# Hiring as Exploration*

Danielle Li
MIT & NBER

Lindsey Raymond
MIT

Peter Bergman
Columbia & NBER

December 30, 2021

Please see here for latest version

**Abstract**

This paper views hiring as a contextual bandit problem: to find the best workers over time, firms must balance "exploitation" (selecting from groups with proven track records) with "exploration" (selecting from under-represented groups to learn about quality). Yet modern hiring algorithms, based on "supervised learning" approaches, are designed solely for exploitation. Instead, we build a resume screening algorithm that values exploration by evaluating candidates according to their statistical upside potential. Using data from professional services recruiting within a Fortune 500 firm, we show that this approach improves the quality (as measured by eventual hiring rates) of candidates selected for an interview, while also increasing demographic diversity, relative to the firm's existing practices. The same is not true for traditional supervised learning based algorithms, which improve hiring rates but select far fewer Black and Hispanic applicants. In an extension, we show that exploration-based algorithms are also able to learn more effectively about simulated changes in applicant hiring potential over time. Together, our results highlight the importance of incorporating exploration in developing decision-making algorithms that are potentially both more efficient and equitable.

**JEL Classifications**: D80, J20, M15, M51, O33

**Keywords**: Hiring, Machine Learning, Algorithmic Fairness, Diversity, Bandit Problems.

*Correspondence to d_li@mit.edu, lraymond@mit.edu, and bergman@tc.columbia.edu. We are grateful to David Autor, Pierre Azoulay, Dan Bjorkegren, Emma Brunskill, Max Cytrynbaum, Eleanor Dillon, Alex Frankel, Bob Gibbons, Nathan Hendren, Max Kasy, Pat Kline, Jin Li, Fiona Murray, Anja Sautmann, Scott Stern, John Van Reenen, Kathryn Shaw, and various seminar participants for helpful comments and suggestions. The content is solely the responsibility of the authors and does not necessarily represent the official views of MIT, Columbia University, or the NBER.

Algorithms have been shown to outperform human decision-makers across an expanding range of settings, from medical diagnosis to image recognition to game play.[1] Yet the rise of algorithms is not without its critics, who caution that automated approaches may codify existing human biases and allocate fewer resources to those from under-represented groups.[2]

A key emerging application of machine learning (ML) tools is in hiring, a setting where decisions matter for both firm productivity and individual access to opportunity, and where algorithms are increasingly used at the "top of the funnel," to screen job applicants for interviews.[3] Modern hiring ML typically relies on "supervised learning," meaning that it forms a model of the relationship between applicant covariates and outcomes in a given training dataset, and then applies this model to predict outcomes for subsequent applicants.[4] By systematically analyzing historical examples, these tools can unearth predictive relationships that may be overlooked by human recruiters; indeed, a growing literature has shown that supervised learning algorithms can more effectively identify high quality job candidates than human recruiters.[5] Yet because this approach implicitly assumes that past examples extend to future applicants, firms that rely on supervised learning may tend to select from groups with proven track records, raising concerns about access to opportunity for non-traditional applicants.[6] Because algorithms are most frequently used at the very top of the hiring funnel, a reliance on supervised learning models may prevent non-traditional workers from accessing even initial interviews.

In this paper, we develop and evaluate a resume screening algorithm that explicitly values exploration and provide the first empirical evidence that algorithmic design impacts access to job opportunity. Our approach begins with the idea that the hiring process can be thought of as a contextual bandit problem: in looking for the best applicants over time, a firm must balance "exploitation" with "exploration" as it seeks to learn the predictive relationship between applicant

[1] For example, see Yala et al. (2019); McKinney (2020); Mullainathan and Obermeyer (2019); Schrittwieser et al. (2019); Russakovsky et al. (2015)

[2] A widely publicized example is Amazon's use of an automated hiring tool that penalized the use of the term "women's" (for example, "women's crew team") on resumes: https://www.reuters.com/article/us-amazon-com-jobs-automation-insight/amazon-scraps-secret-ai-recruiting-tool-that-showed-bias-against-women-idUSKCN1MK08G. Obermeyer et al. (2019); Datta et al. (2015); Lambrecht and Tucker (2019) document additional examples in the academic literature.

[3] While exact adoption figures are difficult to come by, a recent survey of human resource executives indicates that 39% use some form of predictive analytics in 2020, up from 10% in 2016 (Mercer, 2020). Another survey of technology companies indicates that 60% plan on investing in AI-powered recruiting software in 2018, and over 75% of recruiters believe that artificial intelligence will transform hiring practices (Bogen and Rieke, 2018).

[4] ML tools can be used in a variety of ways throughout the hiring process but, by far, algorithms are most commonly used in the first stages of the application process to decide which applicants merit further human review (Raghavan et al., 2019). In this paper, we will use the term "hiring ML" to refer primarily to algorithms that help make the initial interview decision, rather than the final offer.

[5] See, for instance, Hoffman et al. (2018); Cowgill (2018).

[6] For example, Kline and Walters (2020) test for discrimination in hiring practices, which can both be related to the use of algorithms and influence the data available to them. Bogen and Rieke (2018), in their review of commercial hiring algorithms, state "although it might seem natural for screening tools to consider previous hiring decisions, those decisions often reflect the very patterns many employers are actively trying to change through diversity and inclusion initiatives".

covariates (the "context") and applicant quality (the "reward"). Whereas the optimal solution to bandit problems is widely known to incorporate some exploration, supervised learning based algorithms engage only in exploitation because they are designed to solve static prediction problems. By contrast, ML tools that incorporate exploration are designed to solve dynamic prediction problems that involve learning from sequential actions: in the case of hiring, these algorithms value exploration because learning improves future choices.

Incorporating exploration into screening technologies may also shift the demographic composition of selected applicants. While exploration in the bandit sense—that is, selecting candidates with covariates for which there is more uncertainty—need not be the same as favoring demographic diversity, it is also the case that Black, Hispanic, and female applicants are less likely to be employed in high-income jobs, meaning that they appear less often in the historical datasets used to train hiring algorithms. Because data under-representation tends to increase uncertainty, adopting bandit algorithms that value exploration (for the sake of learning) may expand representation even when demographic diversity is not part of their mandate. This logic is consistent with a growing number of studies showing that firms may hold persistently inaccurate beliefs about the quality of minority applicants, and may benefit from nudges (algorithmic or otherwise) that generate additional signals of their quality.[7]

Our paper uses data from a large Fortune 500 firm to study the decision to grant first-round interviews for high-skill positions in consulting, financial analysis, and data science—sectors which offer well-paid jobs with opportunities for career mobility and which have also been criticized for their lack of diversity. Relative to human screening decisions, we show that contextual bandit algorithms increase the quality of interview decisions (as measured by hiring yield) while also selecting a more diverse set of applicants. Yet, in the same setting, we also show that this is not the case for traditional supervised learning approaches, which increase quality but at the cost of vastly reducing Black and Hispanic representation. Our results therefore demonstrate the potential of algorithms to improve the hiring process, while cautioning against the idea that they are generically equity or efficiency enhancing.

Like many firms in its sector, our data provider is overwhelmed with applications and rejects the vast majority of candidates on the basis of an initial resume screen. Motivated by how ML tools are typically used in the hiring process, our goal is to understand how algorithms can impact this consequential interview decision. In our analysis, we focus on hiring yield as our primary measure of quality. Because recruiting is costly and diverts employees from other productive work, our firm would like to adopt screening tools that improve its ability to identify applicants who will ultimately receive and accept an offer; currently, our firm's hiring rate among those interviewed is only 10%. As such, for most of our analysis, we define an applicant's quality as her "hiring potential"—that

[7]For instance, see Miller (2017); Bohren et al. (2019a,b); Lepage (2020a,b).

is, her likelihood of being hired, were she to receive an interview.[8] Because an applicant's hiring outcome may reflect preferences that are unrelated to her true productivity, we interpret hiring yield as a measure of whether a candidate meets the firm's internal hiring criteria, irrespective of what those criteria are. Our research therefore tests whether ML tools can help a firm achieve *its own* objectives. [9]

We build three different resume screening algorithms—two based on supervised learning, and one based on a contextual bandit approach—and evaluate the candidates that each algorithm selects, relative to the actual interview decisions made by the firm's human resume screeners. We observe data on an applicant's demographics (race, gender, and ethnicity), education (institution and degree), and work history (prior firms). Our goal is to maximize the quality of applicants who are selected for an interview; although we will also evaluate their diversity, we do not incorporate any explicit diversity preferences into our algorithm design.

Our first algorithm uses a static supervised learning approach (hereafter, "static SL") based on a logit LASSO model. Our second algorithm (hereafter, "updating SL") builds on the same baseline model as the static SL model, but updates the training data it uses throughout the test period with the hiring outcomes of the applicants it chooses to interview.[10] While this updating process allows the updating SL model to learn about the quality of the applicants it selects, it is myopic in the sense that it does not incorporate the value of this learning into its selection decisions.

Our main approach implements an Upper Confidence Bound (hereafter, "UCB") contextual bandit algorithm: in contrast to the static and updating SL algorithms, which evaluates candidates based on their *point estimates* of hiring potential, a UCB contextual bandit selects applicants based on the upper bound of the *confidence interval* associated with those point estimates. That is, there is implicitly an "exploration bonus" that is increasing in the algorithm's degree of uncertainty about quality. Exploration bonuses will tend to be higher for groups of candidates who are under-represented in the algorithm's training data because the model will have less precise estimates for these groups. In our implementation, we allow exploration bonuses to be based on a wide set of applicant covariates: the algorithm can choose to assign higher exploration bonuses on the basis of race or gender, but it is not required to and the algorithm could, instead, focus on other variables such as education or work history. Once candidates are selected, we incorporate their realized hiring outcomes into the training data and update the algorithm for the next period.[11] Standard

[8] Henceforth, this paper will use the terms "quality," "hiring potential," and "hiring likelihood" interchangeably, unless otherwise noted.

[9] While we do have a small amount of data on on-the-job performance, it's not sufficient to train an algorithm and also may be susceptible to bias.

[10] In practice, we can only update the model with data from selected applicants who are actually interviewed (otherwise we would not observe their hiring outcome). See Section 3.2.2 for a more detailed discussion of how this algorithm is updated.

[11] Similar to the updating SL approach, we only observe hiring outcomes for applicants who are actually interviewed in practice, we are only able to update the UCB model's training data with outcomes for the applicants it selects

and contextual bandit UCB algorithms have been shown to be optimal in the sense that they asymptotically minimize expected regret[12] and have begun to be used in economic applications.[13]

We have two main sets of results. First, our SL and UCB models differ markedly in the demographic composition of the applicants they select to be interviewed. Implementing a UCB model would more than double the share of interviewed applicants who are Black or Hispanic, from 10% to 24%. The static and updating SL models, however, would both dramatically decrease the combined share of Black and Hispanic applicants to 2% and 5%, respectively. In the case of gender, all algorithms would increase the share of selected applicants who are women, from 35% under human recruiting, to over 40% for all algorithms. This increase in diversity is persistent over time.

Our second set of results shows that, despite differences in their demographic profiles, all of our ML models substantial increase hiring yield relative to human recruiters. We note that assessing quality differences between human and ML models is more difficult than assessing diversity because we face a sample selection problem, also known in the literature as a "selective labels" problem:[14] although we observe demographics for all applicants, we only observe "hiring potential" for applicants who are interviewed. To address this, we take three complementary approaches, each based on different assumptions, all of which show that ML models would improve hiring yield relative to humans. We also discuss how our observational approach may differ from an ideal experimental implementation, and provide some evidence on how the presence of unobservables may shape these differences in our setting.

First, we focus on the sample of interviewed candidates for whom we directly observe hiring outcomes. Within this sample, we ask whether applicants preferred by our ML models have a higher likelihood of being hired than applicants preferred by a human recruiter. We find that, across all of our ML models, applicants with high scores are much more likely to be hired than those with low scores. In contrast, there is almost no relationship between an applicant's propensity to be selected by a human, and his or her eventual hiring outcome; if anything, this relationship is negative.

Our second approach uses inverse propensity score weighting to recover an estimate of mean hiring likelihood among applicants selected from our full applicant sample. This approach infers hiring outcomes for applicants who are not interviewed using observed outcomes among interviewed applicants with similar covariates. We continue to find that ML models improve hiring yield: average hiring rates among those selected by the UCB, updating SL, and static SL models are 33%, 35%, and 24%, respectively, compared with 10% among those selected by human recruiters.

who are also interviewed in practice. See Section 3.2.3 for a detailed discussion of how our implementation ("feasible" UCB might differ from an idealized implementation).

[12] Lai and Robbins (1985); Abbasi-Yadkori et al. (2019); Li et al. (2017) prove regret bounds for several different UCB algorithms. We follow the approach in Li et al. (2017) that extends the contextual bandit UCB for binary outcomes. See Section 2.1 and 3 for a detailed discussion.

[13] For example, see Currie and MacLeod (2020); Stefano Caria and Teytelboym (2020); Kasy and Sautmann (2019); Bergemann and Valimaki (2006); Athey and Wager (2019); Krishnamurthy and Athey (2020); Zhou et al. (2018); Dimakopoulou et al. (2018a).

[14] See, for instance, Lakkaraju et al. (2017); Kleinberg et al. (2018a); Arnold et al. (2020).

Our third approach uses an instrumental variables strategy to address concerns about potential selection on unobservables. In our setting, applicants are randomly assigned to initial resume screeners, who vary in their leniency in granting an interview. We show that applicants selected by stringent screeners (and therefore subject to a higher bar) have no better outcomes than those selected by more lax screeners: this suggests that humans are not positively screening candidates based on their unobservables. We use this same variation to identify the returns to following ML recommendations on the margin by looking at instrument compliers. We find that marginal candidates with high UCB scores have better hiring outcomes and are also more likely to be Black, Hispanic, or female. Such a finding suggests that following UCB recommendations on the margin would increase both the hiring yield and the demographic diversity of selected interviewees. In contrast, following SL recommendations on the margin would generate similar increases in hiring yield but decrease minority representation.

Finally, we provide some evidence relating hiring yield to other measures of applicant quality. We observe job performance ratings and promotion outcomes for a small subset of workers hired in our sample. Among this selected group, we show that our ML models (trained to maximize hiring likelihood) appear more positively correlated with on the job performance ratings and future promotion outcomes than a model trained to mimic the choices of human recruiters. This provides suggestive evidence that following ML recommendations designed to maximize hiring yield does not come at the expense of on the job performance, relative to following human recommendations.

Together, our main findings show that there need not be an equity-efficiency tradeoff when it comes to expanding diversity in the workplace. Specifically, firms' *current* recruiting practices appear to be far from the Pareto frontier, leaving substantial scope for new ML tools to improve both hiring rates and demographic representation. Even though our UCB algorithm places no value on diversity in and of itself, incorporating exploration in our setting would lead our firm to interview twice as many under-represented minorities while more than doubling its predicted hiring yield. At the same time, our SL models lead to similar increases in hiring yield, but at the cost of drastically reducing the number of Black and Hispanic applicants who are interviewed. This divergence in demographic representation between our SL and UCB results demonstrates the importance of algorithmic design for shaping access to labor market opportunities.[15]

This paper contributes to our understanding of hiring, exploration, and algorithms in several key ways.

We are the first paper to empirically document the impact of algorithm design on firms' hiring processes. To the best of our knowledge, machine-learning based screening tools are largely based on

[15]In an extension, we simulate applicant data to show that the efficiency gains of exploration-based algorithms are higher when the quality of traditionally under-represented candidates is changing. We also explore the impact of blinding the models to demographic variables. We show that removing information about race and gender leads to a model that achieves similar improvements in hiring yield, but with more modest increases in Black and Hispanic representation, a decrease in White representation and a large increase in Asian representation.

"supervised learning," an approach that prioritizes applicants who most closely resemble those who were successful in the firm's historical data.[16] As a result, existing research on the real-world impact of hiring algorithms also focuses on supervised learning approaches, despite a growing recognition of their potential to encode historical biases.[17] Our paper contributes to this work by highlighting the value of an alternative class of algorithms that have thus far not been applied in the context of job screening. We focus on a wide range of professional services positions within a large firm whose applicant pool and screening practices are representative of its industry. In this setting, we show that bandit algorithms improve hiring yield while also expanding access to opportunity for women and minorities—in contrast with traditional approaches which we show reduce representation.

Our paper also contributes to a literature focused on the value of non-algorithmic forms of exploration in hiring. The idea that firms can benefit from exploration in hiring was first formalized by Lazear (1998), which presents a theoretical model in which firms benefit from selecting risky workers because they have higher upside potential.[18] Despite this, empirical work has largely highlighted uncertainty about a worker's quality as a barrier to hiring (Kuhnen and Oyer, 2016).[19] A smaller literature has shown, however, that firms can benefit from policies that push them to explore: Miller (2017), for instance, shows that temporary affirmative action policies can generate persistent gains in minority representation.[20] Conceptually, we connect Lazear (1998)'s focus on learning about the individual upside potential of workers to a broader literature on bandits, in which exploration gives firms the option value to learn about entire groups of applicants. Empirically, while "exploration" does not require an algorithm, existing literature shows that humans may exhibit biases when asked to achieve subjective goals.[21] We contribute by showing that algorithms can implement exploration in a systematic and theoretically motivated way. Indeed, our results are

[16] Although many firms do not publicly provide information on the specifics of their proprietary algorithms, several industry sources have indicated that this is true of their own algorithms. In addition, Raghavan et al. (2019) survey firm approaches. In their paper, they find that vendors of ML-based recruiting tools vary in their choices of what outcome to predict (e.g. what measure of worker quality), as well as in their choice of or access to historical training data (e.g. whether they use data from the focal client firm only or from a collection of other firms). However, Raghavan et al. (2019) make no mention of firms incorporating dynamic learning into their models. In some cases, firms claim to "de-bias" their predictions; these attempts however, are generally not validated, and aim to ensure legal compliance, rather than highlighting the value of exploration.

[17] For example, Hoffman et al. (2018); Cowgill (2018) both consider the impact of supervised learning based ML on hiring outcomes. Moreover, the relationship between existing hiring practices and algorithmic biases is theoretically nuanced; for a discussion, see Rambachan et al. (2020); Rambachan and Roth (2019).

[18] Bollinger and Hotchkiss (2003) find empirical support for Lazear (1998)'s hypothesis using data from baseball players. Our paper focuses on a broader class of professional services jobs while also focusing on the value of learning about group rather than individual quality.

[19] Sterling and Fernandez (2018) show that a reluctance to take hiring risks disproportionately hurts women.

[20] In addition, Whatley (1990) documents a similar finding by examining the racial integration of firms following WWI. Outside of firms, a larger literature shows that individuals can form more positive beliefs after being randomly exposed to diverse peers (Bagues and Roth, 2021; Rao, 2019).

[21] Crandall and Eshleman (2003), for instance, argue that subjective assessments allow for deniability in making biased assessments. Benson et al. (2021) show that women receive lower subjective evaluations despite performing well on more objective measures.

consistent with work examining non-algorithms based exploration, suggesting that our findings are not unique to the specific algorithm or setting we focus on.

Finally, our paper contributes to a growing literature on algorithmic bias and fairness.[22] While the potential for algorithms to encode historical patterns of bias has been widely recognized, this literature has largely focused on defining different notions of fairness and estimating the "cost" of fairness: that is, what are the tradeoffs when fairness goals are incorporated into the optimal algorithm, relative to when they are not?[23] We take a complementary approach: rather than focusing on the ethical value of diversity, our approach views diversity as part of exploration which, in turn, is part of optimal learning. In this way, we do not require firms to commit to any definitions of fairness, nor to specify which groups are to be included in such a notion. Finally, by focusing on firms' current behavior as a benchmark (rather than that of a theoretically optimal algorithm), we demonstrating that, relative to current practices, equity goals need not come at the expense of efficiency.

# 1 Background

## 1.1 Setting

We focus on recruiting for high-skilled, professional services positions, a sector that has seen substantial wage and employment growth in the past two decades (BLS, 2019). At the same time, this sector has attracted criticism for its perceived lack of diversity: female, Black, and Hispanic applicants are substantially under-represented relative to their overall shares of the workforce (Pew, 2018). This concern is acute enough that companies such as Microsoft, Oracle, Allstate, Dell, JP Morgan Chase, and Citigroup offer scholarships and internship opportunities targeted toward increasing recruiting, retention, and promotion of those from low-income and historically under-represented groups.[24] However, despite these efforts, organizations routinely struggle to expand the demographic diversity of their workforce—and to retain and promote those workers—particularly in technical positions (Jackson, 2020; Castilla, 2008; Athey et al., 2000).

Our data come from a Fortune 500 company in the United States that hires workers in several job families spanning business and data analytics. All of these positions require a bachelor's degree,

[22] Surveys of algorithmic fairness, see Bakalar et al. (2021); Barocas and Selbst (2016); Corbett-Davies and Goel (2018); Cowgill and Tucker (2019). For a discussion of broader notions of algorithmic fairness, see Kasy and Abebe (2020); Kleinberg et al. (2016).

[23] For instance, Dwork et al. (2011) study fairness in classification, where the goal is to maintain utility while preventing discrimination based on group membership and Schumann et al. (2020) provide regret bounds on a contextual multi-armed bandit that accommodates two definitions of fairness; equal group probability and proportional parity. A more recent literature considers how regulators may achieve fairness and other goals when algorithmic designers may be strategic: Blattner et al. (2021), for instance, formalize the tension between explainability and accuracy.

[24] For instance, see here for a list of internship opportunities focused on minority applicants. JP Morgan Chase created Launching Leaders and Citigroup offers the HSF/Citigroup Fellows Award.

with a preference for candidates graduating with a STEM major, a master's degree, and, often, experience with programming in Python, R or SQL. Like other firms in its sector, our data provider faces challenges in identifying and hiring applicants from under-represented groups. As described in Table 1, most applicants in our data are male (68%), Asian (58%), or White (29%). Black and Hispanic candidates comprise 13% of all applications, but under 5% of hires. Women, meanwhile, make up 33% of applicants and 34% of hires.

In our setting, initial interview decisions are a crucial part of the hiring process. Openings for professional services roles are often inundated with applications: our firm receives approximately 200 applications for each worker it hires. Interview slots are scarce: because they are conducted by current employees who are diverted from other types of productive work, firms are extremely selective when deciding which of these applicants to interview: our firm rejects 95% of applicants prior to interviewing them.

Securing an initial interview therefore represents a key barrier for applicants seeking access to job opportunities. Moreover, because initial decisions are often made quickly on the basis of relatively little information, recruiters may easily make mistakes by choosing to interview candidates who turn out to be weak, while passing over candidates who would have been strong. Finally, in addition to mattering for firm productivity, mistakes at the interview stage may particularly restrict access to economic opportunity. When decisions need to be made quickly, human recruiters may rely on heuristics that overlook talented individuals who do not fit traditional models of success (Friedman and Laurison, 2019; Rivera, 2015).

In light of these issues, we believe that it is particularly important to understand whether algorithmic tools can be used to improve decisions at the critical initial screening stage.

