# The Effect of Internal Migration on Local Labor Markets: American Cities during the Great Depression

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The Great Depression offers a unique laboratory to investigate the causal impact of migration on local labor markets. We use variation in the generosity of New Deal programs and extreme weather events to instrument for migrant flows to and from U.S. cities. In-migration had little effect on the hourly earnings of existing residents. Instead, in-migration prompted some residents to move away and others to lose weeks of work or access to relief jobs. For every 10 arrivals, we estimate that 1.9 residents moved out, 2.1 were prevented from finding a relief job, and 1.9 shifted from full-time to part-time work.

## I. Introduction

The large flow of immigrants to the United States over the past 3 decades has renewed public debate about the economic costs and benefits of immigration. Similar debates swept the nation during the Great Depression,

although the primary concern at the time was with internal migration. The hostility of Californians toward migrants fleeing the Dust Bowl, vividly portrayed in John Steinbeck's novel *The Grapes of Wrath* (1939), illustrates the depth of the social and economic alarm over internal migration in this period.[1]

The 1930s are a unique laboratory for exploring the causal impact of migration on local labor markets. During the Depression, many internal moves were motivated by negative shocks to a migrant's home economy. Because these moves originated within the United States, we have access to a wealth of data about sending areas, which can be used to instrument for migration flows. In particular, we use data on a set of "push" factors, including variation in the generosity of New Deal policies and the presence of extreme weather conditions, to predict outflows from major U.S. markets. We assign these predicted migration flows to destinations on the basis of the geographic proximity between source and destination areas. This empirical approach allows us to address a core source of endogeneity in the immigration literature—namely, that migrants are not randomly assigned to destinations but rather tend to be attracted to cities with high wages or strong wage growth.

We find that in-migration to a metropolitan area reduced work opportunities for existing residents. Residents of high-migration areas were also less likely to hold a public relief job, conditional on being out of work, and more likely to leave the area altogether. For every 10 arrivals into an area, we estimate that 1.9 existing residents left the area, 2.1 were prevented from finding a relief job, and 1.9 shifted from full-time to part-time work. The adjustment to migration supply shocks, which occurred through reductions in employment rather than through lower wages, is

[1] The California Indigent Act of 1933 made it a crime to bring anyone likely to become a public charge into the state. While rarely enforced, the law was used to prosecute some Dust Bowl migrants who helped their relatives enter the state, until it was invalidated by the Supreme Court in 1941 (*Edwards v. California*). Californians also tried to scare possible migrants away from the state. For example, one billboard in Oklahoma carried the message: "NO JOBS in California / If YOU are looking for work—KEEP OUT / 6 men for every job / No state relief available for non-residents." For more on the politics of migration control in California, see Gregory (1989).

consistent with the presence of sticky wages and high unemployment during the Depression (Bernanke 1986; Margo 1993; Hanes 1996).[2]

## II. Previous Work on the Economic Effects of Immigration

One of the underpinnings of antimigrant sentiment is the fear that new arrivals drive wages down (and the local cost of living up), lowering the standard of living for the existing population. In a basic model of the labor market, migration flows into a city represent an outward shift in the supply of labor. This supply shift would lead to an unambiguous decrease in wages, unless the new population increased the demand for locally produced goods and services—and thus labor demand—sufficiently to offset the increase in labor supply. Because American cities are tied to an integrated national market for products, it is likely that the supply effect dominates the increase in derived labor demand.

Scholars have explored the relationship between immigrant arrivals and native wages in the United States in a variety of historical periods. Goldin (1994) documents that the mass migration from Europe at the turn of the twentieth century led to a large reduction in the wages of native-born workers in cities that experienced high in-migration.[3] However, few studies using modern data have detected a wage response of this magnitude across cities, and many find no effect on wages at all.[4] Card (1990), for example, finds that the Mariel boat lift from Cuba, which led to an immediate 7% increase in Miami's labor force, had no discernible effect on the wages or unemployment rates of low-skilled native-born workers in Miami relative to comparison cities. The disparity in the estimated effects of immigration on native outcomes over the century could reflect a true change in the response to a local shock over time—for example, the national integration of the labor market may allow a local disturbance to dissipate more rapidly today—or could simply be the result of differences in available measures and research design.

The weakness of the relationship between immigration and wages in port-of-entry labor markets has prompted discussion about other margins along which local areas may adjust to migrant supply shocks. Borjas, Freeman, and Katz (1997) point out that if new arrivals to a city induce some of the existing workforce to relocate, these out-migrants will spread the economic costs of immigration to other markets. More generally, with the free flow of factors between cities, the initial wage or employment

[2] For contemporary policy statements on job sharing, see Walker (1933) and Moulton (1936).

[3] Hatton and Williamson (1995) use a national time series of immigrant arrivals to estimate the impact of immigration on native wages. See also Ferrie (1996) and Carter and Sutch (1999).

[4] Altonji and Card (1991) is one exception. For a survey of this literature, see Friedberg and Hunt (1995).

response to a labor supply shock might be tempered by the out-migration of labor or the in-migration of capital (Blanchard and Katz 1992). Thus, the downward pressure of immigration on wages at the national level might be larger than comparisons of local labor markets would suggest (Borjas 2003; Ottaviano and Peri 2006).[5]

The empirical evidence on factor mobility in response to immigrant arrivals is mixed. Filer (1992) finds that immigrants crowd out existing workers on a one-for-one basis. However, more recent studies have not detected an appreciable out-migration response to international arrivals (see Wright, Ellis, and Reibel 1997; Card 2001; Kritz and Gurak 2001).[6] On the capital side, Lewis (2003, 2004) proposes that local labor markets have adjusted to low-skilled immigrants through slower adoption of skill-biased computer and information technology.

The literature on the labor market effects of migration is primarily focused on the foreign-born. Unlike many immigrants, internal migrants moving within the United States speak English, are educated in American schools, and may be difficult to distinguish in appearance or accent. If anything, then, we may expect internal migrants to be closer substitutes to existing residents and to have stronger negative effects on their economic outcomes. In a recent paper, Frank (2007) finds that the arrival of East Germans into West Germany had no effect on native wages and short-lived, positive effects on employment.

## III. Data on Internal Migration and Labor Market Outcomes in the 1930s

Given that local responses to a migration shock can take many forms, we aim to provide a comprehensive picture of local adjustment to internal migration during the Great Depression. We examine the wages and employment rates of local workers as well as the out-migration of existing residents and the creation of new establishments. We combine aggregate data on in- and out-migration rates at the city level with individual-level economic outcomes from the 1940 census. The census provides information on employment, earnings, and out-migration for city residents, along with a set of demographic and socioeconomic controls.

### A. Internal Migration

The 1940 census was the first to gather systematic information on internal mobility in the United States. Respondents were asked to report both their place of residence in 1935 and their current location in 1940. We

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[5] Borjas (2006) demonstrates that state-based estimates are only half as large as their national equivalents; metropolitan area estimates are even smaller. He argues that around half of this reduction is due to native out-migration.

