Duration Dependence and Labor Market Conditions: Theory and Evidence from a Field Experiment

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July 2012[1]

Abstract

This paper studies the role of employer behavior in generating "negative duration dependence" – the adverse effect of a longer unemployment spell – by sending fictitious resumes to real job postings in 100 U.S. cities. Our results indicate that the likelihood of receiving a callback for an interview significantly decreases with the length of a worker's unemployment spell, with the majority of this decline occurring during the first 8 months. We explore how this effect varies with local labor market conditions, and we find that duration dependence is stronger when the labor market is tighter. We develop a theoretical framework that shows how the sign of this interaction effect can be used to discern among leading models of duration dependence based on employer screening, employer ranking, and human capital depreciation. Our results suggest that employer screening plays an important role in generating duration dependence; in particular, our results are consistent with employers using unemployment spell length as a signal of unobserved productivity, and recognizing that this signal is less informative in weak labor markets. (JEL J64)

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1 Introduction

Does the length of time out of work diminish a worker's job market opportunities? This question attracts substantial attention from policymakers and researchers alike, reflecting the widespread belief that the adverse effect of a longer unemployment spell – what economists call "negative duration dependence" – undermines the functioning of the labor market and entails large social costs.[2] Recently, the sharp rise in long-term unemployment has renewed interest in duration dependence; according to a recent report by the Congressional Budget Office, long-term unemployment may "produce a self-perpetuating cycle wherein protracted spells of unemployment heighten employers' reluctance to hire those individuals, which in turn leads to even longer spells of joblessness" (CBO 2012).

Despite this widespread interest, it has proven very difficult to credibly establish that a longer unemployment spell has a genuine causal effect on an individual's job finding probability. The difficulty arises from a substantial practical challenge with identifying duration dependence using conventional observational data: workers with different unemployment spell lengths who appear (otherwise) similar to researchers may actually look very different to employers. In this case, job finding rates could decline with unemployment duration either because of "true" duration dependence or because unemployment spell lengths are correlated with other characteristics that are observed by employers but not researchers.[3] The state of the empirical literature is succinctly summarized by Ljungqvist and Sargent (1998), who write: "It is fair to say that the general evidence for duration dependence is mixed and controversial".

In this paper, we confront this challenge by estimating duration dependence using a large-scale resume audit study. We submit fictitious resumes to real, online job postings in each of the 100 largest metropolitan areas in the U.S., and we track "callbacks" from employers for each submission. In total, we respond to roughly 3,000 job postings in sales, customer service, administrative support, and clerical job categories, and we submit roughly 12,000 resumes. In designing each resume, we explicitly randomize both the employment status and the length of the current unemployment spell (if the worker is unemployed), so that the unemployment spell length is (by construction) orthogonal to all of the other characteristics of the resume that are observable by potential employers. Our experiment therefore directly uncovers duration dependence arising through employers' beliefs about unemployed workers.

To help guide our empirical analysis and interpret our experimental results, we develop a simple matching model of unemployment. Firms are identical and workers can be of two types: high type or low type. Workers are born continuously into unemployment and meet firms randomly. We characterize the job

[2] Throughout the paper we will use the term "duration dependence" in place of "negative duration dependence".

[3] More broadly, this challenge relates to the standard econometric problem of distinguishing state dependence from unobserved heterogeneity. Previous work has shown that without functional form assumptions on job finding rates, it is not possible to distinguish between duration dependence and unobserved heterogeneity using observational data with a single unemployment spell for each individual (Heckman and Singer 1984). Multiple-spell data can resolve this identification problem, but at the cost of strong assumptions on how job finding rates vary across unemployment spells within an individual. For a summary of the duration dependence literature, see Machin and Manning (1998).

finding probability for an unemployed worker (at a given unemployment duration) under three assumptions. First, firms are more likely to hire high type workers ("Assumption 1"). Second, for both types of workers, the chance of getting hired increases with the share of high types ("Assumption 2"). Third, worker type and the share of high types are both weakly separable from market tightness in the job finding rate ("Assumption 3"). Assumptions 1-2 imply negative duration dependence in job finding rates, and Assumptions 1-3 imply that duration dependence is stronger when the labor market is tighter (i.e., a higher ratio of vacancies to unemployed workers).

Since the model does not contain any microfoundations for firm or worker behavior, we label it a "mechanical model". There are two key advantages of pursuing this reduced-form approach. First, it allows us to isolate in a clear way the assumptions that are pivotal in generating negative duration dependence. Second, it is useful in discerning among leading models of duration dependence. We show that employer screening models (Vishwanath 1989; Lockwood 1991) satisfy Assumptions 1-3 and therefore predict negative duration dependence and a positive relationship between duration dependence and market tightness. On the other hand, we show that alternative models of duration dependence do not satisfy all three assumptions and therefore differ in their predictions regarding how duration dependence varies across local labor markets. The employer ranking model (Blanchard and Diamond 1994; Moscarini 1997) predicts a negative interaction between duration dependence and market tightness, and a model of human capital depreciation (Acemoglu 1995) predicts no interaction when the rate of human capital depreciation does not vary with market tightness. This motivates our decision to implement our field experiment across 100 local labor markets and forms the basis of our second empirical test, which estimates how duration dependence varies with market tightness.

Turning to our empirical results, simple plots of the raw data show clear visual evidence of negative duration dependence, with the effect most pronounced during the first 8 months of unemployment. OLS regression results confirm these patterns: the estimated effect of unemployment duration on the probability of a callback is both statistically and economically significant. At average levels of market tightness, we find that the callback rate declines rapidly during the first 8 months of unemployment and stabilizes afterwards. At 8 months of unemployment, callbacks are about 25 percent lower than at 1 month of unemployment, as the callback rate falls from roughly 7% to 4% over this range To benchmark the magnitude of this result, in their study of racial discrimination, Bertrand and Mullainathan (2004) found that Black sounding names received about 33 percent fewer callbacks than White sounding names.

We next estimate how the magnitude of duration dependence varies with labor market conditions. Our results indicate that the magnitude of duration dependence is significantly larger when the local labor market is tight. This finding is robust to using several different measures of market tightness: first, the callback rate for a newly unemployed individual in each city, a measure which is motivated by the mechanical model; second, metropolitan area monthly unemployment rates; finally, city-level data on vacancies and unemployment (from the Help Wanted OnLine Index and the BLS, respectively). As the model makes clear, these results provide evidence in support of the employer screening model, although we emphasize that our evidence does not rule out a role for employer ranking or human capital depreciation, as well. In particular, our results are consistent with employers using unemployment spell length as a signal of unobserved productivity, and employers recognizing that this signal is less informative in weak labor markets.

Most closely related to our work is Oberholzer-Gee (2008) and Eriksson and Rooth (2011), who also investigate how employers respond to unemployment spells using a resume audit study. Oberholzer-Gee (2008) analyzes Swiss employer responses to 628 resume submissions. Eriksson and Rooth (2011) submit 8,466 job applications to 3,786 employers in Sweden and compare the effects of contemporary and past unemployment spells (e.g., unemployment spells at graduation). Both of these studies report results consistent with the long-term unemployed being less likely to receive callbacks, but neither of these papers considers how duration dependence varies across local labor markets. Additionally, both of these studies randomize across a small number of unemployment spell lengths, which makes it difficult to flexibly estimate the (possibly highly nonlinear) relationship between callback rate and unemployment spell length.

Lastly, to address the concern (both in our work and in the related papers above) that the unemployment spell length is not salient to employers, we administer a web-based survey to MBA students. Our survey results indicate that the length of the current unemployment spell is salient to the survey participants; in particular, subjects were able to recall a worker's employment status and unemployment spell length with roughly the same degree of accuracy and precision as other resume characteristics such as education and job experience. This supports our assumption that the way we represent unemployment durations on resumes is salient to the employers in our experiment.

The remainder of the paper proceeds as follows: Section 2 develops the mechanical model. Section 3 describes the main predictions from three specific models of duration dependence: the employer screening model, the employer ranking model, and the human capital depreciation model. Section 4 describes the experimental design, the empirical models, and results from the web-based survey. Section 5 describes our experimental results. Section 6 concludes.

2 The Model

This section describes a mechanical model of unemployment. We purposely label the model "mechanical" since it is not explicitly based on microfoundations; it does not specify an information structure, firm or worker objectives, or a wage setting process. The question we address is the following: What properties on the job finding process lead to duration dependence and how does duration dependence vary with labor market conditions? By considering a reduced-form approach to this problem, we simplify the analysis considerably. More importantly, this approach demonstrates that the predictions of the model are general: we show in Section 3 that a class of employer screening models, including a generalized version of the screening model in Lockwood (1991), map into the reduced-form of this mechanical model and therefore generate the same comparative statics. Thus, the approach pursued here can be thought of as identifying the pivotal assumptions in screening models that feature duration dependence in unemployment. We refer the reader interested in a screening model with microfoundations to Section 3 and the Appendix.

2.1 Population Flows

We consider economies in steady state where inflows into unemployment are equal to outflows out of unemployment. A population of mass 1 is born continuously into unemployment. There are two possible types: either "high" types ($y = h$) or "low" types ($y = l$). The fraction of these two types are π_0 and $1 - \pi_0$, respectively. In the screening model below, worker type will correspond to unobserved worker productivity.

In the unemployed population, we allow the share of high types to depend on unemployment duration, which we denote by d. Formally, we define:

$$\pi(d) \equiv \Pr(y = h|d) \tag{1}$$

In terms of outflows, we assume that individuals transition out of unemployment either by finding a job or by retiring. We assume that the job finding rate depends on worker type and the share of high types, and we denote this rate by $h_y(\pi(d))$.

We assume that individuals retire at an exogenous rate δ which does not depend on worker type or labor market status. The decision to assume away job separations is primarily to keep the analysis simple. Employer screening models that feature job separations are more complicated since this provides another source of information to potential employers to learn about worker productivity. Our interest is characterizing the adverse effects of a current spell of unemployment. While characterizing the effects of a worker's entire work history is interesting, it is beyond the scope of our study. We therefore follow Lockwood (1991) and assume that a worker is employed with at most a single firm in his lifetime.

The individual exit rate out of unemployment is the sum of the individual job finding rate and retirement rate:

$$h_y(\pi(d)) + \delta \tag{2}$$

Given the escape rate in equation (2), $\pi(d)$ satisfies:

$$\pi(d) = \frac{\pi_0 \exp(-\int_0^d (h_h(\pi(\tau)) + \delta)\, d\tau)}{\pi_0 \exp(-\int_0^d (h_h(\pi(\tau)) + \delta)\, d\tau) + (1 - \pi_0) \exp(-\int_0^d (h_l(\pi(\tau)) + \delta)\, d\tau)} \tag{3}$$

To interpet equation (3), recall that the unemployed population at $d = 0$ is normalized to 1, so π_0 gives the number of newly unemployed high types and the term $\exp(-\int_0^d (h_h(\pi(\tau)) + \delta)\, d\tau)$ is the survival function for high types. Thus, the numerator exactly represents the number of high types that are unemployed after d periods. By similar logic, the denominator is the total number of individuals that are unemployed at duration d. Thus, their ratio pins down $\pi(d)$.

Finally, given the individual job finding rate and the share of high types, we can define the population job finding rate at a given duration as a mixture of the type-specific job finding rates:

$$h(\pi(d)) = \pi(d)\, h_h(\pi(d)) + (1 - \pi(d))\, h_l(\pi(d)) \tag{4}$$

Expression (4) shows that the population job finding rate varies with unemployment duration through two channels. First, it varies directly with duration through the skill share. This source of variation represents "unobserved heterogeneity". Intuitively, the composition of types at risk of leaving unemployment shifts over time. Second, it varies indirectly with duration through the individual job finding rates. This source of variation captures "true duration dependence" and represents how the population job finding rate varies over the spell, holding the skill share constant. Ultimately, both sources depend on how the share of high types varies over the unemployment spell, so that the two sources of duration dependence interact and reinforce each other.

2.2 Duration Dependence

To operationalize the model, we impose two key assumptions on the individual job finding rate. The first assumption governs how the individual job finding rate varies with worker type and the second assumption governs how it varies with the share of high types.

Assumption 1 *At a given unemployment duration, high types find jobs at higher rates than low types:*

$$h_h(\pi(d)) > h_l(\pi(d)) \tag{5}$$

Assumption 2 *The individual job finding rates increase in the share of high types:*

$$\frac{\partial h_y(\pi(d))}{\partial \pi} \geq 0 \tag{6}$$

Assumptions 1 and 2 are intuitively explained in the context of employer screening models (Vishwanath 1989; Lockwood 1991). In such models, worker types differ in productivity, firms draw signals on worker productivity, and firms set a hiring threshold for signals. Since the signals are informative on worker productivity, high types are more likely to draw high signals and be hired. Assumption 2 is satisfied since the hiring threshold decreases in the firm's prior that a worker is productive. Under rational expectations, this threshold equals the share of high types in the unemployed population.

The next proposition states that the proportion of high types among the unemployed and, as a result, the job finding rates decline with duration.

Proposition 1 *The proportion of high types in the unemployed population in equation* (3) *and the population job finding rate* (4) *decline strictly with duration. The individual job finding rate declines weakly with duration* d*. Thus, the model features negative duration dependence in unemployment.*

The proof is straightforward. First, by Assumption 1, high types find jobs more frequently than low types. As a result, the composition of the unemployed shifts to low types at longer durations. Since the share of high types declines over the spell and since individual job finding rates are increasing in the share of high types (Assumption 2), individual job finding rates decline over the spell. Finally, the population job finding rate declines due to true negative duration dependence and unobserved heterogeneity.[4]

To study how duration dependence interacts with market tightness, we impose more structure on individual job finding rates. First, we assume that a single worker and a single firm randomly meet according to the constant returns to scale (CRS) matching function $m(U,V)$, where U and V are the number of unemployed workers and vacancies, respectively. Defining $x = \frac{V}{U}$ as labor market tightness, the CRS assumption implies that the rate at which unemployed individuals are matched with vacancies depends only on labor market tightness. We denote the arrival rate of job offers by $m_u(x)$.[5] Note that under this assumption, worker type does not influence the arrival rate of jobs offers. This is useful since it allows us to isolate the consequences of employer behavior.

Next, once a firm and worker have matched, we assume that the conditional hiring rate depends on worker type and the share of high types in the population. We denote this rate by $l_y(\pi(d;x))$ and assume that $l_h(\pi) > l_l(\pi)$ and $\frac{\partial l_y(\pi)}{\partial \pi} \geq 0$, which ensures that the individual job finding rate continues to satisfy

[4] More formally, define $\theta(d) = \frac{\pi(d)}{1-\pi(d)}$. Differentiating $\theta(d)$ with respect to d and applying Assumption 1 gives $\pi'(d) < 0$. This combined with Assumption 2 delivers $\frac{\partial h_y(\pi(d))}{\partial d} \leq 0$. Finally, $\frac{\partial h(\pi(d))}{\partial d} = (1-\pi(d))\underbrace{\frac{\partial h_l(\pi(d))}{\partial d}}_{\leq 0} + \pi(d)\underbrace{\frac{\partial h_h(\pi(d))}{\partial d}}_{\leq 0} + \underbrace{\frac{\partial \pi(d)}{\partial d}}_{<0}\underbrace{(h_h(\pi(d)) - h_l(\pi(d)))}_{>0} < 0$.

[5] Note that $m_u(x) = \frac{m(U,V)}{U} = m(1, \frac{V}{U})$. Analogously, the rate at which vacancies meet applicants is $m_v(x) = \frac{m(U,V)}{V} = m(\frac{U}{V}, 1)$. The relation between the two arrival rates is given by $m_u(x) = x m_v(x)$. We assume that $\lim_{x\to\infty}(m_u(x)) = \infty$ and $\lim_{x\to 0}(m_u(x)) = 0$.

Assumptions 1 and 2. The third key assumption of the model governs how individual job finding rates vary with market tightness.

Assumption 3 *In the individual job finding rate, the type of the worker and the share of high types conditional on duration are weakly separable from market tightness:*

$$h_y(d;x) = m(x) \times l_y(\pi(d;x)) \tag{7}$$

In screening models, market tightness affects the job finding rate through two channels. First, it affects the rate at which workers meet firms. Second, it controls the precision of the information revealed by a worker's unemployment duration. In tight markets, a long unemployment spell reveals that the worker has likely been previously found unsuitable by prospective employers. Thus, the conditional hiring rate will implicitly depend on market tightness. When $d = 0$, the share of high types π_0 does not vary with market tightness and thus, the job finding rate varies with market tightness only through the congestion effect.

To study how duration dependence varies with labor market tightness, we define:

$$r(d;x) = \frac{h(d;x)}{h(0;x)} \tag{8}$$

The function $r(d;x)$ is the ratio of the population job finding rate evaluated at duration d to the population job finding rate among the newly unemployed. This is an intuitive measure of the strength of duration dependence. If there is negative duration dependence, this ratio is below 1; conversely, if there is positive duration dependence, this ratio exceeds 1.[6] A key property of this ratio is that it depends on market tightness only through the share of high types. The effect of market tightness on duration dependence which occurs through the arrival rate is not operational since it affects the job finding rate at all durations in a uniform way. This leads to the second proposition.

Proposition 2 *Duration dependence is stronger (more negative) if labor markets are tighter.*

Formally, proposition 2 states that $r(d;x)$ for tight labor markets (large x) lies everywhere below the function $r(d;x)$ observed in loose labor markets (small x); i.e., $\partial r(d;x)/\partial x < 0$.[7] Intuitively, in tight labor markets, workers are more likely to meet early on with firms. By Assumption 3, this rate of matching does not depend on worker type. Assumption 1 guarantees that high types are relatively more likely to exit unemployment when matched with a firm. This selection effect implies that the share of low types is

[6] An alternative measure of how duration dependence varies with market tightness is the cross-derivative of this function: $\frac{\partial^2 r(d;x)}{\partial d \partial x}$. However, the cross-derivative is local, and it can be positive for some values of d and negative for others. As it turns out, our measure has no general implications for such local measures of duration dependence. Instead, we will use a global measure that holds for all positive values of d.

[7] For a mathematical proof, we refer the reader to Appendix B.

relatively larger among the long-term unemployed. By Assumption 2, this strengthens duration dependence. By contrast, in loose labor markets, both worker types are less likely to meet open vacancies. Therefore, the share of high types will vary little over time, generating less duration dependence.

2.3 Adapting Model to Incorporate Callbacks

Propositions 1 and 2 deliver testable predictions on job finding rates. However, in our audit experiment, we do not observe hiring decisions, but rather whether applicants are called back for interviews. To align the theory more closely with our empirical application, this section adapts the model to incorporate an interview stage and callbacks. We define the callback rate as the probability that a worker gets invited for an interview. This is to be distinguished from the job finding rate, the (joint) probability a worker receives a callback and gets hired.