### 1.2 Applicant quality

In our paper, we focus on how firms can improve their interview decisions, as measured by hiring rates—that is, whether they are able to efficiently identify applicants who meet the firm's own hiring criteria. We focus on this margin because it is empirically important for our firm, it is representative of commercially available hiring ML, and because we have enough data on interview outcomes (hiring or not) to train ML models to predict this outcome.

A key challenge that our firm faces is being able to hire qualified workers to meet its labor demands; yet even after rejecting 95% of candidates in deciding whom to interview, 90% of interviews do not result in a hire. These interviews are moreover costly because they divert high-skill current employees from other productive tasks (Kuhn and Yu, 2019). This suggests that there is scope to improve interview practices by either extending interview opportunities to a more appropriate set of candidates, or reducing the number of interviews needed to achieve current hiring outcomes.

Of course, in deciding whom to interview, firms may also care about other objectives: they may look for applicants who have the potential to become superstars—either as individuals, or in their ability to manage and work in teams—or they may avoid applicants who are more likely to become toxic employees (Benson et al., 2019; Deming, 2017; Housman and Minor, 2015; Reagans and Zuckerman, 2001; Schumann et al., 2019). In these cases, a more appropriate measure of applicant quality would be based on the job performance. Unfortunately, we do not have enough data to train an ML model to reliably predict these types of outcomes. In Section 4.2.6, however, we are able to examine the correlation between ML scores and two measures of on the job performance, which we observe for a small subset of hired workers. This analysis provides noisy but suggestive evidence that ML models trained to maximize hiring rates are also positively related to performance ratings and promotion rates.

Finally, we note that all of the quality measures we consider—hiring rates, performance ratings, and promotion rates—are based on the discretion of managers and therefore potentially subject to various types of evaluation and mentoring biases (Rivera and Tilcsik, 2019; Quadlin, 2018; Castilla, 2011). Managers, for example, may be biased against minority applicants or they may have a preference for diversity regardless of quality. Without a truly "objective" measure of quality, we interpret our results, especially those related to hiring yield, as asking whether ML tools can improve firm decisions, as measured by its own revealed preference metrics (whether it chooses to hire, promote, or rate someone highly).

# 2 Conceptual Framework

## 2.1 Resume Screening: Contextual Bandit Approach

### 2.1.1 Model Setup

We model the firm's interview decision as a contextual bandit problem. Decision rules for standard and contextual bandits have been well studied in the computer science and statistics literatures (cf. Bubeck and Cesa-Bianchi, 2012). In economics, bandit models have been applied to study doctor decision-making, ad placement, recommendation systems, and adaptive experimental design (Thompson, 1933; Berry, 2006; Currie and MacLeod, 2020; Kasy and Sautmann, 2019; Dimakopoulou et al., 2018b; Bergemann and Valimaki, 2006). Our set up adapts Li et al. (2017) into the context of interview screening.

Each period $t$, the firm sees a set of job applicants indexed by $i$ and for each of them must choose between one of two actions or "arms": interview or not, $I_{it} \in \{0, 1\}$. The firm would only like to interview candidates it would hire, so a measure of an applicant's quality is her "hiring potential": $H_{it} \in \{0, 1\}$ where $H_{it} = 1$ if an applicant would be hired if she were interviewed. Regardlfirms may also care about otheress, the firm pays a cost, $c_t$, per interview, which can vary exogenously

with time to reflect the number of interview slots or other constraints in a given period. The firm's "reward" for each applicant is therefore given by:

$$Y_{it} = \begin{cases} H_{it} - c_t \text{ if } I_{it} = 1 \\ 0 \text{ if } I_{it} = 0 \end{cases}$$

After each period $t$, the firm observes the reward associated with its chosen actions.

So far, this set up follows a standard multi-armed bandit (MAB) approach, in which the relationship between action and reward is invariant. The optimal solution to MAB problems is characterized by Gittins and Jones (1979) and Lai and Robbins (1985). Our application departs from this set up because, for each applicant $i$ in period $t$, the firm also observes a vector of demographic, education, and work history information, denoted by $X'_{it}$. These variables provide "context" that can inform the expected returns to interviewing a candidate.

In general, the solutions to MABs are complicated by the presence of context. If firms could perfectly observe how all potential covariates $X'_{it}$ relate to hiring quality $H_{it}$, then it would simply interview the applicants whose quality is predicted to be greater than their cost of interviewing. In practice, however, the dimension of the context space makes estimating this relationship difficult, preventing firms from implementing the ideal decision rule.

To make our model more tractable, we follow Li et al. (2010, 2017) and assume that the relationship between context and rewards follows a generalized linear form. In particular, we write $E[H_{it}|X'_{it}] = \mu(X'_{it}\theta^*_t)$, where $\mu : \mathbb{R} \to \mathbb{R}$ is a link function and $\theta^*_t$ is an unobserved vector describing the true predictive relationship between covariates $X'_{it}$ and hiring potential $H_{it}$. We allow for the possibility that this relationship may change over time, to reflect potential changes in either the demand or supply of skills.

We express a firm's interview decision for applicant $i$ at time $t$:

$$I_{it} = \mathbb{I}(s_t(X'_{it}) > c_t) \tag{1}$$

where $s_t(X'_{it})$ can be thought of as a score measuring the value the firm places on a candidate with covariates $X'_{it}$ at time $t$. This score can reflect factors such as the firm's beliefs about a candidate's hiring potential and can be a function of both the candidate's covariates $X'_{it}$, as well as the data available to the firm at time $t$. As is standard in the literature on bandit problems, we express the firm's objective function in terms of choosing an interview policy $I$ to minimize expected cumulative "regret," the difference in rewards between the best choice at a given time and the firm's actual choice. The firm's goal is to identify a scoring function $s_t(X'_{it})$ that leads it to identify and interview applicants with $H_{it} = 1$ as often as possible.

### 2.1.2 "Greedy" solutions

Before turning toward more advanced algorithms, we first note that one class of potential solutions to bandit problems are given by so-called "greedy" or "exploitation only" algorithms. These types of algorithms ignore the dynamic learning problem at hand and simply choose the arm with the highest expected reward in the present. In our case, a firm following a greedy solution would form its best guess of $\theta_t^*$ given the training data it has available, and then score candidates on their basis of their expected hiring likelihood, so that Equation (1) becomes: $I_{it}^{\text{Greedy}} = \mathbb{I}(\mu(X_{it}'\hat{\theta}_t) > c_t)$.

Supervised learning algorithms are designed to implement precisely this type of greedy solution. That is, a standard supervised learning model forms an expectation of hiring likelihood using the data it has available at time $t$ and selects applicants based solely on this measure. If this model is calculated once at $t = 0$ and invariant thereafter, this decision rule would correspond to our "static SL" model; if it is re-estimated in each period to incorporate new data, then this is equivalent to our "updating SL" model.

### 2.1.3 Exploration-based solutions

It is widely known, however, that greedy algorithms are inefficient solutions to contextual bandit problems because they do not factor the ex post value of learning into their ex ante selection decisions (Dimakopoulou et al., 2018b).[25]

While there is in general no generic optimal strategy for contextual bandits, an emerging literature in computer science focuses on developing a range of computationally tractable algorithms that work well in practice.[26] For example, recently proposed contextual bandit algorithms include UCB Auer (2002), Thompson Sampling (Agrawal and Goyal (2013)), and LinUCB (Li et al. (2010)).[27] All of these algorithms share the feature that they will sometimes select candidates who do not have the highest expected quality, but whose interview outcomes could improve the estimates of hiring potential in the future.

We follow Li et al. (2017) and implement a generalized linear model version of the UCB algorithm, which assumes that $E[H_{it}|X_{it}']$ follows the functional form given by $\mu(X_{it}'\theta_t^*)$, as discussed above.[28]

[25] Bastani et al. (2019) show that exploration-free greedy algorithms (such as supervised learning) are generally sub-optimal.

[26] In particular, the best choice of algorithm for a given situation will depend on the number of possible actions and contexts, as well as on assumptions regarding the parametric form relating context to reward.

[27] In addition, see Agrawal and Goyal (2013), and Bastani and Bayati (2019). Furthermore, the existing literature has provided regret bounds—e.g., the general bounds of Russo and Roy (2015), as well as the bounds of Rigollet and Zeevi (2010) and Slivkins (2014) in the case of non-parametric function of arm rewards—and has demonstrated several successful applications areas of application—e.g., news article recommendations (Li et al. (2010)) or mobile health (Lei et al. (2017)). For more general scenarios with partially observed feedback, see Rejwan and Mansour (2019) and Bechavod et al. (2020). For more on fairness and bandits, see Joseph et al. (2016) and Joseph et al. (2017).

[28] Li et al. (2017) generalizes the classic general LinUCB algorithm for nonlinear relationship between context and reward. Theorem 2 of that paper gives the regret bound and Equation 6 shows the algorithm implementation we follow.

Given this assumption, Li et al. (2017) shows that the optimal solution assigns a candidate to the arm (interview or not) with the highest combined expected reward and "exploration bonus."[29]

Exploration bonuses are assigned based on the principle of "optimism in the face of uncertainty": the more uncertain the algorithm is about the quality of a candidate based on her covariates, the higher the bonus she receives. This approach encourages the algorithm to focus on reducing uncertainty, and algorithms based on this UCB approach have been shown to be asymptotically efficient in terms of reducing expected regret (Lai and Robbins, 1985; Li et al., 2017; Abbasi-Yadkori et al., 2019). We discuss the specifics of our implementation and discuss theoretical predictions in the next section.

# 3 Algorithm Construction

## 3.1 Data

We have data on 88,666 job applications from January 2016 to April 2019, as described in Table 1. We divide this sample up into two periods, the first consisting of the 48,719 applicants that arrive before 2018 (2,617 of whom receive an interview), and the second consisting of the 39,947 applications that arrive in 2018-2019 (2,275 of whom are interviewed). We begin by training a supervised learning model on the 2016-2017 period and testing its out-of-sample validity on the 2018-2019 data. This serves as our "static" supervised learning baseline. In addition, we build an "updating" supervised learning model, as well as a contextual bandit UCB model. Both of these models begin with the 2016-2017 trained baseline model but continue to train and learn on the 2018-2019 sample. We build our initial "training" dataset using the earliest years of our sample (rather than taking a random sample) in order to more closely approximate actual applications of hiring ML, in which firms would likely use historical data to train a model that is then applied prospectively.

### 3.1.1 Input Features

We have information on applicants' educational background, work experience, referral status, basic demographics, as well as the type of position to which they applied. Appendix Table A.1 provides a list of these raw variables, as well as some summary statistics. We have self-reported race (White, Asian, Hispanic, Black, not disclosed and other), gender, veteran status, community college experience, associate, bachelor, PhD, JD or other advanced degree, number of unique degrees, quantitative background (defined having a degree in a science/social science field), business background, internship experience, service sector experience, work history at a Fortune 500 company,

[29] In particular Li et al. (2017) show that the GLM-UCB algorithm has a regret bound of order $\tilde{\mathcal{O}}(d\sqrt{T})$, where $d$ is the number of covariates and $T$ is the number of rounds.

and education at elite (Top 50 ranked) US or non-US educational institution. We record the geographic location of education experience at an aggregated level (India, China, Europe). We also track the job family each candidate applied to, the number of applications submitted, and the time between first and most recent application.

To transform this raw information into usable inputs for a machine learning model, we create a series of categorical and numerical variables that serve as "features" for each applicant. We standardize all non-indicator features to bring them into the same value range. Because we are interested in decision-making at the interview stage, we only use information available as of the application date as predictive features. Our final model includes 106 input features.

#### 3.1.2 Interview Outcomes

Each applicant has an indicator for whether they received an interview. Depending on the job family, anywhere from 3-10% of applicants receive an interview. Among candidates chosen to be interviewed, we observe interview ratings, whether the candidate received an offer, and whether the candidate accepted and was ultimately hired. Roughly 20% of candidates who are interviewed receive an offer and, of them, approximately 50% accept and are hired. We will focus on the final hiring outcome as our measure of an applicant's quality, keeping in mind that this is a potential outcome that is only observed for applicants who are actually interviewed.

Finally, for 180 workers who are hired and have been employed for at least 6 months, we observe a measure of performance ratings on the job. Because this number is too small to train a model on, we will use these data to examine the relationship between maximizing hiring likelihood and on the job performance.

### 3.2 Models

Here we describe how we construct three distinct interview policies based on static and updating supervised learning, and contextual bandit UCB. For simplicity, we will sometimes write $I^{ML}$ to refer to the interview policy of any of these ML models.

#### 3.2.1 Static Supervised Learning ("$SSL$" or "static SL")

We first use a standard supervised learning approach to predict an applicant's likelihood of being hired, conditional on being interviewed. At any given time $t$ (which indexes an application round that we observe in the testing period) applicants $i$ are selected according to the following interview policy, based on Equation (1) of our conceptual framework:

$$I_{it}^{SSL} = \mathbb{I}(s_0^{SSL}(X'_{it}) > c_t), \text{ where } s_0^{SSL}(X'_{it}) = \hat{E}[H_{it}|X'_{it}; D_0] \tag{2}$$

Here, we emphasize that the firm's estimate of hiring potential at time $t$ depends on the training data that it has available at the time. In the static SL model, we write this data as $D_0$ to emphasize that it is determined at time $t = 0$ and is not subsequently updated. Using this data, we form an estimate of $s^{SSL}(X'_{it})$ using a L1-regularized logistic regression (LASSO), fitted using three-fold cross validation.[30]

We evaluate out-of-sample performance on randomly-selected balanced samples from our 2018-2019 "testing" period after training our models on the 2016-2017 sample. Appendix Figure A.1 plots the receiver operating characteristic (ROC) curve and its associated AUC, or area under the curve. Our model has an AUC of 0.64, meaning that it will rank an interviewed applicant who is hired ahead of an interviewed but not hired applicant 64 percent of the time.[31]

We note that training on candidates selected by human recruiters may lead to biased predictions because of selection on unobservables. There are a growing set of advanced ML tools that seek to correct for training-sample selection.[32] While promising, testing these approaches is outside of the scope of this paper, and we are not aware of any commercially-available hiring ML that does attempt to correct for sample selection.[33] In Section 4.2.4, we use an IV approach to provide evidence that selection on unobservables does not appear to be a large concern in our data, likely because we have access to the same resume variables that human recruiters observe.

We also note that another approach that is commonly used by commercial vendors of hiring algorithms is to set $H_{it} = 0$ for all applicants who are not interviewed. We choose not to follow this approach as it runs counter to our view that $H_{it}$ should be thought of as a potential outcome and because Rambachan and Roth (2019) show that such an approach often leads to algorithms that are more biased against racial minorities.

In general, we emphasize that the ML models we build should not be thought of as an optimal ML model in either its design or its performance but as an example of what could be feasibly achieved by most firms able to organize their administrative records into a modest training dataset, with a standard set of resume-level input features, using a standard ML toolkit.[34]

[30] Following best practices as described in Kaebling (2019), we randomly subsample our training data to create a balanced sample, half of whom are hired and half of whom are not hired. Our results are also robust to using an ensemble logit lasso and random forest approach, which delivers slightly higher predictive validity (0.67 vs. 0.64 AUC). In our paper, we choose to stick to the simple logit model for transparency and to ensure consistency with our UCB model, which is based on Li et al. (2017)'s implementation that uses a logit model to predict expected quality.

[31] The AUC is a standard measure of predictive performance that quantifies the tradeoff between a model's true positive rate and its false positive rate. Formally, the AUC is defined as $\Pr(s(X'_{it}) > s(X'_{jt}) | H_{it} = 1, H_{jt} = 0)$. We also plot the confusion matrix in Appendix Figure A.2, which further breaks down the model's classification performance.

[32] See, for example, Dimakopoulou et al. (2018a), Dimakopoulou et al. (2018b) which discuss doubly robust estimators to remove sample selection and Si et al. (2020).

[33] Raghavan et al. (2019) surveys the methods of commercially available hiring tools and finds that the vast majority of products marketed as "artificial intelligence" do not use any ML tools at all, and that the few that do simply predict performance using a static training dataset.

[34] We would ideally like to compare our AUC to those of commercial providers, but Raghavan et al. (2019) reports that no firms currently provide information on the validation of their models.

### 3.2.2 Updating Supervised Learning ("$USL$" or "updating SL")

Our second model presents a variant of the static SL model in which we begin with the same baseline model as the static SL, but actively update the model as it makes decisions during the 2018-2019 period. That is, we start with a model that is trained on the 2016-2017 data, allow it to make selection decisions in the 2018-2019 period, but then also update its training data to reflect the outcomes of these newly selected applicants.[35] Once the training data is updated, we retrain the model and use its updated predictions to make selection decisions in the next round. At any given point $t$, the updating SL's interview decision for applicant $i$ is given by:

$$I_{it}^{USL} = \mathbb{I}(s_t^{USL}(X'_{it}) > c_t), \text{ where } s_t^{USL}(X'_{it}) = \hat{E}[H_{it}|X'_{it}; D_t^{USL}]. \tag{3}$$

Here, $D_t^{USL}$ is the training data available to the algorithm at time $t$.

It is important to emphasize that we can only update the model's training data with *observed* outcomes for the set of applicants selected in the previous period: that is, $D_{t+1}^{USL} = D_t^{USL} \cup (I_t^{USL} \cap I_t)$. Because we cannot observe hiring outcomes for applicants who are not interviewed in practice, we can only update our data with outcomes for applicants selected by both the model and by actual human recruiters. This may impact the degree to which the updating SL model can learn about the quality of the applicants it selects, relative to a world in which hiring potential is fully observed for all applicants and we discuss this in more detail shortly, in Section 3.2.4.

### 3.2.3 Upper Confidence Bound ("UCB")

As discussed in Section 2.1, we implement a UCB-GLM algorithm as described in Li et al. (2017). We calculate predicted quality $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}]$ using a regularized logistic regression (Cortes, 2019). At time $t = 0$ of the testing sample, our UCB and SL models share the same predicted quality estimate, which is based on the baseline model trained on the 2016-2017 sample. Our UCB model, however, makes interview decisions for applicant $i$ in period $t$ based on a different scoring function:

$$I_{it}^{UCB} = \mathbb{I}(s_t^{UCB}(X'_{it}) > c_t), \text{ where } s_t^{UCB}(X'_{it}) = \hat{E}[H_{it}|X'_{it}; D_t^{UCB}] + \alpha B(X'_{it}; D_t^{UCB}). \tag{4}$$

In Equation (4), the scoring function $s_t^{UCB}(X'_{it})$ is a combination of the algorithm's expectations of an applicant's quality based on its training data and an exploration bonus that varies with an applicant's covariates $X'_{it}$. Following the model described in Section 2.1, we assume that $E[H_{it}|X'_{it}]$ can be expressed as a generalized linear function $\mu(X'_{it}\theta_t^*)$. In our specific implementation, we

[35] Specifically, we divide the 2018-2019 data up into "rounds" of 100 applicants. After each round, we take the applicants the model has selected and update its training data with the outcomes of these applicants, for the subset of applicants for whom we observe actual hiring outcomes.

assume that $\mu$ is a logistic function and, in each round $t$, estimate $\theta_t^*$ using a maximum likelihood estimator so that $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}] = \mu(X'_{it}\hat{\theta}_t^{UCB})$. Next, we calculate the exploration bonus as

$$B(X'_{it}; D_t^{UCB}) = \sqrt{(X_{it} - \bar{X}_t)' V_t^{-1} (X_{it} - \bar{X}_t)}, \text{ where } V_t = \sum_{j \in D_t^{UCB}} (X_{jt} - \bar{X}_t)(X_{jt} - \bar{X}_t)'. \quad (5)$$

Intuitively, Equation (4) breaks down the value of an action into an exploitation component and an exploration component. In any given period, a strategy that prioritizes exploitation would choose to interview a candidate on the basis of her expected hiring potential: this is encapsulated in the first term, $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}]$. In contrast, a strategy that prioritizes exploration would choose to interview a candidate on the basis of the distinctiveness of her covariates; this is encapsulated in the second term, $B(X'_{it}; D_t^{UCB})$, which shows that applicants receive higher bonuses if their covariates deviate from the mean in the population $(X_{it} - \bar{X}_t)$, especially for variables $X'_{it}$ that generally have little variance, as seen in the training data (weighted by the precision matrix $V_t^{-1}$). To balance exploitation and exploration, Equation (4) combines these two terms so that candidates are judged not only on their mean expected quality, but rather on the mean plus standard error of their estimated quality—hence the term upper confidence bound. Li et al. (2017) shows that following such a strategy asymptotically minimizes regret in our setting.

As with the updating SL model, we update the UCB model's training data with the outcomes of applicants it has selected—$D_{t+1}^{UCB} = D_t^{UCB} \cup (I_t^{UCB} \cap I_t)$. Based on these new training data, the UCB algorithm updates both its beliefs about hiring potential and the bonuses it assigns. As was the case with the updating SL model, we can only add applicants who are selected by the model and also interviewed in practice. We now turn to the implications of this sample selection.

#### 3.2.4 Feasible versus Live Model Implementation

In a live implementation, each algorithm would select which applicants to interview, and the interview outcomes for these applicants would be recorded. Our retrospective analysis is limited by the fact that we only observe interview outcomes for applicants who were actually interviewed (as chosen by the firm's human screeners) and, as such, we are only able to update our USL and UCB models with outcomes for candidates in the intersection of human and algorithmic decision making. Here, we discuss how the actual interpretation of our models—which we term "feasible" USL or UCB—may differ from a live implementation.

For concreteness, suppose that in period 1 of our analysis, the UCB model wants to select 50 Black applicants with humanities degrees in order to explore the applicant space. But, in practice, only 5 such applicants are actually interviewed. In our feasible implementation, we would only be able to update the UCB's training data with the outcomes of these 5 applicants, whereas in a live implementation, we would be able to update with outcomes for all 50 UCB-selected candidates.

We first consider the case in which there is no selection on unobservables on the part of the human recruiters. In this case, the feasible UCB model's estimates of the expected quality of Black humanities majors next period would be the same as the live UCB's estimates, because, in expectation, the quality of the 5 applicants it was able to learn about is the same as the quality of the 50 applicants it wanted to learn about. That said, the feasible UCB model would have considerably more uncertainty about the quality of this population relative to a live UCB, because its updated estimates are based on 5 applicants rather than 50. This uncertainty would show up in the next period via the exploration bonus term of Equation (4): even though it has the same beliefs about quality, the feasible UCB would likely select more Black humanities majors in the next period relative to a live UCB because it was not able to learn as much, due to limited updating. In this way, selection on observables should be thought of as slowing down the process of learning for our UCB (and USL) models. In the limit, the feasible and live UCB (and USL) models should converge to the same beliefs regarding the quality of the applicants they observe. This would translate into the same actions because, with a large enough sample, there would be little uncertainty driving exploration bonuses.[36]

Next, we consider the case in which human recruiters screen on variables that are unobserved to us. A particularly concerning version of this type of selection occurs if human recruiters positively screen on unobservables, so that $E[H_{it}|X'_{it}, I = 1] > E[H_{it}|X'_{it}]$. In this case, the 5 Black humanities majors that are actually selected by human interviewers will tend to be higher quality than the 50 Black humanities majors that the UCB model wanted to select. This means that our feasible UCB model will be too optimistic about the quality of this population, relative to a live UCB model that would learn about the quality of all 50 applicants. In the next period, the feasible UCB model will select more Black humanities majors than a live implementation, both because uncertainty for these applicants remains higher and because selection on unobservables induces upwardly biased beliefs. This latter bias can lead the feasible and live UCB models to select different applicants in the long run. Specifically, our approach may select too many applicants from groups whose weaker members are screened out of the model's training data by human recruiters.

