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Audits as Evidence: Experiments, Ensembles, and Enforcement[*]

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Abstract

We develop tools for utilizing correspondence experiments to detect illegal discrimination by individual employers. Employers violate US employment law if their propensity to contact applicants depends on protected characteristics such as race or sex. We establish identification of higher moments of the causal effects of protected characteristics on callback rates as a function of the number of fictitious applications sent to each job ad. These moments are used to bound the fraction of jobs that illegally discriminate. Applying our results to three experimental datasets, we find evidence of significant employer heterogeneity in discriminatory behavior, with the standard deviation of gaps in job-specific callback probabilities across protected groups averaging roughly twice the mean gap. In a recent experiment manipulating racially distinctive names, we estimate that at least 85% of jobs that contact both of two white applications and neither of two black applications are engaged in illegal discrimination. To assess more carefully the tradeoff between type I and II errors presented by these behavioral patterns, we consider the performance of a series of decision rules for investigating suspicious callback behavior under a simple two-type model that rationalizes the experimental data. Though, in our preferred specification, only 17% of employers are estimated to discriminate on the basis of race, we find that an experiment sending 10 applications to each job would enable accurate detection of 7-10% of discriminators while falsely accusing fewer than 0.2% of non-discriminators. A minimax decision rule acknowledging partial identification of the joint distribution of callback rates yields higher error rates but more investigations than our baseline two-type model. Our results suggest illegal labor market discrimination can be reliably monitored with relatively small modifications to existing audit designs.

Keywords: Audit Study, Empirical Bayes, Discrimination, Indirect Evidence, Partial Identification, False Discovery Rates, Large-Scale Inference

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1 Introduction

It is illegal to use information on age, race, or sex to make employment decisions in the United States. Though economists are often called upon to evaluate claims of illegal employment discrimination, academic research in labor economics provides surprisingly little methodological guidance for assessing whether particular employers are discriminating. Rather, the focus of the voluminous empirical literature on labor market discrimination (Altonji and Blank, 1999; Guryan and Charles, 2013) has centered around methods for establishing whether *markets* discriminate against particular groups of workers on average. At least since the work of Becker (1957), however, it has been recognized that employers may vary substantially in the extent to which they are discriminatory, and that this variation (in particular, the difference in prejudice between the marginal and average firm) influences the adverse impact of discrimination on outcomes for minority workers (Charles and Guryan, 2008). It is therefore essential to understand heterogeneity in discrimination across employers, both for assessing the economic consequences of discrimination and for enforcing anti-discrimination law.

This paper develops tools for detecting discrimination by individual employers. The proposed methods rely on correspondence experiments in which fictitious applications with randomly assigned characteristics are submitted to actual job vacancies (Bertrand and Duflo, 2017 provide a review). A key advantage of correspondence studies over traditional in-person audits is that the perceived traits of an applicant can be independently manipulated, revealing the *ceteris paribus* influence of protected attributes such as race or gender on employer behavior. Starting with the seminal work of Bertrand and Mullainathan (2004), it has become standard to sample thousands of jobs and send each of them four applications. Bertrand and Mullainathan found callback rates to distinctively white names to be roughly 50% higher than those to distinctively black names, leading them to conclude that discrimination was operative in the markets they studied. Our basic insight is that such a study is best viewed as an *ensemble* of many small exchangeable experiments. From this ensemble, one can infer properties of the distribution of discriminatory behavior which can, in turn, be used to form posteriors about the probability that any given employer is discriminating. These posteriors can then aid in making decisions about which employers to investigate.

To ground our analysis, we develop a formal econometric framework for analyzing correspondence studies. The foundation of this framework is the assumption that callback outcomes at a particular job constitute independent Bernoulli trials governed by job- and race-specific callback probabilities. We show that this assumption is testable and document empirical support for it in correspondence study data. Treating the pair of callback rates at each job as a random draw from a stable super-population, we denote the joint cumulative distribution function of white and black callback probabilities by $G(p_w, p_b) : [0,1]^2 \rightarrow [0,1]$. We then establish which moments of $G(\cdot,\cdot)$ are identified as a function of the number of applications of each race sent to each job. Though our focus is on correspondence studies, these results are more broadly applicable to ensembles of randomized experiments implemented across many sites.

Building on our identification results, we propose shape-constrained Generalized Method of Moment (GMM) estimators of the identified moments of the callback distribution that require the moment estimates be rationalizable by a coherent bivariate probability distribution. We apply these estimation methods to three experimental datasets: the original Bertrand and Mullainathan (2004) study of racial discrimination, a larger, more recent study by Nunley et al. (2015) of racial discrimination in the market for recent college graduates, and a study by Arceo-Gomez and Campos-Vasquez (2014) that used eight applications per job. In each study, we find overwhelming evidence of heterogeneity across jobs in the extent of discrimination. In the more recent studies, where third and higher moments are identified, we find evidence of skew and thick tails in the distribution of discriminatory behavior: while most firms barely discriminate, a few discriminate very heavily.

Next we consider what race-specific callback distributions $G(\cdot,\cdot)$ are consistent with the identified moments of the callback distribution. Of particular interest is the fraction of jobs exhibiting any discrimination. We derive an analytic lower bound on the fraction of jobs that engage in discrimination conditional on the total number of callbacks. We then show how sharp bounds can be computed via a linear programming routine that works with a discrete approximation to $G(\cdot,\cdot)$ to characterize the relevant moment constraints. These bounds extend some results in the literature on large scale inference concerned with identification of the fraction of null hypotheses that are true (Benjamini and Hochberg, 1995; Efron et al., 2001; Storey, 2002; Efron, 2004, 2012). We find that the linear programming bounds are significantly tighter than our analytic bound and are informative even among the sub-population of jobs calling back no applications. In the Bertrand and Mullanaithan experiment, we estimate that at least half of the jobs calling back one, two, or three of the four applications sent to each job are discriminating based upon race. By contrast, as few as 20% of the jobs that call back all four applications are discriminating and as few as 5% of the jobs that call back no applications are discriminating.

These bounds on the fraction of jobs that discriminate constitute a form of "indirect evidence" (Efron, 2010) that can be used to refine an assessment of whether any individual employer is discriminating. Consider, for example, the case of a job sent four applications that calls back only the two white applications. Under the null hypothesis that callbacks do not depend on race at this job, the probability of only the two white applications being contacted given that two applications have been called back in total is 1/6. But in the Bertrand and Mullainathan data, we estimate that at most 56% of the jobs that call back two applications in total are not discriminating, so only a very weak presumption of innocence is justified. Moreover, calling back only the white applications is relatively common, occuring in 34% of the cases where two total applications are called back. Bayes' rule then implies the probability that such a job is not discriminating is at most $\frac{1}{6} \times \frac{.56}{.34} \approx .27$. Here, the indirect evidence tips the scale slightly in the employer's favor but allows us to conclude that, at most, 27% of such jobs are not discriminating on the basis of race. This need not be the case in general; in the Nunley et al. (2015) experiment, for example, we estimate that at most 15% of jobs that contact two white and zero black applicants are not discriminating.

Making decisions based upon lower-bound posterior probabilities of discrimination may yield overly conservative inferences. We develop a decision theoretic framework formalizing the problem of a hypothetical "auditor" such as the Equal Employment Opportunity Commission (EEOC) that has been charged with making decisions about whether to investigate particular employers. Specifically, we consider Bayes auditing rules under a linear loss function where each type I and type II error incurs a fixed cost. The Bayes decision rule takes the usual form, where investigations are conducted when the posterior probability of discrimination crosses a fixed threshold. To approximate the Bayes decision problem, we work with a mixed logit representation of the data generating process (DGP) in the Nunley et al. (2015) experiment that also incorporates application characteristics other than race. This model provides a good empirical fit to the Nunley et al. (2015) data and reproduces the qualitative patterns uncovered in our GMM analysis. We use the mixed logit estimates to form empirical Bayes posteriors of the probability that any given employer is discriminating. For instance, the posterior probability that an employer that contacts only the two white applications is discriminating is 62%. But when the two white applications have other characteristics indicating they are of low quality and the two black applications have high-quality characteristics, this posterior rises to 80%.

We then compute the tradeoff between type I and type II errors implied by the logit DGP under different experimental designs. With only two white and two black applications per job, it is difficult to reliably identify discriminating employers. But with just 10 applications per job, we find that it is possible to correctly identify 7% of discriminating jobs with type I error rates of less than 0.2%. Moreover, we show that by optimizing over combinations of race and other resume characteristics to maximize the amount of information generated by the experiment, it is possible to boost the fraction of discriminators detected to roughly 10% while continuing to hold the type I error rate under 0.2%. By contrast, conducting investigations based upon a frequentist p-value cutoff of 0.01 tends to yield substantially more accusations, the majority of which are erroneous accusations of non-discriminators.

Finally, we consider how our assessment of various auditing rules changes if we relax the assumption that callbacks are actually generated by the logit DGP and instead consider the broader class of callback distributions capable of rationalizing the Nunley et al. (2015) experiment. A natural benchmark for decisionmaking with an unknown risk function is the minimax auditing rule, which minimizes the maximum risk that can arise given the identified moments of $G(\cdot,\cdot)$. We develop a linear programming algorithm for computing an estimate of the maximum risk function for classes of decision rules ordered by their logit posteriors. Applying our algorithm, we find that the gap between logit and worst case risk is decreasing in the share of jobs investigated. This pattern leads a minimax auditor to investigate more jobs than would an auditor who knew for certain that the logit DGP governed behavior. The minimax auditor, it turns out, is more concerned with the possibility that she is passing over a vast number of jobs engaged in modest amounts of discrimination than that a few non-discriminators are improperly investigated.

Our results highlight the potential of experimental methods to guide regulatory enforcement. Because employers vary tremendously in their propensity to discriminate against protected groups, regulators charged with enforcing anti-discrimination laws face a difficult inferential task. The methods developed here treat this task as an exercise in large scale testing, which serves to discipline the conclusions drawn regarding particular employers. Our findings suggest that accurately monitoring illegal discrimination in online labor markets is feasible with relatively small modifications to conventional audit designs. Analogous methods are likely to be useful in other contexts in which policymakers and researchers seek to draw conclusions about specific individuals using noisy observations across many units. Candidates for such applications include evaluations of teachers, schools, hospitals, and neighborhoods (Chetty et al., 2014b; Angrist et al., 2017; Hull, 2018; Chetty and Hendren, 2018; Chetty et al., 2018).

The rest of the paper is structured as follows. The next section offers a formal definition of employer discrimination in resume correspondence experiments. Section 3 lays out the problem of an auditor seeking to identify discriminatory employers with correspondence study data. Section 4 develops identification results for moments of $G(\cdot,\cdot)$ and bounds on posterior probabilities of discrimination. Section 5 describes the data, and Section 6 uses it to test our modeling framework. Section 7 provides estimated moments of $G(\cdot,\cdot)$ and Section 8 reports posterior bounds. Section 9 develops and estimates a mixed logit model of callback decisions, and Section 10 uses the logit estimates to evaluate prospects for detecting discrimination in alternative experimental designs. Section 11 contrasts the logit results with a minimax analysis acknowledging underidentification of $G(\cdot,\cdot)$. Section 12 offers concluding thoughts.

2 Defining Discrimination

Title VII of the Civil Rights Act of 1964 prohibits employment discrimination on the basis of race and sex, while the Age Discrimination Act of 1975 prohibits certain forms of discrimination on the basis of age. Violations of these statutes typically involve one of two types of claims. The first, *disparate treatment*, consists of employment practices that explicitly treat potential employees unequally based upon protected characteristics. While economists have debated whether such behavior arises from statistical discrimination versus racial animus (Guryan and Charles, 2013), the law makes no distinction between these motives: both are clearly illegal (Kleinberg et al., 2019). The second sort of claim, *disparate impact*, involves employment practices that, while not explicitly based on protected characteristics, clearly work to disadvantage members of such groups without offering a corresponding productive justification.

Guided by these legal doctrines, we now develop a formal notion of discrimination tailored to the analysis of correspondence studies. To simplify exposition we focus on race, which we code as binary ("white"/ "black"), with the understanding that other protected characteristics such as gender or age can play the same role. Suppose that we have a sample of J jobs with active vacancies. To each of these jobs, we send L_w applications with distinctively white names and L_b applications with distinctively black names as in Bertrand and Mullainathan (2004), for a total of $L = L_w + L_b$ applications. Denote the race associated with the name used in application $\ell \in \{1, ..., L\}$ to job

$j \in \{1, ..., J\}$ as $R_{j\ell} \in \{w, b\}$. Let the function $Y_{j\ell}(r) : \{w, b\} \rightarrow \{0, 1\}$ denote whether job j would call back application ℓ as a function of that application's assigned race. Note that this definition of potential outcomes builds in the Stable Unit Treatment Value Assumption (SUTVA) of Rubin (1980) by ruling out dependence on the races assigned to other resumes, including other applications to the same job $\{R_{jk}\}_{k \neq \ell}$. Observed callbacks decisions are then given by $Y_{j\ell} = Y_{j\ell}(R_{j\ell})$.

When $Y_{j\ell}(w) \neq Y_{j\ell}(b)$ job j has engaged in racial discrimination with application ℓ. Notably, even if racially distinctive names influence employer behavior only through their role as a proxy for parental background (Fryer and Levitt, 2004), using the names at any point in the hiring process is likely to be viewed by courts as a pretext for discrimination (see, e.g., the discussion in U.S. Equal Employment Opportunity Commission v. Target Corporation, 460 F.3d 946, 7th Cir. Wis. 2006). While courts are typically interested in establishing whether a particular plaintiff experienced discrimination in precisely this sense, we will take the perspective of a regulator interested in assessing prospectively whether an employer systematically treats applicants differently based upon race. Such "systematic" forms of discrimination are also relevant in establishing class certification in class action suits. Formalizing this notion requires some additional assumptions regarding how employers behave on average.

To this end, we work with the following representation of potential outcomes:

$$Y_{j\ell}(r) = Y_j(r, U_{j\ell}),$$

where $Y_j(\cdot)$ is a job-specific decision rule and $U_{j\ell}$ represents all factors that affect employer j's decision to contact application ℓ other than race. These factors include the attributes other than race assigned to the resume as well as unobserved fluctuations in conditions at the job such as changes in the time available for reading applications. We normalize the marginal distribution of $U_{j\ell}$ to be uniform, an assumption that is without loss of generality since $Y_j(\cdot)$ is an unrestricted non-separable function. Our first substantive assumption is that these unobserved factors are independent across applications.

Assumption 1. *The factors that influence callbacks are independent and identically distributed across applications at each job:*

$$U_{j\ell} | R_{j1} ... R_{jL} \overset{iid}{\sim} Uniform(0, 1).$$

Note that random assignment of racially distinctive names to applications guarantees independence of $U_{j\ell}$ from $\{R_{jk}\}_{k=1}^{L}$. The key behavioral restriction in Assumption 1 is that the $U_{j\ell}$ are mutually independent, which implies that each job's decision-making is characterized by a stable decision rule applied independently to each resume. This rules out, for example, a scenario in which the job calls back the first qualified applicant and disregards all subsequent applications. As we discuss later, this restriction turns out to be testable, and we find that it provides a good empirical approximation to behavior in the correspondence experiments we study.

Assumption 1 implies that each job is associated with a stable pair of race-specific callback probabilities, defined as follows:

$$p_{jr} \equiv \int_0^1 Y_j\left(r,u\right)du,\ r \in \{b,w\}.$$

The probability p_{jr} may be interpreted as the callback rate that would emerge in a hypothetical experiment in which a large number of applications of race r are sent to job j. Assumption 1 implies that callbacks take the form of (race-specific) binomial trials governed by these probabilities. Letting $C_{jr} = \sum_{\ell=1}^{L} 1\left\{R_{j\ell} = r\right\} Y_{j\ell}$ denote the number of applications of race r to job j that were called back, we can write the probability $\Pr\left(C_{jw} = c_w, C_{jb} = c_b | p_{jw}, p_{jb}\right)$ that employer j calls back c_w out of L_w white applications and c_b out of L_b black applications as:

$$f\left(c_w, c_b | p_{jw}, p_{jb}\right) = \left(\begin{array}{c} L_w \\ c_w \end{array}\right)\left(\begin{array}{c} L_b \\ c_b \end{array}\right) p_{jw}^{c_w}\left(1-p_{jw}\right)^{L_w - c_w} p_{jb}^{c_b}\left(1-p_{jb}\right)^{L_b - c_b}. \qquad (1)$$

We are now ready to offer a definition of systematic discrimination, which we will henceforth refer to simply as discrimination.

Definition. *Job j engages in discrimination when $p_{jb} \neq p_{jw}$.*

We can now label discriminatory jobs with the indicator function $D_j = 1\{p_{jb} \neq p_{jw}\}$. Note that this definition is prospective in that an employer with $D_j = 1$ will eventually discriminate against an applicant, though it may not do so in any particular finite sample. Indeed, it is likely that many of the jobs sampled in audit experiments are engaging in illegal discrimination but have not discriminated against any of the fictitious applicants in the study because none of the fictitious applicants would have been called back regardless of their race.

3 Ensembles and Decision Rules

The above framework treats each job's callback decisions as a set of race-specific Bernoulli trials. We next consider what can be learned from a collection of experiments conducted at many jobs. This idea is formalized in the following exchangeability assumption on the jobs.

Assumption 2. *Race-specific callback probabilities are independent and identically distributed:*

$$p_{jw}, p_{jb} \overset{iid}{\sim} G\left(\cdot,\cdot\right).$$

The distribution function $G\left(p_w, p_b\right) : [0,1]^2 \to [0,1]$ describes the population of jobs from which a study samples. In practice, audit studies usually draw small random samples of jobs from online job boards. The *iid* assumption abstracts from the fact that there are a finite number of jobs on these boards. Note that by virtue of random assignment (Assumption 1) p_{jw} and p_{jb} are independent of the racial mix of applications to job j as well as any other resume characteristics that are randomized.