The decision to interview a worker can in principle depend on individual characteristics in addition to the duration of unemployment. We represent these characteristics by the vector ϕ and denote its distribution conditional on type y and duration d by $\Phi_y(.|d)$. The unconditional distribution is given by $\Phi(.|d)$.[8] We denote the share of high types in the population by $\pi(d;\phi)$.

We consider the case where the callback rate has the form $c(\pi(d;\phi);x)$ and we assume that it is weakly increasing in the high type share. When $d=0$, the callback rate varies with market tightness only through the congestion channel; in particular, the information channel is absent. Intuitively, a newly unemployed worker reveals no information to firms that can be used to predict productivity. Thus, in our empirical work, we will use the callback rate of a newly unemployed worker as a measure of market tightness. In the employer screening model developed below, the callback rate has this functional form. Intuitively, as the firm's prior that a worker is productive increases, it is more likely that the benefits of interviewing the worker outweigh the interview costs. The population job finding rate, conditional on ϕ, is obtained by replacing $\pi(d)$ with $\pi(d;\phi)$ and $h_y(\pi(d))$ with $h_y(\pi(d;\phi))$ in equation (4). This immediately implies the following corollary to Proposition 1:

Corollary 1 *Callback rates exhibit negative duration dependence.*

Intuitively, once all of a worker's characteristics (ϕ) are accounted for, back luck that leads to a longer duration at the individual level will lead to a callback rate that declines at the individual level. Next, as an analog to the function $r(d;x)$, we define the relative ratio of callbacks rates:

$$r_c(d;x,\phi) = \frac{c(d;\phi;x)}{c(0;\phi;x)} \tag{9}$$

This delivers the following corollary to Proposition 2:

[8] $\Phi(.|d)$ is equal to $\pi(d)\Phi_h(.|d) + (1-\pi(d))\Phi_l(.|d)$.

Corollary 2 *Duration dependence in the callback rate is stronger if markets are tighter.*

Formally, this says that conditional on ϕ, $r_c(d; x, \phi) \geq r_c(d; x', \phi)$ if $x < x'$. The practical value of this corollary is that it implies we can use callback rates to test the implications on job finding rates that we derived above. In practice, it is difficult to empirically test for true duration dependence in callback rates using observational data. If an econometrician cannot fully account for the impact of ϕ on callbacks, the estimate of $\frac{\partial c(\pi(d;\phi))}{\partial d}$ will be confounded by composition bias. For example, it is easy to account for some characteristics on a resume (such as gender, education and experience). However, resumes are complex and it is difficult to fully control for all characteristics. Even though the econometrician might have access to the entire resume, he will not know the complete mapping between callbacks and all of the variables on the resume and potential interactions between them.

To illustrate this bias more formally, consider the extreme case where callbacks depend only on ϕ, so that we may write the callback rate as $c(\phi)$. Furthermore, assume that $c'(\phi) > 0$. This represents a situation where firms do not condition their callback decisions on d; there is no true duration dependence under this formulation. Assume that ϕ is unobserved by the econometrician. The population callback rate $c(d)$ is defined as follows:

$$c(d) \equiv \int c(\phi) \frac{d\Phi(\phi|d)}{d\phi} d\phi \tag{10}$$

In Appendix B, we establish that $c'(d) < 0$. Intuitively, the unemployment distribution shifts to those with low ϕ as spell lengths increase; resumes with long current spells of unemployment are more likely to be low ϕ and thus likely to have lower callback rates, even in the absence of true duration dependence. Thus, in the absence of any true duration dependence, callback rates will decline unless we are able to control for all relevant components of the CV.

In our resume audit study, randomization of unemployment durations ensures that the distribution of unobserved characteristics ϕ is independent of the duration of unemployment, and so the composition bias described above will be absent. Since we randomize unemployment duration, our experiment recovers how the average callback rate evolves with unemployment duration. More formally, define the distribution $\widetilde{\Phi}(.)$ as the distribution of characteristics on our experimental set of CVs. Note that this distribution will not be the population distribution. Instead, we recover the following object:

$$\widetilde{c}(d) = E_{\widetilde{\Phi}}\left[c(\pi(d;\phi))\,|d\right] = \int_{\phi} c(\pi(d;\phi))\, d\widetilde{\Phi}(\phi) \tag{11}$$

The function $\widetilde{c}(d)$ is an average over the callback rates for which the above corollary holds and the predictions of this corollary therefore also apply to $\widetilde{c}(d)$. This implies that we can use the callback rates elicited in our experiment to test the implications of the model. Finally, it is worth noting that even conditional on ϕ, the population job finding rate, $h(\pi(d;\phi))$, will nevertheless decline in d due in part to unobserved heterogeneity. This occurs since $h_h(\pi(d;\phi)) > h_l(\pi(d;\phi))$. To see the intuition for this, consider a firm who interviews a high type and a low type worker, both of whom have the same value of ϕ. As we show in the screening model below, it is more likely that a firm draws a relatively higher signal (z) for the high type worker. Thus, workers with long durations will be those with low values of ϕ and low values of z. An econometrician who observes ϕ – but not the signal z at the hiring stage – may be led to conclude that there is true duration dependence in job finding rates when in fact the estimates are picking up a selection effect. In this sense, it is more straightforward to identify true duration dependence in callback rates than in job finding rates, since an econometrician only needs to condition on the information that a potential employer sees at the interview stage, not the hiring stage.

3 Employer Screening, Human Capital Depreciation, and Employer Ranking

The mechanical model described in section 2 is not based on behavioral microfoundations but rather on reduced form assumptions on hiring rates. This is intentional to show that its predictions are fairly general – any structural model that has the same implications for hiring and matching rates generates the same predictions. However, the model is not so general so as to be vacuous: there are behavioral models which do not map into this structure and which do not generate the same predictions. In this section, we briefly (and informally) discuss the three leading behavioral models of employer-driven duration dependence. These models are presented formally in Appendixes C, D, and E.

In Appendix C, we develop a simple screening model of employer-driven duration dependence.[9] Matching frictions and firm screening of workers interact to generate duration dependence in job finding rates. Search frictions result in equilibrium unemployment: unemployed workers and vacancies meet at a rate determined by aggregate unemployment and vacancies in the economy. Upon a meeting between a vacancy and a job seeker, the potential employer obtains a signal on worker productivity. One can conceptualize this signal as the job seeker's resume. Interviewing an applicant is costly, and firms call applicants for interviews only if they receive sufficiently high signals. If a worker is called in for an interview, the firm observes an additional signal, providing new information on worker productivity. A worker with a sufficiently high signal is hired and earns his outside option.[10] In this model, firms lower their priors about individuals with long durations,

[9] Our model is related to Lockwood (1991). However, we depart from Lockwood in two ways. First, we allow for an interview stage that is distinct from the hiring stage. Second, in the hiring stage, we allow for a more general signal distribution. In particular, Lockwood (1991) imposes that applicants signals are either high or low and that productive types always send the high signal, whereas unproductive types send the high signal with a probability less than one. Lockwood's model results in extreme duration dependence in the sense that upon hitting a certain duration, the probability of finding a job for a low productive type declines to zero. This extreme assumption makes it difficult to map empirical results into the framework of Lockwood (1991). By allowing for a more general, continuously distributed signal, we allow for a continuous form of duration dependence; in our model job finding rates decline smoothly with durations.

[10] The screening model can allow for less restrictive assumptions on wage setting. What is required is that applicants and firms, upon receipt of the signal, are more likely to enter into an employment relationship if the expected productivity of a worker is higher. We conjecture that most bargaining models satisfy this requirement.

since they believe that these have been found wanting by other potential employers. This induces negative duration dependence.

The screening model satisfies Assumptions 1-3 of the simple mechanical model. First, high productivity workers signal their type during the interview and are hired at greater frequency than low productivity workers (Assumption 1). Second, the hiring rate increases with π because employers' prior is π (Assumption 2). Third, the matching structure implies weak separability between market tightness and π (Assumption 3). Thus, in the screening model, duration dependence gets stronger in tight labor markets (Proposition 2). In tight markets, employers believe that the unemployed were evaluated more frequently and rejected by potential employers and firms therefore adjust their priors more. In contrast, in loose markets, it is less likely that applicants met with firms in the past. Thus, screening predicts that as labor markets tighten, duration dependence gets stronger. Therefore, the screening model can be tested by examining duration dependence in callback rates and how duration dependence interacts with market tightness.

As a contrast to the screening model, we consider two alternative models of duration dependence that do not map into the structure above. The first model, developed in Appendix D, is a model of human capital depreciation. The rate at which applicants meet firms is again governed by a matching function that depends on market tightness. Individuals' human capital affects their productivity in all firms in the same way. This human capital – common to all employers – depreciates while workers are unemployed and the rate of this depreciation does not depend on market tightness. In addition to this common human capital component, there is a worker-firm specific match component. In order to isolate the implications of human capital depreciation from screening considerations, we assume that firms are fully informed about the human capital parameter. However, firms need to interview workers in order to learn their match-specific component. As in the simple screening model, wages are given by a fixed outside option. The expected firm surplus from a given match therefore declines if an applicant has been unemployed for longer and the model generates negative duration dependence. However, the frequency with which the unemployed are matched to firms does not affect the speed with which their human capital depreciates. Therefore, this model does not generate any interaction between duration dependence and market tightness.

In Appendix E, we consider the well-known ranking model proposed by Blanchard and Diamond (1994). This model emphasizes the consequences of crowding in the labor market; vacancies potentially receive multiple applications. It is assumed that if a firm meets multiple workers, it hires the worker with the minimum duration. The ranking model predicts that job finding rates decline with unemployment durations. Intuitively, a worker with a long duration is more likely to face competition for a job and more likely to be rejected. Therefore, the ranking model also generates negative duration dependence. However, in tight markets, applicants for a given position are less likely to face competition from applicants with shorter durations. Therefore, under ranking duration dependence is less negative if labor markets are tight.

The key insight from our theoretical framework is that these three mechanisms for duration dependence (screening, ranking, and human capital depreciation) differ in their prediction about how duration dependence varies with local labor market conditions: screening generates a positive interaction effect, ranking generates a negative interaction effect, and human capital depreciation does not generate an interaction effect. Therefore, by estimating how duration dependence varies with labor market tightness, we will be able to shed light on the relative importance of these mechanisms. This theoretical prediction motivates a key aspect of the design of our resume audit study, which focuses on estimating duration dependence in a large number of local labor markets.

4 Experimental Design

The design of the field experiment follows Bertrand and Mullainathan (2004), Lahey (2008), and Oreopolous (2011) in how we generate fictitious resumes, find job postings, and measure callback rates. All of the experimental protocols (as well as the web-based survey for MBA students) were reviewed and approved by the Institutional Review Board (IRB) at the University of Chicago. The IRB placed several constraints on the field experiment.[11] First, none of the researchers involved in the study could contact the firms at any time, either during or after the experiment. Second, in order to ensure that the individual representatives of the prospective employers could never be identified, we were required to delete any e-mails or voice messages that we received from employers after ascertaining the information from the message needed for the experiment. Finally, we were not able to preserve any identifying information about the prospective employers other than the industry. By contrast, we were approved to preserve richer information on the characteristics of the job posting, such as the posted wage and required experience, among other things.

The setting for our experiment is a single major online job board in the U.S. This online job board contains jobs advertised across most cities in the U.S., allowing us to implement our experiment in a broad set of local labor markets. Following earlier audit studies, we focus on 3 job categories: Administrative/Clerical, Customer Service, and Sales. Within these job categories, we sent roughly 12,000 fictitious resumes to 3,000 job openings located in the largest 100 Metropolitan Statistical Areas (MSAs) in the U.S. according to population (as measured in 2010 Census). The distribution of the 3,000 jobs across the 100 MSAs was fixed prior to the experiment and primarily reflected the population distribution across MSAs.[12] For example, we planned on submitting resumes to roughly 200 jobs to the MSA New York-Northern New Jersey-Long Island, NY-NJ-PA and roughly 15 jobs to the Raleigh-Cary, NC MSA. However, we also chose to oversample the bottom 10 and top 10 MSAs (within the set of the 100 largest) based on the unemployment rate in July

[11] The web-based survey instrument described below was approved with no additional constraints.

[12] Our initial motivation for sampling based on population size was to achieve a nationally representative sample of job postings. As the experiment proceeded, however, we discovered another practical benefit of this decision, which is that we found it easier to find suitable jobs for the experiment in larger cities.

2011.[13] Within each MSA, 30% of jobs were allocated to Administrative/Clerical, 30% to Customer Service, and 40% to Sales.

In choosing a job to apply to, we began by randomly sampling without replacement from the distribution of MSA and job category combinations. Upon being assigned an MSA and job category, we had a Research Assistant (RA) visit the online job board and search for jobs within the pre-determined city for the pre-determined job type. When picking jobs to apply to, we imposed several restrictions. First, we did not pick jobs that were posted by recruiting agencies. Second, we avoided independent outside sales positions (e.g., door-to-door sales). Third, we do not pick jobs that require advanced skill sets, licenses, or advanced degrees (beyond a standard 4-year college degree). Typically, a job opening within a given category and MSA that satisfies these criteria is immediately available, or (in rare cases) becomes available within one or two weeks.

Once a job was identified, the next step was to construct 4 fictitious resumes that we would customize and e-mail to this job opening. The design of these resumes was based on roughly 1,200 real resumes of job seekers that we manually collected from various online job boards. These resumes were selected based on the job categories we focused on: i.e., individuals applying to Administrative/Clerical, Customer Service, and Sales positions. A descriptive analysis of these resumes revealed several findings that informed the design of our fictitious resumes. First, we found that workers do not "shroud" their unemployment spells: approximately 75% of resumes from workers who were currently unemployed listed both the year *and month* of when they last worked. Second, among the currently unemployed, roughly 95% of resumes do not provide any discernable explanation for the gap (e.g., obtained a license or certificate, engaged in community service, worked as a volunteer, training, etc); moreover, this percentage did not vary by gender or by the length of the unemployment spell. Given this, we designed all of our resumes to contain both the year and the month of last employment, and we did not purposefully try to provide any information that could be seen as accounting for the gap.

In total, we created ten resume templates that were based on the most frequent resume formats observed in this database. From this set of templates, four templates (one per fictitious resume) were selected according the following rule. If an RA applied to a given MSA and job category combination before, she reused the templates from that application. Otherwise, she randomly drew four new templates from the ten possible templates, drawing without replacement to ensure that no two resumes being sent to a given job share the same resume template. There were 6 more steps in designing a fictitious resume:

1. We decided whether each resume would be male or female. For two of the job categories (Customer Service and Sales), we sent two female and two male resumes. For Administrative/Clerical jobs, we

[13] We designed the experiment this way in order to help identify the interaction between market tightness and duration dependence. The 20 oversampled MSAs were the following: Washington, D.C.; Miami, FL; Boston, MA; Detroit, MI; Riverside, CA; Minneapolis, MN; Sacramento, CA; Las Vegas, NV; Oklahoma City, OK; Honolulu, HI; Tulsa, OK; Omaha, NE; Bakersfield, CA; McAllen, TX; Stockton, CA; Des Moines, IA; Madison, WI; Lancaster, PA; Modesto, CA.

sent four female resumes. This decision the protocol of Bertrand and Mullainathan (2004).

2. We randomly generated a name for the resume. The bank of names were chosen based on common frequency census data and were chosen to be minimally informative about the race of the applicant.

3. We chose the home address, phone number, and e-mail address. In general, we constructed addresses based on addresses that were listed in the real resumes in the database of actual resumes described above, and we modified these addresses by choosing a non-existent street number. We purchased 400 unique local phone numbers (4 per MSA) and we created roughly 1,600 unique e-mail addresses.[14] Both the phone numbers and e-mail addresses allowed us to track callbacks on an ongoing basis.[15]

4. The next step was updating the fictitious resume's job history, educational history and the objective summary to match the job that we applied to. Work histories were constructed from the sample of real resumes that we self-collected. For instance, if the job was for an Administrative Assistant position, we identified a resume with experience as an Administrative/Executive Assistant and used this to construct the work history. For resumes that were sent to jobs in the same city, we never shared work histories. In terms of education, we searched for large, local degree-granting institutions. Finally, we verified that there was not a real individual with a similar background on any of the major social network and job network websites (e.g., Facebook and LinkedIn).

5. We defined a measure of "quality" for each resume. A "low quality" resume is one that satisfies the minimum qualifications required for the job. These resumes were always assigned the minimum qualifications listed in the job posting (in terms of experience and education). A "high quality" resume had qualifications that exceeded these minimum requirements. Specifically, these resumes had a couple of extra years of experience and an extra "level" of education. For instance, if the job states high school completion as a requirement, we would list associate degree or if the job requires an associate degree, we would list a bachelor degree. For jobs that required a bachelor degree, we did not bump up the education level for the high quality resumes. For each job that we applied to, two resumes were low quality and two were high quality. This means we either had a set of {one high quality male, one high quality female, one low quality male, one low quality female} or we had a set of two high-quality female resumes and two low-quality female resumes (depending on the gender ratio that the job category calls for).

6. The final and most important step was to randomize employment status and the length of the current unemployment spell, which is described in more detail below when introducing the empirical model.

[14] As explained above, we re-use resumes when we can. When we do that, we re-use both the name and e-mail address.

[15] We forwarded each of the 400 phone numbers to one of four voice mailboxes, ensuring that each voice mailbox received messages from only one phone number in each city.

4.1 Measuring Salience of Resume Characteristics

Our field experiment assumes that the information on the resume regarding a job applicant's employment status and unemployment spell length is salient to employers. To test this assumption, we designed and conducted a web-based survey. A link to the survey was e-mailed to 365 MBA students at the University of Chicago Booth School of Business. A total of 91 students completed the survey. This section summarizes the design and results of the survey; more details on the survey can be found in the Appendix.

The survey took place in three stages. First, respondents were asked to read a hypothetical job posting and to consider two resumes for the job opening (see Appendix Figure A1). The respondent was then asked to select one of the two resumes to contact for an in-person job interview. Second, once the respondent made a selection, she was then asked to recall specific information on the resume such as total work experience, tenure at last job, level of education, current employment status, and the length of unemployment spell (Appendix Figure A2).[16] We use these responses to evaluate the extent to which the various characteristics on the resume are salient to subjects. Finally, we asked respondents several demographic questions; in particular, whether they had prior experience evaluating resumes (Appendix Figure A3).

