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Incentivized Resume Rating: Eliciting Employer Preferences without Deception

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Abstract

We introduce a new experimental paradigm to evaluate employer preferences, called Incentivized Resume Rating (IRR). Employers evaluate resumes they know to be hypothetical in order to be matched with real job seekers, preserving incentives while avoiding the deception necessary in audit studies. We deploy IRR with employers recruiting college seniors from a prestigious school, randomizing human capital characteristics and demographics of hypothetical candidates. We measure both employer preferences for candidates and employer beliefs about the likelihood candidates will accept job offers, avoiding a typical confound in audit studies. We discuss the costs, benefits, and future applications of this new methodology.

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1 Introduction

How labor markets reward education, work experience, and other forms of human capital is of fundamental interest in labor economics and the economics of education (e.g., Autor and Houseman [2010], Pallais [2014]). Similarly, the role of discrimination in labor markets is a key concern for both policy makers and economists (e.g., Altonji and Blank [1999], Lang and Lehmann [2012]). Correspondence audit studies, including resume audit studies, have become powerful tools to answer questions in both domains.[1] These studies have generated a rich set of findings on discrimination in employment (e.g., Bertrand and Mullainathan [2004]), real estate and housing (e.g., Hanson and Hawley [2011], Ewens et al. [2014]), retail (e.g., Pope and Sydnor [2011], Zussman [2013]), and other settings (see Bertrand and Duflo [2016]). More recently, resume audit studies have been used to investigate how employers respond to other characteristics of job candidates, including unemployment spells [Kroft et al., 2013, Eriksson and Rooth, 2014, Nunley et al., 2017], for-profit college credentials [Darolia et al., 2015, Deming et al., 2016], college selectivity [Gaddis, 2015], and military service [Kleykamp, 2009].

Despite the strengths of this workhorse methodology, however, resume audit studies are subject to two major concerns. First, they use deception, generally considered problematic within economics [Ortmann and Hertwig, 2002, Hamermesh, 2012]. Employers in resume audit studies waste time evaluating fake resumes and pursuing non-existent candidates. If fake resumes systematically differ from real resumes, employers could become wary of certain types of resumes sent out by researchers, harming both the validity of future research and real job seekers whose resumes are similar to those sent by researchers. These concerns about deception become more pronounced as the method becomes more popular.[2] To our knowledge, audit and correspondence audit studies are the only experiments within economics for which deception has been permitted, presumably because of the importance of the underlying research questions and the absence of a method to answer them without deception.

A second concern arising from resume audit studies is their use of "callback rates" (i.e., the rates at which employers call back fake candidates) as the outcome measure that proxies for employer

[1] Resume audit studies send otherwise identical resumes, with only minor differences associated with a treatment (e.g., different names associated with different races), to prospective employers and measure the rate at which candidates are called back by those employers (henceforth the "callback rate"). These studies were brought into the mainstream of economics literature by Bertrand and Mullainathan [2004]. By comparing callback rates across groups (e.g., those with white names to those with minority names), researchers can identify the existence of discrimination. Resume audit studies were designed to improve upon traditional audit studies of the labor market, which involved sending matched pairs of candidates (e.g., otherwise similar study confederates of different races) to apply for the same job and measure whether the callback rate differed by race. These traditional audit studies were challenged on empirical grounds for not being double-blind [Turner et al., 1991] and for an inability to match candidate characteristics beyond race perfectly [Heckman and Siegelman, 1992, Heckman, 1998].

[2] Baert [2018] notes 90 resume audit studies focused on discrimination against protected classes in labor markets alone between 2005 and 2016. Many studies are run in the same venues (e.g., specific online job boards), making it more likely that employers will learn to be skeptical of certain types of resumes. These harms might be particularly relevant if employers become aware of the existence of such research. For example, employers may know about resume audit studies since they can be used as legal evidence of discrimination [Neumark, 2012].

interest in candidates. Since recruiting candidates is costly, firms may be reluctant to pursue candidates who will be unlikely to accept a position if offered. Callback rates may therefore conflate an employer's interest in a candidate with the employer's expectation that the candidate would accept a job if offered one.[3] This confound might contribute to counterintuitive results in the resume audit literature. For example, resume audit studies typically find higher callback rates for unemployed than employed candidates [Kroft et al., 2013, Nunley et al., 2017, 2014, Farber et al., 2018], results that seem much more sensible when considering this potential role of job acceptance. In addition, callback rates can only identify preferences at one point in the quality distribution (i.e., at the threshold at which employers decide to call back candidates). While empirically relevant, results at this callback threshold may not be generalizable [Heckman, 1998, Neumark, 2012]. To better understand the underlying structure of employer preferences, we may also care about how employers respond to candidate characteristics at other points in the distribution of candidate quality.

In this paper, we introduce a new experimental paradigm, called Incentivized Resume Rating (IRR), which avoids these concerns. Instead of sending fake resumes to employers, IRR invites employers to evaluate resumes known to be hypothetical—avoiding deception—and provides incentives by matching employers with real job seekers based on employers' evaluations of the hypothetical resumes. Rather than relying on binary callback decisions, IRR can elicit much richer information about employer preferences; any information that can be used to improve the quality of the match between employers preferences and real job seekers can be elicited from employers in an incentivized way. In addition, IRR gives researchers the ability to elicit a single employer's preferences over multiple resumes, to randomize many candidate characteristics simultaneously, and to collect supplemental data about the employers reviewing resumes and their firms. Finally, IRR allows researchers to study employers who would not respond to unsolicited resumes.

We deploy IRR in partnership with the University of Pennsylvania (Penn) Career Services office to study the preferences of employers hiring graduating seniors through on-campus recruiting. This market has been unexplored by the resume audit literature since firms in this market hire through their relationships with schools rather than by responding to cold resumes. Our implementation of IRR asked employers to rate hypothetical candidates on two dimensions: (1) how interested they would be in hiring the candidate and (2) the likelihood that the candidate would accept a job offer if given one. In particular, employers were asked to report their interest in hiring a candidate on a 10-point Likert scale under the assumption that the candidate would accept the job if offered—mitigating concerns about a confound related to the likelihood of accepting the job. Employers were additionally asked the likelihood the candidate would accept a job offer on a 10-point Likert scale. Both responses were used to match employers with real Penn graduating seniors.

[3] Researchers who use audit studies aim to mitigate such concerns through the content of their resumes (e.g., Bertrand and Mullainathan [2004] notes that the authors attempted to construct high-quality resumes that did not lead candidates to be "overqualified," page 995).

We find that employers value higher grade point averages as well as the quality and quantity of summer internship experiences. Employers place extra value on prestigious and substantive internships but do not appear to value summer jobs that Penn students typically take for a paycheck, rather than to develop human capital for a future career, such as barista, server, or cashier. This result suggests a potential benefit on the post-graduate job market for students who can afford to take unpaid or low-pay internships during the summer rather than needing to work for an hourly wage.

Our granular measure of hiring interest allows us to consider how employer preferences for candidate characteristics respond to changes in overall candidate quality. Most of the preferences we identify maintain sign and significance across the distribution of candidate quality, but we find that responses to major and work experience are most pronounced towards the middle of the quality distribution and smaller in the tails.

While we do not find that employers are more or less interested in female and minority candidates on average, we find some evidence of discrimination against white women and minority men among employers looking to hire candidates with Science, Engineering, and Math majors.[4]

The employers in our study report having a positive preference for diversity in hiring.[5] In addition, employers report that white female candidates are less likely to accept job offers than their white male counterparts, suggesting a novel channel for discrimination.

Of course, the IRR method also comes with some drawbacks. First, while we attempt to directly identify employer interest in a candidate, our Likert-scale measure is not a step in the hiring process and thus—in our implementation of IRR—we cannot draw a direct link between our Likert-scale measure and hiring outcomes. However, we imagine future IRR studies could make advances on this front (e.g., by asking employers to guarantee interviews to matched candidates). Second, because the incentives in our study are similar but not identical to those in the hiring process, we cannot be sure that employers evaluate our hypothetical resumes with the same rigor or using the same criteria as they would real resumes. Again, we hope future work might validate that the time and attention spent on resumes in the IRR paradigm is similar to resumes evaluated as part of standard recruiting processes.

Our implementation of IRR was the first of its kind and thus left room for improvement on a few fronts. For example, as discussed in detail in Section 4, we attempted to replicate our study at the University of Pittsburgh to evaluate preferences of employers more like those traditionally targeted by resume audit studies. We underestimated how much Pitt employers needed candidates

[4]We find suggestive evidence that discrimination in hiring interest is due to implicit bias by observing how discrimination changes as employers evaluate multiple resumes. In addition, consistent with results from the resume audit literature finding lower returns to quality for minority candidates (see Bertrand and Mullainathan [2004]), we also find that—relative to white males—other candidates receive a lower return to work experience at prestigious internships.

[5]In a survey employers complete after evaluating resumes in our study, over 90% of employers report that both "seeking to increase gender diversity / representation of women" and "seeking to increase racial diversity" factor into their hiring decisions, and 82% of employers rate both of these factors at 5 or above on a Likert scale from 1 = "Do not consider at all" to 10 = "This is among the most important things I consider."

with specific majors and backgrounds, however, and a large fraction of resumes that were shown to Pitt employers were immediately disqualified based on major. This mistake resulted in highly attenuated estimates. Future implementations of IRR should more carefully tailor the variables for their hypothetical resumes to the needs of the employers being studied. We emphasize other lessons from our implementation in Section 5.

Despite the limitations of IRR, our results highlight that the method can be used to elicit employer preferences and suggest that it can also be used to detect discrimination. Consequently, we hope IRR provides a path forward for those interested in studying labor markets without using deception. The rest of the paper proceeds as follows: Section 2 describes in detail how we implement our IRR study; Section 3 reports on the results from Penn and compares them to extant literature; Section 4 describes our attempted replication at Pitt; and Section 5 concludes.

2 Study Design

In this section, we describe our implementation of IRR, which combines the incentives and ecological validity of the field with the control of the laboratory. In Section 2.1, we outline how we recruit employers who are in the market to hire elite college graduates. In Section 2.2, we describe how we provide employers with incentives for reporting preferences without introducing deception. In Section 2.3, we detail how we created the hypothetical resumes and describe the extensive variation in candidate characteristics that we included in the experiment, including grade point average and major (see 2.3.1), previous work experience (see 2.3.2), skills (see 2.3.3), and race and gender (see 2.3.4). In Section 2.4, we highlight the two questions that we asked subjects about each hypothetical resume, which allowed us to get a granular measure of interest in a candidate without a confound from the likelihood that the candidate would accept a job if offered.

2.1 Employers and Recruitment

IRR allows researchers to recruit employers in the market for candidates from particular institutions and those who do not screen unsolicited resumes and thus may be hard—or impossible—to study in audit or resume audit studies. To leverage this benefit of the experimental paradigm, we partnered with the University of Pennsylvania (Penn) Career Services office to identify employers recruiting highly skilled generalists from the Penn graduating class.

Penn Career Services sent invitation emails (see Appendix Figure A.1 for recruitment email) in two waves during the 2016-2017 academic year to employers who historically recruited Penn seniors (e.g., firms that recruited on campus, regularly attended career fairs, or otherwise hired students). The first wave was around the time of on-campus recruiting in the fall of 2016. The second wave was around the time of career-fair recruiting in the spring of 2017. In both waves, the recruitment email invited employers to use "a new tool that can help you to identify potential job candidates." While the recruitment email and the information that employers received before rating resumes (see

Appendix Figure A.3 for instructions) noted that anonymized data from employer responses would be used for research purposes, this was framed as secondary. The recruitment process and survey tool itself both emphasized that employers were using new recruitment software. For this reason, we note that our study has the ecological validity of a field experiment.[6] As was outlined in the recruitment email (and described in detail in Section 2.2), each employer's one and only incentive for participating in the study is to receive 10 resumes of job seekers that match the preferences they report through rating the hypothetical resumes.

2.2 Incentives

The main innovation of IRR is its method for incentivized preference elicitation, a variant of a method pioneered by Low [2017] in a different context. In its most general form, the method asks subjects to evaluate candidate profiles, which are known to be hypothetical, with the understanding that more accurate evaluations will maximize the value of their participation incentive. In our implementation of IRR, each employer evaluates 40 hypothetical candidate resumes and their participation incentive is a packet of 10 resumes of real job seekers from a large pool of Penn seniors. For each employer, we select the 10 real job seekers based on the employer's evaluations.[7] Consequently, the participation incentive in our study becomes more valuable as employers' evaluations of candidates better reflect their true preferences for candidates.[8]

A key design decision to help ensure subjects in our study truthfully and accurately report their preferences is that we provide no additional incentive (i.e., beyond the resumes of the 10 real job seekers) for participating in the study, which took a median of 29.8 minutes to complete. Limiting the incentive to the resumes of 10 job seekers makes us confident that participants value the incentive, since they have no other reason to participate in the study. Since subjects value the incentive, and since the incentive becomes more valuable as preferences are reported more accurately, subjects have good reason to report their preferences accurately.

2.3 Resume Creation and Variation

Our implementation of IRR asked each employer to evaluate 40 unique, hypothetical resumes, and it varied multiple candidate characteristics simultaneously and independently across resumes,

[6] Indeed, the only thing that differentiates our study from a "natural field experiment" as defined by Harrison and List [2004] is that subjects know that academic research is ostensibly taking place, even though it is framed as secondary relative to the incentives in the experiment.

[7] The recruitment email (see Appendix Figure A.1) stated: "the tool uses a newly developed machine-learning algorithm to identify candidates who would be a particularly good fit for your job based on your evaluations." We did not use race or gender preferences when suggesting matches from the candidate pool. The process by which we identify job seekers based on employer evaluations is described in detail in Appendix A.3.

[8] In Low [2017], heterosexual male subjects evaluated online dating profiles of hypothetical women with an incentive of receiving advice from an expert dating coach on how to adjust their own online dating profiles to attract the types of women that they reported preferring. While this type of non-monetary incentive is new to the labor economics literature, it has features in common with incentives in laboratory experiments, in which subjects make choices (e.g., over monetary payoffs, risk, time, etc.) and the utility they receive from those choices is higher as their choices more accurately reflect their preferences.

allowing us to estimate employer preferences over a rich space of baseline candidate characteristics.[9] Each of the 40 resumes was dynamically populated when a subject began the survey tool. As shown in Table 1 and described below, we randomly varied a set of candidate characteristics related to education; a set of candidate characteristics related to work, leadership, and skills; and the candidate's race and gender.

We made a number of additional design decisions to increase the realism of the hypothetical resumes and to otherwise improve the quality of employer responses. First, we built the hypothetical resumes using components (i.e., work experiences, leadership experiences, and skills) from real resumes of seniors at Penn. Second, we asked the employers to choose the type of candidates that they were interested in hiring, based on major (see Appendix Figure A.4). In particular, they could choose either "Business (Wharton), Social Sciences, and Humanities" (henceforth "Humanities & Social Sciences") or "Science, Engineering, Computer Science, and Math" (henceforth "STEM"). They were then shown hypothetical resumes from the set of majors they selected. As described below, this choice affects a wide range of candidate characteristics; majors, internship experiences, and skills on the hypothetical resumes varied across these two major groups. Third, to enhance realism, and to make the evaluation of the resumes less tedious, we used 10 different resume templates, which we populated with the candidate characteristics and component pieces described below, to generate the 40 hypothetical resumes (see Appendix Figure A.5 for a sample resume). We based these templates on real student resume formats (see Appendix Figure A.6 for examples).[10] Fourth, we gave employers short breaks within the study by showing them a progress screen after each block of 10 resumes they evaluated. As described in Section 3.4 and Appendix B.4, we use the change in attention induced by these breaks to construct tests of implicit bias.

2.3.1 Education Information

In the education section of the resume, we independently randomized each candidate's grade point average (GPA) and major. GPA is drawn from a uniform distribution between 2.90 and 4.00, shown to two decimal places and never omitted from the resume. Majors are chosen from a list of Penn majors, with higher probability put on more common majors. Each major was associated with a degree (BA or BS) and with the name of the group or school granting the degree within Penn (e.g., "College of Arts and Sciences"). Appendix Table A.3 shows the list of majors by major category, school, and the probability that the major was used in a resume.

[9] In a traditional resume audit study, researchers are limited in the number of resumes and the covariance of candidate characteristics that they can show to any particular employer. Sending too many fake resumes to the same firm, or sending resumes with unusual combinations of components, might raise suspicion. For example, Bertrand and Mullainathan [2004] send only four resumes to each firm and create only two quality levels (i.e., a high quality resume and a low quality resume, in which various candidate characteristics vary together).

[10] We blurred the text in place of a phone number and email address for all resumes, since we were not interested in inducing variation in those candidate characteristics.

Table 1: Randomization of Resume Components

Resume Component	**Description**	*Analysis Variable*
Personal Information		
First & last name	Drawn from list of 50 possible names given selected race and gender (names in Tables A.1 & A.2) Race drawn randomly from U.S. distribution (65.7% White, 16.8% Hispanic, 12.6% Black, 4.9% Asian) Gender drawn randomly (50% male, 50% female)	*Female, White* (32.85%) *Male, Non-White* (17.15%) *Female, Non-White* (17.15%) *Not a White Male* (67.15%)
Education Information		
GPA	Drawn $Unif[2.90, 4.00]$ to second decimal place	*GPA*
Major	Drawn from a list of majors at Penn (Table A.3)	*Major* (weights in Table A.3)
Degree type	BA, BS fixed to randomly drawn major	*Wharton* (40%)
School within university	Fixed to randomly drawn major	*School of Engineering and Applied Science* (70%)
Graduation date	Fixed to upcoming spring (i.e., May 2017)	
Work Experience		
First job	Drawn from curated list of top internships and regular internships	*Top Internship* (20/40)
Title and employer	Fixed to randomly drawn job	
Location	Fixed to randomly drawn job	
Description	Bullet points fixed to randomly drawn job	
Dates	Summer after candidate's junior year (i.e., 2016)	
Second job	Left blank or drawn from curated list of regular internships and work-for-money jobs (Table A.5)	*Second Internship* (13/40) *Work for Money* (13/40)
Title and employer	Fixed to randomly drawn job	
Location	Fixed to randomly drawn job	
Description	Bullet points fixed to randomly drawn job	
Dates	Summer after candidate's sophomore year (i.e., 2015)	
Leadership Experience		
First & second leadership	Drawn from curated list	
Title and activity	Fixed to randomly drawn leadership	
Location	Fixed to Philadelphia, PA	
Description	Bullet points fixed to randomly drawn leadership	
Dates	Start and end years randomized within college career, with more recent experience coming first	
Skills		
Skills list	Drawn from curated list, with two skills drawn from {Ruby, Python, PHP, Perl} and two skills drawn from {SAS, R, Stata, Matlab} shuffled and added to skills list with probability 25%.	*Technical Skills* (25%)

Resume components are listed in the order that they appear on hypothetical resumes. Italicized variables in the right column are variables that were randomized to test how employers responded to these characteristics. Degree, first job, second job, and skills were drawn from different lists for Humanities & Social Sciences resumes and STEM resumes (except for work-for-money jobs). Name, GPA, work-for-money jobs, and leadership experience were drawn from the same lists for both resume types. Weights of characteristics are shown as fractions when they are fixed across subjects (e.g., each subject saw exactly 20/40 resumes with a *Top Internship*) and percentages when they represent a draw from a probability distribution (e.g., each resume a subject saw had a 32.85% chance of being assigned a white female name).

2.3.2 Work Experience

We included realistic work experience components on the resumes. To generate the components, we scraped more than 700 real resumes of Penn students. We then followed a process described in Appendix A.2.5 to select and lightly sanitize work experience components so that they could be randomly assigned to different resumes without generating conflicts or inconsistencies (e.g., we eliminated references to particular majors or to gender or race). Each work experience component included the associated details from the real resume from which the component was drawn, including an employer, position title, location, and a few descriptive bullet points.

Our goal in randomly assigning these work experience components was to introduce variation along two dimensions: *quantity* of work experience and *quality* of work experience. To randomly assign quantity of work experience, we varied whether the candidate only had an internship in the summer before senior year, or also had a job or internship in the summer before junior year. Thus, candidates with more experience had two jobs on their resume (before junior and senior years), while others had only one (before senior year).

To introduce random variation in *quality* of work experience, we selected work experience components from three categories: (1) "top internships," which were internships with prestigious firms as defined by being a firm that successfully hires many Penn graduates; (2) "work-for-money" jobs, which were paid jobs that—at least for Penn students—are unlikely to develop human capital for a future career (e.g., barista, cashier, waiter, etc.); and (3) "regular" internships, which comprised all other work experiences.[11]

The first level of quality randomization was to assign each hypothetical resume to have either a top internship or a regular internship in the first job slot (before senior year). This allows us to detect the impact of having a higher quality internship.[12]

The second level of quality randomization was in the kind of job a resume had in the second job slot (before junior year), if any. Many students may have an economic need to earn money during the summer and thus may be unable to take an unpaid or low-pay internship. To evaluate whether employers respond differentially to work-for-money jobs, which students typically take for pay, and internships, resumes were assigned to have either no second job, a work-for-money job, or a standard internship, each with (roughly) one-third probability (see Table 1). This variation

[11]See Appendix Table A.4 for a list of top internship employers and Table A.5 for a list of work-for-money job titles. As described in Appendix A.2.5, different internships (and top internships) were used for each major type but the same work-for-money jobs were used for both major types. The logic of varying internships by major type was based on the intuition that internships could be interchangeable within each group of majors (e.g., internships from the Humanities & Social Sciences resumes would not be unusual to see on any other resume from that major group) but were unlikely to be interchangeable across major groups (e.g., internships from Humanities & Social Sciences resumes would be unusual to see on STEM resumes and vice versa). We used the same set of work-for-money jobs for both major types, since these jobs were not linked to a candidate's field of study.

[12]Since the work experience component was comprised of employer, title, location, and description, a higher quality work experience necessarily reflects all features of this bundle; we did not independently randomize the elements of work experience.

allows us to measure the value of having a work-for-money job and to test how it compares to the value of a standard internship.

2.3.3 Leadership Experience and Skills

Each resume included two leadership experiences as in typical student resumes. A leadership experience component includes an activity, title, date range, and a few bullet points with a description of the experience (Philadelphia, PA was given as the location of all leadership experiences). Participation dates were randomly selected ranges of years from within the four years preceding the graduation date. For additional details, see Appendix A.2.5.

With skills, by contrast, we added a layer of intentional variation to measure how employers value technical skills. First, each resume was randomly assigned a list of skills drawn from real resumes. We stripped from these lists any reference to Ruby, Python, PHP, Perl, SAS, R, Stata, and Matlab. With 25% probability, we appended to this list four technical skills: two randomly drawn advanced programming languages from {Ruby, Python, PHP, Perl} and two randomly drawn statistical programs from {SAS, R, Stata, Matlab}.

2.3.4 Names Indicating Gender and Race

We randomly varied gender and race by assigning each hypothetical resume a name that would be indicative of gender (male or female) and race (Asian, Black, Hispanic, or White).[13] To do this randomization, we needed to first generate a list of names that would clearly indicate both gender and race for each of the groups. We used birth records and Census data to generate first and last names that would be highly indicative of race and gender, and combined names within race.[14] The full lists of names are given in Appendix Tables A.1 and A.2 (see Appendix A.2.3 for additional details).

For realism, we randomly selected races at rates approximating the distribution in the US population (65.7% White, 16.8% Hispanic, 12.6% Black, 4.9% Asian). While a more uniform variation in race would have increased statistical power to detect race-based discrimination, such an approach would have risked signaling to subjects our intent to study racial preferences. In our analysis, we pool non-white names to explore potential discrimination of minority candidates.

[13] For ease of exposition, we will refer to race / ethnicity as "race" throughout the paper.

[14] For first names, we used a dataset of all births in the state of Massachusetts between 1989-1996 and New York City between 1990-1996 (the approximate birth range of job seekers in our study). Following Fryer and Levitt [2004], we generated an index for each name of how distinctively the name was associated with a particular race and gender. From these, we generated lists of 50 names by selecting the most indicative names and removing names that were strongly indicative of religion (such as Moshe) or gender ambiguous in the broad sample, even if unambiguous within an ethnic group (such as Courtney, which is a popular name among both black men and white women). We used a similar approach to generating racially indicative last names, assuming last names were not informative of gender. We used last name data from the 2000 Census tying last names to race. We implemented the same measure of race specificity and required that the last name make up at least 0.1% of that race's population, to ensure that the last names were sufficiently common.

2.4 Rating Candidates on Two Dimensions

As noted in the Introduction, audit and resume audit studies generally report results on callback, which has two limitations. First, callback only identifies preferences for candidates at one point in the quality distribution (i.e., at the callback threshold), so results may not generalize to other environments or to other candidate characteristics. Second, while callback is often treated as a measure of an employer's interest in a candidate, there is a potential confound to this interpretation. Since continuing to interview a candidate, or offering the candidate a job that is ultimately rejected, can be costly to an employer (e.g., it may require time and energy and crowd out making other offers), an employer's callback decision will optimally depend on both the employer's interest in a candidate and the employer's belief about whether the candidate will accept the job if offered. If the likelihood that a candidate accepts a job when offered is decreasing in the candidate's quality (e.g., if higher quality candidates have better outside options), employers' actual effort spent pursuing candidates may be non-monotonic in candidate quality. Consequently, concerns about a candidate's likelihood of accepting a job may be a confound in interpreting callback as a measure of interest in a candidate.[15]

An advantage of the IRR methodology is that researchers can ask employers to provide richer, more granular information than a binary measure of callback. We leveraged this advantage to ask two questions, each on a Likert scale from 1 to 10. In particular, for each resume we asked employers to answer the following two questions (see an example at the bottom of Appendix Figure A.5):

1. "How interested would you be in hiring **[Name]**?"
   (1 = "Not interested"; 10 = "Very interested")

2. "How likely do you think **[Name]** would be to accept a job with your organization?"
   (1 = "Not likely"; 10 = "Very likely")

In the instructions (see Appendix Figure A.3), employers were specifically told that responses to both questions would be used to generate their matches. In addition, they were told to focus only on their interest in hiring a candidate when answering the first question (i.e., they were instructed to assume the candidate would accept an offer if given one). We denote responses to this question "hiring interest." They were told to focus only on the likelihood a candidate would accept a job offer when answering the second question (i.e., they were instructed to assume they candidate had been given an offer and to assess the likelihood they would accept it). We denote responses to this question a candidate's "likelihood of acceptance." We asked the first question to assess how resume characteristics affect hiring interest. We asked the second question both to encourage employers

[15] Audit and resume audit studies focusing on discrimination do not need to interpret callback as a measure of an employer's interest in a candidate to demonstrate discrimination (any difference in callback rates is evidence of discrimination).

to focus only on hiring interest when answering the first question and to explore employers' beliefs about the likelihood that a candidate would accept a job if offered.

