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Local Labor Market Scale, Search Duration, and Re-Employment Match Quality for U.S. Displaced Workers

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LOCAL LABOR MARKET SCALE, SEARCH DURATION, AND RE-EMPLOYMENT MATCH QUALITY FOR U.S. DISPLACED WORKERS

BY

KELLY RAY WILKIN

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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ABSTRACT

LOCAL LABOR MARKET SCALE, SEARCH DURATION, AND RE-EMPLOYMENT MATCH QUALITY FOR U.S. DISPLACED WORKERS

BY

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Geographic space is an important friction preventing the instantaneous matching of unemployed workers to job vacancies. Cities reduce spatial frictions by decreasing the average distance between potential match partners. Owing to these search efficiencies, or economies of scale, theories of agglomeration predict that unemployed workers in larger labor markets find employment more quickly than observationally similar workers in smaller markets.

Existing studies typically rely on cross-sectional variation in aggregate unemployment rates across spatially distinct labor markets to test for scale effects in job search. A major difficulty with these studies is that the unemployment rate is, at any given time, simultaneously determined by net flows into (incidence) and out of (duration) unemployment. Therefore, conclusions about unemployment exits using the unemployment rate are confounded by transitions into unemployment.

This dissertation examines the relationship between market scale and the duration of unemployment for permanently laid off workers in the U.S. Using a large sample of individual unemployment spells in 259 MSAs, Cox proportional hazard model estimates predict a strong negative relationship between local market scale and the hazard of exiting unemployment. This effect is strengthened when space is explicitly controlled for and measured with greater precision. These results are consistent with the hypothesis that unemployed workers react to search efficiencies by raising increasing their reservation wages.

Using a 2SLS framework, we show that re-employment earnings for permanently laid off workers increase with market scale *after* controlling for endogenous search duration. These effects are robust to standard demographic and educational controls, as well as controls for local labor market conditions. These results challenge the view that search efficiencies lead to lower unemployment rates through faster job-finding rates of permanently laid off workers.

PREFACE

Economists have long recognized the efficiency gains that occur when trading partners locate close together in geographic space, such as in cities. These efficiency gains, called *agglomeration economies*, arise from proximity which reduces the cost of transacting over physical space. While advancements in communications and commuting technologies have, to a large degree, reduced the costs of moving goods over long distances, the cost of moving people remains high (Glaeser, 2010, p. 7).

Distance between workers and firms plays an important role in the level of frictional uemployment in a labor market. Frictional unemployment arises because jobs are continuously created and destroyed, information about location and characteristics of workers and jobs is imperfect, and it takes time for workers and firms to find one other (e.g., Ehrenberg and Smith, 2003). Proximity makes it easier to find information about jobs through word of mouth or a shopping externality, as well as being able to undertake more formal job search activities like visiting firms to fill out an application or participate in interviews.

One source of agglomeration economies is *labor pooling*. Labor pooling asserts that workers (firms) benefit by being in a market with many employers (workers) because they can change jobs (fill vacancies) without changing locations (e.g., Krugman, 1991; Glaeser, 2010). As a result, unemployment rates are predicted to be lower in larger labor markets since workers who lose jobs at contracting firms have many potential employment opportunities at nearby expanding firms and thus ought to find work more quickly than those in less populated markets where opportunities are scant.[1]

[1] The labor pooling hypohesis assumes that productivity shocks are random and uncorrelated across firms within a location (Krugman, 1991).

Existing studies test the labor pooling hypothesis by comparing aggregate unemployment rates to population levels across spatially distinct labor markets.[2] The finding that unemployment rates are inversely related to city size is taken as evidence of labor pooling that works through a faster job-finding rate. While this seems to be a reasonable first step, two issues arise with this approach. First, the unemployment rate at any given time is determined by the gross flows of workers into (*incidence*) and out of (*duration*) unemployment from the previous period.[3] Any observed relationship between city size and the unemployment rate may be due to greater job-finding rates of unemployed workers (shorter durations) or less incidence of unemployment in larger markets, neither of which can be isolated from the other using aggregate unemployment rate data.[4]

Second, studies based on aggregate data ignore the tradeoffs that individual workers face in choosing to accept one job over another following a job loss. For example, it is often argued that the labor pooling hypothesis may operate between firms within the same industry or between industries.[5] However, workers with high levels of accumulated human capital within a particular industry or occupation, seniority or union rents, may be willing increase their search durations in order to find a job where they can transfer more of their human capital which translates to higher earnings on re-employment. There is a tradeoff, then, between search duration and re-employment earnings. If search costs are lower in larger, more compact cities, then workers can afford to search for longer periods which should lead to a more productive worker-firm match. Therefore, the effect of scale on unemployment duration may be to actually lengthen spell durations. If so, then

[2]Simon (1988) and Diamond and Simon (1990) find that local unemployment rates vary negatively with industry diversity. To the extent that diversity increases with city size, these results may point to a similar relationship. Diamond and Simon (1990) report Herfindahl index calculations, which measure the degree of industrial specialization, for 43 MSAs and find no pattern in specialization and size. While it is typically assumed that the variability in worker and job types increases with city size (e.g., Sato, 2001), more work needs to be done to measure this empirically.

[3]Gross flows are described in detail in Perry et al. (1972) and Hogue and Flaim (1986).

[4]To the extent that worker-firm match quality improves in larger markets (e.g., Kim, 1989, 1990), expect incidence may vary inversely with size.

[5]Agglomeration economies within industries are referred to as *localization* economies while agglomeration economies that accrue between industries are called *urbanization* economies.

the conclusion that the negative association between city size and unemployment is through the rate of job finding may not be correct.

This disseration analyzes the relationship between the scale of an unemployed worker's local labor market and the expected duration of unemployment, focusing in particular on the role of labor market density in affecting the contact rate between unemployed workers and potential job opportunities. Density is assumed to be the appropriate measure of "scale" in the contact of job search because it captures the rate of contact between search partners (Teulings and Gautier, 2003, 2004). Because density is directly related to the number of searching parties in the economy, we also compare the effects of market scale when measured using city size to density, showing that indeed density has a greater effect on search durations.

We begin by developing a highly stylized model of job search, which characterizes and individual unemployed worker's decision to accept an offer for employment or continue looking for a better one. We show that density, through an increase in the contact rate between workers and firms, leads to two offsetting effects on expected search durations: the *direct effect* on the contact rate, which tends to shorten durations by increasing the rate at which workers sample wage offers from firms, and the *indirect effect* where workers endogenously revise upward their reservation wage requirements associated with the lower costs of making a contact. The total effect on the expected duration of unemployment depends on which of these two effects dominates, which cannot be answered analytically. We estimate a proportional hazard model to estimate the total effect of scale on the job-finding rate of unemployed workers.

One difficulty in testing this relationship is collecting data on individual unemployment spells across numerous spatial labor markets. First, identifying and accurately measuring unemployment durations requires following individuals over time and carefully observing the timing of their movements into and out of unemployment. Longitudinal studies, such as the Survey of Income and Program

Participation (SIPP) or the Panel Study of Income Dynamics (PSID), are ideally suited for measuring spell durations but they are particularly poor at offering enough observations to evaluate relationships involving very disaggregate data.[6] Further, because longitudinal surveys often collect much more detailed on any individual, their residential location is typically suppressed to the state or regional level to protect the identities of respondents.

To get around this issue, we use longitudinally matched consecutive monthly pairs of the Current Population Survey (CPS). In any month, the CPS interviews roughly 60,000 U.S. households. Each household remains in the survey for a total of eight months, where they are in the survey for four consecutive months, out for the next eight months, and back in for four consecutive months. It is possible to identify specific individuals within each household and, if they complete the interview each period, follow them over time. The basic function of the CPS is to identify labor market activity in the U.S., thus it collects detailed labor market information including labor force status (i.e., employed, unemployed, or not in the labor force), industry, occupation, demographic characteristics, and the duration of unemployment for workers who are determined to be without work but looking for a job. In addition, the CPS reports the Metropolitan Statistical Area (MSA) of residence, if any. Thus, we identify individual spell durations as a movement from unemployment in one month to employment in the next month. Further, since observed transitions occur within a single labor market (MSA), we can attach measures of labor market scale to individual spell durations.[7]

We develop measures of labor market scale for 259 MSAs. Scale is measured as the size of the labor force in each MSA as well as three different measures of density. Density is calculated as the number of workers per square mile. We use a novel approach to measure MSA area, relying on NASA satellite data to identify the geographic extent of an urban area. MSA boundaries are determined by the

[6] The administrative costs of longitudinal surveys are increasing in sample size and frequency.

[7] MSA location is based on the household at which the CPS is conducted. It is possible that individuals may live in one MSA but commute to another for work or job search activities. There is no way to discern the location of employment or search area in the CPS.

boundaries of their component counties which are tied to historical and political decisions, not the actual extent of urban development. We show that density measures that rely on political boundaries tend to overstate land area and thus understate measures of density.

We find strong evidence that workers in denser areas search for longer durations than observationally similar workers in less dense areas. We also find that controlling explicitly for the geographic area over which search takes place reinforces the positive relationship between market scale and search duration. Further, the effect is strengthened when density is measured with greater precision.

In the context of the search model, this result suggests that the indirect effect (or reservation wage response) outweights the direct effect. That is, workers choose to search for longer periods in denser areas but it pays off in higher earnings following unemployment. These results contradict the view that the negative relationshp between market scale and unemployment rates is due to faster job-finding rates of unemployed workers.[8]

We then extend the analysis to the Displaced Workers Supplement (DWS), a biennial supplement to the CPS that identifies individuals who have lost a job due to a plant closing, slack work, or abolition of shift. The major advantage of the DWS is that it records detailed information on the type of displacement as well as earnings, industry, occupation, union status, and job tenure on the pre-displacement job and re-employment job (if any). This allows us to estimate the effect of market scale on earnings conditional on search duration. We show that search duration and a worker's choice of reservation wage are simultaneously determined. To deal with simultaneity, we estimate a re-employment earnings equation via two-stage least squares using predicted values from a first-stage unemployment duration equation. We estimate an elasticity of density and re-employment

[8]We report a least squares dummy variable model of unemployment rates and density in Appendix A. We show that, even after controlling for MSA-specific fixed effects and time, there is a negative relationship between density and the unemployment rate.

earnings to be around 0.10 holding constant spell duration and observed and unobserved heterogeneity.

In addition, the DWS collects information relating to the unemployed worker's receiving unemployment insurance (UI) benefits and whether those benefits were exhausted, variables that are not available in the basic monthly CPS. This allows us to control for the effect of UI on the hazard of exiting unemployment. If urban areas are associated with more generous UI policies and UI receipt is associated with longer spell durations, then the results from the basic CPS may be picking up this effect. However, we find that the density effect on the probability of exiting unemployment is robust and similar in magnitude to that estimated from the CPS. Thus, MSA generosity in UI are not thought to be driving the observed negative relationship.

The remainder of the dissertation is organized as follows. Chapter I presents the basic job search model and develops the conditions under which expected spell durations are increasing or decreasing with respect to market scale. We introduce the data and methods used to construct the distribution of completed spells and measures of local labor market scale. It presents both continuous- and discrete-time proportional hazard model specifications and empirical results. In Chapter II we introduce the DWS and present proportional hazard and 2SLS model estimates. Chapter III discusses model estimates after controlling for local labor market conditions, such as changes in the number of establishments and the relative share of industry and occupational employment in a worker's own industry.

Chapter I

UNEMPLOYMENT DURATION AND LABOR MARKET SCALE

1 Introduction

This chapter introduces the basic theoretical and empirical methods used to analyze the relationship between labor market scale and the duration of unemployment. We begin by introducing a very simple and highly stylized job search model which describes the behavior of an individual unemployed worker looking for a job. The job search model helps us understand how changes in the unemployed worker's local labor market affect individual search strategies and ultimately determine the expected duration of unemployment. The key assumption is that density of economic activity in a labor market reduces the average distance between workers and firms and therefore increases the rate of contact between them. We establish the well-known result that a change in the contact rate has offsetting effects on expected spell durations (e.g., van den Berg, 1994). While the search model offers no definitive analytical prediction of the relationship between density and duration, it can be used to inform empirical estimates and thus paint a clear picture of how scale influences the behavior of unemployed workers in the U.S.

We then introduce the data used to test theoretical predictions from the job search model. We address some of the unique features associated with measuring individual spell durations, in particular how they are identified and the problem of censoring. Unemployment data typically come from household surveys which are administered by federal statistical agencies, such as the Bureau of Labor Statistics

(BLS), which is part of the U.S. Department of Labor, and the U.S. Census Bureau to name a few. We use data from the Current Population Survey (CPS), which is the primary survey used to measure labor market activity in the United States. By longitudinally matching pairs of consecutive monthly surveys, we construct a sample of over 81,000 unemployment spells in 260 spatially distinct labor markets.

Economists are particularly interested in the private costs of involuntary job loss. Displaced workers–those individuals who lose their job through a plant closing, abolition of shift, or permanent layoff–tend to experience long periods of joblessness and large wage losses upon re-employment (Farber, 1993, 2011; Fallick, 1996). We show that displaced workers do indeed experience longer durations of unemployment and are more likely to be employed for longer than 26 weeks (the official measure of long-term unemployment) than workers who quit or are temporarily laid off with the expectation of recall. These differences are shown to persist across the business cycle and density of a worker's local labor market.

Unemployment duration resulting from displacement is also attractive for empirical analysis. Quits represent voluntary, or worker-initiated, separations. From the worker's perspective, then, unemployment is a choice variable. This self-selection can pose problems for empirical analyses where the decision is not fully modeled. Workers who are laid off with an expectation of recall (so-called temporary layoffs) face very different search strategies that are largely based on the layoff and hiring decisions of firms. Displacements, on the other hand, are unexpected shocks to a worker's unemployment status.[9] Thus, we can think of displacements as an exogenous sample of unemployed workers.

Economists typically think of cities as being spatially distinct labor market areas. A major challenge in measuring labor market density is correctly determining the geographic space occupied by cities. Density is routinely calculated as the number of individuals per unit of area. Any measurement error in urban area boundaries will understate or overstate density. City boundaries are not

[9]The Displaced Workers Supplement used in Chapter II show that only 35 percent of urban displaced workers received advanced notice of their job loss.

easily identified and may be based on distinct municipal rules that may change frequently as the city expands or contracts. One standardized measure of cities is the Metropolitan Statistical Area (MSA). MSAs are a collection of counties where one county contains an urbanized core containing 50,000 or more residents and other counties are included based on sufficient commuting flows to the urban core.[10] MSA component counties may change over time but county definitions rarely change and measuring them over time is less costly. However, counties are arbitrary political boundaries that may not accurately measure the extent of the urban area. This problem is particularly severe in western states where states tend to have fewer but much larger counties. To get around this issue, we combine MSA definitions with satellite imagery to identify the developed, or urban, portions of MSAs. The satellite imagery is precise within fine levels of spatial measurement and allow for a more accurate measure of a city's footprint.

We compare these measures to alternative measures, such as county-based MSA area measures and urbanized areas, showing the relationship between density and duration is strengthened when density is measured with greater precision. In addition, we compare the density effect to city size. We show that density has a much stronger effect on duration than city size, indicating that physical space is an important dimension affect search behavior.

Finally, we introduce the proportional hazards estimation framework, a regression-based approach that makes it possible to relate individual spell durations to observable worker and labor market characteristics. Specifically, it allows us to relate measures of labor market scale to the probability that a worker will exit unemployment, or the hazard rate, at various spell durations. A useful feature of this approach is that we can generate estimates of the effect of density on hazard rates without any explicit knowledge of the hazard function. We use our survey and

[10]United States Census Bureau, "Metropolitan and Micropolitan Statistical Areas Main," United States Census Bureau, http://www.census.gov/population/metro/ (accessed May 26, 2012).

density measures to examine how unemployed workers search behavior is affected by density.

The rest of this chapter is organized as follows. Section 2 presents the job search model and develops the theoretical relationship between average spell duration and density through its effect on job-offer arrival rates. Section 3 discusses the individual unemployment spell data and the data used to calculate labor market density. Section 4 discusses the proportional hazards framework and derives the maximum likelihood estimator.

2 Job Search

In this section, I develop a very simple and highly stylized *job search* model in the vein of Lippman and McCall (1976).[11] The job search model characterizes the unemployed worker's decision of how long to search for a job in a situation of uncertainty. Uncertainty arises from the worker's lack of information about which firms in their local labor market are offering the highest wages. Workers know that there are firms looking to fill vacancies at various wage rates and they know the distribution of those wages. Workers can sample from the wage offer distribution by making contact with a firm, which occurs through search. Workers would prefer to search until they find the best wage offer, but search is costly and they can only contact a finite number of firms each per unit of time. Workers face a tradeoff between the costs of search and the prospect of finding a higher wage offer. In equilibrium, workers adopt a search strategy of setting a constant *reservation wage* that equates the marginal benefit of search to the marginal costs and accepting the first offer that meets or exceeds it.

Equilibrium reservation wages in the basic model depend primarily on the wage offer distribution, arrival rate of job offers, job-destruction rates, real interest rates, non-employment income, and search costs. Here, we extend the basic model to allow for the job-offer arrival rate to be a function of labor market scale. Labor

[11] I follow the notation supplied in Cahuc and Zylberberg (2004, ch. 3). A nice survey of job search theory is provided by Rogerson et al. (2005)

market density increases the job-offer arrival rate and reduces search costs, altering the incentives for continued search versus accepting a given wage offer.

Consider an economy consisting of a finite number of distinct labor markets, $m = 1, 2, \ldots, M$. Labor is the only commodity traded in each market. Labor markets differ only by density ρ_m, which is defined as the number of workers and firms per unit of geographic area over which labor is traded. The job-offer arrival rate λ measures the rate of contact between workers and firms in a labor market m. By definition, the average distance between trading partners increases with density. The job-offer arrival rate is then a function of density, $\lambda = \lambda(\rho)$ where $\lambda' > 0$. Workers are assumed to be mobile within labor markets but immobile between them in the short run.

We assume that workers and jobs are homogeneous. There is no on-the-job search and the acceptance of an employment offer results in a contract for employment indefinitely at wage w. For a finite interval of time dt, the discounted expected utility $V_e(w)$ of an employed worker earning wage rate w is

$$V_e(w) = \frac{1}{1 + rdt} \left[wdt + (1 - qdt) V_e(w) + qdtV_u \right] \tag{1}$$

which is equal to the present discounted value of instantaneous wage earnings in the current period plus expected income in the future. Future income is the expected value of staying employed at w plus the utility associated with being unemployed V_u which occurs with probability qdt. We can simplify this relationship by multiplying both sides of equation (1) by $1 + rdt$ and rearranging terms

$$rV_e(w) = w + q \left(V_u - V_e(w) \right) \tag{2}$$

which states that the discounted flow of employment income in each period is equal to the instantaneous earned wage plus the expected income arising from a termination of the employment relationship.

Subtracting rV_u from both sides of equation (2) yields an expression for the difference between the expected value of employment and unemployment

$$V_e(w) - V_u = \frac{w - rV_u}{r + q}. \tag{3}$$

Equation (3) shows that the difference in expected income from employment at wage w and unemployment is non-negative if and only if $w \geq rV_u$. That is, a worker is indifferent between employment at w and unemployment when $V_e(w) = V_u$. As long as the instantaneous wage earned w is greater than or equal to the flow income from unemployment rV_u the worker will be no worse off from accepting a job offer than he would remaining unemployed. The wage that satisfies this condition is the *reservation wage*

$$w^* = rV_u. \tag{4}$$

Equation (4) is referred to as a *stopping rule* because it determines the point at which the worker will choose to stop searching and accept employment at w. So long as a given wage offer $w \geq w^*$ the worker is better off by accepting the wage offer than remaining unemployed.

To get an explicit expression for the reservation wage w^* in terms of exogenous parameters we need to model the expected utility of an unemployed worker V_u. While unemployed a worker receives non-employment income $b > 0$, such as unemployment insurance benefits or other sources of non-wage income. Search is costly and workers incur costs $c > 0$, which may include direct out-of-pocket expenditures (e.g., printing résumés, fuel for driving to job interviews) and the opportunity costs of time, foregone consumption of leisure, or foregone earnings from employment at a previously rejected wage offer. Unemployed workers make contact with vacancies at a rate of $\lambda(\rho)$ per period. The expected utility of an unemployed worker is thus

$$V_u = \frac{1}{1+rdt}\left[(b-c)dt + \lambda(\rho)dtV_\lambda + (1-\lambda(\rho)dt)\,V_u\right] \tag{5}$$

where V_λ is the expected utility from making contact with a firm and $b-c$ is the instantaneous net benefits of search. Workers and firms make contact with a probability $\lambda(\rho)$ per unit of time dt. With probability $1-\lambda(\rho)$ the unemployed worker is not able to contact any firms. In this case, the worker receives utility V_u until the next period when he searches again. Multiplying both sides by $1+rdt$ and rearranging terms yields

$$rV_u = b - c + \lambda(\rho)\left(V_\lambda - V_u\right) \tag{6}$$

which states that the flow value of unemployment income is equal to the instantaneous net benefits of search plus the expected value of receiving an offer.

The worker doesn't know the exact wage offer for a given job until he makes contact with a firm. The cumulative distribution of wage offers in the local market is $F(w)$ and is known to the worker. Contact with a firm is the same as a worker taking a sample from $F(w)$. Upon meeting with a vacant firm, an offer for employment is proposed at some wage rate w. As we saw in equation (4), it's rational for a worker to accept wage offers such that $w \geq w^*$. The expected value of receiving an offer is equal to

$$V_\lambda = \int_0^{w^*} V_u\,dF(w) + \int_{w^*}^{\infty} V_e(w)\,dF(w). \tag{7}$$

Substituting equation (7 into equation (6) and rearranging terms we arrive at a simplified expression for the flow value of unemployment income

$$rV_u = b - c + \lambda(\rho)\int_{w^*}^{\infty}\left[V_e(w) - V_u\right]\,dF(w) \tag{8}$$

which is equal to the instantaneous net benefit associated with unemployment and the expected value of receiving an offer of employment.

Now that we have expressions for the discounted expected utility of employment, unemployment, and an equation characterizing the optimal stopping rule for an unemployed worker, we can derive an expression for the reservation wage

$$w^* = b - c + \frac{\lambda(\rho)}{r+q} \int_{w^*}^{\infty} (w - w^*)\ dF(w). \tag{9}$$

It is convenient to use integration by parts[12] on equation (9), yielding

$$w^* = b - c + \frac{\lambda(\rho)}{r+q} \int_{w^*}^{\infty} [1 - F(w)]\ dw. \tag{10}$$

Equation (10) implicitly describes the reservation wage as a function of all exogenous variables. Therefore, equilibrium reactions of an unemployed worker's search strategy (i.e., choice of reservation wage) can be analyzed by setting equation (10) equal to zero and applying the Implicit Function Theorem.

2.1 Hazard Rates and the Average Duration of Unemployment

The probability that an unemployed worker will transition to employment in any period is called the *hazard rate*. The hazard rate H is equal to the probability of receiving an offer multiplied by the probability that a given wage offer is acceptable, or

$$H = \lambda(\rho)\left[1 - F(w^*)\right]. \tag{11}$$

Since the hazard rate measures the rate of unemployment-to-employment transitions over a fixed time interval it follows a Poisson process (e.g., Lancaster, 1990). The average duration D of an unemployment spell that has a probability H of

[12]See Rogerson et al. (2005), for example.

ending per time period t is then (e.g., Rogerson et al., 2005)

$$D = \int_0^\infty tHe^{-Ht}\,dt = \frac{1}{H}. \tag{12}$$

The relationship between market scale and average unemployment duration is shown by taking the partial derivative of equation (12) with respect to labor market scale ρ

$$\frac{\partial D}{\partial \rho} = -\frac{1}{H^2}\left(\frac{\partial H}{\partial \rho}\right) \tag{13}$$

where the relationship depends on the sign of $\partial H/\partial\rho$. If scale and the hazard rate are positively related the average duration of unemployment will decline, whereas average duration will lengthen if the relationship is positive.

Taking the partial derivative of H with respect to ρ and rearranging terms yields

$$\begin{aligned}\frac{\partial H}{\partial \rho} &= \frac{\lambda'(\rho)}{\lambda(\rho)} + \frac{dw^*}{d\rho}\left[\frac{\frac{\partial}{\partial w^*}(1-F(w^*))}{(1-F(w^*))}\right] \\ &= \varepsilon_\lambda + \frac{dw^*}{d\rho}\varepsilon_{w^*}\end{aligned} \tag{14}$$

where the first term on the right side of the equality is the elasticity of the job-offer arrival rate in terms of market scale ε_λ, and the second term is the elasticity of the wage offer distribution with respect to changes in the reservation wage (the term in parentheses) weighted by the total derivative of the reservation wage with respect to ρ. The sign of $fracdw^*d\rho$ is unambiguously positive.[13] Therefore, the sign of equation (14) depends on the signs of ε_λ and ε_{w^*}. By assumption $\lambda'(\rho) > 0$, and then $\varepsilon_\lambda > 0$.[14] For a fixed wage offer distribution $F(w)$, any increase in the reservation will decrease the complement of the cumulative wage

[13] See Appendix B for derivation of $fracdw^*d\rho$.

[14] Seater (1979) shows that the contact rate increases with density. Barron and Gilley (1981) offers empirical evidence, showing in a CPS special supplement on job search activity that unemployed workers in MSAs contacted 10 percent more vacancies than similar non-MSA unemployed workers.

distribution $1-F(w^*)$, thus $\varepsilon_{w^*} < 0$. It follows that the total effect of market scale on the expected duration of unemployment depends analytically on the relative magnitudes of the "direct" effect ε_λ of scale on the job-offer arrival rate and the "indirect," or "endogenous," reservation wage response to a change in labor market scale.

Although equation (14) doesn't provide a simple analytic solution it is still quite useful for informing and analyzing empirical results. The hazard rate is increasing in ρ (thus expected duration D is decreasing) when $\varepsilon_\lambda > (dw^*/d\rho)\varepsilon_{w^*}$ and decreasing (D increasing) in ρ when $\varepsilon_\lambda < (dw^*/d\rho)\varepsilon_{w^*}$. Any observed positive relationship between market scale and average duration may be due to a greater endogenous reservation wage response to higher contact (job-offer arrival) rates, which motivates the need for the collection and analysis of data that can test for those effects. But we begin by trying to find the empirical sign of $\partial H/\partial\rho$.

3 Data

This section discusses the data used to test for the empirical relationship between duration and market scale. Individual unemployment spell data is taken from the Current Population Survey (CPS). The CPS provides a large, nationally representative survey of the US population. It collects information on the duration of in-progress spells for those who are unemployed, as well as the MSA in which the households are located. This is a major advantage of the CPS. We also present the data sources and methods used to identify local labor market areas and compute measures of market scale.

3.1 Individual Unemployment Spells

Individual unemployment spell data are taken from the basic monthly files of the CPS for the period January 1994 to February 2012. The CPS is a monthly survey of roughly 60,000 households and serves as the primary source of information of labor market activity in the United States. Households are followed in the survey for a total of eight months: in for four consecutive months, out for the next eight months, and back in for the subsequent four consecutive months. While the CPS is typically used cross sectionally (e.g., unemployment rates), its rotating structure can be used to longitudinally match individuals over time.

