earners relative to below-average earners, including discretely beginning with the $30,000-$45,000 and $45,000-$60,000 earnings bins as in Figure 5c. The smooth progression of the two series makes it clear that there was no such relative decline among above-average earners. Thus higher moving costs were unlikely responsible for the decline in migratory insurance between the two recessions.

Second, if weaker information regarding employment conditions in far-away labor markets was responsible for the decline in migratory insurance among above-average earners, then one would expect that migration directedness fell uniquely among above-average earners. Figure 7b plots the lack-of-directedness measure (defined above and illustrated in Figure 6b) by recession and pre-recession wage earnings bin. Migration was less directed overall over the Great Recession relative to the 2001 recession. But as with migration rates, there was no relative decline in directedness among above-average earners relative to below-average earners. Thus weaker information regarding far-away employment conditions was not likely responsible for the decline in migratory insurance between the two recessions.

Third, if weaker access to employment upon moving to a less-depressed CZ was responsible for the decline in migratory insurance, then one would expect that the observed benefit to moving to a less-depressed CZ would have declined for above-average earners over the Great Recession. This is indeed what I find. Figure 7c plots the observed benefit to moving to a less-depressed CZ (γ in equation 7) by recession and pre-recession wage earnings bin. As expected, the observed benefit to moving to a less-depressed CZ is positive for all groups in both recessions. The benefit was similar for below-average earners in both recessions, but the benefit was much larger for above-average earners over the 2001 recession than over the Great Recession.

Quantitatively, a Blinder (1973)-Oaxaca (1973) analysis suggests that this lower observed benefit to moving among above-average earners explains eighty percent of the difference in migratory insurance between the two recessions. Specifically, recall from Figure 3a that the average incidence rate over the Great Recession was 93.4%. When replacing actual Great Recession migratory insurance with hypothetical migratory insurance under the 2001 recession migration benefit, migration rate, and migration directedness, I obtain a hypothetical incidence rate of 84.7%. When replacing actual Great Recession migratory insurance with hypothetical migratory insurance under the 2001 recession migration benefit but the Great Recession migration rate and migration directedness, I obtain a hypothetical incidence rate of 86.4%. Hence, the change in the observed benefit to moving to a less-depressed CZ explains 80.0% (= [.934 − .864]/[.934 − .847]) of the decline in migratory insurance between the two recessions. Hence, the decomposition evidence suggests that migratory insurance was lower over the Great Recession primarily because of unusually weak employment access among above-average earners migrating from heavily-depressed CZ's to less-depressed CZ's—rather than because of unusually high moving costs or because of unusually poor information on where to move.

IX Conclusion

Some U.S. local labor markets experienced employment and income declines over the Great Recession that were twice as large as the aggregate decline. This paper has investigated whether migration and cost-of-living changes insured workers against this large idiosyncratic variation in local shocks.

I find that large spatial variation in income declines remain after accounting for cost-of-living changes, so only migration could have insured workers. In geo-coded administrative panel data through 2011, I find that migration has diffused only 7% of local shocks across workers nationwide. These local shocks were likely place-based labor demand shocks that migration could in principle have diffused. Migration diffused the incidence of local shocks at a three-times-higher rate over the 2001 recession, driven entirely by more effective diffusion among above-average earners. A simple cross-sectional decomposition of migratory insurance shows that the distinctive feature of above-average-earner migration over the Great Recession was a lower observed benefit to moving to a stronger labor market—rather than a lower likelihood of moving to a stronger labor market. This suggests that the relative failure of migratory insurance over the Great Recession was due to unusually weak access to employment among migrants from heavily-shocked areas—rather than due to higher moving costs or to worse information about where to move.

For labor economics, the paper shows that place-specific labor demand shocks can lastingly affect specific people, not just places, in spite of high migration rates. For public economics, the results suggest that past location may be a powerful tag for directing social insurance. For urban economics, the results demonstrate an important case of medium-run spatial equilibrium failing to equalize real income changes across space. For macroeconomics, the paper shows that spatial adjustment frictions in the U.S. labor market can be large, with potential implications for cyclical policy.

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Appendix A: Alternative Expression of Migratory Insurance

Section V.B specified the following estimating equation for the degree of migratory insurance:

$$\Delta e_i = \alpha + \beta \Delta \bar{e}_{c(i,2006)} + \varepsilon_i \tag{9}$$

for the simple case of no control variables, where estimated migratory insurance equals $1-\hat{\beta}$. If no one migrated out of their 2006 CZ, $\hat{\beta}$ clearly equals one by construction of CZ shocks $\Delta \bar{e}_{c(i,2006)}$ as equal to the 2011 employment rate in $c(i,2006)$ minus the 2006 employment rate in $c(i,2006)$. Inter-CZ migration allows for migratory insurance, i.e. for employment changes to move less than one-for-one with CZ shocks. To facilitate intuition for the specific features of migration required for substantial migratory insurance, this appendix expresses estimated migratory insurance as a function of the full matrix of migrant and non-migrant employment rates.

Let $\pi_k^c \equiv E[c(i,2011) = k | c(i,2006) = c]$ equal the share of the c-cohort that lived in CZ k in 2011, and let $\bar{e}_{k2011}^c \equiv E[e_{i2011} | c(i,2011) = k, c(i,2006) = c]$ equal the 2011 employment rate of the share of the c-cohort that lived in CZ k in 2011. Each cohort's employment rate change $\Delta \bar{e}^c$ can be written as the identity:

$$\begin{aligned} \Delta \bar{e}^c &= \Delta \bar{e}_c + \sum_k \pi_k^c \left(\bar{e}_{k2011}^c - \bar{e}_{c2011} \right) \\ &\equiv \Delta \bar{e}_c + COHORTDEVIATION^c \end{aligned} \tag{10}$$

where $COHORTDEVIATION^c$ denotes the summation term and equals the deviation of the c-cohort's employment rate change from the shock to c. Consider the linear projection of $COHORTDEVIATION^c$ on $\Delta \bar{e}_c$:

$$COHORTDEVIATION^c = \alpha' + \beta' \Delta \bar{e}_c + \zeta_c \tag{11}$$

The estimated parameter $-\hat{\beta}'$ equals estimated migratory insurance $1-\hat{\beta}$. To see this, rewrite identity (10) as:

$$\Delta \bar{e}^c = \alpha' + \left(1 + \beta'\right) \Delta \bar{e}_c + \zeta_c$$

where $\hat{\beta} = 1 + \hat{\beta}'$ by construction.[39]

Expressing the degree of migratory insurance as in equation (11) makes explicit the features of migration that are necessary for cohort employment changes to be independent of CZ shocks. Consider a heavily-shocked CZ. Algebraically, migratory insurance requires some combination of large out-migration by out-migrants who experience relatively high 2011 employment rates $\left(\sum_{k:k\neq c} \pi_k^c \left(\bar{e}_{k2011}^c - \bar{e}_{c2011}\right) >> 0\right)$ and large in-migration of people who experience relatively low 2011 employment rates $\left(\pi_c^c \left(\bar{e}_{c2011}^c - \bar{e}_{c2011}\right) >> 0\right)$.[40] Appendix B formalizes these conditions in terms of the migration rate between heavily-shocked CZ's and lightly-shocked CZ's and of migrants' employment rates relative to their destination's employment rate.

[39] OLS estimation of individual-level regression (9) yields the same coefficients as OLS estimation of the cohort-level equation $\Delta \bar{e}^c = \alpha + \beta \Delta \bar{e}_c + \varepsilon_c$.

[40] If no one migrates into the heavily shocked CZ or if in-migrants have a higher 2011 employment rate than the CZ overall, then of course those who stay in the heavily shocked CZ must have a lower 2011 employment rate than the CZ overall ($\bar{e}_{c2011}^c - \bar{e}_{c2011} \leq 0$).

Appendix B: Necessary Conditions for Full Insurance

Appendix A expressed migratory insurance as a function of the full matrix of migrant and non-migrant employment rates. The resulting expression made clear that migratory insurance requires some combination of large out-migration from heavily-shocked CZ's by out-migrants who experience relatively high 2011 employment rates and large in-migration to heavily-shocked CZ's of people who experience relatively low 2011 employment rates. This appendix formalizes these algebraic conditions for a stylized economy that approximates the actual U.S. economy over the 2006-2011 period.

Divide arbitrarily many CZ's into two groups: CZ's that experienced a shock below the 2006-population-weighted median shock ("heavily-shocked CZ's") and CZ's that experienced a shock above 2006-population-weighted median shock ("lightly-shocked CZ's"). Assume that there are sufficiently many CZ's that these two groups have equal 2006 populations. For the rest of this appendix, classify the group of heavily-shocked CZ's *as a single CZ* called P ("Phoenix") and classify the group of lightly-shocked CZ's *as a single CZ* called S ("San Antonio"). Assume that P and S had equal 2006 employment rates (which Figure 3b indicates was approximately true empirically) and experienced no population change between 2006 and 2011 (which Figure 2b shows was approximately true empirically).