In Section 4.2.4, we discuss the possibility of selection on unobservables in more detail, and provide IV-based evidence that human recruiters do not appear to be selecting on unobservables. In addition, Section 5.1 shows simulation results in which we are able to observe outcomes for all ML-selected candidates. This allows us to explore the learning behavior of our USL and UCB models in various settings that more closely approximate a live implementation. Finally, in Section

[36]Formally, the distinction between the feasible and live versions of our ML models is related to regression in which outcomes are missing at random conditional on unobservables. Under the assumption of no selection on unobservables, common support, and well-specification of the regression function (in our case, the logit), the feasible and live versions of our models should both be consistent estimators of the underlying parameter $\theta^*$ linking covariates with hiring outcomes: $E[H_{it}|X'_{it}] = \mu(X'_{it}\theta^*_t)$ (Wang et al., 2010; Robins et al., 1995). In a finite sample, of course, the point estimates of the feasible and live models may differ.

4.2.3 we provide evidence that there is common support amongst the applicants chosen by our ML models and who is chosen by human recruiters.

## 3.3 Comparison of SL and UCB models

The use of static SL, updating SL, and UCB models can potentially lead to a variety of differences in the composition and quality of selected applicants in both the short and long term. Before describing our empirical results, we focus on the theoretical differences between these models in terms of both quality and demographic diversity.

### 3.3.1 Quality of Selected Applicants

As discussed in Section 2, theory predicts that while models that focus on exploration may end up selecting applicants with lower expected hiring potential in the short run (relative to SL models), they should eventually minimize regret via more efficient learning (Li et al., 2017; Dimakopoulou et al., 2018b).

In the long run, the quality of selection decisions made by the UCB and SL algorithms may or may not differ. One possibility is that, despite selecting different candidates in earlier periods, both algorithms eventually observe enough examples to arrive at similar estimates of quality for all applicants: $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}] = \hat{E}[H_{it}|X'_{it}; D_t^{USL}]$ for sufficiently large $t$. If this were the case, then both UCB and SL models will make the same interview decisions in the long run.

It is also possible, however, for the two types of algorithms to make persistently different interview decisions. To see this, suppose that we observe only one covariate, $X \in \{0, 1\}$, designating group membership, and $E[H|X = 0] = 0.2$ while $E[H|X = 1] = 0.4$. Suppose that the cost of interviewing is 0.3 so that the firm would like to interview all $X = 1$ candidates and no $X = 0$ candidates. Suppose, however, that the firm's initial training data $D_0$ consists of three candidates total: two $X = 0$ applicants with $H = 1$ and $H = 0$ and only one $X = 1$ applicant with $H = 0$. A static SL model trained on these data would predict $E[H|X = 0; D_0] = 0.5$ while $E[H|X = 1; D_0] = 0$ and therefore interview all $X = 0$ candidates and no $X = 1$ candidates in the next period. Moreover, because its training data is never updated, it will continue to do this no matter what outcomes are realized in the future. Meanwhile, an updating SL model would continue selecting $X = 0$ candidates until it encounters a sufficient number with $H = 0$ such that $E[H|X = 0, D_t^{USL}] < 0.3$. However, because $E[H|X = 1; D_0] = 0$, it will never select any $X = 1$ candidates and therefore never have the opportunity to learn about their quality.

By contrast, a UCB based approach would evaluate candidates on the basis of both their expected quality and their statistical distinctiveness. Thus, even though $X = 1$ candidates begin with an expected quality of 0, they would receive an exploration bonus of $\sqrt{2/3}$, meaning that the UCB

model would choose to interview $X = 1$ candidates next period, increasing its chances of learning about their true expected quality, 0.4.

While a UCB based approach will theoretically out-perform SL models in the long run, the quality difference we will observe in practice is ambiguous and captures both the long term benefits of learning and the short term costs of exploration. This tradeoff will also depend on the specifics of our empirical setting. In particular, if quality is not evolving and there is relatively rich initial training data, SL models may perform as well as if not better than UCB models because the value of exploration will be limited. If, however, the training data were sparse or if the predictive relation between context and rewards evolves over time, then the value of exploration is likely to be greater.

### 3.3.2 Diversity of Selected Applicants

All of our models are designed to maximize applicant quality, as defined by hiring rates, and have no additional preferences related to diversity. Any differences in the demographics of the candidates they choose to select will be based on the predictive relation between demographic variables and hiring outcomes, and will depend on the specifics of our empirical set up.

As can be seen in Equation (5), contextual bandit UCB algorithms are designed to favor candidates with distinctive covariates, because this helps the algorithm learn more about the relationship between context (e.g. applicant covariates) and rewards (e.g. hiring outcomes). This suggests that a UCB model would—at least in the short run—select more applicants from demographic groups that are under-represented in its training data, relative to SL models. This tendency to favor demographic minorities, however, will depend on the extent to which demographic minorities are also minorities along other dimensions such as educational background and work history. Asian applicants, for example, make up the majority of our applicant sample and so would receive low exploration bonuses on the basis of race alone; however, they are also more likely to have non-traditional work histories or have gone to smaller international colleges, factors that make them appear more distinctive to the UCB model. In our UCB implementation, we place greater exploration weight on applicants who are distinctive on dimensions in which candidates in the training data have been relatively homogenous.

As discussed above, long run differences in selection patterns between SL and UCB models are driven by differences in beliefs. That is, even if the UCB model initially selects more demographic minorities because it assigns them larger exploration bonuses, this would impact long run differences in diversity between UCB and SL models only insofar as it generates differences training data that lead to differences in beliefs: $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}]$ vs. $\hat{E}[H_{it}|X'_{it}; D_t^{USL}]$.

While we (eventually) expect UCB models to outperform SL models in terms of maximizing applicant quality, it is unclear this would result in more diversity in the long run. For example, it is possible for exploration to work against minority applicants: if a UCB model initially selects

more Hispanic applicants in order to explore, but discovers that these additional candidates are particularly weak, then it may end up with worse beliefs about Hispanic applicants than the SL model. In general, the impact of adopting SL vs. UCB models on diversity will depend on the nature of the training data that the models start with, and how applicant covariates are actually related to hiring outcomes. In Section 5.1, we conduct various simulations and document cases in which UCB models can increase or decrease diversity.

In Section 4.3, we argue that these results are broadly consistent with other papers suggesting that (non algorithmic) policies nudging firms toward increased minority hiring can result in persistent increases in representation (Miller, 2017). Together, these suggest that, in practice, many firms may have inefficiently low beliefs about the quality of minority candidates.

# 4 Main Results

We now turn to discussing our main results. For notational simplicity, we suppress the subscripts for applicant $i$ at time $t$ for the remainder of the paper except as needed for clarity.

## 4.1 Impacts on Diversity of Interviewed Applicants

We begin by assessing the impact of each policy on the diversity of candidates selected for an interview in our test sample. This is done by comparing $E[X|I = 1]$, $E[X|I^{SSL} = 1]$, $E[X|I^{USL} = 1]$, and $E[X|I^{UCB} = 1]$, for various demographic measures $X$, where we choose to interview the same number of people as the actual recruiter. We observe demographic covariates such as race and gender for all applicants, regardless of their interview status.

We focus on the racial composition of selected applicants. At baseline, 54% of applicants in our test sample are Asian, 25% are White, 8% are Black, and 4% are Hispanic. Panel A of Figure 1 shows that, from this pool, human recruiters select a similar proportion of Asian and Hispanic applicants (57% and 4%, respectively), but relatively more White and fewer Black applicants (34% and 5%, respectively). Panel B shows that our static SL model—designed to mimic the approach most commonly used by practitioners—reduces the combined share of selected applicants who are Black or Hispanic from 10% (under humans) to less than 3%. This is accompanied by an increase in the proportion of interviewed candidates who are White (from 34% to 43%) and a slight decrease in the share who are Asian (57% to 55%). In Panel C, we show that the updating SL model follows a similar pattern: Black and Hispanic representation falls from 10% to under 5%, White representation increases more modestly from 34% to 42%, and Asian representation stays largely constant. In contrast, Panel D shows that the UCB model increases the Black share of selected applicants from 5% to 15%, and the Hispanic share from 4% to 9%. The White share stays constant, while the Asian share falls from 57% to 44%.

Appendix Figure A.3 plots the same set of results for gender. Panel A shows that 65% of interviewed applicants are men and 35% are women; this is largely similar to the gender composition of the overall applicant pool. Unlike the case of race, all of our ML models are aligned in selecting more women than human recruiters, increasing their representation to 42% (static SL), 40% (updating SL), or 48% (UCB).

Next, we explore why our UCB model selects more Black and Hispanic applicants. While there is no preference for demographic diversity built into our models, Panel A of Appendix Figure A.5 shows that Black and Hispanic applicants receive larger exploration bonuses on average. This reflects both direct differences in population size by race, as well as indirect differences arising from the correlation between race and other variables that also factor into bonus calculations. Appendix Figure A.6 plots the proportion of the total variation in exploration bonuses that can be attributed to different categories of applicant covariates. We find that the greatest driver of variation in exploration bonuses is an applicant's work history variables; Black and Hispanic applicants also receive higher bonuses because they tend to have more distinctive work experiences.

A key question relates to how these selection patterns evolve over time. In particular, one may also be concerned that the UCB model engages in exploration by selecting demographically diverse candidates initially, but then "learns" that these candidates have lower hiring potential, $H$, and selects fewer of them going forward; in this case, the gains we document would erode over time. Appendix Figure A.4 shows that this does not appear to be the case: the proportion of Black and Hispanic candidates selected stays roughly constant over time. This suggests that, in our sample, hiring outcomes for minority applicants are high enough that our models do not update downward upon selecting them. As discussed in Section 3.2.4, one may be concerned that the stability of our demographic results represents a failure to learn due to biases arising from sample selection. In Selection 4.2.4, we provide IV-based evidence against this possibility driving our results.

## 4.2 Impacts on Quality of Interviewed Applicants

### 4.2.1 Overview

Next, we ask if and to what extent the gains in diversity made by the UCB model come at the cost of quality, as measured by an applicant's likelihood of actually being hired. To assess this, we would ideally like to compare the average hiring likelihoods of applicants selected by each of the ML models to the actual hiring likelihoods of those selected by human recruiters: $E[X|I = 1]$, $E[X|I^{SSL} = 1]$, $E[X|I^{USL} = 1]$, and $E[X|I^{UCB} = 1]$.

Unlike demographics, however, an applicant's hiring potential $H$ is an outcome that is only observed when applicants are actually interviewed. We therefore cannot directly observe hiring potential for applicants selected by either algorithm, but not by the human reviewer. To address this, we take three complementary approaches, described in turn below. Across all three approaches,

we find evidence that both SL and UCB models would select applicants with greater hiring potential, relative to human screening.

### 4.2.2 Interviewed sample

Our first approach compares the quality of applicants selected by our algorithms among the sample of applicants who are interviewed, for whom we directly observe hiring outcomes.

To compare our model's preferences to that of humans, we train an additional model to predict an applicant's likelihood of being selected for an interview by a human recruiter. This is necessary because all applicants in this interviewed sample are—by definition—selected by human recruiters, so we need to generate additional variation in human preferences. Specifically, we generate a model of $E[I|X]$ where $I \in \{0, 1\}$ are realized human interview outcomes, using same ensemble approach described in Section 3.2.1.[37]. This model allows us to order interviewed applicants in terms of their "human score," $s^H$, in addition to their algorithmic scores, $s^{SSL}$, $s^{USL}$, and $s^{UCB}$.[38] Appendix Figure A.7 plots the ROC associated with this model. Our model ranks a randomly chosen interviewed applicant ahead of a randomly chosen applicant who is not interviewed 76% of the time.[39]

Figure 2 plots a binned scatterplot depicting the relationship between algorithm scores and hiring outcomes among the set of interviewed applicants; each dot represents the average hiring outcome for applicants in a given scoring ventile. Among those who are interviewed, applicants' human scores are uninformative about their hiring likelihood; if anything this relationship is slightly negative.[40]

In contrast, all ML scores have a statistically significant, positive relation between algorithmic priority selection scores and an applicant's (out of sample) likelihood of being hired.[41]

Table 2 examines how these differences in scores translate into differences in interview policies. To do so, we consider "interview" strategies that select the top 25, 50, or 75% of applicants as ranked by each model; we then examine how often these policies agree on whom to select, and which policy performs better when they disagree. Panel A compares the updating SL model to the human interview model and shows that the human model performs substantially worse in terms of predicting hiring likelihood when the models disagree: only 5-8% of candidates favored by the

[37] The only methodological difference between this model and our baseline static SL model is that, because we are trying to predict interview outcomes as opposed to hiring outcomes conditional on interview, our training sample consists of all applicants in the training period, rather than only those who are interviewed.

[38] Our IV results, discussed later, do not require us to model human interview practices.

[39] Although a "good" AUC number is heavily context specific, a general rule of thumb is that models with an AUC in the range of $0.75 - 0.85$ have acceptable discriminative properties, depending on the specific context and shape of the curve (Fischer et al., 2013).

[40] This weak relation between human preferences and outcomes is consistent with existing work documenting that humans often have incorrect perceptions of worker quality. For instance, Hoffman et al. (2018) find that firms see worse hiring outcomes when humans make exceptions to algorithmic suggestions. In a study of personnel assessment, Yu and Kuncel (n.d.) find that the scores of expert human resource managers were at best very weakly related to on the job performance.

[41] Appendix Table A.2 shows these results as regressions to test whether the relationships are statistically significant.

human model are eventually hired, compared with 17-20% of candidates favored by the updating SL model. Panel B finds similar results when comparing the human model to the UCB model. Finally, Panel C shows that, despite their demographic differences, the updating SL and UCB models agree on a greater share of candidates relative to the human model, and there do not appear to be significant differences in overall hiring likelihoods when they disagree: if anything, the UCB model performs slightly better.

For consistency, Appendix Figure A.9 revisits our analysis of diversity using the same type of selection rule described in this section: specifically, picking the top 50% of candidates among the set of interviewed. Again, we find that UCB selects a substantially more diverse set of candidates than SL models.

#### 4.2.3 Full sample, assuming no selection on unobservables

A concern with our analysis on the $I = 1$ sample is that human recruiters may add value by screening out particularly poor candidates so that they are never observed in the interview sample to begin with. In this case, then we may see little relation between human preferences and hiring potential among those who are interviewed, even though human preferences are highly predictive of quality in the full sample.

In this section, we estimate the average quality of *all* ML-selected applicants, $E[H|I^{ML} = 1]$. To do this, we infer hiring likelihoods for ML-selected applicants who were not actually interviewed using observed hiring outcomes from applicants with similar covariates who were interviewed, assuming no selection on unobservables: $E[H|I^{ML} = 1, X] = E[H|I^{ML} = 1, I = 1, X]$. We provide evidence for the plausibility of this assumption in Section 4.2.4.

Following Hirano et al. (2003), we write the inverse propensity weighted estimate of the unconditional mean of interest as:

$$
\begin{aligned}
E[H|I^{ML} = 1] &= \sum_X p(X|I^{ML} = 1)E[H|I^{ML} = 1, X] \\
&= \sum_X \frac{p(I^{ML} = 1|X)p(X)}{p(I^{ML} = 1)} E[H|I^{ML} = 1, X] \\
&= \frac{1}{p(I^{ML} = 1)} \sum_X p(I^{ML} = 1|X)p(X)E[H|I^{ML} = 1, X] \frac{p(X|I = 1)p(I = 1)}{p(I = 1|X)p(X)} \\
&= \frac{p(I = 1)}{p(I^{ML} = 1)} \sum_X E[H|I = 1, X] \frac{p(I^{ML} = 1|X)p(X|I = 1)}{p(I = 1|X)} \\
&\quad \text{(Assuming selection on observables)} \\
&= \frac{p(I = 1)}{p(I^{ML} = 1)} E\left[\frac{p(I^{ML} = 1|X)}{p(I = 1|X)} H|I = 1\right] \qquad (6)
\end{aligned}
$$

Equation (6) says that we can recover the mean quality of ML-selected applicants by reweighting outcomes among the human-selected interview sample, using the ratio of ML and human-interview propensity scores. For both the SL and UCB models, the ML decision rule is a deterministic function of covariates $X$, meaning that the term $p(I^{ML} = 1|X)$ is an indicator function equal to one if the ML rule would interview the applicant, and zero if not. To proxy for the human selection propensity, we use the same machine-learning based model of human interview practices, $\hat{E}[I|X]$, as described in Section 4.2.2. Finally, because we always select the same number of applicants as are actually interviewed in practice, the term $\frac{p(I=1)}{p(I^{ML}=1)}$ is equal to one by construction.

Using this approach, Figure 3 again shows that ML models outperform human recruiting practices. Among those selected by human recruiters, the average observed hiring likelihood is 10%. In contrast, our calculations show that ML models select applicants with almost 3 times higher predicted hiring potential. In particular, the average expected hiring likelihood for applicants selected by the UCB model is 32%, compared to 36% and 24% for the static and updating SL models, respectively. The slightly weaker performance of the UCB model may be explained by the fact that an emphasis on exploration means that the UCB algorithm may select weaker candidates, particularly in earlier periods. Together, this set of results are consistent with our findings from the interviewed-only subsample: the hiring yield of ML algorithms are similar to each other and in all cases better than the human decision-maker. We find no evidence that the gains in diversity that we document in Section 4.1 come at the cost of substantially reducing hiring rates among selected applicants.

We note that our analysis above relies on a common support assumption to form our reweighted estimates: intuitively, we are only able to infer the quality of the ML-selected applicant pool from the set of human-selected applicants if the candidates that are selected by the ML have some non-zero probability of being selected by human recruiters. Appendix Figure A.10 plots the distribution of a candidate's estimated propensity to be selected by a human recruiter, for the set of applicants chosen by each of our three ML models: SSL, USL, and UCB. In all cases, we find that all ML-selected applicants have a human selection propensity strictly between 0 and 1; we see no mass at or near zero.

#### 4.2.4 Testing for selection on unobservables

Our results so far have not taken into account the possibility that selection on unobservables can lead to biases. We are particularly concerned that there is positive selection on unobservables. In this case, human decisions may be more accurate than our previous analysis suggests, calling into question the potential benefits of ML relative to human screening, and potentially overstating UCB's diversity gains relative to a live implementation.

Before turning to our data, we first emphasize that the scope for selection on unobservables in our setting is limited by the fact that recruiters have very little additional information relative to what we also observe. Screeners never interact with applicants and make interview decisions on the basis of applicant resumes. Because the hiring software used by our data firm further standardizes this information into a fixed set of variables, they generally do not observe cover letters or even resume formatting. Given this, the types of applicant information that are observable to recruiters but not to the econometrician are predominately related to resume information that we do not code into our feature set. For example, we convert education information into indicator variables for college major designations, institutional ranks, and types of degree. A recruiter, by contrast, will see whether someone attended the University of Wisconsin or the University of Michigan.[42] In addition to worker characteristics, our models also include characteristics of the job search itself to account for factors that influence hiring demand independent of applicant characteristics.

To test for the possibility of selection on unobservables, we use an IV approach to identify the quality of applicants selected on the margin. Our instrument is assignment to initial resume screeners, following the methodology pioneered by Kling (2006). Applicants in our data are randomly assigned to screeners who review their resumes and make initial interview decisions. These screeners vary greatly in their propensity to pass applicants to the interview round: an applicant may receive an interview if she is assigned to a generous screener and that same applicant may not if she is assigned to a stringent one. For each applicant, we form the jackknife mean pass rate of their assigned screener and use this as an instrument, $Z$, for whether the applicant is interviewed.

Appendix Figure A.11 plots the distribution of jackknife interview pass rates in our data, restricting to the 54 recruiters (two thirds of the sample) who evaluate more than 50 applications (the mean in the sample overall is 156). After controlling for job family, job level, and work location fixed effects, the 75th percentile screener has a 50% higher pass rate than the 25th percentile screener. Appendix Table A.3 shows that this variation is predictive of whether a given applicant is interviewed, but is not related to any of the applicant's covariates.

We are also concerned about violations of monotonicity: a lenient screener may have a different preference ordering of applicants, relative to a strict screener. We examine this in two ways. First, following the literature, e.g. Frandsen et al. (2019); Leslie and Pope (2017); Dobbie et al. (2018), Appendix Table A.4 shows that our leniency instrument is positively correlated with an applicant's interview likelihood across demographic, and educational groups. We also examine the preferences of lenient and strict screeners directly: using our training period sample (2016-2017), we build two models predicting an applicant's likelihood of being interviewed: one trained on data from lenient screeners and one is trained from strict screeners. In Appendix Table A.5, we show that the within-individual correlation between these two selection propensities in our analysis data

[42] Adding additional granularity in terms of our existing variables into our model does not improve its AUC.

(2018-2019) is high: applicants that are favored by strict reviewers are likely to be favored by lenient reviewers as well.

Figure 4 plots the relationship between screener leniency and interview outcomes. If humans are, on average, positively selecting candidates, then it should be the case that applicants selected by more stringent reviewers—e.g. those who are subjected to a higher bar—should be more likely to be hired conditional on being interviewed than those selected by more lenient reviewers. Panel A of Figure 4 shows that there does not appear to be such a relationship when we do not control for applicant covariates, indicating that, at least on the margin, humans do not necessarily interview applicants with stronger covariates. In Panel B, we introduce controls for applicant demographics and qualifications and show that there does not appear to be positive selection on unobservables either. In both panels, we include job family, job level, and work location fixed effects to account for the possibility that interview rates may be associated with differences in hiring demand.

#### 4.2.5 Marginally interviewed sample

Our interview instrument allows us to consider an alternative approach for valuing the performance of ML models relative to human decisions: instead of comparing hiring outcomes across the full sample (which requires that we assume no selection on unobservables), we show that firms can improve hiring yield by relying on algorithmic recommendations in cases where human screeners are on the margin of granting an interview.

To demonstrate this, consider the following counterfactual interview policy, given our recruiter leniency instrument $Z$:

$$\tilde{I} = \begin{cases} I^{Z=1} \text{ if } s^{ML} \geq \tau, \\ I^{Z=0} \text{ if } s^{ML} < \tau. \end{cases}$$

The policy $\tilde{I}$ takes the firm's existing interview policy, $I$, and modifies it at the margin. The new policy $\tilde{I}$ favors applicants with high ML scores by asking the firm to make interview decisions $I$ as if these applicants were randomly assigned to a generous initial screener ($Z = 1$).[43] That is, $I^{Z=1}$ refers to the counterfactual interview outcome that would be obtained, if an applicant were evaluated by a lenient screener. Similarly, $\tilde{I}$ penalizes applicants with low ML scores by making interview decisions for them as though they were assigned to a stringent screener ($Z = 0$).