Assumption 2 implies that the unconditional distribution of callbacks can be expressed as a mixture of binomial trials. Let $C_j \equiv (C_{jw}, C_{jb})$ denote the callback counts for job j. We denote the unconditional probability of observing the callback vector $c = (c_w, c_b)$ by

$$\begin{aligned}
\bar{f}(c) &\equiv \Pr(C_j = c) \\
&= \int f(c_w, c_b | p_w, p_b)\, dG(p_w, p_b)\,. \qquad (2)
\end{aligned}$$

The distribution $G(\cdot,\cdot)$ will serve as a key object of interest in our analysis. One reason for interest in $G(\cdot,\cdot)$ is that it characterizes both the prevalence and extent of discrimination in a population. For instance, the proportion of jobs that are *not* engaged in discrimination can be written:

$$\pi^0 \equiv \Pr(D_j = 0) = \int dG(p,p)\,.$$

A second reason for interest in $G(\cdot,\cdot)$ lies in its potential forensic value as a tool for identifying which jobs are discriminating. The quantity

$$\pi(c) \equiv \Pr(D_j = 1 | C_j = c)$$

gives the proportion of jobs with callback vector c that are discriminating. Though this quantity has a clear frequentist interpretation as the fraction of discriminators that would be found under repeated sampling, we can also think of it as giving a posterior probability of discrimination given the "evidence" C_j. Specifically, invoking Bayes' rule, we can write this posterior as a functional of the "prior" $G(\cdot,\cdot)$:

$$\begin{aligned}
\pi(c) &= \frac{\Pr(C_j = c | D_j = 1)\left(1 - \pi^0\right)}{\bar{f}(c)} \\
&= \frac{1 - \pi^0}{\bar{f}(c)} \int_{p_w \neq p_b} f(c_w, c_b | p_w, p_b)\, dG(p_w, p_b) \\
&\equiv \mathcal{P}\left(\underbrace{c}_{\text{direct}}, \underbrace{G(\cdot,\cdot)}_{\text{indirect}}\right).
\end{aligned}$$

The dependence of $\pi(c)$ on $G(\cdot,\cdot)$ is an example of what Efron (2010) refers to as "indirect evidence." To understand the logic of incorporating indirect evidence, suppose $\pi^0 = 1$ so that no jobs discriminate. Then $\pi(c) = 0$ regardless of job j's callback outcomes – any seemingly suspicious callback decisions are due to chance. Likewise, if $\pi^0 = 0$, all jobs are discriminators and there is no need for direct evidence on the behavior of particular jobs. But in intermediate cases, where some fraction of jobs are discriminators, and some are not, it is optimal to blend the direct evidence C_j from a particular job with contextual information on the population $G(\cdot,\cdot)$ from which that job was drawn to make decisions. We next analyze how exactly such indirect evidence should feature in decision-making under uncertainty.

The Auditor's Problem

Consider the problem of an auditor who knows the distribution $G(\cdot,\cdot)$ and aims to decide which jobs to investigate for the presence of illegal discrimination using a dataset of callbacks $\{C_j\}_{j=1}^{J}$ as evidence. The auditor uses a deterministic decision rule $\delta(c) : \{0,...,L_w\} \times \{0,...,L_b\} \to \{0,1\}$ that maps the callback vector $c = (c_w, c_b)$ to a binary inquiry decision.[1]

The auditor's loss function from applying a decision rule $\delta(\cdot)$ to a dataset of J jobs is:

$$\mathcal{L}_J(\delta) = \sum_{j=1}^{J} \left\{ \underbrace{\delta(C_j)(1-D_j)}_{\text{Type I}} \kappa + \underbrace{[1-\delta(C_j)] D_j}_{\text{Type II}} \gamma \right\}. \tag{3}$$

This loss function places a cost κ on investigating "innocent" jobs with $D_j = 0$ (type I errors) and a cost γ of not investigating "guilty" jobs with $D_j = 1$ (type II errors). Because the $\{D_j\}_{j=1}^{J}$ are not known, the auditor minimizes expected loss (i.e. risk), which we denote by $\mathcal{R}$:

$$\begin{aligned}
\mathcal{R}_J(G,\delta) &\equiv \mathbb{E}[\mathcal{L}_J(\delta)] \\
&= \sum_{j=1}^{J} \mathbb{E}[\delta(C_j)(1-\mathcal{P}(C_j,G))\kappa + [1-\delta(C_j)]\mathcal{P}(C_j,G)\gamma] \\
&= J\mathbb{E}[\delta(C_j)(1-\mathcal{P}(C_j,G))\kappa + [1-\delta(C_j)]\mathcal{P}(C_j,G)\gamma],
\end{aligned}$$

where $\mathbb{E}[\cdot]$ denotes the expectation operator, the second line follows from iterated expectations, and the third uses that the $\{C_j, D_j\}_{j=1}^{J}$ are *iid* across jobs. The following Lemma, which mirrors a standard result in statistical decision theory (e.g., DeGroot, 2004, Theorem 8.11.1), establishes that the optimal strategy of the auditor is to investigate jobs that exceed a posterior threshold.

Lemma 1 (Posterior Threshold Rule)**.** *The decision rule* $\delta(C_j) = 1\left\{\mathcal{P}(C_j,G) > \frac{\kappa}{\gamma+\kappa}\right\}$ *minimizes* $\mathcal{R}_J(G,\delta)$.

Proof. Risk can be rewritten:

$$\begin{aligned}
\mathcal{R}_J(G,\delta) &= J\sum_{c_w=0}^{L_w}\sum_{c_b=0}^{L_b}\int \{\delta(c_w,c_b)(1-\mathcal{P}(c_w,c_b,G))\kappa + [1-\delta(c_w,c_b)]\mathcal{P}(c_w,c_b,G)\gamma\} \\
&\times\ f(c_w,c_b|p_w,p_b)\,dG(p_w,p_b).
\end{aligned}$$

Minimizing this integral pointwise, we see that for any $c = (c_w, c_b)$ such that $\mathcal{P}(c,G) < \frac{\kappa}{\gamma+\kappa}$, the integrand is minimized by setting $\delta(c) = 0$. Otherwise, risk is minimized by setting $\delta(c) = 1$. □

One can think of the decision rule $\delta(C_j)$ as offering an economically motivated definition of "reasonable doubt": when the posterior probability of discriminating crosses the cost-based threshold $\kappa/(\kappa+\gamma)$, it is rational to conduct an inquiry.

[1] We confine ourselves to deterministic rules because randomized decision rules violate commonly held horizontal equity principles.

Remark 1. Recent work by economists emphasizes the role of preferences in the optimal design of experiments (Manski, 2000; Kitagawa and Tetenov, 2018; Narita, 2019). In our setting, an auditor might benefit from choosing the application design (L_w, L_b) in addition to the decision rule $\delta(\cdot)$ to minimize risk. We consider such an exercise empirically in Section 10.

Connection to Large Scale Testing

An interesting connection exists between the auditor's problem and the literature on large scale testing, which is concerned with deciding which hypotheses to reject based upon the results of a very large number of tests (Efron, 2012 provides a review). A seminal contribution to this literature comes from Benjamini and Hochberg (1995), who proposed controlling the False Discovery Rate (FDR): the expected share of rejected null hypotheses that are true. We next show that the auditor's optimal decision rule will control an analogue of the FDR.

Letting $N_J \equiv \sum_{j=1}^{J} \delta(C_j)$ denote the total number of investigations resulting from the auditing rule $\delta(\cdot)$, we can define the *Positive False Discovery Rate* (Storey, 2003) as:

$$pFDR_J = \mathbb{E}\left[N_J^{-1} \sum_{j=1}^{J} \delta(C_j)(1-D_j)|N_J \geq 1\right].$$

In words, $pFDR_J$ gives the proportion of investigated jobs that are not discriminating, conditional on at least one investigation taking place. The following Lemma establishes that the optimal decision rule controls $pFDR_J$ at a level determined by the ratio κ/γ.

Lemma 2 ($pFDR_J$ Control)**.** *If* $\delta(C_j) = 1\left\{\mathcal{P}(C_j, G) > \frac{\kappa}{\gamma+\kappa}\right\}$ *then* $pFDR_J \leq \frac{\gamma}{\kappa+\gamma}$.

Proof. Storey (2003, Theorem 1) showed that $pFDR_J = \Pr(D_j = 0|\delta(C_j) = 1)$ for any deterministic decision rule $\delta(\cdot)$ obeying $\Pr(\delta(C_j) = 1) > 0$ (see Appendix A for a self-contained proof of this result). Therefore the optimal auditing rule $\delta(C_j) = 1\left\{\mathcal{P}(C_j, G) > \frac{\kappa}{\gamma+\kappa}\right\}$ yields

$$\begin{aligned} pFDR_J &= \Pr\left(D_j = 0|\mathcal{P}(C_j, G) > \frac{\kappa}{\gamma+\kappa}\right) \\ &\leq \Pr\left(D_j = 0|\mathcal{P}(C_j, G) = \frac{\kappa}{\gamma+\kappa}\right) = 1 - \frac{\kappa}{\gamma+\kappa}. \end{aligned}$$

□

By contrast, consider an auditor who bases investigations on a classical hypothesis test $\delta^{\dagger}(C_j)$ that controls size at a fixed level $\tilde{\alpha} < 1$. To simplify exposition, suppose that the test is pivotal under the null of non-discrimination so that

$$\Pr\left(\delta^{\dagger}(C_j) = 1|p_{jw} = p, p_{jb} = p\right) = \tilde{\alpha}, \quad \forall p \in [0,1].$$

We can write the resulting $pFDR_J$ of this rule

$$\begin{aligned}
\Pr\left(D_j = 0|\delta^{\dagger}\left(C_j\right) = 1\right) &= \frac{\Pr\left(\delta^{\dagger}\left(C_j\right) = 1|D_j = 0\right)\pi^0}{\Pr\left(\delta^{\dagger}\left(C_j\right) = 1|D_j = 0\right)\pi^0 + \Pr\left(\delta^{\dagger}\left(C_j\right) = 1|D_j = 1\right)\left(1-\pi^0\right)} \\
&\geq \frac{\tilde{\alpha}\pi^0}{\tilde{\alpha}\pi^0 + 1 - \pi^0}.
\end{aligned}$$

To see that $\delta^{\dagger}\left(C_j\right)$ fails to control $pFDR_J$, note that $\lim_{\pi^0 \uparrow 1} \frac{\tilde{\alpha}\pi^0}{\tilde{\alpha}\pi^0+1-\pi^0} = 1$: when nearly all jobs are innocent, classical hypothesis testing will result in the vast majority of investigations being false accusations.

Remark 2. The *False Discovery Rate* of Benjamini and Hochberg (1995) can be written $FDR_J = pFDR_J \times \Pr\left(N_J \geq 1\right)$. Because $\Pr\left(N_J \geq 1\right) \leq 1$, the optimal auditing rule also controls FDR_J.

Remark 3. The auditor's risk can be written

$$\mathcal{R}_J\left(G, \delta\right) = J\left\{\kappa \times pFDR_J \times \Pr\left(\delta\left(C_j\right) = 1\right) + \gamma \times pFNR_J \times \left[1 - \Pr\left(\delta\left(C_j\right) = 1\right)\right]\right\},$$

where $pFNR_J = \mathbb{E}[(J - N_J)^{-1} \sum_{j=1}^{J}\left(1 - \delta(C_j)\right) D_j | N_J < J]$ is the *Positive False Nondiscovery Rate* (Storey, 2003, Corollary 4). Hence, the auditor's marginal rate of substitution between the Positive False Discovery and Positive False Nondiscovery rates is $\frac{\kappa}{\gamma}\frac{\Pr(\delta(C_1)=1)}{1-\Pr(\delta(C_1)=1)}$.

Auditing under Ambiguity

The distribution $G\left(\cdot,\cdot\right)$ will not, in general, be point identified even by experiments with many applications per job. When G is only known to lie in some identified set Θ of distributions, many possible decision rules are consistent with rationality. Among those rules, an important benchmark is the minimax decision rule (Wald, 1945; Savage, 1951; Manski, 2000), which minimizes the maximum risk that may arise from the experiment. We define the maximum risk function and the associated minimax decision rule respectively as:

$$\mathcal{R}_J^m\left(\Theta, \delta\right) \equiv \sup_{G \in \Theta} \mathcal{R}_J\left(G, \delta\right) \quad \text{and} \quad \delta^{mm} \equiv \arg\inf_{\delta \in \mathscr{D}} \mathcal{R}_J^m(\Theta, \delta), \tag{4}$$

where $\mathscr{D}$ is the set of deterministic decision rules.

Unlike in the case where $G\left(\cdot,\cdot\right)$ is known, an auditor that only knows $G \in \Theta$ cannot consult a single posterior probability to make the decision of whether to investigate. Rather, the maximum risk of each decision rule must be computed to obtain the minimax decision rule. The next section establishes more carefully what features of $G\left(\cdot,\cdot\right)$ are identified by a given experimental design and provides an approach to computing $\mathcal{R}^m\left(\Theta, \delta\right)$.

Remark 4. Rules that minimize maximum risk over a restricted set Γ of distributions were considered by Hodges et al. (1952) and Robbins (1964) and are sometimes referred to as $\Gamma-$minimax estimators (see, e.g., Berger, 1979; Noubiap et al., 2001; Lehmann and Casella, 2006; Berger, 2013). While the statistics literature has typically chosen the set of candidate distributions based upon prior beliefs, the definition in (4) restricts consideration to distributions that match the identified features of $G(\cdot,\cdot)$.

Remark 5. The minimax decision rule δ^{mm} is also a Bayes rule if there is a $\bar{G} \in \Theta$ such that $\arg\inf_{\delta\in\mathscr{D}} \mathcal{R}_J\left(\bar{G},\delta\right) = \delta^{mm}$. When such a $\bar{G}$ exists, we call it a least favorable distribution because $\bar{G} \in \arg\sup_{G\in\Theta} \mathcal{R}_J\left(G,\delta^{mm}\right)$ (see, e.g., Lehmann and Casella, 2006).

Remark 6. One can think of the minimax decision rule δ^{mm} as an estimator of the latent discrimination indicators $\{D_j\}_{j=1}^J$. It is interesting to contrast δ^{mm} with standard "shrinkage" estimators, which are typically motivated by appeal to a parametric mixing distribution that serves the role of a prior (Kane and Staiger, 2008; Chetty et al., 2014a; Angrist et al., 2017; Chetty and Hendren, 2018; Finkelstein et al., 2017). When it has a Bayes interpretation, the minimax estimator δ^{mm} can be thought of as shrinking towards a least favorable prior distribution $\bar{G}$.

4 Identification of G

In this section we establish that certain moments of $G\left(\cdot,\cdot\right)$ are non-parametrically identified and then proceed to derive bounds on the posterior probability function $\pi\left(c\right)$.

Moments

From (2) we can write

$$\bar{f}(c_w,c_b) = \left(\begin{array}{c} L_w \\ c_w \end{array}\right)\left(\begin{array}{c} L_b \\ c_b \end{array}\right)\mathbb{E}\left[p_{jw}^{c_w}\left(1-p_{jw}\right)^{L_w-c_w}p_{jb}^{c_b}\left(1-p_{jb}\right)^{L_b-c_b}\right]$$

$$= \left(\begin{array}{c} L_w \\ c_w \end{array}\right)\left(\begin{array}{c} L_b \\ c_b \end{array}\right)\sum_{x=0}^{L_w-c_w}\sum_{s=0}^{L_b-c_w}(-1)^{x+s}\left(\begin{array}{c} L_w-c_w \\ x \end{array}\right)\left(\begin{array}{c} L_b-c_b \\ s \end{array}\right)\mathbb{E}\left[p_{jw}^{c_w+x}p_{jb}^{c_b+s}\right]. \quad (5)$$

Hence, the reduced form callback probabilities can be written as linear functions of uncentered moments $\mu\left(m,n\right) \equiv \mathbb{E}\left[p_{jw}^m p_{jb}^n\right] = \int p_w^m p_b^n dG\left(p_w,p_b\right)$ of the latent callback probabilities.

Letting $\bar{f} = \left(\bar{f}\left(1,0\right),...,\bar{f}\left(L_w,0\right),...,\bar{f}\left(L_w,L_b\right)\right)'$ denote the vector of frequencies for all possible callback outcomes excluding $(0,0)$ and $\mu = \left(\mu\left(1,0\right),...,\mu\left(L_w,0\right),...,\mu\left(L_w,L_b\right)\right)'$ the corresponding list of moments, we can write the equations in (5) as a linear system:

$$\bar{f} = B\mu,$$

where B is a known non-singular square matrix of binomial coefficients. We can therefore solve the linear system as:

$$\mu = B^{-1}\bar{f}. \quad (6)$$

Hence, for a given application design (L_w,L_b), all moments $\mu(m,n)$ for $0 \le m \le L_w$ and $0 \le n \le L_b$ are identified.

From μ we can compute centered moments of the callback distribution, which are typically easier to interpret. An example of particular interest is the standard deviation of discrimination across jobs:

$$\begin{aligned}\mathbb{V}\left[p_{jb}-p_{jw}\right]^{1/2} &= \sqrt{\mathbb{E}\left[\left(p_{jb}-p_{jw}\right)^{2}\right]-\mathbb{E}\left[p_{jb}-p_{jw}\right]^{2}} \\ &= \sqrt{\mu\left(0,2\right)+\mu\left(2,0\right)-2\mu\left(1,1\right)-\mu\left(0,1\right)^{2}-\mu\left(1,0\right)^{2}+2\mu\left(0,1\right)\mu\left(1,0\right)}.\end{aligned}$$

This quantity is identified for any application design that sends at least two resumes per racial group ($\min\left\{L_w, L_b\right\}\geq 2$).

Remark 7. The variance of unit-specific treatment effects is typically treated as under-identified in standard analyses of randomized experiments (Neyman, 1923; Heckman et al., 1997; Imbens and Rubin, 2015). Identification is secured here by the assumption that callbacks are generated by race-specific binomial trials with stable probabilities, essentially allowing repeated observations on the same units with and without treatment.

Remark 8. In experiments where the application design (L_w, L_b) varies randomly across jobs, some moments of $G(\cdot,\cdot)$ are over-identified. We exploit these over-identifying restrictions in estimation to improve precision and test our modeling assumptions.

