Racial Disparities in Job Finding and Offered Wages

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Abstract

The extent to which discrimination can explain racial wage gaps is one of the most divisive issues in the social sciences. Using a newly available data set, this paper develops a simple empirical test that, under plausible (but not innocuous) conditions, provides a lower bound on the extent of discrimination in the labor market. Taken at face value, our estimates imply that differential treatment accounts for at least one-third of the black-white wage gap. We argue that the patterns in our data are most naturally rationalized through a search-matching model in which employers statistically discriminate on the basis of race when hiring unemployed workers but learn about their marginal product over time.

1. Introduction

In the past 5 decades, social scientists have attempted to identify discrimination in a variety of ways. These include estimating residual wage gaps net of the effect of observable characteristics and premarket skills (for example, Corcoran and Duncan 1979; Reimers 1983; O'Neill 1990; Neal and Johnson 1996; Black et al. 2010), developing structural models of the labor market (for example, Bowlus and Eckstein 2002; Eckstein and Wolpin 1999), and conducting audit studies

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and related experiments (Ayres and Siegelman 1995; Neumark, Bank, and Van Nort 1996; Bertrand and Mullainathan 2004; Pager 2007).

Surprisingly, these approaches arrive at starkly different conclusions. While experimental and structural analyses often report differential treatment by race, the best available reduced-form evidence seems to suggests that "the black-white wage gap primarily reflects a skill gap" (Neal and Johnson 1996, p. 869) and that "labor market discrimination is no longer a first-order quantitative problem in America" (Heckman 1998, p. 101).

However, all of these methods are subject to important limitations. Estimating Mincerian equations to account for racial differences in individuals' endowments and premarket factors will misstate the extent of discrimination if skill bundles or other important characteristics are unobservable. Structurally modeling unobserved heterogeneity sidesteps this issue but comes at the cost of imposing parametric restrictions, and (quasi-)experimental evidence of differential treatment by race may mistake discriminatory tastes of the average employer for market discrimination (Heckman 1998).[1]

Gaining a better understanding of the impact of labor market discrimination on racial wage gaps is of great importance, as the appropriate policy lever, if any, depends critically on the answer. If discrimination is quantitatively important, then the case for antidiscrimination policy or even affirmative action may be justified. If, however, racial wage gaps are determined before individuals enter the labor market, or if discrimination is not a first-order problem, then the case for government intervention is much weaker.

Using rich longitudinal data on a large sample of unemployed workers in New Jersey who completed weekly interviews for up to 12 weeks, we develop a simple test for the presence of racial discrimination in the labor market. Four features of this data set—information on search behaviors and search strategies, data on offered (as opposed to only accepted) wages, administrative information on previous earnings, and timing (data were collected during a period of mass unemployment)—enable us to conduct a novel test of racial discrimination in job finding and offered wages. The key idea is that under the null hypothesis of equal treatment, wages will closely resemble a worker's marginal product. Hence, conditional on wage on the previous job, there should be no racial differences in wage offers. By controlling for previous wage, we account for the market valuation of skill bundles, noncognitive skills, and similar variables that previous research treated as unobservable. Finding racial differences after controlling for previous earnings would thus lead us to reject the null hypothesis of no discrimination.

This approach rests on two important identifying assumptions. First, we assume that, ceteris paribus, blacks and whites draw job offers from a comparable

[1] In addition to these points, Charles and Guryan's (2011) discussion of challenges to identifying discrimination also emphasizes that individuals' self-identified race is a social construct, which may be endogeneous to labor market success, thereby complicating the identification of discrimination.

set of firms in similar markets, which implies that search intensities, search strategies, discount rates, and so on, do not differ significantly across racial groups. This assumption is partially testable. Adding controls for hours spent looking for a job, the number of firms an individual contacted, the types of jobs to which she applied, bargaining behavior, or discounting does not significantly alter the results.

The second assumption is that previous wage does not systematically overstate blacks' productivity relative to that of whites. If previous wage equals marginal product, then this assumption holds and our approach will correctly identify racial discrimination. If previous wage is a function of both productivity and differential treatment by race, then our approach will provide a lower bound on the impact of discrimination. Conversely, if previous wage captures marginal product plus a diversity preference or the effect of affirmative action, then the second assumption is violated, and we will overstate the amount of discrimination in the market. Unfortunately, this assumption is not directly testable.

The bottom line is simple: if one believes that, conditional on previous wage, blacks are at least as qualified as whites, then our approach identifies a lower bound of discrimination in the labor market. If one believes the opposite to be true, then our approach is invalid.[2]

The results from our test of racial discrimination in the labor market are both interesting and informative. While the raw black-white gap in our data, −.404 log point, is slightly larger than the gaps in such commonly used data sets as the Current Population Survey (CPS), the U.S. Census, or the National Longitudinal Survey of Youth 1979 (NLSY79), controlling for previous wage decreases the gap to −.169 (.056). Adding additional controls for industry, occupation, duration of unemployment, bargaining behavior, geographic characteristics, search behavior and search intensity, discount rates, and metropolitan area fixed effects reduces the gap by, at most, .042 log point. Thus, under the two assumptions above, our data reveal that the impact of racial discrimination on offered wages is at least one-third of the raw gap for blacks.[3]

Although these results do not depend on the particulars of any economic theory, we argue that our empirical findings are most naturally rationalized by a search-matching model of the labor market—similar to that developed by Jovanovic (1979)—in which employers statistically discriminate on the basis of race when hiring from the market but learn about their employees' productivity over time. The model has three stages. In the first stage, unemployed workers are stochastically matched with firms. After observing a productivity signal, the

[2] Our findings are qualitatively robust to potential confounding factors, such as mean reversion in wages, severe measurement error, or different empirical models. Robustness checks on these dimensions are contained in online Appendix C. If the effects of discrimination accumulate with labor market experience, then our lower bound is not likely to bind. We thank Betsey Stevenson for making this point.

[3] These estimates are similar to those recently reported by Lang and Manove (2011) using the National Longitudinal Survey of Youth 1979 (NLSY79) and controlling for educational attainment as well as a test score taken when individuals were in middle or high school.

firm offers a worker her expected marginal product, and the worker decides whether to accept the offer. If she declines, she remains unemployed but has the chance of being rematched in the next period. If the worker accepts, she works for one period, and in the next period both the worker and the firm learn the true productivity of their match. Firms then offer a worker her match-specific marginal product. The worker decides whether to continue the employment relationship (until an exogenous separation occurs) or to transition to unemployment and search for a better match.

The model's predictions are borne out in the data. As in Black (1995), the presence of statistical discrimination in our search-matching model implies that reservation wages are lower for blacks. Empirically, we estimate that blacks have a 7 percent lower reservation wage than similar whites. Moreover, if blacks are more likely than whites to incur a job separation, then the model predicts that the aggregate black-white wage gap may increase with age or experience across firms. This fact has been documented by Altonji and Blank (1999), Altonji and Pierret (2001), and Oettinger (1996). Within firms, however, racial wage gaps are predicted to decrease with tenure, as employers learn about a worker's marginal product. Using both our data and detailed data on work histories from the NLSY79, we show that the data support this prediction. In our data from New Jersey, for instance, blacks experience a return to tenure that is 1.1 percentage points higher than that of whites. Extending the empirical work of Altonji and Pierret (2001), we demonstrate that although the black-white wage gap widens by .9 percentage point per year of potential labor market experience, it decreases by 1.2 percentage points per year of tenure with a given employer.

Finally, our analysis addresses a common critique of statistical discrimination models (for example, Neal 2006). Simple models of this kind predict lower returns to education for blacks than for whites. Yet, if anything, the opposite appears to be true empirically. While we do not model human capital investments directly, our dynamic search-matching model of statistical discrimination is flexible enough to account for this important point. For instance, blacks may experience weakly higher returns on investment in our model if education reduces the variance in the signal to employers (for empirical evidence, see Arcidiacono, Bayer, and Hizmo 2010), or if educational attainment decreases the probability of job loss (Kletzer 1998), thereby allowing blacks to garner larger returns to tenure.

Although our model of statistical discrimination is consistent with the patterns in our data and sidesteps common critiques of such models, we cannot rule out other forms of discrimination. Premarket factors alone, however, cannot explain the full set of facts.[4] Thus, if our estimates are taken at face value, labor market discrimination appears to be an important impediment to racial income equality.

[4] Charles and Guryan (2008) argue that taste-based discrimination in the spirit of Becker (1957) explains about one-quarter of the black-white gap. While we cannot rule out that taste-based discrimination per se, the patterns in our data are inconsistent with models that rely exclusively on racial animus.

This suggests that alleviating racial inequality may require a combination of policies to both eliminate barriers to investing in premarket skills and enforce antidiscrimination policies so that minorities are appropriately rewarded for those skills.

The remainder of the paper proceeds as follows. Section 2 provides a brief overview of the literature on racial discrimination in the labor market. Section 3 outlines a search-matching model in which firms statistically discriminate on the basis of race. Section 4 describes the data used in our analysis as well as our econometric approach. Empirical evidence on racial differences in wage offers and job finding is presented in Section 5. Section 6 tests additional predictions of our model, and Section 7 discusses to which extent alternative theories may reconcile our findings. There are three appendices. Appendix A contains technical proofs, Appendix B describes the construction of our samples as well as the coding of variables, and online Appendix C contains additional empirical results.

2. Race and the Labor Market

There exists a very large literature on racial differences in wages.[5] In what follows, we divide the literature into three categories based on the strategy used to identify discrimination. Section 2.1 describes analyses using Mincerian equations and the assumptions needed to obtain causal estimates. Section 2.2 discusses the literature that imposes parametric restrictions to estimate structural models of the labor market, and Section 2.3 reviews experimental approaches. Broadly summarized, the existing evidence is inconclusive as to whether discrimination is of first-order importance in today's labor market.

2.1. Mincerian Equations

A large number of empirical studies estimate Mincerian equations and define labor market discrimination as the wage differential between racial groups net of a set of observable characteristics, such as age, education, occupation, geographic location, and labor market experience (for example, Corcoran and Duncan 1979; Reimers 1983; Smith and Welch 1986; Blau and Beller 1992; Oaxaca 1973; Oaxaca and Ransom 1994; Darity and Mason 1998). While this approach is useful in accounting for racial differences in endowments, it will identify the causal effect of discrimination if and only if unobservable determinants of individuals' wages do not systematically differ by race. Therefore, estimates of racial discrimination in this tradition depend crucially on the set of included controls.

Corcoran and Duncan (1979) constitutes an early attempt to account for a comprehensive set of covariates. The findings indicate that blacks and whites enjoy similar returns to observable characteristics, yet racial differences in these factors account for only half of the raw wage gap. The authors interpret this as

[5] For an excellent (though somewhat dated) review, see Altonji and Blank (1999).

evidence of pervasive discrimination. Similarly, paying careful attention to selection bias, Reimers (1983) estimates that discrimination is responsible for up to 86 percent of the total difference in the wages between Hispanic and non-Hispanic white men and for about 60 percent of the black-white wage gap.

Fairlie and Kletzer (1998) examine black-white disparities in job displacement and reemployment rates. They document approximately 30 percent higher rates of displacement and substantially lower reemployment probabilities for black workers. Although observable factors (in particular, education and occupation) play an important role in accounting for the raw racial difference, a large fraction of the gap remains unexplained, leaving ample room for discrimination.

In stark contrast, the seminal contributions of O'Neill (1990) and Neal and Johnson (1996) demonstrate that racial disparities in wages narrow dramatically—and sometimes even reverse—upon accounting for a measure of premarket skill. In particular, using data from the NLSY79, Neal and Johnson (1996) report that conditioning only on age and an individual's score on the Armed Forces Qualification Test (AFQT) reduces the raw racial gap in wages by more than 70 percent. The resulting residual black-white wage differences are −.072 and .035 log point for men and women, respectively. On the basis of this evidence, Neal and Johnson (1996), as well as many subsequent observers, conclude that the black-white wage gap is primarily due to differences in premarket skills as opposed to discrimination. Thus, it is often argued that appropriate public policies for alleviating racial differences in wages should be aimed at eliminating the hurdles that black children face in acquiring marketable skills (for example, Fryer 2011).

Lang and Manove (2011), however, point out that racial gaps in wages reemerge when one controls for educational attainment in addition to AFQT scores (see also Carneiro, Heckman, and Masterov 2005). In particular, they show that the gap increases from −.09 to −.15 log point when including years of schooling in Neal and Johnson's (1996) original specification and argue that when one controls for AFQT performance, blacks have higher educational attainment than whites and that the labor market discriminates against blacks by not financially rewarding them for greater education.[6]

2.2. Structural Models of the Labor Market

Recognizing the inherent problems of the Mincerian approach, another strand of the literature develops structural models of the labor market to estimate the effect of discrimination (for example, Wolpin 1992; Eckstein and Wolpin 1999; Bowlus and Eckstein 2002). Blinder (1973), for instance, uses a simultaneous-equation specification to account for the endogeneity of education and union

[6] In an appendix, Neal and Johnson (1996) show that, conditional on both Armed Forces Qualification Test (AFQT) scores and education, racial wage gaps are larger at the bottom of the education distribution and smaller at the top. Lang and Manove (2011) argue that the convergence at high levels of skill is a consequence of statistical discrimination, since informational asymmetries likely decrease for college graduates.

status. He estimates that between 40 and 70 percent of the racial gap in the Panel Study of Income Dynamics is due to discrimination.

However, if individuals engage in a costly job search, then the distribution of observed wages will not correspond to the distribution of wage offers, and estimates of discrimination based on the former may confound disparate treatment with any other factor determining reservation wages (in particular, search costs). To address this issue, Eckstein and Wolpin (1999) developed a two-sided search-matching model that delivers an upper bound on the impact of discrimination. Estimates from the NLSY79 indicate that discrimination can potentially explain the entire gap.

Similarly, in an attempt to disentangle unobserved productivity differences from discrimination by firms, Bowlus and Eckstein (2002) estimate an equilibrium search model in which some employers incur disutility from hiring blacks. Their results imply that the productivity of blacks is, on average, only 3.3 percent lower than that of whites, whereas employers' distaste for blacks is equivalent to 31 percent of whites' productivity level, and 56 percent of firms discriminate. An important limitation to the structural approach is its reliance on restrictive assumptions to ensure identification.

2.3. Field and Quasi Experiments

A third branch of the literature seeks to identify discrimination by using field and quasi experiments. In-person audit studies, for instance, compare the probability of receiving a callback or job offer across carefully matched pairs of black and white individuals who pose as applicants in real-world job searches (Turner, Fix, and Struyk 1991; Bendick, Jackson, and Reinoso 1994; Pager 2003; Pager, Western, and Bonikowski 2009).[7] Almost uniformly, these studies find that black testers fare substantially worse than their white counterparts, which is commonly interpreted as strong evidence of discrimination. However, as emphasized by Heckman (1998), the validity of this approach depends crucially on the assumption not only that tester pairs are similar on observables but that the distribution of unobservable characteristics does not differ by race. Moreover, it is not possible to infer market discrimination from the discriminatory tastes of the average employer (see Becker 1957).

Correspondence studies provide a partial solution to the first concern (Firth 1981; Esmail and Everington 1993; Bertrand and Mullainathan 2004). Bertrand and Mullainathan (2004) sent almost 5,000 fictitious resumes with randomly assigned black- or white-sounding names to more than 1,200 help-wanted ads in Boston and Chicago. Ceteris paribus, white-sounding names received about 50 percent more callbacks. Yet it remains unclear whether the marginal (as opposed to the average) employer treats blacks and whites differently.

The approach that we take in this paper combines aspects of the Mincerian

[7] There also exists a large (quasi-)experimental literature on discrimination in housing and product markets. See Riach and Rich (2002) for a useful review.

and structural literatures. Our empirical work is strongly guided by theory, but uncertainty over which form of discrimination is generating the data leads us to eschew structurally estimating the parameters of our model. Instead, the richness of our data permits reduced-form estimation of parameters that are typically structurally estimated, such as arrival rates, reservation wages, or wage offer distributions.

Ultimately, our contribution to the literature on labor market discrimination is threefold. We provide the first descriptive details of racial differences in search behavior from a large sample of job seekers. We develop a novel empirical test which, under plausible conditions, provides a lower bound on the extent of discrimination. And we show that the patterns in our data are consistent with a search-matching model of the labor market in which employers statistically discriminate based on race.[8]

3. A Search-Matching Model of the Labor Market

To fix ideas and to provide a framework for interpreting our empirical work, we outline a simple search-matching model of the labor market. Although our interpretation of the data is heavily guided by theory, it is important to emphasize that our empirical test for discrimination is general and does not depend on the specifics of the model.

The model is a discrete-time simplification of Jovanovic (1979), along the lines of that developed by Sargent (1987) and Prescott and Townsend (1980), with statistical discrimination. First, we describe the case in which there are no racial differences and in which firms do not discriminate on the basis of race. We then briefly describe how one introduces these features.

