A MATTER OF DEGREES:
EDUCATIONAL CREDENTIALS AND RACE AND GENDER DISCRIMINATION IN
THE LABOR MARKET

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ABSTRACT

S. MICHAEL GADDIS: A Matter of Degrees: Educational Credentials and Race and Gender Discrimination in the Labor Market (Under the direction of Karolyn Tyson)

Racial and gender inequality in economic outcomes, particularly among the college educated, persists throughout U.S. society. Scholars debate whether this inequality stems from differences in human capital (e.g. college selectivity, GPA, major) or employer discrimination on the basis of race and gender. However, limited measures of human capital and the inherent difficulties in measuring discrimination using observational data make determining the cause of these differences in labor market outcomes a difficult endeavor. This research examines employment opportunities for hypothetical graduates of elite top-ranked universities versus less selective institutions. I use an experimental computerized audit design to create matched candidate pairs and apply for 1,008 jobs on a national job search website. The results show that although a credential from an elite university results in more call-backs for all candidates, black candidates from elite universities only do as well as white candidates from less selective universities. Moreover, race results in a double penalty: when employers respond to black candidates it is for jobs with lower starting salaries and of lower quality than those of white peers. These racial differences in response rates and starting salary ranges suggest that a bachelor’s degree, even one from an elite institution, cannot fully counteract the importance of race in U.S. society. Although gender differences are not
statistically significant, race and gender interact to create a tiered system of opportunities. Finally, the results suggest that college major selection plays a critical role for black but not female candidates. Overall this research finds that both racial discrimination and differences in human capital contribute to economic inequality.
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INTRODUCTION

Economic inequality on the basis of race and gender remains an important topic in U.S. society today. Prior to the civil rights movement of the 1950s and 1960s and the feminist movement in the 1960s through the 1980s, racial and gender inequality were a given. Structural barriers and outright discrimination prevented or severely limited African-Americans and women from certain educational and occupational sectors. When doors were open to these groups, they often were for opportunities that did not equal those of white men.

However, during the past three decades racial and gender economic inequality both have declined significantly. Policy, public opinion, and social backlash over unequal treatment have slowly prodded progress forward. But despite the expansion of opportunities at all levels of education and more diversity across occupational sectors, economic inequalities still persist. Among bachelor's degree holders, women make approximately 75% of the wages of men, black men make approximately 75% of the wages of white men, and black women make approximately 90% of the wages of white women (Bradbury 2002). In fact, racial differences in earnings (Cancio, Evans, and Maume 1996; Zhang 2008) and unemployment (Wilson, Tienda, and Wu 1995) are highest among bachelor's degree holders. This raises the question: What explains current economic inequality?
One potential explanation is simple differences in educational credentials across groups. Even among bachelor’s degree holders, important qualitative variables such as where people go to college and their major of choice may explain economic inequality. However, we must come to two conclusions to accept the educational credentials argument. First, there must be a tangible effect of educational credentials on labor market outcomes. In other words, does having a college degree from an elite university such as Harvard increase an individual’s opportunities in the labor market over a degree from a less selective university such as the University of Massachusetts at Amherst? Second, there must be significant disparities from the overall population in the percentages of individuals from different social background categories with certain educational credentials. For instance, are black graduates from Harvard underrepresented compared to white graduates from Harvard?

A significant portion of this dissertation addresses the educational credentials issue. There is no denying that simply obtaining a college degree is beneficial. Individuals with a bachelor's degree earn nearly $22,000 more per year than individuals with just a high school diploma and expected lifetime earnings for holders of bachelor's degrees are 66% higher than for high school graduates (Baum, Ma, and Payea 2010). What is still unclear is what contributes to the inequality among bachelor’s degree holders. As educational attainment has expanded dramatically over the past few decades, obtaining a bachelor's degree differentiates an individual from other individuals less now than it did in the past. This increase in bachelor's degree holders has been matched by an expansion of inequality among college graduates (Hoxby and Long 1999; Levy and
Murnane 1992). Essentially, educational credentials from different institutions and in different majors of study result in horizontal stratification, or differences between individuals with the same educational attainment (Gerber and Cheung 2008). With the number of college graduates growing every year, attention to this additional layer of inequality that stems from higher education is more important now than ever.

**Discrimination**

The other major explanation for economic inequality is discrimination. But does discrimination in the U.S. still occur today? This may seem like a simple question for social science researchers, but measuring and explaining discrimination is far from a simple task. Surveys show that the general public either doesn't believe racial discrimination still exists or that it is not a major hindrance in the lives of non-whites (Bonilla-Silva 2010; Schuman et al. 2001). Social science research on discrimination has come under attack, while media figures debate the existence of discrimination and its impact on various groups. Even prominent economist James Heckman has referred to discrimination as “the problem of an earlier era” (1998:102), while Stephen and Abigail Thernstrom argue that the serious decline in overt racial discrimination can be equated to a more racially harmonious America (1997). The matter is not just academic; discrimination has been cited as both a reason to overturn affirmative action in college admissions (and to legal effect in California) (Connerly 2008) and as a reason to maintain such programs (Bowen and Bok 2000).

The modern debate has shifted due to the legal changes just a few short decades ago that now protect women and minorities. While researchers in the 1980s and even early 1990s were able to assess employers’ overt thoughts on discrimination through
surveys and interviews, fear of lawsuits and social desirability bias reduce the viability of these research methods in assessing discrimination today. Moreover, many employers may rationally engage in a form of discrimination known as statistical discrimination by using stereotypes and inferred group averages to make important employment decisions without malicious intent.

Using An Audit Study to Examine Inequality

I suggest that researchers have ignored an important tool in the methodological repertoire that can help shed some light on educational credentials and discrimination in the labor market. An experimental research design known as the audit study provides a unique way to match similar individuals to examine how differences between them affect an observed outcome. This methodology has been used in a variety of disciplines to examine issues such as housing and labor market discrimination. In particular, researchers have focused on the low-wage labor market but this method also lends itself well to examine questions regarding the effect of educational credentials on labor market outcomes. In this dissertation, I use a more modern application of the audit methodology to examine educational credentials and discrimination in greater detail.

Organization

In the first chapter of this dissertation, I review the evidence on the effects and mechanisms of educational credentials in the labor market with particular attention to differences in attainment across groups and over time. In Chapter 2, I explore the literature on discrimination and highlight the evolution of methods to distinguish discrimination as the nature of discrimination itself changed over time. Chapter 3 explains the audit method generally and details the three phases of data collection for this
dissertation: two rounds of pilot studies and a full round of data collection from which I draw on in the results chapters. The next two chapter include analyses of the results of my audit study. These chapters are divided into two types of dependent variables: employer responses to candidates and characteristics about the jobs for which candidates receive responses. In Chapter 6, I wrap-up with discussions of the implications of my findings and directions for future research.
CHAPTER 1.
THE EFFECTS AND MECHANISMS OF EDUCATIONAL CREDENTIALS IN
THE LABOR MARKET

Does Education Affect Labor Market Outcomes?

“The development of the diploma from universities...support[s] their holders’
claims to...monopolize socially and economically advantageous positions.”
(1922/1978:1000)

In theorizing about the role of education in society, Weber (1922/1978) suggested
that education may serve as a selection mechanism and promote the most deserving
members of society in a meritocratic fashion, or may conversely serve as an instrument of
those in power to maintain and reproduce the current system. This basic question has led
to much debate among scholars in an effort to determine whether education serves as a
pathway for mobility or if the game is rigged and simply reproduces the existing social
structure. From one perspective, the U.S. education system is a model of contest
mobility, one that promotes individual competition based on merit (Turner 1960). From
another perspective, it is a nearly closed system that restricts mobility to a limited few
(Sorokin 1927/1959).

Early queries into this issue, known as status attainment research in sociology,
sought to uncover the most important factors in determining occupational status and
earnings. Such research hinges on two possible findings. First, if background
characteristics, such as father's occupation or social class, are the most important factors
in an individual's adult status, the education system limits social mobility. Conversely, if years of education are the most important factor in an individual's adult status, the education system promotes social mobility.

Among early status attainment research, a number of scholars using different data sources consistently found that educational attainment is the most important factor in explaining occupational status. One standout piece of work is Blau and Duncan's *The American Occupational Structure* (1967). Using advanced statistical techniques for the time, the authors found that a respondent's education has the largest effect on both the occupational status of their first job and their current (at the time of the survey) occupation. However, father's education and occupation also have direct effects on respondent's education and thus indirect effects on respondent's occupation. This mediating effect is of critical importance because it suggests reality is somewhere between a full meritocratic system and a system the reproduces social structure.

Other status attainment research and research in the tradition of the Wisconsin Model (Sewell and Hauser 1975) supports Blau and Duncan's findings and suggests that the education system contributes to social mobility, albeit not completely devoid of influence from social class origin (Alexander and Eckland 1975; Duncan, Featherman, and Duncan 1972; Eckland 1965; Lipset and Bendix 1959; Sewell, Haller, and Ohlendorf 1970; Sewell, Haller, and Portes 1969; Sewell and Hauser 1972, 1975; Sewell and Shah 1967). However, it is important to note that the vast majority of this research is based on the experiences of white men. Additionally, findings from the Coleman report (Coleman et al. 1966), which suggest that family background characteristics play a significant role in academic achievement, coupled with other evidence on the role of family background
(Jencks et al. 1972; Jencks et al. 1979) highlight the importance of social class and other background characteristics in mediating the effect of education on earnings. Although this research raises doubts about the overall contribution of education in social mobility versus social reproduction, it still presents significant evidence of the influence of education on labor market outcomes.

Recent, albeit correlational and cross-sectional, data give some additional insights on the connection between educational attainment and labor market outcomes. As Table 1.1 indicates, educational attainment is negatively correlated with unemployment rates. Even during the recession economy of 2010, unemployment rates were lower for more highly educated groups. This pattern is replicated across all racial groups and for both men and women, even if some groups benefit more than others (see section 2 for an in-depth discussion). Scholars suggest that the benefit to education is even more pronounced during a recession: employers are more reluctant to lay-off more educated workers (Mincer 1991) and simultaneously increase job qualification requirements (Devereux 2004).

Table 1.2 shows mean earnings by educational attainment in 2010 using data from the Current Population Survey. This table suggests that earnings are positively correlated with educational attainment. Even individuals with some college, but no degree, earn more than high school graduates. Bachelor's degree holders earn nearly 180% as much as individuals with a high school degree. Once again, all racial groups and both men and women have similar patterns.

More sophisticated models agree with the basic patterns presented here (Grusky and DiPrete 1990; Kerckhoff, Raudenbush, and Glennie 2001; Korenman and Winship
Since the early status attainment studies, researchers have developed more advanced statistical techniques and collected better data with larger sets of control variables. The effects of social origin (e.g. social class) and ascribed statuses (e.g. race, gender) on educational attainment (Alon and Tienda 2007; Buchmann and DiPrete 2006; Goldrick-Rab 2006; Lucas 2001), as well as the effect of educational attainment on labor market outcomes (Elman and O’Rand 2004; Goldin and Katz 2008; Warren, Hauser, and Sheridan 2002) are well established areas of inquiry in sociology and economics. However, scholars typically simplify educational attainment by using either a linear variable representing the total number of years of schooling or a non-linear variable representing the highest degree obtained (see Smith 1995). In the following sections, I make a case that in today's society of expanding educational credentials more attention should be paid to the nuances within these categories, as horizontal stratification may serve to help researchers explain social inequality and processes of mobility in greater detail. First, though, I turn to the rich theory regarding the mechanisms of educational credentials.

Why Do Educational Credentials Matter?

“Education is the most important determinant yet discovered of how far one will go in today's world.” Randall Collins, from The Credential Society (1979:3)

In 1980, just one year after Collins' influential book The Credential Society, 17.0% of the U.S. population 25 and older had at least a bachelor's degree, up from 7.7% in 1960 (U.S. Department of Education 2009: Table 8). By 2009, this percentage had grown to 29.5% (ibid). In just a few short decades, a college education for their children has become the goal of the vast majority of families. But college requires foregoing wages in the short term for a large investment of time and money. The trends in
increasing college enrollments and graduates suggest that both potential employees and employers see value in education. If we believe that education has at least some influence independent of social class origin on occupational status, the next step is to investigate why education has value in the labor market.

Scholars invoke two main sociological theories, the functionalist and the conflict perspectives, as potential explanations for the necessity of education and credentials in the labor market and thus the rise of educational attainment over time. Functionalist theory stems from Davis and Moore (1945), who argue that society must match a variety of individuals to locations within society in an efficient manner (also see Merton 1963 and Parsons 1967 for more detailed treatises on functionalism). An adaptation of this theory states that education serves to provide individuals with the skills and knowledge required of employees to perform certain jobs (Collins 1971). Thus, as society becomes more technologically advanced, employers require more workers with greater education. Certain jobs are always more complex and/or important than others and society must be able to steer the best individuals to those jobs. The value of education comes in matching the needs of employers with the skills and knowledge of employees.

A. Why Do Educational Credentials Matter? Human Capital

David Bills (2004) frames the functionalist perspective as one based in a meritocratic world. An efficient society must get the most intelligent, able, and driven members of society into the most complex, demanding, and important jobs. In a fully or mostly meritocratic society, this is accomplished by associating the highest level of rewards with these jobs. Individuals are then encouraged to work hard to win these occupations and in turn, society benefits from this system.
The idea of the U.S. as a meritocratic society is undoubtedly the dominant paradigm in popular culture and media. Economists since at least the 1950s have adopted these ideas in developing theories and empirical research to explain the links between educational attainment and the labor market (Becker 1964; Blaug 1972; Bowman 1966; Mincer 1958, 1989; Schultz 1962). Known as human capital theory, this work suggests that individuals enter the labor force with no previous work experience and thus no history of job specific skills. Schooling, however, provides individuals with general skills and abilities (human capital) that are valuable in a wide variety of jobs. Human capital theorists argue that individuals act rationally by investing in education, while employers act rationally by investing in those individuals who are educated. Thus, under human capital theory, the value of education comes from the general skills and abilities that education provides to students.

Human capital theory is not without its critics, however. What is unclear from human capital theory is how employers measure the skills and abilities obtained from education, as researchers themselves do not measure but instead proxy for these concepts using educational attainment (Bills 2003; Kingston 2006; Rosenbaum 1986). Nor do researchers even have a firm grasp of what these skills are, as Bowles, Gintis, and Osborne point out: “...we know surprisingly little about what skills make up the vector of individual capabilities contributing to higher earnings” (2001:1137). Like the functionalist perspective itself, the existence of the specified outcome is often used as evidence supporting the theory (Bills 2004). This critique has led, in part, to more specific although not more easily measurable theories regarding the link between educational attainment and the labor market: screening (or filtering) and signaling (Bills
Screening, also known as filtering, provides employers a way to sort individuals on typically difficult to observe measures such as skills, ability, motivation, punctuality, or perseverance (Arrow 1973a; Riley 1976; Stiglitz 1975; Wolpin 1977), while signaling provides individuals with a way to tell employers about these characteristics (Berg 1971; Spence 1973, 1974, 1981, 2002). Employers have imperfect information about these individual characteristics but know that they impact productivity and job performance. Yet, according to these theories, individuals know themselves better than anyone else. Education, then, serves two purposes: (1) it provides individuals with a way to signal information about their skills and abilities to employers and (2) it provides employers with an existing sorting mechanism, as only the most motivated individuals with the highest ability levels can get into and complete college. Under this theory, education may or may not increase an individual's knowledge, abilities, and skills but nonetheless is useful to employers (Psacharopoulos 1979; Thurow 1975; and see Brown and Sessions 1999 for a review).

Just as human capital theory struggles with issues of measurement, testing theories of screening and signaling is difficult as well. From the signaling perspective, how does one measure how well individual choice in education captures skills, ability, and motivation? Scholars have been critical of the lack of empirical tests of these theories (Borjas 1991; Manski 1993). However, recent work examining what students learn in college suggests that overall modern colleges are not significantly increasing the skills and abilities of graduates, but rather putting a stamp of approval on their pre-existing abilities and passing them along to the labor market (Arum and Roksa 2011).
B. Why Do Educational Credentials Matter?  Cultural Capital

In contrast to the functionalist perspective, conflict theory suggests that certain groups have status within certain domains, such as economic, power, or cultural, and work to maintain an advantage over other groups (Weber 1922/1978). Thus, groups can maintain advantages in employment by enacting cultural standards of educational requirements as barriers to entry (Collins 1971). Employers maintain status groups by hiring similar individuals on the basis of group membership using educational credentials as a marker. Scholars also often draw upon the work of Pierre Bourdieu (Bourdieu 1977; Bourdieu and Passeron 1970) to explain credentialism as a form of cultural capital that works to exclude members outside of the upper class.

One of the earliest works from this viewpoint on educational credentials was Ivar Berg's *Education and Jobs: The Great Training Robbery* (1971). Using data from the 1950s and 1960s to measure change, he argues that educational attainment during this time outstripped the skill demands of jobs. He finds that skills are mostly learned after employment. Additionally, he uses interview data to examine the use of educational requirements and finds that employers often report the use of education as a “screening device” (15). He uses this evidence to make the case that the human capital theorists are wrong and education serves a different function in society:

“Educational credentials...will most certainly reinforce the formidable class barriers that remain, even without the right within families to pass benefices from parents to their children.” (1971:185).

Thus, educational attainment may signal something different to employers other than the unobservable characteristics suggested by economists: social class.

In his review of the previous research and data, he finds a pattern of evidence that supports the conflict theory. Like Berg, he suggests that there is a mismatch between educational attainment and the technological skill requirements of jobs and that skills come from experience in the labor force. Moreover, he suggests that the available evidence indicates that education does not increase productivity. Like Bourdieu (1977), Collins argues that educational attainment creates and reinforces a particular cultural disposition through socialization and credentials serve to certify the existence of that cultural disposition.

In an argument that parallels the cultural capital hypothesis, Samuel Bowles and Herbert Gintis (1976) reject the human capital focus on skills and instead posit that employers seek certain types of behavior that are enforced by schools. Known as the correspondence principle, this theory suggests that higher education and more selective schools enforces critical thinking and independence, while lower tracks of educational attainment enforce passivity and obedience. Over time, their argument has shifted somewhat to focus on the importance of more general non-cognitive skills and behavior (2000; 2002) but still suggests the importance of educational attainment to employers.

The language behind these theories is broad and often does not clearly specify some of the concepts used to explain why educational attainment matters in the labor market. Although scholars use a number of data sources and conceptualizations to try to understand these processes, empirical support is scattered and somewhat weak (Kingston 2006). Much of the debate on human capital in recent years focuses on the separate effects of ability (IQ, test scores, grades, etc.) and educational attainment (Bowles, Gintis, and Osborne 2001; Kerckoff, Raudenbush, and Glennie 2001; Korenman and Winship
2000). Some credentialists cite a “sheepskin effect”, or a non-linear effect of educational attainment on labor market outcomes to suggest evidence against human capital theory (Belman and Heywood 1991), although there is limited empirical support for this effect (Hunter and Leiper 1993; Jaeger and Page 1996; Jencks et al. 1979). Still others recommend more macro-level approaches that focus on over-education and skill differentials between workers and positions (Brown 1995; Labaree 1997).

Employment projections from the U.S. Bureau of Labor Statistics (2010) shed some light on the overall situation in the labor market. The projections suggest that between 2008 and 2018, nearly 51 million job openings will become available. An overwhelming majority of these job positions require less than a bachelor's degree. In a separate analysis of the match between required education and training versus actual employment by educational attainment, data from 2008 suggest that many employees are overeducated compared to what their job requires (Ramey 2010). These results are quite suggestive of Berg (1971), Collins (1979) and the other scholars in the tradition of credentialism.

It is important to pause and take stock of the theories presented here. In a society based on merit, employers use educational attainment to help them select the employees with the best skills, abilities, motivation, etc. In a society based on social reproduction, employers use educational attainment to help them select the employees with the proper group membership, cultural values, and behavior. The literature suggests that the U.S. is a mix of these two worlds, perhaps leaning more to the side of meritocracy. My intent is not to adjudicate between these theories, but rather to highlight the importance of education in the labor market. Whatever the true reason for employers to use educational
credentials in making hiring decisions, one thing is for certain: during the early stages of an individual’s employment career, the information available to employers is extremely limited. These individuals do not yet have a proven track record of employment, forcing employers to rely on educational credentials as a proxy. In the next section, I suggest that the expansion of educational attainment in the U.S. has increased the importance of

*where* individuals obtain their degrees.

*Why Might Where You Receive Your Education Matter?*

In the previous two sections, I established that education affects labor market outcomes and discussed theories regarding why education matters for entry into the labor market. However, differentiation of educational attainment has dramatically changed in the decades that followed much of the theories discussed. In 1970, a high school diploma was still a credential that separated an individual from nearly half of the U.S. population (see Table 1.3). By 2010, that mark signifies nearly nothing on its own, as 87% of the population has at least completed high school (ibid). The percentage of college graduates has followed a similar path, increasing every decade. Between 1970 and 2010, the percentage of the population with a bachelor's degree or higher grew from 11% to nearly 30%. A number of scholars argue that it is the desire of individuals to distinguish themselves through educational attainment, and not the skill needs of the labor market, that has driven increases in educational credentials (Brown 1995; Collins 1979; Labaree 1997).

Prior theoretical work on educational inequality suggests that as a particular level of educational attainment becomes more commonplace, individuals must distinguish themselves in other ways to maintain an advantage from education (Alon 2009; Karabel
With the expansion of higher education, the premium in the return to a college degree over a high school degree has fluctuated but inequality among college graduates has risen (Freeman 1976; Hoxby and Long 1999; Levy and Murnane 1992). Employers now have a larger pool of applicants to choose from who fit certain educational requirements. If the skill requirements of jobs have kept up with the expansion of education, or if employers want to keep barriers at the same relative levels, they realistically have two options: increase the educational requirements of jobs even further or be more selective among those with similar educational attainment. The first option would lead to more job openings requiring master's and higher degrees. The second option would lead to greater value to credentials from more selective colleges.

To address the first option, let's examine the number of degrees conferred by type over time. The numbers in Table 1.4 indicate that in 1970, 62% of degrees conferred were bachelor's degrees and 16% of degrees conferred were master's degrees. By 2009, these numbers had shifted so that only 50% of degrees conferred were bachelor's degrees and 21% of degrees conferred were master's degrees. In raw numbers, 809,052 more bachelor's degrees were conferred in 2009 compared to 1970 (a 102% increase) and 448,493 more master's degrees were conferred in 2009 compared to 1970 (a 215% increase). We can also see that over this time the growth of associate's degrees (282% increase) outpaced the growth of both bachelor's and master's degrees. These changes likely indicate more specific specialization on the low end of the labor market (associate's degrees prepare individuals for specific jobs) or the population’s growing understanding that some higher education is necessary at the low end of the labor market and increased
job requirements at the high end of the labor market. A movement to increase job
requirements and select only those with greater than a bachelor's degree is possible,
although unlikely the norm. Data from the U.S. Bureau of Labor Statistics indicate that
in 2008, 90% of all jobs required a bachelor's degree or less, suggesting that many
employees are overeducated (Ramey 2010). Bachelor's degrees still make up the
majority among degrees conferred and degree holders (in terms of those who are college
educated). Thus, most employers choose employees from a large group of individuals
entering the job market who are homogeneous in terms of total educational attainment,
but heterogeneous in other ways. Perhaps the most important or at least easily measured
way is where an individual obtains their degree.

A. Why Might Where You Receive Your Education Matter? Human Capital

If we believe that the U.S. is a meritocratic society, one where employers look for
the most skilled employees, why might we expect where an individual obtains their
college degree to matter? One reason is that more selective schools produce graduates
with better skills and abilities. In fact, if we accept the ideas of some screening theorists
(Psacharopoulos 1979; Thurow 1975), the “black box” of education doesn't even have to
alter the skills and abilities of the inputs. The most selective colleges simply work as a
screening device for employers, selecting the best individuals out of high school and
eventually passing them along into the labor market.

At minimum, then, do the top colleges and universities have students with greater
skills and abilities at entry? This is a difficult question to answer because, as you will
recall, the human capital scholars admit to having only a vague sense of what these skills
and abilities are. Some research suggests that IQ, cognitive ability or simply general test
scores are related to job performance and productivity independent of educational attainment (Hunter 1986; Schmidt 2002; Schmidt and Hunter 1998; although see Hauser 2011 for an alternative view). Scholars also suggest that using educational attainment as a screening device results in candidates with higher cognitive ability (Berry, Gruys, and Sackett 2006). Thus, if we consider cognitive ability to at least proxy for the skills and abilities that human capital researchers stress, we can examine how this varies among different colleges.

If we believe SAT scores are a valid measure of cognitive ability, then more selective schools do indeed have students with greater ability: the top 20 colleges in the U.S. News and World Report national rankings (2011) have average SAT scores between 2002 (25th percentile) and 2286 (75th percentile), while colleges ranked 100 positions lower (101-120) have average SAT scores between 1552 (25th percentile) and 1893 (75th percentile) (author calculations from National Center for Education Statistics 2011).

Additional findings from the Intercollegiate Studies Institute suggest that political and historical literacy are correlated with selectivity (Toby 2010:126). Other research suggests that the most selective colleges also rely on test scores more in admissions factors (Alon and Tienda 2007). These results suggest that the most elite colleges at least select the students with the highest skills and abilities, even if they do not alter them during an individual's college career.


Conversely, if the U.S. is a society that reproduces the social structure, one where employers look for the individuals from the “proper” social background, why might we expect where an individual obtains their college degree to matter? One reason is that
more selective schools enroll students of high social status and class backgrounds. As Table 1.5 shows, nearly 40% of college students from the lowest income bracket attend public 2-year colleges, compared to only 17% of college students from the highest income bracket. Meanwhile, 45% of college students from the highest income bracket attend public 4-year colleges, compared to only 32% of college students from the lowest income bracket, and 26% of college students from the highest income bracket attend private not-for-profit colleges, compared to only 12% of college students from the lowest income bracket. Data from the National Educational Longitudinal Survey suggest that the disparity is even wider when selectivity is considered (see Soares 2007:4). Clearly, students from privileged social class background take more of the enrollment slots at the top colleges and universities (for additional research, see Kingston and Lewis 1990). Attendance, if not acceptance, at the top universities likely presents a class barrier due to the maintenance over time of a large gap (at least 2.0 since 1970) between the estimated attendance costs of a 4-year private versus 4-year public institution (see Table 1.6). Finally, at least among public universities, students from the most advantaged social backgrounds are more likely to graduate (Bowen, Chingos, and McPherson 2009) and the graduation rate gaps between income quartiles are highest at the most selective colleges (Carnevale and Rose 2003).