A Bound on Posterior Probabilities

Though the study of moments of the callback distribution $G(\cdot,\cdot)$ can shed light on underlying heterogeneity in callback behavior, the posterior probability $\pi\left(c\right)$ need not admit a representation in terms of a finite number of moments. However, a simple analytic bound on the posterior can be derived from an application of Bayes rule that conditions on the total number of callbacks $C_{jb}+C_{jw}$ to firm j. Let $\bar{f}_t\left(c\right)\equiv\Pr\left(\left(C_{jw},C_{jb}\right)=\left(c_w,c_b\right)|C_{jb}+C_{jw}=t\right)$ denote the probability mass function for callbacks in the stratum of jobs that call back t applicants in total. Similarly, let $\pi_t^0\equiv\Pr\left(D_j=0|C_{jb}+C_{jw}=t\right)$ denote the share of jobs in this stratum that are not discriminating. Letting $t=c_w+c_b$, we can then write the posterior probability of innocence as:

$$1-\pi\left(c\right)=\Pr\left(C_j=c|D_j=0,C_{jb}+C_{jw}=t\right)\frac{\pi_t^0}{\bar{f}_t\left(c\right)}. \tag{7}$$

Note that after conditioning on the total number of callbacks, the callback likelihood for non-discriminators is free of nuisance parameters:

$$\Pr\left(C_j=c|D_j=0,C_{jb}+C_{jw}=t\right)=\left(\begin{array}{c}L_w\\ c_w\end{array}\right)\left(\begin{array}{c}L_b\\ c_b\end{array}\right)/\left(\begin{array}{c}L\\ t\end{array}\right)\equiv\bar{f}_t^0\left(c\right).$$

This ratio forms the basis of Fisher's exact test for independence in contingency tables (Fisher, 1922). For example, with two white and two black applications and two callbacks, the probability of both callbacks being to white applications under the null hypothesis of non-discrimination is

$$\bar{f}_2^0(2,0) = \begin{pmatrix} 2 \\ 2 \end{pmatrix} \begin{pmatrix} 2 \\ 0 \end{pmatrix} / \begin{pmatrix} 4 \\ 2 \end{pmatrix} = \frac{1}{6}.$$

Since the function $\bar{f}_t(c)$ is directly identified by random sampling, the sole under-identified quantity in equation (7) is π_t^0, which serves as the auditor's prior probability that an employer is innocent knowing only that it made t callbacks in total. The following Lemma provides a tractable bound on this quantity.

Lemma 3 (Upper Bound on Stratum Prior)**.** $\pi_t^0 \leq \min_{c:c_w+c_b=t} \min\left\{\frac{\bar{f}_t(c)}{\bar{f}_t^0(c)}, \frac{1-\bar{f}_t(c)}{1-\bar{f}_t^0(c)}\right\}$.

Proof. By the law of total probability:

$$\bar{f}_t(c) = \bar{f}_t^0(c)\pi_t^0 + \bar{f}_t^1(c)\left(1-\pi_t^0\right),$$

where $\bar{f}_t^1(c) \equiv \Pr(C_j = c | D_j = 1, C_{jb} + C_{jw} = t)$. The result follows immediately from observing that $\bar{f}_t^1(c) \in [0,1]$. □

Plugging the upper bound of Lemma 3 into (7) therefore yields a *lower* bound on the posterior probability of discrimination:

$$\pi(c) \geq 1 - \frac{\bar{f}_t^0(c)}{\bar{f}_t(c)} \min_{c':c'_w+c'_b=t} \min\left\{\frac{\bar{f}_t(c')}{\bar{f}_t^0(c')}, \frac{1-\bar{f}_t(c')}{1-\bar{f}_t^0(c')}\right\}. \tag{8}$$

Remark 9. A bound of the sort derived in Lemma 3 was used by Efron et al. (2001, p. 1154) to control FDR_J in a multiple testing analysis of a microarray experiment. Storey (2002) proposed a related class of upper bounds that are generally looser, but easier to estimate (see Armstrong, 2015 for an approach to inference on these bounds).

Sharp Bounds

While the bounds in Lemma 3 are easy to compute, they need not be sharp, as restrictions across strata defined by the number of callbacks $C_{jb} + C_{jw}$ have been ignored. An upper bound on the prior π_t^0 that exploits all of the logical restrictions in our framework can be written as the solution to the following constrained optimization problem:

$$\max_{G(\cdot,\cdot)\in\mathscr{G}} \frac{\begin{pmatrix} L \\ t \end{pmatrix}}{\sum_{(c'_w,c'_b):c'_w+c'_b=t} \bar{f}\left(c'_w, c'_b\right)} \int p^t (1-p)^{L-t} dG(p,p), \tag{9}$$

$$s.t.\ \ \bar{f}(c_w, c_b) = \begin{pmatrix} L_w \\ c_w \end{pmatrix} \begin{pmatrix} L_b \\ c_b \end{pmatrix} \int p_w^{c_w} (1-p_w)^{L_w-c_w} p_b^{c_b} (1-p_b)^{L_b-c_b} dG(p_w, p_b), \tag{10}$$

$$\text{for } (c_w = 0, .., L_w; c_b = 0, .., L_b).$$

To make this problem computationally tractable, we consider a space $\mathscr{G}$ of discretized approximations to the unknown distribution function $G(\cdot,\cdot)$ (see Noubiap et al., 2001 and Tebaldi et al., 2019 for related approaches). Because both the objective and constraints are linear in the probability mass function associated with $G(\cdot,\cdot)$, we can apply linear programming routines to compute bounds given an estimate of the callback probabilities $\left\{\bar{f}\left(c_w, c_b\right)\right\}_{c_w,c_b}$ (we defer the discussion of estimation to Section 7). Details of our computational procedure are given in Appendix B.

Remark 10. Unlike the conditional bounds of Lemma 3, the solution to (9) enforces constraints across callback strata. As a result, we can obtain informative bounds on the fraction of discriminatory jobs even among those that call no applications back.

Remark 11. Analogous linear programming formulations can be used to bound any linear functional of $G(\cdot,\cdot)$, including other measures of discrimination. For example, we can bound from below the fraction of employers discriminating against whites by replacing the objective in (9) with $\min_{G(\cdot,\cdot)\in\mathscr{G}} \int_{p_w<p_b} dG\left(p_w, p_b\right)$. We leverage this insight to bound a variety of features of $G(\cdot,\cdot)$ in the empirical work to follow.

Maximum risk

Relying on a discretized function space $\mathscr{G}$ also enables us to compute the maximum risk function $\mathcal{R}_J^m$ consistent with a set of experimental callback probabilities.[2] For a given decision rule $\delta(\cdot)$, $\mathcal{R}_J^m(\delta)$ can be expressed as the solution to the following optimization problem:

$$\begin{aligned}\mathcal{R}_J^m(\delta) &= \max_{G\in\mathscr{G}} J \sum_{c_w=1}^{L_w}\sum_{c_b=1}^{L_b}\int \left\{\delta\left(c_w,c_b\right)1\left\{p_w=p_b\right\}\kappa+\left[1-\delta\left(c_w,c_b\right)\right]1\left\{p_w\neq p_b\right\}\gamma\right\} \\ &\times \; f\left(c_w,c_b|p_w,p_b\right)dG\left(p_w,p_b\right) \quad s.t.\; (10)\,.\end{aligned}$$

When $\mathscr{G}$ is a family of discrete distributions, the objective and constraints are both linear in the probability masses associated with $G(\cdot,\cdot)$ and $\mathcal{R}_J^m(\delta)$ can be computed numerically as the solution to a linear programming problem. The minimax decision rule $\delta^{mm}(\cdot)$ can be found by computing $\mathcal{R}_J^m(\delta)$ for each candidate rule $\delta\in\mathscr{D}$ and choosing the rule that yields lowest maximal risk.

Remark 12. With L_w white and L_b black applications there are $2^{(1+L_w)(1+L_b)}$ distinct rules to consider which, in practice, prohibits brute force enumeration when $L_w+L_b>4$. To circumvent this obstacle, we consider in our empirical application a restricted set $\mathscr{D}^\dagger\subset\mathscr{D}$ of decision rules that rely upon a nominal ordering of $\mathscr{D}$ and a threshold rule.

5 Data

We apply our methods to data from three correspondence experiments summarized in Table I. Bertrand and Mullainathan (BM, 2004) applied to 1,112 job openings in Boston and Chicago, sub-

[2] See Müller and Wang (2019) for a closely related approach to minimizing weighted average risk involving discretization of unbiasedness constraints.

mitting four applications to each job. Of the four applications, two were assigned black-sounding names while the remaining two were assigned white-sounding names. The callback rate to applications with black sounding names was 3.1 percentage points lower than to applications with white sounding names.

Nunley et al. (2015) studied racial discrimination in the market for new college graduates by applying to 2,305 listings on an online job board, again sending four resumes per job opening. Unlike Bertrand and Mullainathan, the names assigned to the four resumes were sampled without replacement from a pool of eight names, four of which were distinctively black and four of which were distinctively white. This led the fraction of names sent to each job that were distinctively black to vary randomly in increments of 25% from 0% to 100%. Interestingly, the average callback rate in the Nunley study was more than twice as high as in the Bertrand and Mullainathan study, perhaps because the fictitious applicants were more highly educated. On average, black sounding names had a 2.6 percentage point lower callback rate than white names.

Arceo-Gomez and Campos-Vasquez (AGCV, 2014) applied to 802 job openings through an online job portal in a study of race and gender discrimination in Mexico City, Mexico. They sent eight fictitious applications to each job, and the applicants were all recent college graduates. For simplicity, we focus on gender in this experiment, as AGCV used a three-category definition of race that is more complicated to analyze and may be less generalizable to other settings. In their data, women are 3.4 percentage points more likely to receive callbacks than men. While the Arceo-Gomez and Campos-Vasquez (2014) experiment looks at a very different context than Bertrand and Mullainathan (2004) and Nunley et al. (2015), this data set allows us to demonstrate the gains from doubling the number of applications per job opening.

6 Are Callbacks Rival?

We begin by considering tests of the binomial trials assumption (Assumption 1) that undergirds our econometric framework. Fundamentally, we are concerned that the probability of application ℓ receiving a callback from job j might depend not only on its own characteristics but the characteristics of the other applications sent to it. To assess this possibility, we fit linear probability models of the form:

$$Y_{j\ell} = \lambda_0 + X'_{j\ell}\lambda_1 + \bar{X}'_{j\ell}\lambda_2 + \varepsilon_{j\ell}, \tag{11}$$

where $X_{j\ell}$ is a vector of application characteristics and $\bar{X}_{j\ell} = (L-1)^{-1}\sum_{k\neq\ell} X_{jk}$ gives the "leave out" mean of those characteristics among the applications sent to job j excluding application ℓ. While the coefficient vector λ_1 gives the direct effect of application characteristics on callbacks, the coefficient vector λ_2 captures the "peer effect" of other applications to the same job on application ℓ's callback propensity. Assumption 1 restricts these peer effects to be zero ($\lambda_2 = 0$).

For OLS estimates of (11) to identify a causal effect of $\bar{X}_{j\ell}$, we need for $\bar{X}_{j\ell}$ to be uncorrelated with any omitted application characteristics $Z_{j\ell}$ that influence callbacks. In Bertrand and Mullainathan (2004)'s study, this condition is violated because of two features of the design. First, application characteristics were assigned according to their joint distribution in a training sample, making it likely that $X_{j\ell}$ and $Z_{j\ell}$ are correlated. Second, the application characteristics were chosen to yield a good match with the job (see pp. 996), leading $Z_{j\ell}$ to be correlated with its leave out mean $\bar{Z}_{j\ell}$ and hence with $\bar{X}_{j\ell}$. For this reason, we focus on the Nunley et al. (2015) study which assigned both race and application characteristics independently of each other and across applications. Note that even when this independence holds, $X_{j\ell}$ and $\bar{X}_{j\ell}$ will tend to be negatively correlated when L is small (Angrist, 2014). Omitting $X_{j\ell}$ from (11) would therefore tend to lead to a spurious finding of negative peer effects. So long as $X_{j\ell}$ is included, however, a finding that $\lambda_2 \neq 0$ provides evidence of a peer effect.

Table II reports estimates of the parameters in (11) for the Nunley et al. (2015) study, with each row showing the coefficients from a separate regression. While applications with distinctively black names are significantly less likely to be called back, we find no significant effect on callback probabilities of changing the racial mix of the other 3 applications to the same job. In fact, the point estimate indicates that increasing the share of applicants with distinctively black names insignificantly lowers callback rates, which is the opposite of what one would expect if callbacks were rival. Across the 12 covariates we consider only one (an indicator for 3+ months of unemployment) finds a significant peer effect at conventional levels. However, a joint test fails to reject that all of the leave out mean coefficients are jointly zero.

As another composite test, we report the results of a model in which the peer effects are restricted to be proportional to the main effects of the application's own characteristics $X_{j\ell}$. The row titled "predicted callback rate" pools all the application characteristics into an index $X_{j\ell}\hat{\lambda}_{1(j)}$ where $\hat{\lambda}_{1(j)}$ is the leave out OLS coefficient vector obtained from regressing the callback indicator on application covariates after leaving out all applications to job j. In the Nunley et al. (2015) study, a unit increase in $X_{j\ell}\hat{\lambda}_{1(j)}$ is associated with roughly half of a callback on average. Note that if we had used the leave-in OLS prediction $X_{j\ell}\hat{\lambda}_1$ as the regressor, this coefficient would mechanically equal one. Though $X_{j\ell}\hat{\lambda}_{1(j)}$ strongly predicts callbacks, its average value among competing applications $(L-1)^{-1}\sum_{k\neq\ell} X_{jk}\hat{\lambda}_{1(j)}$ has no statistically discernible impact on callbacks.

Columns 3 and 4 of Table II report corresponding estimates for the AGCV data. Unfortunately, the covariates in this dataset have insignificant direct effects, so tests of indirect effects are fundamentally under-powered. Empirically then Assumption 1 seems to provide a good approximation to callback behavior in the Nunley et al. (2015) experiment. A possible explanation for this result may be that these researchers applied to posted vacancies where employers were capable of hiring multiple applicants to the same job. Whatever the reason, we now proceed to estimating the moments of the callback distribution based on Assumption 1.

7 Moment Estimates

Tables III-V report estimates of identified moments of the callback distribution in each of our three experiments. We report method of moments estimates that apply equation (6) to the sample callback frequencies as well as "shape constrained" GMM estimates that require the callback frequencies be rationalizable by a proper (discretized) probability distribution $G \in \mathscr{G}$. Imposing shape constraints serves two goals. First, we need the moment estimates to be rationalizable by some $G \in \mathscr{G}$ in order to subsequently use them as constraints when estimating bounds via our linear programming method. Second, when the constraints bind, the resulting estimates are typically closer to the truth and more precise (see Chetverikov et al., 2018 for a review). Details of the shape constrained estimation procedure appear in Appendix C. Table VI uses the shape constrained estimates to summarize key features of the distribution of callback probabilities in each experiment.

Bertrand and Mullainathan (2004)

Estimates of centered moments in the Bertrand and Mullainathan experiment are shown in Table III. Because this study employed a single design with $L_w = L_b = 2$, the moments reported are just identified. The first column of Table III reports method of moments estimates with standard errors computed via the delta method. The first row of the Table shows the mean callback probabilities of white and black applications across jobs which, because of the balanced application design, match the callback rates reported in Table I. More interesting are the second moments: there is substantial over-dispersion in callback probabilities, with standard deviations across jobs for each race-specific probability more than double the mean probability. As expected, there is also a strong positive covariance between white and black callback rates, reflecting that some employers simply call back more applications of all types.

As shown in column 2, the shape constraints do not bind in the Bertrand and Mullainathan data, which means the sample frequencies can be rationalized to numerical precision by a discretized probability distribution. Consequently, the resulting moment estimates are identical to the method of moments estimates of column 1. Though the constraints do not bind, it is hypothetically possible for the variability of the constrained estimator to be lower if some of the constraints are near-binding. However, our standard error estimates, which rely on the "numerical bootstrap" procedure of Hong and Li (2017) (described in Appendix C), suggest that the constrained GMM estimator is roughly as variable as the unconstrained method of moments estimator.

Columns 1-3 of Table VI report transformations of the moments in Table III that are somewhat easier to interpret. Most notably, we find substantial heterogeneity in the difference in race specific callback rates $p_{jb} - p_{jw}$ across jobs. The standard deviation of the job-specific causal effect is more than twice as large as the mean effect. The third row of Table VI shows the correlation between white and black callback probabilities is very large, at 0.927. However, the correlation between the discriminatory gap in callback rates $p_{jb} - p_{jw}$ and the white callback probability p_{jw} is strongly negative, suggesting that discrimination tends to be stronger when firms have higher chances of calling back more white workers. This reflects, in part, a mechanical boundary effect, as an employer with very low callback rates has little opportunity to discriminate. Since the white callback rate in this study is only around 10%, boundary effects are likely to be a quantitatively important phenomenon.

Nunley et al. (2015)

Moment estimates from the Nunley et al. (2015) study are reported in Table IV. Recall that Nunley et al. (2015) employed five distinct application designs with $(L_{jw}, L_{jb}) \in \{(4,0), (3,1), (2,2), (1,3), (0,4)\}$. Columns 1-3 of Table IV report design-specific method of moments estimates of all identified moments for the three designs with the largest sample sizes.[3] As expected, the design-specific estimates are generally close to one another. The sole moment that appears to differ across designs is the mean white callback rate, which is somewhat lower in the $(1,3)$ design than the $(3,1)$ design. However, column 5 of Table IV shows that the differences between the designs are not jointly statistically significant, which is in line with our findings from the previous section.

To pool the designs efficiently, we again use a shape constrained GMM estimator that requires the moments be rationalizable by a proper probability distribution $G \in \mathscr{G}$. The pooled estimates, reported in column 5 of Table IV, lie closest to those from the $(2,2)$ design, which has the largest sample size. Moments identified solely by the $(1,3)$ and $(3,1)$ designs change more substantially when pooling across designs, as the binomial structure of the probabilities imposes restrictions across moments. As usual, the minimized value of our GMM criterion function provides a measure of the goodness of fit of our model. Applying the bootstrap method of Chernozhukov et al. (2015) yields a p-value of 0.19 for the null hypothesis that the results for all experimental designs are jointly rationalized by the model. As expected, pooling the designs substantially improves the precision of the estimated coefficients. Notably, the standard error estimates fall substantially even for many just-identified moments due to the cross-moment restrictions implied by the model.

Consistent with our findings for the Bertrand and Mullainathan data, columns 4-6 of Table VI reveal substantial heterogeneity in race-specific callback rates in the Nunley et al. (2015) experiment, with standard deviations roughly twice their mean. The imbalanced design used by Nunley et al. (2015) allows us to identify higher moments than the earlier Bertrand and Mullainathan study despite the total number of applications sent being the same. While race-specific callback rates are right skewed, racial gaps in callback probabilities $p_{jb} - p_{jw}$ are left-skewed, suggesting a long tail of heavy discriminators.