For simplicity and without loss of generality, assume that the 2006 employment rate in both P and S was 100%. Denote the 2011 employment rate in P as $\bar{e}_{P2011} = \mu - \phi > 0$, and denote the 2011 employment rate in S as $\bar{e}_{S2011} = \mu + \phi < 1$. Let π denote the share of each cohort that in 2011 does not reside in their 2006 CZ (i.e. the share of the P cohort that migrates to S and the share of S that migrates to P).[41] As in Appendix A, let $\bar{e}^{c}_{2011} \equiv E[e_{i2011}|c(i,2006) = c]$ denote the 2011 employment rate of the c-cohort, and let $\bar{e}^{c}_{k2011} \equiv E[e_{i2011}|c(i,2011) = k, c(i,2006) = c]$ denote the 2011 employment rate of the share of the c-cohort that lives in CZ k in 2011.

Three conditions must hold under any feasible state of the 2011 economy:

$$(1-\pi)\,\bar{e}^{P}_{P2011} + \pi\bar{e}^{S}_{P2011} = \mu - \phi \tag{12}$$

$$(1-\pi)\,\bar{e}^{S}_{S2011} + \pi\bar{e}^{P}_{S2011} = \mu + \phi \tag{13}$$

$$\bar{e}^{P}_{P2011}, \bar{e}^{S}_{P2011}, \bar{e}^{S}_{S2011}, \bar{e}^{P}_{S2011} \in [0,1] \tag{14}$$

where the first two conditions merely restate the 2011 employment rates of P and S in terms of the employment rates of stayers and in-migrants. Full migratory insurance requires that the P-cohort and the S-cohort have the same employment rate (equal to μ) in 2011:

$$(1-\pi)\,\bar{e}^{P}_{P2011} + \pi\bar{e}^{P}_{S2011} = (1-\pi)\,\bar{e}^{S}_{S2011} + \pi\bar{e}^{S}_{P2011} = \mu \tag{15}$$

These four conditions are sufficient to prove the following two necessary conditions for full insurance.

Proposition 1 ("minimum directed migration"): The condition $\pi \geq \phi$ is necessary for full insurance.

Proof: Suppose that $\pi < \phi$ is consistent with full insurance. For a given μ and ϕ, the P-cohort's 2011 employment rate is maximized when all migrants from P to S are employed in 2011 ($\bar{e}^{P}_{S2011} = 1$) and when all migrants from S to P are not employed in 2011 ($\bar{e}^{S}_{P2011} = 0$). Equation (12) and $\bar{e}^{S}_{P2011} = 0$ imply that the 2011 employment rate of non-migrating members of the P-cohort have employment rate $\bar{e}^{P}_{P2011} = (\mu - \phi)/(1-\pi)$. Thus the P-cohort has a maximum 2011 employment rate equal to:

$$\bar{e}^{P}_{2011} = \mu - \phi + \pi$$

For $\pi < \phi$, we have $\bar{e}^{P}_{2011} < \mu$, which is inconsistent with full insurance (15). □

[41] Because the two CZ's populations are equal in both 2006 and 2011, the migration rate from P to S equals the migration rate from S to P.

Proposition 2 ("minimum selective migration"): Suppose that $\pi < .5$. *Then* $\bar{e}^P_{S2011} - \bar{e}^S_{P2011} > \bar{e}_{S2011} - \bar{e}_{P2011}$ *is necessary for full insurance.*

Proof: By (12)-(14), any feasible state of the 2011 economy exhibits:

$$\pi\left(\bar{e}^P_{S2011} - \bar{e}^S_{P2011}\right) + (1-\pi)\left(\bar{e}^S_{S2011} - \bar{e}^P_{P2011}\right) = \bar{e}_{S2011} - \bar{e}_{P2011}$$

By full insurance (15), we further have:

$$2\pi\left(\bar{e}^P_{S2011} - \bar{e}^S_{P2011}\right) = \bar{e}_{S2011} - \bar{e}_{P2011}$$

For $\pi < .5$, we have $\bar{e}^P_{S2011} - \bar{e}^S_{P2011} > \bar{e}_{S2011} - \bar{e}_{P2011}$. $\square$

These necessary conditions for full insurance are straightforward and informative. First, full insurance requires migration between heavily-shocked CZ's and lightly-shocked CZ's to be sufficiently large. Put in terms of migration out of a continuum of individual CZ's, out-migration rates out of individual CZ's must be sufficiently large and this migration must be sufficiently directed in the sense that a sufficiently large share of out-migrants from heavily-shocked CZ's must migrate to lightly-shocked CZ's rather than to other heavily-shocked CZ's, and vice versa for out-migrants from lightly-shocked CZ's.

Second, full insurance when most people do not move requires migration to be sufficiently selective in the sense that migrants from heavily-shocked CZ's to lightly-shocked CZ's must be employed at especially high rates relative to migrants moving in the opposite direction. Intuitively, full insurance in this stylized example requires that the Phoenix and San Antonio cohorts have equal 2011 employment rates. When only a minority of people move, this requires that migrants from Phoenix to San Antonio must be employed in 2011 at a strictly higher rate than the average 2011 San Antonio resident, or that migrants from San Antonio to Phoenix must be employed at strictly lower rate than the average 2011 Phoenix resident, or both. Thus full insurance requires that, in the cross section, moving to a CZ with a one-percentage-point-higher 2011 employment rate is associated with a *greater-than-one*-percentage-point increase in one's own likelihood of being employed.

These necessary conditions motivate the migratory insurance decomposition of Section VIII.B.

TABLE 1
Variation in 2006-2011 Mean Income Changes across the United States

A. Across U.S. States

	Δ Mean personal wage income	Δ Mean family wage income	Δ Mean total post-transfer family income	Δ Mean total post-transfer family income, deflated by Local CPI 2	Δ Mean total post-transfer family income and food stamps, deflated by Local CPI 2
	(1)	(2)	(3)	(4)	(5)
90-10 spread / 2006 mean level	12.4%	12.2%	9.8%	9.0%	8.7%
75-25 spread / 2006 mean level	4.7%	4.2%	4.5%	2.5%	2.2%
95-5 spread / 2006 mean level	14.4%	13.0%	10.6%	9.4%	9.0%
Standard deviation / 2006 mean level	4.7%	4.2%	3.6%	2.9%	2.8%

B. Across Commuting Zones

	Δ Mean personal wage income	Δ Mean family wage income	Δ Mean total post-transfer family income	Δ Mean total post-transfer family income, deflated by Local CPI 2	Δ Mean total post-transfer family income and food stamps, deflated by Local CPI 2
	(6)	(7)	(8)	(9)	(10)
90-10 spread / 2006 mean level	13.9%	12.9%	11.8%	11.2%	10.9%
75-25 spread / 2006 mean level	6.3%	6.0%	5.6%	4.5%	4.5%
95-5 spread / 2006 mean level	18.1%	17.2%	14.8%	14.1%	14.3%
Standard deviation / 2006 mean level	5.7%	5.3%	4.6%	4.5%	4.5%

Notes - This table uses the 2006 and 2011 American Community Surveys (ACS) and a local price deflator to report statistics on the spatial variance in 2006-2011 mean income declines. As in subsequent analyses, the sample is restricted to males aged 25-59 in 2006 (30-64 in 2011) living in the continental United States. Panel A reports statistics at the state level; panel B reports statistics at the Commuting Zone (CZ) level. CZ's are aggregations of counties designed to approximate local labor markets based on commuting patterns in the 1990 Census (Tolbert and Sizer 1996). The continental United States comprises 722 commuting zones, and every spot in the United States lies in exactly one CZ. The income variables are taken directly from the ACS. Changes are defined as the 2011 mean minus the 2006 mean. Local CPI 2 is the more aggressive of Moretti's (2012) local consumer price indices and accounts for spatial differences in housing rent levels as well as the dependence of local non-housing costs on local real estate prices. For panel A, food stamps equals the total cash value of Supplemental Nutrition Assistance Program transfers to residents of the given state as reported by the U.S. Department of Agriculture; for panel B, food stamps equals the total cash value allocated to CZ's as described in the text. Distributional statistics are reported as percentages of the 2006 mean. The X/Y spread is defined as the population-weighted X^{th} percentile of the distribution of the variable listed in the column heading, minus the population-weighted Y^{th} percentile.