To empirically measure salience, we rely on two proxies: (1) the fraction of time the respondents correctly recall the information, and (2) the correlation between the reported and true answers. As shown in Appendix Table A1, across both of our measures of salience, respondents are able to recall information about applicant's employment status and length of unemployment spell about as well as they are able to recall information about other resume characteristics (such as education, total work experience, and tenure at last job). When we restrict the sample to respondents who report having "high experience" reviewing resumes in column (2), we find that the respondents are even more likely to correctly identify employment status and the length of unemployment spell. These results are consistent with our assumption that the employment status and the length of unemployment spell are salient features of the fictitious resumes.[17]

4.2 Measuring Callbacks

We track callbacks from employers by matching voice or email messages to resumes. We record the date of the callback, and we follow Bertrand and Mullainathan (2004) by defining a callback as a message from an employer explicitly asking to set up an interview. The callbacks were coded independently by two Research Assistants (RAs) who were not otherwise involved in the project, and the two RAs agreed virtually all of the time. In Table 4, we report results which use an alternative definition of a callback based on whether the employer left any voice message at all, even if the message simply asked for more information. In ongoing

[16] Importantly, the survey was designed so that the respondent was unable to view the resumes again after making her selection.

[17] One important caveat to our interpretation is that the survey respondents are not a representative sample of the individuals evaluating resumes in our field experiment. Nonetheless, we find it reassuring that our results persist in the subsample of MBA students with high levels of experience actually reviewing resumes.

work, we are using the date and time of the callback to see if employers reveal preferences over the four resumes sent to each job.

4.3 Empirical Models

In terms of the experimental design, we have created two treatment groups:

- **Treatment 1:** Individuals are randomly assigned to employment status "Employed" with probability 0.25. In this case, the resume indicates that the person is still working at her current job. Let $E_{i,c}$ denote an indicator variable that equals 1 if individual i in city c is employed and 0 otherwise.

- **Treatment 2:** Individuals that are not assigned to the Employed treatment are unemployed and randomly assigned an (integer) unemployment duration or "gap" (in months) according to a discrete uniform distribution on the interval [1, 36]. Let $\log(d_{i,c})$ denote the log of the (randomly assigned) unemployment spell for individual i in city c. Employed individuals are assigned $\log(d_{i,c}) = 0$. Let $y_{i,c}$ be a callback indicator that equals 1 if individual i in city c receives a callback for an interview.

In our analysis of the experimental data below, we estimate the following linear probability model that includes, for efficiency gains, individual, job, and city characteristics $X_{i,c}$:

$$y_{i,c} = \beta_0 + \beta_1 E_{i,c} + \beta_2 \log(d_{i,c}) + X_{i,c}\Gamma + \varepsilon_{i,c} \tag{12}$$

Given our randomized design, the coefficients β_1 and β_2 provide unbiased estimates of the mean impact of being employed versus unemployed and the mean impact of changes in unemployment duration (conditional on being unemployed). Since the effect may differ in magnitude across different unemployment durations, we also report alternative functional forms. In particular, we examine the data nonparametrically by using moving averages and plot callback rates as a function of unemployment durations. We also examine the robustness of our results to using alternative specifications, such as a probit model.[18]

To examine how duration dependence varies with local labor market conditions, we pursue two complementary approaches. First, we estimate fixed effects and (correlated) random effects models of the following form:

$$y_{i,c} = \alpha_1 E_{i,c} + \delta^c + \gamma^c \log(d_{i,c}) + X_{i,c}\Gamma + \varepsilon_{i,c} \tag{13}$$

In the fixed effects model, δ^c is a city fixed effect and γ^c is a city-specific estimate of the effect of unemployment duration on callbacks. In the random effects model, δ^c is a city random effect and γ^c is a city-specific random coefficient on unemployment duration. This specification is directly motivated by our mechanical

[18] When we consider interactions in non-linear models such as the probit model, we estimate the interaction effects using Ai and Norton (2003).

model which indicates that there is a one-to-one relationship between the intercept δ^c (i.e., the callback rate for a newly unemployed individual) and the level of market tightness. Therefore, the covariance between δ^c and γ^c (i.e., $E[(\delta^c - \bar{\delta}^c)\gamma^c]$) indicates the extent to which duration dependence varies with market tightness. For the fixed effects model, we prove in the Appendix that an unbiased estimate of this covariance is given by the following expression:

$$E[(\delta^c - \bar{\delta}^c)\gamma^c] = \frac{1}{C}\sum_{c=1}^{C}\hat{\delta}^c\hat{\gamma}^c + \frac{1}{C}\sum_{c=1}^{C}\frac{\hat{\sigma}_c^2}{N^c}\frac{E_c[\log(d)]}{Var(\log(d))} \tag{14}$$

where C is the total number of cities in the sample, $\hat{\delta}^c$ and $\hat{\gamma}^c$ are the estimated city fixed effects and city-specific estimates of the effect of unemployment duration, $\hat{\sigma}_c^2$ is the estimated city-specific residual variance and N^c is the number of observations in the city. The second term in equation (14) represents a bias correction to account for the negative mechanical correlation between the city-specific estimates $\hat{\delta}^c$ and $\hat{\gamma}^c$. Intuitively, the slope and intercept estimates in an OLS regression are correlated, so in order to obtain an unbiased estimator of the covariance of the estimated intercept and slope parameters across cities, we need to adjust for this "mechanical" bias using equation (14) above. We then convert the covariance estimate to a correlation by dividing by the standard deviation of the estimates city-specific interaction terms and the estimates of the city fixed effects.[19] For the random effects model, the covariance is estimated by specifying that δ^c and γ^c are jointly normally distributed and estimating the variance-covariance parameters of the joint normal distribution.

Our second approach to estimating how duration dependence varies with market tightness is to estimate the following linear probability model:

$$y_{i,c} = \beta_0' + \beta_1' E_{i,c} + \beta_2' \log(d_{i,c}) + \beta_3' \log(d_{i,c}) \times u_c + \beta_4' u_c + X_{i,c}\Gamma' + \varepsilon_{i,c} \tag{15}$$

This specification includes an interaction between log duration and proxies for market tightness (u_c). We explore several alternative proxies in the specifications below, including the city unemployment rate and city-level estimates of the ratio of vacancies to unemployed population.

5 Experimental Results

Our final sample includes 12,041 resumes submitted to 3,034 jobs.[20] Of these 12,041 resumes, 9,222 of the resumes had (ongoing) unemployment spells of at least one month, with the remaining 2,819 resumes

[19] We adjust the estimates of standard deviation of the interaction terms and city fixed effects by a "shrinkage factor" based on an estimate of the residual variance.

[20] We needed to submit to more than $3,000$ jobs to reach $12,000$ resumes beacuse there were several instances where the job posting was taken down before we were able to submit all four the resumes prepared for the job. This happened on occassion because we waited one day between each resume submission.

conveying that the worker was currently employed.[21] Table 1 reports descriptive statistics for the sample. About 12 percent of our resumes elicit some response by employers. However, not all of these are callbacks for interviews. About one-third of total callbacks were classified as callbacks for interviews, for a total callback rate of about 4.5 percent. In terms of demographics, roughly two-thirds of our resumes are female, and most of our resumes show relatively little experience. Mean experience is 5 years with a max of 15 years of experience. Compared to the types of jobs that individuals are applying to, the resume sample is fairly educated: one-third of the respondents have college degrees. This is primarily due to our strategy of sending out both resumes that just match the minimum requirements and resumes that are of higher quality. Due the randomized design of the field experiment, there is balance across all covariates (across employed/unemployed and across the distribution of unemployment durations), as shown in Table 2.

5.1 Estimating Duration Dependence

Before turning to regression results, we begin with a simple plot of the average callback rate. Using the sample of unemployed individuals (N = 9,222), Figure 1 reports the relationship between unemployment duration and the callback rate. The figure shows a clear negative relationship, with the steepest decline coming in the early months. Figure 2 shows a similar pattern using the ratio of the callback rate in each bin divided by the callback rate in the first bin (i.e., the lowest unemployment durations). This figure corresponds more closely to the function $r(d;x)$ defined above.

The evidence from regressions confirms this graphical analysis. Table 3 reports OLS regression results estimating equation (12). Column (1) reports results for the entire sample. The results show that longer unemployment durations are associated with lower callback rates. A 1 log point change in unemployment duration is associated with a strongly statistically significant 1.1 percentage point decline in the callback probability, from a mean of 4.5 percentage points. This corresponds to a 24 percent decline in the callback rate. Surprisingly, the results in the second row suggest that employed applicants are actually *less* likely to receive callbacks (relative to newly unemployed individuals). In discussions with human resource professionals, we have learned that some employers prefer to hire a short-term unemployed worker over a worker who is currently employed, which is consistent with these results. This could be due to the fact that on the margin, it is harder to recruit a worker who is currently employed relative to a job seeker who is out of a job. Columns (2) through (4) reports similar results which use alternative functional forms for unemployment duration. Column (2) reports a linear specification rather than a log specification, and column (3) reports results using a quadratic in unemployment duration. Finally, column (4) reports results from a spline regression which allows for a structural break in the effect of unemployment duration at 8

[21] The share of resumes currently employed is 23.4%, which is less than 25% (which was the experimental protocol). The discrepency comes from roughly 600 resumes where the employment status was randomized slightly differently (in particular, employment was chosen with $p = 1/37$ rather than $p = 0.25$). All results are robust to dropping these observations.

months (the location of the structural break is determined through auxiliary regressions which choose the location of the break to maximize the R^2). The results in this column suggest that callbacks are sharply decreasing for the first 8 months and nearly flat after that.

As expected given the randomized nature of the data, the results in this table are very similar all controls are excluded, as shown in Table 4. This table also shows that the main results are very similar using an alternative definition of callbacks and to using a probit specification instead of a linear probability model, which addresses concerns arising from the low average callback rate in the experiment. In all cases, the results from these alternative specifications are very similar to our preferred specification. Interestingly, most of the estimates of coefficients on control variables turn out not to be statistically significant (Appendix Table A2); however, we do find that the "high quality" resume indicator significantly predicts callbacks. Appendix Table A2 also shows that when we drop city fixed effects and include the city unemployment rate as an additional control instead, we find that the unemployment rate strongly predicts callbacks. This is consistent with a large literature which finds that aggregate labor market variables predict individual unemployment druation to a greater extent than individual-level covariates.

5.2 Duration Dependence and Labor Market Conditions

The results presented so far address the question of whether callback rates vary with unemployment duration. However, they do not address the question of how this relationship varies with market tightness. To explore this question, we first provide graphical evidence. Figures 3 and 4 show plots analogous to Figures 1 and 2, but they split the experimental sample depending on whether the local unemployment rate is above or below 8.8% (the median unemployment rate across cities in the experiment). As before, we first plot the raw callback rates (Figure 3) and then we plot the relative callback rate, the callback rate divided by average callback rate in the first "bin" (Figure 4), which corresponds to the function $r(d;x)$ introduced in Section 2. As discussed in Section 3, the screening model implies that $\frac{\partial r(d;x)}{\partial x} < 0$ for all d (except $d = 0$), whereas the ranking model implies that $\frac{\partial r(d;x)}{\partial x} > 0$, and the human capital depreciation model implies that $\frac{\partial r(d;x)}{\partial x} = 0$. Figure 4 therefore allows us to discern among these models and it finds evidence in favor of the screening model: the relative callback rates are always lower in markets with lower unemployment. These patterns are robust to other proxies for labor market tightness. For example, Figures 5 and 6 show similar results when the sample is split based on median ratio of vacancies to unemployment (V/U ratio), as measured by the Help Wanted Online Index, while Figures 7 and 8 show similar results when the sample is split based on whether the unemployment rate increased by more than 3.6 percentage points between 2008 and 2011 (the median percentage point increase across the cities in the experiment).

The regression evidence confirms the visual evidence from these figures. We begin with a simple test of whether there is heterogeneity in duration dependence across labor markets. Table 5 reports results which test whether the effect of unemployment duration on callbacks is the same across all metropolitan areas based on our fixed effects estimates from equation (13). We interact a full set of metropolitan area fixed effects with unemployment duration and conduct an F-test of equality across all of the estimated coefficients for these interaction terms. Based on the results in column (1), we can confidently reject the null hypothesis that the effect of unemployment duration is the same across all metropolitan areas, as the p-value of this test is strongly statistically significant ($p < 0.001$). To test exactly how the effect of unemployment duration varies with market tightness, we construct an estimate of the correlation between the estimates of the city-specific interaction terms and the city fixed effects estimated in equation (13). Consistent with the results in Figures 4 through 7, we estimate a statistically significant negative correlation between δ^c and γ^c; i.e., $\widehat{corr(\delta^c, \gamma^c)} = -0.805$; s.e. $= 0.191$.[22] Under the assumption that the city fixed effects are valid proxies for market tightness, these results imply that duration dependence is stronger (i.e., more negative) in tight labor markets, consistent with the predictions of the employer screening model.

The remaining columns in Table 5 repeat this same exercise, reporting the covariance in equation (14), but replacing $\log(d)$ with other covariates in $X_{i,c}$.[23] Columns (2) through (4) test for similar hetereogeneous effects across labor markets for the effect of gender, education, and "skill" (as measured by whether or not resume was "high quality"), and we find no significant evidence that the effect of any of these covariates varies across cities. Columns (5) and (6) show that the callback rate of customer service jobs and sales jobs (relative to admin/clerical jobs) varies strongly across cities. However, these effects are correlated with the average callback rate within the experiment to a much lesser extent, and the sign is not consistent across the two types of jobs. In particular, cities with higher average callback rates are not relatively more likely to call back applicants to customer service jobs or sales jobs, even though these jobs have higher average callback rates. We interpret this as evidence against a "mechanical" interpretation of our results in column (1): specifically, these results are inconsistent with low average callback rates in a city simply being associated with attenuating the effect of all covariates. In this case, one would expect that cities with higher average callback rates to also have higher callback rates for customer service jobs and sales jobs relative to admin/clerical jobs, and we do not find evidence that this is the case.

Table 6 reports similar results based on estimating a correlated random coefficients model, where the regression model in equation (13) above is given an alternative interpretation: specfically, δ^c is assumed to be a city-specific random effect and γ^c is a city-specific random coefficient on unemployment duration. The random coefficients are allowed to be flexibly correlated and assumed to be jointly normally distributed across cities. As in Table 5, we report whether we find evidence that the random coefficients on unemployment duration are statistically significantly different, and we also report estimates of the correlation

[22] See the Appendix for details on constructing standard errors for inference.

[23] When we replace $\log(d)$ with one of the covariates in $X_{i,c}$, we place $\log(d)$ in $X_{i,c}$ vector.

between the random coefficient and the city-specific random effect. The results are qualitatively similar: for unemployment duration we estimate a significant negative correlation ($\widehat{corr(\delta^c, \gamma^c)} = -0.905$; s.e. $= 0.057$), which implies that cities with higher average callback rates within the experiment have stronger duration dependence.

Our final test of how duration dependence varies with labor market conditions uses metropolitan area unemployment rates to construct various (observable) proxies for market tightness. These results are reported in Table 7, which reports OLS regression results estimating equation (15). Using several alternative functional forms for the unemployment rate and the vacancy-to-unemployed ratio, we consistently estimate that the effect of unemployment duration is stronger when the local labor market is relatively tight (i.e., either the v-u ratio is relatively high or the unemployment rate is relatively low). Morever, these patterns are robust to including both city fixed effects as well as a wide range of interactions between unempoyment duration and city characteristics (e.g., population, GDP, income, fraction of population in poverty, fraction of population non-white). Overall, these results are consistent with the results in Tables 5 and 6; across all of these tables, we interpret the results as consistent with the screening model discussed above, as the results suggest the magnitude of negative duration dependence increases with market tightness.

Finally, Table 8 reports several additional results to explore variation across different subsamples. The point estimates suggest somewhat larger duration dependence estimates for women (as compared to men) and for workers applying to sales jobs (as compared to customer service jobs). However, none of these differences across sub-samples are statistically significant at conventional levels.

6 Conclusion

This paper discusses results from a field experiment studying duration dependence. Our results suggest that the likelihood of receiving a callback declines with unemployment duration. This effect is especially pronounced in the first 8 months after becoming unemployed. Our estimates suggest that this effect is quantitatively important, and, additionally, our results suggest that duration dependence is stronger when jobs are relatively abundant.

Our conceptual framework shows that these results are consistent with a model where employers statistically discriminate against workers with longer unemployment durations, and we emphasize that our results are not easily generated by a model of human capital depreciation when the rate of human capital depreciation is the same across labor markets. The results are also not consistent with a model of duration dependence based on a simple model of employer ranking (Blanchard and Diamond 1994). Therefore, we conclude that our results are consistent with employer screening playing an important role in generating duration dependence, although we emphasize that we do not rule out the existence of these other mechanisms.

The results in our experiment suggest several additional areas for future research. Empirically, we think it is important to examine whether our results generalize to the economy more broadly. Future audit studies might explore whether duration dependence varies across occupations or over time. Such studies might also expand the coverage to a broader segment of the economy where online job search is less prevalent. Theoretically, there are close connections between the screening model that is supported by our data and the literature on rational herding that bear exploring.[24] Our screening model assumes that employers meet workers sequentially. Employers then use the information about prior actions of other firms, embedded in the duration of unemployment, to learn about worker productivity. However, they do not observe the private signals received by the other firms. This structure is very similar to the structure of the standard rational herding model.[25]

In ongoing work, we are exploiting the insights of the rational herding literature to study the policy implications of our findings. In herding models, if the public belief is sufficiently strong, agents will ignore their private information and end up in informational cascades where no further learning takes place. From the standpoint of social welfare, such cascades are sub-optimal, since they imply that information does not aggregate. We study the conditions that give rise to informational cascades in our setting and ask whether a social planner would want to distort firms' hiring decisions. We also explore the implications of the screening model for the optimal design of Unemployment Insurance (UI) benefits, in order to examine the optimal time path of UI benefits in an environment with asymmetric information and social learning.[26]

[24] For a review of herding models, see Bikhchandani et al (1998).

[25] The main difference is an asymmetry in the learning process that is present in our model: once a worker is hired by a firm, the public learning process stops.

[26] For related studies on the optimal design of UI benefits, see Shavell and Weiss (1979), Shimer and Werning (2006, 2008) and Pavoni 2009.

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Appendix A: Properties of Incomplete and Complete Spells

We distinguish between two duration concepts – "incomplete" and "completed spells". That is, we define the random variable D as the length of an unemployed worker's *incomplete spell*. This random variable is to be distinguished from S, the length of a *completed* spell of unemployment. Clearly, realizations s and d of these random variables satisfy $s \geq d$. Denote the conditional distribution and density functions of completed spells at time t as $G_y^S(S)$ and $g_y^S(S)$. For incomplete spells, we will use the notation $G_y^D(D)$ and $g_y^D(D)$.