Arceo-Gomez and Campos-Vasquez (2014)

Column 1 of Table V reports just-identified method of moments estimates for the AGCV data. Column 2 imposes shape constraints on the moments, which bind strongly in this case, presumably because the design of the AGCV experiment involves many small cells. Despite substantial movement in the moment estimates, the bootstrap p-value on the null hypothesis that the callback frequencies are generated by the model is 0.79, indicating that the raw callback frequencies are rationalizable by a well-behaved underlying joint distribution of callback probabilities. Moreover,

[3] The remaining designs were omitted from this analysis due to small sample sizes. Only 22 jobs were in the $(L_{jw} = 0, L_{jb} = 4)$ design while 43 jobs fell in the $(L_{jw} = 4, L_{jb} = 0)$ design.

imposing the shape constraints reduces the estimated standard errors of some of these moments. It is important to note, however, that the asymptotic distribution of the shape constrained estimator will tend to be non-normal (Fang and Santos, 2018) and so standard errors provide only a heuristic guide to the uncertainty associated with each moment estimate.

Columns 7-9 of Table VI report key moment estimates from the AGCV data. The behavior of the first two moments is similar to that reported in the prior two experiments, with gender-specific standard deviations roughly twice their mean callback probabilities. However, the greater number of applications used in this design helps enormously with the precision of higher moment estimates.[4] We find strong evidence of left-skew in the distribution of gender gaps in callback probabilities as well as evidence of excess kurtosis in the distribution of gaps. While many jobs discriminate little, there is a thick tail of heavy discriminators.

8 Posterior Bounds

Our analysis of moments revealed substantial heterogeneity in callback probabilities and discrimination across employers. Next, we compute lower bound estimates of the probability that a given employer is discriminating. In computing both the analytic bounds of Lemma 3 and the sharp bounds of (9), we replace the unknown callback probabilities $\bar{f}$ with estimates $\hat{\bar{f}} = B\hat{\mu}$, where $\hat{\mu}$ is the relevant vector of shape constrained moment estimates reported in Tables III-V. To ensure our bounds are not artificially tight, our linear programming algorithm employs a grid with 36 times as many points as the grid used in our earlier GMM step.

Bertrand and Mullainathan (2004)

Table VII reports upper bounds on the fraction of jobs that are not engaged in discrimination by the number of applications called back in the Bertrand and Mullainathan experiment. Column 1 of Table VII reports estimates of the analytic bounds in Lemma 3: at most 62% of the jobs that call back 2 applications are innocent of discrimination, while at most 56% of jobs that call back 3 applications are not discriminating. Column 2 of Table VII reports estimates of the sharp linear programming bounds. The sharp upper bounds are somewhat lower than their analytical counterparts, revealing that at most 56% of the jobs calling back two applicants are not discriminating. Among jobs that call back three applications, at most half are not discriminating on the basis of race. In this callback stratum, our estimates suggest jobs should not logically be presumed innocent.

The linear programming approach also generates informative bounds in callback strata for which analytical bounds are not available. Overall, at most 87% of jobs do not discriminate on the basis of race. Notably, at most 96% of jobs that call back no applications are not engaged in discrimination,

[4] Though the standard errors reported in Table VI suggest imprecision in our estimates of the higher moments of the female callback rate distribution, this appears to be a consequence of the asymptotic non-normality of the shape-constrained estimator. For example, the numerical bootstrap gives a 90-percent confidence interval of [5.37, 7.49] for the excess kurtosis of p_{jf} while the corresponding standard error equals 8.79.

while at most 79% of jobs that call back all four applications do not discriminate on the basis of race. Since neither of these strata exhibit any difference in black-white callback rates, all of the relevant information on discrimination in these strata comes from the total number of callbacks blended with the indirect evidence from the callback distribution $G(\cdot, \cdot)$.

Column 3 of Table VII reports linear programming-based upper bounds on the proportion of jobs with white callback probabilities greater than or equal to their black callback probability, $\Pr(p_{wj} \geq p_{bj})$. We find an upper bound of exactly one in each callback stratum, indicating that the callback probabilities can be rationalized without any employers engaging in "reverse discrimination" against whites. Column 4 of Table VII reports upper bounds on the proportion of jobs with white callback probabilities less than or equal to their black callback probabilities, $\Pr(p_{wj} \leq p_{bj})$. These upper bound estimates coincide exactly with those reported in column 2. Accordingly, we easily reject the null hypothesis of no discrimination against blacks.

Figure I converts the upper bound estimates in column 2 of Table VII to lower bound posterior probabilities of discrimination. Overall, at least 13% of jobs engage in discrimination. However, at least 72% of jobs that call back two white and no black applications are discriminating, while a job that calls back one white and no black applications has at least a 58% chance of discriminating. Highlighting the role of indirect evidence, we estimate that at least 4% of jobs that call back no applicants and at least 21% of jobs that call back all applicants discriminate on the basis of race.

Nunley et al. (2015)

Table VIII reports upper bound estimates of the probability of innocence from the Nunley et al. (2015) study for each application design involving both races. In column 1, our analytic bound formula suggests at most 72% of the jobs calling back two applicants in a balanced design with $L_{jw} = L_{jb} = 2$ are not discriminating – slightly higher than the corresponding estimate in Bertrand and Mullainathan. This upper bound is higher in the two imbalanced designs $(L_{jw} = 3, L_{jb} = 1)$ and $(L_{jw} = 1, L_{jb} = 3)$.

Applying the linear programming approach tightens these bounds dramatically and provides additional bounds on the prevalence of discrimination among jobs that make no callbacks or that call every application. We estimate that at most 64% of all jobs have equal white and black callback probabilities, with that share falling to under 31% among employers who call back two applicants in a balanced $(2, 2)$ design. However, some of this discrimination is estimated to be against whites. Column 3 shows that our shape constrained callback probabilities $\hat{\bar{f}}$ imply that at most 85% of employers have white callback probabilities greater than or equal to black probabilities. However, these moments are estimated with error, and a bootstrap test of the null hypothesis that all employers have white callback probabilities weakly exceeding their black callback probability yields a p-value of 0.12. If we attribute the evidence of reverse discrimination to sampling error, we can take the estimates in column 3 as the relevant upper bounds on non-discrimination, which are closer to the analytical bounds reported in column 1. Column 4 of Table VIII reports that at most 83% of jobs have white callback probabilities less than or equal to black probabilities.

Unsurprisingly, we decisively reject the null hypothesis that this upper bound is one, indicating that discrimination against blacks is substantial.

Figure II converts the upper bound priors reported in column 3 of Table VIII into posterior estimates of the share of employers with selected callback configurations engaged in discrimination against blacks. Overall, at least 15% of jobs discriminate against blacks (i.e., have $p_{jw} > p_{jb}$). However, we estimate that at least 85% of the employers calling back two white and no black applicants in a balanced $(2, 2)$ design are discriminating against blacks. Interestingly, calling back three whites and no blacks in a $(3, 1)$ design is estimated to be even more suspicious, with at least 90% of the employers generating this callback evidence engaged in discrimination against blacks.

Arceo-Gomez and Campos-Vasquez (2014)

Table IX reports upper bound estimates of the probability of innocence in the AGCV experiment. Focusing on the sharp bounds reported in column 2, we find that at most 72% of jobs are not engaged in discrimination against either gender. Remarkably, this share falls to 11% among jobs calling back a single applicant and rises to only 28% among jobs calling back two applicants. This bound is much lower than the analytic bound in column 1, showing that cross-stratum restrictions in a design with eight applications are very useful for tightening bounds in strata with few callbacks. Evidently, jobs that call back few applicants in the AGCV experiment are very likely to engage in discrimination.

Some of this discrimination appears to be "reverse" discrimination against women. Column 3 shows that at most 91% of jobs do not discriminate against women and a bootstrap test of the null hypothesis that this bound equals one is decisively rejected. An employer that calls back a single application has at most a 59% chance of not discriminating against women. Column 4 shows that at most 81% of jobs do not discriminate against men, and our bootstrap p-value indicates this bound is also statistically distinguishable from one. The mean difference in callback rates in the ACGV experiment therefore masks gender discrimination operating in both directions. An employer that calls back a single application has at most a 52% chance of not discriminating against men.

Figure III plots lower bound posterior probabilities of discrimination against men and women, respectively, for selected callback configurations. Unconditionally, at least 20% of jobs discriminate against men (i.e., have $p_{jm} < p_{jf}$), while at least 10% of jobs discriminate against women (i.e., have $p_{jf} < p_{jm}$). At least 97% of the jobs that call back four women and no men are estimated to discriminate against men. But even an employer that calls back a single woman and no men has at least a 90% chance of discriminating against men. Likewise, at least 85% of jobs that call back a single man and no women are estimated to be discriminating against women. Note that the under null of non-discrimination, the probability of a particular gender being contacted given a single callback in total is $\bar{f}_1^0(1, 0) = \bar{f}_1^0(0, 1) = 1/2$. That we obtain such strikingly informative posteriors in settings with a single callback demonstrates the tremendous value of indirect evidence in this setting.

9 Parametric Models

The previous section demonstrated that standard audit experiments allow robust non-parametric inferences to be drawn about the discriminatory status of particular jobs. In this section, we contrast the non-parametric bounding methods developed above with the results of considering a simple parametric family $\mathbb{G}_\theta$ of distributions for $G(\cdot,\cdot)$. We estimate the parameter vector θ by maximum likelihood. If the true $G(\cdot,\cdot)$ lies in $\mathbb{G}_\theta$ then this approach will yield consistent and efficient estimates of θ, while if the model is misspecified, maximum likelihood will still provide an approximation to whatever features of $G(\cdot,\cdot)$ are identified. Parametric modeling also facilitates incorporating other application characteristics into the callback probabilities. This can serve to generate more nuanced posteriors; for example, an employer that calls back both of two low quality white applications but neither of two high quality black applications is particularly suspicious.

We work with a mixed logit model of the form

$$\Pr\left(Y_{j\ell}=1|R_{j\ell},X_{j\ell},\alpha_j,\beta_j\right)=\Lambda\left(\alpha_j-\beta_j 1\left\{R_{j\ell}=b\right\}+X_{j\ell}'\psi\right),$$

where $\Lambda(\cdot)=\frac{\exp(\cdot)}{1+\exp(\cdot)}$ is the standard logistic CDF, $X_{j\ell}$ is a vector of de-meaned application covariates, and (α_j,β_j) are random coefficients governing the odds of a white callback and discrimination against blacks respectively. To allow for heterogeneity in white callback rates we assume that $\alpha_j \overset{iid}{\sim} N\left(\alpha_0,\sigma_\alpha^2\right)$. Discrimination is modeled as a two-type (conditional) mixture:

$$\beta_j|\alpha_j=\begin{cases}\beta_0 & \text{w/ prob. } \Lambda\left(\tau_0+\tau_\alpha\alpha_j\right),\\ 0 & \text{w/ prob. } 1-\Lambda\left(\tau_0+\tau_\alpha\alpha_j\right).\end{cases}$$

The above specification allows for some fraction of jobs to not discriminate at all, while the remaining jobs depress the odds of calling back blacks relative to whites by roughly β_0%. When $\tau_\alpha\neq 0$, the probability of a job discriminating depends on α_j, which governs the white callback rate. Note that random assignment of the covariates $X_{j\ell}$ implies they can safely be excluded from the type probability equation.

Model Estimates

Table X shows the results of fitting the above model to the Nunley et al. (2015) experiment. Column 1 provides a standard "random effects" logit model with heterogeneity confined to the intercept as in Farber et al. (2016). We find substantial variability across jobs in the overall odds of a callback: a 0.1 standard deviation increase in the intercept α_j is estimated to raise the odds of a callback by 47%. We also find clear evidence of market-wide discrimination: black applications have roughly 46% lower odds of being called back than their white counterparts.

Column 2 allows the race effect β_j to vary across employers, which yields a significant improvement in model fit. The types specification finds that only about 17% of jobs discriminate against blacks – very near the lower bound estimate of 15% produced by our linear programming routine (see column 3 of Table VIII). However, the degree of discrimination among such jobs is estimated to be very severe – the odds of receiving a call back are roughly $\exp(4) - 1 \approx 53$ times higher for white applications than blacks. Column 3 allows the probability of being the discriminatory type to depend on the white callback rate, which yields a negligible improvement in model fit. Surprisingly, α_j and β_j are found to be nearly independent, which implies that the negative correlation between $p_{jb} - p_{jw}$ and p_{jw} reported in Table VI is attributable to boundary effects. Again, this model finds roughly 17% of jobs discriminate against blacks.

Because we cannot reject the null hypothesis that $\tau_\alpha = 0$ (i.e. that discrimination is independent of white callback rates), we work with the more parsimonious model in column 2 in the exercises that follow. Figure IV provides a goodness of fit diagnostic for this model, plotting the empirical callback rates in each black / white callback by application design cell against the logit model's predicted callback probability in that cell. The empirical frequencies track the model predictions closely and a naive Pearson χ^2 test fails to reject the null hypothesis that the model rationalizes the cell frequencies up to sampling error.

Posteriors

Figure V reports the distribution of posterior probabilities $\Pr(D_j = 1 | \{Y_{j\ell}, R_{j\ell}, X'_{j\ell}\psi\}_{\ell=1}^{L})$ implied by the parameter estimates reported in column 2 of Table X. To summarize the influence of the covariates, we evaluate the posteriors at two points within each race group, corresponding to the estimated index $X'_{j\ell}\hat{\psi}$ being a standard deviation above or below its empirical mean, which we refer to as "high" and "low" quality applications respectively. By construction, the mean posterior coincides with the fraction of jobs that are estimated to be discriminating. The types model finds that only 17% of jobs are discriminating, yielding a strong prior of innocence. Calling back only white applicants still justifies a substantial degree of suspicion, however: 62% of the jobs that call back two whites and no blacks are discriminating. Imbalances in the covariate mix of applicants can substantially intensify this suspicion. Specifically, 79% of the jobs that call back two low quality white applications and neither of two high quality black applications are discriminating. Evidently, even in models with a strong presumption of innocence, four applications can provide enough information to cast substantial doubt on whether individual employers are in compliance with US employment law. However, it is only under the most extreme callback configurations that we can detect discriminators with reasonable certainty. In the next section, we study more carefully the tradeoff between type I and II errors presented by the two-type model, and how that tradeoff evolves with the number of applications sent.

10 Experimental Design and Detection Error Tradeoffs

We now study the ability of a hypothetical auditor to detect employer discrimination in a hypothetical population characterized by the two-type logit model of callback rates reported in column 2 of Table X. This parameterization found that discrimination was very rare, with only 17% of jobs engaging in discrimination. It is of considerable interest then to understand how the tradeoff between type I ("false accusation") and type II ("false acquittal") errors faced by an auditor changes with the number of applications sent to each job.

Recall from Lemma 1 that an auditor's optimal decision rule is to investigate when the posterior probability of discrimination crosses a cost-based threshold. We presume the auditor forms posteriors using her prior knowledge of $G(\cdot,\cdot)$ which coincides with the two-type estimates reported in Table X. This corresponds to a thought experiment in which the auditor fits the two-type model to the Nunley et al. (2015) experiment, forms consistent estimates of the logit parameters, and then sends applications to additional vacancies posted on the same online job board from which the original study sampled.

Figure VI displays a rescaling of the type I and II error rates that arise from implementing decision rules corresponding to varying posterior thresholds. The horizontal axis of Figure I gives the share of jobs engaged in discriminating that are investigated ("accused"). The vertical axis plots the share of non-discriminators that are not investigated (i.e., that are "acquitted"). Note that this quantity corresponds to $1-(FDR_1/\pi^0)$ (see Remark 2). Each point gives the values of these shares corresponding to a particular posterior decision threshold. The bold point corresponds to a posterior threshold of 80%.

In the canonical design with only 4 applications (2 white and 2 black), the 80% posterior threshold yields almost no false accusations. This control over type I errors comes at the cost of a very high type II error rate – very few accusations of any sort are made, leading to a negligible fraction of discriminators being detected. Note that conducting a classical hypothesis test (e.g., Fisher's exact test) at the 1% level is equivalent to controlling the fraction of correct acquittals, which is depicted by the horizontal line at 0.99. This rule would yield more accusations but most of those accusations would be erroneous – the equivalent posterior threshold in the 2 pair design is only about 33%.

Expanding the design to 5 pairs of applications (5 white and 5 black) yields a very substantial outward shift in the detection error tradeoff curve. Using a posterior threshold of 80% keeps the fraction of employers falsely accused of discrimination below 0.2% while allowing detection of roughly 7.5% of the jobs that are actually discriminating. Evidently, ten applications is enough to accurately detect a non-trivial fraction of discriminators.

The third line probes the potential for further gains by choosing the racial mix and covariates of the applications optimally. Specifically, we consider the set of 10-application portfolios generated by all possible combinations of race and two covariate-based application "quality" bins, and select the portfolio that minimizes risk for a given posterior threshold. We then vary that threshold to trace out the detection error tradeoff. Choosing applications optimally yields modest improvements in type I and II error rates. Using an 80% posterior threshold, the share of non-discriminators investigated remains below 0.2%, while the share of discriminators investigated rises to roughly 10%.

Interestingly, the risk minimizing portfolio for an auditor with an 80% posterior threshold consists of 5 high quality black applications and 5 low quality white applications. Intuitively, an employer that calls back low quality white applications more often than high quality black applications is very likely to be discriminating against blacks. Of course, the results of such an experiment would be difficult to interpret without prior knowledge of $G(\cdot,\cdot)$, as one would not be able to parse the separate effects of race and quality. Consequently, the gains from optimizing the portfolio of applications are in practice only achievable in a sequential experiment in which a first wave is used to estimate $G(\cdot,\cdot)$.

11 Ambiguity and Auditing Thresholds

The analysis of the previous section suggested that a favorable mix of type I and type II errors can be achieved when 10 applications are sent to each job and jobs with a posterior probability of discriminating greater than 80% are investigated. However, that analysis assumed that $G(\cdot,\cdot)$ was characterized by the parameters of our two-type logit model, which provides only one of many possible rationalizations of the moments identified by the Nunley et al. (2015) experiment. To assess how our hypothetical auditor's risk might change under different distributional assumptions, and how best to respond to this ambiguity, we now study the maximum risk function $\mathcal{R}_J^m(\delta)$. In this exercise, we assume each job j is characterized by a tuple $\left(p_{jw}^H, p_{jw}^L, p_{jb}^H, p_{jb}^L\right)$ of race by quality callback probabilities drawn from the joint distribution $G\left(p_w^H, p_w^L, p_b^H, p_b^L\right)$. For comparison with the logit model, which only allowed discrimination against blacks, we define discrimination as occuring when callback probabilities are *higher* for whites within either quality stratum; that is, we define $D_j = 1 - 1\{p_{jb}^H \geq p_{jw}^H\}1\{p_{jb}^L \geq p_{jw}^L\}$.[5]

To facilitate comparison with the logit model, we consider a restricted family $\mathcal{D}^\dagger$ of decision rules of the form $\delta(C_j, q) = 1\{\mathcal{P}(C_j, X'\psi, G_{logit}) \geq q\}$, where $q \in (0,1)$ is a cutoff and G_{logit} is the logit model reported in column 2 of Table X.[6] Computing the maximal risk for this family of decision rules can be thought of as a way of "second guessing" the risk associated with each logit posterior threshold without debating the logit model's ordering of the underlying evidence configurations. In computing $\mathcal{R}_J^m(\delta)$, we use the logit predictions of $\bar{f}(c_w, c_b)$ within each of the two quality bins of $X'\hat{\psi}$ as constraints (see Appendix D for details) and choose loss parameters $\kappa = 4$ and $\gamma = 1$ so that, under the logit DGP, an 80% posterior threshold would minimize risk.

