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Do Employers Prefer Workers Who Attend For-Profit Colleges? Evidence from a Field Experiment

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This paper reports results from a resume-based field experiment designed to examine employer preferences for job applicants who attend for-profit colleges. We sent over 8,000 fictitious resumes of young job applicants who recently completed their schooling to online job postings in six occupational categories and seven major cities in the United States. Resumes were randomly assigned to list either no postsecondary schooling or sub-baccalaureate credentials from a for-profit or public institution. We find no evidence to suggest that employers prefer applicants with for-profit college credentials relative to those with credentials from public community colleges. If anything, our results suggest employers prefer applicants who attended public community colleges. In our comparisons between applicants with and without postsecondary experience, we cannot statistically distinguish an effect of 2-year college credentials on employer response rates. Our estimates do not rule out modest returns to postsecondary experience, particularly at public community colleges, but our findings are inconsistent with there being large effects of sub-baccalaureate postsecondary credentials on employer interest in job applicants.

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1. Introduction

The growth of the for-profit sector over the past 20 years is one of the most striking developments in the United States market for higher education. Enrollment in for-profit colleges has more than tripled in the past decade, while non-profit college enrollment increased by less than thirty percent (National Center for Education Statistics (NCES), 2013a). This growth is all the more remarkable given that for-profit colleges represent an expensive postsecondary alternative, with average annual tuition costs at 2-year for-profit colleges that are nearly $12,000 higher than at public community colleges (Knapp, Kelly-Reid and Ginder, 2011). Partly reflecting this tuition differential, students attending for-profit colleges received 21-27 percent of federal Pell Grant and subsidized student loan disbursements in recent years despite comprising a much smaller fraction of total postsecondary enrollment (Baum and Payea, 2013).[1]

The growth of the for-profit sector in higher education is seen by some as a market response to unmet educational needs. Indeed, for-profit institutions advertise that they tailor educational programs to students seeking skills and training with direct labor market applications (Bailey, Badway, and Gumport, 2001; also see Gilpin, Saunders and Stoddard, 2013). These colleges are also well-suited to serve students without access to courses at capacity-constrained public colleges, particularly during difficult economic times (Bohn, Reyes, and Johnson, 2013; Pearson Foundation, 2011). However, for-profit colleges have been criticized in the media and by government agencies for providing low-quality educational programs at high cost, and for engaging in questionable recruiting practices.[2] These criticisms have motivated proposals to

[1] The proportion of for-profit students receiving federal grants is approximately twice that of public and private non-profit colleges (NCES, 2011). A 2012 report (U.S. Senate Committee on Health, Education, Labor and Pensions, 2012) found that over 80 percent of revenues at the 30 for-profit colleges they reviewed came from federal funds.
[2] See for instance Golden (2010a, 2010b), Goodman (2010), the U.S. Government Accountability Office (2010), and the U.S. Senate Committee on Health, Education, Labor and Pensions (2012).

strengthen regulation and oversight of the for-profit sector and have drawn attention to the issue of whether students benefit from for-profit college attendance.[3] There are relatively few studies that provide empirical evidence on the labor-market returns to attending a for-profit college.[4]

In this paper, we present results from a field experiment designed to determine employer preferences for job applicants who attend for-profit colleges. In the experiment, we randomly assign information about postsecondary education to the resumes of fictitious applicants for advertised job openings.[5] We compare employer responses to resumes that list for-profit colleges to those that list either a public community college or no postsecondary schooling at all. This study is not only the first to experimentally examine the effect of for-profit college attendance on labor market outcomes, but it is also the first experimental analysis of the effect of sub-baccalaureate education more generally.

To carry out the experiment, we sent resumes in response to posted job openings in six broad occupational categories (sales, administrative assistant, customer service, information technology, medical assistantship, and medical billing/office) in seven major cities in the United States (Atlanta, Boston, Chicago, Houston, Philadelphia, Sacramento and Seattle). We constructed resumes that list randomly assigned colleges for applicants, which were drawn from a pool of up to three for-profit and three community college institutions for each city-occupation

[3] For instance, the U.S. Department of Education recently proposed regulations that would tie an institution's eligibility to receive federal financial aid to the labor market success and loan repayment of its students (Anderson, 2014; Department of Education, 2011).

[4] Deming, Goldin, and Katz (2012), Lang and Weinstein (2013), and Chung (2008) use a "selection on observables" strategy to examine the differential return to for-profit relative to not-for-profit postsecondary schooling. Cellini and Chaudhary (2012) use a worker fixed-effects strategy to examine the return to sub-baccalaureate credentials and the differential return by profit or non-profit sector. Survey responses suggest that employers have generally favorable opinions of for-profit college programs, but even more positive impressions of public competitors (Hagelskamp, Schleifer, and DiStasi, 2014).

[5] This "resume audit study" design has been used to examine discrimination based on race (Bertrand and Mullainathan, 2004), age (Lahey, 2008), gender (Riach and Rich, 2006), obesity (Rooth, 2009) and nativity (Oreopoulos, 2011). Kroft, Lange, & Notowidgo (2013) use this approach to examine the effects of unemployment spells. In education, resume audit studies have been used to examine teacher employment (Hinrichs, 2013) and the effects of math skills (Koedel and Tyhurst, 2012).

combination. Thus, our findings pertain to a broad swath of postsecondary institutions across a geographically diverse set of major cities. The experiment is designed to cover "general" occupations used in other resume audit studies (e.g., Bertrand and Mullainathan, 2004; Kroft et al., 2013) as well as occupations requiring more specialized training that may be particularly relevant given the vocational focus of many for-profit colleges.[6]

We focus on job applicants with sub-baccalaureate credentials because of the significant role played by the for-profit sector in producing sub-baccalaureate degrees in the United States – for-profit colleges confer almost one-third of associate degrees and sub-baccalaureate certificates (NCES, 2013a). Focusing on the sub-baccalaureate level also corresponds to a niche that for-profit institutions claim to fill. In particular, many for-profit colleges market themselves as offering vocational programs that have been traditionally offered in community colleges (Turner, 2007).

Because community colleges offer programs that are potentially close substitutes for the programs offered at for-profit colleges, but at a much lower direct cost to students, the question of whether for-profit colleges offer any advantages in the labor market over their community-college counterparts merits attention. It is also important to understand whether employers prefer workers who attended a for-profit college relative to those with only a high school diploma. The for-profit sector justifies its disproportionate accrual of public funds with claims that it draws students into postsecondary schooling who otherwise would not attend college (e.g., Guryan and Thompson, 2010); however, government officials have questioned the value of the education

[6] The general occupations used in other audit studies include sales, customer service, and administrative support. We also analyze more specialized occupations in the fields of information technology, medical assisting, and medical billing/office for which there exists a sizable market of for-profit training providers.

provided at some for-profit schools relative to high school in order to motivate increased regulation.[7]

Our experiment does not reveal any evidence to suggest that resumes listing for-profit colleges are more likely to garner interest from employers relative to resumes that list public community colleges. In fact, our point estimates indicate that applicants who attend for-profit colleges receive less interest from employers than do applicants who attend public community colleges, and in some specifications these estimates are statistically significant. We cannot statistically distinguish between applicants with postsecondary experience from either sector and high-school graduates. However, for applicants with public-college credentials, our estimates do not rule out modest returns to postsecondary experience; in contrast, the nominal differences in employer response rates between for-profit college attendees and high school graduates are much smaller and inconsistent in sign. These latter findings suggest that even if the for-profit sector induces some students to attend college who would otherwise not have pursued postsecondary schooling, the benefits may be limited, at least along the dimension of generating employer interest in the labor market.[8]

2. The For-Profit Postsecondary Schooling Sector

Until the late 1990s, enrollment in for-profit colleges comprised only a small share of the higher education market. Since then the share of college students enrolled in for-profits has increased sharply and currently stands at approximately 11 percent (NCES, 2014). For-profit

[7] The U.S. Secretary of Education was recently quoted as saying that of the for-profit colleges subject to action under newly proposed federal regulations, "the significant majority, 72 percent — produce graduates who on average earned less than high school dropouts" (Kessler, 2014).

