Dear Professor Hoxby: We would like to apologize about this draft being a little rough. By the time of the spring meeting in May we will have a much cleaner manuscript. Also, we will have incorporated 2,000 to 3,000 additional data points and two additional cities into our experiment (data collection is already underway in these cities), so the results in this draft are subject to some revision. If the schedule for the spring meeting fills up, we would also like to be considered for the Summer Institute. Thanks for reviewing our paper.

***** DRAFT: PLEASE DO NOT CITE OR CIRCULATE WITHOUT PERMISSION *****

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

Rajeev Darolia
University of Missouri

Cory Koedel
University of Missouri

Paco Martorell
RAND

Katie Wilson
RAND

March 2014

We perform a resume-based field experiment to examine employer preferences for job applicants who attend for-profit colleges. Our experiment compares applicants who are randomly assigned sub-baccalaureate credentials from for-profit and public postsecondary institutions. We find no evidence to suggest that employers prefer applicants who attend for-profit colleges over their public-college competitors. In fact, if anything, our results point to employers preferring applicants from public colleges. We also compare applicants who attend for-profit and public colleges to applicants who do not attend college at all. Applicants who attend public colleges outperform applicants without any postsecondary education, but we cannot statistically distinguish between applicants who attend for-profit colleges and those without higher education experience.

Acknowledgements
The authors thank Scott Delhommer, Jared Dey, Lucas Singer, Trey Sprick and David Vaughn for research assistance. They thank the Spencer Foundation and the Economic and Policy Analysis Research Center at the University of Missouri for research support.

1. Introduction

Enrollment in for-profit colleges has more than tripled in the past decade, while enrollment in public and private non-profit colleges grew by less than 30 percent over the same time period (National Center for Education Statistics (NCES), 2013a). For-profit colleges are particularly popular at the sub-baccalaureate level, accounting for almost one-third of associate's degrees and certificates conferred (NCES, 2013a). Students attending for-profit colleges receive a disproportionate share of federal financial aid. For example, although for-profit students accounted for just 12 percent of total postsecondary enrollment in 2012-2013, they accrued 21 percent of federal Pell Grant and subsidized student loan disbursements (Baum and Payea, 2013).[1]

One perspective views the growth of the for-profit sector in higher education as a market response to educational needs that are unmet by public and private non-profit colleges. Indeed, for-profit institutions market themselves as offering educational opportunities tailored to students seeking skills and training with direct labor market applications (Bailey, Badway, and Gumport, 2001; also see Gilpin, Saunders and Stoddard, 2013). However, for-profit colleges have been criticized of late for providing a high-cost, low-quality education (Golden, 2010a; 2010b; 2010c; Goodman, 2010), and prominent federal agencies have released reports that are critical of the practices of for-profit colleges (U.S. Government Accountability Office, 2010; U.S. Senate Committee on Health, Education, Labor and Pensions, 2012).[2] These criticisms have motivated proposals to strengthen regulation and oversight of the for-profit sector and drawn public attention to the issue of whether students benefit from for-profit college attendance. Despite the

[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] Hoxby and Avery (2012) estimate that for-profit 2-year colleges have considerably lower per-pupil expenditures than their public counterparts.

attention for-profit colleges have drawn from policymakers, the media, and the public at large, there are relatively few studies that provide empirical evidence on the labor-market returns to attending a for-profit college.

We contribute to the literature by using a resume-based field experiment to determine whether employers prefer job applicants who attend for-profit colleges. In the experiment we randomly assign for-profit and public education credentials to applicants for advertised job openings. We measure the effect of for-profit college attendance by the rate at which employers positively inquire about applications. Our experimental design enables us to sidestep the issue of selection into the for-profit sector.

We track employer responses to resumes across six broad occupational categories – administrative assisting, customer service, information technology, medical assisting, medical office/billing, and sales – and in five major United States cities – Atlanta, Boston, Houston, Philadelphia and Sacramento – for the experiment. We focus primarily on comparing for-profit and public education credentials at the sub-baccalaureate level. We also compare for-profit and public college attendees to high-school graduates. Our primary comparison is motivated by the large and growing for-profit market share at the sub-baccalaureate level and the premium that students pay (and taxpayers subsidize) to attend for-profit colleges. The average annual tuition difference between for-profit and public community colleges is almost $12,000 per year (Baum and Ma, 2013).

Our secondary comparison between for-profit college attendance and no college attendance is also potentially important. The educational alternative for for-profit students is not obvious. While it is likely that some for-profit students are substituting away from community colleges, which motivates our primary comparison, it may also be the case that the for-profit

sector induces additional postsecondary entry. To the extent that enrollment in the for-profit sector partly reflects students who would not otherwise attend college, it is important to understand how for-profit attendees are perceived by the labor market relative to individuals who do not obtain any schooling beyond high school.

We find that for-profit college credentials do not increase the likelihood that an applicant will hear back from an employer relative to the public alternative. In fact, if anything, employers appear to prefer applicants who attend public community colleges. Furthermore, our findings are only weakly supportive of the idea that applicants benefit from for-profit college attendance relative to the alternative of not attending college at all. This latter result suggests that even if the for-profit sector induces some students to attend college by offering, say, more-flexible coursework and degree programs, the benefits are limited, at least along the dimension of generating employer interest in the labor market.[3]

2. Background

There is a large literature examining the labor-market returns to college attendance (for literature reviews see Card, 1999, and Goldberg and Smith, 2008). At the sub-baccalaureate level research has focused primarily on earnings outcomes for public community college students. Quasi-experimental estimates in the literature indicate a return of between 5 and 13 percent to each year of education and a return of between 18 and 40 percent to an associate's degree from community college (e.g., Jacobson, LaLonde, & Sullivan, 2005; Jepsen, Troske, & Coomes, 2011; Kane & Rouse, 1999).

