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 almost 9,000 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 resumes that list a for-profit college relative to those whose resumes list a public community college. If anything, our results suggest employers prefer applicants who attended public community colleges. Similarly, we find little indication that employers prefer workers who attended a for-profit college relative to those with no postsecondary schooling.

## 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, especially compared to public community colleges (Knapp, Kelly-Reid and Ginder, 2011; Cellini, 2012). Partly reflecting the difference in the cost of attendance across sectors, for-profit students disproportionately receive federal Pell Grants and subsidized student loan disbursements (Baum and Payea, 2013).[1]

The for-profit sector's rapid growth could represent a market response to unmet educational needs. Indeed, for-profit colleges claim that unlike traditional higher education institutions, their programs address student demand for skills and training with direct labor market applications (Bailey, Badway, and Gumport, 2001; Gilpin, Saunders and Stoddard, 2013). But for-profit colleges have been criticized for providing low-quality educational programs at high cost, and for engaging in questionable recruiting practices.[2] These criticisms have motivated proposals to 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] To

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

date, however, there have been relatively few studies examining the labor-market returns to attending a for-profit college.[4]

In this paper we present results from a field experiment designed to examine employer preferences for job applicants who attend for-profit colleges. In the experiment, we randomly assign information about sub-baccalaureate postsecondary education to the resumes of fictitious applicants for advertised job openings. Employer responses to the resumes are then used to make inferences about how the educational information affects employer interest in job applicants.[5] 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.

Our primary comparison is between resumes that list for-profit and public-community-college credentials. This comparison is important in light of research demonstrating that community colleges offer programs that are potentially close substitutes for those offered by many for-profit colleges (Cellini, 2009; Turner, 2006) but at much lower cost (Cellini, 2012). The cost differential makes it important to understand whether for-profit colleges offer labor market benefits that exceed those of community colleges. We focus on sub-baccalaureate

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[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. These studies generally find null to negative effects of for-profit college attendance on earnings relative to community college attendance, although Cellini and Chaudhary (2012) find a positive relationship between for-profit attendance and earnings relative to no postsecondary schooling.

[5] The "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 a resume audit study 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).

credentials because for-profit colleges award a sizable share – roughly one-third – of sub-baccalaureate certificates and degrees in the United States (NCES, 2013a).

We also compare resumes that list a for-profit college to those that do not list any postsecondary schooling. The motivation for this comparison lies in the claim that the for-profit sector draws some students into postsecondary schooling who otherwise would not have attended college at all. This claim has been used to justify the disproportionate accrual of public financial aid spending at for-profit colleges and to argue against proposals to strengthen regulations of for-profit institutions (e.g., Guryan and Thompson, 2010). Finally, our research design also allows us to compare resumes that list a public community college to those with no college. This comparison speaks to the question of the returns to sub-baccalaureate postsecondary schooling in the public sector.[6]

To carry out the experiment, we sent resumes to job postings in seven major cities in the United States (Atlanta, Boston, Chicago, Houston, Philadelphia, Sacramento and Seattle). The postsecondary institutions listed on the resumes were randomly selected from among the for-profit and public community colleges in each metropolitan area. Thus, our findings pertain to a broad swath of postsecondary institutions across a geographically diverse set of major cities. The experiment was 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.[7] We used resumes that randomly varied in the educational attainment level (i.e.,

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[6] Studies on the return to community college include Kane and Rouse (1995), Jacobson, LaLonde and Sullivan (2005) and Jepsen, Troske, and Coomes (2011).
[7] 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

associate degree, certificate, coursework with no credential) because attainment levels differ substantially among students who pursue sub-baccalaureate higher education (NCES, 2012).

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. This finding holds when we pool across educational attainment levels as well as when we allow the for-profit effect to vary by attainment level. We also find little evidence of a benefit to listing a for-profit college relative to no college at all – our point estimates for this comparison are close to zero and we can rule out positive for-profit college effects that are considerably smaller than the effects that have been estimated for other resume characteristics in previous audit studies (e.g., Bertrand and Mullainathan, 2004; Lahey, 2008; Oreopoulous, 2011). The estimated effects of listing a public community college relative to no college are also statistically insignificant, although the point estimates are consistently larger and our confidence intervals leave open the possibility of somewhat higher returns to community college credentials.

We interpret these findings to indicate that the labor market payoff to attending a for-profit college may be limited, especially in comparison to the much-cheaper community college alternative. While our research design does not allow us to address all possible ways that for-profit colleges can affect labor market outcomes (e.g., effects that materialize at the interview stage of the hiring process or later), the results presented here complement recent non-experimental findings (Deming, Katz, and Goldin, 2012; Cellini and Chaudhary, 2012; Lang and Weinstein, 2013) that also find limited labor market benefits to attending a for-profit college.

assisting, and medical billing/office for which there exists a sizable market of for-profit training providers. See below for additional details.

2.   The For-Profit Sector in Higher Education

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).[8] For-profit

colleges tend to offer relatively short degree programs with a strong vocational focus, flexible

course scheduling, extensive on-line instruction and support, and that aim to have real-world

applicability (Bailey, Badway, and Gumport, 2001; Turner, 2006; Breneman, Pusser, and Turner,

2006).[9] 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).

Despite these differences between for-profit and public colleges, the two sectors compete

for students, especially at the two-year level (Cellini, 2009), and many for-profit institutions can

be seen as providing alternatives to the vocational degree and certificate programs offered by

community colleges. Indeed, studies comparing community and for-profit colleges have found

substantial overlap in the programs offered by the two sectors (Cellini, 2005; 2009; Turner,

2006).[10] These patterns can be seen in Table 1, which shows the fraction of associate degrees and

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[8] As documented in Deming, Goldin and Katz (2012), much of this growth has been driven by national chains and institutions that provide much of their instruction online. They also show that for-profit colleges serve a disproportionate share of minorities and students from disadvantaged backgrounds.
[9] For-profit colleges have also taken a number of steps to lower instructional expenditures relative to public community colleges. For instance, they are more likely to rent their facilities, have higher student-to-instructor ratios, and generally lower per-pupil expenditures than non-profit institutions (Bennett et al., 2010; Hoxby and Avery, 2012). 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.
[10] In addition to offering programs comparable to those in for-profit colleges, community colleges also resemble for-profit colleges in the extensive use of online instruction and scheduling courses at a variety of times to accommodate students' schedules (Deming, Goldin and Katz, 2013).

vocational certificates awarded at for-profit institutions by field of study. Across all fields, for-profit colleges award about one-third of sub-baccalaureate credentials. This exceeds the for-profit sector's share of total postsecondary enrollment and demonstrates that sub-baccalaureate instruction is relatively important at for-profit colleges. At the same time, even in fields of study where the for-profit market share is relatively high, for-profit colleges award less than half of sub-baccalaureate credentials, which suggests that public community colleges offer programs that are substitutes for those offered by for-profit colleges.[11] These observations motivate our interest in sub-baccalaureate education and our choices regarding which occupations to include in the experiment.