TABLE 2
Summary Statistics of Panel Data Analysis Sample

A. Outcomes

	2006 mean	2011 mean
	(1)	(2)
Wage earnings	$45,438	$42,469
Employment (wage earnings >$15k)	73.0%	65.9%
Locally-deflated employment (locally deflated wage earnings >$15k)	72.8%	65.8%
Any employment (wage earnings >$0)	84.5%	78.1%

B. Characteristics (defined as of 2006)

	Share of sample
	(3)
Age	
25-29	13.4%
30-39	27.6%
40-49	31.6%
50-59	27.4%
Number of kids	
0	54.9%
1	17.6%
2+	27.5%
Married	58.5%
Home ownership	58.9%
Number of observations	60,312,920

Notes - This table lists summary statistics for the paper's main sample, drawn from the universe of U.S. tax returns: all males aged 25-59 in 2006 with a continental U.S. ZIP code in years 2006 and 2011. Wage earnings equals the sum of wage earnings reported by employers on all Form W-2's issued on the individual's behalf in the given year, inflated to 2010 dollars. Those without a W-2 are assigned zero wage earnings. I cap wage earnings at $200,000. Locally deflated wage earnings equals wage earnings deflated by Moretti's (2012) Local CPI 2 to account for local cost-of-living differences as detailed in Section III. ZIP code is drawn from W-2 forms and from the 28 other information returns filed automatically by institutions (e.g. employers, mortgage servicers, the Social Security Disability Insurance administration). I estimate that I locate over 90% of middle-aged males, based on comparisons to the American Community Survey. Number of kids equals the number of child exemptions claimed on the individual's 2006 Form 1040; I set this variable to zero if the individual did not file a Form 1040 in 2006. Married equals one if and only if the individual filed a 2006 Form 1040 with married-filing-jointly or married-filed-separately status. Home ownership equals one if and only if the individual had a Form 1098 mortgage interest information return filed on his or his wife's behalf by a mortgage servicer (where wife is identified through the individual's 2006 Form 1040) or if the individual claimed the mortgage interest deduction or property tax deduction on his 2006 Form 1040 Schedule A.

TABLE 3
Incidence of Great Recession Local Employment and Income Shocks
With and Without Instruments

Individual-level outcome:	Δ Employment (wages >$15K definition)	Δ Employment (wages >$0 definition)	Δ Employment (wages > locally deflated $15K definition)	Δ Wage earnings
	(1)	(2)	(3)	(4)
Instrument				
None	0.934	0.882	0.905	0.979
	(0.026)	(0.019)	(0.022)	(0.040)
Δ Outcome per capita in individual's birth state	0.944	0.983	1.001	1.020
	(0.019)	(0.013)	(0.017)	(0.078)
2006-2011 Bartik shock	0.827	0.923	0.925	0.800
	(0.019)	(0.012)	(0.015)	(0.045)
2006 debt-to-income ratio	1.092	0.956	0.930	1.327
	(0.071)	(0.055)	(0.067)	(0.157)
All instruments together	0.982	0.969	0.982	1.037
	(0.029)	(0.032)	(0.029)	(0.052)
N (people)	60,312,920	60,312,920	60,312,920	60,312,920
Clusters (CZs)	722	722	722	722

Notes - Each cell of this table reports an estimate from a separate regression of the share of the average CZ shock borne by the 2006 residents (the "cohort") of the CZ, rather than diffused across cohorts nationwide. See the notes to Table 1 for the definition of CZ's. For a given outcome, I define the CZ shock as the 2011 outcome mean among the CZ's 2011 residents, minus the 2006 outcome mean among the CZ's 2006 residents. I then regress 2006-2011 changes in individual-level outcomes on the shock to the individual's *2006* CZ. The resulting coefficient equals the incidence of the average CZ shock on the CZ's cohort. CZ-cohort outcomes can differ from CZ outcomes only through migration, so I define the degree of migratory insurance as 100% minus this estimated incidence rate. The second row uses Great Recession shocks to the individual's state of birth to instrument for the individual's CZ shock. The third row uses 2006-2011 employment changes predicted by nationwide changes in each CZ's 2006 industries as in Bartik (1991). The fourth row uses the CZ's 2006 ratio of aggregate household debt to aggregate Form 1040 adjusted gross income as in Mian and Sufi (2013). The fifth row uses all three instruments together.

TABLE 4
Incidence of Great Recession Local Employment Shocks
With and Without Controls

Outcome:	Individual's 2006-2011 change in employment status: {-1,0,1}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ employment rate in individual's 2006 CZ	0.884	0.934	0.941	0.960	0.992	0.948	0.953	0.928
	(0.026)	(0.026)	(0.056)	(0.038)	(0.031)	(0.168)	(0.167)	(0.029)
Controls								
Age		X	X	X	X	X	X	X
2006 wage earnings			X	X	X	X	X	X
2006 personal demographics				X	X	X	X	X
2006 CZ demographics					X	X	X	X
2006 employer FEs						X	X	
2006 employer FEs × 2006 wage earnings							X	
2008-2011 UI durations								X
N (people)	60,312,920	60,312,920	60,312,920	60,312,920	60,312,920	3,097,279	3,097,279	60,312,920
Clusters (CZs)	722	722	722	722	722	722	722	722

Notes - This table displays incidence rates of CZ shocks on CZ-cohorts; see Tables 2-3 for definitions and the basic specification. The rows indicate the controls used in each regression. The age control is implemented by reweighting the sample on two-year age bins across CZ's based on the full-sample age distribution. The personal demographic controls comprise indicators for marriage, home ownership, and having zero, one, or two-or-more children, respectively, as of 2006. The wage earnings controls comprise indicators for $15,000-wide wage earnings bins, where all individuals with wage earnings above $90,000 are combined into a single bin. Employer fixed effects comprise fixed effects for the individual's employer as listed on the individual's highest-paying 2006 W-2. The specifications using employer fixed effects are confined to the subsample of men who worked for a firm in 2006 that employed at least ten men in at least ten CZ's, excluding men who in 2006 lived in the CZ of the firm's corporate headquarters as listed on the firm's corporate tax return. The UI duration controls comprise quartics in the average annual maximum unemployment insurance duration available in the state of the individual's 2006 CZ, for each year 2008-2011.

TABLE 5
Incidence of Great Recession Local Employment Shocks
Split by Geographic Shock Concentration

A. 250-mile Concentration Radius

Concentration measure:	Relative		Absolute	
Threshold:	Median	Quartile	Median	Quartile
	(1)	(2)	(3)	(4)
CZ shock	0.932	0.932	0.931	0.930
	(0.030)	(0.030)	(0.029)	(0.030)
CZ shock × Surrounded by differently-shocked CZ's	-0.013	-0.016	-0.012	-0.006
	(0.014)	(0.020)	(0.014)	(0.016)
N (people)	31,046,664	31,046,664	31,046,664	31,046,664
Clusters (CZs)	449	449	449	449

B. 500-mile Concentration Radius

Concentration measure:	Relative		Absolute	
Threshold:	Median	Quartile	Median	Quartile
	(5)	(6)	(7)	(8)
CZ shock	0.926	0.932	0.926	0.929
	(0.032)	(0.030)	(0.031)	(0.030)
CZ shock × Surrounded by differently-shocked CZ's	0.014	-0.017	0.015	-0.001
	(0.024)	(0.018)	(0.024)	(0.014)
N (people)	31,046,664	31,046,664	31,046,664	31,046,664
Clusters (CZs)	449	449	449	449

Notes - This table reports results of tests for significantly different incidence rates between cohorts of "isolated" CZ's (CZ's that were surrounded by differently shocked CZ's) and cohorts of other CZ's. The regressions follow the specification underlying Table 4 column 2 except that an interaction term between CZ shocks and an indicator for isolation is included, and the sample is confined to cohorts of CZ's in the population-weighted top quartile or bottom quartile of CZ shocks. In all regressions, a given CZ's isolation is a function of its "neighbor shock": the population-weighted mean shock to the CZ's within a given radius (250 miles in panel A, 500 miles in panel B) of the given CZ's geographic centroid. In columns 1 and 5, a top-quartile (bottom-quartile) CZ is classified as isolated if and only if its neighbor shock was below (above) the population-weighted median neighbor shock. Columns 2 and 6 use the same definition except with reference to the population-weighted 75^{th} percentile (25^{th} percentile) neighbor shock. The remaining columns adopt these definitions, respectively, except that the percentiles refer to the CZ shock distribution rather than the neighbor shock distribution. See Appendix Table 2 for results from regressions that allow intercepts to vary by CZ isolation.

TABLE 6
Incidence of 2001 Recession Local Employment Shocks
With and Without Controls

Outcome:	Individual's 2005-2000 change in employment status: {-1,0,1}				
	(1)	(2)	(3)	(4)	(5)
Δ employment rate in individual's 2000 CZ	0.938	1.021	0.649	0.663	0.757
	(0.040)	(0.042)	(0.079)	(0.058)	(0.055)
Controls					
Age		X	X	X	X
2000 wage earnings			X	X	X
2000 personal demographics				X	X
2000 CZ demographics					X
N (people)	58,159,472	58,159,472	58,159,472	58,159,472	58,159,472
Clusters (CZs)	722	722	722	722	722

Notes - This table replicates columns 1-5 of Table 4 for the 2001 recession. The sample is defined analogously to the main sample: males aged 25-59 in 2000 who possess a continental U.S. ZIP code in 2000 and 2005. See the notes to Table 4 for additional details.