Competitive pressure should result in similar labor market returns in the for-profit sector, adjusted for the higher direct cost of attendance. Cellini (2012) estimates the returns that would

[3] Our study is restricted to young job applicants with short work histories. Our findings may or may not translate to older workers with extended employment histories. We discuss this issue in more detail below.

be needed in order to justify the higher costs associated with for-profit education. She finds that for-profit students would need at least an 8.5 percent return for each year of schooling compared to a 5.3 percent return for community college students.[4] However, the theoretically-predicted equating of the returns on investment across sectors may fail to materialize for a number of reasons including barriers to entry, capacity constraints in the public sector, and a lack of information available to students, among others. It may also be the case that the higher direct costs of for-profit attendance are offset (at least to some degree) by lower indirect costs (e.g., more-flexible course scheduling).

Researchers have only recently begun to provide empirical evidence on the returns to for-profit education, with somewhat mixed results.[5] Cellini and Chaudhary (2012) and Chung (2008) find positive effects of for-profit college attendance. Alternatively, Deming, Goldin, and Katz (2012) and Lang and Weinstein (2013) find null or negative effects. To the best of our knowledge, our study is the first to use an experimental design to approach this problem. The key benefit of using an experiment is that we can break the link between for-profit status and other worker characteristics. Similar field-experiment designs have been used to investigate the labor-market effects of a variety of worker characteristics in recent years (Bertrand & Mullainathan, 2004; Hinrichs, 2013; Koedel and Tyhurst, 2012; Kroft, Lange, & Notowidgo, 2013; Lahey, 2008; Oreopoulos, 2011; Riach & Rich, 2006; Rooth, 2009; Weichselbaumer, 2003). A well-understood limitation of resume-based field experiments is that the outcome measure is neither wages nor employment, but rather an indicator for employer interest. However, as noted by

[4] Note that theory does not predict as large a difference in the private return if there are differences in the amount of public support and social benefit across sectors. Nonetheless, from a societal perspective it is still interesting to examine if the differential private return is large enough to justify the higher "sticker price" cost of for-profit colleges.

[5] There is also a literature that examines the return to public versus private non-profit college attendance (Black and Smith, 2004; Brewer, Eide, and Ehrenberg, 1999; Dale and Krueger, 2002; 2011; Monks, 1999). However, this research mainly focuses on comparisons among baccalaureate-granting institutions that tend to have selective admissions policies.

Bertrand and Mullainathan (2004), as long as there are at least moderate frictions in the search process employer response rates will translate into job offers, which will translate into employment and wage outcomes. We trade off the crudeness of the outcome measure to improve identification of the for-profit college effect.

3. Experimental Design

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's degree. Resumes that list coursework or an associate's 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 profit colleges account for a large and growing share of certificates and degrees conferred at this level. 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.

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 hypothesize that our focus on workers with shorter work histories (up to three years) will increase the importance of the educational treatments on the resumes relative to using workers with longer, and therefore more informative, work histories. 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. Put differently, resumes either indicate 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. Randomly assigning work histories after the postsecondary intervention could generate bias in the estimated effects of college attendance.

3.2 Labor Markets

We sent resumes to advertised job openings in the following five metropolitan areas: Atlanta, Boston, Houston, Philadelphia and Sacramento. These cities represent a geographically diverse set of large urban areas in the United States. We focus on larger cities because they have larger for-profit and public college sectors. This increases the number of colleges in each city-by-sector cells 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.

3.3 Occupations

Resumes were submitted to advertised positions in in six broad occupational categories: administrative assisting, customer service, information technology, medical assisting (excluding nursing), medical office/billing 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 in our study that we classify as "specialized" are information technology, medical assisting and medical office/billing. 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 the most common associate's degrees conferred by for-profit colleges are in business, the health professions, and computer and information technology. Although for-profit colleges certainly target specialized occupations, and perhaps disproportionately in their marketing efforts, the table shows that for-profit colleges are also substantially involved in training targeted at general occupations (e.g., certificate in administrative assisting, associate of science in business administration, etc.). While certificate programs at for-profit colleges are concentrated in health and personal/culinary services (the latter being an occupational category that we do not cover in our experiment), less than half of the associate's degrees awarded are in specialized fields.[6]

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. In addition to 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.

[6] 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 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 more detail below.

3.4 Colleges and Fields of Study

We constructed all of the resumes to reflect local applicants who attended local colleges. We selected colleges in each city by first identifying associate's 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. The colleges included in our study were selected at random based on an enrollment-weighted selection probability. We included 12-14 public and for-profit colleges in each city.

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's 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's 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 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.

3.5 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 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.[7]

[7] First and last names were combined such that Megan and Brian were only paired with Anderson and Thompson, Isabella and Carlos were only paired with Garcia and Hernandez, and Chloe and Ryan were only paired with Washington and Jefferson. 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.

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 the applicant's commute would be manageable.

The second section of each resume lists education credentials starting with a randomly assigned local high school.[8] Resume 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.[9]

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 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

[8] 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.

[9] 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.

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.

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 constructed two resume formats and each employer received one resume in each format.