The CPS collects detailed information on the labor-market activity of household members in the survey. In any month t, working-age individuals (those aged 15 and up) are asked a series of questions that are used to classify them as being in one of three labor market states: employed (E_t), unemployed (U_t), or not in the labor force (N_t).[15] By matching individuals across consecutive survey months and comparing their labor market status over time, it is possible to identify individual transitions across labor market states. In the aggregate, these movements–called *gross flows*–are often used to understand determinants of the overall unemployment rate (e.g., Fallick and Fleischman, 2004). But transitions of individual workers from unemployment to employment can be combined with detailed location information to analyze the effects of local labor market conditions on average duration of unemployment spells.

The CPS relies on unique household and person identifiers that make identification of particular respondents over time relatively straightforward. However, sample attrition from unit non-response, mortality, geographic mobility, as well as data recording errors in household and personal identifiers, make the task more difficult (e.g., Abraham and Shimer, 2001). By comparing observed individual characteristics over time, one can correctly identify whether matched respondents are actually the same individual. Conceivably one could ensure that individuals are the same by requiring many observable characteristics to match up, but random reporting errors or unit or item non-response creates a tradeoff between matching efficiency and sample size.[16] I use a variation of the Madrian and Lef-

[15]Labor market states are recorded based on the individual's status in the *reference week*, or the week prior to the interview. Interviews are typically conducted around the 19th day of each month, thus the reference week usually includes the 12th day of each month (U.S. Census Bureau, 2006, p. 5-2).

[16]Item non-responses are assigned imputed (allocated) values from a donor, thus leading to mismatches on variables not used in the hot deck imputation procedure.

gren (1999) matching algorithm to identify matches, which balances the need to identify matches without imposing too strict a matching criteria.

The matching algorithm is implemented as follows. First, CPS surveys for two consecutive months, t and $t+1$, are stacked and sorted by unique person identifiers.[17] Cases where an individual is observed in only one of the two months are discarded. Second, the respondent's sex, race, and age are compared between each survey month. To be considered a valid match, the sex and race must be identical across survey months and age in $t+1$ must be no more than one year greater than the reported age in month t.[18] This algorithm is able to match about 70 percent of all possible matches (Abraham and Shimer, 2001).[19]

Individuals unemployed in month t also report the duration of their current, in-progress unemployment spell (measured in weeks). When that same individual is interviewed in the following month $t+1$, their labor force status is updated. If the respondent is still unemployed and held no job since the previous interview date, the number of weeks elapsed since the last survey date are automatically added to their previously reported duration.[20] If the respondent is employed or is no longer searching for a job in $t+1$ then no duration value is recorded; from the perspective of the CPS, the unemployment spell is no longer active. We use unemployment-to-employment, or UE, transitions as the set of completed unemployment spells.

There are several data issues associated with using the CPS to measure the distribution of unemployment spells. First, the CPS doesn't continuously monitor individuals; rather it interrupts spells in progress. We observe when a spell begins

[17]Unique person identifiers are constructed by concatenating the following fields: HRHHID, HRSAMPLE, HRSERSUF, HUHHNUM, and PULINENO. More information on these fields is available in the basic monthly CPS data dictionary (e.g., `http://smpbff2.dsd.census.gov/pub/cps/basic/201001-/jan10dd.txt`).

[18]Madrian and Lefgren (1999) specify an age difference of no more than two years given they are matching outgoing rotation groups which are one year apart. For our purposes, a one-year age difference is reasonable given that only one month separates observations.

[19]It is not possible to match survey months from June 1995 to October 1995 due to a change in the household identifier. In addition, substate areas are suppressed in the public-use files from June 1995 to August 1995. See, for example, `http://www.census.gov/apsd/techdoc/cps/dec94/usernote.html`.

[20]This is a result of dependent interviewing which was implemented in the CPS beginning in January 1994.

but only observe the interval over which it ends, namely sometime between t and $t+1$. This source of measurement is caused by *interval-based sampling.* We assume that the unemployment spell ends at the midpoint between the two survey months and therefore add two weeks to the reported duration in month t. In addition, individuals may drop out of the survey before the ending of their unemployment spell. This problem is known as *right censoring.*[21]

The other issue is sample selection induced by *length-biased sampling.* The CPS measures unemployment durations in weeks. This means that very short spells are much less likely to be observed. Take, for example, an individual who is observed EE between months t and $t+1$. It is possible that the worker may have lost his job after the survey in t but quickly found work before being observed again in $t+1$. The intervening unemployment spell is unobserved.[22]

While these issues present some level of concern when using the CPS to identify unemployment durations, there is simply no other survey that provides information on individual unemployment durations over such a long time period and covering as many spatially distinct labor markets. Longitudinal data sets, such as the Survey of Income and Program Participation (SIPP) or the Panel Survey of Income Dynamics (PSID), have the advantage of following the same individuals over time with greater frequency. However, this level of individual detail comes at a cost of being able to follow fewer individuals due to the increased burden for survey administrators and respondents. Moreover, in these smaller surveys, geographic information at the MSA level is typically restricted from public-use files to protect the anonymity of survey respondents, making it impossible to examine conditions of a worker's local labor market without obtaining special permissions.

The matched CPS sample is restricted to the experienced labor force age 20 to 65 who live in MSAs. While the CPS does not identify individuals in very small MSAs (typically those below about 100,000 residents), about 70 percent of

[21]Left censoring, or when the beginning of spell duration is not observed, is less of a problem in the CPS.

[22]See Kiefer (1988) for an excellent discussion of the issues associated with measuring duration data.

the labor force reside in MSAs identified in the CPS. We exclude unemploymed individuals who lost temporary jobs, new entrants, and re-entrants to the labor force. We retain those who are unemployed through temporary layoffs and quits. While our interest lies with permanent layoffs, this allows us to show descriptively how displaced workers' unemployment outcomes fare compared to other types of unemployment.[23]

Transitions that occur between the household's fourth and fifth month in sample (MIS) are excluded. There are eight months in between the fourth and fifth month where households are not observed. These spells are more likely than others to be censored or contain unobserved UE transitions and predicting the termination date will introduce considerable measurement error.

Another issue to deal with is multiple spells. A maximum of six labor market transitions may be observed for each individual after deleting transitions that occur between months four and five. We restrict each individual to having only one spell, which removes any undue influence of a single individual on results. If multiple UE transitions are observed, the first one is retained and the rest are deleted. If individuals have a combination of UE and UU spells, the first UE spell is retained and all the rest are deleted. Finally, if a worker is only observed being unemployed (i.e., UU spells), then the most recent transition is retained. In many cases, multiple UU spells may be part of the same spell of unemployment, but other transitions are possible if they occurred while the household was not observed in the CPS.[24]

We identify 230,173 two-month labor market transitions in the January 1994 to February 2012 CPS sample. Of these, 134,879 spells are retained such that a

[23]In addtion, these comparisons will reinforce the notion that workers on temporary layoff, permanent layoff, and those who quit face different search strategies which should be reflected in their relative search outcomes.

[24]This analysis makes no attempt to model out-of-labor-force transitions, such as UN or NE. Clark and Summers (1979) show that temporary spells out of the labor force may actually be part of a single spell of unemployment (for example, three-month transitions that follow a UNU or UNE pattern). Rothstein (2011) examines the effects of UI insurance on spell duration using linked three-month panels in the CPS, but makes no comparison of the results using two- or three-month transitions.

single spell is observed for each individual. Of the 95,294 spells that are deleted: (1) 62,049 are cases of multiple UU spells for individuals whose spells are censored (i.e., no observed UE transition), (2) 1,830 are cases of multiple UE transitions with no observed UU transition, (3) 27,926 are UU observations for individuals that are also observed with a single UE transition, and (4) 3,439 are lost from individuals who are observed having two or more UE transitions and at least one observed UU transition. Most of the sample loss (94 percent) is due to the deletion of multiple UU spells that may be part of a single unemployment spell or a censored spell following an observed UE transition (i.e., items (1) and (3)).[25] Less than 6 percent of the lost sample constitutes a UE transition (item (2) and a fraction of item (4)). Because an individual is observed in our sample for a maximum of 12 consecutive weeks, we are confident in deleting the second observation of a UE transition since these are likely to comprise very short spells and are unlikely to be representative of the population.

Finally, we omit spells that are part of the inexperiened labor force, workers who are self-employed or worked without pay on their pre- or post-displacement job, and the non-MSA sample. In addition, we delete observations with imputed full- and part-time status and industry and occupation on either the pre- or post-displacement job. We also delete observations with unemployment durations less than two weeks (319 spells) and one observation with a reported month in sample equal to 1 as they are likely to be coding errors). The sample excludes workers in agriculture, forestry and fishing, mining, and those who are in the armed forces.[26] We restrict the sample to the contiguous 48 states which results in the omission of 1,118 spells in Anchorage, AK MSA and Honolulu, HI MSA. Finally, we delete 1,991 spells show an industry or occupation change for workers with censored spells. The final sample comprises 81,446 individual unemployment spells.

[25] Multiple observations of a single unemployment spell are likely to be captured by item (1). Item (3) contains UU transitions that may occur as part of a single spell preceding the observed UE transition or potentially UU spell(s) following an observed UE transition.

[26] Workers with reported detailed industry code equal to 3990 are omitted since a specifiy two-digit North American Industrial Classification code cannot be assigned to this industry.

3.2 Local Labor Market Density

Labor market density, $d_{m,t}$, is measured as the number of workers in the labor force, $L_{m,t}$, in metropolitan area m at time t per square mile of MSA land area, A_m, or

$$d_{m,t} = \frac{L_{m,t}}{A_m}, \tag{15}$$

where the labor force is the sum of unemployed and employed workers in the local labor market. MSA labor force counts are available from the Bureau of Labor Statistics *Local Area Unemployment Statistics* (LAU) on a monthly basis. I use labor force rather than population to measure scale because it is more likely to capture the part of the urban population actively working or searching for jobs and thus those benefiting most from proximity to potential matching partners.

There are two main issues associated with using MSAs as a measure of a single labor market. First, MSA definitions undergo regular changes to reflect changes in how people organize themselves across space. New MSAs area added and others are deleted, while the county composition of others may change. The most recent change occurred in the CPS in May 2004. MSA codes were updated from 1990-based three-digit MSA codes to four-digit Core-Based Statistical Area (CBSA) codes according to the 2000 Census. While the name of the MSA codes changed, their basis on county boundaries did not. I standardize MSA codes to December 2003 definitions by comparing component counties between 1990-based and December 2003 MSA codes. I am able to identify 259 unique MSAs that are constant within the CPS for the 1994–2010 period.[27] This ensures that changes in observed densities over time are due to changes in the labor force and not changes in MSA definitions. The procedure for deriving the set of consistent MSAs for the CPS sample is discussed in Appendix C.

[27]The CPS also reports New England City and Township Areas (NECTAs) which are not county based. I assign NECTAs a MSA codes based on which county the NECTA is located. If a NECTA spans multiple counties it is assigned to the county in which the greatest area of the NECTA lies.

Second, any calculation of density based on Equation 15 will be measured with error to the extent that county boundaries do not track closely with actual (unobserved) urban boundaries (this is in addition to any measurement error in the labor force).[28] I deal with this issue by using two additional measures of land area: the urbanized area and remote sensing data.

An urbanized area (UA) is an area with a central place of at least 50,000 residents and the overall population density is at least 1,000 people per square mile. UA boundaries are available for decennial census years only because they are closely tied to Census blocks, a population density-based measure of land area used by Census Bureau. UA boundaries may also be related to municipal boundaries or natural geologic or hydrologic features such as rivers. I use UA definitions from the 2000 U.S. Census, which are available as boundary files for use in Geographic Information Systems (GIS). UA areas are assigned MSA codes by overlaying the Census 2000 UA boundary file with a constructed county-based boundary file in GIS. From there, it's a straightforward calculation to get land area of the UA.

While using UA area to measure A_m addresses some of the issues with underestimating $d_{m,t}$, it is far from ideal. It suffers similar drawbacks as county-based measures in that it relies on arbitrary boundaries specified by municipalities, census designers, or civil engineers. An alternative is to use remote-sensing data. The *Landsat Program* (hereafter Landsat), first launched in 1972 and jointly managed by NASA and the U.S. Geological Survey (USGS), is a series of satellite missions intended to map and continuously observe the Earth's land coverage using spectral imagery.[29]

The most recent mission, Landsat7, was launched in April 1999. Its updated "Enhanced Thematic Mapper +" (ETM+) sensor scans the earth with an eight-

[28]This is more of a potential problem in the western United States where states tend to have fewer and much larger counties, leading to a larger understatement of the actual labor market density compared to MSAs in the eastern U.S. where counties tend to be much smaller. But with fewer MSAs in the western U.S., measurement error in land area will be concentrated in eastern states where measurement error is lower.

[29]For more information on Landsat see `http://landsat.gsfc.nasa.gov/`.

band, spectral radiometer capable of detecting land coverage in a 30-meter spatial resolution, which is fine enough detail to detect roads and other man-made structures.[30] By combining different combinations of bands, researchers can detect specific types of land coverage.

Landsat7 data are compiled into the National Land Cover Database (NLCD), a 16-class database of land coverage (e.g., deciduous forest, cultivated crop land, woody wetlands) for the contiguous 48 United States (Fry et al., 2011). The NLCD is managed by the USGS and is available for the years 1992, 2001, and 2006. I use NLCD2006 because it provides data corresponding to a time period closer to the 2003 MSA definitions, in addition to resolving some issues with measuring urban land coverages in NLCD2001.[31]

NLCD2006 identifies four levels of urban development which are based on the amount of "impervious surface," or constructed materials present in each pixel: developed, open space; developed, low intensity; developed, medium intensity; and developed, high intensity.[32] Developed, open space areas have less than 20 percent of constructed materials and most commonly include large-lot single-family housing units, parks, golf courses, and vegetation planted in developed settings for recreation, erosion control, or aesthetic purposes. Developed, low intensity areas have between 20 percent and 49 percent impervious surface land cover and most commonly include single-family housing units. Developed, medium intensity areas have between 50 percent and 79 percent total coverage being impervious surface and most commonly include single-family housing units. Developed, high intensity areas have 80 percent or greater impervious surface and are areas where people live and work in large numbers, such as apartment complexes, row houses, and commercial and industrial development.

[30] Each image is stored as a raster, where each pixel represents a land area of 30m^2. For more information, see `http://landsat.gsfc.nasa.gov/about/etm+.html`.

[31] The 2001 NLCD was recently updated so that it is directly comparable to NLCD 2006. See, for example, `http://www.mrlc.gov/nlcd2006.php`. Future work will explore the relationship between these two databases.

[32] The characteristics of each land coverage is available at `http://www.mrlc.gov/nlcd06_leg.php`.

NLCD2006 is downloadable from the USGS as a GIS image file and is straightforwardly matched to CPS metropolitan area codes using MSA boundary files. I use a cookie-cutter approach that "clips" NLCD2006 raster data with each MSA's boundary to identify the portion of NLCD2006 within each MSA.[33] MSA land area is then obtained by summing the number of urban pixels in each MSA and multiplying that total by 900m^2. From there, it's a simple task to convert square meters to square miles.

Table 1.1 compares density rankings by measurement type for the 30 most-dense MSAs (sorted by on Landsat7 density). Labor force is the annual average monthly sum of unemployed and employed workers in the MSA for 2006. In all specifications, the Los Angeles-Long Beach-Santa Ana, CA MSA is the most dense in the U.S. with just over 3,400 workers per square mile, followed closely by New York-Northern New Jersey-Long Island, NY-NJ-PA at just over 3,250 workers per square mile.

The strong understatement of density can be seen in MSAs such as Las Vegas-Paradise, NV, which is the fifth-most dense MSA according to NLCD2006 but the 123rd most dense according to the county-based (MSA) measure. Clark County, Nevada is the only component county in the Las Vegas-Paradise, NV MSA. Clark County's total land area is 8,091 square miles, while Landsat7 shows the urban land coverage in Clark County to be a mere 398 square miles, an overstatement–and hence understatement of density–by a factor of 20. Table 1.1 illustrates other similar cases.[34] Further, this overstatement will be greatest in states that have larger counties. As a comparison, the 28-county Atlanta-Sandy Springs-Marietta, GA MSA spans an area of 8,480 square miles which is only 4.3 times greater than the 1,968 square miles of urban area observed by Landsat7. Note also that there

[33] NLCD2006 data are available for the contiguous 48 United States, which excludes Anchorage, AK MSA and Honolulu, HI MSA from our sample. Therefore, we are able to produce density measures for 257 CPS MSAs for the period January 1994–February 2012.

[34] The Landsat7 measure is far from perfect. It is unable to account for differences in transportation systems that may reflect differences in density (Teulings and Gautier, 2003). A future version of this paper will incorporate measures of public transportation usage associated with dense areas (e.g., subway systems) in order to better identify differences in labor market scale.

Table 1.1
Metropolitan Statistical Area Density Rankings Comparison, Top 30 by Landsat7 Area Measure, 2006

	Labor Market Area Measure		
Name	MSA	Urbanized Area	Landsat7
Los Angeles-Long Beach-Santa Ana, CA	1	1	1
New York-Northern New Jersey-Long Island, NY-NJ-PA	2	5	2
San Francisco-Oakland-Fremont, CA	5	3	3
Miami-Fort Lauderdale-Miami Beach, FL	15	11	4
Las Vegas-Paradise, NV	123	2	5
Washington-Arlington-Alexandria, DC-VA-MD-WV	13	27	6
Bridgeport-Stamford-Norwalk, CT	4	177	7
Boston-Cambridge-Quincy, MA-NH	6	135	8
San Jose-Sunnyvale-Santa Clara, CA	26	4	9
Trenton-Ewing, NJ	3	86	10
Baltimore-Towson, MD	14	75	11
Boulder, CO	48	36	12
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	9	96	13
Chicago-Naperville-Joliet, IL-IN-WI	7	29	14
San Diego-Carlsbad-San Marcos, CA	21	38	15
Denver-Aurora, CO	77	7	16
Buffalo-Niagara Falls, NY	20	102	17
Sacramento–Arden-Arcade–Roseville, CA	62	18	18
Oxnard-Thousand Oaks-Ventura, CA	50	22	19
Salt Lake City-Ogden-Clearfield, UT*	188	34	20
Santa Cruz-Watsonville, CA	24	28	21
Colorado Springs, CO	121	90	22
Vallejo-Fairfield-Napa, CA	73	20	23
El Paso, TX	31	129	24
Seattle-Tacoma-Bellevue, WA	30	67	25
New Haven-Milford, CT	8	230	26
Milwaukee-Waukesha-West Allis, WI	12	113	27
Minneapolis-St. Paul-Bloomington, MN-WI	33	65	28
Phoenix-Mesa-Scottsdale, AZ	99	16	29
Providence-New Bedford-Fall River, RI-MA	17	184	30

NOTES: Rankings are out of 257 MSAs in the continental United States as identified in the January 1994–December 2012 Current Population Survey (Anchorage, AK and Honolulu, HI excluded in NLCD2006). MSAs standardized to December 2003 Office of Management and Budget definitions (`http://www.census.gov/population/metro/files/lists/2003/0312msa.txt`). Rankings are sorted by LANDSAT7-based density measured at 2006 annual average labor force size. MSA labor force data taken from the BLS' *Local Area Unemployment Statistics*.
* Denotes an MSA that was manually constructed by combining adjacent MSAs. A complete correspondence table of all MSAs and their components is available by request from the author. See Appendix C for more information on MSA identification.

appears to be no real relationship between the ability of UA boundaries to track Landsat7 measures. In some cases they perform close to Landsat7 calculations and in other cases MSA-based density measures track more closely to Landsat7 measures.

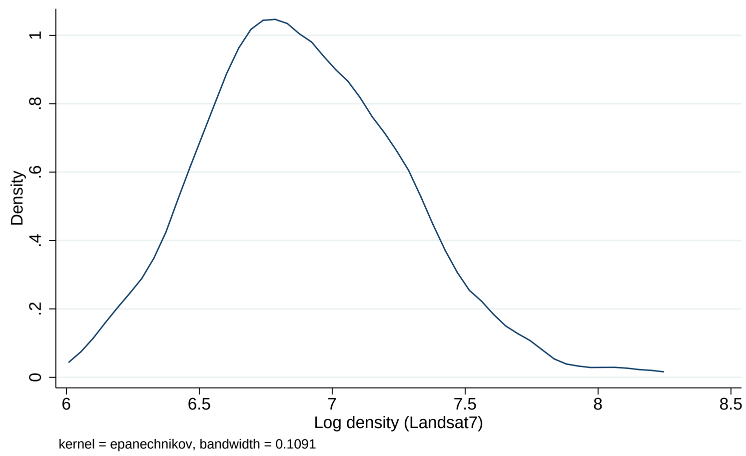

Figure 1
Kernel Density Estimate of Log Labor Market Density, Landsat7 Area Measure, 2006

Figure 1 presents a kernel density estimate of the distribution of MSA log density based on the Landsat7. The median density is 962 workers per square mile, which include MSAs such as Kalamazoo-Portage, MI, Corpus Christi, TX, and Chattanooga, TN-GA. MSAs in the 95th log density percentile have densities with 1,960 workers per square mile such as Baltimore-Towson, MD. MSAs in the 25th percentile (those with over 750 workers per square mile) include Billings, MT and Augusta-Richmond County, GA-SC.

4 Empirical Framework

This section develops the empirical framework used to estimate the effect of labor market scale on the time it takes for an unemployed job seeker to find work. The outcome of interest is the elapsed time, or duration, between a worker entering the unemployment state and transitioning to the employment state. The standard approach for dealing with such data is survival analysis. The method models the conditional probability that an individual unemployed worker will “survive” in the unemployed state conditional on having survived in that state for t time periods. The hazard rate is then the probability that an unemployment spell "dies," or ends in an exit to unemployment, after t periods. Further, the framework allows for the use of covariates which can shift survival (and hence failure) probabilities.

A challenge in estimating these probabilities is that deriving predicted durations from such models requires a specification of the underlying distribution of failure times. However, there may be no clear guidance from economic theory about what these distributions ought to be, and any specification of the model through the imposition of strict distributional assumptions on durations can induce inconsistent parameter estimates (e.g. Lancaster, 1990; Cameron and Trivedi, 2005). But in situations such as this, where we are only interested in how covariates affect hazard rates, the proportional hazards (PH) framework allows us to estimate how covariates, such as labor market scale, affect survival probabilities without specifying the distribution of failure time.

We begin by introducing the continuous-time version of the Cox PH model. This model assumes that the distribution of failure times is continuous. Even though we are not interested in modeling the distribution of failure times *per se*, estimation of continuous-time duration models are relatively easy to estimate in that the data do not have to be adjusted in any way (e.g., one record per subject), and it is generally simpler to derive the analytical likelihood functions. But as we saw in Section 3, CPS duration data are not measured in continuous time; rather, CPS duration data are *grouped* in weekly intervals, which leads to the possibility of "ties." Ties occur when multiple spells end at the same time (e.g., Kiefer, 1988). Ties are not possible (or are extremely rare) in a continuous distribution.[35] Since individuals with varying levels of observable characteristics may be observed leaving unemployment at the same durations, an adjustment is required to account for the marginal impact of each individual's characteristics to the likelihood of exiting unemployment at given durations. There are several methods for dealing

[35] Recall from probability theory that for a continuous distribution $G(x)$ the probability of observing any individual x is zero.

with ties, each satisfying a different tradeoff between computing time and accuracy, and parameter estimates are sensitive to which methods are used.

A more appropriate approach to dealing with grouped data is to utilize a discrete-time PH framework. This method treats durations as a series of binary outcomes where, each week, an unemployed individual may stay unemployed or exit to employment (e.g., Wooldridge, 2002, p. 706). The major advantage of this framework is that it controls for the explicit grouping of data at various intervals. Further, it is necessary to model the baseline hazard simultaneously with parameter estimates. But because we have information on every instant (here, week) at which an individual can exit, it is straightforward to specify very flexible–even fully non-parametric–baseline hazard functions.[36] However, the data must be transformed into an unbalanced panel where each individual has an observation for each time period they are at risk of exiting unemployment. Thus, data sets can get large very quickly depending on the number of individual spells and the number of periods those spells are at risk. Moreover, computing time can be an issue as the number of parameters to be estimated are introduces, as in the case of specifying a fully non-parametric baseline hazard function.

We finish this section by briefly describing the discrete-time proportional hazards model. Due to the large sample size of the CPS, we are able to specify an almost completely non-parametric baseline hazard. Therefore, we use the full information in the CPS to model the conditional probability of exiting unemployment as a function of market scale and individual characteristics while holding constant a fully flexible baseline hazard function. The discrete-time framework used is directly related to the continuous-time Cox PH model. As a result, our estimates allow for a direct comparison of grouped data modeled as continuous

[36] Another advantage is the ability to incorporate time-varying covariates. Since the periods at risk are organized sequentially for each individual, one can incorporate regressors that vary with each instant at risk. The major difficulty with adding time-varying covariates to the basic CPS data is that few external data sources measure important economic variables at a frequency that matches the CPS. At best, we can hope to get important local industry or occupation data on a quarterly basis (e.g., BLS *State and Local Area Employment Statistics*), but most public data sources are available annually.

and discrete processes. Since it is common for many researchers to simply estimate discrete data as if they are continuous (again, for simplicity) these results can shed some light on how parameter estimates are affected by treating CPS-type duration data as continuous.

4.1 Continuous-Time Proportional Hazard Model

In general, the PH framework specifies the conditional hazard as

$$h(t|\mathbf{x}, \rho; \beta, \alpha) = h_0(t)\phi(\mathbf{x}, \rho; \boldsymbol{\beta}, \alpha) \tag{16}$$

where $\mathbf{x}$ is a vector of k time-invariant individual characteristics, ρ is labor market scale, $h_0(t) > 0$ is the baseline hazard function, and $\phi(\mathbf{x}, \rho; \boldsymbol{\beta}, \alpha)$ is a non-negative function of individual-level covariates and parameters to be estimated. The baseline hazard is a function of survival time, t, alone and is common to all individuals in the population. Individual hazard functions are shifted proportionally according to variation in $\mathbf{x}$ and ρ.

I use a semi-parametric specification of equation (16) in which no functional form is specified for $h_0(t)$ and $\phi(\mathbf{x}, \rho; \boldsymbol{\beta}, \alpha)$ is fully specified using the exponential function

$$h(t|\mathbf{x}, \rho) = h_0(t)\exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho) \tag{17}$$

which ensures that $\phi(\cdot) > 0$ and allows for relatively simple interpretation of coefficient estimates.[37] To see this, divide both sides of equation (17) by the baseline hazard and take the natural logarithm

$$\ln\left(\frac{h(t|\mathbf{x}, \rho)}{h_0(t)}\right) = \mathbf{x}'\boldsymbol{\beta} + \alpha\rho \tag{18}$$

where $h(t|\mathbf{x}, \rho)/h_0(t)$ is the *hazard ratio.* The hazard ratio compares the likelihood that an unemployed job seeker with characteristics $\{\mathbf{x}, \rho\}$ will exit unemployment at time t relative to the baseline hazard, or where all regressors are equal to

[37] See, for example, (Wooldridge, 2002, p. 691) and (Cameron and Trivedi, 2005, p. 593).

zero. It is clear from equation (18) that the coefficients β_k (or α) measure the semielasticity of the hazard ratio with respect to x_k (or elasticities if x_k or ρ are measured in logarithms).