[8] Belfield and Bailey (2011) review the current evidence base for community colleges, which relies on studies of observational data, and conclude that substantial earnings gains are associated with attendance and degree completion. A recent study by Jepsen, Troske and Coomes (2014) supports the consensus of the studies covered by Belfield and Bailey with respect to associate-degree attainment, but not for other types of credentials (e.g., certificate, coursework), which they conclude offer much less value in the labor market.

colleges are particularly prevalent at the sub-baccalaureate level. Like non-profit colleges, most for-profit colleges offer traditional academic degree programs along with certificates in specific occupational fields, and there is strong evidence that the two sectors compete for students (Cellini, 2010; Turner, 2012). Nonetheless, the instructional offerings in public and for-profit colleges can differ in important ways. For-profit colleges tend to offer flexible course scheduling, more on-line assistance and support, and shorter degree programs (Bailey, Badway, and Gumport, 2001). Although for-profit colleges have been criticized for spending large sums on marketing and recruiting (U.S. Senate Committee on Health, Education, Labor and Pensions, 2012), they also direct more resources toward student advising, career counseling, and job placement than public colleges (Rosenbaum, Deil-Amen and Person, 2006). Even when course offerings appear similar, the content of the courses in for-profit colleges tends to have more emphasis on "real world" applicability (Bailey, Badway, and Gumport, 2001). For-profit colleges are also more likely to rent their facilities, have higher student-to-instructor ratios, and generally lower per-pupil expenditures than non-profits (Bennett et al., 2010; Hoxby and Avery, 2012).[9]

A key difference between for-profit and public colleges is the direct cost of attendance. Annual average tuition is nearly five times higher at for-profit colleges than at public community colleges (Baum and Ma, 2013; Knapp, Kelly-Reid and Ginder, 2011). Although for-profits might be more effective at securing financial aid for their students (Rosenbaum, Deil-Amen and Person, 2006), students attending for-profit colleges likely pay substantially more out-of-pocket than their peers attending comparable public schools (Cellini, 2010; Bailey, Badway, and

[9] While student-to-instructor ratios are higher in for-profit colleges, they also tend to have fewer very large classes than public colleges (Bennett et al., 2010). Moreover, lower per-pupil expenditures could be beneficial if this reflects greater efficiency in the for-profit sector.

Gumport, 2001). Underscoring this point, students who attend for-profit colleges have much larger student loan burdens on average (Deming, Goldin, and Katz, 2012, 2013) and a large and growing share of total student loan defaults come from for-profit students (U.S. Department of Education, 2010).

Given the cost differences across sectors, competitive pressure should result in a higher labor market return in the for-profit sector. Cellini (2012) estimates that all else equal, the return to a year of instruction in a for-profit college would need to be 60 percent higher than the return to a year of community college to justify the direct-cost difference.[10] This has important implications for decisions such as whether a student ought to select a for-profit or public college, or to which sector marginal public investments ought to be directed. Policymakers have become increasingly concerned with whether students who attend for-profit colleges experience post-college success. A prominent example of this concern in action is the "gainful employment" rule that ties eligibility for receiving federal financial aid to student loan repayment rates, which the government uses to proxy for labor market outcomes.[11]

[10] Cellini (2012) calculates that if the return to a year of community college is 5.3 percent, then the cost differential would imply that the return to a year of for-profit college would need to be 8.5 percent, or 60 percent higher. The return need not be this large if the higher direct cost is offset in part by lower indirect costs (e.g., more-flexible course scheduling). Moreover, the differential in the rate of return would not need to be as large from a societal standpoint because the lower cost of public community college already reflects sizable public investments.

[11] The gainful employment rule that the U.S. Department of Education proposed in March 2014 stipulates that postsecondary programs would be at risk of losing eligibility for federal financial aid if the estimated loan payment of a typical graduate exceeds 30 percent of discretionary income or 12 percent of annual income. The estimated annual loan payment of typical graduates exceeds 20 percent of their discretionary income or 8 percent of total income. It also requires that the default rate for former students not exceed 30 percent. These rules are proposed under the clause in the Higher Education Act of 1965 that indicates that the federal government will only support postsecondary programs that "prepare students for gainful employment in a recognized occupation" (see 20 U.S.C. 1001.)

3. Experimental Design and Procedures

3.1 Education Treatments

The resumes in our experiment indicate one of four education levels: (1) a high school diploma, (2) college coursework, (3) a non-academic vocational certificate, or (4) an associate degree. Resumes that list coursework or an associate degree indicate two years of college experience and resumes that list a certificate indicate one year. Resumes with at least some postsecondary education denote attendance at either a for-profit or public college. We focus on sub-baccalaureate schooling because, as noted earlier, for-profit colleges account for a large and growing share of certificates and degrees conferred at this level, and also because many for-profit colleges market themselves as offering educational programs that provide students with specific vocational skills over a shorter time-span than a typical four-year university.

We constructed all of the resumes to reflect local applicants who attended colleges with physical locations in the city. We selected colleges in each city by first identifying associate-degree-granting public and for-profit colleges in the Integrated Postsecondary Education Data System (IPEDS) with physical addresses in the metropolitan area. We then examined each college's course catalog and website to determine whether it offers programs that fit the focal occupational categories.[12] The colleges included in our study were selected at random based on

[12] Specifying the fields of study on the resumes is complicated by the fact that different colleges have different names for similar programs. For example, in a single city, associate degrees in medical assisting will have a variety of titles such as "Associate of Science in Medical Assisting," "Associate of Occupational Science in Medial Assisting," and "Associate in Applied Science in Medical Assisting." Similarly, some colleges have sub-associate degree "diplomas," while others have "certificates." These differences can reflect differences in program curriculum and accreditation requirements. One option for dealing with these differences is to create common names for similar educational programs across colleges, which has the advantage of allowing us to compare for-profit and public colleges on equal ground. However it may also be the case that part of the for-profit college effect is the production of a more-carefully specified degree that will appeal to employers. If the unique names of educational programs across colleges constitute part of their treatment effects, it would be preferable to use the institution-specific names for educational programs on the resumes. Neither option is clearly preferred. In setting up the experiment, we structured it so half of the resumes take the "common name" approach and half use the college-specific names of

an enrollment-weighted selection probability. We included 12-14 public and for-profit colleges in each city.