3.6 Applying to Jobs

We sent resumes to advertisements for job openings in the five cities between May 2013 and February 2014. The experiment started with a pilot in Houston, which was used to establish protocols for sending the resumes and to address any unforeseen issues. The pilot ran smoothly so we include most of the data from Houston in the analytic sample (we exclude the first two weeks of data collection in Houston because experimental protocols were in the process of being developed; including all data from Houston does not affect our findings). We launched data collection in each city as soon as the materials (i.e., the resumes) could be constructed. We started collecting data in Atlanta in July, Boston and Philadelphia in August, and Sacramento in October.[10] 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 skills that were not conveyed by any of our resumes (e.g., certified radiological technician). In

[10] We were delayed in setting up the Sacramento experiment for reasons unrelated to this project. 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).

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, although some recruiters mask the fact that they are not the primary employer, so entirely avoiding recruiters was not 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 office/billing and sales. The numbers of suitable advertisements in information technology and medical assisting were lower across all cities.[11] The discrepancy in 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.[12] So, if anything, our study over-represents these fields that require more specialized skills.

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). We did not always send two resumes

[11] 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.

[12] The protocol was simple: for each period of work, RAs 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.

because, for example, the second resume in a pair with non-overlapping characteristics might contain an error (such as a blank work history). This is why the ratio of resumes to job posting in each city in Table 2 is always less than two.

Each resume came with a greeting from the applicant. We used two simple greetings that were constructed to be distinct enough so that employers would not suspect that the two resumes were related. One greeting was purposefully written to be more enthusiastic.[13] Although neither greeting was particularly enthusiastic, the more-enthusiastic greeting is associated with a higher response rate from employers (see Appendix Table A.1), suggesting that seemingly trivial differences in the application process affect employer responses.

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. Empirical Strategy

Randomization makes estimation straightforward. To determine the effect of for-profit status on employer response rates we estimate the following model, which we specify as a logit:

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

[13] The more enthusiastic greeting included an additional sentence from the applicant indicating his/her feeling that he/she "would be a great fit for the position."

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.[14] 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).

Our experiment is roughly powered to detect a 10 percent effect 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 10.6 percent (see Table 3), this corresponds to just over a one percentage-point change.[15] In the interview-request models we are powered to detect an effect size of approximately 0.6-0.8 percentage points, which corresponds to a 15- to 20-percent effect given that interview requests are less common. We powered our comparisons involving high school at a lower level because we expected a larger effect – there we can detect differences between the high school treatment and either college treatment that are as small as 1.6 percentage points for overall response rates, and 1.0 percentage point for interview requests.

[14] 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 the 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.

[15] Our *ex ante* power calculations, based on the Houston pilot, were based on a 10 percent overall resume response rate, which means that a 10-percent effect would correspond to a 1 percentage point change in the response rate.

The above-described power figures are based on aggregating all of the for-profit and public college resumes, as shown in equation (1). This raises an interpretation issue. Specifically, while in the raw data the three postsecondary education levels are roughly evenly represented (Table 2), entering 2-year college students are not equally likely to obtain each education level. Based on summary statistics from the Digest of Education Statistics (2011), in our main models we weight the data so that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of college goers in each sector, respectively. The weighting approach treats the representation of education levels in our data relative to the population as a sampling issue, up-weighting (down-weighting) education levels that are under-sampled (over-sampled) in our study (e.g., coursework).[16] As we show below, our findings are not qualitatively sensitive to whether we use the education-level weights or leave the data unweighted.

Another weighting issue arises with regard to the representation of cities in our sample. *Ex ante*, there is no reason to expect data from one of our cities to be more valuable than data from another in terms of informing our understanding of the effect of for-profit colleges. However, 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.[17] To address this issue we report estimates that additionally impose equal city weights. Relative to the education-level weighting, applying the city weights has more significant power consequences,

[16] The *ex post* re-weighting by education level comes with a very modest power penalty, which we illustrate below. The reason we did not impose the weights *ex ante* is that we wanted to preserve as much power as possible to detect for-profit effects specific to particular education levels. Although in the end our results are too noisy to gain sharp inference at any particular education level (see below), it was not obvious that this would be the case at the onset of the project. Note that the education-level weights are constructed based on the pooled for-profit and public resumes.
[17] 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.

although we still generally have sufficient power for inference in the city-weighted models.[18] 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.

Finally, we opted not to make any weighting adjustments for occupations in our primary analysis. Although we could impose weights so that each occupation is evenly represented, the representation of job advertisements in our data reflects the larger labor market. As a practical matter, our findings are not qualitatively sensitive to imposing equal occupation weights (see Appendix Table A.2).

5. Results

5.1 Descriptive Statistics

Tables 2, 3, 4 and 5 show descriptive statistics for the 6,639 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 12.4 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.

Table 3 shows response rates and interview-request rates across occupations and cities. The overall response rate is 10.6 percent and 4.7 percent of applicants received an interview request. Prior resume field experiments indicate response rates in the range of 8-12 percent

[18] One reason for this is that lower response rates generate more power, and our city with the lowest response rate – Atlanta – has the highest resume count (see Tables 2 and 3). Therefore, as we weight away from Atlanta we lose power.

(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 office/billing openings. Interview request rates are between 30 and 50 percent of total response rates. This ratio is consistent across cities and occupations.

The outlying city in Table 3 is Atlanta, where overall response rates and interview request rates are much lower than in the other cities. 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 (for example, perhaps we picked an odd set of addresses and/or high schools for individuals). Although the Atlanta response rates clearly diverge from the response rates in the other cities, there were no obvious violations to the experiment protocol in Atlanta.

Table 4 breaks out the sample by treatment condition. Although there are some nominal 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.