The theory and data presented in this section suggest that employers have more power than individual job seekers. With the percentage of college graduates continually increasing, employers can afford to be more discerning among college graduates, whether justified by job demands or not. It is unlikely that this oversupply of college graduates will soon subside. We now know that educational attainment affects labor market
outcomes, there are well defined, if not well tested reasons why this occurs, and the expansion of higher education has changed the options for employers. In the next section, I turn to the limited existing research on the effect of college selectivity on labor market outcomes.

**Does Where You Receive Your Education Matter?**

In 2010, tuition and fees cost $38,416 at Harvard University, the number 1 ranked national university by U.S. News and World Report (2011). By contrast, out-of-state tuition and fees at the 99th ranked University of Massachusetts-Amherst were a little over half that sum at $20,307 (ibid). Students, parents, educators, and researchers alike stand to benefit from knowing if more selective schools are worth the additional cost. Surprisingly, some of the most recent research in this area suggests that they are not, at least in terms of an effect on income (Dale and Krueger 2002, 2011). Still, other results show that students at more selective colleges are more likely to graduate, more likely to attend graduate or professional programs, and earn higher wages on the labor market (Alon and Tienda 2005; Bowen and Bok 2000; Brand and Halaby 2006; Brewer and Ehrenberg 1996; Light and Strayer 2000; Zhang 2005).

Early research in this area typically uses broad categorical classification such as elite or prestigious schools and estimates the effects of these categories on occupational status and income later in life (Griffin and Alexander 1978; Havemann and West 1952; Morgan and Duncan 1979; Solmon 1973; Solmon and Wachtel 1975). These studies find positive and significant effects of college type on occupational status and wages, but much like the early status attainment literature, many of these studies focus only on samples of white men.
More recent studies using the categorical comparison use more representative samples. One such study uses data from the National Longitudinal Study of the High School Class of 1972 (NLSHS72) and High School and Beyond to examine the effect of a degree from different colleges on wages (Brewer, Eide, and Ehrenberg 1999). The authors find significant positive effects on wages from attending elite and middle-tier private institutions and a limited effect from attending an elite public institution when compared to a bottom-tier public institution. Additionally, Monks (2000) finds a wage benefit to a degree from a research institution compared to a liberal arts university. However, not all research finds significant effects of college selectivity, suggesting that there may be difficulties in measuring returns in the labor market (see Dale and Krueger 2002, 2011).

Research that uses non-experimental data is subject to potential bias due to the correlation between unobserved factors that may influence both admission and attendance at selective colleges and the examined outcomes, such as graduation rates and wages (Foster and Rodgers 1979; Gerber and Cheung 2008). For example, student ability and motivation may be partially or completely unmeasured, but we may assume these variables are correlated with both a student's attendance at a more selective school and her success on the labor market. A student may choose not to go to a more selective college if she believes the eventual wage benefit will not outweigh the cost of tuition, leading to biased estimates in regression coefficients. Selection bias makes it difficult to determine if employers place different values on college degrees based on college selectivity.

More recent research continues to use non-experimental data while also using
more sophisticated methodological techniques to address selection bias. Black and Smith (2004) primarily use ASVAB test scores to match similar individuals from different institutions using propensity scores and find that college selectivity has a positive effect on wages. However, the authors also note a number of difficulties in using this method with their data, particularly in matching attendees of non-selective colleges with high propensities to attend a highly selective college. Using a regression discontinuity design Hoekstra (2009) finds that white men who barely made the admissions cut-off at a flagship state university experience 20% higher wages than white men who barely missed the admissions cut-off, suggesting that the credential itself, rather than prior human capital, increases value in the labor market. Dale and Krueger (2002) use the College and Beyond Survey (C&B) to examine wage returns 15 years after graduation. The authors find no effect of college selectivity when matching students based on institutions they were admitted to but did not attend. In a follow-up piece, Dale and Krueger (2011) include an additional cohort from the C&B and again find no effect of college selectivity on earnings in models adjusted for selection. Additional research that addresses selection bias finds varying results on the existence and size of bias in non-adjusted OLS regression estimates examining the effect of college selectivity on wages (Behrman, Rosenzweig, and Taubman 1996; Brand and Halaby 2006; Long 2008). Thus, although research that does not adjust for selection suggests that there is an effect of college selectivity on wages, the research that does adjust for selection presents mixed findings.

One recent qualitative study focuses on the employers and recruiters who are in positions to hire recent graduates (Rivera 2011). The author finds that nearly 80% of the employers she interviewed in top-tier firms use school prestige to weed out potential
candidates. Additionally, these employers often mentioned that they only reviewed candidates from elite private schools. In some cases, firms had strong ties with specific elite schools, spending millions of dollars on recruitment activities and using a full time employee liaison at these schools. This research suggests that, at least in some cases of elite schools, individuals may reap large benefits from both the signal of their degree and the social capital unlocked by their institution.

*Why Might Your College Major Matter?*

Although the choice of where to obtain a degree is ultimately vitally important to showing human and cultural capital in the labor market, what to study, or college major choice, is also a critical part of an individual’s ability to display their skills to employers in the labor market. In fact, college major choice may represent both human capital, such as ability and preparation, and other characteristics, such as expectations regarding future earnings and preferences (Altonji, Blom, and Meghir 2012). Thus, although college major choice may represent human capital, employers do not know for certain how strong the link between college major choice and human capital is for any single individual. Some college majors have clear pathways between choice and career (also known as occupational specificity – see Roksa and Levey 2010 for a review), such as engineering or nursing. Other college majors ideally prepare individuals for a wider range of employment prospects, such as math and the social sciences. In this section, I am more interested in those college majors because they require employers to make assumptions and predictions about human capital.

Data from the National Center for Education Statistics’ Baccalaureate and Beyond Survey suggest that, at the very minimum, there are associates between human capital at
college entry and college major choice. Table 1.7 shows a selected group of college majors, the overall share from the sample for each major, and the average SAT scores for each major. Students who major in social work and human resources (460 math, 487 verbal SAT scores) or business management and administration (522 math, 510 verbal SAT scores) appear on the low end of the human capital scale. Students who major in economics (597 math, 573 verbal SAT scores) or history (558 math, 595 verbal SAT scores) appear on the low end of the human capital scale. These descriptive statistics show that there is a significant amount of variation in human capital among college major choice.

**Does Your College Major Matter?**

Much like the literature on college selectivity, the literature on college major choice is unclear on whether certain college majors have a causal effect on earnings and other labor market outcomes or simply reflect selection into certain majors on the basis of prior human capital characteristics. Cross-sectional data show significant variation in mean wages based on college major. Table 1.8 shows that individuals who major in social work and human resources ($23,190) or psychology ($24,610) are on the low end of wages, while individuals who major in economics are on the high end of wages ($43,150).

Early research in this area suggests large effects on earnings for business, engineering, health, mathematics, and science focused majors (Berger 1988; Rumberger 1984; Rumberger and Thomas 1993). In an attempt to separate human capital from selection or preferences, Rumberger and Thomas (1993) separately control for college major and GPA and find that some of these majors, specifically business, health,
mathematics, and science majors, are rewarded for higher GPAs within major while GPA does not matter for other majors. Other more recent studies also report premiums to certain majors even after accounting for GPA, changes in major over time, and rigor of curriculum (Arcidiacono 2004; Grogger and Eide 1995; Hamermesh and Donald 2008).

An analysis of nationally representative data examining college selectivity and college major on first year earnings also does well in capturing employer assumptions about the human capital value of particular degrees since employer have no lengthy employment and salary history to draw upon during the decision making process. Thomas (2000) conducts such an analysis using the Baccalaureate and Beyond and finds that engineering and health major significantly outperform education and humanities majors. Research examining specific majors instead of broad categories is quite limited but one example finds large effects on wages for majors such as computer science, economics, engineering, and mathematics (Altonji, Blom, and Meghir 2012).

One final possibility remains in explaining differences in the economic returns to college major. Not all college majors lead to similar employment opportunities across sectors. For instance, Roksa (2005) finds that graduates from certain majors, particularly those that are traditionally female-dominated, are more likely to be employed in the public and nonprofit sectors, which explains a significant portion of their lower wages.

**Summary and Research Questions on Educational Credentials**

The evidence on the *quantitative* aspect of educational credentials is clear: more years of educational attainment results in better outcomes on the labor market. *Qualitatively*, however, the findings are somewhat mixed. Do college selectivity and college major have causal effects on labor market outcomes? If so, just how large are
those effects? The answer to these questions represents an important piece in understanding racial and gender economic inequality in the U.S. today. If educational credentials, particularly the differences in college selectivity and college major, have no effects on labor market outcomes, it suggests that racial and gender discrimination must play an incredibly large role in economic inequality. However, if these differences among college degree holders are important to labor market outcomes, discrimination might be a minor factor in economic inequality. These questions lead me to my first set of formal research questions for this study:

(1) Does having a degree from an elite college, rather than a less selective college, have positive effects on (a) the likelihood of receiving an employer response, (b) the salary range of jobs, and (c) the type of job?

(2) Does having an economics degree, rather than a psychology degree, have positive effects on (a) the likelihood of receiving an employer response, (b) the salary range of jobs, and (c) the type of job?

To address these questions regarding educational credentials, I conducted an experiment known as an audit study that examines the effects of college selectivity and college major in the early stages of the labor market. Chapter 3 discusses this type of research in more detail and gives the specifics of my data collection and analysis. In Chapters 4 and 5, I present the results from this research. There are some advantages and disadvantages to this research in reference to the previous literature.
First, much of the previous literature struggles with addressing specific mechanisms of educational credentials. The theory is well developed, and Gerber and Cheung (2008) suggest that research examining college selectivity can consolidate the various theories into four main mechanisms: (1) human capital, (2) signaling, (3) social capital, and (4) selection. In terms of college selectivity, I cannot address differences between mechanisms (1), (2), and (4) but I can remove social capital as a potential mechanism. In my research, I used candidates to apply for jobs online and employers had no existing contact or network connections to each candidate, thus eliminating any social capital effects. Additionally, in terms of college major, I am able to control for selection into certain employment sectors and examine only outcomes for private and nonprofit sectors.

Next, it is important to note the differences in outcomes and timing from most of the prior literature. In my research, candidates did not move beyond the initial contact phase (typically a request for an interview). Although this is an important phase in telling us how far candidates might potentially go by showing how opportunities become constrained early in the process, it does not tell us who would eventually get a job or their final salary. Moreover, my research focuses on the labor market immediately after graduation, before employers have additional signals of human capital such as employment and salary history. As other researchers suggest (Ishida, Spilerman, and Su 1997) the effect of college selectivity at an individual's entry point into the labor market is likely driven mostly by the signaling effect. The critical contribution of addressing these questions using the audit data is that it presents the opportunity to examine the transition from school to work, instead of looking at wages much later in an individual's
career.

The audit method allows me to isolate the effects of college selectivity and college major on likelihood of employer response and other characteristics associated with the job application. Thus, the first stage of this research is to examine whether educational credentials matter in the labor market, separate from social background characteristics. In the next chapter, I review the literature on discrimination and race and gender differences in the labor market.
## Table 1.1. Unemployment Rate by Educational Attainment, 2010

<table>
<thead>
<tr>
<th>Educational Attainment</th>
<th>Unemployment Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than a high school degree</td>
<td>14.9</td>
</tr>
<tr>
<td>High school degree</td>
<td>10.3</td>
</tr>
<tr>
<td>Some college</td>
<td>9.2</td>
</tr>
<tr>
<td>Associate degree</td>
<td>7.0</td>
</tr>
<tr>
<td>Bachelor's degree or more</td>
<td>4.7</td>
</tr>
</tbody>
</table>

Table 1.2. Earnings by Educational Attainment, 2010

<table>
<thead>
<tr>
<th>Educational Attainment</th>
<th>Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than a high school degree</td>
<td>$22,635</td>
</tr>
<tr>
<td>High school degree</td>
<td>$32,812</td>
</tr>
<tr>
<td>Some college</td>
<td>$38,612</td>
</tr>
<tr>
<td>Associate degree</td>
<td>$41,529</td>
</tr>
<tr>
<td>Bachelor's degree or more</td>
<td>$68,603</td>
</tr>
</tbody>
</table>

From: http://www.census.gov/hhes/www/cpstab/032010/perinc/new03_000.htm
<table>
<thead>
<tr>
<th>Year</th>
<th>High school completion or higher</th>
<th>Bachelor's degree or higher</th>
</tr>
</thead>
<tbody>
<tr>
<td>1910</td>
<td>13.5</td>
<td>2.7</td>
</tr>
<tr>
<td>1920</td>
<td>16.4</td>
<td>3.3</td>
</tr>
<tr>
<td>1930</td>
<td>19.1</td>
<td>3.9</td>
</tr>
<tr>
<td>1940</td>
<td>24.5</td>
<td>4.6</td>
</tr>
<tr>
<td>1950</td>
<td>34.3</td>
<td>6.2</td>
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<tr>
<td>1960</td>
<td>41.1</td>
<td>7.7</td>
</tr>
<tr>
<td>1970</td>
<td>55.2</td>
<td>11.0</td>
</tr>
<tr>
<td>1975</td>
<td>62.5</td>
<td>13.9</td>
</tr>
<tr>
<td>1980</td>
<td>68.6</td>
<td>17.0</td>
</tr>
<tr>
<td>1985</td>
<td>73.9</td>
<td>19.4</td>
</tr>
<tr>
<td>1990</td>
<td>77.6</td>
<td>21.3</td>
</tr>
<tr>
<td>1995</td>
<td>81.7</td>
<td>23.0</td>
</tr>
<tr>
<td>2000</td>
<td>84.1</td>
<td>25.6</td>
</tr>
<tr>
<td>2005</td>
<td>85.2</td>
<td>27.6</td>
</tr>
<tr>
<td>2010</td>
<td>87.1</td>
<td>29.9</td>
</tr>
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</table>

From: http://nces.ed.gov/programs/digest/d10/tables/dt10_008.asp
Table 1.4. Number of Degrees Conferred over Time

<table>
<thead>
<tr>
<th></th>
<th>Associate's</th>
<th>Bachelor's</th>
<th>Master's</th>
<th>First-professional</th>
<th>Doctor's</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970</td>
<td>206,023</td>
<td>792,316</td>
<td>208,291</td>
<td>34,918</td>
<td>29,866</td>
<td>1,271,414</td>
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<td></td>
<td>16.2%</td>
<td>62.3%</td>
<td>16.4%</td>
<td>2.8%</td>
<td>2.4%</td>
<td></td>
</tr>
<tr>
<td>1975</td>
<td>360,171</td>
<td>922,933</td>
<td>292,450</td>
<td>55,916</td>
<td>34,083</td>
<td>1,665,553</td>
</tr>
<tr>
<td></td>
<td>21.6%</td>
<td>55.4%</td>
<td>17.6%</td>
<td>3.4%</td>
<td>2.1%</td>
<td></td>
</tr>
<tr>
<td>1980</td>
<td>400,730</td>
<td>929,417</td>
<td>298,081</td>
<td>70,131</td>
<td>32,615</td>
<td>1,730,974</td>
</tr>
<tr>
<td></td>
<td>23.2%</td>
<td>53.7%</td>
<td>17.2%</td>
<td>4.1%</td>
<td>1.9%</td>
<td></td>
</tr>
<tr>
<td>1985</td>
<td>454,712</td>
<td>979,477</td>
<td>286,251</td>
<td>75,063</td>
<td>32,943</td>
<td>1,828,446</td>
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<tr>
<td></td>
<td>24.9%</td>
<td>53.6%</td>
<td>15.7%</td>
<td>4.1%</td>
<td>1.8%</td>
<td></td>
</tr>
<tr>
<td>1990</td>
<td>455,102</td>
<td>1,051,344</td>
<td>324,301</td>
<td>70,988</td>
<td>38,371</td>
<td>1,940,106</td>
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<tr>
<td></td>
<td>23.5%</td>
<td>54.2%</td>
<td>16.7%</td>
<td>3.7%</td>
<td>2.0%</td>
<td></td>
</tr>
<tr>
<td>1995</td>
<td>539,691</td>
<td>1,160,134</td>
<td>397,629</td>
<td>75,800</td>
<td>44,446</td>
<td>2,217,700</td>
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<tr>
<td></td>
<td>24.3%</td>
<td>52.3%</td>
<td>17.9%</td>
<td>3.4%</td>
<td>2.0%</td>
<td></td>
</tr>
<tr>
<td>2000</td>
<td>564,933</td>
<td>1,237,875</td>
<td>457,046</td>
<td>80,057</td>
<td>44,808</td>
<td>2,384,719</td>
</tr>
<tr>
<td></td>
<td>23.7%</td>
<td>51.9%</td>
<td>19.2%</td>
<td>3.4%</td>
<td>1.9%</td>
<td></td>
</tr>
<tr>
<td>2005</td>
<td>696,660</td>
<td>1,439,264</td>
<td>574,618</td>
<td>87,289</td>
<td>52,631</td>
<td>2,850,462</td>
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<tr>
<td></td>
<td>24.4%</td>
<td>50.5%</td>
<td>20.2%</td>
<td>3.1%</td>
<td>1.9%</td>
<td></td>
</tr>
<tr>
<td>2009</td>
<td>787,325</td>
<td>1,601,368</td>
<td>656,784</td>
<td>92,004</td>
<td>67,716</td>
<td>3,205,197</td>
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<td></td>
<td>24.6%</td>
<td>50.0%</td>
<td>20.5%</td>
<td>2.9%</td>
<td>2.1%</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>2009</td>
<td>+ 282%</td>
<td>+ 102%</td>
</tr>
<tr>
<td></td>
<td>+ 102%</td>
<td>+ 215%</td>
</tr>
<tr>
<td></td>
<td>+ 102%</td>
<td>+ 215%</td>
</tr>
<tr>
<td></td>
<td>+ 163%</td>
<td>+ 127%</td>
</tr>
<tr>
<td></td>
<td>+ 152%</td>
<td>+ 152%</td>
</tr>
</tbody>
</table>

Note: Author calculations from data from degree-granting institutions that grant associate's or higher degrees and participate in Title IV federal financial aid programs. Source: National Center for Education Statistics, 2010 Digest of Education Statistics, Table 287.
<table>
<thead>
<tr>
<th>Table 1.5. Postsecondary Sector by Family Income, 2007-8</th>
<th>&lt; $40k</th>
<th>40k – 79.999</th>
<th>80k-119.999</th>
<th>&gt;= 120k</th>
</tr>
</thead>
<tbody>
<tr>
<td>Public 2 year</td>
<td>39%</td>
<td>37%</td>
<td>28%</td>
<td>17%</td>
</tr>
<tr>
<td>Public 4 year</td>
<td>32%</td>
<td>36%</td>
<td>44%</td>
<td>45%</td>
</tr>
<tr>
<td>Private Not for profit</td>
<td>12%</td>
<td>14%</td>
<td>18%</td>
<td>26%</td>
</tr>
<tr>
<td>For profit</td>
<td>8%</td>
<td>4%</td>
<td>2%</td>
<td>1%</td>
</tr>
<tr>
<td>Other</td>
<td>9%</td>
<td>9%</td>
<td>9%</td>
<td>10%</td>
</tr>
</tbody>
</table>

Source: Author calculations from National Center for Education Statistics, 2011.
Table 1.6. Estimated Average Total Cost of Attendance over Time

<table>
<thead>
<tr>
<th>Year</th>
<th>4-year Public</th>
<th>4-year Private</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970</td>
<td>$1,287</td>
<td>$2,530</td>
<td>1.97</td>
</tr>
<tr>
<td>1980</td>
<td>$2,327</td>
<td>$5,013</td>
<td>2.15</td>
</tr>
<tr>
<td>1990</td>
<td>$4,975</td>
<td>$12,284</td>
<td>2.47</td>
</tr>
<tr>
<td>2000</td>
<td>$8,275</td>
<td>$20,706</td>
<td>2.50</td>
</tr>
<tr>
<td>2005</td>
<td>$11,426</td>
<td>$26,257</td>
<td>2.30</td>
</tr>
<tr>
<td>2010</td>
<td>$15,014</td>
<td>$35,061</td>
<td>2.34</td>
</tr>
</tbody>
</table>

Note: Figures are listed in current dollars. Source: National Center for Education Statistics, 2010 Digest of Education Statistics, Table 345.
From: http://nces.ed.gov/fastfacts/display.asp?id=76
Table 1.7. College Major by Share and Average SAT Scores

<table>
<thead>
<tr>
<th>Major</th>
<th>Share</th>
<th>SAT math</th>
<th>SAT verbal</th>
</tr>
</thead>
<tbody>
<tr>
<td>Art and art history</td>
<td>1.5%</td>
<td>555</td>
<td>592</td>
</tr>
<tr>
<td>Biological sciences</td>
<td>4.5%</td>
<td>577</td>
<td>575</td>
</tr>
<tr>
<td>Business management and administration</td>
<td>6.8%</td>
<td>522</td>
<td>510</td>
</tr>
<tr>
<td>Communications</td>
<td>2.9%</td>
<td>512</td>
<td>537</td>
</tr>
<tr>
<td>Computer and information technology</td>
<td>2.9%</td>
<td>582</td>
<td>556</td>
</tr>
<tr>
<td>Economics</td>
<td>2.1%</td>
<td>597</td>
<td>573</td>
</tr>
<tr>
<td>Finance</td>
<td>2.2%</td>
<td>563</td>
<td>534</td>
</tr>
<tr>
<td>History</td>
<td>2.3%</td>
<td>558</td>
<td>595</td>
</tr>
<tr>
<td>Marketing</td>
<td>2.3%</td>
<td>526</td>
<td>514</td>
</tr>
<tr>
<td>Mathematics</td>
<td>1.5%</td>
<td>592</td>
<td>538</td>
</tr>
<tr>
<td>Mechanical engineering</td>
<td>1.5%</td>
<td>613</td>
<td>566</td>
</tr>
<tr>
<td>Political science</td>
<td>2.4%</td>
<td>542</td>
<td>571</td>
</tr>
<tr>
<td>Psychology</td>
<td>4.8%</td>
<td>530</td>
<td>540</td>
</tr>
<tr>
<td>Social work and human resources</td>
<td>1.5%</td>
<td>460</td>
<td>487</td>
</tr>
</tbody>
</table>

Note: Selected majors reproduced from Altonji, Blom, and Meghir 2012, Table 1. Original source: 1993/2003 National Center for Education Statistics’ Baccalaureate and Beyond Survey.
<table>
<thead>
<tr>
<th>Table 1.8. Mean Wages of Selected College Majors</th>
<th>Mean Wages</th>
</tr>
</thead>
<tbody>
<tr>
<td>Art and art history</td>
<td>$25,570</td>
</tr>
<tr>
<td>Biological sciences</td>
<td>$27,260</td>
</tr>
<tr>
<td>Business management and admin</td>
<td>$31,560</td>
</tr>
<tr>
<td>Communications</td>
<td>$28,170</td>
</tr>
<tr>
<td>Computer and information tech</td>
<td>$35,830</td>
</tr>
<tr>
<td>Economics</td>
<td>$43,150</td>
</tr>
<tr>
<td>Finance</td>
<td>$38,210</td>
</tr>
<tr>
<td>History</td>
<td>$29,520</td>
</tr>
<tr>
<td>Marketing</td>
<td>$32,900</td>
</tr>
<tr>
<td>Mathematics</td>
<td>$37,760</td>
</tr>
<tr>
<td>Mechanical engineering</td>
<td>$40,430</td>
</tr>
<tr>
<td>Political science</td>
<td>$33,320</td>
</tr>
<tr>
<td>Psychology</td>
<td>$24,610</td>
</tr>
<tr>
<td>Social work and human resourc</td>
<td>$23,190</td>
</tr>
</tbody>
</table>

Note: Selected majors reproduced from Altonji, Blom, and Meghir 2012, Supplemental Table 2. Original source: 2009 American Community Survey. Wages shown only for individuals with a bachelor’s degree, age 23-59, working 34-40 hours per week.
CHAPTER 2.
THE INFLUENCE OF RACE AND GENDER

Race and Gender Differences in Educational Credentials and Labor Market Outcomes

The early status attainment literature examined the effects of educational attainment on a variety of labor market outcomes, although, as previously mentioned, much of this literature focused only on white men. Since the 1970s, non-whites (see Table 2.1) and women (see Table 2.2) have closed gaps in educational attainment with whites and men respectively and now make up an increasingly larger share of bachelor's degree holders. But do these individuals get the same returns to their educational credentials as white men? And if they do not, how can we explain this inequality? In this section, I review the existing literature and some supplementary data on educational credentials by race and gender before moving to the question of why race and gender matter.

In his landmark and controversial book *The Declining Significance of Race* (1978/1980), William Julius Wilson argued that the progress of blacks in achieving upward mobility had changed the landscape of inequality to the point that social class was more important than race. As Table 2.1 shows, there is no arguing that blacks experienced dramatic increases in educational attainment levels between 1940 and 1980. Research indicates that racial income inequality decreased during this time period as well, although racial gaps in unemployment grew (Farley and Allen 1987). Wilson
(1979) followed his first book with a more intricate argument that pointed to minimal racial wage gaps among young bachelor's degree holders and large racial gaps among older and less educated workers. His work speculated that racial parity in wages was not far down the road.

Sometime during the mid-to-late 1980s, the racial wage parity path took a left turn. Since then, a number of scholars have demonstrated the existence of racial differences in wages and occupational status among individuals with similar levels of educational attainment (Bernstein 1995; Smith and Welch 1989; Weinberger 1998; also see Leicht 2008 and Morris and Western 1999 for reviews). Data from the early 2000s suggest that among bachelor's degree holders, black men make approximately 75% of the wages of white men (Bradbury 2002; Hacker 2003) and Hispanic men make approximately 80% of the wages of white men (Bradbury 2002). These differences have grown over time with the expansion of educational attainment (Bradbury 2002; Cancio, Evans, and Maume 1996). Evidence also suggests that racial inequality increases over the career but is typically lowest at the point of entry into the labor market (Tomaskovic-Devey, Thomas, and Johnson 2005). Perhaps one of the most important findings from the post-Wilson literature is that higher educational attainment results in higher racial inequality in wages, at least between blacks and whites (Cotton 1989; Thomas 1993, 1995; Thomas and Horton 1992; Thomas et al. 1994, 1995; Tomaskovic-Devey, Thomas, and Johnson 2005; Grodsky and Pager 2001).

Since the 1940s, women have experienced significant gains in educational attainment compared to men (see Table 2.2). The increasing numbers of female college graduates on the labor market have been met with increasing wages relative to men
In the early 1980s, women with a bachelor's degree or more earned approximately 65% as much as their male counterparts. By 2000, this figure had increased to 75%, although still a significant gap between men and women (ibid). Similar to the scholarly attention on racial gaps, there is a rich literature on gender differences in wages and occupational status among individuals with similar levels of educational attainment (Bertrand, Goldin, and Katz 2010; Black, Haviland, Sanders, and Taylor 2008; Blau, Ferber, and Winkler 2001; Bradbury 2002; Brown and Corcoran 1997; Joy 2003, 2006; McDonald and Thornton 2007; Weinberger 1998). This research also suggests that gender inequality in wages increases over the career (Marini 1989). Unlike racial wage gaps, these differences have shrunk over time with the expansion of educational attainment (Blau, Ferber, and Winkler 2001; Brown and Corcoran 1997; Weinberger and Kuhn 2010).