APPENDIX TABLE 1
Incidence of Great Recession Local Shocks
With and Without Controls – Alternative Labor Market Outcomes

A. Locally-deflated Employment

Outcome:	Individual's 2006-2011 change in locally-deflated employment status: {-1,0,1}							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ locally-deflated employment rate in individual's 2006 CZ	0.871	0.885	0.890	0.927	0.950	1.100	1.102	0.891
	(0.021)	(0.028)	(0.048)	(0.031)	(0.028)	(0.182)	(0.182)	(0.027)
Controls								
Age		X	X	X	X	X	X	X
2006 wage earnings			X	X	X	X	X	X
2006 personal demographics				X	X	X	X	X
2006 CZ demographics					X	X	X	X
2006 employer FEs						X	X	
2006 employer FEs × 2006 wage earnings							X	
2008-2011 UI durations								X
N (people)	60,312,920	60,312,920	60,312,920	60,312,920	60,312,920	3,097,279	3,097,279	60,312,920
Clusters (CZs)	722	722	722	722	722	722	722	722

B. Wage Earnings

Outcome:	Individual's 2006-2011 change in wage earnings							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Δ mean wage earnings in individual's 2006 CZ	0.960	0.965	0.973	0.988	0.993	0.914	0.947	0.968
	(0.039)	(0.047)	(0.053)	(0.059)	(0.038)	(0.188)	(0.189)	(0.033)
Controls								
Age		X	X	X	X	X	X	X
2006 wage earnings			X	X	X	X	X	X
2006 personal demographics				X	X	X	X	X
2006 CZ demographics					X	X	X	X
2006 employer FEs						X	X	
2006 employer FEs × 2006 wage earnings							X	
2008-2011 UI durations								X
N (people)	60,312,920	60,312,920	60,312,920	60,312,920	60,312,920	3,097,279	3,097,279	60,312,920
Clusters (CZs)	722	722	722	722	722	722	722	722

Notes - This table replicates Table 4 for the outcomes of locally-deflated employment and wage earnings. See the notes to Table 4 for specification details. See the notes to Table 2 for outcome definitions.

APPENDIX TABLE 2
Incidence of Great Recession Local Employment Shocks
Split by Geographic Shock Concentration – Allowing Intercepts to Vary

A. 250-mile Concentration Radius

Concentration measure:	Absolute		Relative	
Threshold:	Median	Quartile	Median	Quartile
	(1)	(2)	(3)	(4)
CZ shock	0.927	0.928	0.929	0.930
	(0.032)	(0.031)	(0.032)	(0.031)
Surrounded by differently-shocked CZ's	0.368	0.782	0.328	0.653
	(0.412)	(0.551)	(0.465)	(0.554)
CZ shock × Surrounded by differently-shocked CZ's	0.028	0.066	0.023	0.046
	(0.045)	(0.055)	(0.050)	(0.060)
N (people)	31,046,664	31,046,664	31,046,664	31,046,664
Clusters (CZs)	449	449	449	449

B. 500-mile Concentration Radius

Concentration measure:	Absolute		Relative	
Threshold:	Median	Quartile	Median	Quartile
	(5)	(6)	(7)	(8)
CZ shock	0.959	0.928	0.958	0.931
	(0.024)	(0.031)	(0.024)	(0.031)
Surrounded by differently-shocked CZ's	-1.194	1.032	-1.172	0.356
	(0.739)	(0.741)	(0.756)	(0.449)
CZ shock × Surrounded by differently-shocked CZ's	-0.136	0.095	-0.133	0.019
	(0.082)	(0.066)	(0.083)	(0.052)
N (people)	31,046,664	31,046,664	31,046,664	31,046,664
Clusters (CZs)	449	449	449	449

Notes - This table replicates Table 5 while allowing isolated CZ's to have a different intercept in each underlying regression. See the notes to that table for details.

APPENDIX TABLE 3
Incidence of 2001 Recession Local Shocks
With and Without Controls – Alternative Labor Market Outcomes

A. Locally-deflated Employment

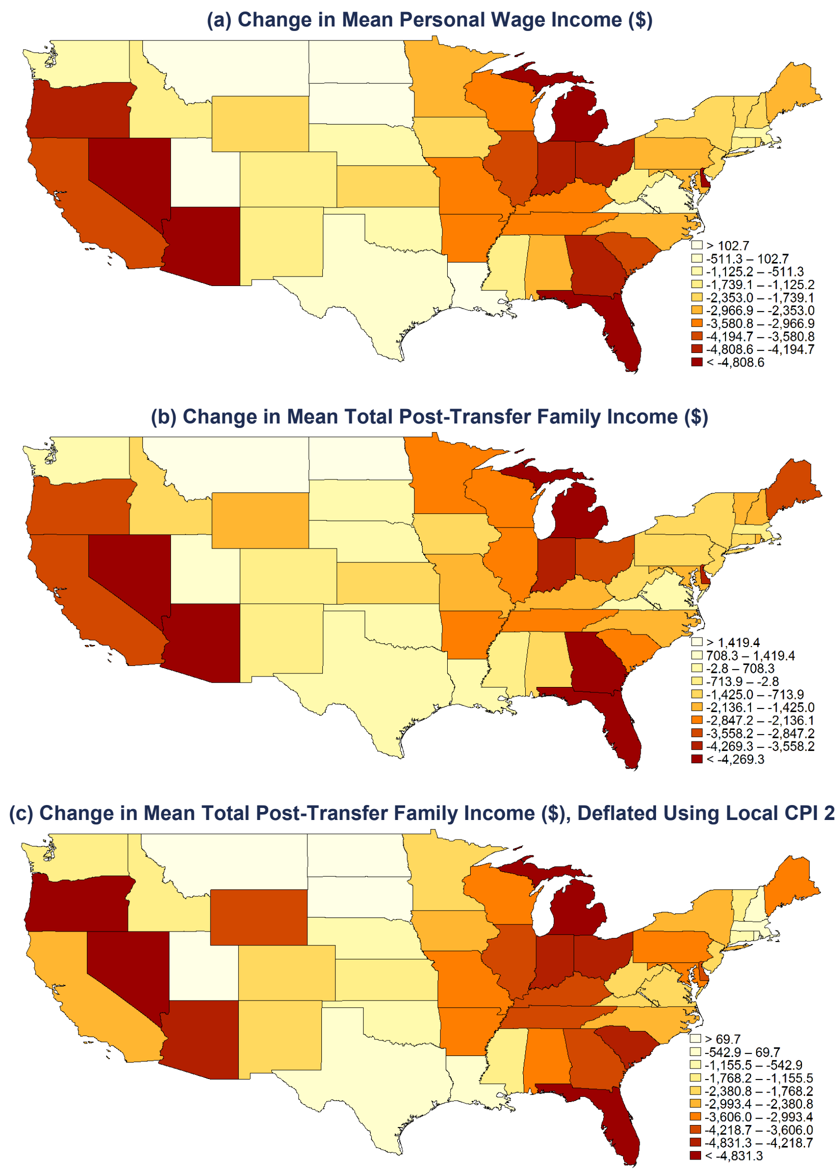
Outcome:	Individual's 2000-2005 change in locally-deflated employment status: {-1,0,1}				
	(1)	(2)	(3)	(4)	(5)
Δ locally-deflated employment rate in individual's 2000 CZ	0.954	1.008	0.677	0.700	0.754
	(0.032)	(0.032)	(0.065)	(0.049)	(0.052)
Controls					
Age		X	X	X	X
2000 wage earnings			X	X	X
2000 personal demographics				X	X
2000 CZ demographics					X
N (people)	58,159,472	58,159,472	58,159,472	58,159,472	58,159,472
Clusters (CZs)	722	722	722	722	722

B. Wage Earnings

Outcome:	Individual's 2000-2005 change in wage earnings				
	(6)	(7)	(8)	(9)	(10)
Δ mean wage earnings in individual's 2000 CZ	0.974	0.987	0.732	0.739	0.749
	(0.057)	(0.069)	(0.063)	(0.072)	(0.039)
Controls					
Age		X	X	X	X
2000 wage earnings			X	X	X
2000 personal demographics				X	X
2000 CZ demographics					X
N (people)	58,159,472	58,159,472	58,159,472	58,159,472	58,159,472
Clusters (CZs)	722	722	722	722	722

Notes - This table replicates Table 6 for the outcomes of locally-deflated employment and wage earnings. See the notes Table 6 for specification details. See the notes to Table 2 for outcome definitions.