5.2 For Profit College Effects

Tables 6 and 7 show our primary estimates of the effects of for-profit colleges on employer responses and interview requests, respectively. The tables report marginal effects for high school and for-profit college relative to community college. Statistical tests for the null hypothesis that the high-school and for-profit-college effect are equal are reported in the third row of each table for each model. In results omitted for brevity we also estimate analogous linear probability models and obtain similar results.

The tables are structured to show the consistency of our findings across different weighting schemes and as we expand the set of control variables for resumes characteristics. The first two columns in each table illustrate that the practical consequence of re-weighting the data by education level is small – in both tables the estimates are very similar with and without education-level weighting. Next, we show estimates with and without city weighting for three different models that differ by the comprehensiveness of the X-vector. Adding the city weights reduces power to some extent but does not meaningfully affect the point estimates in any model. Including more control variables increases precision and, as expected, does not affect the point estimates for the parameters of interest. Appendix Table A.1 shows parameter estimates from the X-vector for the moderately detailed models in Tables 6 and 7.

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. Moving to Table 7, when we examine interview request rates our results are substantively similar although more precisely estimated, and the estimates with city- and credential-weighting are

statistically significant and suggest a negative effect of for-profit status on interview requests. Overall, the results in Table 6 and 7 provide no indication that employers prefer workers who attended for-profit colleges. Employers may even be less likely to request interviews from such workers relative to workers who went to public colleges.

Turning to the comparisons involving high-school graduates, both tables identify a large and statistically significant gap in response rates between applicants with high school versus public-college credentials. This is a notable finding as it is, to the best of our knowledge, the first experimental evidence on the labor-market return to higher education in the United States. This result complements a long-standing literature that estimates the return to community college attendance using a "selection on observables" approach (Kane and Rouse, 1995) as well as studies that use "natural experiments" to identify the return to college (e.g., proximity to college as in Card, 1995).

Resumes listing for-profit colleges also generate higher employer response rates than resumes without any postsecondary schooling. However, these differences are not statistically significant. Thus, while our results offer some indication that employers prefer workers who attend for-profit colleges to workers with only a high school diploma, we do not have sufficient statistical power to make a conclusive statement. This reflects the small magnitude of the nominal for-profit advantage, coupled with the fact that we designed the experiment expecting to find a larger gap in response rates between applicants with for-profit credentials and those without any postsecondary experience.

Our study was not powered to detect for-profit effects specific to particular education levels. With that qualifier, Table 8 breaks out selected results from Tables 6 and 7 by education level. The omitted comparison group in Table 8 is an associate's degrees from community

college. The table shows that applicants with associate's degrees from community college nominally outperform all other groups in our experiment, although again, the standard errors in Table 8 are mostly too large to draw strong inference. It is of some interest that the public/for-profit gap is largest for resumes that indicate coursework only. This is consistent with previous research indicating that program completion is particularly important for for-profit students to realize labor market benefits (Cellini & Chaudhary, 2012) and provides some context for our main results.

6. Sensitivity Analysis and Qualifications

In this section we perform sensitivity analyses and qualify our findings. One issue with the results we have presented thus far is that the occupational shares reflected in our data (Table 2) may not be the right ones to use for comparing the public and for-profit sectors. Again, a benefit of using the raw data is that they provide a reasonable depiction of the larger job market faced by individuals at this skill level. However, for completeness we estimate models using equal occupation weights in Appendix Table A.2. The estimates with equal occupation weights are qualitatively similar to our main findings as reported above.

A second issue is that the for-profit and public sectors may have different degree-attainment rates. 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 (Marcus, 2012; also see Mullin, 2012). Transfer students are more common in the public sector.

Note that the education-level weights we use for both sectors thus far are in line with the NCES for-profit attainment rates. If we leave the for-profit rates fixed, but increase the coursework weight for public college students to 80 percent and correspondingly lower the certificate and associate's degree rates (despite the concerns raised by the AACC), our results are qualitatively unaffected (although our estimates are less precise). We omit the results for brevity as they can be inferred from Table 8. The table shows that coursework nominally underperforms an associate's degree and nominally outperforms a certificate in community college. Therefore, when we pull weight away from these education levels and load it onto coursework, we obtain results that are substantively similar to what we report in previous sections.

Finally, we return to the issue of our coverage of the medical assisting field. A limitation of our research design for this field is that we do not specify medical certifications on the resumes (other than, of course, credentials that come directly from the colleges). This 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. To determine whether our findings are sensitive to whether we include medical assisting resumes at all, Appendix Table A.3 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.

A second issue with medical assisting certifications 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 a general qualification to our study – 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. Our experiment is structured to estimate the causal effect of for-profit college status as a signal on the resume – whether as a signal of ability, training, or both – but it cannot speak to all dimensions of how student experiences may differ across sectors.

7. Conclusion

Improving our understanding of the labor-market returns to for-profit college attendance is of critical importance given the rapid growth of the for-profit sector in higher education. Our findings contribute to policy discussions surrounding for-profit colleges by providing credible, causal estimates of the impacts of for-profit college attendance on employer interest in job applicants. 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. Furthermore, we are unable to statistically distinguish between applicants who attend for-profit colleges and applicants who do not obtain any postsecondary schooling. Although our field experiment design precludes us from examining every dimension along which for-profit colleges can improve student outcomes, these results will help to inform ongoing policy debates regarding the investment returns to students and taxpayers of for-profit higher education in the United States.