All resumes indicate that the applicant earned a high school diploma in 2010 and, for those who attended college, finished their postsecondary schooling in 2013. Thus, our experiment is structured to examine how for-profit college attendance affects the employability of young, recent entrants into the labor market. We chose to focus on recent labor market entrants because education treatments are more likely to influence outcomes for this group given that they have shorter and less informative work histories than older workers. This view is supported by research on employer learning which shows that the market learns about worker productivity quickly and educational signals are the most valuable early in a worker's career (Altonji and Pierret, 2001; Lange, 2007). A benefit of the larger effect size that we expect for younger workers is that it can be statistically distinguished with a smaller sample, which is useful because power is an important consideration in the design of the experiment. We also set up the resumes so that there are not any new jobs listed after the college experience. All resumes indicate either the continuation of a pre/during-college job, or in the case of some resumes with work-history gaps, that the applicant is not employed.[13]

3.2 Labor Markets and Occupations

We sent resumes to advertised job openings in the following seven metropolitan areas: Atlanta, Boston, Chicago, Houston, Philadelphia, Sacramento and Seattle.[14] These cities represent a geographically diverse set of large urban areas in the United States. We focus on

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degree programs. In our primary models we combine these two types of resumes because supplementary analysis indicates that that this distinction is not of substantive importance.

[13] This also helps to ensure that the education treatments are not diluted by work experience. Another problem with listing randomly assigned post-college work experience on the resume is that it would amount to controlling for work histories which in principle should be endogenous to the education treatment.

[14] At the time when this draft was written data collection was still underway (but wrapping up) in Chicago and Seattle.

larger cities because they have larger for-profit and public college sectors. This increases the number of colleges in each city-by-sector cell in our experiment, which reduces the role of idiosyncratic aspects of any one college in driving our findings. Of course, it is also important that an ample supply of job advertisements is available and larger cities have more advertised openings.

Resumes were submitted to advertised positions in six broad occupational categories: administrative assisting, customer service, information technology, medical assisting (excluding nursing), medical billing/office and sales. Several considerations went into the choice of these occupations. First, we wanted occupations that vary in the kinds of skills that they require. In particular, we wanted some occupations that involve specialized skills for which specific vocational training that could be acquired through a postsecondary schooling program would be directly beneficial. For-profit colleges market themselves as providing this type of training in many cases. The occupations that we classify as "specialized" are information technology, medical assisting and medical billing/office. We also wanted to include occupations that have less emphasis on specific technical skills but are still reasonable target occupations for for-profit college attendees. The occupations that we classify as "general" are administrative assisting, customer service and sales.

Table 1 shows that while for-profit colleges certainly target specialized occupations (e.g., health and information technology), they provide training targeted at general occupations as well (e.g., business).[15] Our experiment is designed to examine for-profit college effects for degrees in

[15] We did not send resumes to occupations that have strict professional certification requirements because of concerns about how the layering of certifications onto the educational treatments would affect interpretation, along with concerns about statistical power (in the case of, for example, within-education-sector variation in certifications). A consequence of this decision is that we did not evaluate nursing as an occupation because these jobs almost always require professional certification. On the other hand, we did send resumes to medical assisting positions even though job advertisements in this field often request certified workers. We did not apply to medical

fields broadly captured in the first three rows of Table 1. We avoid occupations where for-profit colleges provide almost all (e.g., personal and culinary services) or almost none (e.g., liberal and general studies) of the share of sub-baccalaureate credentials.

In addition to using the prevalence of training at 2-year colleges to determine the focal occupational categories in our study, another important consideration is access to a sufficient number of job advertisements. We chose the occupational categories based in part on informal reviews of job boards where employers post available positions. Beyond the practical consideration that we require job advertisements to generate data for the experiment, our focus on occupational categories with many openings makes our study informative about the larger labor market into which students are graduating.

3.3 Constructing the Resumes

We used computer software developed by Lahey and Beasley (2009) to generate a large bank of randomly-generated resumes for the experiment. All resumes share a common structure but the specific characteristics that end up on each resume are randomly assigned. The resumes include up to four sections.

The first section indicates the applicant's name and contact information (street address, local phone number, and email address). Applicants' first names were chosen to convey gender. We used census data to identify common first names for each racial/ethnic group represented in our study: African American, Hispanic, and white. Only the Hispanic first names have an obvious racial/ethnic connotation. We selected three female-sounding first names – Isabella, Megan and Chloe – and three male-sounding first names – Brian, Carlos and Ryan – with "Isabella" and "Carlos" indicating Hispanic origin. Last names were chosen to indicate that the

assisting jobs that explicitly requested certification, which means that we applied to a selected sample of these types of jobs. We elaborate on this caveat in Section 6.

applicant was likely to be African American (Washington and Jefferson), Hispanic (Hernandez and Garcia) or white (Anderson and Thompson), again using census data to identify names that strongly associate with a particular racial/ethnic group.[16]

We listed local phone numbers and email addresses for all applicants, which we used to track responses. We selected home addresses in zip codes where median household incomes were in the middle quintile in the metropolitan area. We used zip codes close to the center of each city so as to allow for a larger set of jobs for which applicants' commutes would be manageable.

The second section of each resume lists education credentials starting with a randomly assigned local high school.[17] Resumes that indicate college attendance list the field of study and degree/certificate conferred, if any. Resumes that do not indicate a degree or certificate indicate "coursework" in the field of study.[18]

The third section of each resume details the applicant's work history. For each job the resume indicates the dates of employment, employer name, job title, and a bulleted list of job responsibilities and accomplishments. The work histories are modeled based on real resumes for job seekers collected in the design phase of the experiment. When selecting the entries for the work histories we chose a combination of entry-level jobs related to the relevant occupational

[16] In contrast to Bertrand and Mullainathan (2004), we did not use distinctly African American-sounding first names, as these names are more commonly given to children from lower socioeconomic status households (Fryer and Levitt, 2004), which could confound the effect of race. The cost of doing so, of course, is that the "Washington" and "Jefferson" surnames might be less strong signals that someone is African American than a distinctive first name. Appendix Table A.1 reports selected estimates of race and gender effects on employer responses. See Darolia et al. (2014) for a more-detailed discussion of the race and gender results from our experiment.

[17] High schools were chosen from among the set of public high schools in the metropolitan area, including high schools from the primary urban public school district as well schools from surrounding suburban districts. In all cases, we selected schools with demographically diverse student bodies and with average statewide test scores in the middle or fourth quintile.

[18] The randomizer selected level of schooling, college name and field of study simultaneously. These elements were not chosen independently because the name of the field of study depends on the level of schooling, and in resumes where the field of study is allowed to be college-specific, the field of study depends on the college.

category and general low-skilled jobs (e.g., retail clerk). Based on our perusal of real resumes, and similarly to previous audit studies (e.g., Bertrand and Mullainathan, 2004; Lahey, 2008), we generated some resumes with work-history gaps. Table 2 documents the prevalence of work-history gaps in the resumes.

The final section of each resume provides a list of randomly assigned general skills and qualifications for the applicant, again in bulleted format. For each occupational category we selected skills from real resumes of relatively inexperienced workers seeking jobs in the appropriate occupation. Some resumes do not include the final section. Based on our review of real resumes posted by job seekers, it is quite common for resumes at this level to omit this information.

3.4 Applying to Jobs and Recording Employer Responses

We sent resumes to advertisements for job openings in the seven cities between May 2013 and May 2014. The experiment started with a pilot in Houston. We include all but the first two weeks of data collection from Houston, when experimental protocols were in the process of being developed, in the analytic sample (including all data from Houston does not affect our findings). We started collecting data in Atlanta in July, Boston and Philadelphia in August, Sacramento in October, Chicago in January, and Seattle in February.[19] Note that there is substantial time overlap across cities in terms of when the data were collected.