Let there be a unit mass of infinitely lived individuals who are looking for work. Each period, unemployed workers and firms are randomly matched with probability $\delta \in (0, 1)$. An agent's marginal product is match specific and is denoted θ.

Workers maximize the present discounted value of wages. But before a matched worker receives an offer, she and the firm observe a common noisy signal of her productivity, $\theta + \xi$. We assume that θ and ξ are independently and normally distributed random variables: $\theta \sim N(\mu, \sigma_\theta^2)$ and $\xi \sim N(0, \sigma_\xi^2)$. Using Bayes's rule, both the worker and the firm draw inferences about θ. That is, conditional on having observed $\theta + \xi$, θ is distributed normally with mean $\omega = [\sigma_\xi^2/(\sigma_\theta^2 + \sigma_\xi^2)]\mu + [\sigma_\theta^2/(\sigma_\theta^2 + \sigma_\xi^2)](\theta + \xi)$ and variance $\sigma_{\theta\mid\theta+\xi}^2 = \sigma_\theta^2/(\sigma_\theta^2 + \sigma_\xi^2)$.[9]

To simplify the analysis, we assume that firms operate in a perfectly competitive

[8] Our evidence is consistent with the findings of List (2004) for the sports card market. List (2004) conducts a series of complementary field experiments demonstrating that statistical, as opposed to animus-based, discrimination is the reason why minorities receive lower initial and final offers in this market.

[9] One can show that the forthcoming results generalize if we dispense with the normality assumption and assume that the wage is stochastically increasing in the signal (see Border 1996).

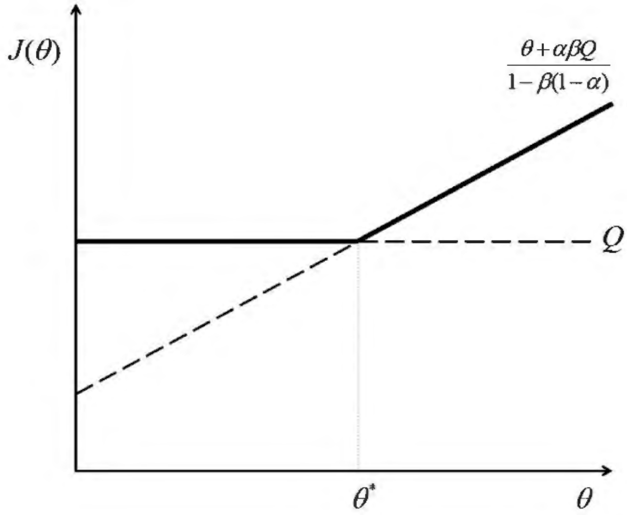

Figure 1. Bellman's functional equation

market with free entry. Moreover, firms employ a constant-returns-to-scale technology for which labor is the only input. In equilibrium, each firm offers an initial wage $\omega = E[\theta \mid \theta + \xi]$, with the understanding that in subsequent periods it will pay the worker its marginal product as it obtains more information about θ.[10]

Given this strategy of the firm, the worker must decide whether to accept the offer and work this period receiving ω or refuse and remain unemployed for one period, with a chance of being matched with another firm in the next one. If she accepts, her true productivity is revealed in the subsequent period. After learning her marginal product, the firm offers to pay θ until the match is exogenously terminated (which occurs with probability $\alpha \in (0, 1)$ at the end of every period). The worker then decides whether to accept or reject this offer.

Let $J(\theta)$ denote the expected present value of the wages of a worker whose marginal product is known to be θ with certainty and who behaves optimally. If she accepts the offer, the value of the match is given by $\theta + \alpha\beta Q + (1 - \alpha)\beta J(\theta)$, where $\beta \in (0, 1)$ is an exogenously determined discount factor and Q denotes the expected present value of wages if unemployed. Workers who reject the match are unemployed during this period with the chance of being rematched in the next one. Thus, we can write Bellman's functional equation as

$$J(\theta) = \max\{\theta + \alpha\beta Q + (1 - \alpha)\beta J(\theta),\ Q\}.$$

This equation is graphed in Figure 1. It admits a solution of the familiar form

[10] Jovanovic (1979) proves that this constitutes an equilibrium strategy, although other equilibria do exist.

$$J(\theta) = \begin{cases} \dfrac{\theta + \alpha\beta Q}{1 - \beta(1 - \alpha)} & \text{for } \theta \geq \theta^{\star} \\ Q & \text{for } \theta < \theta^{\star}. \end{cases} \tag{1}$$

As is typical in these models, workers follow a reservation wage policy: accept offers $\theta \geq \theta^{\star}$ and reject offers $\theta < \theta^{\star}$, where $\theta^{\star}$ solves

$$\theta^{\star} = (1 - \beta)Q. \tag{2}$$

We now turn to the worker's choice in the intermediate stage. After being matched with a firm and having observed $\theta + \xi$, the worker has to decide whether to accept a wage offer ω and thereby retain the option value of learning θ.

Let $V(\omega)$ be the expected present value of the wages of a worker who has an initial offer ω in hand and who behaves optimally. Then,

$$V(\omega) = \max\{\omega + \beta \int J(s)dF(s \mid \omega, \sigma^2_{\theta \mid \theta+\xi}), Q\},$$

where $F(s \mid \omega, \sigma^2_{\theta+\xi})$ denotes the posterior cumulative distribution function of θ, conditional on $\theta + \xi$. Note that both ω and $\beta \int J(s)dF(s \mid \omega, \sigma^2_{\theta+\xi})$ are increasing in ω, whereas Q is constant. Thus, workers again follow a reservation wage policy. The functional equation has the solution

$$V(\omega) = \begin{cases} \omega + \beta \displaystyle\int J(s)dF(s \mid \omega, \sigma^2_{\theta \mid \theta+\xi}) & \text{for } \omega \geq \omega^{\star} \\ Q & \text{for } \omega < \omega^{\star}, \end{cases} \tag{3}$$

and the reservation wage, $\omega^{\star}$, in the intermediate stage is implicitly defined by

$$\omega^{\star} + \beta \int J(s)dF(s \mid \omega^{\star}, \sigma^2_{\theta+\xi}) = Q. \tag{4}$$

In equilibrium, the average accepted wage of workers in the intermediate stage is given by

$$E[\omega \mid \omega \geq \omega^{\star}] = \frac{\int_{\omega^{\star}}^{\infty} w dG(w \mid \mu, \sigma^2_{\omega})}{1 - G(\omega^{\star} \mid \mu, \sigma^2_{\omega})},$$

and that of tenured workers equals

$$E[\theta \mid \theta > \theta^{\star}, \omega > \omega^{\star}] = \frac{\int_{\omega^{\star}}^{\infty} \int_{\theta^{\star}}^{\infty} s dF(s \mid w, \sigma^2_{\theta \mid \theta+\xi}) dG(w \mid \mu, \sigma^2_{\omega})}{\int_{\omega^{\star}}^{\infty} \int_{\theta^{\star}}^{\infty} dF(s \mid w, \sigma^2_{\theta \mid \theta+\xi}) dG(w \mid \mu, \sigma^2_{\omega})}.$$

It is straightforward to verify that mean wages decrease when workers are willing to accept worse matches—that is, as reservation wages decline. In Appendix A, we also prove that $\theta^{\star} > \omega^{\star}$. Consequently, wages increase on average with tenure in the firm but decrease as tenured workers lose their jobs and are being rematched.

To close the model, the present discounted value of wages when unemployed is given by

$$Q = \delta\beta \int V(w) dG(w \mid \mu, \sigma_\omega^2) + (1 - \delta)\beta Q, \tag{5}$$

where $G(w \mid \mu, \sigma_\omega^2)$ denotes a normal cumulative distribution function with mean μ and variance $\sigma_\omega^2 \equiv \sigma_\theta^4/(\sigma_\xi^2 + \sigma_\theta^2)$.

The model straightforwardly generalizes to incorporate a variety of differences in worker characteristics. After all, each parameter in the set $\{\beta, \alpha, \delta, \mu, \sigma_\theta^2, \sigma_\xi^2\}$ can vary by group identity. If groups differ on observable characteristics, such as race, and if these characteristics are correlated with any of the parameters, then firms will treat each group of workers as if they belonged to a separate market of that type. In particular, under the assumptions above, it continues to be an equilibrium to pay each worker her expected marginal product, given all available information (see Jovanovic 1979). There are thus many ways to introduce racial disparities in wages.[11]

Disparities in the arrival rate of matches due, for example, to differences in search behavior or discriminatory practices of firms can be captured by assuming that $\delta^B < \delta^W$. This relationship is reported in several audit studies in sociology and economics (for example, Bendick, Jackson, and Reinoso 1994; Pager 2003; Bertrand and Mullainathan 2004). From equations (2) and (5), it is straightforward to show that $d\theta^*/d\delta > 0$. Thus, if blacks are less likely to receive job offers, then they also have lower reservation wages and will accept worse matches. In equilibrium this results in racial wage gaps.

Blacks may also be more likely to lose their job (Fairlie and Kletzer 1998). As reported in Stratton (1993), disparities in arrival and separation rates lead to large racial differences in unemployment rates. Moreover, it is easy to show that increasing the chance of an exogenous separation lowers the reservation wage; that is, $d\theta^*/d\alpha < 0$. All else equal, this would result in lower wages for blacks.

Now consider racial differences in the distribution of the match quality signal (Phelps 1972; Arrow 1973; Cornell and Welch 1996; Lang 1986). In Arrow's (1973) model, the average of ξ differs between blacks and whites, which results in racial differences in initial wage offers. Conversely, in Phelps's (1972) or Aigner and Cain's (1977) framework, the variance of ξ is larger for blacks than for whites. In this case, employers put more weight on average group ability when evaluating blacks' signals than when inferring the ability of a white candidate. While this will not lead to mean differences in ω if both groups are equally

[11] Of course, there exist many other equilibria. For instance, search frictions and the existence of market power may induce firms to offer lower wages to groups of workers with lower reservation wages (Black 1995). Without free entry and a perfectly elastic supply of entrepreneurs, biased employers may trade profits for a desire to discriminate and survive in equilibrium. To fix ideas and to focus on the core aspects of job search as well as learning, we choose to maintain the simpler, more tractable—but admittedly less realistic—assumptions of Jovanovic (1979).

skilled, if $\mu^B < \mu^W$, then black workers will, on average, receive lower wage offers than whites with the same signal. In either case, our model predicts the black-white wage gap to converge with tenure in the firm, since workers of equal ability earn the same wage after their true ability has been revealed. This prediction distinguishes our search-learning theory from traditional models of discrimination, and as shown later, it is indeed borne out in the data.

4. Data and Econometric Approach

4.1. Data and Descriptive Statistics

The primary data set used in this paper was collected by the Princeton University Survey Research Center during the fall of 2009 and early 2010.[12] It is important to recognize that the data were collected during a period of mass unemployment, thereby lessening potential selection problems into the pool of unemployment insurance (UI) recipients (Gibbons and Katz 1991). Although we have do not have compelling empirical evidence in favor of this assertion, it seems likely that layoffs during the 2009 recession were more random than during periods of a tight labor market.[13]

Starting from the universe of UI recipients in New Jersey as of September 28, 2009, the Princeton University Survey Research Center drew a stratified random sample of 68,313 unemployed individuals. The sampled population was then contacted by the New Jersey Department of Labor and Workforce Development and invited to participate in a confidential Web survey for 12 consecutive weeks.[14]

The survey consisted of an initial entry questionnaire and weekly follow-up interviews that were remarkably rich. The former elicited information on demographics, previous employment, asset holdings, and spouses' employment status, whereas the latter inquired about job search activities, time use, reservation wages, and job offers, among other topics. Participants were given the choice of receiving either an incentive payment of $20 within a few days of completing the entry questionnaire or $40 at the end of the 12-week survey period.

An important caveat to the data is that only 6,025 (roughly 10 percent) of the sampled individuals participated in the entry wave, and those who responded to the initial survey completed only about 40 percent of weekly follow-ups. The

[12] In what follows, we draw heavily on Krueger and Mueller (2011). For a comprehensive description of the sampling and interviewing procedures, interested readers should consult their appendix.

[13] In a seminal paper, Gibbons and Katz (1991) argue that unemployed workers are negatively selected and demonstrate that wage losses following displacement are larger after layoffs than after plant closings (which presumably provide little or no signal about worker ability). Hu and Taber (2011) show that this holds only among white males, whereas blacks appear to suffer greater declines in wages following plant closings. Hu and Taber (2011) rationalize this finding by appealing to heterogeneous human capital. In Section 7, we argue that discrimination can rationalize this finding, as it may give blacks more of an incentive to invest in firm-specific human capital than whites.

[14] Individuals who were unemployed for 60 weeks or longer at the beginning of the survey were later asked to participate in an additional 12 weeks of interviewing, for a maximum of 24 weeks. In this paper, however, we restrict attention to the first 12 weeks for all respondents.

likelihood of responding varies by race. Of the sample of respondents, 15.3 percent were black (compared with 18.6 percent in the sample frame) and 68 percent were white (relative to 61.7 percent in the sample frame) (see Krueger and Mueller 2011). Participants were more educated, were more likely to be female, and had higher previous earnings than the baseline population. Using rich administrative data, Krueger and Mueller (2011) create sampling weights to adjust for the stratified survey design as well as nonresponse. Comparing characteristics of respondents with the universe of UI recipients along a number of dimensions, including those that were not used to construct the weights (for example, income and weekly exit rates from UI), they conclude that the low response rate did not significantly skew the sample. After applying weights, blacks make up 20 percent of the sample (compared with 20.8 percent in the universe of UI recipients) and whites make up 59.8 percent (compared with 58.9 percent).

Throughout our analysis, we use the weights created by Krueger and Mueller (2011) and follow their coding of wages by dropping wage offers below $5 an hour and wage offers above $100 per hour. Moreover, we restrict attention to respondents with nonmissing information on race who are not listed as previously self-employed, for a final sample of 5,251 individuals. Appendix B provides additional detail on the construction of our sample as well as precise definitions of all variables.

Summary statistics for the variables used in our main specifications are displayed by race in Table 1, with "white" referring solely to non-Hispanic whites. Our primary outcomes of interest are offered wages and whether a job offer was received. Each of the follow-up surveys asked whether respondents had received any job offer within the past 7 days; if so, how many; and what the wage associated with the best offer was.

In any given week, about 6.5 percent of job seekers received at least one job offer, and conditional on receiving any offer, approximately 84 percent of individuals were offered exactly one job. Blacks filled out 1.3 more applications per week than whites, but they were slightly less likely to apply for white-collar jobs.[15] Interestingly, and in contrast to results in the audit study literature, blacks had arrival rates that are 2.2 percentage points higher than those of whites—at least in the raw data. However, the mean offered hourly wage for whites equals $23.40, which is far in excess of the $12.30 offered to blacks. Differences in the distribution of wage offers, as shown in Figure 2, are stark. The modal job offer is roughly the same across racial groups, but the right tail of the offer distribution for whites is significantly larger. A Kolmogrov-Smirnov test for equality in distributions is rejected at the 1 percent level.

The remainder of Table 1 presents summary statistics for other variables used in our analysis. About 45 percent of white respondents and 58 percent of black respondents were female. On average, blacks were almost 7 years younger than

[15] Pager and Pedulla (2012) report that blacks and whites apply to similar jobs, but blacks consider a greater range of possibilities.