There is a connection between the distribution of completed and ongoing spells in the steady state. Salant (1977) demonstrates that in the steady-state, the density of on-going spells is connected to the distribution of completed spells as follows:

$$g_y^D(\tau) = \frac{1 - G_y^S(\tau)}{E[S]} \text{ for } \tau \geq 0 \tag{16}$$

We can also define the escape rate conditional on spell length τ as the rate at which individuals leave the population of unemployed at τ. This can be expressed as a function of the density and distribution of completed spells:

$$h_y(\tau) = \frac{g_y^S(\tau)}{1 - G_y^S(\tau)} = -\frac{d}{dS} \ln \left[\int_\tau^\infty g_y^S(\mu) d\mu \right]$$

Rewriting this, we get

$$\begin{aligned} \frac{d}{d\tau} \ln \left[1 - G_y^S(\tau)\right] + h_y(\tau) &= 0 \\ \frac{d}{d\tau} \left[1 - G_y^S(\tau)\right] + (1 - G_y^S(\tau)) h_y(\tau) &= 0 \end{aligned}$$

This is a first-order differential equation with a variable coefficient. The general solution of this equation, the distribution function for completed spells, satisfies:

$$\Pr(S > \tau) = 1 - G_y^S(\tau) = \exp\left(-\int_0^\tau h_y(\mu) d\mu\right) \tag{17}$$

This is simply the probability that the individual has not been hired up to S. And, this implies the following density:

$$g_y^S(\tau) = h_y(\tau) \exp\left(-\int_0^\tau h_y(\mu) d\mu\right) \tag{18}$$

Appendix B: Proof of Proposition 2 and Composition Bias

Proof of Proposition 2

Recall that $r(d;x)$ and $\theta(d;x)$ are defined as follows:

$$\begin{aligned} r(d;x) &= \frac{h(d;x)}{h(0;x)} \\ \theta(d;x) &= \frac{\pi(d;x)}{1 - \pi(d;x)} \end{aligned}$$

From the expressions above, it is clear that $r(\pi(d;x);x)$ increases in $\pi(d;x)$. Therefore, to establish the proposition, we need to establish the relationship between $\pi(d;x)$ and x. It is sufficient to sign the relationship between $\theta(d,x)$ and x.

First, note that $\theta(0,x) = \theta(0,x')$ for $x \neq x'$. This follows from the assumption that $\pi(0;x) = \pi_0$. Next, from definition of $\theta(d, x)$, it is simple to show that

$$\frac{\partial\theta\left(d;x\right)}{\partial d}=-m(x)\times\theta(d;x)\times\left(l_h\left(\pi(d;x)\right)-l_l\left(\pi(d;x)\right)\right)$$

Since $m'(x) > 0$ and $l_h(\pi_0) > l_l(\pi_0)$, $\left|\frac{\partial\theta(0;x')}{\partial d}\right| > \left|\frac{\partial\theta(0;x)}{\partial d}\right|$. This establishes that for small $\varepsilon > 0$, $x' > x$ $\Rightarrow \theta(\varepsilon; x) > \theta(\varepsilon; x')$. In other words, the share of high types is initially lower in tighter markets. This is intuitive as high types get selected out of unemployment relatively faster.

To complete the proof, we need to show that $\forall\ d > 0$, $x' > x \Rightarrow \theta(d; x) > \theta(d; x')$. We will proceed by contradiction. Suppose that this were not true. Then since $\theta(d; x')$ initially lies below $\theta(d; x)$, $\exists\ d^* > 0$ such that $\theta(d^*; x) = \theta(d^*; x')$ and $\theta(d^*+\varepsilon; x) < \theta(d^*+\varepsilon; x')$. By the definition of d^*, $\left|\frac{\partial\theta(d^*;x')}{\partial d}\right| > \left|\frac{\partial\theta(d^*;x)}{\partial d}\right|$. However, this would imply that $\theta(d^*+\varepsilon; x) > \theta(d^*+\varepsilon; x')$, a contradiction. Thus, it follows that a single crossing property has to hold for $\theta(d; x)$ and $\theta(d; x')$. And, since $\theta(0; x) = \theta(0; x')$, we have that $\theta(d; x) > \theta(d; x')$ and consequently $r(d; x) > r(d; x')$ for all $d > 0$.

Composition Bias

Recall equation (10)

$$c\left(d\right)\equiv\int c\left(\phi\right)\frac{d\Phi\left(\phi|d\right)}{d\phi}d\phi$$

Differentiating with respect to d yields:

$$c'\left(d\right)\equiv\int c\left(\phi\right)\frac{d^2\Phi\left(\phi|d\right)}{d\phi dd}d\phi \tag{19}$$

Note that

$$\Phi(\phi|d)=\pi\left(d\right)\Phi_h\left(\phi|d\right)+\left(1-\pi\left(d\right)\right)\Phi_l\left(\phi|d\right)$$

where $\pi\left(d\right)=\int\pi\left(d,\phi\right)d\Phi\left(\phi|d\right)$. Hence,

$$\frac{d\Phi\left(\phi|d\right)}{d\phi}=\pi\left(d\right)\frac{d\Phi_h\left(\phi\right)}{d\phi}+\left(1-\pi\left(d\right)\right)\frac{d\Phi_l\left(\phi\right)}{d\phi}$$

Thus,

$$\frac{d^2\Phi(\phi|d)}{d\phi dd}=\frac{d\pi\left(d\right)}{dd}\left(\frac{d\Phi_h\left(\phi\right)}{d\phi}-\frac{d\Phi_l\left(\phi\right)}{d\phi}\right)$$

Plugging this back into (19), we get

$$\begin{aligned} c'\left(d\right) &= \frac{d\pi\left(d\right)}{dd}\left[\int c\left(\phi\right)d\Phi_h\left(\phi\right)-\int c\left(\phi\right)d\Phi_l\left(\phi\right)\right] \\ c'\left(d\right) &= \frac{d\pi\left(d\right)}{dd}\left[E_h\left[c\left(\phi\right)\right]-E_l\left[c\left(\phi\right)\right]\right] \end{aligned}$$

By the proposition above, $\frac{d\pi(d)}{dd} < 0$. By first-order stochastic dominance and the fact that $c'(\phi) > 0$, the expression inside the brackets is positive. This establishes that $c'(d) < 0$.

Appendix C: Model of Employer-Screening

In this section, we show that a model of search frictions with employer screening will satisfy the requirements of the mechanical model of Section 2. We assume that (i) firms open vacancies subject to a zero-profit condition; (ii) workers and firms meet according to a reduced-form meeting function; (iii) upon meeting a worker, firms receive a signal ϕ on the worker's productivity and decide whether or not to interview the worker at a cost; (iv) some applicants are called back for an interview (a costly screen) where the firm obtains additional information in the form of signal z. If the expected profit of firms exceeds the required wage then individuals are offered jobs. The expected profit depends on the wage and we need to make an assumption on wage setting. In the simplest version of the model, we assume that wages offered by all firms will equal the outside opportunity of workers which is denoted by b. As we show below, this model maps into the mechanical structure discussed in Section 2 since (i) a matching function of the required type is assumed to govern the rate at which firms and workers meet; (ii) hiring rates conditional on matching decline with $\pi(d;\phi)$ and (iii) high type applicants are more likely to be hired conditional on matching than low type applicants.

While we discuss the model for the simplest possible form of wage setting with w=b, it is also possible to allow for more general forms of wage setting. For instance, we could assume that wages are set to be equal to b plus a fixed share in the expected surplus from a given job. For instance, we might expect that $w = b + \lambda\left(E[y|\pi(d;\phi), z] - b\right)$ where $\lambda \in [0,1]$. In this case, firms would invite fewer individuals for interviews, since the expected surplus $(1-\lambda)\left(E[y|\pi(d;\phi), z] - b\right)$ going to the firm would be smaller and thus the interview costs would be covered in fewer cases. Therefore a model with surplus sharing will have inefficiently low interview rates. Notwithstanding the fact that the welfare implications would differ under this form of wage setting, it is possible to show that the requirements (ii) and (iii) on hiring rates conditional on matching will still be satisfied and that therefore the predictions of the mechanical model still apply.

Model Setup

Population Dynamics and Workers

We maintain the assumptions on matching and on the life-cycle of individuals that we have described in Section 2.1. In addition, we assume that workers receive benefits b when unemployed and we assume that $l < b < h$. These benefits are constant with respect to productivity and they determine the outside option of unemployed workers.[27][28]

Firms / Vacancies

There is no fixed cost to opening a vacancy, but each period that a vacancy is open a flow cost c needs to be paid. There is free-entry.[29] Filling or keeping open the current vacancy does not affect the ability to open future vacancies, nor does it have any impact on the costs and benefits associated with any future vacancies. Thus, firms fill vacancies as soon as they find a match such that the expected profit of the vacancy is positive. Firms care only about the productive type of a worker.

We assume that firms offer a wage of b. Offering b represents a Nash equilibrium because we assume that applicants accept job offers with a pay-off equal to the expected pay-off from remaining unemployed. If all firms offer b, then the pay-off from remaining unemployed is also b and workers accept these job offers. Further, no firm has an incentive to make a higher offer.

Once a vacancy is filled, it generates an output stream y until the individual retires. Since the wage b exceeds the productivity of the less able type, firms have an interest in hiring high productivity workers.[30]

[27] Type fully predicts productivity in this model. Another formulation would allow type to determine productivity probabilistically. See Gonzales and Shi (2009) for learning model.

[28] Krueger and Mueller (2011) find empirical support for the observation that the reservation wage does not vary with unemployment durations. See also Kasper (1967) as well as Feldstein and Poterba (1984).

[29] The matching technology generates a rate of matching for a given vacancy that is independent of the number of vacancies a firm opens. Further, the flow cost of maintaining a vacancy is likewise independent of the number of vacancies a firm opens. These constant returns to scale assumptions imply that the size of firms is indeterminate. We will therefore treat each vacancy as a firm in its own right.

[30] We are assuming here that firms hold onto non-profitable workers ($b > l$) forever. In other words, ex ante profits are driven to 0 in eqm but ex post there will be workers on which firms make losses. The assumption that relationships are maintained regardless of their productivity is clearly ad-hoc. We have in mind that firms incur losses on workers that are not productive and that they will therefore strive to avoid hiring low-productivity workers.

We assume that firms hold rational expectations. Thus, firms beliefs about the probability that a worker of duration d and signals ϕ and z is of high type will equal the distribution that arises in equilibrium.

The Signals

We assume the same matching process as described above. Upon meeting, firms observe how long an individual has been unemployed (a draw d from the random variable D). The firm also observes an additional signal ϕ on the productivity of the worker - which we interpret to reflect the unobserved characteristics of the CV as described above. Given this additional signal, the firm can decide whether or not to interview the worker at a fixed cost ξ. If the firm chooses to interview the worker, it then receives another signal z on the worker type y. Without loss of generality, we assume that this signal represents new information about the worker type that is orthogonal to the prior $\pi(d,\phi)$ that firms hold about worker productivity when they make call-back decisions. For simplicity, we assume that distribution of the scalar signal only depends on the type y and write the distribution function for z conditional on productivity y as $F_y^z(.)$. Assume further that $F_l^z(.)$ and $F_h^z(.)$ satisfy a monotone likelihood ratio property. This captures the idea that z is informative about the underlying type.

Assumption 4 *[Monotone Likelihood Ratio] $F_l^z(.)$ and $F_h^z(.)$ satisfy the MLR property so that $\frac{f_h^z(k)}{f_l^z(k)}$ (strictly)*[31] *increases in k.*[32]

Implied in this assumption is first-order stochastic dominance ($F_l^z(k) - F_h^z(k) > 0$ for all k). Continuous distributions satisfying MLR include the exponential family and the normal distribution. This assumption implies that firms pursue a "reservation signal policy" for both call-backs and hiring decisions. When a firm has observed $(Z = z, \phi, D = d)$, the firm decides whether or not to hire the worker.

Equilibrium

We begin by defining an equilibrium and the pay-off functions for firms in this economy. Denote by J_u the value of an open vacancy, by J_m the value of matching to an applicant before deciding on whether to interview this applicant and by J_I the value of having interviewed an applicant with duration d and signal z. Equation (20) says that the return on an unfilled vacancy depends on the flow cost of each vacancy, market tightness x, and the joint distribution of duration and signals $G^D(d,\phi)$ in the population. At rate $m_v(x)$, a vacancy meets with a worker, who is drawn from the joint distribution of incomplete spells (see Appendix A) and signals ϕ: $G^D(d,\phi)$:

$$rJ_u = -c + m_v(x)\int_d\int_\phi J_m(d,\phi)dG^D(d,\phi) \tag{20}$$

The value of a match depends on the signal ϕ drawn for this match and the duration d of the applicant. This value equals the maximum of the value of keeping the vacancy open and the expected value of interviewing the worker net of interview cost ξ[33]:

$$J_m(d,\phi) = \max\left\{J_u, \int_z J_I(d,z,\phi)dF(z|d,\phi) - \xi\right\} \tag{21}$$

The distribution $F(z|d,\phi)$ depends only on the prior $\pi(d,\phi)$:

$$\begin{aligned} F(z|d,\phi) &= \pi(d,\phi)F_h(z) + (1-\pi(d,\phi))F_l(z) \\ &= F(z|\pi(d,\phi)) \end{aligned}$$

[31] By assuming that the likelihood ratio strictly increases, we ensure that as z increases, the posterior probability of being the high type will approach 1.

[32] WLOG, because we can always reassign the support of z.

[33] Here we assume that to hire an applicant, an interview is always necessary. This assumption can be justified by the fact that workers in our experiment are always required to submit a CV to a vacancy and rarely are they offered a job at this stage.

Upon interviewing the candidate, the firm updates its beliefs and obtains the value $J_I(d, z, \phi)$. $J_I(d, z, \phi)$ is the maximum of the expected present discounted value of profits from hiring this interviewee and the value of rejecting her and keeping the vacancy open. The expected flow return to a filled vacancy is the expected productivity conditional on the observed signals net of the wage (b). Expected productivity depends on the prior $\pi(d)$ as well as the signals ϕ and the signal z.[34] With rate δ, individuals retire and the match is consequently dissolved. Thus, the flow return from a filled vacancy is discounted using both the interest rate r and the retirement rate δ.

$$J_I(d,z,\phi) = \max\left\{J_u, \frac{1}{r+\delta}\left(E\left[y|z,\pi(d,\phi)\right]-b\right)\right\} \tag{22}$$

$$= J_I(z,\pi(d,\phi)) \tag{23}$$

The rational expectations equilibrium consists of $x = V/U$, an interview rule, a hiring rule, and a joint distribution $G^D(d, \phi, z, y)$ that satisfy:

1. Firms interview workers if and only if $\int_z J_I(z,\pi(d,\phi))\,dF(z|\pi(d,\phi)) \geq \xi$.
2. Firms hire workers if and only if $E[y|\phi,d,z] \geq b$.
3. Given x and implied $m_v(x)$, vacancies do not earn profits in expectation: $J_u = 0$.
4. Beliefs about the distribution of productivity $\pi(\phi, d)$ equal the equilibrium realized distribution $\pi(\phi, d)$.

Characterizing Firm's Behavior

It is easy to show the hiring rates in this model satisfy the two requirements of the mechanical model. We will show this for a given ϕ. These properties of the hiring rates are maintained when we aggregate across ϕ.

1. **Conditional on π, hiring rate for high types exceeds that for low types.**
   For a given $\pi(d, \phi)$, the interview rate is the same for high or low types. However, the expected productivity $E[y|\phi, d, z] = E[y|\pi(\phi, d), z]$ increases in z. Since $F_h(z)$ FOSD $F_l(z)$, high types are more likely to receive high signals than low types. The equilibrium condition 2 on hiring is therefore satisfied more often for high rather than low types.

2. **Conditional on type, hiring rates increase in π**
   By FOSD, we have that $F(z|\pi)$ increases in π and that $J_I(z, \pi)$.increases in z and π. Therefore, $\int_z J_I(z,\pi(d,\phi))\,dF(z|\pi(d,\phi))$ increases in π. Thus, call-back rates for any type of worker (high or low) increase in π, satisfying the conditions of corrolary 3. Furthermore, we have again that $E[y|\pi, z]$ increases in π. Since the type-specific distribution $F_y(z)$ does not depend on π, hiring rates for a given type increase in π.

Thus, both conditions on hiring rates and the matching structure of the mechanical model are satisfied by this screening model. Furthermore, the condition of corollary 3 is satisfied. It follows that the model exhibits negative duration dependence and that the model implies that duration dependence worsens if markets are tighter.

[34] To form this expectation, firms use the joint distribution of incomplete durations, signals, and productivity at time t: $G^D(D, Z, y; t)$.

Appendix D: Model of Human Capital Depreciation

An alternative interpretation of duration dependence in unemployment hazards is that workers skills depreciate during unemployment. We will present here a simple model that captures this idea. Our main point is that this model does not imply that that duration dependence interacts with market tightness. We can thus test this model based on human capital depreciation against the screening model using the interaction between market tightness and unemployment durations.

In contrast to the screening model, the idea of skill depreciation does not emphasize information problems on the part of employers about the productivity of applicants. Instead, human capital explanations are models in which all information about the general skills of workers are known to employers. Assume therefore that, conditional on ϕ, all individuals have the same market skills. Instead of introducing an additional variable, we will simply assume that ϕ equals the human capital / productivity of a worker. Let ϕ at $d = 0$ be given by ϕ_0 and use Φ to denote the distribution of $\phi_0 : \phi_0 \sim \Phi$. We assume that individual human capital depreciates exponentially at rate ρ while unemployed. At $d > 0$, individual human capital is given by $\phi(d) = \phi_0 \exp(-\rho d)$.

In addition to general human capital, we assume that the match between workers and firms has a match specific component. That is, we assume that the output of any match is given by by $\phi + \varepsilon_{ij}$ where ε_{ij} is independent of ϕ and drawn from distribution F_ε. The independence assumption on ε captures the intuition that this component does not depend on worker or firm characteristics but is instead specific to each match.

As above, the unemployed and vacancies are matched at rate $m(x)$. Upon meeting, a firm observes ϕ and d,[35] but needs to interview a worker in order to discover the match specific component ε. As before, we assume that interviews are costly and for simplicity we assume that firms can not hire a worker without interviewing her first.