Figure VII plots average risk $\mathcal{R}_J^m(\delta(\cdot,q))/J$ against the nominal logit posterior threshold q. For each decision rule, the maximal risk is much higher than the average logit risk, with the ratio between the two risks growing (discontinuously in some cases) with the posterior cutoff. As the posterior threshold approaches one – so that no jobs are accused – the maximal risk approaches one because the least favorable $G(\cdot,\cdot)$ entails every job being guilty. Conversely as the posterior threshold approaches zero – so that all jobs are accused – the maximum risk approaches four

[5] Defining discrimination more narrowly as any difference between white and black callback probabilities within either quality stratum yields nearly identical results.

[6] In cases where multiple evidence configurations yield posterior q, we consider separate rules that investigate each of these configurations individually.

because the least favorable $G(\cdot,\cdot)$ is one where nearly all jobs are innocent. Recall however from Table VIII that not all jobs can be innocent in the Nunley et al. (2015) experiment, which is why $\sup_{\delta\in\mathcal{D}^{\dagger}} \mathcal{R}^{m}_{J}(\delta)/J$ is a value less than four.

While the logit risk function $\mathcal{R}_J(G_{logit},\delta)$ is minimized by the decision rule with a threshold nearest 80%, $\mathcal{R}^{m}_{J}(\delta)$ is minimized by a rule with an implicit (logit-based) threshold of only 18%. This lower threshold implies a minimax auditor would investigate many more jobs than an auditor with the same preferences who knows $G(\cdot,\cdot)$ to be logit. Evidently, the minimax auditor is more concerned with the possibility that she is passing over a vast number of jobs engaged in modest amounts of discrimination than that a few non-discriminators are improperly investigated. To gain some intuition for this result, note that the minimax decision rule occurs at a threshold where the fraction of jobs that are engaged in discrimination more than triples. If the worst case DGP is one where most jobs are guilty, it makes sense to accuse more jobs. The lesson here is that although mispecification can lead to substantially higher risk, ambiguity regarding $G(\cdot,\cdot)$ will tend to lead to more rather than fewer audits.

12 Conclusion

Correspondence studies are powerful tools that have been extensively used to detect market level averages of discriminatory behavior. Revisiting three such studies, we find tremendous heterogeneity across employers in their degree of discriminatory behavior. This heterogeneity presents authorities charged with enforcing anti-discrimination laws with a difficult inferential task. Our analysis suggests that when ensemble evidence is used, 10 applications per employer is enough to accurately detect a non-trivial share of discriminatory employers. This finding opens the possibility that discrimination can be monitored – perhaps in real time – at the employer level.

Our results also provide a number of methodological lessons regarding the design and analysis of correspondence studies, and of experimental ensembles more generally. First, we demonstrate that indirect evidence can serve as a valuable supplement to direct evidence when making inferences regarding the behavioral responses of particular experimental units. Our logit results, in particular, suggest that accurately monitoring illegal discrimination in online labor markets is feasible with relatively small modifications to conventional audit designs once knowledge of the callback distribution $G(\cdot,\cdot)$ has been obtained. Whether such knowledge is better obtained through sequential experimentation (e.g., Chakraborty and Murphy, 2014; Dimakopoulou et al., 2017; Narita et al., 2018) or static empirical Bayes methods of the sort considered in this paper is an interesting question for future work.

Second, our analysis demonstrates that partial identification of the population distribution of response heterogeneity does not preclude "borrowing strength" from experimental ensembles. Using only a few moments of the callback distribution, we are able to derive informative lower bounds on the fraction of jobs engaging in illegal discrimination. These bounds are shown to allow precise inferences to be drawn about some jobs even in standard designs with only four applications per job. In the Arceo-Gomez and Campos-Vasquez (2014) study, which sent eight applications to each job, we are able to deduce informative lower bound rates of discrimination against men and women separately.

Third, our results highlight that the appropriate use of indirect evidence depends critically on the objectives of the investigator, formalized in our framework by the loss function of a hypothetical auditor. While in point identified settings it is straightforward to characterize the tradeoff between type I and II errors implied by different decision rules, partial identification of heterogeneity distributions tends to undermine identifiability of this tradeoff itself, an issue emphasized by Manski (2000). In our setting acknowledging the ambiguity stemming from partial identification turns out to lead to "bolder" inferences, but it is easy to envision settings where the opposite would be true. An interesting topic for future research is the extent to which the policy implications of recent econometric evaluations of teachers, schools, hospitals, and neighborhoods (e.g., Chetty et al., 2014b; Angrist et al., 2017; Hull, 2018; Chetty and Hendren, 2018; Chetty et al., 2018) vary with alternative notions of risk.

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Appendix A: Proof That $pFDR_J = \Pr\left(D_j = 0 | \delta(C_j) = 1\right)$

By iterated expectations

$$\begin{aligned}
\mathbb{E}\left[N_J^{-1}\sum_{j=1}^{J}\delta\left(C_j\right)\left(1-D_j\right)|N_J \geq 1\right] &= \sum_{n=1}^{J} n^{-1}\mathbb{E}\left[\sum_{j=1}^{J}\delta\left(C_j\right)\left(1-D_j\right)|N_J = n\right]\Pr\left(N_J = n|N_J \geq 1\right) \\
&= \sum_{n=1}^{J} n^{-1}J\mathbb{E}\left[\delta\left(C_j\right)\left(1-D_j\right)|N_J = n\right]\Pr\left(N_J = n|N_J \geq 1\right) \\
&= \sum_{n=1}^{J} n^{-1}J\Pr\left(D_j = 0|\delta(C_j) = 1, N_J = n\right)\Pr\left(N_J = n|N_J \geq 1\right) \\
&\quad \times \Pr\left(\delta(C_j) = 1|N_J = n\right) \\
&= \sum_{n=1}^{J}\Pr\left(D_j = 0|\delta(C_j) = 1, N_J = n\right)\Pr\left(N_J = n|N_J \geq 1\right) \\
&= \Pr\left(D_j = 0|\delta(C_j) = 1\right)\sum_{n=1}^{J}\Pr\left(N_J = n|N_J \geq 1\right) \\
&= \Pr\left(D_j = 0|\delta(C_j) = 1\right),
\end{aligned}$$

where the second and fifth lines use that the $\{C_j, D_j\}_{j=1}^{J}$ are *iid* and the fourth uses the fact that $\Pr\left(\delta\left(C_j\right) = 1|N_J = n\right) = n/J$. Hence, $pFDR_J$ gives the probability $\Pr\left(D_j = 0|\delta(C_j) = 1\right)$ that an investigated job is innocent.

Appendix B: Discretization of G and Linear Programming Bounds

To compute the solution to the problem in (9), we approximate the CDF $G\left(p_w, p_b\right)$ with the discrete distribution

$$G_K\left(p_w, p_b\right) = \sum_{k=1}^{K}\sum_{l=1}^{K}\pi_{kl}1\left\{p_w \leq \varrho\left(k,l\right), p_b \leq \varrho\left(l,k\right)\right\},$$

where the $\{\pi_{kl}\}_{k=1,l=1}^{K,K}$ are probability masses and $\{\varrho\left(k,l\right), \varrho\left(l,k\right)\}_{k=1,l=1}^{K,K}$ comprise a set of mass point coordinates generated by the function

$$\varrho\left(x,y\right) = \underbrace{\frac{\min\left\{x,y\right\}-1}{K}}_{\text{diagonal}} + \underbrace{\frac{\max\left\{0,x-y\right\}^2}{K\left(1+K-y\right)}}_{\text{off-diagonal}}.$$

This discretization scheme can be visualized as a two-dimensional grid containing K^2 elements. The diagonal entries on the grid represent jobs where no discrimination is present. The first term above ensures the mass points are equally spaced along the diagonal from $(0,0)$ to $\left(\frac{K-1}{K}, \frac{K-1}{K}\right)$. The second term spaces off diagonal points quadratically according to their distance from the diagonal in order to accomodate jobs with very low levels of discrimination while economizing on the number of grid points. Note that $\lim_{K\to\infty} \varrho(K,y) = 1$ ensuring the grid asymptotically spans the unit square.

With this notation, the constraints in (10) can be written:

$$\bar{f}(c_w, c_b) = \left(\begin{array}{c} L_w \\ c_w \end{array}\right)\left(\begin{array}{c} L_b \\ c_b \end{array}\right)\sum_{k=1}^{K}\sum_{l=1}^{K}\pi_{kl}\varrho(k,l)^{c_w}(1-\varrho(k,l))^{L_w-c_w}\varrho(l,k)^{c_b}(1-\varrho(l,k))^{L_b-c_b}, \tag{12}$$

for $c_w = (1, ..., L_w)$ and $c_b = (1, ..., L_b)$. Hence, our composite discretized optimization problem is to

$$\max_{\{\pi_{kl}\}} \frac{\left(\begin{array}{c} L \\ t \end{array}\right)}{\sum_{(c'_w, c'_b):c'_w+c'_b=t}\bar{f}(c'_w, c'_b)}\sum_{l=0}^{K}\sum_{k=0}^{K}\pi_{kl}\varrho(k,l)^t(1-\varrho(k,l))^{L-t},$$

subject to (12) and

$$\sum_{k=1}^{K}\sum_{l=1}^{K}\pi_{kl} = 1, \quad \pi_{kl} \geq 0,$$

for $k = 1, ..., K$ and $l = 1, ..., K$. We solve this problem numerically using the Gurobi software package. Because setting K too low will tend to yield artificially tight bounds, we set $K = 900$ in all bound computation steps, which yields $(900)^2 = 810,000$ distinct mass points.

Appendix C: Shape Constrained GMM

To accomodate the Nunley et al. (2015) study which employs multiple application designs, we introduce the variable $A_j = (A_{jw}, A_{jb})$ which gives the number of white and black applications sent to job j. Collecting the design-specific callback probabilities $\{\Pr(C_{jw} = c_w, C_{jb} = c_b | A_j = a)\}_{c_w, c_b}$ into the vector f_a, our model relates these probabilities to moments of the callback distribution via the linear system $f_a = B_a\mu$, for B_a a fixed matrix of binomial coefficients. Letting f denote the vector formed by "stacking" the $\{f_a\}$ across designs in an experiment, we write $f = B\mu$. Let π be a $K^2 \times 1$ vector comprised of the probability masses $\{\pi_{kl}\}_{k=1,l=1}^{K,K}$ (see Appendix B). For GMM estimation we set $K = 150$ (larger values yield very similar results). From (12), we can write $\mu = M\pi$ where M is a $dim(\mu) \times K^2$ matrix comprised of entries with typical element $\varrho(k,l)^m(1-\varrho(k,l))^{s-m}\varrho(l,k)^n(1-\varrho(l,k))^{t-n}$. Defining $R = BM$, we have the moment restriction $f = R\pi$.

Let $\tilde{f}$ denote the vector of empirical call back probabilities with typical element:

$$\frac{J^{-1}\sum_{j=1}^{J}1\{C_{jw}=c_w, C_{jb}=c_b, A_j=a\}}{J^{-1}\sum_{j=1}^{J}1\{A_j=a\}}.$$

Our shape constrained GMM estimator of π can be written as the solution to the following quadratic programming problem:

$$\hat{\pi} = \arg\inf_{\pi} (\tilde{f} - R\pi)'W(\tilde{f} - R\pi) \tag{13}$$

$$\text{s.t.} \quad \pi \geq 0, \ \mathbf{1}'\pi = 1,$$

where W is a fixed weighting matrix. Note that because $G(\cdot, \cdot)$ is not identified, there are many possible solutions $\hat{\pi}$ to this problem, but these solutions will all yield the same values of $R\hat{\pi}$. Our shape constrained estimate of the moments is $\hat{\mu} = M\hat{\pi}$ while our estimator of the callback probabilities is $\hat{f} = R\hat{\pi}$. We follow a two-step procedure, solving (13) with diagonal weights proportional to the number of jobs used in the application design and then choosing $W = \hat{\Sigma}^{-1}$ where $\hat{\Sigma} = \text{diag}\left(\hat{f}^{(1)}\right) - \hat{f}^{(1)}\hat{f}^{(1)\prime}$ is an estimate of the variance-covariance matrix of the callback frequencies implied by the first step shape-constrained callback probability estimates $\hat{f}^{(1)}$.

Hong and Li (2017) standard errors

Standard errors on the moment estimates $\hat{\mu}$ are computed via the numerical bootstrap procedure of Hong and Li (2017) using a step size of $J^{-1/4}$ (we found qualitatively similar results with a step size of $J^{-1/3}$). Our implementation of the numerical bootstrap proceeds as follows: the bootstrap analogue μ^* of $\hat{\mu}$ solves the quadratic programming problem in (13) where $\tilde{f}$ has been replaced by $\left(\tilde{f} + J^{-1/4} f^*\right)$. The bootstrap probabilities f^* have typical element:

$$\frac{J^{-1}\sum_{j=1}^{J}\omega_j^* 1\left\{C_{jw}=c_w, C_{jb}=c_b, A_j=a\right\}}{J^{-1}\sum_{j=1}^{J}\omega_j^* 1\{A_j=a\}},$$

where $\left\{\omega_j^*\right\}_{j=1}^{J}$ are a set of iid draws from an exponential distribution with mean and variance one. For any function $\phi(\hat{\mu})$ of the moment estimates $\hat{\mu}$ reported, we use as our standard error estimate the standard deviation across bootstrap replications of $J^{-1/4}\left[\phi(\mu^*) - \phi(\hat{\mu})\right]$.

Chernozhukov et al. (2015) goodness of fit test

To formally test whether there exists a π in the K^2 dimensional probability simplex such that $f = R\pi$ holds, we rely on the procedure of Chernozhukov et al. (2015). Our test statistic (the "J-test") can be written:

$$T_n = \inf_{\pi} \ (\tilde{f} - R\pi)'\hat{\Sigma}^{-1}(\tilde{f} - R\pi)$$

$$\text{s.t.} \quad \pi \geq 0, \ \mathbf{1}'\pi = 1.$$

Letting $\mathbb{F}^* = f^* - \tilde{f}$ denote the (centered) bootstrap analogue of the callback frequencies $\tilde{f}$ and W^* a corresponding bootstrap weighting matrix, our bootstrap test statistic takes the form:

$$T_n^* = \inf_{\pi,h} \ (\mathbb{F}^* - Rh)'W^*(\mathbb{F}^* - Rh) \tag{14}$$

$$\text{s.t.} \quad (\tilde{f} - R\pi)'W(\tilde{f} - R\pi) = T_n, \ \pi \geq 0, \ \mathbf{1}'\pi = 1, \ h \geq -\pi, \ \mathbf{1}'h = 0$$

As in the full sample problem, we conduct a two-step GMM procedure in each bootstrap replication, setting $W^* = \left[\text{diag}(R\pi^{(1)*}) - (R\pi^{(1)*})(R\pi^{(1)*})'\right]^{-1}$ where $\pi^{(1)*}$ is a first-step diagonally weighted estimate of the probabilities in the bootstrap sample. The goodness of fit p-value reported is the fraction of bootstrap samples for which $T_n^* > T_n$.

To simplify computation of (14), we re-formulate the problem in two ways. First, we define primary and auxilliary vectors of errors for each moment condition. Letting $\xi_h = \mathbb{F}^* - Rh$ and $\xi_\pi = \tilde{f} - R\pi$, the problem can be re-posed as:

$$T_n^* = \inf_{\xi_h, \xi_\pi} \ \xi_h' W^* \xi_h,$$

$$\text{s.t. } \xi_\pi' W \xi_\pi = T_n, \ \ Rh + \xi_h = \mathbb{F}^*, \ \ R\pi + \xi_\pi = \tilde{f}, \ \ \mathbf{1}'h = 0, \ \ \mathbf{1}'\pi = 1, \ \ h \geq -\pi, \ \ \pi \geq 0.$$

Now letting $h^+ = h + \pi$, we can further rewrite the problem as:

$$T_n^* = \inf_{\xi_h, \xi_\pi} \ \xi_h' W^* \xi_h,$$

$$\text{s.t. } \xi_\pi' W \xi_\pi = T_n, \ \ Rh^+ + \xi_h + \xi_\pi = \mathbb{F}^*, \ \ R\pi + \xi_\pi = \tilde{f}, \ \ \mathbf{1}'h^+ = 1, \ \ \mathbf{1}'\pi = 1, \ \ h^+ \geq 0, \ \ \pi \geq 0.$$

Note that this final representation replaces a $K^2 \times K^2 + 1$ (inequality) constraint matrix encoding $\xi_h \geq -\xi_\pi$ and $\xi_\pi \geq 0$ with a $2K^2 \times 1$ vector encoding $h^+ \geq 0$ and $\pi \geq 0$. Because this problem still involves a quadratic constraint in ξ_π, we make use of Gurobi's Second Order Cone Programming (SOCP) solver to obtain a solution.

Appendix D: Computing Maximum Risk

We approximate $G\left(p_w^H, p_w^L, p_b^H, p_b^L\right)$ with the discretized distribution

$$G_K\left(p_w^H, p_w^L, p_b^H, p_b^L\right) = \sum_{k=1}^K \sum_{l=1}^K \sum_{k'=1}^K \sum_{l'=1}^K \pi_{klk'l'} 1\left\{p_w^H \leq \varrho(k,l), p_w^L \leq \varrho\left(k',l'\right), p_b^H \leq \varrho(l,k), p_b^L \leq \varrho\left(l',k'\right)\right\},$$

which has K^4 mass points. In practice, we choose $K = 30$, which yields the same number of points as the approximation described in Appendix B.