Now let us return to recent data from the Bureau of Labor Statistics to examine the connections between educational attainment and labor market outcomes cross-tabulated by race and gender. As Table 2.3 indicates, educational attainment is negatively correlated with unemployment rates. Among individuals with a high school degree, Asians have the lowest unemployment rates, followed by whites, Hispanics, and then blacks. The unemployment rate among individuals with at least a bachelor's degree is lower for all racial groups, but the return to education is greatest among whites (54.7% reduction in the unemployment rate). This pattern is similar between men and women. Both groups have lower unemployment rates with higher levels of educational attainment, but the return to education is greater among men (57.5% reduction in the unemployment rate).
The data from Table 2.4 tell a similar story. The wages for all groups increase as we move across the educational attainment spectrum. However, among individuals with a bachelor's degree or more, Asians and whites are close in wages and then there is a big gap to the wages of Hispanics and blacks. Overall, the return to education is greatest among Asians (139% premium over high school degree holders), followed by Hispanics and whites, and then lowest among blacks (88% premium). If we consider gender, men once again have a greater return to education than women (121% premium for bachelor's degree or more over high school degree holders).

Examination of these data is a simple correlational exercise designed to establish a baseline of differences, but researchers continue to work to identify the reasons behind these gaps. Scholars routinely diverge in their beliefs behind why these ascriptive characteristics matter and what other characteristics and variables can help explain or support these beliefs. In the next section, I review the theoretical debate and supporting empirical research in these areas.

Why Do Race and Gender Matter?

It’s clear that race and gender matter in the labor market. But scholars question why inequality still exists among the highly educated. These discussions often pit economists who argue that effects stem from human capital\(^1\) differences, or different levels of ability, skills, and effort (Becker, 1985; Card and Krueger 1990; Farkas and Vicknair 1996; Neal and Johnson 1996; Polachek, 1979; Smith and Welch 1989), against sociologists who argue that effects stem from employer biases and discrimination (Lucas

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\(^1\) It is important to note that economists refer to ability, skills, effort, and educational credentials as human capital. However, because researchers cannot easily separate out mechanisms such as human capital and cultural capital, I use the broader term “educational credentials” instead of “human capital” where possible in the rest of this section.
2008; Pager 2007a; Petersen and Saporta 2004). Resolving this debate is important, as each side suggests very different solutions to modern economic inequality. But a number of limitations, in reference to research methods, stand in the way.

Portions of the research presented below apply generally to individuals and groups in the labor force. The research doesn't always necessarily examine individuals at the time of labor market entry but rather later in their careers after an accumulation of human capital. At the end of this section, I argue that the combined literature on educational credentials and discrimination suggests that employers have information about prior or current employees (whether accurate or not) that may shape the experiences of prospective employees in race and gender differentiated ways.

A. Why Do Race and Gender Matter? Human Capital

To examine one explanation for racial and gender economic inequality, we once again return to human capital theory. Scholars in this tradition argue that employers look to make the best possible investments in terms of who they hire. If employers believe that race or gender influences the abilities, skills, or effort of potential employees, even among the same levels of educational attainment, non-whites and women will have worse labor market outcomes. At the heart of this research is an effort to explain differences in outcomes based on a number of variables, such as educational preparation, knowledge or IQ, effort, selection of major and experience, which are undoubtedly correlated.

Early work on racial differences and human capital suggested that differences in the quality of schools (both elementary and secondary) that blacks and whites attended was at least part of the reason for different returns to educational attainment in the labor market (Card and Krueger 1990; Farkas and Vicknair 1996; Margo 1986; O'Neil 1990;
One way to measure human capital and proxy for this difference in education quality is to include some measure of cognitive skill (typically a standardized test score) separate from the measure of educational attainment. In the controversial *The Bell Curve* (1994), Richard Herrnstein and Charles Murray suggest that Armed Forces Qualification Test (AFQT) scores account for nearly all of the difference in the white/black wage gap. Other research finds similar results for both white/black and white/Hispanic comparisons (Farkas, England, Vicknair, and Kilbourne 1997). Thus, this branch of research suggests that some, if not all, of the differences in returns to educational attainment is a function of cognitive skill.

One critique of the human capital model is a practical one. Critics point out that the model fails to explain how employers separate these abilities and skill out from educational attainment when making choices about hiring. Another critique of the human capital model is a methodological one. In an effort to figure out what human capital is, researchers control for too many correlated variables without understanding the processes of how these characteristics shape each other (Tomaskovic-Devey, Thomas, and Johnson 2005). More recent research that attempts to more accurately model these processes finds racial differences in the return to education even after accounting for human capital (Alon and Haberfeld 2007; Tomaskovic-Devey, Thomas, and Johnson 2005).

Scholars also use the human capital model to explain gender inequality in earnings (Becker, 1985; Polachek 1979). Similar to the work on racial differences and human capital, scholars suggest that differences in cognitive skills explain some of the differences in returns to educational attainment in the labor market (Blau and Kahn 2007; Farkas et al. 1997; Filer 1983; Paglin and Rufolo 1990), although gender differences in
standardized tests, particularly the SAT, tend to be small (Hyde 2005; Korbin, Sathy, and Shaw 2007). However, there are two additional explanations within the human capital framework that are of interest that apply to gender: effort and college major.

One branch of the human capital framework states that the main contribution in wage inequality between men and women is effort. The argument is that women are more “drained” than men from housework, such as cooking, cleaning, and taking care of children, and thus have less energy and motivation in their work lives. Women are less valuable to employers than men, less productive in the workplace, and get less human capital out of their occupational experience. This model represents a bifurcated system of gender responsibilities leading to differences in desirability in the workplace by employers. Thus, in Becker's argument, equalization in education and occupational experience would not eliminate the wage gap, as women would still maintain an “effort gap”. Some research in the 1980s suggested that women actually devoted more effort to work than men but the results were preliminary evidence against the human capital model at best (Major, McFarlin, and Gagnon 1984). Additional secondary data-analysis in the late 1980s provided stronger evidence of women exerting more work effort than men but further questions remained (Bielby and Bielby 1988). However, economists have additionally considered that women intentionally choose jobs that are more flexible in accepting the timeline, responsibilities, and other constraints of motherhood. The qualities of these jobs include better working conditions, more flexible hours, and less demanding jobs. Thus, under this theory, occupational segregation results from women choosing certain job qualities which are much different than the job qualities prioritized by men. Once again, early research found evidence against this argument, as Rosenfeld
(1983) found that marriage status (a cornerstone of the argument) did not affect typification of job changes for women. Thus, the evidence for all aspects of the human capital model is mixed.

In response to the economic model of human capital and to better understand the gender wage gap, Paula England has undertaken a number of research studies over the years which examine gender wage inequality (e.g. England et al. 1988; England 1992; Budig and England 2001). In early work, England and colleagues attempt to determine the effect of the percentage of women in an occupation on wages (England et al. 1988). The authors use fixed-effects models to eliminate the selection bias of unmeasured individual time-invariant characteristics such as human capital. The variation in occupational percent female is picked up on an individual level: when a woman changes jobs. Additionally, changes in experience are captured for the individual over time. This longitudinal fixed-effects model represents a powerful advantage over the previous cross-sectional models. The authors' results indicate that there is a direct negative effect of the gender composition of an occupation on wages: the higher the percentage of women in an occupation, the lower the wages, “[n]et of human capital, skill demands, and working conditions” (1988; 554). This research leaves us to question the hypothesis that human capital or compensating differentials are at work in explaining the wage differential in sex-segregated occupations.

As the debate evolved, the question of a wage penalty for mothers and not just women became salient in the discussion. If the human capital model holds any weight, it should be specifically seen in the explanation of the wage gap for mothers. In 2001, Budig and England published a study which addressed the issue. In this study, the
authors attempted to assess five potential explanations for “the wage penalty of motherhood”: (1) motherhood results in less job experience and full-time employment, (2) motherhood results in a different importance of job characteristics (compensating differentials), (3) motherhood reduces effort and productivity at work, (4) motherhood is subject to discrimination in the job market, and (5) a potential spurious effect. Once again, using a longitudinal dataset and fixed-effects models, the authors remove omitted variable bias and address the question in a more sophisticated way than prior cross-section research had done. In addition, the authors make a strong case by showing the results of a “gross effect of motherhood” with their data using a cross-sectional OLS model (indicating an 11% motherhood penalty per child), a pooled OLS model (8%), and a fixed-effects model (7%). These results indicate some reduction of bias just from using a fixed-effects models. Their final models indicate about a 4% wage penalty for a first child, which they suggest could come from the discrimination or productivity explanations that they cannot control for. More recent research finds that the motherhood penalty is lower for college graduates than for high school graduates, suggesting the effort hypothesis of the human capital theory does not entirely account for this effect (Anderson, Binder, and Krause 2003). Overall, this research presents a relatively strong case against the human capital model.

A final branch of the human capital framework suggests that college major influences either actual human capital or employers perceptions and thus explains a portion of the gender gap in the return to educational attainment (Bobbitt-Zeher 2007; Bradley 2000; Brown and Corcoran 1997; Davies and Guppy 1997; Gerber and Schaefer 2004; Joy 2000; Zhang 2008). These studies find that anywhere from 25% to 50% of the
gender difference in wages among college graduates comes from selection of major. However, this literature is still relatively underdeveloped when compared to other areas of inquiry into gender gaps in labor market outcomes. More recent work finds that even among similar types of majors, in terms of their relatedness to occupational specificity, female college graduates lag behind their male counterparts in the return to wages (Roksa and Levey 2010). These studies suggest college major, which is easily measured and controlled, plays an important role in the gender differences in labor market outcomes.

The basis of the human capital argument is that employers have imperfect information and thus must attempt to hire the best employees based on what they can observe or infer from other characteristics. These judgments may come from knowledge of aggregate group characteristics or past experiences which, in turn, shape beliefs and hiring decisions (Pager and Karafin 2009). In their research, Pager and Karafin (ibid) find that past experiences with black workers do not seem to influence employers' racial attitudes. The authors suggest that knowledge of aggregate group characteristics and cultural and media stereotypes may influence race and gender effects on labor market outcomes. I suggest that this places more emphasis on even small differences in educational credentials between race and gender groups at various education levels and can lead to statistical discrimination.

B. Why Do Race and Gender Matter? Discrimination

While some researchers in the human capital tradition believe that differences in ability and skills between whites and blacks or men and women are the cause of racial and gender gaps in labor market outcomes, the evidence is ambiguous. A number of scholars suggest that discrimination must account for some portion of race and gender
differences in labor market outcomes. Measuring discrimination requires a clear and precise definition (Blank, Dabady, and Citro 2004; Lucas 2008). Because discrimination is both an issue of concern for the legal system in the U.S. and a phenomenon for examination through social science research, definitions of discrimination vary. The legal definition of discrimination encompasses two parts: (1) disparate treatment and (2) disparate impact (Blank et al. 2004; Lucas 2008). Disparate treatment discrimination focuses on the actions of individuals who treat non-whites (or women) differently than whites (or men). Under the legal definition, disparate treatment discrimination also requires explicit proof of discrimination based on a specific characteristic (e.g. race or gender). For example, a real estate agent who tells potential black clients that he does not work with non-whites has committed disparate treatment discrimination. However, disparate impact discrimination is less straightforward. This type of discrimination occurs when criteria used for treatment (i.e. hiring decisions, admissions, loan application approval, amount of raise in wages, etc.) are not based on race or gender but result in differences between groups nonetheless. Additionally, the criteria must be judged unnecessary and illegitimate in the selection process to prove disparate impact discrimination under the law, thus resulting in a higher burden of proof than disparate treatment discrimination.

Envisioning discrimination in economic terms, Gary Becker (1957) suggests that individuals have a certain “taste for discrimination.” Becker sees discrimination as an individual choice of employers that costs them money. Not all owners of capital want to discriminate, nor at the same levels. Thus, participating in discrimination has penalties for multiple actors. In his work, Becker employs equations to show how these various
actors lose from an employer's taste for discrimination. In essence, he argues that
employers pay a premium to hire only white or male workers due to differences between
white and black wages and male and female wages. Arguably, Becker's work led to the
use of statistical analysis of residuals in discrimination research. His use of equations to
measure a residual proved an easy way to avoid direct observation of discrimination and
also did not require researchers to understand the individual motivations behind
discrimination (Lucas 2008). Although this opened the door to more examination of
discrimination in general, it also setup a defensive position in explaining discrimination
due to the lack of explicitly measured causes. Lucas (2008) maintains that Becker's
critical contribution to discrimination research has been three very important points in
understanding discrimination:

“(1) the recognition that, even in the absence of discrimination, equality between
groups may not prevail; (2) the recognition that the targeted groups are not the
only ones to pay costs of discrimination; and (3) the recognition that the amount
of discrimination and the experience of discrimination are not the same.” (163)

These three points have been all but forgotten in research design involving measurement
of discrimination. As Lucas notes, researchers have instead maintained Becker's use of a
residual as discrimination research has been forced to move towards an examination of
more subtle forms of discrimination.

Another theory of discrimination that gained traction during the 1970s is
statistical discrimination. This theory suggests that employers are unable to collect
perfect data on job applicants and thus use group averages on certain characteristics to
make their decisions about individuals (Aigner and Cain 1977; Arrow 1973b; Phelps
1972). Thus, they may view non-whites and women as less intelligent in general and
penalize an individual job applicant without knowing her true intelligence. The theory of
statistical discrimination differs from Becker's theory because employers do not necessarily have a malicious intent in mind, but rather are making rational judgment calls in a world of imperfect information (Arrow 1973b; Phelps 1972). There are at least two problems with this employer strategy. First, employers may have incorrect information regarding a particular group's characteristics. This may include a stereotype of a group that in reality doesn't hold for a majority of the group. Second, employers judge individuals on the basis of group averages (whether accurate or not) and may rule out individuals whose characteristics differ from the average. Some research also suggests that employers may also be able to screen for difficult to observe characteristics only when job candidates are similar to themselves (Cornell and Welch 1996). Additionally, a self-perpetuating cycle may form, reducing incentives for non-whites and women to apply for certain positions and reducing returns to education (Coate and Loury 1993; England 1992; Lundberg and Startz 1998).

Explicit examination of discrimination in the labor market is an important but difficult endeavor for social science research. Some scholars attempt to disentangle other competing effects (such as the human capital theorists suggest) by focusing on the possibility of discrimination for entry-level job seekers (Bendick, Jackson, and Reinoso 1994; Bertrand and Mullainathan 2004; Kirschenman and Neckerman 1991; Pager 2007a). Although no one can argue against the merit of Title VII of the Civil Rights Act of 1968, which gives individuals the right to sue discriminatory employers, it appears to have made identifying discriminatory actions more difficult while not eliminating discrimination entirely. As Doug Massey states, “when pushed by the federal government to end overt discriminatory practices, [whites] are likely to innovate new and
more subtle ways to maintain their privileged position in society.” (2007:54).

To examine the literature on race and gender discrimination in the labor market, let us first turn to the rich set of studies using interviews with employers. This research lends some credibility to the idea that statistical discrimination drives employer decisions but even the authors of these studies suggest caution in that interpretation. Perhaps the most well-known article on this topic comes from interviews with employers in Chicago (Kirschenman and Neckerman 1991). This research was designed to focus on entry-level jobs and capture a generalizable sample of employment for those jobs while also oversampling inner-city businesses. The authors uncovered a number of key beliefs and actions of employers that led to discrimination in the labor market. First, many employers conflated race, class, and geographic location within Chicago. For instance, most employers made the assumption that non-whites were lower class and came from the inner-city or south side of Chicago, while whites were thought of as middle class and from the suburbs or north side of Chicago. Second, the authors found that statistical discrimination appeared to shape many employers' labor forces. Non-whites, especially blacks, were thought of as having a low work-ethic, attitude problems, and low skills. The authors consistently found employers referencing their experiences with a group as a whole and referring to characteristics on average: the definition of statistical discrimination. Third, some discrimination was based on an applicant's educational credentials, because “[b]eing educated in Chicago public schools [had] become a way of signaling 'I'm black, I'm poor, and I'm from the inner city' to employers” (215). Employers participate in statistical discrimination through all three of these judgments: people of a certain class or a certain geographic location must be non-white and people
with a certain education must be non-white and non-whites are less desirable employees because of certain average traits. The authors note that employers sometimes did hire non-whites but the bar was set higher for these applicants and the applicants had to prove that they did not fit the stereotypes that employers held. The findings also suggest that although Hispanics faced discrimination, many employers were more willing to hire Hispanics than blacks, a finding that was confirmed in Los Angeles as well (Waldinger and Lichter 2003).

In addition to the Chicago interviews, a large scale study known as the Multi-City Study of Urban Inequality used employer interviews and found similar responses from employers in Atlanta, Boston, Detroit, and Los Angeles (Moss and Tilly 2001). Other research based on interviews with employers indicates that employers engage in statistical discrimination but also may be fearful of discrimination lawsuits if they hire a black person and must terminate them for some reason (Wilson 1996). Some employers believe certain immigrant groups have more desirable average characteristics than blacks (Waldinger 1997) or just simply will work harder for lower wages (Thomas 2003).

Kirschenman and Neckerman's (1991) research also revealed that employers may engage in discrimination because of customer preferences. At least one employer stated that customers would not be happy with non-white employees so he would not hire them. This idea is similar to one promoted in the literature as “soft skills” or the focus on employee-customer interaction (Cappelli 1995; Moss and Tilly 1996) which may just be a discrimination code word for denying employment or channeling non-whites into lower paying jobs away from customers (Pager, Western, and Bonikowski 2009).

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2 This could be a slightly altered form of Becker's (1957) “taste for discrimination” where employers are at the whim of their customers' taste. Of course we cannot discount that this could also be an excuse for an employer to hide his own taste of discrimination.
Finally, a number of studies using interviews link employer discrimination with social capital (see more below). Employers do so by advertising in ethnically targeted newspapers or keeping job information within racially segregate networks by relying heavily on word-of-mouth from current employers for recruitment (Kasinitz and Rosenberg 1996; Moss and Tilly 2001; Kirschenman and Neckerman 1991). These processes allow employers to keep the labor force at individual firms racially segregated without having to explicitly turn down particular candidates.

These interviews go a long way in helping to explain why employers discriminate. However, they also raise a pertinent methodological question: do the views expressed really represent statistical discrimination, or do employers hide their prejudice behind what they believe to be as a more socially acceptable view. This point is critical to understanding discrimination in the labor market. It addresses the theoretical connections and, in the latter case, suggests that raising employer awareness of the inaccuracies of stereotypes might help alleviate some racial discrimination.

An increasingly popular type of research method for examining discrimination is an employment audit study. In these studies, two auditors are matched with similar characteristics to narrow the focus of differential outcomes to an explanation of race or gender (more details on this methodology below). This research consistently finds that whites receive call-backs and interviews at higher rates than blacks and Hispanics (Pager 2003, 2007a; Pager, Western, and Bonikowski 2009). In a study conducted in Washington, D.C. and Chicago, blacks were more likely than whites to face resistance and negative comments during the application process and less likely to be able to submit an application, to be offered an interview, and to be offered a job (Turner, Fix, and Struyk
A similar design implemented in San Diego and Chicago found that Hispanics were also less likely than whites to progress through the three stages tested (Cross et al. 1990). Additional audits studies have shown that blacks have lower rates of call-backs than whites in Milwaukee (Pager 2003, 2007a) and New York (Pager, Western, and Bonikowski 2009), while Hispanics have lower rates of call-backs than whites in Washington D.C. (Bendick, et al. 1991) and New York (Pager, Western, and Bonikowski 2009).

An alternative type of audit study, sometimes referred to as a correspondence study, has also been utilized for studying labor market discrimination. In this type of experiment, newspaper ads are used to find jobs and resumes are sent through the mail. Although this method has been used to examine racial discrimination in Britain (see Riach and Rich 2002 for a review of these studies), this method has seen limited use in the U.S. Bertrand and Mullainathan (2004) conducted a correspondence audit study in Boston and Chicago by assessing the call-backs of blacks and whites using high-quality and low-quality resumes. In this study, race was conveyed by using ethnic-sounding names. Their results indicated that whites with both high-quality (10.79%) and low-quality (8.50%) resumes received call-backs at rates greater than blacks with high-quality (6.70%) resumes. In fact, blacks with low-quality (6.19%) resumes only fared marginally worse than those with high-quality resumes. The growing number of experimental studies that find racial differences in labor market outcomes, coupled with the magnitude of their findings, leaves one to conclude that race is still an important factor in labor market outcomes.

One similar correspondence audit study found that women without children were
more likely to get call-backs than women with children, men with children, and men without children, suggesting that perhaps women have an advantage over men in the labor market only if they do not have children (Correll, Benard, and Paik 2007). In a variation of the labor market audit study Foschi and Valenzuela (2008) use test subjects to rate hypothetical candidates instead of using real world scenarios and find no differences in ratings between men and women candidates and likelihood of hire. Still, yet research examining the hiring processes of U.S. symphony orchestras found that in instances where auditions were blind, women were much more likely to be hired, suggesting substantial gender discrimination in non-blind processes (Goldin and Rouse 2000).

C. Why Do Race and Gender Matter? Affirmative Action and Diversity Policies

One final reason why race and gender might be important in the labor market is for diversity reasons. Employers with federal government contracts and fifty or more employees are required to have an affirmative action plan, although many firms that do not meet these guidelines have voluntary plans. Additionally, all employers with at least fifteen employees are covered by anti-discrimination laws enforced by the U.S. Equal Employment Opportunity Commission (EEOC) and must state their status as an equal opportunity employer. Many firms today include diversity plans ranging from managerial and human resource personnel training highlight the effects of hiring decisions to outreach programs. Whether employers legitimately want to create a diverse workplace, adhere to legal requirements, or simply reach minimum diversity threshold levels to avoid public problems, it is possible that black and female candidates might receive a boost during the job application process.

Research specifically on affirmative action is difficult to undertake, as a large
portion of employers are subject to the law. However, many of these studies find that women and minorities are hired at greater rates in firms that are subject to affirmative action rather than those that are not (Carrington, McCue, and Pierce 2000; Leonard 1984, 1990). For instance, Holzer and Neumark (1999) found across four urban areas that white men held about 15-20% less of the total share of jobs in affirmative action firms than in non-affirmative action firms. Overall, the research is relatively consistent in suggesting that affirmative action has positive but modest effects on the employment prospects of black and female candidates (also see Baron, Mittman, and Newman 1991; Heckman and Payner 1989; Heckman and Wolpin 1976; Holzer and Neumark 2006). Scholars debate the appropriateness of such methods to detect a true causal effect and whether these effects are of large substantive importance. Additionally, although there is some tentative evidence on differences across the occupational structure (see Kurtulus 2012), it is unclear whether we should expect affirmative action policy to have a larger effect early in an individual’s career rather than later and how the effect size might vary based on educational credentials.

Above and beyond affirmative action, employers may institute diversity plans for a variety of reasons, including hiring more minorities and women. Of primary importance to the present research is diversity plans that provide training to employees to reduce bias in the hiring process (see Kalev, Dobbin, and Kelly 2006 and Dobbin, Kim, and Kalev 2011 for more on other types of plans). The research on the ability of firms to proactively combat discrimination through this type of training is sparse, but one such study suggests that these plans have only modest positive effects on the promotion of white women to management and negative effects on the promotion of black men and
women to management (Kalev, Dobbin, and Kelly 2006). Additional research finds no positive effects of diversity plans for blacks and women in the labor market (Dobbin and Kalev 2007; Edelman and Peterson 1999) and employers often do not use existing diversity criteria in hiring decisions (Rivera 2012a).

Although much debate continues throughout the literature regarding how to define and measure human capital, whether discrimination still exists, at what levels, and for whom, and how affirmative action and diversity policies shape employment outcomes, the theory and research presented in this section suggest that non-whites and women do not experience the same returns to educational attainment in the labor market as their white and male counterparts. It is important to consider the intersection of these theories. If employers think that non-whites and women have less human capital from the same levels of educational attainment as whites and men, whether they actually have less human capital or not does not matter. Thus, employers are likely to engage in statistical discrimination, just as some of the research shows. However, it is important to recall that employers may use more detailed qualitative information from educational credentials to make employment decisions absent other measures of human capital. In the next section, I suggest that these race and gender differences in labor market outcomes may shrink or expand among college graduates based on where individuals obtain their degree.

How Might Race and Gender Interact with Educational Credentials?

In the previous portions of this section, I established that race and gender affect labor market outcomes and outlined the theories suggesting the reasons for this effect. However, the drastically different landscape of educational attainment for non-whites and women of the present day (see Tables 2.1 and 2.2) raises suspicions about the salience of
these effects. Do these theories still apply to the modern labor market? If so, how might they be different? Examining some data that may contribute to employers' beliefs may lend credence to human capital and discrimination theories in connection to college selectivity.

A portion of researchers' understanding of the effects of race and gender in reference to educational attainment and labor market outcomes is outdated and misleading. A number of studies examine data (nationally representative or otherwise) that include cohorts who graduated before non-whites and women made huge strides in educational attainment. Nearly 3.5x as many blacks and women have bachelor's degrees in 2010 as they did in 1970 (see Tables 2.1 and 2.2). Undoubtedly, employers beliefs about human capital and their ability to discriminate are drastically different in 2010 than in 1970. However, just as I suggested in Section 1, employers may now turn to more nuanced views of the vast category of bachelor's degree holders.

One possibility that could explain race and gender differences in the labor market in regards to college selectivity is differences in admission across levels of selectivity. If non-whites and women have lower attendance rates at the most selective colleges, and employers value degrees from these colleges at higher rates, then inequality might reflect differences in educational credentials more than discrimination. Data from college cohorts in the 1990s indicate that the most selective colleges admit smaller percentages of female and black students than less selective colleges (Soares 2007:174-5). Additional research confirms that competition for the coveted spots in these institutions translates into a higher education system stratified by race, as black students are much less likely to attend highly selective institutions than white students (Alon and Tienda 2007; Bowen
and Bok 2000; Carnevale and Rose 2003).

If employers look to hire candidates with the best skills and abilities, I suggested earlier that they may turn to candidates from more selective colleges and with particular college majors to capitalize on the higher cognitive abilities of these graduates. Rational employers may have information or make inferences about whether the gaps in cognitive ability between whites and non-whites and men and women are lower at more selective colleges than less selective colleges. If, say, blacks and whites at more selective colleges have similar SAT scores, there may be no moderating effect of race for more selective colleges because there is no difference in human capital. Conversely, if blacks and whites at less selective colleges have very large gaps in SAT scores, there may be a substantial moderating effect of race for less selective colleges.