The 10-point scale has two advantages. First, it provides additional statistical power, allowing us to observe employer preferences toward characteristics of inframarginal resumes, rather than identifying preferences only for resumes crossing a binary callback threshold in a resume audit setting. Second, it allows us to explore how employer preferences vary across the distribution of hiring interest, an issue we explore in depth in Section 3.3.

3 Results

3.1 Data and Empirical Approach

We recruited 72 employers through our partnership with the University of Pennsylvania Career Services office in Fall 2016 (46 subjects, 1840 resume observations) and Spring 2017 (26 subjects, 1040 resume observations).[16]

As described in Section 2, each employer rated 40 unique, hypothetical resumes with randomly assigned candidate characteristics. For each resume, employers rated hiring interest and likelihood of acceptance, each on a 10-point Likert scale. Our analysis focuses initially on hiring interest, turning to how employers evaluate likelihood of acceptance in Section 3.5. Our main specifications are ordinary least squares (OLS) regressions. These specifications make a linearity assumption with respect to the Likert-scale ratings data. Namely, they assume that, on average, employers treat equally-sized increases in Likert-scale ratings equivalently (e.g., an increase in hiring interest from 1 to 2 is equivalent to an increase from 9 to 10). In some specifications, we include subject fixed effects, which account for the possibility that employers have different mean ratings of resumes (e.g., allowing some employers to be more generous than others with their ratings across all resumes), while preserving the linearity assumption. To complement this analysis, we also run ordered probit regression specifications, which relax this assumption and only require that employers, on average, consider higher Likert-scale ratings more favorably than lower ratings.

In Section 3.2, we examine how human capital characteristics (e.g., GPA, major, work experience, and skills) affect hiring interest. These results report on the mean of preferences across

[16]The recruiters who participated in our study as subjects were primarily female (59%) and primarily white (79%) and Asian (15%). They reported a wide range of recruiting experience, including some who had been in a position with responsibilities associated with job candidates for one year or less (28%); between two and five years (46%); and six or more years (25%). Almost all (96%) of the participants had college degrees, and many (30%) had graduate degrees including an MA, MBA, JD, or Doctorate. They were approximately as likely to work at a large firm with over 1000 employees (35%) as a small firm with fewer than 100 employees (39%). These small firms include hedge fund, private equity, consulting, and wealth management companies that are attractive employment opportunities for Penn undergraduates. Large firms include prestigious Fortune 500 consumer brands, as well as large consulting and technology firms. The most common industries in the sample are finance (32%); the technology sector or computer science (18%); and consulting (16%). The sample had a smaller number of sales/marketing firms (9%) and non-profit or public interest organizations (9%). The vast majority (86%) of participating firms had at least one open position on the East Coast, though a significant number also indicated recruiting for the West Coast (32%), Midwest (18%), South (16%), or an international location (10%).

the distribution; we show how our results vary across the distribution of hiring interest in Section 3.3. In Section 3.4, we discuss how employers' ratings of hiring interest respond to demographic characteristics of our candidates. In Section 3.5, we investigate the likelihood of acceptance ratings and identify a potential new channel for discrimination. In Section 3.6, we compare our results to prior literature.

3.2 Effect of Human Capital on Hiring Interest

Employers in our study are interested in hiring graduates of the University of Pennsylvania for full-time employment, and many recruit at other Ivy League schools and other top colleges and universities. This labor market has been unexplored by resume audit studies, in part because the positions employers aim to fill through on-campus recruiting at Penn are highly unlikely to be filled through online job boards or by screening unsolicited resumes. In this section, we evaluate how randomized candidate characteristics—described in Section 2.3 and Table 1—affect employers' ratings of hiring interest.

We denote an employer i's rating of a resume j on the 1–10 Likert scale as V_{ij} and estimate variations of the following regression specification (1). This regression allows us to investigate the average response to candidate characteristics across employers in our study.

$$\begin{aligned} V_{ij} = {} & \beta_0 + \beta_1\, \textit{GPA} + \beta_2\, \textit{Top Internship} + \beta_3\, \textit{Second Internship} + \beta_4\, \textit{Work for Money} + \\ & \beta_5\, \textit{Technical Skills} + \beta_6\, \textit{Female, White} + \beta_7\, \textit{Male, Non-White} + \\ & \beta_8\, \textit{Female, Non-White} + \mu_j + \gamma_j + \omega_j + \alpha_i + \varepsilon_{ij} \end{aligned} \tag{1}$$

In this regression, *GPA* is a linear measure of grade point average. *Top Internship* is a dummy for having a top internship, *Second Internship* is a dummy for having an internship in the summer before junior year, and *Work for Money* is a dummy for having a work-for-money job in the summer before junior year. *Technical Skills* is a dummy for having a list of skills that included a set of four randomly assigned technical skills. Demographic variables *Female, White*; *Male, Non-White*; and *Female, Non-White* are dummies equal to 1 if the name of the candidate indicated the given race and gender.[17] μ_j are dummies for each major. Table 1 provides more information about these dummies and all the variables in this regression. In some specifications, we include additional controls. γ_j are dummies for each of the leadership experience components. ω_j are dummies for the number of resumes the employer has evaluated as part of the survey tool. Since leadership experiences are independently randomized and orthogonal to other resume characteristics of interest, and since resume characteristics are randomly drawn for each of the 40 resumes, our results should be robust

[17] Coefficient estimates on these variables report comparisons to white males, which is the excluded group. While we do not discuss demographic results in this section, we include controls for this randomized resume component in our regressions and discuss the results in Section 3.4 and Appendix B.4.

to the inclusion or exclusion of these dummies. Finally, α_i are employer (i.e., subject) fixed effects that account for different average ratings across employers.

Table 2 shows regression results where V_{ij} is *Hiring Interest*, which takes values from 1 to 10. The first three columns report OLS regressions with slightly different specifications. The first column includes all candidate characteristics we varied to estimate their impact on ratings. The second column adds leadership dummies γ and resume order dummies ω. The third column also adds subject fixed effects α. As expected, results are robust to the addition of these controls. The fourth column, labeled *GPA-Scaled OLS*, rescales all coefficients from the third column by the coefficient on GPA (2.196) so that the coefficients on other variables can be interpreted in GPA points. These regressions show that employers respond strongly to candidate characteristics related to human capital.

GPA is an important driver of hiring interest. An increase in GPA of one point (e.g., from a 3.0 to a 4.0) increases ratings on the Likert scale by 2.1–2.2 points. The standard deviation of quality ratings is 2.6, suggesting that a point improvement in GPA moves hiring interest ratings by about 0.8 of a standard deviation.

As described in Section 2.3.2, we created *ex ante* variation in both the quality and quantity of candidate work experience. Both affect employer interest. The quality of a candidate's work experience in the summer before senior year has a large impact on hiring interest ratings. The coefficient on *Top Internship* ranges from 0.9–1.0 Likert-scale points, which is roughly a third of a standard deviation of ratings. As shown in the fourth column of Table 2, a top internship is equivalent to a 0.41 improvement in GPA.

Employers value a second work experience on the candidate's resume, but only if that experience is an internship and not if it is a work-for-money job. In particular, the coefficient on *Second Internship*, which reflects the effect of adding a second "regular" internship to a resume that otherwise has no work experience listed for the summer before junior year, is 0.4–0.5 Likert-scale points—equivalent to 0.21 GPA points. While listing an internship before junior year is valuable, listing a work-for-money job that summer does not appear to increase hiring interest ratings. The coefficient on *Work for Money* is small and not statistically different from zero in our data. While it is directionally positive, we can reject that work-for-money jobs and regular internships are valued equally ($p < 0.05$ for all tests comparing the *Second Internship* and *Work for Money* coefficients). This preference of employers may create a disadvantage for students who cannot afford to accept (typically) unpaid internships the summer before their junior year.[18]

We see no effect on hiring interest from increased *Technical Skills*, suggesting that employers on average do not value the technical skills we randomly added to candidate resumes or that listing technical skills does not credibly signal sufficient mastery to affect hiring interest (e.g., employers may consider skills listed on a resume to be cheap talk).

[18]These results are consistent with a penalty for working-class candidates. In a resume audit study of law firms, Rivera and Tilcsik [2016] found that resume indicators of lower social class (such as receiving a scholarship for first generation college students) led to lower callback rates.

Table 2: Hiring Interest

	Dependent Variable: Hiring Interest				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.125	2.190	2.196	1.000	0.891
	(0.145)	(0.150)	(0.129)	(.)	(0.063)
Top Internship	0.902	0.900	0.897	0.409	0.378
	(0.095)	(0.099)	(0.081)	(0.043)	(0.040)
Second Internship	0.465	0.490	0.466	0.212	0.206
	(0.112)	(0.118)	(0.095)	(0.045)	(0.047)
Work for Money	0.116	0.157	0.154	0.070	0.052
	(0.110)	(0.113)	(0.091)	(0.042)	(0.046)
Technical Skills	0.046	0.053	-0.071	-0.032	0.012
	(0.104)	(0.108)	(0.090)	(0.041)	(0.043)
Female, White	-0.152	-0.215	-0.161	-0.073	-0.061
	(0.114)	(0.118)	(0.096)	(0.044)	(0.048)
Male, Non-White	-0.172	-0.177	-0.169	-0.077	-0.075
	(0.136)	(0.142)	(0.115)	(0.053)	(0.058)
Female, Non-White	-0.009	-0.022	0.028	0.013	-0.014
	(0.137)	(0.144)	(0.120)	(0.055)	(0.057)
Observations	2880	2880	2880	2880	2880
R^2	0.129	0.181	0.483		
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No

Ordered probit cutpoints: 1.91, 2.28, 2.64, 2.93, 3.26, 3.60, 4.05, 4.51, and 5.03.

Table shows OLS and ordered probit regressions of *Hiring Interest* from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS presents the results of Column 3 divided by the Column 3 coefficient on GPA, with standard errors calculated by delta method. The p-values of tests of joint significance of major fixed effects are indicated (F-test for OLS, likelihood ratio test for ordered probit).

Table 2 also reports the p-value of a test of whether the coefficients on the major dummies are jointly different from zero. Results suggest that the randomly assigned major significantly affects hiring interest. While we do not have the statistical power to test for the effect of each major, we can explore how employers respond to candidates being from more prestigious schools at the University of Pennsylvania. In particular, 40% of the Humanities & Social Sciences resumes are assigned a BS in Economics from Wharton and the rest have a BA major from the College of Arts and Sciences. In addition, 70% of the STEM resumes are assigned a BS from the School of Engineering and Applied Science and the rest have a BA major from the College of Arts and Sciences. As shown in Appendix Table B.2, in both cases, we find that being from the more prestigious school—and thus receiving a BS rather than a BA—is associated with an increase in hiring interest ratings of about 0.4 Likert-scale points or 0.18 GPA points.[19]

We can loosen the assumption that employers treated the intervals on the Likert scale linearly by treating *Hiring Interest* as an ordered categorical variable. The fifth column of Table 2 gives the results of an ordered probit specification with the same variables as the first column (i.e., omitting the leadership dummies and subject fixed effects). This specification is more flexible than OLS, allowing the discrete steps between Likert-scale points to vary in size. The coefficients reflect the effect of each characteristic on a latent variable over the Likert-scale space, and cutpoints are estimated to determine the distance between categories. Results are similar in direction and statistical significance to the OLS specifications described above.[20]

As discussed in Section 2, we made many design decisions to enhance realism. However, one might be concerned that our independent cross-randomization of various resume components might lead to unrealistic resumes and influence the results we find. We provide two robustness checks in the appendix to address this concern. First, our design and analysis treat each work experience as independent, but, in practice, candidates may have related jobs over a series of summers that create a work experience "narrative." In Appendix B.1 and Appendix Table B.1, we describe how we construct a measure of work experience narrative, we test its importance, and find that while employers respond positively to work experience narrative ($p = 0.054$) our main results are robust to its inclusion. Second, the GPA distribution we used for constructing the hypothetical resumes did not perfectly match the distribution of job seekers in our labor market. In Appendix B.2, we re-weight our data to match the GPA distribution in the candidate pool of real Penn job seekers and show that our results are robust to this re-weighting. These exercises provide some assurance that our results are not an artifact of how we construct hypothetical resumes.

[19]Note that since the application processes for these different schools within Penn are different, including the admissions standards, this finding also speaks to the impact of institutional prestige, in addition to field of study (see, e.g., Kirkeboen et al. [2016]).

[20]The ordered probit cutpoints (2.14, 2.5, 2.85, 3.15, 3.46, 3.8, 4.25, 4.71, and 5.21) are approximately equally spaced, suggesting that subjects treated the Likert scale approximately linearly. Note that we only run the ordered probit specification with the major dummies and without leadership dummies or subject fixed effects. Adding too many dummies to an ordered probit can lead to unreliable estimates when the number of observations per cluster is small [Greene, 2004].

3.3 Effects Across the Distribution of Hiring Interest

The regression specifications described in Section 3.2 identify the average effect of candidate characteristics on employers' hiring interest. As pointed out by Neumark [2012], however, these average preferences may differ in magnitude—and even direction—from differences in callback rates, which derive from whether a characteristic pushes a candidate above a specific quality threshold (i.e., the callback threshold). For example, in the low callback rate environments that are typical of resume audit studies, differences in callback rates will be determined by how employers respond to a candidate characteristic in the right tail of their distribution of preferences.[21] To make this concern concrete, Appendix B.3 provides a simple graphical illustration in which the average preference for a characteristic differs from the preference in the tail of the distribution. In practice, we may care about preferences in any part of the distribution for policy. For example, preferences at the callback threshold may be relevant for hiring outcomes, but those thresholds may change with a hiring expansion or contraction.

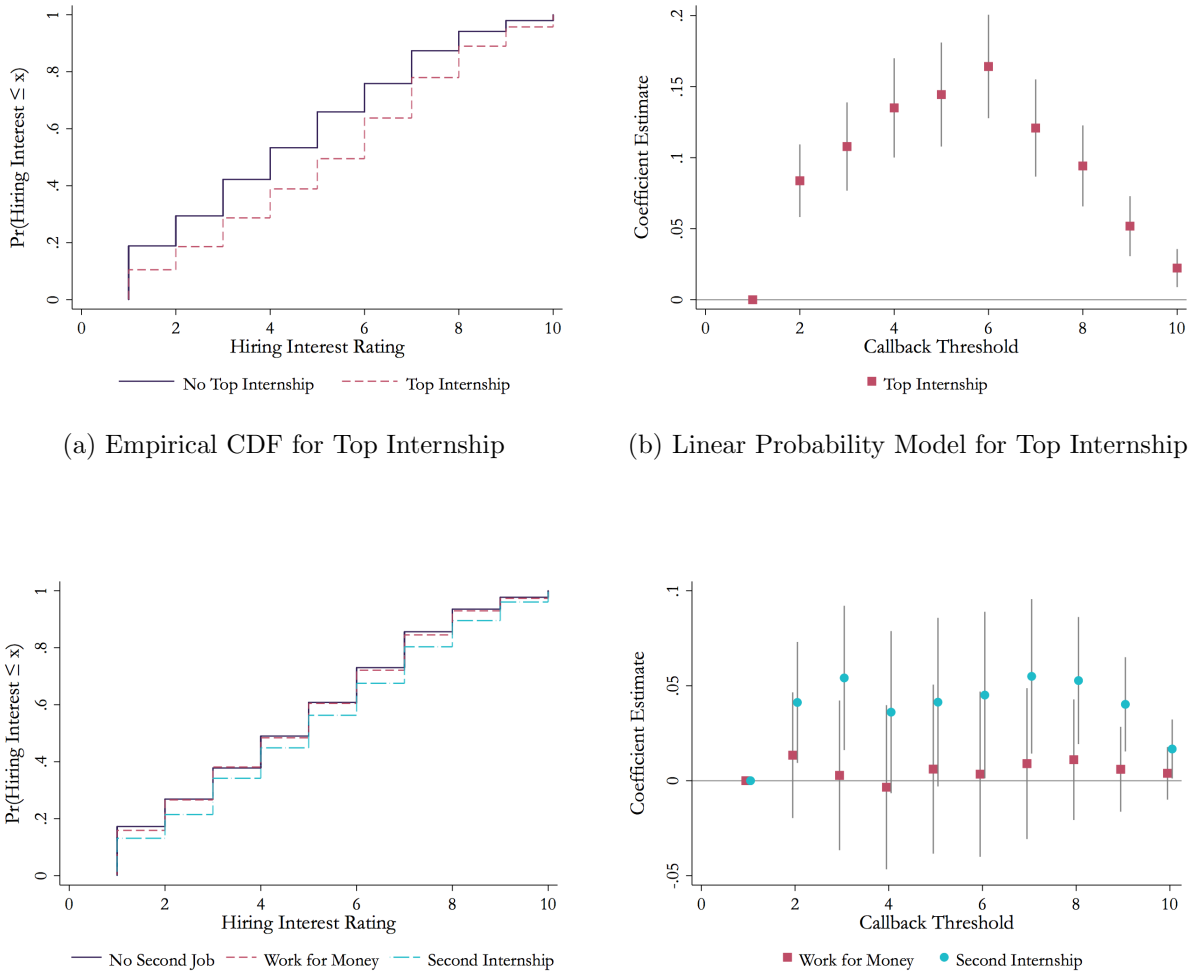
An advantage of the IRR methodology, however, is that it can deliver a granular measure of hiring interest to explore whether employers' preferences for characteristics do indeed differ in the tails of the hiring interest distribution. We employ two basic tools to explore preferences across the distribution of hiring interest: (1) the empirical cumulative distribution function (CDF) of hiring interest ratings and (2) a "counterfactual callback threshold" exercise. In the latter exercise, we impose a counterfactual callback threshold at each possible hiring interest rating (i.e., supposing that employers called back all candidates that they rated at or above that rating level) and, for each possible rating level, report the OLS coefficient an audit study researcher would find for the difference in callback rates.

While the theoretical concerns raised by Neumark [2012] may be relevant in other settings, the average results we find in Section 3.2 are all consistent across the distribution of hiring interest, including in the tails (except for a preference for Wharton students, which we discuss below). The top half of Figure 1 shows that *Top Internship* is positive and statistically significant at all levels of selectivity. Panel (a) reports the empirical CDF of hiring interest ratings for candidates with and without a top internship. Panel (b) shows the difference in callback rates that would arise for *Top Internship* at each counterfactual callback threshold. The estimated difference in callback rates is positive and significant everywhere, although it is much larger in the midrange of the quality distribution than at either of the tails.[22] The bottom half of Figure 1 shows that results across

[21] A variant of this critique was initially brought up by Heckman and Siegelman [1992] and Heckman [1998] for in-person audit studies, where auditors may be imperfectly matched, and was extended to correspondence audit studies by Neumark [2012] and Neumark et al. [2015]. A key feature of the critique is that certain candidate characteristics might affect higher moments of the distribution of employer preferences so that how employers respond to a characteristic on average may be different than how an employer responds to a characteristic in the tail of their preference distribution.

[22] This shape is partially a mechanical feature of low callback rate environments: if a threshold is set high enough that only 5% of candidates with a desirable characteristic are being called back, the difference in callback rates can be no more than 5 percentage points. At lower thresholds (e.g., where 50% of candidates with desirable characteristics

Figure 1: Value of Quality of Experience Over Selectivity Distribution



(a) Empirical CDF for Top Internship

(b) Linear Probability Model for Top Internship

(c) Empirical CDF for Second Job Type

(d) Linear Probability Model for Second Job Type

Empirical CDF of *Hiring Interest* (Panels 1a & 1c) and difference in counterfactual callback rates (Panels 1b & 1d) for *Top Internship*, in the top row, and *Second Internship* and *Work for Money*, in the bottom row. Empirical CDFs show the share of hypothetical candidate resumes with each characteristic with a *Hiring Interest* rating less than or equal to each value. The counterfactual callback plot shows the difference between groups in the share of candidates at or above the threshold—that is, the share of candidates who would be called back in a resume audit study if the callback threshold were set to any given value. 95% confidence intervals are calculated from a linear probability model with an indicator for being at or above a threshold as the dependent variable.

the distribution for *Second Internship* and *Work for Money* are also consistent with the average results from Section 3.2. *Second Internship* is positive everywhere and almost always statistically significant. *Work for Money* consistently has no impact on employer preferences throughout the distribution of hiring interest.

As noted above, our counterfactual callback threshold exercise suggests that a well-powered audit study would likely find differences in callback rates for most of the characteristics that we estimate as statistically significant on average in Section 3.2, regardless of employers' callback threshold. This result is reassuring both for the validity of our results and in considering the generalizability of results from the resume audit literature. However, even in our data, we observe a case where a well-powered audit study would be unlikely to find a result, even though we find one on average. Appendix Figure B.1 mirrors Figure 1 but focuses on having a Wharton degree among employers seeking Humanities & Social Sciences candidates. Employers respond to Wharton in the middle of the distribution of hiring interest, but preferences seem to converge in the right tail (i.e., at hiring interest ratings of 9 or 10), suggesting that the best students from the College of Arts and Sciences are not evaluated differently than the best students from Wharton.

3.4 Demographic Discrimination

In this section, we examine how hiring interest ratings respond to the race and gender of candidates. As described in Section 2 and shown in Table 1, we use our variation in names to create the variables: *Female, White*; *Male, Non-White*; and *Female, Non-White*. As shown in Table 2, the coefficients on the demographic variables are not significantly different from zero, suggesting no evidence of discrimination on average in our data.[23] This null result contrasts somewhat with existing literature—both resume audit studies (e.g., Bertrand and Mullainathan [2004]) and laboratory experiments (e.g., Bohnet et al. [2015]) generally find evidence of discrimination in hiring. Our differential results may not be surprising given that our employer pool is different than those usually targeted through resume audit studies, with most reporting positive tastes for diversity.

While we see no evidence of discrimination on average, a large literature addressing diversity in the sciences (e.g., Carrell et al. [2010], Goldin [2014]) suggests we might be particularly likely to see discrimination among employers seeking STEM candidates. In Table 3, we estimate the regression in Equation (1) separately by major type. Results in Columns 5-10 show that employers looking for STEM candidates display a large, statistically significant preference for white male candidates over white females and non-white males. The coefficients on *Female, White* and *Male, Non-White* suggest that these candidates suffer a penalty of 0.5 Likert-scale points—or about 0.27 GPA points—that is robust across our specifications. These effects are at least marginally significant even after multiplying our p-values by two to correct for the fact that we are

are called back), differences in callback rates can be much larger. In Appendix B.3, we discuss how this feature of difference in callback rates could lead to misleading comparisons across experiments with very different callback rates.

[23] In Appendix Table B.6, we show that this effect does not differ by the gender and race of the employer rating the resume.

Table 3: Hiring Interest by Major Type

	Dependent Variable: Hiring Interest									
	Humanities & Social Sciences					STEM				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.208	2.304	2.296	1.000	0.933	1.932	1.885	1.882	1.000	0.802
	(0.173)	(0.179)	(0.153)	(.)	(0.074)	(0.267)	(0.309)	(0.242)	(.)	(0.112)
Top Internship	1.075	1.043	1.033	0.450	0.452	0.398	0.559	0.545	0.289	0.175
	(0.108)	(0.116)	(0.095)	(0.050)	(0.046)	(0.191)	(0.216)	(0.173)	(0.100)	(0.078)
Second Internship	0.540	0.516	0.513	0.224	0.240	0.242	0.307	0.311	0.165	0.111
	(0.132)	(0.143)	(0.114)	(0.051)	(0.056)	(0.208)	(0.246)	(0.189)	(0.103)	(0.088)
Work for Money	0.087	0.107	0.116	0.050	0.037	0.151	0.275	0.337	0.179	0.076
	(0.129)	(0.134)	(0.110)	(0.048)	(0.055)	(0.212)	(0.254)	(0.187)	(0.102)	(0.088)
Technical Skills	0.063	0.084	-0.050	-0.022	0.013	-0.028	-0.113	-0.180	-0.096	-0.001
	(0.122)	(0.130)	(0.106)	(0.046)	(0.052)	(0.197)	(0.228)	(0.186)	(0.100)	(0.083)
Female, White	-0.047	-0.117	-0.054	-0.024	-0.015	-0.419	-0.612	-0.545	-0.290	-0.171
	(0.134)	(0.142)	(0.117)	(0.051)	(0.057)	(0.215)	(0.249)	(0.208)	(0.115)	(0.089)
Male, Non-White	-0.029	-0.010	-0.026	-0.011	-0.007	-0.567	-0.617	-0.507	-0.270	-0.265
	(0.158)	(0.169)	(0.137)	(0.059)	(0.066)	(0.271)	(0.318)	(0.257)	(0.136)	(0.111)
Female, Non-White	0.085	0.101	0.091	0.040	0.024	-0.329	-0.260	-0.046	-0.025	-0.142
	(0.160)	(0.171)	(0.137)	(0.060)	(0.068)	(0.264)	(0.301)	(0.261)	(0.138)	(0.111)
Observations	2040	2040	2040	2040	2040	840	840	840	840	840
R^2	0.128	0.196	0.500			0.119	0.323	0.593		
p-value for test of joint significance of Majors	0.021	0.027	0.007	0.007	0.030	< 0.001	0.035	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No	No	No	Yes	Yes	No

Ordered probit cutpoints (Column 5): 2.25, 2.58, 2.96, 3.26, 3.60, 3.94, 4.41, 4.86, 5.41.