Table 1

Summary Statistics for New Jersey Unemployment Insurance Data

Variable	Full Sample	White	Black	Other Race
Demographics:				
Female	.471	.445	.579	.439
	(.499)	(.497)	(.494)	(.597)
Age	41.6	44.7	37.6	38.2
	(13.5)	(13.6)	(12.3)	(12.6)
Educational Attainment:				
High School Dropout	.072	.039	.109	.112
	(.258)	(.193)	(.311)	(.316)
High School Graduate	.301	.293	.360	.269
	(.459)	(.455)	(.480)	(.443)
Some College	.352	.349	.362	.349
	(.478)	(.477)	(.481)	(.477)
College Graduate	.275	.319	.169	.270
	(.447)	(.466)	(.375)	(.444)
Weeks Unemployed (at beginning of survey)	42.9	42.2	46.6	41.4
	(30.9)	(30.2)	(34.2)	(29.4)
Previous job:				
Previous Weekly Earnings (U.S.$)	857	992	603	779
	(688)	(767)	(438)	(604)
Tenure on Previous Job (years)	4.69	5.32	3.47	4.35
	(6.46)	(6.84)	(5.49)	(6.19)
Last Job Was Temporary	.157	.140	.190	.164
	(.364)	(.347)	(.393)	(.370)
Quit Last Job	.045	.039	.062	.044
	(.208)	(.194)	(.241)	(.204)
Laid Off from Last Job	.798	.820	.747	.793
	(.401)	(.384)	(.435)	(.406)
Previous Industry:				
Mining, utilities, and construction	.066	.086	.024	.059
	(.249)	(.280)	(.153)	(.235)
Manufacturing	.099	.090	.049	.165
	(.299)	(.286)	(.215)	(.372)
Wholesale and retail trade	.243	.247	.242	.236
	(.429)	(.431)	(.429)	(.425)
Professional, scientific, and technical services	.329	.326	.376	.295
	(.470)	(.469)	(.485)	(.456)
Educational and health care services	.119	.097	.162	.131
	(.323)	(.296)	(.369)	(.337)
Arts, recreation, and food services	.062	.063	.064	.058
	(.241)	(.243)	(.245)	(.233)
Other services	.018	.019	.019	.013
	(.132)	(.138)	(.135)	(.112)
Public administration	.063	.071	.064	.045
	(.243)	(.257)	(.246)	(.207)
Job Offers:				
Received Offer Last Week	.065	.053	.075	.082
	(.247)	(.223)	(.263)	(.274)
Offered Hourly Wage	18.5	23.4	12.3	16.9
	(13.6)	(16.0)	(4.8)	(12.2)
Accepted Job Offer in Hand	.788	.791	.860	.741
	(.409)	(.407)	(.348)	(.439)
Accepted Hourly Wage	20.0	25.3	12.3	18.7
	(15.2)	(16.9)	(6.0)	(14.9)

Table 1 (*Continued*)

Variable	Full Sample	White	Black	Other Race
Search intensity:				
Looking for Job Last Week	.818	.821	.832	.802
	(.386)	(.383)	(.374)	(.399)
Hours Spent Searching (per week)	10.5	10.9	11.0	9.3
	(14.3)	(14.0)	(15.9)	(13.8)
Number of Applications	4.88	4.75	6.03	4.43
	(7.76)	(7.99)	(8.29)	(6.87)
Search Strategies:				
Did Not Apply to Some Job Ad Last Week	.078	.084	.092	.059
	(.268)	(.277)	(.290)	(.235)
Did Not Apply Since Too Far Away	.295	.353	.314	.163
	(.456)	(.478)	(.465)	(.370)
Distance Traveled Looking for Work (miles)	16.22	14.82	15.93	18.97
	(22.84)	(22.49)	(19.75)	(24.99)
Applied to Any White-Collar Job Last Week	.804	.838	.801	.746
	(.397)	(.369)	(.399)	(.435)
Applied to Any Blue-Collar Job Last Week	.089	.065	.052	.154
	(.285)	(.247)	(.222)	(.361)
Applied to Any Service Job Last Week	.054	.051	.061	.055
	(.227)	(.220)	(.240)	(.228)
Reservation Wage	20.0	22.1	15.8	19.1
	(11.5)	(12.4)	(8.0)	(11.0)
Bargaining and Discounting:				
Bargained over Offer in Hand	.245	.297	.149	.241
	(.430)	(.457)	(.357)	(.429)
Chose $20 Now over $40 in 12 Weeks	.540	.459	.712	.571
	(.498)	(.498)	(.453)	(.495)
N	5,251	3,566	839	846

Note. Entries are weighted means and standard deviations, in parentheses, for those individuals with nonmissing information.

whites and were much more likely to be single. Consistent with national patterns, blacks in our sample were less educated than whites. For instance, about 32 percent of white respondents reported having at least a college education, compared with 17 percent of black respondents.[16] Blacks had longer ongoing unemployment spells than whites, earned almost $400 less per week on their previous job, and accumulated substantially less tenure than whites. We also have data on the industry in which an individual previously worked. Blacks were less

[16] Compared with unemployed residents of New Jersey in the 2009 American Community Survey or the 2010 March Current Population Survey, data from our respondents show broadly similar educational attainment, although self-reported dropouts are somewhat underrepresented and individuals with an incomplete college education are overrepresented. It is important to note that the numbers pertaining to educational achievement in Table 1 do not match those in table 2.1 of Krueger and Mueller (2011). To compare the sample of survey respondents with the universe of unemployment insurance (UI) recipients, they convert administrative data on years of schooling (for both populations) into degrees but rely on self-reported educational attainment throughout the rest of their analysis.

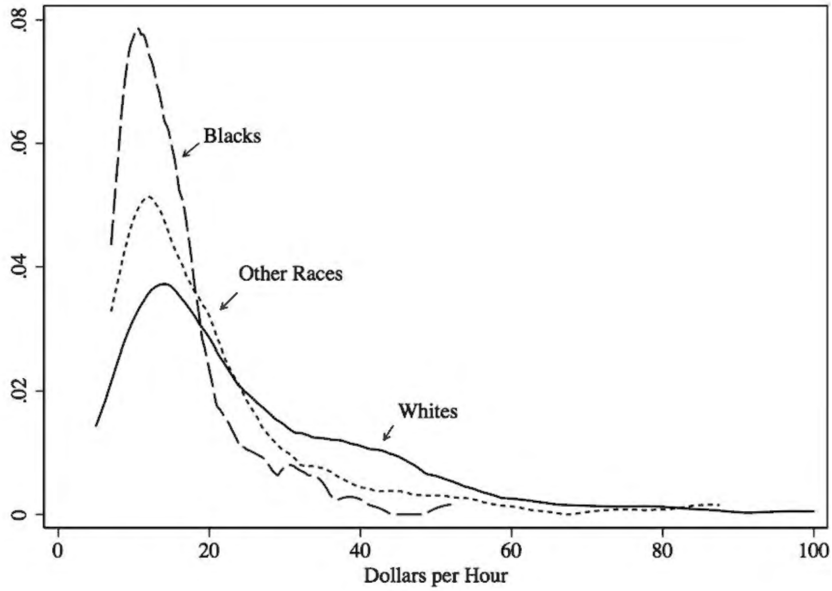

Figure 2. Distribution of wage offers, by race

likely than whites to have worked in construction and manufacturing. Instead, they were more concentrated in education and health care services.[17]

4.2. Identifying Discrimination

Four important features of the data described above enable us to conduct a novel test for racial discrimination: information on wage offers (as opposed to just accepted wages), search strategies and intensities, administrative data on previous earnings, and timing (the data were collected during a period of mass unemployment). Although the model outlined above guides our thinking about the data, the key idea of our empirical test is independent of the theory. Under the null hypothesis of no discrimination, wages will proxy for marginal productivity. Hence, conditional on wage on the previous job, there should be no racial differences in wage offers, as controlling for previous wage implicitly accounts for the market valuation of all factors such as ability, noncognitive skills, and so on, which previous research treated as unobservable. Observing racial differences in wage offers after accounting for previous earnings would, therefore, lead us to reject the null hypothesis of no discrimination.

More formally, let $\omega_{i,j}$ denote the wage associated with the jth job offer to

[17] Compared with the universe of UI recipients, construction workers are slightly underrepresented in the weighted data (Krueger and Mueller 2011).

individual i, and consider the data-generating process

$$\ln(\omega_{i,j}) = \kappa_0 + \text{Race}_i'\Gamma_0 + \boldsymbol{X}_i'\beta_0 + \alpha_0\theta_i + \nu_{i,j}, \tag{6}$$

where Race_i is an indicator variable for i's race, $\boldsymbol{X}_i$ are individual-level covariates, θ_i denotes i's unobserved ability, and $\nu_{i,j}$ is white noise.[18] Although race and skill level generally will be correlated, if employers do not discriminate it must be the case that $\Gamma_0 = 0$.

Further assume that previous earnings, w_i, are related to unobserved skill in the following sense:

$$\theta_i = \mu + \lambda\ln(w_i) + \boldsymbol{X}_i'\zeta + u_i,$$

where $\lambda \neq 0$, $E[u_i] = 0$, $\text{Cov}(\boldsymbol{X}_i, u_i) = 0$, and $\text{Cov}(\ln(w_i), u_i) = 0$. This assumption is fairly benign. As a matter of statistically decomposing θ, one can always write unobserved skill as a linear combination of previous earnings and individual-level covariates. In this case, u_i corresponds to the least squares residual, which means that $E[u_i] = 0$ and $\text{Cov}(\ln(w_i), u_i) = 0$ are automatically satisfied. For $\lambda \neq 0$ to hold, it needs to be the case that even after controlling for $\boldsymbol{X}_i$, previous wages predict ability, as seems likely.[19] With this framework in mind, we can formalize our test of discrimination.

Proposition 1. Let $\hat{\Gamma}_{\text{OLS}}$ denote the ordinary least squares (OLS) estimate of Γ in the empirical model

$$\ln(\omega_{i,j}) = \kappa + \text{Race}_i'\Gamma + \boldsymbol{X}_i'\beta + \varphi\ln(w_i) + \varepsilon_{i,j},$$

and suppose that the true data-generating process is given by equation (6). If (i) $\text{Cov}(\text{Race}_i, \nu_{i,j}) = 0$ and (ii) $\text{Cov}(\text{Race}_i, u_i) \geq 0$, then $\text{plim}\hat{\Gamma}_{\text{OLS}} \geq \Gamma_0$.

Proof. See Appendix A.

The proposition implies that if assumptions i and ii hold, one can reject the null hypothesis of no discrimination whenever $\hat{\Gamma}_{\text{OLS}}$ is negative and statistically significant.

It is very important to note at the outset that the two identifying assumptions are not innocuous. Assumption ii requires that, conditional on covariates, previous earnings do not systematically overstate blacks' true ability. If, for instance, blacks were subject to discrimination while performing their last job, then they would earn less than equally skilled whites, which implies that $\text{Cov}(\text{Race}_i, u_i) > 0$, and the assumption holds. Yet if blacks actually earned higher wages relative to their white counterparts (for instance, because previous earnings capture marginal productivity plus a diversity preference or because

[18] We assume that skills command positive returns; that is, $\alpha_0 \geq 0$.

[19] It is important to note that previous earnings, w_i, generally will not follow the same data-generating process as wage offers, ω_i. For instance, workers might gain seniority or engage in additional training, or wages might increase with tenure in the firm, as new information about workers' productivity arrives (compare with the model in Section 3). It would, therefore, be incorrect to set previous wages equal to wage offers and rearrange equation (6) to recover θ_i.

whites have significantly more amenities that are not captured in previous wages), then $\mathrm{Cov}(\mathrm{Race}_i, u_i) < 0$, and our test will overstate the true amount of discrimination.[20]

Another potential violation of assumption ii arises from severe measurement error in previous wages. To see this, consider the extreme case in which wages are pure noise. If blacks have lower mean ability than whites, then it will be the case that $\mathrm{Cov}(\mathrm{Race}_i, u_i) < 0$. More generally, assumption ii fails whenever previous wage is a noisy enough measure of productivity for blacks to appear to be paid more on average than their equally skilled white counterparts, despite the possible impact of discrimination. In an attempt to mitigate this concern, we use administrative data on previous earnings, which is likely much more accurate than the usual self-reported data. In fact, administrative information usually serves as the benchmark in evaluation studies of various surveys (see, for instance, Rodgers, Brown, and Duncan [1993] and Bound et al. [1994], and the discussion in Bound, Brown, and Mathiowetz [2001]). However, as individuals' true productivity is unobservable, we ultimately are not able to rule out this concern completely.[21]

The first assumption is that $\mathrm{Cov}(\mathrm{Race}_i, \nu_{i,j}) = 0$, which is automatically satisfied if $\nu_{i,j}$ is, in fact, white noise. Intuitively, this assumption requires that, ceteris paribus, blacks and whites do not systematically differ in their search behavior and draw wage offers from a comparable sets of firms. If, for instance, blacks are more likely than whites to receive offers from firms particularly hard-hit by the 2009 recession, then this assumption might be violated. Similarly, assumption i might fail if whites have lower discount rates and firms adjust their offers accordingly or if blacks do not bargain as aggressively as whites over offers.

In contrast to the second assumption, however, assumption i is testable. Exploiting the richness of our data, we can account for racial differences in search strategies, search intensity, geographic location, industry and occupation, and bargaining behavior, as well as discounting. Reassuringly, there is little indication that differences along these lines explain our findings.

5. Racial Disparities in Job Finding and Offered Wages

5.1. *Testing for Discrimination*

Table 2 presents a series of estimates of racial disparities in offered wages. The coefficients therein correspond to the empirical model

$$\ln(\omega_{i,j}) = \mathrm{Race}_i'\Gamma + \boldsymbol{X}_i'\beta + \varphi\ln(w_i) + \varepsilon_{i,j}. \quad (7)$$

[20] Adding controls that proxy for whether an individual had health insurance on her previous job does not alter the forthcoming results.

[21] Another potential violation of assumption ii comes from differential selection into the pool of unemployed individuals. If, conditional on previous wage, whites who are let go are more able than their black counterparts, then assumption ii would fail, and the findings of our test would be invalid.

All regressions include a full set of race indicators, with "white" serving as the omitted category. Consequently, the coefficients on race capture the gap between the named racial category and whites. Our primary emphasis, however, is on the black-white wage gap. The vector of other covariates included in the specification, denoted $\boldsymbol{X}_i$, varies across columns in Table 2. As one moves to the right, the set of covariates steadily grows. In all instances is the estimation carried out using weighted least squares with weights corresponding to the sampling weights calculated by Krueger and Mueller (2011). Standard errors are clustered by individual to account for the fact that some job seekers received more than one offer during the survey period.[22]

Column 1 in Table 2 displays racial differences in offered wages after controlling for age and gender. The raw black-white difference is estimated to equal −.404 log point, or approximately 33 percent. Accounting for racial disparities in formal education reduces the gap by .118 log point, but it remains economically large and statistically significant.

These estimates are somewhat larger than those obtained from commonly used data sets such as the CPS or the NLSY79. Note, however, that there is an important difference compared with previous work. The estimates in Table 2 refer to wage offers as opposed to actual wages. The search model in Section 3 predicts that, depending on arrival rates and the shape of the wage offer distribution, racial differences in accepted wages may be smaller or larger than that in offers. For completeness, online Table C1 displays estimates for accepted wages in the New Jersey UI data. Despite the fact that almost all papers estimating Mincerian regressions rely on self-reported wages, our preferred outcome is wage offers, as this allows us to circumvent potentially important selection bias in the offers that workers accept.

Column 3 of Table 2 adds log Previous Weekly Earnings to the set of controls.[23] As evidenced by the stark increase in R^2, previous earnings are an excellent predictor of offered wages. Importantly, controlling for previous earnings almost halves the difference in offered wages between blacks and whites. However, with −.169 log point, the gap does remain economically large and statistically significant. On a purely descriptive level, these results imply that blacks suffer a greater decline in wages after being laid off. Under the identifying assumptions of our approach, we can reject the null hypothesis of no discrimination.

Yet so far it is unclear whether assumptions i and ii do, indeed, hold. In particular, it is questionable whether blacks and whites receive job offers from a comparable set of firms, especially during the 2009 recession, the impact of

[22] Because of the small sample size in the New Jersey UI data, we pool males and females in our main regressions. For a detailed set of results differentiated by gender, see online Appendix C. Broadly summarized, estimates of the black-white wage gap are qualitatively similar for males and females, but they are much more precise for the latter.

[23] Since we have administrative data on average weekly earnings during the previous year but only self-reported information on hours on the last job (which are reported to have varied in many cases), we choose to control for previous weekly earnings instead of hourly wages. Our main results are qualitatively and quantitatively robust to controlling for previous hourly wages instead.