Unfortunately, I am unable to currently test this hypothesis but limited data from a cohort in the 1980s suggests that the gap in SAT scores between whites and blacks is larger in the top quintile of selective colleges than in the bottom four quintiles of selective colleges (Kane 1998; Kane and Dickens 1996). Similar data on the differences in SAT scores between women and men by college selectivity are not available. However, graduation data may help shed some light on this question as well. Graduation could serve as a proxy for group inferences about human capital, as only those individuals with the best skills and abilities graduate. So how do graduation gaps compare across selectivity? Data from multiple sources (High School and Beyond, National Education Longitudinal Study, and College and Beyond) confirm that blacks students not only graduate at higher rates at more selective colleges, but also narrow the gaps in graduation.

---

Surprisingly, a thorough reading of Bowen and Bok's *The Shape of The River* reveals no mention of the necessary statistics to test this hypothesis, despite the fact that their dataset (College and Beyond) clearly has this information. The previously mentioned BPS restricted dataset also has this information.
rates between whites by a substantial margin, although gaps between men and women are
similar across selectivity levels (see Alon and Tienda 2005; Bowen and Bok 2000;

Finally, employers may see gendered difference in human capital across
selectivity levels if there are differences in the effort gap. This is a difficult aspect to
assess. However, research using multiple data sources finds that, for women, college
selectivity has a negative effect on an individual's likelihood to marry and to have
children 6 years after graduating from college (Long 2010). Although this is somewhat
weakly connected evidence, the possibility for differences in the moderating effect of
gender by selectivity remains.

What can we make of these mixed findings? To employers, non-whites appear to
have less human capital than whites at selective colleges but perhaps (depending on how
you measure it) there is a smaller difference-in-difference between non-whites and whites
at more selective versus less selective colleges. I suggest that this could lead to smaller
gaps in outcomes between white and non-white candidates at more selective schools than
less selective schools, or an interaction effect between race and college selectivity.
Additionally, the minimal differences between men and women lead me to suggest that
any gaps in outcomes between men and women will not differ based on college
selectivity.

The theory and data presented in this section suggest that race and gender affect
labor market outcomes and there is some limited evidence that the strength of these
effects, particularly race, may vary based on college selectivity. With more non-whites
and women entering the labor force with college degrees, employers may shift their
views of human capital difference between groups based on the demographics at colleges of different selectivity levels. We now know that race and gender have clear effects on labor market outcomes, there are well defined and somewhat connected reasons why this occurs, and there are reasons to believe the strength of these effects varies with college selectivity. In the next section, I review research that examines race and gender differences in the effect of college selectivity on labor market outcomes.

*Are There Race and Gender Differences in the Effect of College Selectivity on Labor Market Outcomes?*

Even among colleges of similar selectivity, there are reasons to believe that graduates will encounter variations in the labor market. Attention to differences in college experiences and eventual graduation highlights the divergent paths for students who attend college (Bowen and Bok 2000; Charles et al. 2009; Espenshade and Radford 2009; Massey et al. 2003). Gaps in graduation rates between both whites and blacks and whites and Hispanics are lower at selective colleges compared to less selective colleges, but significant gaps still remain (Espenshade and Radford 2009). Additionally, women are more likely to graduate from college than men (Buchmann, DiPrete, and McDaniel 2008).

Studies that examine differences in returns to education based on gender or race struggle with similar methodological issues as general research on college selectivity. The debate surrounding both the race and gender wage gaps attributes some portion of these gaps to differences in human capital (including educational attainment and, among college graduates, major selection), differences in returns to education, and unexplained differences, which is sometimes considered a sign of labor market discrimination (Leicht
Research suggests that the effect of college selectivity on wages does vary by race and gender. In one of the first such studies, Behrman and colleagues (1996b) use the NLSHS72 to examine differential effects of college selectivity on wages. The authors find that the positive effects of college selectivity predict the highest wages for non-white males, followed by white males, white females, and finally non-white females. However, this study does not account for differences in college major or job type. Some research supports the finding that the positive effect of college selectivity on wages is larger for blacks than whites (Loury and Garman 1995), but other research finds that black males receive the lowest returns (Cooper and Cohn 1997). Hoekstra (2009) finds no benefit for white women in attending a large flagship state university. Monks (2000) finds differences in the effect of college selectivity for women and non-whites. His research indicates that women realize smaller gains from research institutions, non-whites have no significant effect from the most competitive institutions, and both groups have large positive effects from specialized institutions. Finally, Long (2010) examines data from different cohorts over time and finds that males have a larger positive effect of college selectivity on annual earnings compared to women and finds mixed results concerning the differences between whites and non-whites.

Thus, the literature on race and gender differences in the effect of college selectivity on labor market outcomes is sparse and difficult to interpret. Some of these studies are missing key human capital variables, such as college major and occupation. If individuals vary in their selection of major across college selectivity, this factor could account for some of the findings. Additionally, these studies typically examine individuals well into their career. There may be additional confounders or cumulative
effects that these models cannot properly account for. Since some of the previously mentioned human capital explanations for differences in wages (particularly major choice and the effort gap) affect women only, and the evidence for varying strength of a gender moderator is weak, I am somewhat hesitant to suggest that gender plays a significant role in moderating the effect of college selectivity on labor market outcomes. However, as I explain below, I believe there is sufficient evidence to suggest race operates differently.

**Summary and Research Questions on Race, Gender, and Educational Credentials**

The second part of my original research will address the questions and issues that arise from the theory, prior research, and data presented above. Throughout this section, I suggest that race and gender may have effects on labor market outcomes. Furthermore, those effects may vary based on an individual’s educational credentials. These issues lead me to my second set of formal research questions for this study:

1. Does having a black name, rather than a white name, have negative effects on (a) the likelihood of receiving an employer response, (b) the salary range of jobs, and (c) the type of job?

2. Does having a female name, rather than a male name, have negative effects on (a) the likelihood of receiving an employer response, (b) the salary range of jobs, and (c) the type of job?

3. Does having both a black name and a degree from an elite college alter any effects from above?
(4) Does having both a female name and a degree from an elite college alter any effects from above?

(5) Does having both a black name and an economics degree alter any effects from above?

(6) Does having both a female name and an economics degree alter any effects from above?
# Table 2.1. Educational Attainment over Time by Race

<table>
<thead>
<tr>
<th>Year</th>
<th>White</th>
<th>Black</th>
<th>Hispanic</th>
<th>Asian</th>
<th>White</th>
<th>Black</th>
<th>Hispanic</th>
<th>Asian</th>
</tr>
</thead>
<tbody>
<tr>
<td>1940</td>
<td>26.1</td>
<td>7.7</td>
<td>--</td>
<td>--</td>
<td>4.9</td>
<td>1.3</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>1950</td>
<td>36.4</td>
<td>13.7</td>
<td>--</td>
<td>--</td>
<td>6.6</td>
<td>2.2</td>
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<td>--</td>
</tr>
<tr>
<td>1960</td>
<td>43.2</td>
<td>21.7</td>
<td>--</td>
<td>--</td>
<td>8.1</td>
<td>3.5</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>1970</td>
<td>57.4</td>
<td>36.1</td>
<td>--</td>
<td>--</td>
<td>11.6</td>
<td>6.1</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>1975</td>
<td>65.8</td>
<td>42.6</td>
<td>38.5</td>
<td>--</td>
<td>14.9</td>
<td>6.4</td>
<td>6.6</td>
<td>--</td>
</tr>
<tr>
<td>1980</td>
<td>71.9</td>
<td>51.4</td>
<td>44.5</td>
<td>--</td>
<td>18.4</td>
<td>7.9</td>
<td>7.6</td>
<td>--</td>
</tr>
<tr>
<td>1985</td>
<td>77.5</td>
<td>59.9</td>
<td>47.9</td>
<td>--</td>
<td>20.8</td>
<td>11.1</td>
<td>8.5</td>
<td>--</td>
</tr>
<tr>
<td>1990</td>
<td>81.4</td>
<td>66.2</td>
<td>50.8</td>
<td>84.2</td>
<td>23.1</td>
<td>11.3</td>
<td>9.2</td>
<td>41.7</td>
</tr>
<tr>
<td>1995</td>
<td>85.9</td>
<td>73.8</td>
<td>53.4</td>
<td>83.8</td>
<td>25.3</td>
<td>13.3</td>
<td>9.3</td>
<td>38.5</td>
</tr>
<tr>
<td>2000</td>
<td>88.4</td>
<td>78.9</td>
<td>57.0</td>
<td>85.7</td>
<td>28.1</td>
<td>16.6</td>
<td>10.6</td>
<td>44.4</td>
</tr>
<tr>
<td>2005</td>
<td>90.1</td>
<td>81.4</td>
<td>58.5</td>
<td>87.7</td>
<td>30.6</td>
<td>17.6</td>
<td>12.0</td>
<td>50.4</td>
</tr>
<tr>
<td>2010</td>
<td>92.1</td>
<td>84.6</td>
<td>62.9</td>
<td>89.1</td>
<td>33.2</td>
<td>20.0</td>
<td>13.9</td>
<td>52.8</td>
</tr>
</tbody>
</table>

Table 2.2. Educational Attainment over Time by Gender

<table>
<thead>
<tr>
<th>Year</th>
<th>High school completion or higher</th>
<th>Bachelor's degree or higher</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Male</td>
<td>Female</td>
</tr>
<tr>
<td>1940</td>
<td>22.7</td>
<td>26.3</td>
</tr>
<tr>
<td>1950</td>
<td>32.6</td>
<td>36.0</td>
</tr>
<tr>
<td>1960</td>
<td>39.5</td>
<td>42.5</td>
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<tr>
<td>1970</td>
<td>55.0</td>
<td>55.4</td>
</tr>
<tr>
<td>1980</td>
<td>69.2</td>
<td>68.1</td>
</tr>
<tr>
<td>1990</td>
<td>77.7</td>
<td>77.5</td>
</tr>
<tr>
<td>1995</td>
<td>81.7</td>
<td>81.6</td>
</tr>
<tr>
<td>2000</td>
<td>84.2</td>
<td>84.0</td>
</tr>
<tr>
<td>2005</td>
<td>84.9</td>
<td>85.5</td>
</tr>
<tr>
<td>2010</td>
<td>86.6</td>
<td>87.6</td>
</tr>
</tbody>
</table>

Table 2.3. Unemployment by Educational Attainment, 2010

<table>
<thead>
<tr>
<th>Unemployment rate</th>
<th>Less than a high school degree</th>
<th>High school degree</th>
<th>Some college</th>
<th>Associate degree</th>
<th>Bachelor's degree or more</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall</td>
<td>14.9</td>
<td>10.3</td>
<td>9.2</td>
<td>7.0</td>
<td>4.7</td>
</tr>
<tr>
<td>Men</td>
<td>15.0</td>
<td>11.3</td>
<td>9.7</td>
<td>7.8</td>
<td>4.8</td>
</tr>
<tr>
<td>Women</td>
<td>14.6</td>
<td>9.0</td>
<td>8.7</td>
<td>6.3</td>
<td>4.7</td>
</tr>
<tr>
<td>White</td>
<td>13.9</td>
<td>9.5</td>
<td>8.4</td>
<td>6.5</td>
<td>4.3</td>
</tr>
<tr>
<td>Black</td>
<td>22.5</td>
<td>15.8</td>
<td>13.2</td>
<td>10.8</td>
<td>7.9</td>
</tr>
<tr>
<td>Asian</td>
<td>11.1</td>
<td>7.6</td>
<td>9.5</td>
<td>6.2</td>
<td>5.5</td>
</tr>
<tr>
<td>Hispanic</td>
<td>13.2</td>
<td>11.5</td>
<td>10.1</td>
<td>8.8</td>
<td>6.0</td>
</tr>
</tbody>
</table>

Table 2.4. Earnings by Educational Attainment, 2010

<table>
<thead>
<tr>
<th>Mean earnings in dollars</th>
<th>Less than a high school degree</th>
<th>High school degree</th>
<th>Some college</th>
<th>Associate degree</th>
<th>Bachelor's degree or more</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall</td>
<td>$22,635</td>
<td>$32,812</td>
<td>$38,612</td>
<td>$41,529</td>
<td>$68,603</td>
</tr>
<tr>
<td>Men</td>
<td>$25,674</td>
<td>$38,098</td>
<td>$46,650</td>
<td>$49,831</td>
<td>$84,072</td>
</tr>
<tr>
<td>Women</td>
<td>$17,453</td>
<td>$25,957</td>
<td>$30,203</td>
<td>$34,786</td>
<td>$52,344</td>
</tr>
<tr>
<td>White</td>
<td>$25,800</td>
<td>$34,621</td>
<td>$40,347</td>
<td>$43,251</td>
<td>$70,282</td>
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<tr>
<td>Black</td>
<td>$21,526</td>
<td>$29,415</td>
<td>$33,081</td>
<td>$34,556</td>
<td>$55,204</td>
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<tr>
<td>Asian</td>
<td>$21,419</td>
<td>$31,207</td>
<td>$34,424</td>
<td>$43,278</td>
<td>$74,510</td>
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<tr>
<td>Hispanic</td>
<td>$20,858</td>
<td>$28,226</td>
<td>$36,144</td>
<td>$36,076</td>
<td>$57,864</td>
</tr>
</tbody>
</table>

From: http://www.census.gov/hhes/www/cpstable032010/perinc/new03_000.htm
CHAPTER 3.
AN AUDIT STUDY: DATA AND METHODS

Experimental Research Design - Audit Methodology

“An experimental study of hiring practices, comparing employers’ evaluations of resumes that are equivalent except for the status of the undergraduate degree, would help shed light on the payoff of an elite degree.” Ann L. Mullen, from Degrees of Inequality (2010:215; emphasis in original)

An audit study is a field experiment that matches two individuals with nearly identical characteristics to participate in a test of some outcome. Ideally, the only variation between the two individuals is on the characteristic(s) of interest (independent variable). The audit method takes on a few variations: in-person, correspondence by mail, and computerized (online correspondence). In-person audits rely on trained assistants, armed with similar credentials and characteristics other than race, to pose as job or housing applicants, typically in examinations of discrimination (see Pager 2003; Yinger 1995). In correspondence audits, researchers respond by mail to advertisements (newspaper or otherwise) without face-to-face interaction in an attempt to eliminate the error of the human assistant component. Finally, scholars discuss computerized audits as an alternative to correspondence audits to increase efficiency (Lahey and Beasley 2009). In each variation of the audit method, careful sampling and randomization of certain components along with matching on all potential important criteria between auditors allows researchers to observe specific differences in outcomes. To date, only a handful of researchers have implemented computerized audit studies (e.g. Ahmed and Hammarstedt
Previous audit studies have successfully examined labor market outcomes with a number of treatment variables such as criminal record, race, gender, sexual orientation, age, and quality of resume (e.g. Bertrand and Mullainathan 2004 and Pager 2007a). This type of research examines labor market outcomes by creating two job candidates with similar resumes or job applications. Researchers randomly select and apply for jobs with one of the two candidates receiving random assignment to the treatment (e.g. criminal record) and the other candidate receiving assignment to the control. Thus, researchers examine treatment effects and their moderators by comparing the rates of call-backs from employers.

In-person audits require human assistants, known as auditors or testers. Depending on the research, auditors drop off resumes, talk to other individuals, or are otherwise involved in the process. Although some scholars praise the in-person audit technique, it is not without its critics (Heckman 1998; Heckman and Siegelman 1993). Near the top of the list of critiques is the possibility that researchers are unable to control for important characteristics that differ between individual auditors or testers. A computerized audit study alleviates many of the problems encountered by in-person audit studies, such as delays in speech, differences in poise, etc.; in other words, differences between testers that employers can witness but the researcher cannot. By removing the human element of the audit, researchers eliminate some potential measurement error.

The correspondence technique has a few advantages over the typical audit study. It is much easier and less expensive to obtain a larger sample size. For instance, Bertrand
and Mullainathan used resumes to submit 4,870 applications for jobs, while the most recent in-person employment audit study (Pager, Western, and Bonikowski 2009) submitted 1,020 applications for jobs. The correspondence audit study also addresses some of the critiques of the in-person audit (Heckman 1998; Heckman and Siegelman), such as minute differences between testers (response time, appearance, attractiveness), and other aspects of the human element. Additionally, the counterfactual is more closely addressed, as names (and thus race) are assigned randomly to resumes, something that cannot be done with in-person audits. However, the issue of conveying race through a resume is also a disadvantage. A name, such as Rasheed Jackson, may conflate race with social class. Although Bertrand and Mullainathan (2004) examined this possibility and suggest it was not an issue in their research, results from qualitative research (Kirschenman and Neckerman 1991) suggest that names convey both social class and race.4

It is important to note that the decision about which type of audit study to use must align with standard practices of the real world scenario. For instance, audits of the low-wage labor market require and in-person method because these jobs are not traditionally listed online. Thus, a computerized audit study closely mimics the real experience of college-educated job seekers today because employers are increasingly less likely to accept job applications in-person or by mail for positions that require a college degree.

To implement my version of this experiment, I created resumes and cover letters for hypothetical job candidates and applied for jobs through a major national job search website (i.e., Monster.com). I created a series of candidate profiles by varying each

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4 See the Appendix in this chapter for more on social class and names.
candidate’s listed college of attendance, college major, race, gender, social class, and geographic location. I then matched profiles and applied for jobs with two candidates per job listing. In the sections that follow, I discuss the specific steps I took to complete this experiment during three phases: (1) pilot data collection round 1, (2) pilot data collection round 2, and (3) full data collection.

Audit Methodology – Pilot Data Collection Round 1

In November 2010, I began the first round of pilot data collection to explore and test multiple aspects of the feasibility of this project. I investigated the Monster.com website to see how applicants would apply for jobs and how employers would eventually see those applications. I then created a series of resumes, cover letters, Skype phone numbers, Gmail accounts, and Monster accounts for each candidate. Next, I searched for available jobs on the website and noted differences in the number of results for certain search terms. Finally, I randomly selected a series of jobs, recorded the information on those jobs, and applied for those jobs with my candidates. Below, I explain in greater detail these steps and the outcome from the first round of pilot data collection.

A. Early Exploration

To begin my pilot data collection, I wanted to know exactly what applicants and employers would see when using the Monster.com website. I first created an account on the website and browsed through some job listings to become familiar with how applicants search for jobs, what information about a job is displayed, and how that information is presented. Because of the standardized way jobs are presented, this exploration also gave me confidence that I could later automate at least a portion of the process. When applicants search for jobs, the advanced search gives a number of field
options: job title, total years of experience, skills/keywords, location, company, industries, job posting date, job type, salary, education level, career level, and categories. Once the user selects a job, the website provides information on these same variables for the specific job (although not all jobs provide all of this information) and a short description written by the employer.

From the employer’s perspective, the Monster.com website looks quite different.\textsuperscript{5} Employers access a “Hiring Library” that shows information on each applicant as applications are submitted. Employers can sort information on candidates in a number of ways (e.g., name, location, when the resume was received, work experience, highest education, career level) and take actions for specific candidates, such as rating them or responding to them. In short, the system that employers view is simply a computerized database of stacks of resumes, cover letters, and applicant information.

\textit{B. Creating Candidate Profiles}

After exploring the website in detail, I decided to create candidates to apply for jobs in the first round of pilot data collection. The first step was to select my initial location and schools. Due to the large number of jobs available in California (preliminary searches consistently yielded \(> 500\) jobs), I decided to focus my pilot data collection on that region. I limited myself to one elite private university and one respected large state university. I wanted to be sure that my selected universities had some distance between them in terms of rankings and selectivity. After considering a number of options in California, I decided to use Stanford University (US News and

\textsuperscript{5} Although I am unable to create my own employer account, Monster.com has created a series of training videos and other materials that can be accessed for free. These materials guide users through the employer's view of the Hiring Library. For more information see http://media.monster.com/mm/usen/help/hiring/tour/
World Report rankings: #1 in California, #5 nationally, and a selectivity rating of “most selective” with an 8% acceptance rate) and the University of California – Riverside (U.S. News and World Report rankings: #12 in California, #94 nationally, and a selectivity rating of “more selective” with a 78% acceptance rate) (U.S. News and World Report 2011).

The next step was to create candidates from these schools. I began by creating resumes with the intention of being as close to realistic as possible. To simplify the pilot data process, I decided to limit myself to one white female and one black female candidate (each matched with both schools for 4 total possibilities).\(^6\) I chose to use first names from the previously mentioned Bertrand and Mullainathan (2004) study and last names from the 2000 U.S. Census, which provides the racial frequency of the 1000 most frequently occurring last names (U. S. Census 2010). I selected “Kristen Thomas” as the name for my white candidate and “Ebony Williams” as the name for my black candidate. Over 55% of individuals in the 2000 U.S. Census with the last name “Thomas” are white and nearly an equal number of individuals with the last name “Williams” are white (48.52%) or black (46.72%).\(^7\)

With these names selected, I used Google to investigate student housing in Palo Alto (Stanford) and Riverside (UCR) to select addresses for my candidates. I chose apartment complexes in each city that were similar in terms of market price (comparable between markets so no potential confounding issues of social class might arise) using a

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\(^6\) My decision to use only female candidates came from the necessity of a small sample. I believe that women are possibly less likely to get called back (due to discrimination and comprising a larger portion of the newly college educated workforce) so I decided this may be useful for the full data collection sample size determination using a power analysis.

\(^7\) In the pilot data, I chose to simplify the last name selection process by using frequently occurring last names that were almost evenly split between whites and blacks.
simple cost of living calculator. I then assigned each candidate a real address with the exception that the specific apartment number does not exist. In this case, both Stanford candidates had the same address and both UCR candidates had the same address; however, no single employer would ever see two candidates from the same college and city.

Next, I created email accounts and Skype phone numbers for each of the candidates. Although at this point there were two candidate names, two addresses, and two schools, I had four unique candidates (two names \* two schools), so I created four unique Gmail email addresses using the candidate's name and a random number. I also created four unique telephone numbers using Skype, which allows users to select an area code based on the city of their choosing. Thus, all candidates were assigned a local phone number to accept employers' calls. I set up each phone number with voicemail capability to record messages from employers. To standardize the outgoing voicemail message, I had assistants of the proper race and gender record a message that differed only in the name used on the recording.\(^8\)

Following the methods of Bertrand and Mullainathan (2004), I next researched actual resumes on the Monster.com website to help me finish creating the information that I would use in my candidates' resumes. I focused my search on recent graduates (from spring/summer 2010) and students about to graduate (fall/winter 2010) to get information on the objectives, extracurricular activities, typical work experience, and skills listed on real resumes. I used these examples to write two short objective statements to use in my resumes. I decided to give candidates 4-5 activities in

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\(^8\) The exact message was: “Hello, this is [name]. I'm not available right now but if you leave your name and number, I will get back to you soon.”
organizations (no dates listed) that were not exact, but similar. For instance, if one candidate was listed as a member of a business focused organization, the other candidate was listed as a member of a different business focused organization. For each activity, I searched through the proper university's list of student organizations to verify that the organization existed on that campus. Additionally, candidates were assigned a small leadership role in two organizations (e.g., secretary treasurer, team leader). To avoid raising employers' suspicions of resumes that were too similar, I attempted to balance activities with skills: candidates with more activities listed fewer skills and vice versa. I assigned skills based on the frequently listed skills from resumes of their real peers (many technologically based) and in accordance with skills that would have reasonably been learned or used in the listed employment history and course information from cover letters (see below). Each candidate was also assigned an employment history that included work in typical part-time student jobs (e.g., salesperson, wait staff) and one internship style position. Each place of employment in this history is a real, local employer. I equalized the total time of employment across candidates.

The next step in creating my candidates was to compile this information into a believable resume. I used two basic templates in Microsoft Word to create my resumes (each candidate could be assigned either template but each job never had candidates with the same template). I then entered the pertinent information for each candidate into the resume template. For the pilot data collection, I chose to give each candidate a bachelor's degree in economics because I wanted to focus this round of data collection on a broad number of jobs. I assigned a GPA based on the requirements listed for graduation with honors (cum laude) for each school. However, I recognize that employers will not...
necessarily know this. At the very least, this creates candidates with slightly higher GPAs at lower ranked schools than higher ranked schools (for instance, the Stanford candidates’ GPAs were 3.41 and the UCR candidates’ GPAs were 3.64).

Once the above information was compiled into a basic resume, I created two possible options for each candidate/school combination: (1) template 1 with employment history 1, and (2) template 2 with employment history 2. Because these resumes were randomly assigned to each job posting, I can control for employment history as a possible confounder from the analysis (or alternatively, analyze employment history separately).

The last step in completing a full candidate profile was to create cover letters. Once again, I used example cover letters to guide me through this process. I created two different cover letters that I could assign to each candidate. The overall content of each cover letter was the same, but the wording and order differed. Each cover letter contained information on college courses, leadership experience, skills, and an explanation that the candidate was in the process of moving from their college town to a residence local to the employer. Due to the nature of the research, I was unable to tailor each cover letter specifically to the jobs, but when possible I added information such as the company name and the reference code into each cover letter.

C. Job Search, Sampling, and Submission Processes

After completing the basic candidate profiles, my next step was to search for jobs in the California area.\footnote{Because this search occurred one day before starting the application process, I consider it day 0 (December 9, 2010) in reference to the application timeline.} During this round of data collection, I searched for jobs within a 75 mile radius of some of the largest cities in California (Los Angeles, San Diego, and San Francisco). I restricted my searches to full time jobs that were posted in the last 45
days and listed as student or entry level. I conducted multiple searches for areas and included options for education level, category, and keywords. Depending on how restrictive my search was, the number of jobs varied from 55 to 622 in these locations. I eventually decided to stick with the broadest sampling frame for the pilot data collection in the hope that I would learn more about matching candidates with types of jobs for future data collection. Using search criteria that limited job categories only to those that did not require specialized degrees or training (such as nursing, engineering, etc.) eventually yielded a sampling frame of over 500 jobs for each of the three locations in California.

With a sampling frame established, next I entered the website addresses for each job posting into an Excel spreadsheet. I then generated a random number for each job posting and sorted them. I chose to apply for the first 240 job postings based on this sort. To ensure random assignment and control for differences between candidate profiles, I had to carefully create a matrix of each possibility. At this stage, I was concerned with the following differences: 2 names, 2 schools, 2 employment histories, and 2 cover letters. Table 3.1 shows this matrix and indicates that there were 16 possible candidate profiles that I could use. However, each candidate profile had to be matched with its counterpart. Table 3.2 shows these matches, the number of jobs I applied for with each candidate profile, and the ordering of application submission. I intended to apply to a total of 30 jobs with each candidate profile. Although there were 240 unique job postings, I submitted two applications to each job posting in accordance with the design of the audit study. Thus, 30 applications x 16 candidate profiles = 480 total applications.