Another way to see how each covariate affects the hazard rate is to differentiate equation (17) with respect to ρ

$$\begin{aligned} \frac{\partial h(t|\mathbf{x},\rho)}{\partial \rho} &= \alpha h_0(t)\exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho) \\ &= \alpha h(t|\mathbf{x},\rho;\beta,\alpha) \end{aligned} \tag{19}$$

where the second line results from substitution of equation (16). Equation (19) states that the change in the hazard rate with respect to labor market scale is simply the coefficient α multiplied by the original hazard rate. Thus, all that is needed to estimate the impact of regressors on the hazard rate is information on $\boldsymbol{\beta}$ and α but not the hazard function $h_0(t)$.

4.2 Partial Likelihood Estimation

Estimation of $\boldsymbol{\beta}$ and ρ is achieved using the partial likelihood approach developed by Cox (1975). It is called a *partial*, or *limited-information*, likelihood because only part of the model is specified (e.g., Lancaster, 1990). In contrast, the *full-information* likelihood would specify the complete model.

The goal is to estimate the probability that an unemployment spell will terminate, or transition to the employment state, at time t conditional on having survived to t. The estimation framework needs to account for the fact that CPS unemployment duration data are "grouped" in discrete weekly intervals and right censoring. The adjustment is necessary because of how each spell contributes to the likelihood of an exit at time t. Because CPS durations are measured in weekly intervals rather than continuous time, there will be a bunching, or grouping, of spells at each weekly interval. The econometrics literature refers to these as "ties." I use the relatively standard "Breslow" approximation. Censored observations don't contribute to the set of observed failures, but they contribute to the size of the risk until they are censored.[38]

The setup requires that the failure times be ordered from shortest to longest and categorized into those that have failed and those at risk of failing at each time interval. Let T be the random variable denoting an unemployment spell, t be an observation of T, and $t_1 < t_2 < \cdots < t_j < \cdots < t_J$ be the observed set of discrete failure times in a sample of N unemployment spells, $N \geq J$. The spells at risk of failing at the jth interval is $R(t_j)$ and the set of actual transitions at t_j is $D(t_j)$. With ties, d_j is the number of deaths that occur at a specific interval.

The probability that a given spell will end at the jth interval is the conditional probability that a particular spell ends at the jth interval divided by the conditional probability that any spell in the risk set fails, or

$$\begin{aligned}
\Pr[T_j = t_j | R(t_j)] &= \frac{\Pr[T_j = t_j | T_j \geq t_j]}{\sum_{l \in R(t_j)} \Pr[T_l = t_l | T_l \geq t_j]} \\
&= \frac{h(t|\mathbf{x}, \rho)}{\sum_{l \in R(t_j)} h(t|\mathbf{x}, \rho)} \\
&= \frac{\exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho)}{\sum_{l \in R(t_j)} \exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho)}
\end{aligned} \tag{20}$$

where the baseline hazard $h_0(t)$ drops out due to the PH assumption.

The partial likelihood function with an adjustment for ties using the Breslow method is

$$L(\beta, \alpha) = \prod_{i=1}^{k} \frac{\prod_{m \in D(t_j)} \exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho)}{\left[\sum_{l \in R(t_j)} \exp(\mathbf{x}'\boldsymbol{\beta} + \alpha\rho)\right]^{d_j}}. \tag{21}$$

The log-likelihood function with censored observations is then

$$\ln L(\beta, \alpha) = \sum_{i=1}^{N} c_i \left[\mathbf{x}_i'\boldsymbol{\beta} + \alpha\rho - \ln\left(\sum_{l \in R(t_i a)} \exp(\mathbf{x}_l'\boldsymbol{\beta} + \alpha\rho\right)\right] \tag{22}$$

[38] See Cameron and Trivedi (2005, p. 594) for more information on partial likelihood estimation in the presence of censoring and ties.

where c_i is a binary indicator of right censoring. Estimation of the parameters in equation (22) is easily implemented in modern statistical packages, such as Stata.

4.3 Discrete-Time Proportional Hazard Framework

The discrete-time PH framework is similar to the continuous-time PH framework but it explicitly accounts for the grouped nature of the data.[39] The standard approach is to set up an unbalanced panel of potential failure times for each observation and use a binary choice framework to model transitions from unemployment to employment.

The primary difference with grouped data is that the potential time intervals over which a failure (i.e., unemployment exit) can occur are known, discrete intervals rather than a sampling T from a known cumulative distribution of failure times $F(t)$. Following Cameron and Trivedi (2005), the discrete-time transition model is

$$\Pr\left[t_{a-1} \leq T < t_a | T \geq t_{a-1} | \mathbf{x}\right] = F\left(h_a^d + \mathbf{x}'(t_a)\boldsymbol{\beta}\right), \tag{23}$$

where t_a are the grouping points such that $a = 1, \ldots, A$ (for our purposes, each grouping point is a week), h_a^d is the hazard rate at interval a, and $\mathbf{x}$ and $\boldsymbol{\beta}$ are the same as in the continuous case. Equation (23) states that the probability that a given spell T fails in the interval between week t_{a-1} and week t_a conditional on having survived in the unemployment state through interval t_{a-1} and having covariates $\mathbf{x}$ is equal to some function of the baseline hazard at each interval, individual characteristics, and parameters.

Some possible functional forms for $F(\cdot)$ are the logit or probit. We use the complementary log-log model ("cloglog") for $F(\cdot)$ because it generalizes to the continuous-time PH model. That is, exponentiated coefficient estimates can be interpreted directly as hazard ratios just as in the continuous Cox PH model. Fur-

[39] Here, we simply discuss a few of the major differences in the approach and present a fairly general version of the hazard function and describe the basic data set-up needed to implement estimation. For a formal discussion of how the continuous- and discrete-time PH models are related, as well as a formal characterization of the log-likelihood function, see Cameron and Trivedi (2005, p. 600-601).

ther, it is appropriate when outcomes are "rare" because it is asymmetric around zero (Cameron and Trivedi, 2005, p. 466).[40] This is not surprising considering that, when the data are "expanded" to create one observation for each interval at risk, there is at most one period per individual over which a failure can occur. All other periods are time at risk (i.e., censored spells). Further, some spells are not observed terminating at any point which further increases the set of at-risk periods relative to failures.

The discrete-time hazard function is then defined as

$$h^d(t_a|\mathbf{x}) = 1 - \exp\left(-\exp(g(t_a) + \mathbf{x}(t_{a-1})'\boldsymbol{\beta})\right) \tag{24}$$

where $g(t_a)$ is a function of the time intervals over which failures occur. The $g(\cdot)$ function can be specified any number of ways, including logarithmic, polynomial, or fully non-parametric. Given the large sample size of the CPS, we specify $g(\cdot)$ to be an almost fully non-parametric function of t_a. The only issue with doing so is that each interval must have at least one failure to be identified. In cases where no failures occur in an interval, adjacent intervals are combined such that each interval is populated with failures. We discuss this further in the next section. Derivation of the discrete-time survivor function and log-likelihood function is available in Cameron and Trivedi (2005, p. 603).

Finally, the data must be set up a particular in order to estimate the parameters of the discrete-time PH model.[41]. First, the data are "expanded" such that each individual i has one observation for each time period at risk T_i. Second, a sequence variable of positive integers is constructed $t = 1, \ldots, T_i$. Third, an indicator of failure d_i is created for each individual, where $d_i = 1$ if the spell ultimately ends and $d_i = 0$ if it is censored. Fourth, a period-specific failure indicator d^*_{it} where

[40] Failures account for only 2 percent of the entire sample (that is, 38,727 of the 1,880,983 total intervals at risk). Other important differences to note between the cloglog and related specifications is that the cumulative distribution function of the cloglog is the extreme value distribution whereas the CDFs of the logit and probit are the logistic and standard normal, respectively.

[41] See, for example, StataCorp. 2011. *Stata Survival Analysis and Epidemiological Tables Reference Manual. Release 12.* College Station, TX: StataCorp LP

$d_{it}^* = 1$ if $t = T_i$ and $d_i = 1$, else $d_{it}^* = 0$. Therefore, d_{it}^* is the dependent variable. Finally, it is necessary to define a variable or variables as a function of t to characterize the baseline hazard. We use dummy variables for each t for $t = [1, \ldots, 40]$ and then we group the remaing weeks in five-week intervals.[42] In total, we specify a piece-wise constant baseline hazard function that contains 55 splines, where the first 40 splines are fully nonparametric.

5 Estimation and Results

Economists are interested in the unemployment and post-unemployment outcomes of workers who lose their jobs through a permanent layoff. These workers are less likely to find employment relatively quickly—if at all—and, if they do, they tend to suffer higher earnings losses than observationally similar workers who are unemployed for different reasons (e.g., Fallick, 1996). We begin by comparing re-employment probabilities and spell durations of permanently laid off workers to those who were temporarily laid off or voluntarily decided to quit their jobs. As we will see, permanently laid off workers are less likely to be re-employed prior to leaving the sample, face much longer spell durations, and are more likely to qualify as "long-term" unemployed. These differences persist across variation in the business cycle and local markets scale.

We also explore the rate of industry and occupational mobility of the re-employed subsample as well as their movements between full- and part-time employment. One source for earnings losses following displacement is the loss of accumulated industry- or occupation-specific human capital that cannot be transferred to the new job (Kletzer, 1998).[43] In addition, permanently laid off workers may experience underemployment where, despite wanting to work full-time, they can only find part-time work. Unfortunately, the basic CPS does not collect earnings information for unemployed workers, making it impossible to evaluate

[42] The last interval contains six weekly intervals comprising spell durations of 116-121 weeks, where 121 weeks is the maximum observed duration in the sample.

[43] Neal (1995) and Carrington (1993) find that earnings losses tend to be firm- or job-specific.

how permanently laid off workers fare compared to other unemployed workers. Nonetheless, we can get a sense of the incidence of industry and occupational mobility as well as aggregate movements between full- and part-time work following a spell of unemployment.

Finally, we employ a regression-based framework that relates measures of local labor market scale to the hazard of exiting unemployment while simultaneously controlling for observable individual and aggregate business cycle characteristics that may affect spell duration. We start by estimating a continuous-time Cox PH model and evaluate its fit with and without regressors. We then estimate a discrete-time binary choice model that explicitly accounts for the grouping of failure times at discrete weekly intervals in the CPS.

5.1 Summary Statistics

Table 1.2 presents re-employment probabilities of unemployed workers by unemployment type and variation in the business cycle. The business cycle is broken down into two distinct periods: the period prior to the Great Recession that began in December 2007 and the period from the Great Recession on.[44] Less than half of workers on permanent layoff are observed exiting unemployment compared to just over half of quits and nearly three-quarters of temporarily laid off workers. These probabilities decline for all groups during and after the Great Recession, with permanent layoffs and quits facing the biggest declines. Since the Great Recession began, just over 25 percent of permanently laid off workers are observed finding employment compared to over one-third of quits and 70 percent of temporary layoffs.

[44]While the sample includes a recession for the period March to November in 2001, the most recent downturn was especially severe. In addition, the persistence of the downturn's effects on the labor market at the time of this writing warrant including the months following the official end of the Great Recession in the post-recession subsample. For an excellent review of how unemployment was affected by the Great Recession, see Elsby et al. (2010). Farber (2011) addresses Great Recession's effects on displaced workers. For a complete list of official recession dates, see `http://wwwdev.nber.org//cycles.html`.

Table 1.2
Probability of Observed UE Transition for MSA Workers by Type of Unemployment, Current Population Survey January 1994–February 2012

	1994-2012	1994-2007	2008-2012
Permanent Layoffs	0.35	0.42	0.28
	(0.48)	(0.49)	(0.45)
	[47,586]	[27,625]	[19,961]
Temporary Layoffs	0.73	0.75	0.70
	(0.44)	(0.43)	(0.46)
	[19,108]	[13,488]	[5,620]
Quits	0.52	0.57	0.38
	(0.50)	(0.50)	(0.49)
	[14,752]	[11,248]	[3,504]
Full Sample	0.47	0.53	0.37
	(0.50)	(0.50)	(0.48)
	[81,446]	[52,361]	[29,085]

NOTES: Author's caculations from constructed two-month panels of the Current Population Survey (CPS). All values weighted by CPS "final" weights (PWSSWGT) at the time of transition. Sample comprised of UE transitions in 259 metropolitan areas. Standard deviation in parentheses. Unweighted sample size in brackets.

Table 1.3 presents mean and median spell durations by type of unemployment over changes in the business cycle. The first three columns report results for the full sample, which includes complete and incomplete spells, and the last three columns report summary statistics for completed spells only. It is evident that workers on permanent layoff face longer unemployment spells than other types of unemployment and these differences persist over changes in the business cycle. In the period before the 2008, completed spells for displaced workers averaged 16.9 weeks compared to 8.6 weeks and 10.6 weeks for temporary layoffs and quits, respectively. In addition, median durations for displaced workers are nearly double that of both temporary layoffs and quits. Average completed durations ballooned to nearly 27 weeks for displaced workers during the Great Recession, nearly double that of those who quit. Note how mean and median durations are virtually unchanged for the temporary layoffs between time periods. This suggests that workers on temporary layoff face very different incentives for search while unemployed, which is largely reflective of their expectations of recall to their old firm. Also note that displacement accounts for over 50 percent of all unemployment.

Economists also pay special attention to the long-term unemployed, or those individuals with unemployment spells lasting 27 weeks or more.[45] Table 1.4 shows the fraction of long-term unemployed by unemployment type and time period. For completed spells prior to the Great Recession, workers on permanent layoff are nearly four times as likely to experience long-term unemployment than those on temporary layoff and more than twice as likely as those who quit. Both quits and permanent layoffs saw a near doubling of the incidence of long-term unemployment during the Great Recession while those on temporary layoff show only a slight increase. Over the entire sample, more than one out three permanently laid off workers had spell durations longer than 26 weeks, five times the incidence of those on temporary layoff and more than double of those who quit.

Next, we compare how local labor market scale, measured by market size and density, affects search duration by type of unemployment. Table 1.5 shows mean completed spell durations according to six categories of city size. City size is measured as the average annual labor force in the year of unemployment exit and includes the full sample from 1994–2012. The relative differences in unemployment duration between displaced workers and those who are temporarily laid off persist across markets of different sizes. In nearly every size class, displaced workers face longer spells of unemployment. There appears to be very slight positive correlation between market size and average unemployment duration for quits and permanent layoffs and slightly negative correlation for temporary layoffs. Permanently laid off workers and those who quit search for about 2.5 weeks longer in the largest

[45]Prior to the Great Recession, federal unemployment insurance (UI) benefits had a maximum duration period of 26 weeks. During periods of relatively high unemployment, the federal government may offer "emergency" extended UI benefits. During the Great Recession, maximum combined state and federal UI benefit duration was extended to 99 weeks, with the maximum benefit duration declining to 79 weeks by the end of 2012.

Table 1.3
Mean and Median Unemployment Duration by Unemployment Type and Time Period, Full Sample and Completed Spells

	All Spells			Completed Spells		
	1994-2012	1994-2007	2008-2012	1994-2012	1994-2007	2008-2012
Permanent Layoff	29.2	22.4	37.7	20.3	16.9	26.9
	(28.4)	(22.8)	(32.2)	(22.6)	(18.7)	(27.3)
	{18}	{14}	{26}	{11}	{10}	{15}
	[47,586]	[27,625]	[19,961]	[16,862]	[11,379]	[5,483]
Temporary Layoff	10.2	9.8	11.0	8.7	8.6	9.0
	(11.9)	(11.5)	(12.7)	(10.7)	(10.8)	(10.5)
	{6}	{6}	{7}	{5}	{5}	{5}
	[19,108]	[13,488]	[5,620]	[14,019]	[10,121]	[3,898]
Quits	17.7	15.0	25.7	11.5	10.6	15.5
	(21.3)	(18.2)	(27.2)	(14.7)	(13.5)	(18.6)
	{10}	{8}	{15}	{6}	{6}	{9}
	[14,752]	[11,248]	[3,504]	[7,751]	[6,401]	[1,350]
Full Sample	22.8	17.6	31.3	14.4	12.5	19.1
	(25.6)	(20.2)	(30.8)	(18.4)	(15.6)	(23.2)
	{12}	{10}	{19}	{7}	{6}	{10}
	[81,446]	[52,361]	[29,085]	[38,632]	[27,901]	[10,731]

NOTES: Author's calculations from constructed two-month unemployment transition pairs, Current Population Survey, January 1994–February 2012. Standard deviation in parentheses, median duration in braces, and sample sizes in brackets. The period following December 2007 is designed to capture the effects of the "Great Recession." Transitions that occurred between December 2007 and January 2008 are included in the 2008 sample, while transitions that happened from November 2007 and December 2007 are in the pre-recession sample.

Table 1.4
Long-Term Unemployment Spells as a Share of Total Spells by Unemployment Type, Current Population Survey January 1994–February 2012

	All Spells			Completed Spells		
	1994-2012	1994-2007	2008-2012	1994-2012	1994-2007	2008-2012
Permanent Layoffs	0.36	0.26	0.49	0.25	0.19	0.35
	(0.48)	(0.44)	(0.50)	(0.43)	(0.40)	(0.48)
	[47,586]	[27,625]	[19,961]	[16,862]	[11,379]	[5,483]
Temporary Layoffs	0.07	0.06	0.08	0.05	0.05	0.06
	(0.25)	(0.23)	(0.28)	(0.22)	(0.21)	(0.23)
	[19,108]	[13,488]	[5,620]	[14,019]	[10,121]	[3,898]
Quits	0.18	0.14	0.30	0.09	0.08	0.15
	(0.38)	(0.35)	(0.46)	(0.29)	(0.27)	(0.36)
	[14,752]	[11,248]	[3,504]	[7,751]	[6,401]	[1,350]
Full Sample	0.26	0.18	0.39	0.15	0.12	0.22
	(0.44)	(0.39)	(0.49)	(0.35)	(0.32)	(0.42)
	[81,446]	[52,361]	[29,085]	[38,632]	[27,901]	[10,731]

NOTES: Author's calculations from constructed two-month unemployment transition pairs, Current Population Survey, January 1994–February 2012. Standard deviation in parentheses and sample sizes in brackets. Long-term unemployment spells are defined as those lasting 27 weeks or more. The period following December 2007 is designed to capture the effects of the "Great Recession." Transitions that occurred between December 2007 and January 2008 are included in the 2008 sample, while transitions that happened from November 2007 and December 2007 are in the pre-recession sample. Summary statistics are weighted by CPS "final" weights (PWSSWGT).

markets compared to the smallest ones, or an increase of about 13 and 23 percent respectively.

Table 1.5
Mean Unemployment Duration by Unemployment Type and Labor Force Size, Completed Spells, Current Population Survey January 1994–February 2012

	Labor Force Size (thousands)					
	≤100	100-250	250-500	500-1,000	1,000-2,000	2,000+
Permanent Layoff	18.1	18.5	19.7	20.1	19.4	21.6
	(20.7)	(21.0)	(21.5)	(22.6)	(21.9)	(23.6)
	[672]	[2,135]	[1,940]	[3,113]	[2,677]	[6,325]
Temporary Layoff	9.1	9.2	9.1	8.8	8.6	8.4
	(10.4)	(11.3)	(10.6)	(11.6)	(10.4)	(10.2)
	[802]	[2,356]	[1,641]	[2,568]	[2,095]	[4,557]
Quits	10.4	10.9	10.9	10.9	11.1	12.8
	(11.8)	(13.7)	(12.8)	(13.3)	(14.4)	(17.0)
	[434]	[1,301]	[976]	[1,564]	[1,255]	[2,221]

Notes: Author's calculations from constructed two-month unemployment-to-employment transitions, Current Population Survey, January 1994–February 2012. Standard deviation in parentheses, sample size in brackets. Labor force size is the level of MSA employed and unemployed workers in the month of unemployment exit. Summary statistics are weighted by CPS "final" weights (PWSSWGT).

Table 1.6 repeats the previous exercise but measures market scale according to density based on Landsat7 data. Again, we see that displaced workers experience relatively longer spell durations across markets of differing density than other types of unemployment. Where we saw a slightly negative relationship between market size and duration for temporarily laid off workers, the relationship between density and duration is more or less flat. For permanent layoffs, however, we see fairly large and, with the exception of the 1,000–1,250 labor-force-per-mile category, monotonic increase in spell durations across density classes. Workers in the densest areas search on average for 7.8 weeks—nearly two months—longer than those in the least-dense MSAs, a difference of 52 percent. Quits show a similar trend, with search lasting about 33 percent, or three weeks, longer in densest compared to the least-dense MSAs.

Industry mobility is an important reallocation mechanism in the labor market, where workers can move from industries with low labor demand (excess supply) to those with higher labor demand (Fallick, 1993). As a reallocation mechanism, however, it may be more costly to move workers across occupations which differ by required levels of education, technical expertise, and so on.[46]

Table 1.7 shows the fraction of re-employed workers who change industry and occupation by unemployment type and time period. We present three levels of industry and occupation mobility with increasing levels of aggregation: detailed, broad (three-digit), and major (two-digit). We expect generally less mobility across more aggregated groupings since, by construction, there are fewer classes of industries or occupations. In addition, to the extent that industry and occupation classification systems accurately group similar detailed industries and occupations, movements across more aggregate categories may indicate a greater difference the types of tasks performed between the pre- and post-unemployment job. Interestingly, there is very little difference in mobility rates over the business cycle within unemployment-type classifications, but quits and permanent layoffs show a high degree of mobility. Quits are more likely to have changed broad and major occupations during the period including and following the Great Recession and are slightly more likely to change industry and occupation than permanent layoffs. Nearly three-quarters of workers on permanent layoff who find employment do so in a different industry or occupation than their previous job and 60 percent change both. About two-thirds change broad industry and occupation categories and about half change both broad industry and occupation. Temporarily laid off workers are much more likely to return to their previous industry and occupation, One advantage of living in a large urban area is that there are more opportunities for unemployed workers to find work in the same industry or occupation as their

[46]This point was made by Oi (1987). We explore this issue in greater detail in Chapter III by looking at re-employment outcomes based on the task differences between a worker's old and new job. The idea being that workers are immediately more substitutable across industries in the same occupation than across occupations within the same industry. Industries refer to the type of, or *what*, output (is) produced whereas occupations refer to *how* output is produced.

Table 1.6
Mean Unemployment Duration by Unemployment Type and MSA Labor Force Density, Completed Spells

	Labor Force Density (persons per square mile)							
	≤500	500-750	750-1,000	1,000-1,250	1,250-1,500	1,500-2,000	2,000-2,500	2,500+
Permanent Layoff	14.9	16.8	19.2	18.8	19.5	21.2	21.9	22.7
	(15.8)	(19.9)	(21.2)	(21.1)	(22.0)	(23.4)	(23.8)	(24.4)
	[77]	[1,150]	[2,219]	[2,302]	[3,012]	[4,076]	[1,521]	[2,505]
Temporary Layoff	9.3	8.9	9.0	8.7	8.7	8.6	9.0	8.7
	(10.3)	(11.2)	(11.4)	(10.2)	(10.5)	(10.3)	(11.0)	(10.9)
	[66]	[1,180]	[2,052]	[2,163]	[2,567]	[3,212]	[951]	[1,828]
Quits	10.3	9.9	10.4	10.8	11.9	11.9	13.2	13.3
	(12.1)	(11.7)	(12.9)	(13.0)	(15.2)	(15.5)	(16.4)	(18.2)
	[63]	[755]	[1,308]	[1,287]	[1,427]	[1,654]	[512]	[745]

NOTES: Author's calculations from constructed two-month unemployment-to-employment transitions from the Current Population Survey, January 1994–February 2012. Standard deviation in parentheses, sample size in brackets. Density measures calculated using Landsat7-based area measures described in Section 3.2. Summary statistics are weighted by CPS "final" weights (PWSSWGT).

Table 1.7

Mean Duration and Industry and Occupation Mobility Probabilities, Current Population Survey January 1994–February 2012

	1994-2002			2003-2012		
	Perm Layoff	Temp Layoff	Quits	Perm Layoff	Temp Layoff	Quits
Mean Duration	15.7	8.6	9.8	23.1	8.9	13.3
	17.6	10.6	12.8	24.7	10.7	16.3
Changed detailed industry	0.72	0.30	0.76	0.72	0.29	0.76
Changed detailed occupation	0.73	0.44	0.77	0.76	0.42	0.79
Changed detailed industry and occupation	0.61	0.21	0.67	0.64	0.20	0.68
Changed broad industry	0.65	0.27	0.68	0.65	0.25	0.68
Changed broad occupation	0.64	0.35	0.67	0.68	0.34	0.74
Changed broad industry and occupation	0.51	0.17	0.56	0.56	0.17	0.60
Changed major industry	0.60	0.24	0.63	0.62	0.23	0.65
Changed major occupation	0.53	0.29	0.57	0.58	0.26	0.65
Changed major industry and occupation	0.40	0.13	0.44	0.48	0.14	0.53
Number of observations	6,699	6,178	4,063	10,105	7,800	3,658

NOTES: Author's calculations from the Current Population Survey. Detailed industry or occupation switch refers to 3 or 4-digit industry or occupation code (years prior to 2003 and post 2003, respectively). Broad refers to 2-digit industry or occupation code, and major refers to 1-digit industry or occupation code. CPS "final" (PWSSWGT) sampling weights used.

Table 1.8
Industry, Occupation, and Full- and Part-Time Mobility of MSA Workers, Permanent Layoffs by MSA Size, Current Population Survey January 1994–February 2012

	Labor Force Size (thousands)					
	≤100	100-250	250-500	500-1000	1000-2000	2000+
Re-employment Probability	0.37	0.37	0.35	0.36	0.35	0.35
Changed detailed industry	0.75	0.75	0.72	0.74	0.73	0.70
Changed detailed occupation	0.78	0.77	0.76	0.77	0.75	0.72
Changed detailed industry and occupation	0.67	0.66	0.64	0.65	0.63	0.60
Changed broad industry	0.67	0.68	0.66	0.68	0.66	0.63
Changed broad occupation	0.71	0.69	0.68	0.68	0.68	0.64
Changed broad industry and occupation	0.57	0.57	0.54	0.56	0.54	0.51
Changed major industry	0.63	0.63	0.61	0.64	0.62	0.60
Changed major occupation	0.58	0.59	0.57	0.58	0.57	0.54
Changed major industry and occupation	0.45	0.47	0.45	0.47	0.45	0.43
Full-time old job, full-time new job	0.70	0.69	0.69	0.70	0.71	0.74
Full-time old job, part-time new job	0.28	0.29	0.28	0.27	0.27	0.24
Part-time old job, full-time new job	0.003	0.005	0.006	0.006	0.007	0.006
Part-time old job, part-time new job	0.01	0.02	0.02	0.02	0.02	0.01
Number of observations*	1,832	5,694	5,399	8,775	7,690	18,196

NOTES: Author's calculations from constructed two-month unemployment-to-employment transitions, Current Population Survey, January 1994–February 2012. Standard deviation in parentheses, sample size in brackets. Labor force size is the level of MSA employed and unemployed workers in the month of unemployment exit. Summary statistics are weighted by CPS "final" weights (PWSSWGT).
* Refers to the full sample of permanent layoffs. Mobility rates correspond to the re-employed sample for which industry- and occupation-change comparisons could be made. Industry and occupation changes for UE transitions occurring between December 2002 and January 2003 are excluded due to a change in industry and occupation coding schemes in the CPS.

previous job, thereby transferring more of their previous job experience to the next job which may translate into higher earnings following displacement.

