

Statistical Discrimination and Duration Dependence in the Job Finding Rate*

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Abstract

We propose an equilibrium search model to quantify the implications of employer discrimination in callbacks against the long-term unemployed for job finding rates and long-term unemployment. In our framework, dynamic selection on unobservables endogenously generates statistical discrimination at the interview, or callback stage. In the estimated model, interview invitations for observationally equivalent workers decline by 50% as unemployment duration increases from 1 to 8 months, in line with recent resume audit studies. Yet long-term unemployment and job-finding rates at all durations are almost identical in a full information benchmark where employers do not discriminate. Interviews lost to statistical discrimination impact individual job-finding rates solely if they would have counterfactually led to jobs. We show that such false negatives are rare because firms only discriminate when they anticipate being unable to form a viable match. Discrimination in callbacks is thus largely a *response* to dynamic selection rather than a *cause* of true duration dependence.

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1 Introduction

A string of recent work has documented that firms use unemployment duration as a tool to aid in interview decisions. For example, using a large-scale resume audit study, [Kroft et al. \(2013\)](#) document that callback rates for interviews decline substantially with unemployment duration. All else equal, they find that a worker unemployed for 8 months is 45 percent less likely to receive a callback for an interview than an observationally equivalent newly unemployed worker. [Farber et al. \(2015\)](#), [Eriksson and Rooth \(2014\)](#), [Ghayad \(2013\)](#), and [Oberholzer-Gee \(2008\)](#) also study, via an audit approach, how unemployment duration affects a worker’s chances of receiving a callback. While these studies differ in their quantitative findings, they share the notion that detecting discrimination in callbacks can inform the impact of firms’ hiring policies on job-finding rates and long-term unemployment in the aggregate labor market.

However, there is no direct mapping between duration dependence in callbacks and duration dependence in individual job finding rates when discrimination is statistical. This is because audit studies do not allow us to observe employers’ ultimate hiring decisions beyond this first step in the hiring process. In particular, they do not allow us to observe whether workers rejected for interviews would have been hired.¹ Inevitably, interview decisions will consist of both false positives and false negatives: some workers are interviewed and ultimately not hired while some workers are not interviewed for jobs that may have been a good match. Only the latter event affects employment outcomes; discrimination only matters to the extent that it alters who receives and who is denied job opportunities. Accordingly, some “true” duration dependence, that is a decline in individual job finding rates over the unemployment spell, will arise as workers are denied job opportunities simply because of their unemployment durations. A direct corollary is that at least some equilibrium unemployment is a consequence of statistical discrimination. This seems to be what leads many to believe that duration dependence in callbacks as documented in the experimental literature maps directly into true duration dependence in the job finding rate. Our goal, then, is to quantify the extent to which such inference is valid.²

In this paper, we provide a quantitative link between duration dependence in callbacks and individual job finding rates. We develop a model of the labor market where dynamic sorting on unobservables gives rise to employer discrimination at the interview stage. The model allows us to tie the evidence on discrimination in callbacks to real outcomes in the labor market, and to assess

¹[Eriksson and Rooth \(2014\)](#) describe that, “a limitation of [the field experiment] approach is that [it] studie[s] only the early stages of the hiring process since we do not know whom the employers eventually decide to hire.” Similarly, [Kroft et al. \(2013\)](#) recognize that their field experiment “can only shed light on negative duration dependence in callback rates.” See [Heckman \(1998\)](#) for similar arguments.

²For arguments linking a steep “baseline hazard” in the job finding rate to the experimental evidence on callbacks, see [Kroft et al. \(2015\)](#) and [Kekre \(2015\)](#).

the importance of the screening mechanism in generating true duration dependence in individual job finding rates and long-term unemployment.³ Our findings suggest that the quantitative link between true duration dependence in callbacks and job finding rates is, at best, weak. While there are other candidate mechanisms generating true duration dependence in job-finding rates, the contribution of employer discrimination is minimal and, in particular, orders of magnitude smaller than the decline in callbacks. Interviews lost to statistical discrimination impact individual job-finding rates solely if they would have counterfactually led to jobs. We show that such false negatives are rare because firms only discriminate when they anticipate being unable to form a viable match. While highly qualified workers are sharply affected by discrimination in callbacks at longer unemployment durations, they are rarely unemployed long enough to experience that discrimination; or, if they were, we could not rationalize the empirical amount of discrimination documented in the field experiments.

Our framework begins from the observation in [Kroft et al. \(2013\)](#) that duration dependence in callbacks becomes stronger in tighter labor markets, suggesting that such employer behavior is statistical in nature and responds to changes in the pool of workers in unemployment.⁴ Thus, we propose a model of the labor market in which rational firms endogenously discriminate among applicants because unemployment duration is an informative signal about a worker's quality, which is unobservable to a firm on a resume. If better workers find jobs at faster rates than less qualified workers, the latter will be over-represented in the unemployment pool at longer durations due mechanically to dynamic sorting. Consequently, if interviewing workers is costly, firms condition their interview decisions on unemployment duration to economize on interview costs. In this setup discrimination arises endogenously as a response to dynamic selection, rather than through pure taste biases as in [Blanchard and Diamond \(1994\)](#) or through self-fulfilling prophecies as in [Coate and Loury \(1993\)](#).⁵

We estimate the underlying distribution of unobserved heterogeneity in our model that is consistent with the decline in the average job-finding probability by unemployment duration from the Current Population Survey (CPS). We show that our model closely captures the empirical decline in the callback rate as documented in [Kroft et al. \(2013\)](#) without having explicitly targeted it.⁶ That is, if one were to conduct a resume audit study in our model economy, one would detect

³In what follows, we refer to discrimination in callbacks simply as discrimination.

⁴While this does not imply that all of the detected discrimination is statistical, we believe that in the context of discrimination based on unemployment duration, a story of taste bias can be legitimately ruled out.

⁵In [Blanchard and Diamond \(1994\)](#) firms rank applicants according to unemployment duration without having any reason to do so. In [Coate and Loury \(1993\)](#), discrimination is a feature of an equilibrium where employer beliefs are self-fulfilled through worker investment behavior. In a setup like ours, where the underlying dynamic sorting that leads to group differences is mechanical, there is no equilibrium in which firms do not discriminate under plausible assumptions on the production technology spelled out below.

⁶This is a feature that is unique to our framework, as most other studies of employer discrimination feature

a degree of negative duration dependence in callbacks in line with what has been found in several field experiments.⁷ Importantly, our framework also captures how discrimination varies with local labor market conditions as in [Kroft et al. \(2013\)](#), although this feature is not something we explicitly target either. We show that firms discriminate substantially less in our model if the labor market has slack because dynamic selection weakens when workers meet firms less frequently. The quantitative response to variations in the unemployment rate matches up very closely with the empirical evidence. A model of taste-based discrimination, such as [Blanchard and Diamond \(1994\)](#), cannot immediately account for this fact.

To quantify the consequences of employer discrimination, we contrast the estimated model with two different no-discrimination benchmarks in which all job applicants receive an interview during which their underlying type gets revealed. In the first, interviews are free, so all workers get invited for interviews, regardless of their unemployment duration. In the second, we restrict the firm’s interview decision to be independent of unemployment duration. We find that both job-finding rates at all durations and the incidence of long-term unemployment differ only marginally between the full model and the two no-discrimination benchmarks.

To develop a deeper understanding of these results, we use the estimated model to study individual declines in job-finding rates that occur with unemployment duration in the face of employer discrimination. The estimated model allows us to condition on unobservables to quantify the degree of true duration dependence in individual job finding rates.⁸ In this sense, our model-based approach can be seen as an alternative to the statistical approaches taken in the micro-empirical literature that attempt to separately identify state dependence from unobserved heterogeneity (e.g. [Heckman and Singer \(1984\)](#) and [Van den Berg \(2001\)](#)). In contrast to this literature, we explicitly model individual job-finding rates as an equilibrium outcome. This allows us to directly quan-

discrimination at the hiring stage, rather than at the callback stage. For example, [Vishwanath \(1989\)](#) and [Lockwood \(1991\)](#) study stylized models in which employers endogenously interpret the results of a test or signal in light of an applicant’s unemployment duration. Hiring is conditioned on the result of the test and in the equilibrium with testing all firms interview their applicants. [Doppelt \(2015\)](#) studies the impact of unemployment stigma generated by a worker’s entire labor market history on labor market outcomes. Job interviews get interpreted in the light of such stigma, but are always conducted. In [Fernandez-Blanco and Preugschat \(2015\)](#), firms interview multiple applicants for suitability and endogenously hire the suitable candidate with the lowest duration. Discrimination at the interview stage is the correct counterpart to the audit study evidence.

⁷We use [Kroft et al. \(2013\)](#) as our main benchmark because they find the largest degree of discrimination in callbacks with duration. There is generally a wide range of estimates from the aforementioned audit studies, importantly including a zero found in [Farber et al. \(2015\)](#). We believe that our theory can shed light on the variation in findings and, as we discuss below, centers critically around the nature of unobserved heterogeneity.

⁸As will become clear, true duration dependence in the baseline framework can only be a result of employer discrimination. There are many candidate drivers of true duration dependence in job finding rates more generally, but we are interested in quantifying the impact of employer discrimination on individual job finding rates per se. Thus, we are only interested in other mechanisms to the extent that they interact with discrimination. We study these cases in various model extensions.

tify the consequences of one candidate mechanism for state dependence in the job finding rate, namely employer discrimination.⁹ By sampling declines in individual job finding rates for the distribution of long-term unemployed workers, we find that true duration dependence in the job-finding rate generated by employer discrimination is negligible, despite the fact that these workers face far less interview opportunities.

This result is best illustrated through analyzing a firm's decision to discriminate. We relate this decision to the probability of forming a viable match with a long-term unemployed worker. A firm is indifferent about interviewing an applicant if the probability of hiring the worker times the value of the formed relationship conditional on a hire is equal to the cost of interviewing. Empirically, interview costs are small while viable matches last long and are thus valuable to employers. This implies that, if a firm does not invite members of a group for an interview, it is because they are very unlikely to hire them. The incidence of false negatives is thus low and few of the interviews lost to discrimination would have led to jobs. Therefore, statistical discrimination is largely a response to dynamic adverse selection rather than a cause of true duration dependence.

We further demonstrate that our quantitative observations hold even when we allow for several other mechanisms to interact with discrimination and amplify its effects. We study an extension where skills decay during unemployment; an extension where search effort is endogenous and discrimination can breed discouragement; an extension where firms can receive multiple applications for a single job opening and workers with low duration can potentially crowd out the long-term unemployed; an extension with a noisy hiring process; and an extension where firms make systematic mistakes in their callback decisions. While the consequences of discrimination for duration dependence in the job finding rate and long-term unemployment rise relative to our baseline model, the effects remain small, and more than an order of magnitude lower than the degree of duration dependence in callbacks. We find the largest results for the skill-loss extension where .03 percentage points of the long-term unemployment rate of 1.65 can be attributed to discrimination.

More broadly, our results offer an important qualification for the audit-study approach to the labor market. Insofar as the varied observables are correlated with unobservables that matter for employer's hiring decisions, the mapping between documented callbacks and job-finding rates is entirely unclear. We thus view modeling discrimination and its sources - with a quantitative theory that is disciplined by the experimental evidence - as a promising way to conceptualize and quan-

⁹Alvarez et al. (2015) offer a statistical framework to sort out "structural" duration dependence in the job finding rate from composition dynamics. They find little evidence for structural declines in the job finding rate, consistent with our observations. For similar results, see Ahn and Hamilton (2015). The key difference is that their frameworks lump all potential mechanisms generating structural duration dependence (for example, skill decay, stock-flow matching, discrimination, discouragement) whereas ours aims at isolating the quantitative contribution of statistical discrimination by employers.

tify the causal impact of discrimination in callbacks on job-finding rates and unemployment.¹⁰ In turn, our theory can inform the wide array of estimates of duration dependence in callbacks that researchers have difficulty reconciling. Unemployment duration is a useful piece of information to employers when there is a lot of uncertainty regarding unobserved heterogeneity. It is likely that, given the variation in applicant pools across the aforementioned experiments, the degree of unobserved heterogeneity underlying the applicant pool is also changing. Unobserved heterogeneity can thus potentially reconcile the large differences in findings across studies, highlighting the role of modeling its interaction with employer discrimination in equilibrium.