By construction, the interview policy $\tilde{I}$ differs from the status quo policy $I$ only in its treatment of instrument compliers. An applicant who would not be interviewed regardless of whether she is assigned to a lenient or strict screener is considered a "never taker," and would be rejected under both $\tilde{I}$ and $I$. Similarly, applicants who are "always takers" will be interviewed under both $\tilde{I}$ and $I$. The difference between $\tilde{I}$ and $I$ arises on the margin: instrument compliers with high ML scores

[43] For simplicity in exposition, we let $Z$ be a binary instrument in this example (whether an applicant is assigned to an above or below median stringency screener) though in practice we will use a continuous variable.

will be selected under $\tilde{I}$ because they are always treated as if they are assigned to lenient recruiters. Conversely, compliers with low ML scores are always rejected because they are treated as if they are assigned to strict reviewers. As such, the returns to following ML recommendations on the margin is determined by whether compliers with high ML scores have greater hiring potential than compliers with low ML scores, $E[H|I^{Z=1} > I^{Z=0}, s^{ML} \geq \tau]$ vs. $E[H|I^{Z=1} > I^{Z=0}, s^{ML} < \tau]$.

To compute the hiring potential of compliers, we estimate the following regressions following Benson et al. (2019) and Abadie (2003):

$$H_i \times I_i = \alpha_0 + \alpha_1 I_i + X_i'\alpha + \varepsilon_i \text{ if } s^{ML}(X_i') \geq \tau \quad (7)$$

$$H_i \times I_i = \beta_0 + \beta_1 I_i + X_i'\beta + \varepsilon_i \text{ if } s^{ML}(X_i') < \tau \quad (8)$$

In Equation (7), $H_i \times I_i$ is equal to applicant $i$'s hiring outcome if she is interviewed or to zero if she is not. This regression is structured so that the OLS coefficient $\hat{\alpha}_1^{OLS}$ estimates average hiring potential among all interviewed applicants with high ML scores. The IV estimate $\hat{\alpha}_1^{IV}$, in contrast, is an estimate of hiring potential among high ML-score compliers: $E[H|I^{Z=1} > I^{Z=0}, s^{ML} \geq \tau]$. Similarly, $\hat{\beta}_1^{IV}$ in Equation (8) is the analogous estimate for low ML-score compliers: $E[H|I^{Z=1} > I^{Z=0}, s^{ML} < \tau]$. This logic is analogous to the idea that IV estimates identify a LATE amongst compliers.[44]

Figure 5 plots the characteristics of instrument compliers with high and low UCB scores, $s^{UCB}$. In Panel A, we see that compliers with high UCB scores are more likely to be hired than those with low scores. This indicates that, on the margin, nudging interview decisions toward the UCB's preferences would increase expected hiring yield. In addition to examining hiring likelihood, we can also consider demographics. In Panels B through D, we show that compliers with high UCB scores are more likely to be Black, Hispanic, and female. As such, the interview policy defined by $\tilde{I}$ would increase quality and diversity on the margin, relative to the firm's current practices.

Appendix Figure A.12 repeats this exercise using updating SL scores. Again, we see that compliers with high scores were more likely to be hired than those with low scores. However, in contrast to the UCB scores, compliers with with high supervised learning scores are less diverse: they are less likely to be Black or Hispanic.

These results are again consistent with our earlier results. In both cases, following UCB recommendations can increase hiring yield and diversity relative to the firm's present policies, while following traditional SL recommendations increases quality but decreases demographic diversity.

[44] In standard potential outcomes notation, the LATE effect is $E[Y^1 - Y^0|I^{Z=1} > I^{Z=0}]$. In our case, we are only interested in the average potential outcome of compliers: $E[Y^1|I^{Z=1} > I^{Z=0}]$. Here, $Y^1$ is equivalent to a worker's hiring outcome if she is interviewed—this is what we have been calling quality, $H$. For a formal proof, see Benson et al. (2019).

#### 4.2.6 Other measures of quality

One concern with our analysis so far is that our measure of quality—likelihood of receiving and accepting an offer—may not be the ultimate measure of quality that firms are seeking to maximize. If firms ultimately care about on the job performance metrics, then they may prefer that its recruiters pass up candidates who are likely to be hired in order to look for candidates that have a better chance of performing well, if hired.

Our ability to assess this possibility is limited by a lack of data on tracking on the job performance. Ideally, we would like to train a model to predict on the job performance (instead of or in addition to hiring likelihood) and then compare the performance of that model to human decision-making. However, of the nearly 49,000 applicants in our training data, only 296 are hired and have data on job performance ratings, making it difficult to accurately build such a model.

We take an alternative approach and correlate measures of on the job performance with our ML scores and human SL score, using data from our training period. If it were the case that humans were trading off hiring likelihood with on the job performance, then our human SL model (e.g. predicting an applicant's likelihood of being interviewed) should be positively predictive of on the job performance, relative to our ML models.

Table 3 presents these results using two measures of performance: on the job performance ratings from an applicant's first mid-year review, and an indicator for whether an applicant has been promoted. On the job performance ratings are given on a scale of 1 to 3, referring to below, at, or above average performance; 13% receive an above average rating. We also examine whether a worker is promoted within the time seen in our sample; this occurs for 8% of hires in the test period.

Panel A examines the correlation between our model of human interview behavior, our "human SL" model, and performance rating and promotion outcomes. Columns 1 and 3 present raw correlations and Columns 2 and 4 control for our static SL, updating SL, and UCB scores so that we are examining the relative correlation between the human model and performance outcomes. In all cases, we observe a negatively signed and sometimes statistically significant relationship: if anything, human recruiters are less likely to interview candidates who turn out to do well on the job. By contrast, Panels B through D conduct the same exercise for each of our ML models; Columns 1 and 3 present raw correlations and Columns 2 and 4 control for the human score. For our SL models, these correlations are positively signed and statistically insignificant. For the UCB, we see a negatively signed but close to zero relationship between UCB scores and whether a candidate receives a top performance rating, and a positively and statistically significant relationship between UCB scores and future promotion.

We caution that these data are potentially subject to strong sample selection—they examine the correlation between applicant scores among the 233 hires in our test sample, only 180 of whom have mid-year evaluation data. That said, our results provide no evidence to support the hypothesis that

human recruiters are successfully trading off hiring likelihood in order to improve expected on the job performance among the set of applicants they choose to interview.

### 4.3 Discussion and External Validity

All the ML tools we use are able to identify candidates who meet the firm's own hiring criteria more often than candidates selected by the firm's own recruiters. At the same time, we show that different algorithms may have very different implications for demographic representation: while our UCB model increases minority representation relative to the firm's existing hiring practices, traditional SL models, like those used by many commercial vendors, substantially reduce the share of Black and Hispanic applicants who are interviewed.

When comparing only the outcomes of human recruiters and SL models, our results are consistent with the idea that human recruiters make a Pareto tradeoff by placing greater value on interviewing a diversity of candidates, at the cost of reducing overall hiring yield.[45] While a supervised learning model is designed to maximize only efficiency, human recruiters may still be making optimal interview decisions if they separately value diversity. Yet when considered alongside our UCB results, this explanation becomes less likely. By demonstrating that exploration-focused algorithmic tools can increase both diversity and hiring yield, our UCB results suggest that human recruiters may simply be inefficient at valuing diversity: they pass up stronger minority candidates in favor of weaker ones because they are not as good at predicting hiring outcomes.

A key question that remains relates to external validity: is it generally the case that an exploration-based model will generate Pareto improvements in quality? While our results are robust to a variety of algorithmic formulations within our sample, we cannot directly verify whether this same result would occur in other firms or at other times.

Indeed, as discussed in Section 3.3.2, if exploration leads a contextual bandit algorithm to select minority candidates who are systematically weaker than those an SL model would select, then the bandit model will more quickly learn to avoid minority candidates. Such behavior would be consistent with the phenomenon of "bias reversal," where decision-makers (in this case an SL model) exposed to minorities who survive an initial discriminatory selection process end up with upwardly biased beliefs about their quality (Bohren et al., 2019a; Rambachan and Roth, 2019). In such cases, selecting more minorities decreases average beliefs about their quality.

Our results are driven by a case in which exploration leads firms to interview more minority applicants, whose underlying quality appears to be just as high as the model's prior. That is, there were many minority candidates who met the firm's hiring criteria, but who had not been given the opportunity to be interviewed. While this need not always be the case, other studies have also

[45] This pattern could also be consistent with a more perfunctory notion of affirmative action, in which recruiters select Black and Hispanic candidates to ensure a diverse interview pool, regardless of whether these candidates are truly likely to be hired.

documented cases in which increased exposure to minorities generates increased representation and improved beliefs. Miller (2017), for instance, shows that temporary affirmative action policies that lead to more minority hiring leads firms to continue hiring more minorities even after the policy is no longer binding for the firm; Whatley (1990) studies the integration of racially homogenous firms following WWI and finds similar results. A larger literature, for example Rao (2019); Carrell et al. (2019), documents the positive effects of exposure on attitude updating more generally. Together, these papers suggest that, in practice, there are many cases in which exploration can lead to sustainable gains in representation.

# 5 Extensions

Our main results raise several additional questions, which we explore in extensions. First, our main analysis is limited by the short time span of our test period and our inability to update our models with outcomes for ML-selected candidates who are not interviewed in practice. Both of these factors can limit the scope for learning in our setting in a way that is not representative of real-life applications. We therefore conduct simulations to see if exploration is more valuable in settings where these constraints on learning are not in place. Second, our ML algorithms all make explicit use of race, ethnicity, and gender as model inputs, raising questions about their legality under current employment law. To explore these issues, we show how our UCB algorithm is impacted when we restrict its access to demographic information.

## 5.1 Learning over time

### 5.1.1 Changes in applicant quality

In this section, we examine the value of exploration and learning in greater depth. In particular, our main analysis—based on applicants from January 2018 to April 2019—is limited in two ways. First, we are only able to observe how our algorithms would behave on this relatively short span of data, making it difficult to understand whether our results would hold in other instances, particularly those where the simulated quality of applicants changes substantially over time. Second, the degree of learning in our models is limited by our updating procedure, which only allows us to add in hiring outcomes for ML-selected candidates who are actually interviewed. If ML models choose candidates that are very different from those selected by human recruiters, they will not be able to observe hiring outcomes for these candidates and this will slow down their learning.

To address these issues, we conduct simulations in which we change the quality of applicants who enter our test sample in 2018. We assign simulated values of $H_{it}$ in the following manner: in each simulation, we choose one racial group, $R$, to experience an increase (or decrease) in hiring likelihood. At the start of 2018, we assume that group $R$ applicants have the same average hiring

likelihood as their true 2018 mean.[46] Over the course of 2018, we assume that their quality linearly increases from there so that, by the end of 2018, all incoming group $R$ candidates have $H_{it} = 1$. In the meantime, we hold the quality of applicants from all other groups constant at their true 2018 mean. We assign values of hiring potential to *all* applicants, regardless of whether they are interviewed in practice. In this way, our simulation comes closer to a live-implementation in which we would be able to update our model with the hiring outcomes of all applicants selected by our models.

To assess our models' ability to learn about changes in applicant quality, we consider how they would evaluate the *same* cohort of candidates at different points in time. Specifically, we take the actual set of candidates who applied between January 2019 and April 2019 (hereafter, the "evaluation cohort"), and estimate their ML model scores at different points in 2018. By keeping the evaluation cohort the same, we are able to isolate changes in the algorithm's scores that arise from differences in learning and exploration over time, rather than from differences in the applicant pool.

For intuition, consider the scores of candidates on January 1, 2018, the first day of the test period. In this case, all three ML algorithms would have the same beliefs about the hiring potential of candidates in the evaluation cohort, because they share the same estimate of $E[H_{it}|X'_{it}; D_0]$ trained on the initial data $D_0$. The static SL and updating SL models would therefore have the same scores; the UCB would have the same "beliefs" but a different score, because it also factors in its exploration bonus. On December 31, 2018, however, the models may have different beliefs if they encountered this same set of candidates. Because its training data is never updated, the static SL model would have the same scores as it did on January 1. The updating SL and UCB algorithms would have both different beliefs (based on their potentially different history of selected applicants) and different scores (because the UCB factors in its exploration bonus in addition to expectations of quality). To better understand how the UCB model differs from the the updating SL, we also consider a fourth variant, which tracks who the UCB model would have selected based on its estimates of $E[H_{it}|X'_{it}; D_t^{UCB}]$ alone; this model allows us to track the evolution of the UCB model's beliefs separately from its exploration behavior.

### 5.1.2 Results

Panel A of Figure 6, plots the share of Black applicants who are selected in the simulation where we increase the hiring potential of Black applicants in the manner described above. We report the results of four different selection criteria. The flat solid line, which hovers at just over 1%, represents the proportion of evaluation cohort applicants who would be selected by the static SL algorithm if they arrived at time $t$ between Jan 1, 2018 and December 31, 2018. This line is flat by construction

[46] Because $H_{it}$ is binary, we accomplish this by sampling from a binomial distribution with the given mean we are seeking to reproduce.

because the static supervised algorithm's beliefs do not change, so it makes the same selection decisions in the evaluation cohort regardless of the date.

Next, the green dash-dot line reports the selection decisions of the UCB model. In strong contrast with the static SL model, the UCB rapidly increases the share of Black candidates it selects. To better understand why this happens, we plot a red dash-dot-dot line, which tracks the UCB model's *beliefs*: that is, the share of Black applicants it would select if its decisions were driven by the $\hat{E}[H_{it}|X'_{it}; D_t^{UCB}]$ component of Equation (4) only, leaving out the exploration bonus. Initially, the green dash-dot line is above the red dash-dot-dot line; this means that the UCB model begins by selecting more Black applicants not because it necessarily believes that they have strong hiring potential, but because it is looking to explore. Over time, the red dash-dot-dot line increases as the models see more successful Black candidates and positively updates its beliefs. At some point, the two lines cross: at this point, the UCB model has strong positive beliefs about the hiring potential of Black applicants, but it holds back from selecting more Black candidates because it would still like to explore the quality of other candidates. By the end of the simulation period, however, exploration bonuses have declined enough so that the UCB model's decisions are driven by its beliefs, and it selects only Black candidates.

The orange dashed line shows this same process using the updating SL model. While it is eventually able to learn about the simulated increase in the hiring prospects of Black applicants, it does so at a significantly slower rate relative to UCB. Because supervised learning algorithms focus on maximizing current predicted hiring rates, the updating SL model does not go out of its way to select Black candidates. As such, it has a harder time learning that these candidates are now more likely to be hired. This is unsurprising considering Panel C of Figure 1, which shows that, based on its initial training data, SL models are very unlikely to select Black applicants (0.7% of the interviewed sample).

Panel B of Figure 6 plots the percentage of Hispanic applicants who are selected, under the analogous simulation in which we increase their hiring potential. Our results are broadly similar although, in this case, the difference in learning speed is less stark than for Black applicants because the baseline SL model selects a higher share (1.4%) of Hispanic applicants.

Panels C and D of Figure 6 plot outcomes for Asian and White applicants, under simulations in which quality of these groups is assumed to increase, respectively. Because these groups are already well represented in our training data, our results are slightly different. Here, the updating SL model learns much more quickly about changes in the quality of Asian and White applicants because it selects a large number of candidates from these groups at baseline, making it easier to pick up on changes in their quality. Another feature to note in Panels C and D is that there is a large gap between the UCB model's beliefs and its selection choices: the UCB algorithm learns very quickly about increases in the quality of Asian and White applicants but does not initially select as many

of these candidates. This occurs because the UCB model is hesitant to exclusively select members of a large group (White or Asian), having seen very few Black and Hispanic applicants.[47]

In Appendix Figure A.13, we present analogous results from simulations in which we decrease quality. When we do this for Black and Hispanic applicants, the UCB's beliefs fall very quickly. However, because Black and Hispanic candidates continue to be so rare in the data, the UCB model continues to select a small number of these candidates, in order to continue exploring, even as their overall share among those selected trends down over time. This is an example of how the UCB model trades off immediate gains in hiring yield for the option value of increased learning in the future. When we do the same for White or Asian candidates, both the updating SL and UCB models reduce the share of such applicants that they select at approximately the same rate; this is because both models have already seen a large number of applicants from these groups.

We note that these selection patterns differ from a "quota-based" system that sets minimum levels of representation; under all of our ML models, representation for any group can go to zero if its realized outcomes fall sufficiently.

### 5.2 Blinding the Model to Applicant Demographic Characteristics

So far, our algorithms have used race, ethnicity, and gender as explicit model inputs. This means that our algorithms engage in "disparate treatment" on the basis of protected categories, in possible violation of employment and civil rights law (Kleinberg et al., 2018b).[48] A natural question, then, is how much of our results would hold if we eliminated the use of race and gender as model inputs (as a practical matter, we continue to allow the inclusion of other variables, such as geography, which may be correlated)?

In our UCB model, race and gender enter in two ways: first, as predictive features of the model that are used to predict an applicant's chances of being hired if interviewed; and second, as inputs into how exploration bonuses are assigned. The model may, for instance, be able to select more Black applicants by recognizing race as a dimension on which these applicants are rare, relative to those that are Asian or White. If this were the case, then restricting the use of race as a model input could hinder the algorithm's ability to assign higher bonuses to minorities on average; whether this is the case or not depends on whether Black and Hispanic applicants are under-represented on other dimensions that the model can still use.

In this section, we re-estimate the UCB model without the use of applicants' race, gender, and ethnicity in either prediction or bonus provision. Figure 7 shows how this type of algorithmic blinding impacts diversity. Panels A and C reproduce the race and gender composition of applicants

[47] In contrast, the UCB is much more willing to exclusively select Black or Hispanic applicants in the simulation results from Panels A and B because it already has more certainty about the quality of White or Asian applicants.

[48] A number of recent papers have considered the impacts of anonymizing applicant information on employment outcomes (Goldin and Rouse, 2000; Åslund and Skans, 2012; Behaghel et al., 2015; Agan and Starr, 2018; Alston, 2019; Doleac and Hansen, 2020; Craigie, 2020; Kolev et al., 2019).

selected by the unblinded UCB model and Panels B and D track the blinded results. Blinding reduces the share of selected applicants who are Black or Hispanic, from 23% to 15%, although there is still greater representation relative to human hiring (10%). The most stark differences, however, come in the treatment of White and Asian applicants. In the non-blinded model, White and Asian applicants make up a similar share of interviewed applicants (33% and 44%, respectively), even though there are substantially more Asian applicants. When the algorithm is blinded, however, many more Asian applicants are selected relative to White applicants (62% vs. 22%, recalling that Asian and White applicants make up 57% and 30% of the applicant pool at large, respectively). In our data, this likely arises for two reasons. First, Asian applicants are more likely to have an advanced degree, a trait that is more strongly rewarded for White applicants: blinding the algorithm to race therefore increases the returns to education among Asian applicants. Second, in the race-aware model, Asian applicants received smaller exploration bonuses because they comprised a majority of the applicant pool; when bonus provision is blinded, exploration bonuses for Asian applicants increase because they are more heterogeneous on other dimensions (such as having niche majors) that lead to higher bonuses. In Panels C and D, we find that blinding decreases the share of women who are selected from 48% to 41%, although this new share is still higher than the 35% share of women chosen by human recruiters.

Finally, Panel E of Figure 7 examines the predictive accuracy of blinded vs. unblinded UCB, using the reweighting approach described in Section 4.2.3. While we may expect blinding to reduce the quality of algorithmic predictions, we do not detect a difference in hiring quality between the blinded and non-blinded models. In our specific case, this likely arises from the fact that Asian applicants—who are more frequently selected by the race blind model—tend to have relatively high hiring rates. The efficiency gains associated with shifting exploration toward a higher yield group appears to counterbalance any potential loss in predictive accuracy, at least in the short to medium run.

# 6 Conclusion

This paper makes progress on understanding how algorithmic design shapes access to job opportunity. While a growing body of work has pointed out potential gains from following algorithmic recommendations, our paper goes further to highlight the role of algorithm design on the impact and potential consequences of these decision tools. In particular, we show that, by following an algorithm that prioritizes exploration, firms can identify more candidates that meet their hiring criteria, while also increasing the representation of Black and Hispanic applicants. This occurs even though our algorithm is not explicitly charged with increasing diversity, and even when it is blinded to demographic inputs.

Our results shed light on the nature of the relationship between efficiency and equity in the provision of job opportunities. In our data, supervised learning algorithms substantially increase applicants' predicted hiring potential, but decrease their demographic diversity relative to the firm's actual practices. A natural interpretation of this result is that algorithms and human recruiters make different tradeoffs at the Pareto frontier, with humans placing greater value on equity at the expense of efficiency. Our UCB results, however, show that such explanations may be misleading. By demonstrating that an algorithmic approach can improve hiring outcomes while also expanding representation, we provide evidence that human recruiters are operating inside the Pareto frontier: in seeking diversity (relative to our SL models), they end up selecting weaker candidates over stronger candidates from the same demographic groups. Such behavior leaves substantial room to design and adopt data-driven approaches that are better able to identify strong candidates from under-represented backgrounds. Our results are consistent with other papers showing that non-algorithm induced exposure to minorities can also lead to sustained increases in representation, suggesting that our findings are not unique to this specific research setting (Miller, 2017).

Finally, our findings raise important directions for future research. We focus on the use of ML to hire for high skill professional services firms; the patterns we find may not fully generalize across sectors or across firms that vary in their ability or propensity to adopt ML tools.[49] More research is needed to understand how changes in the composition of a firm's workforce—say as a consequence of adopting ML tools—would impact its future productivity and organizational dynamics. For example, there is considerable debate about the impact of diversity on team performance and how changes in the types of employees may impact other firm practices.[50] Last, as firms increasingly adopt algorithmic screening tools, it becomes crucial to understand the general equilibrium labor market effects of such changes in HR practice. For example, when adopted by a single firm, an exploration-focused algorithm may identify strong candidates who are overlooked by other firms using more traditional screening techniques; yet if all firms adopt similar exploration based algorithms, the ability to hire such workers may be blunted by supply-side constraints or competition from other firms. Such shifts in the aggregate demand for skill may also have long run impacts on the supply of skills in the applicant pool and on the returns to those skills. Both the magnitude and direction of these potentially conflicting effects deserve future scrutiny.

[49] For example, our firm has a fairly rigorous data collection process: firms that do not may make different adoption decisions and have different potential returns (Athey and Stern, 1998).

[50] For instance, see Reagans and Zuckerman (2001) for a discussion of the role of diversity, and, for instance, Athey et al. (2000) and Fernandez and Moore (2000) for a discussion of how changes in firm composition can shift mentoring, promotion, and future hiring patterns.

# References


**Abadie, Alberto**, "Semiparametric instrumental variable estimation of treatment response models," *Journal of econometrics*, 2003, *113* (2), 231–263.

**Abbasi-Yadkori, Yasin, Peter Bartlett, Kush Bhatia, Nevena Lazic, Csaba Szepesvari, and Gellért Weisz**, "POLITEX: Regret bounds for policy iteration using expert prediction," in "International Conference on Machine Learning" 2019, pp. 3692–3702.

**Agan, Amanda and Sonja Starr**, "Ban the box, criminal records, and racial discrimination: A field experiment," *The Quarterly Journal of Economics*, 2018, *133* (1), 191–235.