Table 1.8 reports re-employment probabilities, industry and occupation mobility, and full- and part-time movements for the re-employment sample of permanent layoffs by MSA labor force size. Re-employment probabilities show no variation by MSA size. There is little variation in the likelihood of changing detailed industries or occupations *alone* except for workers in the largest MSA size class, showing about a 3 to 5-percent reduction in industry or occupation mobility compared to the other size classes. There is increasingly less incidence of changing detailed industry *and* occupation as MSA labor size increases: 60 percent of re-employed permanent layoffs change detailed industry and occupation in the largest MSA size class, a reduction from 67 percent in the smallest class. There is little variation in the propensity of changing industry and occupation measured at higher levels of aggregation as well, with the only real reduction seen in MSAs with 2 million workers or more. That is, re-employed workers in the largest MSAs are more likely to find work in the same industry and occupation as their previous job, although these differences aren't large.

Table 1.8 also reports the degree of full- and part-time mobility between the old job and new job for permanent layoffs in markets of different size. Interestingly, nearly three-quarters of re-employed workers who were permanently laid off from full-time jobs ultimately found full-time work.[47] The share of workers laid off from full-time jobs in the largest MSAs is 74 percent compared to about 70 percent in all MSAs with less than 2 million workers. In addition, there is less incidence of workers losing full-time jobs and finding part-time employment in the largest MSA size class.

Table 1.9 repeats the previous analysis but uses density as a measure of local market scale. Re-employment probabilities steadily decline from 40 percent to 34

[47] Although not reported here, permanently laid off workers were more likely to be laid off from full-time work and more likely to find full-time work upon re-employment than those on temporary layoff or quits. A much larger share of temporary layoffs are from part-time jobs and they ultimately find part-time work.

Table 1.9

Industry, Occupation, and Full- and Part-Time Mobility of MSA Workers, Permanent Layoffs by MSA Density, Current Population Survey January 1994–February 2012

	Labor Force Density (persons per mi^2)							
	$\leq$500	500-750	750-1000	1000-1250	1250-1500	1500-2000	2000-2500	2500+
Re-employment Probability	0.40	0.39	0.36	0.38	0.35	0.35	0.32	0.34
Changed detailed industry	0.74	0.75	0.74	0.75	0.72	0.72	0.70	0.68
Changed detailed occupation	0.81	0.76	0.77	0.76	0.75	0.75	0.72	0.72
Changed detailed industry and occupation	0.66	0.66	0.65	0.65	0.63	0.63	0.59	0.60
Changed broad industry	0.62	0.70	0.66	0.68	0.66	0.65	0.63	0.62
Changed broad occupation	0.76	0.69	0.69	0.68	0.67	0.66	0.63	0.64
Changed broad industry and occupation	0.55	0.57	0.56	0.57	0.54	0.53	0.50	0.52
Changed major industry	0.56	0.65	0.61	0.64	0.61	0.61	0.60	0.59
Changed major occupation	0.58	0.58	0.57	0.58	0.57	0.56	0.53	0.54
Changed major industry and occupation	0.43	0.47	0.45	0.47	0.45	0.44	0.42	0.43
Full-time old job, full-time new job	0.68	0.71	0.70	0.71	0.72	0.71	0.74	0.73
Full-time old job, part-time new job	0.30	0.28	0.28	0.27	0.25	0.26	0.24	0.25
Part-time old job, full-time new job	0.009	0.001	0.006	0.005	0.008	0.005	0.008	0.007
Part-time old job, part-time new job	0.01	0.01	0.01	0.02	0.01	0.02	0.02	0.01
Number of observations[a]	192	2,939	6,042	6,114	8,502	11,699	4,780	7,318

NOTES: Author's calculations from constructed two-month unemployment-to-employment transitions from the Current Population Survey, January 1994–February 2012. Standard deviation in parentheses, sample size in brackets. Density measures calculated using Landsat7-based area measures described in Section 3.2. Summary statistics are weighted by CPS "final" weights (PWSSWGT).

[a] Refers to the full sample of permanent layoffs. Mobility rates correspond to the re-employed sample for which industry- and occupation-change comparisons could be made. Industry and occupation changes for UE transitions occurring between December 2002 and January 2003 are excluded due to a change in industry and occupation coding schemes in the CPS.

percent from the least-dense to the most-dense MSAs. We already found in Table 1.6 that workers in dense areas search for longer periods, therefore we should expect to see them transitioning less often given our short periods of observation in the CPS. We also see in Table 1.9 an almost monotonic decrease in detailed industry and occupation mobility rates as density increases. In the densest areas, 32 and 28 percent of re-employed workers find work in the same industry or occupation, respectively, up from 26 and 19 percent in the least-dense MSAs. Moreover, 40 percent of re-employed workers find employment in their same industry *and* occupation in the densest MSAs, up from 34 percent in the least dense (with an almost monotonic increase). There is much less variation in broad and major levels of industry and occupation aggregation except for occupation change. Roughly 36 percent of re-employed workers in the densest MSAs find work in the same broad occupational category, up from 24 percent in the least dense.

Finally, 73 percent of re-employed workers who lost full-time jobs ultimately find full-time employment in the densest MSAs, an increase from 68 percent in the least dense. In addition, there is less incidence of re-employed workers who lost full-time jobs ultimately finding part-time work in the densest MSAs compared to less-dense MSAs.

The descriptive evidence thus far is conditional on only a few dimensions and does not control for other factors that may be associated with search durations, such as individual demographic characteristics and education levels. Moreover, the broad measures of scale used in the previous tables account for only six MSA-size and eight density classes. By moving to a regression-based framework, we can isolate the effect of scale on the hazard of exiting unemployment while simultaneously accounting for observable characteristics and the influence of censored unemployment spells.

5.2 Cox PH Model Estimates

Table 1.10 presents summary statistics of the sample used for Cox PH model estimation. The average age of a permanently laid-off worker is about 37 years which is identical to those who quit or who are on temporary layoff. Workers who have yet to find employment are older than the re-employed sample by an average of three years. This could be due to a greater willingness of older workers to prolong their search durations in order to find a job where they can transfer more of their generally higher levels of accumulated industry- or occupation-specific human capital. Older workers tend to have longer job tenure which is associated with higher returns to seniority, experience, or a very productive match with the previous firm.

The average duration of unemployment is 20 weeks for those who are observed to find employment and 34 weeks for those whose spells are ongoing. Men are more likely to be permanently laid off, although women comprise a greater share of censored spells than they do for completed spells. About three-quarters of the re-employed sample is white, with blacks and Hispanics comprising the greatest share of permanent layoffs. Blacks comprise a smaller share of the completed spell sample than Hispanics but comprise a larger share of censored spells. Because the sampling strategy we adopt can only follow individuals for a maximum of 12 consecutive weeks, those individuals who tend to have longer average durations are more likely to show up as censored.

Figure 2 presents Kaplan-Meier survivor function estimates for the full and re-employment sample of permanent layoffs. The survivor function is the fraction of unemployment spells that survive to $t+1$ conditional on having survived up to the previous period t. Panel A presents the empirical survivor function for the full sample of permanent layoffs, showing relatively rapid rate of UE transitions over shorter durations but tends slows as durations increase. However, the overall rate of unemployment-exits in the sample is strikingly low. After 20 weeks, roughly 25 percent of the sample has left unemployment and only half have after 60 weeks.

Table 1.10
Sample Summary Statistics: Permanent Layoffs, January 1994–February 2012

	Completed Spells		Censored Spells	
	Mean	Std. Dev.	Mean	Std. Dev.
Duration of unemployment (weeks)	20.3	22.6	34.0	30.0
Age	37.2	11.3	40.3	11.9
Female	0.38	0.49	0.42	0.49
White	0.76	0.43	0.72	0.45
African American	0.18	0.38	0.21	0.41
American Indian	0.01	0.10	0.01	0.09
Asian or Pacific Islander	0.04	0.19	0.04	0.21
Hispanic	0.20	0.40	0.16	0.37
Foreign-born, non-citizen	0.06	0.24	0.08	0.26
Foreign-born, citizen	0.13	0.33	0.09	0.29
Married	0.47	0.50	0.44	0.50
Never married	0.37	0.48	0.35	0.48
High school or GED	0.35	0.48	0.36	0.48
Some college, no degree	0.21	0.41	0.20	0.40
Two-year degree, vocational	0.04	0.21	0.04	0.19
Two-year degree, academic	0.04	0.20	0.04	0.20
Four-year degree	0.15	0.36	0.16	0.36
Master's degree	0.04	0.19	0.04	0.21
Professional degree	0.004	0.07	0.005	0.07
Doctoral degree	0.004	0.06	0.005	0.07
Detailed industry switcher	0.72	0.45	–	–
Detailed occupation switcher	0.75	0.44	–	–
Detailed industry and occupation switcher	0.63	0.48	–	–
Number of observations	16,862		30,724	

Notes: Author's calculations from the Current Population Survey. Summary statistics are for the sample of workers on permanent layoff. All values weighted by CPS "final" weights (PWSSWGT).

It is important to note that 65 percent of spells in the sample are censored, which is most likely due to the very short (three-month maximum) interval over which we can observe any individual. Since censored spells contribute to the total number of at-risk spells at any time interval ($R(t_j)$ in equation (20)) but not the number of failures, the high incidence of censoring at all durations lead to an upward shift in the survivor function.

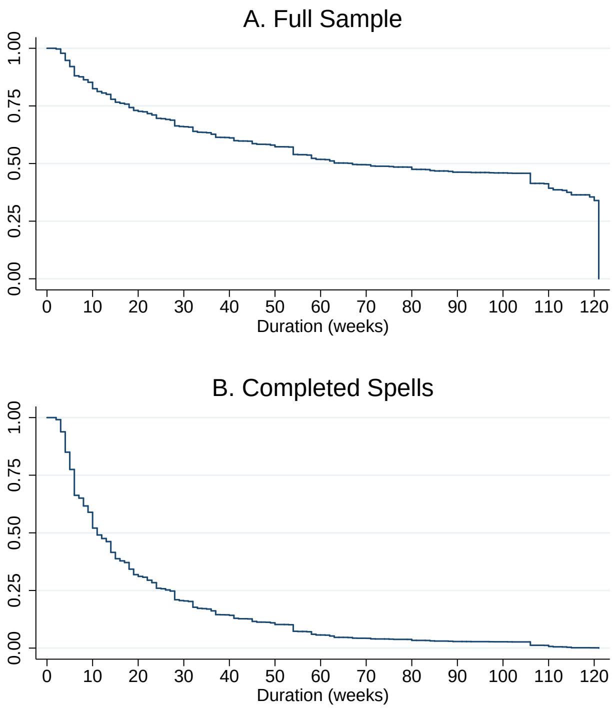

Figure 2
Kaplan-Meier Survival Estimates, Full Sample and Completed Spells, Current Population Survey January 1994–February 2012

Panel B presents the empirical survivor function for the subsample of completed spells. The empirical survivor function for UE transitions reveals a much stronger negative relationship between spell duration and the probability of remaining unemployed, with most transitions occurring at very short durations. Nearly 25 percent find employment within the first 10 weeks and about 75 percent do so between 20 and 30 weeks. The negative slope begins to flatten out as durations increase. Therefore workers who are unemployed for longer durations face declining odds of being re-employed.

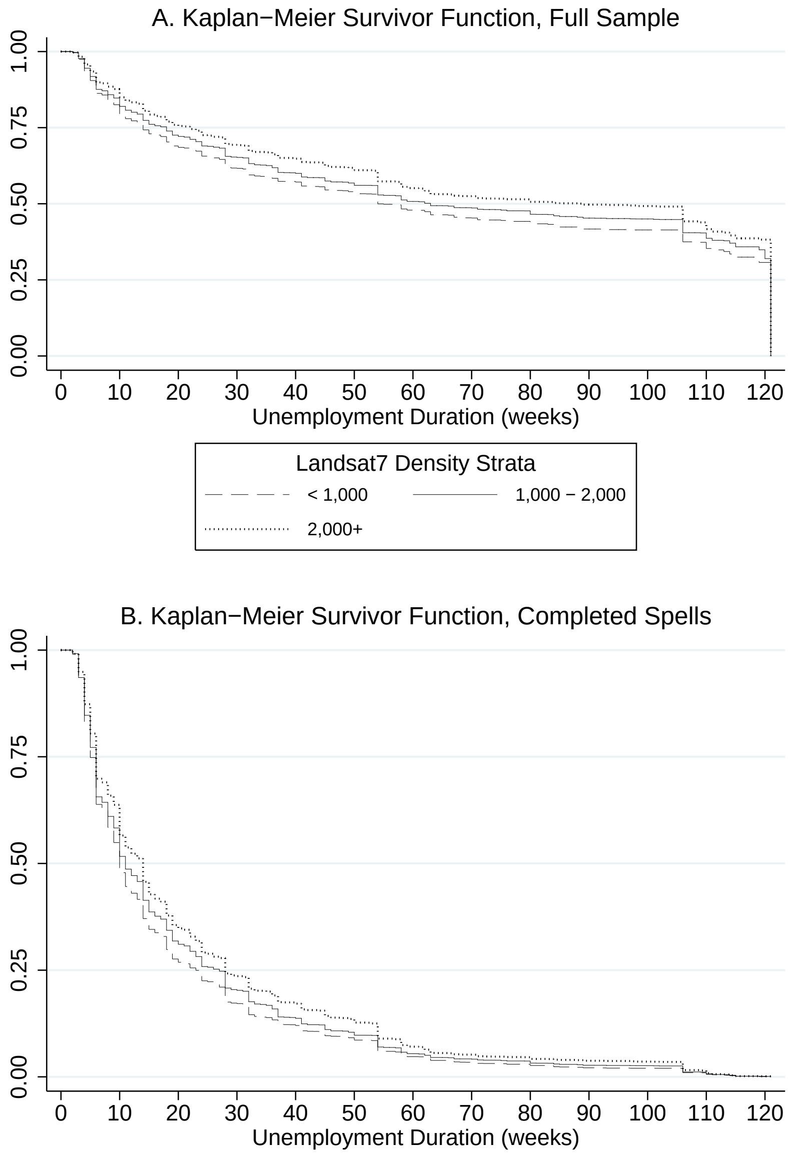

Figure 3
Kaplan-Meier Survival Estimates by Landsat7 Density Strata, Full Sample and Completed Spells, Current Population Survey January 1994–February 2012

As mentioned earlier, the attractive feature of the Cox PH framework is the ability to derive estimates of the marginal effect of observable characteristics on the rates of transition from unemployment to employment without an explicit

specification of the underlying baseline hazard function. This convenience comes at a cost, however, in that misspecification arises if the proportional hazards assumption is violated. Typically, researchers are interested in outcomes for two or more distinct groups, such as treated and control groups in an experimental setting. One simple test of the PH assumption involves a visual comparison of Kaplan-Meier survival functions for each group. If the two functions are parallel, or proportional, to each other then the PH assumption is valid. If they intersect then the PH assumption may be violated.

Figure 3 presents Kaplan-Meier survivor function estimates for three strata of Landsat7-density measures.[48] We specify three strata to reduce clutter. The first stratum (blue line) contain unemployment spells less than 1,000 workers per square mile, the second stratum (maroon line) is for workers in areas of 1,000 to 2,000 workers per square mile, and the third stratum (green line) corresponds to workers in areas with 2,000 or more workers per square mile. As before, Panel A presents survivor function estimates for the full sample and Panel B reports estimates for the UE subsample. In both pictures there appears to be an upward scaling of the survivor estimate as density increases, suggesting that the PH assumption holds for these data. Taking the UE subset, for example, more than half of workers find jobs within 10 weeks in the smallest density stratum compared to much less than half in the densest stratum. The differences become much less discernible for durations lasting 60 weeks or more.

We begin by specifying a baseline model that includes only labor market scale and controls for the year and month of the survey as regressors.[49] Baseline propor-

[48]Because density is continuous, it is necessary to stratify density values into discrete groups in order to compare empirical survivor functions at varying levels of density.

[49]Time-period dummies correspond the second period of a transition. Therefore, it refers to the year and month of re-employment for the UE subsample and month of censoring for the UU subsample.

tional hazard ratio estimates are presented in Table 1.11. The first column uses log labor force size as the scale measure and columns (2), (3), and (4) introduce log density as measured by county, urbanized area, and Landsat7 boundaries. In each specification, the estimated hazard ratios are less than one, indicating that increases in market scale decrease the probability of exiting unemployment over the baseline hazard. Recall that the hazard ratio estimates the probability that a worker will exit unemployment at any given interval relative to the baseline hazard, or the hazard rate that is common to the full sample. With the exception of the UA measure, estimated hazard ratios are statistically significant at the one-percent level. Perhaps more importantly, the positive relationship between market scale and duration is reinforced when the average proximity of workers and firms is controlled for.[50] Moreover, the effect is strengthened when density is measured with greater precision.

Hazard ratio estimates from the Cox PH model are reported in Table 1.11. The marginal change of an increase in the exogenous variables on the hazard ratio is determined by subtracting 1 from the exponentiated coefficient for each variable. For example, a one-percent increase in the size of the worker's local labor force reduces the probability of exiting unemployment at any time period 6.1 percent ($.939 - 1 = -.061 \times 100$ percent). Therefore, hazard ratio estimates greater than one indicate that the hazard increases (durations decrease) while estimates less than one predict a decrease in the hazard (increase in duration). Estimated hazard ratios decline when density is measured with more precision, decreasing from .891 to .810 when measured using county-based MSA boundaries and Landsat7, respectively.

Alternatively, the elasticity of the hazard ratio with respect to market scale can be determined by taking the natural logarithm of the hazard ratio.[51] The hazard ratio elasticity with respect to labor force size is $-.063$, indicating that a

[50] Recall from equation (12) in Section 2 that the expected duration of unemployment is inversely related to the hazard rate.

[51] This is due to the fact that hazard ratios are exponentiated coefficients and local market scale is measured using the natural logarithm.

doubling of labor force size reduces the hazard ratio 6.3 percent holding all else constant.

Table 1.11
Cox PH Hazard Ratio Estimates, Baseline Model

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.939***	0.891***	0.893*	0.810***
	(0.0136)	(0.0152)	(0.0590)	(0.0311)
Year and month controls	Yes	Yes	Yes	Yes
Demographic controls	No	No	No	No
Education controls	No	No	No	No
Observations	47,586	47,586	47,586	47,586
Log-likelihood	-168,576.90	-168,521.03	-168,622.13	-168,558.05
Number of clusters	257	257	257	257

NOTES: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers. Log variables correspond to the natural logarithm. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Similarly, a doubling of labor force density reduces the hazard ratio by 11.5 and 21 percent when measured using MSA and Landsat7 measures, respectively. Putting these figures in perspective, individuals in the San Francisco-Oakland-Fremont, CA MSA (2,707 workers per mi^2) are 21 percent less likely to exit unemployment at any given time relative to the baseline hazard than observationally similar workers in the Atlanta-Sandy Springs-Marietta, GA MSA (1,351 workers per mi^2).[52]

Table 1.12 presents Cox PH estimates for the full specification that includes controls for demographic and education characteristics. These estimates show a positive and robust relationship between market scale and unemployment durations for MSA permanently laid off workers. We see the same trend in the scale effect where hazard ratios tend to decline (thus durations increasing) when space is explicitly controlled for and measured with increasing precision. The magnitudes of estimated scale effects are qualitatively identical to baseline estimates. Includ-

[52]Based on 2006 annual average Landsat7 density measures.

ing controls, however, does modestly improve the fit of the model as evidenced by the less-negative log-likelihood value.

Figure 4 presents Cox-Snell residual plots for the baseline and fully specified Cox PH models (Panel A and B, respectively). If the model fits well, then the conditional cumulative hazard function should be distributed unit exponential with hazard rate equal to one. Therefore, the cumulative hazard function, approximated by the Nelson-Aalen estimator, should follow the 45-degree line.[53] Both models fit reasonably well, with the residuals closely following the 45-degree line for shorter durations with slight deviation at higher durations.[54] Both models show a degree of misspecification at very long spell durations where the cumulative hazard function diverges from the 45-degree line.

One explanation for the poor fit at higher durations could be the presence of unobserved heterogeneity. A priori, the source of any unobserved heterogeneity is not clear, therefore making any adjustment may lead to additional misspecification. Another potential source of misspecification may come from the grouping of spell durations at weekly intervals. Therefore, a more appropriate specification would be to model durations in a discrete setting that explicitly takes into account the grouped nature of the data rather than imposing a continuous structure on spell durations. We turn to this next.

5.3 Discrete-Time PH Model Estimates

Table 1.13 presents discrete-time proportional hazard model estimates using the same specifications in the previous section. Each specification includes 55 dummy variables for time intervals at risk which serve as a non-parametric (piecewise constant) specification of the baseline hazard.[55]

[53] This test is mentioned in Cameron and Trivedi (2005) and presented in summary form for estimation in Stata at `http://www.ats.ucla.edu/stat/stata/seminars/stata_survival/default.htm`.

[54] The cumulative hazard function is sorted by duration, therefore smaller values of the residuals correspond to shorter durations and higher values to longer durations.

[55] A failure must occur in each interval for the specification to be identified. We include dummies for each weekly interval from 1 to 40 weeks at risk, then create 5-week interval dummies for the remaining intervals at risk.

Table 1.12
Cox PH Model Hazard Ratio Estimates, Full Specification

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.942***	0.901***	0.858**	0.803***
	(0.0137)	(0.0143)	(0.0537)	(0.0261)
High school or GED	1.091***	1.094***	1.087***	1.092***
	(0.0290)	(0.0292)	(0.0286)	(0.0288)
Some college, no degree	1.225***	1.225***	1.222***	1.228***
	(0.0355)	(0.0354)	(0.0353)	(0.0352)
Two-year degree, vocational	1.354***	1.354***	1.349***	1.357***
	(0.0586)	(0.0585)	(0.0580)	(0.0583)
Two-year degree, academic	1.240***	1.249***	1.224***	1.243***
	(0.0648)	(0.0663)	(0.0641)	(0.0648)
Four-year degree	1.202***	1.210***	1.185***	1.210***
	(0.0397)	(0.0388)	(0.0385)	(0.0390)
Master's degree	1.138***	1.147***	1.123**	1.148***
	(0.0537)	(0.0542)	(0.0512)	(0.0537)
Professional degree	1.240*	1.253*	1.210	1.256*
	(0.151)	(0.153)	(0.149)	(0.152)
Doctoral degree	0.962	0.971	0.952	0.977
	(0.103)	(0.104)	(0.102)	(0.105)
Year and month controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Observations	47,586	47,586	47,586	47,586
Log-likelihood	-167,453.66	-167,413.83	-167,478.56	-167,425.99
Number of clusters	257	257	257	257

NOTES: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers. Log variables correspond to the natural logarithm. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

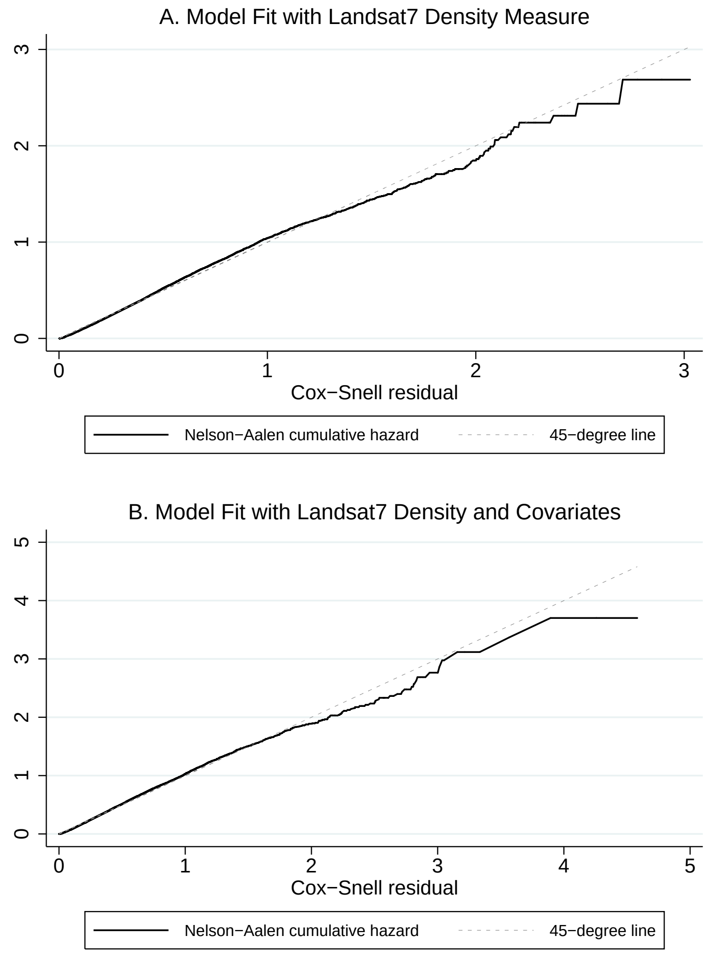

Figure 4
Cox-Snell Residual Plot of Cox Proportional Hazard Model, Current Population Survey January 1994–February 2012

The parameter estimates in Table 1.13 are exponentiated coefficients and can be interpreted has hazard ratios.

As in the continuous case, each of the hazard ratio estimates on labor market scale are less than one, indicating that unemployed workers in larger and denser areas are less likely to exit unemployment in any given period holding all else constant. Interestingly, the estimated scale effect on the unemployment-exit hazard is much stronger in the discrete-time case compared to the continuous-time estimations. For example, the hazard ratio falls from 0.942 to 0.810 for MSA size and from 0.803 to 0.566 for Landsat7-based density.

As before, the effect of scale strengthens both when space is explicitly controlled for and measured with greater precision. Controlling for space reduces the hazard ratios from 0.810 to 0.764 using MSA boundaries to control for the geographic extent over which search takes place. When using Landsat7 to measure density, the estimated hazard ratio falls to 0.566. These estimates suggest that each one-percent increase in density reduces the hazard ratio 43.4 percent relative to the baseline hazard.

5.4 Differences by Gender, Race and Ethnicity, and Business Cycle

In this section we compare the effects of density on the hazard of exiting unemployment by gender, marital status, race and ethnicity, and differences in the business cycle. Continuous-time Cox PH model coefficient estimates for Landsat7 density are reported in Table 1.14.