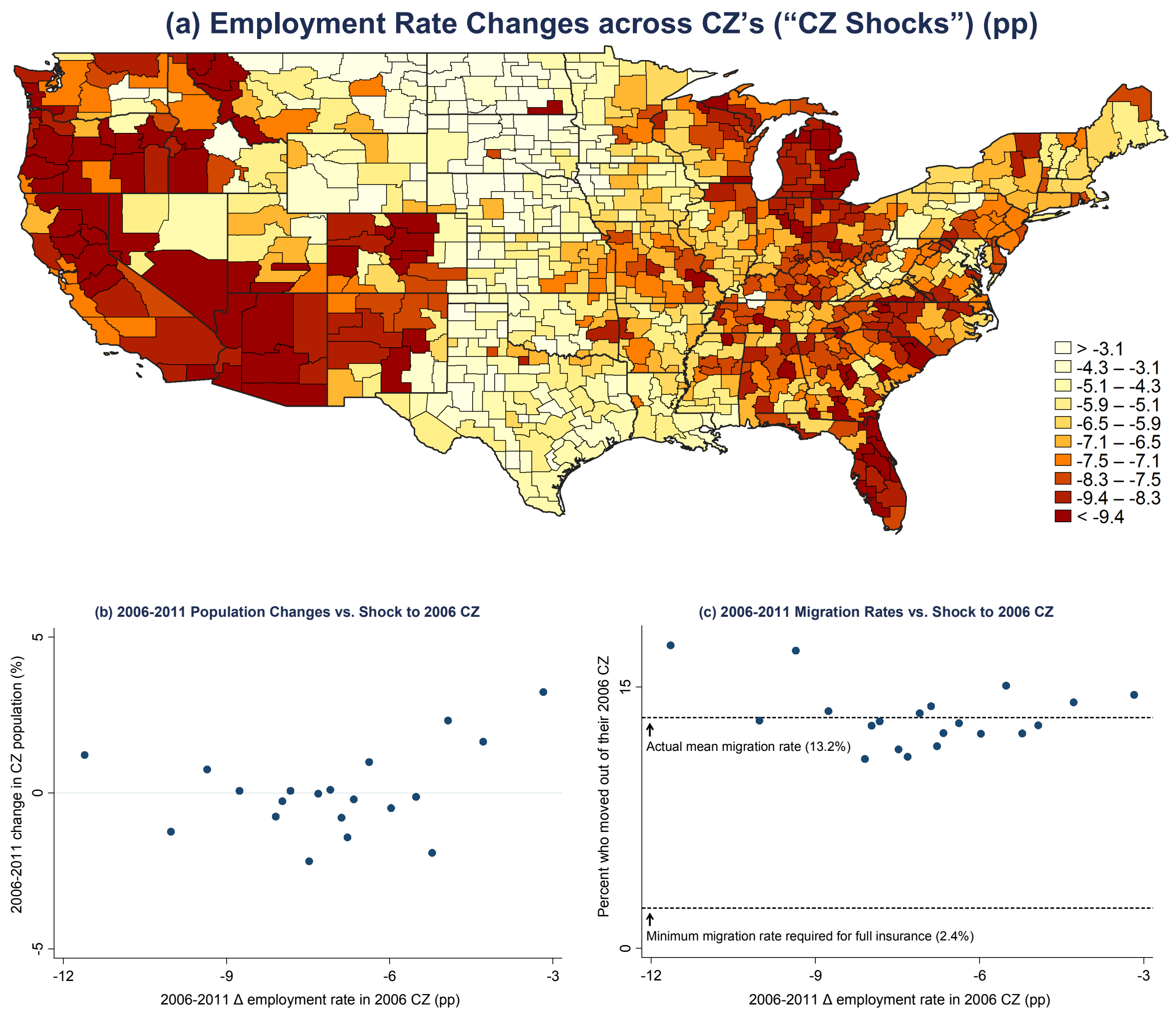
FIGURE 1
Variation in 2006-2011 Mean Income Changes across the United States



Notes: This graph presents evidence on whether government transfers and local rent changes equalized 2006-2011 real income declines across space. Each panel displays state-level changes in mean income between 2006 and 2011 using the American Community Survey, restricted to males living in the continental United States and aged 25-59 in 2006 (30-64 in 2011) as in subsequent analyses using panel data. Changes are defined as the 2011 mean minus the 2006 mean. Panel C deflates post-transfer family income using Local CPI 2, the more aggressive of the two local consumer price indices used by Moretti (2012). The difference between the population-weighted 90^{th} percentile change across states and the 10^{th} percentile change equals 12.4% of the 2006 mean in panel A, 9.8% of the 2006 mean in panel B, and 9.0% of the 2006 mean in Panel C. See Table 1 for additional statistics.

FIGURE 2

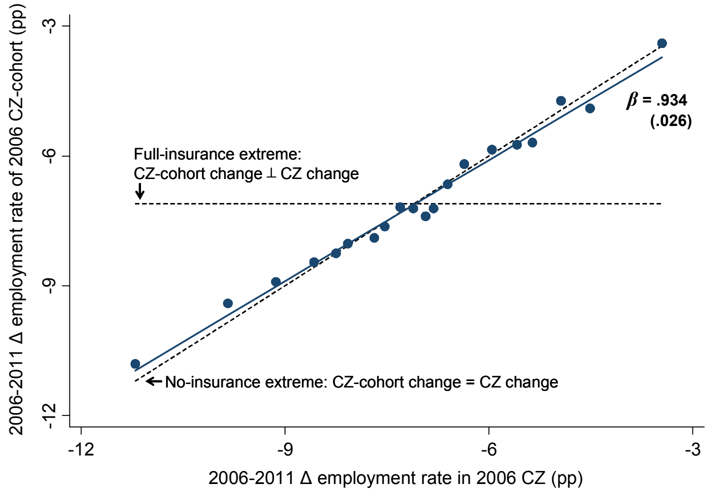
2006-2011 Employment Rate Changes, Population Changes, and Migration Rates across Commuting Zones



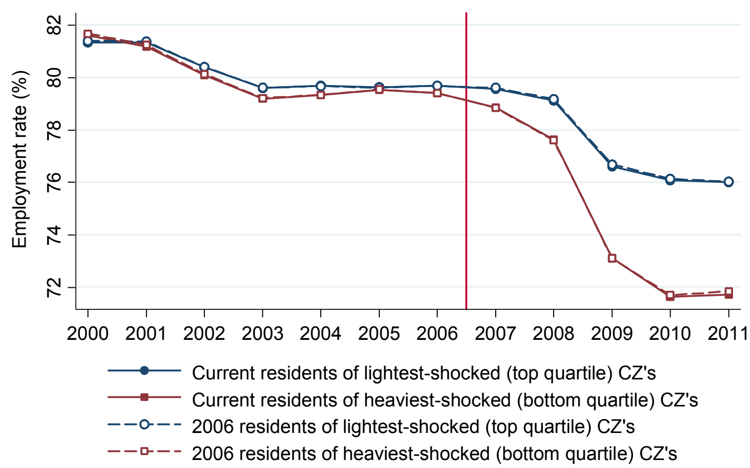
Notes: Panel A displays CZ-level employment changes ("CZ shocks") in this paper's main balanced panel sample: 60.3 million males living in the continental United States in 2006 and 2011 and aged 25-59 in 2006. Commuting Zones (CZ's) are aggregations of counties designed to approximate local labor markets based on commuting patterns in the 1990 Census (Tolbert and Sizer 1996); the continental United States comprises 722 CZ's, and every spot in the United States lies in exactly one CZ. The employment rate change of a given CZ equals the employment rate of the CZ's 2011 residents minus the employment rate of the CZ's 2006 residents. The difference between the population-weighted 90^{th} percentile employment change and the 10^{th} percentile employment change across CZ's was 4.8 percentage points. Panel B displays CZ population changes versus CZ shocks. To construct this binned scatterplot, I bin CZ's into twenty equal-sized groups (five-percentile-point bins) of employment rate changes weighted by 2006 population and then plot the 2006-population-weighted mean population change in each bin. Because population changes averaged approximately zero across CZ's, migratory insurance through the bulk of the CZ shock range required 2006-2011 inter-CZ migration rates of at least $2.4\% = (4.8\%/2)$. Panel C plots actual migration rates (the share of a CZ's 2006 residents who lived in a different CZ in 2011) versus CZ shocks using the method of panel B.