References

Bailey, Thomas, Norena Badway, and Patricia J. Gumport. 2001. "For-Profit Higher Education and Community Colleges." National Center for Postsecondary Improvement, Stanford, CA.

Baum, Sandy and Kathleen Payea. 2013. *Trends in Student Aid 2013.* New York: College Board.

Baum, Sandy and Jennifer Ma. 2013. *Trends in College Pricing 2013.* New York: College Board.

Bertrand, Marianne and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review* 94(4): 991-1013.

Black, Dan A. and Jeffrey A. Smith. 2004. "How Robust is the Evidence on the Effects of College Quality? Evidence from Matching." *Journal of Econometrics* 121 (1-2): 99-124.

Brewer, Dominik, Eric Eide and Ronald Ehrenberg, 1999. "Does it Pay to Attend an Elite Private College? Cross-Cohort Evidence on the Effects of College Type on Earnings." *Journal of Human Resources* 34(1): 104-123

Bohn, Sarah, Belinda Reyes, and Hans Johnson. 2013. "The Impact of Budget Cuts on California's Community Colleges." San Francisco, CA: Public Policy Institute of California.

Card, David. 1999. "The Causal Effect of Education on Earnings." In the *Handbook of Labor Economics Vol. 3A*, edited by Orley Ashenfelter and David Card. North-Holland: Amsterdam.

Cellini, Stephanie R. 2012. "For-Profit Higher Education: An Assessment of Costs and Benefits." *National Tax Journal* 65(1): 153-180.

Cellini, Stephanie R. and Latika Chaudhary. 2012. "The Labor Market Returns to a For-Profit College Education." NBER Working Paper No. 18343.

Chung, Anna S. 2008. "Effects of For-Profit Training on Earnings." Unpublished manuscript.

Dale, Stacey Berg and Alan B. Krueger, 2002. "Estimating the Payoff to Attending A More Selective College: An Application of Selection on Observables and Unobservables." *Quarterly Journal of Economics* 117(4): 1491-1527.

Darolia, Rajeev, Cory Koedel, Paco Martorell and Katie Wilson. 2014. Race and Gender Effects on Employer Interest in the Labor Market: An Update. Unpublished manuscript.

Deming, David J., Claudia Goldin, and Lawrence F. Katz. 2012. "The For-Profit Postsecondary School Sector: Nimble Critters or Agile Predators?" *Journal of Economic Perspectives* 26(1): 139-164.

Fryer, Roland and Steven Levitt. 2004. "The Causes and Consequences of Distinctively Black Names." *Quarterly Journal of Economics* 119(3): 767-805.

Gilpin, Gregory A., Joe Saunders and Christiana Stoddard. 2013. "Why Have For-Profit Colleges Expanded so Rapidly? The Role of Labor Market Changes in Student Enrollment and Degree Completion at Two-Year Colleges." Unpublished manuscript.

Goldberg, J., & Smith, J. 2008. "The Effects of Education on Labor Market Outcomes." In *Handbook of Education Finance and Policy*, edited by Edward Fiske and Helen Ladd. New York: Routledge.

Golden, Daniel. 2010a. "Homeless High School Dropouts Lured by For-Profit Colleges." *Bloomberg*, April 29. Accessed June 16, 2011. http://www.bloomberg.com/news/2010-04-30/homeless-dropouts-from-high-school-lured-by-for-profit-colleges-with-cash.html.

Golden, Daniel. 2010b. "Kaplan Quest for Profits at Taxpayer Expense Ensnares Veteran." *Bloomberg*, November 1. Accessed June 16, 2011. http://www.bloomberg.com/news/2010-11-01/kaplan-quest-for-profit-at-taxpayer-expense-ensnares-disabled-u-s-veteran.html.

Golden, Daniel. 2010c. "Veterans Failing Shows Hazards of For-Profit Schools in GI Bill." *Bloomberg*, September 23. Accessed June 16, 2011. http://www.bloomberg.com/news/2010-09-23/veterans-failing-to-learn-show-hazards-of-for-profit-schools-under-gi-bill.html.

Goodman, Peter S. 2010. "In Hard Times, Lured into Trade School and Debt." *The New York Times*, March 13. Accessed March 2, 2014. http://www.nytimes.com/2010/03/14/business/14schools.html.

Hinrichs, Peter. 2013. "What Kind of Teachers are Schools Looking For? Evidence from a Randomized Field Experiment." Unpublished manuscript.

Hoxby, Caroline M. and Christopher Avery. 2012. The Missing "One-Offs": The Hidden Supply of High-Achieving, Low Income Students. NBER Working Paper No. 18586.

Jacobson, Louis, Robert LaLonde and Daniel G. Sullivan. 2005. "Estimating the Returns to Community College Schooling for Displaced Workers." *Journal of Econometrics*, 125: 271-304.

Jepsen, Christopher, Kenneth Troske, and Paul Coomes. 2011. "The Labor Market Returns to Community College Degrees, Diplomas, and Certificates." IZA Discussion Paper No. 6902.

Kane, Thomas J. and Cecilia E. Rouse. 1995. "The Labor Market Returns to Two- and Four-Year College." *American Economic Review*. 85(3): 600-614.

Koedel, Cory and Eric Tyhurst. 2012. "Math Skills and Labor-Market Outcomes: Evidence from a Resume-Based Field Experiment." *Economics of Education Review* 31(1): 131-140.

Kroft, Kory, Fabian Lange, and Matthew J. Notowidigdo. 2013. "Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment." *Quarterly Journal of Economics* 128(3): 1123-1167.