Table 2
Racial Differences in Hourly Wage Offers (log Value): New Jersey Unemployment Insurance Data

Independent Variable	(1)	(2)	(3)	(4)	(5)
Black	−.404	−.286	−.169	−.165	−.160
	(.059)	(.061)	(.056)	(.057)	(.060)
Other Race	−.243	−.233	−.170	−.173	−.201
	(.085)	(.072)	(.063)	(.065)	(.063)
Female	−.162	−.248	−.101	−.096	−.114
	(.064)	(.062)	(.051)	(.050)	(.048)
Age	.041	.042	.003	.007	.002
	(.016)	(.016)	(.015)	(.015)	(.014)
Age^2/100	−.040	−.042	−.000	−.004	.001
	(.020)	(.020)	(.019)	(.018)	(.017)
High School Graduate		−.074	−.124	−.142	−.086
		(.120)	(.102)	(.108)	(.099)
Some College		.251	.060	.041	.084
		(.122)	(.102)	(.107)	(.094)
College Graduate		.451	.164	.150	.206
		(.129)	(.101)	(.106)	(.098)
log Previous Weekly Earnings			.416	.408	.417
			(.052)	(.051)	(.051)
Weeks Unemployed				−.005	−.006
				(.003)	(.003)
Weeks Unemployed^2/100				.003	.003
				(.002)	(.002)
Quit Last Job				.019	−.006
				(.112)	(.105)
Last Job Was Temporary				.054	.044
				(.068)	(.069)
Constant	2.068	1.840	−.005	.093	.146
	(.321)	(.342)	(.368)	(.384)	(.390)
Previous Industry fixed effects	No	No	No	No	Yes
R^2	.192	.320	.479	.489	.512

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. $N =$ 1,194.

which differed greatly by industry. To address this concern, columns 4 and 5 in Table 2 add controls for duration of unemployment, the reason why the last job ended, and previous industry fixed effects. While these factors are correlated with offered wages, the racial gaps remain almost unchanged. After controlling for previous earnings and the full set of covariates, we find that the difference in wage offers between blacks and whites in the New Jersey UI data equals −.160 log point, or 14.7 percent, and is statistically highly significant. Thus, if the assumptions in proposition 1 hold, then we estimate a lower bound on the impact of discrimination of almost 40 percent of the raw gap.[24]

[24] Lang and Manove (2011) find almost no racial differences in wages at the top of the skill distribution. While we have tried interacting previous earnings with race, our results are not suffi-

Table 3

Racial Differences in Current Hourly Wages (log Value): Displaced Workers Survey, 2008 and 2010

Independent Variable	(1)	(2)	(3)	(4)
Black	−.278	−.224	−.157	−.170
	(.040)	(.038)	(.035)	(.035)
Other Race	−.252	−.137	−.060	−.063
	(.028)	(.029)	(.026)	(.026)
Female	−.253	−.293	−.182	−.193
	(.022)	(.021)	(.019)	(.021)
Age	.106	.086	.058	.058
	(.009)	(.008)	(.008)	(.008)
Age^2/100	−.123	−.098	−.069	−.068
	(.011)	(.011)	(.010)	(.010)
Urban	.167	.106	.073	.088
	(.029)	(.029)	(.028)	(.028)
High School Graduate		.172	.111	.106
		(.034)	(.031)	(.031)
Some College		.268	.180	.165
		(.036)	(.033)	(.034)
College Graduate		.627	.399	.366
		(.037)	(.036)	(.036)
log Previous Weekly Earnings			.515	.493
			(.020)	(.021)
Constant	4.250	4.352	1.739	1.909
	(.151)	(.149)	(.169)	(.184)
Previous Industry fixed effects	No	No	No	Yes
R^2	.151	.235	.374	.390

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. Year and state fixed effects are included in all regressions. $N = 5{,}098$.

To demonstrate that this result is not an artifact of our data from New Jersey, Table 3 presents estimates similar to those in Table 2, as obtained from the nationally representative Displaced Workers Survey (DWS), a biannual supplement to the CPS administered to workers who lost their job during the previous 3 years. Table 4 presents such estimates for the NLSY79.[25] Although low response rates and selective attrition are less of a concern in these data, we do not observe wage offers and must therefore rely on accepted wages instead. Reassuringly, the same basic pattern seen in the New Jersey UI data emerges. Controlling for earnings on the respondent's previous job substantially reduces racial disparities, but the black-white gap remains statistically significant and economically large—approximately 15.6 percent in the DWS and about 8 percent in the NLSY79.[26]

ciently precise to draw any conclusions about whether racial differences in offered wages are smaller or larger among previously highly paid individuals.

[25] See Appendix B for a description of these data.

[26] Our specifications using the NLSY79 also control for AFQT scores. The fact that we still observe sizable differences by race rules out the possibility that our results are driven by the possibility that less skilled workers have greater wage penalties for losing their jobs and that skill is negatively correlated with being black.

Table 4

Racial Differences in Current Hourly Wages (log Value): National Longitudinal Survey of Youth 1979, 2000–2006

Independent Variable	(1)	(2)	(3)	(4)	(5)
Black	−.306	−.225	−.129	−.081	−.083
	(.020)	(.018)	(.020)	(.016)	(.016)
Hispanic	−.104	.013	.068	.053	.049
	(.025)	(.023)	(.023)	(.018)	(.017)
Female	−.267	−.297	−.287	−.177	−.138
	(.020)	(.018)	(.018)	(.015)	(.016)
Age	−.099	−.048	−.042	−.032	−.010
	(.061)	(.056)	(.055)	(.048)	(.047)
$Age^2/100$	.122	.058	.051	.035	.011
	(.075)	(.068)	(.067)	(.059)	(.057)
Urban	.183	.096	.089	.061	.059
	(.022)	(.019)	(.019)	(.016)	(.016)
Years of Schooling		.094	.072	.049	.052
		(.004)	(.004)	(.003)	(.003)
AFQT			.109	.060	.055
			(.013)	(.010)	(.010)
$AFQT^2$			−.012	−.022	−.021
			(.011)	(.009)	(.008)
log Previous Wage				.520	.493
				(.022)	(.022)
Constant	4.662	2.427	2.616	1.421	.979
	(1.259)	(1.148)	(1.141)	(.996)	(.967)
Previous Industry fixed effects	No	No	No	No	Yes
R^2	.101	.280	.299	.438	.470

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. The sample consists of black, Hispanic, and non-Hispanic white individuals in the civil labor force who change employers between two successive interview rounds. Hence, Previous Wage refers to the wage associated with the job held at the time of the last interview. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. Year fixed effects are included in all regressions. $N =$ 6,074.

The latter estimate is remarkably close to that in Neal and Johnson (1996), but its interpretation is very different. While Neal and Johnson (1996) argue that their estimates of the residual black-white wage gap likely overstates the extent of labor market discrimination, under assumptions i and ii above, the point estimate in this paper represents a lower bound.

As an additional robustness check, online Table C1 explores the sensitivity of our results across a variety of specifications. Column 1 contains our main result and is identical to column 5 of Table 2. Column 2 adds a quadratic in previous wage as an additional covariate. Column 3 alters the outcome to be the best offer an individual receives (rather than including all offers), and we investigate racial differences in accepted (rather than offered) wages in column 4. Our next specification check uses the nearest-neighbor matching estimator in Abadie and Imbens (2002), which provides a more flexible way of controlling for our set of covariates. In column 6, we allow β to differ by race and estimate the racial gap by assuming that blacks have white coefficients (Oaxaca 1973). Across these specifications, the results are similar, but large standard errors make inference challenging. The black coefficient ranges from −.227 (.031) for the matching estimator to −.107 (.068) when we use accepted wage. In half the cases, imprecision prohibits us from distinguishing the coefficient on our robustness tests from zero or the baseline result in column 1.

Finally, one might be worried that our results are driven by measurement error or mean reversion in wages.[27] A simple way to address this issue is to restrict the coefficient on previous wage to equal one. If measurement error or mean reversion were, indeed, driving our results, one would expect the coefficient on race in this specification to equal zero. The result is presented in column 7 of online Table C1. The coefficient on Black decreases .049 log point (to −.111), and the standard error increases by more than 50 percent, which leaves the coefficient on Black economically large but statistically insignificant. It is unclear whether the differences between column 1 and column 7 of online Table C1 are due to true measurement error in the wages reported to the New Jersey Department of Labor and Workforce Development or to imposing restrictions on the data that are not warranted.[28]

5.2. A Partial Test of the Identifying Assumptions

In this section, we turn to the assumptions in proposition 1. Recall that for our empirical approach to identify a lower bound on the impact of discrimination, be it statistical or taste based, it needs to be the case that (i) blacks and whites do not systematically differ in their search behavior, search intensity, discount rates, the markets in which they search, and so on; that is, $\mathrm{Cov}(\mathrm{Race}_i, \nu_{i,j}) = 0$; and (ii) previous wages do not systematically overstate blacks' true ability; that is, $\mathrm{Cov}(\mathrm{Race}_i, u_i) \geq 0$.

Assumption ii is not directly testable with our data. If previous wage equals marginal product, then this assumption holds and our approach will correctly identify racial discrimination. If previous wage is a function of both productivity and differential treatment by race, then our approach will provide a lower bound on the impact of discrimination. Conversely, if previous wage captures marginal product plus a diversity preference, the effect of affirmative action, or significant

[27] We are grateful to Joseph Altonji and David Card for making this point.

[28] A further test of our approach is to see whether variables known to influence wages but not related to information have a coefficient close to zero after controlling for previous earnings. One such variable is Age. While Age is an important predictor of wages in columns 1–3 of Table 2, once we control for previous wage, the coefficients on Age and Age^2 are nearly zero and are relatively precisely estimated. A similar pattern can be observed with respect to being married. This is not the case for our measures of educational attainment. Note, however, that if the market possesses more information about college than high school graduates (see Arcidiacono, Bayer, and Hizmo 2010), then one might expect there to be wage differentials by educational attainment, even conditional on previous wage. The fact that the coefficients on educational attainment decline but do not decrease to zero after controlling for previous earnings is thus consistent with a model of statistical discrimination. We are grateful to Kevin Lang and an anonymous referee for pointing this out to us.

measurement error in wages (for example, mean reversion, unmeasured amenities, and so on), then assumption ii is violated and we will overstate the amount of discrimination in the market.[29]

By contrast, assumption i is testable. Guided by the model in Section 3, we explore five plausible violations of this assumption: spatial mismatch, racial differences in search behavior, search strategies, bargaining, and discount rates. On a purely descriptive level, the results below constitute one of the first analyses of racial differences in job finding (for complementary evidence based on the same data set, see Pager and Pedulla 2012).

5.2.1. Spatial Mismatch

Table 5 probes whether differences in the geographic location of blacks and whites across New Jersey can explain the estimated wage gaps (Cutler and Glaeser 1997; Jencks and Mayer 1990; Kain 1968; Holzer 1991). For instance, if blacks live in blighted neighborhoods with few high-paying jobs, then this may lead them to draw wage offers from a different set of firms, and it may reconcile why, even conditional on previous earnings, they are offered lower wages. While a priori plausible—particularly during a period of mass unemployment such as the 2009 recession—the spatial mismatch theory receives only scant support in the data.

We test for the impact of spatial mismatch in two ways. First, we control for the distance that respondents reported traveling to search for a job to proxy for searching in similar markets (recall that we also include industry fixed effects). Second, we include metropolitan area fixed effects. In both cases, the coefficient on Black is not greatly affected: adding controls for distance traveled to search increases the coefficient on Black to −.179 (.057), and adding fixed effects reduces it to −.152 (.065).

5.2.2. Search Intensity

Next we turn to racial differences in search behavior. Table 6 displays estimates of equation (7) in which the outcome variable has been replaced with proxies for search intensity. For each outcome, we estimate raw racial differences accounting only for gender and age (baseline controls), as well as gaps controlling for the full set of covariates including previous earnings (full set of controls).

Taking the point estimates at face value, blacks are 2.7 percentage points more likely to be looking for work during the last 7 days, spend 1.5 hours more per week searching, write an additional 1.3 applications, and consequently are about

[29] It is unlikely that diversity preferences or affirmative action can explain our results, as our estimates are significantly larger than conventional estimates of the impact of Title VII of the Civil Rights Act, affirmative action, or the Civil Rights Act of 1991 (Ashenfelter and Heckman 1976; Heckman and Payner 1989; Chay 1998; Leonard 1984a, 1984b, 1990; Smith and Welch 1984). While some industries have seen large relative improvements for blacks, in particular with respect to employment (Heckman and Payner 1989; McCrary 2007), the impact of affirmative action on the labor market as a whole has been much more limited.

Table 5
Testing for Spatial Mismatch in Hourly Wage Offers (log Value)

Independent Variable	(1)	(2)	(3)	(4)
Black	−.404	−.160	−.179	−.152
	(.059)	(.060)	(.057)	(.065)
Other Race	−.243	−.200	−.207	−.224
	(.085)	(.063)	(.060)	(.069)
Female	−.162	−.114	−.103	−.106
	(.064)	(.048)	(.047)	(.047)
Age	.041	.002	.005	.005
	(.016)	(.014)	(.013)	(.011)
$Age^2/100$	−.040	−.001	−.003	−.002
	(.020)	(.017)	(.016)	(.013)
High School Graduate		−.086	−.072	−.077
		(.099)	(.085)	(.099)
Some College		.084	.114	.172
		(.094)	(.081)	(.090)
College Graduate		.206	.204	.238
		(.098)	(.083)	(.091)
log Previous Weekly Earnings		.417	.397	.413
		(.051)	(.047)	(.045)
Weeks Unemployed		−.006	−.006	−.007
		(.003)	(.002)	(.002)
Weeks $Unemployed^2/100$		.003	.004	.004
		(.002)	(.001)	(.001)
Quit Last Job		−.006	−.049	.002
		(.105)	(.104)	(.097)
Last Job Was Temporary		.044	.036	.069
		(.069)	(.071)	(.065)
Distance Traveled Looking for Work:				
1–5 miles			−.230	−.244
			(.071)	(.073)
6–10 miles			−.133	−.172
			(.070)	(.073)
11–25 miles			−.223	−.219
			(.055)	(.055)
26–50 miles			−.217	−.203
			(.062)	(.060)
51–100 miles			.093	.128
			(.144)	(.138)
More than 100 miles			.003	−.033
			(.189)	(.158)
Constant	2.068	.146	.374	.275
	(.321)	(.390)	(.372)	(.365)
Previous Industry fixed effects	No	Yes	Yes	Yes
Metropolitan Area fixed effects	No	No	No	Yes
R^2	.192	.512	.539	.580

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. $N =$ 1,194.

2 percentage points more likely to receive a job offer. Although these differences are in most cases not very precisely estimated, we are able to rule out moderately sized gaps in favor of whites. Interestingly, blacks are significantly more likely to report contacting employers directly, contacting public employment agencies, and using informal networks. Thus, if anything, unemployed blacks appear to search more intensely for work across a variety of channels and generate more offers than their white counterparts.

5.2.3. Search Strategies

Racial differences in search strategies are investigated in Table 7. For the sake of brevity, we restrict attention to six outcomes.[30]

Broadly summarized, the evidence in Table 7 does not reveal significant differences in search strategies between blacks and whites. For instance, blacks appear to be only slightly more likely than whites to ignore job ads and are even less likely than whites to do so because of transportation difficulties. Moreover, after controlling for a host of individual characteristics (including previous earnings and previous industry), there are almost no differences in the types of jobs to which blacks and whites apply, although in the raw data blacks are significantly less likely to apply to white-collar jobs. There is one exception, however. Blacks are estimated to be more likely to accept an offer in hand, even after controlling for previous wages. Although this difference is nontrivial in magnitude, it is not statistically significant due to large standard errors.

5.2.4. Bargaining and Discount Rates

Estimates of racial differences in bargaining and a proxy for discount rates are presented in Table 8. Columns 1 and 2 show that after adjusting for only age and gender, blacks are approximately 11 percentage points less likely to negotiate wages conditional on being offered a job. Yet after including our full set of covariates, this difference halves and becomes statistically insignificant.

As part of the initial survey, respondents were asked whether they preferred to receive a $20 Visa gift card within a few days or a $40 gift card in 12 weeks. Columns 3 and 4 of Table 8 use respondents' actual choice as an admittedly crude proxy for discount rates.[31] As evidenced by point estimates of 22 and 15.6 percentage points, blacks are substantially more likely than whites to opt for $20 now, which suggests that differences in time preferences may explain part of the gap, at least if employers take these into account when making job offers.[32]

[30] Results from other variables that proxy for search strategies are available from the authors on request.

[31] It is not entirely clear whether the choice between a $20 gift card within a few days or a $40 one in 12 weeks elicits only time preferences or whether issues of trust and the like also play a role, despite assurances that the respondent would receive the gift card even if she did not participate in any of the follow-up surveys.

[32] Note, however, that such behavior might in itself be considered discriminatory.

Table 6
Racial Differences in Search Intensity

	Baseline Controls		Full Set of Controls	
Dependent Variable	Black	Other Race	Black	Other Race
Looking for Job Last Week	.027	−.013	.027	−.022
	(.024)	(.025)	(.024)	(.024)
Hours Spent Searching	.679	−1.494	1.533	−.981
	(1.104)	(.924)	(1.109)	(.850)
Hours spent:				
Contacting employers directly	.989	.266	.927	.160
	(.325)	(.274)	(.330)	(.227)
Contacting public employment agency	.968	.224	.929	.159
	(.313)	(.254)	(.320)	(.217)
Contacting private employment agency	.193	.317	.289	.340
	(.075)	(.172)	(.090)	(.152)
Contacting friends or relatives	.189	.337	.287	.349
	(.078)	(.182)	(.093)	(.161)
Contacting school or university employment eenter	.059	−.004	.053	−.010
	(.049)	(.035)	(.049)	(.034)
Contacting union or professional registers	−.066	−.116	.011	−.085
	(.047)	(.032)	(.054)	(.031)
Attending job training programs or courses	−.152	−.123	−.114	−.088
	(.182)	(.200)	(.206)	(.207)
Placing or answering ads	−.379	−.746	−.337	−.715
	(.229)	(.228)	(.237)	(.231)
Going to interviews	.081	.033	.132	.066
	(.079)	(.066)	(.074)	(.061)
Sending out résumés or filling out applications	.638	−.404	.827	−.307
	(.439)	(.296)	(.435)	(.290)
Looking for ads	−.459	−1.096	−.399	−1.048
	(.329)	(.282)	(.328)	(.277)
Other job search activities	−.122	−.139	−.114	−.120
	(.103)	(.101)	(.110)	(.100)
Number of Applications	1.259	−.470	1.253	−.376
	(.611)	(.447)	(.608)	(.430)
Received Offer Last Week	.018	.021	.021	.024
	(.013)	(.013)	(.012)	(.013)

Note. Entries are coefficients and standard errors on racial identifiers obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions.