The first two rows of Table 1.14 present density coefficient estimates for males and females, respectively. The hazard ratios for males and females are both less than one and are qualitatively similar. The female hazard ratio is about 4 percent smaller than that for males, suggesting that they search for longer periods than males in denser areas holding all else equal. One potential source for this difference is the increasing labor force participation of women and the "reversal" of the college gender gap (Goldin et al., 2006). Females with higher relative levels of education may be willing to search for longer periods in denser areas to find a more productive match. In our sample, women are more likely than males to have attended some college or earned a two-year degree than men (32 percent compared to 26 percent, respectively) and men are more likely to have only attained a high school diploma (four-year college degree, master's, and professional degree rates are qualitatively similar, while men have a slight edge in Ph.D. attainment). However, the estimates in Table 1.12 show that unemployment exit rates increase with educational attainment. This suggests that the differential response to density in male and female unemployment exit rates may exist along other dimensions, such

Table 1.13
Discrete-Time PH Model Hazard Ratio Estimates, Current Population Survey, January 1994–February 2012

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.810***	0.764***	0.532***	0.566***
	(0.00594)	(0.0147)	(0.0135)	(0.0136)
Age	0.971***	0.965***	0.976***	0.975***
	(0.001)	(0.0011)	(0.001)	(0.001)
Female	0.969**	0.943***	0.999	0.991
	(0.0131)	(0.0131)	(0.0135)	(0.0136)
African American	0.754***	0.724***	0.715***	0.731***
	(0.0187)	(0.0180)	(0.0173)	(0.0163)
Native American	0.918	0.856**	1.006	0.960
	(0.0547)	(0.0549)	(0.0635)	(0.0578)
Asian or Pacific Islander	0.753***	0.738***	0.796***	0.777***
	(0.0538)	(0.0463)	(0.0390)	(0.0435)
Hispanic	0.996	0.899**	1.063	1.033
	(0.0332)	(0.0392)	(0.0423)	(0.0387)
Foreign born, citizen	1.145***	1.166***	1.077***	1.139***
	(0.0298)	(0.0285)	(0.0286)	(0.0288)
Foreign born, non-citizen	1.444***	1.405***	1.424***	1.478***
	(0.0402)	(0.0384)	(0.0407)	(0.0412)
Year and month controls	Yes	Yes	Yes	Yes
Education controls	Yes	Yes	Yes	Yes
Piecewise constant baseline	Yes	Yes	Yes	Yes
Observations	1,880,983	1,880,983	1,880,983	1,880,983
Log-likelihood	-409,152,382	-412,526,143	-406,312,045	-406,750,280
Number of clusters	257	257	257	257

NOTES: Estimates produced by the complementary log-log model. The dependent variable is a binary indicator of unemployment exit at time T_i. Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers. Log variables correspond to the natural logarithm. CPS "final" weights used (PWSSWGT). Cluster-robust standard errors in parentheses (clustered by MSA). Education controls are binary indicators for each CPS educational attainment level from high school degree and above; high school dropouts and less than high school comprise the omitted category. White and all other races comprise the omitted race category. The piece-wise constant baseline hazard function has 55 dummy for failure intervals. Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 1.14
Cox PH Coefficient Estimates of Landsat7 Density by Select Demographic Characteristics and the Business Cycle, Current Population Survey, January 1994–February 2012

Gender	Coefficient	Std. Error	Observations	Clusters
Male	0.818***	(0.0294)	27,561	256
Female	0.786***	(0.0260)	20,026	257
Marital Status				
Married	0.796***	(0.0261)	26,249	257
Unmarried	0.817***	(0.0314)	21,338	256
Married, female	0.779***	(0.0413)	8,069	252
Married, male	0.842***	(0.0340)	13,269	254
Unmarried, female	0.795***	(0.0277)	11,957	257
Unmarried, male	0.800***	(0.0346)	14,292	253
Ethnicity and Race				
Hispanic	0.793***	(0.0418)	7,471	200
Non-Hispanic	0.811***	(0.0249)	40,116	256
Hispanic, U.S. born	0.736***	(0.0570)	3,569	188
Hispanic, foreign born (FB)	0.854***	(0.0517)	3,902	157
Hispanic, FB, non-citizen	0.612***	(0.0739)	1,074	106
Hispanic, FB, U.S. citizen	0.929	(0.0609)	2,828	143
Non-Hispanic, black	0.756***	(0.0327)	8,221	227
Non-Hispanic, white	0.820***	(0.0308)	29,235	255
Business Cycle†				
Pre-Recession	0.768***	(0.0333)	27,625	256
Post-Recession	0.899***	(0.0363)	19,962	248

NOTES: Estimates reported as hazard ratios. Coefficient estimates based on log Landsat7 density measure (see Section 3.2). Sample includes completed and censored spells for MSA displaced workers. Each estimation includes demographic, education, and year and month controls. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Year controls refer to year of survey. A complete list of included demographic and education controls are reported in Table 2.15. Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

† Pre-recession refers to the January 1994–December 2007 sample, or the period prior to the "Great Recession." Post-recession refers to the period including the Great Recession and after. The Great Recession is officially recorded as the period December 2007 to June 2009 (for more information on recessions, see `http://www.nber.org/cycles.html`.

as industry or occupation. For example, females comprise a disproportionate share of those displaced from health care (79 percent), health care support (86 percent), and personal care (74 percent) occupations in the post-2002 subsample. To the extent that labor demand is increasing over time in these occupations and these occupations are concentrated in denser areas, the difference in re-employment probabilities between males and females may be due to increased opportunity combined with greater selectivity of searchers in typically female-dominated occupations. This is an interesting area for future research.

Rows three through six show the density effect on hazard ratios by marital status and by gender. All hazard ratios are less than 1. Married workers on permanent layoff have slightly lower hazard ratios than unmarried workers. Married workers may afford to be more selective and thus search for longer periods if their spouse is employed and earning income.[56] Married and unmarried females in denser areas have lower unemployment exit probabilities than their male counterparts with a larger gap existing for married couples.

Rows nine through 16 of Table 1.14 present density hazard ratios by race and ethnicity. All hazard ratios are less than 1 which is consistent with the view that workers react to cost savings in search by adopting more selective search strategies. There is very little difference in the density effect for Hispanic and non-Hispanic workers. A different picture emerges when Hispanic workers are broken down into foreign born and non-foreign born. The hazard ratio for foreign-born Hispanic workers is 16 percent less than domestic-born Hispanic workers, suggesting that foreign-born Hispanics are less choosy than their domestic-born counterparts. One reason for this difference may be that foreign-born Hispanics are concentrated in a particularly narrow set of industries or occupations, perhaps due to not having the same level of English-speaking ability as domestic-born Hispanics, where they are more directly substitutable. Informal job search networks may be particularly important to foreign-born Hispanics. If referrals are more important for this group

[56]Spouse identification is possible in the CPS. Future research should include the spouse employment status.

than traditional formal search methods, this it may explain the declining importance of density compared to domestic-born Hispanics.[57] Further, foreign-born Hispanics are concentrated in a more narrow set of MSAs than domestic-born (157 compared with 188), suggesting their unemployment outcomes may be more directly tied to the fortunes of a small set of metropolitan areas.

Both non-Hispanic whites and blacks have longer average search durations in denser areas. Blacks search for longer periods than whites in denser areas. While this result suggests that both blacks and whites tend to be more selective in denser areas, labor market discrimination or residential location may explain the additional decline in re-employment probabilities in denser areas for laid off black workers. Residential location is an important determinant of unemployment outcomes because it determines the worker's accessibility to jobs.[58] Since blacks tend to live in urban centers while job opportunities have tended to move to urban fringes. As a result, spatial segregation leaves blacks with relatively poor access to jobs (Ihlanfeldt and Sjoquist, 1990; Zenou, 2009) which may explain the difference in re-employment probabilities between blacks and whites.[59]

The last two columns of Table 1.14 present density hazard ratios for the period prior to the Great Recession and the period including the Great Recession and after. Interestingly, density is associated with longer search durations in each period, *ceteris paribus*. As expected, workers in the post-Great Recession period face fewer job opportunities and therefore can afford to be less selective about their next job should they become displaced. Indeed, the density hazard ratio for the post-Great Recession period is much closer to 1 (and about 17 percent larger) than the period before the downturn. Nonetheless, density is significantly associated with smaller re-employment probabilities, which we take as evidence

[57]Bayer et al. (2008) show that individuals residing within the same local neighborhood (or Census block) are 33 percent more likely to be employed at the same employer than those living in different neighborhoods.

[58]This is the so-called *spatial-mismatch hypothesis* (e.g., Zenou, 2000).

[59]Rogers (1997) presents evidence that accessibility to jobs is associated with shorter average unemployment durations.

that workers in dense areas face lower search costs and react by trading longer search durations for better re-employment matches.

6 Summary

In this chapter we developed a simple job search model to demonstrate how an unemployed worker's job search behavior is affected by labor market scale. Our key assumption is that proximity increases search efficiency by reducing the average distance and therefore the average cost of making contact with potential trading partners. Therefore, unemployed workers and firms with vacancies are assumed to make more frequent contacts in dense areas where they are, on average, situated closer together in space. We show that the overall effect on duration depends on two offsetting influences: a direct increase in the contact rate which decreases average search durations and the endogenous response of workers to set higher reservation wages in response to lower search costs, which raises average search durations. In order to test which of the two effects dominates, we collected a large sample of individual unemployment spells over 261 spatially distinct labor markets. Using measures of local labor market scale for each market, we applied continuous- and discrete-time proportional hazard models to estimate the total effect of market scale on the hazard rate of an unemployed worker finding employment.

Local market scale is measured using the total number of workers (employed and unemployed but looking for work) in 261 MSAs and the number of workers per unit of land area occupied by the MSA, or density. While it may be sensible to think of scale as a size issue, we take the view that density more accurately captures the notion of relatively proximity to potential searching partners. We construct three different measures of density, each varying in the precision with which urban area boundaries are measured. As expected, MSA density measures that rely on county boundaries to define the extent of urban areas are sensitive to the size of each county comprising the MSA. Therefore, MSAs densities are more likely to be underestimated in larger states with fewer counties, such as in the western

U.S. To get around this issue, we introduce remote sensing data from the NASA Landsat7 program to identify the extent of urban coverage within traditionally defined MSA boundaries. In all cases, Landsat7-based density measures show an increase in the calculated density. On average, using MSA boundaries understates measured density by an average of 903 square miles compared to Landsat7 area measures.

Our sample of completed unemployment spells is taken from matched pairs of consecutively monthly surveys of the CPS, which allows for identification of individual movements across labor market states. The CPS is a particularly valuable survey for answering questions pertaining to local labor market effects on individual job search behavior as it is the only publicly available survey of its kind that routinely collects information on unemployment duration, detailed industry and occupation, demographic, and disaggregated location. Further, it is a relatively large (60,000 households) and high-frequency survey which produces sample sizes large enough to estimate the effect of local area conditions on important economic outcomes. The tradeoff is that the CPS does not follow individual over long periods of time like a longitudinal survey does. Therefore, many unemployed spells in the CPS are censored. Further, CPS durations are measured with error since we only know that a spell terminates over a four-week interval but not the specific time within that interval. In addition, workers may have multiple transitions over a four-week period.

Continuous-time Cox PH model estimates suggest that market scale is negatively related to the hazard rate of exiting unemployment. A doubling of the number of workers in the local labor force is associated with a decrease in the hazard of exiting unemployment 6 percent relative to the baseline hazard. When space is explicitly controlled for, the decrease in the hazard rate for a doubling of the number of workers is 10.4 percent when measured using MSA boundaries and 21 percent when measured using Landsat7 data. These results are robust to the inclusion of demographic and educational characteristics related to duration, and the positive relationship increases in magnitude as density is measured with greater precision. These results are reinforced when we apply a discrete-time specification, where a one-percent increase in labor market density is associated with a 43.4 percent reduction in the likelihood that an unemployed worker will exit unemployment over the baseline hazard.

In the context of the job search model, these results suggest that the reservation wage response to an increase in the contact rate outweighs the direct effect of higher contact rates on the conditional probability of unemployment exit. That is, density lowers the cost of making contact with potential search partners which, in equilibrium, raises the expected value of continued search relative to the total costs of search. For a fixed wage-offer distribution, any increase in the reservation wage reduces the likelihood that any given wage offer will satisfy the reservation wage condition. As a result, workers in denser areas will search on average for a greater number of periods, but eventually find a higher wage upon reemployment.

An addition to measurement error issues associated with identifying a completed spell duration and measuring its length, several other important sampling issues may be biasing my results toward a finding of a negative relationship between density and average spell durations. First, our sample omits individuals who do not reside in MSAs. Indeed, MSA identification is crucial for relating measures of market scale to individual search durations. Population densities are lower in outlying areas and substate areas smaller than counties, which are often suppressed in the CPS, are not typically recorded. Moreover, it is unclear where exactly in relation to any given MSA non-urban (i.e., rural) residents live. It is possible that rural residents may benefit by being near the fringe of urban areas, where they can consume greater quantities of housing but still benefit from job-search efficiencies associated with density. If there were enough of these workers who experience systematically shorter durations through proximity to urban densities, then it is possible that the estimates reported here overstate the reservation wage effect on the hazard rate. However, about 70 percent of all Americans live in

MSAs, so it's unlikely that omitting "fringe" workers could be biasing our result toward longer durations in denser markets.

Second, given that firms have some discretion over whom to layoff, the least-productive workers are more likely to be laid off. The layoff event signals to other firms in the market that a permanently laid off worker is a "lemon" (Gibbons and Katz, 1991). Thus, permanently laid of workers may face more difficult re-employment prospects and are more likely to search for long periods. Gibbons and Katz (1991) identify this effect by making the assumption that workers displaced through plant closings are less likely than other permanent layoffs to lose their jobs through a lemon effect. For our purposes, the CPS does not identify the particular cause of permanent layoff. We explore this in greater detail in the following chapter.

Third, the CPS offers no information on geographic mobility. When workers change residences–either within or across MSAs—they are lost from the CPS sample. It is likely that our sample misses individuals who moved out of MSAs as well as those moving into others. To the extent that worker geographic mobility decisions are made based on expectations about future displacements, then we may be missing individuals who are leaving poorly performing labor markets and those who enter markets where labor demand is high. Absent data on geographic moves it is very difficult to identify this effect. Although we restrict our sample to unemployment spells that are observed for very short intervals and that are assumed to occur within a single spatially distinct market, we still lack information on whether they are indigenous to a particular MSA or have moved.

Finally, workers may face congestion externalities from other unemployed workers especially in periods of high unemployment. However, absent data on local vacancies by job type (e.g., industry, occupation, education and experience requirements) it is difficult to assess this possibility. In addition, the model used here doesn't account for heterogeneities in worker and job types. While it is possible that many workers are substitutable across jobs and, indeed, there is a high incidence of industry and occupation switching, all workers are not substitutable across all job types. Thus, competition for jobs depends on local conditions in vacancy creation and the skill endowments of the workers competing for those jobs.

In the next chapter, we continue with our analysis by testing whether displaced workers in dense areas actually earn higher wages following displacement. This requires the introduction of new data sources that report earnings and detailed location information.

Chapter II

LABOR MARKET SCALE, UNEMPLOYMENT DURATION, AND RE-EMPLOYMENT EARNINGS OF DISPLACED WORKERS

1 Introduction

In the previous chapter, we developed a simple model of individual search behavior and demonstrated that labor market density increases the job-offer arrival rate which has two offsetting effects on the equilibrium duration of unemployment: negatively through a direct increase in the rate of contact between unemployed workers and vacancies and positively through an increase in workers' reservation wages. We showed that the total effect of density, or market scale, on duration depends on which of these two effects dominates. Using a large sample of individual unemployment spells of workers in spatially distinct labor markets, we found a robust negative relationship between labor market density and the hazard rate, suggesting that workers in denser areas are willing to trade longer spells of unemployment for better wage offers.

While the results presented in the previous chapter point to a conclusion that average unemployment durations are longer in denser areas due to the adoption of more selective search strategies on the part of workers, a more complete test of the theory requires direct comparison between the reservation wage and job-offer arrival rate elasticities with respect to market scale. Absent explicit measures of reservation wages or vacancy contacts—neither of which is available in the CPS—such a comparison is not possible. However, if unemployed workers are willing to trade longer search durations for higher earnings due to lower costs of search, then we ought to observe workers in denser areas searching longer *and* earning higher wages upon re-employment.

This chapter uses data from the Displaced Workers Supplement (DWS) to examine the relationship between labor market density and post-unemployment earnings of permanently laid-off workers in U.S. metropolitan areas. The DWS is a biennial supplement to the CPS that collects additional information on the unemployment experience of workers who permanently lost a job within three years of the survey date. In addition to the variables collected by the basic monthly CPS we used in Chapter I, the DWS reports earnings, industry, occupation, and union status on both the displacement job and the current job (if any), as well as the duration of unemployment and other information related to the unemployment experience of displaced workers. Therefore, the DWS make it possible to relate post-unemployment earnings to the duration of unemployment and measures of local labor market scale.

To that end, we estimate a reduced-form post-unemployment earnings equation that controls for unemployment duration and the labor market density measures developed in Chapter I. If workers react to lower search costs by adopting choosier search strategies, then we expect density to be positively related to post-unemployment earnings after conditioning on spell duration and other determinants of earnings.

A major hurdle with this approach is that including unemployment duration as an explanatory variable in the post-displacement earnings equation produces inconsistent parameter estimates due to simultaneity between re-employment earnings and the length of the unemployment intervening spell (e.g., Greene, 2003, p. 75). Indeed, in choosing a reservation wage a worker is simultaneously choosing an expected duration through its feedback into the hazard rate. As we showed in equation (11) in Chapter I, the probability that a given wage offer will meet or exceed the reservation wage w^* for a fixed wage-offer distribution $F(w)$ is $[1-F(w^*)]$.

Therefore, any change in the reservation wage will affect this probability and therefore the hazard rate and expected duration of unemployment.

To deal with simultaneity bias, we use a two-stage least squares (2SLS) estimator for post-displacement earnings that uses fitted values from a first-stage duration equation as an instrumental variable for spell duration.[60] We also use earnings on the pre-displacement job to control for unobserved heterogeneity.

2 Data

2.1 Displaced Workers

Individual unemployment data are taken from the 1996–2010 waves of the Displaced Workers Supplement (DWS), a biennial supplement to the Current Population Survey (CPS).[61] The DWS, which is conducted in the first or second month of even-numbered years, identifies workers between the ages of 20 and 65 who lost a job within the last three years due to an employer-initiated separation, such as a plant closing, slack work, or because their shift was abolished. These individuals, or "displaced" workers, are asked follow-up questions about the characteristics of their displacement job (hereafter "old" job) including industry, occupation, and earnings. Earnings, hours, and detailed job attributes are also reported for the job held at the time of the survey, if any.

An individual's duration of unemployment is recorded as the number of weeks elapsed between displacement from the old job and finding the next one.[62] If a worker reports having at least one job since displacement, then the worker's reported duration represents a completed spell. A spell is right censored if the worker held no jobs since being displaced. Because the DWS records any job separation that occurred within the last three years, the current job may not correspond to their first job following displacement.

[60] This approach was first used by Addison and Portugal (1989).

[61] The first DWS was added to the CPS in January 1984.

[62] Reported durations may include time spent not looking for a job, where they would not be officially counted as "unemployed." Therefore, it may be more appropriate to refer to the worker's search duration as a "jobless" spell.

Job search theory relies on the reservation wage to inform the worker's decision rule to enter into an employment relationship. The reservation wage captures the explicit tradeoff between the net present discounted value of accepting a given wage offer and staying unemployed. The DWS, like most surveys, doesn't collect information on reservation wages. Since the reservation wage is the minimum wage offer the worker must receive to be willing to exit unemployment, the earnings received on the first job following displacement gives the best conceptual measure of a reservation wage in the DWS. Therefore, I restrict the sample to workers holding their first job following displacement.

Like the CPS, the DWS also records MSA of residence for displaced workers. To ensure that the individual's residence remained in the same labor market area for the duration of their unemployment spell, I exclude individuals who report making a geographic move following their displacement as well as those who do not reside within a MSA.[63]

Usual weekly earnings on the pre- and post-displacement job are available for most respondents. All earnings are deflated to July 2011 dollars using the Consumer Price Index for all urban consumers (CPIAUCNS).[64]

Observations for completed spells missing relevant information for either job, including detailed industry and occupation and weekly earnings are discarded. Right-censored observations are retained despite missing current-job industry and occupation in order to properly control for the effects of censoring.

The DWS sample used for this analysis includes 8,627 individual unemployment spells (6,188 complete, 2,439 censored). Sample summary statistics are reported in Table 2.15. Displaced workers tend to be a little older. Most were displaced from full-time employment and 81 percent of those who found jobs by the time of the survey were re-employed in full-time jobs. On average, displaced

[63]A geographic move in the DWS consists of a residential relocation across counties or MSAs. It is possible that an individual may move across counties within the same MSA but DWS public-use files provide no way of discerning the precise origins and destinations of a geographic move.

[64]CPI data taken from the St. Louis Federal Bank FRED II (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`.

workers' earnings were about 5 percent lower than their displacement job. As with the CPS sample, over two-thirds of re-employed displaced workers found employment in a different detailed industry or occupation than their displacement job and 54 percent changed both.

3 Estimation and Results

The main thesis of this chapter is that workers in dense labor markets respond to lower search costs by adopting choosy search strategies where they are willing to trade longer spell durations for a higher wage offer. If this is so, we expect workers in denser markets to earn higher wages upon re-employment holding constant their duration of unemployment.

3.1 Re-Employment Earnings

To estimate the effect of density on post-displacement earnings, I estimate a standard Mincerian earnings equation

$$\ln(w_{i,c}) = \beta_0 + \beta_1 \ln(h_{i,c}) + \beta_2 \ln(w_{i,d}) + \beta_3 \ln(T_i) + \beta_4 \ln(\rho_m) + \mathbf{X}_i'\boldsymbol{\theta} + u_{i,c,d,m} \quad (25)$$

where $w_{i,c}$ is usual weekly earnings of worker i on the current job; $w_{i,d}$ is usual weekly earnings on the displacement job; $h_{i,c}$ is usual weekly hours worked on the current job; T_i is the elapsed duration between displacement and the next job; ρ_m is density of metropolitan area m; $\mathbf{X}_i$ is a vector of individual characteristics and economic characteristics; $\beta_0 \ldots \beta_4$ and θ are parameters to be estimated; and u is a random error component. Our primary interest is in the estimate of β_4, which is expected to be positive.

A major difficulty in estimating the parameters of equation (25) using OLS is simultaneity between spell duration and post-displacement earnings. As we saw in Chapter I, in choosing a reservation wage the worker is simultaneously selecting

Table 2.15
Sample Summary Statistics: Displaced Workers Supplement, 1996–2010

	Completed Spells		Censored Spells	
	Mean	Std. Dev.	Mean	Std. Dev.
Unemployment duration (weeks)	12.3	18.0	28.0	26.4
Weekly earnings lost job	962	863.78	892	833.20
Weekly earnings current job	920	839.28	–	–
Relative change in weekly earnings	-0.048	0.52	–	–
Tenure on lost job (years)	4.9	6.0	4.9	6.5
Age	39.7	10.9	40.8	11.8
Female	0.44	0.50	0.41	0.49
Married	0.59	0.49	0.51	0.50
Foreign born	0.16	0.36	0.19	0.39
Hispanic	0.14	0.34	0.17	0.38
African American	0.11	0.32	0.18	0.39
Native American	0.007	0.08	0.008	0.09
Asian or Pacific Islander	0.04	0.20	0.04	0.21
High school or equivalent	0.30	0.46	0.33	0.47
Some college, no degree	0.21	0.41	0.20	0.40
Two-year degree, vocational	0.05	0.21	0.04	0.20
Two-year degree, academic	0.05	0.23	0.04	0.19
Four-year degree	0.21	0.40	0.18	0.38
Master's degree	0.06	0.24	0.04	0.20
Professional degree	0.009	0.10	0.005	0.07
Doctoral degree	0.008	0.09	0.003	0.06
Received UI benefits	0.40	0.49	0.67	0.47
Received and exhausted UI benefits*	0.33	0.47	0.20	0.40
Received advanced notice of layoff	0.37	0.48	0.33	0.47
Expect recall within 6 months	0.003	0.05	0.013	0.11
Union member on lost job	0.08	0.27	0.11	0.31
Changed detailed occupation	0.69	0.46	–	–
Changed detailed industry	0.67	0.47	–	–
Changed detailed industry and occupation	0.54	0.50	–	–
Full time to full time	0.81	0.39	–	–
Full time to part time	0.09	0.28	–	–
Part time to full time	0.05	0.22	–	–
Number of observations	6,188		2,439	

NOTES: Author calculations from the MSA subsample of the Displaced Workers Supplement to the Current Population Survey. Observations with missing or zero reported tenure on displacement job are omitted. Re-employment industry, occupation, and earnings variables available for completed spells only. Earnings deflated to constant July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`). All values weighted by CPS final weights (PWSSWGT).
* Value corresponds to the share of UI recipients.

an expected duration of unemployment. Further, spell duration is closely related to expected earnings upon re-employment. Generally, this implies that T_i is correlated with u_i which violates the orthogonality condition necessary for consistency of OLS (e.g., Wooldridge, 2002, p. 51).

To deal with simultaneity between duration and earnings, I implement a two-step procedure that uses predicted spells durations from a first-stage unemployment duration equation as a regressor in the second-stage post-displacement earnings equation. This is commonly referred to as the two-stage least squares (2SLS) estimator. If we can find a variable z_i that is correlated with T_i but uncorrelated with u_i, then 2SLS will produce consistent coefficient estimates in equation (25) (e.g. Greene, 2003).[65]

For the first stage, we specify the following reduced-form unemployment duration equation

$$\ln(T_i) = \alpha_0 + \alpha_1 \ln(\rho_m) + \mathbf{X}_i'\boldsymbol{\phi} + \gamma z_i + \nu_{i,m} \tag{26}$$

where z_i is a variable such that $E[zu] = 0$, $\mathbf{X}_i$ is the same set of exogenous variables in equation (25), and ν is the normal iid error term. In the second stage, fitted values of equation (26) are used in place of $\ln(T_i)$ in the post-displacement earnings equation.

A similar approach was used by Addison and Portugal (1989) to estimate the re-employment earnings effects of spell duration for displaced workers. One key difference in our approach is that the first-stage duration equation is estimated using OLS, whereas Addison and Portugal (1989) specify a fully parametric accelerated failure-time (AFT) model. The AFT approach has the advantage of being able to directly handle censored and uncensored observations, thereby producing more consistent parameter in the duration equation. One difficulty, however, lies in choosing the functional form for the underlying parametric distribution of unemployment spells. Different parametric distributions may impose an unrealistic

[65]We will use as instruments log tenure on displacement job and receipt of UI from that displacement, as discussed below.

structure on the relationship between the hazard rate and spell duration (Cameron and Trivedi, 2005).[66] Further, unless the functional form in the first stage is specified perfectly, misspecification from the first stage will not generate consistent second-stage estimates (Angrist and Krueger, 2001, p. 80).

3.2 Results

This section presents empirical results for unemployment duration and re-employment earnings for the MSA sample of the DWS. We begin by examining the cross-sectional relationship between market size, density, and mean unemployment durations for completed spells in the DWS. Cox PH model estimates and 2SLS estimates of equations (25) and (26) follow.

Table 2.16 presents mean duration of completed spells by labor force size and labor force density (based on Landsat7 area measures). The DWS shows a modest positive relationship between density and duration. Average duration in the smallest density category is almost as long as the largest, but the former is only supported by 22 observations. Taking the next size category up, we see that the most dense labor markets have an average duration 3.2 weeks longer than the second-least dense. As with the CPS calculations, there doesn't appear to be much of a relationship between labor force size and average spell durations. Therefore, the DWS sample appears to reflect the same information found in the CPS; if space reduces search frictions, workers react by increasing their search durations.

3.2.1 Cox PH Estimates

Because the DWS sample differs from the CPS sample, we begin by estimating a set of Cox PH models relating market scale to individual unemployment exit probabilities. This accomplishes two objectives. First, it provides an additional sample by which to test the relationship between market scale and search strate-

[66] This is commonly referred to as *duration dependence*. For example, an exponential specification treats the hazard rate as constant, while the commonly used Weibull specification assumes that hazard rates decline with duration. Other parametric distributions may offer more flexibility.