The paper proceeds as follows. The next section introduces the model. In Section 3, we estimate the model using indirect inference, and in Section 4 we use the estimated model to quantify the real effects of discrimination in the labor market. In section 5, we study the robustness of our results under various model extensions before concluding in section 6.

2 Model

2.1 Environment

We next introduce a simple search and matching model of the labor market that endogenously gives rise to employer discrimination.

Time is discrete and runs forever. Agents discount the future at rate β , have linear preferences, and produce and consume a single homogeneous good.

There is a unit mass of infinitely lived workers indexed by their time-invariant productivity $x \in X = (x, \bar{x})$. x represents skills that are unobservable on a resume, and are (at least partially) observable to an employer during an interview.¹¹ The exogenous and time-invariant distribution of worker types is given by $l(x)$. For convenience, we assume full support on X . Workers are either employed or unemployed with current duration $\tau \in [0, \infty]$. Unemployed workers receive utility b per period.

On the other side of the labor market is a continuum of heterogeneous firms indexed by their

¹⁰In our setting, where we believe that modeling employer discrimination as entirely statistical is warranted, we show that a model of the labor market that is disciplined by even the most extreme findings in the literature fails to produce any meaningful impacts on job-finding rates. A similar approach could be used to analyze the evidence on discrimination in callbacks based on other unobservables. In such settings, modeling taste-biases become more important.

¹¹As in Arrow (1973), let x capture “more subtle types of personal deprivation and deferment of gratification which lead to the habits and action of thought that favor good performance in skilled jobs, steadiness, punctuality, responsiveness, and initiative.”

type $y \in Y = (y, \bar{y})$, with an exogenous distribution of vacancies $v(y)$.¹² Firms y have access to a production technology $p(x, y)$ when matched with a type x worker. We make the following assumptions on the production function:

Assumption 1. 1) $p(x, y)$ is nondecreasing in x , and strictly increasing over some interval $\tilde{X} \subseteq X$

2) $\{y \in Y : p(x, y) > b\} \neq \emptyset$ and $\{y \in Y : p(x, y) < b\} \neq \emptyset$, that is the least productive worker has positive (net) output with some, but not all, firms.

These assumptions are sufficient to ensure that workers with lower productivity find jobs at a slower, yet strictly positive, pace and that firms have higher joint surplus with more productive workers.

Search is random and an unemployed worker's resume makes contact with a vacancy at exogenous rate λ . Upon receiving a resume, firms cannot observe the worker's type x , but can readily observe the worker's current unemployment duration τ . In our framework, a resume is entirely summarized by τ , as τ is the only information on a resume which is relevant to an employer.¹³ If there is any information contained in duration, firms can form expectations about the worker's type conditional on the observed duration. They can choose to reject the worker without an interview, or they can pay a cost κ to interview the worker and learn the worker's type x . For simplicity, we assume that information about the worker's type is fully revealed to the firm during an interview.¹⁴ Once the firm has learned x , it then decides on whether or not to consummate the match. Matches separate with exogenous probability δ . In this event, the worker returns to unemployment and the job disappears.

2.2 Workers

Let $I(\tau)$ denote the set of firms y that interview workers with duration τ , and $H(x, \tau)$ the set of firms hiring a type (x, τ) worker which we characterize in the next subchapter. A worker (x, τ) thus exits unemployment upon meeting a firm $y \in I(\tau) \cap H(x, \tau)$ and her value can be written as

$$(1) \quad U(x, \tau) = b + \beta \left(U(x, \tau + 1) + \lambda \int_{I(\tau) \cap H(x, \tau)} (W(x, y, \tau) - U(x, \tau + 1)) v(y) dy \right)$$

¹²We do not explicitly model job creation. However, as will become clear below, the interview invitation can be viewed as isomorphic to the standard vacancy creation decision as interview costs are borne by firms.

¹³While this may seem like a stark assumption it allows our framework to directly speak to the resume audit studies which isolate the role of current unemployment duration by orthogonalizing all other information on the fictitious resumes.

¹⁴We allow for noisy interviews in an extension.

where $W(x, y, \tau)$ is the value of being employed at y after having exited unemployment at duration τ , given by

$$(2) \quad W(x, y, \tau) = w(x, y, \tau) + \beta (W(x, y, \tau) + \delta (U(x, 0) - W(x, y, \tau))).$$

The worker receives a wage $w(x, y, \tau)$ which can generally depend upon her spell length when exiting unemployment. With probability δ the match separates and the worker becomes unemployed with duration $\tau = 0$.¹⁵

Wage Setting

In our baseline model, we assume that wages are Nash-bargained and fixed for the duration of a match. We further assume that firms have all the bargaining power. The worker's value functions and wages are then independent of unemployment duration τ and employer type y , $U(x, \tau) = U = W(x, y, \tau) = W = \frac{b}{1-\beta}$, where $w(x, y, \tau) = b \forall x, y, \tau$. Further, the hiring sets $H(x, \tau) = H(x) \forall \tau$, since the worker's outside option is independent of duration τ . Besides simplifying the computation of the model, without this assumption firms have a preference for workers with high unemployment duration (all else equal), and some low y firms discriminate against the short-term unemployed because of reservation wages. Therefore, while this assumption generates empirically implausible wages, we find that it is innocuous for labor market flows, and thus with regard to the center of our analysis, the consequences of discrimination for the job finding rate.¹⁶

2.3 Firms - Interviewing and Hiring

The value of a job filled with a worker x to a firm y , $J(x, y)$, just equals the joint surplus of the match,

$$(3) \quad J(x, y) = \frac{p(x, y) - b}{1 - \beta(1 - \delta)}.$$

We assume that an unfilled vacancy has no continuation value. As a consequence, a vacancy that meets some worker x will never be held open due to option value considerations.

¹⁵As can be seen in equation (2), we do not model on-the-job search which is why the model cannot speak to the callback rate of currently employed workers. Allowing for random on-the-job search along the lines of [Burdett \(1978\)](#) would allow us to capture the finding in [Kroft et al. \(2013\)](#) that employed workers have lower callback rates than the newly unemployed since poaching an employed worker is less attractive than hiring a newly unemployed worker. We do not suspect, however, that our main quantitative findings are affected by this modeling choice.

¹⁶See section 5.2 for the model with positive bargaining power for the worker.

After a firm has interviewed a worker and learned her type x , it decides whether or not to hire her. Only matches with positive joint surplus are formed, so a worker's hiring set $H(x)$ satisfies

$$(4) \quad y \in H(x) \text{ iff } p(x, y) \geq b$$

The joint density of unemployed workers of skill x and duration τ , $u(x, \tau)$, depends on the hiring and interview decisions of firms. Firms know $u(x, \tau)$ and form expectations about x , conditional on the worker's unemployment duration τ .¹⁷ If the expected joint surplus covers the interview cost κ , the firm calls back, pays κ , and interviews the worker.¹⁸ If not, the firm discards the application. Thus a worker with duration τ is invited for an interview by firms $y \in I(\tau)$, satisfying

$$(5) \quad y \in I(\tau) \text{ iff } \int_X \max\{J(x, y), 0\} u(x|\tau) dx \geq \kappa.$$

In section 5, we discuss the sensitivity of our results to a variety of the assumptions made thus far.

2.4 Steady State Stocks

In steady state, the stock of unemployed type x workers with duration τ satisfies

$$(6) \quad u(x, \tau) = \begin{cases} \delta e(x) & \text{if } \tau = 0 \\ u(x, \tau - 1)(1 - \lambda \int_{I(\tau-1) \cap H(x)} v(y) dy) & \text{if } \tau > 0. \end{cases}$$

Anyone who separates will be unemployed with duration $\tau = 0$. Workers who do not find work increase their duration by one period. The stock of employed type x workers is given by the non-separating incumbents plus the new hires from unemployment,

$$e(x) = (1 - \delta)e(x) + \lambda \sum_{\tau=0}^{\infty} u(x, \tau) \int_{I(\tau) \cap H(x)} v(y) dy$$

¹⁷The robustness section 5.6 contrasts our quantitative results with a case where firms beliefs about $u(x, \tau)$ are systematically wrong.

¹⁸In general, there might be cases where it is optimal for firms to skip the interview and hire the worker. We rule this out by assumption. To microfound this, one could assume the existence of a zero measure of agents that cause employers losses sufficiently large for them to never hire an unscreened worker.

2.5 Equilibrium

A steady state equilibrium of this economy is a triple $\{H, I, u\}$ where the interview set I satisfies (5), the hiring set H satisfies (4), and the joint distribution of types and duration in unemployment $u(x, \tau)$ satisfies (6).

2.6 Characterization

We next characterize the nature of the hiring and interview sets in this environment. Both sets will be defined by a cutoff rule. According to equation (4), a firm is willing to hire any worker with x high enough to deliver positive surplus. It follows that higher types exit unemployment faster, implying that the distribution of worker types in unemployment at τ first order stochastically dominates the distribution at $\tau + 1$. Since this implies expected surplus is declining with unemployment duration, firms choose duration cutoffs and do not interview workers whose current spell exceeds their cutoff. We next formally define discrimination as follows:

Definition 1. The equilibrium features discrimination against unemployment duration iff $I(\tau + 1) \subset I(\tau) \forall \tau > 0$.

This definition implies that, with discrimination, the probability to receive an interview is strictly decreasing in unemployment duration for any given worker. We next formally characterize the equilibrium:

Proposition 1. *The hiring decision for a firm y is a cutoff rule in which it hires only workers of type $x \geq x^*$ where $x^*(y) = \min x \in X : p(x, y) \geq b$. Firms use cutoff strategies for interviews, interviewing only workers with $\tau < \tau^*(y)$ where $\tau^*(y) = \max \tau : \int_X \max\{J(x, y), 0\} u(x|\tau) dx \geq \kappa$. The equilibrium exhibits discrimination, that is $I(\tau + 1) \subset I(\tau) \forall \tau > 0$.*

Proof. See Appendix. □

The Consequences of Discrimination

For a worker (x, τ) to exit unemployment she needs to meet a firm y which is in her interview set $I(\tau)$ and in her hiring set $H(x)$. As an unemployed worker's duration increases she receives fewer interviews since firms discriminate, $I(\tau + 1) \subset I(\tau)$. A key observation is that a worker x experiences a decline in her chances of finding a job between τ and $\tau + 1$ only to the extent that firms that stop calling her back are in her hiring set. The share of firms $y \in I(\tau) \cap I(\tau + 1)^c$ constitute the decline in the callback rate between τ and $\tau + 1$. The share of firms $y \in I(\tau) \cap$

$I(\tau + 1)^c \cap H(x)$ constitute the decline in the job-finding rate for a worker of type x . That is, interviews lost to discrimination reduce the job finding rate only if they would have led to jobs.¹⁹

3 Estimation

For the quantitative section, we follow [Albrecht and Vroman \(2002\)](#) and specify the production function to take the following form

$$(7) \quad p(x, y) = \begin{cases} y, & \text{if } x \geq y \\ 0, & \text{otherwise} \end{cases}$$

This particular functional form ensures that the model generates positive assortative matching since it is the highly productive firms which discriminate. It also has the appealing interpretation that a worker is *qualified* for a job if $x \geq y$ and allows for a very clean mapping of discrimination into the job finding rate: Discrimination affects a worker’s job finding rate if and only if it comes from jobs she is qualified for.²⁰ Further, the production function implies that, absent discrimination, a worker’s job finding rate is given by the share of vacancies with $y \leq x$.