Perhaps the most important difference between for-profit and public colleges is cost. Average annual 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) and although for-profits may be more effective at securing financial aid for their students (Rosenbaum, Deil-Amen and Person, 2006), students attending for-profit colleges amass much larger student loan burdens than students who attend public colleges (Deming, Goldin, and Katz, 2012; 2013). Cellini (2009) estimates that for a year of sub-baccalaureate instruction in the for-profit sector to provide net benefits to students and taxpayers, the required earnings return is 36 percent higher than in the public sector.[12]

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[11]Detailed analyses of the programs offered by for-profit and community colleges such as in Cellini (2009) also reveal considerable overlap consistent with the tabulations in Table 1.

[12] Students bear a larger share of the costs of attending a for-profit college than a community college, so the breakeven private return is even higher (60 percent). Although for-profit colleges rely heavily on federal financial aid programs for their revenue and account for a disproportionate share of spending on such programs (U.S. Senate Committee on Health, Education, Labor and Pensions; Baum and Payea, 2013), public community colleges are heavily subsidized by taxpayers, and a year of community college instruction costs taxpayers about $11,000 compared to $7,600 at for-profit colleges (Cellini, 2009).

The cost differential and large student loan burdens accumulated by students attending for-profit colleges have motivated a variety of policies designed to strengthen regulation of the for-profit sector. A notable example is the "gainful employment" rule proposed by the United States Department of Education in March of 2014. The rule would tie the eligibility of colleges to receive federal financial aid dollars to the loan repayment rates of students, which serve as a proxy for labor market outcomes.[13]

3.   What can we Learn from the Experiment?

Before discussing the details of how we implemented the experiment it is useful to consider what questions we can and cannot answer with our study. Our goal is to contribute to the understanding of whether for-profit colleges affect students' labor market outcomes. We do so by examining whether information about for-profit college attendance listed on a resume affects employer responses to job applicants. The rationale is that employer responses to fictitious job applications provide information as to how real applicants will fare in the labor market. 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 frictions in the job-search process, employer response rates will translate into job offers, which will translate into employment and wage outcomes.

The effects captured by our experiment could be driven by several possible mechanisms. For the comparisons between resumes that list for-profit and community colleges, these mechanisms include differences in perceptions of the quality of instruction provided across

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[13] The gainful employment rule 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. It also would require that the default rate for former students not exceed 30 percent. The proposed rule does not single out programs in a particular sector, although Secretary of Education Arne Duncan has indicated he expects for-profit programs to fail to comply at a higher rate (Fain, 2014).

sectors, name recognition and personal affinity for particular schools, and employer beliefs about differences in pre-college student characteristics not included on the resume (e.g., family background).[14] The effects of listing no college relative to listing a for-profit college (or community college) could be driven by perceived human capital effects of postsecondary schooling (Becker, 1964) or by employers using postsecondary schooling as a signal of unobserved skill (Spence, 1973).

Our experiment captures the reduced-form effect of the education treatments and does not allow us to separately identify the influence of these various mechanisms.[15] However, the total effect identified by our research design is an important parameter. For instance, knowing if employers prefer workers who have postsecondary schooling as well as knowing whether college sector influences this preference would be valuable to students deciding whether and where to attend college. Similarly, policymakers evaluating regulations such as the proposed gainful employment rule, or deciding how to allocate marginal public investments, would benefit from knowing whether for-profit colleges generate better or worse labor market outcomes than community colleges or not attending college at all.

At the same time, it is important to recognize that our research design will produce estimates that do not capture some potential effects of for-profit college attendance. For example, any effects on skill differences that only become apparent to employers at the interview stage or

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[14] Whether differences in worker skills and backgrounds actually differ by college sector is an interesting question that cannot be addressed with our research design. However, to the extent that employers have imperfect information about a job applicant's skill at the time hiring decisions are made, initial employment and wage offers are likely to depend heavily on perceived skill differences. See Altonji and Pierret (2001) and Lange (2009) for empirical evidence on how quickly employers learn about worker productivity.

[15] This point is not specific to resume-based experiments. For instance, a hypothetical study that examined the impact of randomly assigning students to attend either a for-profit or community college on the likelihood of receiving an interview call-back would also not be able to identify the mechanisms driving any effects.

later, or effects that arise because of differences in the ability of colleges to link students to employers (e.g. through differences in the effectiveness of job-placement services), will not be reflected in our estimates. Our estimates will also exclude any effect of college sector that arises through differences in degree attainment across sectors, as discussed in previous research (e.g., Deming, Goldin, and Katz, 2012).[16] Finally, our estimates do not capture long-run effects of the educational treatments because our experiment is structured to capture effects that arise immediately after a job seeker finishes college. We return to these issues in more detail in Section 6 after we describe the experimental design and findings.

3.   **Experimental Design and Procedures**

In this section we briefly cover the procedures of the experiment. Appendix B provides additional procedural details.

3.1 Education Treatments

The resumes in the experiment indicate one of four education levels: (1) a high school diploma, (2) college coursework with no formal credential, (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 community college. The proportion of resumes we sent to employers is roughly even across the for-profit and community college sector, with a smaller number of resumes indicating no postsecondary experience (see Table 2 below). This allocation was chosen to maximize statistical power for the comparison between for-profit and community colleges while still

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[16] As we explain below, college sector and educational attainment level are orthogonal in our resumes so that the effect of college attainment does not confound the effect of college sector (and vice versa).

maintaining reasonable power to detect effects of the college treatments relative to high school, which we expected to be especially large given findings in the existing literature.

To maximize the chances that employers would be familiar with the colleges listed on the resumes, we used colleges with physical locations in each city.[17] We selected the colleges at random based on an enrollment-weighted selection probability from the list of institutions in the Integrated Postsecondary Education Data System (IPEDS), restricting the sampling to institutions that offered a sub-baccalaureate program that was relevant for one of the six occupational categories examined in the study. In each city we used about 14 public and for-profit colleges to populate the resumes. Each resume also lists a high school that was randomly selected from the primary urban public school district or a surrounding suburban district. 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.

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 educational treatments are more likely to influence outcomes for this group given that they have shorter and less informative work histories relative to older workers. This view is supported by research on employer learning which shows that the labor market learns

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[17] While we used only colleges with some brick-and-mortar presence in a given city, many of the colleges included offer both online and face-to-face instruction. The extent to which online instruction varies by college sector and the extent to which employers are aware of such a difference is part of the treatment effect our estimates capture.

about worker productivity quickly and educational signals are the most valuable early in a worker's career (Altonji and Pierret, 2001; Lange, 2007).