**Agrawal, Shipra and Navin Goyal**, "Further optimal regret bounds for thompson sampling," in "Artificial intelligence and statistics" 2013, pp. 99–107.

**Alston, Mackenzie**, "The (Perceived) Cost of Being Female: An Experimental Investigation of Strategic Responses to Discrimination," *Working paper*, 2019.

**Arnold, David, Will S Dobbie, and Peter Hull**, "Measuring Racial Discrimination in Algorithms," Working Paper 28222, National Bureau of Economic Research December 2020.

**Åslund, Olof and Oskar Nordströum Skans**, "Do anonymous job application procedures level the playing field?," *ILR Review*, 2012, *65* (1), 82–107.

**Athey, Susan and Scott Stern**, "An Empirical Framework for Testing Theories About Complimentarity in Organizational Design," Working Paper 6600, National Bureau of Economic Research 1998. Series: Working Paper Series.

_ **and Stefan Wager**, "Efficient Policy Learning," *arXiv:1702.02896 [cs, econ, math, stat]*, September 2019. arXiv: 1702.02896.

_ **, Christopher Avery, and Peter Zemsky**, "Mentoring and Diversity," *American Economic Review*, September 2000, *90* (4), 765–786.

**Auer, Peter**, "Using Confidence Bounds for Exploitation-Exploration Trade-offs," *Journal of Machine Learning Research*, 2002, *3* (Nov), 397–422.

**Bagues, Manuel and Chris Roth**, "Interregional Contact and National Identity," *CEPR Working Paper 15576*, 2021.

**Bakalar, Chloé, Renata Barreto, Stevie Bergman, Miranda Bogen, Bobbie Chern, Sam Corbett-Davies, Melissa Hall, Isabel Kloumann, Michelle Lam, Joaquin Quiñonero Candela, Manish Raghavan, Joshua Simons, Jonathan Tannen, Edmund Tong, Kate Vredenburgh, and Jiejing Zhao**, "Fairness On The Ground: Applying Algorithmic Fairness Approaches to Production Systems," 2021.

**Barocas, Solon and Andrew D. Selbst**, "Big Data's Disparate Impact," *SSRN Electronic Journal*, 2016.

**Bastani, Hamsa and Mohsen Bayati**, "Online Decision-Making with High-Dimensional Covariates," 2019, p. 58.

_ **,** _ **, and Khashayar Khosravi**, "Mostly Exploration-Free Algorithms for Contextual Bandits," *arXiv:1704.09011 [cs, stat]*, November 2019. arXiv: 1704.09011.

**Bechavod, Yahav, Katrina Ligett, Aaron Roth, Bo Waggoner, and Zhiwei Steven Wu**, "Equal Opportunity in Online Classification with Partial Feedback," 2020.

**Behaghel, Luc, Bruno Crépon, and Thomas Le Barbanchon**, "Unintended effects of anonymous resumes," *American Economic Journal: Applied Economics*, 2015, *7* (3), 1–27.

**Benson, Alan, Danielle Li, and Kelly Shue**, "Promotions and the Peter Principle," *The Quarterly Journal of Economics*, 2019, *134* (4), 2085–2134.

_ , _ , and _ , "Potential and the Gender Promotion Gap," *mimeo*, 2021.

**Bergemann, Dirk and Juuso Valimaki**, "Bandit Problems," 2006.

**Berry, Donald A**, "Bayesian clinical trials," *Nature reviews Drug discovery*, 2006, *5* (1), 27–36.

**Blattner, Laura, Scott Nelson, and Jann Spiess**, "Unpacking the Black Box: Regulating Algorithmic Decisions," 2021, p. 36.

**BLS**, "Industries with the largest wage and salary employment growth and declines," 2019.

**Bogen, Miranda and Aaron Rieke**, "Help Wanted: An Examination of Hiring Algorithms, Equity, and Bias," 2018.

**Bohren, J. Aislinn, Alex Imas, and Michael Rosenberg**, "The Dynamics of Discrimination: Theory and Evidence," *American Economic Review*, October 2019, *109* (10), 3395–3436.

_ , **Kareem Haggag, Alex Imas, and Devin G Pope**, "Inaccurate Statistical Discrimination: An Identification Problem," Working Paper 25935, National Bureau of Economic Research June 2019.

**Bollinger, Christopher and Julie Hotchkiss**, "The Upside Potential of Hiring Risky Workers: Evidence from the Baseball Industry," *Journal of Labor Economics*, 2003, *21* (4), 923–944.

**Bubeck, Sébastien and Nicolo Cesa-Bianchi**, "Regret analysis of stochastic and nonstochastic multi-armed bandit problems," *arXiv preprint arXiv:1204.5721*, 2012.

**Carrell, Scott E., Mark Hoekstra, and James E. West**, "The Impact of College Diversity on Behavior toward Minorities," *American Economic Journal: Economic Policy*, November 2019, *11* (4), 159–82.

**Castilla, Emilio**, "Bringing Managers Back In," *American Sociological Review*, 09 2011, *76*, 667–694.

**Castilla, Emilio J.**, "Gender, Race, and Meritocracy in Organizational Careers," *American Journal of Sociology*, 2008, *113* (6), 1479–1526.

**Corbett-Davies, Sam and Sharad Goel**, "The Measure and Mismeasure of Fairness: A Critical Review of Fair Machine Learning," *arXiv:1808.00023 [cs]*, August 2018. arXiv: 1808.00023.

**Cortes, David**, "Adapting multi-armed bandits policies to contextual bandits scenarios," *arXiv:1811.04383 [cs, stat]*, November 2019. arXiv: 1811.04383.

**Cowgill, Bo**, "Bias and productivity in humans and algorithms: Theory and evidence from resume screening," *Columbia Business School, Columbia University*, 2018, *29*.

_ **and Catherine E Tucker**, "Economics, fairness and algorithmic bias," *preparation for: Journal of Economic Perspectives*, 2019.

**Craigie, Terry-Ann**, "Ban the Box, Convictions, and Public Employment," *Economic Inquiry*, 2020, *58* (1), 425–445.

**Crandall, Christian S and Amy Eshleman**, "A justification-suppression model of the expression and experience of prejudice.," *Psychological bulletin*, 2003, *129* (3), 414.

**Currie, Janet M. and W. Bentley MacLeod**, "Understanding Doctor Decision Making: The Case of Depression Treatment," *Econometrica*, 2020, *88* (3), 847–878.

**Datta, Amit, Michael Carl Tschantz, and Anupam Datta**, "Automated experiments on ad privacy settings," *Proceedings on privacy enhancing technologies*, 2015, *2015* (1), 92–112.

**Deming, David J.**, "The Growing Importance of Social Skills in the Labor Market," *Quarterly Journal of Economics*, 2017, *132* (4), 1593–1640.

**Dimakopoulou, Maria, Zhengyuan Zhou, Susan Athey, and Guido Imbens**, "Balanced Linear Contextual Bandits," *arXiv:1812.06227 [cs, stat]*, December 2018. arXiv: 1812.06227.

**_ , _ , _ , and _** , "Estimation Considerations in Contextual Bandits," *arXiv:1711.07077 [cs, econ, stat]*, December 2018. arXiv: 1711.07077.

**Dobbie, Will, Jacob Goldin, and Crystal S. Yang**, "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges," *American Economic Review*, February 2018, *108* (2), 201–40.

**Doleac, Jennifer L and Benjamin Hansen**, "The unintended consequences of "ban the box": Statistical discrimination and employment outcomes when criminal histories are hidden," *Journal of Labor Economics*, 2020, *38* (2), 321–374.

**Dwork, Cynthia, Moritz Hardt, Toniann Pitassi, Omer Reingold, and Rich Zemel**, "Fairness Through Awareness," 2011.

**et. al. McKinney Scott Mayer**, "International evaluation of an AI system for breast cancer screening," *Nature*, 2020, *577* (7788), 89–94.

**Fischer, Christine, Karoline Kuchenbäcker, Christoph Engel, Silke Zachariae, Kerstin Rhiem, Alfons Meindl, Nils Rahner, Nicola Dikow, Hansjörg Plendl, Irmgard Debatin et al.**, "Evaluating the performance of the breast cancer genetic risk models BOADICEA, IBIS, BRCAPRO and Claus for predicting BRCA1/2 mutation carrier probabilities: a study based on 7352 families from the German Hereditary Breast and Ovarian Cancer Consortium," *Journal of medical genetics*, 2013, *50* (6), 360–367.

**Frandsen, Brigham R, Lars J Lefgren, and Emily C Leslie**, "Judging Judge Fixed Effects," Working Paper 25528, National Bureau of Economic Research February 2019.

**Friedman, Sam and Daniel Laurison**, *The Class Ceiling: Why It Pays to Be Privileged*, University of Chicago Press, 2019.

**Gittins, John C and David M Jones**, "A dynamic allocation index for the discounted multiarmed bandit problem," *Biometrika*, 1979, *66* (3), 561–565.

**Goldin, Claudia and Cecilia Rouse**, "Orchestrating impartiality: The impact of "blind" auditions on female musicians," *American economic review*, 2000, *90* (4), 715–741.

**Gordon, Maximilian Kasy Soha Osman Simon Quinn Stefano Caria Grant and Alex Teytelboym**, "An Adaptive Targeted Field Experiment: Job Search Assistance for Refugees in Jordan," Working Paper, Oxford University 2020.

**Hirano, Keisuke, Guido W. Imbens, and Geert Ridder**, "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score," *Econometrica*, 2003, *71* (4), 1161–1189. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/1468-0262.00442.

**Hoffman, Mitchell, Lisa B Kahn, and Danielle Li**, "Discretion in hiring," *The Quarterly Journal of Economics*, 2018, *133* (2), 765–800.

**Housman, Michael and Dylan Minor**, "Toxic Workers," Working Paper 16-057, Harvard Business School 2015.

**Jackson, Summer**, "Not Paying for Diversity: Repugnance and Failure to Choose Labor Market Platforms that Facilitate Hiring Racial Minorities into Technical Positions," 2020.

**Joseph, Matthew, Michael Kearns, Jamie Morgenstern, and Aaron Roth**, "Fairness in Learning: Classic and Contextual Bandits," 2016.

**_ , _ , _ , Seth Neel, and Aaron Roth**, "Fair Algorithms for Infinite and Contextual Bandits," 2017.

**Kaebling, Leslie P**, "Lecture Notes in 6.862 Applied Machine Learning: Feature Representation," February 2019.

**Kasy, Maximilian and Anja Sautmann**, "Adaptive treatment assignment in experiments for policy choice," 2019.

_ **and Rediet Abebe**, "Fairness, equality, and power in algorithmic decision making," *Workshop on Participatory Approaches to Machine Learning, International Conference on Machine Learning*, 2020.

**Kleinberg, Jon, Himabindu Lakkaraju, Jure Leskovec, Jens Ludwig, and Sendhil Mullainathan**, "Human decisions and machine predictions," *The quarterly journal of economics*, 2018, *133* (1), 237–293.

_ **, Jens Ludwig, Sendhil Mullainathan, and Cass R Sunstein**, "Discrimination in the Age of Algorithms," *Journal of Legal Analysis*, 2018, *10.*

_ **, Sendhil Mullainathan, and Manish Raghavan**, "Inherent Trade-Offs in the Fair Determination of Risk Scores," *arXiv:1609.05807 [cs, stat]*, November 2016. arXiv: 1609.05807.

**Kline, Patrick M and Christopher R Walters**, "Reasonable Doubt: Experimental Detection of Job-Level Employment Discrimination," Working Paper 26861, National Bureau of Economic Research March 2020.

**Kling, Jeffrey R**, "Incarceration length, employment, and earnings," *American Economic Review*, 2006, *96* (3), 863–876.

**Kolev, Julian, Yuly Fuentes-Medel, and Fiona Murray**, "Is Blinded Review Enough? How Gendered Outcomes Arise Even Under Anonymous Evaluation," Working Paper 25759, National Bureau of Economic Research April 2019.

**Krishnamurthy, Sanath Kumar and Susan Athey**, "Survey Bandits with Regret Guarantees," *arXiv:2002.09814 [cs, econ, stat]*, February 2020. arXiv: 2002.09814.

**Kuhn, Peter J and Lizi Yu**, "How Costly is Turnover? Evidence from Retail," Technical Report, National Bureau of Economic Research 2019.

**Kuhnen, Camelia M. and Paul Oyer**, "Exploration for Human Capital: Evidence from the MBA Labor Market," *Journal of Labor Economics*, 2016, *34* (S2), S255–S286.

**Lai, Tze Leung and Herbert Robbins**, "Asymptotically efficient adaptive allocation rules," *Advances in applied mathematics*, 1985, *6* (1), 4–22.

**Lakkaraju, Himabindu, Jon Kleinberg, Jure Leskovec, Jens Ludwig, and Sendhil Mullainathan**, "The selective labels problem: Evaluating algorithmic predictions in the presence of unobservables," in "Proceedings of the 23rd ACM SIGKDD International Conference on Knowledge Discovery and Data Mining" 2017, pp. 275–284.

**Lambrecht, Anja and Catherine Tucker**, "Algorithmic Bias? An Empirical Study of Apparent Gender-Based Discrimination in the Display of STEM Career Ads," *Management Science*, 2019, *65* (7), 2966–2981.

**Lazear, Edward**, "Hiring Risky Workers," in Tachibanaki T. Ohashi I., ed., *Internal Labour Markets, Incentives and Employment*, Palgrave Macmillan, London., 1998.

**Lei, Huitian, Ambuj Tewari, and Susan A. Murphy**, "An Actor-Critic Contextual Bandit Algorithm for Personalized Mobile Health Interventions," 2017.

**Lepage, Louis-Pierre**, "Endogenous Learning, Persistent Employer Biases, and Discrimination," *University of Michigan, mimeo*, November 2020.

_ , "Experimental Evidence on Endogenous Belief Formation in Hiring and Discrimination," *University of Michigan, mimeo*, September 2020.

**Leslie, Emily and Nolan G. Pope**, "The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from New York City Arraignments," *The Journal of Law and Economics*, 2017, *60* (3), 529–557.

**Li, Lihong, Wei Chu, John Langford, and Robert E. Schapire**, "A Contextual-Bandit Approach to Personalized News Article Recommendation," *Proceedings of the 19th international conference on World wide web - WWW '10*, 2010, p. 661. arXiv: 1003.0146.

**_ , Yu Lu, and Dengyong Zhou**, "Provably Optimal Algorithms for Generalized Linear Contextual Bandits," in "Proceedings of the 34th International Conference on Machine Learning - Volume 70" ICML'17 JMLR.org 2017, p. 2071–2080.

**M., Emilio J. Castilla Fernandez Roberto and Paul Moore**, "Social Capital at Work: Networks and Employment at a Phone Center," *American Journal of Sociology*, 2000, *105* (5), 1288–1356.

**Mercer**, "Global Talent Trends 2020," 2020.

**Miller, Conrad**, "The Persistent Effect of Temporary Affirmative Action," *American Economic Journal: Applied Economics*, July 2017, *9* (3), 152–90.

**Mullainathan, Sendhil and Ziad Obermeyer**, "Who is Tested for Heart Attack and Who Should Be: Predicting Patient Risk and Physician Error," *NBER WP*, 2019.

**Obermeyer, Ziad, Brian Powers, Christine Vogeli, and Sendhil Mullainathan**, "Dissecting racial bias in an algorithm used to manage the health of populations," *Science*, 2019, *366* (6464), 447–453.

**Pew Research Center**, *Women and Men in STEM Often at Odds Over Workplace Equity* January 2018.

**Quadlin, Natasha**, "The Mark of a Woman's Record: Gender and Academic Performance in Hiring," *American Sociological Review*, 2018, *83* (2), 331–360.

**Raghavan, Manish, Solon Barocas, Jon Kleinberg, and Karen Levy**, "Mitigating bias in algorithmic employment screening: Evaluating claims and practices," *arXiv preprint arXiv:1906.09208*, 2019.

**Rambachan, Ashesh and Jonathan Roth**, "Bias In, Bias Out? Evaluating the Folk Wisdom," 2019.

**_ , Jon Kleinberg, Sendhil Mullainathan, and Jens Ludwig**, "An Economic Approach to Regulating Algorithms," Working Paper 27111, National Bureau of Economic Research May 2020.

**Rao, Gautam**, "Familiarity Does Not Breed Contempt: Generosity, Discrimination, and Diversity in Delhi Schools," *American Economic Review*, March 2019, *109* (3), 774–809.

**Reagans, Ray and Ezra W. Zuckerman**, "Networks, Diversity, and Productivity: The Social Capital of Corporate R&D Teams," *Organization Science*, 2001, *12* (4), 502–517.

**Rejwan, Idan and Yishay Mansour**, "Top-k Combinatorial Bandits with Full-Bandit Feedback," 2019.

**Rigollet, Philippe and Assaf Zeevi**, "Nonparametric Bandits with Covariates," 2010.

**Rivera, Lauren**, *Pedigree: How Elite Students Get Elite Jobs*, Princeton University Press, 2015.

**Rivera, Lauren A. and Andreas Tilcsik**, "Scaling Down Inequality: Rating Scales, Gender Bias, and the Architecture of Evaluation," *American Sociological Review*, 2019, *84* (2), 248–274.

**Robins, James M., Andrea Rotnitzky, and Lue Ping Zhao**, "Analysis of Semiparametric Regression Models for Repeated Outcomes in the Presence of Missing Data," *Journal of the American Statistical Association*, 1995, *90* (429), 106–121. Publisher: [American Statistical Association, Taylor & Francis, Ltd.].

**Russakovsky, Olga, Jia Deng, Hao Su, Jonathan Krause, Sanjeev Satheesh, Sean Ma, Zhiheng Huang, Andrej Karpathy, Aditya Khosla, Michael Bernstein, and et al.**, "ImageNet Large Scale Visual Recognition Challenge," *International Journal of Computer Vision*, Apr 2015, *115* (3), 211–252.

**Russo, Daniel and Benjamin Van Roy**, "An Information-Theoretic Analysis of Thompson Sampling," 2015.

**Schrittwieser, Julian, Ioannis Antonoglou, Thomas Hubert, Karen Simonyan, Laurent Sifre, Simon Schmitt, Arthur Guez, Edward Lockhart, Demis Hassabis, Thore Graepel, Timothy Lillicrap, and David Silver**, "Mastering Atari, Go, Chess and Shogi by Planning with a Learned Model," 2019.

**Schumann, Candice, Zhi Lang, Jeffrey S. Foster, and John P. Dickerson**, "Making the Cut: A Bandit-based Approach to Tiered Interviewing," 2019.

**_ , _ , Nicholas Mattei, and John P. Dickerson**, "Group Fairness in Bandit Arm Selection," 2020.

**Si, Nian, Fan Zhang, Zhengyuan Zhou, and Jose Blanchet**, "Distributional Robust Batch Contextual Bandits," 2020.

**Slivkins, Aleksandrs**, "Contextual Bandits with Similarity Information," 2014, p. 36.

**Sterling, Adina D. and Roberto M. Fernandez**, "Once in the Door: Gender, Tryouts, and the Initial Salaries of Managers," *Management Science*, 2018, *64* (11), 5444–5460.

**Thompson, William R**, "On the likelihood that one unknown probability exceeds another in view of the evidence of two samples," *Biometrika*, 1933, *25* (3/4), 285–294.

**Wang, Lu, Andrea Rotnitzky, and Xihong Lin**, "Nonparametric Regression With Missing Outcomes Using Weighted Kernel Estimating Equations," *Journal of the American Statistical Association*, September 2010, *105* (491), 1135–1146. Publisher: Taylor & Francis _eprint: https://doi.org/10.1198/jasa.2010.tm08463.

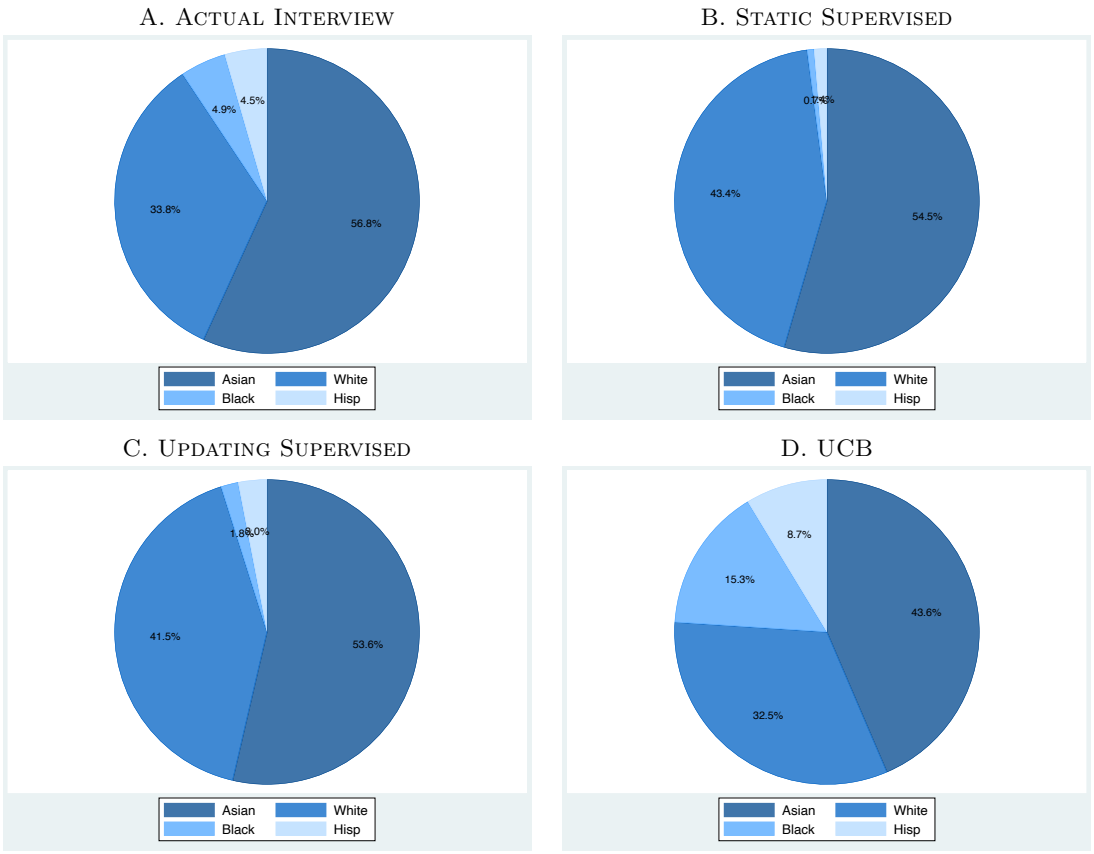
**Whatley, Warren C.**, "Getting a Foot in the Door: "Learning," State Dependence, and the Racial Integration of Firms," *The Journal of Economic History*, 1990, *50* (1), 43?66.

**Yala, Adam, Constance Lehman, Tal Schuster, Tally Portnoi, and Regina Barzilay**, "A Deep Learning Mammography-based Model for Improved Breast Cancer Risk Prediction," *Radiology*, 2019, *292* (1), 60–66. PMID: 31063083.