Ordered probit cutpoints (Column 10): 1.44, 1.90, 2.22, 2.51, 2.80, 3.14, 3.56, 4.05, 4.48.

Table shows OLS and ordered probit regressions of *Hiring Interest* from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS presents the results of Column 3 and Column 8 divided by the Column 3 and Column 8 coefficients on GPA, with standard errors calculated by delta method. The p-values of tests of joint significance of major fixed effects are indicated (F-test for OLS, likelihood ratio test for ordered probit) after a Bonferroni correction for analyzing two subgroups.

analyzing our results within two subgroups (uncorrected p-values are: $p = 0.009$ for *Female, White*; $p = 0.049$ for *Male, Non-White*). Results in Columns 1-5 show no evidence of discrimination in hiring interest among Humanities & Social Sciences employers.

As in Section 3.3, we can examine these results across the hiring interest rating distribution. Figure 2 shows the CDF of hiring interest ratings and the difference in counterfactual callback rates. For ease of interpretation and for statistical power, we pool female and minority candidates and compare them to white male candidates in these figures and in some analyses that follow. The top row shows these comparisons for employers interested in Humanities & Social Sciences candidates and the bottom row shows these comparisons for employers interested in STEM candidates. Among employers interested in Humanities & Social Sciences candidates, the CDFs of *Hiring Interest* ratings are nearly identical. Among employers interested in STEM candidates, however, the CDF for white male candidates first order stochastically dominates the CDF for candidates who are not white males. At the point of the largest counterfactual callback gap, employers interested in STEM candidates would display callback rates that were 10 percentage points lower for candidates who were not white males than for their white male counterparts.

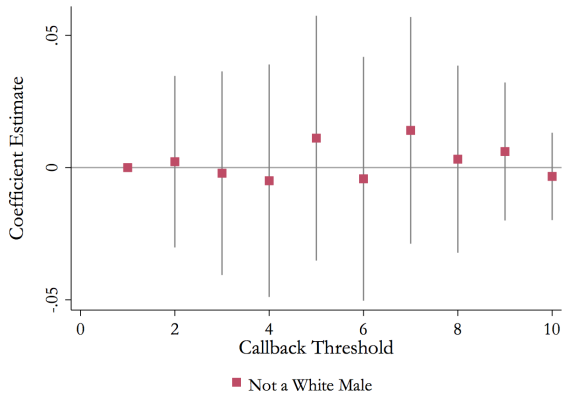
One might be surprised that we find any evidence of discrimination, given that employers may have (correctly) believed we would not use demographic tastes in generating their matches and given that employers may have attempted to override any discriminatory preferences to be more socially acceptable. One possibility for why we nevertheless find discrimination is the role of implicit bias [Greenwald et al., 1998, Nosek et al., 2007], which Bertrand et al. [2005] has suggested is an important channel for discrimination in resume audit studies. In Appendix B.4, we explore the role of implicit bias in driving our results.[24] In particular, we leverage a feature of implicit bias—that it is more likely to arise when decision makers are fatigued [Wigboldus et al., 2004, Govorun and Payne, 2006, Sherman et al., 2004]—to test whether our data are consistent with employers displaying an implicit racial or gender bias. As shown in Appendix Table B.7, employers spend less time evaluating resumes both in the latter half of the study and in the latter half of each set of 10 resumes (after each set of 10 resumes, we introduced a short break for subjects), suggesting evidence of fatigue. Discrimination is statistically significantly larger in the latter half of each block of 10 resumes, providing suggestive evidence that implicit bias plays a role in our findings, although discrimination is not larger in the latter half of the study.

Race and gender could also subconsciously affect how employers view other resume components. We test for negative interactions between race and gender and desirable candidate characteristics, which have been found in the resume audit literature (e.g., minority status has been shown to lower returns to resume quality [Bertrand and Mullainathan, 2004]). Appendix Table B.8 interacts *Top*

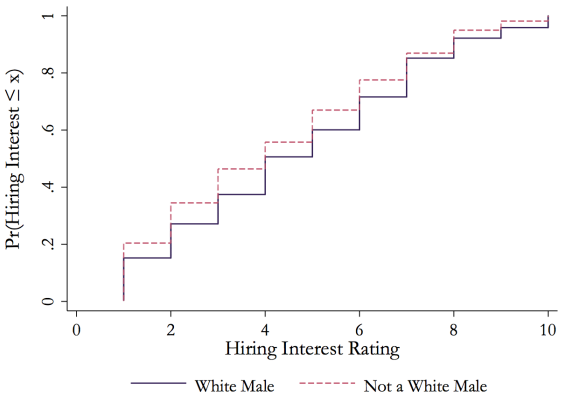
[24] Explicit bias might include an explicit taste for white male candidates or an explicit belief they are more prepared than female or minority candidates for success at their firm, even conditional on their resumes. Implicit bias [Greenwald et al., 1998, Nosek et al., 2007], on the other hand, may be present even among employers who are not explicitly considering race (or among employers who are considering race but attempting to suppress any explicit bias they might have).

Figure 2: Demographics by Major Type Over Selectivity Distribution

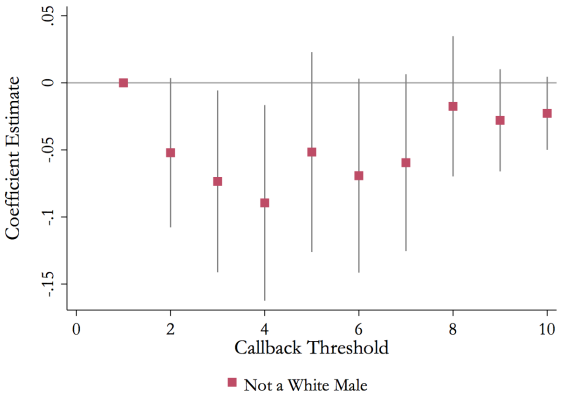
(a) Empirical CDF: Not a White Male, Humanities & Social Sciences



(b) Linear Probability Model: Not a White Male, Humanities & Social Sciences



(c) Empirical CDF: Not a White Male, STEM



(d) Linear Probability Model: Not a White Male, STEM

Empirical CDF of *Hiring Interest* (Panels 2a & 2c) and difference in counterfactual callback rates (Panels 2b & 2d) for *White Male* and *Not a White Male*. Employers interested in Humanities & Social Sciences candidates are shown in the top row and employers interested in STEM candidates are shown in the bottom row. Empirical CDFs show the share of hypothetical candidate resumes with each characteristic with a *Hiring Interest* rating less than or equal to each value. The counterfactual callback plot shows the difference between groups in the share of candidates at or above the threshold—that is, the share of candidates who would be called back in a resume audit study if the callback threshold were set to any given value. 95% confidence intervals are calculated from a linear probability model with an indicator for being at or above a threshold as the dependent variable.

Internship, our binary variable most predictive of hiring interest, with our demographic variables. These interactions are all directionally negative, and the coefficient *Top Internship* × *Female, White* is negative and significant, suggesting a lower return to a prestigious internships for white females. One possible mechanism for this effect is that employers believe that other employers exhibit positive preferences for diversity, and so having a prestigious internship is a less strong signal of quality if one is from an under-represented group. This aligns with the findings shown in Appendix Figure B.6, which shows that the negative interaction between *Top Internship* and demographics appears for candidates with relatively low ratings and is a fairly precisely estimated zero when candidates receive relatively high ratings.

3.5 Candidate Likelihood of Acceptance

In resume audit studies, traits that suggest high candidate quality do not always increase employer callback. For example, several studies have found that employers call back employed candidates at lower rates than unemployed candidates [Kroft et al., 2013, Nunley et al., 2017, 2014, Farber et al., 2018], but that longer periods of unemployment are unappealing to employers. This seeming contradiction is consistent with the hypothesis that employers are concerned about the possibility of wasting resources pursuing a candidate who will ultimately reject a job offer. In other words, hiring interest is not the only factor determining callback decisions. This concern has been acknowledged in the resume audit literature, for example when Bertrand and Mullainathan [2004, p. 992] notes, "In creating the higher-quality resumes, we deliberately make small changes in credentials so as to minimize the risk of overqualification."

As described in Section 2.4, for each resume we asked employers "How likely do you think **[Name]** would be to accept a job with your organization?" Asking this question helps ensure that our measure of hiring interest is unconfounded with concerns that a candidate would accept a position when offered. However, the question also allows us to study this second factor, which also affects callback decisions.

Table 4 replicates the regression specifications from Table 2, estimating Equation (1) when V_{ij} is *Likelihood of Acceptance*, which takes values from 1 to 10. Employers in our sample view high quality candidates as *more likely* to accept a job with their firm than low quality candidates. This suggests that employers in our sample believe candidate fit at their firm outweighs the possibility that high quality candidates will be pursued by many other firms. In Appendix B.5, we further consider the role of horizontal fit and vertical quality and find that—holding hiring interest in a candidate constant—reported likelihood of acceptance falls as evidence of vertical quality (e.g., GPA) increases. This result highlights that there is independent information in the likelihood of acceptance measure.

Table 4 shows that employers report female and minority candidates are less likely to accept a position with their firm, by 0.2 points on the 1–10 Likert scale (or about one tenth of a standard deviation). This effect is robust to the inclusion of a variety of controls, and it persists when we

hold hiring interest constant in Appendix Table B.9. Table 5 splits the sample and shows that while the direction of these effects is consistent among both groups of employers, the negative effects are particularly large among employers recruiting STEM candidates.

If minority and female applicants are perceived as less likely to accept an offer, this could induce lower callback rates for these candidates. Our results therefore suggest a new channel for discrimination observed in the labor market, which is worth exploring. Perhaps due to the prevalence of diversity initiatives, employers expect that desirable minority and female candidates will receive many offers from competing firms and thus will be less likely to accept any given offer. Alternatively, employers may see female and minority candidates as less likely to fit in the culture of the firm, making these candidates less likely to accept an offer. This result has implications for how we understand the labor market and how we interpret the discrimination observed in resume audit studies.[25]

[25] In particular, while audit studies can demonstrate that groups are not being treated equally, differential callback rates need not imply a lack of employer interest. The impact of candidate characteristics on likelihood of acceptance is a case of omitted variable bias, but one that is not solved by experimental randomization, since the randomized trait endows the candidate with hiring interest and likelihood of acceptance simultaneously.

Table 4: Likelihood of Acceptance

	Dependent Variable: Likelihood of Acceptance			
	OLS	OLS	OLS	Ordered Probit
GPA	0.605	0.631	0.734	0.263
	(0.144)	(0.150)	(0.120)	(0.060)
Top Internship	0.683	0.677	0.664	0.285
	(0.094)	(0.098)	(0.076)	(0.040)
Second Internship	0.418	0.403	0.394	0.179
	(0.112)	(0.119)	(0.091)	(0.047)
Work for Money	0.197	0.192	0.204	0.088
	(0.111)	(0.116)	(0.090)	(0.047)
Technical Skills	-0.051	-0.059	-0.103	-0.025
	(0.104)	(0.108)	(0.086)	(0.044)
Female, White	-0.231	-0.294	-0.258	-0.093
	(0.114)	(0.118)	(0.094)	(0.048)
Male, Non-White	-0.125	-0.170	-0.117	-0.060
	(0.137)	(0.142)	(0.110)	(0.057)
Female, Non-White	-0.221	-0.236	-0.162	-0.103
	(0.135)	(0.142)	(0.112)	(0.057)
Observations	2880	2880	2880	2880
R^2	0.070	0.124	0.492	
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	No
Order FEs	No	Yes	Yes	No
Subject FEs	No	No	Yes	No

Ordered probit cutpoints: -0.26, 0.13, 0.49, 0.75, 1.12, 1.49, 1.94, 2.46, and 2.83.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects are indicated (F-test for OLS, likelihood ratio test for ordered probit).

Table 5: Likelihood of Acceptance by Major Type

	Dependent Variable: Likelihood of Acceptance							
	Humanities & Social Sciences				STEM			
	OLS	OLS	OLS	Ordered Probit	OLS	OLS	OLS	Ordered Probit
GPA	0.581	0.610	0.694	0.251	0.688	0.724	0.813	0.314
	(0.176)	(0.186)	(0.142)	(0.072)	(0.251)	(0.287)	(0.237)	(0.110)
Top Internship	0.786	0.773	0.754	0.316	0.391	0.548	0.527	0.190
	(0.111)	(0.118)	(0.089)	(0.046)	(0.178)	(0.199)	(0.171)	(0.078)
Second Internship	0.481	0.422	0.424	0.201	0.254	0.324	0.301	0.119
	(0.136)	(0.148)	(0.109)	(0.055)	(0.198)	(0.230)	(0.187)	(0.088)
Work for Money	0.206	0.173	0.187	0.084	0.155	0.346	0.350	0.092
	(0.135)	(0.144)	(0.108)	(0.055)	(0.194)	(0.239)	(0.186)	(0.088)
Technical Skills	-0.094	-0.103	-0.106	-0.046	0.050	0.000	-0.116	0.032
	(0.125)	(0.134)	(0.104)	(0.052)	(0.190)	(0.217)	(0.179)	(0.083)
Female, White	-0.175	-0.211	-0.170	-0.062	-0.365	-0.572	-0.577	-0.177
	(0.139)	(0.148)	(0.116)	(0.056)	(0.198)	(0.236)	(0.194)	(0.089)
Male, Non-White	-0.069	-0.076	-0.046	-0.030	-0.269	-0.360	-0.289	-0.147
	(0.161)	(0.172)	(0.130)	(0.066)	(0.259)	(0.302)	(0.246)	(0.110)
Female, Non-White	-0.244	-0.212	-0.163	-0.107	-0.200	-0.108	-0.010	-0.105
	(0.162)	(0.175)	(0.130)	(0.068)	(0.243)	(0.278)	(0.245)	(0.110)
Observations	2040	2040	2040	2040	840	840	840	840
R^2	0.040	0.107	0.516		0.090	0.295	0.540	
p-value for test of joint significance of Majors	0.798	0.939	0.785	0.598	< 0.001	0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	No	No	Yes	Yes	No
Order FEs	No	Yes	Yes	No	No	Yes	Yes	No
Subject FEs	No	No	Yes	No	No	No	Yes	No

Ordered probit cutpoints (Column 4): -0.23, 0.14, 0.50, 0.75, 1.11, 1.48, 1.93, 2.42, 2.75.

Ordered probit cutpoints (Column 8): -0.23, 0.20, 0.55, 0.83, 1.25, 1.64, 2.08, 2.71, 3.57.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1). *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects are indicated (F-test for OLS, likelihood ratio test for ordered probit) after a Bonferroni correction for analyzing two subgroups.

3.6 Comparing our Demographic Results to Previous Literature

3.6.1 Qualitative comparison

Our results can be compared to those from other studies of employer preferences, with two caveats. First, our measure of the firms' interest in hiring a candidate may not be directly comparable to findings derived from callback rates, which likely combine both hiring interest and likelihood of acceptance into a single binary outcome. Second, our subject population is made up of firms that would be unlikely to respond to cold resumes and thus may have different preferences than the typical firms audited in prior literature.

Resume audit studies have consistently shown lower callback rates for minorities. We see no evidence of lower ratings for minorities on average, but we do see lower ratings of minority male candidates by STEM employers. Results on gender in the resume audit literature have been mixed. In summarizing results from 11 studies conducted between 2005 and 2016, [Baert, 2018] finds four studies with higher callback rates for women, two with lower callback rates, and five studies with no significant difference. None of these studies found discrimination against women in a U.S. setting. This may be due to resume audit studies targeting female-dominated occupations, such as clerical or administrative work. Riach and Rich [2006], which specifically targets male-dominated occupations, shows lower callback rates for women. Outside the labor market, Bohren et al. [2018] and Milkman et al. [2012] found evidence of discrimination against women using audit-type methodologies. We find that firms recruiting STEM candidates give lower ratings to white women, demonstrating the importance of being able to reach new subject pools with IRR. We also find that white women receive a lower return to prestigious internships. This result matches a type of discrimination—lower return to quality—seen in Bertrand and Mullainathan [2004], but we find it for gender rather than race.

We also find that employers believe white women are less likely to accept positions if offered, which could account for discrimination found in the resume audit literature. For example, Quadlin [2018] finds that women with very high GPAs are called back at lower rates than women with lower GPAs, which could potentially arise from a belief these high quality women will be recruited by other firms, rather than from a lack of hiring interest.

3.6.2 Quantitative comparison using GPA as a numeraire

In addition to making qualitative comparisons, we can conduct some back-of-the-envelope calculations to compare the magnitude of our demographic effects to those in previous studies, including Bertrand and Mullainathan [2004]. We conduct these comparisons by taking advantage of the ability—in our study and others—to use GPA as a numeraire.

In studies that randomize GPA, we can divide the observed effect due to race or gender by the effect due to GPA to compare with our GPA-scaled estimates. For example, exploiting the random variation in GPA and gender from Quadlin [2018], we calculate that being female leads

to a decrease in callback equivalent to 0.23 GPA points.[26] Our results (shown in Tables 2 and 3) suggest that being a white female, as compared to a white male, is equivalent to a decrease of 0.073 GPA points overall and 0.290 GPA points among employers recruiting for STEM.

When a study does not vary GPA, we can benchmark the effect of demographic differences on callback to the effect of GPA on counterfactual callback in our study. For example, in Bertrand and Mullainathan [2004], 8% of all resumes receive callbacks, and having a black name decreases callback by 3.2 percentage points. 7.95% of resumes in our study receive a 9 or a 10 rating, suggesting that receiving a 9 or higher is a similar level of selectivity as in Bertrand and Mullainathan [2004]. A linear probability model in our data suggests that each 0.1 GPA point increases counterfactual callback at this threshold by 1.13 percentage points. Thus, the Bertrand and Mullainathan [2004] race effect is equivalent to an increase of 0.28 GPA points in our study.[27] This effect can be compared to our estimate that being a minority male, as compared to a white male, is equivalent to a decrease of 0.077 GPA points overall and 0.270 GPA points among employers recruiting for STEM.

4 Pitt Replication: Results and Lessons

In order to explore whether preferences differed between employers at Penn (an elite, Ivy League school) and other institutions where recruiters might more closely resemble the employers of typical resume audit studies, we reached out to several Pennsylvania schools in hopes of running an IRR replication. We partnered with the University of Pittsburgh (Pitt) Office of Career Development and Placement Assistance to run two experimental rounds during their spring recruiting cycle.[28] Ideally, the comparison between Penn and Pitt would have given us additional insight into the extent to which Penn employers differed from employers traditionally targeted by audit studies.

[26] Quadlin [2018] reports callback rate in four GPA bins. The paper finds callback is lower in the highest GPA bin than the second highest bin, which may be due to concerns about likelihood of acceptance. Looking at the second and third highest bins (avoiding the non-monotonic bin), we see that an increase in GPA from the range [2.84, 3.20] to [3.21, 3.59]—an average increase of 0.38 GPA points—results in a callback rate increase of 3.5 percentage points. Dividing 0.38 by 3.5 suggests that each 0.11 GPA points generates 1 percentage point difference in callback rates. Quadlin [2018] also finds a callback difference of 2.1 percentage points between male (14.0%) and female (11.9%) candidates. Thus, applicant gender has about the same effect as a 0.23 change in GPA.

[27] Bertrand and Mullainathan [2004] also varies quality, but through changing multiple characteristics at once. Using the same method, these changes, which alter callback by 2.29 percentage points, are equivalent to a change of 0.20 GPA points, providing a benchmark for their quality measure in our GPA points.

[28] Unlike at Penn, there is no major fall recruiting season with elite firms at Pitt. We recruited employers in the spring semester only, first in 2017 and again in 2018. The Pitt recruitment email was similar to that used at Penn (Figure A.1), and originated from the Pitt Office of Career Development and Placement Assistance. For the first wave at Pitt we offered webinars, as described in Appendix A.1, but since attendance at these sessions was low, we did not offer them in the second wave. We collected resume components to populate the tool at Pitt from real resumes of graduating Pitt seniors. Rather than collect resumes from clubs, resume books, and campus job postings as we did at Penn, we used the candidate pool of job-seeking seniors both to populate the tool and to suggest matches for employers. This significantly eased the burden of collecting and scraping resumes. At Pitt, majors were linked to either the "Dietrich School of Arts and Sciences" or the "Swanson School of Engineering". Table C.1 lists the majors, associated school, major category, and the probability that the major was drawn. We collected top internships at Pitt by identifying the firms hiring the most Pitt graduates, as at Penn. Top internships at Pitt tended to be less prestigious than the top internships at Penn.

Instead, we learned that we were insufficiently attuned to how recruiting differences between Penn and Pitt employer populations should influence IRR implementation. Specifically, we observed significant attenuation over nearly all candidate characteristics in the Pitt data. Table 6 shows fully controlled OLS regressions highlighting that our effects at Pitt (shown in the second column) are directionally consistent with those at Penn (shown in the first column for reference), but much smaller in size. For example, the coefficient on GPA is one-tenth the size in the Pitt data. We find similar attenuation on nearly all characteristics at Pitt for both *Hiring Interest* and *Likelihood of Acceptance*, in the pooled sample and separated by major type. We find no evidence of Pitt employers responding to candidate demographics. (Appendix C provides details for our experimental implementation at Pitt and Tables C.2, C.3, and C.4 display the full results.)

We suspect the cause of the attenuation at Pitt was our failure to appropriately tailor resumes to meet the needs of Pitt employers who were seeking candidates with specialized skills or backgrounds. A large share of the resumes at Pitt (33.8%) received the lowest possible *Hiring Interest* rating, more than double the share at Penn (15.5.%). Feedback from Pitt employers suggested that they were also less happy with their matches: many respondents complained that the matches lacked a particular skill or major requirement for their open positions.[29] In addition, the importance of a major requirement was reflected on the post-survey data in which 33.7% of Pitt employers indicated that candidate major was among the most important considerations during recruitment, compared to only 15.3% at Penn.

After observing these issues in the first wave of Pitt data collection, we added a new checklist question to the post-tool survey in the second wave: "I would consider candidates for this position with any of the following majors...." This question allowed us both to restrict the match pool for each employer, improving match quality, and to directly assess the extent to which our failure to tailor resumes was attenuating our estimates of candidate characteristics. Table 6 shows that when splitting the data from the second wave based on whether a candidate was in a target major, the effect of GPA is much larger in the target major sample (shown in the fourth column), and that employers do not respond strongly to any of the variables when considering candidates with majors that are not *Target Majors*.

The differential responses depending on whether resumes come from *Target Majors* highlights the importance of tailoring candidate resumes to employers when deploying the IRR methodology. We advertised the survey tool at both Pitt and Penn as being particularly valuable for hiring skilled generalists, and we were ill equipped to measure preferences of employers looking for candidates with very particular qualifications.

This was a limitation in our implementation at Pitt rather than in the IRR methodology itself. That is, one could design an IRR study specifically for employers interested in hiring registered nurses, or employers interested in hiring mobile software developers, or employers interested in

[29] As one example, a firm wrote to us in an email: "We are a Civil Engineering firm, specifically focused on hiring students out of Civil and/or Environmental Engineering programs... there are 0 students in the group of real resumes that you sent over that are Civil Engineering students."

Table 6: Hiring Interest at Penn and Pitt

	Dependent Variable: Hiring Interest			
	Penn	Pitt	Pitt, Wave 2 Non-Target Major	Pitt, Wave 2 Target Major
GPA	2.196	0.265	-0.196	0.938
	(0.129)	(0.113)	(0.240)	(0.268)
Top Internship	0.897	0.222	0.020	0.098
	(0.081)	(0.074)	(0.142)	(0.205)
Second Internship	0.466	0.212	0.095	0.509
	(0.095)	(0.085)	(0.165)	(0.220)
Work for Money	0.154	0.153	0.144	0.378
	(0.091)	(0.081)	(0.164)	(0.210)
Technical Skills	-0.071	0.107	0.125	-0.035
	(0.090)	(0.077)	(0.149)	(0.211)
Female, White	-0.161	0.028	-0.015	-0.151
	(0.096)	(0.084)	(0.180)	(0.212)
Male, Non-White	-0.169	-0.040	0.002	-0.331
	(0.115)	(0.098)	(0.185)	(0.251)
Female, Non-White	0.028	-0.000	0.182	-0.332
	(0.120)	(0.100)	(0.197)	(0.256)
Observations	2880	3440	642	798
R^2	0.483	0.586	0.793	0.596
p-value for test of joint significance of Majors	< 0.001	< 0.001	0.120	0.850
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	Yes	Yes	Yes	Yes
Order FEs	Yes	Yes	Yes	Yes
Subject FEs	Yes	Yes	Yes	Yes

Table shows OLS regressions of hiring interest from Equation (1). Sample differs in each column as indicated by the column header. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in all specifications. R^2 is indicated for each OLS regression. The p-value of an F-test of joint significance of major fixed effects is indicated for all models.

hiring electrical engineers. Our failure at Pitt was in showing all of these employers resumes with the same underlying components. We recommend that researchers using IRR either target employers that specifically recruit high quality generalists, or construct resumes with appropriate variation within the employers' target areas. For example, if we ran our IRR study again at Pitt, we would ask the *Target Majors* question first and then only generate hypothetical resumes from those majors.