We assume one model period to be a week and aggregate up to a monthly frequency to match available data from the CPS. We work with 100 worker types and 100 firm types, with support $(1, 2)$.²¹ We fix some parameters exogenously. We set the discount rate to .999, consistent with a 5% annual interest rate, and the separation rate to .008 to match a monthly separation rate of 3% from [Shimer \(2012\)](#). Further, we set the value of leisure to .7.²²

Perhaps the newest parameter relative to standard models of search in the labor market is the value of interview costs. We appeal to [Silva and Toledo \(2009\)](#) who, using the Small Business Administration Survey, document that employers spend on average 16 hours per hire. To mimic this we set $\kappa = .34$. This turns out to be roughly 10 hours of output per worker in equilibrium, assuming a 35 hour work week. Given the fact that not every interview turns into a hire, this is thus likely an upper bound on the cost of an interview. Finally, we assume that the distribution

¹⁹The interview and hiring decisions turn out to be simple cutoff rules in the baseline model. Thus, another way of putting this observation is that workers of type x experience a decline in their job finding rate between τ and $\tau + 1$ only if (some of) the firms that interview τ -workers but not $\tau + 1$ -workers are willing to hire workers with ability less than or equal to x . We use the general notation which can adapt to the extensions in the appendix.

²⁰In the robustness section, we check that our results are robust to the specification of the production function.

²¹Clearly, the production function requires the same support for $l(x)$ and $v(y)$. In turn, the location and size of the support are irrelevant given the production function. We have opted to shift the support away from zero to avoid the case that some technologies y cannot possibly cover strictly positive interview costs.

²²This falls broadly into the wide range of estimates for the opportunity cost of work ([Chodorow-Reich and Karabarbounis \(2015\)](#)). What is key is that $b \leq 1$ which implies that all workers accept all jobs they are qualified for.

of vacancies is uniform, $v(y) \sim U(1,2)$.²³ This implies that x equals the share of jobs a worker is qualified for. The set of fixed parameters is summarized in Table 1. In Appendix B.1, we study the sensitivity of our quantitative findings to a wide range of values for $[\beta, \delta, \kappa, b]$.

Parameter	Value	Source/Target/Reason
β	0.999	5% interest rate
δ	0.008	3% monthly separation rate
κ	.34	Silva & Toledo (2007)
b	.7	Opportunity Cost of Employment
$v(y)$	$U(1,2)$	Normalization

Table 1: Calibrated Parameters

3.1 Estimated Parameters and Targets

The remaining parameters govern the efficiency of the matching function, λ , and the distribution of unobserved heterogeneity among workers, $l(x)$. We estimate these fully parametrically using Simulated Method of Moments.²⁴ We assume that the unobserved heterogeneity is distributed $x \sim \text{beta}(A, B) + 1$, where A and B are to be estimated.

We target moments which are informative about these three parameters: The unconditional job finding at various unemployment durations τ as well as the aggregate unemployment rate. Clearly, there is a direct mapping between the latter and λ . Our empirical target for the aggregate unemployment rate is 8.5%.²⁵ Next, A and B jointly govern the variance and skewness of the distribution of unobserved heterogeneity. We make a heuristic argument how these higher moments shape the unconditional job finding rate. Increasing heterogeneity increases the slope of the job finding rate against unemployment duration since dynamic selection becomes more pronounced. Further, the skewness of the underlying distribution of heterogeneity governs the convexity of the unconditional job finding rate as a function of unemployment duration. To see this, note that a distribution with positive skew has few high quality workers and many low quality ones. In that case, we should see an initially steep slope which subsequently flattens. The opposite occurs for a distribution with negative skew. We confirm the validity of these conjectured relationships using the large number of model simulations from constructing the Markov Chains.

To construct an empirical target for the unconditional job finding rate, we follow Kroft et al. (2015) and estimate the negative exponential relationship between the empirical job-finding

²³We argue, though we have not proven, that this is a normalization in that what matters is, for given interview costs, the distribution of potential job opportunities across workers.

²⁴Specifically, we employ Markov Chain Monte Carlo as originally proposed in Chernozhukov and Hong (2003). For details of our implementation see Lamadon (2014), Jarosch (2015) or Lise et al. (2015).

²⁵This reflects the average unemployment rate while the resume audit study in Kroft et al. (2013) was conducted.

probability and unemployment duration (in months) in the Current Population Survey (CPS) via weighted nonlinear least squares. That is, we estimate the following functional form for the average job-finding probability at duration τ relative to the average job finding probability of workers who have been unemployed one month or less:

$$(8) \quad \frac{\bar{f}(\tau)}{\bar{f}(0)} = b_1 + (1 - b_1) \exp(-b_2 \cdot \tau)$$

Figure 1 plots the raw data (relative to their level in the first month) along with the fitted curve implied by specification (8). Since our model does not speak to dynamic sorting along observables we strip out the effects of a variety of observables. Even doing so, the job-finding probability declines by around 50% during the first year of unemployment. Our empirical estimates are $\hat{b}_1 = .407$ and $\hat{b}_2 = .223$. We do not directly target, in an indirect inference sense, the empirical coefficients \hat{b}_1 and \hat{b}_2 . Instead, have found it more informative to target values of $\frac{\bar{f}(\tau)}{\bar{f}(0)}$ as obtained through equation (8) at various durations τ .²⁶

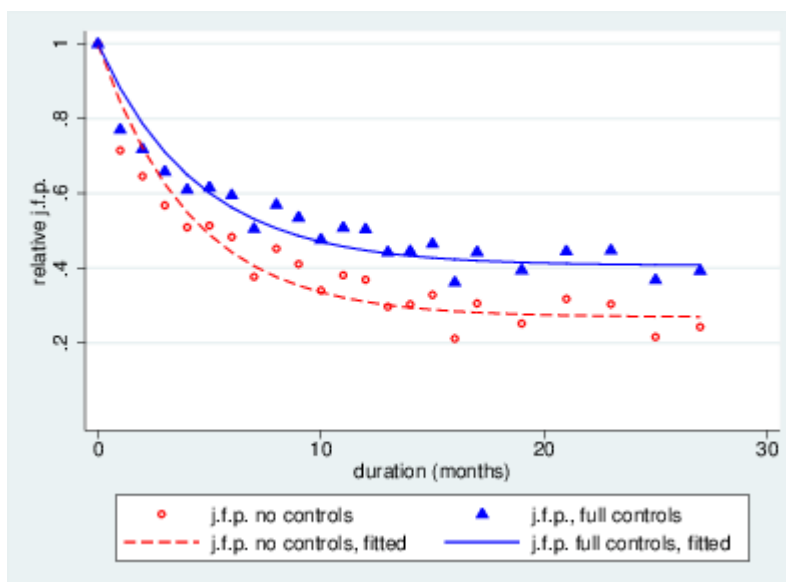


Figure 1: Relative Job-Finding Probabilities by Unemployment Duration

Notes: We use pooled CPS data from 1976-present. Following Kroft et al. (2015), our controls are gender, a fifth degree polynomial in age, three race dummies (white/black/other), five education category dummies, and gender interactions with all these covariates.

²⁶The reason is that “minimizing the distance” between model generated and empirical coefficients is not equivalent to minimizing the distance between the implied hazard rates. Thus, we instead directly do the latter. In principle, one could target a long, weighted vector of fitted empirical values $\frac{\bar{f}(\tau)}{\bar{f}(0)}$. In practice, we have found it sufficient to target $\frac{\bar{f}(\tau)}{\bar{f}(0)}$ at $\tau = 8$ and $\tau = 16$ with an identity weighting matrix to obtain a decent fit as we report in the next section.

3.2 Estimation Results and Model Fit

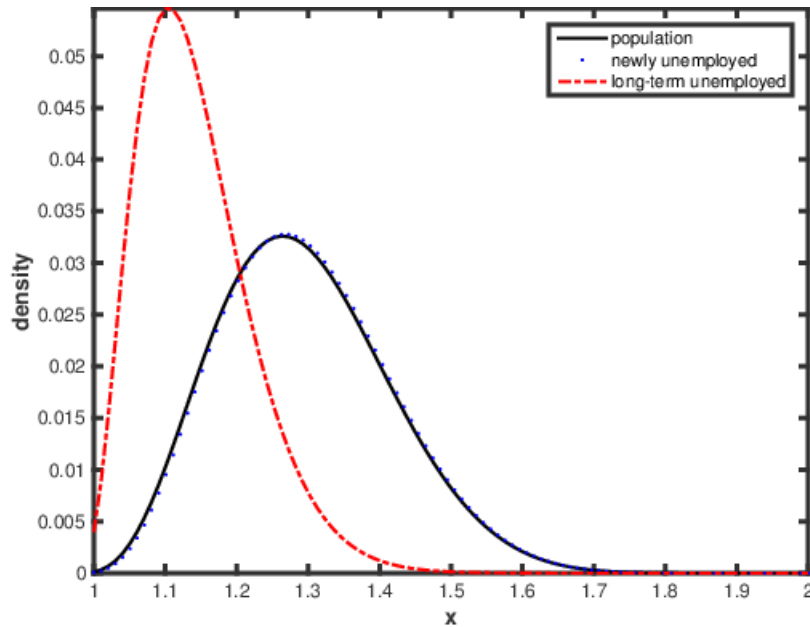
Table 2 reports the estimated parameters values. Our A and B imply a distribution of heterogeneity that is skewed to the right, as shown in Figure 3. Note that this finding is in line with the argument on the relationship between skewness and the shape of duration dependence above.

Table 2: *Estimated Parameters*

Parameter	Description	Estimate
A	Beta distribution	4.862
B	Beta distribution	11.243
λ	contact rate	0.4

Figure 3 plots the skill composition in the population and across short-term and long-term unemployed workers. Since all jobs are equally likely to get destroyed, skills among the newly unemployed equal those of the employed which is why they first order stochastically dominate the population distribution. Further, the average x is substantially lower among the long-term unemployed. We again highlight that, given our assumptions on the production technology and the normalization of $v(y)$, Figure 3 captures the distribution of job finding rates (absent discrimination) for different parts of the population.

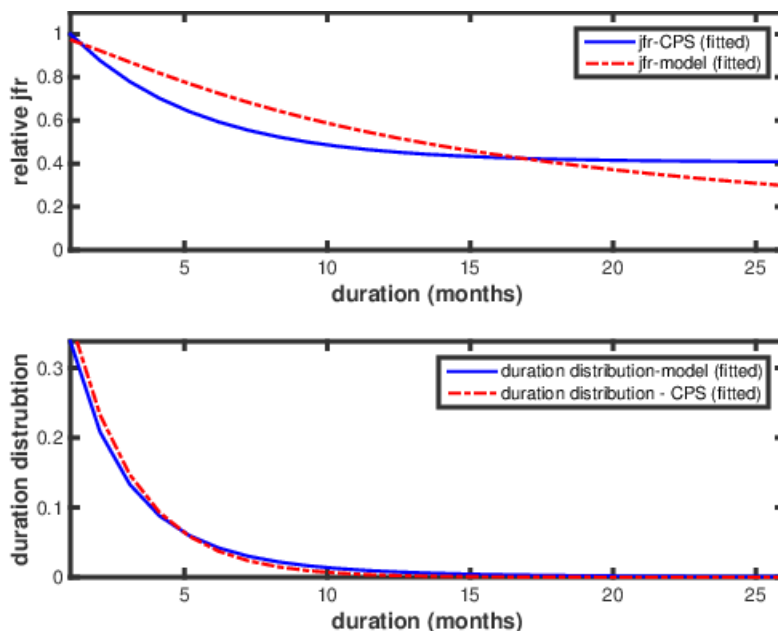
Figure 3: Unobserved Heterogeneity



Notes: The solid line plots the estimated beta density. The dash-dotted line plots beta density fitted to the histogram of x among workers with unemployment duration τ equal to 0 weeks in the steady state of the model. The dashed line does so for workers with duration $\tau \geq 26$.

The model captures the normalized, unconditional decline in the job finding rate in the CPS quite well. Fitting specification (8) to model generated data, we generate duration dependence in the job finding rate quite close to the empirical counterpart: The top panel of Figure 4 plots the estimated decline in the job finding rate in the model against the empirical target. The bottom panel contrasts the implied density of unemployment duration among the unemployed. While the model closely fits the data, the parametric restriction on the distribution of unobservables prevents the model from exactly capturing the empirical counterpart. In terms of the final targeted moment, our framework generates an aggregate unemployment rate of 7.3%.