3.2 Labor Markets and Occupations

We sent resumes to job openings advertised online in the following seven metropolitan areas: Atlanta, Boston, Chicago, Houston, Philadelphia, Sacramento and Seattle. These cities represent a geographically diverse set of large urban areas in the United States. We focus on larger cities both because they have an ample supply of job advertisements and because they have a greater number of for-profit and community colleges than would be found in smaller cities. Using a larger number of institutions reduces the possibility that the idiosyncratic aspects of any one college drive our findings.

We sent resumes to positions in six broad occupational categories: administrative assisting, customer service, information technology, medical assisting (excluding nursing), medical billing/office and sales. In doing so, we designed the experiment to examine for-profit college effects for credentials in fields represented in the first three rows of Table 1. Several considerations went into this decision. First, we chose occupational categories for which the for-profit and community college sectors both offer a large number of relevant programs. We avoided occupations where for-profit colleges provide almost all (e.g., personal and culinary services) or almost no (e.g., liberal and general studies) sub-baccalaureate credentials. Second, we chose occupations for which there would be enough job advertisements to allow us to send a sufficient number of resumes. This is obviously important for generating data for the experiment and is also useful because it makes our study informative about the larger labor market into which students are entering. Finally, we wanted occupations that vary in the kinds of skills that they require. In particular, we wanted occupations for which the vocational training for-profit

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colleges purport to provide could be directly beneficial (information technology, medical assisting and medical billing/office) as well as occupations that have less emphasis on specific technical skills but are still reasonable target occupations for for-profit college attendees (administrative assisting, customer service and sales).

3.3 Other Resume Components

Aside from the educational treatments the most important section of the resumes is the work history.[18] The entries in each work history were constructed based on real resumes posted online by job seekers. The work histories include a combination of entry-level jobs related to the relevant occupational category and general low-skill 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 (see Table 2). 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.[19]

The names and contact information on the resumes were chosen so that job applicants would vary in terms of gender and likely ethnicity. We assigned addresses in 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 final section of each resume provides a list of randomly assigned general skills and qualifications for the applicant, again based on resumes posted by real job

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[18] The specific characteristics that comprise each resume were randomly assigned using computer software developed by Lahey and Beasley (2009). See Appendix B for further details.

[19] Not including any jobs obtained after college helps ensure that the education treatments are not diluted by work experience that an applicant acquired after finishing schooling. Another problem with listing randomly assigned post-college work experience on the resume is that in principle it should be endogenous to the education treatment.

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seekers in each occupational category, with resumes randomly varying as to whether they have such a section.

3.4 *Applying to Jobs and Recording Employer Responses*

We sent job applications to postings for positions we deemed suitable for inclusion in the study. We did not send resumes to jobs for which the applicant was clearly underqualified (e.g., database administrator with 7+ years of experience) and/or listed narrow 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).

One practical issue was that job advertisements were more abundant in some fields than others. Openings for which our applicants were reasonably qualified were more common in administrative assisting, customer service, medical billing/office and sales. The number of suitable advertisements in information technology and medical assisting was lower across all cities.[20] The discrepancy in suitable job advertisements across fields is an important aspect of the labor market for individuals at this skill level and is reflected in our data in the shares of applications sent to jobs in each occupational category (see the discussion of Table 2 below). That said, we did prioritize sending applications in response to job advertisements in medical assisting and information technology when they were available, so, if anything, our study over-represents these fields that require more specialized skills.

We sent resumes to advertisements between May 2013 and May 2014. For a given city, we began sending applications to job postings once the resumes for that city had been prepared. This resulted in variation across cities in the timing and intensity of data collection, which as we

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[20] Seattle is an exception for information technology, likely reflecting the rapid growth of the information technology industry in Seattle (Taylor, 2014).

describe below resulted in some cities being overrepresented in the data. Nonetheless, there was substantial time overlap across cities in terms of when the data were collected, and no one city appears to be driving our results (see Section 5.3).

We sent at most two resumes to each job advertisement. The resumes sent to the same employer were in different formats and had no overlap in resume characteristics so that employers would not see a resemblance between the resumes.[21] Employers responded to the resumes via email and phone and we generated two outcome variables based on their responses. The first is an indicator for the employer responding positively to the application (non-perfunctory) and the second is an indicator for the employer explicitly requesting an interview (interview requests are a subset of positive responses).

5. Empirical Analysis and Results

5.1 Descriptive Statistics

Tables 2-4 show descriptive statistics for the 8,914 resumes in our analytic sample. Tables 2 and 3 divide the data by city and Table 4 shows descriptive statistics by the primary educational treatment conditions (for-profit, community college, high-school only). Beginning with Table 2, over 40 percent of the resumes have a one-year work-history gap and an additional 13 percent have a two-year gap (recall that these are young workers and many of them have concurrent schooling). Most resumes have 1-2 years of work experience in the relevant occupation. 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.

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[21] 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.

Table 3 shows response rates and interview-request rates across occupations and cities. The overall response rate is 11.4 percent, and 4.9 percent of applicants received an interview request. Prior resume field experiments indicate response rates in the range of 8-12 percent, with interview request rates of 3-5 percent (Oreopoulos, 2011; Hinrichs, 2013; Koedel and Tyhurst, 2012; Kroft, Lange, & Notowidigdo, 2013; Lahey, 2008). 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 in information technology 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.

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. This indicates that the randomization was implemented successfully.

5.2   Results

Table 5 shows raw response rates and interview request rates by treatment condition. Responses and interview requests are highest for resumes listing community college and lowest for resumes listing no college. However, none of the differences across treatments in Table 5 are statistically significant.

Table 6 shows estimated marginal impacts of listing a public community college or no postsecondary experience on the resume, relative to listing a for-profit college, based on logistic regressions where the dependent variable is a positive employer response. Table 7 shows analogous results when the dependent variable is an interview request. 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.[22] 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. 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). [23]

Focusing first on our primary comparison between for-profit and public colleges, the results in Tables 6 and 7 provide no indication that employers prefer applicants who attended for-profit colleges. In fact, all of the point estimates suggest employers prefer applicants from community colleges, although none are statistically significant. The point estimates from the richest specification in Table 6 are about 0.004 percentage points, or 3.5 percent of the sample mean. The point estimates for the interview-request models in Table 7 are about 0.004-0.006, or 8-12 percent of the sample mean. For both outcomes, the results are not very sensitive to whether the cities are weighted equally, although the estimates are somewhat more precise when the city weights are not used.