Table 2.16

Mean Duration of Completed Spells by Local Market Size and Density, Displaced Workers Supplement 1996–2010

	Labor Force Density (persons per square mile)							
	≤500	500-750	750-1,000	1,000-1,250	1,250-1,500	1,500-2,000	2,000-2,500	2,500+
Mean duration (weeks)	13.3	10.8	11.0	11.7	12.5	12.7	13.4	14.0
Std. Dev.	(20.5)	(14.4)	(17.3)	(17.2)	(18.3)	(18.8)	(18.0)	(18.6)
Number of observations	22	476	878	890	1,158	1,472	572	720

	Labor Force Size (thousands)					
	≤100	100-250	250-500	500-1,000	1,000-2,000	2,000+
Mean duration (weeks)	10.3	12.2	12.1	11.6	12.3	13.3
Std. Dev.	(15.1)	(18.8)	(17.5)	(16.9)	(18.1)	(18.4)
Number of observations	241	912	745	1,132	1,149	2,009

NOTES: Author's calculations from the Displaced Workers Supplement 1996 − 2010. All values are weighted by Current Population Survey "final" weights (PWSSWGT). Density measures are based on Landsat7 area calculations as described in Section 3.2.

gies. Second, if the two samples show similar search outcomes, then the results from the DWS can be generalized to the CPS sample. This is particularly useful given that the DWS collects important information about individual unemployment spells that the basic CPS does not, such as information about unemployment insurance benefits and pre- and post-displacement earnings. The estimation framework is identical to that presented in the previous chapter. The sample used is described in detail in Section 2.1.

Table 2.17
Cox PH Model Baseline Estimates: Displaced Workers Supplement, 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.932***	0.888***	0.874	0.792***
	(0.0195)	(0.0224)	(0.0733)	(0.0459)
Year controls	Yes	Yes	Yes	Yes
Demographic controls	No	No	No	No
Education controls	No	No	No	No
Observations	7,431	7,431	7,431	7,431
Log-likelihood	-40,431.09	-40,418.26	-40,446.39	-40,424.28
Number of clusters	247	247	247	247

NOTES: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers (1,196 spells having a duration of zero weeks are omitted). Log variables correspond to the natural logarithm. Sampling weights *not* used. Year controls refer to year of survey. Cluster-robust standard errors in parentheses (clustered by MSA). Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Baseline Cox PH hazard ratio estimates using the same four measures of labor market scale are reported in Table 2.17. The baseline model includes density and year controls as the only regressors. As with the basic CPS, the estimated hazard ratios are less than 1, indicating that workers in larger and more dense labor markets are less likely to exit unemployment compared to the baseline hazard at any duration. The negative relationship between market scale and the hazard rate is strengthened when we explicitly control for space and this effect becomes stronger as the labor market area is more precisely measured. Note, however, that the hazard ratio estimate is not statistically significant for the UA-based density measure. The magnitudes of the hazard ratios are qualitatively similar to those reported in the basic CPS.

Table 2.18 reports Cox hazard ratio estimates that include individual demographic and educational controls, unemployment insurance (UI) receipt, and union status and tenure on the displacement job. All hazard ratio estimates for labor market scale continue to be less than 1, although their effects are weakened slightly. As before, the negative effect of labor market scale on the hazard rate increases with the precision of labor market area measurement. These results may indicate that the estimated negative association between market scale and hazard ratios calculated from the CPS is likely to be slightly overstated due to unobserved job tenure and union status on the displacement job, and UI benefits. Individuals who receive UI benefits are almost half as likely at any given spell duration to exit relative to those who do. Put another way, those who receive unemployment insurance benefits have unemployment spells that are almost twice as long as those who do not, all else equal. This suggests that labor market scale measures may be picking up geographic differences in unemployment insurance generosity in the basic CPS, however this effect on the estimated impact of market scale on hazard rates appears to be small.

Labor market scale has a robust negative effect on the rate of unemployment exit for displaced workers. This effect is reinforced when space is explicitly controlled for and measured with increasing precision. These results are consistent with the view that unemployed workers react to lower search costs by adopting more selective search strategies. These results also support the view that space represents an important friction in search.

We apply the same plot of residuals agains the 45-degree line for the baseline and fully specified model in Figure 5. Reelative to the previous chapter which uses data from the CPS, the continuous Cox PH specification performs relatively poorly with DWS data. Residuals from the baseline model in Panel A show a much closer correspondence with the unit exponential function but including regressors

Table 2.18
Cox PH Estimates, Full Specification: Displaced Workers Supplement, 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.956**	0.936***	0.871**	0.838***
	(0.0176)	(0.0218)	(0.0566)	(0.0408)
Received UI benefits	0.572***	0.575***	0.569***	0.574***
	(0.0201)	(0.0204)	(0.0196)	(0.0202)
Exhausted UI benefits	0.562***	0.562***	0.561***	0.561***
	(0.0191)	(0.0192)	(0.0191)	(0.0192)
Received advance notice	1.040	1.043	1.039	1.042
	(0.0311)	(0.0309)	(0.0309)	(0.0308)
Union member on lost job	0.881**	0.881**	0.878**	0.877**
	(0.0477)	(0.0479)	(0.0478)	(0.0480)
Log tenure on lost job (years)	1.031**	1.031**	1.030**	1.030**
	(0.0144)	(0.0144)	(0.0145)	(0.0144)
Year controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Education controls	Yes	Yes	Yes	Yes
Observations	7,431	7,431	7,431	7,431
Log-likelihood	-39,875.02	-39,872.80	-39,877.31	-39,868.70
Number of clusters	247	247	247	247

Notes: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers (1,196 spells having a duration of zero weeks are omitted). Log variables correspond to the natural logarithm. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Year controls refer to year of survey. A complete list of included demographic and education controls are reported in Table 2.15. Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

(Panel B) drastically reduces the model fit. Just like the CPS in Chapter I, DWS durations are grouped by weekly intervals in which case a more appropriate specification would be a discrete-time PH model.

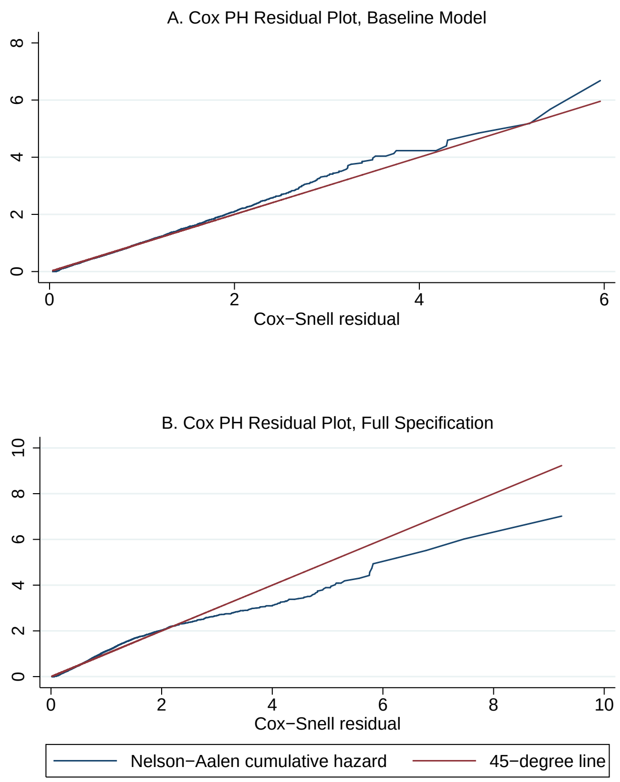

Figure 5
Cox PH Residual Plot Against 45-Degree Line, Displaced Workers Supplement, 1996–2010

Table 2.19 presents discrete-time PH model estimates for the DWS sample using the same procedure described in the previous chapter. Like the basic CPS, the baseline hazard is specified to be a non-parametric piecewise constant function of time. Because there are fewer observations in the DWS, we only specified 29 intervals to insure that all intervals contained failures. As before, the negative effect of duration on the re-employment hazard incrases when scale is measured with greater prcecision. Moreover, discrete-time model estimates show a stronger negative effect on the re-employment hazard compared to the continuous-time specification. Taking the Landsat7 measure (column (4)), the estimated hazard ratio increases from 0.838 to 0.809.

Table 2.19
Discrete-Time Proportional Hazard Model Estimates, Displaced Workers Supplement, 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.948**	0.913***	0.827**	0.809***
	(0.0217)	(0.0251)	(0.0731)	(0.0498)
Age	0.981***	0.982***	0.981***	0.981***
	(0.00160)	(0.00159)	(0.00167)	(0.00162)
Female	0.966	0.966	0.964	0.967
	(0.0349)	(0.0351)	(0.0349)	(0.0350)
Married	1.169***	1.165***	1.167***	1.162***
	(0.0379)	(0.0379)	(0.0382)	(0.0381)
Foreign born	0.996	1.004	0.992	1.017
	(0.0508)	(0.0517)	(0.0539)	(0.0552)
Hispanic	1.009	1.007	1.017	1.022
	(0.0596)	(0.0623)	(0.0596)	(0.0619)
African American	0.747***	0.746***	0.735***	0.741***
	(0.0424)	(0.0425)	(0.0404)	(0.0419)
Native American	0.890	0.871	0.894	0.885
	(0.164)	(0.159)	(0.164)	(0.160)
Asian or Pacific Islander	0.950	0.956	0.967	0.964
	(0.0987)	(0.0984)	(0.0976)	(0.0976)
Plant closed or moved	1.084**	1.085**	1.082*	1.083**
	(0.0441)	(0.0442)	(0.0445)	(0.0442)
Insufficient work	0.977	0.977	0.976	0.979
	(0.0351)	(0.0353)	(0.0351)	(0.0355)
Education controls	Yes	Yes	Yes	Yes
Time controls	Yes	Yes	Yes	Yes
Observations	143,989	143,989	143,989	143,989
Log-likelihood	-48090169	-48073571	-48092819	-48067506
Number of clusters	250	250	250	250

NOTES: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Sample includes completed and censored spells for MSA displaced workers (1,196 spells having a duration of zero weeks are omitted). Log variables correspond to the natural logarithm. Sampling weights *not* used. Year controls refer to year of survey. Cluster-robust standard errors in parentheses (clustered by MSA). Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

3.2.2 2SLS Earnings Estimates

Table 2.20 presents 2SLS estimates of post-unemployment earnings of displaced workers. Re-employment earnings are available for those individuals who are employed at the time of the survey, thus censored observations are omitted. Standard OLS estimates of equation (25) are reported in Panel A. In all specifications, the elasticity between duration and post-displacement earnings is $-.04$, which is about half the magnitude reported by (Addison and Portugal, 1989). One interpretation of this negative coefficient is that reservation wages decline with spell duration. However, focusing on the urban subsample and controlling explicitly for market scale, the smaller coefficient may indicate that search is more productive in larger and more dense markets which requires smaller reservation wage declines.

The coefficient on labor market scale is positive and statistically significant at the one-percent level in all specifications. The scale elasticity increases in magnitude as the labor market area is estimated with greater precision, ranging from a value of 0.04 using county-based area measures to 0.11 using Landsat7-based measures.

Panel B reports post-displacement earnings estimates using 2SLS. These specifications are identical to those in Panel A except that fitted values of equation (26) are used in place of duration to control for simultaneity with re-employment earnings.[67] Exclusion restrictions in the first stage are the log of tenure on displacement job and a binary indicator of whether the displaced worker received UI benefits.[68]

[67]First-stage OLS duration estimates are reported in Table D.33. A useful feature of OLS is it offers a relatively simple estimate of the duration-density elasticity, which is estimated to be 0.10 based on the Landsat7 area measure. The difficulty, however, is that it doesn't handle censoring as well as techniques designed to handle duration data. Each specification includes a censoring dummy variable, which allows the use of the full sample of spell durations while controlling for difference in spell durations for spells that have yet to be completed. OLS and 2SLS estimates are not adjusted for within-cluster heteroskedasticity due to issues associated with conducting a Hausman test on heteroskedasticity adjusted standard errors.

[68]Validity of the instruments requires that they be correlated with the first stage duration equation but uncorrelated with post-displacement earnings. Coefficient estimates for UI receipt and log tenure on pre-displacement job in Table D.33 show that both are economically and statistically significantly related to log spell duration (although they tend to work in opposite directions): workers receiving UI benefits have 138 percent longer durations on average than those who do not, and each year of tenure on the pre-displacement job is associated with 3.5- to 3.7-percent reduction in average spell duration. Both are significant at the 1-percent level.

In each specification, the duration-earnings elasticity becomes more negative, which is consistent with Addison and Portugal (1989). In particular, accounting for simultaneity increases the absolute magnitude of estimated duration-earnings elasticity about 50 percent to -0.06 according to the Landsat7-based specification. The positive earnings-density elasticity is robust. When density is measured with precision, the positive effect of scale more than offsets any negative effects of duration.

If duration is endogenous to post-displacement earnings, then OLS estimates will be efficient but inconsistent and 2SLS will be consistent but less efficient (the loss of efficiency comes about due to the additional error associated with predicting duration from the first stage). Table 2.20 reports Hausman test statistics of the null hypothesis that OLS estimates are consistent.[69] In each specification, the Hausman test statistic is statistically significant at the five-percent level, which favors rejecting the null hypothesis that OLS estimates are consistent.

The 2SLS estimator is requires that the exclusion variables are are correlated with duration but uncorrelated with post-displacement earnings (e.g., Greene, 2003). If they are not, then 2SLS estimates are no more consistent than OLS and we would be better off using the efficient estimator. First-stage estimates show that these variables are positively and statistically significant determinants of duration. In addition, regressions of residuals from Panel A estimations on exogenous variables and the excluded variables show no statistically significant association between these variables and the unexplained part of earnings.

The justification for use of 2SLS as an estimator relies on the large-sample properties of the IV estimator (e.g., Wooldridge, 2002, p. 101–102). If there is

We test for exogeneity in the second stage by including the instruments in the re-employment earnings equation. Results are presented in Table D.32. Column (1) reports estimates without the exclusion restrictions and column (2) reports estimates with. Both coefficient estimates are close to zero and statistically insignificant. Moreover, including the instruments has no effect on column (1) estimates with the exception of log spell duration. We take this as evidence that the effect of the instruments on post-displacement earnings works solely through their effect on spell duration. Thus, we are reasonably confident in the validity of these variables as instruments.

[69]The relevant test statistic is based on the chi-squared distribution with one degree of freedom. The critical value for this test is 3.841 at the five-percent level. Degrees of freedom are determined by taking the number of exogenous variables, or IVs, less the number of endogenous variables.

Table 2.20
Re-Employment Earnings Estimates, OLS and 2SLS: Displaced Workers Supplement, 1996–2010

	A. OLS Estimates			
	(1)	(2)	(3)	(4)
	LF Size	MSA	UA	Landsat7
Log weekly earnings on lost job	0.475***	0.476***	0.479***	0.473***
	(0.00937)	(0.00938)	(0.00936)	(0.00936)
Log usual weekly hours	0.820***	0.821***	0.821***	0.824***
	(0.0177)	(0.0177)	(0.0177)	(0.0176)
Log labor market scale	0.0334***	0.0400***	0.0887***	0.110***
	(0.00414)	(0.00551)	(0.0155)	(0.0120)
Log duration (weeks)	-0.0405***	-0.0409***	-0.0400***	-0.0408***
	(0.00327)	(0.00328)	(0.00328)	(0.00327)
Observations	6,188	6,188	6,188	6,188
R^2	0.705	0.704	0.703	0.706

	B. 2SLS Estimates			
	(1)	(2)	(3)	(4)
	LF Size	MSA	Urbanized Area	Landsat7
Log weekly earnings on lost job	0.475***	0.476***	0.479***	0.473***
	(0.00950)	(0.00951)	(0.00949)	(0.00950)
Log usual weekly hours	0.830***	0.831***	0.830***	0.833***
	(0.0178)	(0.0178)	(0.0178)	(0.0178)
Log labor market scale	0.0342***	0.0416***	0.0913***	0.113***
	(0.00419)	(0.00559)	(0.0157)	(0.0122)
Log duration (weeks)	-0.0575***	-0.0596***	-0.0569***	-0.0591***
	(0.00760)	(0.00764)	(0.00761)	(0.00760)
Observations	6,188	6,188	6,188	6,188
R^2	0.700	0.700	0.699	0.701

	C. Hausman Test Statistics			
Chi-squared statistic (d.f.= 1)	94.10	92.67	90.71	93.32

Notes: Dependent variable is the natural logarithm of post-displacement weekly earnings. 2SLS estimates use fitted values of log duration (UI receipt and log tenure on lost job used as exclusion restrictions). All regressions include controls for year, education, demographics, and major industry and occupation. A complete list of included demographic and education variables reported in Table 2.15. Year controls refer to year of survey. Homoskedastic standard errors in parentheses. Sample corresponds to the set of completed spells. Fitted durations use values predicted from Table D.33. Log variables are calculated using the natural logarithm. Usual weekly hours are based on current job at time of survey. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

sufficiently weak correlation between the IVs and the endogenous variable, the instruments are "weak" can cause inconsistency in small samples. Staiger and Stock (1997) show that a simple test of weak instruments involves looking at the F test statistics from the first stage. When F statistics are small, the asymptotic properties of the IV estimator break down. A simple rule is that F statistics should be greater than 10. First-stage F statistics are reported in Table D.33. Each is greater than 190 which is far in excess 10, suggesting that our IVs are strongly correlated with duration. Therefore, we are reasonably confident that our 2SLS estimates are consistent.

3.2.3 Differences by Gender, Race and Ethnicity, and Business Cycle

In this section we examine the duration and earnings effects of density by select demographic and business cycle characteristics. Unlike the previous chapter, DWS estimates make it possible to examine the differential effects of density on earnings, giving a more complete picture of the search outcomes faced by displaced workers. Results for duration and earnings are reported in Tables 2.21 and 2.22, respectively.

Hazard ratio estimates in Table 2.21 show less variation in the density effect on search duration between males and females compared to the CPS. As before, females search slightly longer than males in denser areas but both males and females have similar unemployment exit rates with respect to density.

There is a reversal in the density effect for married and unmarried workers in the DWS compared to the CPS. The density effect is slightly larger for married workers than unmarried workers although the difference is qualitatively nil. Married males tend to search for longer periods in denser areas than do married females, which is a reversal compared to CPS results. Consistent with previous results, unmarried males tend to exit unemployment at a higher rate in denser areas than do unmarried females.

Hispanics tend to exit unemployment at lower rates in denser areas than do non-Hispanics and this difference is much larger than that shown in the basic CPS.

As in the previous chapter, Hispanics tend to be clustered in a fewer number of cities, which pegs their fortunes to the labor demand conditions in those areas. Moreover, Hispanics may face similar spatial mismatch issues as blacks, which also show lower re-employment exit rates in denser areas than whites. Foreign-born Hispanics tend to have higher re-employment probabilities in denser areas than their U.S.-born counterparts. As before, we take this as evidence that informal networks may be important to this group.

Interestingly, re-employment outcomes for workers displaced in the pre-Great Recession sample have qualitatively similar (although still lower) re-employment probabilities in denser areas than the post-Great Recession sample. Much of this difference is likely due to fact that many of the unemployment spells during this period are not picked up by the DWS. The latest year of the DWS is 2010, which will include individuals displaced between 2007 and 2010. The incidence of long-term unemployment rose drastically over this period. However, many of these spells, although censored, will be relatively short by the 2010 survey date. We suspect that the density effect difference between these two periods will increase when the 2012 DWS is included.

Elasticities between density and re-employment earnings by demographic and business cycle are reported in Table 2.22. The density elasticity of re-employment earnings for females is nearly double that of males (0.158 and 0.08, respectively). Taken with the result that women search for longer periods on average than men, this suggests that women are trading longer spell durations for better re-employment match outcomes.

The density elasticity on re-employment earnings is negative and statistically insignificant for Hispanic workers. Non-Hispanic workers, on the other hand, have a positive elasticity of 0.133, which is similar in magnitude to the overall sample. The insignificant density elasticity for Hispanics appears to be driven by differences in place of birth. The estimated density-earnings elasticity is $-.138$ for foreign-

Table 2.21

Cox PH Coefficient Estimates of Landsat7 Density by Select Demographic Characteristics and the Business Cycle, Displaced Workers Supplement, 1996–February 2010

Gender	Coefficient	Std. Error	Observations	Clusters
Male	0.840***	(0.0400)	4,134	236
Female	0.830**	(0.0606)	3,297	224
Marital Status				
Married	0.840***	(0.0508)	3,282	230
Unmarried	0.831***	(0.0392)	4,149	234
Married, female	0.840*	(0.0747)	1,641	200
Married, male	0.815***	(0.0467)	2,508	216
Unmarried, female	0.819**	(0.0751)	1,656	200
Unmarried, male	0.861***	(0.0493)	1,626	207
Ethnicity and Race				
Hispanic	0.749**	(0.102)	1,013	118
Non-Hispanic	0.855***	(0.0363)	6,418	243
Hispanic, U.S. born	0.712*	(0.126)	449	93
Hispanic, foreign born (FB)	0.774**	(0.0963)	564	84
Non-Hispanic, black	0.748***	(0.0677)	905	136
Non-Hispanic, white†	0.879***	(0.0403)	5,212	239
Business Cycle‡				
Pre-Recession	0.825***	(0.0441)	5,064	228
Post-Recession	0.834**	(0.0666)	2,367	218

NOTES: Estimates reported as hazard ratios. Coefficient estimates based on log Landsat7 density measure (see Section 3.2). Sample includes completed and censored spells for MSA displaced workers. Each estimation includes demographic, education, and year and month controls. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Year controls refer to year of survey. A complete list of included demographic and education controls are reported in Table 2.15. Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate; hazard ratios greater than one indicate a speeding up of hazard.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

† This sample of whites also includes other races (excluding Asian or Pacific Islander, African American, or Native American. These will be excluded in a later draft of this work.

‡ Pre-recession refers to the January 1994–December 2007 sample, or the period prior to the "Great Recession." Post-recession refers to the period including the Great Recession and after. The Great Recession is officially recorded as the period December 2007 to June 2009 (for more information on recessions, see `http://www.nber.org/cycles.html`.

Table 2.22
2SLS Earnings Estimates of Landsat7 Density by Select Demographic Characteristics and the Business Cycle, Displaced Workers Supplement, 1996–2010

Gender	Coefficient	Std. Error	Observations	Clusters
Male	0.0814**	(0.0341)	0.674	231
Female	0.158***	(0.0183)	0.730	229
Ethnicity and Race				
Hispanic	-0.0123	(0.0349)	0.610	107
Non-Hispanic	0.133***	(0.0203)	0.713	246
Hispanic, U.S. born	0.157**	(0.0691)	0.693	82
Hispanic, foreign born (FB)	-0.138***	(0.0426)	0.628	81
Non-Hispanic, black	0.112***	(0.0319)	0.748	122
Non-Hispanic, white†	0.152***	(0.0220)	0.710	243
Marital Status				
Married	0.123***	(0.0286)	0.705	226
Unmarried	0.105***	(0.0264)	0.696	234
Married, female	0.156***	(0.0238)	0.739	198
Married, male	0.07**	(0.0337)	0.659	211
Unmarried, female	0.149***	(0.0239)	0.734	198
Unmarried, male	0.0963**	(0.0417)	0.692	191
Business Cycle‡				
Pre-Recession	0.11***	(0.0220)	0.711	227
Post-Recession	0.133***	(0.0430)	0.697	203

NOTES: Dependent variable is the natural logarithm of post-displacement weekly earnings. 2SLS estimates use fitted values of log duration (UI receipt and log tenure on lost job used as exclusion restrictions). All regressions include controls for year, education, demographics, and major industry and occupation. A complete list of included demographic and education variables reported in Table 2.15. Year controls refer to year of survey. Homoskedastic standard errors in parentheses. Sample corresponds to the set of completed spells. Fitted durations use values predicted from Table D.33. Log variables are calculated using the natural logarithm. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
† This sample of whites also includes other races (excluding Asian or Pacific Islander, African American, or Native American. These will be excluded in a later draft of this work.
‡ Pre-recession refers to the January 1994–December 2007 sample, or the period prior to the "Great Recession." Post-recession refers to the period including the Great Recession and after. The Great Recession is officially recorded as the period December 2007 to June 2009 (for more information on recessions, see `http://www.nber.org/cycles.html`.

born Hispanics compared to 0.157 for U.S.-born Hispanics. That foreign-born Hispanic workers search for relatively shorter periods *and* experience large re-employment earnings losses in denser areas than do their U.S.-born counterparts suggests that these workers face increasing competition from one another in the labor market. If these workers are relatively homogeneous in the types of jobs they can perform, then these workers will be less selective with respect to density; on the contrary, density will require that they lower their reservation wages in order to outbid (i.e., underbid) a competing offer. Further, foreign-born Hispanics are located in 81 of the 231 MSAs captured in this sample. This suggests that competition from other searchers is a primary determinant of unemployment outcomes for these workers. This looks to be a fruitful area for future research.

Black and white workers have positive density and re-employment earnings elasticities of 0.11 and 0.15, respectively. Although Cox PH model estimates show some evidence of spatial mismatch, the positive re-employment earnings elasticity suggest that blacks, like whites, increase their reservation wages in response to lower search costs.

Married and unmarried workers both show evidence of choosy search behavior with respect to density, and the male-female earnings elasticities persist across marital status.

Interestingly, the earnings-density elasticity is higher for the post-Great Recession sample than the pre-recession sample. We argued that the increase in the hazard ratio between the pre- and post-Great Recession samples suggests a decrease in the choosiness of workers given the relative abundance of workers to jobs. The earnings results, however, suggest that waiting is more profitable in the Great Recession period. More research is needed, but our initial reaction is that competition effects (that should lower earnings) are not an issue in the aggregate due to heterogeneity in worker and job types.

4 Summary

This chapter extended the analysis from Chapter I to the Displaced Workers Supplement to the CPS. The DWS reports detailed information regarding a worker's spell duration and their pre- and post-displacement earnings. This allows for the estimation of duration and re-employment earnings equations in order to control for workers' simultaneous choice of expected spell duration and reservation wage.

We began by estimating continuous- and discrete-time proportional hazard models for the DWS data. Our results are consistent with the basic CPS estimates that re-employment probabilities are negatively related to MSA density. As before, the negative effect of density is strengthened when it is measured with greater precision.

We then used a 2SLS modeling framework to estimate the marginal effect of density on re-employment earnings conditional on unobserved heterogeneity (proxied by pre-displacement earnings), observable characteristics, and spell duration. Fitted values of a first-stage (OLS) duration equation were included in the second-stage earnings equation to account for the simultaneous choice of reservation wage and spell duration. Parameter estimates indicate an elasticity between labor market density and re-employment earnings of 0.113. Taken with PH model estimates, these results indicate that workers react to cost savings in search by adopting selective search strategies; that is, workers are willing to trade longer durations in denser areas for higher post-displacement earnings. This result supports the view that density reduces spatial frictions to search and thereby increases search efficiency.