FIGURE 3
Incidence of CZ Employment Shocks on CZ-Cohorts

(a) CZ-Cohort Employment Changes vs. CZ Employment Shocks

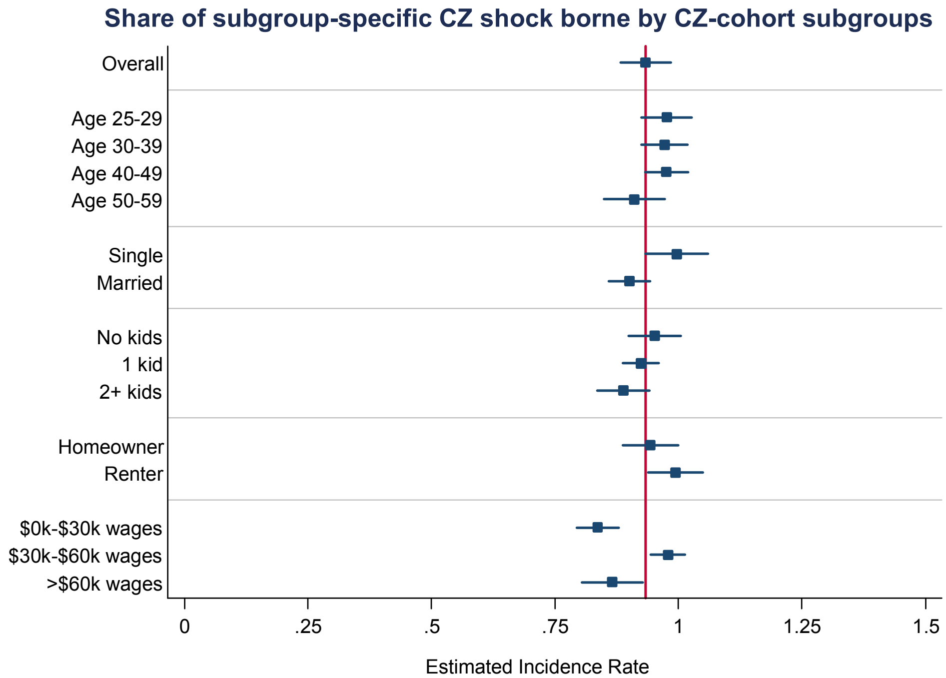


(b) Employment Rates across CZ's and CZ-Cohorts
CZ's split by 2006-2011 employment rate change. Males aged 35-49 in 2006.



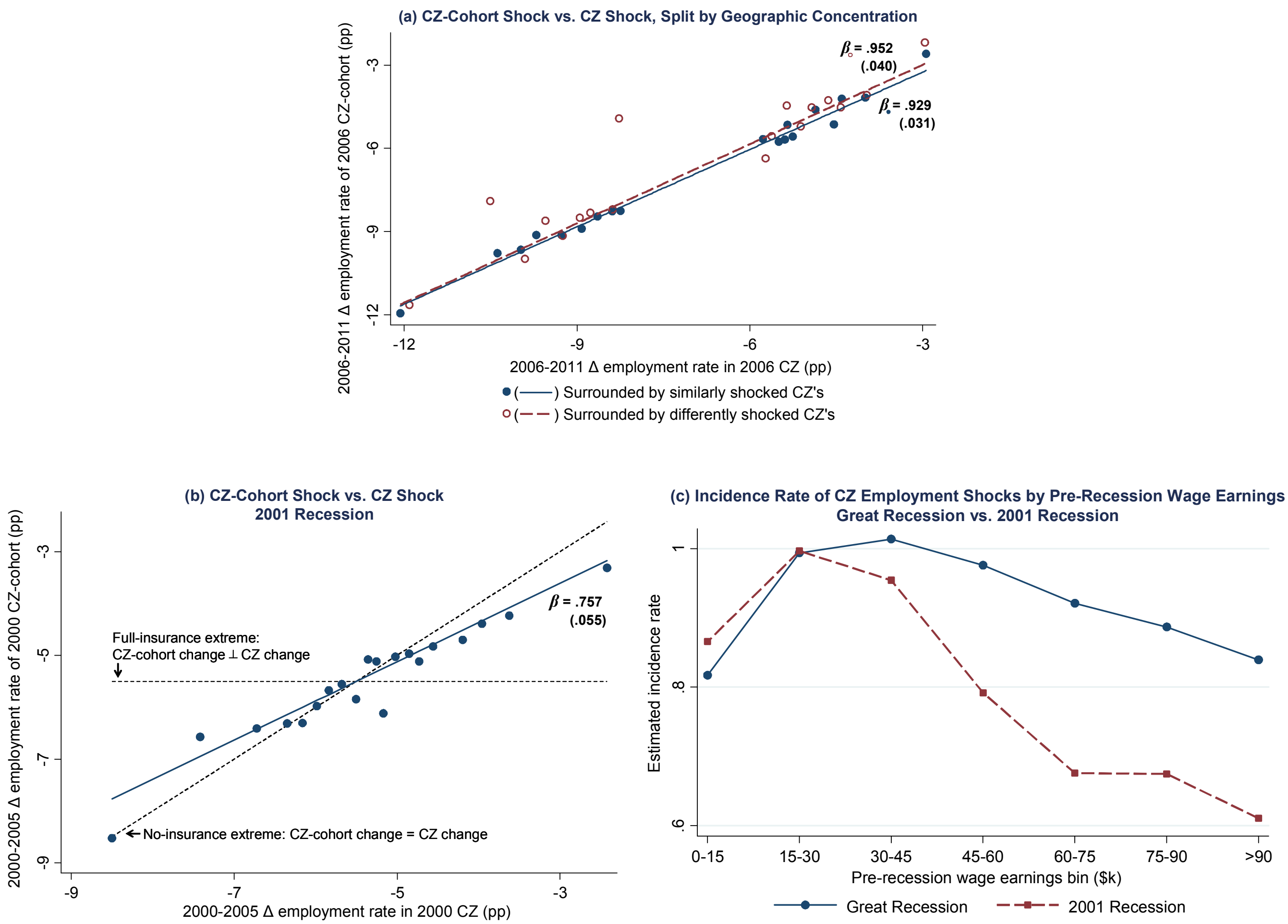
Notes: Panel A plots the 2006-2011 employment rate change of CZ-cohorts versus CZ shocks for the main analysis sample. See Figure 2 for the details on the CZ local area concept and definition of CZ shocks. A CZ's cohort is defined as the set of people who lived in the CZ in 2006, regardless of where they lived in other years. The data are binned as in Figure 2b. The estimated incidence rate—the share of the average CZ shock borne by the CZ's cohort relative to the mean—is overlaid on the scatter plot with a standard error clustered at the cohort level. This incidence rate equals the share of the shock not diffused by migration, so I define the degree of migratory insurance as one minus the incidence rate. Panel B plots the 2000-2011 time series of employment rates of CZ's and of CZ-cohorts among males aged 35-49 in 2006 who lived in the continental United States in all years 2000-2011. The blue lines (circular markers) denote the population-weighted top quartile of CZ's that experienced the smallest 2006-2011 employment declines, while the red lines (square markers) denote the bottom quartile that experienced the largest declines. The solid lines (filled markers) refer to current residents of the given CZ group, while the dashed lines (hollowed markers) refer to CZ-cohorts. Each CZ-cohort series lies nearly on top of its respective CZ series.

FIGURE 4
Incidence Rate Heterogeneity



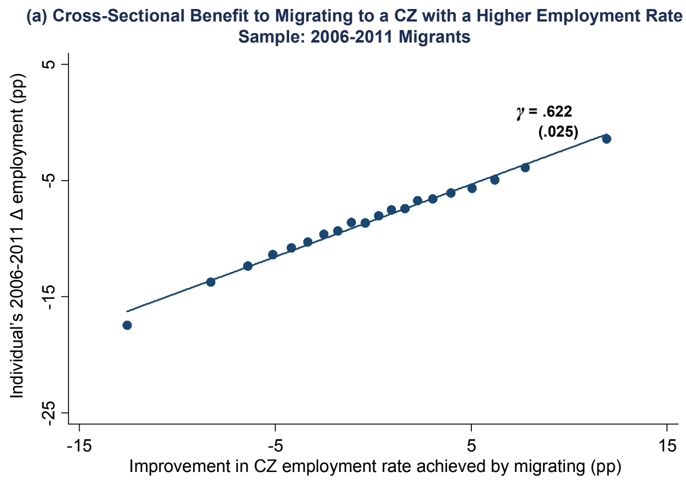
Notes: This figure plots demographic-subgroup-specific incidence estimates and 95% confidence intervals clustered at the CZ-cohort level. The first row reports the overall estimate illustrated in Figure 3a. Each remaining row reports the estimate from a regression of individual-level employment changes among the subgroup (defined as of 2006) denoted in the row on the employment rate change of that demographic subgroup in the individual's 2006 CZ; this specification isolates the differential ability of demographic subgroups to diffuse local employment shocks via migration. For example, the coefficient estimate displayed in the "Age 25-29" row is derived from a regression on the subsample of men who were aged 25-29 in 2006 and in which the CZ shock regressor equals the 2011 employment rate in the individual's 2006 CZ among people who were aged 30-34 in 2011, minus the 2006 employment rate in the individual's 2006 CZ among people who were aged 25-29 in 2006. See Appendix Figure 2 for an illustration of the non-parametric relationships underlying selected estimates plotted here, as well as for results from an alternative specification.