Generalizing the notation of Appendix C, let the vector $A_j = \left(A_{jw}^H, A_{jw}^L, A_{jb}^H, A_{jb}^L\right)$ record the number of high quality and low quality applications of each race sent to job j and let $C_j = \left(C_{jw}^H, C_{jw}^L, C_{jb}^H, C_{jb}^L\right)$ record the corresponding numbers of callbacks. The posterior probability of discrimination is $\Pr\left(D_j = 1 | A_j, C_j\right) = \mathcal{P}\left(C_j, A_j, G\right)$. The space of auditing rules we consider is of the form $\delta\left(C_j, A_j, q\right) = 1\left\{\mathcal{P}\left(C_j, A_j, G_{\text{logit}}\right) > q\right\}$.With this notation, we can write the risk function

$$\begin{aligned}\mathcal{R}_J(q) &= \sum_{j=1}^J \Pr\left(\delta\left(C_j, A_j, q\right) = 1, D_j = 0\right)\kappa + \Pr\left(\delta\left(C_j, A_j, q\right) = 0, D_j = 1\right)\gamma \\ &= J \times \sum_{a \in \mathscr{A}_1} w_a \left\{\Pr\left(\delta\left(C_j, a, q\right) = 1, D_j = 0\right)\kappa + \Pr\left(\delta\left(C_j, a, q\right) = 0, D_j = 1\right)\gamma\right\}.\end{aligned}$$

where $\mathscr{A}_1$ is the set of all $2^5 = 36$ binary quality permutations possible in a design with 5 white and 5 black applications and $w_a = \begin{pmatrix} 5 \\ a_w^H \end{pmatrix} \begin{pmatrix} 5 \\ a_b^H \end{pmatrix} \left(\frac{1}{2}\right)^{10}$ is the set of weights that arise when quality is assigned at random within race.

To further evaluate the above risk expression note that when $D_j = 1 - 1\left\{p_{jb}^H \geq p_{jw}^H\right\} 1\left\{p_{jb}^L \geq p_{jw}^L\right\}$, we can write:

$$
\begin{aligned}
\Pr\left(\delta\left(C_j, a, q\right) = 1, D_j = 0\right) &= \sum_{c_w^H=0}^{a_{jw}^H} \sum_{c_b^H=0}^{a_{jb}^H} \sum_{c_w^L=0}^{a_{jw}^L} \sum_{c_b^L=0}^{a_{jb}^L} \delta\left(c, a, q\right) \\
&\times \sum_{k=1}^{K} \sum_{l=1}^{K} \sum_{k' \geq k}^{K} \sum_{l' \geq l}^{K} \pi_{klk'l'} \times \begin{pmatrix} a_w^H \\ c_w^H \end{pmatrix} \begin{pmatrix} a_b^H \\ c_b^H \end{pmatrix} \begin{pmatrix} a_w^L \\ c_w^L \end{pmatrix} \begin{pmatrix} a_b^L \\ c_b^L \end{pmatrix} \\
&\times \varrho\left(k, l\right)^{c_w^H} \left(1 - \varrho\left(k, l\right)\right)^{a_w^H - c_w^H} \varrho\left(l, k\right)^{c_b^H} \left(1 - \varrho\left(l, k\right)\right)^{a_b^H - c_b^H} \\
&\times \varrho\left(k', l'\right)^{c_w^L} \left(1 - \varrho\left(k', l'\right)\right)^{a_w^L - c_w^L} \varrho\left(l', k'\right)^{c_b^L} \left(1 - \varrho\left(l', k'\right)\right)^{a_b^L - c_b^L}.
\end{aligned}
$$

$$
\begin{aligned}
\Pr\left(\delta\left(C_j, a, q\right) = 0, D_j = 1\right) &= \sum_{c_w^H=0}^{a_{jw}^H} \sum_{c_b^H=0}^{a_{jb}^H} \sum_{c_w^L=0}^{a_{jw}^L} \sum_{c_b^L=0}^{a_{jb}^L} \left(1 - \delta\left(c, a, q\right)\right) \\
&\times \sum_{k=1}^{K} \sum_{l=1}^{K} \sum_{k' < k}^{K} \sum_{l' < l}^{K} \pi_{klk'l'} \times \begin{pmatrix} a_w^H \\ c_w^H \end{pmatrix} \begin{pmatrix} a_b^H \\ c_b^H \end{pmatrix} \begin{pmatrix} a_w^L \\ c_w^L \end{pmatrix} \begin{pmatrix} a_b^L \\ c_b^L \end{pmatrix} \\
&\times \varrho\left(k, l\right)^{c_w^H} \left(1 - \varrho\left(k, l\right)\right)^{a_w^H - c_w^H} \varrho\left(l, k\right)^{c_b^H} \left(1 - \varrho\left(l, k\right)\right)^{a_b^H - c_b^H} \\
&\times \varrho\left(k', l'\right)^{c_w^L} \left(1 - \varrho\left(k', l'\right)\right)^{a_w^L - c_w^L} \varrho\left(l', k'\right)^{c_b^L} \left(1 - \varrho\left(l', k'\right)\right)^{a_b^L - c_b^L}.
\end{aligned}
$$

Using these expressions, maximal risk can therefore be written as the solution to the following linear programming problem taking the form:

$$
\mathcal{R}_J^m(q) = J \times \max_{\{\pi_{klk'l'}\}} \sum_{a \in \mathscr{A}_1} w_a \left\{\Pr\left(\delta\left(C_j, a, q\right) = 1, D_j = 0\right) \kappa + \Pr\left(\delta\left(C_j, a, q\right) = 0, D_j = 1\right) \gamma\right\}
$$

subject to the constraint that the $\pi_{klk'l'}$ are non-negative and sum to one and that the following moment restrictions hold:

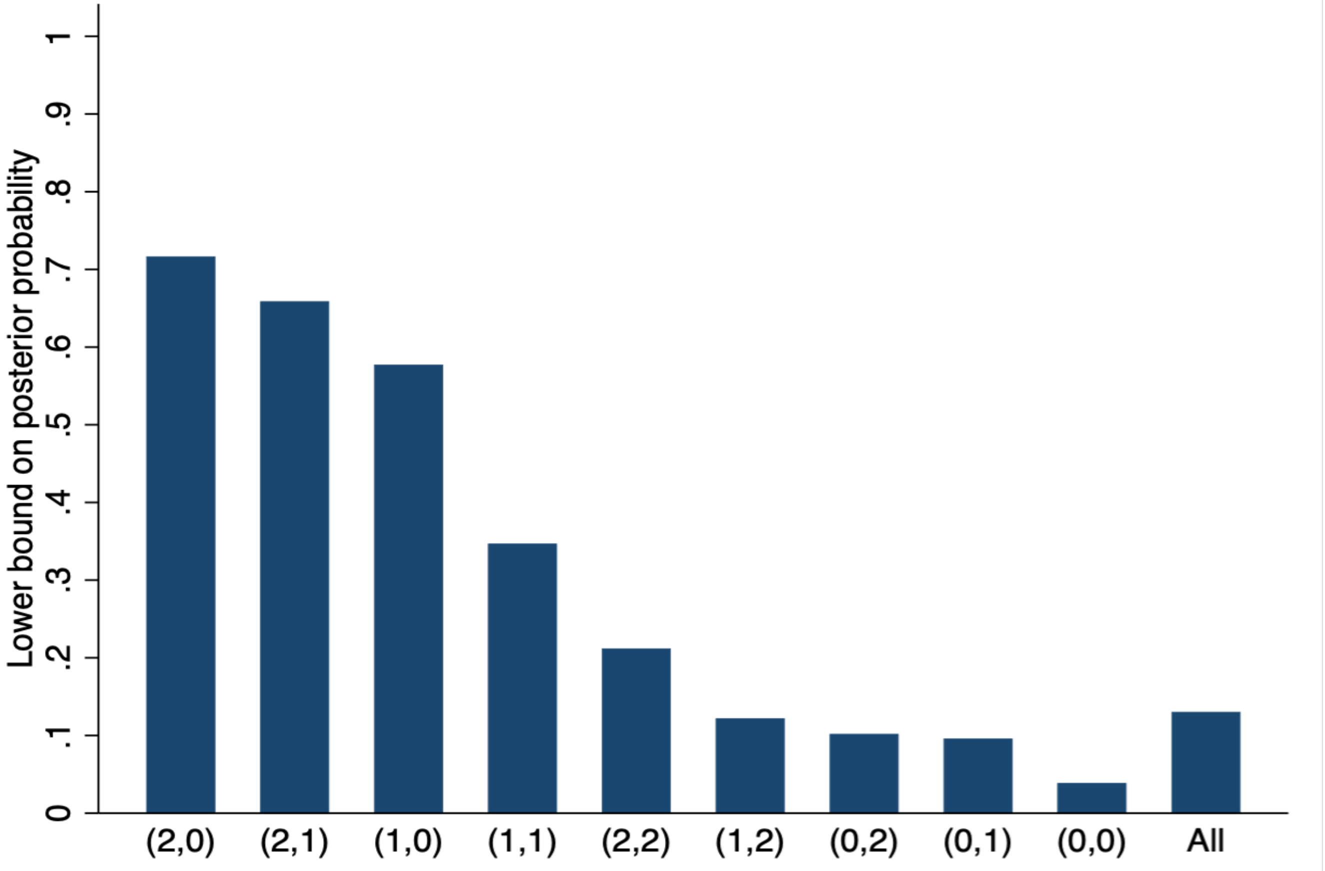
$$
\begin{aligned}
\Pr\left(C_j = c | A_j = a\right) &= \begin{pmatrix} a_w^H \\ c_w^H \end{pmatrix} \begin{pmatrix} a_b^H \\ c_b^H \end{pmatrix} \begin{pmatrix} a_w^L \\ c_w^L \end{pmatrix} \begin{pmatrix} a_b^L \\ c_b^L \end{pmatrix} \sum_{k=1}^{K} \sum_{l=1}^{K} \sum_{k'=1}^{K} \sum_{l'=1}^{K} \pi_{klk'l'} \\
&\times \varrho\left(k, l\right)^{c_w^H} \left(1 - \varrho\left(k, l\right)\right)^{a_w^H - c_w^H} \varrho\left(l, k\right)^{c_b^H} \left(1 - \varrho\left(l, k\right)\right)^{a_b^H - c_b^H} \\
&\times \varrho\left(k', l'\right)^{c_w^L} \left(1 - \varrho\left(k', l'\right)\right)^{a_w^L - c_w^L} \varrho\left(l', k'\right)^{c_b^L} \left(1 - \varrho\left(l', k'\right)\right)^{a_b^L - c_b^L}.
\end{aligned}
$$

Specifically, we impose these restrictions for the following set of designs, all of which are present in the Nunley et al. (2015) experiment:

$$\mathscr{A}_2 = \{(2,0,2,0), (2,0,0,2), (0,2,2,0), (0,2,0,2)\}.$$

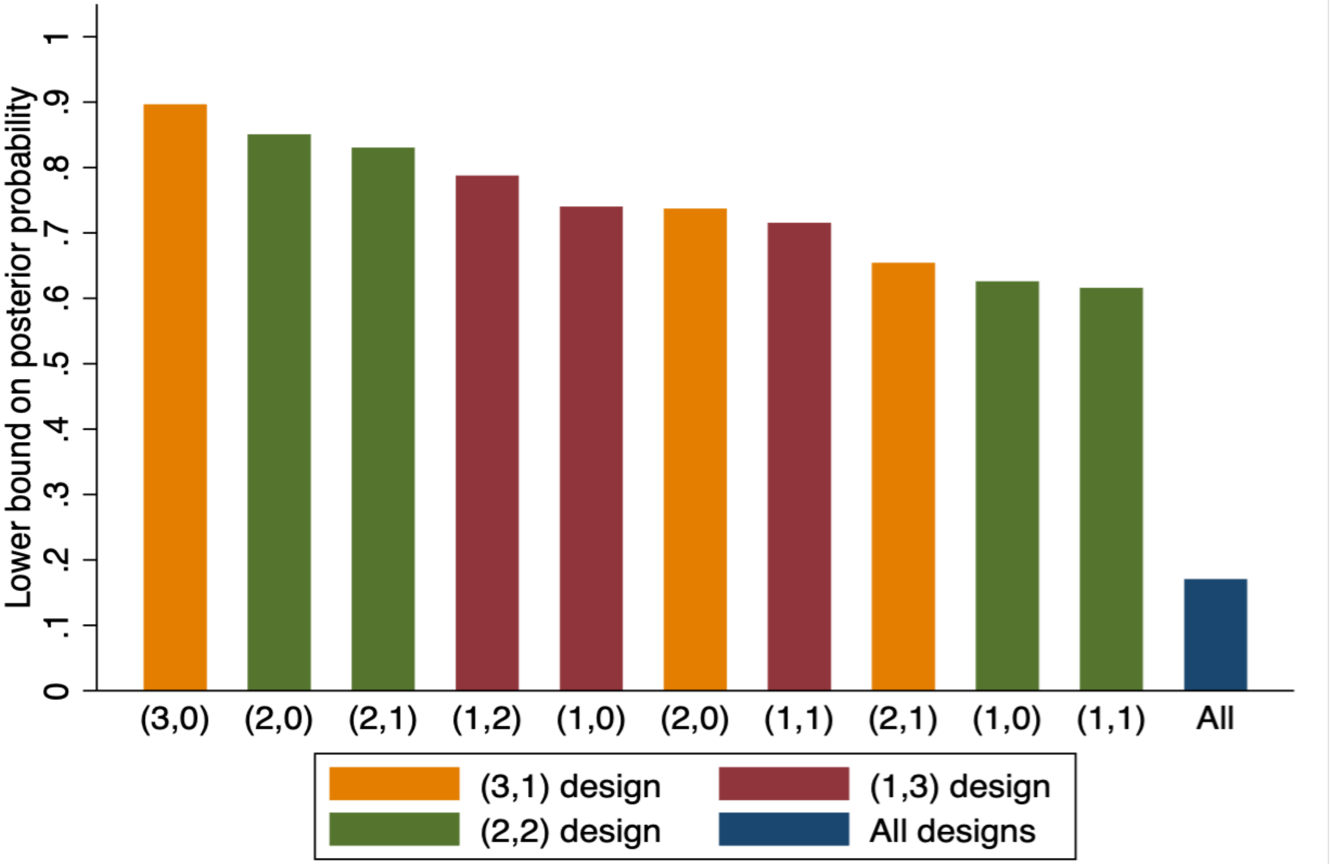
To operationalize these constraints, we replace the unknown cell probabilities $\Pr(C_j = c | A_j = a)$ for all c and a in $\mathscr{A}_2$ with their predictions under the logit model reported in column 2 of Table X. Using the logit predictions serves as a form of smoothing that allows us to avoid problems that arise with small cells when considering quality variation due to covariates.

Figure I: Lower bounds on posterior probabilities of discrimination, BM data



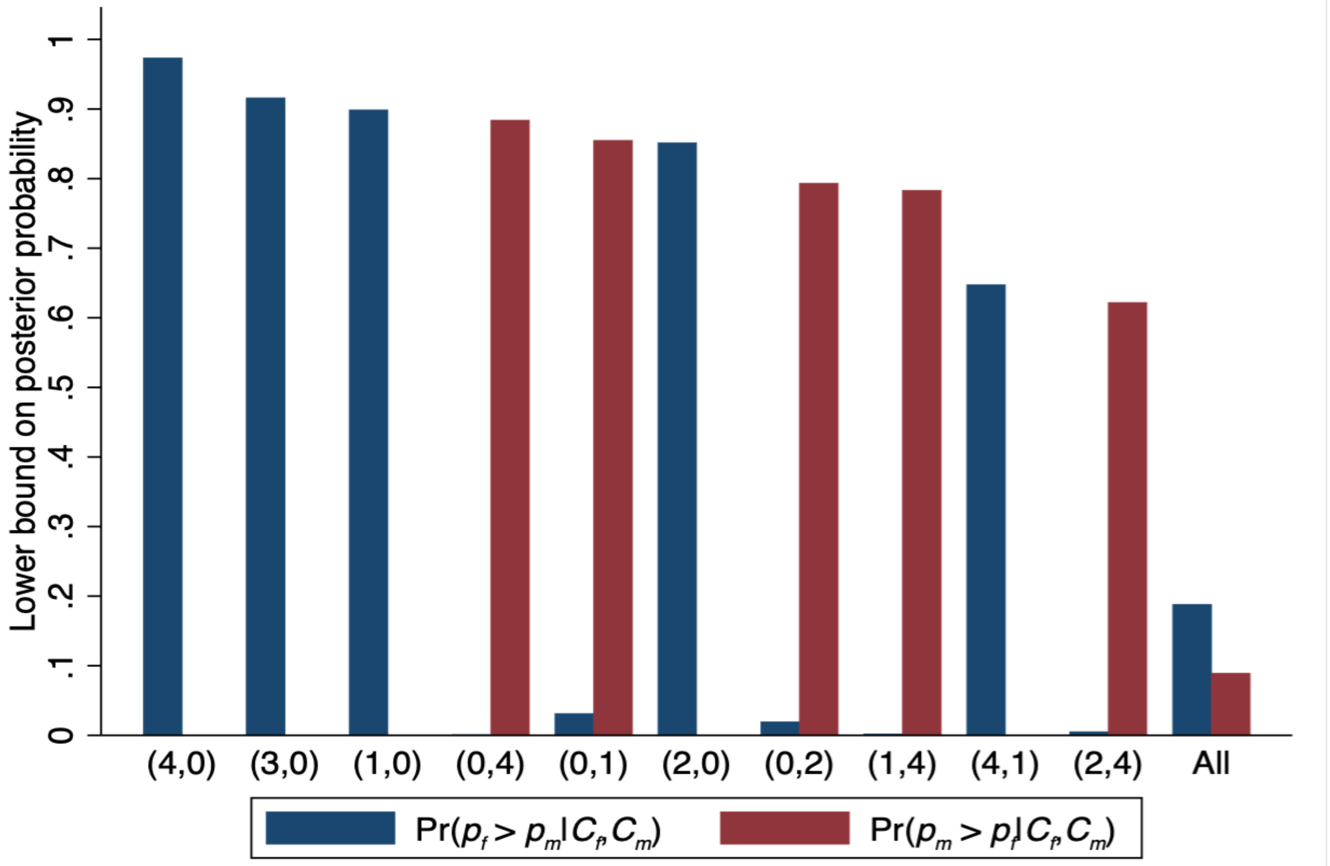
Notes: This figure displays lower bounds on the probability that jobs in the Bertrand and Mullainathan (2004) data discriminate based upon race given their callback configurations. Each bar reports a lower bound on the posterior probability of discrimination conditional on (C_w, C_b), where C_w is the number of white callbacks and C_b is the number of black callbacks.