5 Conclusion

This paper introduces a novel methodology, called Incentivized Resume Rating (IRR), to measure employer preferences. The method has employers rate candidate profiles they know to be hypothetical and provides incentives by matching employers to real job seekers based on their reported preferences.

We deploy IRR to study employer preferences for candidates graduating from an Ivy League university. We find that employers highly value both more prestigious work experience the summer before senior year and additional work experience the summer before junior year. We use our rating data to demonstrate that preferences for these characteristics are relatively stable throughout the distribution of candidate quality.

We find no evidence that employers are less interested in female or minority candidates on average, but we find evidence of discrimination among employers recruiting STEM candidates. Moreover, employers report that white female candidates are less likely to accept job offers than their white male counterparts, a novel channel for discrimination. We also find evidence of lower returns to prestigious internships for women and minorities. One possible story that can explain these results is that employers believe other firms have a positive preference for diversity, even though they do not display this preference themselves. It may thus be fruitful to examine in future research whether employers have distorted beliefs about aggregate preferences for diversity, which could harm female and minority candidates in the job market.

Here, we further discuss the benefits and costs of the IRR methodology, highlight lessons learned from our implementation—which point to improvements in the method—and discuss directions for future research.

A key advantage of the IRR methodology is that it avoids the use of deception. Economics experiments generally aspire to be deception-free, but the lack of an incentivized method to elicit employer preferences without deception has made correspondence audits a default tool of choice for labor economists. Audit studies have also expanded into areas where the continued use of deception may be more fraught, since such deception has the potential to alter the preferences or beliefs of subjects.[30] We hope that further development of the IRR method will provide a useful

[30] As prominent examples, researchers have recently audited college professors, requesting in-person meetings [Milkman et al., 2012, 2015], and politicians, requesting information [Butler and Broockman, 2011, Distelhorst and Hou, 2017]. Professors are likely to learn about audit studies *ex post* and may take the existence of such studies as an

alternative for researchers, reducing the need for deceptive field experiments. This would both limit any potential harms of deception—such as to applicants whose profiles may resemble researcher-generated ones—as well as provide a positive externality for researchers whose work requires an audit design (by reducing potential contamination of the subject pool).

A second advantage of the IRR method is that it elicits richer preference information than binary callback decisions.[31] In our implementation, we elicit granular measures of employers' hiring interest and of employers' beliefs about the likelihood of job acceptance. We also see the potential for improvements in preference elicitation by better mapping these metrics into hiring decisions, by collecting additional information from employers, and by raising the stakes, which we discuss below.

The IRR method has other advantages. IRR can access subject populations that are inaccessible with audit or resume audit methods. IRR allows researchers to gather rich data from a single subject—each employer in our implementation rates 40 resumes—which is helpful for power and makes it feasible to identify preferences for characteristics within individual subjects. IRR allows researchers to randomize many candidate characteristics independently and simultaneously, which can be used to explore how employers respond to interactions of candidate characteristics. Finally, IRR allows researchers to collect supplemental data about research subjects, which can be correlated with subject-level preference measures and allows researchers to better understand their pool of employers.

A final advantage of IRR is that it may provide direct benefits to subjects and other participants in the labor market being studied; this advantage stands in stark contrast to using subject time without consent, as is necessary in audit studies. We solicited subject feedback at numerous points throughout the study and heard very few concerns.[32] Instead, many employers reported positive feedback. Positive feedback also came by way of the career services offices at Penn and Pitt, which were in more-direct contact with our employer subjects. Both offices continued the experiment for a second wave of recruitment and expressed interest in making the experiment a permanent feature of their recruiting processes. In our meetings, the career services offices reported seeing value in IRR to improve their matching process and to learn how employers valued student characteristics (e.g., informing the advice they could give to students about pursuing summer work

excuse to ignore emails from students in the future. Audits of politicians' responses to correspondence from putative constituents might distort politicians' beliefs about the priorities of the populations they serve, especially when researchers seek a politician-level audit measure, which requires sending many fake requests to the same politician.

[31] Bertrand and Duflo [2016] argues that the literature has generally not evolved past measuring differences in callback means between groups, and that it has been less successful in illuminating mechanisms driving these differences. That said, there have been some exceptions, like Bartoš et al. [2016], which uses emails containing links to learn more about candidates to show that less attention is allocated to candidates who are discriminated against. Another exception is Bohren et al. [2018], which uses evaluations of answers posted on an online Q&A forum—which are not conflated with concerns about likelihood of acceptance—to test a dynamic model of mistaken discriminatory beliefs.

[32] First, we solicited feedback in an open comments field of the survey itself. Second, we invited participants to contact us with questions or requests for additional matches when we sent the 10 resumes. Third, we ran a follow-up survey in which we asked about hiring outcomes for the recommended matches (unfortunately, we offered no incentive to complete the follow-up survey and so its participation was low).

and leadership experience and how to write their resumes). While we did not solicit feedback from student participants in the study, we received hundreds of resumes from students at each school, suggesting that they valued the prospect of having their resumes sent to employers.[33]

Naturally, IRR also has some limitations. Because the IRR method informs subjects that responses will be used in research, it may lead to experimenter demand effects (see, e.g., de Quidt et al. [2018]). We believe the impact of any experimenter demand effects is likely small, as employers appeared to view our survey tool as a way to identify promising candidates, rather than as being connected to research (see discussion in Section 2). For this reason, though, as well as others highlighted in Section 3.4, IRR may be less well equipped to identify explicit bias than implicit bias. More broadly, we cannot guarantee that employers treat our hypothetical resumes as they would real job candidates. As discussed in the Introduction, however, future work could help validate employer attention in IRR studies.[34] In addition, because the two outcome measures in our study are hypothetical objects rather than stages of the hiring process, in our implementation of IRR we cannot draw a direct link between our findings and hiring outcomes. Below, we discuss how this might be improved in future IRR implementations.

A final cost of running an IRR study is that it requires finding an appropriate subject pool and candidate matching pool, which may not be available to all researchers. It also requires an investment in constructing the hypothetical resumes (e.g., scraping and sanitizing resume components) and developing the process to match employer preferences to candidates. Fortunately, the time and resources we devoted to developing the survey tool software can be leveraged by other researchers.

Future research using IRR can certainly improve upon our implementation. First, as discussed at length in Section 4, our failed attempt to replicate at Pitt highlights that future researchers must take care to effectively tailor the content of resumes to match the hiring needs of their subjects. Second, we suggest developing a way to translate Likert-scale responses to the callback decisions typical in correspondence audit studies. One idea is to ask employers to additionally answer, potentially for a subset of resumes, a question of the form: "Would you invite **[Candidate Name]** for an interview?" By having the Likert-scale responses and this measure, researchers could identify what combination of the hiring interest and likelihood of acceptance responses translates into a typical callback decision (and, potentially, how the weight placed on each component varies by firm). Researchers could also explore the origin and accuracy of employer beliefs about likelihood of acceptance by asking job candidates about their willingness to work at participating firms. Third, researchers could increase the stakes of IRR incentives (e.g., by asking employer subjects

[33]Student involvement only required uploading a resume and completing a short preference survey. We did not notify students when they were matched with a firm, in order to give the firms freedom to choose which students to contact. Thus, most students were unaware of whether or not they were recommended to a firm. We recommended 207 unique student resumes over the course of the study, highlighting the value to students.

[34]The time employers spent evaluating resumes in our study at Penn had a median of 18 seconds and a mean that was substantially higher (and varies based on how outliers are handled). These measures are comparable to estimates of time spent screening real resumes (which include estimates of 7.4 seconds per resume [Dishman, 2018] and a mean of 45 seconds per resume [Culwell-Block and Sellers, 1994]).

to guarantee interviews to a subset of the recommended candidates) and gather more information on resulting interviews and hiring outcomes (e.g., by building or leveraging an existing platform to measure employer and candidate interactions).[35]

While we used IRR to measure the preferences of employers in a particular labor market, the underlying incentive structure of the IRR method is much more general, and we see the possibility of it being applied outside of the resume rating context. At the heart of IRR is a method to elicit preference information from experimental subjects by having them evaluate hypothetical objects and offering them an incentive that increases in value as preference reports become more accurate. Our implementation of IRR achieves this by eliciting continuous Likert-scale measures of hypothetical resumes, using machine learning to estimate the extent to which employers care about various candidate characteristics, and providing employers with resumes of real candidates that they are estimated to like best. Researchers could take a similar strategy to explore preferences of professors over prospective students, landlords over tenants, customers over products, individuals over dating profiles, and more, providing a powerful alternative to deceptive studies.

[35] An additional benefit of collecting data on interviews and hiring is that it would allow researchers to measure the impact of IRR matches on hiring, in addition to validating the quality of the matches (e.g., researchers could identify 12 potential matches and randomize which 10 are sent to employers, identifying the effect of sending a resume to employers on interview and hiring outcomes). If employers do respond to the matches, one could imagine using IRR as an intervention in labor markets to help mitigate discrimination in hiring, since IRR matches can be made while ignoring race and gender.

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Incentivized Resume Rating: Eliciting Employer Preferences without Deception

Corinne Low, Judd B. Kessler, Colin D. Sullivan

FOR ONLINE PUBLICATION ONLY

Appendices

We provide three appendices. In Appendix A, we describe the design of our experiment in detail, including recruitment materials (A.1), survey tool construction (A.2), and the candidate matching process (A.3). In Appendix B, we present additional analyses and results, including human capital results (B.1), regressions weighted by GPA (B.2), a discussion of our discrimination results (B.4), and a discussion of preferences over the quality distribution (B.3). In Appendix C, we discuss additional details related to replicating our experiment at Pitt.

A Experimental Design Appendix

A.1 Recruitment Materials

University of Pennsylvania Career Services sent recruitment materials to both recruiting firms and graduating seniors to participate in the study. All materials marketed the study as an additional tool to connect students with firms, rather than a replacement for any usual recruiting efforts. The recruitment email for employers, shown in Figure A.1, was sent to a list of contacts maintained by Career Services and promised to use a "newly developed machine-learning algorithm to identify candidates who would be a particularly good fit for your job based on your evaluations." In our replication at the University of Pittsburgh, a similar email was sent from the Pitt Office of Career Development and Placement Assistance. Penn Career Services recruited graduating seniors to participate as part of the candidate matching pool through their regular newsletter called the "Friday Flash." The relevant excerpt from this email newsletter is shown in Figure A.2.

We timed recruitment so that employers would receive their 10 resume matches around the time they were on campus in order to facilitate meeting the job seekers. In addition, we offered webinars for employers who were interested in learning about the survey screening experience before they participated. Employers could anonymously join a call where they viewed a slideshow about the survey software and could submit questions via chat box. Attendance at these webinars was low.

Figure A.1: Employer Recruitment Email

From: upenn@csm.symplicity.com [mailto:upenn@csm.symplicity.com]
Sent: Tuesday, July 26, 2016 1:34 PM
To: [redacted]
Subject: Identify Top Penn Students for your Firm

Dear [redacted]

This year, Penn Career Services is participating in a pilot with two Wharton professors who are developing a new tool that can help you to identify potential job candidates from the University of Pennsylvania for post-graduate positions.

The tool is designed to identify top candidates for your open positions and provides you with those candidates' contact information and resumes so you can invite them to coffee chats, to info sessions, and to apply for a job at your organization. Since the tool uses data-driven methods to identify candidates, we see this as a useful complement to firms' existing methods for identifying promising candidates.

Completing the tool takes about 30 minutes and involves evaluating 40 hypothetical resumes. After evaluating these resumes, the tool uses a newly developed machine-learning algorithm to identify candidates who would be a particularly good fit for your job based on your evaluations. The Wharton professors will also use a completely anonymized version of your data to perform research on broader trends in what firms value in hiring, and they will be glad to share these insights with your company once the research is complete. To be provided with potential candidates for a position, at least one person from your firm must complete the tool. If possible, having multiple individuals participate will help increase the accuracy of the algorithm's recommendations. Additionally, if you are hiring for different positions within your organization, we recommend at least one person from your organization take the tool for each open position so you get a list of candidates tailored for each job opening. Rising Penn seniors will be invited to participate in the trial by submitting their resumes beginning on August 22nd, and we plan to have candidate recommendations to you by early September.

To take the tool, please click the link here:

https://wharton.qualtrics.com/SE/?SID=SV_3I3ohtNPn2R8c97

If you you would like to discuss more about how the tool could be useful for your firm, or have any questions, please contact the Wharton researchers: Judd B. Kessler (judd.kessler@wharton.upenn.edu) and Corinne Low (corlow@wharton.upenn.edu).

Sincerely,

Barbara Hewitt, Senior Associate Director, Career Services

Email sent to firms recruiting at Penn originating from the Senior Associate Director of Career Services at the University of Pennsylvania. Subjects who followed the link in the email were taken to the instructions (Figure A.3).

Figure A.2: Email Announcement to Graduating Seniors

From: Career Services - Wharton Class of 2017 <CAREERSERVICES2017@LISTS.UPENN.EDU> On Behalf Of Ross, S. David
Sent: Friday, August 26, 2016 5:20 PM
To: CAREERSERVICES2017@LISTS.UPENN.EDU
Subject: Wharton Seniors: Penn Career Services Senior Friday Flash, August 26, 2016

Welcome back! I hope you had a wonderful and productive summer. This is the first issue of the senior Career Services Friday Flash for the year. Barbara Hewitt is the Senior Associate Director in the Career Services office working with Wharton undergraduate students and alumni - she will manage the Career Services listserv for Wharton seniors and will be sending you weekly Friday Flash e-mails to keep you updated on workshops, job postings, employer presentations, career resources and more. Barbara and I look forward to working with you this year as you begin (or continue!) to think about life after Penn. Please do come in to speak with either of us about your plans. Also, please note that On Campus Recruiting activities have started, so don't delay if you would like to participate!

[OTHER TEXT APPEARED HERE]

Announcements

An Opportunity To Reach More Employers

This year, Penn Career Services is working with two Wharton professors on a pilot that can help you get noticed by top employers in all fields. Wharton professors Judd B. Kessler and Corinne Low have developed a tool that analyzes employer preferences for job candidates and then uses machine learning to identify Penn seniors who may be a good fit for the employer's positions. Employers across a variety of industries (e.g. consulting, finance, technology, etc.) have already participated in the pilot by providing preferences for job candidates. Upload your resume now to be eligible to participate! Only candidates who upload their resume through this link can participate in the pilot. To upload your resume, click here: https://wharton.qualtrics.com/SE/?SID=SV_bryPbgBn4rEXD0h. If you have any questions about the pilot, please contact the Wharton professors running it: Judd B. Kessler (judd.kessler@wharton.upenn.edu) and Corinne Low (corlow@wharton.upenn.edu). (Note: this pilot will be run in parallel to all existing recruiting activities.)

Excerpt from email newsletter sent to the Career Services office mailing list. The email originated from the Senior Associate Director of Career Services at the University of Pennsylvania. Students following the link were taken to a survey page where they were asked to upload their resumes and to answer a brief questionnaire about their job search (page not shown).

A.2 Survey Tool Design

In this appendix, we describe the process of generating hypothetical resumes. This appendix should serve to provide additional details about the selection and randomization of resume components, and as a guide to researchers wishing to implement our methodology. In Section A.2.1, we describe the structure of the IRR survey tool and participant experience. In Section A.2.2, we describe the structure of our hypothetical resumes. In Section A.2.3, we detail the randomization of candidate gender and race through names. Section A.2.4 details the randomization of educational background. Section A.2.5 describes the process we used to collect and scrape real resume components to randomize work experience, leadership experience, and skills.

A.2.1 Survey Tool Structure

We constructed the survey tool using Qualtrics software for respondents to access from a web browser. Upon opening the survey link, respondents must enter an email address on the instructions page (see Figure A.3) to continue. Respondents then select the type of candidates they will evaluate for their open position, either "Business (Wharton), Social Sciences, and Humanities" or "Science, Engineering, Computer Science, and Math." In addition, they may enter the position title they are looking to fill. The position title is not used in determining the content of the hypothetical candidate resumes. The major selection page is shown in Figure A.4. After this selection, the randomization software populates 40 resumes for the respondent to evaluate, drawing on different content by major type. The subject then evaluates 40 hypothetical resumes. After every 10 resumes, a break page encourages subjects to continue.

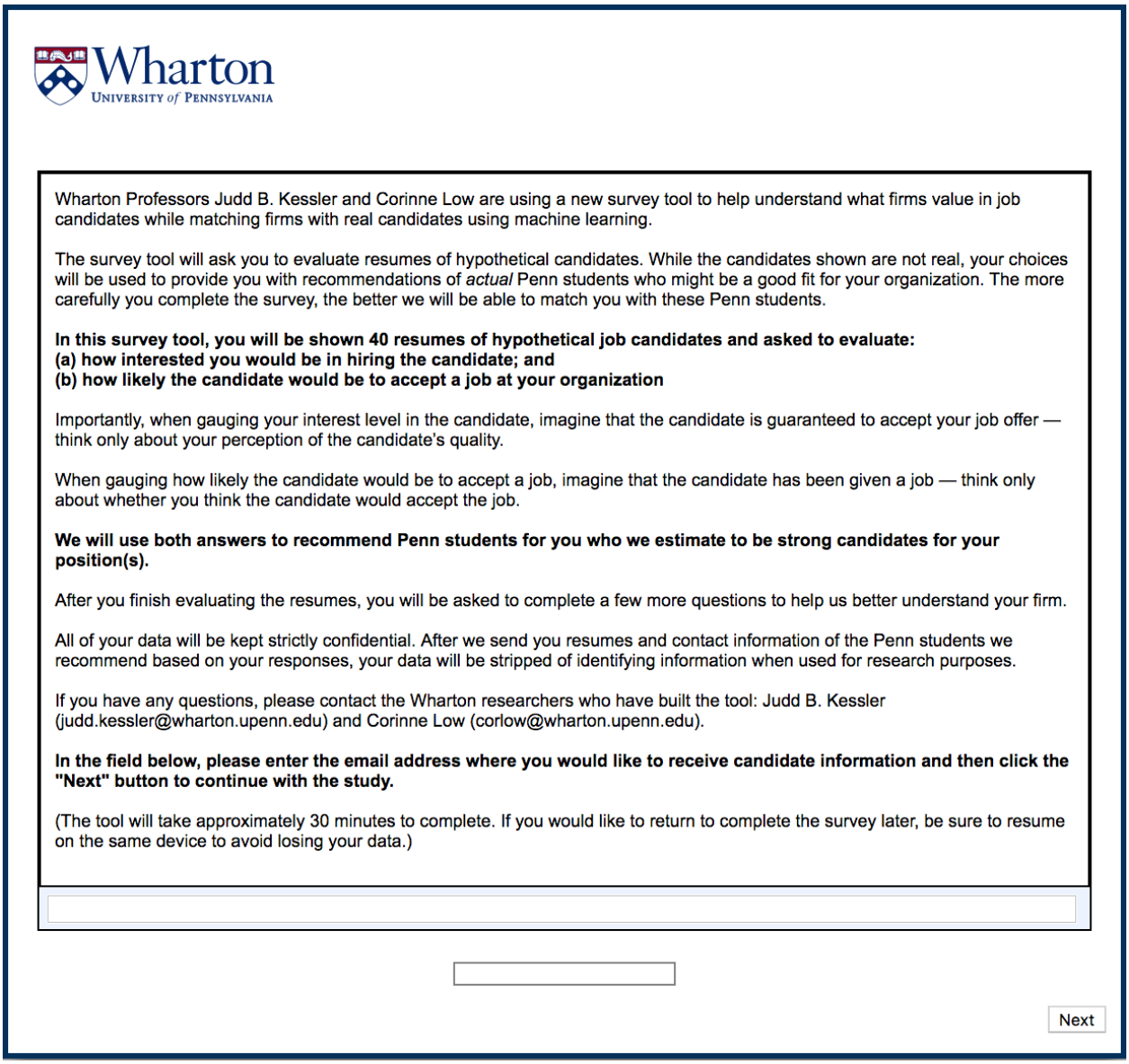
A.2.2 Resume Structure

We designed our resumes to combine realism with the requirements of experimental identification. We designed 10 resume templates to use as the basis for the 40 resumes in the tool. Each template presented the same information, in the same order, but with variations in page layout and font. Figures A.5 and A.6 show sample resume templates. All resumes contained five sections, in the following order: Personal Information (including name and blurred contact information); Education (GPA, major, school within university); Work Experience; Leadership Experience; and Skills.[36] While the real student resumes we encountered varied in content, most contained some subset of these sections. Since our main objective with resume variation was to improve realism for each subject rather than to test the effectiveness of different resume formats, we did not vary the order of the resume formats across subjects. In other words, the first resume always had the same font and page layout for each subject, although the content of the resume differed each time. Given that formats are in a fixed order in the 40 hypothetical resumes, the order fixed effects

[36] These sections were not always labelled as such on candidate resumes. Personal Information was generally not identified, though each resume contained a name and blurred text in place of contact information. Skills were also marked as "Skills & Interests" and "Skill Summary".

included in most specifications control for any effect of resume format. Resumes templates were built in HTML/CSS for display in a web browser, and populated dynamically in Qualtrics using JavaScript. Randomization occurred for all 40 resumes simultaneously, without replacement, each time a subject completed the instructions and selected their major category of interest. Each resume layout was flexible enough to accommodate different numbers of bullet points for each experience, and different numbers of work experiences. If only one job was listed on the resume, for instance, the work experience section of the resume appeared shorter rather than introducing empty space.

Figure A.3: Survey Tool Instructions & Contact Information



Wharton
UNIVERSITY of PENNSYLVANIA

Wharton Professors Judd B. Kessler and Corinne Low are using a new survey tool to help understand what firms value in job candidates while matching firms with real candidates using machine learning.

The survey tool will ask you to evaluate resumes of hypothetical candidates. While the candidates shown are not real, your choices will be used to provide you with recommendations of *actual* Penn students who might be a good fit for your organization. The more carefully you complete the survey, the better we will be able to match you with these Penn students.

In this survey tool, you will be shown 40 resumes of hypothetical job candidates and asked to evaluate:
(a) how interested you would be in hiring the candidate; and
(b) how likely the candidate would be to accept a job at your organization

Importantly, when gauging your interest level in the candidate, imagine that the candidate is guaranteed to accept your job offer — think only about your perception of the candidate's quality.

When gauging how likely the candidate would be to accept a job, imagine that the candidate has been given a job — think only about whether you think the candidate would accept the job.

We will use both answers to recommend Penn students for you who we estimate to be strong candidates for your position(s).

After you finish evaluating the resumes, you will be asked to complete a few more questions to help us better understand your firm.

All of your data will be kept strictly confidential. After we send you resumes and contact information of the Penn students we recommend based on your responses, your data will be stripped of identifying information when used for research purposes.

If you have any questions, please contact the Wharton researchers who have built the tool: Judd B. Kessler (judd.kessler@wharton.upenn.edu) and Corinne Low (corlow@wharton.upenn.edu).

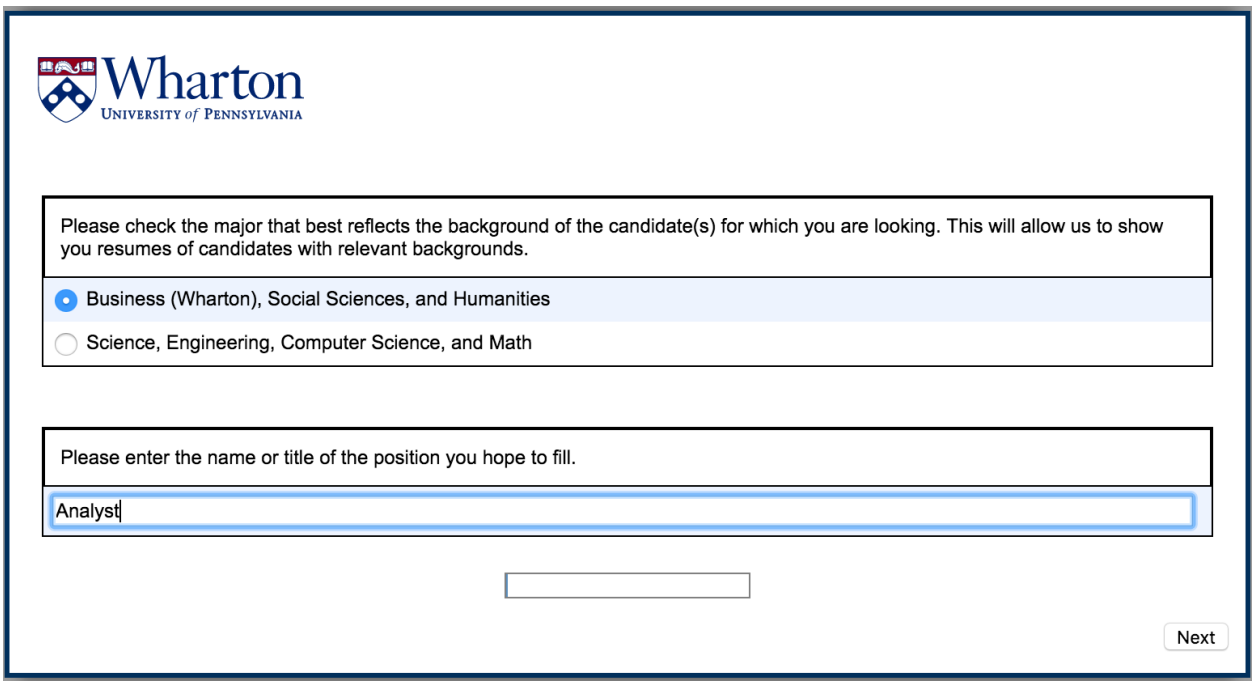
In the field below, please enter the email address where you would like to receive candidate information and then click the "Next" button to continue with the study.