**Yu, Martin and Nathan R. Kuncel**, "Pushing the Limits for Judgmental Consistency: Comparing Random Weighting Schemes with Expert Judgments," *Personnel Assessment and Decisions*, *6*.

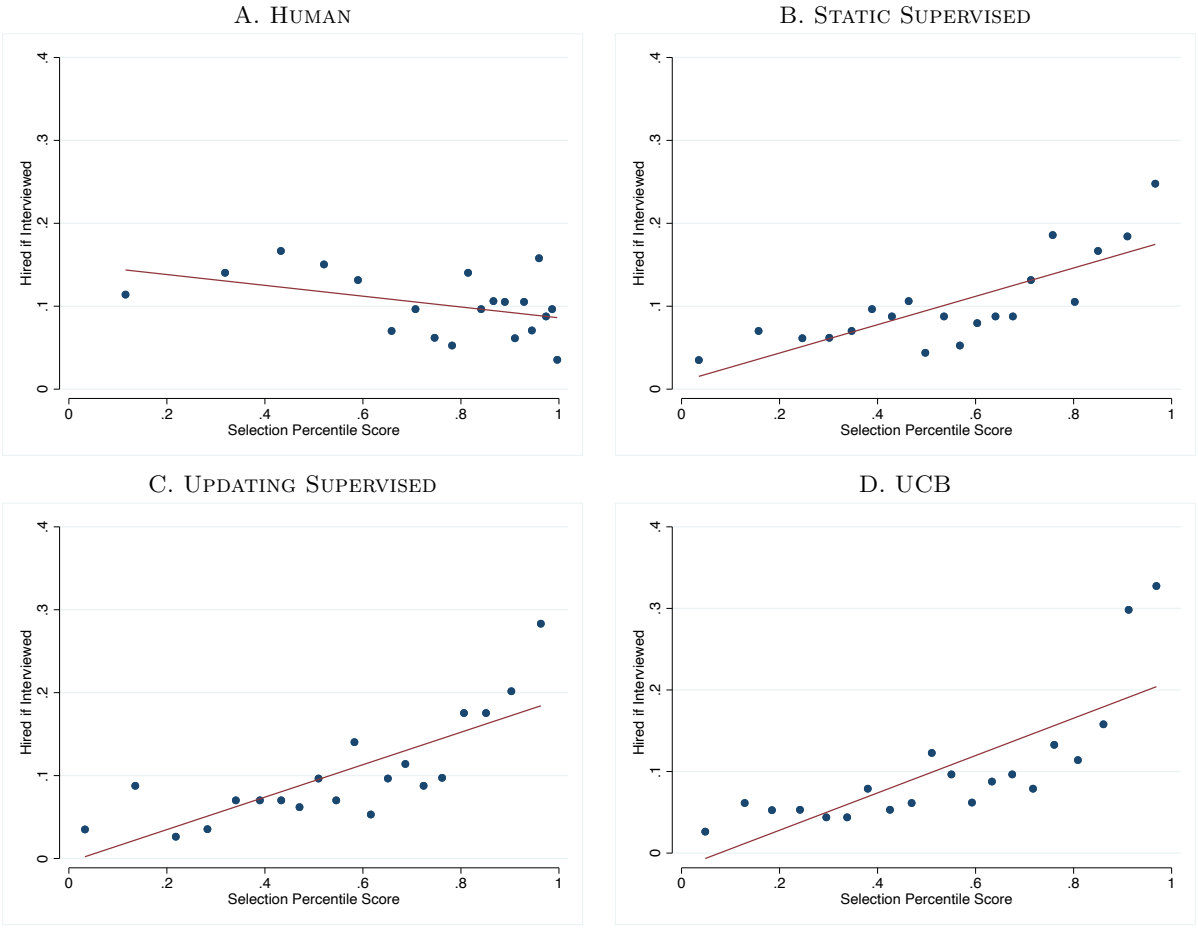
**Zhou, Zhengyuan, Susan Athey, and Stefan Wager**, "Offline Multi-Action Policy Learning: Generalization and Optimization," *arXiv:1810.04778 [cs, econ, stat]*, November 2018. arXiv: 1810.04778.

FIGURE 1: RACIAL COMPOSITION



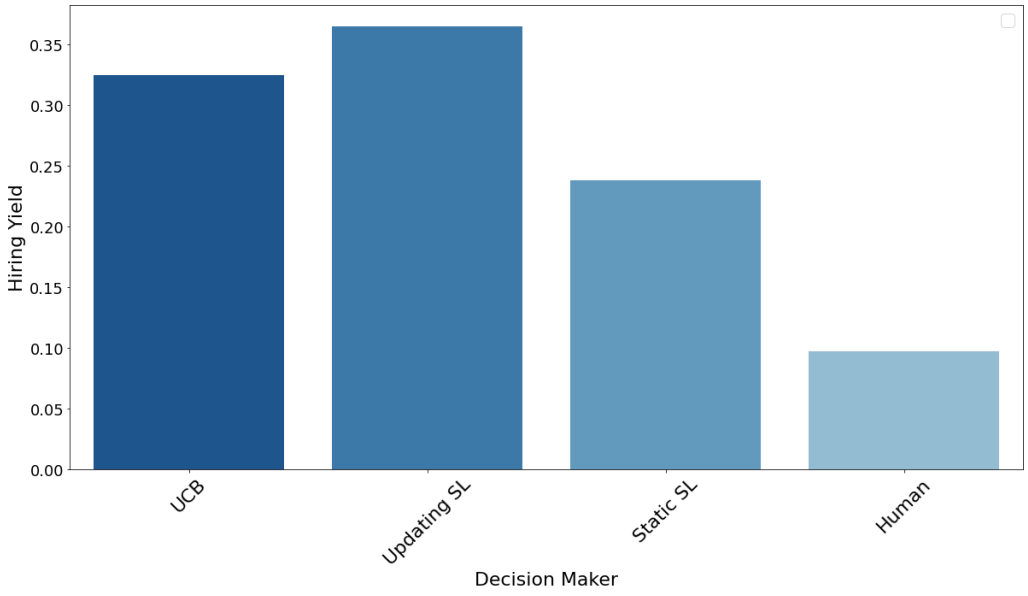

NOTES: Panel A shows the racial composition of applicants actually selected for an interview by the firm. Panel B shows the composition of those who would be selected if chosen by the static supervised learning algorithm described in Equation (2). Panel C shows the racial composition of applicants who would be selected if chosen by the updating supervised learning algorithm described in Equation (3). Finally, Panel D shows the composition of applicants who would be selected for an interview by the UCB algorithm described in Equation (4). All data come from the firm's application and hiring records.

FIGURE 2: CORRELATIONS BETWEEN ALGORITHM SCORES AND HIRING LIKELIHOOD




NOTES: Each panel of this figure plots algorithm selection scores on the $x$-axis and the likelihood of an applicant being hired if interviewed on the $y$-axis. Panel A shows the selection scores from an algorithm that predicts the firm's actual selection of which applicants to interview. Panel B shows the selection scores from the static supervised learning algorithm described by Equation (2). Panel C shows selection scores from the updating supervised learning algorithm described in Equation (3). Panel D shows the selection scores from the UCB algorithm described in Equation (4).

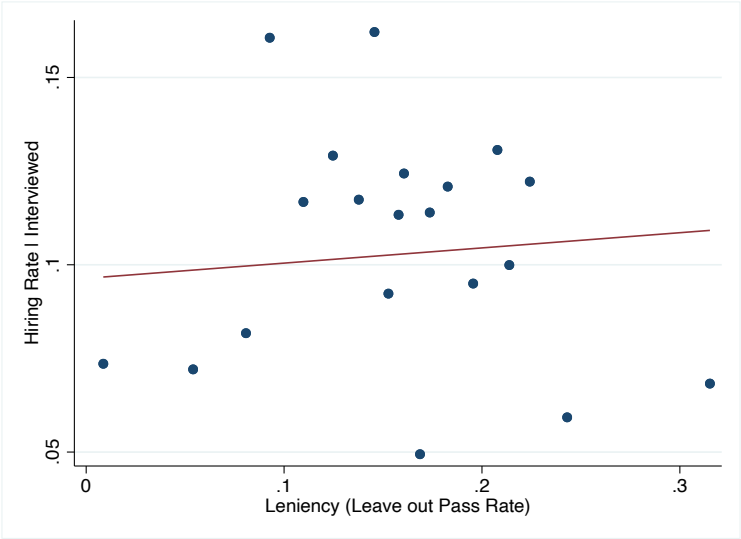
FIGURE 3: AVERAGE HIRING LIKELIHOOD, FULL SAMPLE



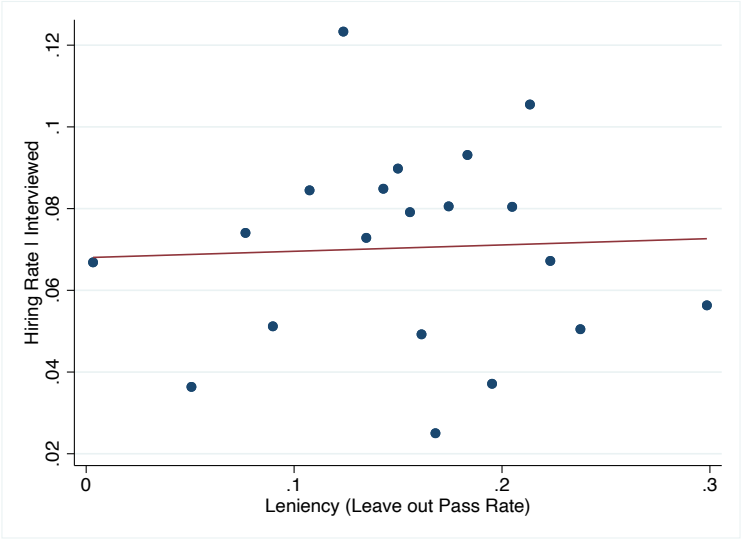

NOTES: This figure shows our decomposition-reweighting estimates of $E[H|I^{ML} = 1]$ for each algorithmic selection strategy alongside actual hiring yields from human selection decisions.

Figure 4: Testing for Positive Selection

A. Full Sample, No Controls

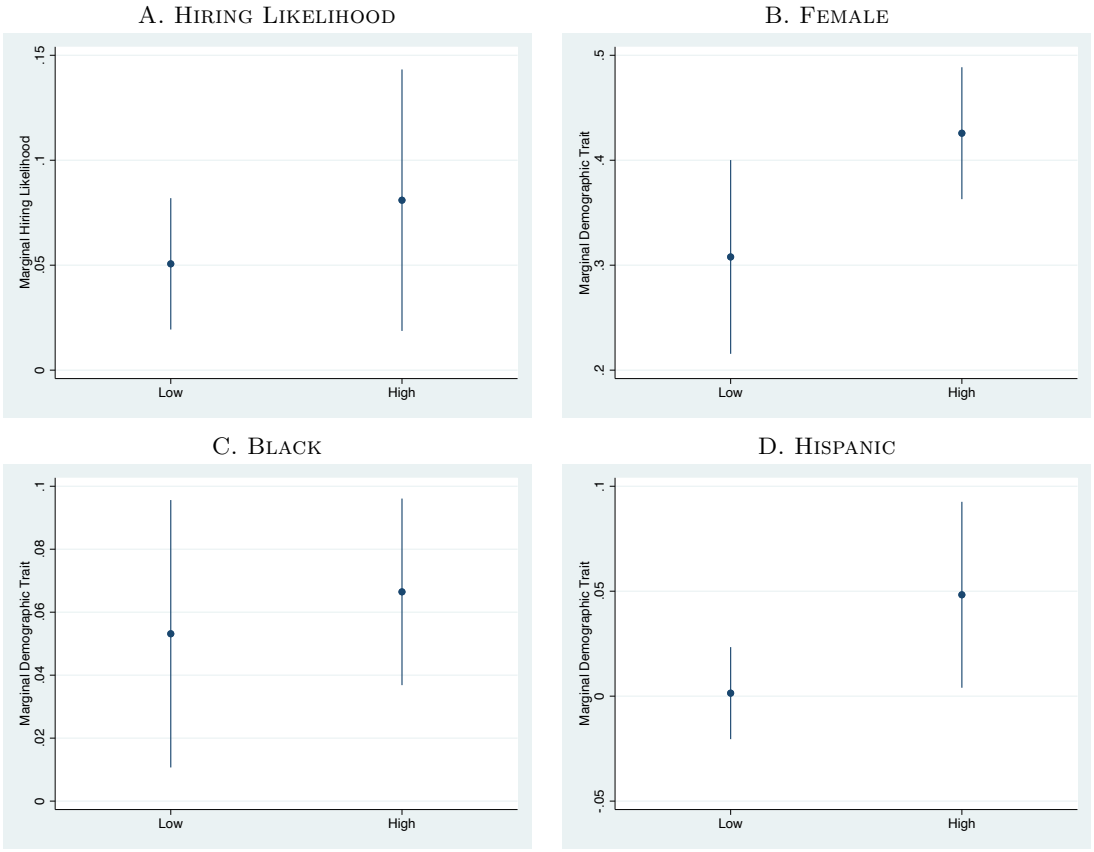



B. Full Sample, Controls



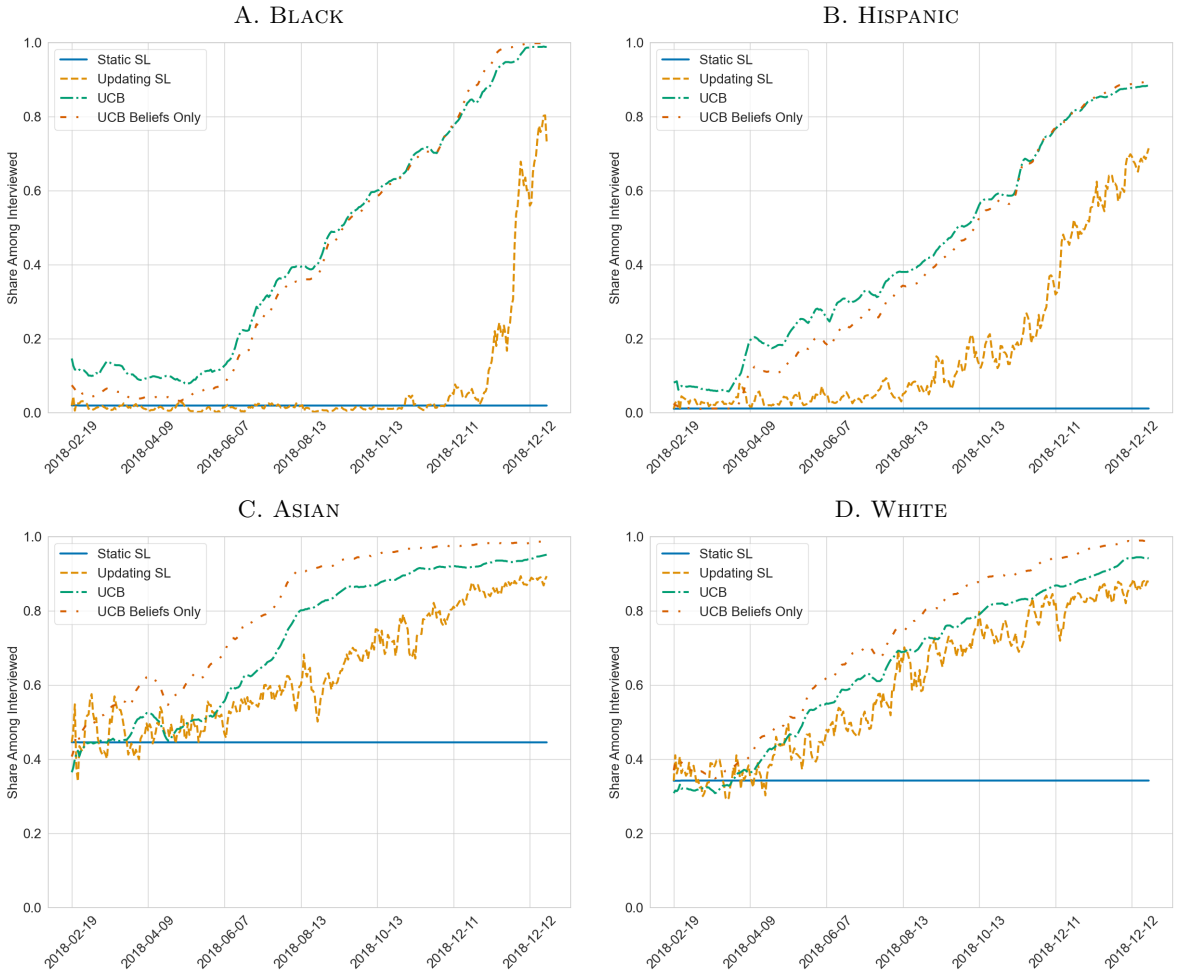

Notes: These binned scatterplots show the relationship between the leniency of randomly assigned screeners and the hiring outcomes of the applicants they select to be interviewed. Panel A plots this relationship, controlling only for job level characteristics: fixed effects for type of job, seniority level, work location, and application year. Panel B plots this relationship after adding controls for applicant characteristics: education, work history, and demographics.

FIGURE 5: CHARACTERISTICS OF MARGINAL INTERVIEWEES, BY UCB SCORE



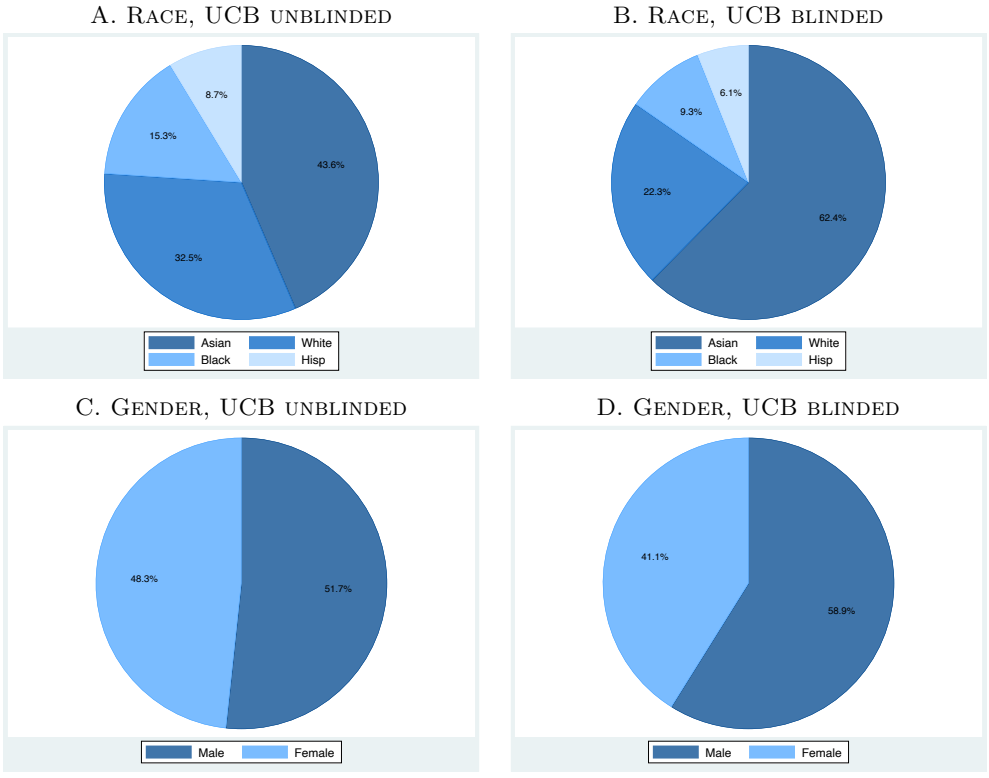

NOTES: Each panel in this figure shows the results of estimating the characteristics of applicants interviewed on the margin. In each panel, these characteristics are estimated separately for applicants in the top and bottom half of the UCB algorithm's score. In Panel A, the $y$-axis is the average hiring likelihood of marginally interviewed candidates; the $y$-axis in Panel B is proportion of marginally interviewed candidates who are female; Panels C and D examine the share of Black and Hispanic applicants, respectively. The confidence intervals shown in each panel are derived from robust standard errors clustered at the recruiter level.

FIGURE 6: DYNAMIC UPDATING, INCREASED QUALITY




NOTES: This figure shows the share of applicants recommended for interviews under four different algorithmic selection strategies: static SL, updating SL, UCB, and the beliefs component of UCB (that is, the $\hat{E}_t[H|X; D_t^{UCB}]$ term in Equation (4)). In each panel, the $y$-axis graphs the share of "evaluation cohort" (2019) applicants who would be selected under each simulation. Panel A plots the share of evaluation cohort Black applicants who would be selected under the simulation in which the hiring potential of Black candidates increases linearly over the course of 2018, as described in Section 5.1. Panel B shows results from a simulation in which the hiring potential of Hispanic candidates in 2018 increases. Similarly, Panels C and D show results from simulations in which the hiring potential of White and Asian applicants increases, respectively.

FIGURE 7: DEMOGRAPHICS BLINDING




NOTES: Panels A-D shows the race and gender composition of applicants recommended for interviews by the UCB algorithm when this algorithm explicitly incorporates race and gender in estimation (race and gender "unblinded") and when it excludes these characteristics in estimation (race and gender "blinded"). Panel E shows our decomposition-reweighting estimates of $E[H|I^{ML} = 1]$ for blinded vs. unblinded UCB alongside actual hiring yields from human selection decisions. All data come from the firm's application and hiring records.

TABLE 1: APPLICANT SUMMARY STATISTICS

| Variable | Mean Training | Mean Test | Mean Overall |
|---|---|---|---|
| Black | 0.09 | 0.09 | 0.09 |
| Hispanic | 0.04 | 0.04 | 0.04 |
| Asian | 0.57 | 0.59 | 0.58 |
| White | 0.30 | 0.28 | 0.29 |
| Male | 0.68 | 0.66 | 0.67 |
| Female | 0.32 | 0.34 | 0.33 |
| Referred | 0.14 | 0.11 | 0.13 |
| B.A. Degree | 0.23 | 0.24 | 0.24 |
| Associate Degree | 0.01 | 0.01 | 0.01 |
| Master's Degree | 0.61 | 0.64 | 0.63 |
| Ph.D. | 0.07 | 0.07 | 0.07 |
| Attended a U.S. College | 0.75 | 0.80 | 0.77 |
| Attended Elite U.S. College | 0.13 | 0.14 | 0.13 |
| Interviewed | 0.05 | 0.05 | 0.05 |
| Hired | 0.01 | 0.01 | 0.01 |
| Observations | 48,719 | 39,947 | 88,666 |

NOTES: This table shows applicants' demographic characteristics, education histories, and work experience. The sample in Column 1 consists of all applicants who applied to a position during our training period (2016 and 2017). Column 2 consists of applicants who applied during the test period (2018 to Q1 2019). Column 3 presents summary statistics for the full pooled sample. All data come from the firm's application and hiring records.