Our results are robust to differences in gender, race and ethnicity, and the business cycle. One notable exception, however, is the foreign-born Hispanic subsample. These workers search for longer periods in denser areas but they earn vastly less in denser areas than other subsamples of workers (e.g., whites, blacks, males, females). In particular, foreign-born Hispanic workers have an estimated elasticity between density and re-employment earnings of -0.133. In addition, foreign-born Hispanics search on average for fewer periods than Hispanics born in the United States (hazard ratios of 0.77 and 0.72, respectively). These results, taken with the fact that foreign-born Hispanics are observed in only 84 of the 236 MSAs included in our DWS sample, suggest that these workers face strict competition for jobs from other foreign-born Hispanics due to a relatively narrow set of jobs for which they are close substitutes. Future research is needed to explore this phenomenon more closely.

We also find potential evidence for spatial mismatch in urban search markets. Although racial differences suggest that black and white workers adopt choosy search strategies in denser areas, black displaced workers face additional barrier to employment that lengthens their average spell durations. For example, the hazard ratio with respect to density is 0.75 for blacks and 0.88 for whites, while the respective elasticities between density and re-employment earnings are 0.112 and 0.152. Undoubtedly, some of this difference is likely to be explained by differences in education, occupation, and industry. But to the extent that black workers are concentrated in dense urban cores far away from jobs, a portion of the decreased hazard ratio with respect to density is likely due to differences in job accessibility through racial sorting. This remains an important area for future research.

A criticism of the DWS sample is that it comprises the least-productive workers. If firms have some discretion over whom to let go then it is in their best interests to let go the least-productive workers in the firm. Therefore, our estimated negative relationship between market scale and the re-employment probability is just picking up the inability of less-productive workers, or lemons, to find employment. Gibbons and Katz (1991) argue that the "lemons" effect (i.e., longer spell durations, lower post-displacement earnings) should show up in workers who are laid off due to the abolition of their position or insufficient work, but not those displaced from a plant closing.[70]. Tables D.30 and D.31 present Cox PH and 2SLS estimates by type of displacement. In each case, density main-

[70] The assumption is that a plant closing affects all workers, not just those who are terminated at the firm's discretion

tains a robust negative relationship with the re-employment hazard. Interestingly, "lemons" show a higher return to density than those on plant closings, but also a higher re-employment wage penalty associated with duration.

In the next chapter, we extend the analysis to control for variation in local industry and occupation employment, local industry demand, and industry and occupation change.

Chapter III

INDUSTRY AND OCCUPATION CHANGE, JOB TASKS, AND LOCAL MARKET CONDITIONS

1 Introduction

An issue that has yet to be covered is industry and occupation change. Tables 1.10 and 2.15 reveal that displaced workers show a high degree of industry and occupation mobility following displacement. Nearly three-quarters of all displaced workers were re-employed in a different detailed industry or occupation upon re-employment and nearly two-thirds changed both. A similar pattern holds in the DWS with just over two-thirds changing detailed industry or occupation and over half changing both.

This chapter introduces additional controls for industry and occupation change. The objective is not to model industry and occupational mobility in an urban area as it relates to duration or post-displacement earnings. Rather, we simply wish to control for additional variation that may explain our observed relationship between duration, earnings, and density.

Displacement may be very costly to workers who have accumulated high levels of human capital that are specific to a firm or a particular industry that cannot be transferred to other firms in the same or different industries (e.g., Kletzer, 1998). Although there is some debate in the literature as to whether returns to tenure are industry- or firm specific (e.g., Kletzer, 1996), displaced workers suffer large wage losses as a result of industry or occupational mobility (Neal, 1995; Carrington, 1993). As a result, high-tenure workers may adopt search strategies favoring firms in the same industry as the one they were displaced in order to capture more of their previous accumulated human capital to their next job (Thomas, 1996, 1998).

The extent of mismatch between a worker's displacement and post-displacement job is likely to be closely related to the tasks performed on each job. Workers are arguably able to transfer a greater amount of their pre-displacement tenure to the new job if the tasks requires of each job are similar. Occupations can be thought of as a bundle of indivisible activities that must be performed jointly to produce work (Autor and Handel, 2009). However, there is a great deal of variation in the tasks performed on different occupations. Therefore, an occupation change in itself may not be costly if the tasks performed on the job are similarly close. Using quantitative data on job tasks, we estimate the extent of earnings losses associated with displacement by comparing the quantitative difference in the tasks performed between the old job and new.[71]

Fallick (1993) and Carrington (1993) show that changes in industrial conditions influence the search behavior and unemployment outcomes for displaced workers. In particular, workers displaced from declining industries are more likely to search for and find employment in other industries while workers displaced from growing industries are more likely to seek out and find employment in the same industry their old job. We introduce local industry establishment counts to the Cox PH models for the basic CPS data.

Teulings and Gautier (2004) demonstrate that search costs are closely related to the substitutability of workers across jobs in the urban area. Search costs are higher when workers are less substitutable, therefore very specialized labor (and firms) has an incentive to locate in denser areas where search costs are lower. In a labor market characterized by worker-firm heterogeneity, it is unlikely that all workers will be suitable for employment in all jobs. Therefore, the "relevant" scale of the labor market is market density weighted by the share of jobs s for which a

[71] From the perspective of an unemployed job seeker, the local density of jobs in the urban area that require task inputs similar to the worker's skill endowment is the most important factor in determining unemployment outcomes. While this is not studied in this work, we hope that these results point to a new direction of research moving forward.

worker is substitutable, or ρs. If workers are more closely substituted across jobs in the same two-digit occupation group, then the share of occupations in the urban area may serve as a useful measure of s. We estimate a series of Cox PH models that incorporate local occupation shares as measures of the relevant density of the local market.

The rest of this chapter is organized as follows. In Section 2 we introduce the sources of industry and occupation data. Occupational task data are presented and we demonstrate how we define the quantitative difference between tasks. Section 3 presents 2SLS and Cox PH estimation results using these data.

2 Industry and Occupation Data

In this section we briefly describe the source of industry and occupation data used to measure the relevant scale of a local labor market as well as changes in local differences in industry labor demand.

2.1 Industry Employment

Industry data come from the *County Business Patterns* (CBP). CBP data are collected by the U.S. Census Bureau as part of the "Business Register," which is a record of all establishments in the U.S. (and its territories) with paid employees. We collect data at the county level which are then used to aggregate up to the MSAs.[72] CBP data report both the number of employees and number of establishments at each county. Public-use CBP data are available on an annual basis from 1986 to 2009. Data prior to 1998 are based on the Standard Industrial Classification system (SIC), while data from 1999 on is based on the North American Industrial Classification System (NAICS). We restrict our counts to the two-digit level. Note also that CBP omits the public sector.

Some industry employment counts are suppressed due to confidentiality restrictions. However, the CBP reports the number of establishments in discrete size

[72]Counties are matched to MSAs using state-county FIPS codes.

groups (e.g., the number of establishment with 10–19 employees) which makes it possible to estimate local industry employment counts. We use the median of each discrete establishment-size category multiplied by the number of establishments in that size category to get an estimate of total industrial employment. Further, employment counts are topcoded at 5,000 employees. Since no information is available on the distribution of top-coded employment counts for each location, we simply set the employment level of these firms to 5,000.

No attempt is made to convert SIC establishment and employment counts to NAICS codes. We simply assign the SIC-based codes to the CPS for the years 1994–1998 and NAICS-based counts for the years 2003–2009. Therefore, the sample cover 1999–2002 and 2010–2012 are excluded.

2.2 Occupational Employment

Occupational employment counts are taken from the BLS' *Occupational Employment Statistics* (OES). OES data are reported at the MSA level according to the Standard Occupational Classification (SOC) system on an annual basis for 1997–2011 (they are typically collected in May but are also available for November in 2003 and 2004). The 1997–1999 samples use the 1990 SOC while the 2000–2011 samples are based on the 2000, 2002, and 2010 SOC systems as they are updated. Because there is no clean way to link the pre-2000 SOC codes to the current ones, we only match two- and three-digit SOC data for the CPS sample covering 2003–2011.

2.3 O*NET Task Data

This section describes the method used to compare the similarity of occupations based on the tasks that are performed on the job. Occupational task ratings are taken from the Occupational Information Network, or O*NET. I use a simple distance measure to determine the relative proximity of any two jobs which can then be related to individual job movements.

O*NET is the successor to the Department of Labor's *Dictionary of Occupational Titles* (DOT). Professional job analysts and incumbent interviews determine quantitative attribute ratings for over 900 detailed Standard Occupational Classification (SOC) occupations. The O*NET "content model" provides ratings that are intended to capture worker-oriented, job-oriented, cross-occupation, and occupation-specific features of jobs. This paper uses O*NET version 14, which was released in June 2009.

In order to use O*NET ratings with the Current Population Survey (and its supplements such as the DWS), I implement a fairly simple algorithm based on the Standard Occupational Classification (SOC) structure that assigns O*NET occupation ratings to CPS occupations. O*NET reports attribute data for over 900 six- to eight-digit occupations. The CPS uses the Census occupation classification system, which and identifies a set of 502 occupation codes, typically up to the third or fourth digit of the SOC.[73] For cases where more than one O*NET occupation could be assigned to a single CPS occupation, attribute ratings are distributed to CPS occupations using 2009 occupation employment levels provided by the BLS *Occupational Employment Statistics* (OES).[74]

One difficulty in analyzing occupational characteristics within the CPS over time is that the SOC undergoes frequent updates, making it impossible to make direct comparisons over time.[75] O*NET version 14 is based on the 2002 SOC system which applies to CPS monthly surveys from January 2003 to the present. In order to apply O*NET ratings to CPS occupations prior to 2003 and therefore take advantage of our full sample going back to 1994, probabilistic matching is used to link O*NET 14 ratings to legacy occupation codes. Probability weights

[73] For more information on the BLS SOC hierarchy, visit `http://www.bls.gov/SOC/`.

[74] The full description and data files used to implement the O*NET-to-CPS matching algorithm are available from the author by request.

[75] The SOC's most recent update occurred in 2010 and fully implemented into the CPS in January 2011. Prior to that, CPS occupation codes were based on the 2002 SOC scheme which was implemented in January 2003, and before that it used the 1990 SOC structure.

between Census 1990 and 2000 are based on employment ratios which are available from IPUMS.[76]

To the extent that the job tasks for occupations changed over time, current O*NET attributes will not accurately represent the set of tasks performed in the same occupation in the past. This may be an issue as job analysts are instructed by the DOL to analyze jobs as they are at the time, not as an analyst feels they ought to be or believe them to be (U.S. Department of Labor, Employment and Training Administration, 1991).

It's possible to match 185 individual O*NET attribute ratings to 485 CPS occupations in the post-2003 sample and 493 in the pre-2003 sample. Additional occupation codes are matched to the pre-2003 sample because the probabilistic matching weights may assign many post-2003 occupation counts to a few pre-2003 occupation codes. This will attribute a greater number of between CPS file matches despite there being a limited number of occupation codes being matched using the O*NET-CPS matching algorithm.

I use Euclidean distance to measure the degree of dissimilarity between any two occupations. This distance measure is simple to implement and takes into account the multiple dimensional nature of occupations. Due to different measurement scales of particular task ratings, however, it's necessary to normalize the set of occupation tasks.

Dissimilarities are sensitive to attribute measurement scales (Kaufman and Rousseeuw, 1990, p. 6). Any differences in the measurement scales of different attributes can affect the relative weight of a particular occupation attribute on the magnitude of Euclidean distances. O*NET attributes are measured according to ordinal scales but the range of those scales may differ greatly. For example, the "level" scale is measured in integers over the $[0, 1, \ldots, 7]$ interval but "context" variables are integers measured over the $[0, 1, \ldots, 5]$ interval. It's easy to see that

[76]Source: `http://usa.ipums.org/usa/resources/chapter4/occ_90-00.xls`.

taking raw differences between attributes will apply greater weight to occupational differences in variables measured by "levels" than those measured as "context."

For ordinal data measured on different scales, it is sensible to convert attribute values to their ranks and applying the following transformation Kaufman and Rousseeuw (1990, p. 30),

$$z_{i,f} = \frac{r_{i,f} - 1}{M_f - 1}, \tag{27}$$

where $r_{i,f}$ is the rank of the ith occupation in the fth attribute and M_f is the highest rank of the fth attribute. Converting variables to ranks preserves the ordinal nature of the data while reducing the influence of different measurement scales on the calculated dissimilarity.[77] By construction, $z_{i,f} \in [0, 1]$.

Occupational differences are organized into a dissimilarity matrix, $\mathbf{A}$, which is a $F \times F$ array of pairwise occupational attribute differences. Each element of $\mathbf{A}$, $\alpha(i, j)$, is calculated as the squared Euclidean distance between the set of occupational attributes for each occupation pair, or

$$\alpha(i, j) = \sum_{f=1}^{F} (z_{i,f} - z_{j,f})^2, \tag{28}$$

where $z_{i,f}$ is defined in equation 27. From equation 28, the estimated match quality between occupation i and j, is calculated as the absolute deviation between any pair of occupations, or $\hat{\alpha}(i, j) = \sqrt{\alpha(i, j)}$.

Figure 6 presents histograms of the Euclidean distance metric for occupational tasks. Panel A corresponds to O*NET 14 data applied to pre-2003 CPS data. In all, there are 1,770 pairwise differences.[78] The average task difference is 4.61 units with a standard deviation of 1.20. The median distance is 4.67 and the interquartile range is 1.82.

Panel B corresponds to O*NET 14 data applied to the post-2003 set of OES occupations. There are 1,942 occupation pairs with a mean task difference of

[77] Ranks are assigned in a similar fashion as athletic rankings where the highest ranked team is assigned a value of 1. Ties are assigned the same rank.

[78] Duplicates are not counted. This consists of unique pairings in the upper or lower diagonal of matrix $\mathbf{A}$.

4.77 and standard deviation 1.15. The median task difference is 4.78 with an interquartile range. In both data sets, task differences are closely normally distributed with a little less smoothness in panel A due to probabilistically matching current occupation codes to those in the older CPS surveys.

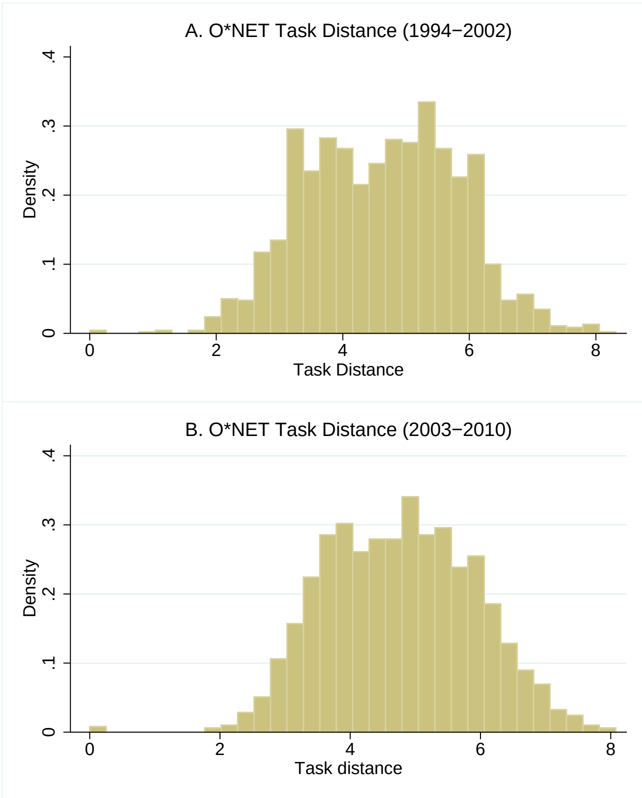

Figure 6
Histogram of O*NET Euclidean Task Distances, Pre- and Post-2002 Samples

3 Estimation and Results

This section presents results from 2SLS earnings equations and Cox PH models after controlling for industry and occupation change and local area industry labor demand. We begin by examining post-displacement earnings as estimated in the previous chapter. We then introduce measures of industry establishment growth and "relevant" density in Cox PH models.

Table 3.23 presents 2SLS estimates of post-displacement earnings of the full sample including controls for industry and occupation change. Industry and occupation change is measured using a binary indicator of whether or not the worker changed detailed industry or occupation. The coefficient estimates on industry- and occupation-change dummies are $-.058$ and $-.082$, respectively. That is, workers who find employment in a different industry or occupation on average face earnings losses of nearly 6 and 8 percent, respectively. The estimated earnings-density elasticity is robust to the inclusion of these variables.

The magnitude of the occupation-change coefficient is over 50 percent higher than the coefficient on industry change. This suggests that much of the earnings loss following displacement may be due to a greater deal of mismatch between the skills of the worker and the tasks actually performed on the job. We estimate this effect by including a measure of the absolute difference in job tasks between the worker's old job and their re-employment job. These results are reported in Table 3.24.

The estimated coefficients on industry change and labor market scale are robust, even though fewer observations are included.[79] The estimated coefficient on occupation change remains negative but is not statistically significant after controlling for job-task differences. Job-task differences show a strong and statistically significant negative relationship with earnings. A one standard deviation increase in the task distance between the pre- and post-displacement job is associated with a 2-percent decrease in re-employment earnings.[80] The average task change in the sample is 2.17, which is associated with a 3.8-percent reduction in re-employment earnings. For those who do not find re-employment in the same occupation, the average task distance betwee pre- and post-displacement job is 3.66 which is associated with a 6.5-percent reduction in average re-employment earnings, which is nearly two-thirds of the estimated re-employment wage impact of an occupational change in Table 3.23. Therefore, much of the estimated wage penalty associated with the occupational change following displacement are

[79]Recall from Section 2.3 that task data are not available for the full set CPS or DWS occupations. In addition, the change-in-task measure requires that task information be available for both the displacement and re-employment jobs.

[80]This assumes a standard deviation of 1.15 as described in Section 2.3.

Table 3.23
2SLS Post-Displacement Earnings Estimates, Industry and Occupation Change, Displaced Workers Supplement 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log earnings on lost job	0.466***	0.467***	0.471***	0.465***
	(0.0157)	(0.0155)	(0.0155)	(0.0156)
Log usual weekly hours worked	0.821***	0.822***	0.821***	0.824***
	(0.0301)	(0.0300)	(0.0294)	(0.0301)
Log labor market scale	0.0329***	0.0398***	0.0863**	0.108***
	(0.00690)	(0.0115)	(0.0334)	(0.0250)
Log duration (weeks)	-0.0494***	-0.0514***	-0.0488***	-0.0510***
	(0.00893)	(0.00929)	(0.00883)	(0.00920)
Changed detailed industry	-0.0587***	-0.0581***	-0.0596***	-0.0585***
	(0.0137)	(0.0138)	(0.0139)	(0.0138)
Changed detailed occupation	-0.0829***	-0.0836***	-0.0827***	-0.0816***
	(0.0149)	(0.0147)	(0.0146)	(0.0147)
Constant	0.191	0.416**	-0.0172	-0.137
	(0.188)	(0.175)	(0.286)	(0.217)
Year controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Education controls	Yes	Yes	Yes	Yes
Industry and occupation controls	Yes	Yes	Yes	Yes
Number of observations	6,188	6,188	6,188	6,188
R^2	0.706	0.705	0.704	0.707
Number of clusters	247	247	247	247

NOTES: Dependent variable is the natural logarithm of post-displacement weekly earnings. 2SLS estimates use fitted values of log duration (UI recipient dummy and log tenure on lost job used as exclusion restrictions). Usual weekly hours refer to current job. All regressions include controls for year, education, demographics, and major industry and occupation. Cluster-robust standard errors in parentheses (clustered by MSA). Sample corresponds to the set of completed spells. Fitted durations use values predicted from Table D.33. Log variables are calculated using the natural logarithm. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 3.24
2SLS Post-Displacement Earnings Estimates with Industry, Occupation, and Job Task Changes, Displaced Workers Supplement 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log earnings on lost job	0.459***	0.461***	0.463***	0.458***
	(0.0165)	(0.0164)	(0.0163)	(0.0165)
Log usual weekly hours worked	0.813***	0.813***	0.813***	0.816***
	(0.0307)	(0.0306)	(0.0301)	(0.0309)
Log labor market scale	0.0319***	0.0388***	0.0864***	0.107***
	(0.00639)	(0.0108)	(0.0320)	(0.0238)
Log duration (weeks)	-0.0470***	-0.0489***	-0.0466***	-0.0487***
	(0.00970)	(0.0100)	(0.00960)	(0.0100)
Changed detailed industry	-0.0543***	-0.0537***	-0.0551***	-0.0543***
	(0.0141)	(0.0141)	(0.0141)	(0.0141)
Changed detailed occupation	-0.0232	-0.0259	-0.0225	-0.0220
	(0.0257)	(0.0257)	(0.0259)	(0.0258)
Task distance (absolute value)	-0.0177***	-0.0172***	-0.0178***	-0.0176***
	(0.00577)	(0.00575)	(0.00579)	(0.00580)
Constant	0.267	0.485***	0.0464	-0.0643
	(0.181)	(0.170)	(0.276)	(0.212)
Year controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Education controls	Yes	Yes	Yes	Yes
Industry and occupation controls	Yes	Yes	Yes	Yes
Observations	5,795	5,795	5,795	5,795
R^2	0.705	0.704	0.704	0.706
Number of clusters	247	247	247	247

Notes: Dependent variable is the natural logarithm of post-displacement weekly earnings. Estimates use fitted values for log duration based on Table D.33 (UI recipient dummy and log tenure on lost job used as exclusion restrictions). Usual weekly hours refer to current job. All regressions include controls for year, education, demographics, and major industry and occupation. Cluster-robust standard errors in parentheses (clustered by MSA). Sample corresponds to the set of completed spells. Log variables are calculated using the natural logarithm. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 3.25
Cox PH Hazard Ratio Estimates, Industry Establishment Growth, Current Population Survey 2003–2009

	(1) LF Size	(2) MSA	(3) Urbanized Area	(4) Landsat7
Log mean labor market	0.956***	0.926***	0.953	0.876***
	(0.0132)	(0.0166)	(0.0505)	(0.0333)
Industry establishment growth	2.117***	1.855**	2.064***	2.075***
	(0.582)	(0.486)	(0.566)	(0.569)
Year and month controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Education controls	Yes	Yes	Yes	Yes
Observations	7,009	7,009	7,009	7,009
Log-likelihood	-55,254.48	-55,245.93	-55,264.31	-55,254.46
Number of clusters	252	252	252	252

NOTES: Estimates reported as hazard ratios. Columns (1)-(4) represent various measures of labor market scale; columns (2)-(4) are density measures described in Section 3.2. Establishment employment based on re-employment industry. Log variables correspond to the natural logarithm. Industry establishment counts based on two-digit NAICS industries, and growth rate refers to the relative growth from the previous year to the survey year. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Time controls refer to year and month of unemployment exit or censoring date. A complete list of included demographic and education controls are reported in Table 1.10.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

attributed to the difference in tasks performed on the pre- and post-displacement job.

Table 3.25 presents Cox PH model estimates incorporating controls for the annual MSA growth rate of establishments in the worker's post-displacement industry. Industry establishment growth rates control for industry-specific differences in labor demand within the local labor market. Industry establishment growth rates are much larger than one and statistically significant in all specifications, suggesting that displaced workers are much more likely to find employment than the baseline hazard in industries that are growing fast. With the exception of the UA-based density measure, coefficient estimates of market scale are less than one. Therefore, the positive relationship between market scale and duration is robust to changes in local labor demand that may occur across industries.

Table 3.26 presents Cox PH estimates for the CPS incorporating controls for local two-digit industry employment growth in the worker's pre-displacement industry, the local share of industry employment in the worker's pre-displacement occupation, the annual change in occupation employment in the worker's pre-displacement three-digit occupation, and the location quotient which measures the relative concentration of a worker's three-digit pre-displacement industry. The first column presents estimates for the full sample, which includes censored and uncensored spells for the period 2003–2010. The second column reports estimates for the set of completed spells only.

Labor market density effect is robust in magnitude but it is significant at the 10-percent level. Annual industry establishment growth has a strong positive effect on the rate of re-employment. A one-percent increase in the number of establishments in a worker's pre-displacement industry increases the re-employment hazard by a factor of 10. This suggests that a worker's re-employment hazard is hugely dependent on local labor demand conditions.[81]. Occupation share and employment growth show a positive relationship with the re-employment hazard but are not significantly different from zero.

The second column presents estimates for the sample of completed spells. As before, density is robust and significantly different from zero: workers in denser areas search for longer periods. Industry establishment growth is positive and significant at the 1-percent level, but the hazard ratio is 3.8. In addition, a one-percent increase in the local share of employment in a worker's displacement occupation increases the probability of exiting unemployment by a factor of 4. This supports the view that search frictions are reduced in labor markets where workers are relatively substitutable across jobs. In addition, a one-percent increase in own-occupation employment over the previous year increase the hazard rate by 12.8 percent. Finally, a one-unit change in the relative concentration of a worker's pre-displacement industry tends to lower the re-employment hazard by roughly 5 percent, although this coefficient estimate is significant at the 10-percent level.

[81]Fallick (1993) shows that employment growth in a worker's own industry increase the likelihood that the worker will find work in the same industry. Future work with these data should explore the propensity of workers to move across industries in response to changes in *local* employment conditions across industries

These results indicate that local labor market labor demand conditions are important in determining search outcomes. Future research needs to address these issues in the context of within-market industry and occupation mobility.

Table 3.26
Cox PH Hazard Ratio Estimates with Industry, Occupation, and Job Task Changes, Current Populatin Survey, 2003–2010

	Full Sample	Completed Spell
Log labor market density (Landsat7)	0.806*	0.748***
	(0.0891)	(0.0725)
Industry establishment growth[a]	10.07***	3.808***
	(3.388)	(1.288)
Share of three-digit occupational[b] employment	3.060	3.952*
	(2.360)	(3.005)
Three-digit occupational[b] employment growth	1.120	1.128*
	(0.0883)	(0.0699)
Location quotient[c]	0.957	0.949*
	(0.0262)	(0.0270)
Demographic controls	Yes	Yes
Education controls	Yes	Yes
Year and month controls	Yes	Yes
Observations	19,057	6,928
Log-likelihood	-62,725.63	-54,520.85
Number of clusters	255	249

Notes: Estimates reported as hazard ratios. Columns (1) and (2) present estimates for the full sample and sample of completed spells, respectively. Sample covers the 2003–2010 period. Establishment employment based on re-employment industry. Log variables correspond to the natural logarithm. Industry establishment counts based on two-digit NAICS industries. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Time controls refer to year and month of unemployment exit or censoring date. A complete list of included demographic and education controls are reported in Table 1.10.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$
[a] Annual growth rate on two-digit NAICS industry at the year of the survey.
[b] Based on three-digit 2003 OES occupation code.
[c] Based on three-digit NAICS industry code.

Chapter IV

CONCLUSION

The purpose of this study was to investigate the relationship between labor market scale and the mean duration of unemployment. The key assumption is that the scale of the market increases the rate of contact between unemployed workers and firms holding vacancies by decreasing the average distance between potential match partners. In this regard, labor market density is the appropriate measure of scale. We contend that density lowers the costs of search which induces an endogenous response of workers to adopt more selective, or "choosier," search strategies where a longer search duration is exchanged for a better wage offer that comes with continued search. Therefore, increasing returns to scale in afford workers the opportunity to find higher-paying jobs following a displacement.