(The tool will take approximately 30 minutes to complete. If you would like to return to complete the survey later, be sure to resume on the same device to avoid losing your data.)

Next

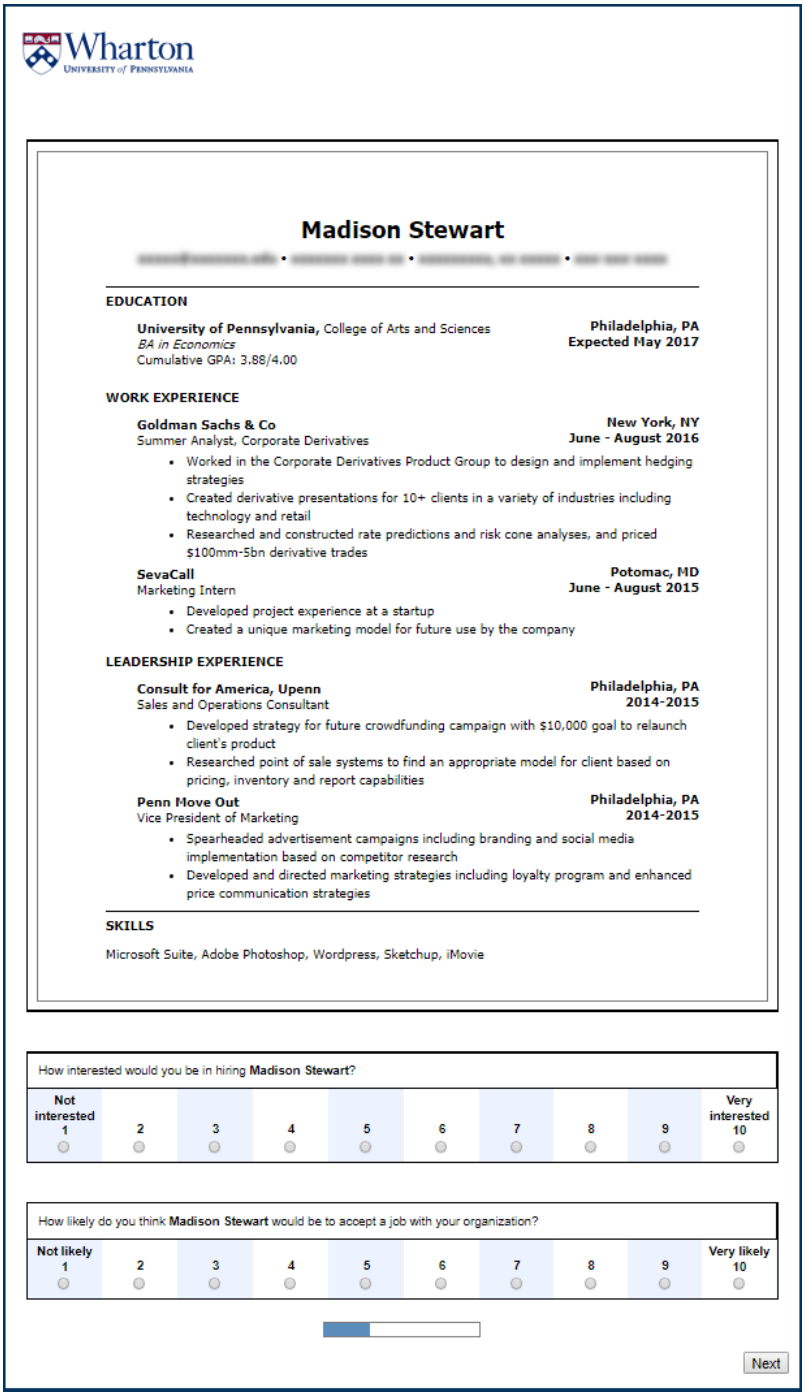
Screenshot of the instructions at the start of the survey tool. This page provided information to subjects and served as instructions. Subjects entered an email address at the bottom of the screen to proceed with the study; the resumes of the 10 real job seekers used as an incentive to participate are sent to this email address.

Figure A.4: Major Type Selection



Screenshot of major selection page, as shown to subjects recruiting at the University of Pennsylvania. Subjects must select either Business (Wharton), Social Sciences, and Humanities, or Science, Engineering, Computer Science, and Math. Subjects may also enter the name of the position they wish to fill in the free text box; the information in this box was not used for analysis. Here, we have selected Business (Wharton), Social Sciences, and Humanities and entered "Analyst" as a demonstration only—by default all radio boxes and text boxes were empty for all subjects.

Figure A.5: Sample Resume



Wharton
UNIVERSITY of PENNSYLVANIA

Madison Stewart

[redacted]

EDUCATION

University of Pennsylvania, College of Arts and Sciences — **Philadelphia, PA**
BA in Economics — **Expected May 2017**
Cumulative GPA: 3.88/4.00

WORK EXPERIENCE

Goldman Sachs & Co — **New York, NY**
Summer Analyst, Corporate Derivatives — **June - August 2016**

- Worked in the Corporate Derivatives Product Group to design and implement hedging strategies
- Created derivative presentations for 10+ clients in a variety of industries including technology and retail
- Researched and constructed rate predictions and risk cone analyses, and priced $100mm-5bn derivative trades

SevaCall — **Potomac, MD**
Marketing Intern — **June - August 2015**

- Developed project experience at a startup
- Created a unique marketing model for future use by the company

LEADERSHIP EXPERIENCE

Consult for America, Upenn — **Philadelphia, PA**
Sales and Operations Consultant — **2014-2015**

- Developed strategy for future crowdfunding campaign with $10,000 goal to relaunch client's product
- Researched point of sale systems to find an appropriate model for client based on pricing, inventory and report capabilities

Penn Move Out — **Philadelphia, PA**
Vice President of Marketing — **2014-2015**

- Spearheaded advertisement campaigns including branding and social media implementation based on competitor research
- Developed and directed marketing strategies including loyalty program and enhanced price communication strategies

SKILLS

Microsoft Suite, Adobe Photoshop, Wordpress, Sketchup, iMovie

How interested would you be in hiring **Madison Stewart**?

Not interested 1	2	3	4	5	6	7	8	9	Very interested 10

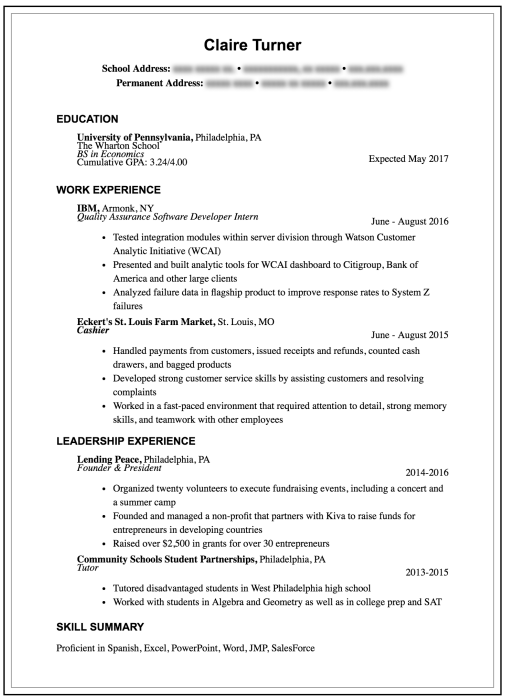
How likely do you think **Madison Stewart** would be to accept a job with your organization?

Not likely 1	2	3	4	5	6	7	8	9	Very likely 10

Next

A sample resume rating page from the Incentivized Resume Rating tool. Each resume is dynamically generated when the subject begins the study. Each resume has five sections: Personal Information (including first and last name, and blurred text to represent contact information); Education Information (university, school within university, degree, major, GPA, and expected graduation date); Work Experience (one or two experiences with employer name, location, job title, date, and descriptive bullet points); Leadership Experience (two experiences with organization, location, position title, date, and descriptive bullet points); and Skills. Resume randomization described in detail in Section 2 and Appendix A.2. At the bottom of each resume, subjects must respond to two questions before proceeding: "How interested would you be in hiring [**Name**]?" and "How likely do you think [**Name**] would be to accept a job with your organization?"

Figure A.6: Four Sample Resumes



Claire Turner

School Address: • •
Permanent Address: • •

EDUCATION

University of Pennsylvania, Philadelphia, PA
The Wharton School
BS in Economics Expected May 2017
Cumulative GPA: 3.24/4.00

WORK EXPERIENCE

IBM, Armonk, NY
Quality Assurance Software Developer Intern June - August 2016

- Tested integration modules within server division through Watson Customer Analytic Initiative (WCAI)
- Presented and built analytic tools for WCAI dashboard to Citigroup, Bank of America and other large clients
- Analyzed failure data in flagship product to improve response rates to System Z failures

Eckert's St. Louis Farm Market, St. Louis, MO
Cashier June - August 2015

- Handled payments from customers, issued receipts and refunds, counted cash drawers, and bagged products
- Developed strong customer service skills by assisting customers and resolving complaints
- Worked in a fast-paced environment that required attention to detail, strong memory skills, and teamwork with other employees

LEADERSHIP EXPERIENCE

Lending Peace, Philadelphia, PA
Founder & President 2014-2016

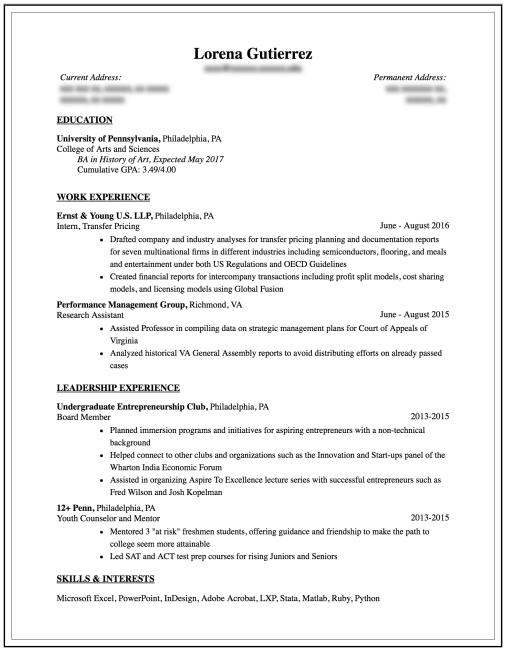
- Organized twenty volunteers to execute fundraising events, including a concert and a summer camp
- Founded and managed a non-profit that partners with Kiva to raise funds for entrepreneurs in developing countries
- Raised over $2,500 in grants for over 30 entrepreneurs

Community Schools Student Partnerships, Philadelphia, PA
Tutor 2013-2015

- Tutored disadvantaged students in West Philadelphia high school
- Worked with students in Algebra and Geometry as well as in college prep and SAT

SKILL SUMMARY

Proficient in Spanish, Excel, PowerPoint, Word, JMP, SalesForce



Lorena Gutierrez

Current Address: *Permanent Address:*

EDUCATION

University of Pennsylvania, Philadelphia, PA
College of Arts and Sciences
BA in History of Art, Expected May 2017
Cumulative GPA: 3.49/4.00

WORK EXPERIENCE

Ernst & Young U.S. LLP, Philadelphia, PA
Intern, Transfer Pricing June - August 2016

- Drafted company and industry analyses for transfer pricing planning and documentation reports for seven multinational firms in different industries including semiconductors, flooring, and meals and entertainment under both US Regulations and OECD Guidelines
- Created financial reports for intercompany transactions including profit split models, cost sharing models, and licensing models using Global Fusion

Performance Management Group, Richmond, VA
Research Assistant June - August 2015

- Assisted Professor in compiling data on strategic management plans for Court of Appeals of Virginia
- Analyzed historical VA General Assembly reports to avoid distributing efforts on already passed cases

LEADERSHIP EXPERIENCE

Undergraduate Entrepreneurship Club, Philadelphia, PA
Board Member 2013-2015

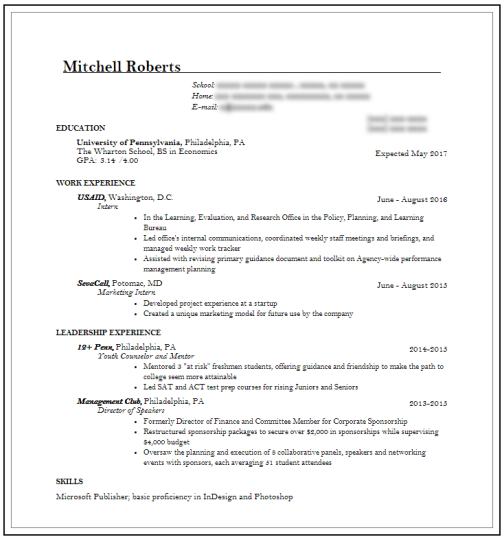
- Planned immersion programs and initiatives for aspiring entrepreneurs with a non-technical background
- Helped connect to other clubs and organizations such as the Innovation and Start-ups panel of the Wharton India Economic Forum
- Assisted in organizing Aspire To Excellence lecture series with successful entrepreneurs such as Fred Wilson and Josh Kopelman

12+ Penn, Philadelphia, PA
Youth Counselor and Mentor 2013-2015

- Mentored 3 "at risk" freshmen students, offering guidance and friendship to make the path to college seem more attainable
- Led SAT and ACT test prep courses for rising Juniors and Seniors

SKILLS & INTERESTS

Microsoft Excel, PowerPoint, InDesign, Adobe Acrobat, LXP, Stata, Matlab, Ruby, Python



Mitchell Roberts

School:
Home:
E-mail:

EDUCATION

University of Pennsylvania, Philadelphia, PA
The Wharton School, BS in Economics Expected May 2017
GPA: 3.14 /4.00

WORK EXPERIENCE

USAID, Washington, D.C. June - August 2016
Intern

- In the Learning, Evaluation, and Research Office in the Policy, Planning, and Learning Bureau
- Led office's internal communications, coordinated weekly staff meetings and briefings, and managed weekly work tracker
- Assisted with revising primary guidance document and toolkit on Agency-wide performance management planning

SeneCell, Potomac, MD June - August 2015
Marketing Intern

- Developed project experience at a startup
- Created a unique marketing model for future use by the company

LEADERSHIP EXPERIENCE

12+ Penn, Philadelphia, PA 2014-2015
Youth Counselor and Mentor

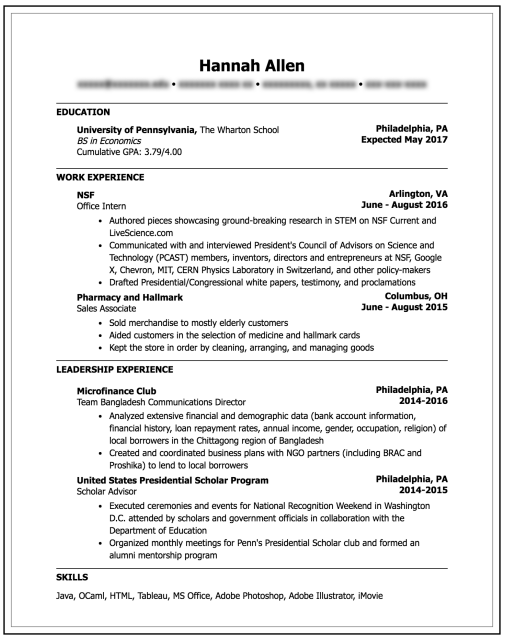
- Mentored 3 "at risk" freshmen students, offering guidance and friendship to make the path to college seem more attainable
- Led SAT and ACT test prep courses for rising Juniors and Seniors

Management Club, Philadelphia, PA 2013-2015
Director of Speakers

- Formerly Director of Finance and Committee Member for Corporate Sponsorship
- Restructured sponsorship packages to secure over $5,000 in sponsorships while supervising $4,000 budget
- Oversaw the planning and execution of 8 collaborative panels, speakers and networking events with sponsors, each averaging 81 student attendees

SKILLS

Microsoft Publisher; basic proficiency in InDesign and Photoshop



Hannah Allen

• • •

EDUCATION

University of Pennsylvania, The Wharton School Philadelphia, PA
BS in Economics Expected May 2017
Cumulative GPA: 3.79/4.00

WORK EXPERIENCE

NSF Arlington, VA
Office Intern June - August 2016

- Authored pieces showcasing ground-breaking research in STEM on NSF Current and LiveScience.com
- Communicated with and interviewed President's Council of Advisors on Science and Technology (PCAST) members, inventors, directors and entrepreneurs at NSF, Google X, Chevron, MIT, CERN Physics Laboratory in Switzerland, and other policy-makers
- Drafted Presidential/Congressional white papers, testimony, and proclamations

Pharmacy and Hallmark Columbus, OH
Sales Associate June - August 2015

- Sold merchandise to mostly elderly customers
- Aided customers in the selection of medicine and hallmark cards
- Kept the store in order by cleaning, arranging, and managing goods

LEADERSHIP EXPERIENCE

Microfinance Club Philadelphia, PA
Team Bangladesh Communications Director 2014-2016

- Analyzed extensive financial and demographic data (bank account information, financial history, loan repayment rates, annual income, gender, occupation, religion) of local borrowers in the Chittagong region of Bangladesh
- Created and coordinated business plans with NGO partners (including BRAC and Proshika) to lend to local borrowers

United States Presidential Scholar Program Philadelphia, PA
Scholar Advisor 2014-2015

- Executed ceremonies and events for National Recognition Weekend in Washington D.C. attended by scholars and government officials in collaboration with the Department of Education
- Organized monthly meetings for Penn's Presidential Scholar club and formed an alumni mentorship program

SKILLS

Java, OCaml, HTML, Tableau, MS Office, Adobe Photoshop, Adobe Illustrator, iMovie

Four sample resumes generated by the survey tool. Note that the resumes each have a different format, differentiated by elements such as font, boldface type, horizontal rules, location of information, and spacing. All resumes have the same five sections: Personal Information, Education, Work Experience, Leadership Experience, and Skills. Resumes differ in length based on the dynamically selected content, such as the randomized number of work experiences and the (non-randomized) number of description bullet points associated with an experience.

A.2.3 Names

A hypothetical candidate name appears as the first element on each resume. Names were generated to be highly indicative of race and gender, following the approach of Fryer and Levitt [2004]. As described in Section 2.3.4, first names were selected from a dataset of all births in the state of Massachusetts between 1989-1996 and in New York City between 1990-1996. These years reflect the approximate birth years of the job seekers in our study. We identified 100 first names with the most indicative race and gender for each of the following race-gender combinations: Asian Female, Asian Male, Black Female, Black Male, Hispanic Female, Hispanic Male, White Female, and White Male. We then eliminated names that were gender-ambiguous in the broad sample even if they might be unambiguous within an ethnic group. We also eliminated names strongly indicative of religion. We followed a similar process for last names, using name and ethnicity data from the 2000 Census. Finally, we paired first and last names together by race and selected 50 names for each race-gender combination for randomization. Names of hypothetical female candidates are shown in Table A.1; names of hypothetical male candidates are shown in Table A.2.

At the point of randomization, names were drawn without replacement according to a distribution of race and gender intended to reflect the US population (50% female, 50% male; 65.7% White, 16.8% Hispanic, 12.6% Black, 4.9% Asian). Gender and race were randomized independently. In other words, we selected either Table A.1 or Table A.2 with equal probability, then selected a column to draw from according to the race probabilities. Finally, names were selected uniformly and without replacement from the appropriate column of the table. We use the variation induced by these names for the analysis variables *Female, White*; *Male, Non-White*; *Female, Non-White*; and *Not a White Male*.

A.2.4 Education

We randomized two components in the Education section of each resume: grade point average (GPA) and major. We also provided an expected graduation date (fixed to May 2017 for all students), the name of the university (University of Pennsylvania), the degree (BA or BS) and the name of the degree-granting school within Penn to maintain realism.

GPA We selected GPA from a $Unif[2.90, 4.00]$ distribution, rounding to the nearest hundredth. We chose to include GPA on all resumes, although some students omit GPA on real resumes. We decided to avoid the complexity of forcing subjects to make inferences about missing GPAs. The range was selected to approximate the range of GPAs observed on real resumes. We chose a uniform distribution (rather than, say, a Gaussian) to increase our power to identify preferences throughout the distribution. We did not specify GPA in major on any resumes. We use this variation to define the variable *GPA*.

Table A.1: Female Names Populating Resume Tool

Asian Female	**Black Female**	**Hispanic Female**	**White Female**
Tina Zheng	Jamila Washington	Ivette Barajas	Allyson Wood
Annie Xiong	Asia Jefferson	Nathalie Orozco	Rachael Sullivan
Julie Xu	Essence Banks	Mayra Zavala	Katharine Myers
Michelle Zhao	Monique Jackson	Luisa Velazquez	Colleen Peterson
Linda Zhang	Tianna Joseph	Jessenia Meza	Meghan Miller
Anita Zhu	Janay Mack	Darlene Juarez	Meaghan Murphy
Alice Jiang	Nia Williams	Thalia Ibarra	Lindsey Fisher
Esther Zhou	Latoya Robinson	Perla Cervantes	Paige Cox
Winnie Thao	Jalisa Coleman	Lissette Huerta	Katelyn Cook
Susan Huang	Imani Harris	Daisy Espinoza	Jillian Long
Sharon Yang	Malika Sims	Cristal Vazquez	Molly Baker
Gloria Hwang	Keisha James	Paola Cisneros	Heather Nelson
Diane Ngo	Shanell Thomas	Leticia Gonzalez	Alison Hughes
Carmen Huynh	Janae Dixon	Jesenia Hernandez	Bridget Kelly
Angela Truong	Latisha Daniels	Alejandra Contreras	Hayley Russell
Janet Kwon	Zakiya Franklin	Iliana Ramirez	Carly Roberts
Janice Luong	Kiana Jones	Julissa Esparza	Bethany Phillips
Irene Cheung	Ayana Grant	Giselle Alvarado	Kerry Bennett
Amy Choi	Ayanna Holmes	Gloria Macias	Kara Morgan
Shirley Yu	Shaquana Frazier	Selena Zuniga	Kaitlyn Ward
Kristine Nguyen	Shaniqua Green	Maribel Ayala	Audrey Rogers
Cindy Wu	Tamika Jenkins	Liliana Mejia	Jacquelyn Martin
Joyce Vu	Akilah Fields	Arlene Rojas	Marissa Anderson
Vivian Hsu	Shantel Simmons	Cristina Ochoa	Haley Clark
Jane Liang	Shanique Carter	Yaritza Carillo	Lindsay Campbell
Maggie Tsai	Tiara Woods	Guadalupe Rios	Cara Adams
Diana Pham	Tierra Bryant	Angie Jimenez	Jenna Morris
Wendy Li	Raven Brown	Esmeralda Maldonado	Caitlin Price
Sally Hoang	Octavia Byrd	Marisol Cardenas	Kathryn Hall
Kathy Duong	Tyra Walker	Denisse Chavez	Emma Bailey
Lily Vang	Diamond Lewis	Gabriela Mendez	Erin Collins
Helen Trinh	Nyasia Johnson	Jeanette Rosales	Marisa Reed
Sandy Oh	Aliyah Douglas	Rosa Castaneda	Madeleine Smith
Christine Tran	Aaliyah Alexander	Beatriz Rodriguez	Mackenzie King
Judy Luu	Princess Henderson	Yessenia Acevedo	Sophie Thompson
Grace Cho	Shanae Richardson	Carolina Guzman	Madison Stewart
Nancy Liu	Kenya Brooks	Carmen Aguilar	Margaret Parker
Lisa Cheng	Charisma Scott	Yesenia Vasquez	Kristin Gray
Connie Yi	Shante Hunter	Ana Munoz	Michaela Evans
Tiffany Phan	Jada Hawkins	Xiomara Ortiz	Jaclyn Cooper
Karen Lu	Shanice Reid	Lizbeth Rivas	Hannah Allen
Tracy Chen	Chanelle Sanders	Genesis Sosa	Zoe Wilson
Betty Dinh	Shanequa Bell	Stephany Salinas	Caitlyn Young
Anna Hu	Shaniece Mitchell	Lorena Gutierrez	Charlotte Moore
Elaine Le	Ebony Ford	Emely Sandoval	Kaitlin Wright
Sophia Ly	Tanisha Watkins	Iris Villarreal	Holly White
Jenny Vo	Shanelle Butler	Maritza Garza	Kate Taylor
Monica Lin	Precious Davis	Marilyn Arroyo	Krista Hill
Joanne Yoon	Asha Willis	Lourdes Soto	Meredith Howard
Priya Patel	Ashanti Edwards	Gladys Herrera	Claire Turner

Names of hypothetical female candidates. 50 names were selected to be highly indicative of each combination of race and gender. A name drawn from these lists was displayed at the top of each hypothetical resume, and in the questions used to evaluate the resumes. First and last names were linked every time they appeared. For details on the construction and randomization of names, see Section 2.3.4 and Appendix A.2.3.