5.2.5. Understanding the Impact of Search Strategies, Search Intensity, Bargaining, and Discount Rates

Table 9 provides a concise summary of the effect of each of the five potential violations of assumption i. The estimates shown in Table 9 correspond to the coefficient on Race_i—that is, Γ in specification (7)—and denote racial differences in offered wages relative to whites. If assumption i does indeed hold, then adding additional controls for each of the outcomes investigated above should not decrease the gap in a statistically meaningful way.

Table 7
Racial Differences in Search Strategies

Independent Variable	Did Not Apply to Job Ad (1)	(2)	Did Not Apply Since Too Far Away (3)	(4)	Applied to White-Collar Jobs (5)	(6)	Applied to Blue-Collar Jobs (7)	(8)	Applied to Service Jobs (9)	(10)	Accepted Offer (11)	(12)
Black	.002	.014	−.038	−.006	−.074	−.011	.028	−.000	.001	−.006	.087	.050
	(.017)	(.016)	(.075)	(.064)	(.037)	(.033)	(.019)	(.022)	(.020)	(.019)	(.062)	(.062)
Other Race	−.032	−.032	−.196	−.177	−.071	−.058	.088	.066	−.003	.004	−.051	−.026
	(.017)	(.018)	(.065)	(.062)	(.047)	(.034)	(.041)	(.030)	(.021)	(.018)	(.075)	(.063)
Female	.000	.002	−.052	−.051	.207	.188	−.158	−.127	.013	−.000	−.053	−.025
	(.013)	(.013)	(.057)	(.049)	(.031)	(.025)	(.022)	(.017)	(.014)	(.015)	(.058)	(.054)
Age	−.002	−.003	.000	−.001	.005	−.001	.003	.001	−.006	−.000	−.009	−.012
	(.004)	(.003)	(.013)	(.010)	(.008)	(.007)	(.006)	(.005)	(.004)	(.004)	(.010)	(.010)
$Age^2/100$	−.001	.002	−.003	−.001	−.005	−.000	−.002	−.000	−.005	.000	.011	.012
	(.004)	(.003)	(.013)	(.010)	(.009)	(.007)	(.007)	(.006)	(.004)	(.004)	(.011)	(.011)
High School Graduate		−.004		−.164		.223		−.215		−.004		.032
		(.025)		(.143)		(.094)		(.090)		(.052)		(.145)
Some College		.010		−.006		.422		−.312		−.053		.011
		(.025)		(.153)		(.092)		(.090)		(.048)		(.149)
College Graduate		.028		−.028		.436		−.326		−.030		.078
		(.024)		(.144)		(.092)		(.091)		(.049)		(.150)
log Previous Weekly Earnings		.015		.016		.095		−.014		−.054		.002
		(.009)		(.039)		(.022)		(.014)		(.017)		(.048)
Weeks Unemployed		.000		.004		.708		−.001		−.000		.004
		(.001)		(.002)		(.154)		(.001)		(.001)		(.002)
Weeks $Unemployed^2$ (÷100)		−.000		−.002		−.001		−.001		−.000		−.003
		(.000)		(.001)		(.001)		(.000)		(.000)		(.001)
Quit Last Job		−.024		.005		−.080		.110		.010		.043
		(.019)		(.115)		(.057)		(.068)		(.034)		(.076)
Last Job Was Temporary		−.031		−.081		−.033		−.019		−.007		.217
		(.011)		(.060)		(.037)		(.022)		(.024)		(.054)
Constant	.138	.054	.418	.286	.615	−.246	.032	.461	.188	.460	.987	.868
	(.094)	(.110)	(.354)	(.318)	(.186)	(.180)	(.110)	(.131)	(.083)	(.110)	(.225)	(.329)
Previous Industry fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R^2	.004	.020	.038	.096	.075	.302	.107	.258	.482	.141	.017	.149
N	26,901	26,901	3,218	3,218	22,569	22,569	22,569	22,569	22,569	22,569	1,023	1,023

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions.

Table 8
Racial Differences in Bargaining and Discount Rates

	Bargained over Offer in Hand		Chose \$20 Now over \$40 in 12 Weeks	
Independent Variable	(1)	(2)	(3)	(4)
Black	−.112	−.050	.220	.156
	(.054)	(.053)	(.029)	(.029)
Other Race	−.041	−.051	.076	.059
	(.066)	(.057)	(.033)	(.031)
Female	−.057	−.038	−.044	−.067
	(.047)	(.048)	(.024)	(.024)
Age	.009	.002	−.020	−.010
	(.009)	(.009)	(.005)	(.005)
Age^2 (÷100)	−.008	.000	.016	.006
	(.011)	(.010)	(.006)	(.006)
High School Graduate		−.039		.015
		(.113)		(.053)
Some College		−.001		−.054
		(.117)		(.053)
College Graduate		.080		−.187
		(.118)		(.055)
		.066		−.091
		(.044)		(.021)
Weeks Unemployed		−.006		.000
		(.002)		(.001)
Weeks $Unemployed^2$/100		.004		.000
		(.001)		(.001)
Quit Last Job		−.060		−.010
		(.090)		(.054)
Last Job Was Temporary		−.131		.039
		(.074)		(.032)
Constant	.100	−.075	1.033	1.470
	(.189)	(.314)	(.106)	(.152)
Previous Industry fixed effects	No	Yes	No	Yes
R^2	.027	.097	.079	.136
N	1,225	1,225	5,230	5,230

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions.

Column 1 displays racial differences after accounting for the set of covariates used in Table 2. Each subsequent column controls for one or more of the different dimensions of search behavior explored in Tables 5–8. For instance, column 2 also includes controls for whether the respondent was looking for work during the last week, how many hours she spent searching, and the number of applications she wrote. Column 3 adds indicator variables for whether she did not apply to any job ad she saw within the last week, whether she did so because the job was too far away, and whether she applied to a job opening in any of 22 major groups in the Standard Occupational Classification (SOC) system.

Despite the richness of the included covariates, the residual black-white dif-

Table 9
Residual Racial Differences in Hourly Wage Offers (log Value)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black	−.160	−.127	−.162	−.152	−.160	−.152	−.160
	(.060)	(.058)	(.054)	(.059)	(.060)	(.065)	(.060)
Other Race	−.201	−.184	−.191	−.194	−.199	−.224	−.199
	(.063)	(.063)	(.050)	(.063)	(.063)	(.069)	(.063)
Controls:							
Search intensity	No	Yes	No	No	No	No	Yes
Search strategy	No	No	Yes	No	No	No	Yes
Bargaining	No	No	No	Yes	No	No	Yes
Discounting	No	No	No	No	Yes	No	Yes
Distance Traveled Looking for Work and Metropolitan Area fixed effects	No	No	No	No	No	Yes	Yes

Note. Entries are coefficients and standard errors obtained by estimating the empirical model by weighted least squares. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. Controls for demographics, education, Weeks Unemployed, Quit Last Job, Previous Industry, and Previous Earnings are included in all regressions.

ference in wage offers remains almost unaffected. Separately accounting for the effect of search intensity, search strategies, bargaining, time preferences, or geographic location reduces the gap by, at most, .033 log point, compared with a standard error of .060. Even after jointly controlling for all of these factors, job offers to blacks are still estimated to be .160 log point lower than offers to observationally equivalent whites.[33] In sum, under the identifying assumptions of proposition 1, we can conclude that discrimination accounts for at least one-third of the black-white wage gap.

6. Evidence Consistent with a Search-Matching Model

Recall that the findings from our empirical test are independent of whether discrimination is statistical or animus based. We argue, however, that the search-matching model in Section 3 constitutes a natural way to rationalize the data.

As explained above, there are several ways to introduce racial differences into this framework. If, for instance, blacks are on average less skilled than whites, that is, $\mu_W > \mu_B$ (as documented by Neal and Johnson [1996], among others), then group membership constitutes a valuable signal of ability, and unemployed black workers will be offered lower initial wages than equally qualified whites. In symbols, $E[\omega_W] > E[\omega_B]$.

Our model provides three additional predictions, which distinguish it from a number of alternative theories. First, similar to the findings of Black (1995), statistical discrimination in a search framework yields a lower reservation wage for the disadvantaged group. Second, our model predicts the black-white wage

[33] This difference is strikingly similar in size to that reported by Lang and Manove (2011) for the NLSY79. after controlling for both education and AFQT scores.

gap to narrow with tenure in the firm. Third, if blacks are significantly more likely than whites to experience separations (as argued in Kletzer [1998]), then aggregate wage gaps across firms will increase with labor market experience. Below, we explore the extent to which these predictions are borne out in the data.

6.1. Racial Differences in Reservation Wages

Table 10 presents evidence on racial differences in reservation wages (see Holzer [1986] for earlier evidence).[34] Reservation wages are gleaned from a question that asks, "Suppose someone offered you a job today, what is the lowest wage or salary you would accept (before deductions) for the type of work you are looking for?"[35]

Column 1 in Table 10 shows that, after accounting for age and gender, blacks are willing to accept substantially lower offers than whites. The gap in reservation wages with these baseline controls equals −.232 log point. Accounting for educational achievement reduces the difference to −.160 log point, but it remains statistically significant. Similar to wage offers, earnings on the previous job are a very good predictor of reservation wages. Moving from column 2 to column 3, we see that R^2 increases from .323 to .549 and reduces the coefficient on Black to −.067 (.028). Put differently, on average, blacks are willing to accept almost 7 percent lower wages than whites who previously earned just as much. Adding additional controls for the duration of unemployment, the reason the last job ended, or previous industry fixed effects does little to alter this result.

6.2. Returns to Tenure within Firms

In our model, blacks having lower mean premarket skill results in their having lower intermediate-stage wages. Over time, however, employers learn workers' true marginal product, which results in no wage differences among equally productive tenured individuals. This provides a testable prediction: within the firm, racial differences in wages should narrow with tenure.

Empirical evidence in support of this prediction is presented in Table 11 (see also Goldsmith, Hamilton, and Darity 2006). Using New Jersey UI data, Table 11 displays estimates of our empirical specification in which the outcome variable has been replaced by the natural logarithm of previous earnings. In addition, we control for tenure on the previous job and interact it with race. As predicted, wages increase with tenure for all racial groups. More important, however, blacks have a return-to-tenure rate that is 1.1 percentage points higher than that for

[34] It is important to note that differences in reservation wages need not necessarily be due to discriminatory hiring practices. Instead, they might simply reflect racial differences in discount rates or savings that could be used to smooth consumption while unemployed (Chetty 2008).

[35] Krueger and Mueller (2011) report that whites are more likely than blacks to accept wage offers below their stated reservation wage, which could be due to a variety of factors, such as misinterpretation of the survey question or individuals adjusting their reservation wage as they search.

Table 10
Racial Differences in Reservation Wages (log Value)

Independent Variable	(1)	(2)	(3)	(4)	(5)
Black	−.232	−.160	−.067	−.064	−.072
	(.033)	(.033)	(.028)	(.029)	(.028)
Other Race	−.112	−.087	−.053	−.051	−.061
	(.046)	(.040)	(.036)	(.035)	(.031)
Female	−.144	−.154	−.068	−.066	−.075
	(.029)	(.026)	(.026)	(.026)	(.027)
Age	.049	.044	.017	.017	.016
	(.006)	(.005)	(.005)	(.005)	(.005)
Age^2 ($\div$100)	−.047	−.041	−.015	−.016	−.015
	(.006)	(.006)	(.006)	(.006)	(.006)
High School Graduate		.065	.010	.005	.021
		(.047)	(.041)	(.040)	(.041)
Some College		.286	.150	.145	.157
		(.045)	(.040)	(.040)	(.038)
College Graduate		.554	.290	.283	.290
		(.045)	(.040)	(.041)	(.038)
log Previous Weekly Earnings			.424	.423	.415
			(.027)	(.028)	(.029)
Weeks Unemployed				−.002	−.002
				(.001)	(.001)
Weeks Unemployed^2 ($\div$100)				−.001	.001
				(.000)	(.000)
Quit Last Job				−.040	−.039
				(.052)	(.045)
Last Job Was Temporary				.014	.011
				(.037)	(.034)
Constant	1.873	1.656	−.405	−.356	−.293
	(.126)	(.120)	(.174)	(.181)	(.189)
Previous Industry fixed effects	No	No	No	No	Yes
R^2	.162	.323	.549	.551	.559

Note. Entries are coefficients and standard errors obtained by weighted least squares estimation. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. $N =$ 25,436.

whites. Not only does the black-white difference in the return to tenure carry the expected sign, it is also highly statistically significant.

A potential confounding factor of the above approach is that blacks are more likely than whites to have short tenure, and, as Toppel (1991) shows, the returns to tenure are heavily weighted to the first years on a job. Thus, the above analysis could be confusing nonlinearities in the returns to tenure as evidence in favor of the model. To test this possibility, we reestimate the model in Table 11 on various subsamples of the data. The results are shown in online Table C9. For comparison, column 1 displays the baseline findings from Table 11. Column 2 excludes all individuals with less than 2 years of tenure, and column 3 does so for workers who had been with their previous employer for more than 10 years. Last, column 4 excludes both of these groups. Reading across columns, the

Table 11

Racial Differences in log Weekly Earnings on Previous Job and the Return to Tenure: New Jersey Unemployment Insurance Data

Independent Variable	(1)	(2)	(3)
Black	−.320	−.219	−.252
	(.032)	(.029)	(.032)
Other Race	−.140	−.116	−.123
	(.036)	(.032)	(.039)
Female	−.229	−.213	−.210
	(.025)	(.023)	(.022)
Age	.072	.059	.054
	(.005)	(.005)	(.005)
Age^2 ($\div$100)	−.068	−.055	−.054
	(.006)	(.006)	(.006)
High School Graduate		.135	.130
		(.049)	(.047)
Some College		.314	.316
		(.048)	(.047)
College Graduate		.643	.655
		(.048)	(.047)
Weeks Unemployed		−.001	−.000
		(.001)	(.001)
Weeks Unemployed^2 ($\div$100)		−.000	−.001
		(.001)	(.001)
Quit Last Job		−.024	.006
		(.050)	(.049)
Last Job was Temporary		−.153	−.110
		(.034)	(.033)
Tenure on Previous Job			.019
			(.002)
Tenure on Previous Job × Black			.011
			(.004)
Tenure on Previous Job × Other Race			.000
			(.005)
Constant	5.042	5.060	5.096
	(.111)	(.119)	(.115)
Previous Industry fixed effects	No	Yes	Yes
R^2	.207	.407	.415

Note. Entries are coefficients and standard errors obtained by estimating the empirical model by weighted least squares. Heteroskedasticity-robust standard errors are in parentheses. In addition to the variables shown, indicator variables for missing values on each covariate are also included in the regressions. $N =$ 5,207.

interaction term becomes, if anything, larger (but is less precisely estimated), which lets us rule out this concern.

6.2. Aggregate Racial Gaps across Firms

In stark contrast to the previous discussion, when workers who have been with the same firm for a considerable time lose their job, the black-white wage gap reemerges as these workers are matched with a new firm. Thus, if blacks are sufficiently more likely than whites to incur a separation, the black-white wage gap will increase with labor market experience. Altonji and Pierret (2001) demonstrate that racial differences are small when workers just enter the labor market but widen with potential experience (see also Oettinger 1996).

Table 12 augments Altonji and Pierret's (2001) original analysis of the NLSY79. Using data for the period 1979–92, columns 1–4 replicate the upper panel of their table 1.[36] The negative coefficient on the interaction term between Black and Potential Experience indicates that the black-white wage widens by roughly 1 percent per year of experience.

In column 5, we extend the analysis of Altonji and Pierret (2001) by adding Tenure and its interaction with race to the set of covariates. As predicted by our theory, blacks experience a return to tenure that is 1.1 percentage points higher than that for whites. Not only is the difference statistically significant, it is also strikingly close to our estimate from the New Jersey UI data. The remainder of Table 12 shows that this result is robust to including additional years of data and does not depend on Altonji and Pierret's (2001) choice to control for a cubic time trend interacted with Black. Although the black-white wage gap increases as individuals change employers and accumulate labor market experience, it is estimated to be substantially smaller among those who have been with the same firm for a long time.

7. Discussion

To conclude our analysis, we explore the extent to which discrimination based on animus or differences in premarket skills can account for our set of facts: blacks incur larger losses than whites with job separations, have lower reservation wages, and garner higher returns to tenure in a firm.