Figure II: Lower bounds on posterior probabilities of discrimination, Nunley et al. data



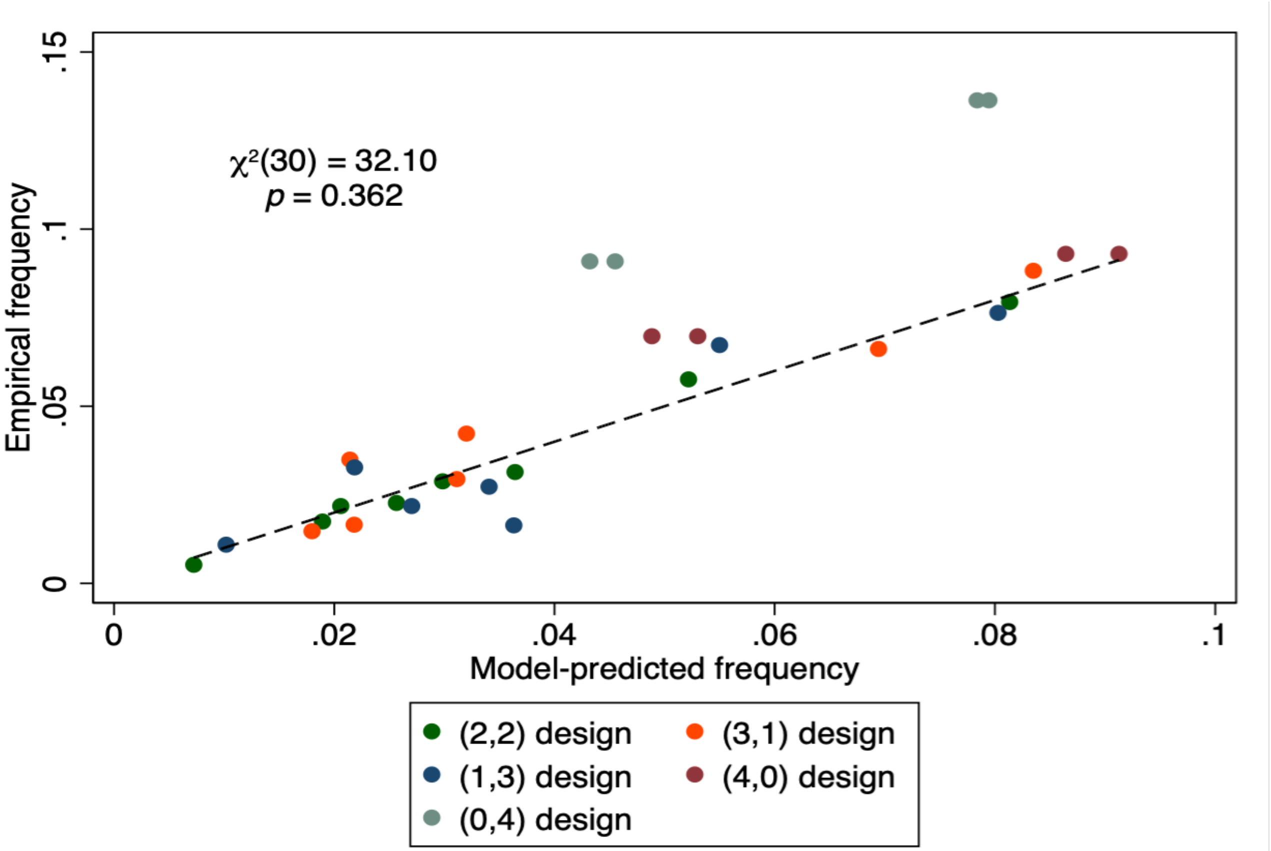
Notes: This figure displays lower bounds on the probability that jobs in the Nunley et al. (2015) data discriminate against black workers for the 10 callback configurations with highest posterior bounds. Each bar reports a lower bound on the posterior probability that $p_w > p_b$ conditional on (C_w, C_b), where C_w is the number of white callbacks and C_b is the number of black callbacks. Orange bars correspond to an experimental design with 3 white and 1 black application, green bars correspond to a design with 2 white and 2 black applications, and red bars correspond to a design with 1 white and 3 black applications. The blue bar reports the lower bound on the prior probability of discrimination.

Figure III: Lower bounds on posterior probabilities of discrimination, AGCV data



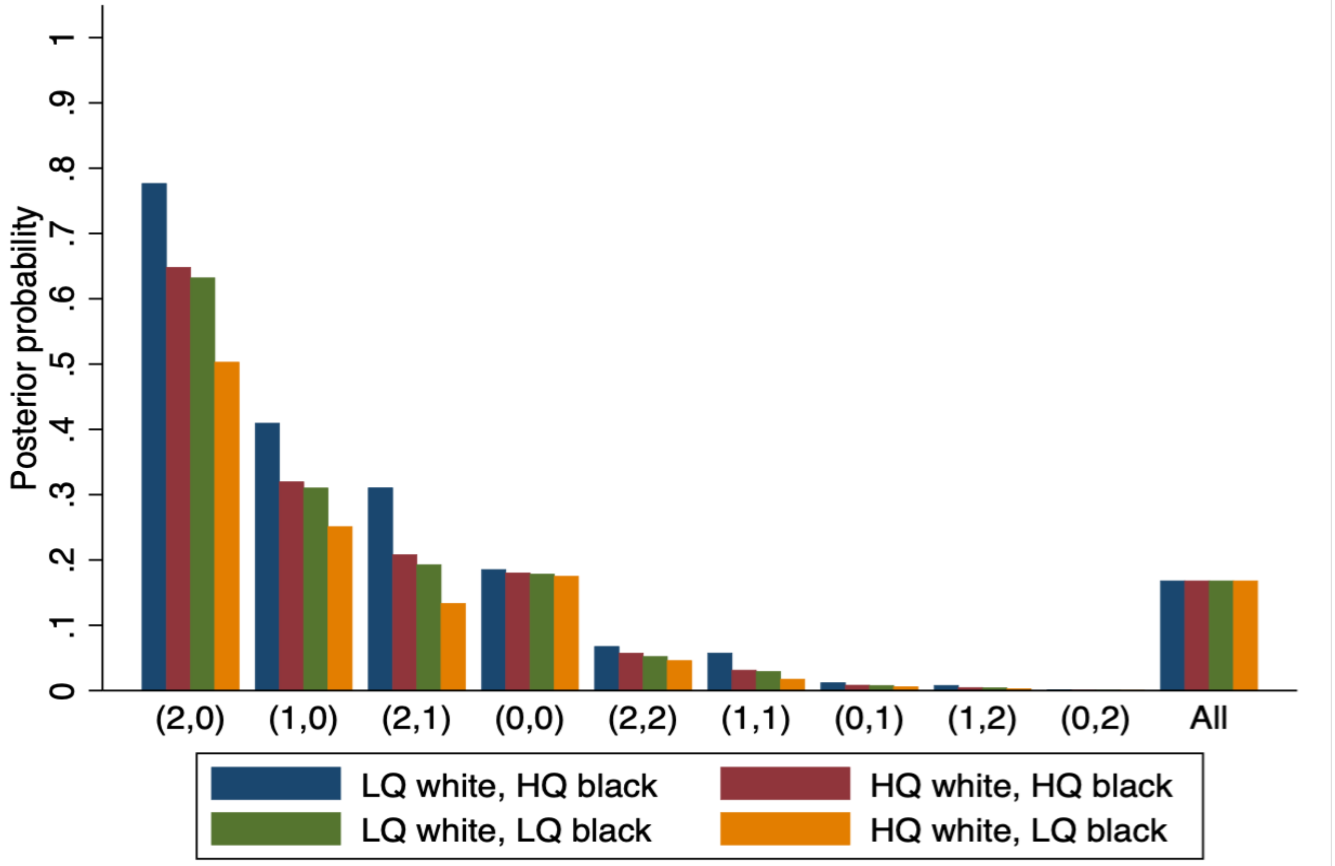
Notes: This figure displays lower bounds on the probability that jobs in the Arceo-Gomez and Campos-Vasquez (2014) data discriminate based upon sex for the 10 callback configurations with highest posterior bounds. Each bar reports a lower bound on the posterior probability of discrimination conditional on (C_f, C_m), where C_f is the number of female callbacks and C_m is the number of male callbacks. Blue bars report lower bounds on the probability of discriminating against men, and red bars report lower bounds on the probability of discriminating against women.

Figure IV: Mixed logit model fit



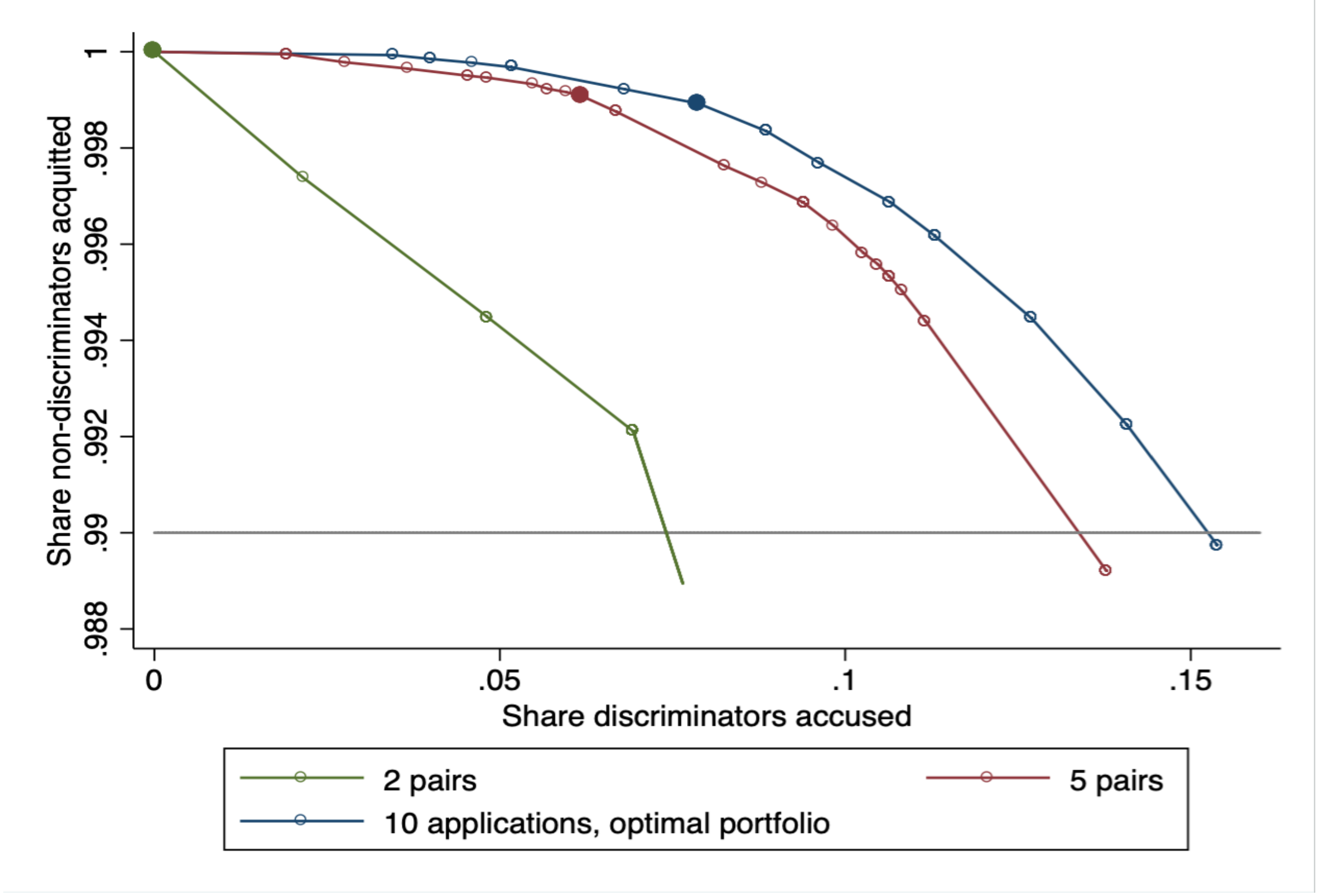
Notes: This figure compares mixed logit predicted frequencies for callback events in the Nunley et al. (2015) data with corresponding empirical frequencies. The horizontal axis plots model-predicted probabilities for each possible combination of white and black callback counts (excluding zero total callbacks), separately by experimental design. Model predictions are calculated by simulating the logit model in column (2) of Table X 10,000 times for each job in the Nunley et al. data set. The vertical axis plots the observed frequency of each event. Green dots show frequencies for a design with two white and two black applications, while orange, blue, red, and grey points show frequencies for designs with 3 white and 1 black, 1 white and three black, 4 white and zero black, and 0 white and 4 black applications, respectively. The dashed line is the 45-degree line. The chi-squared statistic and *p* -value come from a test that all model-predicted and empirical frequencies match, treating the model predictions as fixed.

Figure V: Mixed logit estimates of posterior discrimination probabilities, Nunley et al. data



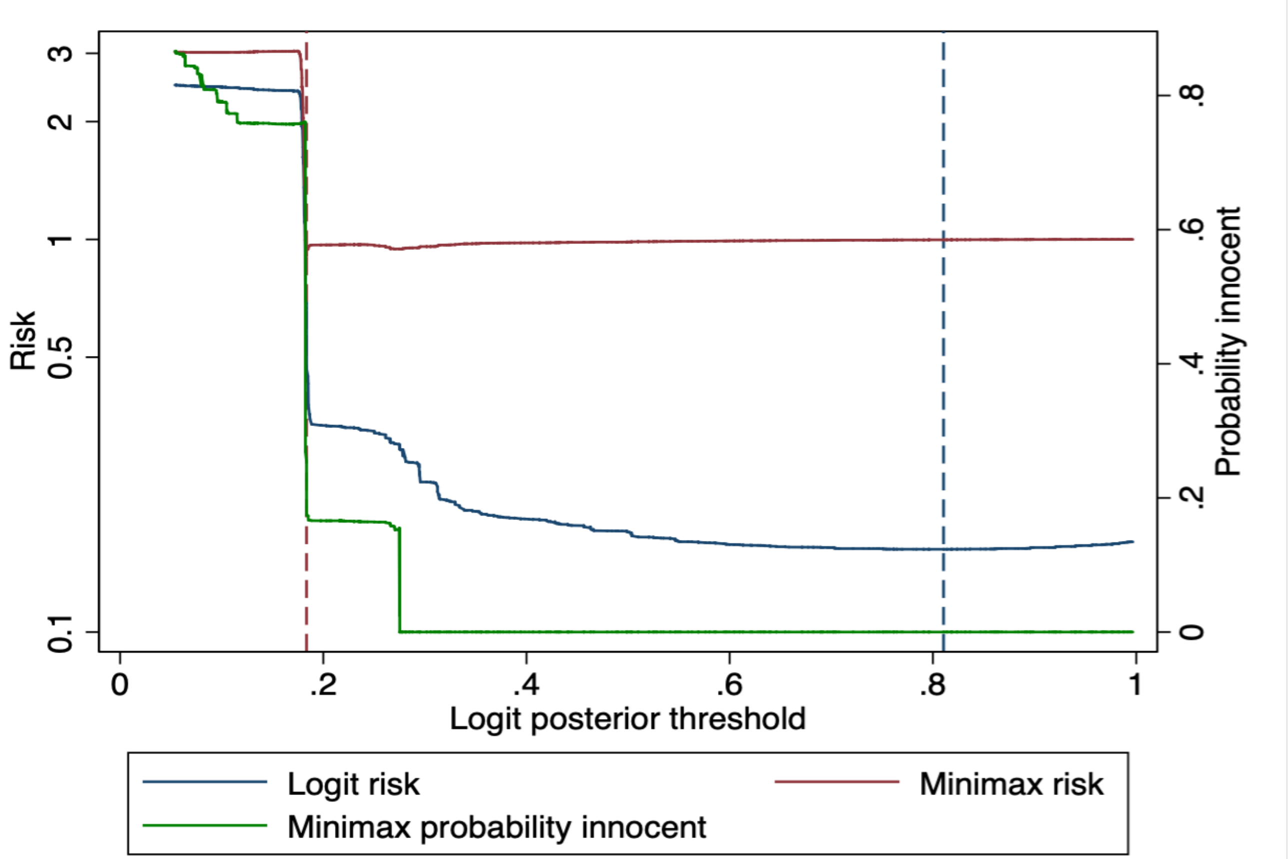
Notes: This figure displays mixed logit estimates of the posterior probability that jobs in the Nunley et al. (2015) data discriminate against black workers conditional on (C_w, C_b), where C_w is the number of white callbacks and C_b is the number of black callbacks. Blue bars show posteriors for a design sending two low quality (LQ) white applications and two high quality (HQ) black applications, where low and high quality are defined based on a logit covariate index 1 standard deviation below or above the mean. Red bars show posteriors for a design sending two HQ white and two HQ black applications. Green bars show posteriors for a design sending two LQ white and two LQ black applications. Orange bars show posteriors for a design sending two HQ white and two LQ black applications.

Figure VI: Detection/error tradeoffs, Nunley et al. data



Notes: This figure displays detection/error tradeoff curves based on models fit to the Nunley et al. (2015) data. Estimates come from decision rules applied to experiments generated from the logit model in column (2) of Table X. The horizontal axis measures the share of discriminating jobs accused by each decision rule, while the vertical axis measures the share of non-discriminating jobs not accused (acquitted). The curves are generated by varying the posterior threshold at which firms are accused. The green curve corresponds to an experiment that sends two white and two black applications to each firm, and the red curve corresponds to sending five applications of each race. These two curves randomly assign a 2-valued covariate index of resume quality (high or low), defined as +/-1 the empirical standard deviation of this index. The blue curve shows results from sending the optimal mix of race and resume quality for each posterior threshold. Bold points correspond to 80% posterior thresholds.

Figure VII: Logit and minimax risk, Nunley et al. data



Notes: This figure displays average risk generated by a hypothetical experiment sending five white and five black applications to jobs in the population studied by Nunley et al. (2015). Resumes are randomly assigned to high or low quality with equal probability, where quality is defined as +/-1 the empirical standard deviation of the logit covariate index. The horizontal axis plots the posterior threshold at which jobs are accused of discrimination. The risk function is 4 times the number of non-discriminators accused plus the number of discriminators not accused. Discrimination is defined as calling white applicants at a higher rate than black applicants. The blue curve displays risk using the logit data generating process in column (2) of Table X. The red curve plots minimax risk calculated by choosing the joint distribution of callback probabilities to maximize risk for each decision rule subject to the moments identified by the Nunley et al. (2015) experiment, restricted to decision rules that order jobs by the logit posterior threshold. Vertical dashed lines indicate risk-minimizing thresholds. The green curve displays the share of jobs that are innocent in the worst-case data generating process.