We identified suitable job advertisements in part based on a set of rules and in part based on judgment. We used rules to avoid sending resumes to jobs for which the applicant was clearly underqualified (e.g., database administrator with 7+ years of experience) and/or listed narrow

[19] Once data collection began in Boston we stopped collecting data in Houston. Data collection in Houston was never as intensive as in the other cities, and as such, our sample size in Houston is small relative to the other cities (see Table 2).

skills that were not conveyed by any of our resumes (e.g., certified radiological technician). In cases where our applicants were on the margin of being qualified, we sent the resume(s) (e.g., bachelor's degree preferred but not required). We also trained research assistants to use their judgment to avoid job postings that were unlikely to be credible, such as sales jobs promising substantial earnings for limited work. We avoided sending resumes to recruiters to the extent possible.

Another issue with sending the resumes is that job advertisements are more abundant in some fields than others. Openings for which our applicants were reasonably qualified were more common in the following occupational categories: administrative assisting, customer service, medical billing/office and sales. The numbers of suitable advertisements in information technology and medical assisting were lower across all cities.[20] The discrepancy in suitable job advertisements across fields reflects an important aspect of the labor market for individuals at this skill level, and our experiment partly reflects this aspect of the labor market through its natural weighting toward the occupational categories with more abundant advertisements (see Table 2). That said, we did implement a protocol in each city to prioritize job advertisements in medical assisting and information technology when they were available.[21] So, if anything, our study over-represents these fields that require more specialized skills.

We sent two resumes to each job advertisement. We structured the resume sampling procedure to ensure that there was no overlap in the information on the two resumes. We also

[20] For medical assisting, the lower number of suitable job advertisements is driven in part by the fact that we did not apply to advertisements stipulating that a certification was required, although this is not the only reason. For information technology, the lack of advertisements is attributable solely to the fact that fewer advertisements are posted, and fewer still for which our applicants are reasonably qualified. The information technology industry is currently experiencing rapid growth in Seattle, making it an exception in this occupational category (Taylor, 2014).
[21] The protocol was simple: for each period of work, research assistants were instructed to check job boards for medical assisting and information technology positions first. Because suitable advertisements in the other occupational categories were so much more abundant, even with this protocol our data are still weighted toward the other occupations.

constructed two resume formats, and each employer received one resume in each format. The second resume was sent to each employer at least four hours after the first. Most second resumes were sent within 48 hours of the initial resume (as shown in Appendix Table A.1, second resumes received fewer employer responses). The ratio of resumes to job posting in each city in Table 2 is always less than two because the random resume generator sometimes produced resumes with errors; when the second resume in a sampled pair had an error, we sent just the first resume.

Employers responded to the resumes via email and phone. Phone calls were sent to voicemail. In the analysis below we consider two outcome variables based on employer responses. The first is a binary indicator for whether the employer legitimately responded to the resume (we did not code perfunctory emails as responses – e.g., emails that simply confirmed receipt of the resume). The second focuses on the intensity of the response and is a binary indicator for whether the employer explicitly requested an interview with the applicant. We did not specify any rules about the time between the initial application and the employer response, although most responses came within 1-3 days of the initial application.

4. What Can We Learn from the Experiment?

The goal of the experiment is to examine whether employer preferences for job applicants systematically vary with information about postsecondary schooling provided on an applicant's resume. While employer responses do not provide direct evidence about wage and employment outcomes, they are informative. As noted by Bertrand and Mullainathan (2004), as long as there are at least moderate frictions in the job-search process, employer response rates will translate into job offers, which will translate into employment and wage outcomes.

The key benefit of our experimental design is that we can circumvent the issue of selection into schooling level and sector by randomly assigning education credentials. There are reasons to expect selection to be an important concern along both dimensions. As noted above, similar resume audit studies have been used to examine how a number of applicant characteristics influence employment outcomes in previous research (e.g., see Bertrand and Mullainathan, 2004; Hinrichs, 2013; Koedel and Tyhurst, 2012; Kroft, Lange, and Notowidgo,2013; Lahey, 2008; Oreopoulos, 2011; Riach and Rich, 2006; Rooth, 2009).

A limitation of the audit-study design is that we are unable to identify the mechanisms that underlie our findings. For instance, we cannot provide direct evidence on whether employers think that for-profit colleges offer better or worse instructional programs than community colleges, or whether they expect higher-ability candidates to come from a particular sector.[22] Additionally, by randomly assigning for-profit and public college credentials to resumes, our research design is necessarily silent about some ways that colleges may affect student outcomes. For example, it could be that for-profit and public colleges offer differential job-placement services, and by design our experiment does not allow for more intensive job search (aided or not) by applicants from either sector. Relatedly, it is important to recognize that our comparisons identify the effect of college sector on employer responses net of any effect of sector on college attainment. This is because we construct the resumes so that the college listed on the resume is orthogonal to the level of educational attainment. Thus, differences in college completion rates by sector that have been found in other research (e.g., Deming, Goldin, and Katz, 2012) will not

[22] Note that a related issue applies to other resume audit studies. For example, in audit studies examining labor market discrimination (e.g., Bertrand and Mullainathan, 2004; Lahey, 2008), it is not possible to determine if discrimination is a result of personal animus or statistical discrimination.

be reflected in our estimated effects of college sector.[23] However, this is necessary to ensure that the effect of college attainment does not confound the effect of college sector (and vice versa), and at least with respect to this last limitation we are able to provide some indirect evidence on its likely importance in a supplementary analysis (see below).

In summary, our experiment is structured to estimate the causal effect of listing a for-profit postsecondary institution on a resume, but it cannot speak to all dimensions of how student experiences may differ across sectors.

5. Empirical Analysis and Results

5.1 Descriptive Statistics

Tables 2, 3, 4 and 5 show descriptive statistics for the 8,376 resumes in our analytic sample. Tables 2 and 3 divide the data by city; Tables 4 and 5 divide the data by treatment condition (for-profit, community college, high-school only). Beginning with Table 2; the race, gender, and education-group shares are roughly equal across the analytic sample. More than half of the resumes have at least a one-year work-history gap, and 12 percent have a two-year gap (recall that these are young workers and many of them have concurrent schooling). Although there is some variation in the occupational shares across cities, likely reflecting differences in local labor markets, consistent patterns emerge. As noted above, information technology and medical assisting have the smallest shares.[24]

Table 3 shows response rates and interview-request rates across occupations and cities. The overall response rate is 11.1 percent and 4.7 percent of applicants received an interview

[23] Andrews et al. (2012) use the college that workers graduate from to define the "treatment" in their analysis of the return to college quality on earnings. They also find similar results when using college attended to define the treatment.

[24] Again, the information technology industry is currently experiencing rapid growth in Seattle, making it an exception in this occupational category (Taylor, 2014).

request. Prior resume field experiments indicate response rates in the range of 8-12 percent (Oreopoulos, 2011; Hinrichs, 2013; Koedel and Tyhurst, 2012; Kroft, Lange, & Notowidigdo, 2013; Lahey, 2008), with interview request rates of 3-5 percent. Our response rates are in line with the extant literature.

Response rates are consistently the highest for sales, customer service and information technology positions. The relatively high response rate for information technology openings is interesting given that the number of job advertisements is low; it suggests a lower supply of qualified applicants for advertised positions. Response rates are lower for applications to administrative assisting, medical assisting and medical billing/office openings. Interview request rates are between 30 and 50 percent of total response rates. This ratio is generally consistent across cities and occupations.