Crucially, we have sufficient statistical power to rule out all but very small negative effects of community college relative to for-profit college. For the employer response models in

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[22] Estimates of the effects of the other resume characteristics in Tables 6 and 7 can be found in Appendix Table A.1.

[23] 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 underrepresented in the raw data because it was the last city in which we began collecting data.

Table 6, the lower bound of the 95 percent confidence interval in the model with the most detailed set of baseline covariates is about -.007 percentage points, or 6 percent of the sample mean. When we examine interview requests in Table 7, we can rule out negative community college effects below-0.002 (4 percent of the sample mean). To assess the magnitudes of these estimates it is useful to compare them to estimated effects of other resume characteristics found in previous resume-based audit studies. To take two examples from the discrimination literature, Bertrand and Mullainathan (2004) find that applicants with white-sounding names receive 50 percent more call-backs than those with African American-sounding names, and Lahey (2008) finds that younger workers receive 40 percent more call-backs than older workers. We can rule out positive for-profit effects (relative to community college) that are much smaller than these effect sizes despite the fact that for-profit colleges cost substantially more than community colleges.

Turning to the comparisons between for-profit college attendees and high-school graduates, we again find no evidence that job applicants benefit from attending a for-profit college. The point estimates are all small and statistically insignificant. Estimates from the positive employer response models in Table 6 with the richest set of controls are between -0.0016 and -0.0032, or 2-3 percent of the sample mean, depending on whether we apply the city weights. We can rule out positive for-profit effects on employer responses of about 2.0 percentage points, or 17.5 percent of the sample mean. For the interview-request models the point estimates are not consistent in sign; if taken at face value, the city-weighted models imply that the resumes without postsecondary experience fare better. We can rule out positive for-profit effects larger than about 1.2 percentage points, or 24 percent of the sample mean, in the fully-specified models.

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Finally, we examine whether resumes listing community colleges elicit more call-backs than resumes listing no college experience. In the employer-response and interview-request models, the estimates of the community college effect are consistently positive but not statistically significant. The estimates for positive employer responses range from 0.006 – 0.008 percentage points in Table 6, or 5-7 percent of the mean response rate. For interview requests, the estimates as a percent of the sample mean are a little larger (8-14 percent). Using the most precisely estimated coefficients from the employer-response and interview-request models, we can rule out community college effects larger than about 20 and 30 percent of the sample mean, respectively.

5.3. Sensitivity Analysis

The results in Tables 6 and 7 pool the educational attainment levels for applicants who attended college. This is done to maximize power for detecting for-profit effects and also because it reflects the fact that students who enter two-year colleges leave with a variety of credentials, and most do not earn an associate degree (NCES, 2012). Nonetheless, it is still interesting to examine for-profit effects specific to particular education levels. Results from a model with interactions between the education levels and for-profit college status are reported in Table 8. The table shows results from models that split out the effects of each credential-by-sector treatment in the data (there are seven – see Table 2).[24] The omitted comparison group is an associate degree from for-profit college.

Although the estimated effects in Table 8 are too small to be statistically significant, it does appear that job applicants with public-college credentials nominally outperform applicants

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[24] Table 8 and the other tables that examine the sensitivity of our findings use model 2 as the baseline specification. As illustrated in Tables 6 and 7, our findings are not qualitatively sensitive to which model we use – Table A.1 shows the coefficients for the other control variables from model 2 as estimated for Tables 6 and 7.

with for-profit credentials within each education level. Moreover, no particular education credential establishes itself as clearly preferred to the others. This last finding is important as it suggest that our findings would not differ substantively to what we report in Tables 6 and 7 if we chose to re-weight the data so that the educational-level shares would be different from what they are in the raw data. Similarly, it suggests that the results would not be different if we only focused on a single level of attainment (e.g., applicants who completed an associate degree).

Next we look for heterogeneity in our findings across occupations with the caveat that we do not have sufficient statistical power to detect a moderately-sized effect of for-profit college attendance in any particular occupational group (we did not design the experiment with this goal in mind). 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 Tables A.4 and A.5 we verify that our findings are not sensitive to excluding data from a particular city or occupation by estimating models that leave out data from one city (Table A.4) and one occupation (Table A.5) in turn. In addition to verifying the general robustness of our findings, Table A.5 helps to indirectly address a potential limitation of our study 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, and thus our findings may not be broadly representative of the field. 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. Appendix Table A.5 shows that our findings are not qualitatively sensitive to omitting data from the medical-assisting field or any other field. Although this does not resolve any potential limitations related to our partial coverage of the medical assisting field, it does suggest that our primary findings are not unduly affected by the medical-assisting resumes, and thus at the very least they are applicable for the other fields in the experiment.

Finally, in results omitted for brevity we considered the possibility that for-profit college attendance interacts with field-relevant work experience. As shown in Appendix Table A.1, field-relevant work experience is one of the strongest independent determinants of employer interest in our applicants. We find no evidence that field-relevant work experience affects the labor market returns to for-profit college attendance.

6.   Discussion

6.1   Effects of For-Profit Colleges Relative to Community College

Our results provide no indication that resumes that list for-profit college credentials generate more employer interest than those that list community college credentials. If anything, the opposite may be true. A simple explanation for this result is that job applicants who attended for-profit and community colleges who otherwise have similar characteristics do not

systematically differ in skills valued by employers. This interpretation is consistent with several recent non-experimental studies that find that the earnings returns to for-profit college attendance are equal to or lower than the returns to attending public community college (Deming, Goldin and Katz, 2012; Cellini and Chaudhary, 2012; Lang and Weinstein, 2013). If true, this would have the important implication that the higher cost associated with for-profit colleges results in little labor market payoff.

However, other explanations could also account for our findings. One possibility is that employers are simply unaware of differences in quality across sectors. If this is the case then it may be possible for for-profit college attendance to affect worker productivity, thereby improving wage and employment outcomes, without the effect showing up in initial employer responses. We cannot rule out this explanation empirically. However, it is worth noting that the amount of information that employers have with regard to which institutions are for-profit and public colleges, and their expectations regarding skill accumulation across institutions in each sector, represents an equilibrium outcome. In particular, employers would benefit from knowing of the existence of large skill differences between workers who attended for-profit and community colleges, and the absence of effects of college sector on employer responses to job applications suggests such differences may be small or nonexistent. This interpretation is also consistent with a survey of employers by Hagelskamp, Schleifer, and DiStasi (2014) showing that employers either perceive few differences between for-profit and community colleges, or view community colleges as more effective at preparing students.