7.1. Taste-Based Discrimination

Discriminatory tastes of employers, coworkers, or customers can give rise to black-white wage differences (Becker 1957). If, for instance, some fraction of employers incurs disutility from interacting with black workers, then the wage offered to blacks must be lower than that of whites for the employer to be indifferent. In equilibrium, the marginal discriminator determines the black-white wage gap. Similar arguments apply when customers or coworkers discriminate based on animus.[37]

Traditional models of taste-based discrimination can rationalize equilibrium wage gaps, but they have difficulty explaining why, after losing their jobs, blacks are offered lower wages than previously equally well paid whites. Unless the

[36] Altonji and Pierret (2001) had a sample of 21,058 observations, while our sample is 21,026. This small difference is due to missing information on wages in the work history file of the NLSY79. Nevertheless, our estimates are almost identical to theirs. For a detailed description of the sample construction procedures, see Appendix B or the data appendix in Altonji and Pierret (2001).

[37] In a rare empirical test of this theory, Charles and Guryan (2008) argue that animus accounts for about one-quarter of the observed black-white wage gap.

Table 12

Racial Differences in the Return to Tenure and Labor Market Experience, by log Hourly Wage: National Longitudinal Survey of Youth 1979

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	−.159	−.158	−.006	−.055	−.087	−.089	−.034	−.032	−.069
	(.026)	(.026)	(.057)	(.067)	(.065)	(.065)	(.064)	(.064)	(.018)
Years of Schooling	.055	.077	.060	.073	.080	.080	.069	.070	.071
	(.011)	(.014)	(.011)	(.014)	(.014)	(.014)	(.012)	(.012)	(.012)
AFQT	.083	.001	.083	.031	.014	.013	.077	.079	.068
	(.014)	(.033)	(.014)	(.039)	(.038)	(.039)	(.036)	(.036)	(.031)
Years of Schooling × Potential Experience	−.003	−.002	−.001	−.002	−.004	−.004	−.002	−.002	−.002
	(.003)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
AFQT × Potential Experience		.008		.005	.005	.005	.001	.002	.003
		(.003)		(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Black × Potential Experience			−.014	−.010	−.009	−.009	−.010	−.009	−.006
			(.005)	(.006)	(.006)	(.006)	(.006)	(.006)	(.002)
Tenure					.030	.030	.019	.019	.019
					(.002)	(.003)	(.002)	(.002)	(.002)
Tenure × Black					.011	.012	.008	.007	.007
					(.004)	(.005)	(.003)	(.003)	(.003)
Tenure × AFQT						.000		−.001	−.001
						(.002)		(.001)	(.001)
Black × cubic time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Time period	1979–92	1979–92	1979–92	1979–92	1979–92	1979–92	1979–2006	1979–2006	1979–2006
R^2	.292	.296	.296	.297	.326	.326	.406	.406	.406
N	21,026	21,026	21,026	21,026	21,026	21,026	33,931	33,931	33,931

Note. Entries in columns 1–4 are coefficients and standard errors obtained by replicating the upper panel of table 1 in Altonji and Pierret (2001). Columns 5-9 extend their analysis by controlling for tenure and adding additional years of data. As in Altonji and Pierret (2001), the sample consists of black and white non-Hispanic males, and the base year of the time trends is the last year of the period under consideration. Heteroskedasticity-robust standard errors, clustered by individual, are in parentheses. See Appendix B as well as Altonji and Pierret (2001), for a description of the sample construction procedures. Additional controls for Urban Residence, two-digit Occupation at First Job , Year fixed effects, Years of Schooling × cubic time trend, and AFQT × cubic time trend are included in all regressions.

marginal discriminator changes during the year when our data were collected, controlling for previous earnings should eliminate the black-white wage gap, even in a world where taste-based discrimination is present. This is inconsistent with what we find in the data.

In contrast to neoclassical models of the labor market, models that include search frictions, such as Black (1995), predict that minorities will, on average, be paid lower wages as long as any discriminatory employer is in the market. Expecting discrimination, blacks have lower reservation wages than whites. Moreover, in a world in which blacks invest more in firm-specific human capital than whites, the former have higher returns to tenure and incur larger losses from job separations. The key to this explanation is that blacks matched with a nondiscriminatory employer may have more of an incentive to invest in firm-specific human capital, as the market provides less insurance than it does for equally skilled whites.[38] Thus, depending on whether blacks do, in fact, invest more than whites in firm-specific human capital, our set of facts may also be consistent with a taste-based model of discrimination that features search frictions.

7.2. Racial Differences in Premarket Factors

A separate strand of the literature relies on disparities in premarket factors, such as education and skill, to explain racial wage gaps.[39] For instance, O'Neill (1990) and Neal and Johnson (1996) show that after controlling for AFQT scores, which presumably measure skill prior to entry into the labor market, the black-white wage difference in the NLSY79 narrows substantially or even reverses. This theory finds mixed support in our data.

Racial differences in premarket factors can explain racial differences in reservation wages. Also, to the extent that the price of skill increases with labor market experience or skill gaps widen with labor market experience, racial differences in premarket factors can also explain why aggregate wage gaps increase with age. Indeed, Altonji and Pierret (2001) demonstrate that the importance of AFQT increases with labor market experience (Table 12).

To explain why blacks incur a higher wage penalty from job separations than whites, one must assume that there is a significant amount of firm-specific investment among workers with low premarket skills. Moreover, a premarket theory predicts that, conditional on AFQT scores, there are no racial differences in the returns to tenure. This prediction is at odds with the data. Columns 7 and 8 in Table 12 reveal that, conditional on AFQT scores, blacks have significantly higher returns to tenure than whites. Without controlling for AFQT scores, blacks have a rate of return to tenure that is 1.1 percentage points higher. Accounting

[38] Of course, blacks might acquire more firm-specific human capital than whites for reasons other than discrimination. Hu and Taber (2011), for instance, present a model in which different types of workers simply perform different tasks.

[39] It is theoretically unclear whether disparities in premarket factors cause racial wage gaps or whether the latter lead minorities to invest less and thereby cause the former (for equilibrium analyses, see Lundberg and Startz 1983; Coate and Loury 1993).

for AFQT scores increases the coefficient to 1.2 percentage points.[40] Given these data, it is unlikely that premarket factors alone explain the patterns in our data, although racial differences in premarket factors are important determinants of black-white inequality and may even give rise to statistical discrimination.

7.3. A Note on Statistical Discrimination and the Return to Education

Neal (2006) describes an important critique regarding the empirical content of models of statistical discrimination, as developed by Arrow (1973) and Coate and Loury (1993). In these models, blacks anticipate discrimination in the labor market and, expecting this, invest less in skills than whites. Empirically, however, many have shown that the return to investment in skills is, if anything, higher for blacks than whites, which is inconsistent with simple versions of the theory (Neal and Johnson 1996).

While we do not explicitly model investment in skills, there are several viable ways to incorporate this critique into our theory. Generally, it is important to note that the return to education is determined at the margin. In other words, just because statistical discrimination causes blacks to earn, on average, lower wages, it does not necessarily mean that the function mapping educational investment into earnings must be flatter. In equilibrium it will be the case that the gross return on investment equals individuals' opportunity cost (Becker 1962, 1993). Hence, if blacks are more likely to be cash constrained or face higher cost of investing, then this alone may give rise to statistical discrimination and explain higher estimated payoffs, assuming decreasing marginal returns.

Moreover, blacks may experience weakly higher returns on investment if education reduces the variance in the signal to employers (for empirical evidence, see Arcidiacono, Bayer, and Hizmo 2010), which is, in fact, the assumption that drives the model in Lang and Manove (2011). In their analysis, blacks overinvest in education to signal their (unobserved) ability. If μ depends not only on group investments but also on environmental factors, such as school or neighborhood quality, then in equilibrium it might well be the case that $\mu_B < \mu_W$, despite higher returns for blacks.

An alternative way to rationalize higher returns for blacks is to assume that educational attainment decreases the probability of job loss (Kletzer 1998). In this case, blacks would experience a higher return to education than whites because it shields them from costly job losses.

7.4. Concluding Remarks

The racial wage gap is a robust empirical regularity. Simple comparisons of mean wages typically find black-white wage differences in excess of 30 percent. While there exists almost unanimous consensus that differences in formal schooling and premarket skill are important determinants of the observed disparities,

[40] Accounting for nonlinearities in the returns to tenure by including years on the job in four categories does not alter this result.

the extent to which discrimination contributes to the gap remains one of the most debated issues in the social sciences.

In this paper, we develop a novel test for the presence of discrimination using a newly available data set. Results from this test suggest that the impact of racial discrimination on offered wages is at least one third of the raw black-white wage gap in our data, subject to our identifying assumptions.

Taking our estimates at face value, labor market discrimination appears to be an impediment to racial income equality. This suggests that alleviating racial inequality may take a combination of policies to both eliminate barriers to investing in education and other premarket skills and enforce antidiscrimination policies, so that minorities are rewarded for those skills.

Appendix A

Technical Appendix: Proofs

A1. Proof of Proposition 1

Replacing θ_i in equation (6) with $\theta_i = \mu + \lambda \ln(w_i) + \boldsymbol{X}_i'\zeta + u_i$ yields the estimable specification

$$\ln(\omega_{i,j}) = (\kappa_0 + \alpha_0\mu) + \text{Race}_i'\Gamma_0 + \boldsymbol{X}_i'(\beta_0 + \alpha_0\zeta) + \alpha_0\lambda\ln(w_i) + (\alpha_0 u_i + \nu_{i,j}),$$

in which only $\alpha_0 u_i + \nu_{i,j}$ is not observed by the econometrician.

The Frisch-Waugh theorem (Frisch and Waugh 1933) implies that

$$\text{plim}\,\hat{\Gamma}_{\text{OLS}} = \Gamma_0 + \frac{\text{Cov}(\widetilde{\text{Race}}_i,\ \alpha_0 u_i + \nu_{i,j})}{\text{Var}(\widetilde{\text{Race}}_i)},$$

where $\widetilde{\text{Race}}_i$ denotes the residual from projecting Race_i onto $\boldsymbol{X}_i$, $\ln(w_i)$, and a constant. Since $\text{Var}(\widetilde{\text{Race}}_i) > 0$, it suffices to show that $\text{Cov}(\widetilde{\text{Race}}_i,\ \alpha_0 u_i + \nu_{i,j}) \geq 0$.

From the definition of $\widetilde{\text{Race}}_i$, and from using the Frisch-Waugh theorem again, one obtains

$$\text{Cov}(\widetilde{\text{Race}}_i,\ \alpha_0 u_i + \nu_{i,j}) = \text{Cov}\Bigg\{\text{Race}_i - \chi - \boldsymbol{X}_i'\frac{\text{Cov}(\text{Race}_i,\ \widetilde{\boldsymbol{X}}_i)}{\text{Var}(\widetilde{\boldsymbol{X}}_i)} - \frac{\text{Cov}(\text{Race}_i,\ \widetilde{\ln(w_i)})}{\text{Var}(\widetilde{\ln(w_i)})}\ln(w_i),\ \alpha_0 u_i + \nu_{i,j}\Bigg\},$$

where

$$\chi \equiv E[\text{Race}_i] - E[\boldsymbol{X}_i']\frac{\text{Cov}(\text{Race}_i,\ \widetilde{\boldsymbol{X}}_i)}{\text{Var}(\widetilde{\boldsymbol{X}}_i)} - \frac{\text{Cov}(\text{Race}_i,\ \widetilde{\ln(w_i)})}{\text{Var}(\widetilde{\ln(w_i)})}E[\ln(w_i)],$$

and $\widetilde{\boldsymbol{X}}_i$ ($\widetilde{\ln(w_i)}$) corresponds to the residual from projecting $\boldsymbol{X}_i$ ($\ln(w_i)$) onto

$\ln(w_i)$ ($\boldsymbol{X}_i$) and a constant. Since $\boldsymbol{X}_i \perp (u_i, \nu_{i,j})$ and $\ln(w_i) \perp (u_i, \nu_{i,j})$, we have that $\text{Cov}(\widetilde{\text{Race}}_i, \alpha_0 u_i + \nu_{i,j}) = \text{Cov}(\text{Race}_i, \alpha_0 u_i + \nu_{i,j})$. Assumptions i and ii in proposition 1 ensure that $\text{Cov}(\text{Race}_i, \alpha_0 u_i + \nu_{i,j}) \geq 0$. Hence, $\text{plim}\,\hat{\Gamma}_{\text{OLS}} \geq \Gamma_0$, as desired. Q.E.D.

A2. Proof of Proposition 2

Proposition 2. The sequence of reservation wages is increasing; that is, $\theta^\star > \omega^\star$.

Note that $\Pr[\theta > \theta^\star \mid \theta + \xi] > 0$ and $J(\theta) \geq Q$, with the inequality being strict for $\theta > \theta^\star$. It then follows that

$$\omega^\star + \beta \int J(s)\, dF(s \mid \omega^\star, \sigma^2_{\theta \mid \theta+\xi}) > \omega^\star + \beta Q.$$

Hence, equation (4) implies $\omega^\star < (1-\beta)Q$. Recognizing that $\theta^\star = (1-\beta)Q$ by equation (2) completes the proof. Q.E.D.

A3. Proof of Proposition 3

Proposition 3. On average wages increase with tenure in the firm; that is, $E[\omega \mid \omega > \omega^\star] < E[\theta \mid \theta > \theta^\star, \omega > \omega^\star]$.

For completeness, we reproduce the proof in Sargent (1987, pp. 79).

First, note that the mean wage of previously unemployed workers is given by

$$E[\omega \mid \omega > \omega^\star] \equiv \overline{\omega} = \frac{\int_{\omega^\star}^{\infty} \omega\, dG(\omega \mid \mu, \sigma^2_\omega)}{\int_{\omega^\star}^{\infty} dG(\omega \mid \mu, \sigma^2_\omega)}$$

and that of tenured workers equals

$$E[\theta \mid \theta > \theta^\star, \omega > \omega^\star] \equiv \overline{\theta} = \frac{\int_{\omega^\star}^{\infty}\int_{\theta^\star}^{\infty} \theta\, dF(\theta \mid \omega, \sigma^2_{\theta \mid \theta+\xi})\, dG(\omega \mid \mu, \sigma^2_\omega)}{\int_{\omega^\star}^{\infty}\int_{\theta^\star}^{\infty} dF(\theta \mid \omega, \sigma^2_{\theta \mid \theta+\xi})\, dG(\omega \mid \mu, \sigma^2_\omega)}.$$

From the standard properties of normally distributed random variables, it follows that

$$\overline{\omega} = \mu + \sigma_\omega \frac{\phi((\mu - \omega^\star)/\sigma_\omega)}{\Phi((\mu - \omega^\star)/\sigma_\omega)}.$$

Moreover, since $\int_{\theta^\star}^{\infty} dF(\theta \mid \omega, \sigma^2_{\theta \mid \theta+\xi}) = \Phi((\omega - \theta^\star)/\sigma_{\theta \mid \theta+\xi})$, we also have that

$$\int_{\theta^\star}^{\infty} \theta\, dF(\theta \mid \omega, \sigma^2_{\theta \mid \theta+\xi}) = \omega\Phi\left(\frac{\omega - \theta^\star}{\sigma_{\theta \mid \theta+\xi}}\right) + \sigma\phi\left(\frac{\omega - \theta^\star}{\sigma_{\theta \mid \theta+\xi}}\right) > \omega\Phi\left(\frac{\omega - \theta^\star}{\sigma_{\theta \mid \theta+\xi}}\right).$$

This implies

$$\overline{\theta} > \frac{\int_{\omega^\star}^{\infty} \omega \Phi((\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) dG(\omega \mid \mu,\ \sigma_\omega^2)}{\int_{\omega^\star}^{\infty} \Phi((\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) dG(\omega \mid \mu,\ \sigma_\omega^2)} \equiv \tilde{\theta}.$$

Hence, it suffices to show that $\tilde{\theta} \geq \overline{\omega}$.

Next, let $g(\omega \mid \mu,\ \sigma_\omega^2)$ denote the probability density function associated with $G(\omega|\mu,\ \sigma_\omega^2)$, and note that the density of observed wages in the second stage is given by

$$\tilde{g}(\omega \mid \mu,\ \sigma_\omega^2) = \begin{cases} \dfrac{g(\omega \mid \mu,\ \sigma_\omega^2)}{\int_{\omega^\star}^{\infty} dG(s \mid \mu,\ \sigma_\omega^2)} & \text{if } \omega \geq \omega^\star \\ 0 & \text{if } \omega < \omega^\star. \end{cases}$$

Similarly, define

$$\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2) = \begin{cases} \dfrac{\Phi((\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) g(\omega \mid \mu,\ \sigma_\omega^2)}{\int_{\omega^\star}^{\infty} \Phi((s - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) dG(s \mid \mu,\ \sigma_\omega^2)} & \text{if } \omega \geq \omega^\star \\ 0 & \text{if } \omega < \omega^\star, \end{cases}$$

and let $\tilde{G}(\omega \mid \mu,\ \sigma_\omega^2)$ and $\tilde{\tilde{G}}(\omega \mid \mu,\ \sigma_\omega^2)$ denote the cumulative distribution functions associated with $\tilde{g}(\omega \mid \mu,\ \sigma_\omega^2)$ and $\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2)$, respectively.