We began our argument by introducing a highly stylized but simple model of an individual worker's decision to look for a job. The worker's optimal search strategy is to choose a reservation wage which, given a fixed wage distribution, also chooses an expected search duration. We show that an increase in the job-offer arrival (or contact) rate associated with a decrease in the average distance between potential match partners has two offsetting effects on the expected duration of unemployment: (1) a negative effect by increasing the likelihood that any given contact will be result in an acceptable wage offer and (2) a positive effect from workers raising their reservation wages as a result of a decrease in the relative cost of search. We show that the duration-density relationship is ultimately determined by which effect dominates. More importantly, the comparative statics of the job search model helped us understand how to interpret the empirical relationship between scale and duration.

We then collected data on individual unemployment spells over a large number of spatially distinct U.S. MSAs from the CPS. We constructed detailed measures of labor market density based on the size of the labor force per unit of land area occupied by the MSA, using an innovative measure of MSA land area that resolves an issue of relying on political boundaries to define the spatial extent of an urban labor market. We show that using remotely sensed data can be used to get very precise measurements of the urban area footprint and give a brief overview of the measurement error associated with using traditional measures. Sample summary statistics showed a robust positive relationship between labor force density and completed spell duration, a relationship that was not apparent by looking at market size alone. On the first look, then, space showed itself to be an important source of friction in the search process.

We then estimated Cox PH models in order to simultaneously control for individual demographic and educational characteristics that may be associated with duration and market scale. We found that labor market scale is negatively related to the hazard rate of exiting unemployment at any time. More importantly, this effect is strengthened when we explicitly control for the spatial organization of employment in the urban area, and it is strengthened further when area is measured with greater precision.

Based on the predictions of the theoretical model, the observed positive and robust relationship between duration and density suggest that workers in denser areas adopt choosier search strategies because the expected earnings from continued search exceed the costs due to search efficiency gains. But if workers in dense areas are more selective over the jobs, then they ought to be observed earning higher wages upon re-employment conditional on their search duration. To test this, we introduce data from the DWS, a supplement to the CPS that reports additional information relevant to a worker's unemployment spell (such as

UI receipt and previous job tenure) as well as earnings information. We estimate post-displacement earnings equations that control for duration, market density, individual unobserved heterogeneity, current hours, and a host of demographic, education, and industry and occupation controls. We use 2SLS to control for simultaneity between duration and earnings. In each specification, density is positively associated with earnings upon re-employment. More importantly, when density is measured with greater precision the marginal effect of density on earnings vastly outstrips any effect of duration, suggesting that workers in denser market earn higher wages following displacement.

We then introduce additional robustness checks associated with earnings losses and spell durations. Displaced workers are more likely to experience large and persistent earnings losses. One explanation is that displaced workers give up their accumulated firm- or industry-specific human capital. We show that most displaced workers who are observed to find employment do so in an industry or occupation from their old job. Even after controlling for industry and occupation change, density is strongly and positively related to post-displacement earnings.

Estimated post-displacement earnings equations shows that occupational change is associated with higher wage penalties than industry change. Occupation change may be a better measure of lost accumulated human capital because the tasks performed on the post-displacement job may be different, perhaps very different, from that of the displacement job. We collected detailed job-task data from O*NET and calculated quantitative measures of dissimilarity between a worker's old and new job. We show that the majority of earnings losses associated with occupation change are due to changes in the tasks performed on the job. While we offer no real interpretation other than that, we expect this to be a fruitful area of future research. For example, associating task measures to unemployed workers and open vacancies can be used to identify the level of mismatch existing in the labor market, or perhaps explicit measures of structural employment. From the perspective of agglomeration economies, it would be very useful to measure the degree to which workers are substituted across jobs in the urban area and firms within and between industries.

Finally, we introduce measures of growth rates in local industry establishment growth to control for differences in local labor demand in the CPS. We find that workers are more likely to exit unemployment at any time if they are re-employed in industries that are increasing the number of establishments. More importantly, the positive association between density and duration remains robust.

This work has important implications for how economists should view market scale in urban search markets. First, more work needs to be done to explain the observed negative relationship between city size and the unemployment rate. The unemployment is negatively related the unemployment-to-employment exit rate. Since the unemployment rate is dependent on not only UE flows but EU and other flows, it makes sense to investigate this further. In addition, we show that displaced workers and quits both experience longer unemployment spells in denser markets. These two sources of unemployment represent the largest amount of unemployment at any given time and are what economists typically regard as healthy labor market churn. Therefore, if unemployment rates are negatively related to market size, it must be either due to their greater use of temporary layoffs or a more modest incidence of job loss; that is, fewer EU flows.

In addition, the negative relationship between industrial diversity and unemployment rates need to be re-examined. Studies that rely on the aggregate relationship between market characteristics, such as the dispersion of industry in a locale, cannot accurately capture the micro incentives for search in a given industry. Displaced workers who accept re-employment in an industry or occupation different from their old one face significant wage losses. These wage losses reduce the incentive to search in different industries (not counting for differences in employment opportunities across industries) and may lead to longer spell durations.

Economists could also benefit by looking at alternative measures of a local labor market. The MSA definition is a useful and indispensable tool, but researchers need to be aware of how those definitions affect density measures. One limitation of the Landsat7 data used here is that they not collected regularly. However, they can show both increases in the footprint as well as additional infill development that may occur in a city. Future research should try to incorporate direct structural density measures using these data. Further, they are adaptable to any other form of two-dimensional urban boundary measure, such as Public-Use Micro Areas (PUMAs) or municipal boundaries.

Finally, we identify at least two major challenges to our conclusion that the positive duration (negative re-employment hazard) and positive earnings relationships with market scale are due to search efficiencies in denser markets. First, our pattern of results may be consistent with the view that workers in larger or denser markets have higher levels of non-wage (or leisure) income. Job search theory predicts that search durations increase with leisure income through a similar reservation wage effect predicted by the job offer arrival rate: reservation wages increase with non-wage income, reflecting the fact that a higher employment income is required to compensate the unemployed worker for foregone non-wage earnings net search costs, which would reduce the probability that any given wage offer is acceptable and increasing expected re-employment earnings.

The exact source of leisure income in denser areas is not clear a priori. For example, larger cities may have more generous UI benefits (typically considered part of non-wage income). However, our DWS sample shows that only 40 percent of those re-employed at the survey date received UI benefits and of those, only 33 percent exhausted them (the incidence of UI receipt is much higher for censored spells). Therefore, important levels of non-wage income may come from other sources. For example, urban residents may be have productive marriage matches where the employed spouse may offset the costs of prolonged search. In addition, urban residents may be more likely to share housing (especially younger workers) where the rent costs are shared among residents. Since housing rents are higher in urban areas, unemployed workers may be able to defer some liquidity constraints associated with housing costs if they can temporarily pass those costs onto employed roommates. One difficulty with this explanation, however, is that the average age of our DWS sample is about 40 years old and are less likely to live in such arrangements. Nonetheless, identifying potential sources of leisure income is an important area for future research.

Second, our results may be consistent with models of endogenous search effort (e.g., Mortensen, 1986). Search effort models predict that workers can influence their job offer arrival rates if they search more intensively (in fact, the technique for modeling endogenous search effort involves a similar parametrization of the job-offer arrival rate used here). The basic prediction is that the job-offer arrival rate increases with effort which tends to lower expected spell durations. If one takes the view that permanently laid off (or displaced) workers represent the least-productive workers of the firm and if this low level of productivity carries over into search behavior, then our results may be picking up the effects of a non-random sample of low-effort searchers.

However, this fact is more difficult to reconcile with observed higher post-displacement earnings. Mortensen (1986) demonstrates that earnings increase with an increase in mean wage offers. Since earnings increase with density, then re-employment earnings should increase in denser markets. But we observe that earnings increase with density after controlling for earnings on the displacement job which should be picking up area-specific earnings differences. It is possible, of course, that workers are finding employment in industries or occupations with higher average earnings relative to their displacement job, but this is difficult to reconcile with the observation that industry and occupation change is associate with strong earnings losses. Further, the concept of search effort and search efficiency are difficult to disentangle. In effect, density lowers the costs of search for any level effort in the way it is modeled here. Further research in isolating one effect from the other would be useful.

Appendix A

UNEMPLOYMENT RATE AND LABOR MARKET DENSITY

Table A.27
Least-Squares Dummy Variable Estimates of Labor Market Size and MSA Unemployment Rates, January 1990–March 2012

	Coefficient Estimates
Log labor force size	-0.0108***
	(0.000837)
Constant	-1.934***
	(0.115)
MSA fixed effects	Yes
Year-Month fixed effects	Yes
Number of observations	69,687
R^2	0.798

NOTES: Data taken from the Bureau of Labor Statistics *Local Area Unemployment Statistics*. Dependent variable is MSA unemployment rate (relative frequency). Specification includes controls for MSA and year-month. Log variables are scaled according to the natural logarithm. Estimates are include 261 MSAs in the continental U.S. over a period of 267 consecutive months.

Appendix B

RESERVATION WAGE COMPARATIVE STATICS

We can determine the comparative statics properties of reservation wages by setting equation (10) equal to zero

$$\Phi(w^*, b, c, \lambda, \rho, r, q) = w^* - (b - c) - \frac{\lambda(\rho)}{r + q} \int_{w^*}^{\infty} [1 - F(w)] \, dw = 0. \qquad \text{(B.29)}$$

Equation (B.29) implicitly defines the relationship between the reservation wage (i.e., the endogenous variable) as a function of all exogenous variables. The effect of labor market density on reservation wages is shown by applying the implicit function theorem and Leibniz integral rule to (B.29),

$$\begin{aligned} \frac{dw^*}{d\rho} &= -\frac{\Phi_\rho}{\Phi_{w^*}} \qquad \text{(B.30)} \\ &= \frac{\lambda'(\rho) \int_{w^*}^{\infty} [1 - F(w)] \, dw}{r + q + \lambda(\rho) [1 - F(w^*)]} > 0 \end{aligned}$$

which is unambigously positive for any $w^* \in [0, \infty)$.

Appendix C

IDENTIFICATION OF TIME-CONSISTENT METROPOLITAN AREAS

This appendix describes the method for defining the set of 259 MSAs used for this analysis. Our objective is to identify all MSAs used in the CPS sample such that local market measures such as density and industry and occupation employment can be merged with individual unemployment spell data. In May 2004, the CPS adopted June 2003 OMB MSA definitions. Prior to that (back to January 1994, the beginning of our sample), the CPS used Census 1990 MSA definitions. Since MSA definitions change over time, it is necessary to maintain a time-consistent measure of market area such that the observed variation in labor market activity can be attributed to variation in the characteristics of the labor market and not due to the ways in which they are defined. We fix MSA definitions to those in December 2003 and standardize legacy MSA codes to this period.[82] As of December 2003, there are 369 MSAs identified by the OMB, of which 8 are in Puerto Rico (and thus not in the CPS) and 361 in the 50 United States. We do not consider micropolitan statistical areas.

Our strategy is simple: (1) collate all CPS monthly samples from May 2004 to February 2012 to identify the full set of unique MSAs in the CPS, (2) do the same for the set of MSAs in the January 1994 to April 2003 sample, and (3) use the MSA definition files for each period to match 1990-based MSA codes to their

[82] Office of Management and Budget (OMB) definitions for December 2003 are available at http://www.census.gov/population/metro/files/lists/2003/0312mfips.txt.

December 2003 equivalents based on the counties comprising each MSA.[83] We manually construct MSAs for cases where Census 1990 MSAs comprise counties that are in more than one December 2003 MSA. We discuss this further below.

First, we collate all CPS monthly files for the sampling period covering May 2004–February 2012 to determine the set of MSAs existing in the CPS sample. We identify 281 MSAs over this period. Of these, 6 are legacy MSA codes (i.e., Census 1990) and 18 are "New England city and town areas," or NECTAs.[84] We exclude the legacy codes from the set of December 2003 MSAs since they are picked up in the second stage of our matching strategy.[85]

NECTAs pose a problem for us because they define metropolitan areas in a fundamentally different way than MSAs. Namely, NECTAs may comprise counties that are not unique to a single NECTA, whereas a single county can belong to no more than one MSA. To avoid complications with differing metropolitan area definitions, we assign MSA codes to NECTAs based on the counties comprising each geograhic entity.[86] Of the 18 NECTAs in the CPS, only two comprise counties that are unique to each NECTA (70750 and 72400) and three comprise counties that are collectively part of individual MSAs (72850, 76750, and 78700). The remaining 13 NECTAs share a portion of a county (or counties) with at least one other NECTA. While it is possible to assign a MSA code to each portion (county) of a NECTA, the CPS typically suppresses county information for most respondents. Where possible, state information is used to assign MSA codes to particular NECTAs. It is important to note, however, that some degree of MSA misclassification is introduced when assigning MSAs to NECTAs in the absence

[83] Component counties are identified by 5-digit FIPS code. MSA definition files are available from the U.S. Census Bureau at `http://www.census.gov/population/metro/data/metrodef.html`. Census 1990 MSA codes are located in the "Historical Definition Files" at `http://www.census.gov/population/metro/data/pastmetro.html`.

[84] The legacy codes are based on Census 1990 definitions and include 0460, 3000, 3160, 3610, 3720, and 6450. These legacy codes are found in the May 2004 to July 2005 samples.

[85] One exception is (legacy) MSA code 3610 (Jamestown, NY MSA). This MSA code comprises a single county whihc is not classified as a MSA in the December 2003 definitions. As a result, CPS observations for this MSA are treated as non-urban and are excluded from the analysis.

[86] NECTA definition files are available at `http://www.census.gov/population/metro/files/lists/2003/0312nfips.txt`.

of explicit information regarding county of residence.[87] Table C.28 presents the correspondence between NECTA and MSA codes using this approach, resulting in the recoding of 18 NECTAs to 13 MSAs.

After excluding the 6 legacy MSAs and recasting the 18 NECTAs into 13 MSAs, we identify 270 unique MSAs ($281-6-18+13=270$) in the May 2004–February 2012 sample.[88]

Second, we repeat the same procedure with the pre-May 2004 sample. We identify 281 MSAs but 8 are excluded from the analysis.[89] This leaves 273 Census 1990 MSAs that we match with December 2003–based CPS MSA codes.

Third, we match legacy MSA codes to December 2003 MSA codes by their component counties using state and county FIPS codes. For the set of 273 legacy MSAs, we find that 11 comprise counties also comprised by more than one December 2003 MSA. To avoid the complication of trying to determine how to allocate these observations to December 2003 MSA codes, we manually combine December 2003 MSA codes for these counties as shown in Table C.29. These 11 MSAs are coded into 10 time-consistent MSAs. Further, 74 legacy MSAs are matched to a set of 27 December 2003 codes. Taking these combined and multiple-area MSA codes, the set of 273 legacy MSA codes are matched to 225 December 2003 codes ($273-11+10-74+27=225$).

In addition, Table C.29 shows that 21 December 2003 MSAs are merged to form the same set of 10 combined MSAs, leaving the final sample of 259 time-consistent MSAs ($270-21+10=259$) for the January 1994–February 2012 sample.

[87] We identify nine counties in NECTAs that are potential sources of location misclassification in this approach: 09007, 09009, 09011, 25013, 25017, 25021, 25023, 25027, 33015. In addition, several NECTAs comprise counties (or parts thereof) that are not included in MSA definitions. As a result, these individuals are misclassified as being MSA according to our approach. The counties in question are (by FIPS): 23009, 23017, 23027, 25007, 33003, 50001, and 50015.

[88] Note that Florence, AL MSA is defined in the CPS by code 22460 but is 22520 in the December 2003 definitions. The component county is the same in each case. All CPS observations for 22460 are re-coded to 22520.

[89] The excluded MSAs are 2655, 2880, 3610, 4320, 4800, 7720, 8320, and 9140. With the exception of 3610 (Jamestown, NY MSA), these exclusions are erroneous and will be corrected in a future update of this work. Jamestown, NY MSA comprises a single county which is not classified as a MSA in the December 2003 definitions; rather, it is a micropolitan area. As a result, CPS observations for this MSA are treated as non-urban and thus excluded from the analysis.

Location-based area (e.g., Landsat7, MSA boundaries), labor force (LAU data), and local industry employment and establishement counts (e.g., County Business Patterns) are calculated by summing up the total values for each component county in each area. This makes it possible to restrict local market measurements to the time-constant locations derived here. A complete correspondence table between legacy, December 2003, and combined MSA codes and their component counties are available from the author by request.

Table C.28
Assignments of MSA Codes to CPS NECTAs

MSA	Description	CBSA	Description
70750	Bangor, ME	12620	Bangor, ME MSA
70900	Barnstable Town, MA	12700	Barnstable Town, MA MSA
71650	Boston-Cambridge-Quincy, MA-NH[a]	14460	Boston-Cambridge-Quincy, MA-NH MSA
71950	Bridgeport-Stamford-Norwalk, CT	14860	Bridgeport-Stamford-Norwalk, CT MSA
72400	Burlington-South Burlington, VT	15540	Burlington-South Burlington, VT MSA
72850	Danbury, CT	14860	Bridgeport-Stamford-Norwalk, CT MSA
73450	Hartford-West Hartford-East Hartford, CT	25540	Hartford-West Hartford-East Hartford, CT MSA
74500	Leominster-Fitchburg-Gardner, MA	49340	Worcester, MA MSA
74950	Manchester, NH[f]	31700	Manchester-Nashua, NH MSA
75550	New Bedford, MA[f]	14460	Boston-Cambridge-Quincy, MA-NH MSA
75700	New Haven, CT	35300	New Haven-Milford, CT MSA
76450	Norwich-New London, CT-RI[b]	35980	North Wilkesboro, NC MSA
76750	Portland-South Portland, ME	38860	Portland-South Portland-Biddeford, ME MSA
77200	Providence-Fall River-Warwick, RI-MA[c]	39300	Providence-New Bedford-Fall River, RI-MA MSA
77350	Rochester-Dover, NH-ME[d]	38860	Portland-South Portland-Biddeford, ME MSA
78100	Springfield, MA-CT[e]	44140	Springfield, MA MSA
78700	Waterbury, CT	35300	New Haven-Milford, CT MSA
79600	Worcester, MA-CT	49340	Worcester, MA MSA

NOTES: MSA refers to "Metropolitan statistical area" based on Census 1990 definitions and CBSA refers to "Core-based statistical areas" according to December 2003 OMB definitions. Correspondence based on matching 5-digit FIPS code based on December 2003 OMB metropolitan area definitions.

Table C.29

Components of Manually Combined Metropolitan Areas, Census 1990 to December 2003 OMB Definitions

Combined	MSA	Description	CBSA	Description
99990	7160	Salt Lake City-Ogden, UT MSA	36260	Ogden-Clearfield, UT MSA
99990			41620	Salt Lake City, UT MSA
99991	460	Appleton-Oshkosh-Neenah, WI MSA	11540	Appleton, WI MSA
99991			36780	Oshkosh-Neenah, WI MSA
99992	560	Atlantic City, NJ MSA	12100	Atlantic City, NJ MSA
99992			36140	Ocean City, NJ MSA
99993	2000	Dayton-Springfield, OH MSA	19380	Dayton, OH MSA
99993			44220	Springfield, OH MSA
99994	3000	Grand Rapids, MI MSA	24340	Grand Rapids-Wyoming, MI MSA
99994	5320	Muskegon, MI MSA	26100	Holland-Grand Haven, MI MSA
99994			34740	Muskegon-Norton Shores, MI MSA
99995	3660	Johnson City-Kingsport-Bristol, TN-VA MSA	27740	Johnson City, TN MSA
99995			28700	Kingsport-Bristol-Bristol, TN-VA MSA
99996	4680	Macon-Warner Robins, GA MSA	31420	Macon, GA MSA
99996			47580	Warner Robins, GA MSA
99997	6640	Raleigh-Durham, NC MSA	20500	Durham, NC MSA
99997			39580	Raleigh-Cary, NC MSA
99998	8720	Vallejo-Fairfield-Napa, CA PMSA	34900	Napa, CA MSA
99998			46700	Vallejo-Fairfield, CA MSA
99999	3120	Greensboro–Winston-Salem–High Point, NC MSA	24660	Greensboro-High Point, NC MSA
99999			49180	Winston-Salem, NC MSA

NOTES: MSA refers to “Metropolitan statistical area” based on Census 1990 definitions, CBSA refers to “Core-based statistical areas” according to December 2003 OMB definitions, and “combined” refers to manually constructed MSA definitions based on a matching of 5-digit FIPS code between Census 1990 and December 2003 OMB metropolitan area definitions.

Appendix D

ROBUSTNESS CHECKS

Table D.30
Cox PH Model Estimates by Type of Displacement: Displaced Workers Supplement, 1996–2010

	(1) Closed	(2) Insufficient	(3) Abolished
Log labor market density†	0.863**	0.780***	0.864***
	(0.0550)	(0.0482)	(0.0484)
Received UI benefits	0.535***	0.555***	0.615***
	(0.0269)	(0.0413)	(0.0337)
Exhausted UI benefits	0.558***	0.578***	0.549***
	(0.0353)	(0.0318)	(0.0336)
Received advance notice	1.004	0.956	1.110*
	(0.0557)	(0.0556)	(0.0610)
Union member on lost job	0.781***	0.920	0.967
	(0.0687)	(0.0740)	(0.104)
Log tenure (years) on lost job	1.015	1.090***	0.962
	(0.0215)	(0.0244)	(0.0230)
Observations	2,452	2,944	2,035
Log-likelihood	-12,088.04	-12,689.31	-9,587.74
Number of clusters	222	224	209

NOTES: Estimates reported as hazard ratios. Columns (1), (2), and (3) correspond to workers who lost their job through a plant closing or move, insufficient work, or abolition of their shift or position, respectively. Log variables correspond to the natural logarithm. Sampling weights *not* used. Cluster-robust standard errors in parentheses (clustered by MSA). Hazard ratios less than 1 imply a slowing of the hazard rate relative to the baseline hazard given a marginal change in the respective covariate (thus duration increasing); hazard ratios greater than one indicate a speeding up of hazard (durations decreasing).

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

† Density measured using Landsat7.

Table D.31
2SLS Post-Displacement Earnings Estimates by Type of Displacement: Displaced Workers Supplement, 1996–2010

	(1) Closed	(2) Insufficient	(3) Abolished
Log weekly earnings, lost job	0.455***	0.441***	0.510***
	(0.0253)	(0.0234)	(0.0283)
Log usual weekly hours, current job	0.827***	0.804***	0.881***
	(0.0487)	(0.0586)	(0.0529)
Log labor market density†	0.0720***	0.109***	0.185***
	(0.0255)	(0.0416)	(0.0272)
Fitted values of log duration (weeks)	-0.0485***	-0.0608***	-0.0707***
	(0.0146)	(0.0155)	(0.0159)
Constant	0.0661	0.0104	-1.166***
	(0.283)	(0.305)	(0.319)
Industry dummies	Yes	Yes	Yes
Occupation dummies	Yes	Yes	Yes
Education and demographic controls	Yes	Yes	Yes
Observations	2,264	2,149	1,775
R^2	0.699	0.691	0.730
Number of clusters	219	208	200

Notes: Dependent variable is the natural logarithm of post-displacement weekly earnings. 2SLS estimates use fitted values of log duration (UI receipt and log tenure on lost job used as exclusion restrictions). First-stage duration equation includes control for censoring. 2SLS estimates are for non-ccensored sample only. All regressions include controls for year, education, demographics, and major industry and occupation. A complete list of included demographic and education variables reported in Table 2.15. Year controls refer to year of survey. Homoskedastic standard errors in parentheses. Sample corresponds to the set of completed spells. Log variables are calculated using the natural logarithm. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$
† Density measured using Landsat7.

Table D.32
2SLS Post-Displacement Earnings Estimates of First Stage Exclusion Restrictions: Displaced Workers Supplement, 1996–2010

	(1)	(2)
Log weekly earnings, lost job	0.473***	0.475***
	(0.0145)	(0.0149)
Log usual weekly hours, current job	0.824***	0.823***
	(0.0307)	(0.0306)
Log labor market density (Landsat7)	0.110***	0.110***
	(0.0240)	(0.0240)
Log duration of unemployment (weeks)	-0.0408***	-0.0380***
	(0.00344)	(0.00375)
Log tenure on lost job (years)†		-0.00202
		(0.00491)
Recived UI benefits†		-0.0169
		(0.0143)
Constant	-0.295	-0.311
	(0.206)	(0.216)
Observations	6,188	6,188
Number of clusters	248	248
R^2	0.706	0.706

NOTES: Dependent variable is the natural logarithm of post-displacement weekly earnings. All regressions include controls for year, education, demographics, and major industry and occupation. A complete list of included demographic and education variables reported in Table 2.15. Year controls refer to year of survey. Homoskedastic standard errors in parentheses. Sample corresponds to the set of completed spells. Log variables are calculated using the natural logarithm. Earnings are deflated to July 2011 U.S. dollars using the Consumer Price Index for all urban consumers (CPIAUCNS) from the Federal Reserve Bank of St. Louis Federal Reserve Economic Data (`http://research.stlouisfed.org/fred2/series/CPIAUCNS?cid=9`).
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$
† Exclusion restrictions for first stage duration equation.

Table D.33
First-Stage OLS Duration Estimates: Displaced Workers Supplement, 1996–2010

	(1) LF Size	(2) MSA	(3) UA	(4) Landsat7
Log labor market scale	0.0372***	0.0530***	0.0983**	0.103***
	(0.0109)	(0.0145)	(0.0408)	(0.0316)
Recived UI benefits	1.383***	1.379***	1.384***	1.381***
	(0.0281)	(0.0282)	(0.0281)	(0.0281)
Log tenure on lost job (years)	-0.0364***	-0.0370***	-0.0356***	-0.0363***
	(0.0108)	(0.0108)	(0.0108)	(0.0108)
High school degree or GED	-0.146***	-0.148***	-0.146***	-0.148***
	(0.0479)	(0.0479)	(0.0479)	(0.0479)
Some college, no degree	-0.246***	-0.244***	-0.244***	-0.246***
	(0.0514)	(0.0514)	(0.0514)	(0.0514)
Two-year degree, vocational	-0.341***	-0.338***	-0.340***	-0.341***
	(0.0746)	(0.0745)	(0.0746)	(0.0746)
Two-year degree, academic	-0.164**	-0.164**	-0.160**	-0.166**
	(0.0735)	(0.0735)	(0.0735)	(0.0736)
Four-year degree	-0.172***	-0.173***	-0.167***	-0.175***
	(0.0529)	(0.0529)	(0.0529)	(0.0530)
Master's degree	-0.231***	-0.231***	-0.223***	-0.231***
	(0.0710)	(0.0709)	(0.0709)	(0.0710)
Professional degree	-0.407***	-0.403***	-0.395**	-0.404***
	(0.154)	(0.154)	(0.154)	(0.154)
Doctoral degree	-0.0409	-0.0379	-0.0493	-0.0484
	(0.169)	(0.169)	(0.170)	(0.170)
Year controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Number of observations	8,623	8,623	8,623	8,623
R^2	0.376	0.376	0.375	0.376
F	191.63	191.73	191.29	191.57

Notes: Dependent variable is the natural logarithm of unemployment duration (weeks). Sample includes censored and uncensored spells, with each specification including a binary indicator of censoring as a control. Log variables correspond to the natural logarithm. Year controls refer to year of survey. A complete list of included demographic controls are reported in Table 2.15 (high school dropout is the omitted education category). All regressions weighted by Current Population Survey final weights (PWSSWGT). Homoskedastic standard errors in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

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