The value function for an open vacancy is very similar to that of the screening model given in eq. (20) :

$$rJ_u = -c + m_v(x) \int_d \int_{\phi_0} J_m(d, \phi_0) dG^D(d, \phi_0) \tag{24}$$

Upon meeting, the firm again has to choose whether to interview the worker with characteristics ϕ and pay the interview cost of ξ. The problem is similar to the one above, except that the expectation is taken over the match specific component ε

$$J_m(d, \phi_0) = \max \left\{ J_u, \int_\varepsilon J_I(\phi_0, d, \varepsilon) dF(\varepsilon) - \xi \right\} \tag{25}$$

We maintain the wage setting assumption that workers are paid their outside option b. The value of a filled vacancy is therefore

$$J_I(d, \phi_0, \varepsilon) = \max \left\{ J_u, \frac{1}{r+\delta} \left(\phi_0 \exp(-\rho d) + \varepsilon - b \right) \right\}$$

Imposing the free entry condition, we have that a job is filled if

$$\varepsilon \geq b - \exp(-\rho d) \phi_0 \tag{26}$$

Thus, conditional on matching and interviewing, the rate at which interviewees with (ϕ_0, d) are hired is $l(\phi_0, d) = 1 - F_\varepsilon(b - \exp(-\rho d)\phi_0)$. This rate declines in d. Now, since $J_I(d, \phi_0, \varepsilon)$ increases in ϕ_0 and decreases in d, we have that the call-back rate $c(\phi_0, d)$ increases in ϕ_0 and decreases in d. We thus have that the hiring rate has $h(\phi_0, d) = m(x) c(\phi_0, d) l(\phi_0, d)$ satisfies $\frac{\partial h(\phi_0, d)}{\partial d} < 0$.

[35] We assume that the firm knows the relationship $\phi(d) = \phi_0 \exp(-\rho d)$ so, given ϕ and d, it can recover ϕ_0. Thus, observing ϕ and d is equivalent to observing ϕ_0 and d. We adopt this convention when defining the value functions below.

Thus, the model generates true duration dependence in hiring rates and our experiment will find true duration dependence in call-back rates $c(\phi_0, d)$. However, consider the functions $r(d,x) = \frac{h(d,x,\phi)}{h(0,x,\phi)}$ and $r^c(d,x) = \frac{c(d,x,\phi)}{c(0,x,\phi)}$ that we have used to generate a testable implication for models following the structure of the mechanical model described in Section 2. For the model based on human capital depreciation, these two functions are:

$$\begin{aligned} r(d,x,\phi_0) &= \frac{c(\phi_0,d)\, l(\phi_0,d)}{c(\phi_0,0)\, l(\phi_0,0)} \\ r^c(d,x,\phi_0) &= \frac{c(\phi_0,d)}{c(\phi_0,0)} \end{aligned}$$

Neither of them depend on market tightness x. Therefore, it is possible to distinguish the depreciation model from the screening models described above exploiting the functions $r^c(d,x,\phi)$. Crucial however is again that the distribution of characteristics ϕ is adequately controlled for – and as we argue above, this requires experimental data of the type we exploit below.

Appendix E: Ranking as an Alternative Model of Employer Generated Duration Dependence

As an alternative to screening, Blanchard and Diamond (1994) (BD) developed a model of employer driven duration dependence building on the idea of ranking. According to the ranking model, vacancies accept multiple applications over a discrete, positive duration of time and then rank all applications against each other according to their duration. The ranking hypothesis is that firms hire the applicant with the shortest duration. Naturally, this model as discussed by BD generates duration dependence.

In each period, workers are assumed to send out an application with probability a. BD further assume that markets are large in the sense that $U(d)$ and $V \to \infty$, where $U(d)$ is the number of unemployed with duration less than d.[36] Given this assumption, the probability that any vacancy receives an application of an individual with duration d or less is equal to $1 - \exp\left(-\frac{aU(d)}{V}\right)$.

Since applications are independently assigned to vacancies, this probability is also the probability that an applicant of duration d will find himself applying to a vacancy for which another individual with a shorter duration also applies. The probability that an unemployed individual of duration d finds a job is therefore equal to the product of the probability that he sends an application times the probability that nobody of shorter duration applies to the same vacancies. Denoting by $h_R(d)$ the hazard function from leaving unemployment in BD's model, we obtain[37]:

$$h_R(d) = a \exp\left(\frac{-aU(d)}{V}\right) \tag{27}$$

In this model, the probability a worker matches with a firm, $m_u(x) = a$, does not depend on market tightness.[38] Conditional on a worker matching with a firm, the probability he gets hired is $l(d) = \exp\left(\frac{-aU(d)}{V}\right)$. Thus, the job finding rate $h_R(d)$ has a similar structure to the mechanical model above; namely the match probability times the hiring probability.

Since $U(d)$ is by construction an increasing function, we have that $\frac{\partial h_R(d)}{\partial d} < 0$. Thus, the ranking and the screening models both generate true duration dependence and it is not possible to distinguish between them on the basis of this finding. However, as we will argue below, the models differ fundamentally in how labor market conditions affect duration dependence – screening predicts that tighter markets lead to more

[36] We do not fully develop the BD model here, but refer the reader to the original work.

[37] This is equation (15) in BD.

[38] Blanchard and Diamond note that in a more realistic model, a would depend on the state of labour market. They do not consider this possibility.

duration dependence, whereas ranking predicts that tighter markets lead to less duration dependence.

Interaction Between Duration Dependence and Market Conditions

Consider the function $r_R(d) = \frac{h_R(d)}{h_R(0)}$ obtained from the ranking model:

$$r_R\,(d) = \exp(-a\frac{U(d) - U(0)}{V}) = \exp(-a\frac{U(d)}{V}) \tag{28}$$

where we use the fact that in continuous time there is no mass of individuals with durations less than or equal to $d = 0$. Thus, we see directly how duration dependence as measured by $r_R\,(d)$ depends on a particular measure of market conditions: the ratio of the currently unemployed with durations shorter than d to the total number of vacancies. If market conditions tighten in the sense that this ratio declines, then $r_R\,(d)$ increases.[39] Thus, in this sense tighter labor markets are associated with less duration dependence.

Therefore, we can distinguish the screening, human capital depreciation and the ranking model by either (i) examining whether durations vary with current or with past market conditions or (ii) by examining whether duration dependence is more or less negative in permanently tighter labor markets. It is this second implication that we use to motivate the design and implementation of our resume audit study.

Appendix F: Measuring Salience of Resume Characteristics Using Web-Based Survey of MBA Students

Our experiment assumes that employers are aware of (and can therefore respond to) information about a job applicant's unemployment spell. To test this assumption, we designed and conducted a web-based survey. We recruited 365 first-year MBA students at the University of Chicago Booth School of Business by e-mail on April 9, 2012, and the web-based survey was successfully completed by 90 MBA students.[40] The students did not receive any compensation for participation, and they took roughly 5-10 minutes on average to complete the survey.[41]

The survey took place in three stages. In the first stage (Appendix Figure A1), respondents were asked to read a hypothetical job posting and consider two resumes for the job opening. The job posting was chosen at random from one of three candidate job postings. These job postings were designed based on real job postings from our field experiment, each one correponding to one of the three job categories used in the field experiment (i.e., administrative/clerical, sales, customer service). We created six candidate resumes for each of the three possible job postings, and the two resumes presented to the respondent are chosen randomly from the appropriate set of six (and ordered randomly on the web page). These resumes were designed based on the fictitious resumes actually used in our field experiment. After being presented with the job posting and the two resumes, the respondent was then asked to select one of the two resumes to contact for an in-person job interview.

In the second stage (Appendix Figure A2), the respondent was required to perform two tasks. First, she was asked to recall specific information on each of the two resumes, such as total work experience, tenure at last job, level of education, current employment status, and the length of unemployment spell.[42] Importantly, the respondent was precluded from viewing the resumes after making her selection. If the respondent attempted to click the "Back" button on her browser, she was warned that this would invalidate her survey response. Second, the respondent was asked to indicate which two resume attributes were most

[39] We refer the reader to Blanchard and Diamond who show more directly that $h(d) = a \exp\left(-a\frac{U(d)}{V}\right)$ is decreasing in labor market tightness, $\frac{U}{V}$.

[40] There were 91 students who completed the survey, but one of the responses contained missing responses for most of the requested information and so was dropped from the analysis.

[41] We measure time-to-completion by treating the IP address of the respondent as a unique identifier.

[42] The ordering of these questions was chosen at random for each respondent.

important in evaluating the job applicant's resume, and to rank these two attributes by importance.[43] In the third stage of the survey (Appendix Figure A3), the respondent is asked several demographic questions.

We use the responses to the "recall" questions in the second stage to measure the salience of the various resume characteristics. The results are reported in Appendix Table A1. The full sample used to measure salience comprises all of the resumes evaluated by all of the respondents, which is $N = 180$, since each of the 90 respondents had to recall information for two resumes.

In Panel A of Appendix Table A1, we report results which compute how often the respondent correctly recalled the information, and we repeat this for each resume characteristic. The first row shows that respondents were able to correctly recall the level of education on the resume 65% of the time. This is similar to 66% of the time that the respondents were able to correctly recall whether or not the job applicant was currently employed. The respondents were particularly likely to recall the number of jobs that the applicant held; this information is correctly recalled 85% of the time. The last three rows of Panel A report results for the length of the unemployment spell, total work experience, and tenure at previous job, respectively. For these cases, we define the respondent as correctly recalling the information if the response is within a given window around the "actual" value, where the window varies by characteristic (and roughly scales with the average value of the characteristic across the resumes used in the survey).[44] Using this definition, respondents correctly recall length of unemployment spell 52% of the time, total work experience 64% of the time, and tenure at previous job 47% of the time. The second column of Panel A reports analgous results for the subsample of respondents who report "high experience" in reviewing resumes (corresponding to a 4 or a 5 on a 5-point scale, which comprises roughly 19% of the full sample). The results are broadly similar for this subsample, with more respondents in this subsample correctly recalling the length of unemployment spell and whether job applicant was currently employed.

Next, in Panel B we report an alternative measure of salience: the correlation between the "recalled" information and the "actual" resume characteristic. This correlation is based on the variation across resumes in the values of these characteristics. Across all of the rows in the table, the two values are strongly and significantly correlated, suggesting that the respondents were able to recall information. Additionally, the correlations are generally higher among the subsample of respondents with "high experience". Consistent with the results in Panel A, the correlation for length of unemployment spell is similar in magnitude to the correlations for the other variables. We also report the "mean % error" (defined as the average percentage difference between the "recalled" and "actual" values across all survey responses). This number is similar across characteristics, confirming that the respondents are not substantially biased on average in recalling specific information. We interpret the results in Panel A and Panel B as being broadly consistent with students being aware of employment status and length of unemployment spell, in addition to the other resume characteristics that they were asked to recall.

Lastly, in Panel C we report results from the subjective survey question which asked respondents to list the two most important attributes in evaluating the job applicant's resume. Interestingly, there is overwhelming preference for the resumes to have "relevant work experience", with very few respondents indicating employment status or length of unemployment spell as being one of the two most important attributes.[45] These results may shed light on why resume audit studies typically explain so little variation in callback rates: if employers are primarily trying to gauge whether the work experience is specifically relevant for the job, and this information is not being measured or manipulated by researchers, then the

[43] The ordering of these attributes was chosen at random for each respondent.

[44] The resumes in the survey have 84 months of work experience on average (std. dev. 18 months). For job tenure, the mean is 51 months (std. dev. 20 months). Finally, for length of unemployment spell, the mean (conditional on not being currently employed) is 20 months (std. dev. 9 months). The unemployment spells are chosen from set $\{8, 14, 20, 27, 36\}$. One reason why we choose the 4-month window for the length of unemployment spell is that there is a clear mass of respondents who respond with 12 and 24 months when true value of unemployment spell is 8 and 20, respectively. More than half of the survey respondents only provide year (and no month) for experience, job tenure, and unemployment spell. This could be consistent with a memory-based "heuristic" that rounds to the nearest year, or alternatively the respondents wanted to complete the survey more quickly and did not bother to guess the exact month for these characteristics.

[45] In pilot survey, we did not have "relevant work experience", and every student taking pilot survey responded that this would have been their first choice.

ability of the other covariates to explain variation in callback rates will be limited.

Overall, the results of this survey are consistent with our assumption that employers in our experiment are aware of the employment status and the length of the unemployment spell, at least to the extent that they are aware of other information on the resume, such level of education, total work experience, and tenure at last job. While there is an important caveat that the survey respondents are not a representative sample of the individuals evaluating resumes in our field experiment, we are reassured that our results persist in the subsample of MBA students with high levels of experience actually reviewing resumes.

Appendix G: Covariance Between City Fixed Effects and City-Specific Effect of Unemployment Duration

Recall the following estimating equation from the main text:

$$y_{i,c} = \delta^c + \gamma^c \log(d_{i,c}) + X_{i,c}\Gamma + \varepsilon_{i,c}$$

where δ^c is city fixed effects and γ^c is a city-specific estimate of the effect of unemployment duration. We test for whether duration dependence varies with labor market conditions by treating δ^c as a proxy measure of labor market tightness and then estimating the covariance between δ^c and γ^c; i.e. $E[(\delta^c - \bar{\delta}^c)\gamma^c]$. We compute this by first computing the covariance between the estimates; i.e., $E[\hat{\delta}^c\hat{\gamma}^c]$. Defining $\hat{\eta}^c_\delta$ as estimation error for $\hat{\delta}^c$ (i.e., $\hat{\delta}^c = \delta^c + \hat{\eta}^c_\delta$) and $\hat{\eta}^c_\gamma$ as estimation error for $\hat{\gamma}^c$, then we can compute $E[\hat{\delta}^c\hat{\gamma}^c]$ as follows:

$$\begin{aligned}
E[\hat{\delta}^c\hat{\gamma}^c] &= \frac{1}{C}\sum_{c=1}^{C}\hat{\delta}^c\hat{\gamma}^c \\
&= \frac{1}{C}\sum_{c=1}^{C}(\delta^c + \hat{\eta}^c_\delta)(\gamma^c + \hat{\eta}^c_\gamma) \\
&= \frac{1}{C}\sum_{c=1}^{C}\delta^c\gamma^c + \frac{1}{C}\sum_{c=1}^{C}\delta^c\hat{\eta}^c_\gamma + \frac{1}{C}\sum_{c=1}^{C}\hat{\eta}^c_\delta\gamma^c + \frac{1}{C}\sum_{c=1}^{C}\hat{\eta}^c_\delta\hat{\eta}^c_\gamma
\end{aligned}$$

where C is the total number of cities in the sample. We can re-write this using expectations as follows (using the fact that $E_c[\hat{\eta}^c_\gamma] = 0$ and $E_c[\hat{\eta}^c_\delta] = 0$):

$$E[\hat{\delta}^c\hat{\gamma}^c] = E[\delta^c\gamma^c] + \frac{1}{C}\sum_{c=1}^{C}E_c[\hat{\eta}^c_\delta\hat{\eta}^c_\gamma]$$

Next, we can compute $E_c[\hat{\eta}^c_\delta\hat{\eta}^c_\gamma]$ using standard statistical results:

$$E_c[\hat{\eta}^c_\delta\hat{\eta}^c_\gamma] = -\frac{\sigma^2_c}{N^c}\frac{E_c[\log(d)]}{Var(\log(d))}$$

where σ^2_c is the residual variance for city c, and N^c is the number of observations in the city. Combining the above gives us the following expression for the unbiased estimate of $E[\delta^c\gamma^c_2]$:

$$E[(\delta^c - \bar{\delta}^c)\gamma^c] = \frac{1}{C}\sum_{c=1}^{C}\hat{\delta}^c\hat{\gamma}^c + \frac{1}{C}\sum_{c=1}^{C}\frac{\hat{\sigma}^2_c}{N^c}\frac{E_c[\log(d)]}{Var(\log(d))} \tag{29}$$

In other words, there is a negative bias in estimated covariance if one simply computes empirical covariance based on the regression estimates $\hat{\delta}^c$ and $\hat{\gamma}^c$. Intuitively, this bias comes from the fact that the sampling errors in the estimates for these two parameters for a given city are negatively correlated. While this bias goes away asymptotically, it requires both that $C \to \infty$ and $N^C \to \infty$. In Monte Carlo simulations resembling our experimental data, we find substantial bias unless we use the bias correction above.

We conduct inference on the estimated covariance by computing the following standard error estimate, and we have verified that these standard errors are reliable using Monte Carlo simulations:

$$se(\widehat{E[\delta^c \gamma^c]}) = \sqrt{\frac{1}{C}\left(\frac{1}{C}\sum_{c=1}^{C}(\hat{\delta}^c)^2(\hat{\gamma}^c)^2\right)}$$

Appendix H: Data Sources

This section describes the various city-level data used in the empirical analysis:

Vacancy Data: we ordered the vacancy data from Wanted Analytics (WA), which is part of Wanted Technologies. WA collects hiring demand data and is the exclusive data provider for The Conference Board's "Help-Wanted OnLine Data Series", which is a monthly economic indicator of hiring demand in the US. WA gathers its data from the universe of online advertised vacancies posted on internet job boards or on newspaper online editions. In total, it covers roughly 1,200 online job boards, although the vast majority of the ads appear on a small number of job boards. When the same job ad appears on multiple job boards, WA uses a deduplication procedure to identify unique job ads on the basis of company name, job title and description and city or State.

Sahin et al (2011) document several potential measurement issues related to these data. First, the dataset records a single vacancy per ad, although it is possible that multiple positions are listed in a single ad. Second, it is possible that multiple locations within a state are listed in a single ad for a given position.

We received total job posts, by MSA, 6-digit SOC occupation code and year. Our sample spans 2008, 2009, 2010, 2011 plus year-to-date for 2012.

MSA Unemployment Data: Source: United States Bureau of Labor Statistics. Link: `http://data.bls.gov/cgi-bin/dsrv?la`. Description: Monthly data on number of unemployed persons, number of persons in the labor force, the number of employed persons and the unemployment rate in the given MSA.

GDP in 2010: Source: US Department of Commerce, Bureau of Economic Analysis. Link: `http://www.bea.gov/newsreleases/regional/gdp_metro/gdp_metro_newsrelease.htm` for original press release. Now can be reproduced at http://www.bea.gov/itable. Description: Real US GDP by metropolitan area. This is advanced GDP data, and was released in September 2011. The numbers used are chained 2005 dollars.

Median Income in 2011: Source: Federal Financial Institutions Examination Council, posting data from the Department of Housing and Urban Development. Link: `http://www.ffiec.gov/cra/censusproducts.htm\#MSAincome`. Description: Median Income in 2011 for MSAs in real dollars.

Fraction Poverty in 2010: Source: Census 2010.

Fraction Non-white in 2010: Source: Census 2010. Link: `http://www.census.gov/compendia/statab/2011/tables/11s0023.pdf`. Description: Source provides a breakdown of population by race and Hispanic origin using data from the 2010 Census for MSAs with over 750,000 persons. Fraction Nonwhite was calculated from this data.