Lahey, Joanna N. 2008. "Age, Women, and Hiring: An Experimental Study." *Journal of Human Resources* 43(1): 30-56.

Lahey, Joanna N. and Ryan A. Beasley. 2009. "Computerizing Audit Studies." *Journal of Economic Behavior and Organization* 70(3): 508-514.

Lang, Kevin and Russell Weinstein. 2013. "The Wage Effects of Not-for-Profit and For-Profit Certifications: Better Data, Somewhat Different Results." NBER Working Paper No. 19135.

Lynch, Mamie, Jennifer Engle, and Jose L. Cruz. 2010. *Subprime Opportunity: The Unfulfilled Promise of For-Profit Colleges and Universities*. Washington, DC: The Education Trust.

Marcus, Jon. 2012. Community Colleges Want to Boost Grad Rates – By Changing the Formula. *The Hechinger Report* (03.08.2012).

Mullin, Christopher M. 2012. Why Access Matters: The Community College Student Body. AACC Policy Brief 2012-01PBL.

National Center for Education Statistics. 2014. *Digest of Education Statistics: 2013*. Advanced release of digest tables. Available at: http://nces.ed.gov/programs/digest/2013menu_tables.asp.

National Center for Education Statistics. 2013a. *Digest of Education Statistics: 2012*. Washington, DC: U.S. Department of Education.

National Center for Education Statistics. 2013b. *Condition of Education: 2013*. Washington, DC: U.S. Department of Education.

National Center for Education Statistics. 2012. *Digest of Education Statistics: 2011*. Washington, DC: Washington, DC: U.S. Department of Education.

Oreopoulos, Philip. 2011. "Why Do Skilled Immigrants Struggle in the Labor Market? A Field Experiment with Thirteen Thousand Resumes." *American Economic Journal: Economic Policy* 3(4): 148–171

Riach, Peter A. and Judith Rich. 2006. "An Experimental Investigation of Sexual Discrimination in Hiring in the English Labor Market." *Advances in Economic Analysis & Policy* 6(2): 1-20.

Rooth, Dan-Olof. 2009. "Obesity, Attractiveness, and Differential Treatment in Hiring: A Field Experiment." *Journal of Human Resources* 44(3): 710-735.

U.S. Government Accountability Office. 2010. *For-Profit Colleges: Undercover Testing Finds Colleges Encouraged Fraud and Engaged in Deceptive and Questionable Marketing Practices.* GAO-10-948T. Washington, DC: Government Accountability Office.

U.S. Senate Committee on Health, Education, Labor and Pensions. 2012. *For Profit Higher Education: The Failure to Safeguard the Federal Investment and Ensure Student Success.*

Weichselbaumer, Doris. 2003. "Sexual Orientation Discrimination in Hiring." *Labour Economics* 10(6): 629-642.

Table 1. Certificate and Associate's Degree Shares at For Profit Colleges in the United States, 2011-2012.

	Associate's Degrees	Certificates
Business	0.23	0.02
Computer and Information Technology	0.09	0.01
Health Professions (Excluding Nursing)	0.30	0.47
Nursing	0.04	0.06
Liberal Arts and Sciences, General Studies	0.04	0.00
Personal and Culinary Services	-	0.27
Other	0.30	0.18

Source: 2013 Digest of Education Statistics and IPEDS, for the 2011-2012 school year. Personal and culinary services are not separately identified for associate's degrees.

Table 2. Descriptive Statistics for Submitted Resumes by City.

	All	Atlanta	Boston	Houston	Philadelphia	Sacramento
Female	0.492	0.498	0.501	0.481	0.496	0.463
African American	0.321	0.318	0.313	0.329	0.325	0.329
Hispanic	0.344	0.340	0.351	0.357	0.348	0.328
High-school graduate	0.144	0.131	0.146	0.113	0.159	0.151
Community College: Some College	0.147	0.149	0.152	0.145	0.142	0.164
For Profit: Some College	0.150	0.144	0.146	0.152	0.151	0.146
Community College: Certificate	0.140	0.151	0.135	0.158	0.129	0.141
For Profit: Certificate	0.136	0.140	0.137	0.122	0.130	0.144
Community College: AA Degree	0.138	0.143	0.137	0.135	0.138	0.112
For Profit: Some AA Degree	0.145	0.142	0.146	0.175	0.151	0.141
Single-Year Work History Gap	0.421	0.420	0.430	0.425	0.423	0.403
Two-Year Work History Gap	0.124	0.128	0.138	0.115	0.115	0.116
Admin Share	0.224	0.222	0.273	0.186	0.198	0.215
Customer Service Share	0.179	0.167	0.193	0.162	0.179	0.186
Information Technology Share	0.105	0.128	0.092	0.139	0.102	0.072
Medical Assisting Share	0.125	0.120	0.077	0.132	0.141	0.180
Medical Office/Billing Share	0.164	0.166	0.148	0.181	0.163	0.183
Sales Share	0.202	0.197	0.217	0.199	0.218	0.163
Total Resumes	6639	1854	1592	468	1770	955
Total Unique Job Advertisements	3930	1121	943	354	995	517