By construction,

$$\overline{\omega} = \int_{\omega^\star}^{\infty} \omega d\tilde{G}(\omega \mid \mu,\ \sigma_\omega^2) = \omega^\star + \int_{\omega^\star}^{\infty} 1 - \tilde{G}(\omega \mid \mu,\ \sigma_\omega^2) d\omega, \qquad \text{(A1)}$$

$$\tilde{\theta} = \int_{\omega^\star}^{\infty} \omega d\tilde{\tilde{G}}(\omega \mid \mu,\ \sigma_\omega^2) = \omega^\star + \int_{\omega^\star}^{\infty} 1 - \tilde{\tilde{G}}(\omega \mid \mu,\ \sigma_\omega^2) d\omega, \qquad \text{(A2)}$$

and

$$\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2) = \frac{\Phi((\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) \int_{\omega^\star}^{\infty} dG(s \mid \mu,\ \sigma_\omega^2)}{\int_{\omega^\star}^{\infty} \Phi((s - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}) dG(s \mid \mu,\ \sigma_\omega^2)} \tilde{g}(\omega \mid \mu,\ \sigma_\omega^2).$$

As for $\theta^\star > -\infty$, $\Phi[(\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}]$ is strictly increasing in ω and both $\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2)$ and $\tilde{g}(\omega \mid \mu,\ \sigma_\omega^2)$ are probability densities, it must be the case that there exists exactly one $\omega' > \omega^\star$ such that $\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2) \gtreqless \tilde{g}(\omega \mid \mu,\ \sigma_\omega^2)$ whenever $\omega \gtreqless \omega'$. To see this, note that $\int_{\omega^\star}^{\infty} dG(s \mid \mu,\ \sigma_\omega^2) > \int_{\omega^\star}^{\infty} \Phi[(s - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}] dG(s \mid \mu,\ \sigma_\omega^2)$ and that $0 \leq \Phi[(\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi}] \leq 1$. Thus, $\tilde{g}(\omega \mid \mu, \sigma_\omega^2) < \tilde{\tilde{g}}(\omega \mid \mu, \sigma_\omega^2)$ for sufficiently large ω. Also, recall that $\tilde{g}(\omega'' \mid \mu,\ \sigma_\omega^2) = \tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2) = 0$ for $\omega < \omega^\star$. The fact that $\Phi((\omega - \theta^\star)/\sigma_{\theta\,|\,\theta+\xi})$ is strictly increasing implies that $\tilde{\tilde{g}}(\omega \mid \mu,\ \sigma_\omega^2)/\tilde{g}(\omega \mid \mu,\ \sigma_\omega^2)$ is strictly increasing as well (for $\omega \geq \omega^\star$). Since both densities have to integrate up to one, we have that $\tilde{g}(\omega^\star \mid \mu, \sigma_\omega^2) > \tilde{\tilde{g}}(\omega^\star \mid \mu, \sigma_\omega^2)$ and that the density functions cross only once.

Therefore, $\tilde{\tilde{G}}(\omega \mid \mu,\ \sigma_\omega^2) < \tilde{G}(\omega \mid \mu,\ \sigma_\omega^2)$ for $\omega > \omega'$. From equations (A1) and (A2), it then follows that $\tilde{\theta} > \overline{\omega}$, which completes the proof. Q.E.D.

A4. Proof of Proposition 4

Proposition 4. Lower arrival rates result in lower reservation wages; that is, $d\theta^*/d\delta > 0$.

By equation (2), it suffices to show that $dQ/d\delta > 0$. Rearranging equation (5) and taking the derivative with respect to δ gives

$$\frac{dQ}{d\delta} = \frac{\beta(1-\beta)}{[1-\beta(1-\delta)]^2}\int V(w)dG(w \mid \mu, \sigma^2_\omega) + \frac{\delta\beta}{1-\beta(1-\delta)}\frac{d}{d\delta}\int V(w)dG(w \mid \mu, \sigma^2_\omega). \tag{A3}$$

From the definition of $V(\omega)$, it follows that, for all ω,

$$\frac{dV(\omega)}{d\delta} \geq \min\left\{\beta\frac{d}{d\delta}\int J(s)dF(s \mid \omega, \sigma^2_{\theta\mid\theta+\xi}), \frac{dQ}{d\delta}\right\}, \tag{A4}$$

and from equation (1), we have that

$$\frac{dJ(\theta)}{d\delta} \geq \min\left\{\frac{\alpha\beta}{1-\beta(1-\alpha)}\frac{dQ}{d\delta}, \frac{dQ}{d\delta}\right\}$$

for all θ.

Case 1. Suppose that $dQ/d\delta < 0$. Then, as $0 < \alpha\beta/[1-\beta(1-\alpha)] < 1$, $dJ(\theta)/d\delta \geq dQ/d\delta$. Thus, by expression (A4) we have that $dV(\omega)/d\delta \geq dQ/d\delta$.

Since the distribution of ω is nondegenerate normal and agents can always refuse negative offers, $V(\omega) > 0$ for all ω. Therefore,

$$\frac{dQ}{d\delta} > \frac{\delta\beta}{1-\beta(1-\delta)}\frac{d}{d\delta}\int V(w)dG(w \mid \mu, \sigma^2_\omega) \geq \frac{\delta\beta}{1-\beta(1-\delta)}\frac{dQ}{d\delta}. \tag{A5}$$

Letting $c \equiv \delta\beta/[1-\beta(1-\delta)]$ and rearranging expression (A5) gives

$$(1-c)\frac{dQ}{d\delta} > 0,$$

which produces a contradiction, as $0 < c < 1$.

Case 2. Suppose that $dQ/d\delta \geq 0$. Then, $dJ(\theta)/d\delta \geq \{\alpha\beta/[1-\beta(1-\alpha)]\} \times dQ/d\delta$ and $dV(\omega)/d\delta \geq \{\alpha\beta^2/[1-\beta(1-\alpha)]\} \times dQ/d\delta$. In this case, expression (A5) becomes

$$\frac{dQ}{d\delta} > \frac{\delta\beta}{1-\beta(1-\delta)}\frac{\alpha\beta^2}{1-\beta(1-\alpha)}\frac{dQ}{d\delta}.$$

Again, letting $c \equiv \delta\beta/[1-\beta(1-\delta)] \times \alpha\beta^2/[1-\beta(1-\alpha)]$ and rearranging gives

$$(1-c)\frac{dQ}{d\delta} > 0. \tag{A6}$$

Note that $0 < c < 1$. Hence, expression (A6) demonstrates that $dQ/d\delta > 0$, as desired. Q.E.D.

A5. Proof of Proposition 5

Proposition 5. A higher probability of job loss results in lower reservation wages; that is, $d\theta^*/d\alpha < 0$.

By equation (2), it suffices to show that $dQ/da < 0$. Rearranging equation (5) and taking the derivative with respect to α gives

$$\frac{dQ}{d\alpha} = \frac{\delta\beta}{1-\beta(1-\delta)}\frac{d}{d\alpha}\int V(w)dG(w \mid \mu,\ \sigma_\omega^2). \tag{A7}$$

From equation (3) we have that, for all ω,

$$\frac{dV(\omega)}{d\alpha} \le \max\left\{\beta\frac{d}{d\alpha}\int J(s)dF(s \mid \omega,\ \sigma^2_{\theta \mid \theta+\xi}),\ \frac{dQ}{d\alpha}\right\}, \tag{A8}$$

and from the definition of J—that is, $J(\theta) = \max\{\theta + \alpha\beta Q + (1-\alpha)\beta J(\theta),\ Q\}$—it follows that either

$$\frac{dJ(\theta)}{d\alpha} = \frac{\beta}{1-\beta(1-\alpha)}[Q - J(\theta)] + \frac{\alpha\beta}{1-\beta(1-\alpha)}\frac{dQ}{d\alpha} \tag{A9}$$

or

$$\frac{dJ(\theta)}{d\alpha} = \frac{dQ}{d\alpha}.$$

Since $J(\theta) \ge Q$ for all θ, it must be the case that $dJ(\theta)/d\alpha \le \max\{\alpha\beta/[1-\beta(1-\alpha)] \times dQ/d\alpha,\ dQ/d\alpha\}$.

Case 1. Suppose that $dQ/d\alpha > 0$. Then, as $0 < \alpha\beta/[1-\beta(1-\alpha)] < 1$, $dJ(\theta)/d\alpha \le dQ/d\alpha$. Thus, by expression (A8), we have that $dV(\omega)/d\alpha \le dQ/d\delta$.

This and equation (A7) imply

$$\frac{dQ}{d\alpha} \le \frac{\delta\beta}{1-\beta(1-\delta)}\frac{dQ}{d\alpha}. \tag{A10}$$

Letting $c \equiv \delta\beta/[1-\beta(1-\delta)]$ and rearranging expression (A10) gives

$$(1-c)\frac{dQ}{d\alpha} \le 0,$$

which produces a contradiction, since $0 < c < 1$.

Case 2. Suppose that $dQ/d\alpha \le 0$. Then, as $0 < \alpha\beta/[1-\beta(1-\alpha)] < 1$, $dJ(\theta)/d\alpha \le \alpha\beta/[1-\beta(1-\alpha)] \times dQ/d\alpha$. Thus, by expression (A8), we have that $dV(\omega)/da \le \alpha\beta^2/[1-\beta(1-\alpha)] \times dQ/d\delta$.

In this case, expression (A10) becomes

$$\frac{dQ}{d\alpha} \leq \frac{\delta\beta}{1 - \beta(1 - \delta)} \frac{\alpha\beta^2}{1 - \beta(1 - \alpha)} \frac{dQ}{d\alpha}.$$

Letting $c \equiv \delta\beta/[1 - \beta(1 - \delta)] \times \alpha\beta^2/[1 - \beta(1 - \alpha)]$ and rearranging expression (A10) gives

$$(1 - c)\frac{dQ}{d\alpha} \leq 0. \tag{A11}$$

Note that $0 < c < 1$. Hence, expression (A11) demonstrates that $dQ/d\alpha \leq 0$.

It remains to be shown that $dQ/d\alpha \neq 0$. By way of contradiction, suppose that $dQ/d\alpha = 0$. Then, $dJ(\theta)/d\alpha \leq 0$, with the inequality being strict for large enough θ (because of expression [A9] and the fact that $J(\theta) > Q$ for large θ). From equation (3), we have that $dV(\omega)/d\alpha \leq 0$, again with the inequality being strict for large enough values of ω. This, in connection with equation (A7), implies that $dQ/d\alpha < 0$, which produces the desired contradiction. Q.E.D.

Appendix B

Data Appendix

B1. New Jersey Unemployment Insurance Data

The following description of the New Jersey unemployment insurance (UI) data borrows heavily from Krueger and Mueller (2011). For a more detailed description of the data (in particular, of the sampling procedures, the survey instrument, or details of the implementation), the interested reader should consult the appendix in Kreuger and Mueller (2011).

During the fall of 2009 and early 2010, the Princeton University Survey Research Center (PSRC) collected high-frequency longitudinal information on unemployed individuals in New Jersey. Using a complete list of the approximately 360,000 individuals receiving UI as of September 28, 2009, the PSRC drew a stratified random sample of 63,813 individuals, oversampling the long-term unemployed. The sampled population was then contacted by the New Jersey Department of Labor and Workforce Development and invited (by e-mail or letter) to participate in a confidential Web survey for 12 consecutive weeks. Individuals who were unemployed for 60 weeks or longer at the beginning of the survey were later asked to participate in an additional 12 weeks of interviewing, for a maximum of 24 weeks. In this paper, however, we restrict attention to the first 12 weeks for all respondents.

Six to 10 days after the initial invitation, the PSCR made almost 10,000 phone calls encouraging nonresponders to participate in the survey. Two weeks thereafter, the survey was closed for new participants. To remind respondents to participate in the weekly follow-up surveys, e-mail invitations were sent out 7 days after completion of the most recent online interview, but not on Sundays or Mondays.[41]

As incentive to participate, respondents could choose either a $20 Visa gift card that would mailed to them within a few days or a $40 gift card that would be sent out after the 12-week survey period, regardless of whether the respondent completed any follow-up interviews.

The survey consisted of an initial entry questionnaire and weekly follow-up interviews. The former elicited information on demographics, previous employment, asset holdings, and spouses' employment status, whereas the latter inquired about job search activities, time use, reservation wages, and job offers, among other topics.

Unfortunately, only 6,025 of the sampled individuals participated in the entry wave of the survey, and those who responded to the initial interview completed only about 40 percent of weekly follow-ups. Moreover, participants were more educated, were more likely to be female, and had higher previous earnings than the baseline population. Using rich administrative data, Krueger and Mueller (2011) create sampling weights to adjust for the stratified survey design as well as nonresponse. Comparing characteristics of respondents with the universe of UI recipients along a number of dimensions, they conclude that the low response rate did not significantly skew the sample on observables, even on those that were not used in calculating the weights. Throughout our analysis, we use the weights created by Krueger and Mueller (2011). Moreover, we restrict attention to respondents with nonmissing information on race who are not listed as previously self-employed. Our final sample consists of 5,251 individuals and 26,901 person-week observations.

The following variables are used throughout our analysis:

Race. Race is a set of mutually exclusive indicator variables (that is, White, Black, Hispanic, Asian, and Other Race) that denote a respondent's racial identification. In our regressions, White serves as the omitted category.

Female. Female is an indicator variable that equals one if the respondent is female and zero otherwise.

Age. Age denotes the respondent's age in years.

Number of Children. Number of Children denotes the respondent's number of children. The variable is taken from the entry questionnaire, which asked, "How many children do you have?"

Marital Status. Martial Status is a set of mutually exclusive indicator variables (Single, Married, Divorced, Widowed, and Cohabitating But Not Married) that denote a respondent's marital status. The information is taken from the entry questionnaire, which asked, "What is your marital status?" The set of possible

[41] This restriction was imposed so the time diary would pertain to a weekday. Respondents who completed their most recent survey on a Sunday would receive their invitation on Tuesday of the following week.

answers included single (never married), married, divorced, widowed, and domestic partnership (living together but not married).

Educational Attainment. Educational Attainment is a set of mutually exclusive indicator variables (High School Dropout, High School Graduate, Some College, and College Graduate) that denote a respondent's highest level of completed education. The information is taken from the entry questionnaire, which asked, "What is the highest level of education you have completed?" The set of possible answers was some high school or less, high school diploma or equivalent, some college, college diploma, some graduate school, and graduate degree. For the purposes of this paper, we combine the last three answer choices into one category.

Weeks Unemployed. Weeks Unemployed denotes the duration of a respondent's unemployment spell as of the time she filled out a weekly follow-up survey. The variable was constructed using information from the entry questionnaire on the time that she lost her main job—that is, the one at which she worked the most hours—as well as on the time that she filled out the weekly survey.

Previous Weekly Earnings. Previous Weekly Earnings denotes the respondent's average weekly earnings during the past year. This variable was constructed using administrative information from the New Jersey Department of Labor and Workforce Development on earnings and weeks worked in the base year. Following Krueger and Mueller (2011), we discard observation with weekly earnings of less than $100 or more than $8,000 and impute base year earnings of $152,191 for those individuals subject to top coding.

Tenure on Previous Job. Tenure on Previous Job denotes the time (in years) that the respondent worked for her previous employer. The variable is taken from the entry questionnaire, which asked, "How many years had you worked for that employer when that job ended?"

Reason Last Job Ended. Reason Last Job Ended is a set of mutually exclusive indicator variables (Quit Last Job, Laid Off from Last Job, and Last Job Was Temporary) that denote why the respondent's last job ended. The information is taken from the entry questionnaire, which asked, "Did you lose or quit that job, or was it a temporary job that ended?" The set of possible answers was lost job, quit job, and temporary job ended. In our regressions, Laid Off from Last Job serves as the omitted category.

Previous Industry. Previous Industry refers to a set of nine indicator variables categorizing the industry in which participants previously worked according to the North American Industry Classification System.

Received Offer Last Week. Received Offer Last Week is an indicator variable for whether the respondent reports having received at least one job offer during the previous 7 days. The variable is taken from the weekly follow-up surveys, which asked, "In the last 7 days, did you receive any job offers? If yes, how many?"

Offered Hourly Wage. Offered Hourly Wage denotes the pay associated with the best job offer that the respondent received within the last 7 days, converted into an hourly rate. To construct this variable, we use information from the weekly follow-up surveys, which asked, "What was the wage or salary offered (before deductions)?" "Is that per year, per month, biweekly, weekly, or per hour?" and "How many hours a week would you have to work on that job?" We discard observations with missing information on hours and, following Krueger and Mueller (2011), observations with hourly offered wages of less than $5 or more than $100.

Accepted Job Offer in Hand. Accepted Job Offer in Hand is an indicator variable equal to one if the respondent accepted or thinks she will accept the best offer received within the last 7 days. The variable is taken from the weekly follow-up surveys, which asked, "Have you accepted or do you think you will accept this job offer?" The set of possible answers was yes, no, and don't know yet. Only respondents answering yes are coded as one; those who choose "Don't know yet" are set to missing.