Table 1
Descriptive Statistics

	N	Mean	Std. Dev.	Min	Max
Received callback for interview	12041	0.045	0.207	0	1
Received any phone call from employer	12041	0.123	0.328	0	1
Employed	12041	0.234	0.423	0	1
Months unemployed | Unemployed	**9222**	**18.018**	**10.303**	**1**	**36**
Some college	12041	0.416	0.493	0	1
College degree	12041	0.386	0.487	0	1
High quality	12041	0.502	0.500	0	1
Female	12041	0.637	0.481	0	1
Unemployment rate	12041	9.364	2.482	5.07	17.03
Vacanices/Unemployed ratio	12041	3.797	1.582	0.80	7.47
Administrative/Clerical job	12041	0.293	0.455	0	1
Customer Service job	12041	0.306	0.461	0	1
Sales job	12041	0.401	0.490	0	1

Notes: The first row reports the primary dependent variable which is whether or not the resume received a callback from the employer explicitly asking to set up an interview. The experimental sample is split into resumes where the worker reports currently being employed and resumes where the worker does not report currently being employed (with the gap between when the worker last reported working and when the resume was submitted being uniformly distributed between 1 and 36 months, inclusive).

Table 2
Randomization Tests

	Sample means		p-value of difference in means	Sample means		p-value of difference in means
	Employed	Unemployed		Unemployed ≥18 months	Unemployed <18 months	
Some college	0.409	0.418	0.302	0.421	0.415	0.653
College degree	0.402	0.382	0.233	0.390	0.373	0.290
High quality	0.506	0.501	0.590	0.503	0.498	0.578
Female	0.629	0.639	0.319	0.630	0.648	0.067
Unemployment rate	9.381	9.358	0.976	9.348	9.369	0.747
Vacancies/Unemployed ratio	3.742	3.814	0.379	3.799	3.831	0.731
Administrative/Clerical job opening	0.298	0.291	0.542	0.293	0.289	0.707
Customer Service job opening	0.304	0.307	0.736	0.309	0.305	0.684
Sales job opening	0.397	0.402	0.796	0.398	0.407	0.457
N	2819	9222		4642	4580	

Notes: The first row reports the primary dependent variable which is whether or not the resume received a callback from the employer explicitly asking to set up an interview. The experimental sample is split into resumes where the worker reports currently being employed and resumes where the worker does not report currently being employed (with the gap between when the worker last reported working and when the resume was submitted being uniformly distributed between 1 and 36 months, inclusive).

Table 3
The Effect of Unemployment Duration on Probability of Callback

Dependent variable: Received callback for interview				
	(1)	(2)	(3)	(4)
log(Months unemployed)	**-0.011**			
	(0.003)			
	[0.000]			
1{Employed}	-0.020	-0.004	-0.018	-0.036
	(0.010)	(0.006)	(0.009)	(0.013)
	[0.036]	[0.525]	[0.054]	[0.004]
Months unemployed / 12		-0.008	-0.035	-0.075
		(0.003)	(0.012)	(0.021)
		[0.003]	[0.003]	[0.000]
$(\text{Months unemployed} / 12)^2$			0.009	
			(0.003)	
			[0.010]	
Months unemployed / 12 ×				0.075
1{Months unemployed $\geq$ 8}				(0.022)
				[0.001]
Average callback rate	0.045	0.045	0.045	0.045
N	12041	12041	12041	12041
R^2	0.039	0.038	0.039	0.040
City fixed effects	X	X	X	X
Baseline controls	X	X	X	X

Notes: All columns report OLS linear probability model estimates. Data are resume submissions matched to callbacks from employers to request an interview. The baseline controls included are the following: Some college, College degree, High quality dummy, Female dummy, and job category dummies. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each job posting, are in parentheses and p-values are in brackets.

Table 4
Alternative Specifications

	Dependent variable: Received callback for interview								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
log(Months unemployed)	**-0.011**	**-0.011**	**-0.012**	**-0.010**	**-0.011**	**-0.011**	**-0.010**	**-0.011**	**-0.013**
	(0.003)	**(0.003)**	**(0.003)**	**(0.002)**	**(0.003)**	**(0.003)**	**(0.003)**	**(0.003)**	**(0.004)**
	[0.000]	**[0.000]**	**[0.000]**	**[0.000]**	**[0.000]**	**[0.000]**	**[0.001]**	**[0.000]**	**[0.001]**
1{Employed}	-0.020	-0.024	-0.026	-0.017	-0.020	-0.020	-0.019	-0.019	-0.009
	(0.010)	(0.010)	(0.010)	(0.006)	(0.006)	(0.010)	(0.009)	(0.009)	(0.013)
	[0.036]	[0.015]	[0.010]	[0.003]	[0.002]	[0.036]	[0.040]	[0.047]	[0.475]
Average callback rate	0.045	0.045	0.045	0.045	0.045	0.045	0.045	0.045	0.123
R^2	0.039	0.014	0.002			0.040	0.054	0.092	0.071
Dependent variable: callback for interview	X	X	X	X	X	X	X	X	
Dependent variable: receive any callback									X
Linear probability model	X	X	X			X	X	X	X
Probit model (reported marginal effects at means of controls)				X	X				
Baseline controls	X	X		X		X	X	X	X
Metropolitan area fixed effects	X					X	X	X	X
Resume template and resume font fixed effects						X	X	X	
Year x week fixed effects							X	X	
Metropolitan area × job type fixed effects								X	
Year x week × job type fixed effects								X	

Notes: N = 12041 across all columns. Data are resume-level submissions matched to callbacks from employers to request an interview. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets.

Table 5
Heterogeneity by Local Labor Market: Fixed Effects Estimates

Dependent variable: Received callback for interview						
		Covariate ***X*** = …				
	log(*d*)	Female	College	High quality	Customer Service job	Sales job
	(1)	(2)	(3)	(4)	(5)	(6)
Point estimate on ***X***	**-0.011**	0.002	-0.011	0.010	0.028	0.053
(From model without interactions)	**(0.003)**	(0.004)	(0.007)	(0.004)	(0.006)	(0.007)
	[0.000]	[0.548]	[0.149]	[0.010]	[0.000]	[0.000]
F-test of equality for interaction terms (p-value)	**[0.000]**	[0.279]	[0.077]	[0.913]	[0.000]	[0.000]
(City fixed effect × ***X***)						
Correlation between city fixed effect and	**-0.805**	0	0	0	-0.475	0.010
City-specific interaction term (bias-corrected)	**(0.191)**				(0.235)	(0.247)
	[0.000]				[0.043]	[0.968]
N	**12041**	12041	12041	12041	12041	12041
R^2	**0.082**	0.082	0.082	0.082	0.082	0.082

Notes: All columns report OLS linear probability model estimates. Data are resume-level submissions matched to callbacks from employers to request an interview, restricting sample to unemployed workers. Each column reports results from two separate regressions. The first row reports the point estimate on the covariate included in the column heading, when the effect is constrained to be the same across all cities. The second and third row report results from an alternative regression; specifically, it estimates a full set of interaction terms formed by multiplying indicator variable for each city with the variable listed in the column heading. The second row reports p-value from a test of equality across all of the estimated interaction terms, while the third row reports a bias-corrected estimate of the correlation between the estimated interaction terms and the city fixed effects. All regression include same controls listed in Table 3. If a cell entry has "0" with no standard error or p-value, then this implies that the model does not reject the null that the effect of the variable in the column is the same in all cities. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets.

Table 6

Heterogeneity by Local Labor Market: Correlated Random Effects Estimates

Dependent variable: Received callback for interview

	Covariate ***X*** = …					
	log(*d*)	Female	College	High quality	Customer Service job	Sales job
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of random coefficients for ***X***	**-0.011**	0.002	-0.011	0.010	0.048	0.047
	(0.005)	(0.004)	(0.007)	(0.004)	(0.012)	(0.010)
	[0.019]	[0.548]	[0.149]	[0.010]	[0.000]	[0.000]
Standard deviation of random coefficient estimates	**0.013**	0	0	0	0.042	0.043
	(0.002)				(0.007)	(0.007)
	[0.000]				[0.000]	[0.000]
Correlation between random coefficients for ***X*** and city-specific random effects	**-0.905**	0	0	0	-0.461	0.318
	(0.057)				(0.169)	(0.260)
	[0.000]				[0.006]	[0.221]

Notes: All columns report correlated random effects estimates, where a random coefficicent on the variable listed in the column is allowed to be flexibly correlated with a city-specific random effect parmaeter. The random coefficients are allowed to vary across cities but are constant within a city. Data are resume-level submissions matched to callbacks from employers to request an interview, restricting sample to unemployed workers. Each column reports results from separate regression. The first row reports the mean of the random coefficients estimated on the variable in column heading. The second row reports the standard deviation across the random coefficient estimates. The final row reports the correlation between the random coefficient estimates and the city-specific random effect estimates. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets. If a cell entry has "0" with no standard error or p-value, then this implies that the model does not reject the null that the effect of the variable in the column is the same in all cities. In the case, the model does not estimate city-specific random coefficients for variable in column, and instead only estimates city-specific random effects.

Table 7
How Does Duration Dependence Vary With Labor Market Conditions?

Dependent variable: Received callback for interview

	Interaction term formed using X = …				
	u	u_{2011} - u_{2008}	log(u)	-V/U	- log(V/U)
	(1)	(2)	(3)	(4)	(5)
Panel A: Include baseline controls only					
log(Months unemployed)	-0.011	-0.011	-0.011	-0.011	-0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
log(Months unemployed) × X	0.180	0.336	0.021	0.004	0.009
	(0.066)	(0.123)	(0.007)	(0.001)	(0.004)
	[0.006]	[0.006]	[0.001]	[0.001]	[0.009]
X [Proxy for local labor market conditions]	-0.328	-0.575	-0.036	-0.006	-0.020
	(0.113)	(0.212)	(0.012)	(0.002)	(0.007)
	[0.004]	[0.007]	[0.002]	[0.002]	[0.003]
Standardized effect of interaction term	**0.0045**	**0.0043**	**0.0054**	**0.0057**	**0.0041**
Standardized effect of *X*	**-0.0081**	**-0.0073**	**-0.0093**	**-0.0095**	**-0.0085**
Panel B: Include base controls + city fixed effects					
log(Months unemployed)	-0.011	-0.011	-0.011	-0.011	-0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
log(Months unemployed) × X	0.171	0.322	0.020	0.003	0.009
	(0.066)	(0.121)	(0.007)	(0.001)	(0.004)
	[0.010]	[0.008]	[0.002]	[0.002]	[0.016]
Standardized effect of interaction term	**0.0042**	**0.0041**	**0.0052**	**0.0053**	**0.0038**
Panel C: Include base controls + city FEs + city characteristics × log(Months unemployed)					
log(Months unemployed)	-0.011	-0.011	-0.011	-0.011	-0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
log(Months unemployed) × X	0.235	0.372	0.027	0.005	0.013
	(0.085)	(0.145)	(0.009)	(0.002)	(0.005)
	[0.006]	[0.010]	[0.002]	[0.005]	[0.020]
Standardized effect of interaction term	**0.0058**	**0.0047**	**0.0069**	**0.0072**	**0.0055**
p-value of F-test of joint significance of city characteristic interaction terms	0.001	0.001	0.001	0.003	0.001

Notes: All columns report OLS linear probability model estimates. Data are resume-level submissions matched to callbacks from employers to request an interview. The controls included are the following: Some college, College, High quality dummy, Female dummy, city fixed effects, resume template dummies, resume font dummies, and job category dummies. In Panel C, city characteristics include population, GDP, average income, and fraction of population in poverty, and fraction of population non-white. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets.

Table 8

The Effect of Unemployment Duration Across Subsamples

Dependent variable: Received callback for interview

	Full Sample	Men Only	Women Only	College Degree Only	Non-College Only	High Quality Only	Low Quality Only	Customer Service Jobs	Sales Jobs	Admin / Clerical Jobs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log(*d* = Months unemployed)	**-0.011**	**-0.006**	**-0.013**	**-0.010**	**-0.010**	**-0.012**	**-0.009**	**-0.004**	**-0.017**	**-0.010**
	(0.003)	**(0.005)**	**(0.003)**	**(0.005)**	**(0.004)**	**(0.004)**	**(0.004)**	**(0.005)**	**(0.006)**	**(0.004)**
	[0.000]	**[0.248]**	**[0.000]**	**[0.034]**	**[0.005]**	**[0.004]**	**[0.023]**	**[0.417]**	**[0.005]**	**[0.007]**
1{Employed}	-0.020	-0.001	-0.030	-0.023	-0.016	-0.020	-0.020	0.000	-0.035	-0.018
	(0.010)	(0.016)	(0.011)	(0.015)	(0.012)	(0.014)	(0.013)	(0.014)	(0.019)	(0.012)
	[0.035]	[0.949]	[0.005]	[0.144]	[0.160]	[0.151]	[0.111]	[0.994]	[0.065]	[0.124]
log(*d*) equal across cols. [p-val]		[0.201]		[0.581]		[0.904]			[0.214]	
Average callback rate in sample	0.045	0.054	0.039	0.046	0.044	0.048	0.041	0.043	0.068	0.015
N	12041	4374	7667	4653	7388	6043	5998	3686	4831	3524
R^2	0.038	0.045	0.046	0.047	0.047	0.046	0.044	0.058	0.049	0.067
City fixed effects	X	X	X	X	X	X	X	X	X	X
Baseline controls	X	X	X	X	X	X	X	X	X	X

Notes: All columns report OLS linear probability model estimates. Data are resume-level submissions matched to callbacks from employers to request an interview. The controls included are the following: Some college, College degree, Quadratic in years of experience, Female dummy, metropolitan area fixed effects, resume template dummies, resume font dummies, and job category dummies. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets.

Appendix Table A1
Measuring salience of resume characteristics: MBA student survey

	All resumes (N = 180)		Only resumes reviewed by "High experience" students (N = 34)	
PANEL A: DESCRIPTIVE STATISTICS FROM SURVEY				
	% answering correctly		% answering correctly	
What is the level of education of the applicant? [Bachelors, Associate Degree, GED, High School Grad]	65% correct		65% correct	
Is the job applicant currently employed?	**66% correct**		**82% correct**	
How many jobs has the applicant held?	85% correct		86% correct	
How long is the applicant currently unemployed? [Sample limited to not currently employed]	**52% correct (within 4 months)**		**74% correct (within 4 months)**	
What is the applicant's total work experience?	64% correct (within 24 months)		71% correct (within 24 months)	
How long did the applicant hold his/her last job?	47% correct (within 12 months)		50% correct (within 12 months)	
PANEL B: CORRELATION AND MEAN % ERROR COMPARING "RECALLED" AND "ACTUAL" RESUME CHARACTERISTICS				
	Correlation	Mean % error	Correlation	Mean % error
How many jobs has the applicant held?	0.710	3.4%	0.647	5.9%
	(0.054)	(1.5%)	(0.135)	(3.8%)
How long is the applicant currently unemployed?	**0.499**	**-13.3%**	**0.757**	**-14.3%**
[Sample limited to not currently employed]	**(0.067)**	**(6.0%)**	**(0.115)**	**(7.5%)**
What is the applicant's total work experience?	0.419	-13.0%	0.664	-11.4%
	(0.070)	(2.3%)	(0.132)	(3.8%)
How long did the applicant hold his/her last job?	0.447	-11.1%	0.771	-9.7%
	(0.069)	(5.2%)	(0.113)	(9.3%)
PANEL C: RANKING RESUME ATTRIBUTES BY IMPORTANCE				
Which two attributes were most important in evaluating the job applicant's resume?				
	1st choice	2nd choice	1st choice	2nd choice
Years of work experience	4%	29%	11%	28%
Length of time at most recent job	0%	13%	0%	17%
Level of education	9%	28%	11%	39%
Number of jobs held by applicant	0%	12%	0%	0%
Relevant work experience	84%	8%	74%	6%
Current employment status	1%	2%	0%	0%
Length of time out of work	2%	7%	5%	11%

Notes: This table reports results from a web-based survey administered to first-year MBA students at the University of Chicago Booth School of Business. Details of the survey are given in the Appendix. The table reports results for entire sample as well as a subsample of survey respondents who reported high experience in reviewing resumes (either a 4 or 5 on a 5-point scale). Standard errors are reported in parentheses in Panel B.

Appendix Table A2
The Effect of Unemployment Duration on
Probability of Callback

Dependent variable: Received callback for interview	(2)	(2)
log(Months unemployed)	**-0.011**	**-0.011**
	(0.003)	**(0.003)**
	[0.000]	**[0.000]**
1{Employed}	-0.020	-0.024
	(0.010)	(0.010)
	[0.036]	[0.015]
Unemployment rate		**-0.328**
		(0.113)
		[0.004]
Some college	-0.009	-0.011
	(0.005)	(0.006)
	[0.083]	[0.044]
College degree	-0.011	-0.014
	(0.007)	(0.008)
	[0.149]	[0.067]
High quality	**0.010**	**0.011**
	(0.004)	**(0.004)**
	[0.010]	**[0.005]**
Female	0.002	0.003
	(0.004)	(0.004)
	[0.548]	[0.452]
Customer service job	0.028	0.029
	(0.006)	(0.006)
	[0.000]	[0.000]
Sales job	0.053	0.054
	(0.007)	(0.006)
	[0.000]	[0.000]
Average callback rate in estimation sample	0.045	0.045
N	12041	12041
R^2	0.039	0.015
City fixed effects	X	
Baseline controls	X	X

Notes: All columns report OLS linear probability model estimates. Data are resume-level submissions matched to callbacks from employers to request an interview. The controls included are the following: Some college, College, Quadratic in years of experience, Female dummy, metropolitan area fixed effects, resume template dummies, and resume font dummies. Standard errors, adjusted to allow for an arbitrary variance-covariance matrix for each employment advertisement, are in parentheses and p-values are in brackets.