Another issue is that our research design is only relevant for jobs posted on online job search sites, and misses effects for jobs filled through referrals or with direct job-placement assistance from the college. While such linkages with employers are emphasized in the

marketing materials used by some for-profit colleges, there is no evidence to indicate that for-profit colleges actually offer more-effective career placement services. Deming, Goldin and Katz (2013) express skepticism of the claim that for-profit colleges offer superior student services by noting that for-profit college students have lower levels of satisfaction with their programs than comparable students who attend non-profit institutions. Moreover, we were able to identify a large number of suitable job postings and the call-back rates in our study are in line with those seen in other audit studies, which suggests that online job boards are no less relevant for the applicants in our study than in other studies using the same research design.

Our estimates fail to capture any effect of for-profit college attendance operating through different degree completion rates across sectors. This is because college sector and attainment are orthogonal in our experiment. In fact, it does appear that for-profit college students are more likely to complete sub-baccalaureate programs than students in public community colleges (Deming, Katz and Goldin, 2012). However, the evidence in Table 8 reveals no clear payoff to completing an associate degree relative to earning only a vocational certificate or leaving college without a credential. This suggests that any benefit of for-profit college attendance in terms of a higher likelihood of earning a degree may have limited labor market benefits, at least at the sub-baccalaureate level. Furthermore, the differences in observed degree completion rates across sectors are difficult to interpret. They may reflect differences in unobserved student characteristics, less rigorous programs in for-profit colleges, and/or differences in student aspirations across sectors (in particular, the fact that community-college students are much more likely to transfer to a 4-year college).[25]

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[25] 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

A final possibility is that there may be larger effects of for-profit colleges in other occupations and for different kinds of workers than were used in this experiment. For instance, it may be that for certain specialized occupations, for-profit colleges provide stronger instruction and have better ties to employers than public community colleges. Against this claim, though, some of the occupations we examine do require technical training and as noted earlier, are in fields in which for-profit colleges are well-represented. Furthermore, observational evidence reported in Deming, Goldin and Katz (2013) suggests that students who attended for-profit colleges have worse labor market outcomes than community college students even when they pursue programs in rapidly-growing industries requiring specialized training such as allied health.

In terms of the types of workers in our study, probably the most serious threat to external validity is that we used resumes only of young workers. However, while a sizable share of 2-year college students are adults who return to school after a period of work, a large proportion of students in both for-profit colleges and public community colleges are quite young (nearly 50 percent and 60 percent of students, respectively, are under the age of 25; see NCES (2013a)). Moreover, if anything, the effect of listing a for-profit college is likely to be stronger for younger workers given research showing that educational signals are strongest early in a worker's career (Lange, 2007; Altonji and Pierret, 2001).

To summarize, while we cannot rule out several alternative possibilities, a plausible explanation for our findings is that workers who attended for-profit colleges are no more likely

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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 transfers being more common in the public sector (Marcus, 2012; also see Mullin, 2012).

to possess skills demanded by employers than are workers who attended much less costly community colleges.

6.2   Effects of For-Profit and Community College Relative to No Postsecondary Schooling

Our results also suggest that job applicants with for-profit college experience draw no more interest from employers than those with only a high school diploma. Similarly, community college experience generates only a small, statistically insignificant advantage over no college.[26] These results are surprising given the large non-experimental literature documenting the returns to education in general (Card, 1999; Oreopolous and Petronijevic, 2013), and specifically to sub-baccalaureate education (Kane and Rouse, 1995; 1999; Jepsen, Troske, and Coomes, 2001). However, there are several possible explanations for the apparent discrepancy, which we discuss now.

One simple explanation is that our findings are not inconsistent with meaningful effects of sub-baccalaureate schooling, even though our point estimates are not statistically significant. This explanation is less compelling for the estimated effect of listing a for-profit college relative to high school, where our point estimates are close to zero and inconsistent in sign across specifications. But for the comparison between community college and no college, our point estimates are consistently positive and the confidence intervals include somewhat larger effect sizes than in the comparison between high school and for-profit college.

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[26] One caveat to the community-college versus no-college comparison is that the results only pertain to vocational sub-baccalaureate programs because our resumes do not list any experience in "academic" programs. This distinction is important for community colleges since a key part of their mission is offering programs to help students transfer to university-level higher education. Any return to such programs that operates through allowing students to transfer to a four-year college is not reflected in our estimates. On the other hand, this issue is less important for for-profit colleges because, as noted above, they generally specialize in vocational instruction. See Andrews, Li and Lovenheim (2014) for a discussion of the returns to university-level degrees among community-college transfers.

It is also possible that our findings are an artifact of the particular labor markets we chose to examine (i.e., the occupations and the types of job listings to which we sent applications). In particular, if these labor markets are characterized by jobs that primarily require only rudimentary skills for which postsecondary schooling is not valuable, then we may be missing effects that exist in other labor markets. However, this claim does not appear to be borne out empirically. Appendix Table A.6 shows tabulations from the American Community Survey, which indicate that the most common education level for the occupational categories used in this study is "some college/associate degree."[27] For all occupations except information technology, the second most common educational level is "high school degree only." This suggests that when thinking about the return to postsecondary schooling for the occupations we examine, the most relevant margin is likely to be between high school and sub-baccalaureate education.

Perhaps the most important alternative interpretation is that our estimates only reflect impacts shortly after a job applicant would have completed her schooling. Standard human capital theory predicts that investments in schooling will not immediately lead to higher wages because workers who invest in schooling will be competing with workers who have acquired greater work experience (Mincer, 1974). Thus, the effects we estimate may miss returns to schooling that materialize in the future. To guard against work-experience differences confounding the educational attainment effects we constructed the work histories so that educational attainment and the work histories would be orthogonal. However, it is possible that

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[27] These data are from the American Community Survey 5-year estimates (specifically the EEO-ALL08W 2006-2010 tabulation available from the American FactFinder http://factfinder2.census.gov/faces/nav/jsf/pages/index.xhtml). Occupational categories include detailed occupation groups from the Bureau of Labor Statistics (see http://www.bls.gov/soc/home.htm) that reflect the types of jobs to which we applied.

employers discount work experience listed on a resume concurrent with schooling, which would work against college-goers in our experiment.

To summarize, our results suggest that sub-baccalaureate vocational schooling in either a for-profit or community college does not have a large labor market payoff. However, we are hesitant to interpret this result too strongly because (a) we cannot statistically rule out modest effects of sub-baccalaurate postsecondary schooling, particularly from community college, and (b) there may be longer-run returns to both for-profit and community college attendance that our experiment is not designed to capture.