[6] Exceptions include Frey (1995), Frey et al. (1996), and White and Liang (1998).

use aggregate data on population flows to calculate 5-year internal migration rates at the city level. That is, we calculate the number of migrants arriving in (or leaving) a city between 1935 and 1940 as a share of the existing population. In contrast, other studies typically proxy for a migration shock with the change in the share of the population that is foreign-born, a measure that does not account for the possibility of native outflows. Strict immigration quotas imposed in 1924, coupled with the weak Depression-era economy, drastically curtailed immigration from abroad in the 1930s. As a result, foreign arrivals constituted only 3% of all cross-county moves in the United States from 1935 to 1940. Our results do not qualitatively change when we include foreign arrivals in our migration counts.

In- and out-migration flows are available for the 86 cities with more than 100,000 residents in 1940. Whenever possible, we further aggregate the city data to match standard metropolitan areas (SMA).[7] Because some SMAs are anchored by multiple central cities—for example, Oakland and San Francisco, CA—we are left with 73 metropolitan areas, 69 of which have complete data.[8] While there is substantial overlap between the largest cities in 1940 and today, our historical sample contains more mid-Atlantic cities (e.g., Albany, NY, and Erie, PA) and does not include as many places in the southwest or California (El Paso, TX, Bakersfield, CA).[9] While migration counts are available by gender and race, we do not analyze the effect of female migrants because of their low labor force participation rates during this period. Because black migration is highly skewed toward

[7] We aggregate data from the following cities: Boston, Cambridge, Lowell, and Somerville, MA; Camden, NJ, and Philadelphia, PA; Elizabeth and Newark, NJ; Fall River and New Bedford, MA, and Providence, RI; Fort Worth and Dallas, TX; Kansas City, MO, and Kansas City, KS; Los Angeles and Long Beach, CA; Yonkers and New York City, NY; Oakland and San Francisco, CA; and St. Paul and Minneapolis, MN. Sacramento, CA, and Charlotte, NC, are missing migration data, and Washington, DC, and Flint, MI, are missing census of manufacturing earnings data in 1935. While we use the SMA concept to guide our aggregation decisions, our data capture only those migrants who settle in the central cities of these areas, rather than the surrounding suburban counties. In fact, some individuals who are classified as in-migrants from the balance of a city's own state might have moved to the city from the suburban ring, thus having no net effect on local labor supply. Mismeasurement of this type will attenuate our ordinary least squares (OLS) estimates, providing an additional motivation for our instrumental variable (IV) analysis.

[8] Our sample contains metropolitan areas in each of the nine census regions. The regional breakdown of our sample is New England (5), Middle Atlantic (12), East North Central (14), West North Central (6), South Atlantic (10), East South Central (6), West South Central (8), Mountain (2), and Pacific (6).

[9] Thirty-two percent of the cities in our sample are not included among the 86 largest cities in the United States today, including four cities in New York and three each in Ohio, Pennsylvania, and Tennessee.

a few large cities, we combine data on black and white men into a single flow.[10] We note that our male migration rates include movers of all ages, some of whom may have been too young or too old to participate in the labor market.[11]

## B. Economic Outcomes

We use individual records from the 1940 Integrated Public Use Microdata Series to construct a sample of nonmigrants residing in the 69 metropolitan areas of interest (Ruggles et al. 2008). We limit our attention to men between age 18 and 65 who were not self-employed, living in group quarters, in the armed forces, or enrolled in school.[12]

We explore the effect of net migrant arrivals on three spheres of economic activity: employment, earnings, and out-migration. Following the practice of Depression-era statistics collection, we define employment as holding a private sector or nonrelief public sector job (Lebergott 1964; Darby 1976). We investigate two forms of nonemployment: holding a relief job and being idle (i.e., being unemployed or out of the labor force). Many relief jobs were administered by the Works Progress Administration (WPA). The typical hourly earnings for a WPA job were about 40%–45% of the hourly earnings on regular government construction projects and were designed to enable someone without regular employment to reach a basic consumption standard while seeking work elsewhere. Work relief was intended as temporary employment, but a large number of workers spent several years on work relief (Howard 1943; Margo 1993; Neumann, Fishback, and Kantor 2010).

Annual earnings and their subcomponents—weekly wages and weeks worked during the past year—are computed for men who report being employed at the time of the census, working a positive number of hours in the past week, and earning a positive income in the past year.[13] We also measure hourly wages, but these results are particularly subject to measurement error, given that hours were only reported for the census week. To examine the mobility response of existing residents to new arrivals, we define an indicator equal to one for men who were living in a sample

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[10] If anything, the results get slightly stronger and more precise when we exclude black migrants from the migration rates and consider the effect of migration on white men only.

[11] Our calculations from the microdata indicate that 74% of the male migrants were of prime age (18–65). We take this fact into account in interpreting the magnitudes of our coefficients below.

[12] The census did not collect information on self-employment income in 1940.

[13] Following Goldin and Margo (1992), we replace top-coded incomes with 1.4 times the top code.

metropolitan area in 1935 but were no longer resident in the area by 1940.[14]

The microdata have three key advantages over local area aggregates. First, aggregate earnings data are available at the local level only for three sectors—manufacturing, retail, and wholesale trade—whereas the microdata encompass the whole labor market. Second, we can control for key determinants of wage and employment outcomes at the individual level, including age, education, and race. These variables might prove important if selective out-migration changed the composition of the remaining labor force. Finally, we can directly exclude in-migrants from the sample. Our estimates can then be interpreted as a treatment effect of migration on the existing workforce, rather than a compositional change in the labor force as new workers arrive.[15]

Summary statistics at the metropolitan area level are presented in table A1. The weak economic outcomes in the data are characteristic of the period: 78.5% of men in the average metropolitan area were employed, compared to 84.7% in 1950; 24.8% of men who were out of work held a government relief job. The New Deal migration emphasized here notwithstanding, slack labor conditions during the Great Depression dampened overall geographic mobility, with interstate migration rates in the 1930s far below the peak levels set 10 years later during World War II (Rosenbloom and Sundstrom 2004). The mean in-migration rate of 5.3% in our sample cities is low by historical standards.

The scatter plot in figure 1 depicts the relationship between in-migration to a metropolitan area between 1935 and 1940 and the probability of migrating out of the metropolitan area. We select this outcome to present in graphical form because it has a strong relationship with migration that is apparent even in the raw data. A 1 percentage point increase in the in-migration rate increases the probability of leaving the metropolitan area by 0.6 percentage points. Miami and San Diego are obvious outliers, with in-migration rates of 0.18 and 0.16, respectively. Excluding these two areas strengthens this relationship slightly. In our estimation below, we augment this simple approach by controlling for demographics and initial employment characteristics in the metropolitan area in 1935.

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[14] The mobility variable is constructed by comparing individuals' reported place of residence in 1935 and 1940 in the 1940 census. In order to be in the sample, men must have survived until 1940 to be enumerated. Therefore, we will not mistake mortality for mobility.