Figure 1: Callback Rate vs Unemployment Duration

Sample = Unemployed only

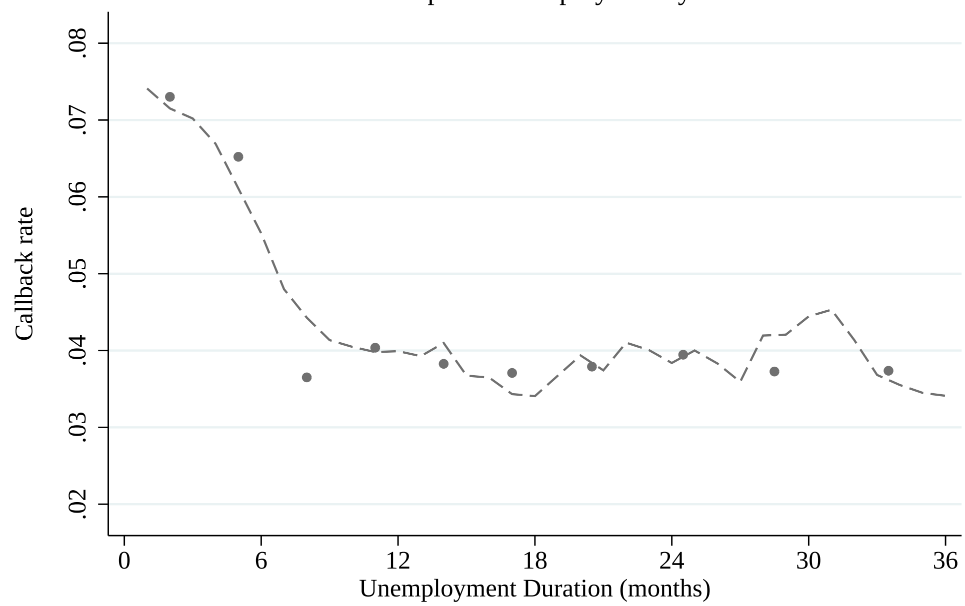
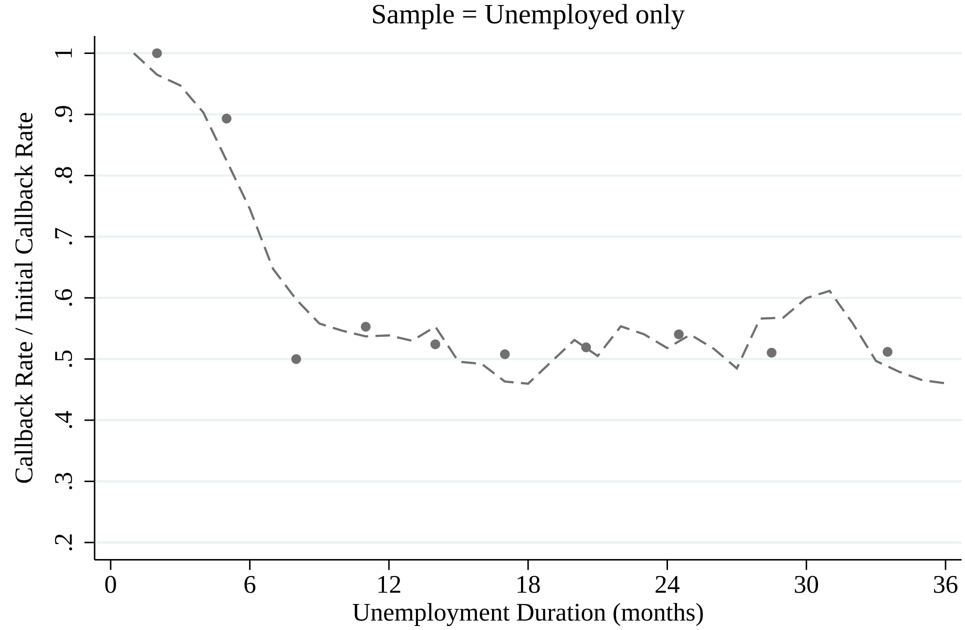


Figure 2: (Relative) Callback Rate vs Unemployment Duration

Sample = Unemployed only



Notes: These figures are generated by computing the average callback rate for each 3-4 month bin in the range [1,36]. Figure 1 reports the average callback rate in each bin, while Figure 2 divides the average callback rate by the average callback rate in the first (lowest) unemployment duration bin [1,3]. The lines show the 5-month moving averages computed at each month.

Figure 3: Callback Rate by Unemployment Rate

Sample = Unemployed only

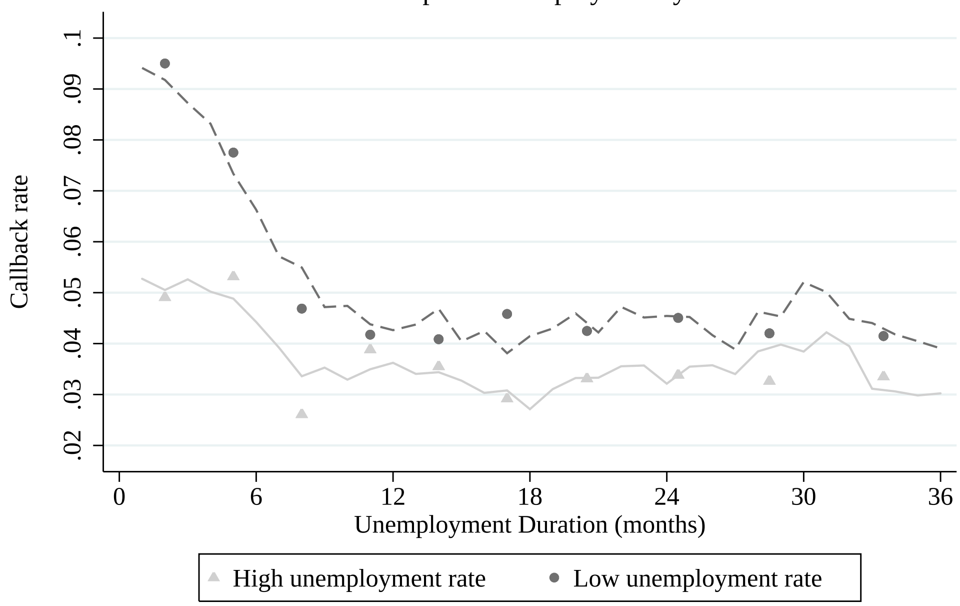
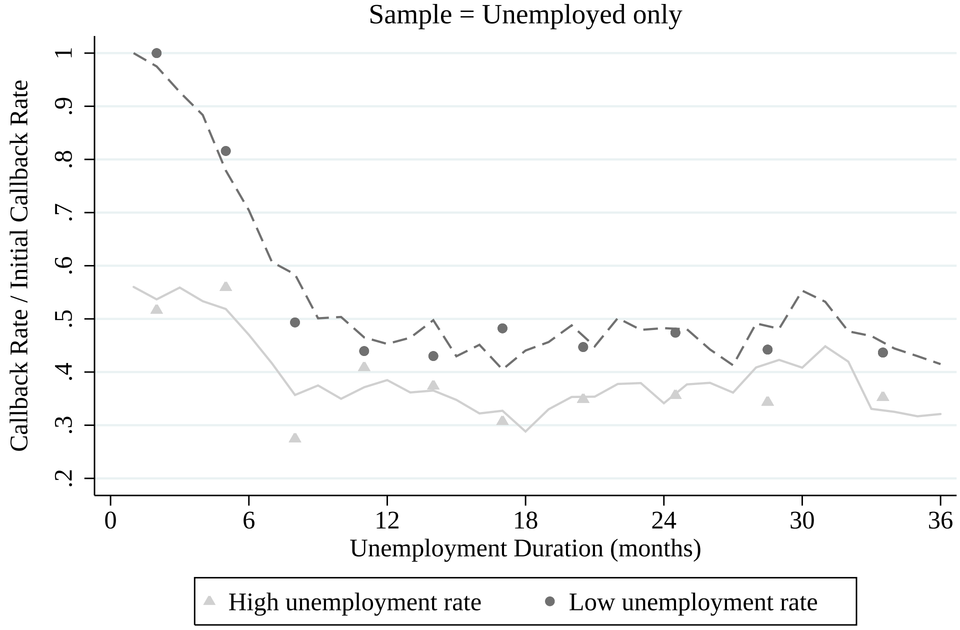
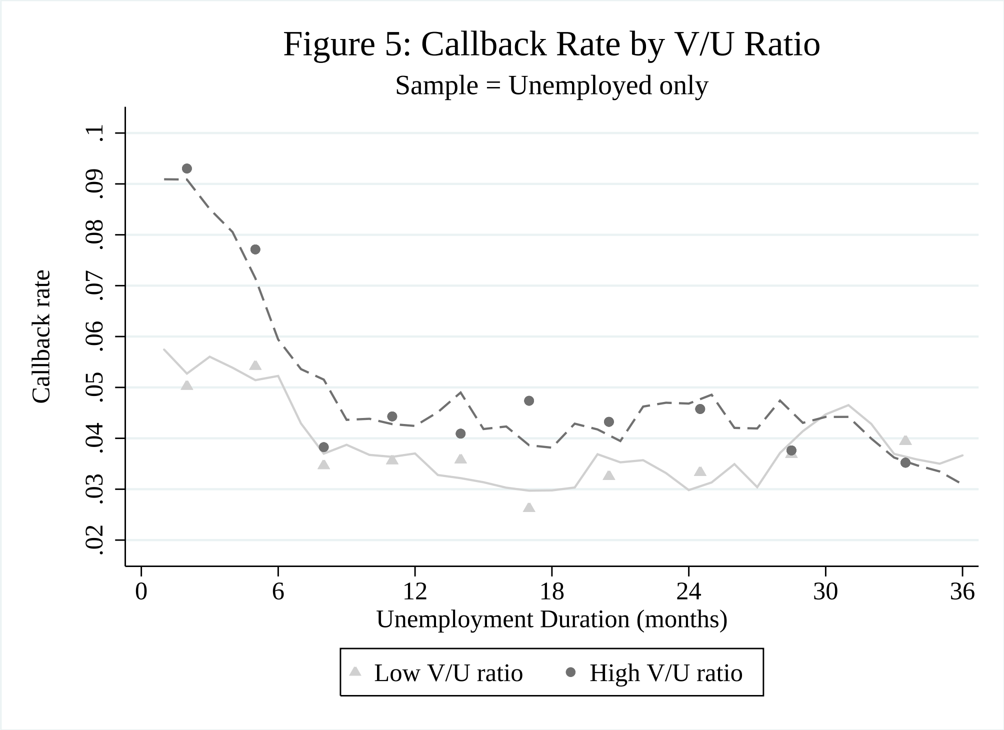


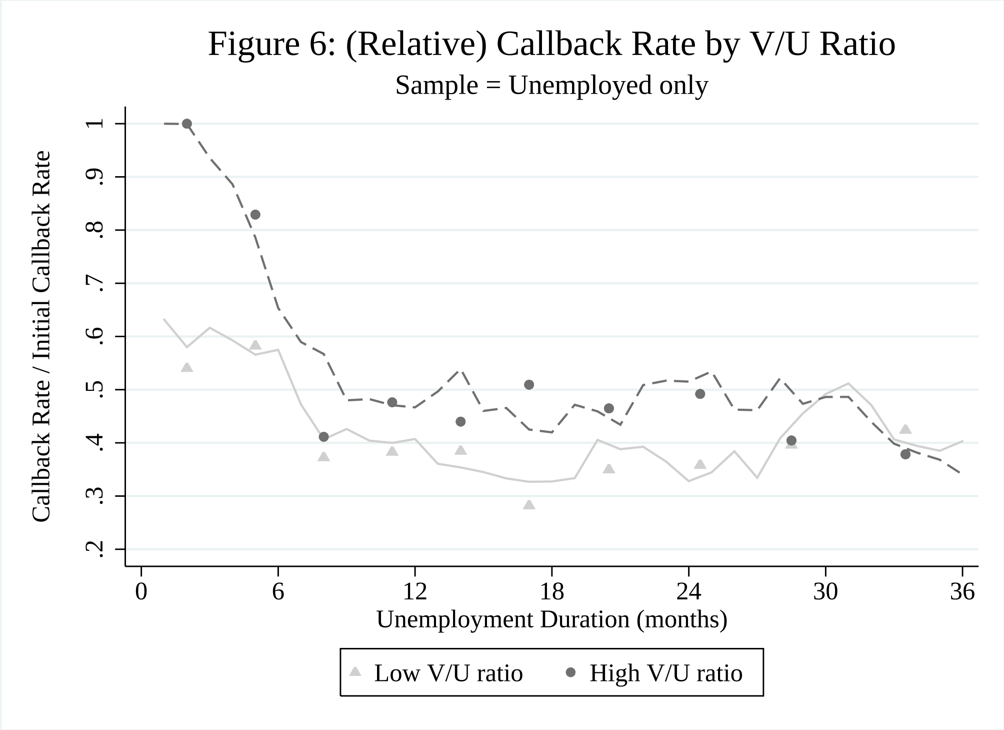
Figure 4: (Relative) Callback Rate by Unemployment Rate

Sample = Unemployed only



Notes: These figures are generated by computing the average callback rate for each 3-4 month bin in the range [1,36] for two sub-samples of the experimental data: data from cities with low unemployment rates ($u < 8.8\%$), and cities with high unemployment rates. Figure 3 reports the average callback rate in each bin for each sub-sample, while Figure 4 divides the average callback rate by the average callback rate in the first (lowest) unemployment duration bin [1,3]. The lines show the 5-month moving averages computed at each month.





<u>Notes:</u> These figures are generated by computing the average callback rate for each 3-4 month bin in the range [1,36] for two sub-samples of the experimental data: data from cities with high vacancy-to-unemployment ratios ($V/U > 3.25$), and cities with low V/U ratios. Figure 5 reports the average callback rate in each bin for each sub-sample, while Figure 6 divides the average callback rate by the average callback rate in the first (lowest) unemployment duration bin [1,3]. The lines show the 5-month moving averages computed at each month.

Figure 7: Callback Rate by Unemp. Rate Growth

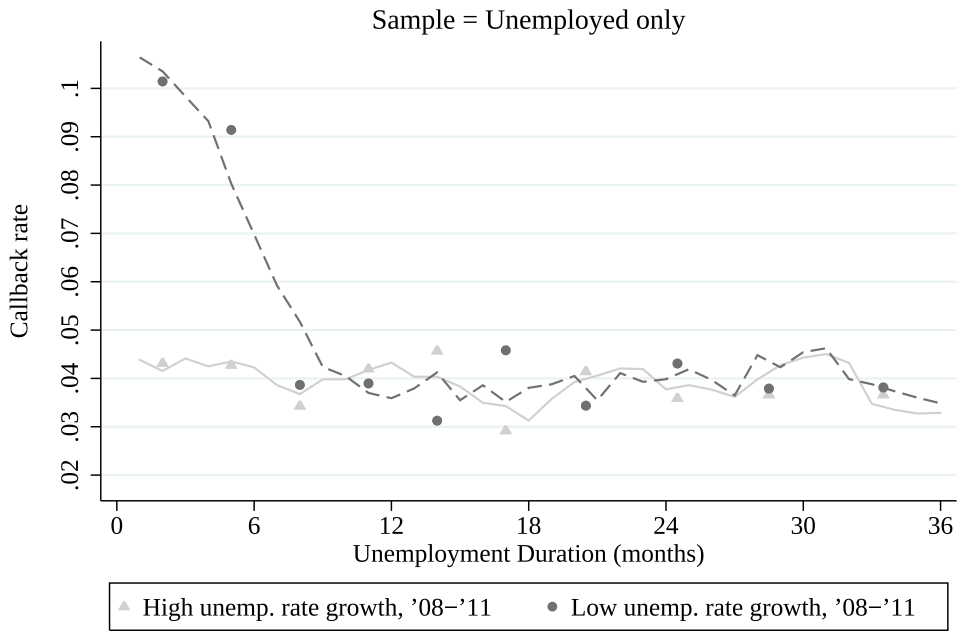
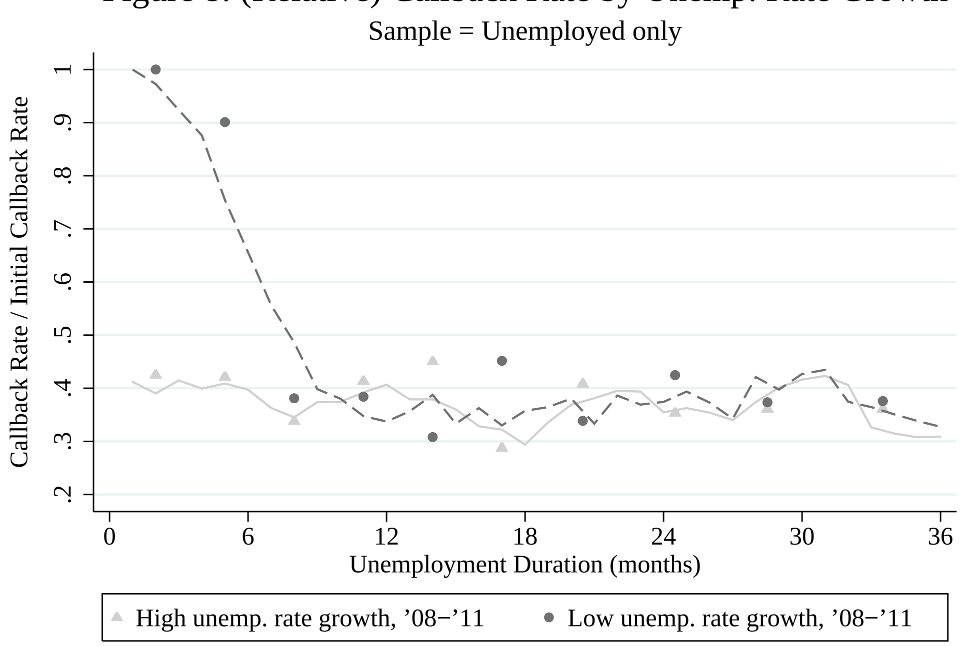
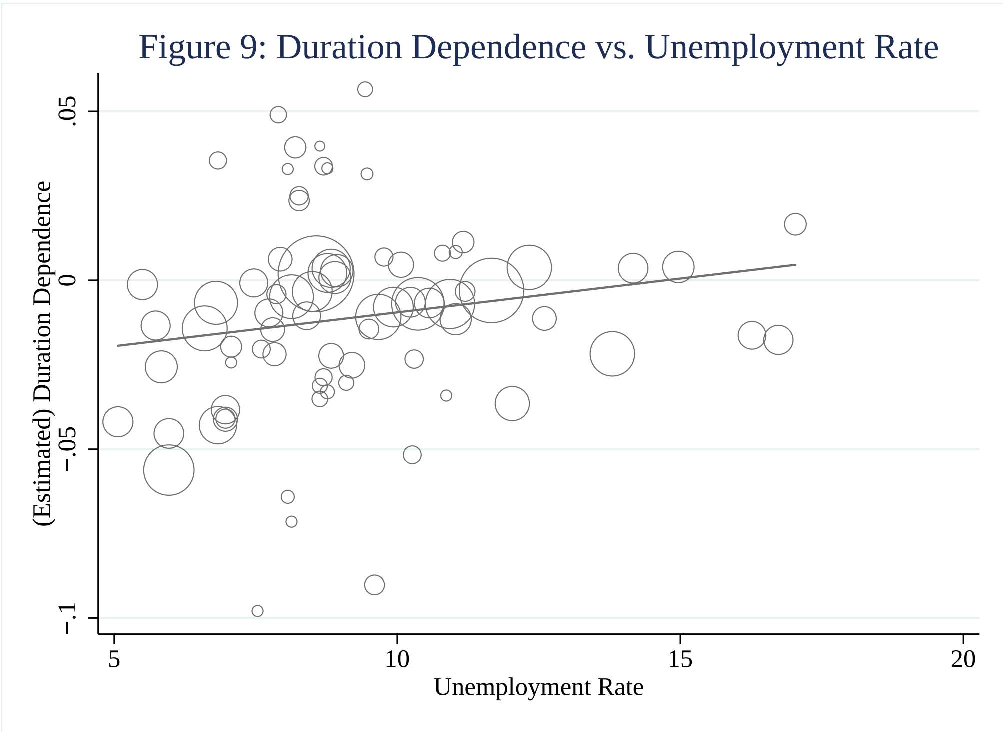


Figure 8: (Relative) Callback Rate by Unemp. Rate Growth



Notes: These figures are generated by computing the average callback rate for each two-month bin in the range [1,36] for two sub-samples of the experimental data: data from cities with low unemployment rate growth (less than 3.6 percentage points between 2008 and 2011), and cities with high unemployment rate growth. Figure 7 reports the average callback rate in each bin for each sub-sample, while Figure 8 divides the average callback rate by the average callback rate in the first (lowest) unemployment duration bin [1,3]. The lines show the 5-month moving averages computed at each month.