The one city with noticeably lower response rates is Atlanta. We cannot find any obvious differences between how the experiment was administered in Atlanta and elsewhere. The lower response rate could be driven by a number of factors ranging from local labor market conditions to inadvertent signaling that we built into the resumes for that city. Although the Atlanta response rates are lower than the response rates in the other cities, there were no obvious violations to the experimental protocol in Atlanta.[25]

Table 4 breaks out the sample by treatment condition. Although there are some differences in resume characteristics across treatments, joint tests fail to reject the null hypothesis that resume characteristics are independent of treatment. Table 5 shows raw response rates by treatment condition. The raw gaps in response rates and interview-request rates in Table 5 preview our main findings.

[25] We estimated our models excluding the data from Atlanta and verified that our findings are qualitatively unaffected (in fact, we verified that our findings are robust to dropping the data from each of the cities individually).

5.2 Empirical Analysis

Randomization makes estimation straightforward. We estimate the effect of for-profit status on employer response rates using the following model, which we specify as a logit:[26]

$$Y_{ijk} = X_{ijk}\beta + HS_{ijk}\gamma + FP_{ijk}\delta + \varepsilon_{ijk} \quad (1)$$

In equation (1), Y_{ijk} is an indicator for the outcome of interest (either "any response" or "interview request") for resume *i* in occupation *j* in city *k*. The X-vector contains resume characteristics including city, occupation, and applicant information.[27] HS_{ijk} is an indicator equal to one if the applicant did not obtain any postsecondary education and FP_{ijk} is an indicator equal to one if the applicant attended a for-profit college. The omitted educational treatment is community college. The X-vector is unnecessary for reducing bias in the estimates of γ and δ because of the randomization, but its inclusion in the models improves precision. Consistent with previous studies, all of our standard errors are clustered at the level of the job advertisement (e.g., Bertrand and Mullainathan, 2004; Oreopoulos, 2011).

Based on preliminary power calculations and resources for the project, we powered the experiment to detect an effect of approximately 10 percent of for-profit status, relative to public college, on the overall employer response rate (at the 5 percent level). Given the baseline response rate of 11.1 percent, this corresponds to just over a one percentage-point change. In the interview-request models we are powered to detect an effect size of approximately 0.6 percentage points, which corresponds to a 13-percent effect.

[26] We also estimate analogous linear probability models and obtain similar results (results not reported for brevity).
[27] More specifically, in our fullest specifications the X-vector includes controls for whether the resume is the first one to be sent to the employer, whether it was accompanied by a more-enthusiastic greeting, city-by-occupation indicators, a flexible time trend to account for seasonality in employer responses, exact name indicators (in sparser specifications the name indicators are aggregated to race-gender indicators), information on work-history gaps and address and high-school indicators.

We powered our comparisons involving high school at a lower level because we expected a larger effect at the onset of the project. For these comparisons we can detect differences between the high school treatment and either college treatment that are as small as 1.6 percentage points for the overall response rate, and 0.9 percentage point for interview requests. These correspond to effect sizes off of the baseline rate of 14 and 19 percent, respectively.

It is important to recognize that the above-described power figures are based on aggregating all of the for-profit and public college resumes, as shown in equation (1). The experiment is not powered to compare individual credentials across sectors – e.g., our standard errors are too large to make credible claims about the relative value of associate degrees at for-profit and public colleges. Faced with the power limitations inherent to resume audit studies, in the design phase of the experiment we concluded that the most useful parameter to target in the experiment is one that compares students with a range of credentials, rather than students with a single credential. Data from the Digest of Education Statistics (2011) indicates that students who enter 2-year colleges in the United States leave with a variety of credentials; most do not earn an associate degree.

As can be seen in Table 2, applicants with associate degrees, certificates and coursework credentials are evenly represented in our data. The estimates from our primary specifications should be interpreted accordingly. In a supplementary analysis presented below we estimate underpowered parameters for each individual credential in our study. Although we do not advocate drawing strong inference from any of the credential-specific estimates because they are not well-powered, the general pattern of estimates is consistent with the notion that a dataset with alternative weights on the different credentials would not generate substantively different results,

and this appears to be the case even if uneven credential weights are applied across sectors (e.g., if we were to allow for a higher associate-degree attainment rate at for-profit colleges).

5.3 Results

Table 6 shows estimated marginal effects from logistic regressions for the parameters of interest where the dependent variable is whether the employer responded to the resume (i.e., any positive, non-perfunctory response). Table 7 shows analogous results when the outcome is an interview request.[28] The tables report estimates from three different models that are increasingly detailed in terms of control variables, and for each model we report results with and without city weights. The city weights re-weight the data so that each city contributes equally to the estimates. Because of variability in city start dates, the availability of job openings, and the availability of research-assistant time, the cities are unevenly represented in the raw data. The rationale behind the city weights is that there is no reason to expect data from one city to be more valuable than data from another in terms of informing our understanding of the effect of for-profit colleges.[29]

Focusing first on our primary comparison between for-profit and public colleges, and the "any response" outcome, Table 6 indicates that employers do not prefer applicants with for-profit credentials. The point estimates for the for-profit effect in Table 6 are consistently negative and insignificant, with standard errors that are small enough to make the null result informative. In particular, we can rule out positive effects of for-profit colleges relative to public community

[28] Estimates of the effects of the other resume characteristics can be found in Appendix Table A.1.

[29] The most obvious city-weighting issue comes from the fact that Houston was used to pilot the experiment and data collection was not carried out with the same intensity there, leading to a much smaller sample. Seattle is also notably under-represented in the raw data, although at the time of this draft data collection is still underway in Seattle. Applying the city weights has power consequences, but we still generally have sufficient power for inference in the city-weighted models. Because it is not obvious that it is optimal to impose equal city weights, we show results for models that do and do not incorporate these weights throughout.

college larger than approximately 0.5 percentage points, or less than five percent of the mean employer response rate.

Moving to Table 7, when we examine interview request rates we can reject even smaller positive for-profit effects. The point estimates for the for-profit college effect relative to public community college are all negative, and are statistically significant at the 10 percent level in the specification with the richest set of controls and in all specifications with the city weights. The point estimates are about 0.5 percentage points in the models with no city weights and 0.7 percentage points in models with city weights, or about 11 percent and 15 percent of the sample mean, respectively. Overall, the results in Table 6 and 7 provide no indication that employers prefer workers who attended for-profit colleges. If anything, employers request interviews more frequently from workers who attended public community colleges.

Turning to the comparisons involving high-school graduates, we cannot statistically distinguish an effect of postsecondary credentials on employer response rates or interview requests for either sector (or in aggregate – results omitted for brevity). Recall from above that our experiment was purposefully powered at a lower level for the comparisons involving high-school graduates because we anticipated a larger effect. Still, we are powered to detect a reasonable effect size on the employer response rate for postsecondary schooling in either sector – roughly 1.6 percentage points, which is about half the size of the percentage-point gap in callback rates for white and African-American sounding names reported by Bertrand and Mullainathan (2004).

We offer two qualifications about our comparisons involving high-school graduates. First, despite our inability to statistically distinguish a general postsecondary-credential effect, our point estimates and standard errors cannot rule out moderately-sized effects on employer

response rates and interview requests, particularly when we compare high-school graduates to individuals with public-college credentials. For instance, the 95 percent confidence intervals in Table 7 for the most precise estimates that compare applicants from public colleges to high school graduates include effects as large as 1.4 percentage points. In contrast, for the comparisons between high school graduates and for-profit college attendees, the nominal differences in employer response rates are much smaller and inconsistent in sign. The second qualification is that for these young applicants, employers may infer more value in the work experience of high-school graduates, who unlike their college-going counterparts, have work histories that are not concurrent with schooling. If experience is valued more by employers for high school graduates, this would attenuate the estimated returns to postsecondary experience in our study.[30]

5.3. Sensitivity Analysis

Although we did not power our experiment to detect for-profit effects specific to particular education levels, in Table 8 we report the education-level-specific estimates. The table reports results from models that split out the effects of each credential-by-sector treatment in the data (there are seven – see Table 2) using model 2 from Tables 6 and 7 as the base model (as with Tables 6 and 7, the findings are substantively similar regardless of which model we use). The omitted comparison group is an associate degree from community college.