[15] Migrants were, on average, 3 years younger than men who remained in the same county between 1935 and 1940 (31.2 vs. 34.0 years of age) and had completed 2 more years of education (9.4 vs. 7.2 years of schooling). Migrants were also twice as likely to have finished high school (16.3% vs. 7.1%).
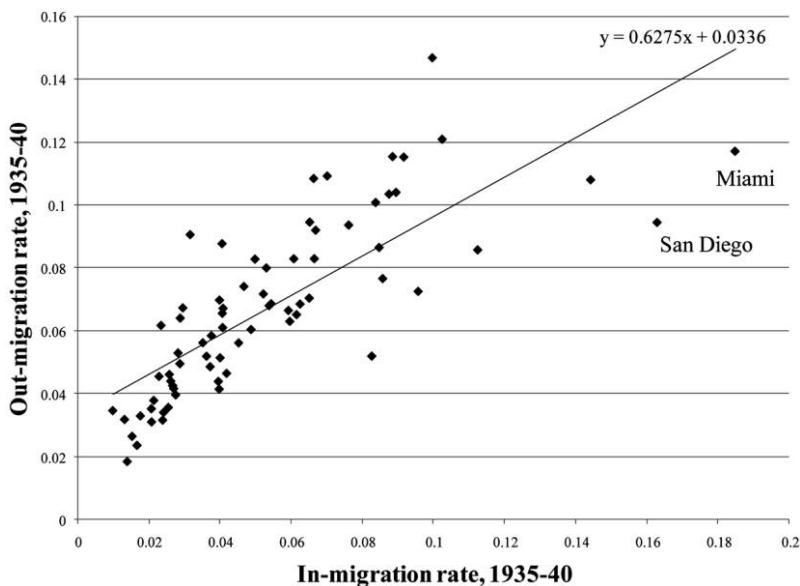
Fig. 1.—In-migration rates and the probability of out-migration among existing residents by metropolitan area, 1935–40. Each dot represents one of the 69 metropolitan areas with complete data in the sample. The numbers of male in- and out-migrants are taken from the 1940 census volume on internal migration (U.S. Bureau of the Census 1943).

## IV. Estimating the Relationship between Internal Migration and Economic Outcomes

### A. Ordinary Least Squares Specification

We are interested in the effect of internal migration to a metropolitan area on the employment rate, annual earnings, and out-migration rate of existing residents. Let $Y_{ijr\,40}$ represent an economic outcome for a nonmigrant $i$ who lives in metropolitan area $j$ in region $r$ in 1940. We posit that $Y_{ijr\,40}$ will be a function of the migrant-induced change in labor supply:

$$Y_{ijr40} = \alpha + \beta m_{jr40-35} + \gamma o_{jr40-35} + \Phi'Y_{jr35} + \Psi'X_{ijr40} + \Pi_r + \varepsilon_{ijr40}, \quad (1)$$

where $m_{jr\,40-35}$ represents the in-migration rate to area $j$ and $o_{jr\,40-35}$ represents the out-migration rate from area $j$. In the earnings and employment equations, we expect $\beta$ to be negative and $\gamma$ to be positive. The most flexible specification, presented here, allows in- and out-migration to have distinct effects on labor market outcomes. For parsimony, we often restrict arrivals and departures to exert equal and opposite effects by including

only the net migration rate on the right-hand side ($n_{jr\,40-35} = m_{jr\,40-35} - o_{jr\,40-35}$).

Variable $Y_{ijr\,40}$ measures the level of earnings or employment of individuals in city $j$ in 1940. The equilibrium wage level in an area is a function of the total labor supply, which is in part determined by the stock of internal migrants. However, our migration variables measure the flow of new arrivals. Ideally, we would use changes in earnings or employment as dependent variables. However, the 1940 census was the first to ask such detailed questions about economic activity. Instead, we include aggregate wage or employment measures from 1935 among our set of regressors ($Y_{jr\,35}$) and interpret the estimating equation as a quasi first difference. Specifically, in our wage and earnings regressions, we control for the logarithm of annual earnings per employee in manufacturing, retail, and wholesale trade in 1935, and in our employment regressions, we include the shares of the city's population that were on work relief in October 1933 and that were unemployed in 1937. The earnings measures are drawn from the censuses of manufacturing and of retail and wholesale trade, and the unemployment figures are from the Federal Emergency Relief Administration's (1934) Unemployment Relief Census for October 1933 and the Census of Partial Employment, Unemployment, and Occupations (U.S. Bureau of the Census 1938).

In addition to initial wage or employment levels, we also control for the share of the city's population that was black, foreign-born, or illiterate and the age distribution of the population in 1930.[16] The microdata allow us to enter a series of individual characteristics ($X_{ijr\,40}$), including race, a cubic polynomial in age, and a set of education dummies for each year of completed schooling. Finally, because the extent of the downturn during the Depression varied geographically, we include $\Pi_r$, a set of regional dummy variables (Wallis 1989; Rosenbloom and Sundstrom 1999).

Errors are clustered at the metropolitan area level to allow for correlation in economic shocks faced by individuals in the same labor market. Although we are using individual-level data, our goal is to examine the impact of mobility on a local labor market. Thus, we weight individual observations from metropolitan area $j$ by the inverse of the area's total number of observations, allowing each metropolitan area to contribute equally to the estimation.[17]

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[16] City-level variables are taken from published census data (U.S. Bureau of the Census 1933) and aggregated to the metropolitan area level.

[17] We find very similar point estimates in aggregate regressions with larger standard errors (available from authors on request). Individual regressions are preferable in this case because they are more efficient.

## B. Developing an Instrument for Migration Rates

Migrants may be attracted to areas with high wages or strong employment performance. Consider an unmeasured, and thus omitted, city characteristic $c$ that belongs in equation (1) and that generates an increase in average wages between 1935 and 1940. If $c$ also attracts migrants to the area, the coefficient on the in-migration rate ($\beta$) will be biased upward, and the coefficient on the out-migration rate ($\gamma$) will be biased downward. This concern can be addressed by instrumenting for the in- and out-migration rates to an area $j$ with variables that are uncorrelated with local economic conditions.

To develop instruments for in- and out-migration, we turn to the standard economic model of migration. Prospective migrants from a source area $k$ compare the expected benefits of moving to new locations $j$ ($j \neq k$) with the costs of migration (Sjaastad 1962). While positive economic shocks pulling migrants to destination $j$ are clearly endogenous, poor economic conditions pushing migrants to leave area $k$ are arguably exogenous to labor market conditions in $j$. Therefore, local economic conditions in areas that typically send migrants to destination $j$ are natural instruments for in-migration to that city.