The timing of application submission was another important concern in this pilot
study. Following the work of Pager (2007a), I introduced a delay between the submission of the first candidate profile and the second candidate profile to an employer through the Monster.com website. Because the candidate profiles are designed to be as similar as possible, an employer might potentially become suspicious of two candidate profiles submitted on the same day. Introducing a delay between submission reduces the risk of discovery. Additionally, because these candidate profiles are stored electronically I chose a longer delay than previous research: 3 days. This delay introduces a new potential problem into the research because one candidate profile is always submitted later than the other, perhaps after the employer has stopped looking at resumes. In some cases, a job posting may be officially closed through the Monster.com website and I can indicate that in the dataset. In other cases, the job may no longer be available and I have no way of knowing that information. To adjust for this possibility, I assigned one candidate profile to apply for half of the jobs first and the other candidate profile to apply for the other half of the jobs first. Thus, Table 3.2 has two entries for each pair but with the order reversed (e.g. 1 / 16 and 16 / 1). As an additional control, I also calculate the length of time between the original posting date for each job and the date of each application submission.

D. Results and Conclusions

After completing all of the candidate profiles and randomly selecting job postings to match with candidate profiles, I was ready to apply for the selected jobs. Once I started applying for jobs, I quickly ran into an unexpected and serious issue. A portion of the job postings (approximately 10-15%) did not use the Monster.com site for the application process. These job postings sent the user to an external site to complete a job
application. Although I originally followed through on a number of these applications, it became apparent that these applications took much longer to complete and were more difficult to standardize. Additionally, I began to see a number of the same designs and software companies on these sites, leading me to believe that some of these websites were simply larger recruiting firms handling multiple clients. I was concerned that I might submit conflicting information about candidates into a shared database and potentially threaten the entire data collection process.

I completed three days of data collection (see Table 3.2) before I was forced to abandon this round of data collection. On day 4 (December 14, 2010), I logged into the various Monster.com accounts of the candidates to find that they had all been suspended due to terms of service violations. I tried to gain more information from Monster.com customer service to no avail. This setback forced me to try to investigate what led to the account suspensions to avoid problems in the future. Some conversations with my brother and users on a message board led me to believe that either (1) the use of the exact same IP addresses, (2) the use of IP addresses outside of the region where my candidates were applying for jobs, (3) the conflicting data in recruiters' databases, or (4) some combination of these factors caused Monster.com to suspend these accounts.

Although the first round of pilot data collection did not go as planned, I learned that to continue with a second round of pilot data collection I had to adjust my strategy

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10 I originally received an email response from the general Monster.com customer service email address but after they forwarded my requests to the Terms of Service team, I received no further responses. These actions are typical in matters regarding a Terms of Service violation. Companies do not want anyone to know what exactly gets an account closed or suspended, otherwise an individual might know how to get around said violation.

11 My brother works for a large university as an Information Security Engineer. His job is figuring out how people avoid being detected on the internet when they intend to inflict harm or cause chaos on computer systems. He is an expert and agreed to consult with me as necessary on this project. Much of the expert knowledge on information security issues comes from him.
so that Monster.com would have no reason to be suspicious of my candidate profiles. In
the next section, I discuss the changes that I made to successfully implement a second
round of pilot data collection and the preliminary results.

Audit Methodology – Pilot Data Collection Round 2

A. Adjustments from Round 1

Before beginning the second round of pilot data collection, I decided to make two
adjustments that might protect the integrity of my project. First, the simplest strategy to
avoid problem 3 above, was to eliminate any job posting that required leaving the main
Monster.com site to process any portion of the application. Although I initially was
worried that this might have serious detrimental effect on the generalizability of my
study, the impact appears to be negligible. Although one might intuit that large
companies only use an external application process because other third parties handle a
majority of their hiring, this appears to be incorrect. Some preliminary investigations
suggest a number of large companies (> 50 employees) do use the normal Monster.com
internal process. Additionally, small companies (< 50 employees) appeared in both the
internal and external processes. In a stratified sample of 200 randomly selected job ads
from California with identifiable information, similar proportions of small (83%) and
large (76%) companies used the internal Monster.com process. Unfortunately, there is no
definitive way to answer this question without obtaining data directly from Monster.com
because a small percentage of job ads (~17% in the pilot data) do not include identifiable
information about the employer. Still, there does not appear to be a systematic difference
between companies using internal versus external processes.

The second major adjustment I made concerned the secrecy and protection of
individual candidate profiles to prevent Monster.com from linking these profiles to each other (problems 1 and 2 above). In general, every device connected to the internet has a unique IP address. These addresses are similar to street addresses or GPS locations. They identify a specific network location and a general physical location of a user.

During my preliminary pilot data collection, I found that a potential problem was either (1) using the same IP addresses or (2) using IP addresses from geographic locations that were not matched to the information in the user's profile. Thus, to be able to use multiple accounts from various geographic locations, I had to obtain unique IP addresses for each user. A number of companies throughout the United States setup physical computer servers in different geographic locations and lease IP space, which allows users to login to their servers and obtain unique IP addresses from those locations. To overcome problem 1 above, at minimum each candidate/credential combination had to have a unique IP address for their job search website account. Additionally, use of this service requires the use of a virtual private network (VPN) login for the server. After investigating a number of options I found a company that I could use for this purpose. I purchased two IP addresses based out of Los Angeles, CA for candidate profiles from UCR and two IP addresses based out of San Jose, CA for candidate profiles from Stanford. Although these IP addresses were not exact matches, they were the closest I could find.\textsuperscript{12}

At this stage, I could have started a second round of pilot data collection but I decided to implement additional protections to mask my actions to Monster.com. I believed that the best course of action was to do everything possible to avoid raising

\textsuperscript{12} Exact matches would be nearly impossible for smaller cities because these IP services are usually based out of large cities. I worked under the assumption that a reasonable geographic distance coupled with unique IP addresses would be enough to avoid raising suspicions at Monster.com.
At that point, if my actions still raised suspicions and Monster.com shut down my accounts, I would know that this research was virtually impossible. Thus, my next step was to implement a system that would avoid passing additional information from the computer and web browsers I used to Monster.com that might link my accounts.

Different web browsers (e.g. Internet Explorer, Mozilla Firefox) pass different information to websites when a user submits a form (i.e. the job application). Using a software package known as VMware makes it possible to create different configurations for additional protection. In short, this software allows a user to create virtual machines within one computer that, coupled with unique IP addresses, appear as different machines and users. These virtual machines have their own separate installations of operating systems, web browsers, and other software. Using this software, I created a separate virtual machine for each candidate/credential combination (four in total). I installed Mozilla Firefox (different versions) on two of these virtual machines, Internet Explorer on one, and Google Chrome on one. I then setup the VPN from within these virtual machines to login to the servers I had leased access to in California and obtain the unique IP addresses I had leased to complete the masking process.

In returning to the candidates profiles I made a few minor adjustments. I chose to keep the same two universities (Stanford and UCR) as my focal schools. I changed the names of my candidates (I selected “Laurie Miller” as the name for my white candidate and “Tanisha Washington” as the name for my black candidate), their phone numbers, email addresses, physical addresses and employment histories to avoid any connection to the previous round of data collection. The process I used to select this information was

13 Although Internet Explorer is currently the most common web browser used in the U.S., it also passes the most information about a user's computer to a website. I decided that using one virtual machine with Internet Explorer would be the safest option.
similar to that used in the first round of pilot data collection. However, in this round I chose to use four unique addresses instead of two. I chose two apartment complexes in each city that were similar in terms of market price (comparable between and within markets) for address assignment.

B. Job Search, Sampling, and Submission Processes

After completing new candidate profiles, my next step was to search for jobs in the California area. During this round of data collection, I searched for jobs within a 75 mile radius of some of the largest cities in California (Los Angeles, San Diego, and San Francisco). Once again, I chose the broadest sampling frame based on various searches. Using search criteria that limited job categories only to those that did not require specialized degrees or training (such as nursing, engineering, etc.) eventually yielded a sampling frame of over 500 jobs for each of the three locations in California.

During this round of pilot data collection, I also began to experiment with automating the process of job selection. I wrote some code using the Ruby on Rails programming platform to query the Monster.com website using my selected search criteria. This script returned the data into a text file and saved all of the individual job posting HTML files locally to my computer. This script allowed me to automate the process of collecting the entire sampling frame.

With a new sampling frame established, next I used Excel to randomly select jobs by generating a random number for each job posting and sorting them. I again chose to apply for the first 240 job postings based on this sort. I used a matrix, shown in Tables 3.3 and 3.4, similar to the first round of pilot data collection to setup the order of

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14 Because this search occurred one day before starting the application process, I consider it day 0 (February 14, 2011) in reference to the application timeline.
applications for each matched candidate profile.

With a slightly more streamlined process, I chose to apply for jobs over a three day period (instead of the five day process used in the first round of pilot data collection). On day 1 (February 15, 2011), I applied for 15 jobs with each candidate profile. On day 3 (February 17, 2011), I applied for each of the jobs with the counterpart of the original candidate. This method yielded two advantages over the process from the first round: (1) each profile had the exact same start date instead of the one day delay used in the first round, and (2) the entire process only had a one day delay between the first and second applicants, thus reducing the chance that the job posting might have closed during the wait period.

C. Results and Conclusions

The most important finding from the second round of pilot data collection is that the adjustments from the first round, namely using unique IP addresses and virtual machines, appeared to mask my actions well. During a month of using new Monster.com accounts associated with the candidate profiles, all of the accounts remained active. Additionally, these candidate profiles have received a number of responses in the form of emails and call-backs from employers. In the following paragraphs, I provide details on all of the findings from the second round of data collection.

One concern with using a delay between submitting applications in this research is that job postings may close between submitting the two applications due to canceled searches, jobs being filled, or other reasons. Although I can only report information on job postings that are officially closed through the Monster.com system, the results suggest that this may not be a serious problem. Between day 0 and day 1, 2.1% of the jobs (5 of

15 In fact, as of January 2013 the accounts from the second round of pilot data collection are still active.
240) were removed before any applicants applied. The possible time period of removal between day 0 and day 1 ranges from 19 hours to 34 hours.\textsuperscript{16} The average time since the original posted date of these jobs is 18.6 days, slightly higher than the average time since the original posted date of the entire sample of jobs (15.4). Between day 1 and day 3, 3.0\% of the jobs (7 of 235) were removed after the first applicant but before the second applicant applied. The average time since the original posted date of these jobs (as of day 0) is 29.0 days, much higher than for the entire sample. These results suggest that restricting the sampling frame to jobs posted within the last 30 days may help reduce the instance of official (and likely unofficial as well) closings during the data collection period. Collecting data on unofficial closings, or jobs that are no longer available but still have open application processes in the Monster.com system, is beyond the scope of this research at this time.

Employers responded to the applications from candidate profiles in one of two ways: email or phone. Employers used email to solicit additional information such as follow-up questionnaires or to setup a time for a phone or in-person interview. In 13 out of 25 (52\%) email responses, employers asked for additional information. In 12 out of 25 (48\%) email responses, employers requested an interview. During the pilot data collection, when employers called candidates they were always doing so to request an interview.

In Table 3.5, I examine the differences in employer responses by college. The rate

\textsuperscript{16} The searches began at roughly 9:00 am on day 0 and ended at 2:00 pm on day 0. The submission of applications began at roughly 9:00 am on day 1 and ended at 7:00 pm on day 1. Thus, the low bound is a job posting found at 2:00 pm on day 0 and submitted at 9:00 am on day 1 (19 hours) and the high bound is a job posting found at 9:00 am on day 0 and submitted at 7:00 pm on day 1 (34 hours). Although I do not have exact calculations for this round of data collection, I recorded this information during the full data collection round.
of email responses for the Stanford candidates was 6.58% compared to 4.39% for the UCR candidates (not significantly different). The rate of phone responses for the Stanford candidates was 8.33% compared to 2.63% for the UCR candidates (significant different at p<0.05). The total employer response rate for the Stanford candidates was 14.91% compared to 7.02% for the UCR candidates (significantly different at p<0.01).

In Table 3.6, I examine the differences in employer responses by race. The rate of email responses for white candidates was 7.02% compared to 3.95% for black candidates (not significantly different). The rate of phone responses for white candidates was 7.89% compared to 3.07% for black candidates (significant different at p<0.05). The total employer response rate for white candidates was 14.91% compared to 7.02% for black candidates (significantly different at p<0.01).

Table 3.7 shows the number of email, phone, and total employer responses by race and college. Although the white Stanford candidate received emails at a rate of 7.76% and the black UCR candidate received email at a rate of 2.59%, there are no significant difference between candidate profiles. However, there is a significant difference (p<0.05) between the call-back rate of the white Stanford candidate and all other candidates. The results indicate that the white Stanford candidate was nearly 4x as likely to get a call-back as the black Stanford candidate. The differences in call-backs rates between all other groups are not significantly different. Finally, the last two rows in Table 3.7 show the total employer responses (email + phone). Once again, there is a significant difference between the responses of the white Stanford candidate and all other candidates. Overall, the white Stanford candidate had a total response rate of 20.69% compared to 8.93% for the black Stanford candidate, 8.93% for the white UCR candidate, and 5.17% for the
black UCR candidate.

The pilot data collection process confirmed the feasibility of this study. Additionally, the results suggest that there is a significantly significant positive effect for a degree from Stanford (over UCR), but that effect is only significant for white candidates and not black candidates. This effect manifests in phone responses, not email responses. Since phone responses are always requests for interviews, these responses are likely more important for employment prospects. Although there are not significant differences between black Stanford, white UCR, and black UCR candidates in email, phone, or total responses, it appears there may be a tiered pattern that could be significant with a larger sample. For instance, the total response data seems to place the white Stanford candidate as the most desirable among employers, followed by the black Stanford and white UCR candidates, with the black UCR candidate as the least desirable among employers. This perhaps indicates a premium to a degree from an elite private school over a large public school for both white and black candidates, but black candidates still lag behind white candidates overall in the return to a college degree.

Audit Methodology – Full Data Collection

The pilot data collection process was simply an exploratory exercise to determine the feasibility of this project and to collect some basic data to undertake a power analysis, yet a number of the results from the pilot data were already significant. In expanding the scope of the data collection and making adjustments from the pilot data collection rounds, I had a number of important options to consider in regards to research ethics and design. In the following sections, I discuss the ethical considerations, logic behind the matching procedure, variables used, data collection process, methods of analysis, and descriptive
A. Research Ethics and the Audit Design

All research must weigh the potential benefits against the potential harms during the design process and audit studies enter into particularly sensitive territory due to their direct involvement in social action and deception of other human actors. Two standard research tactics, voluntary participation and debriefing, usually are not possible due to the nature of these types of experiments.

In my research, voluntary participation and informed consent would completely alter the design. The purpose of an audit study is to examine how employers respond in a real-world scenario, thus informed consent is not an option. Still, the audit method has withstood the scrutiny of the United States Supreme Court as a viable methodological tool, in part due to the creation of the audit method in research sponsored by the Department of Housing and Urban Development (see *Havens Realty Corp. v. Coleman*, 455 U.S. 363, 373 [1982]). Moreover, the American Sociological Association Code of Ethics states:

Despite the paramount importance of consent, sociologists may seek waivers of this standard when (1) the research involves no more than minimal risk for research participants, and (2) the research could not practically be carried out were informed consent to be required. Sociologists recognize that waivers of consent require approval from institutional review boards or, in the absence of such boards, from another authoritative body with expertise on the ethics of research. Under such circumstances, the confidentiality of any personally identifiable information must be maintained. (Section 12.01(b)).

I received said waiver and then had to consider the possible harm that might come to the actors involved. Since the applicants are fictitious, the potential harm focuses on the employers.
First, the Institutional Review Board at the University of North Carolina at Chapel Hill (henceforth known as the IRB) was concerned that employers could face discrimination lawsuits because of the research outcomes. Discrimination in the eyes of the law must be a systemic provable pattern within an employer (Blank et al. 2004). With only two cases per employer, audit studies do not show discrimination by a particular employer but rather across a sample of employers. The distinction is an important one as it allows a researcher to examine discrimination at a larger level without targeting specific employers.

The IRB’s second concern was that media attention from the study might bring bad press to any employers involved in the study. Additionally, employers might fire human resource workers if they had a small HR department and were connected to the study. Although unlikely, keeping sensitive data such as employer names provides no great benefit that outweighs these potential harms. Thus, to help alleviate this concern I agreed to remove all sensitive data as soon as possible.

The final concern brought up by the IRB was the potential waste of employers’ time. Since the time spent reviewing a resume and contacting applicants would not lead to a hire, the IRB suggested this wasted time should be kept to a minimum. Although some prior research based on self-reports suggests that employers may spend as much as 3-5 minutes reviewing a single resume, a recent study using eye-tracking found that professional recruiters average only 6 seconds per resume (Evans, 2012). The computerized audit method is less invasive and less employer time is wasted than with prior in-person audit studies because applicants do not enter a place of business to request applications, do not talk or directly communicate with employers, and do not drop off
applications in a place of business. Testers go through the first stage of the process, but they do not return calls to employers or schedule interviews, ending the process at first contact from an employer. If an employer calls back the applicant, the total time spent likely ranges somewhere between 2-6 minutes per applicant. Thus, the IRB strongly recommended a limit of two applicants per employer, in line with most prior employment audit studies.

I chose not to engage in debriefing the employers in this study because of at least two potential harms and no clear benefits to the employers. Debriefing increases the likelihood of some type of negative backlash in the form of termination or other economic harm from the employer towards individuals on the HR staff. Additionally, debriefing might inflict other harms as employers realize they have wasted time and energy on reviewing fictitious resumes.

One final note on ethics and research design concerns maintaining the integrity of the research project. Employer discovery of the study, in any form, increases the likelihood of a number of the harms listed above. Thus, it was important to avoid accidental employer discovery by placing some restrictions, beyond a limit of two resumes per employer, on the resumes an individual employer saw. I continue this discussion in the next section on the matching procedure.

B. The Matching Procedure

One of the main advantages of the audit method is that a researcher is able to isolate the difference on a single characteristic between two testers in a matched pair to examine the effect of that characteristic on an outcome. Critics are skeptical, suggesting that a variety of unmeasured differences may exist between testers or that true matches
between testers with only one single difference may inflate the importance of that
difference on the outcome (Heckman and Siegelman 1993). Although it is impossible to
know with absolute certainty that there are no unmeasured differences between testers,
prior audit studies have both intentionally and unintentionally examined two measured
differences between testers in a matched pair (Ahmed and Hammarstedt 2008; Bertrand
and Mullainathan 2004). Sometimes the delicate balance between difficult choices in
research design and research ethics necessitates a slightly imperfect design.

In my study, a number of conditions required me to simultaneous vary two
characteristics while matching within pairs. As discussed in the ethical concerns section,
IRB approval required that I only send two resumes to any employer. Thus, if I wanted
to examine differences in college selectivity within pairs as the singular difference, I
would need to simultaneously hold race, gender, and social class constant within pairs.
However, the nature of the measurement of these variables would require that applicants
have the same first name. Employers likely would be more suspicious of two
applications received within days of each other with the same distinctive names. If
employers then closely inspected the resumes, they might possibly eliminate both
candidates from consideration, biasing the results. I could use different names within the
same race/gender/social class categories but this introduces a not easily quantifiable
difference: any perception difference by employers between similar but not exactly the
same names. If instead, I wanted to examine differences in race within pairs as the
singular difference, I would need to simultaneously hold college selectivity, gender, and
social class constant within pairs. This also creates a problem because employers likely
would be more suspicious of two applications received within days of each other from
candidates with the same degree from the same college (particularly the elite schools). As previously mentioned in the ethical considerations section, employer discovery was of great concern in this study, effectively ruling out these options. In short, there are a lot of moving parts to watch over in an intricate audit study. Because race and college selectivity were my leading variables of interest in this research and I wanted to examine differences within pairs for at least one of these variables, I was forced to simultaneously vary both characteristics within pairs.

Within pairs, I made matches on the basis of gender, social class, major, and region. However, within pair comparisons on race and college selectivity match black candidates with an elite degree against white candidates with a less selective degree and black candidates with a less selective degree against white candidates with an elite degree. Table 3.8 shows the basic pairs. This design is very similar to the traditional design of a factorial experiment as all two-by-two combinations are represented in the data (Gonzalez 2009). The only difference between these designs occurs in the outcome phase. Direct comparisons are not always available but indirect comparisons are an option. Although this matching strategy is atypical, below I suggest that it can still produce unbiased estimates.

Audit studies often include two different types of effects without clear language on the differences in these effects. The first type I call direct or within-pair effects. These effects are directly observed because the characteristic differs within matched pairs of two or more testers. The later type I call indirect or between-pair effects. These effects are indirectly observed because the characteristic differs between pairs of two or

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17 Additional variables that were important to the design procedure but not of interest in the analysis include cover letter type, resume type, and application order. These were varied equally across pairs (see The Data Collection Process section below).
more testers. The type of effect has implications for what type of analysis is suitable and the precision of the estimates (more on this in the analysis section below). The existence of direct and indirect effects separates the audit method from more traditional experiments. In their most basic form, traditional experiments randomly assign individuals to a treatment or control condition and examine all individuals on the same outcome measure. Audit studies take a similar basic form but often include random assignment of pairs to social actors or situations which then form the basis of the outcome measure. For example, housing audits randomly assign pairs to real estate agents and employment audits randomly assign pairs to employers. No real estate agent or employer creates the outcome measure for more than one pair. Thus, accurate assessment of indirect or between-pair effects requires that no significant differences across these social actors or situations exist.

Arguably, indirect effects can be properly estimated only when the researcher randomly selects pair assignment during the outcome phase (also see Pager 2003, p 957).

One final critical piece to consider is the employer decision-making process. In the most extreme case, a single employer might receive only two total applications (e.g. black candidate with an elite degree and white candidates with a less selective degree and no other applications from outside this study) and only be able to interview one applicant. In this case, the employer must directly compare the two candidates without the context of other real applicants. This might raise our concern over employees only ever seeing two candidates (instead of four) who differ on two characteristics simultaneously. However, this hypothetical scenario almost certainly never occurred with any of my pairs. Employers often called back multiple candidates and surely had to choose from a
large pool of applicants other than these fictitious candidates.

Thus, I believe that by examining a combination of direct or within-pair effects and indirect or between-pair effects with random assignment of job listings to matched pairs, this research closely approximates a similar design using four applicants per job (black elite degree, black less selective degree, white elite degree, white less selective degree) but without the limitations and ethical concerns discussed above. An additional advantage to this design is that employers do not have to focus on a single small difference between two applicants if they believe they truly can respond to only one (a critique of Heckman and Siegelman 1993). It is highly unlikely that employers ever have to make the unrealistic choices that the typical matched pair process requires of them, potentially inflating the estimates of characteristics such as race in prior audit studies. The tradeoff for this design is that the estimates are less statistically efficient, which I discuss below in the analysis section.

C. Variables

There are six main variables of interest in this research: college selectivity, race, social class, gender, and college major.\(^{18}\) I use two categories each for college selectivity (elite and less selective), race (black and white)\(^{19}\), gender (male and female), and major (economics and psychology) and three categories for social class (lower, middle, and upper). By examining variations on these characteristics, I can adjudicate between discrimination and differences in human capital as mechanisms for differences in first-

\(^{18}\) There are some additional variables in this research which stem from the research design (e.g. resume type, cover letter type, submission order, etc.). Those variables are discussed in the data collection process section below.

\(^{19}\) Although I originally hoped to examine Hispanic outcomes as well, the audit design places a heavy burden of adding additional variables on the required sample size. Additional variables or categories increase the required sample size exponentially.
stage employment outcomes between groups. Additionally, geographic region is important as well, because it allows me to (1) examine how differences vary across regions and (2) increase my sample size without introducing large differences across time in the data collection. Overall, this addition of variables significantly expands the scope of my dissertation research.

To examine college selectivity, I first selected elite universities that ranked highly in both the U.S. News and World Report and Baron's rankings and paired these with a nationally ranked state university in the same state but below the elite university on the U.S. News and World Report rankings (U.S. News and World Report 2011). The pairs I used were: (1) Harvard and University of Massachusetts – Amherst, (2) Stanford and University of California – Riverside, and (3) Duke and University of North Carolina – Greensboro. These choices in schools were driven by a few factors. First, I needed to be sure there was a reasonable distance in rankings between schools to capture any potential effect of selectivity while conforming to a limitation of the data (U.S. News and World Reports limits the numerical ranking of national universities to 200 schools). Second, prior research suggests the effect of selectivity may not be linear but may only come from the elite schools near the very top of the rankings. Finally, these schools correspond to regions which had sizeable numbers of job listings (see below).

The use of race in audit studies remains a popular, yet controversial aspect of this method. In-person audits have the advantage of personal appearance to convey race, but researchers must be concerned about differences between white and black testers in how they carry themselves – the human element. Correspondence and computerized audits

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20 The exact National University Rankings from the U.S. News and World Report are: (1) Harvard, (5) Stanford, (10) Duke, (94) UMass-Amherst, (97) UC-Riverside, and (190) UNC-Greensboro.
can eliminate the human element but must rely on written information to convey race. Still, a number of previous studies have used names as an indicator of race (Bertrand and Mullainathan 2004; Hogan and Berry 2011). However, scholars have raised concerns that racialized names may conflate race and social class and bias the results from an experiment (Fryer and Levitt 2004; Pager 2007b). Although researchers have examined the dual aspects of race and social class in names ex post facto, no research has incorporated this directly into the design stage of the study. To expand our knowledge of the separate effects of race and social class, I used New York state birth record data from the early 2000s to select names.  

I obtained data from the New York State Department of Health on the total number of births listed by name from 2000-2003. These data separately list the total number of births by (1) name and race and (2) name and mother's education.

To search for possible names I limited the criteria to names with at least 50 births in a year in the state and at least 75% one particular race (black or white). I then chose from this list names across race and gender that were similar on mother's education so that I had three names for each race and gender combination representing three tiers of education levels (upper, middle, and lower). In total, I used 12 different names (3 black/male, 3 black/female, 3 white/male, 3 white/female). These names were: Jalen, Lamar, DaQuan, Nia, Ebony, Shanice, Caleb, Charlie, Ronny, Aubrey, Erica, and Lesly. Table 3.9 contains more information on the race and education composition of each name.

There are still a few shortcomings by using names in this way. First, names in

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21 I attempted, unsuccessfully, to obtain data on first names by race from the U.S. Census Bureau. Although these data likely exist, repeated attempts through multiple avenues proved fruitless.
New York state are not likely to be representative of the population. To limit the impact of differences between New York and likely national naming patterns, I chose to reject the use of any obvious immigrant or Muslim black names. Second, the timing of the names data and the cohort completing college during this time is not perfectly aligned. Individuals graduating from college in 2011 were born around 1989-1990. Although a minor concern, it still seems unlikely that the social class or racial naming patterns with these 12 names changed significantly over 10-14 years. Finally, although the data suggest certain patterns of naming, employers may not be aware of these patterns. Particularly in regards to social class, the name signal may be noisy.