With that qualifier that all of the estimates in Table 8 are underpowered, and thus have standard errors that are generally too large for reasonable inference, a notable pattern in the table

[30] The work histories are uncorrelated with the education treatments on the resumes, but a concern is that employers may believe that workers are working less intensively and acquiring less experience while they are in school relative to workers who are not in school (see Mincer, 1974).Whether any attenuation in our estimates owing to this concern is of practical significance is not clear. Put differently, in reality individuals may be choosing between a more intensive post-high-school work experience and attending a 2-year college, in which case one can argue that any labor-market returns associated with that decision should be built into the estimates.

is that applicants with public-college credentials nominally outperform applicants with for-profit credentials within each education level, with the exception being at the associate level in the any-response models. Moreover, no particular education credential establishes itself as clearly preferred to other credentials, which suggests that our findings would not differ substantively to what we report in Tables 6 and 7 if the education levels in our data were weighted differently.[31]

Next we look for heterogeneity in our findings across occupations. Again, we are underpowered in any sub-analyses of the data if the goal is to be able to detect a moderately-sized effect of for-profit college attendance. Nonetheless, the occupation-specific models can be used to test for substantial heterogeneity in the for-profit college effect across occupational categories.

Appendix Tables A.2 and A.3 present results where we divide our data by "specialized" (information technology, medical assisting, medical billing/office) and "general" (administrative assisting, customer service, sales) occupations, respectively. Large differences between for-profit and public colleges do not emerge in the tables. Because of the large standard errors associated with these estimates, we do not offer a strong interpretation of the results.

In Appendix Table A.4 we indirectly address a potential limitation related to our coverage of the medical assisting field. Specifically, we do not indicate medical certifications on the resumes in our study (other than, of course, credentials that come directly from the colleges), which creates two issues. One is that we did not send resumes to medical assisting jobs that explicitly requested certification from a regulatory agency, which was a non-negligible number.

[31] Data from the NCES (2013b) based on the 2008 cohort of entering 2-year college students indicates that 60 percent of for-profit college students obtain a certificate or degree. The corresponding number reported for public college students is only 20 percent. While these numbers suggest attainment rates are higher at for-profit colleges, the American Association of Community Colleges (AACC) argues that the comparison is flawed because it does not account for students who transfer to four-year colleges, with such transfer being more common in the public sector (Marcus, 2012; also see Mullin, 2012). It is not clear what the optimal weights should be, which is why we present results using equal weighting for the three education levels.

Another is that part of the real-world effect of for-profit colleges may include, for example, aid in completing the certification process, which would correspond to higher certification rates and access to more jobs. This is a narrow illustration of the above-described general qualification to our study – by randomly assigning for-profit and public college credentials to resumes, our research design is not informative about some of the ways that colleges may affect student outcomes.

To determine whether our findings are sensitive to whether we include medical assisting resumes at all, Appendix Table A.4 presents results from models where we exclude them entirely. The appendix table shows that our findings are essentially unaffected by whether we include the medical assisting resumes or not. Although it is our view that the medical assisting resumes in our experiment are informative about the for-profit effect in this occupational category, the skeptical reader can at least be confident in drawing inference from our study about the other occupations.

6. Conclusion

Improving our understanding of the labor-market returns to college is critical given the sizable and growing public investment used to support college attendance. The primary motivation for our experiment is to examine the benefits associated with attending a for-profit college. The rapid growth of the for-profit sector lends credence to the belief that it is meeting labor market needs; however, for-profit colleges have been criticized for offering low-quality instruction and taking advantage of uninformed consumers (Deming, Goldin and Katz, 2013; U.S. Government Accountability Office, 2010; Lynch, Engle and Cruz, 2010). In part because students who attend for-profit colleges are disproportionately supported by federal financial aid programs, and disproportionately low-income and at-risk students (Baum and Payea, 2013;

Deming, Goldin, and Katz, 2012, 2013), policymakers have become increasingly concerned with whether for-profit college attendance leads to post-college success. This concern is embodied in the recently proposed "gainful employment" rule. As noted above, the rule ties the eligibility of colleges to receive federal financial aid dollars to student loan repayment rates, which the government uses to proxy for labor market outcomes.

Our findings contribute to the policy debate surrounding for-profit colleges by providing credible evidence on employer preferences for workers who attended for-profit colleges relative to those who attended community college or no college at all. We show that applicants with for-profit college credentials are no more likely to generate interest from employers than their counterparts who attend public college. If anything, applicants from for-profit colleges generate less interest. In our comparisons between applicants with and without any postsecondary experience, we are unable to identify statistically significant effects of 2-year college credentials from either sector on employer response rates. However, for applicants from public colleges we cannot rule out moderately-sized positive effects on employer response rates, whereas when we compare high school graduates to for-profit attendees the nominal differences in employer response rates are much smaller and inconsistent in sign. Although our field experiment design precludes us from examining every dimension along which for-profit colleges can improve student outcomes, these findings inform ongoing policy debates regarding the investment returns to students and taxpayers of for-profit higher education in the United States.

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Table 1. Shares of Certificate and Associate Degrees Issued by For Profit Colleges in the United States by Field, 2011-2012.

	For-Profit College Share
Business	0.25
Computer and Information Systems	0.37
Health Professions	0.47
Liberal Arts & Sciences, General Studies	0.02
Personal & Culinary Services	0.83
Other Disciplines	0.25
Overall	0.32

Note: Statistics generated from 2013 Digest of Education Statistics and IPEDS, for the 2011-2012 school year. For-profit college shares are the fraction of total associate degrees and certificates in a given field that are issued by for-profit colleges.

Table 2. Descriptive Statistics for Submitted Resumes Overall and by City.

	All	Atlanta	Boston	Chicago	Houston	Philadelphia	Sacramento	Seattle
Female	0.49	0.49	0.50	0.51	0.48	0.50	0.47	0.46
African American	0.32	0.32	0.31	0.33	0.33	0.32	0.33	0.33
Hispanic	0.35	0.34	0.35	0.36	0.36	0.35	0.33	0.35
High-school graduate	0.14	0.13	0.15	0.13	0.11	0.16	0.15	0.15
Community College: Some College	0.14	0.14	0.15	0.13	0.15	0.14	0.14	0.13
For Profit: Some College	0.15	0.15	0.15	0.14	0.15	0.15	0.16	0.19
Community College: Certificate	0.14	0.15	0.14	0.15	0.16	0.13	0.13	0.11
For Profit: Certificate	0.14	0.14	0.14	0.16	0.12	0.13	0.14	0.14
Community College: AA Degree	0.14	0.15	0.14	0.14	0.14	0.14	0.12	0.14
For Profit: Some AA Degree	0.15	0.14	0.15	0.15	0.18	0.15	0.15	0.14
Single-Year Work History Gap	0.43	0.42	0.43	0.46	0.43	0.42	0.41	0.43
Two-Year Work History Gap	0.12	0.12	0.14	0.13	0.12	0.12	0.11	0.13
Admin Share	0.23	0.23	0.27	0.27	0.19	0.20	0.20	0.17
Customer Service Share	0.18	0.17	0.19	0.21	0.16	0.18	0.17	0.17
Information Technology Share	0.11	0.12	0.09	0.09	0.14	0.10	0.07	0.22
Medical Assisting Share	0.13	0.13	0.08	0.11	0.13	0.14	0.17	0.14
Medical Billing/Office Share	0.15	0.14	0.15	0.12	0.18	0.16	0.16	0.17
Sales Share	0.21	0.21	0.22	0.20	0.20	0.22	0.22	0.14
Total Resumes	8376	1637	1592	1120	468	1800	1281	478
Total Unique Job Advertisements	4912	992	943	640	354	1012	702	270

Notes: Houston was the pilot city and some resumes were sent out before the structure of the experiment was changed so that we could send two resumes to (most) employers. Thus, the total number of resumes in Houston is lower than in the other cities and the ratio of total resumes to unique job advertisements is lower as well. At the time of this draft, data collection was still ongoing in Chicago and Seattle.