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	Houston	Philadelphia	Sacramento
Response Rate (RR)	0.106	0.054	0.141	0.139	0.117	0.113
RR: Admin	0.050	0.022	0.076	0.069	0.040	0.059
RR: Customer Service	0.125	0.081	0.162	0.145	0.127	0.124
RR: Information Technology	0.119	0.046	0.190	0.154	0.127	0.159
RR: Medical Assisting	0.080	0.027	0.049	0.065	0.100	0.145
RR: Medical Office/Billing	0.053	0.019	0.110	0.059	0.045	0.046
RR: Sales	0.205	0.118	0.235	0.312	0.239	0.192
Interview Request Rate (IRR)	0.047	0.029	0.056	0.066	0.053	0.049
IRR: Admin	0.016	0.010	0.018	0.023	0.014	0.024
IRR: Customer Service	0.064	0.052	0.078	0.053	0.066	0.062
IRR: Information Technology	0.037	0.013	0.054	0.031	0.044	0.072
IRR: Medical Assisting	0.031	0.009	0.025	0.033	0.032	0.064
IRR: Medical Office/Billing	0.026	0.010	0.055	0.035	0.017	0.023
IRR: Sales	0.099	0.068	0.096	0.194	0.119	0.071

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

	For-profit	Community College	High School
Female	0.504	0.490	0.457
African American	0.313	0.327	0.329
Hispanic	0.332	0.354	0.349
Some College	0.348	0.346	N/A
Certificate	0.316	0.330	N/A
AA Degree	0.336	0.324	N/A
Single-Year Work History Gap	0.427	0.417	0.415
Two-Year Work History Gap	0.123	0.126	0.124
Admin Share	0.225	0.232	0.198
Customer Service Share	0.180	0.172	0.194
Information Technology Share	0.109	0.107	0.092
Medical Assisting Share	0.125	0.118	0.140
Medical Office/Billing Share	0.161	0.164	0.174
Sales Share	0.199	0.206	0.201
Total Resumes	2862	2823	954

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	0.105	0.112[b]	0.091[b]
Employer Interview Request Rate	0.046	0.053[a]	0.035[a]
Total Resumes	2862	2823	954

[a] Indicates statistically significant difference between community college and high school at the 5 percent level.
[b] Indicates statistically significant difference between community college and high school at the 10 percent level.

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

	Unweighted	Credential Weighting	Credential and City Weighting	Credential Weighting	Credential and City Weighting	Credential Weighting	Credential and City Weighting
High school	-0.0182	-0.0180	-0.0200	-0.0167	-0.0183	-0.0154	-0.0162
	(0.0089)**	(0.0089)**	(0.0107)*	(0.0088)*	(0.0105)*	(0.0084)*	(0.0100)
For Profit College	-0.0058	-0.0066	-0.0104	-0.0070	-0.0107	-0.0068	-0.0104
	(0.0059)	(0.0060)	(0.0074)	(0.0060)	(0.0072)	(0.0057)	(0.0069)
P-value from test: *H_0: HS = for-profit*	0.17	0.20	0.36	0.26	0.45	0.30	0.54
Basic Application Details	X	X	X	X	X	X	X
City Indicators	X	X	X	X	X		
Occupation Indicators	X	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	6639	6639	6639	6639	6639	6639	6639

** 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. Credential Weighting is such that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of the weight in the data, respectively. These shares are based on outcomes for 2-year college students as reported in the Digest of Education Statistics (2011) as discussed in the text. City weighting is such that all cities receive equal weight in the data.

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

	Unweighted	Credential Weighting	Credential and City Weighting	Credential Weighting	Credential and City Weighting	Credential Weighting	Credential and City Weighting
High school	-0.0124	-0.0121	-0.0110	-0.0113	-0.0103	-0.0105	-0.0095
	(0.0053)**	(0.0053)**	(0.0067)*	(0.0051)**	(0.0063)	(0.0046)**	(0.0056)*
For Profit College	-0.0048	-0.0048	-0.0083	-0.0047	-0.0078	-0.0046	-0.0076
	(0.0036)	(0.0036)	(0.0046)*	(0.0035)	(0.0044)*	(0.0032)	(0.0039)**
P-value from test: *H_0: HS = for-profit*	0.18	0.19	0.65	0.21	0.65	0.21	0.68
Basic Application Details	X	X	X	X	X	X	X
City Indicators	X	X	X	X	X		
Occupation Indicators	X	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	6639	6639	6639	6639	6639	6639	6639

** 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. Credential Weighting is such that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of the weight in the data, respectively. These shares are based on outcomes for 2-year college students as reported in the Digest of Education Statistics (2011) as discussed in the text. City weighting is such that all cities receive equal weight in the data.

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

	Any Response		Interview Request	
	Unweighted	City Weighting	Unweighted	City Weighting
High school	-0.0175	-0.0208	-0.0122	-0.0126
	(0.0103)*	(0.0120)*	(0.0054)**	(0.0063)**
CC Coursework	-0.0012	-0.0060	-0.0006	-0.0016
	(0.0113)	(0.0133)	(0.0062)	(0.0074)
For-Profit Coursework	-0.0134	-0.0197	-0.0095	-0.0131
	(0.0098)	(0.0114)	(0.0051)*	(0.0055)**
CC Certificate	-0.0059	-0.0085	-0.0050	-0.0084
	(0.0111)	(0.0135)	(0.0060)	(0.0064)
For-Profit Certificate	-0.0106	-0.0188	-0.0030	-0.0075
	(0.0099)	(0.0111)*	(0.0057)	(0.0063)
For-Profit AA Degree	0.0001	-0.0015	-0.0041	-0.0078
	(0.0109)	(0.0111)	(0.0059)	(0.0064)
Basic Application Details	X	X	X	X
City Indicators				
Occupation Indicators				
City-by-Occupation Indicators	X	X	X	X
Flexible Time Trend	X	X	X	X
Race & Gender				
Exact Name Indicators	X	X	X	X
Basic Work History	X	X	X	X
Address and High School	X	X	X	X
N	6639	6639	6639	6639

Notes: The omitted treatment is an associate's 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 Table A.1. Marginal Effect Estimates for Control Variables from Model 2, with Credential and City Weighting.