The federal government instituted an unprecedented set of relief efforts in response to the Depression, including work relief and public works projects. During the period of our analysis, emergency relief jobs were offered primarily through the WPA, which began in 1935 and operated as a partnership between the federal government and state or local governments.[18] State or local officials proposed a series of work relief projects, while the WPA chose among the proposed projects and provided the funding to pay workers. As a result, the level of funding both between and within states varied tremendously.[19]

Fishback, Horrace, and Kantor (2006) study the determinants of net migration between U.S. counties in the 1930s. They conclude that variation in the generosity of New Deal programs can explain up to 16% of the difference between net-migrant-receiving versus net-migrant-sending counties in the 1930s. Differences in temperature and precipitation can, together, explain another 4% of the difference. The dramatic increase in federal spending on work relief and public works in the 1930s, along with the variation in the beneficiaries of this spending, made this period an early example of "welfare magnet" migration (see, e.g., Moffit 1992).

We use variation in both New Deal generosity and local weather conditions to predict out-migration from source areas. The predicted out-

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[18] Some emergency relief jobs were offered through the Civilian Conservation Corps and the National Youth Administration or by state and local governments.
[19] Table 2 in Fishback, Horrace, and Kantor (2005) provides state means, standard deviations, and minima and maxima of New Deal spending by county.

migrant flows are then assigned from source areas to destinations using the geographic distance between source-destination pairs. We predict the probability that a migrant leaving area $k$ would settle in destination $j$, using the geographic distance between $j$ and $k$ as the crow flies.[20] Distance is a central determinant of a migrant's location choice and is invariant to contemporary economic conditions in either the sending or the receiving area (Schwartz 1973; Levy and Wadycki 1974; Borjas 2001). In contrast, because migrants are attracted to areas with strong economic conditions, actual settlement patterns may be correlated with unmeasured city characteristics.

An example helps to illustrate this method. An extended period of drought and dust storms elevated the out-migration rate from Oklahoma during the 1930s (Gregory 1989). Because of this economic hardship, we would expect proximate cities (Dallas, TX; Wichita, KS) to receive large migrant flows in the 1930s. Our maintained assumption is that the extreme weather events in Oklahoma did not influence the labor market in Dallas, except through the resulting migration. Our identification strategy bears some resemblance to two recent papers that use internal migration to study the effect of migration on labor market or housing market outcomes (Frank 2007; Boustan 2010).

For the instrument to be valid, the variables used to predict out-migration from sending areas must be uncorrelated with unobserved labor market conditions in destinations, except through migration. Given that many internal migrants relocate over short distances, this assumption can be violated; 46.7% of cross-county moves from 1935 to 1940, for example, took place within the same state. Therefore, we construct our instrument using predicted migrant flows from outside a destination city's own state in order to minimize concerns about the spatially correlated shocks. We also try restricting our attention to predicted migration from outside of a city's own census region.

Migrants who leave their place of origin after a weather shock or because of the unavailability of generous New Deal programs may be negatively selected on ability or motivation (see, e.g., Borjas 1987; Borjas, Bronars, and Trejo 1992; Abramitzky 2009). Therefore, such migrants may be disproportionately represented on the public relief rolls, rather than in private employment. In this sense, our IV estimates provide local average treatment effects of negatively selected migrants on the labor market, and we may not be able to extrapolate from these results to determine the effect of a more positively selected migrant stream.

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[20] An alternative approach is to assign migrants to destinations on the basis of settlement patterns established during a prior migration wave (see, e.g., Card 2001; Lewis 2004; and Boustan 2010). We did not use this approach because 1940 was the first year in which the Census Bureau asked residents about internal mobility.

## C. Constructing the Instruments

We follow a two-step procedure to create the instrument for in-migration to a metropolitan area. First, we predict out-migration rates from source areas, indexed by $k$, using local economic conditions. These rates are used to calculate a predicted outflow from each source area $k$, outside of $j$'s state. In the second step, we predict the probability that migrants leaving source area $k$ settle in destination $j$ on the basis of distance alone ($k \neq j$). The total predicted migrant flow to a destination area $j$ is then the sum of these pair-wise predictions across all possible source areas. This predicted migrant flow becomes our instrument for actual in-migration.[21]

It is instructive to review the construction of our instruments in more detail. In the first step, the out-migration rate ($o_{kr\,40-35}$) from source area $k$ in region $r$ is determined by a vector of local economic conditions ($Z_k$):

$$O_{kr40-35} = \alpha + \Phi'Z_{kr} + \Pi_r + \varepsilon_{kr}, \tag{2}$$

where $Z_{kr}$ includes spending on New Deal programs and measures of extreme weather conditions. These data were collected by Price Fishback and Shawn Kantor's New Deal project.[22] We estimate the parameters of equation (2) from a single OLS regression with 117 observations, one for each sending area (69 metropolitan areas and 48 balance-of-state areas).[23] The predicted flow of migrants leaving area $k$ is the product of the predicted out-migration rate for area $k$ ($o_{kr\,40-35}$) and the population of area $k$ in 1930:

$$O_k = o_{kr40-35} \times \text{population}_{kr30}. \tag{3}$$

Predicted values are written with boldface type.

In the next step, we use the geographic distance in miles between source area $k$ and all possible destinations $j$ to predict the destinations of the

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[21] Wooldridge (2002, 139–41) demonstrates that standard two-stage least squares inference is valid when instruments are functions of estimated parameters. Hence, no standard errors correction is needed. Intuitively, while the instruments are themselves estimates, this fact should simply add noise to the first-stage regressions and should not affect inference in the second stage.

[22] We aggregated county-level data from the New Deal project to match our sample of metropolitan areas and balance-of-state locations. See Fishback, Haines, and Kantor (2001, 2007) and Fishback et al. (2006) for descriptions of the data.

[23] The balance-of-state designation refers to the portion of the state not contained in one of the large metropolitan areas in the sample.

migrants who leave $k$.[24] In particular, for each sending area $k$, we regress the share of people leaving area $k$ who settle in area $j$ (outside of $k$'s state) on a quadratic in the distance between areas $j$ and $k$,[25]

$$P_{kj} = \alpha_k + \theta_k(\text{distance})_{kj} + \gamma_k(\text{distance}^2)_{kj} + \mu_k, \qquad (4)$$

and use the resulting parameters $(\theta_k, \gamma_k)$ to predict $P_{kj}$. We estimate separate versions of equation (4) for each of the 117 source areas, thereby allowing the parameters determining the effect of distance on destination choice to vary by sending area.

The instrument for in-migration to city $j$, then, is the sum over all areas $(k \neq j)$ of the predicted number of migrants leaving area $k$ who are expected to settle in city $j$. To avoid spatial correlation, we exclude migrants predicted to leave source areas in city $j$'s own state:

$$M_j = \sum_{k=1...n(k \neq j)} O_k \times P_{kj}. \qquad (5)$$

The instrument for the in-migration rate divides the predicted flow (eq. [5]) by city $j$'s population in 1930.

Our instrument for the out-migration rate from area $j$ is developed in a symmetric fashion. First, we predict the in-migration rate to each receiving area $k$ as a function of local pull factors in $k$ (note that $k$ now indexes destinations, whereas above it indexed source areas). We then convert the predicted in-migration rate into a predicted migration flow. In the next step, we use the distance between areas $j$ and $k$ to predict the share of in-migrants to area $k$ that hail from each source area $j$. Using these shares, we calculate the predicted number of out-migrants flowing from source area $j$ to destination $k$. Finally, we sum these predicted outflows across all possible destination areas $k$ and convert this flow into a predicted out-migration rate for source area $j$. This value becomes the instrument for the out-migration rate for each city in the sample.