Table 3. Response and Interview-Request Rates by City and Occupation.

	All	Atlanta	Boston	Chicago	Houston	Philadelphia	Sacramento	Seattle
Response Rate (RR)	0.111	0.057	0.141	0.084	0.139	0.119	0.131	0.153
RR: Admin	0.046	0.018	0.076	0.023	0.069	0.040	0.054	0.086
RR: Customer Service	0.127	0.076	0.162	0.109	0.145	0.130	0.117	0.228
RR: Information Technology	0.118	0.055	0.190	0.100	0.154	0.135	0.165	0.067
RR: Medical Assisting	0.088	0.024	0.049	0.017	0.065	0.099	0.165	0.209
RR: Medical Billing/Office	0.059	0.027	0.110	0.022	0.059	0.045	0.062	0.099
RR: Sales	0.216	0.125	0.235	0.206	0.312	0.241	0.229	0.292
Interview Request Rate (IRR)	0.047	0.029	0.056	0.030	0.066	0.053	0.048	0.067
IRR: Admin	0.017	0.007	0.018	0.010	0.023	0.014	0.019	0.062
IRR: Customer Service	0.059	0.047	0.078	0.029	0.053	0.064	0.059	0.114
IRR: Information Technology	0.039	0.015	0.054	0.050	0.031	0.054	0.059	0.019
IRR: Medical Assisting	0.027	0.005	0.025	0.000	0.033	0.032	0.063	0.015
IRR: Medical Billing/Office	0.027	0.014	0.055	0.007	0.035	0.017	0.024	0.049
IRR: Sales	0.098	0.073	0.096	0.076	0.194	0.121	0.068	0.169

Note: At the time of this draft, data collection was still ongoing in Chicago and Seattle. All responses may not be recorded in these cities.

Table 4. Descriptive Statistics for Submitted Resumes by Treatment Condition.

	For-profit	Community College	High School
Female	0.51	0.49	0.46
African American	0.31	0.33	0.33
Hispanic	0.34	0.35	0.35
Some College	0.35	0.34	N/A
Certificate	0.32	0.33	N/A
AA Degree	0.33	0.33	N/A
Single-Year Work History Gap	0.43	0.42	0.43
Two-Year Work History Gap	0.12	0.13	0.12
Admin Share	0.23	0.23	0.21
Customer Service Share	0.18	0.18	0.19
Information Technology Share	0.11	0.11	0.09
Medical Assisting Share	0.13	0.12	0.14
Medical Billing/Office Share	0.15	0.15	0.16
Sales Share	0.21	0.21	0.21
Total Resumes	3661	3516	1199

Notes: As noted in the text, chi-squared tests for the null hypothesis that resume characteristics and treatment conditions are independent were performed jointly and indicate that the randomization procedure was successful. Education levels were not tested jointly across all conditions because of the obvious differences between the postsecondary and high-school-only resumes. Separate tests fail to reject the null hypothesis that education levels are independent of treatment in the postsecondary sample.

Table 5. Raw Differential Response Rates by Treatment Condition.

	For-profit	Community College	High School
Employer Response Rate	0.109	0.115	0.105
Employer Interview Request Rate	0.044	0.051	0.042
Total Resumes	3661	3516	1199

Note: None of the differences across treatments are statistically significant.

Table 6. Logistic Regression Results. Dependent Variable is Any Response. Marginal Effects are Reported.

	Model 1		Model 2		Model 3	
	Unweighted	City Weighting	Unweighted	City Weighting	Unweighted	City Weighting
High school	-0.0088	-0.0056	-0.0078	-0.0043	-0.0066	-0.0032
	(0.0085)	(0.0107)	(0.0084)	(0.0106)	(0.0079)	(0.0098)
For Profit College	-0.0053	-0.0064	-0.0056	-0.0064	-0.0045	-0.0049
	(0.0054)	(0.0066)	(0.0053)	(0.0066)	(0.0051)	(0.0063)
P-value from test: *H_0: HS = for-profit*	0.67	0.95	0.79	0.84	0.78	0.87
Basic Application Details	X	X	X	X	X	X
City Indicators	X	X	X	X		
Occupation Indicators	X	X	X	X		
City-by-Occupation Indicators					X	X
Flexible Time Trend			X	X	X	X
Race & Gender			X	X		
Exact Name Indicators					X	X
Basic Work History			X	X	X	X
Address and High School					X	X
N	8376	8376	8376	8376	8376	8376

** Indicates statistically significant difference between two variables at the 5 percent level.
* Indicates statistically significant difference between two variables at the 10 percent level.
Notes: The omitted treatment is community college. Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data. The flexible time trend includes indicators for 2-week timespans over the course of the experiment.

Table 7. Logistic Regression Results. Dependent Variable is Interview Request. Marginal Effects are Reported.

	Model 1		Model 2		Model 3	
	Unweighted	City Weighting	Unweighted	City Weighting	Unweighted	City Weighting
High school	-0.0055	-0.0008	-0.0052	-0.0005	-0.0049	-0.0010
	(0.0052)	(0.0070)	(0.0049)	(0.0066)	(0.0044)	(0.0057)
For Profit College	-0.0053	-0.0074	-0.0050	-0.0069	-0.0048	-0.0066
	(0.0033)	(0.0042)*	(0.0032)	(0.0040)*	(0.0029)*	(0.0035)*
P-value from test: *H_0: HS = for-profit*	0.93	0.34	0.94	0.33	0.94	0.33
Basic Application Details	X	X	X	X	X	X
City Indicators	X	X	X	X		
Occupation Indicators	X	X	X	X		
City-by-Occupation Indicators					X	X
Flexible Time Trend			X	X	X	X
Race & Gender			X	X		
Exact Name Indicators					X	X
Basic Work History			X	X	X	X
Address and High School					X	X
N	8376	8376	8376	8376	8255	8255

** Indicates statistically significant difference between two variables at the 5 percent level.

* Indicates statistically significant difference between two variables at the 10 percent level.

Notes: The omitted treatment is community college. Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data. The flexible time trend includes indicators for 2-week timespans over the course of the experiment. In the final two columns, 121 observations are dropped because their industry-by-occupation cell perfectly predicts failure (this cell is in a city where data collection is ongoing).

Table 8. Logistic Regression Results for Separate Educational Treatments Using Detailed Models for Both Dependent Variables. Marginal Effects are Reported.