To examine additional human capital differences that may contribute to differences across race and gender in observational studies, I used two possible college majors for each resume: economics and psychology. Each of these majors is one of the top choices by gender for men and women respectively (Altonji, Blom, and Meghir 2012; Carnevale, Strohl, and Melton 2011). Furthermore, these two majors provide general knowledge and skills that can be used to apply for a broader range of jobs than other majors such as engineering or computer science.

One important piece of information that became clear during the pilot data collection process was the limited number of job postings for certain geographic locations. Casual exploratory analysis suggested that some large urban areas, such as Seattle, WA, simply had too few jobs for the purposes of this research. Thus, I chose three geographic regions (Northeast, Southeast, and West) which had sizable numbers of jobs listed across either two or three cities. In the Northeast, candidates applied to jobs in Boston, MA and New York, NY; in the Southeast, Atlanta, GA, Charlotte, NC, and
D. The Data Collection Process

Between March and August of 2011, I conducted a computerized audit study to examine the effects of college selectivity, race, social class, gender, college major, and region on labor market success. To implement this experiment, I created resumes and cover letters for hypothetical job candidates and applied for jobs through a major national job search website (i.e. Monster.com). I created a series of candidate profiles and used matched pairs to apply for jobs in three geographic regions. In total, I used these candidates to apply to 1,008 jobs (or 2,016 total data points). The overall process is very similar to that used during the pilot data collection rounds. In the paragraphs that follow, I discuss the general process and highlight areas of difference from the pilot data collection rounds.

I began researching real resumes on the Monster.com website during March 2011 and once again put together candidate resumes that were a mesh of other resumes used during the previous few months. I created two basic resumes that each included a short objective statement, 4-5 activities in student organizations (no dates listed) and two leadership roles in those activities, a list of skills, and an employment history. Each list of activities in student organizations comes from real organizations on each campus (checked against each college’s websites). Although each college does not have the same organizations, these were matched as closely as possible with similar organizations. The listed skills come from those frequently listed on other resumes viewed on the website that match with skills used in the specified line of course work and employment history. Finally, each candidate’s employment history included work in one typical part-time
student jobs (e.g. salesperson, wait staff) and one internship position using real, local employers that have offices in every region. The total time of employment across candidates is the same.

The next step in creating candidates was to complete full resumes and institute random assignment across any pertinent variables. I used two basic style templates to create my resumes (each candidate could be assigned either template but each job never had two applicants with the same template). I then entered the candidate information into each resume template. I assigned a GPA based on the requirements listed for graduation with honors (cum laude) for each school. After I compiled this information into a basic resume, I created four possible options for each candidate/school combination: (1) template 1 with employment history 1, (2) template 1 with employment history 2, (3) template 2 with employment history 1, and (4) template 2 with employment history 2. Because these resumes were randomly assigned to each job posting, I use these small variations in resumes to minimize experiment discovery but also control for employment history and template. Figures 3.1 and 3.2 show two sample resumes for a matched pair.

I then created two different cover letters for assignment to each candidate. The overall content of each cover letter was the same, but I altered the specific words, phrases, and order. Each cover letter contained information on college courses, leadership experience, skills, and an explanation that the candidate had recently relocated from their college town to a residence local to the employer. Due to the nature of the research, I was unable to extensively customize each cover letter specifically to the job, but I always included some custom information such as the company name and the
reference code into each cover letter. Each cover letter was randomly assigned prior to beginning the job application process so that a matched pair never used the same cover letter. Table 3.10 shows the within pair possibilities for resume template, employment history, and cover letter assignment.

For each candidate/school combination, I established an individual telephone number associated with a local area code and a voice mailbox using Google Voice, a Google e-mail account, and a mailing address. Each voice mailbox had the same recorded message with the candidate’s name substituted in. I enlisted the aid of assistants to record identical messages using individuals of the corresponding race and gender of the fictitious applicant. I created e-mail accounts that contained the applicants name followed by a two to four digit random number. Because employers might be aware of differences in rental prices in local areas, I used Google to investigate apartments and select an address for each candidate. I chose one modest apartment complex in each city that was similar in market price across regions (using a cost of living adjustment calculator). I then assigned each candidate a real address with the exception that the specific apartment number does not exist.

For two separate weeks during May and June 2011, I used a programming script created in Ruby on Rails to query Monster.com and download all posted jobs in the cities in my three selected regions that fit the following search criteria: college degree (BA) required, listed as “entry level” or “student”, posted in the past 30 days, and located in a 50 mile radius of the cities. I then eliminated any jobs that required the applicant to leave the Monster.com site and apply at an external site and any jobs that required specialized degrees or training (e.g. nursing, engineering, etc.). This setup saved the data into a text
file and saved all of the individual job posting HTML files locally to my computer. For each separate region, this became my sampling frame.

In each sampling frame I generated a random number between 0 and 1 for each job, ordered them, and kept the first 336 jobs to create my three samples across regions. With the jobs randomly ordered on the basis of any pertinent variables, I assigned pair IDs (see Table 3.8) to each job and split the application order across pairs.

Once jobs and candidates were matched for a particular geographic region, I applied for 240 jobs (2 candidates per job) in their home region (e.g. Boston and New York City for Harvard and UMass graduates) and 96 jobs in one of the two outside regions (e.g. Los Angeles and San Francisco). I used a 24 hour delay between the first applicant and the second applicant to minimize sample attrition. In total I applied for approximately 1,008 jobs or 2,016 data points. I then waited for ten weeks after the submission of each application for employers to make decisions and call or e-mail candidates with requests for an interview before concluding the data collection phase.

E. Descriptive Results

Table 3.11 shows descriptive statistics for the candidates by order of application. There is an attrition rate of 5.6% (56 ads) due to employers removing a job advertisement before one or both applicants could apply for the job. This is much smaller than in the pilot data collection due to a shorter delay between collecting ads and the first application submission and between the first and the second application submissions. Of the 952 successful candidate pairs submitted, there are equal numbers of white and black candidates (since each pair had one of each) and equal numbers of those with a degree from an elite college and those with a degree from a less selective college. Because each
of the remaining variables differs between pairs, some characteristics are not evenly
divided due to attrition from the job sample. Variables with two characteristics (gender
and college major) are still very close to a 50% split and variables with three
characteristics (social class and region) approach a 33.3% split. Almost 71% of the
successful candidate pairs were submitted in their home region with 29% out of their
home region.

Employers responded to job applications from candidates in one of three ways:
email, phone, or both.22 Employers used email to solicit additional information or to
setup a time for a phone or in-person interview. When employers called candidates, they
almost always explicitly requested an interview, although voicemails were occasionally
vague about whether an intermediate step was required (such as an online questionnaire).
Generally, emails were less urgent and gave employers more power in the relationship
(e.g. “Please fill out this questionnaire if you wish to still be considered for this
position.”) but phone calls were more urgent and gave candidates more power in the
relationships (e.g. “We would love to hear back from you as soon as possible with a time
that works best for you.”). On a few occasions, employers responded to all candidates
via an automated email with a generic response that did not indicate a legitimate interest
in that particular candidate.23 As Table 3.11 shows, the average response rates were 7.4%
by email, 8.2% by phone, 3.5% by both, and 12% total. There are no significant
differences in the response rates between first and second application submissions.

22 Additionally, I calculate total response rates (either email OR phone). Adding email and phone responses
together does not always equal the total response rate due to some employers using both means of
response.

23 I verified these by sending a third test application with credentials that indicated they were not qualified
for the posted job. When the third candidate received a response, I did not count any of these as a
“true” employer response in the data.
Employers made multiple attempts to contact candidates in 16.0% of phone responses compared to 11.3% of email responses.

Table 3.12 shows descriptive statistics for the job advertisements by the application pair. Pair 1 refers to a black candidate with an elite degree and a white candidate with a less selective degree (Pair IDs 13-24 from Table 3.8); pair 2 refers to a white candidate with an elite degree and a black candidate with a less selective degree (Pair IDs 1-12 from Table 3.8). Recall that each job advertisement or employer received only two applicants from my fictional pool, so each pair applied for a different sample of jobs. However, as the table shows, the different pairs did not apply for significantly different types of jobs in respect to occupational category, listed salary ranges, or the rate of attrition. The set of jobs each of the pairs applied for are approximately 23% sales, 17-19% customer service, 15% administrative assistant, 9-10% analyst, 8-9% clerical, 5-6% human resources, 5% managerial, and 13-16% other categories. Pair 1 applied for jobs with listed starting salary ranges averaging between $31,000 and $37,600 and pair 2 applied for jobs with listed starting salary ranges averaging between $31,800 and $37,900. Finally, the attrition rates are similar at 6.2% of job advertisement for pair 1 and 5.0% for pair 2.

In Figure 3.3, I take a closer look at the distribution of listed salary ranges. From the top to the bottom, this figure shows the low, mean, and high salary range variables. Each figure has a normal curve overlaid for reference. As the figure shows, the three variables are relatively close to normal, which is important for the later analysis mentioned at the end of the next section.

F. Methods of Analysis
Examinations of the results consist of four different types of analysis depending on the type of effect measured (direct or indirect) and the outcome variable (employer response or listed salary range). It is important to recall that discussion of sample and sample size in this context is different from that of a traditional experiment. In the context of this research, sample refers to the sample of jobs or employers.

With a binomial distribution and a small sample size, statisticians suggest using a t-test for comparing two proportions. However, a normal approximation is acceptable if n>5 and the following equation is true (Box, Hunter, and Hunter 1978):

\[ | (1/\sqrt{n}) \ast \sqrt{(1-p)/p} - \sqrt{p/(1-p)} | < 0.3 \]  

3.1

In all possible comparison cases in my data this is true; the maximum value of this equation is 0.19. A two-tailed paired t-test is appropriate for testing significant differences within pairs from the same sample (Kutner, Neter, Nachtsheim, and Li 2004). Thus, it is acceptable to use a two-tailed paired t-test to improve efficiency for the estimation of direct effects:

\[ t = \bar{x} / (s/\sqrt{n}) \]  

3.2

However, to examine indirect effects I must use a less efficient estimator because the sample and the sample size varies between the two groups. The Welch’s t-test is appropriate with two independent samples of unequal sample size and unequal variance (ibid):
\[ t = \bar{x}_1 - \bar{x}_2 / \left( \sqrt{\frac{s^2_1}{n_1} + \frac{s^2_2}{n_2}} \right) \]

Examination of either direct or indirect effects using these equations is done in Stata using the \texttt{ttest} command and the appropriate options.

Although these basic significance tests are appropriate for measuring the differences across characteristics both within and between pairs, a logistic regression equation predicting odds-ratios provides somewhat more intuitive results. Additionally, a logistic regression simultaneously controls for all of the observed characteristics, returns estimates that are more appropriately weighted based on the small differences across sample size due to attrition, and allows for cluster-corrected standard errors at the employer level:

\[
\text{logit}(p_i) = \alpha_i + \beta_1 CS_i + \beta_2 R_i + \beta_3 SC_i + \beta_4 G_i + \beta_5 M_i + \beta_6 RE_i + \beta_7 X_i + u_i + e_{ij}
\]

In the equation above, \(\alpha_i\) is the individual-level intercept, the \(\beta\) coefficients 1-6 represent the coefficients for college selectivity, race, social class, gender, college major, and region, respectively, \(X_i\) represents a vector of control variables, \(u_i\) is the individual-level error term, and \(e_{ij}\) is the employer-level error term.

Finally, among only those candidates who receive responses for jobs that include a listed salary range, I run OLS regression models to examine differences in these listed salaries:
\[ Y_i = \alpha_i + \beta_1 CS_i + \beta_2 R_i + \beta_3 SC_i + \beta_4 G_i + \beta_5 M_i + \beta_6 RE_i + \beta_7 X_i + u_i + e_{ij} \]

In the equation above, \( Y_i \) is one of three possible variables that captures information about the salary range: the lowest listed value in the range, the mean of the range, or the highest listed value in the range. I run three separate regressions, one for each possible listed salary outcome variable.
Table 3.1. Audit Design Matrix – First Round of Pilot Data Collection

<table>
<thead>
<tr>
<th>ID #</th>
<th>Name</th>
<th>School</th>
<th>Employment</th>
<th>Cover</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Ebony Williams</td>
<td>Stanford</td>
<td>A</td>
<td>1</td>
</tr>
<tr>
<td>2</td>
<td></td>
<td></td>
<td>A</td>
<td>2</td>
</tr>
<tr>
<td>3</td>
<td></td>
<td></td>
<td>B</td>
<td>1</td>
</tr>
<tr>
<td>4</td>
<td></td>
<td></td>
<td>B</td>
<td>2</td>
</tr>
<tr>
<td>5</td>
<td></td>
<td>UC Riverside</td>
<td>A</td>
<td>1</td>
</tr>
<tr>
<td>6</td>
<td></td>
<td></td>
<td>A</td>
<td>2</td>
</tr>
<tr>
<td>7</td>
<td></td>
<td></td>
<td>B</td>
<td>1</td>
</tr>
<tr>
<td>8</td>
<td></td>
<td></td>
<td>B</td>
<td>2</td>
</tr>
<tr>
<td>9</td>
<td>Kristen Thomas</td>
<td>Stanford</td>
<td>A</td>
<td>1</td>
</tr>
<tr>
<td>10</td>
<td></td>
<td></td>
<td>A</td>
<td>2</td>
</tr>
<tr>
<td>11</td>
<td></td>
<td></td>
<td>B</td>
<td>1</td>
</tr>
<tr>
<td>12</td>
<td></td>
<td></td>
<td>B</td>
<td>2</td>
</tr>
<tr>
<td>13</td>
<td></td>
<td>UC Riverside</td>
<td>A</td>
<td>1</td>
</tr>
<tr>
<td>14</td>
<td></td>
<td></td>
<td>A</td>
<td>2</td>
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<tr>
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<td></td>
<td></td>
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<td>1</td>
</tr>
<tr>
<td>16</td>
<td></td>
<td></td>
<td>B</td>
<td>2</td>
</tr>
</tbody>
</table>
Table 3.2. Candidate Profiles and Submission Timeline – First Round of Pilot Data Collection

<table>
<thead>
<tr>
<th>ID #1</th>
<th>ID #2</th>
<th># Jobs selected to apply for</th>
<th>First candidate submitted</th>
<th>Second candidate submitted</th>
<th>Completed?</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>16</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>2</td>
<td>15</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>3</td>
<td>14</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>4</td>
<td>13</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>5</td>
<td>12</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>6</td>
<td>11</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>7</td>
<td>10</td>
<td>15</td>
<td>Day 1 (12-10-10)</td>
<td>Day 4 (12-13-10)</td>
<td>Y</td>
</tr>
<tr>
<td>8</td>
<td>9</td>
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Table 3.3. Audit Design Matrix – Second Round of Pilot Data Collection

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Table 3.5. Employer Responses by College – Second Round of Pilot Data Collection

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<th>Stanford</th>
<th>UCR</th>
</tr>
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<tbody>
<tr>
<td>Email</td>
<td>15/228 (6.58%)</td>
<td>10/228 (4.39%)</td>
</tr>
<tr>
<td>Phone</td>
<td>19/228 (8.33%)*</td>
<td>6/228 (2.63%)*</td>
</tr>
<tr>
<td>Total</td>
<td>34/228 (14.91%)**</td>
<td>16/228 (7.02%)**</td>
</tr>
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</table>

Note: * denotes the proportion is significantly different from the other college category using a 2-tailed test for proportions.
* $p < 0.05$, ** $p < 0.01$

Table 3.6. Employer Responses by Race – Second Round of Pilot Data Collection

<table>
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<th>White</th>
<th>Black</th>
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<tr>
<td>Email</td>
<td>16/228 (7.02%)</td>
<td>9/228 (3.95%)</td>
</tr>
<tr>
<td>Phone</td>
<td>18/228 (7.89%)*</td>
<td>7/228 (3.07%)*</td>
</tr>
<tr>
<td>Total</td>
<td>34/228 (14.91%)**</td>
<td>16/228 (7.02%)**</td>
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</tbody>
</table>

Note: * denotes the proportion is significantly different from the other racial category using a 2-tailed test for proportions.
* $p < 0.05$, ** $p < 0.01$
### Table 3.7. Employer Responses by Candidate Profile—Second Round of Pilot Data Collection

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<tr>
<td>White candidate</td>
<td>9/116 (7.76%)</td>
<td>7/112 (6.25%)</td>
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<tr>
<td>Black candidate</td>
<td>6/112 (5.36%)</td>
<td>3/116 (2.59%)</td>
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<tr>
<td>White candidate</td>
<td>15/116 (12.93%)&lt;sup&gt;bcd&lt;/sup&gt;</td>
<td>3/112 (2.68%)&lt;sup&gt;a&lt;/sup&gt;</td>
</tr>
<tr>
<td>Black candidate</td>
<td>4/112 (3.57%)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>3/116 (1.72%)&lt;sup&gt;a&lt;/sup&gt;</td>
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<tr>
<td><strong>Total</strong></td>
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<td></td>
</tr>
<tr>
<td>White candidate</td>
<td>24/116 (20.69%)&lt;sup&gt;bcd&lt;/sup&gt;</td>
<td>10/112 (8.93%)&lt;sup&gt;a&lt;/sup&gt;</td>
</tr>
<tr>
<td>Black candidate</td>
<td>10/112 (8.93%)&lt;sup&gt;a&lt;/sup&gt;</td>
<td>6/116 (5.17%)&lt;sup&gt;a&lt;/sup&gt;</td>
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</table>

Note: a denotes the proportion is significantly different from the white Stanford candidate, b from the black Stanford candidate, c from the white UCR candidate, and d from the black UCR candidate at p<0.05 using a 2-tailed test for proportions.
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Note: A1 = applicant 1, A2 = applicant 2, LS = less selective. These 24 pairs represent the total set of candidate pairs that applied to jobs across the three regions.
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<th>% &gt;= Some College</th>
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<td>86.1%</td>
<td>12.7%</td>
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<td>DaQuan</td>
<td>87.3%</td>
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<td>90.1%</td>
<td>9.9%</td>
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<td>14.3%</td>
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<td>39.0%</td>
<td>61.0%</td>
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<tr>
<td>02</td>
<td>1</td>
<td>2</td>
<td>Random</td>
<td>2</td>
</tr>
<tr>
<td>02</td>
<td>2</td>
<td>1</td>
<td>Random</td>
<td>1</td>
</tr>
<tr>
<td>02</td>
<td>2</td>
<td>2</td>
<td>Random</td>
<td>1</td>
</tr>
<tr>
<td>…</td>
<td>…</td>
<td>…</td>
<td>…</td>
<td>…</td>
</tr>
</tbody>
</table>

Table 3.10. Audit Design Matrix – Full Data Collection
Table 3.11. Applicant Descriptive Statistics – Full Data Collection

<table>
<thead>
<tr>
<th></th>
<th>Applicant 1</th>
<th></th>
<th>Applicant 2</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
<td>% (mean)</td>
<td>N</td>
<td>% (mean)</td>
</tr>
<tr>
<td>White</td>
<td>469</td>
<td>49.26%</td>
<td>483</td>
<td>50.74%</td>
</tr>
<tr>
<td>Black</td>
<td>483</td>
<td>50.74%</td>
<td>469</td>
<td>49.26%</td>
</tr>
<tr>
<td>Elite College</td>
<td>482</td>
<td>50.63%</td>
<td>470</td>
<td>49.37%</td>
</tr>
<tr>
<td>Less Selective College</td>
<td>470</td>
<td>49.37%</td>
<td>482</td>
<td>50.63%</td>
</tr>
<tr>
<td>Male</td>
<td>475</td>
<td>49.89%</td>
<td>475</td>
<td>49.89%</td>
</tr>
<tr>
<td>Female</td>
<td>477</td>
<td>50.11%</td>
<td>477</td>
<td>50.11%</td>
</tr>
<tr>
<td>Upper Class</td>
<td>322</td>
<td>33.82%</td>
<td>322</td>
<td>33.82%</td>
</tr>
<tr>
<td>Middle Class</td>
<td>309</td>
<td>32.46%</td>
<td>309</td>
<td>32.46%</td>
</tr>
<tr>
<td>Lower Class</td>
<td>321</td>
<td>33.72%</td>
<td>321</td>
<td>33.72%</td>
</tr>
<tr>
<td>Region - Southeast</td>
<td>318</td>
<td>33.40%</td>
<td>318</td>
<td>33.40%</td>
</tr>
<tr>
<td>Region - Northeast</td>
<td>320</td>
<td>33.61%</td>
<td>320</td>
<td>33.61%</td>
</tr>
<tr>
<td>Region - West</td>
<td>314</td>
<td>32.98%</td>
<td>314</td>
<td>32.98%</td>
</tr>
<tr>
<td>Home Region</td>
<td>673</td>
<td>70.69%</td>
<td>673</td>
<td>70.69%</td>
</tr>
<tr>
<td>Out of Home Region</td>
<td>279</td>
<td>29.31%</td>
<td>279</td>
<td>29.31%</td>
</tr>
<tr>
<td>Major - Economics</td>
<td>479</td>
<td>50.32%</td>
<td>479</td>
<td>50.32%</td>
</tr>
<tr>
<td>Major - Psychology</td>
<td>473</td>
<td>49.68%</td>
<td>473</td>
<td>49.68%</td>
</tr>
<tr>
<td>Response - Email</td>
<td>74</td>
<td>7.77%</td>
<td>67</td>
<td>7.04%</td>
</tr>
<tr>
<td>Response - Phone</td>
<td>76</td>
<td>7.98%</td>
<td>80</td>
<td>8.40%</td>
</tr>
<tr>
<td>Response - Both</td>
<td>32</td>
<td>3.36%</td>
<td>35</td>
<td>3.68%</td>
</tr>
<tr>
<td>Response - Total (either email or phone)</td>
<td>118</td>
<td>12.39%</td>
<td>112</td>
<td>11.76%</td>
</tr>
<tr>
<td>Removed</td>
<td>56</td>
<td>5.56%</td>
<td>56</td>
<td>5.56%</td>
</tr>
<tr>
<td>N</td>
<td>952</td>
<td>94.44%</td>
<td>952</td>
<td>94.44%</td>
</tr>
</tbody>
</table>

Note: Applicant 1 and 2 refers to the order of application to a job within a pair. Removed indicates attrition from the sample – an employer removed a job advertisement before one or both applicants could apply for the job.
### Table 3.12. Employer Descriptive Statistics – Full Data Collection

<table>
<thead>
<tr>
<th>Occupational Category</th>
<th>Pair 1</th>
<th></th>
<th>Pair 2</th>
<th></th>
<th>Difference</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
<td>% (mean)</td>
<td>N</td>
<td>% (mean)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Administrative Assistant</td>
<td>73</td>
<td>15.43%</td>
<td>72</td>
<td>15.03%</td>
<td>0.8631</td>
<td></td>
</tr>
<tr>
<td>Analyst</td>
<td>48</td>
<td>10.15%</td>
<td>45</td>
<td>9.39%</td>
<td>0.6958</td>
<td></td>
</tr>
<tr>
<td>Clerical</td>
<td>39</td>
<td>8.25%</td>
<td>43</td>
<td>8.98%</td>
<td>0.6878</td>
<td></td>
</tr>
<tr>
<td>Customer Service</td>
<td>82</td>
<td>17.34%</td>
<td>91</td>
<td>19.00%</td>
<td>0.5067</td>
<td></td>
</tr>
<tr>
<td>Human Resources</td>
<td>26</td>
<td>5.50%</td>
<td>31</td>
<td>6.47%</td>
<td>0.5266</td>
<td></td>
</tr>
<tr>
<td>Managerial</td>
<td>25</td>
<td>5.29%</td>
<td>24</td>
<td>5.01%</td>
<td>0.8480</td>
<td></td>
</tr>
<tr>
<td>Other – Kids</td>
<td>27</td>
<td>5.71%</td>
<td>21</td>
<td>4.38%</td>
<td>0.3511</td>
<td></td>
</tr>
<tr>
<td>Other – Physical</td>
<td>12</td>
<td>2.54%</td>
<td>13</td>
<td>2.71%</td>
<td>0.8646</td>
<td></td>
</tr>
<tr>
<td>Other</td>
<td>29</td>
<td>6.13%</td>
<td>28</td>
<td>5.85%</td>
<td>0.8529</td>
<td></td>
</tr>
<tr>
<td>Sales</td>
<td>112</td>
<td>23.68%</td>
<td>111</td>
<td>23.17%</td>
<td>0.8541</td>
<td></td>
</tr>
<tr>
<td>Listed Salary - Low</td>
<td>141</td>
<td>$30,977.22</td>
<td>148</td>
<td>$31,789.65</td>
<td>0.4376</td>
<td></td>
</tr>
<tr>
<td>Listed Salary - Mean</td>
<td>141</td>
<td>$34,305.89</td>
<td>148</td>
<td>$34,834.23</td>
<td>0.6396</td>
<td></td>
</tr>
<tr>
<td>Listed Salary - High</td>
<td>141</td>
<td>$37,634.56</td>
<td>148</td>
<td>$37,878.83</td>
<td>0.8546</td>
<td></td>
</tr>
<tr>
<td>Removed</td>
<td>31</td>
<td>6.15%</td>
<td>25</td>
<td>4.96%</td>
<td>0.4099</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>473</td>
<td>93.85%</td>
<td>479</td>
<td>95.04%</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Pair 1 refers to black applicants with an elite degree and white applicants with a less selective degree; Pair 2 refers to white applicants with an elite degree and black applicants with a less selective degree. Difference indicates the p-value of a two-tailed t-test examining the difference in values between Pair 1 and Pair 2. Removed indicates attrition from the sample – an employer removed a job advertisement before one or both applicants could apply for the job.
Figure 3.1. Sample Resume – Aubrey Clark, Duke University

Aubrey Clark
6717 Six Forks Road
Apt 1167
Raleigh, NC 27615
Phone: 910-377-1895
E-mail: aubc.clark@gmail.com

Objective
To work in an organization where my leadership experience, analytical and technical skills from intensive college coursework, and personal communication and networking abilities can help improve profitability for the company and allow me to grow professionally.