Table A.2: Male Names Populating Resume Tool

Asian Male	**Black Male**	**Hispanic Male**	**White Male**
Richard Thao	Rashawn Washington	Andres Barajas	Kyle Wood
Samuel Truong	Devonte Jefferson	Julio Orozco	Derek Sullivan
Daniel Cheung	Marquis Banks	Marcos Zavala	Connor Myers
Alan Tsai	Tyree Jackson	Mike Velazquez	Douglas Peterson
Paul Li	Lamont Joseph	Jose Meza	Spencer Miller
Steven Zhang	Jaleel Mack	Alfredo Juarez	Jackson Murphy
Matthew Zheng	Javon Williams	Fernando Ibarra	Bradley Fisher
Alex Vu	Darryl Robinson	Gustavo Cervantes	Drew Cox
Joshua Vo	Kareem Coleman	Adonis Huerta	Lucas Cook
Brandon Lu	Kwame Harris	Juan Espinoza	Evan Long
Henry Dinh	Deshawn Sims	Jorge Vazquez	Adam Baker
Philip Hsu	Terrell James	Abel Cisneros	Harrison Nelson
Eric Liang	Akeem Thomas	Cesar Gonzalez	Brendan Hughes
David Yoon	Daquan Dixon	Alberto Hernandez	Cody Kelly
Jonathan Yu	Tarik Daniels	Elvin Contreras	Zachary Russell
Andrew Trinh	Jaquan Franklin	Ruben Ramirez	Mitchell Roberts
Stephen Yi	Tyrell Jones	Reynaldo Esparza	Tyler Phillips
Ryan Nguyen	Isiah Grant	Wilfredo Alvarado	Matthew Bennett
Aaron Jiang	Omari Holmes	Francisco Macias	Thomas Morgan
Kenneth Zhao	Rashad Frazier	Emilio Zuniga	Sean Ward
Johnny Hwang	Jermaine Green	Javier Ayala	Nicholas Rogers
Tony Choi	Donte Jenkins	Guillermo Mejia	Brett Martin
Benjamin Luong	Donnell Fields	Elvis Rojas	Cory Anderson
Raymond Tran	Davon Simmons	Miguel Ochoa	Colin Clark
Michael Duong	Darnell Carter	Sergio Carillo	Jack Campbell
Andy Hoang	Hakeem Woods	Alejandro Rios	Ross Adams
Alexander Pham	Sheldon Bryant	Ernesto Jimenez	Liam Morris
Robert Yang	Antoine Brown	Oscar Maldonado	Max Price
Danny Xu	Marquise Byrd	Felix Cardenas	Ethan Hall
Anthony Huynh	Tyrone Walker	Manuel Chavez	Eli Bailey
Jason Liu	Dashawn Lewis	Orlando Mendez	Patrick Collins
John Chen	Shamel Johnson	Luis Rosales	Luke Reed
Brian Vang	Reginald Douglas	Eduardo Castaneda	Alec Smith
Joseph Zhou	Shaquille Alexander	Carlos Rodriguez	Seth King
James Cho	Jamel Henderson	Cristian Acevedo	Austin Thompson
Nicholas Lin	Akil Richardson	Pedro Guzman	Nathan Stewart
Jeffrey Huang	Tyquan Brooks	Freddy Aguilar	Jacob Parker
Christopher Wu	Jamal Scott	Esteban Vasquez	Craig Gray
Timothy Ly	Jabari Hunter	Leonardo Munoz	Garrett Evans
William Oh	Tyshawn Hawkins	Arturo Ortiz	Ian Cooper
Patrick Ngo	Demetrius Reid	Jesus Rivas	Benjamin Allen
Thomas Cheng	Denzel Sanders	Ramon Sosa	Conor Wilson
Vincent Le	Tyreek Bell	Enrique Salinas	Jared Young
Kevin Hu	Darius Mitchell	Hector Gutierrez	Theodore Moore
Jimmy Xiong	Prince Ford	Armando Sandoval	Shane Wright
Justin Zhu	Lamar Watkins	Roberto Villarreal	Scott White
Calvin Luu	Raheem Butler	Edgar Garza	Noah Taylor
Edward Kwon	Jamar Davis	Pablo Arroyo	Ryan Hill
Peter Phan	Tariq Willis	Raul Soto	Jake Howard
Victor Patel	Shaquan Edwards	Diego Herrera	Maxwell Turner

Names of hypothetical male candidates. 50 names were selected to be highly indicative of each combination of race and gender. A name drawn from these lists was displayed at the top of each hypothetical resume, and in the questions used to evaluate the resumes. First and last names were linked every time they appeared. For details on the construction and randomization of names, see Section 2.3.4 and Appendix A.2.3.

Major Majors for the hypothetical resumes were selected according to a predefined probability distribution intended to balance the realism of the rating experience and our ability to detect and control for the effect of majors. Table A.3 shows each major along with its school affiliation and classification as Humanities & Social Sciences or STEM, as well as the probability assigned to each. We use this variation as the variable *Major* and control for it with fixed effects in most regressions.

Table A.3: Majors in Generated Penn Resumes

Type	School	Major	Probability
Humanities & Social Sciences	The Wharton School	BS in Economics	0.4
	College of Arts and Sciences	BA in Economics	0.2
		BA in Political Science	0.075
		BA in Psychology	0.075
		BA in Communication	0.05
		BA in English	0.05
		BA in History	0.05
		BA in History of Art	0.025
		BA in Philosophy	0.025
		BA in International Relations	0.025
		BA in Sociology	0.025
STEM	School of Engineering and Applied Science	BS in Computer Engineering	0.15
		BS in Biomedical Science	0.075
		BS in Mechanical Engineering and Applied Mechanics	0.075
		BS in Bioengineering	0.05
		BS in Chemical and Biomolecular Engineering	0.05
		BS in Cognitive Science	0.05
		BS in Computational Biology	0.05
		BS in Computer Science	0.05
		BS in Electrical Engineering	0.05
		BS in Materials Science and Engineering	0.05
		BS in Networked and Social Systems Engineering	0.025
		BS in Systems Science and Engineering	0.025
	College of Arts and Sciences	BA in Biochemistry	0.05
		BA in Biology	0.05
		BA in Chemistry	0.05
		BA in Cognitive Science	0.05
		BA in Mathematics	0.05
		BA in Physics	0.05

Majors, degrees, schools within Penn, and their selection probability by major type. Majors (and their associated degrees and schools) were drawn with replacement and randomized to resumes after subjects selected to view either Humanities & Social Sciences resumes or STEM resumes.

A.2.5 Components from Real Resumes

For work experiences, leadership experiences, and skills, we drew on components of resumes of real Penn students. This design choice improved the realism of the study by matching the tone and content of real Penn job seekers. Moreover, it improved the validity of our results by ensuring that our distribution of resume characteristics is close to the true distribution. This also helps us

identify the range of interest for the study, since resumes of unrealistically low (or high) quality are unlikely to produce useful variation for identification.

Source resumes came from campus databases (for example, student club resume books) and from seniors who submitted their resumes in order to participate in the matching process. When submitting resumes, students were informed that components of their resumes could be shown directly to employers. We scraped these resumes using a commercial resume parser (the Sovren Parser). From the scraped data we compiled one list with collections of skills, and a second list of experiences comprising an organization or employer, a position title, a location, and a job description (generally in the form of resume bullet points).

Resume components were selected to be interchangeable across resumes. To that end, we cleaned each work experience, leadership experience, and skills list in the following ways:

- Removed any information that might indicate gender, race, or religion (e.g., "Penn Women's Varsity Fencing Team" was changed to "Penn Varsity Fencing Team" and "Penn Muslim Students Association" was not used)
- Screened out components indicative of a specific major (e.g., "Exploratory Biochemistry Intern" was not used)
- Corrected grammatical errors

Work Experience We designed our resumes to vary both the quality and quantity of work experience. All resumes had a work experience during the summer before the candidate's senior year (June–August 2017). This work experience was either a regular internship (20/40) or a top internship (20/40). In addition, some resumes also had a second work experience (26/40), which varied in quality between a work-for-money job (13/40) or a regular internship (13/40). The job title, employer, description, and location shown on the hypothetical resumes were the same as in the source resume, with the minimal cleaning described above.

Before selecting the work experiences, we defined a *Top Internship* to be a substantive position at a prestigious employer. We chose this definition to both identify prestigious firms and distinguish between different types of jobs at those firms, such as a barista at a local Starbucks and a marketing intern at Starbucks headquarters. We identified a prestigious employer to be one of the 50 firms hiring the most Penn graduates in 2014 (as compiled by our Career Services partners). Since experiences at these firms were much more common among Humanities & Social Sciences majors, we supplemented this list with 39 additional firms hiring most often from Penn's School of Engineering and Applied Science. We extracted experiences at these firms from our full list of scraped experiences, and selected a total of 40 *Top Internship* experiences, with 20 coming from resumes of Humanities & Social Sciences majors and 20 from resumes of STEM majors. All of these *Top Internship* experiences had to be believably interchangeable within a major category. These internships included positions at Bain Capital, Goldman Sachs, Morgan Stanley, Northrop

Grumman, Boeing Company, and Google (see Table A.4 for a complete list). This variation identified the variable *Top Internship* in our analysis, which is measured relative to having a regular internship (since all resumes had some job in this position).

Table A.4: Top Internship Employers

Humanities & Social Sciences	**STEM**
Accenture plc	Accenture
Bain Capital Credit	Air Products and Chemicals, Inc
Bank of America Merrill Lynch	Bain & Company
Comcast Corporation	Boeing Company
Deloitte Corporate Finance	Credit Suisse Securities (USA) LLC
Ernst & Young U.S. LLP	Deloitte
Goldman Sachs	Epic Systems
IBM	Ernst & Young
McKinsey & Company	Federal Reserve Bank of New York
Morgan Stanley	Google
PricewaterhouseCoopers	J.P. Morgan
UBS Financial Services Inc.	McKinsey & Company
	Microsoft
	Morgan Stanley Wealth Management
	Northrop Grumman Aerospace Systems
	Palantir Technologies
	Pfizer Inc
	PricewaterhouseCoopers, LLP

Employers of top internships in Humanities & Social Sciences and STEM. A total of 20 *Top Internship* positions were used for each major type; some employers were used multiple times, when they appeared on multiple source resumes. Each firm name was used as provided on the source resume, and may not reflect the firm's official name. The names of some repeat *Top Internship* employers were provided differently on different source resumes (e.g., "Ernst & Young U.S. LLP" and "Ernst & Young"); in this case, we retained the name from the source resume associated with the internship.

We selected 33 regular internships separately for the two major groups: 20 regular internships for randomization in the first work experience position, and 13 for the second position. Regular internships had few restrictions, but could not include employment at the firms who provided top internships, and could not include work-for-money job titles (described below and shown in Table A.5). All jobs had to be believably interchangeable within major category. The regular internships in the second job position defined the variable *Second Internship*, and is measured relative to having no job in the second work experience position. Our dynamically generated resumes automatically adjusted in length when no second job was selected, in order to avoid a large gap on the page.

The remaining 13 jobs in the second work position (the summer after the sophomore year) were identified as *Work for Money*. We identified these positions in the real resume components by compiling a list of job titles and phrases that we thought would be indicative of typical in

this category, such as Cashier, Barista, and Waiter or Waitress (see Table A.5 Columns 2–4 for the full list). We extracted components in our full list of scraped experiences that matched these search terms, and selected 13 that could be plausibly interchangeable across any major. During randomization, these 13 jobs were used for both Humanities & Social Sciences and STEM majors. The first column of Table A.5 shows the job titles that appeared as *Work for Money* jobs in our hypothetical resumes. Columns 2–4 provide the list of job titles used for identifying work-for-money jobs in the scraped data, and for matching candidates to employer preferences.

Table A.5: Work for Money Job Titles & Identifying Phrases

Used for Resume Tool	**Used for Identifying Components & Matching**		
Assistant Shift Manager	Assistant coach	Courier	Phone Bank
Barista	Attendant	Custodian	Prep Cook
Cashier	Babysitter	Customer Service	Receptionist
Front Desk Staff	Backroom Employee	Dishwasher	Retail Associate
Host & Cashier	Bag Boy	Doorman	Rug Flipper
Sales Associate	Bagger	Driver	Sales Associate
Salesperson, Cashier	Bank Teller	Employee	Sales Representative
Server	Barback	Front Desk	Salesman
	Barista	Fundraiser	Salesperson
	Bartender	Gardener	Saleswoman
	Bellhop	Host	Server
	Bodyguard	Hostess	Shift Manager
	Bookseller	House Painter	Stock boy
	Bouncer	Instructor	Stockroom
	Bus boy	Janitor	Store Employee
	Busser	Laborer	Temp
	Caddie	Landscaper	Tour Guide
	Caddy	Librarian	Trainer
	Call center	Lifeguard	Tutor
	Canvasser	Line Cook	Valet
	Cashier	Maid	Vendor
	Caterer	Messenger	Waiter
	Cleaner	Mover	Waitress
	Clerk	Nanny	Work Study
	Counselor	Petsitter	Worker

Position titles and relevant phrases used to identify work for money in hypothetical resumes for evaluation and in candidate pool resumes. The first column contains the eight unique positions randomized into hypothetical resumes; position titles Cashier, Barista, Sales Associate, and Server were used more than once and associated with different firms. Columns 2–4 specify the work-for-money positions used to predict hiring interest of potential candidates from the pool of prospective matches. Any position title containing one of these phrases was identified as work for money for the purposes of matching.

Leadership Experience We defined leadership experiences to be those resume components that indicated membership or participation in a group, club, volunteer organization, fraternity/sorority, or student government. We selected leadership experiences from our full list of scraped experience components, requiring that the positions be clearly non-employment, include a position title, organization, and description, be plausibly interchangeable across gender, race, and major type. While many real resumes simply identified a position title and organization, we required that the components for our hypothetical resumes include a description of the activity for use as bullet points. We curated a list of 80 leadership experiences to use for both Humanities & Social Sciences and STEM resumes. Each resume included two randomly selected leadership experiences. We used the same leadership positions for both major types under the assumption that most extracurricular activities at Penn could plausibly include students from all majors; however, this required us to exclude the few leadership experiences that were too revealing of field of study (e.g., "American Institute of Chemical Engineers").

Every leadership position was assigned to the location of Penn's campus, Philadelphia, PA. This was done for consistency and believability, even if some of the leadership positions were held in other locations in the source resume. We randomly selected two ranges of years during a student's career to assign to the experiences, and we ordered the experiences chronologically on the hypothetical resume based on the end year of the experience.

Skills We selected 40 skill sets from STEM resumes and 40 from Humanities & Social Sciences resumes for randomization in the survey tool. We intended for these skill sets to accurately reflect the types of skills common in the resumes we collected, and to be plausibly interchangeable within a major type. For randomization, skill sets were drawn from within a major type. To induce variation for the variable *Technical Skills*, we randomly upgraded a skill set with probability 25% by adding two skills from the set of programming languages {Ruby, Python, PHP, Perl} and two skills from the set of statistical programming packages {SAS, R, Stata, Matlab} in random order. To execute this randomization, we removed any other references to these eight languages from the skill sets. Many display their skills in list format, with the word "and" coming before the final skill; we removed the "and" to make the addition of *Technical Skills* more natural.

A.3 Matching Appendix

A.3.1 Students

For job-seeking study participants, the career services office sent an email to seniors offering "an opportunity to reach more employers" by participating in our pilot study, to be run in parallel with all existing recruiting activities. The full student recruitment email is reproduced in Appendix A.2. After uploading a resume and answering basic questions on their industry and locations of interest, students were entered into the applicant pool, and we did not contact them again. If matched with

an employer, we emailed the student's resume to the employer and encouraged the employer to contact the student directly. Students received no other incentive for participating.

A.3.2 Matches with Job Seekers

To match job seeking students with the recruiters in our study, we parsed the student resumes and coded their content into variables describing the candidate's education, work experience, and leadership experience, using a combination of parsing software and manual transcription. We did not include any measure of ethnicity or gender in providing matches, nor did we take into account any employer's revealed ethnic or gender preferences. The full list of variables used for matching is shown in Table A.6.

We ran individual ridge regressions for each completed firm-position survey, merging the responses of multiple recruiters in a company if recruiting for the same position. We ran separate regressions using the hiring interest rating (the response to the question "How interested would you be in hiring **[Name]**?") and the likelihood of acceptance (the response to the question "How likely do you think **[Name]** would be to accept a job with your organization?") as outcome variables. We used cross-validation to select the punishment parameter of the ridge regression by running pooled regressions with a randomly selected hold-out sample, and identifying the punishment parameter that minimized prediction error in the hold-out sample. Repeating this process with 100 randomly selected hold-out samples separately for Humanities & Social Sciences and STEM employers, we use the average of the best-performing punishment parameters as the punishment parameter for the individual regressions. Based on the individual regression results, we then generated out-of-sample predictions of hiring interest and likelihood of acceptance for the resumes in our match pool that met minimal matching requirements for industry and geographic location. Finally, we generated a "callback index" as a weighted average of the predicted hiring interest and likelihood of acceptance (callback $= \frac{2}{3}$hiring interest $+ \frac{1}{3}$likelihood of acceptance). The 10 resumes with the highest callback indices for each employer were their matches.

We emailed each employer a zipped file of these matches (i.e., 10 resumes in PDF format). If multiple recruiters from one firm completed the tool for one hiring position, we combined their preferences and provided a single set of 10 resumes to the group.[37] This set of candidate resumes was the only incentive for participating in the study.

[37] In cases where multiple recruiters from a firm completed the tool in order to fill different positions, or where a single recruiter completed multiple times for different positions, we treated these as unique completions and provided them with 10 candidate resumes for each position.

Table A.6: Candidate Matching Variables

Variable	**Definition**
GPA	Overall GPA, if available. If missing, assign lowest GPA observed in the match pool
Engineering	Indicator for Computer Sciences, Engineering, or Math majors (for STEM candidates)
Humanities	Indicator for Humanities majors (for Humanities & Social Sciences Candidates)
Job Count	Linear variable for 1, 2, or 3+ work experiences.
Top Firm	Resume has a work experience at one of the firms hiring the most Penn graduates
Major City	Resume has a work experience in New York, San Francisco, Chicago, or Boston
Work for Money	Resume has a job title including identifying phrase from Table A.5
S&P500 or Fortune 500	Resume has an experience at an S&P 500 or Fortune 500 firm
Leader	Resume has a leadership position as Captain, President, Chair, Chairman, or Chairperson

Variables used to identify individual preferences and recommend matched candidates. Variables were identified in hypothetical resumes and in the candidate resume pool. Subjects were provided with 10 real job seekers from Penn whose qualifications matched their preferences based on predictions from a ridge regression with these features.

B Results Appendix

In this section, we describe additional results and robustness checks to validate our main results. In Section B.1, we show additional analysis related to our main human capital results. In Section B.2, we verify our results after reweighting observations to the true distribution of GPAs in actual Penn student resumes. In Section B.3, we discuss preferences over the quality distribution. In Section B.4, we provide additional results on candidate demographics. Finally, in Section B.5, we discuss the relationship between *Likelihood of Acceptance* and *Hiring Interest*.

B.1 Additional Results on Human Capital

The human capital results in Section 3.2 rely on the independent randomization of work experiences and other resume elements. This randomization leads to some combinations of resume elements that are unlikely to arise in practice, despite drawing each variable from a realistic univariate distribution. If employers value a set of experiences that form a cohesive narrative, independent randomization could lead to strange relationships in our data. If employers value combinations of work experiences, narrative might be an omitted variable that could introduce bias (e.g., if our *Top Internships* are more likely to generate narratives than regular internships, we may misestimate their effect on hiring interest). In Table B.1, we address this concern by showing that the cross-randomization of work experiences does not drive our results. To test this, we had three undergraduate research assistants at the University of Pennsylvania rate all possible combinations of work experiences that could have appeared on our hypothetical resumes.[38] We used their responses to create a dummy—denoted *Narrative*—that is equal to 1 when a resume has a work experience in the summer before junior year that is related to the work experience before senior year, and 0 otherwise. As a result of this process, we identified that 17.5% of the realized resumes in our study (i.e., those resumes actually shown to subjects) had a cohesive work experience narrative. None of these resumes included *Work for Money* because our RA raters did not see these jobs as contributing to a narrative. Appendix Table B.1 runs the same regressions as Table 2 but additionally controls for *Narrative*. All results from Table 2 remain similar in size and statistical significance.

In Table B.2, we estimate the value of degrees from more prestigious schools within Penn. We replace the major fixed effects of Table 2 with binary variables for *School of Engineering and Applied Science* and *Wharton*, as well as a binary control for whether the subject has chosen to

[38] As Penn students, these RAs were familiar with the type of work experiences Penn students typically have in the summers before their junior and senior years. Each RA rated 1040 combinations (40 work experiences in the summer before senior year $\times$ 26 work experiences in the summer before junior year) for Humanities & Social Sciences majors, and another 1040 combinations (40×26) for the STEM majors blind to our results. They rated each combination on the extent to which the two work experiences had a cohesive narrative on a scale of 1 to 3 where 1 indicated "These two jobs are not at all related," 2 indicated "These two jobs are somewhat related," and 3 indicated "These two jobs are very related." The majority of combinations received a rating of 1 so we introduce a binary variable *Narrative* equal to 1 if the jobs were rated as somewhat or very related, and 0 if the jobs were not at all related.

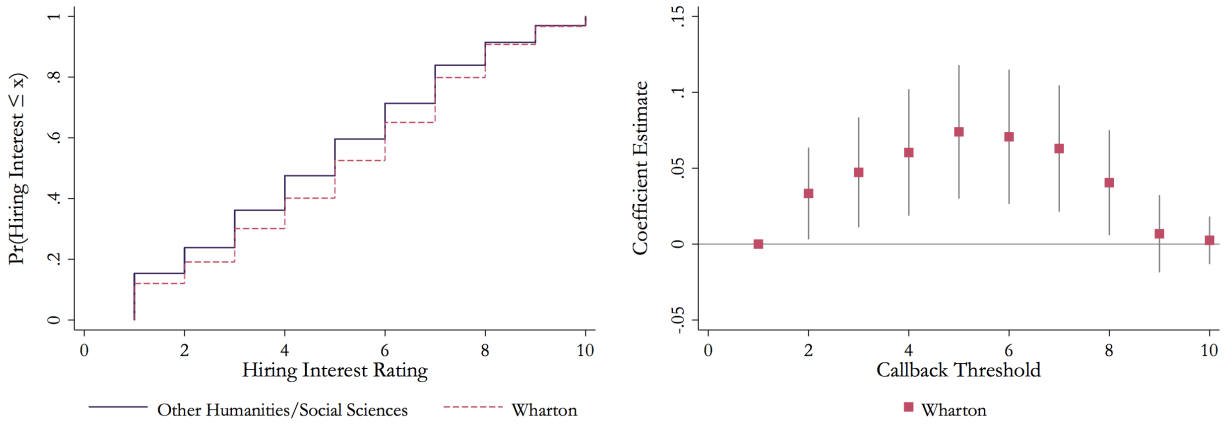
Table B.1: Work Experience Narrative

	Dependent Variable: Hiring Interest				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.128	2.194	2.200	1.000	0.892
	(0.145)	(0.150)	(0.129)	(.)	(0.061)
Top Internship	0.896	0.892	0.888	0.404	0.375
	(0.095)	(0.099)	(0.081)	(0.043)	(0.040)
Second Internship	0.349	0.364	0.319	0.145	0.156
	(0.142)	(0.150)	(0.122)	(0.056)	(0.059)
Work for Money	0.115	0.160	0.157	0.071	0.052
	(0.110)	(0.114)	(0.091)	(0.042)	(0.047)
Technical Skills	0.042	0.049	-0.076	-0.034	0.010
	(0.104)	(0.108)	(0.090)	(0.041)	(0.044)
Female, White	-0.149	-0.213	-0.159	-0.072	-0.060
	(0.114)	(0.118)	(0.096)	(0.044)	(0.048)
Male, Non-White	-0.174	-0.181	-0.175	-0.079	-0.076
	(0.137)	(0.142)	(0.115)	(0.052)	(0.057)
Female, Non-White	-0.011	-0.024	0.026	0.012	-0.015
	(0.137)	(0.144)	(0.120)	(0.055)	(0.058)
Narrative	0.214	0.237	0.278	0.126	0.093
	(0.165)	(0.175)	(0.144)	(0.066)	(0.068)
Observations	2880	2880	2880	2880	2880
R^2	0.130	0.181	0.484		
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No

Ordered probit cutpoints: 1.91, 2.28, 2.64, 2.94, 3.26, 3.6, 4.05, 4.52, and 5.03.

Table shows OLS and ordered probit regressions of hiring interest from Equation (1), with an additional control for *Narrative*. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. *Narrative* is a characteristic of resumes, defined as work experiences that are related in some way. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS presents the results of Column 3 divided by the Column 3 coefficient on GPA, with standard errors calculated by delta method. The p-value of a test of joint significance of major fixed effects is indicated (F-test for OLS regressions, likelihood ratio test for ordered probit regressions).

Figure B.1: Wharton



(a) Empirical CDF

(b) Linear Probability Model

Empirical CDF of *Hiring Interest* (Panel B.1a) and difference in counterfactual callback rates (Panel B.1b) for *Wharton* and *Other Humanities & Social Sciences*. Empirical CDFs show the share of hypothetical candidate resumes with each characteristic with a *Hiring Interest* rating less than or equal to each value. The counterfactual callback plot shows the difference between groups in the share of candidates at or above the threshold—that is, the share of candidates who would be called back in a resume audit study if the callback threshold were set to any given value. 95% confidence intervals are calculated from a linear probability model with an indicator for being at or above a threshold as the dependent variable.

review Humanities & Social Sciences or STEM resumes (coefficients not reported).[39] We find that employers find degrees from these schools 0.4–0.5 Likert-scale points more desirable than degrees from Penn's College of Arts and Sciences. As shown in Figure B.1, and as discussed in Section 3.3, we also investigate the effect of having a degree from Wharton across the distribution of hiring interest.

B.2 Re-weighting by GPA

In generating hypothetical resumes, we randomly selected candidate GPAs from $Unif[2.90, 4.00]$, rather than from the true distribution of GPAs among job seekers at Penn, which is shown in Figure B.2.[40] In this section, we demonstrate that this choice does not drive our results. In Tables B.3, B.4, and B.5, we rerun the regressions of Tables 2, 3, and 4 weighted to reflect the naturally occurring distribution of GPA among our Penn senior candidate pool (i.e., the job seekers used for matching, see Appendix A.3). We do not include missing GPAs in the reweighting, though our

[39] Major fixed effects are perfectly multicollinear with the variables for school, since no two schools grant the same degrees in the same major.