Figure 4: The Decline in the Job Finding Rate and the Duration Distribution of Unemployment



Notes: *Top:* Average job-finding rates (normalized to duration $\tau = 0$) as approximated by equation (8), in model and data. *Bottom:* Density of unemployment duration τ among unemployed workers, in model and data.

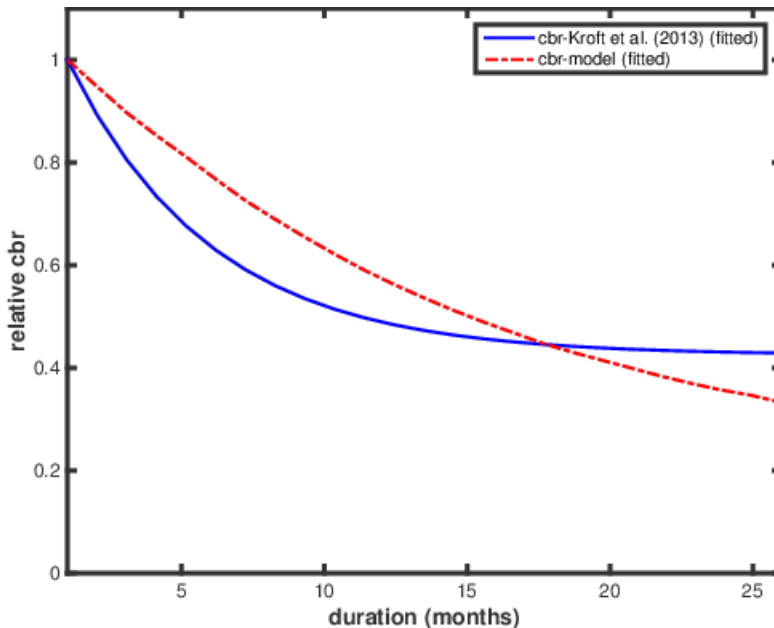
The Callback Rate

We have not targeted the empirical evidence on employer discrimination when taking the model to the data. Instead, we use the experimental evidence from Kroft et al. (2013) in the spirit of an overidentification test to validate the basic mechanism of our model. Specifically, our framework generates the exact counterpart to the empirical callback rate as tracked in the audit studies: We track the probability that any given firm follows up on an application with an interview invitation, depending on current duration τ . Again, we express these probabilities relative to their duration $\tau = 0$ level using the specification in (8).²⁷ We can readily compare this measure of the slope in our model generated callback rate with results in Kroft et al. (2015) who fit the same exponential specification to the experimental data reported in Kroft et al. (2013). Figure 5 documents that the model quantitatively captures the empirical evidence without having

²⁷That is, we target the decline in the callback rate, not its level. We argue that the slope captures the degree of statistical discrimination and hence the key object of interest. Clearly, the model does not capture the level since any active firm in our framework calls back all workers with duration $\tau = 0$. Hence, our intercept is substantially higher than the intercept uncovered in the experimental studies. The case with multiple applications per jobs studied in section 5.3 captures both the slope and the level of the empirical callback rate.

explicitly targeted it in the model estimation.

Figure 5: The Decline in the Callback Rate



Notes: Average callback rates (normalized to duration $\tau = 0$) as approximated by equation (8) in model versus data reported in Kroft et al. (2015).

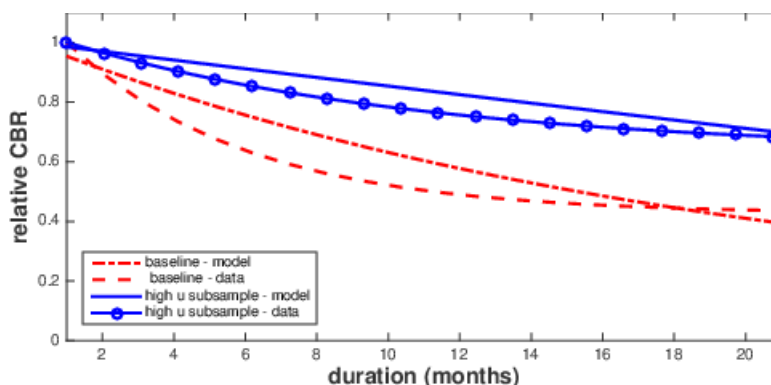
The external validity of the evidence in Kroft et al. (2013) is far from established which warrants some discussion. While the results in Ghayad (2013) are quantitatively similar, Farber et al. (2015) find no evidence of duration dependence in callbacks. Oberholzer-Gee (2008) finds a non-monotonic relationship between duration and callbacks, documenting declining callbacks only for unemployment spells of 18 months or more. Eriksson and Rooth (2014) find large drops in callbacks after 9 months of unemployment for medium and low skill jobs, but not for high skilled jobs. Given that Kroft et al. (2013) work with relatively low-skilled young workers whereas Farber et al. (2015) construct resumes of college educated experienced workers, a common picture emerges that can be interpreted in the context of our theory: The larger the dispersion of unobservables, the larger the impact of correlated observables (such as unemployment duration) on the callback decision as there is less reason to condition the interview on unemployment duration once a resume contains a large amount of other information.²⁸ We have deliberately chosen to

²⁸The log earnings variance patterns documented in Meghir and Pistaferri (2004) are declining in educational achievement. This supports the interpretation of the discrepancy in the audit study results as reflecting different degrees of uncertainty over the candidates quality on the employer side given the design of the study.

contrast our theory with the evidence in [Kroft et al. \(2013\)](#) since it is the study that finds the sharpest results and thus the most interesting case to be mapped into its consequences for job-finding rates and long-term unemployment. More broadly, our framework suggests that it may be the unobservable characteristics we model - not captured in audit experiments - that can explain the variation in results across studies.

We conclude this chapter by comparing the implications of local labor market conditions in our framework against the empirical evidence. [Kroft et al. \(2013\)](#) find a substantially lower slope for the callback rate when the local unemployment rate is high. That is, if the labor market has slack, duration carries less of a signal about unobserved quality and firms discriminate less against the long term unemployed. The same qualitative logic holds in our model. Here, we check whether our framework also captures the quantitative impact of local labor market conditions. To this end, we hold all estimated parameters fixed and adjust the contact rate λ so as to capture (in steady state) the average unemployment rate in the high-unemployment MSA sample studied separately in [Kroft et al. \(2013\)](#). We again compare the normalized and fitted callback rate in our model against the callback rate reported in [Kroft et al. \(2015\)](#). As [Figure 6](#) shows, our framework captures the quantitative response of firms to labor market tightness quite closely. The variation by labor market conditions is the key empirical piece that supports our framework's mechanism: discrimination is an endogenous response to dynamic sorting along unobservables, a force that gets much weaker when the labor market has slack. That our framework quantitatively captures this evidence without targeting it in the estimation is thus reassuring for our approach.

Figure 6: Varying Labor Market Conditions



Notes: The baseline replicates Figure 5. The high unemployment subsample adjusts λ so as to capture the unemployment rate in the *High Unemployment MSAs* listed in Kroft et al. (2013). Using the Bureau of Labor Statistics Local Area Unemployment Statistics, we compute a (population weighted) average unemployment rate of 17.6% in the respective MSAs over the time period of August 2011 to July 2012. We then plot the model generated callback rate (normalized, fitted) under the high-unemployment steady state to the empirical counterpart reported in Kroft et al. (2015).

In sum, the labor market modeled here gives rise to levels of state dependence in callbacks which quantitatively line up with the evidence from various field experiments. In other words, if one were to conduct a resume audit study in the labor market modeled here, the results would closely resemble the ones reported in the experimental literature.

4 The Effects of Discrimination on the Job-Finding Rate

In this section, we use the estimated model to quantify the contribution of statistical discrimination to duration dependence in job finding rates and long-term unemployment.

4.1 Two No-Discrimination Benchmarks

We contrast duration dependence in the job finding rate and the incidence of long-term unemployment in our full model with two alternative benchmarks that display no discrimination.

4.1.1 Zero Interview Cost

The first exercise studies a labor market where interviews are free and all employers hence interview and learn about applicants of *all* durations τ . Specifically, we set $\kappa = 0$ while holding all other parameters fixed according to the previous chapter and contrast several steady state features

	ΔJFR_{ST}	ΔJFR_{MT}	ΔJFR_{LT}	ΔLTU
full information	0.003	0.001	0.001	-0.001
ban	-0.001	0.002	0.001	-0.001

Table 3: The Consequences of Discrimination - Two Benchmarks

Notes: Results for sections 4.1.1 and 4.1.2 in rows 1 and 2, respectively. First column: Percentage point difference in the average job finding rate of short-term unemployed workers ($\tau = 0 - 12$ weeks) between no discrimination benchmark and baseline model. Second Column: $\tau = 13 - 26$ weeks. Third Column: $\tau \geq 27$ weeks. Fourth column: Difference in the share of the population being long-term unemployed ($\tau \geq 27$ weeks) between no discrimination benchmark and baseline model. All columns compare steady states.

from this counterfactual economy with our benchmark model that has positive interview costs.²⁹ If interviews are free, there is no more statistical discrimination at the callback stage and workers who apply to a job they are qualified for exit unemployment with certainty.

The first row of Table 3 reports how the unconditional job finding rate at various unemployment durations differs across the two models. It further contrasts the two economies in terms of the incidence of long-term unemployment. The no-discrimination benchmark has higher job finding rates at all durations and consequently less long-term unemployed workers. The main takeaway from the table, however, is that the numbers are extremely small. For instance, the large decline in callbacks notwithstanding, the weekly job finding rate for the long-term unemployed is 5.63%, only .001 percentage points lower than under the no-discrimination benchmark. Likewise, 1.16% of the population is long-term unemployed in our benchmark model, only .001 percentage points more than under the no-discrimination benchmark. Further, note that the negative consequences of discrimination for the job finding rate are not necessarily largest among the long-term unemployed. As we discuss in section 4.3, the reason is that there are relatively many high- x types in short term unemployment whose job finding rates decline when high y firms stop interviewing.

Table 3 is the main benchmark for the various quantitative robustness checks we carry out. Appendix B.1 studies the sensitivity of our results to higher interview costs, higher separation rates, higher unemployment benefits, and higher discount rates. Likewise, when we check our quantitative findings in various model extensions in section 5, we refer to table 3 as our benchmark results.

²⁹Since our main finding is that none of the counterfactuals generates a sizable equilibrium response given the fixed parameters, fixing the parameters is an innocuous shortcut. However, observe that while we fix the offer arrival rate λ , the endogenous meeting rate is free to respond to changes in interview costs.

4.1.2 Banning Discrimination

In this section, we study an alternative counterfactual with a constant callback rate. Specifically we contrast the benchmark economy with one where all active firms must interview all applicants independent of their unemployment duration. The second row of Table 3 contrasts this no-discrimination environment with our baseline case.

The main message is again that such a policy will have only a marginal impact on job finding rates and the incidence of long-term unemployment. In case of a policy ban firms still have to pay κ and, as a response, some (high- y) firms opt not to interview altogether. That is, the overall hiring process gets more expensive for firms, which is why we find an even smaller response compared to the previous subchapter. Both measures are hence somewhat polluted because they either increase or decrease the overall cost of hiring. The first benchmark ($\kappa = 0$) can thus be thought of as an upper bound for the quantitative impact of discrimination and is hence our preferred measure. For this reason we employ it in the quantitative discussion of model extensions in section 5 and parameter sensitivity in Appendix section B.1.

To lead up to the next subsection, we briefly discuss how the impact of a discrimination ban differs across workers. To that end, we report the increase in the job finding rate at duration $\tau = 52$ weeks for different worker types: A worker at the 10th percentile of the ability distribution has exactly the same job finding rate at $\tau = 52$, with or without discrimination. The same is true for the median worker, while workers at the 75th and 90th percentile have 11.8% and 35.3 higher job finding rates, respectively. Clearly, high types benefit the most from this policy if they “fall through the cracks” and are long-term unemployed. In contrast, low types do not experience any increase in their job finding rate. The reason, as we discuss in detail below, is that low types are not qualified for the job openings that opt not to interview the long-term unemployed. In turn, the high types that benefit from the policy are hardly ever long-term unemployed which is both the reason for why some firms discriminate and why that discrimination is inconsequential for the vast majority of the long-term unemployed.