7. Conclusion

The for-profit college sector in the United States has experienced remarkable growth in recent years. 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). Given their rising prominence, high tuition costs, dependence on federal subsidies, and unique student demographic; for-profit colleges are facing increasing scrutiny. Recent high-profile government reports have been critical of for-profit colleges (U.S. Government Accountability Office, 2010; U.S. Senate Committee on Health, Education, Labor and Pensions, 2012), and concerns about their efficacy are embodied in the recently-proposed "gainful employment" rule by the United States Department of Education.

This paper contributes to the understanding of how for-profit colleges affect labor market outcomes by presenting experimental evidence on the impact of listing for-profit college credentials on a resume. We find little evidence that job applicants with for-profit college credentials attract greater interest from employers than those with credentials from a public community college or no college at all. These findings are particularly noteworthy considering

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the high cost of for-profit college attendance. Our results complement a growing non-experimental literature, which also suggests that for-profit college attendance offers limited labor-market benefits to students.

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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	0.50	0.48	0.50	0.47	0.50
African American	0.32	0.32	0.31	0.32	0.33	0.32	0.33	0.32
Hispanic	0.34	0.34	0.35	0.36	0.36	0.35	0.33	0.33
High-school graduate	0.14	0.13	0.15	0.13	0.11	0.16	0.15	0.16
Community College: Some College	0.14	0.14	0.15	0.14	0.15	0.14	0.14	0.15
For Profit: Some College	0.15	0.15	0.15	0.14	0.15	0.15	0.16	0.18
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	0.13
Community College: AA Degree	0.14	0.15	0.14	0.14	0.14	0.14	0.12	0.13
For Profit: Some AA Degree	0.15	0.14	0.15	0.14	0.18	0.15	0.15	0.15
1-Year Work Experience (2-Year Gap)	0.13	0.12	0.14	0.13	0.12	0.12	0.11	0.15
2-Years Work Experience (1-Year Gap)	0.43	0.42	0.43	0.46	0.43	0.42	0.41	0.42
3-Years Work Experience (No Gap)	0.44	0.46	0.43	0.41	0.45	0.46	0.48	0.43
No Relevant Work Experience	0.12	0.07	0.15	0.16	0.19	0.11	0.08	0.15
1-Year Relevant Work Experience	0.35	0.32	0.39	0.35	0.33	0.36	0.31	0.39
2-Years Relevant Work Experience	0.38	0.43	0.34	0.36	0.34	0.39	0.38	0.35
3-Years Relevant Work Experience	0.15	0.18	0.12	0.13	0.14	0.14	0.23	0.11
Admin Share	0.23	0.23	0.27	0.27	0.19	0.20	0.20	0.21
Customer Service Share	0.19	0.17	0.19	0.23	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	0.18
Medical Assisting Share	0.12	0.13	0.08	0.10	0.13	0.14	0.17	0.11
Medical Billing/Office Share	0.15	0.14	0.15	0.12	0.18	0.16	0.16	0.15
Sales Share	0.21	0.21	0.22	0.18	0.20	0.22	0.22	0.19
Total Resumes	8914	1637	1592	1368	468	1800	1280	769
Total Unique Job Advertisements	5209	992	943	787	354	1012	702	419

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.

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

	All	Atlanta	Boston	Chicago	Houston	Philadelphia	Sacramento	Seattle
Response Rate (RR)	0.114	0.057	0.141	0.083	0.139	0.119	0.131	0.176
RR: Admin	0.050	0.018	0.076	0.030	0.069	0.040	0.054	0.106
RR: Customer Service	0.131	0.076	0.162	0.100	0.145	0.130	0.117	0.266
RR: Information Technology	0.120	0.055	0.190	0.119	0.154	0.135	0.165	0.074
RR: Medical Assisting	0.087	0.024	0.049	0.015	0.065	0.099	0.165	0.184
RR: Medical Billing/Office	0.056	0.027	0.110	0.019	0.059	0.045	0.062	0.070
RR: Sales	0.222	0.125	0.235	0.200	0.312	0.241	0.229	0.347
Interview Request Rate (IRR)	0.049	0.029	0.056	0.031	0.066	0.053	0.048	0.088
IRR: Admin	0.021	0.007	0.018	0.016	0.023	0.014	0.019	0.094
IRR: Customer Service	0.060	0.047	0.078	0.028	0.053	0.064	0.059	0.125
IRR: Information Technology	0.042	0.015	0.054	0.071	0.031	0.054	0.059	0.022
IRR: Medical Assisting	0.027	0.005	0.025	0.000	0.033	0.032	0.063	0.023
IRR: Medical Billing/Office	0.026	0.014	0.055	0.006	0.035	0.017	0.024	0.035
IRR: Sales	0.102	0.073	0.096	0.071	0.194	0.121	0.068	0.194

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

	For-profit	Community College	High School
Female	0.51	0.49	0.47
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
1-Year Work Experience (2-Year Gap)	0.12	0.13	0.12
2-Years Work Experience (1-Year Gap)	0.43	0.43	0.43
3-Years Work Experience (No Gap)	0.45	0.44	0.45
No Relevant Work Experience	0.12	0.12	0.12
1-Year Relevant Work Experience	0.35	0.35	0.35
2-Years Relevant Work Experience	0.38	0.38	0.37
3-Years Relevant Work Experience	0.16	0.15	0.16
Admin Share	0.23	0.24	0.22
Customer Service Share	0.19	0.18	0.20
Information Technology Share	0.11	0.11	0.09
Medical Assisting Share	0.12	0.12	0.14
Medical Billing/Office Share	0.15	0.15	0.16
Sales Share	0.21	0.21	0.20
Total Resumes	3883	3752	1279

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	0.113	0.116	0.106
Employer Interview Request Rate	0.047	0.053	0.042
Total Resumes	3883	3752	1279

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	
Public Community College	0.0039	0.0052	0.0041	0.0053	0.0035	0.0045
	(0.0053)	(0.0063)	(0.0053)	(0.0063)	(0.0050)	(0.0060)
% of Sample Mean	3.4%	4.6%	3.7%	4.6%	3.1%	3.9%
Lower Bound of 95% CI	-0.0065	-0.0074	-0.0062	-0.0070	-0.0063	-0.0073
High School	-0.0039	-0.0021	-0.0038	-0.0025	-0.0032	-0.0016
	(0.0084)	(0.0100)	(0.0083)	(0.0098)	(0.0079)	(0.0094)
% of Sample Mean	-3.4%	-1.8%	-3.3%	-2.2%	-2.8%	-1.4%
Lower Bound of 95% CI	-0.0204	-0.0217	-0.0201	-0.0217	-0.0187	-0.0200
Public CC – HS	0.0078	0.0073	0.0080	0.0078	0.0067	0.0061
% of Sample Mean	6.8%	6.4%	7.0%	6.8%	5.9%	5.4%
P-value for *Public CC = HS*	0.38	0.48	0.36	0.44	0.42	0.52
Equal City Weights	NO	YES	NO	YES	NO	YES
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	8914	8914	8914	8914	8914	8914

** 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 for-profit 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. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels. Appendix Table A.1 reports coefficients for the control variables from Model 2.