[40] We parameterized *GPA* to be drawn $Unif[2.90, 4.00]$ to give us statistical power to test the importance of GPA on hiring interest, but this distribution is not exactly the distribution of GPA among Penn seniors engaging in on campus recruiting.

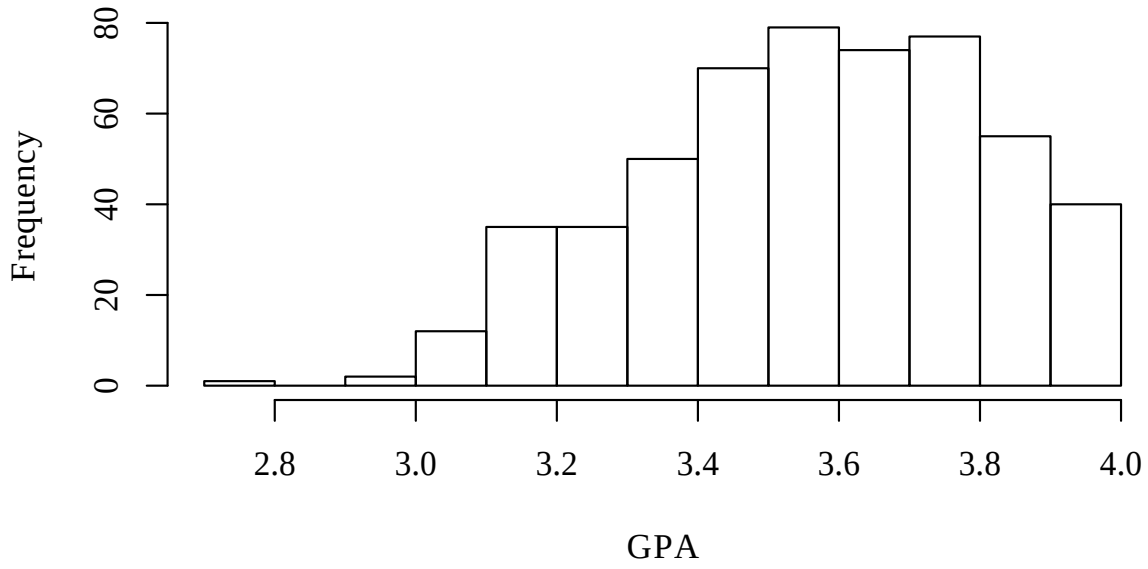
Table B.2: Prestigious Schools

	Dependent Variable: Hiring Interest				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.129	2.187	2.192	1.000	0.887
	(0.145)	(0.149)	(0.128)	(.)	(0.062)
Top Internship	0.908	0.913	0.905	0.413	0.378
	(0.094)	(0.098)	(0.080)	(0.043)	(0.039)
Second Internship	0.443	0.465	0.451	0.206	0.195
	(0.112)	(0.118)	(0.094)	(0.045)	(0.047)
Work for Money	0.108	0.141	0.143	0.065	0.049
	(0.110)	(0.113)	(0.092)	(0.042)	(0.046)
Technical Skills	0.038	0.040	-0.082	-0.037	0.009
	(0.103)	(0.107)	(0.090)	(0.041)	(0.043)
Female, White	-0.146	-0.207	-0.160	-0.073	-0.057
	(0.113)	(0.118)	(0.096)	(0.044)	(0.047)
Male, Non-White	-0.189	-0.196	-0.181	-0.083	-0.080
	(0.137)	(0.142)	(0.115)	(0.053)	(0.057)
Female, Non-White	-0.000	-0.011	0.037	0.017	-0.009
	(0.137)	(0.144)	(0.120)	(0.055)	(0.057)
School of Engineering	0.497	0.441	0.403	0.184	0.239
	(0.199)	(0.206)	(0.164)	(0.076)	(0.086)
Wharton	0.459	0.502	0.417	0.190	0.184
	(0.110)	(0.115)	(0.093)	(0.044)	(0.046)
Observations	2880	2880	2880	2880	2880
R^2	0.115	0.168	0.472		
Major FEs	No	No	No	Yes	No
Leadership FEs	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No

Ordered probit cutpoints: 2.48, 2.84, 3.20, 3.49, 3.81, 4.15, 4.60, 5.06, and 5.57.

Table shows OLS and ordered probit regressions of hiring interest from Equation (1), with effects for school, and a control for whether the employer selected to view Humanities & Social Sciences resumes or STEM resumes (coefficient not displayed). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. *School of Engineering* indicates a resume with a degree from Penn's School of Engineering and Applied Sciences; *Wharton* indicates a resume with a degree from the Wharton School. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. GPA-Scaled OLS presents the results of Column 3 divided by the Column 3 coefficient on GPA, with standard errors calculated by delta method. R^2 is indicated for each OLS regression.

Figure B.2: Distribution of GPA Among Scraped Resumes



Histogram representing the distribution of GPA among scraped resumes in our candidate matching pool. Distribution excludes any resumes for which GPA was not available (e.g., resume did not list GPA, resume listed only GPA within concentration, or parser failed to scrape). GPAs of participating Penn seniors may not represent the GPA distribution at Penn as a whole.

results are robust to re-weighting with missing GPAs treated as low GPAs.[41] These regressions confirm the results of Tables 2, 3, and 4 in direction and statistical significance.

Matching the underlying distribution of characteristics in hypothetical resumes to the distribution of real candidates is also an issue for resume auditors who must contend with a limited number of underlying resumes (i.e., resumes that they manipulate to create treatment variation). Given uncertainty about the characteristics of candidates and the limited number of underlying resumes, resume auditors may not be able to perfectly match the distribution of characteristics of a target population. An additional advantage of the IRR methodology is that it involves collecting a large number of resumes from an applicant pool of real job seekers, which gives us information on the distribution of candidate characteristics that we can use to re-weight the data ex post.

[41] Some students may strategically omit low GPAs from their resumes, and some resume formats were difficult for our resume parser to scrape.

Table B.3: Human Capital Experience—Weighted by GPA

	Dependent Variable: Hiring Interest				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.274	2.339	2.320	1.000	0.963
	(0.175)	(0.168)	(0.146)	(.)	(0.079)
Top Internship	0.831	0.832	0.862	0.372	0.353
	(0.110)	(0.109)	(0.088)	(0.043)	(0.047)
Second Internship	0.488	0.482	0.513	0.221	0.216
	(0.129)	(0.130)	(0.105)	(0.047)	(0.054)
Work for Money	0.178	0.193	0.199	0.086	0.075
	(0.129)	(0.125)	(0.100)	(0.044)	(0.056)
Technical Skills	0.077	0.039	-0.106	-0.046	0.022
	(0.118)	(0.119)	(0.102)	(0.044)	(0.051)
Female, White	-0.057	-0.099	-0.038	-0.016	-0.021
	(0.134)	(0.130)	(0.105)	(0.045)	(0.057)
Male, Non-White	-0.239	-0.181	-0.111	-0.048	-0.097
	(0.154)	(0.154)	(0.123)	(0.053)	(0.066)
Female, Non-White	-0.020	-0.032	0.040	0.017	-0.017
	(0.166)	(0.162)	(0.134)	(0.058)	(0.071)
Observations	2880	2880	2880	2880	2880
R^2	0.146	0.224	0.505		
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No

Ordered probit cutpoints: 2.30, 2.71, 3.04, 3.34, 3.66, 3.99, 4.49, 4.95, and 5.46.

Table shows OLS and ordered probit regressions of *Hiring Interest* from Equation (1), weighted by the distribution of GPA in resumes in the candidate matching pool. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS presents the results of Column 3 divided by the Column 3 coefficient on GPA, with standard errors calculated by delta method. The p-value of a test of joint significance of major fixed effects is indicated for each model (F-test for OLS regressions, χ^2 test for ordered probit regression).

Table B.4: Human Capital Experience by Major Type—Weighted by GPA

	Dependent Variable: Hiring Interest									
	Humanities & Social Sciences					STEM				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.365	2.452	2.476	1.000	1.008	2.028	2.187	2.000	1.000	0.848
	(0.212)	(0.198)	(0.172)	(.)	(0.096)	(0.306)	(0.325)	(0.266)	(.)	(0.133)
Top Internship	0.973	0.941	0.982	0.397	0.412	0.448	0.526	0.581	0.291	0.204
	(0.127)	(0.125)	(0.102)	(0.049)	(0.056)	(0.218)	(0.222)	(0.182)	(0.101)	(0.093)
Second Internship	0.476	0.384	0.494	0.199	0.217	0.529	0.496	0.383	0.192	0.223
	(0.153)	(0.155)	(0.125)	(0.052)	(0.065)	(0.235)	(0.252)	(0.199)	(0.103)	(0.102)
Work for Money	0.091	0.035	0.086	0.035	0.037	0.387	0.459	0.517	0.259	0.182
	(0.152)	(0.145)	(0.118)	(0.048)	(0.065)	(0.247)	(0.270)	(0.201)	(0.106)	(0.106)
Technical Skills	0.089	0.026	-0.146	-0.059	0.026	0.011	-0.059	-0.093	-0.046	0.005
	(0.142)	(0.142)	(0.120)	(0.048)	(0.061)	(0.217)	(0.240)	(0.193)	(0.096)	(0.093)
Female, White	0.110	0.036	0.110	0.044	0.048	-0.460	-0.637	-0.658	-0.329	-0.183
	(0.159)	(0.153)	(0.125)	(0.051)	(0.068)	(0.251)	(0.253)	(0.206)	(0.110)	(0.107)
Male, Non-White	-0.033	0.037	0.038	0.015	-0.006	-0.799	-0.704	-0.590	-0.295	-0.352
	(0.181)	(0.183)	(0.147)	(0.059)	(0.077)	(0.295)	(0.322)	(0.260)	(0.129)	(0.130)
Female, Non-White	0.036	0.024	0.078	0.032	0.001	-0.180	0.014	0.039	0.020	-0.074
	(0.189)	(0.186)	(0.154)	(0.062)	(0.082)	(0.332)	(0.318)	(0.264)	(0.132)	(0.140)
Observations	2040	2040	2040	2040	2040	840	840	840	840	840
R^2	0.141	0.242	0.522			0.150	0.408	0.644		
p-value for test of joint significance of Majors	0.105	0.152	0.022	0.022	0.138	< 0.001	0.003	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No	No	No	Yes	Yes	No

Ordered probit cutpoints (Column 5): 2.54, 2.89, 3.23, 3.54, 3.86, 4.20, 4.71, 5.18, 5.70.

Ordered probit cutpoints (Column 10): 1.78, 2.31, 2.62, 2.89, 3.20, 3.51, 3.98, 4.44, 4.92.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1). *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS columns present the results of Column 3 and Column 8 divided by the Column 3 and Column 8 coefficient on GPA, with standard errors calculated by delta method. The p-values of tests of joint significance of major fixed effects and demographic variables are indicated (F-test for OLS, likelihood ratio test for ordered probit) after a Bonferroni correction for analyzing two subgroups.

Table B.5: Likelihood of Acceptance—Weighted by GPA

	Dependent Variable: Likelihood of Acceptance			
	OLS	OLS	OLS	Ordered Probit
GPA	0.545	0.552	0.663	0.246
	(0.174)	(0.168)	(0.132)	(0.074)
Top Internship	0.725	0.709	0.694	0.299
	(0.111)	(0.108)	(0.083)	(0.047)
Second Internship	0.524	0.456	0.432	0.220
	(0.132)	(0.133)	(0.101)	(0.056)
Work for Money	0.205	0.150	0.185	0.087
	(0.128)	(0.125)	(0.098)	(0.054)
Technical Skills	0.041	-0.039	-0.114	0.012
	(0.120)	(0.120)	(0.097)	(0.050)
Female, White	-0.209	-0.276	-0.224	-0.083
	(0.135)	(0.133)	(0.103)	(0.057)
Male, Non-White	-0.248	-0.273	-0.114	-0.113
	(0.157)	(0.155)	(0.120)	(0.066)
Female, Non-White	-0.174	-0.224	-0.155	-0.086
	(0.160)	(0.156)	(0.124)	(0.068)
Observations	2880	2880	2880	2880
R^2	0.077	0.162	0.509	
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	No
Order FEs	No	Yes	Yes	No
Subject FEs	No	No	Yes	No

Ordered probit cutpoints: -0.09, 0.29, 0.64, 0.90, 1.26, 1.67, 2.13, 2.65, and 3.02.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1), weighted by the distribution of GPA in resumes in our candidate matching pool. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. The p-value of a test of joint significance of major fixed effects is indicated (F-test for OLS regressions, χ^2 test for ordered probit regression).

B.3 Distributional Appendix

As discussed in Section 3.3, average preferences for candidate characteristics might differ from the preferences observed in the tails. The stylized example in Figure B.3 shows this concern graphically. Imagine the light (green) distribution shows the expected productivity—based on the content of their resumes—of undergraduate research assistants (RAs) majoring in Economics at the University of Pennsylvania and the dark (gray) distribution shows the expected productivity of undergraduate RAs enrolled at the Wharton School. In this example, the mean Wharton student would make a less productive RA, reflecting a lack of interest in academic research relative to business on average; however, the tails of the Wharton distribution are fatter, reflecting the fact that admission into Wharton is more selective, so a Wharton student who has evidence of research interest on her resume is expected to be better than an Economics student with an otherwise identical resume. Looking across the panels in Figure B.3, we see that as callback thresholds shift from being high (panel (a), where professors are very selective, only calling back around 8% of resumes) to medium (panel (b), where professors are calling back around 16% of resumes) to low (panel (c), where professors are calling back around 28% of resumes), a researcher conducting a resume audit study might conclude that there is an advantage on the RA market of being at Wharton, no effect, or a disadvantage.[42]

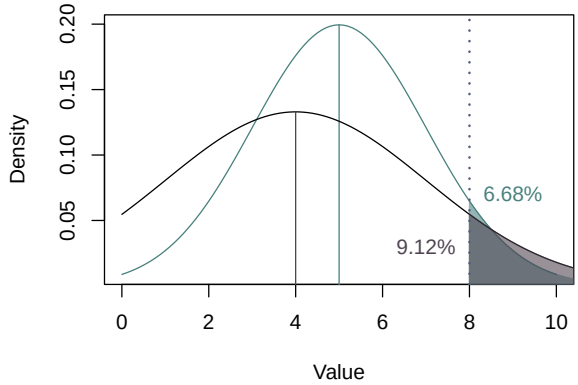
A researcher might particularly care about how employers respond to candidate characteristics around the empirically observed threshold (e.g., the researcher may be particularly interested in how employers respond to candidates in a particular market, with a particular level of selectivity, at a particular point in time). Nevertheless, there are a number of reasons why richer information about the underlying distribution of employer preferences for characteristics would be valuable for a researcher to uncover. A researcher might want to know how sensitive estimates are to: (1) an economic expansion or contraction that changes firms' hiring needs or (2) new technologies, such as video conferencing, which may change the callback threshold by changing the costs of interviewing. Similarly, a researcher may be interested in how candidate characteristics would affect callback in different markets (e.g., those known to be more or less selective) than the market where a resume audit was conducted. To conduct these counterfactual analyses, richer preference information would be valuable.

B.3.1 Comparing Results Across the Distribution

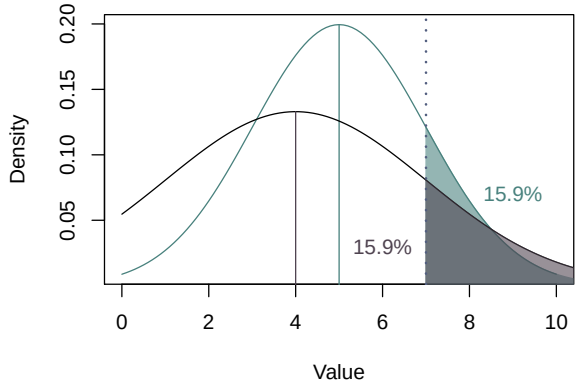
Resume audit studies often report differences in callback rates between two types of job candidates, either in a t-test or in a regression. However, as the overall callback rate becomes very large (i.e., almost all candidates get called back) or very small (i.e., few candidates get called back), the differences in callback rates tend toward zero. This is because, as discussed in footnote 22, the maximum possible difference in callback rates is capped by the overall callback rate.

[42] This stylized example uses two normal distributions. In settings where distributions are less well-behaved, the difference in callback rates might be even more sensitive to specific thresholds chosen.

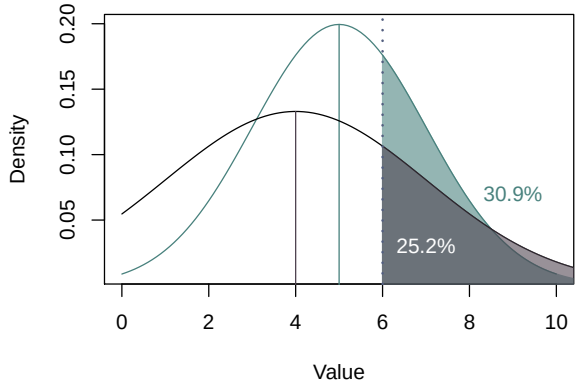
Figure B.3: Callback Thresholds Example



(a) High Threshold



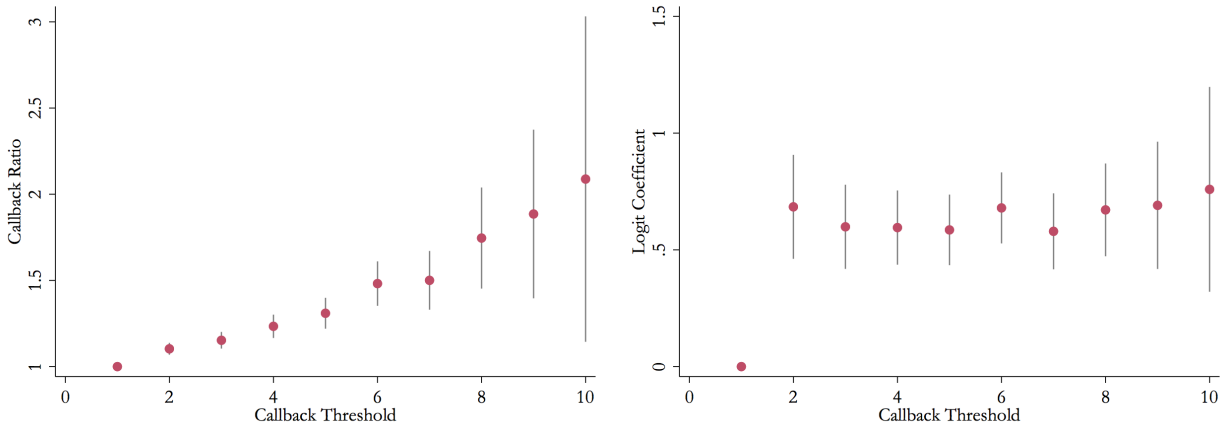
(b) Medium Threshold



(c) Low Threshold

A stylized example where average preferences differ from preferences at the upper tail. The distribution in green has a higher mean and lower variance, leading to thinner tails; the distribution in gray has a lower mean but higher variance, leading to more mass in the upper tail. As the callback threshold decreases from Panel (a) to Panel (c), the share of candidates above the threshold from each distribution changes. Estimating preferences from callbacks following this type of threshold process might lead to spurious conclusions.

Figure B.4: Alternative Specifications: Top Internship



(a) Callback Ratio

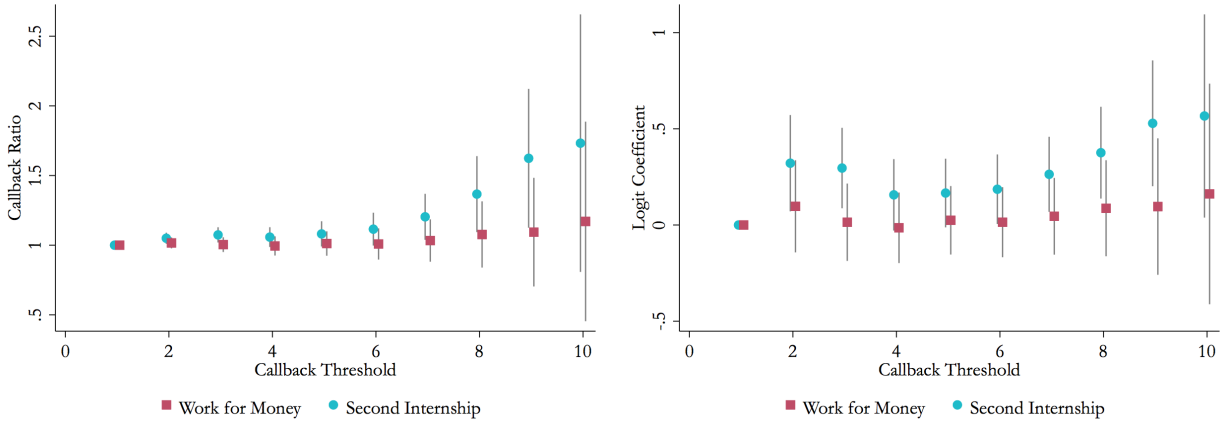
(b) Logit

Counterfactual callback ratios (Panel B.4a) and counterfactual logit coefficients (Panel B.4b) for *Top Internship*. Counterfactual callback is an indicator for each value of *Hiring Interest* equal to 1 if *Hiring Interest* is greater than or equal to the value, and 0 otherwise. Callback ratio is defined as the counterfactual callback rate for candidates with the characteristic divided by the counterfactual callback rate for candidates without. 95% confidence intervals are calculated from a linear probability model using the delta method. Logit coefficients are estimated from a logit regression with counterfactual callback as the dependent variable.

This is not a threat to the internal validity of most resume audit studies executed in a single hiring environment. However, this can cause problems when comparing across studies, or within a study run in different environments. For example, if one wanted to show that there was less racial discrimination in one city versus another, and the underlying callback rates in those cities differed, an interaction between city and race may be difficult to interpret. Note that such an exercise is performed in Kroft et al. [2013] to compare the response to unemployment in cities with high unemployment (and likely low overall callback rates) versus cities with low unemployment rates (and high callback rates). In that particular study, the "bias" caused by comparing across different callback rates does not undermine the finding that high unemployment rate cities respond less to unemployment spells. Nonetheless, researchers should use caution when implementing similar study designs.

In Figures B.4 and B.5, we look at how two different ways of measuring callback differences perform across the distribution compared to the linear probability model. The lefthand side of each figure shows the ratio of the callback rates, another common way of reporting resume audit study results. For the positive effects in our study, this odds ratio tends to be larger at the upper tail, where a small difference in callbacks can result in a large response in the ratio. On the righthand side of each figure, we show effects estimated from a logit specification. We find that in our data, the effects estimated in logistic regression tend to be flatter across the quality distribution.

Figure B.5: Alternative Specifications: Second Job Type



(a) Callback Ratio

(b) Logit

Counterfactual callback ratios (Panel B.5a) and counterfactual logit coefficients (Panel B.5b) for *Work for Money* and *Second Internship*. Counterfactual callback is an indicator for each value of *Hiring Interest* equal to 1 if *Hiring Interest* is greater than or equal to the value, and 0 otherwise. Callback ratio is defined as the counterfactual callback rate for candidates with the characteristic divided by the counterfactual callback rate for candidates without. 95% confidence intervals are calculated from a linear probability model using the delta method. Logit coefficients are estimated from a logit regression with counterfactual callback as the dependent variable.

B.4 Candidate Demographics Appendix

In this section, we provide additional analyses for our main results on candidate demographics. In B.4.1, we analyze our findings by the demographics of employers evaluating resumes. In B.4.2 we describe a test for implicit bias. In B.4.3, we discuss differential returns to quality by demographic group.

B.4.1 Rater Demographics

IRR allows us to collect information about the specific individuals rating resumes at the hiring firm. In Table B.6 we explore our main results by rater gender and race. White and female raters appear more likely to discriminate against male, non-white candidates than non-white or female raters.

B.4.2 Test for Implicit Bias

We leverage a feature of implicit bias—that it is more likely to arise when decision makers are fatigued [Wigboldus et al., 2004, Govorun and Payne, 2006, Sherman et al., 2004]—to test whether our data are consistent with implicit bias. Appendix Table B.7 investigates how employers respond to resumes in the first and second half of the study and to resumes before and after the period

Table B.6: Hiring Interest by Rater Demographics

	Dependent Variable: Hire Rating				
		Rater Gender		Rater Race	
	All	Female Raters	Male Raters	Non-White Raters	White Raters
GPA	2.196	2.357	2.092	2.187	2.131
	(0.129)	(0.170)	(0.212)	(0.378)	(0.146)
Top Internship	0.897	0.726	1.139	1.404	0.766
	(0.081)	(0.105)	(0.140)	(0.234)	(0.091)
Second Internship	0.466	0.621	0.195	0.636	0.459
	(0.095)	(0.126)	(0.154)	(0.273)	(0.107)
Work for Money	0.154	0.303	-0.082	-0.124	0.192
	(0.091)	(0.120)	(0.156)	(0.255)	(0.104)
Technical Skills	-0.071	-0.079	-0.020	-0.123	-0.016
	(0.090)	(0.122)	(0.151)	(0.231)	(0.104)
Female, White	-0.161	-0.202	-0.216	0.004	-0.209
	(0.096)	(0.128)	(0.165)	(0.265)	(0.109)
Male, Non-White	-0.169	-0.311	-0.105	0.119	-0.241
	(0.115)	(0.149)	(0.200)	(0.285)	(0.132)
Female, Non-White	0.028	0.001	-0.065	-0.124	0.097
	(0.120)	(0.159)	(0.202)	(0.325)	(0.137)
Observations	2880	1720	1160	600	2280
R^2	0.483	0.525	0.556	0.588	0.503
Major FEs	Yes	Yes	Yes	Yes	Yes
Leadership FEs	Yes	Yes	Yes	Yes	Yes
Order FEs	Yes	Yes	Yes	Yes	Yes
Subject FEs	Yes	Yes	Yes	Yes	Yes

OLS regressions of *Hiring Interest* on candidate characteristics by rater gender and race. Sample includes 29 male and 42 female subjects; 57 White and 15 non-White subjects. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. R^2 is indicated for each OLS regression. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated.

breaks—after every 10 resumes—that we built into the survey tool.[43] The first and second columns show that subjects spend less time evaluating each resume in the second half of the study and in the latter half of each block of 10 resumes, suggesting evidence of fatigue. The third column reports a statistically significant interaction on *Latter Half of Block* $\times$ *Not a White Male* of -0.385 Likert-scale points, equivalent to about 0.18 GPA points, suggesting more discrimination against candidates who are not white males in the latter half of each block of 10 resumes. The fourth column reports, however, that the bias in the second half of the study is not statistically significantly larger than the bias in the first half. These results provide suggestive, though not conclusive, evidence that the discrimination we detect may indeed be driven by implicit bias.