	Model 2: Any Response		Model 2: Interview Request	
	Unweighted	City Weighting	Unweighted	City Weighting
High school	-0.0065	-0.0015	-0.0046	0.0013
	(0.0108)	(0.0137)	(0.0062)	(0.0085)
CC Coursework	-0.0002	-0.0015	0.0034	0.0052
	(0.0111)	(0.0135)	(0.0068)	(0.0086)
For-Profit Coursework	-0.0092	-0.0087	-0.0041	-0.0026
	(0.0099)	(0.0123)	(0.0056)	(0.0075)
CC Certificate	0.0035	0.0094	-0.0015	0.0008
	(0.0114)	(0.0146)	(0.0065)	(0.0083)
For-Profit Certificate	-0.0039	-0.0061	-0.0023	-0.0047
	(0.0101)	(0.0125)	(0.0059)	(0.0072)
For-Profit AA Degree	0.0001	0.0039	-0.0060	-0.0074
	(0.0104)	(0.0132)	(0.0058)	(0.0071)
Basic Application Details	X	X	X	X
City Indicators	X	X	X	X
Occupation Indicators	X	X	X	X
City-by-Occupation Indicators				
Flexible Time Trend	X	X	X	X
Race & Gender	X	X	X	X
Exact Name Indicators				
Basic Work History	X	X	X	X
Address and High School				
N	8376	8376	8376	8376

Notes: The omitted treatment is an associate degree from community college. Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data.

Appendix A
Supplementary Tables

Appendix Table A.1. Marginal Effect Estimates for Control Variables from Model 2 with City Weights.

	Model 2: Any Response (Table 6)	Model 2: Interview Request (Table 7)
High School	-0.0043	-0.0005
	(0.0106)	(0.0066)
For Profit College	-0.0064	-0.0069
	(0.0066)	(0.0040)*
Basic Application Details		
Positive Greeting	0.0104	0.0041
	(0.0065)	(0.0041)
First Resume	0.0160	0.0100
	(0.0064)**	(0.0039)**
Applicant Race/Gender (as implied by name)		
African American Female	0.0164	-0.0065
	(0.0135)	(0.0065)
African American Male	-0.0098	-0.0099
	(0.0113)	(0.0062)
Hispanic Female	-0.0046	-0.0105
	(0.0121)	(0.0061)*
Hispanic Male	0.0081	-0.0004
	(0.0121)	(0.0063)
White Female	0.0206	-0.0035
	(0.0137)	(0.0066)
Work History (categories are not mutually exclusive)		
Any Work History Gap	-0.0059	-0.0008
	(0.0088)	(0.0053)
Two-Year Work History Gap	-0.0304	-0.0120
	(0.0101)**	(0.0060)**
Currently Unemployed	0.0079	0.0023
	(0.0107)	(0.0067)
Occupational Category		
Administrative	-0.1051	-0.0427
	(0.0072)**	(0.0045)**
Customer Service	-0.0467	-0.0186
	(0.0080)**	(0.0045)**
Information Technology	-0.0603	-0.0310
	(0.0088)**	(0.0041)**
Medical Assisting	-0.0673	-0.0349
	(0.0080)**	(0.0039)**
Medical Billing/Office	-0.0921	-0.0347
	(0.0068)**	(0.0041)**

City			
	Boston	0.1400	0.0338
		(0.0247)**	(0.0127)**
	Chicago	0.0446	0.0298
		(0.0322)	(0.0253)
	Houston	0.0410	0.0214
		(0.0646)	(0.0479)
	Philadelphia	0.1001	0.0326
		(0.0226)**	(0.0131)**
	Sacramento	0.1214	0.0403
		(0.0327)**	(0.0202)**
	Seattle	0.1687	0.103
		(0.0535)**	(0.0480)**

** Indicates statistically significant difference between two variables at the 5 percent level.
* Indicates statistically significant difference between two variables at the 10 percent level.
Notes: The marginal effects for the control variables are qualitatively similar with and without weighting. Time trend coefficients are omitted for brevity. Omitted groups are community college, less-positive greeting, second resume, white male, occupation=sales, city=Atlanta. City weighting is such that all cities receive equal weight in the data.

Appendix Table A.2. Logistic Regression Results for Occupational Categories Information Technology, Medical Assisting, Medical Billing/Office. Marginal Effects are Reported.

	Model 2: Any Response		Model 2: Interview Request	
	Unweighted	City Weighting	Unweighted	City Weighting
High School	0.0068	0.0086	0.0035	0.0039
	(0.0132)	(0.0148)	(0.0088)	(0.0101)
For Profit College	-0.0037	-0.0044	-0.0005	0.0011
	(0.0077)	(0.0085)	(0.0052)	(0.0061)
P-value from test: *H_0: HS = for-profit*	0.38	0.33	0.62	0.75
Basic Application Details	X	X	X	X
City Indicators	X	X	X	X
Occupation Indicators	X	X	X	X
City-by-Occupation Indicators				
Flexible Time Trend	X	X	X	X
Race & Gender	X	X	X	X
Exact Name Indicators				
Basic Work History	X	X	X	X
Address and High School				
N	3206	3206	3033	3033

Notes: Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data. The flexible time trend includes indicators for 2-week timespans over the course of the experiment. For the interview-request models, data from three 2-week time intervals were dropped because no positive responses were obtained.

Appendix Table A.3. Logistic Regression Results for Occupational Categories Administrative Assisting, Customer Service and Sales. Marginal Effects are Reported.

	Model 2: Any Response		Model 2: Interview Request	
	Unweighted	City Weighting	Unweighted	City Weighting
High School	-0.0143	-0.0118	-0.0081	0.0007
	(0.0109)	(0.0151)	(0.0062)	(0.0104)
For Profit College	-0.0061	-0.0045	-0.0070	-0.0116
	(0.0071)	(0.0097)	(0.0041)	(0.0059)**
P-value from test: *H_0: HS = for-profit*	0.44	0.63	0.83	0.23
Basic Application Details	X	X	X	X
City Indicators	X	X	X	X
Occupation Indicators	X	X	X	X
City-by-Occupation Indicators				
Flexible Time Trend	X	X	X	X
Race & Gender	X	X	X	X
Exact Name Indicators				
Basic Work History	X	X	X	X
Address and High School				
N	5170	5170	5134	5134

Notes: Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data. The flexible time trend includes indicators for 2-week timespans over the course of the experiment. For the interview-request models, data from one 2-week time interval early in the experiment was dropped because no positive responses were obtained.

Appendix Table A.4. Logistic Regression Results for All Occupational Categories Except Medical Assisting. Marginal Effects are Reported.

	Model 2: Any Response		Model 2: Interview Request	
	Unweighted	City Weighting	Unweighted	City Weighting
High School	-0.0131	-0.0144	-0.0061	-0.0012
	(0.0089)	(0.0101)	(0.0054)	(0.0075)
For Profit College	-0.0032	-0.0041	-0.0037	-0.0065
	(0.0057)	(0.0071)	(0.0035)	(0.0046)
P-value from test: *H_0: HS = for-profit*	0.27	0.34	0.66	0.48
Basic Application Details	X	X	X	X
City Indicators	X	X	X	X
Occupation Indicators	X	X	X	X
City-by-Occupation Indicators				
Flexible Time Trend	X	X	X	X
Race & Gender	X	X	X	X
Exact Name Indicators				
Basic Work History	X	X	X	X
Address and High School				
N	7319	7319	7264	7264

Notes: Standard errors are clustered by job posting. Most postings received two resumes. City weighting is such that all cities receive equal weight in the data. The flexible time trend includes indicators for 2-week timespans over the course of the experiment. For the interview-request models, data from one 2-week time interval early in the experiment was dropped because no positive responses were obtained.