Notes: See text for details.

Appendix Figure A1

CHICAGO BOOTH

The University of Chicago Booth School of Business

Chicago Booth Web-Based Resume Survey

Survey Instructions

We would like you to put yourself in the hypothetical situation of a Human Resource Manager who is currently trying to fill an opening for a job as a **Customer Service Representative**. You are considering two resumes that were submitted earlier today.

Please spend a few minutes reading the job description and the two resumes below, and then evaluate (in your capacity as the HR Manager) which of the two applicants you would contact to set up an in-person interview for the job.

Once you have finished evaluating the resume, you will then be asked several questions about the job applicant on the next page.

Job Description

Description

Home Buyers Warranty, the leader in the home warranty industry, is seeking an Internet Dispatcher in a high paced call center environment.

Examples of just some of the day-to-day duties include:
- Makes outbound calls to non-enrolled contractors for service requests.
- Receives inbound/outbound calls with regard to network enrollment.
- Sets up claims and dispatch service requests to contractors.
- Enters contractor information into the company databases for SNA.

Resume #1: Jennifer Moore

(303) 223-5715 5872 Ruth Way, Denver, CO 80221 JenniferM.1221@yahoo.com

Jennifer Moore

OBJECTIVE

Seeking a position with a reputable organization where I can utilize my experience in customer service.

SUMMARY OF QUALIFICATIONS

I am a very motivated, responsible, and ambitious individual. I enjoy new challenges and value working with others. I am looking for an organization that values its excellence, while providing a workplace that seeks to both teach and learn from its employees.

EDUCATION

Aprahoe Ridge High School Boulder, CO – G.E.D.

EXPERIENCE

5/2008 – 4/2009 **GEBHARDT BMW DEALERSHIP** Boulder, CO
Receptionist

- Answered phones
- Organized and maintained files
- Sorted sales documents and contracts
- Provided customer service for service pick-ups

7/2006 – 4/2008 **CHARLOTTE RUSSE – SHOPS AT NORTHFIELD STAPLETON** Denver, CO
Supervisor

- Responsible for daily sales
- Provide customer service to store patrons
- Organized inventory and stocked products
- Responsible for balancing cash register drawer
- Completed sales records and bank deposits

1/2004 – 6/2005 **NEW YORK & COMPANY – SHOPS AT NORTHFIELD STAPLETON** Denver, CO
Sales Associate

- Responsible for daily sales
- Provide customer service to store patrons
- Responsible for store displays

SKILLS

Over six years of impeccable customer service experience; Computer- Proficient in Word, Outlook, Win2data, MLS and Win Total; Skilled at sales and adapts well in many different sales environments.

Resume #2: Timothy Collins

TIMOTHY COLLINS
982 Rowena Street, Thornton, Adams, Colorado 80229
(720) 437-8005
timothycollins.8101@yahoo.com

SUMMARY

Customer Service professional with the ability to build strong relationships with customers, coworkers and potential clients. Professional and hardworking individual devoted to improving company's reputation through exceptional customer service.

PROFESSIONAL EXPERIENCE

Target **Littleton, Colorado**
Customer Service Manager
May 2006 – August 2010

- Recommended, selected, and helped locate merchandise based on customer needs and desires.
- Resolved customer issues and inquiries; completed returns and exchanges as necessary.
- Examined merchandise to ensure that it is correctly priced and displayed.
- Conducted market research, identified emerging trends, and introduced marketing strategies.
- Operated cash register, handled money and performed daily check out procedures.

Tebo Store Fixtures **Denver, Colorado**
Customer Service Representative
January 2003 – May 2006

- Trained new hires in a variety of warehouse departments.
- Made sure items were in good shape and that packages were received and shipped timely.
- Conducted and reported monthly inventory of warehouse items.
- Provided office support and administrative assistance for customer orders processing and computer tracking.

EDUCATION

Westwood College – Denver North Campus **Denver, Colorado**
Associate Degree in Information Technology

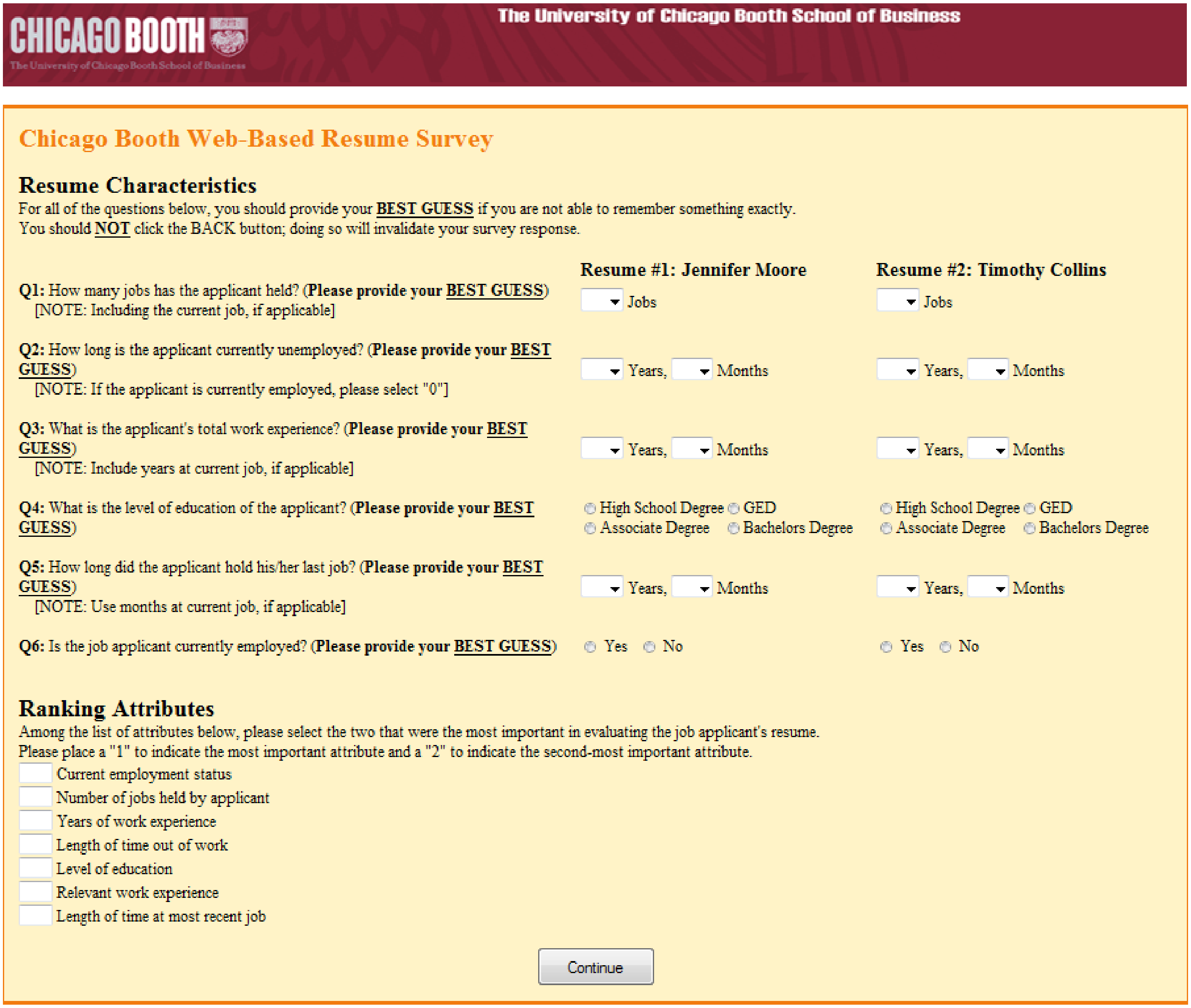
Please select which applicant should be contacted for an interview:

Resume #1: Jennifer Moore

Resume #2: Timothy Collins

Continue

Appendix Figure A2



CHICAGO BOOTH

The University of Chicago Booth School of Business

The University of Chicago Booth School of Business

Chicago Booth Web-Based Resume Survey

Resume Characteristics

For all of the questions below, you should provide your **BEST GUESS** if you are not able to remember something exactly.
You should **NOT** click the BACK button; doing so will invalidate your survey response.

	Resume #1: Jennifer Moore	Resume #2: Timothy Collins
Q1: How many jobs has the applicant held? (**Please provide your BEST GUESS**) [NOTE: Including the current job, if applicable]	Jobs	Jobs
Q2: How long is the applicant currently unemployed? (**Please provide your BEST GUESS**) [NOTE: If the applicant is currently employed, please select "0"]	Years, Months	Years, Months
Q3: What is the applicant's total work experience? (**Please provide your BEST GUESS**) [NOTE: Include years at current job, if applicable]	Years, Months	Years, Months
Q4: What is the level of education of the applicant? (**Please provide your BEST GUESS**)	High School Degree, GED, Associate Degree, Bachelors Degree	High School Degree, GED, Associate Degree, Bachelors Degree
Q5: How long did the applicant hold his/her last job? (**Please provide your BEST GUESS**) [NOTE: Use months at current job, if applicable]	Years, Months	Years, Months
Q6: Is the job applicant currently employed? (**Please provide your BEST GUESS**)	Yes No	Yes No

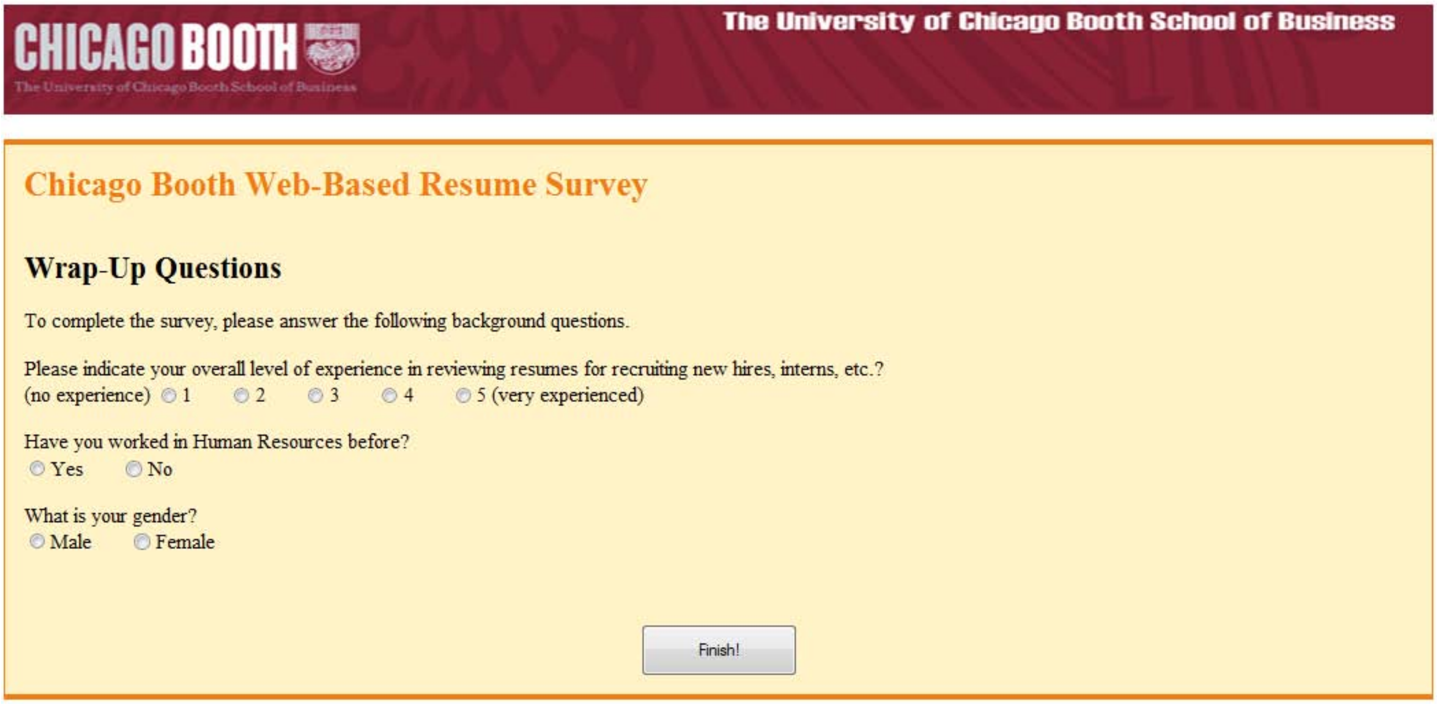
Ranking Attributes

Among the list of attributes below, please select the two that were the most important in evaluating the job applicant's resume.
Please place a "1" to indicate the most important attribute and a "2" to indicate the second-most important attribute.

Current employment status
Number of jobs held by applicant
Years of work experience
Length of time out of work
Level of education
Relevant work experience
Length of time at most recent job

Continue

Appendix Figure A3



CHICAGO BOOTH

The University of Chicago Booth School of Business

The University of Chicago Booth School of Business

Chicago Booth Web-Based Resume Survey

Wrap-Up Questions

To complete the survey, please answer the following background questions.

Please indicate your overall level of experience in reviewing resumes for recruiting new hires, interns, etc.?
(no experience) ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 (very experienced)

Have you worked in Human Resources before?
○ Yes ○ No

What is your gender?
○ Male ○ Female

Finish!

Figure A4: City Fixed Effect vs. Unemployment Rate

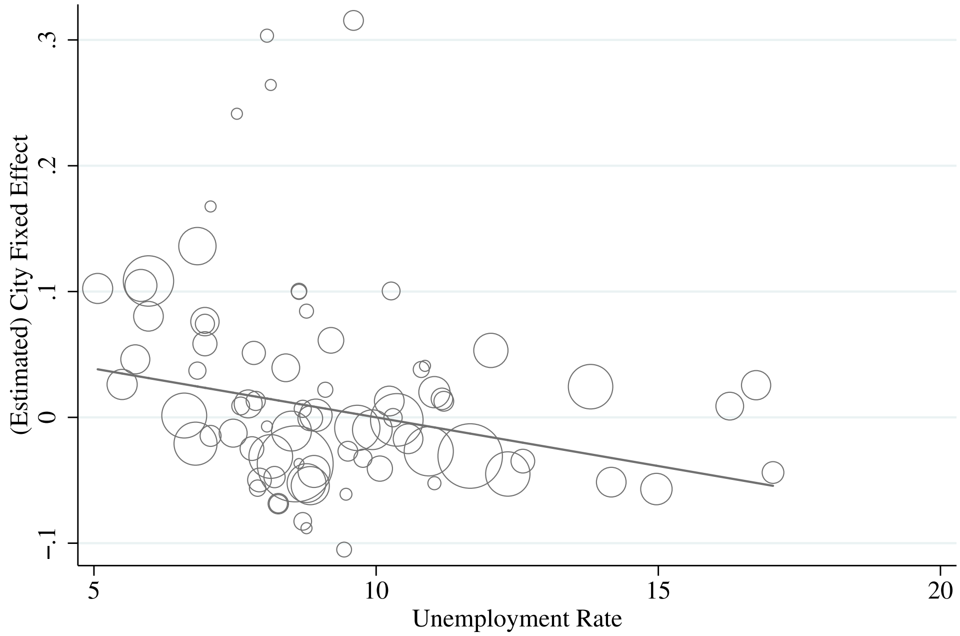
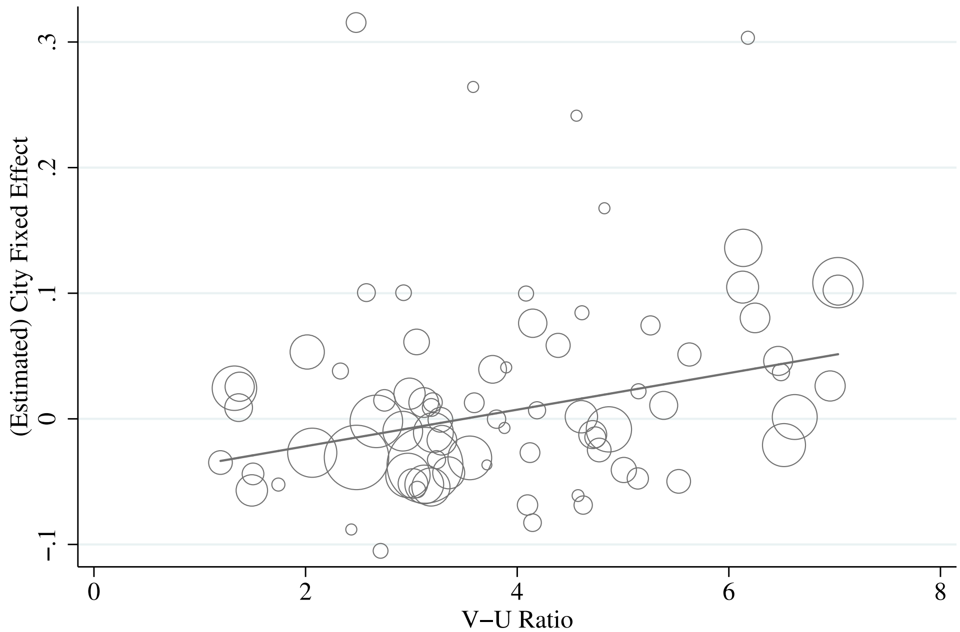


Figure A5: City Fixed Effect vs. V−U Ratio



<u>Notes:</u> These figures show the correlation between the estimated city-specific fixed effects and two alternative proxies for market tightness.