[43] As described in Section 2, after every 10 resumes an employer completed, the employer was shown a simple webpage with an affirmation that gave them a short break (e.g., after the first 10 resumes it read: "You have rated 10 of 40 resumes. Keep up the good work!"). Research suggests that such "micro breaks" can have relatively large effects on focus and attention [Rzeszotarski et al., 2013], and so we compare bias in the early half and latter half of each block of 10 resumes under the assumption that employers might be more fatigued in the latter half of each block of 10 resumes.

Table B.7: Implicit Bias

	Dependent Variable: Response Time		Dependent Variable: Hiring Interest	
Latter Half of Block	-3.518		0.360	
	(0.613)		(0.137)	
Second Half of Study		-4.668		-0.142
		(0.598)		(0.138)
Not a White Male	-0.642	-0.648	0.069	-0.107
	(0.666)	(0.665)	(0.115)	(0.118)
Latter Half of Block × Not a White Male			-0.385	
			(0.165)	
Second Half of Study × Not a White Male				-0.022
				(0.166)
GPA	2.791	2.944	2.187	2.187
	(0.961)	(0.949)	(0.128)	(0.128)
Top Internship	-0.799	-0.638	0.905	0.904
	(0.622)	(0.620)	(0.080)	(0.080)
Second Internship	2.163	2.118	0.471	0.458
	(0.752)	(0.750)	(0.093)	(0.093)
Work for Money	1.850	1.813	0.154	0.140
	(0.741)	(0.740)	(0.091)	(0.091)
Technical Skills	0.881	0.892	-0.067	-0.078
	(0.715)	(0.713)	(0.089)	(0.089)
Observations	2880	2880	2880	2880
R^2	0.405	0.412	0.475	0.475
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	Yes	Yes	Yes	Yes
Order FEs	No	No	No	No
Subject FEs	Yes	Yes	Yes	Yes

Regressions of *Response Time* and *Hiring Interest* on resume characteristics and resume order variables. The first and second columns show *Response Time* regressions; the third and fourth columns show *Hiring Interest* regressions. *Response Time* is defined as the number of seconds before page submission, Winsorized at the 95th percentile (77.9 seconds). Mean of *Response Time*: 23.6 seconds. *GPA*, *Top Internship*, *Second Internship*, *Work for Money*, *Technical Skills*, and *Not a White Male* are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. *Latter Half of Block* is an indicator variable for resumes shown among the last five resumes within a 10-resume block. *Second Half of Study* is an indicator variable for resumes shown among the last 20 resumes viewed by a subject. Fixed effects for subjects, majors, and leadership experience included in all specifications. R^2 is indicated for each OLS regression. The p-value of an $F-$test of joint significance of major fixed effects is indicated for all models.

B.4.3 Interaction of Demographics with Quality

Table B.8 shows that white males gain more from having a *Top Internship* than candidates who are not white males. The largest of these coefficients, that for white females, nearly halves the benefit of having a prestigious internship. We speculate that this may be due to firms believing that prestigious internships are a less valuable signal of quality if the previous employer may have selected the candidate due to positive tastes for diversity. Figure B.6 looks at the relationship between *Top Internship* and being *Not a White Male* throughout the quality distribution. We find that when a candidate is of sufficiently high quality, a *Top Internship* is equally valuable for white male candidates and those who are not white males. This may suggest that other signals of quality may inoculate candidates from the assumption that an impressive work history is the result of diversity initiatives.

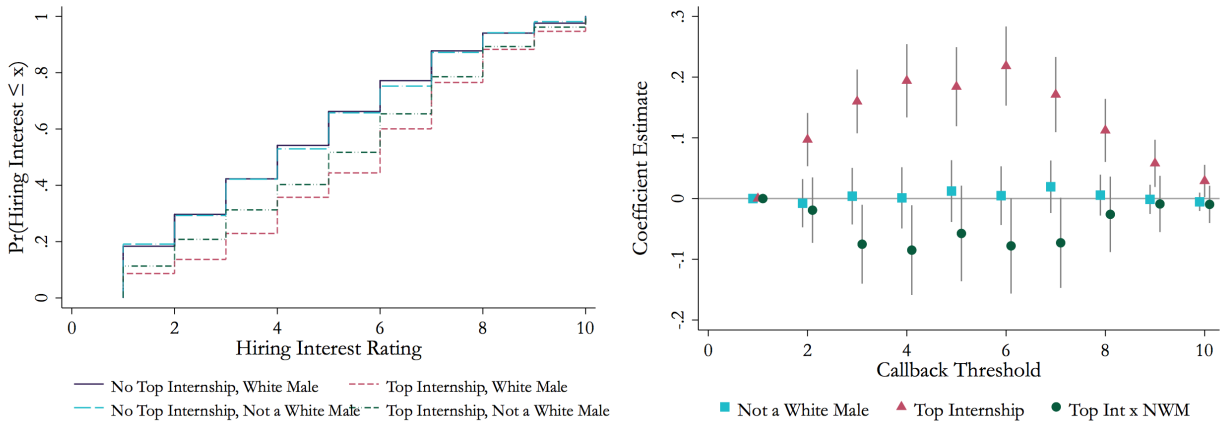
Table B.8: Return to Top Internship by Demographic Group

	Dependent Variable: Hiring Interest				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	2.119	2.184	2.191	1.000	0.889
	(0.145)	(0.150)	(0.129)	(.)	(0.061)
Top Internship	1.147	1.160	1.155	0.527	0.471
	(0.168)	(0.175)	(0.145)	(0.074)	(0.070)
Second Internship	0.468	0.495	0.470	0.214	0.208
	(0.112)	(0.118)	(0.094)	(0.045)	(0.047)
Work for Money	0.109	0.151	0.148	0.067	0.050
	(0.110)	(0.113)	(0.091)	(0.042)	(0.047)
Technical Skills	0.049	0.058	-0.067	-0.031	0.013
	(0.104)	(0.108)	(0.090)	(0.041)	(0.044)
Female, White	0.033	-0.019	0.022	0.010	0.012
	(0.146)	(0.152)	(0.121)	(0.055)	(0.062)
Male, Non-White	-0.060	-0.049	-0.055	-0.025	-0.029
	(0.175)	(0.184)	(0.145)	(0.066)	(0.074)
Female, Non-White	0.081	0.068	0.159	0.073	0.010
	(0.182)	(0.191)	(0.156)	(0.072)	(0.077)
Top Internship × Female, White	-0.464	-0.492	-0.459	-0.209	-0.181
	(0.234)	(0.243)	(0.199)	(0.092)	(0.097)
Top Internship × Male, Non-White	-0.280	-0.316	-0.276	-0.126	-0.116
	(0.279)	(0.288)	(0.233)	(0.107)	(0.116)
Top Internship × Female, Non-White	-0.229	-0.224	-0.316	-0.144	-0.065
	(0.273)	(0.286)	(0.240)	(0.110)	(0.116)
Observations	2880	2880	2880	2880	2880
R^2	0.130	0.182	0.484		
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No

Ordered probit cutpoints: 1.94, 2.31, 2.68, 2.97, 3.29, 3.63, 4.09, 4.55, and 5.06.

Table shows OLS and ordered probit regressions of hiring interest from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. GPA-Scaled OLS presents the results of Column 3 divided by the Column 3 coefficient on GPA, with standard errors calculated by delta method. The p-value of a test of joint significance of major fixed effects is indicated (F-test for OLS, likelihood ratio test for ordered probit).

Figure B.6: Top Internship × Not a White Male



(a) Empirical CDF

(b) Linear Probability Model

Empirical CDF of *Hiring Interest* (Panel B.6a) and difference in counterfactual callback rates (Panel B.6b) for *Top Internship*, *Not a White Male*, and *Top Internship* × *Not a White Male*. Empirical CDFs show the share of hypothetical candidate resumes with each characteristic with a *Hiring Interest* rating less than or equal to each value. The counterfactual callback plot shows the difference between groups in the share of candidates at or above the threshold—that is, the share of candidates who would be called back in a resume audit study if the callback threshold were set to any given value. 95% confidence intervals are calculated from a linear probability model with an indicator for being at or above a threshold as the dependent variable.

B.5 Relationship Between Likelihood of Acceptance and Human Capital

In evaluating candidates' likelihood of accepting a job offer, the firms in our sample exhibit a potentially surprising belief that candidates with more human capital—indicated by higher GPA, more work experience, and a more prestigious internship—are more likely to accept jobs than candidates with less human capital. This correlation could arise in several ways. First, it is possible that the hiring interest question—which always comes first—creates anchoring for the second question that is unrelated to true beliefs. Second, it is possible that likelihood of acceptance is based on both horizontal fit and vertical quality. Horizontal fit raises both hiring interest and likelihood of acceptance, which would lead to a positive correlation between responses; vertical quality, on the other hand, would be expected to increase hiring interest and decrease likelihood of acceptance, since as it increases hiring interest it also makes workers more desirable for other firms.[44]

If the correlation between *Hiring Interest* and *Likelihood of Acceptance* is driven mostly by horizontal fit, it is important to test whether *Likelihood of Acceptance* is simply a noisy measure of *Hiring Interest*, or whether *Likelihood of Acceptance* contains additional, valuable information. This

[44] It is also possible that respondents deliberately overstate candidates' likelihood of acceptance in order to be sent the best quality candidates. However, firms who are willing to do this likely have a low cost of interviewing candidates with a lower probability of acceptance. This is in line with the data, where the firms who consistently rate people a 10 on *Likelihood of Acceptance* are among the most prestigious firms in our sample.

will help us confirm, for example, that the gender bias we find in *Likelihood of Acceptance* is indeed its own result, rather than a result of bias in *Hiring Interest*. Approaching this is econometrically tricky, since *Hiring Interest* and *Likelihood of Acceptance* are both simultaneous products of the rater's assessment of the randomized resume components. We considered multiple approaches, such as subtracting hiring interest from likelihood of acceptance to capture the difference, regressing likelihood of acceptance on hiring interest and taking residuals, and including controls for hiring interest. All yield similar results, and so we use the latter approach, as it is the most transparent. Despite its econometric issues, we believe this is nonetheless a helpful exercise that can be thought of as akin to a mediation analysis. We want to see if all of the effect on *Likelihood of Acceptance* is mediated through *Hiring Interest*, or if there is independent variation in *Likelihood of Acceptance*.

The first two columns of Table B.9 include a linear control for *Hiring Interest*, while Columns 3 and 4 include fixed effect controls for each level of the *Hiring Interest* rating, examining *Likelihood of Acceptance* within each hiring interest band. We find that after controlling for *Hiring interest*, the relationship between GPA and *Likelihood of Acceptance* becomes negative and statistically significant under all specifications. This indicates that the part of *Likelihood of Acceptance* that is uncorrelated with *Hiring Interest* is indeed negatively correlated with one measure of vertical quality. We also find that the coefficients on *Top Internship* and *Second Internship* become statistically indistinguishable from zero.

Under all specifications, the coefficients on *Female, White* and *Female, Non-White* remain negative and significant, indicating that employers believe women are less likely to accept jobs if offered, even controlling for the firm's interest in the candidate.

Thus, we conclude that *Likelihood of Acceptance* does provide some additional information above and beyond *Hiring Interest*. We hope future research will tackle the question of how to measure beliefs about *Likelihood of Acceptance* accurately, how to disentangle them from *Hiring Interest*, and exactly what role they play in hiring decisions.

Table B.9: Likelihood of Acceptance with Hiring Interest Controls

	Dependent Variable: Likelihood of Acceptance			
	OLS	Ordered Probit	OLS	Ordered Probit
GPA	-0.812	-0.638	-0.823	-0.660
	(0.082)	(0.064)	(0.081)	(0.065)
Top Internship	0.033	0.000	0.031	0.001
	(0.053)	(0.041)	(0.053)	(0.041)
Second Internship	0.066	0.051	0.068	0.049
	(0.063)	(0.048)	(0.063)	(0.048)
Work for Money	0.095	0.082	0.095	0.087
	(0.061)	(0.047)	(0.061)	(0.048)
Technical Skills	-0.053	-0.057	-0.061	-0.066
	(0.060)	(0.045)	(0.059)	(0.045)
Female, White	-0.145	-0.078	-0.147	-0.082
	(0.064)	(0.048)	(0.064)	(0.049)
Male, Non-White	0.002	-0.016	0.001	-0.008
	(0.074)	(0.058)	(0.074)	(0.058)
Female, Non-White	-0.182	-0.154	-0.185	-0.159
	(0.074)	(0.059)	(0.074)	(0.059)
Hiring Interest	0.704	0.478	FEs	FEs
	(0.014)	(0.010)		
Observations	2880	2880	2880	2880
R^2	0.766		0.768	
p-value for test of joint significance of Majors	0.025	< 0.001	0.031	< 0.001
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	Yes	No	Yes	No
Order FEs	Yes	No	Yes	No
Subject FEs	Yes	No	Yes	No

Cutpoints (Col 2): -1.82, -1.18, -0.55, -0.11, 0.49, 1.07, 1.71, 2.39, 2.81.
Cutpoints (Col 4): -2.00, -1.26, -0.58, -0.14, 0.45, 1.01, 1.62, 2.28, 2.69.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1), with additional controls for *Hiring Interest*. Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects and demographic variables are indicated (F-test for OLS, likelihood ratio test for ordered probit).

C Pitt Appendix

In our replication study at the University of Pittsburgh, we followed a similar approach to that described for our experimental waves at Penn in Section A.2. The tool structure was essentially the same as at Penn, with references to Penn replaced with Pitt in the instructions, and the reference to Wharton removed from the major selection page. Resume structure was identical to that described in Sections A.2.1 and A.2.2. Names were randomized in the same manner as described in Section A.2.3. The education section of each resume at Pitt followed the same structure as that described in Section A.2.4, but had a degree from the University of Pittsburgh, with majors, schools, and degrees randomly drawn from a set of Pitt's offerings. In selecting majors for our Pitt replication, we attempted to match the Penn major distribution as closely as possible, but some majors were not offered at both schools. When necessary, we selected a similar major instead. The majors, schools, classifications, and probabilities for Pitt are shown in Table C.1.

We used a single pool of Pitt resumes for both the hypothetical resume elements and for a candidate pool for Pitt employers, saving significant effort on scraping and parsing. These components were compiled and randomized in much the same way as at Penn, as described in Section A.2.5. For *Top Internship* at Pitt, we collected work experiences from Pitt resumes at one of Pitt's most frequent employers, or at one of the employers used to define *Top Internship* at Penn. Similarly, Pitt *Work for Money* was identified from the same list of identifying phrases shown in Table A.5. *Technical Skills* were randomized in the same way as at Penn, described in A.2.5.

Table C.1: Majors in Generated Pitt Resumes

Type	**School**	**Major**	**Probability**
Humanities & Social Sciences	Dietrich School of Arts and Sciences	BS in Economics	0.4
		BA in Economics	0.2
		BS in Political Science	0.075
		BS in Psychology	0.075
		BA in Communication Science	0.05
		BA in English Literature	0.05
		BA in History	0.05
		BA in History of Art and Architecture	0.025
		BA in Philosophy	0.025
		BA in Social Sciences	0.025
		BA in Sociology	0.025
STEM	Dietrich School of Arts and Sciences	BS in Natural Sciences	0.1
		BS in Molecular Biology	0.075
		BS in Bioinformatics	0.05
		BS in Biological Sciences	0.05
		BS in Chemistry	0.05
		BS in Mathematical Biology	0.05
		BS in Mathematics	0.05
		BS in Physics	0.05
		BS in Statistics	0.025
	Swanson School of Engineering	BS in Computer Engineering	0.15
		BS in Mechanical Engineering	0.075
		BS in Bioengineering	0.05
		BS in Chemical Engineering	0.05
		BS in Computer Science	0.05
		BS in Electrical Engineering	0.05
		BS in Materials Science and Engineering	0.05
		BS in Civil Engineering	0.025

Majors, degrees, schools within Pitt, and their selection probability by major type. Majors (and their associated degrees and schools) were drawn with replacement and randomized to resumes after subjects selected to view either Humanities & Social Sciences resumes or STEM resumes.

Table C.2: Effects by Major Type at Pitt

	Dependent Variable: Hiring Interest									
	Humanities & Social Sciences					STEM				
	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit	OLS	OLS	OLS	GPA-Scaled OLS	Ordered Probit
GPA	0.249	0.294	0.249	1.000	0.097	0.518	0.445	0.340	1.000	0.167
	(0.189)	(0.203)	(0.150)	(.)	(0.073)	(0.245)	(0.274)	(0.187)	(.)	(0.092)
Top Internship	0.267	0.290	0.298	1.196	0.098	0.164	0.193	0.174	0.513	0.058
	(0.139)	(0.150)	(0.108)	(0.834)	(0.053)	(0.156)	(0.174)	(0.110)	(0.419)	(0.060)
Second Internship	0.438	0.496	0.446	1.791	0.169	-0.022	-0.076	-0.082	-0.243	-0.002
	(0.146)	(0.154)	(0.112)	(1.163)	(0.057)	(0.184)	(0.204)	(0.133)	(0.414)	(0.072)
Work for Money	0.323	0.354	0.355	1.425	0.121	-0.063	-0.039	-0.037	-0.109	-0.001
	(0.145)	(0.155)	(0.109)	(0.958)	(0.057)	(0.186)	(0.207)	(0.129)	(0.386)	(0.072)
Technical Skills	-0.014	-0.036	0.037	0.149	-0.004	0.376	0.459	0.283	0.834	0.153
	(0.131)	(0.143)	(0.103)	(0.418)	(0.051)	(0.179)	(0.199)	(0.129)	(0.611)	(0.067)
Female, White	-0.080	-0.177	-0.043	-0.174	-0.021	-0.043	0.033	0.049	0.145	-0.013
	(0.149)	(0.160)	(0.113)	(0.467)	(0.058)	(0.184)	(0.203)	(0.133)	(0.395)	(0.072)
Male, Non-White	0.089	0.037	-0.155	-0.621	0.044	-0.045	0.028	0.083	0.246	-0.041
	(0.175)	(0.189)	(0.130)	(0.634)	(0.068)	(0.232)	(0.259)	(0.160)	(0.481)	(0.089)
Female, Non-White	-0.196	-0.331	-0.073	-0.294	-0.072	-0.160	-0.055	0.091	0.267	-0.036
	(0.180)	(0.193)	(0.140)	(0.592)	(0.069)	(0.225)	(0.258)	(0.160)	(0.482)	(0.089)
Observations	2000	2000	2000	2000	2000	1440	1440	1440	1440	1440
R^2	0.015	0.078	0.553			0.031	0.109	0.651		
p-value for test of joint significance of Majors	0.713	0.787	0.185	0.185	0.821	0.015	0.023	< 0.001	< 0.001	0.014
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Order FEs	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
Subject FEs	No	No	Yes	Yes	No	No	No	Yes	Yes	No

Ordered probit cutpoints (Column 5): -0.38, -0.13, 0.19, 0.42, 0.68, 0.98, 1.40, 1.88, 2.45.

Ordered probit cutpoints (Column 10): 0.40, 0.61, 0.85, 1.02, 1.16, 1.31, 1.58, 1.95, 2.22.

Table shows OLS and ordered probit regressions of *Hiring Interest* from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects and demographic variables are indicated (F-test for OLS, likelihood ratio test for ordered probit) after a Bonferroni correction for analyzing two subgroups.

Table C.3: Likelihood of Acceptance at Pitt

	Dependent Variable: Likelihood of Acceptance			
	OLS	OLS	OLS	Ordered Probit
GPA	0.178	0.161	0.010	0.071
	(0.148)	(0.155)	(0.101)	(0.057)
Top Internship	0.233	0.245	0.235	0.087
	(0.103)	(0.108)	(0.068)	(0.040)
Second Internship	0.224	0.221	0.199	0.074
	(0.114)	(0.119)	(0.077)	(0.045)
Work for Money	0.142	0.143	0.130	0.050
	(0.114)	(0.120)	(0.074)	(0.044)
Technical Skills	0.195	0.187	0.111	0.084
	(0.106)	(0.110)	(0.070)	(0.040)
Female, White	-0.063	-0.079	0.015	-0.027
	(0.115)	(0.122)	(0.077)	(0.045)
Male, Non-White	-0.000	-0.012	-0.064	-0.011
	(0.139)	(0.145)	(0.091)	(0.054)
Female, Non-White	-0.198	-0.197	-0.048	-0.070
	(0.140)	(0.147)	(0.090)	(0.055)
Observations	3440	3440	3440	3440
R^2	0.037	0.061	0.643	
p-value for test of joint significance of Majors	< 0.001	< 0.001	< 0.001	< 0.001
Major FEs	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	No
Order FEs	No	Yes	Yes	No
Subject FEs	No	No	Yes	No

Ordered probit cutpoints: -0.10, 0.14, 0.38, 0.58, 0.86, 1.08, 1.42, 1.86, and 2.35.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1). Robust standard errors are reported in parentheses. *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included in some specifications as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects and demographic variables are indicated (F-test for OLS, likelihood ratio test for ordered probit).

Table C.4: Likelihood of Acceptance by Major Type at Pitt

	Dependent Variable: Likelihood of Acceptance							
	Humanities & Social Sciences				STEM			
	OLS	OLS	OLS	Ordered Probit	OLS	OLS	OLS	Ordered Probit
GPA	-0.064	-0.044	-0.173	-0.007	0.499	0.427	0.278	0.155
	(0.187)	(0.202)	(0.127)	(0.074)	(0.241)	(0.268)	(0.181)	(0.091)
Top Internship	0.261	0.248	0.263	0.097	0.210	0.227	0.214	0.078
	(0.137)	(0.149)	(0.091)	(0.053)	(0.155)	(0.173)	(0.112)	(0.060)
Second Internship	0.353	0.435	0.373	0.124	0.043	-0.026	-0.020	0.020
	(0.146)	(0.156)	(0.095)	(0.057)	(0.183)	(0.201)	(0.131)	(0.071)
Work for Money	0.271	0.294	0.303	0.100	-0.051	-0.045	-0.034	-0.009
	(0.144)	(0.155)	(0.095)	(0.057)	(0.184)	(0.205)	(0.126)	(0.071)
Technical Skills	-0.013	0.004	-0.008	-0.005	0.521	0.638	0.382	0.214
	(0.130)	(0.140)	(0.086)	(0.051)	(0.178)	(0.195)	(0.128)	(0.066)
Female, White	-0.064	-0.149	-0.001	-0.035	-0.081	-0.007	-0.025	-0.014
	(0.148)	(0.159)	(0.097)	(0.058)	(0.183)	(0.204)	(0.136)	(0.071)
Male, Non-White	0.110	0.060	-0.132	0.033	-0.152	-0.080	0.022	-0.072
	(0.173)	(0.185)	(0.112)	(0.068)	(0.232)	(0.259)	(0.162)	(0.089)
Female, Non-White	-0.138	-0.258	-0.095	-0.062	-0.286	-0.218	-0.031	-0.068
	(0.180)	(0.194)	(0.118)	(0.069)	(0.224)	(0.258)	(0.158)	(0.088)
Observations	2000	2000	2000	2000	1440	1440	1440	1440
R^2	0.010	0.069	0.666		0.036	0.110	0.654	
p-value for test of joint significance of Majors	1.436	1.550	1.061	1.701	0.006	0.016	< 0.001	0.008
Major FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Leadership FEs	No	Yes	Yes	No	No	Yes	Yes	No
Order FEs	No	Yes	Yes	No	No	Yes	Yes	No
Subject FEs	No	No	Yes	No	No	No	Yes	No

Ordered probit cutpoints (Column 4): -0.59, -0.34, -0.11, 0.14, 0.47, 0.76, 1.12, 1.59, 2.37.

Ordered probit cutpoints (Column 8): 0.31, 0.56, 0.78, 0.93, 1.12, 1.25, 1.56, 1.96, 2.26.

Table shows OLS and ordered probit regressions of *Likelihood of Acceptance* from Equation (1). *GPA*; *Top Internship*; *Second Internship*; *Work for Money*; *Technical Skills*; *Female, White*; *Male, Non-White*; *Female, Non-White* and major are characteristics of the hypothetical resume, constructed as described in Section 2.3 and in Appendix A.2. Fixed effects for major, leadership experience, resume order, and subject included as indicated. R^2 is indicated for each OLS regression. The p-values of tests of joint significance of major fixed effects and demographic variables are indicated (F-test for OLS, likelihood ratio test for ordered probit) after a Bonferroni correction for analyzing two subgroups.