4.2 Measuring True Duration Dependence Generated by Discrimination

We next take a more detailed look at the quantitative consequences of discrimination within the model with discrimination before turning to a discussion of why we find such little effects quantitatively in the two no-discrimination benchmarks. In particular, we define and quantify a specific measure of true duration dependence generated by employer discrimination. We further contrast our measure with an alternative, counterfactual measure which leads to vastly different quantitative conclusions to stress the importance of conditioning on the relevant distribution of

unobservables.

In our framework, the job-finding probability for a type x worker with τ periods of unemployment is given by $f(x, \tau) = \lambda \int_{y \in I(\tau) \cap H(x)} v(y) dy$. Let $\bar{f}_\tau(t) \equiv \frac{1}{\int u(x, \tau) dx} \int f(x, t) u(x, \tau) dx$ denote the average job finding rate at duration t when skills are distributed according to the population of unemployed workers at duration τ . Unconditional (on unobservables) duration dependence in the job finding rate as measured in the data is given by

$$(9) \quad D(\tau) = \frac{\bar{f}_\tau(\tau)}{\bar{f}_0(0)}$$

We are interested in quantifying how the fact that the set of interviewing firms $I(\tau)$ shrinks as τ increases contributes to $D(\tau)$. To that end, we construct the following measure of true duration dependence that isolates the effects of statistical discrimination from dynamic selection:

$$(10) \quad TD(\tau) \equiv \frac{\bar{f}_\tau(\tau)}{\bar{f}_\tau(0)}$$

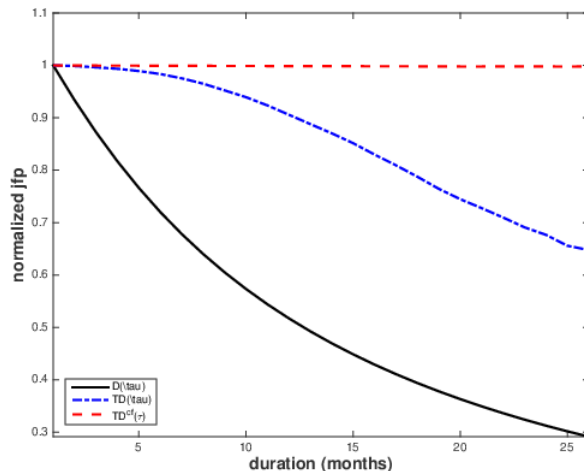
That is, we ask how the job finding rate of a cohort of unemployed workers with duration τ compares to their own duration-0 job finding rate. We contrast this measure of true duration dependence with an alternative, *counterfactual* measure that holds the distribution of ability fixed according to the pool of unemployed workers at duration 0.

$$(11) \quad TD^{cf}(\tau) \equiv \frac{\bar{f}_0(\tau)}{\bar{f}_0(0)}$$

The black solid line in Figure 7 plots the unconditional decline in the job finding rate $D(\tau)$. The red dashed line plots $TD(\tau)$. While $TD(\tau) < 1$ for all $\tau > 0$, the figure implies that the job finding rates of the *relevant* group of unemployed workers, namely that which reflects the skill composition of the unemployed at various durations τ , is hardly affected by the fact that the callback rate substantially falls as duration increases. This stands in sharp contrast to the $TD^{cf}(\tau)$: If, counterfactually, the skill distribution across unemployment durations τ was equal to the one at duration 0, the estimated decline of the callback rate would substantially decrease the exit rate from unemployment. $TD^{cf}(\tau)$ quantifies the experience of representative types “falling through the cracks,” but ignores that the representative newly unemployed worker exits unemployment at a relatively quick pace. The accurate definition of true duration dependence thus quantifies how the job finding rate of a factual cohort of unemployed workers with duration

τ contrasts with their own duration-0 job finding rate.

Figure 7: Duration Dependence



To summarize, these results imply that the decline in the job finding rate in our model is almost only a result of dynamic sorting along unobservables. Statistical discrimination is largely a *response* to dynamic (adverse) selection rather than a *cause* of true duration dependence. For the relevant group of unemployed workers, discrimination has little impact on job-finding rates, as firms that begin to discriminate do so because there are few workers they can form viable relationships with left in the pool of unemployed. In turn, this implies that the incidence of false negatives - no-callback events that prevent a worker from being hired - is much smaller than the incidence of no-callback events as tracked by an audit study.

4.3 Intuition

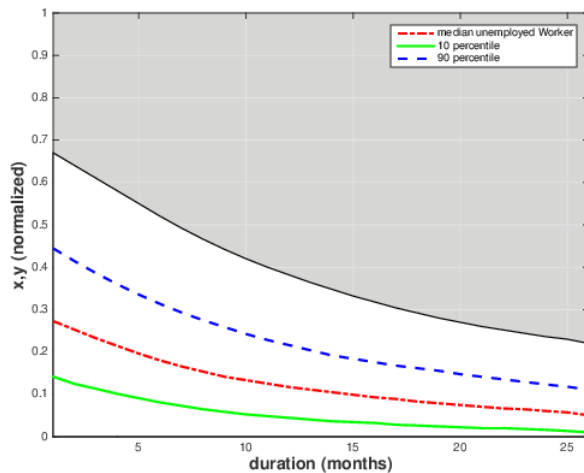
This section offers a detailed qualitative argument helpful in understanding our results thus far and then spells out the key reason for why our quantitative results are very small.

Qualitative

The three lines in Figure 8 plot the 10th, 50th, and 90th percentiles of x among the unemployed of various durations τ . From the perspective of some firm y , the probability of finding a suitable match $x \geq y$ is strictly declining in duration τ . In response, firms that require highly qualified workers start to discriminate: The white area contains the firm types y which invite duration τ

workers for interviews, $I(\tau)$. The lowest firm types have the highest cutoffs because they are able to match with a larger set of workers given the production function we have specified.³⁰

Figure 8: The Incidence and Consequences of Discrimination



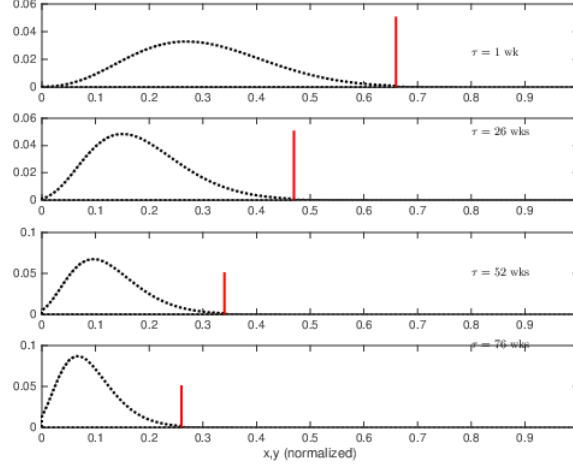
Notes: The three lines plot the 10th, 50th, and 90th percentile of $x - 1$ in the population of unemployed workers at duration τ . The grey, shaded area plots covers all firms $y - 1$ that do not call-back unemployed workers with duration τ . The white are covers all firms $y \in I(\tau)$ that call back workers at duration τ .

Figure 8 provides a straightforward intuition for the difference between $TD(\tau)$ and $TD^{cf}(\tau)$: A worker's hiring set is $y \leq x$. In the context of Figure 8 this implies that only those workers whose x falls into the shaded area experience consequential discrimination impacting their job finding rate. It follows that counterfactually holding the distribution of x fixed at its duration 0 position implies that substantially many workers experience consequences from discrimination as τ increases. In turn, the vast majority of workers who factually stay unemployed until duration τ , while experiencing a substantial fall in callbacks, do not experience falling chances of finding a job. Similarly, the series of graphs in Figure 9 shows how the distribution of worker types evolves with unemployment duration, and how the threshold firm that is indifferent between interviewing and not changes in response to this evolution. Only workers in the tail of the density to the right of the threshold firm lose job opportunities because of discrimination. The next subchapter provides

³⁰Since $v(y)$ is uniform, the lower envelope of the gray area in Figure 8 is the model-generated callback rate. Some firms are permanently inactive since they cannot find enough suitable workers even among the short-term unemployed. This is inconsequential for the vast majority of workers as can be inferred from Figure 3. Having a share s_i of inactive firms y is isomorphic to having no inactive firms and rescaling the meeting rate to $\lambda' = \lambda s_i$. This is similar in spirit to the argument in Flinn and Heckman (1982) that we can never observe wage offers below the reservation wage. Further, while our model captures the (normalized) decline in callbacks as shown below in Figure 5, the baseline model generates callback rate levels that are too high relative to the experimental data. In the robustness chapter we study a framework with multiple applications that generates more realistic levels for the callback rate.

some intuition for why the share of workers experiencing consequences from discrimination for the job finding rate is quantitatively so small.

Figure 9: Discrimination in Response to Dynamic Sorting



Notes: Density of worker types $x - 1$ in the population of unemployed workers at various durations τ . The red bar indicates productivity $\bar{y}(\tau)$ of the threshold firm which is indifferent about interviewing workers with duration τ , corresponding to the lower envelope of the shaded area in Figure 8.

Quantitative

To understand our quantitative findings, we denote the set of applications of worker (x, τ) that are discriminated against yet would have led to jobs by $C(x, \tau)$, that is $y \in C(x, \tau)$ iff $y \in H(x, \tau)$ and $y \notin I(\tau)$. It is straightforward to show that true duration dependence as measured by equation (10) can be written as³¹

$$(12) \quad TD(\tau) \equiv \frac{\bar{f}_\tau(\tau)}{\bar{f}_\tau(0)} = \frac{\bar{f}_0(\tau) - \lambda \int \left(\int_{y \in C(x, \tau) \text{ and } y \notin C(x, 0)} v(y) dy \right) u(x, \tau) dx}{\bar{f}_0(\tau)}$$

$\equiv \Psi(\tau)$. Avg. incr. in the Pr. of meeting $y \in C(.,.)$

In words, the decline in the job finding rates of duration τ unemployed workers that can be causally attributed to statistical discrimination is directly given by the increase in false negatives relative to their occurrence upon entering unemployment, $\Psi(\tau)$. To quantify the latter, consider the threshold firm $\bar{y}(\tau)$ which is indifferent about interviewing workers of duration τ . Let $x^*(y)$

³¹To see this, observe that $f(x, \tau) = f(x, 0) - \lambda \int_{y \in C(x, \tau) \text{ and } y \notin C(x, 0)} v(y) dy$.

denote the minimum skill requirement for a firm y . Then, the threshold firm satisfies

$$(13) \quad \frac{\kappa}{\mathbf{E} \left(\frac{p(x, \bar{y}(\tau)) - b}{(1 - \beta[1 - \delta])} \mid x > x^*(y), \tau \right)} = Pr(x > x^*(\bar{y}(\tau)) \mid \tau)$$

where the right hand side is the probability that the threshold firm meets a worker it hires ex-post among the unemployed of duration τ , and the left hand side is the size of the interview costs relative to the expected present value of match surplus, conditional on positive surplus. Equation (13) implies that, if interview costs are low relative to the value of viable employment relationships, a firm only discriminates against duration τ workers if it is unlikely to find a qualified match among them.

To make analytical progress, assume that $b = 0$, and that $\kappa(y) = \tilde{\kappa} \cdot y$. With our production function, we have that a firm y hires every worker with $x > y$. Thus, the above simplifies to

$$(14) \quad \tilde{\kappa}(1 - \beta(1 - \delta)) = Pr(x \geq \bar{y}(\tau) \mid \tau)$$

Crucially, $Pr(x \geq \bar{y}(\tau) \mid \tau)$ is the fraction of unemployed workers at duration τ that are qualified for a job at the cutoff firm. It thus follows that a share $1 - \tilde{\kappa}(1 - \beta(1 - \delta))$ of all unemployed workers at duration τ does not experience *any* discrimination by relevant firms y . To quantify this share, we again appeal to the micro-evidence on hiring costs discussed above which suggests a value of $\tilde{\kappa} = .34$ at a weekly frequency.³² Inserting our calibrated values for β and δ we find $1 - \tilde{\kappa}(1 - \beta(1 - \delta)) > .997$. That is, more than 99.7% of workers at any duration τ are entirely unaffected by discrimination in terms of their job finding rates. The underlying reason for this result is that the present discounted value of a viable employment relationship vastly exceeds the cost of an interview. This makes missed opportunities to form a viable match costly relative to an interview that does not lead to a hire. Consequently, most interviews lost to discrimination would not have led to a job. Importantly, $Pr(x \geq \bar{y}(\tau) \mid \tau) \sim .003$ is still vastly larger than $\Psi(\tau)$ in (12) since even the .7% of workers who suffer a decline in their job finding rates from discrimination are unlikely to meet a firm in $C(x, \tau)$.