TABLE 2: PREDICTIVE ACCURACY OF HUMAN VS. ML MODELS, AMONG INTERVIEWED APPLICANTS

A. HUMAN VS. UPDATING SL

| Selectivity (Top X%) | Overlap % | Both | Human Only | SL Only |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 25 | 13.33 | 18.52 | 6.83 | 17.78 |
| 50 | 37.22 | 10.99 | 7.47 | 16.67 |
| 75 | 64.93 | 10.31 | 4.67 | 18.68 |

B. HUMAN VS. UCB

| Selectivity (Top X%) | Overlap % | Both | Human Only | UCB Only |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 25 | 15.72 | 17.95 | 6.46 | 20.33 |
| 50 | 36.00 | 12.09 | 6.33 | 16.57 |
| 75 | 61.28 | 10.76 | 3.89 | 16.30 |

C. UPDATING SL VS. UCB

| Selectivity (Top X%) | Overlap % | Both | SL Only | UCB Only |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 25 | 42.43 | 23.39 | 9.91 | 14.22 |
| 50 | 60.59 | 15.33 | 8.21 | 10.71 |
| 75 | 74.43 | 13.14 | 5.98 | 5.98 |

NOTES: This table shows the hiring rates of each algorithm when they make the same recommendation or differing recommendations. The top panel compares the human versus updating SL algorithm, the middle panel compares the human versus the UCB algorithm, and the lower panel compares the updating SL versus the UCB algorithm. Each row of a given panel conditions on selecting either the top 25%, 50%, 75% of applicants according to each of the models. For the two algorithms being compared in a given panel, Column 1 shows the percent of selected applicants that both algorithms agree on. Column 2 shows the share of applicants hired when both algorithms recommend an applicant, and Columns 3 and 4 show the share hired when applicants are selected by only one of two algorithms being compared. All data come from the firm's application and hiring records.

Table 3: Correlations between Human Scores and On the Job Performance

A. Human Scores

| | Top Rating | | Promoted | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Human SL Score | -0.282** | -0.285** | -0.0961 | -0.102 |
| | (0.116) | (0.116) | (0.0782) | (0.0776) |
| Observations | 180 | 180 | 233 | 233 |
| Controls for ML Scores | | X | | X |

B. Static SL Scores

| | Top Rating | | Promoted | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Static SL | 0.111 | 0.109 | 0.0557 | 0.0562 |
| | (0.110) | (0.102) | (0.0628) | (0.0636) |
| Observations | 180 | 180 | 233 | 233 |
| Controls for Human SL | | | | |

C. Updating SL Scores

| | Top Rating | | Promoted | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Updating SL | 0.0642 | 0.0606 | 0.0469 | 0.0480 |
| | (0.107) | (0.102) | (0.0662) | (0.0674) |
| Observations | 180 | 180 | 233 | 233 |
| Controls for Human SL | | | | |

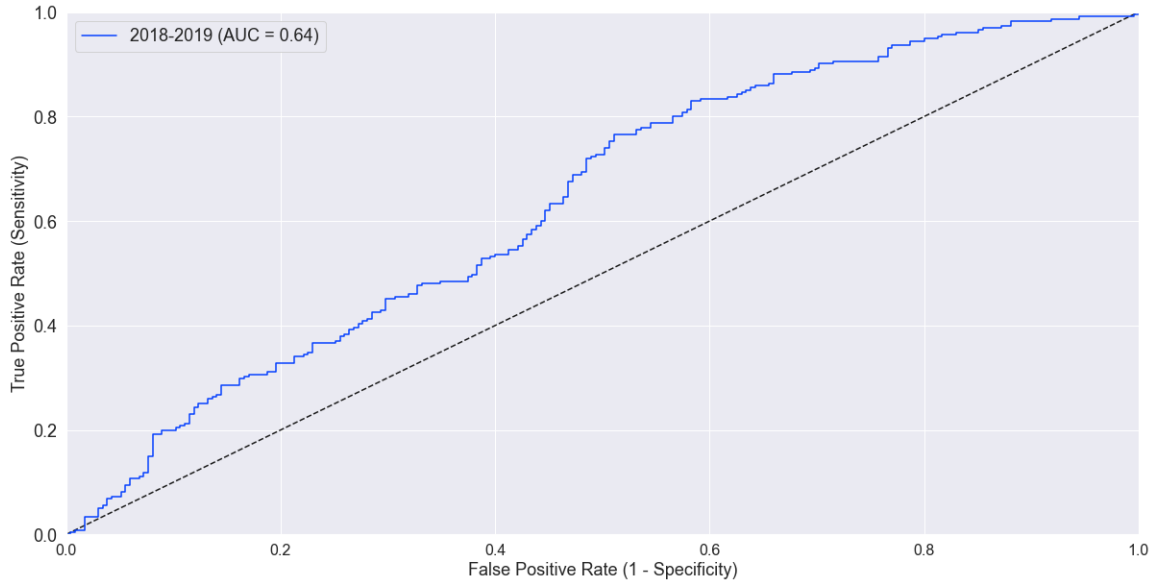
D. UCB Scores

| | Top Rating | | Promoted | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| UCB Score | -0.0556 | -0.0601 | 0.118** | 0.121** |
| | (0.103) | (0.0966) | (0.0535) | (0.0543) |
| Observations | 180 | 180 | 233 | 233 |
| Controls for Human SL | | X | | X |

Notes: This table presents the results of regressing measures of on-the-job performance on algorithm scores, for the sample of applicants who are hired and for which we have available information on the relevant performance metric. "High performance rating" refers to receiving a 3 on a scale of 1-3 in a mid-year evaluation. Controls for ML scores refers to linear controls for static SL, updating SL, and UCB scores. Controls for Human SL refer to controls for our estimates of an applicant's likelihood of being interviewed. Robust standard errors shown in parentheses.

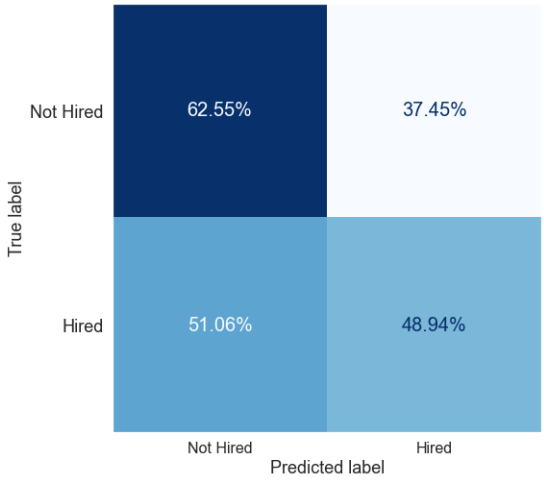
# Appendix Materials – For Online Publication

FIGURE A.1: MODEL PERFORMANCE: PREDICTING HIRING, CONDITIONAL ON RECEIVING AN INTERVIEW



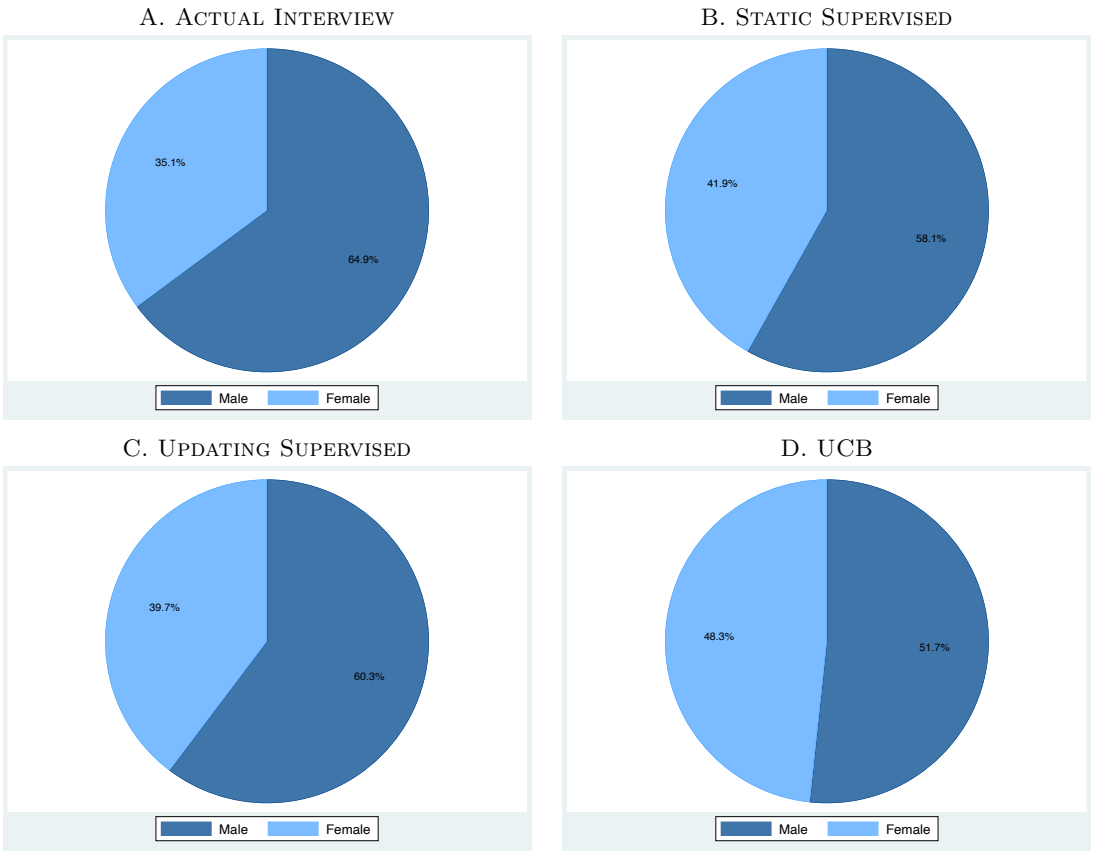

NOTES: This figure shows the Receiver-Operating Characteristic (ROC) curve for the baseline static supervised learning model, which predicts hiring potential. The ROC curve plots the false positive rate on the $x$-axis and the true positive rate on the $y$-axis. For reference, the 45 degree line is shown with a black dash in each plot. All data come from the firm's application and hiring records.

FIGURE A.2: CONFUSION MATRIX MODEL PERFORMANCE: PREDICTING HIRING, CONDITIONAL ON RECEIVING AN INTERVIEW



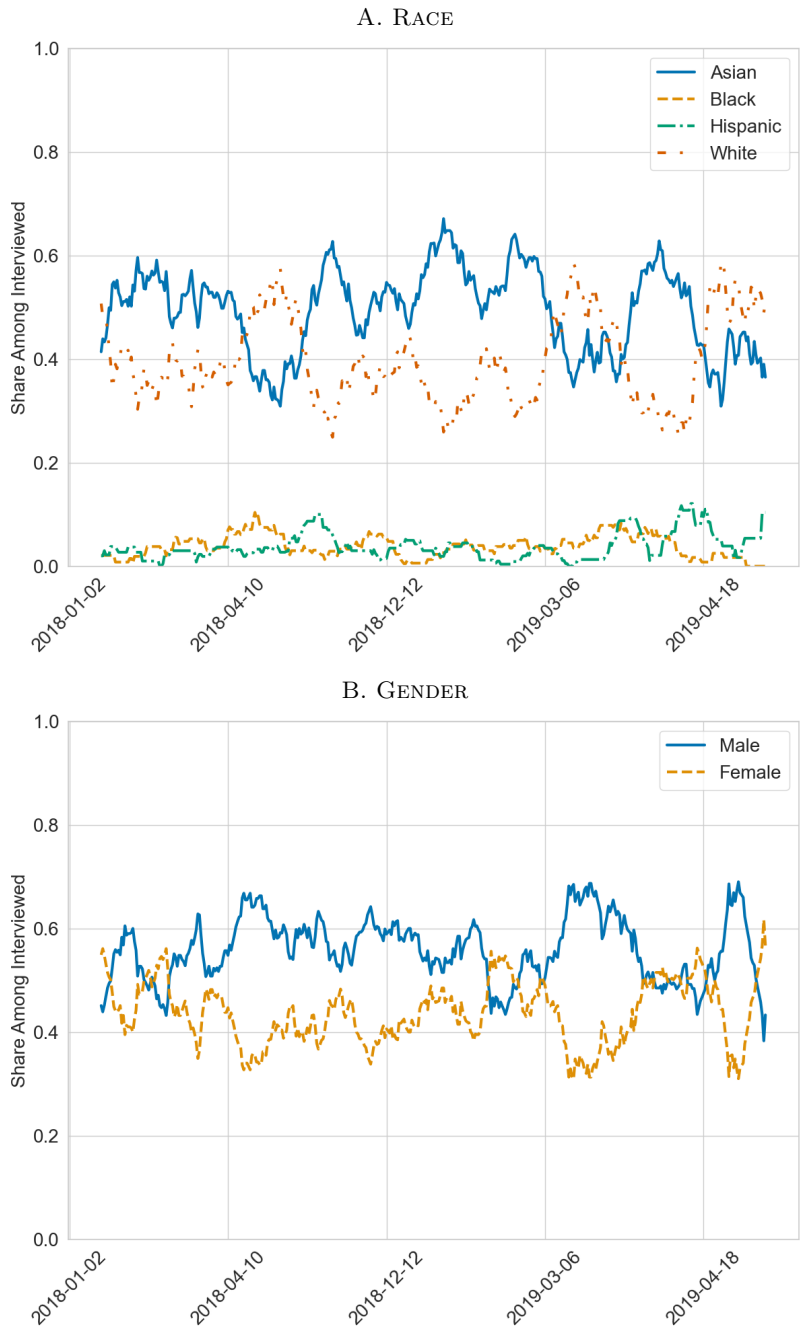

NOTES: This figure shows a confusion matrix for the baseline static supervised learning model, which predicts hiring potential. The confusion plots the predicted label on the the $x$-axis and the true label rate on the $y$-axis. Correctly classified applicants are in the top left cell, "true positives" and bottom right, "true negatives". Examples that are incorrectly classified are in the top left cell ("false positives") and the bottom right ("false negatives"). All data come from the firm's application and hiring records.

FIGURE A.3: GENDER COMPOSITION



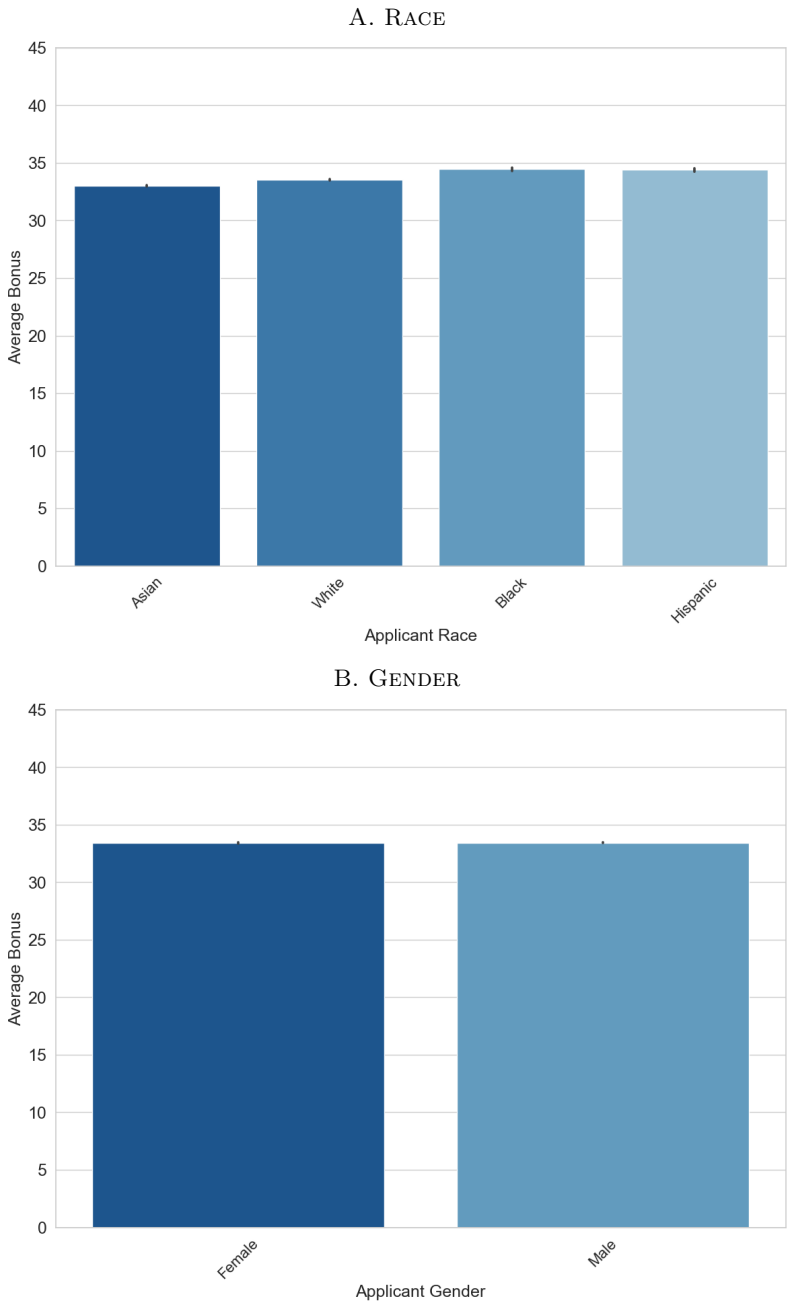

NOTES: Panel A shows the gender composition of applicants actually selected for an interview by the firm. Panel B shows the composition of those who would be selected if chosen by the static supervised learning algorithm described in Equation (2). Panel C shows the gender composition of applicants who would be selected if chosen by the updating supervised learning algorithm described in Equation (3). Finally, Panel D shows the composition of applicants who would be selected for an interview by the UCB algorithm described in Equation (4). All data come from the firm's application and hiring records.

FIGURE A.4: UCB COMPOSITION OF SELECTED CANDIDATES, OVER TIME



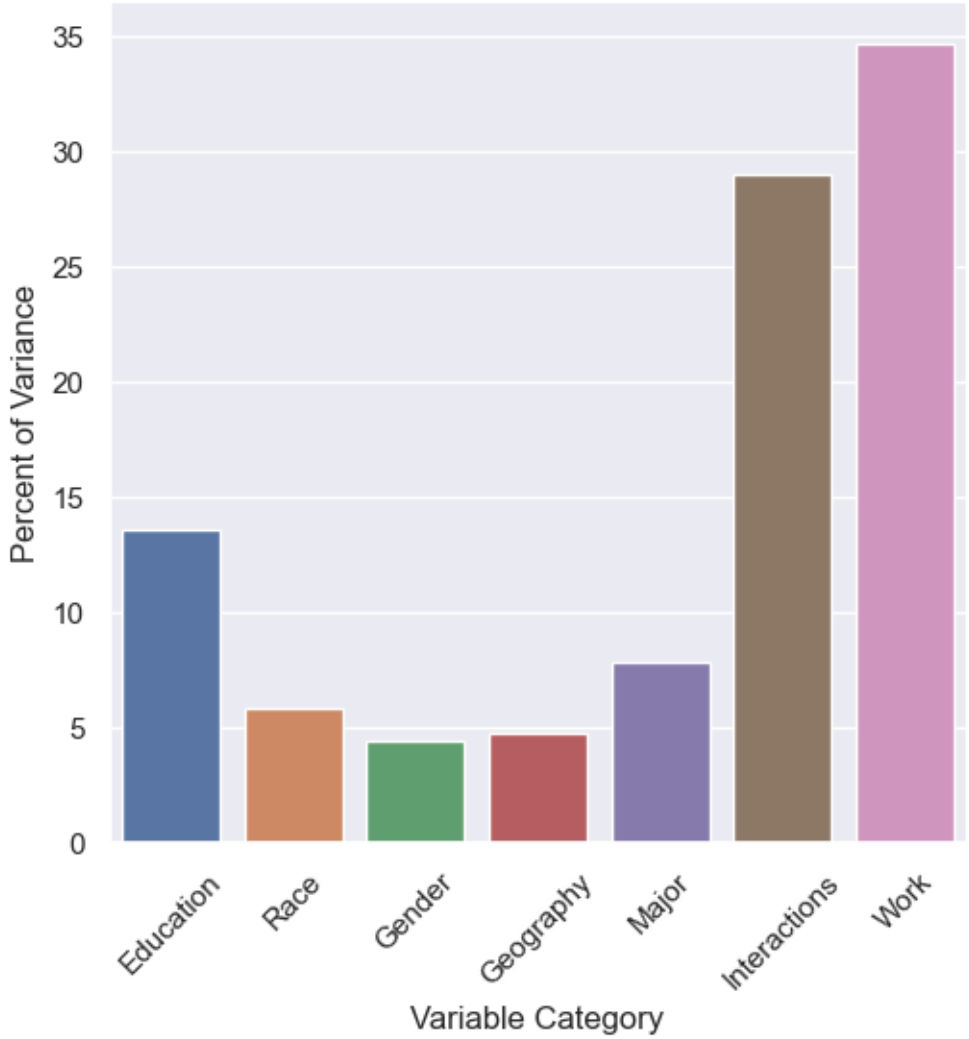

NOTES: This figure shows the composition of applicants selected to be interviewed by the UCB model at each point during the test period. Panel A focuses on race while Panel B focuses on gender.

FIGURE A.5: UCB BONUSES



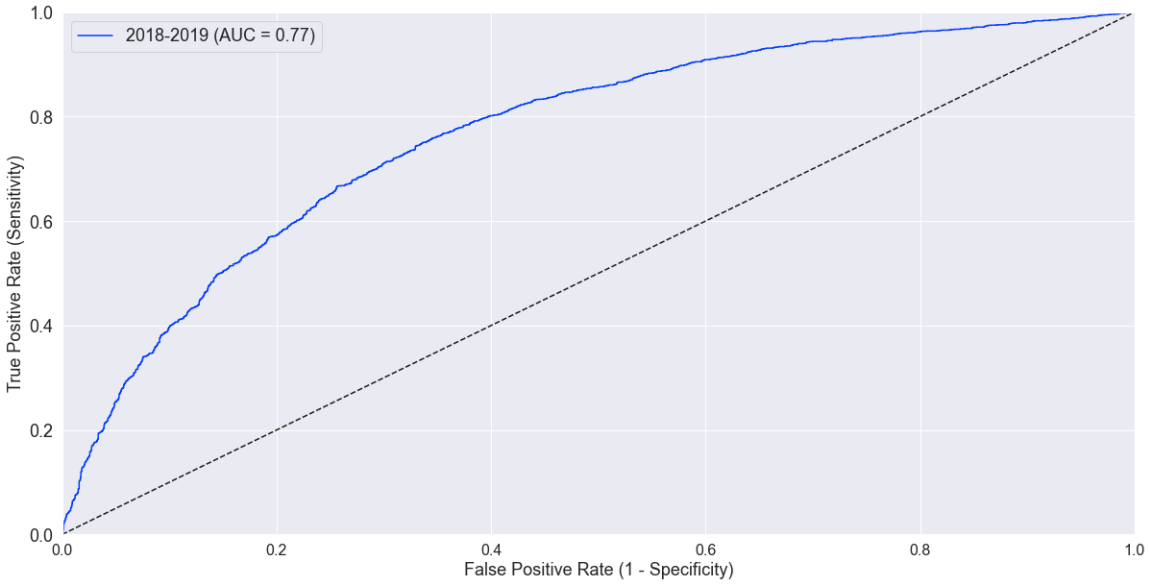

NOTES: This figure shows UCB exploration bonuses averaged over the testing period. Panel A focuses on race while Panel B focuses on gender.