### D. Results from the Regressions Used to Construct the Instruments

Our instruments are based on the output from two sets of regressions: migration rates estimated from local push/pull factors and predicted settlement patterns based on the distance between two markets. We dem-

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[24] We thank Trent Alexander and David Van Riper at the Minnesota Population Center for providing measures of distances in miles between every county group in the United States. We calculate the distance between balance-of-state areas as the average distance from every nonmetropolitan county group in state $A$ to every nonmetropolitan county group in state $B$. A preferred measure may be the distance between the population-weighted centroids of each balance-of-state area. While this measure is feasible, it would be extremely time intensive to produce and would correspond closely to our current method.

[25] We also tried a specification in which the probability of a migrant from $j$ settling in $k$ depended on the logarithm of the distance between $j$ and $k$. Results are similar.

**Table 1**
**Determinants of Male In- and Out-Migration, 1935–40**

| Right-Hand-Side Variable | Dependent Variable | | |
| --- | --- | --- | --- |
| | Net Migration Rate | In-Migration Rate | Out-Migration Rate |
| Metropolitan area | .003 | −.026 | −.029 |
| | (.011) | (.013) | (.009) |
| New Deal expenditures on public works and relief, 1933–39 | .025 | .044 | .019 |
| | (.011) | (.013) | (.009) |
| Months of extreme wetness, 1935–39 | .011 | .017 | .005 |
| | (.002) | (.002) | (.002) |
| Wet months × metro area | −.009 | −.017 | −.007 |
| | (.003) | (.003) | (.002) |
| Average temperature, 1935–39 | .002 | .005 | .003 |
| | (.001) | (.001) | (.001) |
| $F$-statistic for joint significance | 12.61 | 21.95 | 7.58 |

SOURCES.—The number of male migrants is from the 1940 census volume on internal migration (U.S. Bureau of the Census 1943). New Deal spending on public works is from the U.S. Office of Government Reports (1940) and is reported in $100 per capita. Per capita adjustments are based on the 1930 population. The public works data were collected by Price Fishback and Shawn Kantor. The full New Deal data set can be found on Price Fishback's Web site at the University of Arizona (http://www.u.arizona.edu/~fishback/Published_Research_Datasets.html).

NOTE.—Standard errors are in parentheses and are clustered by state. Observations include 69 metropolitan areas and 48 balance-of-state areas; region dummies included; $N = 117$.

onstrate here that the regression results used to build the instrument are economically intuitive and have statistical power.

Table 1 presents results from equation (2), which relates net migration rates or its subcomponents (in- and out-migration rates) to local economic conditions. Higher spending on New Deal public works projects and on relief to the unemployed is associated with net in-migration. An increase in the number of months with severe wetness led to net in-migration in balance-of-state areas, many of which were specialized in agriculture, but had no effect on mobility in metropolitan areas. Higher average temperatures stimulated net in-migration to both urban and rural areas. Perhaps somewhat surprisingly, we find that local push factors that generate higher out-migration rates also lead to more in-migration. This pattern would arise if the departures themselves prompted some in-migration, for example, in response to falling housing prices or the increase in available labor market opportunities accompanying out-migration. On net, we find that both public programs and favorable weather conditions attract new residents to an area. In most of our empirical analysis, we emphasize the effect of these local factors on net migration.

The probability that a migrant from area $k$ settled in destination $j$ is strongly related to the geographic distance between the two markets. The median coefficient from our source area level regressions suggests that increasing the pair-wise distance by 1,000 miles decreases the share of

**Table 2**
**First-Stage Regressions: Relationship between Predicted and Actual
Migration, Men 1935–40**

|  | Assigned Migrant Flows | | |
|---|---|---|---|
|  | All | Out of State | Out of Region |
| Weighted ordinary least squares regression coefficients:* | | | |
| Predicted in-migration rate | 2.589 | 5.632 | 8.352 |
|  | (.317) | (.702) | (1.173) |
| Predicted out-migration rate | −2.879 | −4.627 | −7.392 |
|  | (.450) | (.568) | (1.753) |
| *F*-statistic | 30.18 | 40.61 | 29.68 |
| Implied effects of 1 SD:*,† | | | |
| Predicted in-migration rate | 1.047 | .683 | .506 |
| Predicted out-migration rate | .952 | .685 | .547 |
| Bootstrapped procedure:‡ | | | |
| Predicted in-migration rate | 2.652 | 5.426 | 8.353 |
|  | (.492) | (.969) | (1.732) |
| Predicted out-migration rate | −2.890 | −4.533 | −7.448 |
|  | (.645) | (1.004) | (3.059) |
| Wald's statistic | 29.57 | 35.37 | 23.26 |

NOTE.—Dependent variable is net number of migrants between 1935 and 1940 as a percentage of the 1935 population. Regressions are estimated using individual-level data from the 1940 Integrated Public Use Microdata Series. Standard errors are in parentheses and are clustered by metropolitan area. Regressions include region dummies and the full set of controls from the second stage.

* Based on 131,006 individual observations that are weighted by the inverse of the number of observations in the metropolitan area.

† Implied effect of a 1 SD increase in predicted in- or out-migration in shares of a standard deviation in the actual net migration rate.

‡ Standard errors are bootstrapped to account for the fact that the predicted in- and out-migration rates are generated regressors. To ensure that each city contributes equally to the estimation, we randomly selected the same number of observations per city (295 per city, for a total of 20,355) when performing the bootstrapping procedure.

migrants from area $k$ settling in destination $j$ by 2.8 percentage points.[26] For all but four of the 117 source areas, the coefficients on both the linear and quadratic distance terms in equation (4) were significantly different from zero at the 5% level. The exceptions are San Diego, CA; Miami and Tampa, FL; and Norfolk, VA.

Table 2 contains the results from first-stage regressions, which relate actual net migration to predicted in- and out-migration rates. In addition, the first-stage regressions contain the full set of region dummies and control variables included in the second stage. Our preferred instrument excludes predicted migration flows from within a city's own state. This strategy reduces concerns about spatial correlation of unmeasured eco-

[26] If we use the median coefficient from our distance regressions to assign migrants to destinations, our instrument no longer has predictive power. This fact suggests that some source areas are more sensitive to distance than others. We would be concerned if an area's sensitivity to distance was a function of local economic conditions. We regress the place-specific coefficients from our distance regressions on various local characteristics and find no relationship between economic conditions and sensitivity to distance. Rather, sensitivity to distance is determined by latitude and coastal access.