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

	Model 1		Model 2		Model 3	
Public Community College	0.0041	0.0064	0.0041	0.0059	0.0039	0.0056
	(0.0034)	(0.0042)	(0.0033)	(0.0041)	(0.0030)	(0.0037)
% of Sample Mean	8.4%	13.1%	8.4%	12.0%	8.0%	11.4%
Lower Bound of 95% CI	-0.0026	-0.0020	-0.0024	-0.0021	-0.0020	-0.0017
High School	-0.0022	0.0023	-0.0026	0.0013	-0.0023	0.0011
	(0.0056)	(0.0071)	(0.0054)	(0.0067)	(0.0048)	(0.0060)
% of Sample Mean	-4.5%	4.7%	-5.3%	2.7%	-4.7%	2.2%
Lower Bound of 95% CI	-0.0132	-0.0116	-0.0132	-0.0118	-0.0117	-0.0107
Public CC – HS	0.0063	0.0041	0.0067	0.0046	0.0062	0.0045
% of Sample Mean	12.9%	8.4%	13.7%	9.4%	12.7%	9.2%
P-value for *Public CC = HS*	0.27	0.56	0.23	0.49	0.23	0.45
Equal City Weights	NO	YES	NO	YES	NO	YES
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	8914	8914	8914	8914	8777	8777

** 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 for-profit 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. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels. Appendix Table A.1 reports coefficients for the control variables from Model 2. In the final two columns 137 observations are dropped because their industry-by-occupation cell perfectly predicts failure (Chicago, medical assisting; see Table 3).

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	
High school	-0.0081	-0.0098	-0.0009	0.0023
	(0.0099)	(0.0115)	(0.0070)	(0.0086)
Public CC Coursework	-0.0047	-0.0096	0.0082	0.0099
	(0.0101)	(0.0115)	(0.0075)	(0.0090)
For-Profit Coursework	-0.0106	-0.0135	0.0020	0.0027
	(0.0098)	(0.0113)	(0.0070)	(0.0084)
Public CC Certificate	0.0007	0.0013	0.0037	0.0052
	(0.0099)	(0.0116)	(0.0066)	(0.0081)
For-Profit Certificate	-0.0030	-0.0094	0.0035	0.0004
	(0.0104)	(0.0117)	(0.0074)	(0.0083)
Public CC AA Degree	0.0021	0.0003	0.0065	0.0070
	(0.0102)	(0.0121)	(0.0074)	(0.0088)
Equal City Weights	NO	YES	NO	YES
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	8914	8914	8914	8914

Notes: The omitted treatment is an associate degree from a for-profit 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. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels.

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)
Public Community College	0.0041	0.0041
	(0.0053)	(0.0033)
High School	-0.0038	-0.0026
	(0.0083)	(0.0054)
Basic Application Details		
Positive Greeting	0.0041	-0.0001
	(0.0049)	(0.0031)
First Resume	0.0147	0.0083
	(0.0051)**	(0.0032)**
Applicant Race/Gender (as implied by name)		
African American Female	0.0015	-0.0085
	(0.0096)	(0.0052)
African American Male	-0.0098	-0.0069
	(0.0092)	(0.0053)
Hispanic Female	-0.0025	-0.0071
	(0.0095)	(0.0053)
Hispanic Male	-0.0012	-0.0034
	(0.0089)	(0.0051)
White Female	0.0091	-0.0022
	(0.0100)	(0.0055)
Work History (categories are mutually exclusive)		
2-Years Work Experience (1-Year Gap)	0.0182	0.0018
	(0.0111)*	(0.0066)
3-Years Work Experience (No Gap)	0.0200	0.0019
	(0.0115)*	(0.0072)
1-Year Relevant Work Experience	0.0257	0.0091
	(0.0108)**	(0.0067)
2-Years Relevant Work Experience	0.0326	0.0109
	(0.0115)**	(0.0072)
3-Years Relevant Work Experience	0.0340	0.0217
	(0.0157)**	(0.0113)*
Occupational Category		
Administrative	-0.105	-0.0436
	(0.0061)**	(0.0039)**
Customer Service	-0.0470	-0.0187
	(0.0067)**	(0.0040)**
Information Technology	-0.0514	-0.0261
	(0.0071)**	(0.0038)**
Medical Assisting	-0.0663	-0.0327
	(0.0063)**	(0.0037)**
Medical Billing/Office	-0.0090	-0.0359

	(0.0056)**	(0.0035)**
<u>City</u>		
Boston	0.1305	0.0350
	(0.0224)**	(0.0122)**
Chicago	0.0679	0.0364
	(0.0324)	(0.0239)
Houston	0.0465	0.0275
	(0.0543)	(0.0401)
Philadelphia	0.0935	0.0326
	(0.0202)**	(0.0124)**
Sacramento	0.1286	0.0407
	(0.0318)**	(0.0200)**
Seattle	0.2163	0.1400
	(0.0566)**	(0.0555)**

** 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: Standard errors are clustered by job posting. The marginal effects for the control variables are qualitatively similar with and without weighting. Time trend coefficients are omitted for brevity. Omitted groups are for-profit college, less-positive greeting, second resume, white male, 2-year work history gap, no relevant work experience, 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	
Public Community College	0.0046	0.0052	0.0013	-0.0001
	(0.0076)	(0.0083)	(0.0048)	(0.0054)
High School	0.0075	0.0084	0.0033	0.0019
	(0.0122)	(0.0137)	(0.0078)	(0.0085)
P-value for *Public CC = HS*	0.82	0.82	0.81	0.81
Equal City Weights	NO	YES	NO	YES
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	3358	3358	3226	3226

Notes: The omitted treatment is for-profit 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. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels. No interview requests were obtained in for these occupational categories during one month when limited data were being collected (early on in the experiment); as a result 132 observations were dropped from the interview-request model.

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
Public Community College	0.0037	0.0039	0.0053	0.0101
	(0.0070)	(0.0089)	(0.0044)	(0.0605)
High School	-0.0084	-0.0101	-0.0044	0.0032
	(0.0110)	(0.0135)	(0.0072)	(0.0103)
P-value for *Public CC = HS*	0.30	0.32	0.20	0.48
Equal City Weights	NO	YES	NO	YES
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	5556	5556	5556	5556

Notes: The omitted treatment is for-profit 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. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels.