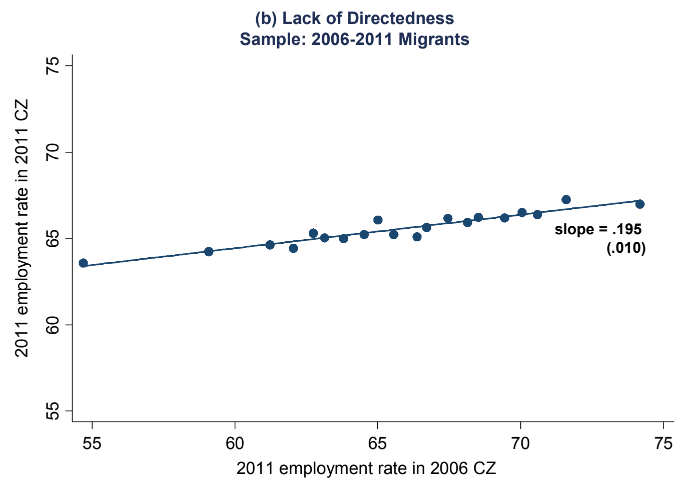
.FIGURE 5
Incidence Rates by Geographic Shock Concentration and over the 2001 Recession



Notes: Panel A investigates whether the overall incidence rate over the Great Recession (displayed in Figure 3a) is significantly smaller among CZ's surrounded by differently shocked CZ's. To construct the graph, I compute a neighbor shock value for each CZ *c*, equal to the population-weighted mean CZ shock among all CZ's whose geographic centroid lies within 250 miles of *c*'s geographic centroid. I then restrict the sample to CZ's in the population-weighted top quartile or bottom quartile of CZ shocks. Finally I define a top-quartile (bottom-quartile) CZ as having been surrounded by differently shocked CZ's if and only if its neighbor shock was below (above) the population-weighted median. The data are binned as in Figure 2b. Panel B replicates Figure 3a for the 2001 recession using the same sampling restrictions except replacing years 2006 and 2011 with years 2000 and 2005 and also displaying values net of a full set of personal and CZ controls (see Table 4 column 5)—the most quantitatively important of which is pre-recession wage earnings. Panel C plots incidence rates by pre-recession wage earnings bin for both the 2001 recession and the Great Recession, estimated separately for each subgroup as in Figure 4.

.FIGURE 6
Non-Parametric Illustration of the Migratory Insurance Decomposition

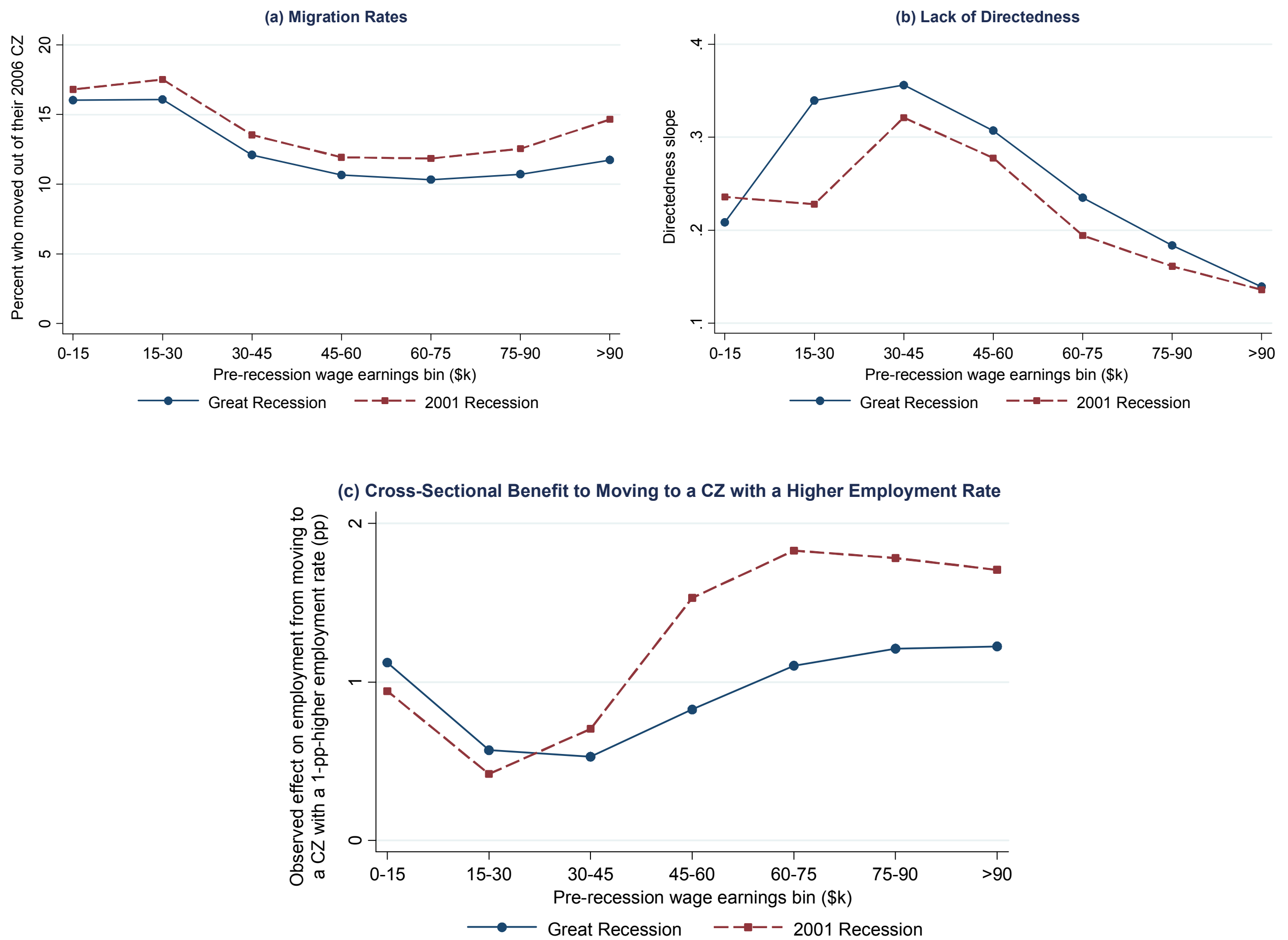




Notes: Section VII decomposes migratory insurance into the product of the migration rate, migration directedness (the degree to which movers from heavily-depressed areas moved to less-depressed areas and vice versa), and the cross-sectional benefit to moving to a less-depressed CZ. The migration rate is easily estimated and displayed in Figure 2c. Panel A non-parametrically illustrates the cross-sectional benefit to moving to a less-depressed CZ among those who moved between 2006 and 2011. It plots individual 2006-2011 employment changes versus the percentage-point improvement in CZ employment rate that the individual achieved by moving—equal to the 2011 employment rate of the individual's 2011 CZ minus the 2011 employment rate of the individual's 2006 CZ—residualized by the individual's CZ shock (defined in Figure 2a). The binned scatterplot is constructed as in Figure 2b. The displayed best-fit regression line equals the cross-sectional benefit to moving (γ in equation 4) among 2006-2011 migrants; this estimate is slightly larger than the corresponding estimate for the full sample (.607). Panel B displays the lack-of-directedness measure that I use to compactly describe migration directedness. It plots the 2011 employment rate of migrants' 2011 CZ versus the 2011 employment rate of migrants' 2006 CZ. Fully directed migration (out-migrants migrating from the most-depressed CZ to the least-depressed CZ and vice versa) would have generated a slope of -1, while fully undirected migration (all out-migrants migrating to an equally depressed CZ) would have generated a slope of 1. The actual slope is therefore a measure of the lack of directedness.