In Appendix B.1 we report our quantitative result for an extreme scenario where we triple the interview cost and separation rate, and set the annual discount factor to 10%. These forces substantially reduce match surplus relative to the cost of interviewing.³³ In this case, equation (14) implies that $Pr(x \geq \bar{y}(\tau) \mid \tau) \sim .26$, that is 26% of workers at any duration τ are qualified

³²Note that the actual interview cost is now proportional to output and an interview costs employers 37% of weekly per-worker output.

³³Since we also set $b = .9$, we first modify equation [14] to allow for positive levels of b by assuming that $b(y) = \tilde{b} \cdot y$ and setting $\tilde{b} = .9$.

to work at firms that discriminate against duration τ workers. This is in the background of our finding that in this extreme case, roughly 4% of the incidence of long-term unemployment can be attributed to discrimination.

To conclude, the relation between the costs of an interview and the value of filled jobs to a firm suggests that firms who are not interviewing do so because they are unlikely to find a suitable partner. It follows that few of the interviews the long-term unemployed lose to statistical discrimination would have led to jobs. Therefore, statistical discrimination is largely inconsequential for the job finding rate.

5 Robustness

In this section, we discuss several important model extensions and the robustness of our quantitative results to each modification. The details for each specification and their implementation can be found in Appendix B.2. All extensions use the estimated parameters from the baseline model and the Appendix spells out how we calibrate any additional parameters.³⁴

In Table 5 we contrast each alternative model with a no-discrimination benchmark that sets interview costs to zero, $\kappa = 0$, and keeps all other parameters unchanged.³⁵ Specifically, we compare steady state job finding rates at various unemployment durations and the overall incidence of long term unemployment, replicating the exercise detailed in section 4.1.1 for each of the richer models. Each specification features different outcomes when interview costs are removed, which we discuss in detail below. However, as shown in Table 5, what all the extensions have in common is that the quantitative impact of removing discrimination is limited, and much smaller than what might be expected given the large amount of discrimination that has been detected in audit studies.

5.1 Skill Depreciation During Unemployment

In the first extension, we allow human capital x to depreciate in unemployment and appreciate during employment.³⁶ Skill depreciation is clearly a prominent theory of what might generate true duration dependence in the job finding rate per se, even in the absence of employer discrimination. Since our focus is the impact of employer discrimination, we care about skill depreciation only in

³⁴In general, we could reestimate unobserved heterogeneity for each of the alternative models. However, in line with the observation that our key quantitative findings are similar across specifications, we find that the extended models generate similar moments as the baseline when we plug in the estimates from the baseline.

³⁵The final exercise in section 5.6 is different in spirit which is why we report its results separately.

³⁶We discipline the rate of skill decay using recent micro-empirical evidence on the impact of time out of work on wages. For details see Appendix B.2.

so far as it interacts with employer discrimination. We capture this interaction in our exercise by contrasting the full model with skill loss with a no-discrimination benchmark that features skill loss.

Intuitively, skill loss may interact with employer discrimination because even a single incident of discrimination will subject the worker to a longer unemployment spell. This will lower the worker’s future employment rate, which feeds back into the evolution of her skills, potentially generating a large multiplier through which equilibrium discrimination may affect the cross-sectional distribution of ability, job finding rates, and long-term unemployment.

As we report in Table 5, the inclusion of skill loss indeed increases the impact of discrimination by more than an order of magnitude relative to the baseline model. When firms are discriminating, long-term unemployment is .03 percentage points higher relative to the benchmark where interviews are costly. This is much larger than in our main specification and is a result of the large multiplier effect outlined above. Discrimination leads to lower skills which hurts a workers unemployment exit rate which in turn lowers skill further. This multiplier effect is particularly pronounced under our production function (7) since skill loss sharply affects the job finding rate. Nonetheless, the results are quantitatively small: The rate of long-term unemployment in the extended model is 1.65% implying that the overall contribution of discrimination to long-term unemployment is still minor. This reflects that the job finding rate only differs by at most .01 percentage points for the long-term unemployed, relative to the no-discrimination benchmark. We thus conclude that our main quantitative finding is robust to this extension.

5.2 Positive Worker Bargaining Power and Endogenous Search Effort

In a second extension of the framework, we relax two assumptions: First, we give workers strictly positive bargaining power.³⁷ The reason this alters our results is straightforward from equation (13). It highlights that it is the size of the interview costs relative to the the value of a match (to a firm) that governs the incidence of discrimination that maps into the job finding rate.³⁸ Furthermore, we endogenize the intensity at which unemployed workers search for jobs. This allows for a potentially important feedback effect because workers can grow discouraged in response to discrimination potentially amplifying its consequences. In particular, if the remaining high types

³⁷As pointed out above, the equilibrium behavior of firms potentially violates definition 1 of “discrimination”: Some low y employers refuse to interview unemployed workers with short duration since they expect workers to reject their jobs because of option value considerations. That is, the callback rate generated by the model is not necessarily monotonic in duration τ . In practice, we find this effect to be dominated by the discrimination arising from high y employers and the comparison with the no-discrimination benchmark is valid in either case.

³⁸For instance, giving workers a bargaining power of $\alpha = \frac{2}{3}$, it follows from equation (13) and approximation (14) that the share of workers that experience discrimination from jobs they are qualified for increases threefold at any duration τ .

among the long-term unemployed become discouraged because discrimination lowers their job finding rates, this reinforces the motives for discrimination since it is even less likely for firms to find qualified workers. This latter mechanism is closely related to the self-fulfilling prophecy modeled in [Coate and Loury \(1993\)](#).

Our quantitative results reported in [Table 5](#) confirm the amplification argument for the long-term unemployed yet job finding rates for the short-term unemployed workers are actually larger in the discrimination case: The threat of experiencing an actual decline in their job finding rates caused by discrimination leads these workers to raise their search effort early on in their unemployment spell. Once long-term unemployed, some workers experience declining job finding rates and become discouraged, relative to the no-discrimination benchmark. Since, overall, the negative consequences for the job finding rate are still extremely small, discrimination does not breed widespread discouragement. As before, the vast majority of workers do not experience an impact of duration on their chances of finding a job and hence do not lower their search effort. The high types “falling through the cracks” now become discouraged as their duration increases, but the overall quantitative impact is small. For the incidence of long-term unemployment, the increased search efforts of the short-term unemployed outweigh the consequences of discrimination which is why the no-discrimination benchmark has slightly higher rates of long-term unemployment.

5.3 Multiple Applications per Job Opening

We also study a version of the model with coordination frictions where firms receive multiple applications and workers’ applications can potentially crowd each other out. This environment may well give rise to a much larger impact of firms’ interviewing decisions on true duration dependence. The reason is the following key difference to our baseline model: There, a firm would discriminate based on duration only if it was quite certain that the worker was not qualified. Here, a firm discriminates against a worker it might be perfectly willing to interview otherwise, as soon as it has another applicant that is more likely to be qualified, that is given that she has shorter duration. It follows that potentially many more workers that would have been hired ex post are not invited for an interview, solely based on their unemployment duration.

To do so, we adopt an urn-ball matching function as for instance in [Blanchard and Diamond \(1994\)](#). As pointed out above, the key difference between our framework and [Blanchard and Diamond \(1994\)](#) is that their discrimination is “taste-based” in the sense of [Becker \(1971\)](#): Firms discriminate because they have a distaste for workers with longer unemployment duration rather than due to an endogenous motive. In turn, discrimination in our framework is statistical which sharply alters both the qualitative and quantitative conclusions. With taste-based discrimination

workers lose a job to discrimination whenever there is a worker with shorter duration in the same queue. In turn, a worker x in our framework loses a job to discrimination whenever there is a qualified worker with shorter duration in the same queue and an interview with worker x would have, counterfactually, led to a hire.³⁹

Specifically, we model an environment where a vacant job receives an exponentially distributed number of applications and firms endogenously rank applicants by their current unemployment duration. A firm chooses a cutoff duration, and sequentially interviews applicants with duration below the cutoff according to their duration, hiring the first qualified worker they find. To establish a frictionless no-discrimination benchmark, we again set $\kappa = 0$. In this case, firms interview all applicants and hire the one with the largest x .⁴⁰

As can be observed in Table 5, discrimination harms the long-term unemployed, yet benefits those with short durations. In the no-discrimination benchmark, the short-term unemployed have lower job finding rates while those with higher durations fare better. Overall, the former effect turns out to dominate slightly and long-term unemployment is slightly higher without costly interviews. In either case, discrimination does cause duration dependence in the job finding rate by helping those in short spells and harming those in long spells. However, recall that the overall decline in callbacks that workers experience after the first year is roughly 50%, most of which does not map into a decline in the job finding rate. Hence, we conclude that like in the other cases, the contribution of duration dependence in callbacks to duration dependence in the job finding rate and long-term unemployment remains small.

5.4 Production Function

Since most of this paper relies on a nonstandard production function we repeat the exercise using additively separable production $p(x, y) = x + y$. As we point out in the main text, if vacancies have no option value any standard production function generates negative assortative matching in equilibrium since the highest y firms are least selective in terms of worker quality.⁴¹ It follows that discrimination arises from low y employers: While high y employers can have positive joint

³⁹Taste-based discrimination is inconsistent with the evidence in Kroft et al. (2013) that callbacks fall less in a slack labor market, and in the context of unemployment duration is unlikely to be present. Duration discrimination also contrasts with other forms of discrimination that are more likely to be taste-based due to historical precedence (such as race or gender discrimination).

⁴⁰Clearly, they are indifferent between all applicants with $x \geq y$ which makes this particular hiring pattern only weakly optimal. However, it seems a natural tie-breaker assumption since it would be strictly optimal with any production function that is strictly increasing in x .

⁴¹While we just assume that vacancies have no option value this would also be the case with a free entry condition where vacancies have zero marginal value. Thus, this argument carries over to production functions with stronger complementarities. We have worked with a CES production function and found that varying the degree of the elasticity of substitution does not affect our quantitative findings.

surplus even with low x workers, a low y job requires a high x worker to cover the outside option b .

Nevertheless, the basic message from the baseline holds: The interviews that become unavailable as duration τ increases are those by low y employers. The vast majority of the long-term unemployed are low x workers who cannot form viable matches with low y employers. Hence, the vanishing interviews do not affect the job finding opportunities of most long-term unemployed workers. In turn, discrimination has an impact on the job finding rate for the high worker types that remain in unemployment for sufficiently long to experience discrimination by employers that would hire them *ex post*. As Table 5 documents, the incidence of such events is not substantially altered when working with a standard production function. Quantitatively, this follows exactly from the argument for the baseline model outlined in section 4.3.

5.5 Noise

The final extension introduces noise into the hiring process. Figure 8 illustrates that dynamic selection in our baseline model is very “clean” in that extremely few high types are long-term unemployed. This extension thus aims at “contaminating” the pool of long-term unemployed workers by making the hiring process more noisy.