Table A.4 Sensitivity of Findings to Leaving out Data from Each City Individually. Dependent Variable is Any Response. Results shown for Model 2 Without City Weights.

	Full Sample (from Table 6)	Omit Data from City:						
		Atlanta	Boston	Chicago	Houston	Philadelphia	Sacramento	Seattle
Public Community College	0.0042	0.0039	0.0069	0.0027	0.0030	0.0027	0.0042	0.0055
	(0.0053)	(0.0063)	(0.0055)	(0.0059)	(0.0054)	(0.0058)	(0.0055)	(0.0054)
High School	-0.0038	-0.0034	-0.0017	-0.0054	-0.0035	-0.0002	-0.0088	-0.0045
	(0.0083)	(0.0100)	(0.0089)	(0.0092)	(0.0084)	(0.0094)	(0.0085)	(0.0084)
P-value for *Public CC = HS*	0.36	0.49	0.44	0.41	0.47	0.76	0.15	0.26
Equal City Weights	NO	NO	NO	NO	NO	NO	NO	NO
Basic Application Details	X	X	X	X	X	X	X	X
City Indicators	X	X	X	X	X	X	X	X
Occupation Indicators	X	X	X	X	X	X	X	X
City-by-Occupation Indicators								
Flexible Time Trend	X	X	X	X	X	X	X	X
Race & Gender	X	X	X	X	X	X	X	X
Exact Name Indicators								
Basic Work History	X	X	X	X	X	X	X	X
Address and High School								
N	8914	7277	7322	7546	8446	7114	7634	8145

Notes: The omitted treatment is for-profit college. Standard errors are reported in parentheses and clustered by job posting. Most postings received two resumes. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels. Parallel results for the interview-request model also show that the findings presented in the text are robust to omitting data from each city in turn (results omitted for brevity).

Table A.5 Sensitivity of Findings to Leaving out Data from Each Occupational Category Individually. Dependent Variable is Any Response. Results shown for Model 2 Without City Weights.

	Full Sample (from Table 6)	Omit Data from Occupation:					
		Administrative Assisting	Customer Service	Information Technology	Medical Assisting	Medical Billing/Office	Sales
Public Community College	0.0042	0.0033	0.0039	0.0043	0.0021	0.0059	0.0056
	(0.0053)	(0.0067)	(0.0055)	(0.0055)	(0.0056)	(0.0060)	(0.0054)
High School	-0.0038	-0.0028	-0.0030	-0.0015	-0.0111	-0.0023	-0.0008
	(0.0083)	(0.0105)	(0.0089)	(0.0086)	(0.0086)	(0.0096)	(0.0084)
P-value for *Public CC = HS*	0.36	0.58	0.46	0.52	0.16	0.41	0.46
Equal City Weights	NO	NO	NO	NO	NO	NO	NO
Basic Application Details	X	X	X	X	X	X	X
City Indicators	X	X	X	X	X	X	X
Occupation Indicators	X	X	X	X	X	X	X
City-by-Occupation Indicators							
Flexible Time Trend	X	X	X	X	X	X	X
Race & Gender	X	X	X	X	X	X	X
Exact Name Indicators							
Basic Work History	X	X	X	X	X	X	X
Address and High School							
N	8914	6867	7253	7970	7822	7592	7066

Notes: The omitted treatment is for-profit college. Standard errors are reported in parentheses and clustered by job posting. Most postings received two resumes. Basic application details include whether the resume was the first or second resume sent and whether it came with a (marginally) more-positive greeting from the applicant. The flexible time trend includes indicators for one-month timespans over the course of the experiment. The basic work history includes indicators for general and occupation-specific experience levels. Parallel results for the interview-request model also show that the findings presented in the text are robust to omitting data from each occupation in turn (results omitted for brevity).

Appendix Table A.6. Educational Attainment Level by Occupational Category in the American Community Survey.

Occupation Category	Less than High School	High School Diploma Only	Some College/ Associate Degree	Bachelor Degree	Postgraduate Degree
Medical Office/Billing	4%	32%	51%	11%	2%
Medical Assisting	3%	25%	63%	7%	2%
Information Technology	1%	10%	43%	36%	11%
Customer Service	7%	29%	45%	17%	2%
Sales	14%	29%	36%	18%	3%
Office Administration	4%	31%	48%	14%	3%

Source: EEO-ALL08W Tabulation 2006-2010 (American Community Survey 5-year estimates)
Notes: Occupation categories listed in the table include only detailed occupations from the Bureau of Labor Statistics Standard Occupational Classification system (see http://www.bls.gov/soc/home.htm) that reflect the types of jobs for which we applied.

Appendix B
Experiment Details

B.1 Resume Construction

In this section we elaborate on how the resumes were constructed for the experiment. As noted in the text, computer software developed by Lahey and Beasley (2009) was used to generate a large bank of randomly-generated resumes. 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 and three male-sounding first names. Only the first names for the Hispanic applicants indicate racial/ethnic origin. Last names were chosen to indicate that the 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.[28]

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

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[28] 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 may 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.

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. High schools were chosen from the primary urban public school district as well as from surrounding suburban districts. We selected schools with demographically diverse student bodies and with average statewide test scores in the middle or fourth quintile. As noted in the text, 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.[29]

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 that are relevant to the occupational category and general low-skilled jobs (e.g., retail clerk). Similarly to previous audit studies, we constructed some resumes with work-history gaps (e.g., see Bertrand and Mullainathan, 2004; Lahey, 2008).

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

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[29] 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, this depends on the college.

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.

B.2 *Applying to Jobs and Recording Employer Responses*

In this section we elaborate on the procedures we used to apply to jobs and track

responses. In selecting appropriate job advertisements, in addition to avoiding jobs for which the

applicant was clearly underqualified and/or listed narrow skills that were not conveyed by any of

our resumes, we also trained research assistants to use their judgment to avoid job postings that

were unlikely to be credible – for example, sales jobs promising substantial earnings for limited

work. We also avoided sending resumes to recruiters to the extent possible.

We sent up to two resumes to each employer. The resume sampling procedure was

structured to ensure that the resumes were in a different format and had no overlapping

information. 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 (Appendix Table A.1 shows

that second resumes received fewer employer responses). The ratio of resumes to job postings in

each city in Table 2 is always less than two because the random resume generator sometimes

produced resumes with errors and when the second resume in a sampled pair had an error, we

sent just the first resume (when the first resume had an error, we re-sampled).[30]

Employers responded to the resumes via email and phone. Phone calls were sent to

voicemail. The "any response" outcome variable was coded as 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 outcome variable was

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[30] The errors were related to the construction of the work histories and owing to the fact that we sampled jobs with replacement. More information about this procedural issue is available from the authors upon request.

coded as 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.