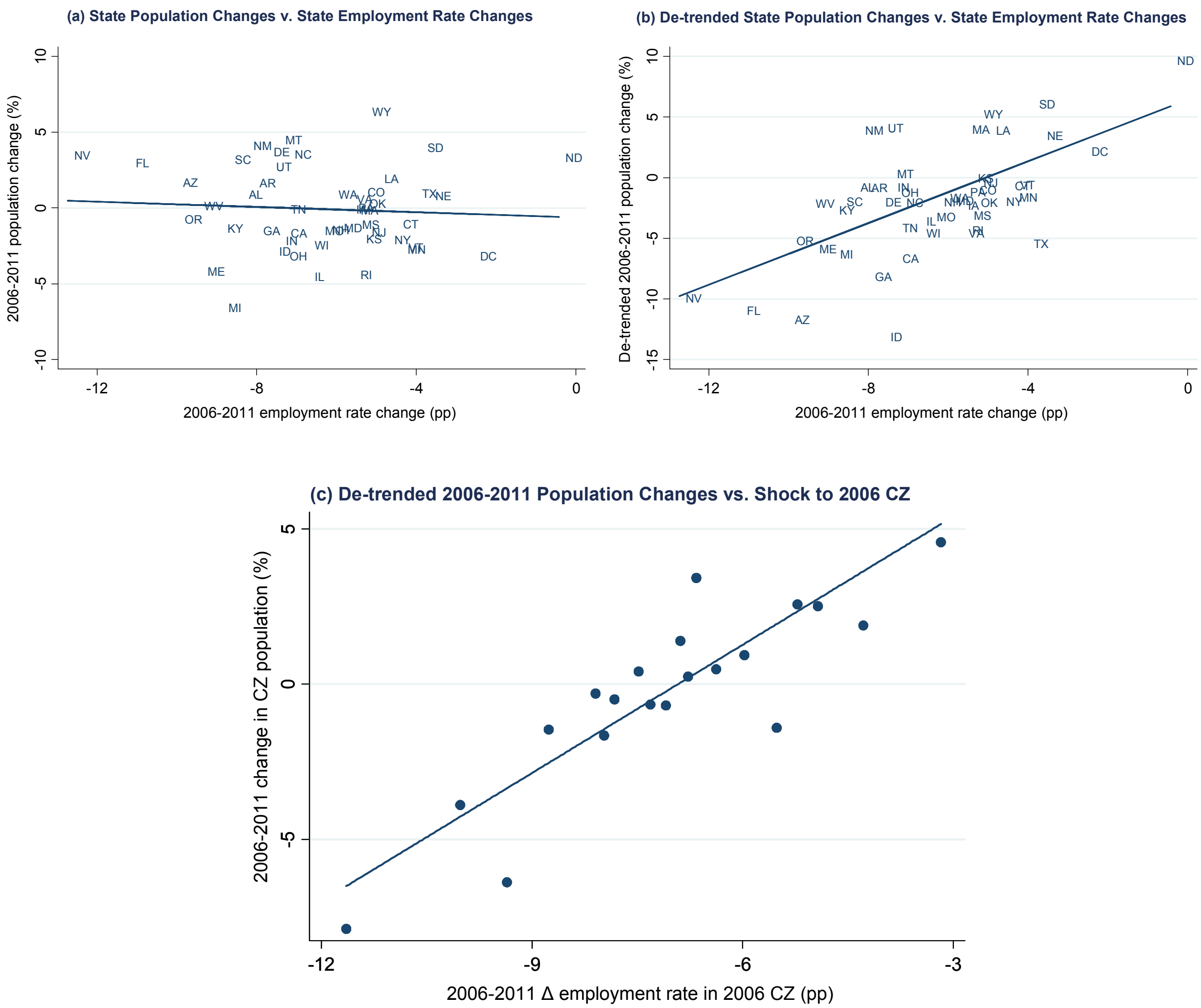
FIGURE 7

Decomposition of Migratory Insurance by Recession and Income Level



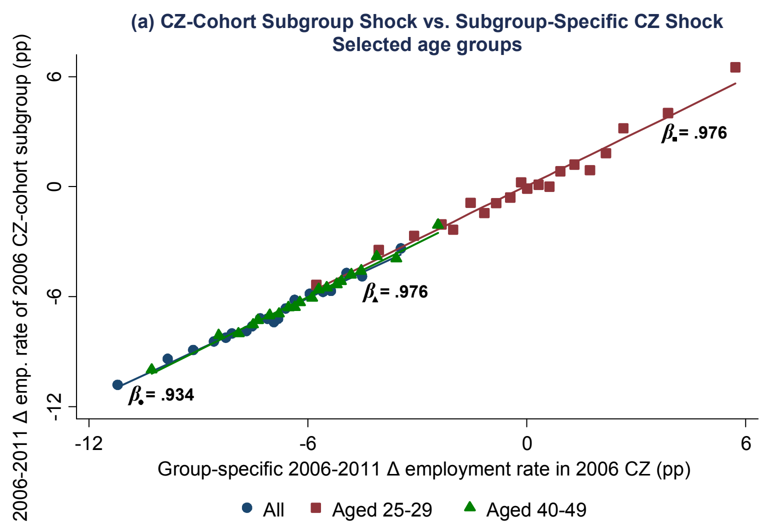
Notes: Figure 5c demonstrated that migratory insurance was substantially greater over the 2001 recession than over the Great Recession, driven by greater migratory insurance among above-average earners. This figure decomposes migratory insurance by recession and wage earnings bin in order to provide suggestive evidence on which type of friction explains the decline in migratory insurance among above-average earners. Specifically, Section VII decomposes migratory insurance into the product of the migration rate, migration directedness (the degree to which movers from heavily-depressed areas moved to less-depressed areas and vice versa), and the cross-sectional benefit to moving to a less-depressed CZ. These three components of migratory insurance are related, respectively, to three potential migratory insurance frictions: moving costs, weak information on far-away employment conditions, and weak employment access upon moving to a stronger labor market. Panels A-C respectively plot these components by recession and by pre-recession wage earnings bin. Panel A plots migration rates (the share of a CZ's 2006 residents who lived in a different CZ in 2011). Panel B plots the lack-of-directedness measure, illustrated and detailed in Figure 6b. Panel C plots the cross-sectional benefit to moving to a less-depressed CZ, illustrated and detailed in Figure 6a.

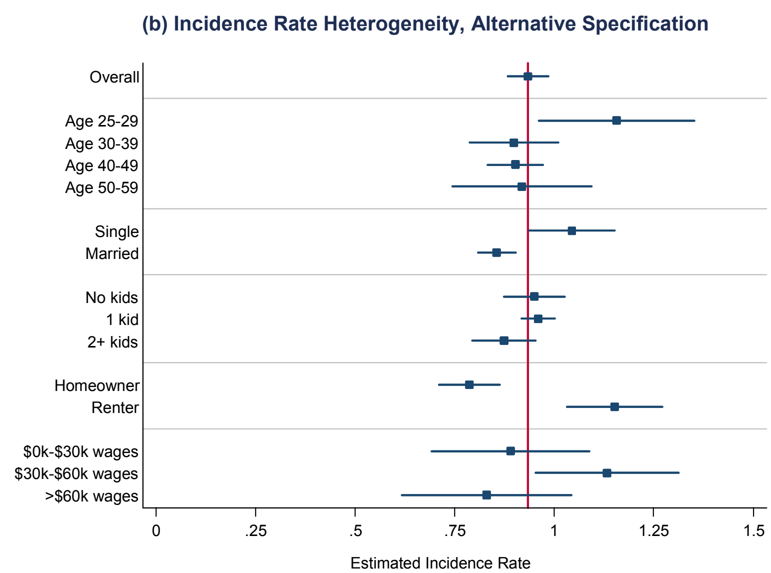
APPENDIX FIGURE 1
Population Changes versus Employment Rate Changes



Notes: Panels A and B plot state-level population and employment rate changes for the continental United States using the 2006 and 2011 American Community Surveys. The sample is the one used in Figure 1 and detailed in Section III: males aged 25-59 in 2006 (30-64 in 2011) and living in the continental United States. A state's 2006-2011 employment rate change equals the state's 2011 employment rate minus the state's 2006 employment rate. A state's 2006-2011 population change equals the non-annualized percentage change in the state's population between 2006 and 2011. A state's de-trended population change equals the state's actual 2006-2011 population change among males aged 25-59 in 2006 (30-64 in 2011), minus the state's 2001-2006 population change among males aged 25-59 in 2001 (30-64 in 2006). Panel C is the de-trended version of Figure 2b, which plots population changes versus employment rate changes across CZ's in the main sample. A CZ's de-trended population change equals the CZ's actual 2006-2011 population change in the main sample (males aged 25-59 in 2006 who lived in the continental United States in 2006 and 2011), minus the CZ's 2001-2006 population change in a comparable sample (males aged 25-59 in 2001 who lived in the continental United States in 2001 and 2006).

APPENDIX FIGURE 2
Incidence Heterogeneity Estimates: Non-parametric Illustration and Alternative Specifications





Notes: Panel A illustrates the non-parametric relationships underlying the "Overall", "Aged 25-29", and "Aged 40-44" estimates reported in Figure 4. See the notes to that figure for details on specifications. These binned scatter plots are constructed using the same procedure used to produce Figure 2b; see the notes to that figure for details. Panel B replicates Figure 4 except that every regression uses the same CZ shock regressor, equal to the 2006-2011 employment rate change in an individual's CZ measured using the full sample.