FIGURE A.6: DRIVERS OF VARIATION IN EXPLORATION BONUSES



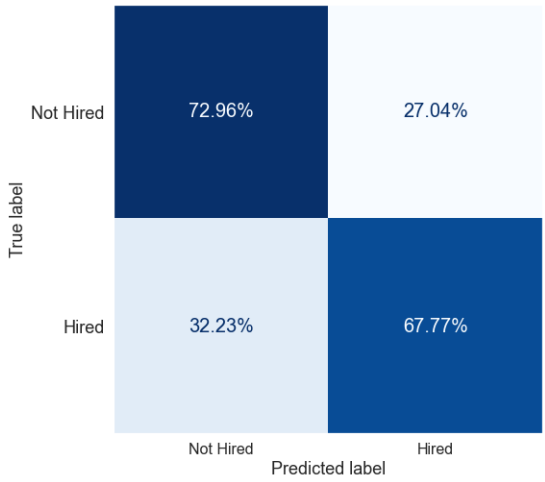

NOTES: This figure shows the percent of applicant covariate-driven variation in exploration bonuses associated with various categories of applicant features. Education refers to information such as college degree and ranking of college attended. Geography captures the geographic location of educational experience, such as India, China or the US. Major includes the coding of majors for each educational degree above high school. Work includes information on previous work experience, such as whether an applicant has experience in a Fortune 500 firm. The interactions category includes race and gender by degree and ranking of college or university.

FIGURE A.7: MODEL PERFORMANCE: PREDICTING INTERVIEW SELECTION



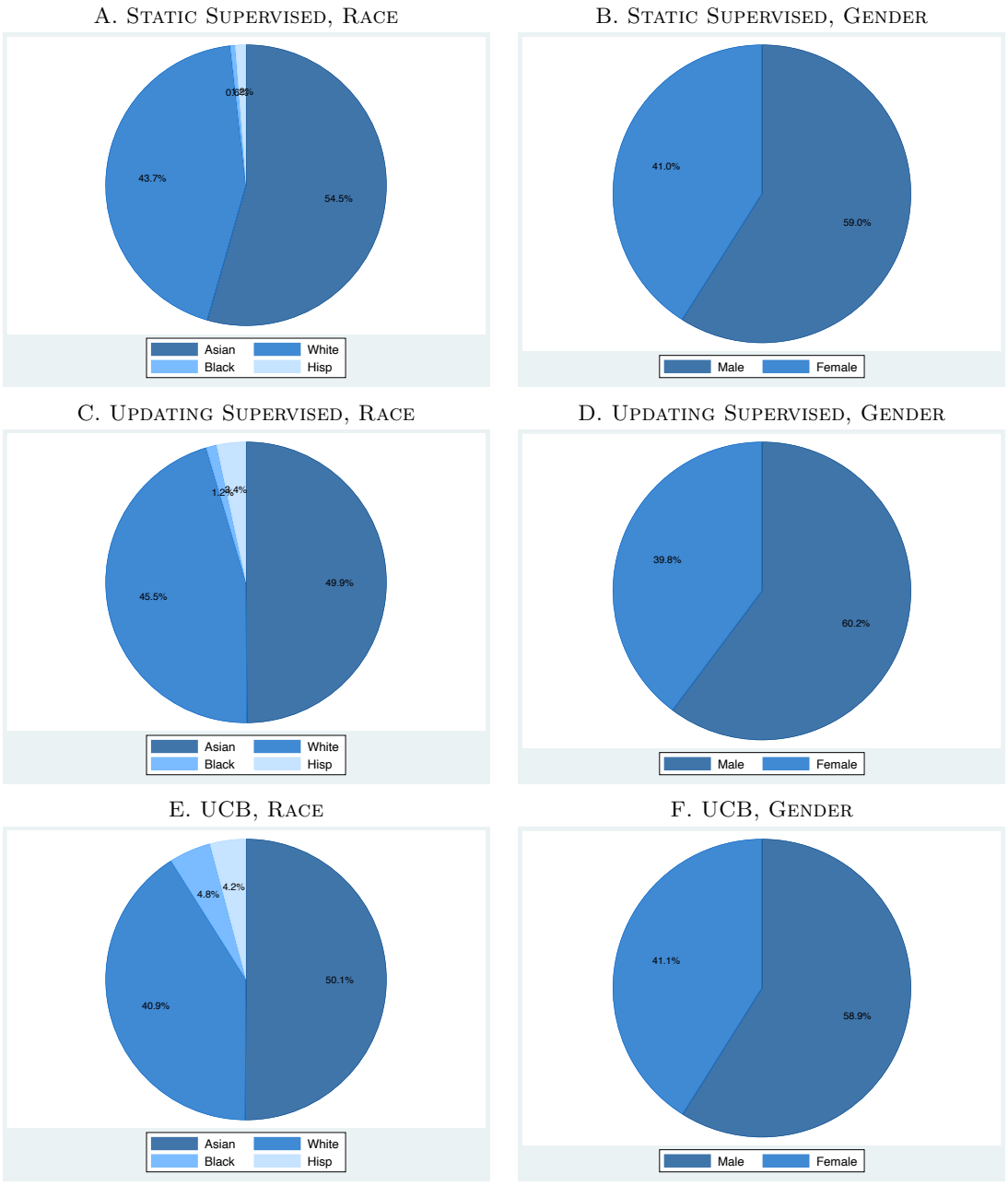

NOTES: This figure shows Receiver-Operating Characteristic (ROC) curve for the human decision making model, which is trained to predict an applicant's likelihood of being selected for an interview. The ROC curve plots the false positive rate on the $x$-axis and the true positive rate on the $y$-axis. For reference, the 45 degree line is shown with a black dash in each plot. All data come from the firm's application and hiring records.

FIGURE A.8: CONFUSION MATRIX MODEL PERFORMANCE: PREDICTING INTERVIEW SELECTION




NOTES: This figure shows a confusion matrix for the human decision making model, which is trained to predict an applicant's likelihood of being selected for an interview. The confusion plots the predicted label on the the $x$-axis and the true label rate on the $y$-axis. Correctly classified applicants are in the top left cell, "true positives" and bottom right, "true negatives". Examples that are incorrectly classified are in the top left cell ("false positives") and the bottom right ("false negatives"). All data come from the firm's application and hiring records.

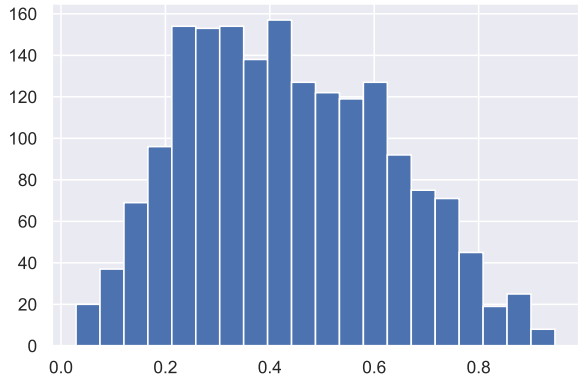
Figure A.9: Demographic Diversity: Selecting Top 50% Among Interviewed



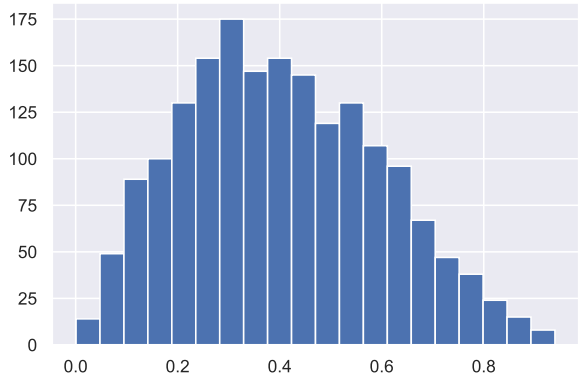

Notes: These panels consider the demographic diversity of candidates, selecting amongst the interviewed candidates. Here, we consider the scenario in which we select the top half of candidates as ranked by each ML score: static supervised learning, updating supervised learning, and contextual bandit upper confidence bound. Results are similar if we use other selection rules. All data come from the firm's application and hiring records.

FIGURE A.10: DISTRIBUTION OF HUMAN SELECTION PROPENSITY, AMONG ML-SELECTED APPLICANTS

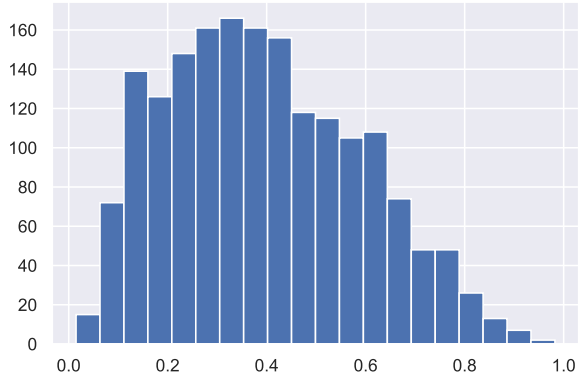
A. STATIC SL SELECTED CANDIDATES




B. UPDATING SL SELECTED CANDIDATES

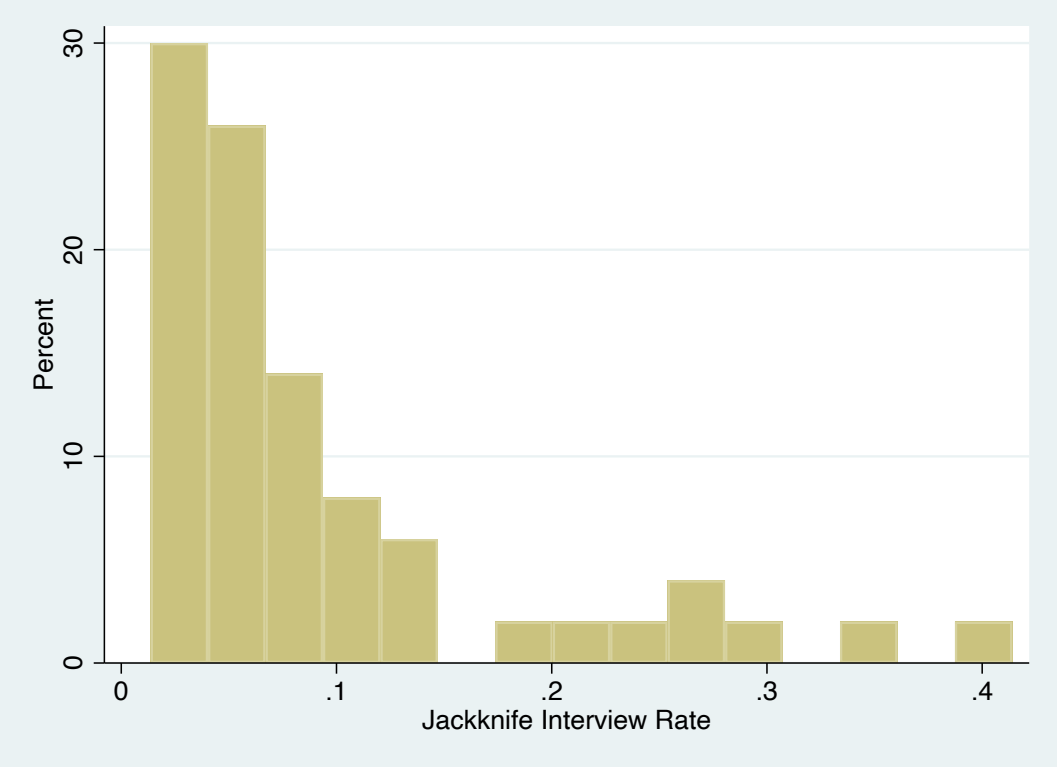



C. UCB SELECTED CANDIDATES



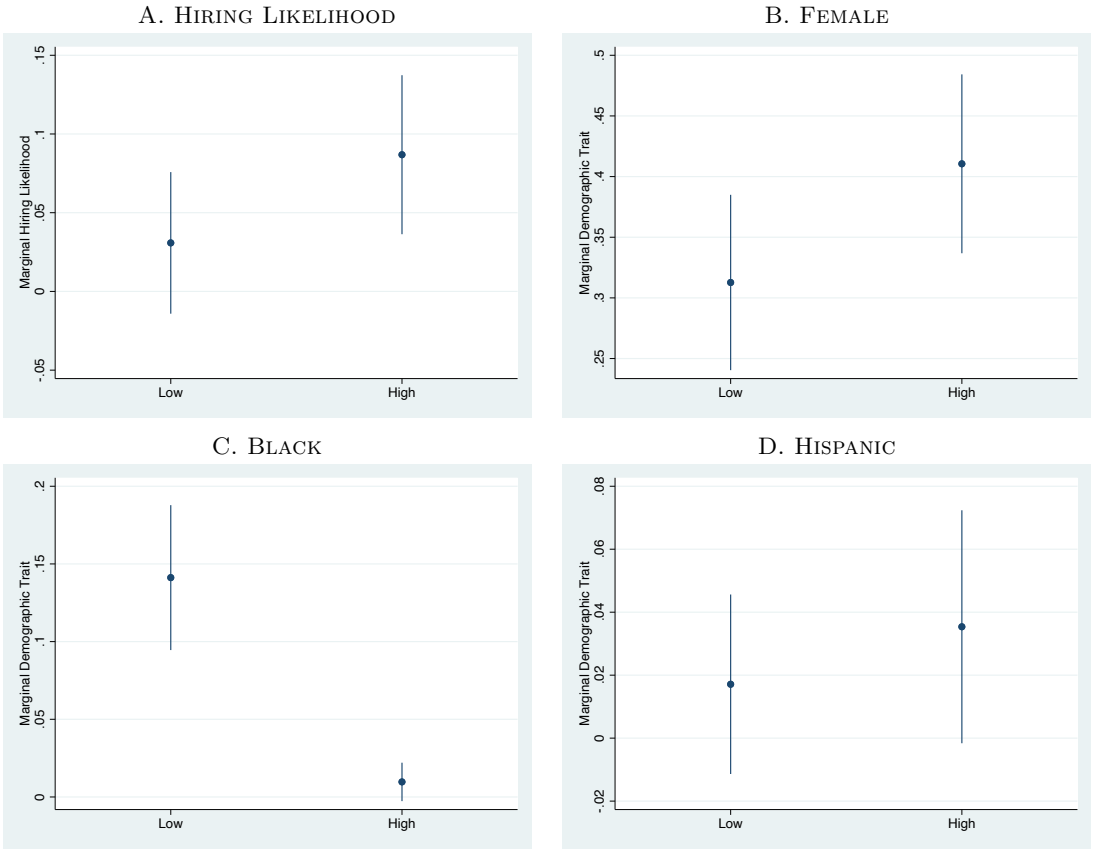

NOTES: This figure shows the distribution of propensity scores for selection into the interview set, $p(I = 1|X)$, by human recruiters under three different algorithmic selection strategies: static SL, updating SL and UCB. In each panel, we plot the distribution of the propensity scores for set of applicants selected by $I^{SSL}$, $I^{USL}$ and $I^{UCB}$.

FIGURE A.11: DISTRIBUTION OF INTERVIEW RATES




NOTES: This histogram shows the distribution of jack-knife interview rates for the 54 screeners in our data who evaluate more than 50 applicants. All data come from the firm's application and hiring records.

FIGURE A.12: CHARACTERISTICS OF MARGINAL INTERVIEWEES, BY UPDATING SUPERVISED SCORE




NOTES: Each panel in this figure shows the results of estimating the characteristics of applicants interviewed on the margin. In each panel, these characteristics are estimated separately for applicants in the top and bottom half of the updating SL algorithm's score. In Panel A, the $y$-axis is the average hiring likelihood of marginally interviewed candidates; the $y$-axis in Panel B is proportion of marginally interviewed candidates who are female; Panels C and D examine the share of Black and Hispanic applicants, respectively. The confidence intervals shown in each panel are derived from robust standard errors clustered at the recruiter level.

FIGURE A.13: DYNAMIC UPDATING, DECREASED QUALITY


NOTES: This figure shows the share of applicants recommended for interviews under four different algorithmic selection strategies: static SL, updating SL, UCB, and the beliefs component of UCB (that is, the $\hat{E}_t[H|X; D_t^{UCB}]$ term in Equation (4)). In each panel, the $y$-axis graphs the share of "evaluation cohort" (2019) applicants who would be selected under each simulation. Panel A plots the share of evaluation cohort Black applicants who would be selected under the simulation in which the hiring potential of Black candidates decreases linearly over the course of 2018, to $H = 0$. Panel B shows results from a simulation in which the hiring potential of Hispanic candidates in 2018 decreases in the same manner. Similarly, Panels C and D show results from simulations in which the hiring potential of White and Asian applicants decreases, respectively.

TABLE A.1: APPLICANT FEATURES AND SUMMARY STATISTICS

| Variable | Mean Training | Mean Test | Mean Overall |
|---|---|---|---|
| Worked at a Fortune 500 Co. | 0.02 | 0.02 | 0.02 |
| Has a Quantitative Background | 0.23 | 0.27 | 0.25 |
| Attended School in China | 0.07 | 0.08 | 0.08 |
| Attended School in Europe | 0.05 | 0.05 | 0.05 |
| Attended School in India | 0.21 | 0.24 | 0.22 |
| Attended School in Latin America | 0.01 | 0.01 | 0.01 |
| Attended School in Middle East/Africa | 0.01 | 0.02 | 0.02 |
| Attended School in Other Asian Country | 0.02 | 0.02 | 0.02 |
| Attended Elite International School | 0.09 | 0.10 | 0.10 |
| Attended US News Top 25 Ranked College | 0.14 | 0.14 | 0.14 |
| Attended US News Top 50 Ranked College | 0.27 | 0.28 | 0.28 |
| Military Experience | 0.04 | 0.04 | 0.04 |
| Number of Applications | 3.5 | 3.8 | 3.5 |
| Number of Unique Degrees | 1.7 | 1.75 | 1.7 |
| Number of Work Histories | 3.8 | 4.0 | 3.9 |
| Has Service Sector Experience | 0.01 | 0.01 | 0.01 |
| Major Description Business Management | 0.17 | 0.15 | 0.17 |
| Major Description Computer Science | 0.14 | 0.13 | 0.14 |
| Major Description Finance/Economics | 0.14 | 0.13 | 0.14 |
| Major Description Engineering | 0.06 | 0.06 | 0.06 |
| Major Description None | 0.20 | 0.25 | 0.22 |
| Observations | 48,719 | 39,947 | 88,666 |

NOTES: This table shows more information on applicants' characteristics, education histories, and work experience. The sample in Column 1 consists of all applicants who applied to a position during our training period (2016 and 2017). Column 2 consists of applicants who applied during the test period (2018 to Q1 2019). Column 3 presents summary statistics for the full pooled sample. All data come from the firm's application and hiring records.

TABLE A.2: CORRELATIONS BETWEEN ALGORITHM SCORES AND HIRING LIKELIHOOD

| | Hired | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Human | -0.0652** | | | |
| | (0.0280) | | | |
| Static SL | | 0.171*** | | |
| | | (0.0266) | | |
| Updating SL | | | 0.196*** | |
| | | | (0.0261) | |
| UCB | | | | 0.229*** |
| | | | | (0.0261) |
| Observations | 2275 | 2275 | 2275 | 2275 |
| Mean of DV: .102 | | | | |

NOTES: This table presents the results of regressing an indicator for being hired on the algorithm scores on the sample of interviewed applicants in the test period. Control variables include fixed effects for job family, application year-month, and seniority level. All data come from the firm's application and hiring records. Robust standard errors shown in parentheses.

TABLE A.3: INSTRUMENT VALIDITY

| | Interviewed | Black | Hisp. | Asian | White | Female | MA | Ref. |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Interviewer | 0.0784*** | 0.000767 | -0.000234 | 0.00812 | -0.00939 | -0.000348 | -0.00888 | 0.00987 |
| Leniency | (0.00881) | (0.00439) | (0.00221) | (0.0108) | (0.00740) | (0.00461) | (0.0104) | (0.00814) |
| Observations | 26281 | 26281 | 26281 | 26281 | 26281 | 26281 | 26281 | 26281 |

NOTES: This table shows the results of regressing applicant characteristics on interviewer leniency, defined as the jack-knife mean-interview rate for the recruiter assigned to an applicant, controlling for fixed effects for job family, management level, application year and location of the job opening. This leave-out mean is standardized to be mean zero and standard deviation one. The outcome in Column 1 is an indicator variable for being interviewed. The outcomes in Columns (2)–(8) are indicators for baseline characteristics of the applicant. The sample is restricted to recruiters who screened at least 50 applicants. All data come from the firm's application and hiring records. Standard errors are clustered at the recruiter level.

Table A.4: Average Monotonicity Test

| | (1) All | (2) White | (3) Black | (4) Asian | (5) Hispanic | (6) Female | (7) Male | (8) MA | (9) Ref. |
|---|---|---|---|---|---|---|---|---|---|
| Interviewer Leniency | 0.08*** | 0.08*** | 0.06*** | 0.09*** | 0.04 | 0.08*** | 0.08*** | 0.08*** | 0.10*** |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) |
| Observations | 23803 | 7055 | 2150 | 13565 | 1097 | 8825 | 16575 | 16384 | 3245 |

Standard errors in parentheses

* p¡0.10, ** p¡0.05, *** p¡0.01

Notes: This table presents results of regressing an applicant's interview status on their jack-knifed screener leniency instrument, by subgroups defined by race, education and gender characteristics. Each column presents the coefficient on the interviewer leniency variable for that subgroup regression. All specifications include controls for job type, job seniority level, work location and application date. Standard errors are clustered at the screener level. We find that being assigned to a lenient screener is on average related to propenisty to get an interview benefits applicants across demographic groups.

TABLE A.5: CORRELATION OF PREFERENCES OF LENIENT AND STRICT SCREENERS

| | Within-Person Correlation Strict and Lenient Selection Scores |
|---|---|
| All | 0.688 |
| White | 0.836 |
| Black | 0.860 |
| Asian | 0.702 |
| Hispanic | 0.760 |
| Female | 0.703 |
| Male | 0.686 |
| MA | 0.686 |
| Referral | 0.689 |

NOTES: This table presents the results of another test of monotonicity. Here, we predict two propensity scores for every applicant: that applicant's likelihood of being selected by a strict screener and that applicant's likelihood of being selected by a lenient screener. We then examine the correlation between these two scores across subgroups in order to ask whether the preferences of lenient and strict screeners are correlated. To do this, we randomly split our sample into a train and test set for applicants assigned to strict screeners and lenient screeners (above and below the median jack-knifed leniency). We train a regularized logit model that predicts interview propensity for the set of strict screeners and a second model on lenient screeners. During our testing period, we generate an out-of-sample predicted probability of interview on all applicants using the strict interviewer model and the lenient screener model. We find that the correlation between the predicted probability of interview under the lenient screener and the strict screeners is positive across race, gender, and education groups.