To that end, we model a worker’s productivity which enters the production function as a match-specific draw which is imperfectly correlated with the workers underlying type x . Increasing the noise has asymmetric effects on workers’ job finding rates: Low x types are not genuinely qualified for most jobs and hence the upside potential dominates. For high types, who are qualified for most jobs, the downside potential dominates. Hence, noise compresses the dispersion in job finding rates and reduces dynamic adverse selection. It follows that the model with noise generates less duration dependence in job finding rates and, as a consequence, less discrimination.⁴² Table 5 again contrasts duration dependence in the job finding rate and long-term unemployment in this model relative to the same model without any discrimination ($\kappa = 0$). In line with the previous arguments, the causal impact of discrimination on realized outcomes in the aggregate labor market is minimal. A noisy hiring technology indeed contaminates the pool of long-term unemployed workers with many high types. In response, employers are less reluctant to interview the long-term unemployed. However, this leaves unaffected the logic we have emphasized throughout: Discrimination adversely affects those who “fall through the cracks.” If this happens more frequently due to a noisy hiring process, employers react to it in terms of their interviewing

⁴²Matching the empirical duration dependence in the hazard rate would thus require reestimating the underlying distribution of x . To reconcile this model with the empirical moments would require a larger amount of underlying heterogeneity across workers.

	ΔJFR_{ST}	ΔJFR_{MT}	ΔJFR_{LT}	ΔLTU
skill loss	0.098	0.012	0.007	-0.029
search intensity	-1.350	-0.378	0.436	0.112
multiple applications	-0.608	0.175	0.060	0.056
additive production	0.351	0.045	0.015	-0.002
noisy interviews	0.000	-0.000	0.000	-0.000

Table 5: Robustness-

Notes: First column: Percentage point change in the average job finding rate of the short-term unemployed workers ($\tau = 0 - 12$ weeks) in the full information case. Second Column: $\tau = 13 - 26$ weeks. Third Column: $\tau \geq 27$ weeks. Fourth column: Difference in the share of the population being long-term unemployed ($\tau \geq 27$ weeks).

policies. In turn, the number of job opportunities which are lost to discrimination remains small.

5.6 Rigid Beliefs

In this final section we study an environment where employers make systematic mistakes in inferring the distribution of skills among the unemployed at various durations, $u(x|\tau)$. In particular, we study the following departure from rational expectations: We start the economy in our baseline steady state and then change the meeting rate λ permanently so as to capture a long and extreme recession.⁴³ However, we fix employers' beliefs about the distribution of x among the unemployed according to its actual pre-recession distribution. This deviation from rationality seems reasonable if employers stick to rules of thumb in turbulent times and has the potential to severely harm the chances of finding a job for many of the long-term unemployed during the recession. Fully rational employers respond to increases in labor market slack by being less selective: A long-term unemployed worker during a recession is likely better qualified than during normal times. If employers stick to their pre-recession beliefs this increases the incidence of false negatives and thus the contribution of discrimination to long-term unemployment. We again contrast long-term unemployment during the recession under rigid beliefs with a counterfactual costless-interview recession. We find that a non-negligible, but still small share of roughly 2.1% of long-term unemployment during the recession can be attributed to such rigid statistical discrimination, around 2 orders of magnitude larger than under fully rational discrimination. The way we designed this exercise makes this number likely an upper bound. First, the exercise is unable to generate any of the changes in employer behavior that are correlated with slack, at odds with the evidence in [Kroft et al. \(2013\)](#). If employers updated their beliefs - even slowly -

⁴³We set λ so as to capture an unemployment rate of 17.6% as in figure 6.

this would mitigate the impact of statistical discrimination on long-term unemployment. Second, we chose to study an unusually deep recession in which unemployment rates more than double. Such a large shock in combination with fully rigid beliefs implies large mistakes on the part of employers. In summary, while we find that such systematic mistakes have the potential to substantially increase the consequences of discrimination, our quantitative findings still suggest that statistical discrimination against unemployment duration contributes little to the overall incidence of long-term unemployment.

6 Conclusion

In this paper we develop an equilibrium search model that gives rise to employer discrimination based on unemployment duration at the interview stage. The quantitative model closely captures empirical evidence on duration dependence in callbacks and its variation with aggregate labor market conditions as documented in [Kroft et al. \(2013\)](#). We use the model to quantify the consequences of employer discrimination for duration dependence in individual job finding rates and long-term unemployment. Our findings suggest that the quantitative link between true duration dependence in callbacks and job finding rates is, at best, weak. Interviews lost to statistical discrimination impact individual job-finding rates solely if they would have counterfactually led to jobs. But by virtue of the fact that the discrimination is statistical, this is rarely the case. If many lost interviews would counterfactually lead to jobs, there would be no basis for discrimination in the first place. Our results also suggest that the ability of resume audit studies to inform reduced form measures of true duration dependence in job finding rates in quantitative models of the aggregate labor market is limited.

Our paper provides the first attempt to quantitatively link the evidence from the audit study literature regarding discrimination in callbacks to real outcomes in the labor market. We believe that explicitly linking this evidence to an equilibrium model of firm hiring is an approach that can be used with audit study evidence examining other observable characteristics, such as race, gender, or age. In particular, the experimental literature can not only help to inform structural models, but the same models can help to interpret the experimental evidence. Our theory suggests that unobservable heterogeneity may be at the heart of the wide range of estimates for duration dependence in callbacks from audit studies, that have thus far not been reconciled. This only underscores the importance of linking theory and evidence in this context.

Finally, our results do not imply the absence of true duration dependence in job finding rates more generally. In fact, there are many other are potentially important sources of true duration dependence like skill decay or stock-flow matching, some of which might warrant significant pol-

icy interventions. However, we find that employer discrimination as found in recent experimental work does not cause a quantitatively sizable “unemployment trap.” This suggests that bans on such discrimination are largely ineffective in combating long term unemployment.

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A Proofs

A.1 Proof of Proposition 1

It follows immediately from assumption 1 on $p(x, y)$ and equation 4 that firms use a cutoff rule for hiring. In order to show the duration cutoff rule we first show that $U(x|\tau)$ first order stochastically dominates $U(x|\tau + 1)$, that is

$$(15) \quad \frac{\int_{\hat{x}}^{\bar{x}} u(x, \tau) dx}{\int_{\underline{x}}^{\bar{x}} u(x, \tau) dx} \geq \frac{\int_{\hat{x}}^{\bar{x}} u(x, \tau) [1 - f(x, \tau)] dx}{\int_{\underline{x}}^{\bar{x}} u(x, \tau) [1 - f(x, \tau)] dx} \quad \forall \hat{x} > \underline{x}$$

Consider two workers with $x_1 > x_2$ of the same duration τ . Whether firms discriminate or not, their chances of receiving an interview are identical. Due to assumption 1, because $H(x_1) \subseteq H(x_2) \forall x_1, x_2, \tau$ it follows that $f(x_1, \tau) \leq f(x_2, \tau) \forall x_1, x_2, \tau$, with strict inequality for a positive measure of pairs (x_1, x_2) .⁴⁴ To prove (15), rearrange it as

$$(16) \quad \frac{\int_{\hat{x}}^{\bar{x}} f(x, \tau) u(x, \tau) dx}{\int_{\hat{x}}^{\bar{x}} u(x, \tau) dx} \geq \frac{\int_{\underline{x}}^{\hat{x}} f(x, \tau) u(x, \tau) dx}{\int_{\underline{x}}^{\hat{x}} u(x, \tau) dx} \quad \forall \hat{x} > \underline{x}$$

Since $f(x, \tau)$ is weakly increasing in x the left hand side is weakly larger than $f(x, \tau)$ and the right hand side is weakly smaller than $f(x, \tau)$ which proves inequality (15). Let $y^*(\tau)$ solve

$$\int_X \max \{p(x, y^*) - b, 0\} u(x|\tau) dx = (1 - \beta(1 - \delta)) \kappa$$

Since expected surplus is declining in duration for all firms, $y^* \in I(\tau)$, but $y^* \notin I(\tau') \forall \tau' > \tau$. To show that if $y \in I(\tau')$ then $y \in I(\tau) \forall \tau < \tau'$ proceed identically. This proves that firms use cutoff strategies and that the equilibrium exhibits discrimination, $I(\tau + 1) \subset I(\tau) \forall \tau > 0$.⁴⁵

A.2 A Cumulative Measure of the Impact of Discrimination

The measure proposed in section 4.2 is a contemporaneous measure contrasting the exit rate from unemployment at some duration τ with the exit rate at the beginning of the spell. However, workers might potentially experience multiple interview rejections during an unemployment spell.

⁴⁴We prove the statement only in its weak form. Since surplus is strictly increasing at least on a nonempty subset of X and $l(x)$ has full support, it is straightforward to verify that (15) holds with strict inequality for some \hat{x} and expected surplus is thus strictly decreasing in τ .

⁴⁵This assumes that the distributions of worker and firm types have full support on X and Y , respectively. This can easily be relaxed to prove Proposition 1 for the following, slightly weaker definition of discrimination: The equilibrium exhibits discrimination in callbacks when $I(\tau + 1) \subset I(\tau) \forall \tau$ and $\exists \tau' > 0 : I(\tau' + 1) \subset I(\tau')$.

To account for that, we propose an alternative measure of true duration dependence which contrasts survivor functions for the representative group of long-term unemployed. Specifically, we contrast the realized survivor function with a counterfactual survivor function that holds the job finding rate constant at its duration 0 level,

$$TD^{\text{cml}}(\tau) = \frac{\prod_{t=0}^{\tau} (1 - \bar{f}_{\tau}(t))}{(1 - \bar{f}_{\tau}(0))^{\tau+1}}$$

where, as before, $\bar{f}_{\tau}(t) \equiv \frac{1}{\int u(x,\tau) dx} \int f(x,t) u(x,\tau) dx$ is the average job finding rate of at duration t of a representative group of duration τ unemployed workers. This measure captures the full, cumulative effects of discrimination on the job finding prospects of various groups of workers. It is conceptually similar to the measure of true duration dependence proposed in [Alvarez et al. \(2015\)](#) and has the appealing feature that we can again directly link it to the (cumulative) incidence of lost interviews that would have led to jobs,

$$TD^{\text{cml}}(\tau) = \frac{(1 - \bar{f}_{\tau}(0))^{\tau+1} + \Psi^{\text{cml}}(\tau)}{(1 - \bar{f}_{\tau}(0))^{\tau+1}}$$

where $\Psi^{\text{cml}}(\tau)$ is the probability, conditional on survival until τ , to have lost (to discrimination) an interview that would have led to a job *at least* once between 0 and τ .⁴⁶

We have contrasted this cumulative measure of duration dependence attributable to employer discrimination relative to our contemporaneous measure for the baseline model and found quantitatively very similar results. Most workers that experience “consequential discrimination” at least once in their unemployment spell are high-types who exit relatively fast, so the cumulative measure is very close to the contemporaneous one.

B Robustness

B.1 Sensitivity Analysis

In this section, we discuss the robustness of our quantitative findings to the key parameters. In particular, we increase interview costs and increase the discount and separation rate so as to

⁴⁶To see this, denote $\psi_{\tau}(t) = \lambda \int \left(\int_{y \in C(x,t)} \text{and } y \notin C(x,0) v(y) dy \right) u(x,\tau) dx$ as in equation (12). Then, we can write $\prod_{t=0}^{\tau} (1 - \bar{f}_{\tau}(t)) = (1 - \bar{f}_{\tau}(0)) (1 - \bar{f}_{\tau}(1)) (1 - \bar{f}_{\tau}(2)) \dots = (1 - \bar{f}_{\tau}(0)) (1 - \bar{f}_{\tau}(0) + \psi_{\tau}(1)) (1 - \bar{f}_{\tau}(0) + \psi_{\tau}(2)) \dots$. Then, collecting all the cross terms it is easy to see that $\prod_{t=0}^{\tau} (1 - \bar{f}_{\tau}(t)) = (1 - \bar{f}_{\tau}(0))^{\tau+1} + \Psi^{\text{cml}}(\tau)$ where $\Psi^{\text{cml}}(\tau) = (1 - \bar{f}_{\tau}(0))^{\tau} \sum_{t=1}^{\tau} \psi_{\tau}(t) + (1 - \bar{f}_{\tau}(0))^{\tau-1} \sum_{t_1=1}^{\tau} \sum_{t_2 \neq t_1}^{\tau} \psi_{\tau}(t_1) \psi_{\tau}(t_2) + \dots$

lower the joint surplus of a match. As in the main body of the paper, we contrast the equilibrium with the new parameter values featuring discrimination with the corresponding costless-interview equilibrium. Table 7 reports the difference in long-term unemployment across the two equilibria, as was reported in the last column of table 3.