Accepted Hourly Wage. Accepted Hourly Wage denotes the hourly wage associated with job offered that the respondent accepted, if she in fact did accept one. The variable is coded in the same way as the two previous variables.

Looking for Job Last Week. Looking for Job Last Week is an indicator variable equal to one if the respondent was actively looking for work during the previous 7 days. The variable is taken from the weekly follow-up surveys, which asked, "Have you done anything to find work during the last 7 days?"

Hours Spent Searching. Hours Spent Searching denotes the total number of hours that the respondent claims to have spent trying to find a job. The variable is constructed using information from the weekly follow-up surveys, which asked, "On the previous page you indicated what kind of methods you used to find work. In the last 7 days, about how many hours and minutes did you spend on each of those methods? Your best guess is okay." The set of methods included contacted employer directly, contacted public employment agency, contacted private employment agency, contacted friends or relatives, contacted school/university employment enter, checked union/professional registers, attended job training programs/courses, placed or answered ads, went to interview, sent out résumés/filled out applications, looked at ads, and other.

Number of Applications. Number of Applications denotes the number of jobs to which the respondent applied within the last 7 days. The variable is taken from the weekly follow-up surveys, which asked, "How many jobs did you apply to in the last 7 days?"

Did Not Apply to Some Job Ad Last Week. Did Not Apply to Some Job Ad Last Week is an indicator variable equal to one if the respondent did not apply for any job for which she was qualified—for any reason. The variable is constructed using information from the weekly follow-up surveys, which asked, "What about jobs you did not apply for? Did you find or hear about any jobs in the last 7 days for which you are qualified but did not apply for?"

Did Not Apply Since Too Far Away. Did Not Apply Since Too Far Away is an indicator variable equal to one if the respondent did not apply to any job for which she was qualified because it was too far away and equal to zero if she did not apply for any other reason. The variable is constructed using information from the weekly follow-up surveys, which asked, "What about jobs you did not apply for? Did you find or hear about any jobs in the last 7 days for which you are qualified but did not apply for?" and "Why not? Please check all that apply."

Applied to Any White-Collar Job Last Week. Applied to Any White-Collar Job Last Week is an indicator variable equal to one if the respondent applied to a white-collar job within the last 7 days and zero otherwise. The variable was constructed using information from the weekly follow-up surveys, which asked, "Please list the three most recent jobs you applied to in the last 7 days. If you applied to more than one of the same kind of job, list each job separately (examples of job titles include: waiter, computer technician, warehouse worker, administrative assistant, etc.)." The answers in the text entry fields were then categorized into Standard Occupational Categories (SOCs) by trained staff at the University of Wisconsin Survey Center. Ninety-seven percent of job titles could be successfully matched to three-digit SOC codes. We code the following 2000 SOC major groups as white collar: Management; Business and Financial Operations; Computer and Mathematical Occupations; Office and Administrative Support; Architecture and Engineering; Life, Physical, and Social Science; Community and Social Services; Legal Occupations; Education, Training, and Library Occupations; Arts, Design, Entertainment, Sports, and Media; and Healthcare Practitioners and Technical Occupations.

Applied to Any Blue-Collar Job Last Week. Applied to Any Blue-Collar Job Last Week is an indicator variable equal to one if the respondent applied to a blue-collar job within the last 7 days and zero otherwise. The variable was constructed using information from the weekly follow-up surveys, which asked, "Please list the three most recent jobs you applied to in the last 7 days. If you applied to more than one of the same kind of job, list each job separately (examples of job titles include: waiter, computer technician, warehouse worker, administrative assistant, etc.)." The answers in the text entry fields were then categorized into SOCs by trained staff at the University of Wisconsin Survey Center. Ninety-seven percent of job titles could be successfully matched to three-digit SOC codes. We code the following 2000 SOC major groups as blue collar: Building and Grounds Cleaning and Maintenance; Farming, Fishing, and Forestry; Construction and Extraction Occupations; Installation, Maintenance, and Repair Occupations; Production; and Transportation and Material Moving.

Applied to Any Service Job Last Week. Applied to Any Service Job Last Week is an indicator variable equal to one if the respondent applied to a service job within the last 7 days and zero otherwise. The variable was constructed using information from the weekly follow-up surveys, which asked, "Please list the three most recent jobs you applied to in the last 7 days. If you applied to more than one of the same kind of job, list each job separately (examples of job titles include: waiter, computer technician, warehouse worker, administrative assistant, etc.)." The answers in the text entry fields were then categorized into SOCs by trained staff at the University of Wisconsin Survey Center. Ninety-seven percent of job titles could be successfully matched to three-digit SOC codes. We code the following 2000 SOC major groups as service jobs: Healthcare Support Occupations, Protective Service Occupations, Food Preparation and Serving Related Occupations, Personal Care and Service Occupations, and Sales and Related Occupations.

Reservation Wage. Reservation Wage denotes the lowest hourly wage at which the respondent would accept a job offer. The variable is taken from the weekly follow-up surveys, which asked, "Suppose someone offered you a job today. What is the lowest wage or salary you would accept (before deductions) for the type of work you are looking for?" In converting answers referring to yearly, monthly, or weekly time frames into hourly wages, we assume 50 workweeks per year and a 40-hour workweek.

Bargained over Offer in Hand. Bargained over Offer in Hand is an indicator variable equal to one if the respondent bargained with an employer over a job offer she had received. The variable is taken from the weekly follow-up surveys, which asked, "When you were offered this job, did the employer make a 'take-it-or-leave-it' offer or was there some bargaining that took place over the pay?" There were two possible answers: take-it-or-leave-it offer and some bargaining over pay.

Chose $20 Now over $40 in 12 Weeks. Chose $20 Now over $40 in 12 Weeks is an indicator equal to one of the respondent chose to receive a $20 Visa gift card now and zero if she opted for a $40 gift card to be received in 12 weeks. The exact phrasing of the question was "Thank you for taking the time to complete this week's survey! . . . To express our thanks, we would like to mail you a Visa gift card. You can choose between the following two options: A $20 Visa gift card that will be mailed to you today. A $40 Visa gift card that will be mailed to you in 12 weeks from today. If you choose the second option, we will send you the Visa gift card even if you don't return to this survey during the next 12 weeks. Please click here if you would like more information about the Visa gift card."

Distance Traveled Looking for Work. Distance Traveled Looking for Work denotes a set of mutually exclusive indicator variables indicating how far the respondent traveled within the last week to look for work. The variable is taken from the weekly follow-up surveys, which asked, "In the last 7 days, what is the farthest distance that you traveled to look for work?" The set of possible answers consisted of the following: did not go out to look (used internet, mail, or telephone), less than 5 miles, 5–10 miles, 11–25 miles, 26–50 miles, 51–100 miles, and over 100 miles.

Metropolitan Area. Metropolitan Area denotes the metropolitan area, as identified in the commercial product GeoLytics Estimates Premium 2010 (GeoLytics, Inc.), in which a respondent resides.

B2. *Displaced Workers Survey*

Sponsored by the Employment and Training Administration of the U.S. Department of Labor, the Displaced Workers Survey (DWS) is a national survey that gathers information on the severity of job displacements and is used to assess employment stability. Since 1994 the DWS was conducted as a biannual supplement to the Current Population Survey (CPS), a monthly nationally representative labor force survey that interviews roughly 56,000 households across the United States.[42] In 2008 and 2010, the DWS was administered together with the January CPS. Our raw data for the DWS (and the January CPS) were obtained from the Web site of the National Bureau of Economic Research.[43]

To be eligible for the DWS, a worker had to be part of the CPS universe, at least 20 years of age, and have lost or left a job during the previous 3 years because her plant or company closed or moved, there was insufficient work for her to do, or her position or shift was abolished. In our analysis, we restrict attention to individuals of prime working age, that is, those who are between 20 and 55 years old, who are (at the time of the survey) neither enrolled in school nor enlisted in the military, and who are neither missing information on their current weekly earnings nor on race. Imposing these restrictions and pooling across the 2008 and 2010 waves leaves us with 5,098 observations. To account for unequal sampling probabilities, we use the DWS probability weights provided with the data.

The variables in our analysis include the following:

Race. Race is a set of mutually exclusive indicator variables (White, Black, Hispanic, Asian, and Other Race) that denote a respondent's racial identification. In our regressions, White serves as the omitted category.

Female. Female is an indicator variable that equals one if the respondent is female and zero otherwise.

Age. Age denotes the respondent's age in years.

Urban. Urban is an indicator variable equal to one if the respondent lives in a metropolitan statistical area and zero otherwise.

Educational Attainment. Educational Attainment is a set of mutually exclusive indicator variables (High School Dropout, High School Graduate, Some College, and College Graduate) that denote a respondent's highest level of completed education. In coding this variable, we combine several categories in the finely distinguished raw data to arrive at this classification.

Weekly Earnings. Weekly Earnings denotes a respondent's weekly earnings on her current (main) job. The information for this variable comes from the main part of the CPS as well as the DWS. We use the edited variables that are released with the data, giving preference to the information contained in the

[42] For more information on the Current Population Survey and its sampling design, see U.S. Department of Labor, Bureau of Labor Statistics, Labor Force Statistics from the Current Population Survey (http://www.bls.gov/cps/).

[43] For the raw data, see National Bureau of Economic Research, Current Population Survey, Basic Monthly Data at the NBER (http://www.nber.org/data/cps_basic.html).

main part of the CPS. We discard observations with reported weekly earnings below $100 or above $8,000.

Previous Earnings. Previous Earnings denotes a respondent's weekly earnings on the job from which she was displaced. The information for this variable stems directly from the DWS. We use the edited variable that is released with the data and discard observations with reported weekly earnings below $100 or above $8,000.

Year. Year denotes the year in which the survey was administered (2008 or 2010).

State. State is the Federal Information Processing Standards code for a respondent's state of residence.

Previous Industry. Previous Industry corresponds to the detailed industry recode with respect to the respondent's job from she was displaced. The variable is released as part of the DWS.

B3. National Longitudinal Survey of Youth 1979

The National Longitudinal Survey of Youth 1979 (NLSY79) is a nationally representative sample of 12,686 young men and women who were between 14 and 22 years old when they were first interviewed in 1979. These individuals were surveyed annually through 1994 and biannually thereafter. Covered topics include family background and demographic characteristics; household composition; marital and fertility histories; labor market experiences; training investments; schooling and aptitude information; military experience; income and assets; health conditions, injuries, and insurance coverage; alcohol and substance use; criminal behavior; attitudes and aspirations; and more.

The NLSY79 consists of three subsamples: (1) a cross-sectional sample of 6,111 individuals designed to be representative of the noninstitutionalized U.S. population between the ages of 14 and 21 as of December 31, 1978; (2) a supplemental sample of 5,295 individuals oversampling civilian Hispanics, blacks, and economically disadvantaged non-Hispanic whites living in the United States and between the ages of 14 and 21 as of December 31, 1978; and (3) a military sample of 1,280 individuals designed to be representative of the population between the ages of 17 and 21 as of December 31, 1978, and enlisted in the active branches of the military as of September 30, 1978. Our data were obtained from the National Longitudinal Survey of Youth Web site.[44] For additional information on the NLSY79, see the National Longitudinal Survey Handbook.[45]

B3.1. Analysis in Table 4

In Table 4, we rely on data from the 2000, 2004, and 2006 waves of the National Longitudinal Survey of Youth (NLSY). We restrict attention to indi-

[44] For the National Longitudinal Survey of Youth (NLSY), see U.S. Department of Labor, Bureau of Labor Statistics, National Longitudinal Surveys (http://www.nlsinfo.org).

[45] For the National Longitudinal Survey Handbook 2005, see U.S. Department of Labor, Bureau of Labor Statistics, NLS Handbook 2005 (http://www.bls.gov/nls/handbook/nlshndbk.htm).

viduals in the civil labor force who change employers between two successive interview rounds and who are not missing information on current hourly wages at their CPS job, for a final sample of 6,047 observations from 4,143 distinct individuals. To account for unequal sampling probabilities, we weight each observation by its cross-sectional year-specific sampling weight provided with the data.

The following variables are used:

Race. Race is a set of mutually exclusive indicator variables (White, Black, and Hispanic) that denote a respondent's racial identification. In our regressions, white serves as the omitted category.

Female. Female is an indicator variable that equals one if the respondent is female and zero otherwise.

Age. Age denotes the respondent's age in years.

Urban. Urban is an indicator variable equal to one if the respondent's current residence is urban and zero if it is rural.

Years of Schooling. Years of Schooling denotes the respondent's highest grade or year of regular school that she has completed and for which she has received credit. The variable is top coded at 20 years of schooling (8 years or more of tertiary education).

AFQT. The variable AFQT denotes an individual's standardized score on the AFQT. We standardize test scores (to have a mean of 0 and a standard deviation of 1) by age at the time the test was taken.

Current Hourly Wage. Current Hourly Wage gives a respondent's hourly wage at the main job she holds at the time of the survey (that is, the CPS job). The variable is taken from the work history file. We discard observations with reported hourly wages below $5 and above $100.

Previous Wage. Previous Wage denotes the respondent's hourly wage at the main job she held at the time of the previous survey. The variable is taken from the work history file. We discard observations with reported hourly wages below $5 and above $100.

Year. Year denotes the year in which the survey was administered (2000, 2004, or 2006).

Previous Industry. Previous Industry is a set of 12 indicator variables (based on three-digit 1970 census classifications) denoting the industry of a respondent's previous employer (that is, the employer at the time of the last survey).

B3.2. Analysis in Table 12

Our analysis in Table 12 closely follows the coding and sample construction procedures in Altonji and Pierret (2001) in order to replicate their results as closely as possible—before extending them. In what follows, we lean heavily on the description in Altonji and Pierret (2001, p. 345). For a more detailed account, the interested reader should consult their data appendix directly.

The sample is limited to black and non-Hispanics white males who left school before 1992. Only jobs held after a person has left school are considered. The time when a person leaves school is defined as the "month and year of the most recent enrollment at the first interview where the respondent is not currently enrolled in school" (Altonji and Pierret 2001, p. 345). We use Altonji and Pierret's (2001) exact coding of this variable. Further sample restrictions include having at least 8 years of education, at least one valid observation on wages, no missing data on the AFQT score, no missing information on first occupation, and no missing information other variables.[46] Altonji and Pierret's (2001) final sample includes 2,976 individuals. To rule out that sample selection drives our results, we use this exact set of individuals in our analysis.

Employment is considered only for the current (or most recent) CPS employer and only if the respondent is working at the job during the week of the interview. In case of two concurrent jobs, only the job at which the respondent works the most hours is considered, regardless of whether it is full- or part-time. Military jobs, however, are excluded. All valid observations are included, even if individuals did not participate in particular waves of the NLSY. Moreover, like Altonji and Pierret (2001), we use all subsamples of the NLSY and focus on the 1979–92 waves. Our replication sample consists of 21,026 observations, compared with 21,058 observations in their table 1.[47] In columns 7 and 8 of Table 12, we add data for the 1993–2006 waves of the NLSY, using the sample restrictions outlined above. This extends the sample to 33,931 observations.

The following describes the coding of the variables used in Table 12:

Race. Race is a set of mutually exclusive indicator variables (White and Black) that denote a respondent's racial identification. In our regressions, White serves as the omitted category.

Years of Schooling. Years of Schooling denotes the respondent's highest grade or year of regular school that she has completed and for which she has received credit. The variable is top coded at 20 years of schooling (8 years or more of tertiary education).

AFQT. The variable AFQT denotes an individual's standardized score on the Armed Forces Qualification Test. We standardize test scores (to have a mean of 0 and a standard deviation of 1) by age at the time the test were taken.

Potential Experience. Potential Experience is coded as a respondent's current age minus her years of schooling minus six.

Hourly Wage. Hourly Wage denotes a respondent's real hourly wage at her CPS job. The raw variable is taken from the work history file. To convert nominal into real wages, we use the Personal Consumption Expenditures: Chain-Type Price Index constructed by the U.S. Department of Commerce's Bureau of Economic Analysis, and set 1987 as the base year. Following Altonji and Pierret

[46] Altonji and Pierret (2001) also drop 805 respondents who left school before 1978 and for whom they could not reconstruct their work history.

[47] For 32 observations included in the sample of Altonji and Pierret (2001), the work history file of the NLSY79 did not contain a valid wage. We suspect this may be due to later revisions and consistency checks applied to the raw data.

(2001), we discard wage observations with real hourly wages below $2 and in excess of $100.

Tenure. Tenure denotes a respondent's total tenure (in years) with the employer at her CPS job.

Urban Residence. Urban Residence is an indicator variable equal to one if the respondent's residence at the time of the survey is urban and zero if it is rural.

Occupation at First Job. Occupation at First Job denotes the two-digit occupation at the respondent's first job after leaving school. We obtain this variable directly from Altonji and Pierret (2001).

Year. Year denotes the year in which the survey was administered. The NLSY79 surveyed participants on a yearly basis from 1979 to 1994 and every other year thereafter.

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