	Any Response (Table 3)	Interview Request (Table 4)
High School	-0.0183	-0.0103
	(0.0105)*	(0.0626)
For Profit College	-0.0107	-0.0078
	(0.0072)	(0.0044)*
Basic Application Details		
Positive Greeting	0.0141	0.0078
	(0.0072)*	(0.0044)*
First Resume	0.0169	0.0126
	(0.0069)**	(0.0042)**
Applicant Race/Gender (as implied by name)		
African American Female	0.0258	-0.0006
	(0.0156)*	(0.0077)
African American Male	-0.0090	-0.0040
	(0.0124)	(0.0075)
Hispanic Female	0.0074	-0.0040
	(0.0143)	(0.0077)
Hispanic Male	0.0138	0.0026
	(0.0136)	(0.0076)
White Female	0.0230	-0.0001
	(0.0147)	(0.0074)
Work History (categories are not mutually exclusive)		
Any Work History Gap	-0.0035	-0.0037
	(0.0095)	(0.0059)
Two-Year Work History Gap	-0.0364	-0.0116
	(0.0101)**	(0.0066)*
Currently Unemployed	0.0286	0.0084
	(0.0129)*	(0.0082)
Occupational Category		
Administrative	-0.0950	-0.0459
	(0.00774)**	(0.0047)**
Customer Service	-0.0483	-0.0194
	(0.0083)**	(0.0047)**
Information Technology	-0.0457	-0.0291
	(0.0102)**	(0.0045)**
Medical Assisting	-0.0689	-0.0304
	(0.0081)**	(0.0044)**
Medical Office/Billing	-0.0898	-0.0348
	(0.0075)**	(0.0046)**
City		
Boston	0.1232	0.0309
	(0.0209)**	(0.0109)**
Houston	0.1089	0.0622
	(0.0583)**	(0.0505)
Philadelphia	0.0922	0.0284
	(0.0195)**	(0.0110)**
Sacramento	0.1047	0.0332
	(0.0269)**	(0.0161)*

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, neutral greeting, second resume, white male, occupation=sales, city=Atlanta. Credential Weighting is such that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of the weight in the data, respectively. These shares are based on reported outcomes for 2-year college students in the Digest of Education Statistics (2011) as discussed in the text. City weighting is such that all cities receive equal weight in the data.

Appendix Table A.2. Logistic Regression Results With Additional Weighting By Occupation. Marginal Effects are Reported.

	Any Response		Interview Request	
	Credential and Occupation Weighting	Credential, Occupation and City Weighting	Credential and Occupation Weighting	Credential, Occupation and City Weighting
High School	-0.0119	-0.0120	-0.0095	-0.0085
	(0.0089)	(0.0108)	(0.0048)**	(0.0059)
For Profit College	-0.0070	-0.0114	-0.0042	-0.0071
	(0.0058)	(0.0070)	(0.0033)	(0.0040)*
P-value from test: *H_0: HS = for-profit*	0.56	0.92	0.28	0.75
Basic Application Details	X	X	X	X
City Indicators				
Occupation Indicators				
City-by-Occupation Indicators	X	X	X	X
Flexible Time Trend	X	X	X	X
Race & Gender				
Exact Name Indicators	X	X	X	X
Basic Work History	X	X	X	X
Address and High School	X	X	X	X
N	6639	6639	6639	6639

Notes: The omitted treatment is an associate's degree from community college. Standard errors are clustered by job posting. Most postings received two resumes. Credential Weighting is such that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of the weight in the data, respectively. These shares are based on outcomes for 2-year college students as reported in the Digest of Education Statistics (2011) as discussed in the text. City weighting is such that all cities receive equal weight in the data. Occupation weighting is such that each occupation receives equal weight in the data.

Appendix Table A.3. Logistic Regression Results Excluding Medical Assisting Resumes, Using Detailed Models for Both Dependent Variables. Marginal Effects are Reported.

	Any Response		Interview Request	
	Credential Weighting	Credential and City Weighting	Credential Weighting	Credential and City Weighting
High School	-0.0198	-0.0231	-0.0133	-0.0141
	(0.0091)**	(0.0105)**	(0.0050)**	(0.0056)**
For Profit College	-0.0045	-0.0069	-0.0036	-0.0073
	(0.0062)	(0.0075)	(0.0035)	(0.0043)*
P-value from test: *H_0: HS = for-profit*	0.10	0.13	0.07	0.22
Basic Application Details	X	X	X	X
City Indicators				
Occupation Indicators				
City-by-Occupation Indicators	X	X	X	X
Flexible Time Trend	X	X	X	X
Race & Gender				
Exact Name Indicators	X	X	X	X
Basic Work History	X	X	X	X
Address and High School	X	X	X	X
N	5812	5812	5757	5757

Notes: The omitted treatment is an associate's degree from community college. Standard errors are clustered by job posting. Most postings received two resumes. Credential Weighting is such that coursework, certificates and associate's degrees account for 40, 35 and 25 percent of the weight in the data, respectively. These shares are based on outcomes for 2-year college students as reported in the Digest of Education Statistics (2011) as discussed in the text. City weighting is such that all cities receive equal weight in the data. Some observations were dropped for having characteristics that perfectly predict the outcome in the "interview request" models (< 1 percent).