Table I: Descriptive statistics for resume correspondence studies

		Bertrand & Mullainathan	Nunley et al.	Arceo-Gomez & Campos-Vasquez
		(1)	(2)	(3)
Number of jobs		1,112	2,305	802
Applications per job		4	4	8
Treatment/control		Black/white	Black/white	Male/female
Callback rates:	Total	0.079	0.167	0.123
	Treatment	0.063	0.154	0.108
	Control	0.094	0.180	0.138
	Difference	-0.031	-0.026	-0.030
		(0.007)	(0.007)	(0.008)

Notes: This table reports sample characteristics based on data from three resume correspondence experiments. Columns (1) and (2) show statistics from Bertrand and Mullainathan's (2004) and Nunley et al.'s (2015) studies of racial discrimination in the United States. Column (3) reports statistics from Arceo-Gomez & Campos-Vasquez's (2014) study of gender discrimination in Mexico. Standard errors for treatment/control differences, clustered at the job level, are in parentheses.

Table II: Tests for dependence across trials

Nunley et al. data			AGCV data		
	Main effect	Leave-out mean		Main effect	Leave-out mean
Variable	(1)	(2)	Variable	(3)	(4)
Black	-0.028	-0.019	Married	0.001	0.002
	(0.010)	(0.027)		(0.008)	(0.033)
Female	0.010	0.009	Age	0.003	0.002
	(0.010)	(0.027)		(0.003)	(0.005)
High SES	-0.233	-0.674	Scholarship	-0.003	-0.060
	(0.174)	(0.522)		(0.010)	(0.050)
GPA	-0.043	-0.153	Predicted callback rate	-0.644	-0.136
	(0.066)	(0.198)		(0.504)	(0.888)
Business major	0.008	0.010			
	(0.008)	(0.021)			
Employment gap	0.011	0.034			
	(0.009)	(0.023)			
Current unemp.: 3+	0.013	0.005			
	(0.012)	(0.032)			
6+	-0.008	-0.038			
	(0.012)	(0.029)			
12+	0.001	0.021			
	(0.012)	(0.032)			
Past unemp.: 3+	0.029	0.065			
	(0.012)	(0.031)			
6+	-0.011	-0.016			
	(0.012)	(0.033)			
12+	-0.004	0.019			
	(0.012)	(0.031)			
Predicted callback rate	0.476	-0.041			
	(0.248)	(0.626)			
Joint *p* -value	0.452		Joint *p* -value	0.589	
Sample size	9,220		Sample size	6,416	

Notes: This table reports results from tests of the assumption that applications at each job are independent trials. Estimates come from regressions of a callback indicator on a resume characteristic and the mean of this characteristic across other resumes at the same job. Columns (1)-(2) use data from Nunley et al. (2015), and columns (3)-(4) use data from Arceo-Gomez and Campos-Vasquez (2014). The predicted callback rate is the fitted value from a regression of a callback indicator on all resume characteristics, jackknifed at the job level. *P* -value comes from a test of the hypothesis that coefficients on the leave-out mean are zero for all individual characteristics. Standard errors, clustered at the job level, appear in parentheses.

Table III: Moments of callback rate distribution, BM data

Moment	No constraints (1)	Shape constraints (2)
$E[p_w]$	0.094	0.094
	(0.006)	(0.007)
$E[p_b]$	0.063	0.063
	(0.006)	(0.006)
$E[(p_w - E[p_w])^2]$	0.040	0.040
	(0.005)	(0.004)
$E[(p_b - E[p_b]\ )^2]$	0.023	0.023
	(0.004)	(0.003)
$E[(p_w - E[p_w]\ )(p_b - E[p_b]\ )]$	0.028	0.028
	(0.004)	(0.003)
$E[(p_w - E[p_w]\ )^2(p_b - E[p_b]\ )]$	0.015	0.014
	(0.003)	(0.002)
$E[(p_w - E[p_w]\ )(p_b - E[p_b]\ )^2]$	0.012	0.012
	(0.003)	(0.002)
$E[(p_w - E[p_w]\ )^2(p_b - E[p_b]\ )^2]$	0.010	0.010
	(0.003)	(0.002)
	J-statistic:	0.00
	P-value:	1.000
Sample size	1,112	

Notes: This table reports generalized method of moments (GMM) estimates of moments of the joint distribution of job-specific white and black callback rates in the Bertrand and Mullainathan (2004) data. Estimates in column (2) come from a shape-constrained GMM procedure imposing that the moments are consistent with a well-defined probability distribution. The J-statistic is the minimized shape-constrained GMM criterion function. The p-value come from a bootstrap test of the hypothesis that the model restrictions are satisfied.

Table IV: Moments of callback rate distribution, Nunley et al. data

	Design-specific estimates				
	(2,2)	(3,1)	(1,3)		Combined
	design	design	design	P-value	estimates
Moment	(1)	(2)	(3)	(4)	(5)
$E[p_w]$	0.174	0.199	0.142	0.027	0.177
	(0.010)	(0.025)	(0.015)		(0.007)
$E[p_b]$	0.148	0.149	0.157	0.854	0.153
	(0.010)	(0.015)	(0.013)		(0.007)
$E[(p_w - E[p_w])^2]$	0.089	0.108	-	0.097	0.095
	(0.007)	(0.009)			(0.004)
$E[(p_b - E[p_b])^2]$	0.085	-	0.083	0.857	0.084
	(0.007)		(0.008)		(0.004)
$E[(p_w - E[p_w])(p_b - E[p_b])]$	0.083	0.084	0.080	0.926	0.084
	(0.006)	(0.009)	(0.009)		(0.004)
$E[(p_w - E[p_w])^3]$	-	0.051	-		0.106
		(0.008)			(0.006)
$E[(p_b - E[p_b])^3]$	-	-	0.044		0.092
			(0.007)		(0.006)
$E[(p_w - E[p_w])^2(p_b - E[p_b])]$	0.044	0.043	-	0.875	0.040
	(0.004)	(0.007)			(0.002)
$E[(p_w - E[p_w])(p_b - E[p_b])^2]$	0.047	-	0.045	0.819	0.042
	(0.005)		(0.007)		(0.002)
$E[(p_w - E[p_w])^3(p_b - E[p_b])]$	-	0.034	-	-	0.035
		(0.005)			(0.002)
$E[(p_w - E[p_w])(p_b - E[p_b])^3]$	-	-	0.037	-	0.037
			(0.006)		(0.002)
$E[(p_w - E[p_w])^2(p_b - E[p_b])^2]$	0.036	-	-	-	0.038
	(0.004)				(0.002)
				J-statistic:	23.09
				P-value:	0.190
Sample size	1,146	544	550		2,240

Notes: This table reports generalized method of moments (GMM) estimates of moments of the joint distribution of job-specific white and black callback rates in the Nunley et al. (2015) data. Columns (1), (2), and (3) show estimates based on jobs that received 2 white and 2 black, 3 white and 1 black, and 1 white and 3 black applications, respectively. Estimates in column (4) come from a shape-constrained GMM procedure imposing that the moments are consistent with a well-defined probability distribution. The J-statistic is the minimized shape-constrained GMM criterion function. The p-value come from a bootstrap test of the hypothesis that the model restrictions are satisfied.

Table V: Moments of callback rate distribution, AGCV data

Moment	No constraints (1)	Shape constraints (2)	Moment	No constraints (3)	Shape constraints (4)
$E[p_f]$	0.138 (0.010)	0.140 (0.009)	$E\left[\left(p_f - E[p_f]\right)^4\right]$	0.023 (0.004)	0.025 (0.003)
$E[p_m]$	0.108 (0.009)	0.114 (0.009)	$E[(p_m - E[p_m])^4]$	0.019 (0.004)	0.024 (0.003)
$E\left[\left(p_f - E[p_f]\right)^2\right]$	0.066 (0.009)	0.066 (0.005)	$E\left[\left(p_f - E[p_f]\right)^4 (p_m - E[p_m])\right]$	0.012 (0.003)	0.011 (0.002)
$E[(p_m - E[p_m])^2]$	0.048 (0.005)	0.054 (0.005)	$E\left[\left(p_f - E[p_f]\right)(p_m - E[p_m])^4\right]$	0.013 (0.003)	0.013 (0.002)
$E[(p_f - E[p_f])(p_m - E[p_m])]$	0.043 (0.005)	0.044 (0.004)	$E\left[\left(p_f - E[p_f]\right)^3 (p_m - E[p_m])^2\right]$	0.012 (0.003)	0.011 (0.002)
$E\left[\left(p_f - E[p_f]\right)^3\right]$	0.025 (0.005)	0.063 (0.007)	$E\left[\left(p_f - \mu_f\right)^2 (p_m - E[p_m])^3\right]$	0.012 (0.003)	0.012 (0.002)
$E[(p_m - E[p_m])^3]$	0.031 (0.005)	0.050 (0.007)	$E\left[\left(p_f - E[p_f]\right)^4 (p_m - E[p_m])^2\right]$	0.010 (0.002)	0.009 (0.001)
$E\left[\left(p_f - E[p_f]\right)^2 (p_m - E[p_m])\right]$	0.020 (0.004)	0.017 (0.002)	$E\left[\left(p_f - E[p_f]\right)^2 (p_m - E[p_m])^4\right]$	0.010 (0.002)	0.009 (0.001)
$E\left[\left(p_f - E[p_f]\right)(p_m - E[p_m])^2\right]$	0.022 (0.004)	0.020 (0.002)	$E\left[\left(p_f - E[p_f]\right)^3 (p_m - E[p_m])^3\right]$	0.009 (0.002)	0.009 (0.001)
$E\left[\left(p_f - E[p_f]\right)^3 (p_m - E[p_m])\right]$	0.015 (0.003)	0.015 (0.002)	$E\left[\left(p_f - E[p_f]\right)^4 (p_m - E[p_m])^3\right]$	0.008 (0.002)	0.007 (0.001)
$E\left[\left(p_f - E[p_f]\right)(p_m - E[p_m])^3\right]$	0.016 (0.003)	0.017 (0.002)	$E\left[\left(p_f - E[p_f]\right)^3 (p_m - E[p_m])^4\right]$	0.008 (0.002)	0.008 (0.001)
$E\left[\left(p_f - E[p_f]\right)^2 (p_m - E[p_m])^2\right]$	0.016 (0.003)	0.016 (0.002)	$E\left[\left(p_f - E[p_f]\right)^4 (p_m - E[p_m])^4\right]$	0.009 (0.002)	0.006 (0.001)
		J-statistic:	3.33		
		P-value:	0.790		
		Sample size:	802		

Notes: This table reports generalized method of moments (GMM) estimates of moments of the joint distribution of job-specific white and black callback rates in the Arceo-Gomez and Campos-Vasques (2014) data. Estimates in columns (2) and (4) come from a shape-constrained GMM procedure imposing that the moments are consistent with a well-defined probability distribution. The J-statistic is the minimized shape-constrained GMM criterion function. The p-value come from a bootstrap test of the hypothesis that the model restrictions are satisfied.

Table VI: Nonparametric estimates of treatment effect variation in resume correspondence studies

	Bertrand & Mullainathan			Nunley et al.			Arceo-Gomez & Campos-Vasquez		
	p_b	p_w	p_b - p_w	p_b	p_w	p_b - p_w	p_m	p_f	p_m - p_f
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean	0.063	0.094	-0.031	0.153	0.177	-0.023	0.114	0.140	-0.025
	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.005)	(0.009)	(0.009)	(0.008)
Standard deviation	0.152	0.199	0.082	0.290	0.308	0.102	0.231	0.257	0.179
	(0.011)	(0.011)	(0.012)	(0.008)	(0.007)	(0.009)	(0.011)	(0.010)	(0.011)
Correlation with p_w or p_f	0.927	1.000	-0.717	0.944	1.000	-0.336	0.735	1.000	-0.483
	(0.055)	-	(0.089)	(0.018)	-	(0.048)	(0.035)	-	(0.051)
Skewness	-	-	-	3.757	3.648	-4.450	4.067	3.748	-1.403
				(0.074)	(0.087)	(0.405)	(0.140)	(1.161)	(0.385)
Excess kurtosis	-	-	-	-	-	-	8.452	5.756	12.227
							(1.458)	(8.790)	(2.291)

Note: This table reports shape-constrained generalized method of moments (GMM) estimates of key features of the joint distribution of treatment and control callback rates in three resume correspondence studies. Columns (1)-(3) show estimates for black and white callback rates in Bertrand and Mullainathan (2004), columns (4)-(6) display estimates for black and white callback rates in Nunley et al. (2015), and columns (7)-(9) show estimates for male and female callback rates in Arceo-Gomez and Campos-Vasquez (2014). Standard error are computed using the numerical bootstrap procedure described by Hong and Li (2017).

Table VII: Upper bounds on shares not discriminating, BM data

	$\Pr(p_w = p_b)$			
	Analytic	Lin. prog.	$\Pr(p_w \geq p_b)$	$\Pr(p_w \leq p_b)$
Callbacks	(1)	(2)	(3)	(4)
All	-	0.870	1.000	0.870
0	-	0.962	1.000	0.962
1	0.637	0.576	1.000	0.576
2	0.621	0.558	1.000	0.558
3	0.560	0.492	1.000	0.492
4	-	0.788	1.000	0.788
	J-statistic:	29.26	0.00	29.26
	P-value:	0.000	1.000	0.000

Notes: This table reports upper bounds on the probability that jobs in the Bertrand and Mullainathan (2004) data do not discriminate based upon race. Column (1) shows upper bounds on the fraction of jobs that have the same callback rate for black and white applicants computed using the analytic formula in the text. Column (2) shows corresponding bounds computed by linear programming imposing constraints across callback strata. Column (3) and (4) show upper bounds on the fraction of jobs that are at least as likely to call white applicants as to call black applicants and vice versa, computed via linear programming. J-statistics and p-values come from bootstrap tests of the hypothesis that these probabilities equal one for all jobs.

Table VIII: Upper bounds on shares not discriminating, Nunley et al. data

		$\Pr(p_w = p_b)$			
		Analytic	Lin. prog.	$\Pr(p_w \geq p_b)$	$\Pr(p_w \leq p_b)$
Design	Callbacks	(1)	(2)	(3)	(4)
All	All	-	0.642	0.846	0.827
(2,2)	0	-	0.848	0.907	0.952
	1	0.824	0.328	0.815	0.567
	2	0.718	0.309	0.984	0.325
	3	0.874	0.179	0.933	0.264
	4	-	0.579	0.743	0.872
(3,1)	0	-	0.853	0.898	0.964
	1	0.861	0.337	0.894	0.549
	2	0.738	0.332	0.998	0.336
	3	0.816	0.151	0.922	0.251
	4	-	0.566	0.767	0.837
(1,3)	0	-	0.839	0.916	0.936
	1	0.810	0.323	0.754	0.594
	2	0.855	0.326	0.958	0.369
	3	0.913	0.204	0.955	0.262
	4	-	0.581	0.723	0.893
		J-statistic:	62.64	23.46	62.64
		P-value:	0.000	0.120	0.000

Notes: This table reports upper bounds on the probability that jobs in the Nunley et al. (2015) data do not discriminate based upon race. Column (1) shows upper bounds on the fraction of jobs that have the same callback rate for black and white applicants computed using the analytic formula in the text. Column (2) shows corresponding bounds computed by linear programming imposing constraints across callback strata. Column (3) and (4) show upper bounds on the fraction of jobs that are at least as likely to call white applicants as to call black applicants and vice versa, computed via linear programming. J-statistics and p- values come from bootstrap tests of the hypothesis that these probabilities equal one for all jobs.

Table IX: Upper bounds on shares not discriminating, AGCV data

	$\Pr(p_f = p_m)$			
	Analytic	Lin. prog.	$\Pr(p_f \geq p_m)$	$\Pr(p_f \leq p_m)$
Callbacks	(1)	(2)	(3)	(4)
All	-	0.723	0.911	0.812
0	-	0.864	0.960	0.905
1	0.968	0.105	0.586	0.520
2	0.460	0.284	0.740	0.544
3	0.495	0.424	0.953	0.472
4	0.522	0.497	0.945	0.553
5	0.687	0.654	0.829	0.825
6	0.662	0.591	0.788	0.803
7	0.875	0.514	0.843	0.671
8	-	0.924	0.989	0.935
	J-statistic:	369.66	33.88	359.95
	P-value:	0.000	0.005	0.000

Notes: This table reports upper bounds on the probability that jobs in the Arceo-Gomez and Campos-Vasquez (2014) data do not discriminate based upon sex. Column (1) shows upper bounds on the fraction of jobs that have the same callback rate for male and female applicants computed using the analytic formula in the text. Column (2) shows corresponding bounds computed by linear programming imposing constraints across callback strata. Column (3) and (4) show upper bounds on the fraction of jobs that are at least as likely to call female applicants as to call male applicants and vice versa, computed via linear programming. *J*-statistics and *p*-values come from bootstrap tests of the hypothesis that these probabilities equal one for all jobs.

Table X: Mixed logit estimates, Nunley et al. data

			Types	
		Constant	No selection	Selection
		(1)	(2)	(3)
Distribution of logit(p_w):	α_0	-4.708	-4.931	-4.927
		(0.223)	(0.242)	(0.280)
	σ_α	4.745	4.988	4.983
		(0.223)	(0.249)	(0.294)
Discrimination intensity:	β_0	0.456	4.046	4.053
		(0.108)	(1.563)	(1.576)
Discrimination logit:	τ_0	-	-1.586	-1.556
			(0.416)	(1.098)
	τ_α	-	-	-0.005
				(0.180)
Fraction with $p_w \neq p_b$:		1.000	0.168	0.170
Log-likelihood		-2,792.1	-2,788.2	-2,788.2
Parameters		15	16	17
Sample size		2,305	2,305	2,305

Notes: This table reports estimates of logit models for callback probabilities in the Nunley et al. (2015) data. All models include a normally distributed job-specific intercept. Column (2) allows for two discrete types of jobs, one of which does not discriminate based upon race. Column (3) allows the probability of discrimination to depend on the job-specific intercept. Models are estimated by simulated maximum likelihood using 1,000 Halton draws of the intercept for each job. All models include resume covariates which are de-meaned in the estimation sample. Robust standard errors in parentheses.