**Table 3**
**Effect of Net Migration on Earnings, Wages, and Work Time in 1940**

| Dependent Variable | Ordinary Least Squares | Instrumental Variable |
|---|---|---|
| ln(annual earnings) | −.218 | −.948 |
| | (.578) | (.610) |
| ln(weekly wage) | .561 | .006 |
| | (.379) | (.536) |
| ln(hourly wage) | .471 | −.521 |
| | (.545) | (.730) |
| ln(weeks worked) | −.779 | −.954 |
| | (.325) | (.304) |
| Work less than 26 weeks | .402 | .528 |
| | (.200) | (.196) |
| ln(hours worked) | .089 | .527 |
| | (.267) | (.295) |

Note.—$N$ = 96,070. Data are coefficients on net number of migrants between 1935 and 1940 as a percentage of the 1935 population. Regressions are estimated at the individual level using data from the 1940 Integrated Public Use Microdata Series and are weighted by the inverse of the number of observations in the metropolitan area. Standard errors are in parentheses and are clustered by metropolitan area. The sample includes only men employed during the census week who report positive earnings. The weekly wage is annual earnings in 1939 divided by weeks worked in 1939. The hourly wage is the weekly wage divided by the hours worked in the week before the census survey in March 1940. Individual controls include an indicator for being nonwhite, a cubic polynomial in age, and a dummy for each year of school completed. The regressions also contain city-level controls for the shares of the population that were black, foreign-born, and illiterate in 1930; city-level age distribution; and the lagged annual earnings or unemployment rates. Sources for these variables are described in the text. The instruments are described in Sec. IV.C.

nomic shocks. For comparison, we also show results using instruments that include all predicted migrant flows or that exclude migrants from within the city's census region. In each case, predicted in- (out-) migration is positively (negatively) related to actual net migration. A 1 SD increase in the predicted in- or out-migration rate is associated with a 0.5–1.0 SD increase in actual net migration. The implied response to a 1 SD increase in predicted migration decreases as we restrict the scope to either out-of-state or out-of-region migration. The *F*-statistic for the joint test of significance on the two instruments ranges between 30 and 40, above the conventional threshold for weak instruments. The instruments are still strong in a similar specification in which we bootstrap the standard errors to account for the fact that the predicted in- and out-migration rates are generated regressors (McKenzie and McAleer 1997).

## V. The Impact of In-Migration on Local Labor Markets

### A. Earnings and Wage Outcomes

Table 3 investigates the relationship between net internal migration to a metropolitan area and five earnings and work opportunity outcomes for existing residents—annual earnings, weekly wages, weeks worked in the previous year, hourly wages, and hours worked during the previous week. Coefficients from an OLS and a corresponding IV specification are presented. Migrant location choices may generate an upward bias in the OLS estimation because higher wages are likely to attract migrants to an area.

**Table 4**
**Effect of Net Migration on Employment and Work Relief**

| Dependent Variable | Multinomial Logistic Regression* | Ordinary Least Squares† | Instrumental Variable† |
|---|---|---|---|
| Employed ($N = 131,006$) | 1.839 | .028 | .018 |
|  | (1.177) | (.179) | (.251) |
| On work relief ($N = 131,006$) | −8.991 | −.344 | −.467 |
|  | (2.829) | (.115) | (.189) |
| On work relief (if out of work; $N = 29,121$)‡ | . . . | −1.695 | −2.561 |
|  |  | (.505) | (.830) |

NOTE.—Data are coefficients on net number of migrants between 1935 and 1940 as a percentage of the 1935 population. Regressions are estimated at the individual level using data from the 1940 Integrated Public Use Microdata Series and are weighted by the inverse of the number of observations in the metropolitan area. Standard errors are in parentheses and are clustered by metropolitan area. The instruments are described in Sec. IV.C.

* Based on three categories: employment in the public or private sector, work relief, or idleness (the base category). Idleness includes men who are either unemployed or out of the labor force.

† Probability of being employed or on work relief is compared to the other two employment categories.

‡ Probability of being on work relief conditional on being out of work (i.e., either on work relief or idle).

Indeed, in the OLS specification, net migration has a positive, but not statistically significant, relationship with weekly and hourly wages. Once we instrument for net migration rates, the coefficients in the wage regressions fall, although they remain statistically indistinguishable from zero.

Residents of metropolitan areas that experienced high in-migration worked significantly fewer weeks over the year. According to the IV estimates, a 1 SD increase in net migration (2.1 percentage points) would have reduced average weeks worked during the year by 2% (0.9 of a week, or 0.6 SD). The reduction in weeks worked resulted in a small—but only marginally statistically significant—decline in annual earnings (0.2 SD). The effect of migration on work opportunities is strongest for men in the bottom decile of the weeks-of-work distribution. At the 10th percentile of this distribution, men worked 26 weeks during the year, a level that may indicate involuntary part-year employment (job sharing). We define an indicator variable equal to one for men who worked 26 or fewer weeks during the year and find that a 1 SD increase in net migration would have increased the probability of part-year employment by 0.4 SD.

### B. Employment, Relief, and Unemployment Outcomes

Our earnings results suggest that 1930s labor markets were more likely to react to migration flows through a decline in work opportunities, rather than through reductions in wages. We next investigate whether migration affected the probability that an individual was employed, on work relief, or idle during the year. Table 4 contains results from a multinomial logistic (MNL) regression of employment and work relief on migration rates; the

base category is being idle. Net in-migration to a metropolitan area does not affect the probability of employment relative to the probability of being idle. However, men in metropolitan areas with high net-migration rates were far less likely to hold a relief job. A 1 SD increase in the net migration rate is associated with a 12.5% (exp(8.991 × 0.014); 3.0 percentage points) reduction in the probability of being on relief. This pattern suggests that migration led to a shift in the daily activities of the unemployed but did not influence the probability of employment.[27]

Migrants may have been attracted to an area by the generosity of local New Deal relief programs. While many states restricted relief to long-term residents, qualification usually required only a single year of residency. Thus, the MNL coefficients may be biased upward by endogenous location decisions. We confirm this pattern by estimating a linear probability model first using OLS and then using our instrument for net migration. As before, migration has no effect on the probability of being employed but reduces the probability of holding a relief job. We also report coefficients from regressions of the probability of being on relief, conditional on being out of work. A 1 SD increase in the net migration rate reduces the conditional probability of holding a relief job by 3.6 points (0.33 SD).

## C. Other Forms of Local Adjustment

We have shown that migration to a metropolitan area reduces the work opportunities of existing residents, in terms of both the number of weeks worked in the private sector and the probability of securing a relief job. Some residents might have responded to these worsening economic conditions by moving elsewhere. Table 5 considers the effect of in-migration to a metropolitan area on the probability that a member of the 1935 population had relocated by 1940. While modern evidence on this question is mixed, we find unequivocal support for the idea that in-migration to a city stimulated outflow during this period.

In OLS regressions, in-migration to a metropolitan area has a large and statistically significant effect on the probability that existing residents leave the area. However, the sign of the potential bias is uncertain. For a given level of labor demand, in-migrants will be attracted to areas with recent departures, leading to an upward bias. However, a negative labor demand shock might simultaneously induce out-migration and repel new arrivals, generating a negative bias. Empirically, we find that the IV coefficient is slightly smaller than OLS, suggesting that, if anything, the former effect dominates. The IV coefficient implies that a 1 SD increase in the in-

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[27] In-migration has a negative and statistically significant effect on employment as well when we exclude black migrants from the migration rates and consider the effect of migration on white men only.