Increasing κ increases the impact of discrimination on long-term unemployment. The largest known value of interview costs we are aware of is from Barron et al. (1997): They report a mean of 10 hours spent on evaluating candidates with a standard deviation of 17. A one standard-deviation upper bound roughly corresponds to tripling κ relative to baseline.⁴⁷ While the average monthly separation rate is .03 this value masks significant heterogeneity: Autor and Scarborough (2008) report a mean separation rate as high as .07 for newly formed matches. We thus increase δ relative to its baseline value, the largest value implying a monthly separation rate of .09. Again, the consequences of discrimination become more severe. Next, we decrease the weekly discount factor β to .998, implying an annual 10% discount rate which could, for instance, reflect liquidity constraints. Finally, we also increase the flow value of unemployment which reduces match surplus. To do so, we set $b = .9$ which implies an average ratio of match output to unemployment benefits of roughly 80%. None of these modifications substantially alter our quantitative conclusion, namely that the consequences of discrimination on job-finding rates and long-term unemployment are only mild.

Finally, we quantify the consequences of discrimination when we set all three parameters to their most extreme level. We take the value of interview costs that are three times baseline, a separation rate that is three times baseline, choose the high level for b , and set $\beta = .998$. This clearly delivers a substantially larger effect of discrimination, and we arrive at an extreme upper bound of discrimination: The long-term unemployment rate for this specification is 2.65%. The last column of table implies that this reduces by .11 percentage points when we lower interview costs to zero. Thus, we find that around 4% of long-term unemployment can be attributed to discrimination in this extreme case.

⁴⁷Tripling our baseline interview costs implies an interview cost exceeding three days of average output in the cross-section of existing matches. Barron et al. (1997) report the findings from several other surveys attempting to measure screening costs. The value we report here, which was originally reported in Barron and Bishop (1985), was the largest value we could find. These measures correspond to the overall time evaluating applicants rather than the interview cost per applicant which is why they constitute upper bounds.

	baseline	3κ	3δ	$\beta = 0.998$	$b = 1.2 * b$	extreme case
ΔLTU	-0.045	-0.793	-0.387	-0.053	-0.061	-11.330

Table 7: Parameter Sensitivity

Notes: All columns report *100 times* the percentage point change in the long-term unemployment rate ($\tau > 27$ weeks) between the equilibrium of the model with the parameters specified in the column headers and positive interview costs, relative to their counterpart with zero interview costs. The last column reports the same statistic when all three parameters are altered to their extreme values.

B.2 Model Extensions

B.2.1 Human Capital Depreciation

We introduce fluctuations in worker's human capital x by specifying the following process for the evolution of ability: When employed, a worker's skill x appreciates from x to $\min\{x + 1, \bar{x}\}$ with probability ϕ_e . When unemployed, a worker's skill decreases with probability ϕ_u to $\max\{x - 1, \underline{x}\}$. The value functions in this case can be written as:

$$U(x, \tau) = b + \beta \mathbf{E}_{x'|x,u} \left(U(x', \tau + 1) + \lambda \int_{I(\tau) \cap H(x')} (W(x', y, \tau + 1) - U(x', \tau + 1)) v(y) dy \right)$$

$$W(x, y, \tau) = w(x, y, \tau) + \beta \mathbf{E}_{x'|x,e} (W(x', y, \tau) + \delta (U(x', 0) - W(x', y, \tau)))$$

$$J(x, y, \tau) = p(x, y) - w(x, y, \tau) + \beta (1 - \delta) \mathbf{E}_{x'|x,e} J(x', y, \tau)$$

where the expectation operator takes into account that the transition matrix for skills is state-dependent.⁴⁸

Importantly, skills in the population are no longer distributed according to $x \sim \text{beta}(A, B) + 1$. Instead, the skill distribution is an endogenous object depending jointly on the skill process and the interviewing decisions of firms. We thus search for parameters (ϕ_e, ϕ_u) such that the steady state skill distribution comes close to the skill distribution we estimated for the baseline model with exogenous $l(x)$. In that case, the extended model captures the slope of both the empirical job finding rate and callback rate closely. Since the extended model is substantially richer, we restrict x to lie on 10 uniformly distributed grid points on the interval $(1, 2)$. We set $\phi_u = .12$ which

⁴⁸We do not need to take a stance about how wages respond to changes in skill because we maintain the zero worker bargaining power assumption in this extension.

implies a substantial loss of skill during unemployment. For instance, a worker with skill which is qualified for 50% of the available jobs at the beginning of her unemployment spell ($x = .5$) is, in expectation, only qualified for 40.4% of available jobs following an unemployment spell of 9 weeks.⁴⁹ In turn, we set $\phi_e = .003$ which implies that, to make up for an unemployment spell of 9 weeks, a worker requires 6 years of work. The reason the model demands such an asymmetric process for skill is that it requires an equilibrium distribution of skills which is heavily right-skewed and concentrated on the lower end of $(1, 2)$ (as depicted in Figure 3).

B.2.2 Search Intensity

Here, instead of meeting firms at exogenous rate λ , workers choose the rate at which they meet firms s . We employ an isoelastic search cost function, following Christensen et al. (2005),

$$c(s) = \frac{\gamma_0}{1 + \gamma_1} s^{1+\gamma_1}$$

where $1 + \gamma_1$ captures the elasticity of search costs with respect to effort. Since γ_0 scales the overall search effort of the unemployed we normalize $\lambda = 1$. Optimal search intensity of an (x, τ) -worker then satisfies the following first order condition:

$$s^*(x, \tau) = \left(\frac{1}{\gamma_0} \beta \int_{I(\tau) \cap H(x, \tau)} \alpha S(x, y, \tau + 1) v(y) dy \right)^{\frac{1}{\gamma_1}}$$

Clearly, workers' bargaining power α must be strictly positive for workers to exert any search effort.⁵⁰ In this scenario, the joint surplus between a worker and a firm depend on (x, y, τ) , which also implies that the hiring rule depends on (x, y, τ) . The value functions remain the same, except $s(x, \tau)$ replaces λ and firms account for varying search intensity when making the decision

⁴⁹In doing so, we choose a speed of skill decay that squares up with recent evidence on the consequences of additional time out of work for future labor market outcomes. For instance, Autor et al. (2015) report that an additional 2.1 months spent out of work reduce long-run earnings by 5.1% for Social Security Disability Insurance applicants. Schmieder et al. (2013) find that an additional month in unemployment due to UI extensions reduce wages by roughly one percent. While our main specification does not allow for meaningful wages, our skill transition matrix implies a productivity loss of 4.1% after a 9 week unemployment spell for the average worker, broadly in line with the empirical evidence.

⁵⁰One issue that arises with positive bargaining power for workers is the possibility that $U(x, \tau) < W(x, y, \tau) < U(x, 0)$. In this event, a worker would start a job merely to reset her unemployment duration to zero and quit after one period. In our simulations, this is the case for an extremely small number of (x, y, τ) cases for the same logic: The only workers who have an incentive to behave this way are those who experience consequential discrimination. We have thus opted to rule out quits by assumption. We also rule out, by assumption, that workers can pay firms to interview them.

whether to interview a duration τ worker, that is the interview set is characterized by

$$y \in I(\tau) \text{ iff } \frac{1}{\int s(x, \tau) u(x|\tau) dx} \int \max\{J(x, y), 0\} s(x, \tau) u(x|\tau) dx \geq \kappa.$$

We set the workers' bargaining power to $\alpha = \frac{1}{2}$ which implies an average wage-to-output ratio of $\frac{2}{3}$, crudely capturing common estimates of the labor share. Further, γ_0 is inversely related to the aggregate unemployment rate since it governs the average exit rate from unemployment. We set $\gamma_0 = 2.5$ to generate an unemployment rate close to our empirical target of 8.5%. Further, we set $\gamma_1 = 1$, which implies quadratic search costs. This comes very close to [Christensen et al. \(2005\)](#), who estimate the elasticity of the search cost function to be approximately 1.85.

B.2.3 Multiple Applications

We study an urn-ball matching function where each worker sends λ applications per period which arrive at a random firm. That is, a firm can potentially receive many applications for a given job opening. A firm hires the first qualified interviewee and interviews workers sequentially according to their unemployment duration up to a cutoff duration. The cutoff duration is, as before, implied by equation (5). It follows that, for a worker to get hired by a firm y , she needs to be below firm y 's cutoff duration and there must be no applications from workers with shorter duration who are qualified, $x \geq y$. Let $Z(x, \tau)$ be the share of unemployed workers with skill above x and duration below τ ,

$$Z(x, \tau) = \frac{\int_0^\tau \int_x^{\bar{x}} u(x', \tau') dx' d\tau'}{\int_0^\infty \int_x^{\bar{x}} u(x', \tau') dx' d\tau'}$$

We normalize the measure of vacancies to the measure of unemployed workers in equilibrium. This is without loss of generality and has the feature that $\frac{1}{\lambda}$ is the vacancy-application ratio or labor market tightness. Then, applying a law of large numbers, we can write the unemployment exit rate of an (x, τ) worker as

$$f(x, \tau) = \lambda \int_{I(\tau) \cap H(x)} \exp(-Z(y, \tau)) v(y) dy.$$

where the term in squared brackets equals the probability that no qualified worker ($x \geq y$) of lower duration applies to the same opening.

B.2.4 Additively Separable Production

All value functions and decision rules are unchanged, with $p(x, y) = x + y$ replacing production function (7). We leave all parameters identical to the baseline model but adjust the flow value of unemployment b for the following reason: Both x and y have support $(1, 2)$. With $v(y)$ uniform, the value of $x - 1$ captures the share of jobs a worker is qualified if production is characterized by equation (7). In turn, with additively separable production, if $b \leq \underline{y} + \underline{x}$ all matches are being formed and all sources of duration dependence are shut down. We hence increase the flow value of unemployment to $b = 2.8$, so as to generate duration dependence in the job finding rate that closely captures its empirical counterpart described in section 3.1. Observe that this value of b implies that the workers with the lowest productivity have positive joint surplus with 20% of jobs. In turn a worker with $x = 1.5$ gets hired by 70% of employers. This comes relatively close to the cross-sectional distribution of job finding rates implied by our main specification.

B.2.5 Noise

We assume that there is an idiosyncratic, time-invariant match-specific component drawn at the interview stage. Specifically, we adjust the production function to

$$(17) \quad p(\hat{x}, y) = \begin{cases} y, & \text{if } \hat{x} \geq y \\ 0, & \text{otherwise} \end{cases}$$

where

$$\hat{x} = x + \eta, \quad \eta \stackrel{\text{iid}}{\sim} \mathcal{N}(0, \sigma_\eta^2).$$

It follows that workers with higher x are still more likely to be qualified for any given job, but that the dynamic adverse selection is less pronounced. In particular, since job opportunities have an additional stochastic component, the pool of long-term unemployed workers becomes more “contaminated” with high types.⁵¹ We assume that firms cannot observe and workers cannot prove the true underlying x and maintain all other assumptions from the baseline model. In this environment, $y \in I(\tau)$ iff $\int_X \max\{J(\hat{x}, y), 0\} u(x|\tau) dx \geq \kappa$, and $y \in H(\tau, \hat{x})$ iff $p(\hat{x}, y) > b$.

We leave all parameters equal to the baseline and set σ_η^2 to .8 for the results reported in Table 5. We have computed the corresponding results for a wide range of values for σ_η^2 and found that our measures of the contribution of discrimination to true duration dependence and long-term

⁵¹Clearly, as $\sigma_\eta^2 \rightarrow \infty$ all duration dependence vanishes from the model and there is no longer any motive for discrimination. In turn, the limiting case of $\sigma_\eta^2 \rightarrow 0$ is our baseline model.

unemployment are close to the results for our baseline model. This is because there is a general tension between contaminating the pool of long-term unemployed and the degree of screening in interviews. If firms recognize that the pool is more contaminated with qualified workers, they respond by interviewing them more often.