**Table 5**
**Other Labor Market Adjustments: Out-Migration of Existing Workers and the Creation of New Establishments, 1935–40**

| Dependent Variable | Ordinary Least Squares | Instrumental Variable |
|---|---|---|
| Left standard metropolitan area, 1935–40 ($N = 142{,}327$)* | .880 | .727 |
| | (.276) | (.323) |
| Self-employed ($N = 149{,}640$)† | .165 | .460 |
| | (.170) | (.260) |
| No. establishments per 1,000 ($N = 69$):† | | |
| Retail | 12.892 | 11.243 |
| | (8.585) | (9.051) |
| Wholesale | 1.128 | .874 |
| | (1.158) | (1.493) |
| Manufacturing | −.151 | .979 |
| | (1.204) | (1.867) |

Sources.—Data on establishments in 1935 are reported in U.S. Bureau of Foreign and Domestic Commerce (1939). Data on establishments in 1939 are from the 1940 census and can be found in Haines (2004).

Note.—Standard errors are in parentheses and are clustered by metropolitan area. The instruments are described in Sec. IV.C.

* Reported coefficients for the gross number of in-migrants between 1935 and 1940 as a percentage of the 1935 population. Regressions estimated at the individual level and weighted by the inverse of the number of observations in the metropolitan area.

† Reported coefficients for the net number of migrants between 1935 and 1940 as a percentage of the 1935 population. Regressions estimated at the city level. Controls include region dummies, the age distribution, and the share of the population that was black, foreign-born, or illiterate in 1930.

migration rate to a metropolitan area increases the probability of out-migration among existing male residents by 2.4 percentage points, or 0.5 SD.

Another form of adjustment available to existing residents displaced from private sector employment or work relief is to enter self-employment. Thus far, we have excluded the self-employed from the analysis because the 1940 census contains limited information on their economic outcomes. However, the census does separately identify the self-employed. We create an expanded sample that contains the self-employed and examine the relationship between net in-migration and the probability of self-employment. In OLS, the relationship between migration and the probability of being self-employed, while positive, is not statistically significant and is relatively small (table 5). After instrumenting for net migration, we find that a 1 SD increase in the net migration rate increases the probability of self-employment by 0.25 SD. Self-employment appears to be one method of adjustment used by residents faced with competition from new migration. However, we cannot assess whether adjustment of this kind was successful in stemming or reversing declines in annual earnings.

If, by shifting out the labor supply, in-migration caused wages to fall, firms in all sectors might have been attracted to the relatively cheap labor available in migrant destinations. Alternatively, in-migration may have increased the local demand for services and other nontraded products,

**Table 6**
**Robustness Checks: Relationship between Migration and Work Opportunities**

| | Out of State | | Net Migration | |
|---|---|---|---|---|
| Dependent Variable | In-Migration (1) | Out-Migration (2) | All (3) | Out of Region (4) |
| On work relief (if out of work)* | −2.538 (.811) | 1.781 (1.110) | −2.475 (.979) | −2.826 (.919) |
| ln(weeks worked)† | −.996 (.313) | 1.136 (.624) | −.731 (.329) | −1.099 (.344) |

Note.—Data are coefficients on net number of migrants between 1935 and 1940 as a percentage of the 1935 population. Standard errors are in parentheses and are clustered by metropolitan area. Regressions are estimated at the individual level and weighted by the inverse of the number of observations in the metropolitan area. See notes to tables 3 and 4 for a list of the individual- and city-level control variables and a discussion of the samples underlying each regression.
* Includes men who are currently out of work ($N = 29{,}121$).
† Includes men who were employed in the census week ($N = 96{,}070$).

leading to the creation of new firms catering to the local population (e.g., retail establishments). We investigate the relationship between net migration and the net creation of retail, wholesale, and manufacturing establishments in table 5. Our dependent variable is the change from 1935 to 1939 in the number of firms in a given sector per 1930 resident from the censuses of manufacturing and retail and wholesale trade. We find no response to net in-migration in the two sectors—wholesale trade and manufacturing—that primarily serve the national market. While the relationship between migration and the number of retail stores is positive, the coefficient is not statistically significant.

### D. Robustness Checks: Migration and Work Opportunities

Table 6 assesses the robustness of the two economic outcomes that we find to be significantly related to net migration: weeks worked during the year for employed men and the probability of holding a relief job, conditional on being out of work. By focusing on net migration, we have implicitly assumed that in- and out-migration have equal and opposite effects on the local labor market. Table 6 (cols. 1 and 2) reports coefficients from regressions in which we enter both in- and out-migration separately. In-migration diminished the work opportunities of existing residents, while out-migration buoyed them. We cannot reject the hypothesis that the in-migration and out-migration coefficients are equal in absolute value for each work opportunity measure.

The main specification uses predicted out-of-state migration to instrument for actual net migration in order to mitigate the possibility of spatially correlated economic shocks. Spatial correlation could lead to a spurious negative relationship between net migration and economic outcomes if cities receive more migrants when their own economy is doing poorly

**Table 7**
**Effect of Net Migration on Work Opportunities by Education Level**

| | Dependent Variable | |
| --- | --- | --- |
| Right-Hand-Side Variable | On Work Relief (If out of Work) | ln(Weeks Worked) |
| Net migration rate | −2.322 | −.611 |
| | (.865) | (.289) |
| Net migration × high school graduate | −.638 | −.660 |
| | (.824) | (.333) |

Note.—Standard errors are in parentheses and are clustered by metropolitan area. Regressions are estimated at the individual level and weighted by the inverse of the number of observations in the metropolitan area. See notes to tables 3 and 4 for a list of the individual- and city-level control variables and a discussion of the samples underlying each regression. The set of completed schooling dummy variables absorbs the main effect of being a high school graduate.

(because a neighboring economy is doing worse). Table 6 (cols. 3 and 4) presents results using either predicted total migration or predicted out-of-region migration instead. All three variants produce similar coefficients.

Depression-era migrants were better educated than nonmovers during the period. In our sample, migrants were twice as likely to have finished high school than the existing workforce (16.3% vs. 7.1%). As a specification check, table 7 allows the effect of in-migration to vary with a resident's education level. In addition to the main effect of net migration, we include an interaction between a metropolitan area's net migration rate and an indicator for holding at least a high school degree. If migrants are more likely to compete with similarly skilled workers, we should observe larger effects of net migration on high school graduates during this period. This should be particularly true in the nonrelief sector, in which a worker's skill is more likely to determine his pool of competitors. The evidence corresponds to these predictions. The effect of net in-migration on the number of weeks worked during the year is twice as large for high school graduates as for nongraduates, and the difference is statistically significant. However, we cannot reject that the impact of migration on the likelihood of holding a relief job is the same for graduates and nongraduates.

### E. Magnitudes in the Context of the Dust Bowl Migration

The largest migration of the Depression era was the flight from the dust storms that decimated agricultural production in the southern Great Plains (Hansen and Libecap 2004; Cunfer 2005; Hornbeck 2009). Many of the former residents of the Dust Bowl areas settled in California. As an illustration of the magnitude of our estimates, we use our coefficients to assess the impact of this migration on California's economy.

We start with a conservative estimate of the number of additional migrants to California from 1930 to 1940 generated by the Dust Bowl

(72,000).[28] Thirty-seven thousand of these migrants were men, representing a 1.8% increase in the state's population. How did this influx affect California's 1 million existing male residents? Our results suggest that, to begin with, a number of male residents who lived in California in 1935 would have left the state. An in-migration of this size would have encouraged 1.3% (0.727 × 0.018 in-migration rate; see table 5) of the state's existing male population, or 13,000 Californians, to relocate from their existing place of residence. Fifty-three percent of these displaced men would have left the state, with the remainder flowing to balance-of-state areas, resulting in 6,900 departures from California. This out-migration would have reduced California's net migration burden to 30,100 arrivals, the equivalent of 1.4% of the population.

In-migrants would also have crowded the queue for relief jobs. The Dust Bowl migration would have reduced the share of out-of-work men holding a relief job by 3.6 percentage points (-2.561 × 0.014 net migration rate; see table 4). This crowding out would have reduced the number of men on relief by 7,920 (220,000 out of work men × 0.036 share estimated to lose relief work). In addition, competition from new migrants would have reduced work opportunities: 86,000 male Californians, or 11.1% of employed men, were working 26 weeks or less in 1940. In-migration would have increased this share to 11.8% (0.528 × 0.014 net migration rate; see table 3), resulting in an additional 6,900 men engaged in job sharing.

Our estimates imply that the 37,000 male Dust Bowl migrants would have caused substantial economic change for a total of 21,700 of California's men. In other words, for every 10 migrant arrivals, nearly six male residents would have experienced some form of economic displacement: 1.9 existing residents would have left the state, 2.1 would have been prevented from finding a relief job, and 1.9 would have shifted from full-time to part-time work. Recall that the aggregate migrant counts include men who are either too young or too old to participate in the labor force. Only 74% of the new arrivals were prime-age men (18–64). Therefore, our results suggest that nearly six male residents would have experienced economic displacement of some kind for every 7.4 prime-age male arrivals.

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[28] While collectively—and pejoratively—referred to as "Okies," the Dust Bowl migrants hailed not only from Oklahoma but also from Arkansas, Missouri, and Texas. Gregory (1989) reports that 315,000 individuals from Arkansas, Missouri, Oklahoma, and Texas settled in California between 1930 and 1940, compared to 243,000 in the previous decade. These figures imply that the Dust Bowl was responsible for 72,000 additional migrants to California during the 1930s. We consider this number to be a conservative estimate because it assumes that the migration flow of the 1920s would have continued unabated in the 1930s if not for the Dust Bowl, a likely overstatement, given that migration rates fell nationwide during the Depression.

While the implied total displacement rate is large, it is not implausibly so; it appears that native workers respond to in-migration with a number of different adjustment mechanisms, including out-mobility, job sharing, and queuing for relief work.

## VI. Conclusion

Throughout American history, there has been extensive debate about the impact of immigrants on the economic status of native-born workers. The conversation has become particularly heated recently as the nation considers immigration reform. Economists working with modern data have not reached a consensus about the impact of immigrants on wages and employment.

This article studies the effect of internal migration in the 1930s on the economic welfare of residents in destination cities. Immigration from abroad slowed to a halt during this period because of both the Depression and the enactment of strict immigration quotas. Even so, residents in areas where the Depression was less severe protested the arrival of migrants from other less fortunate regions. The new arrivals were accused of taking jobs, lowering wages, and burdening relief budgets.

As is often the case in the modern literature, we find that the impact of in-migration on hourly and weekly earnings in the 1930s was small and not statistically significant. The lack of a wage response to migrant arrivals is consistent with the presence of downwardly rigid wages during the Depression. However, migrants threatened the economic prospects of longer-term residents in other ways. Residents of metropolitan areas that experienced high in-migration during the Depression decade worked fewer weeks during the year. Although the probability of obtaining regular employment did not fall, those who were out of work faced greater difficulty in securing a work relief position. Finally, as has been found in the modern era, greater in-migration stimulated out-migration by longer-term residents.

We document stronger effects of migrant arrivals on local labor market outcomes than is typical in the contemporary literature. This difference may be due to the historical context. Goldin (1994) also finds strong wage effects of international arrivals at the city level in the early twentieth century. As the labor market became nationally integrated over the century, the effect of local shocks may have dissipated more rapidly. Alternatively, because those internal migrants spoke English and were educated in American public schools, they may have been closer substitutes with existing residents.

Our results suggest that localized protests against in-migration could still occur today, even if our borders were completely sealed. What opponents of immigration often fail to realize is that disparities in labor

markets across geographic areas encourage people to migrate internally, thus affecting labor market outcomes. While the vehemence of protest might be dampened if newcomers resembled long-standing residents in terms of race, ethnicity, and citizenship, the economic impact of this migration may still generate opposition.

## Appendix

**Table A1**
**Summary Statistics at the Metropolitan Area Level**

|                                                         | Mean      | SD       |
| ------------------------------------------------------- | --------- | -------- |
| Dependent variable:                                     |           |          |
|   Annual earnings (2000 $)                    | 16,215.00 | 1,924.12 |
|   Weekly wage (2000 $)                        | 391.22    | 71.99    |
|   Weeks worked                                | 45.76     | 1.55     |
|   Work less than 26 weeks (conditional on being employed) | .111 | .030 |
|   Hourly wage (2000 $)                        | 10.10     | 2.39     |
|   Hours worked                                | 43.52     | 2.18     |
|   Idle (unemployed or out of the labor force) | .164      | .036     |
|   On work relief                              | .056      | .027     |
|   On work relief (if out of work)             | .248      | .098     |
|   Left standard metropolitan area, 1935–40    | .102      | .049     |
|   Self-employed                               | .135      | .035     |
|   No. establishments per 1,000 (1935):        |           |          |
|     Retail                          | 10.56     | 2.470    |
|     Wholesale                       | 1.56      | .622     |
|     Manufacturing                   | 1.28      | .480     |
| Explanatory variable (1935–40):                         |           |          |
|   In-migration rate                           | .053      | .033     |
|   Out-migration rate                          | .067      | .027     |
|   Net migration rate                          | −.014     | .021     |
| Instruments (predicted rate):                           |           |          |
|   In-migration, all                           | .014      | .010     |
|   Out-migration, all                          | .015      | .007     |
|   In-migration, out of state                  | .005      | .004     |
|   Out-migration, out of state                 | .006      | .004     |
|   In-migration, out of region                 | .003      | .002     |
|   Out-migration, out of region                | .003      | .002     |

NOTE.—Individual-level wage and employment information presented as metropolitan area averages. Each row has 69 observations.

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