Education
B.S. Psychology, Magna Cum Laude, Duke University, May 2011
\begin{itemize}
  \item 3.79 GPA
  \item Activities: Phi Beta Kappa, Duke Debate, Amnesty International, Duke Engage Student Initiative (DESI), and the Economics Student Union (ESU).
  \item Secretary Treasurer of ESU and Vice-President of DESI.
  \item Dean’s list throughout junior and senior years.
\end{itemize}

Experience
Internship (April 2010 – May 2011)
Scottrade - Durham, NC
\begin{itemize}
  \item Assisted clients in opening new accounts and provided existing account support.
  \item Completed extensive cross-checks on financial documents and confirmed accuracy through multiple database sources.
  \item Co-leader of intern research project that eventually led to increased search efficiency.
\end{itemize}
Sales Associate (May 2009 – January 2010)
Border’s Bookstore - Chapel Hill, NC

Skills
\begin{itemize}
  \item Proficient in Microsoft Word, Excel, PowerPoint and Access; HTML.
\end{itemize}

References available upon request.
Figure 3.2. Sample Resume – Nia Price, University of North Carolina at Greensboro

Nia Price

404 W. Smith Street
Apartment 945
Greensboro, NC 27401
Phone: 336-612-4125
Email: nia.d.price@gmail.com

OBJECTIVES
To bring my experience as an administrative assistant, knowledge of research, and general leadership skills to a work environment where I can contribute to improving efficiency and profitability, as well as have a positive impact on my peers.

EDUCATION
B.S. Psychology, University of North Carolina at Greensboro
May 2011
- 3.87 GPA, Magna Cum Laude
  Golden Key International Honour Society
- Member of Alternative Spring Break, Student Government Association, Economics Club, and Habitat for Humanity.
- Leadership experience through Alternative Spring Break – supervised a team of 8 other undergraduate students to rebuild houses in New Orleans. Coordinated and organized fundraising events to benefit a non-profit organizations through Economics Club.

EXPERIENCE
Intern | Marcus and Millichap Real Estate Investment Services – Raleigh, NC
May 2010 – June 2011
- Aided management team by coordinating schedules of and communicating with 14 field agents.
- Supported team of field agents by researching properties and compiling information packets.
- Prepared spreadsheets and figures using research on market conditions and past sales.

Student Service Assistant | UNC-G Library
August 2009 – April 2010

SKILLS
Proficient in MS Office, Adobe Dreamweaver, SPSS statistical software, HTML (intermediate)
Figure 3.3. Employer Descriptive Statistics – Full Data Collection
CHAPTER 4.
CALL ME, MAYBE?
EDUCATIONAL CREDENTIALS AND DISCRIMINATION IN THE LABOR MARKET

“forget the others: HARVARD GRAD”
-- Subject line in an email received by a candidate from an employer.

How much do educational credentials, particularly college selectivity and college major matter in the likelihood of receiving any type of response from an employer? What about employer discrimination on the basis of race and gender? In this chapter, I examine the results from my audit experiment in which I applied for 1,008 jobs using matched candidate pairs. The unique design of an audit study allows me to separately explore the effects of educational credentials and social background characteristics. In turn, I examine individual effects and then interactive effects to see how educational credentials and social background characteristics combine to impact response rates.

But first, what of the quote at the beginning of this chapter? Beyond employer contact with candidates requesting interviews or more information, employers exchanged internal emails amongst themselves. In thirteen cases, employers accidentally included candidates on correspondence that was intended for other employees of the company, presumably in the human resources department. Most of these emails were forwarded versions of the brief email that is sent to employers with limited candidate information notifying them of a new application. Typically, the sender included a sentence indicating
that the intended recipient should examine a particular candidate. In five cases these messages, in an excited or urgent tone, explicitly mentioned the institution from which a candidate held a degree:

“ok, she had me at Stanford. Eat our dust [competitor].”

“forget the others: HARVARD GRAD”

“Kids coming out of Duke are by far the most capable. Push this one to the top of the list.”

“Harvard guy wants to work for us!”

“We had a real bright app pop up this morning – Stanford grad with great credentials.”

These accidental emails provide a small amount of qualitative insight into the importance employers place on a degree from an elite college. In zero of the thirteen cases did an employer explicitly mention one of the less selective college, race, gender, or any other characteristics. Thus, it is likely that the signal of an elite credential is at the forefront of employers’ minds. However, as the results in the following sections show, other characteristics are also extremely important in a candidate’s likelihood of receiving a response from an employer.

Comparisons of Employer Responses by Single Characteristics

Figures 4.1 through 4.6 show the bivariate results of employer responses by each of the main characteristics of interest: college selectivity, race, social class, gender, college major, and region. Each figure shows three sets of bars: the response percentage
separately by email and phone and the total response percentage. The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval. I use the more conservative two-tailed Welch’s t-test to examine statistical significance because each of these bivariate comparisons represent either indirect effects or a combination of direct and indirect and effects.

First, I examine responses by the two educational credentials measures, college selectivity and college major, in Figures 4.1 and 4.2 respectively. Candidates with a degree from an elite college receive more email responses than candidates with a degree from a less selective college at a rate of approximately 1.4 to 1 (8.7% vs. 6.1%). This difference is larger when examining phone responses from employers: 10.7% vs. 5.7%, or a ratio of 1.9. If we examine the results of either an email or phone response (total response) from employers, candidates with a degree from an elite college are 1.7x as likely to get any response as candidates with a degree from a less selective college (15.2% vs. 8.9%). In all of these cases, a two-tailed Welch’s t-test shows that the differences in means between candidates with a degree from an elite college and candidates with a degree from a less selective college are statistically significant (p < 0.05 for email; p < 0.001 for phone and total responses).

The results examining college major show no statistically significant differences between candidates with an economics degree and candidates with a psychology degree (see Figure 4.2). Although candidates with an economics degree receive more email, phone, and total responses in the aggregate, the sample size is not large enough to confidently say that this is a true difference in means (p-values range from 0.28 to 0.38).

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24 The total response percentage does not equal email plus phone because some employers responded by both email and phone.
Still, it is meaningful to note that this small difference is of substantive importance and in the expected direction.

The next set of figures shows the bivariate results of employer responses by social background characteristics. Figure 4.3 reports employer responses for white versus black candidates. White candidates receive more email responses than black candidates at a rate of approximately 1.4 to 1 (8.7% vs. 6.1%) and more phone responses at a rate of approximately 1.6 to 1 (10.0% vs. 6.4%). For total responses from an employers, white candidates are 1.5x as likely to get a response as black candidates (14.5% vs. 9.7%).

These results are significantly different between white and black candidates (p < 0.05 for email; p < 0.01 for phone and total responses). Moreover, when both white and black candidates were contacted by the same employer, blacks candidates experienced a longer wait time between responses (3.6 days longer) than white candidates. In other words, employers gave white candidates more time to respond before moving on to contact black candidates, but more quickly moved on to white candidates after failing to hear back from black candidates.

To examine social class differences in candidate names, I combine upper and middle class names as there are no significant differences between those two categories. Figure 4.4 shows that candidates with a middle or upper class name receive more email responses than candidates with a lower class name at a rate of approximately 1.6 to 1 (8.5% vs. 5.3%). This difference is larger when examining phone responses from employers: 9.5% vs. 5.6%, or a ratio of 1.7. If we examine the results of either an email or phone response (total response) from employers, candidates with a middle or upper class name are 1.5x as likely to get any response as candidates with a lower class name.
(13.7% vs. 8.9%). These results are significantly different between upper/middle class names and lower class names (p < 0.01 for email, phone, and total responses).

Next, Figure 4.5 reports employer responses for male versus female candidates. Male candidates receive more email, phone, and total responses than female candidates at only slightly higher rates (~1.1 to 1 for all three response types). However, none of these differences are statistically significant (p-values range from 0.39 to 0.75). These ratios are in line with expectations for the size and direction of gender inequality based on the recent literature.

Finally, Figure 4.6 shows the differences in responses across the three regions. Email responses by employers are similar between the Southeast and the West (6.3% vs. 6.2%) but higher in the Northeast (9.7%) for a ratio of 1.5 to 1 (the Northeast to other regions). This gap closes slightly in phone responses between the Northeast to other regions (1.4 to 1). In total, the response rate in the Northeast is highest at 14.7% followed by the West at 11.0% and the Southeast at 10.5%. The differences between the Northeast and the Southeast and between the Northeast and the West are statistically significant (p < 0.10 for phone; p < 0.05 for email and total responses).

These results tentatively suggest that both educational credentials and social background characteristics are important. College selectivity, race, and social class all have strong relationships with the rate of employer responses. Although differences in college major and gender are not statistically significant at the bivariate level, these results still conform to expectations based on the prior literature. Additionally, there are regional differences in overall response rates. In the next section, I examine all of these variables further in a series of logistic regressions.
Logistic Regressions of Employer Responses

Due to the small differences in attrition across the two samples of job advertisements (between application pairs), it is important to examine logistic regressions predicting employer responses that include all variables of interest and all other control variables. In Tables 4.1 and 4.2, I show the results of two types of logistic regressions: those that include all completed cases and those that include a fixed-effect for each job advertisement or employer.

Table 4.1 reports the odds ratios for each of the variables of interest. These results closely match those of the bivariate figures presented in the previous section and suggest four variables are statistically significant: race, college selectivity, social class, and region. Compared to whites, blacks are 62.8% as likely to receive any type of employer response. Candidates with a degree from an elite college are 184.1% as likely as candidates with a degree from a less selective college to receive any type of employer response. Compared to candidates with an upper or middle class name, candidates with a lower class name are 60.7% as likely to receive any type of employer response. Finally, compared to the Southeast, candidates in the Northeast are 147.5% as likely to receive any type of employer response.

The results shown in Table 4.2 model 1 are derived from a fixed-effects logistic regression model that includes only variables that have variation within pairs and restricts the sample by dropping cases in which both applicants or neither applicant received an employer response. Thus, this model represents cases where employers made a clear decision between the two submitted applicants rather than contacting both or neither and uses direct effects in the estimation process. Although there are no observable
differences across job advertisement samples, the fixed-effects model also controls for any *unobservable* differences across job advertisement samples. The results show black applicants are only 43.7% as likely as white applicants to receive any type of employer response and candidates with a degree from an elite college are 303.3% as likely as candidates with a degree from a less selective college to receive any type of employer response.

Overall, the bivariate and logistic regression results presented in these first two sections suggest that both educational credentials and social background characteristics have individual effects on the likelihood of any type of employer response. Employers strongly value a degree from an elite college but also discriminate against candidates with black and lower class names. An additional area of inquiry is how these variables work together. For instance, can black candidates close the gap in responses with white candidates when they have a degree from an elite college over a degree from a less selective college? In the next section I explore combinations of two independent variables on response rates and logistic regressions using interactions.

*Educational Credentials or Racial Discrimination?*

In Figure 4.7, I examine total employer responses across race and college selectivity. In two cases (i.e. white candidates with a degree from an elite college vs. black candidates with a degree from a less selective college and black candidates with a degree from an elite college vs. white candidates with a degree from a less selective college) I use a two-tailed paired t-test because it is a direct comparison of matched pairs. In the other two cases I use a two-tailed Welch’s t-test because it compares cases across different job samples. These results suggest a tiered pattern of responses: white
candidates with a degree from an elite college have the highest response rate (17.5%), followed by black candidates with a degree from an elite college (12.9%) and white candidates with a degree from a less selective college (11.4%)\(^{25}\), and finally black candidates with a degree from a less selective college have the lowest response rate (6.5%). The differences between white candidates with a degree from an elite college and all other candidates are statistically significant (\(p<0.05\) for black candidates with a degree from an elite college; \(p<0.01\) for white candidates with a degree from a less selective college; \(p < 0.001\) for black candidates with a degree from a less selective college). The differences between black candidates with a degree from a less selective college and all other candidates are statistically significant (\(p<0.01\) for white candidates with a degree from a less selective college; \(p < 0.001\) for white candidates with a degree from an elite college and black candidates with a degree from an elite college). Another way to consider these response rates is that a white candidate with a degree from an elite college can expect an employer response for every 6 resumes submitted, while an equally qualified black candidate requires submitting 8 resumes to receive a response; white candidates with a degree from a less selectivity college need to submit 9 resumes to expect a response, while a similar black candidate needs to submit 15 resumes to receive a response.

Next, Figure 4.8 reports total employer responses across race and college major. These results suggest that between economics and psychology degrees the college major of white candidates does not matter. White candidates with an economics degree receive a response rate of 14.6% while those with a psychology degree receive a response rate of 14.4%. For black candidates, there is a small contrast between an economics degree and an economics degree and

\(^{25}\) These two categories are never statistically different across any employer response type.
a psychology degree. Black candidates with an economics degree receive a response rate of 11.1% while those with a psychology degree receive a response rate of 8.2% (p-value=0.14). The difference between white and black candidates with an economics degree are marginally significant (p<0.10) and the difference between white and black candidates with a psychology degree are statistically significant (p<0.01).

Thus, educational credentials matter for both white and black candidates. Employer responses are higher for candidates of either race with a college degree from an elite college and higher (although not statistically significant) for black candidates with an economics degree over a psychology degree. However, racial discrimination still exists in the labor market, as black candidates receive fewer employer responses regardless of educational credentials. In Table 4.3 models 1 through 3, I explore potential interaction effects of race and educational credentials. Although the interaction effect of black candidate and degree from an elite college is positive in both models, it is not statistically significant. An additional model using fixed-effects logistic regression does not reach significance either (see Table 4.2 model 2). This suggests that, compared to white candidates, black candidates do not gain more from a degree from an elite college over a degree from a less selective college. In other words, the effects of race and college selectivity are additive but not interactive. What is striking is that black candidates with a degree from an elite college only do as well as white candidates with a degree from a less selective college. Additionally, the interaction effect of black candidate and psychology degree is negative but not statistically significant (see Table 4.3 model 3). Thus, educational credentials and race both matter in the labor market, but in the context of this

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26 Model 2 uses a smaller sample by dropping cases where both applicants received an employer response in an attempt to gain more leverage on variation with employers who made a distinct choice between the two presented applicants.
study with the variables used, racial discrimination would exist regardless of the level of educational credentials for whites and blacks.

**The Combination of Race and Other Characteristics**

Other characteristics may have important interactive or additive effects with race. From the prior literature, there are reasons to expect that the effects of race may vary by the social class connotations of a name, by gender, and across regions. In Figures 4.9 through 4.11, I examine total employer response rates by these combinations of characteristics.

By race and social class, I find a strongly tiered pattern (see Figure 4.9). White candidates with an upper or middle class name receive the highest response (15.7%), followed by white candidates with a lower class name (12.1%), black candidates with an upper or middle class name (11.7%), and then black candidates with a lower class name (5.6%). Class differences within race are not statistically significant for white candidates (p-value=0.13), but are statistically significant for black candidates (p<0.001). Race differences within class are statistically significant for both upper/middle class names (p<0.05) and lower class names (p<0.01). A logistic regression model that includes an interaction effect of black candidate and lower-class name confirms these results (see Table 4.3 model 4). Black candidates with an upper or middle class name are 71.4% as likely to receive any type of employer response compared to white applicants with an upper or middle class name (p<0.05). Black candidates with a lower class name are 44.2% as likely to receive any type of employer response compared to white applicants with an upper or middle class name (p<0.05) and 57.6% as likely to receive any type of employer response compared to white applicants with a lower class name (p<0.05).
These results suggest that the social class connotations of racialized names have strong consequences for blacks but not whites. Moreover, audit studies that use names as a marker for race must be aware of these social class implications as well. For instance, in this study employer discrimination against candidates with a black name varies from a 27% reduction in responses (all black upper/middle class names) to a 66% reduction in responses (all black lower class names).

In Figure 4.10, I examine employer responses by race and gender. White male candidates receive the highest response (16.0%), followed by white female candidates (13.0%), black female candidates (10.7%), and then black male candidates (8.6%). Gender differences within race are not statistically significant for white candidates (p-value=0.19) or black candidates (p-value=0.28). Race differences within gender are statistically significant for males (p<0.001) but not females (p-value=0.27). A logistic regression model that includes an interaction effect of black candidate and female confirms that black females are advantaged over their male counterparts compared to the white gender relationship (see Table 4.3 model 5). Black male candidates are only 48.9% as likely as white male candidates to receive an employer response (p<0.001) while black female candidates are 62.5% as likely as white male candidates to receive an employer response (p<0.05). Although these results do not suggest a statistically significant effect of gender either at the aggregate or for white candidates, they do show a tiered pattern whereby the traditional gender hierarchy is inverted for black candidates. This is important, as much of the prior literature using audit studies to examine racial discrimination has focused on males. However, these findings suggest that black males may face higher levels of discrimination than black females.
Finally, I examine employer responses by race and region in Figure 4.11. In the Northeast, responses are highest overall for both white and black candidates. The ratios of responses for white to black candidates range from 1.35 to 1 in the Northeast to 1.76 to 1 in the West, although these differences across regions are not statistically significant. The differences between white and black candidates in the Northeast are not statistically significant (p-value=0.12) but are marginally significant in the Southeast (p<0.10) and significant in the West (p<0.05). A logistic regression model that includes interaction effects of black candidate and Northeast or West does not alter this story (see Table 4.3 model 6). Although these results do not suggest a statistically significant differences in discrimination across regions, they hint at the possibility with larger sample sizes or perhaps different regions. Unfortunately, due to the lack of repeated data collection over time, it is unclear whether any of these small differences across regions represent differences in discrimination or differences in labor market characteristics.

*Educational Credentials or Gender Discrimination?*

Although I find stark racial differences in the previous sections that suggest the importance of both racial discrimination and educational credentials in receiving a response from an employer, the prior bivariate examinations of gender offer more modest differences. In Figure 4.12, I examine total employer responses across gender and college selectivity. The results suggest that the difference between a degree from an elite college and a degree from a less selective college is statistically significant for both males and females (p<0.01). However, the gender differences within each level of college selectivity are not statistically significant (p-value=0.77 for a degree from an elite college and p-value=0.89 for a degree from a less selective college). A logistic regression model
that includes an interaction effect of female candidate and a degree from an elite college confirms no interactive relationship (see Table 4.4 model 1). Thus, the effect of college selectivity works the same for men and women.

Next, Figure 4.13 reports total employer responses across gender and college major. There are no statistically significant difference within college major by gender. However, within gender by college major I find a substantively interesting pattern: male candidates with an economics degree receive a response rate of 14.3% and those with a psychology degree receive a response rate of 10.3% (p<0.10), while female candidates with a psychology degree receive a response rate of 12.3% and those with an economics degree receive a response rate of 11.4% (p-value=0.64). In a logistic regression, these results do not hold up. There is no statistically significant effect of college major for males or females (see Table 4.4 model 2). Although I am cautious to make too much out of this pattern, it is interesting that with a larger sample size, it could potentially be different from expected, as it suggests college major may not matter for women but may matter for men (between economics and psychology).

In summary, educational credentials matter for both male and female candidates, but there are no differences in the returns on these credentials by gender. Furthermore, I find no solid evidence of gender discrimination in any of my results. The available evidence suggests that given equal educational credentials, employers are just as likely to respond to men as they are to women. However, it is important to recall that in the prior section I found that employers are more likely to respond to black female candidates than black male candidates.

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27 Also see the Appendix in this chapter for additional analyses of gender by other characteristics.
A Snapshot of the Labor Market

Understanding how these characteristics all work in tandem may seem complicated and in some ways is slightly beyond the scope of what the data allow. However, logistic regressions with covariates for specific combinations of candidate characteristics allow me to estimate the predicted probabilities of response for four characteristics simultaneously. In Figure 4.14, I examine these predicted probabilities for every possible race, gender, social class, and college selectivity combination holding all other variables at their means. The dashed line in this figure represents the average response rate (13.7%) of all applicants in the analysis. As indicated, all candidates with a degree from an elite college outperform the mean except two: black male and female candidates with a lower class name. At the low end, a black female candidate with a lower class name and a degree from a less selective college needs to submit 40 applications to receive an employer response (2.5% predicted employer response rate). At the high end, a white male candidate with an upper or middle class name and a degree from an elite college needs to submit only 3.4 applications to receive a response (29.5% predicted employer response rate).

Figure 4.15 presents the results of a similar logistic regression but examines predicted probabilities for every race, gender, college selectivity, and college major combination holding all other variables at their means. White male candidates do quite well, except when they have a degree in psychology from a less selective college. Even white males with an economics degree from a less selective college slightly outperform all black candidates with either major from either level of college selectivity. At the low end, a black male or female candidate with a psychology degree from a less selective

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28 See the Appendix section in this chapter for more on three-way interactions.
college needs to submit 16 applications to receive an employer response (6.1% predicted employer response rate). At the high end, a white male candidate with an economics degree from an elite college needs to submit only 3.9 applications to receive a response (25.8% predicted employer response rate).

These predicted probabilities really put in perspective the opportunities available in the labor market for each individual candidate profile. Overall, this chapter shows that employer responses are consistently higher for candidates with a degree from an elite college but also for white candidates. Clearly, educational credentials matter but racial discrimination still pervades in the labor market. In the next chapter, I examine the effects of educational credentials and social background characteristic on other outcomes, such as potential salary range and type of job, among those candidates who receive responses.
<table>
<thead>
<tr>
<th></th>
<th>Email</th>
<th>Phone</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black (ref: White)</td>
<td>0.677**</td>
<td>0.616***</td>
<td>0.628***</td>
</tr>
<tr>
<td></td>
<td>(0.086)</td>
<td>(0.090)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>Elite (ref: Less Selective)</td>
<td>1.472**</td>
<td>2.007***</td>
<td>1.841***</td>
</tr>
<tr>
<td></td>
<td>(0.188)</td>
<td>(0.300)</td>
<td>(0.211)</td>
</tr>
<tr>
<td>Female (ref: Male)</td>
<td>0.923</td>
<td>0.864</td>
<td>0.956</td>
</tr>
<tr>
<td></td>
<td>(0.200)</td>
<td>(0.166)</td>
<td>(0.161)</td>
</tr>
<tr>
<td>Lower-class (ref: Upper/Middle)</td>
<td>0.599*</td>
<td>0.560*</td>
<td>0.607*</td>
</tr>
<tr>
<td></td>
<td>(0.150)</td>
<td>(0.132)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Major – Psychology (ref: Economics)</td>
<td>0.853</td>
<td>0.825</td>
<td>0.860</td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.159)</td>
<td>(0.145)</td>
</tr>
<tr>
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<td>1.606+</td>
<td>1.412</td>
<td>1.475+</td>
</tr>
<tr>
<td></td>
<td>(0.414)</td>
<td>(0.326)</td>
<td>(0.298)</td>
</tr>
<tr>
<td>Region – West</td>
<td>0.989</td>
<td>1.044</td>
<td>1.052</td>
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<td>(0.278)</td>
<td>(0.257)</td>
<td>(0.226)</td>
</tr>
<tr>
<td>Out of Home Region</td>
<td>0.881</td>
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<td>1.015</td>
</tr>
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<td></td>
<td>(0.211)</td>
<td>(0.221)</td>
<td>(0.186)</td>
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<td>Application submission (2nd)</td>
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<td>1.062</td>
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<td>Constant</td>
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<td>0.135***</td>
</tr>
<tr>
<td>N</td>
<td>1904</td>
<td>1904</td>
<td>1904</td>
</tr>
</tbody>
</table>

Note: All completed cases are included. Regressions also control for resume type, cover letter type, and employment history type. Odds ratios shown. Cluster-corrected (job advertisement level) standard errors in parenthesis.

+= p < 0.10, *= p < 0.05, **= p < 0.01, ***= p < 0.001
### Table 4.2. Fixed-Effects Logistic Regression Predicting Total Employer Response

<table>
<thead>
<tr>
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</tr>
</thead>
<tbody>
<tr>
<td>Black (ref: White)</td>
<td>0.437***</td>
<td>0.459***</td>
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<tr>
<td></td>
<td>(0.098)</td>
<td>(0.103)</td>
</tr>
<tr>
<td>Elite (ref: Less Selective)</td>
<td>3.033***</td>
<td>3.186***</td>
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<tr>
<td></td>
<td>(0.688)</td>
<td>(0.723)</td>
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<tr>
<td>Application submission (2\textsuperscript{nd})</td>
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<td>0.858</td>
</tr>
<tr>
<td></td>
<td>(0.188)</td>
<td>(0.188)</td>
</tr>
<tr>
<td>Black*Elite</td>
<td></td>
<td>0.906</td>
</tr>
</tbody>
</table>

| N                                    | 252        | 252        |

Note: Regression also controls for resume type, cover letter type, and employment history type. Odds ratios shown.

+ = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
Table 4.3. Logistic Regressions Predicting Employer Response (with Race Interactions)

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<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
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<td>0.348**</td>
<td>0.727*</td>
<td>0.714*</td>
<td>0.489***</td>
<td>0.639*</td>
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<td>(0.126)</td>
<td>(0.138)</td>
<td>(0.117)</td>
<td>(0.095)</td>
<td>(0.079)</td>
<td>(0.138)</td>
</tr>
<tr>
<td>Elite (vs. Less Selective)</td>
<td>1.652**</td>
<td>2.758***</td>
<td>1.843***</td>
<td>1.843***</td>
<td>1.846***</td>
<td>1.843***</td>
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<td>(0.701)</td>
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<td>(0.210)</td>
<td>(0.211)</td>
<td>(0.211)</td>
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<tr>
<td>Female (ref: Male)</td>
<td>0.957</td>
<td>0.931</td>
<td>0.956</td>
<td>0.956</td>
<td>0.777</td>
<td>0.956</td>
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<td>(0.161)</td>
<td>(0.147)</td>
<td>(0.161)</td>
</tr>
<tr>
<td>Lower-class (ref: Upper/Middle)</td>
<td>0.607*</td>
<td>0.496**</td>
<td>0.607*</td>
<td>0.740</td>
<td>0.607*</td>
<td>0.607*</td>
</tr>
<tr>
<td>Major – Psychology (ref: Economics)</td>
<td>0.860</td>
<td>0.708+</td>
<td>0.980</td>
<td>0.860</td>
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<td>0.859</td>
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<tr>
<td>Region – Northeast (ref: Southeast)</td>
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<td>1.236</td>
<td>1.478+</td>
<td>1.476+</td>
<td>1.477+</td>
<td>1.412</td>
</tr>
<tr>
<td>Region – West</td>
<td>1.052</td>
<td>0.922</td>
<td>1.052</td>
<td>1.052</td>
<td>1.052</td>
<td>1.135</td>
</tr>
<tr>
<td>Black*Elite</td>
<td>1.306</td>
<td>1.368</td>
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<td>Black*Psychology</td>
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<td>0.728</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black*Lower-class</td>
<td></td>
<td></td>
<td>0.597*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black*Female</td>
<td></td>
<td></td>
<td></td>
<td>1.645*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black*Northeast</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.107</td>
<td></td>
</tr>
<tr>
<td>Black*West</td>
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<td>0.825</td>
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<tr>
<td>Constant</td>
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<td>0.083***</td>
<td>0.127***</td>
<td>0.128***</td>
<td>0.149***</td>
<td>0.134***</td>
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<td>1904</td>
<td>1904</td>
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<td>1904</td>
</tr>
</tbody>
</table>

Note: Models 1 and 3 through 5 include all completed cases. Model 2 does not include cases with an employer response for both applicants. Regressions also control for out of home region, application submission, resume type, cover letter type, and employment history type. Odds ratios shown. Cluster-corrected (job advertisement level) standard errors in parenthesis.

+ = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female (ref: Male)</td>
<td>0.974</td>
<td>0.762</td>
<td>1.092</td>
<td>0.922</td>
</tr>
<tr>
<td></td>
<td>(0.223)</td>
<td>(0.173)</td>
<td>(0.191)</td>
<td>(0.278)</td>
</tr>
<tr>
<td>Elite (vs. Less Selective)</td>
<td>1.870***</td>
<td>1.844***</td>
<td>1.842***</td>
<td>1.842***</td>
</tr>
<tr>
<td></td>
<td>(0.300)</td>
<td>(0.211)</td>
<td>(0.211)</td>
<td>(0.211)</td>
</tr>
<tr>
<td>Black (ref: White)</td>
<td>0.628***</td>
<td>0.627***</td>
<td>0.628***</td>
<td>0.628***</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td>(0.071)</td>
<td>(0.071)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>Lower-class (ref: Upper/Middle)</td>
<td>0.607*</td>
<td>0.610*</td>
<td>0.644</td>
<td>0.603**</td>
</tr>
<tr>
<td></td>
<td>(0.120)</td>
<td>(0.120)</td>
<td>(0.182)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Major – Psychology (ref: Economics)</td>
<td>0.860</td>
<td>0.678</td>
<td>0.859</td>
<td>0.860</td>
</tr>
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<td>(0.145)</td>
<td>(0.162)</td>
<td>(0.145)</td>
<td>(0.146)</td>
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<tr>
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<td>1.482+</td>
<td>1.476+</td>
<td>1.264</td>
</tr>
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<td>(0.298)</td>
<td>(0.300)</td>
<td>(0.298)</td>
<td>(0.357)</td>
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<tr>
<td>Region – West</td>
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<td>1.055</td>
<td>1.051</td>
<td>1.199</td>
</tr>
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<td></td>
<td>(0.226)</td>
<td>(0.227)</td>
<td>(0.226)</td>
<td>(0.351)</td>
</tr>
<tr>
<td>Female*Elite</td>
<td>0.970</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(0.221)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female *Psychology</td>
<td></td>
<td>1.618</td>
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<td></td>
</tr>
<tr>
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<td>(0.546)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female *Lower-class</td>
<td></td>
<td></td>
<td>0.744</td>
<td></td>
</tr>
<tr>
<td></td>
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<td></td>
<td>(0.182)</td>
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</tr>
<tr>
<td>Female *Northeast</td>
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<td></td>
<td>1.355</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.548)</td>
</tr>
<tr>
<td>Female *West</td>
<td></td>
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<td></td>
<td>0.758</td>
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<tr>
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<td>0.133***</td>
<td>0.140***</td>
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<td>1904</td>
<td>1904</td>
<td>1904</td>
</tr>
</tbody>
</table>

Note: All models include all completed cases. Regressions also control for out of home region, application submission, resume type, cover letter type, and employment history type. Odds ratios shown. Cluster-corrected (job advertisement level) standard errors in parenthesis. + = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
Figure 4.1. Employer Responses by College Selectivity

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.2. Employer Responses by College Major

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.3. Employer Responses by Race

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.4. Employer Responses by Social Class

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.5. Employer Responses by Gender

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.6. Employer Responses by Region

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.7. Employer Responses by Race and College Selectivity

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed paired t-test.
Figure 4.8. Employer Responses by Race and College Major

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.9. Employer Responses by Race and Social Class

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.10. Employer Responses by Race and Gender

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.11. Employer Responses by Race and Region

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.12. Employer Responses by Gender and College Selectivity

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.13. Employer Responses by Gender and College Major

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4.14. Predicted Probabilities of Employer Response (Race, Gender, Social Class, and College Selectivity)

Note: Predicted probabilities are based on total employer responses (email or phone) and hold all other variables not listed at their means. The dashed line shows the predicted probability of employer response (13.7%) holding all variables at their means.
Figure 4.15. Predicted Probabilities of Employer Response (Race, Gender, College Selectivity, and College Major)

Note: Predicted probabilities are based on total employer responses (email or phone) and hold all other variables not listed at their means. The dashed line shows the predicted probability of employer response (13.7%) holding all variables at their means.
CHAPTER 5.
SORTED AND SORTED AGAIN: FURTHER EVIDENCE ON EDUCATIONAL CREDENTIALS AND DISCRIMINATION IN THE LABOR MARKET

In the previous chapter, I examined the effects of educational credentials and social background characteristics on employer response rates. But, as this chapter will show, the effects extend to more than just how many responses a candidates receives. Candidates are sorted through a system that restricts their opportunities in multiple ways.

Two additional pieces of information in the job advertisements are the dependent variables of interest in the following sections: the listed salary range and the occupational category of each job.

How Much Do These Jobs Potentially Pay?

When employers post a job advertisement on the website, they include a variety of information to entice potential job candidates. In 289 cases in my completed sample (30.4% of the job advertisements), employers included some information about the salary range. As previously mentioned, I created three variables for listed salary: low, mean, and high values from each job advertisement with means of approximately $31,400, $34,600, and $37,800 respectively (see Table 3.12 and Figure 3.3 for more information). Among those candidates who receive any type of response from an employer, 93 cases come from a job advertisement with a listed salary range (40.4% of the responses). To examine the effects of candidate and application characteristics on the listed salary range for each candidate with a response, I run individual OLS regressions predicting each
salary variable

In Table 5.1, model 1 shows the effects using the low, model 2 for the mean, and model 3 for the high salary variables. Using the low salary variable I find that black candidates receive responses for jobs that have a listed salary $3,865 lower than white candidates (p<0.01). Candidates with a psychology degree receive responses for jobs that have lower posted salaries, although this effect is marginally significant (p<0.10). Additionally, candidates with a degree from an elite college and candidates applying for a job in the Northeast receive responses for jobs that have a marginally significant higher listed salary (p<0.10). In model 2, I find similar results when the outcome is mean salary but the coefficients for a degree from an elite college and Northeast region are now statistically significant at the p<0.05 level and the coefficient for West region is now marginally significant (p<0.10). Using the high salary variable (model 3) I find that black candidates receive responses for jobs that have a listed salary $3,385 lower than white candidates, but this difference is now only marginally significant (p<0.10). Candidates with a degree from an elite college receive responses for jobs that have a listed salary range $3,420 higher than candidates with a degree from a less selective college (p<0.05). Finally, candidates applying for jobs in the Northeast (p<0.05) and West (p<0.10) receive responses for jobs with higher posted salaries compared to those in the Southeast.

The results from Table 5.1 suggest that black candidates face a double penalty of discrimination in the labor market. Not only do they receive lower response rates, but the jobs that are potentially available to them list lower starting salary ranges. Conversely, candidates with a degree from an elite college get a double bonus from their educational
credentials in the labor market in the forms of more responses and higher listed salary ranges.

Another way to check these differences is to include in the regression a variable that controls for whether both applicants in a pair received a response. With this equation, the regression coefficients for race and college selectivity are derived only when there is variation within pairs. Table 5.2 reports these results. Across all three models, the effects for black candidates and candidates with a degree from an elite college are larger and statistically stronger. Black candidates receive responses for jobs that consistently have lower salaries by about $3,800, or a penalty of about 10% from the mean of the pool of candidates. Candidates with a degree from an elite college receive response for jobs that have increasingly higher salaries across the range of listed salaries, or a bonus of about 8-10%.

One final way to run a robustness check on these results is to include dummy variables for the occupational categories of each job. In other words, if we control for potential differences in the types of jobs for which candidates receive responses, is there still evidence of human capital and discrimination effects in the labor market. As Table 5.3 shows, the answer is yes. These coefficients show that for the low and mean salary variables, black candidates still receive responses for jobs with lower salaries. Candidates with a degree from an elite college still receive responses for jobs with higher salaries across all three models. One difference of note here between these models and the earlier models is that the coefficients for a black candidate are reduced in size and somewhat in significance, while the coefficients for a candidate with a degree from an elite college remain largely unchanged. This suggests that the type of job for which black
candidates receive responses accounts for some of the difference in listed salaries. However, candidates with a degree from an elite college appear to receive responses for higher salary jobs regardless of what type of job it is.

**What Types of Jobs Can Candidates Potentially Get?**

The previous section suggests that the inequality of opportunities in the labor market is a layered process. If we consider employer responses to candidates for jobs with higher listed salary ranges a good opportunity, black candidates have worse opportunities and candidates with a degree from an elite college have better opportunities. Thus, educational credentials and discrimination play a large role in multiple types of opportunities in the labor market. One final way we can measure differences in opportunities is by examining the differences in occupational categories of job advertisements for which candidates receive employer responses. Although there are a number of ways to quantify the “best” occupational categories from among those in the sample, I use three criteria: educational credential requirements, listed salary range, and occupational prestige. All of the job advertisements in my sample require a college degree but two occupational categories more consistently list this requirement than others: analyst and managerial. Moreover, those two occupational categories also have higher average listed salary ranges and occupational prestige than the other occupational categories. Although sales also has a higher than average listed salary range the range has significant variation and sales jobs generally have low occupational prestige.\(^{29}\) I deem these two occupational categories “high value” and all other occupational categories “low value”.

\(^ {29}\) From the National Opinion Research Center’s 1989 Occupational Prestige Scores.
educational credentials or discrimination, I run logistic regressions predicting whether an employer response is for a high value occupation or not. This sample only includes candidates who receive any type of employer response. Table 5.4 shows the results from these regressions. In the first model, I find that black candidates are only 56.1% as likely as white candidates to receive a response for a high value occupation vs. a low value occupation (p<0.05). No other variables of note are statistically significant, including the coefficient for a degree from an elite college. Similar to prior analyses, I include a control for both applicants received a response in model 2 and find no significant changes in the effects across models. These results confirm an additional layer of inequality of opportunities for black candidates in the labor market.

The Final Sorting Process

Unfortunately, one significant shortcoming of a computerized audit study is the inability to follow through with the whole employment process. In this case, I do not follow-up with employers after their initial contact. Thus, I cannot see the full picture of how the sorting process would play out in which candidates actually obtain a job offer. What I can see, though, is the opportunity structure for candidates up until the final sort. This chapter presents a clear picture of that process. Educational credentials play a large role, as candidates with a degree from an elite college secure additional opportunities through interviews for jobs that have higher listed salary ranges even after controlling for the types of jobs for which they receive responses. Racial discrimination is also vastly important in the labor market, as black candidates face diminished opportunities beyond their lower response rates in the form of lower potential salaries and lower value jobs. Thus, even if we assume that black candidates and candidates with a degree from a less
selective college simply worked harder and applied to many more jobs than their counterparts, inequality would still pervade in the labor market.
Table 5.1. OLS Regressions Predicting Listed Salary Range of Job Advertisements

<table>
<thead>
<tr>
<th></th>
<th>Low</th>
<th>Mean</th>
<th>High</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black (ref: White)</td>
<td>-3865.10**</td>
<td>-3624.83*</td>
<td>-3384.56+</td>
</tr>
<tr>
<td></td>
<td>(1252.08)</td>
<td>(1542.31)</td>
<td>(1949.17)</td>
</tr>
<tr>
<td>Elite (ref: Less Selective)</td>
<td>2582.33+</td>
<td>3001.37*</td>
<td>3420.42*</td>
</tr>
<tr>
<td></td>
<td>(1333.25)</td>
<td>(1439.17)</td>
<td>(1641.25)</td>
</tr>
<tr>
<td>Female (ref: Male)</td>
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<td>-2457.97</td>
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<tr>
<td></td>
<td>(1755.61)</td>
<td>(1997.43)</td>
<td>(2394.19)</td>
</tr>
<tr>
<td>Lower-class (ref: Upper/Middle)</td>
<td>-210.16</td>
<td>-330.95</td>
<td>-451.74</td>
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<tr>
<td></td>
<td>(1918.83)</td>
<td>(1997.79)</td>
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</tr>
<tr>
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<td>-3475.36+</td>
<td>-3821.97</td>
</tr>
<tr>
<td></td>
<td>(1858.61)</td>
<td>(2070.80)</td>
<td>(2498.79)</td>
</tr>
<tr>
<td>Region – Northeast (ref: Southeast)</td>
<td>3740.20+</td>
<td>4881.76*</td>
<td>6023.32*</td>
</tr>
<tr>
<td></td>
<td>(1956.93)</td>
<td>(2084.30)</td>
<td>(2606.97)</td>
</tr>
<tr>
<td>Region – West</td>
<td>3444.86</td>
<td>4571.99+</td>
<td>5699.11+</td>
</tr>
<tr>
<td></td>
<td>(2392.05)</td>
<td>(2682.52)</td>
<td>(3181.43)</td>
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<td>-517.74</td>
</tr>
<tr>
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<td>(2010.29)</td>
<td>(2110.91)</td>
<td>(2546.22)</td>
</tr>
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<td>761.88</td>
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<tr>
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<td>93</td>
<td>93</td>
</tr>
</tbody>
</table>

Note: Cases with no listed salary range or no employer response are dropped. Regressions also control for resume type, cover letter type, and employment history type. Cluster-corrected (job-level) standard errors in parenthesis.

+ = $p < 0.10$, * = $p < 0.05$, ** = $p < 0.01$, *** = $p < 0.001$
Table 5.2. OLS Regressions Predicting Listed Salary Range of Job Advertisements (Alternate)

<table>
<thead>
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<th>Mean</th>
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<td>-3777.53*</td>
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<td>(1853.44)</td>
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<td>Elite (ref: Less Selective)</td>
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<td>3165.66*</td>
<td>3742.55*</td>
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<tr>
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<td>(1267.80)</td>
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<td>(1485.05)</td>
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<td>-2064.10</td>
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<td>(1728.25)</td>
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</tr>
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<td>(2465.81)</td>
</tr>
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<tr>
<td></td>
<td>(1965.29)</td>
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<td>(2562.06)</td>
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<td>(2360.36)</td>
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<td>(3026.37)</td>
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</table>

Note: Cases with no listed salary range or no employer response are dropped. Regressions also control for resume type, cover letter type, and employment history type. Cluster-corrected (job-level) standard errors in parenthesis.

+ = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
Table 5.3. OLS Regressions Predicting Listed Salary Range of Job Advertisements (Alternate 2)

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<th>Mean</th>
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<td></td>
<td>(1125.15)</td>
<td>(1432.55)</td>
<td>(1887.71)</td>
</tr>
<tr>
<td>Elite (ref: Less Selective)</td>
<td>2601.45*</td>
<td>3240.31*</td>
<td>3879.17*</td>
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<tr>
<td></td>
<td>(1291.51)</td>
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</tr>
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<td></td>
<td>(1935.13)</td>
<td>(2095.53)</td>
<td>(2471.90)</td>
</tr>
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<td>4759.56*</td>
<td>6711.64**</td>
<td>8663.72**</td>
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<td></td>
<td>(2199.89)</td>
<td>(2207.62)</td>
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<td>8472.33*</td>
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<td>(3231.61)</td>
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</tr>
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<td>(1183.06)</td>
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<td>(1482.12)</td>
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</tr>
</tbody>
</table>

Note: Cases with no listed salary range or no employer response are dropped. Regressions also control for occupation type, resume type, cover letter type, and employment history type. Cluster-corrected (job-level) standard errors in parenthesis.

+ = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
Table 5.4. Logistic Regressions Predicting Response of High Value Occupations (Managerial or Analyst vs. All Others)

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<tr>
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<tr>
<td></td>
<td>(0.319)</td>
<td>(0.333)</td>
</tr>
<tr>
<td>Female (ref: Male)</td>
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</tr>
<tr>
<td></td>
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<td>(0.489)</td>
</tr>
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<td>Lower-class (ref: Upper/Middle)</td>
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</tr>
<tr>
<td></td>
<td>(0.458)</td>
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<tr>
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<td>(0.264)</td>
<td>(0.257)</td>
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<tr>
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<tr>
<td></td>
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</tr>
<tr>
<td></td>
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<td>(0.189)</td>
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<tr>
<td></td>
<td>(0.222)</td>
<td>(0.218)</td>
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<tr>
<td>Both applicants received response</td>
<td>1.356</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.566)</td>
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<tr>
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<td>0.386+</td>
</tr>
<tr>
<td>N</td>
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</tr>
</tbody>
</table>

Note: Cases with no employer response are dropped. Regressions also control for resume type, cover letter type, and employment history type. Cluster-corrected (job-level) standard errors in parenthesis. + = p < 0.10, * = p < 0.05, ** = p < 0.01, *** = p < 0.001
Educational Credentials

Although our society subscribes to the notion of education as the great equalizer, we implicitly recognize the important stratifying process of educational credentials through the desire to send our children to the most elite universities. For example, institutions that accept fewer than half of all applicants make up only 18% of the total institutions in the U.S. but receive 31% of all college applications (National Association for College Admission Counseling, 2010). Competition for the coveted spots in these institutions translates into a higher education system stratified by race, ethnicity, and socioeconomic status: blacks, Hispanics, and low-income students are much less likely to attend highly selective institutions than whites, Asians, and high-income students (Alon and Tienda 2007; Bowen and Bok 1998; Carnevale and Rose 2003).

With higher education credentials becoming more common in the labor market, examining labor market outcomes among individuals with a college degree is critical to understanding education's role in reducing or exacerbating inequalities. Yet prior research has failed to adequately address how much the qualitative differences in educational credentials affect success in the labor market, particularly early in an individual’s career when employers have limited information about applicants other than their educational credentials.
Our understanding of economic inequality is limited by the data and measures available to researchers. Although we know that there are demographic differences in the qualitative aspects of educational credentials and these likely have important implications in the labor market, we often do not or are unable to capture these variables in models of economic inequality. This leaves researchers on shaky ground in explaining the reasons for economic inequality.

One of my primary goals in this study was to examine the effects of educational credentials on early stage job market outcomes for recent college graduates to add clarity to the debate on the importance of college selectivity and college major. The results suggest that a degree from an elite college increases the likelihood that an employer will respond to a job application with an offer for an interview and those responses are for jobs with higher listed salaries. I do not find any evidence that having an economics degree increases the likelihood of receiving an employer response over having a psychology degree, but there is some limited evidence that economics degree holders receive responses for jobs with higher listed salaries than psychology degree holders.

It is unclear how much the computerized audit method and using only an online national job search board to apply for jobs affect the results. It is possible that the overall effects of college selectivity estimated here are somewhat conservative. Previous research finds that some benefits of attending a highly selective institution come through the social capital and networks made available from those institutions (Rivera 2011). These effects are likely not captured through an audit study as applicants apply with no prior contact with employers through such networks. Social capital may not only increase any main effects of college selectivity but also potentially exacerbate any racial
differences. Alternatively, if employers using the website do not often see candidates
with a degree from an elite college in their applicant pool, these results may be overstated
compared to the effect of college selectivity across all hiring processes. Limited data are
available which help address this possibility. First, a survey of companies suggests that
25% of new hires come from national job search boards and 89% of all surveyed
companies attribute at least one hire in 2010 to Monster.com (Crispin and Mehler 2011).
Additionally, data from 2006 found that 62% of individuals between 18-28 years old used
the internet for job searches (Brown 2008). Although the likelihood of using the internet
for a job search is positively correlated with education, there is no relationship with race
or gender (ibid). These statistics do not provide definitive answers as to whether or not
the population of jobs and population of job seekers varies between internet job boards
and other means, but they are promising.

Another limitation of this study is that I cannot attribute the effect of educational
credentials to a specific mechanism, whether human or cultural capital. As stated above,
social capital as a mechanism has been effectively ruled out. Employers definitely clue in
on candidates with a degree from an elite college, as evidenced by the quantitative results
and the qualitative email responses. Future research could gain traction on these
mechanisms with more in-depth qualitative analysis (see Rivera 2012b for one such
recent study in the context of elite firms).

A final point is that it is difficult to compare this study with prior work on
educational credentials in the labor market. Although the results seem to refute some of
the most recent and methodologically advanced research using survey data on college
selectivity, prior research has focused on the employment outcomes of older cohorts of
college graduates later in their careers. Both of these time variables may play a role in the differences in findings but we cannot be certain whether differences in qualitative aspects of educational credentials matter more now than in previous years because of quantitative changes in educational credentials, or if qualitative aspects of educational credentials simply matter less later in an individual’s career. Moreover, differences in the outcomes measured could be to blame. We do not have solid information on how job interviews translate to job offers and wages.

Race and Gender

This research finds that educational credentials matter in the job market. We have data that clearly suggest there are racial and gender differences in these educational credentials at the national level, whether the demographics of individuals with a degree from an elite college or those with more high-value college majors. Thus, the findings on educational credentials suggest that at least part of the economic inequality based on race and gender can be attributed to differences in educational credentials.

The other side of this debate about inequality, the side that suggests discrimination still plays a large role, is not wrong either, though. The results suggest that black candidates experience a much lower likelihood that an employer will respond to a job application. Additionally, when black candidates do receive responses, they are for jobs with lower listed salaries and less often for managerial or analyst jobs. Just as employment audit studies have uncovered racial discrimination in the low-wage labor market (Pager 2003; Pager, Western, and Bonikowski 2009), I find significant evidence of racial discrimination in a section of the labor market that demands highly educated employees.
Although I find no significant evidence of gender discrimination there are small differences in a number of outcomes that track in the expected direction. Additionally, I find differences in the level of discrimination between black men and women. Response rates for black women do not match those of white men or women, but they are still significantly greater than those of black men. Moreover, the discrimination that does occur seems to be simultaneously based on both the racial and social class connotations of candidates’ names. These findings have important connotations for future audit studies. Because much of the prior literature has focused on labor market outcomes for black men, the results may overestimate the general levels of discrimination against African-American candidates.

The opportunities that arise upon graduation from an elite college are not equal between whites and blacks. Although there is clearly a premium to a degree from an elite university over a less selective university for both white and black candidates, black candidates still lag behind white candidates in employer responses. Surprisingly, there is no interaction effect between race and college selectivity; the black-white gap in employment outcomes is similar between candidates with a degree from an elite college and candidates with a degree from a less selective college. The results presented here suggest a different picture than the romanticized idea of the U.S. as a post-racial society as well as the notion that education is the great equalizer. On a number of quantitative and qualitative aspects, blacks are at a disadvantage compared to their white peers. Essentially, the effect of race for blacks works similar to the effect of college selectivity for whites. However, while both whites and blacks can alter their educational trajectories and improve their college selectivity, blacks can never shed the penalty of race and catch
This research has important implications for the current debate regarding affirmative action in higher education. Using data prior to statewide bans on affirmative action, researchers have estimated that minority enrollment at highly selective public universities nationwide would drop without affirmative action policies (Bowen and Bok 2000; Espenshade and Chung 2005). Other studies have found that after California, Texas, and Washington implemented bans on affirmative action, state universities systems began to look even more like a racially stratified system with white and Asians at the highly selective flagship universities and blacks and Hispanics at less selective universities (Brown and Hirschman 2006; Card and Krueger 2005; Long 2007). Thus, eliminating affirmative action in higher education would likely guarantee that fewer black students would attend and graduate from highly selective public universities and also lead to increased racial inequality in employment and wages between whites and blacks.

Unfortunately, this study is somewhat circumscribed by time, location, and the chosen set of universities. Unemployment was still somewhat high nationwide during the data collection, potentially giving employers more power and thus exaggerating differences based on college selectivity and race. These results may also not be generalizable to other cities and universities. Finally, the method of this research necessitates stopping at the interview request phase. It is unclear once employers meet a candidate face-to-face how they might respond to the race of a candidate with actual offers of employment, both in terms of hiring and salary, after this phase. Likely, some employers do not pick up on the racial cues of name from an individual’s resume and the levels of discrimination reported here might be underestimated.
This research addresses a number of gaps in our knowledge concerning horizontal stratification and racial inequality and raises a number of important issues. The results suggest that other scholars should be more cautious when measuring any college education as one category of a variable. Although this research only tests employment outcomes at the entry-level stage, college selectivity may be important at other stages of employment and for other important life outcomes. Furthermore, education, even an elite education, does not erase racial inequality at the most preliminary stages of employment. Other research finds that racial inequality in the labor market increases over the career, suggesting that future research should examine whether graduating from an elite university may help to attenuate or exacerbate inequalities over time. This research stands to potentially improve this situation by drawing media and employer attention to the stark racial differences in employment prospects among individuals with the same college degree. Overall, this research contributes to our theoretical and empirical understanding of the possibilities and limits of education in reducing social inequality.
APPENDICIES

CHAPTER 2 APPENDIX.

Although my research does not capture social capital as a mechanism in the effects of educational credentials on labor market outcomes, it is important nonetheless. My research measures the effects of educational credentials, race, and gender on employer response to an application in a “cold call” fashion, separate from the effects of network connections and institutional resources. The scholarly work I discuss in this section finds that social capital has positive effects on labor market outcomes, there are likely differences in social capital among holders of different educational credentials, and that there are race and gender differences in social capital. Thus, I suggest that the magnitudes of the effects in my findings are likely conservative estimates of the full effects of educational credentials, race, and gender in the labor market.

Social Capital as a Mechanism in the Effect of Educational Credentials on Labor Market Outcomes

In general, scholars label social capital as the resources individuals can access through their networks (Lin 1999; Lin, Ensel, and Vaughn 1981; Portes 1998). It's not just the size of an individual's network but the content of their network that matters (see Lin 1999). Additionally, as noted by the homophily principle, individuals are similar to other individuals in their network on the critical dimension of educational attainment (see.
McPherson, Smith-Lovin, and Cook 2001). Thus, as individuals progress through higher levels of educational attainment, it is likely that their networks expand and social capital increases. Some critical research in this area comes from scholars who connect the concentration of alumni from certain elite boarding and preparatory schools to positions of economic and political power (Cookson and Persell 1985; Domhoff 1967; Eckland and Peterson 1969; Persell and Cookson 1990; Useem 1984). Although this research fails to use in-depth analysis of social networks and is far from a causal implication, it is suggestive that the social connections made from educational attainment affect labor market outcomes.

College attendance grants access to a vast array of resources related to employment and the opportunity to expand networks through peer, professor, and alumni connections. Although a number of studies examine the effects of social capital on educational achievement and attainment prior to college enrollment (Carbonaro 1998; Coleman 1988; Dika and Singh 2002; Gaddis 2012) and the effects of social capital on occupational prestige and wages once individuals are in the labor force (Lai, Lin, and Leung 1998; Lin, Ensel, and Vaughn 1981; Marsden and Hurlbert 1988; Mouw 2003), the amount of research that examines the effects of social capital during higher education and the transition to the labor force is extremely limited (more below, but see Grayson 2004; Lee and Brinton 1996; Martin 2009; Mullen 2010).

Institutional endowment funds are correlated with college selectivity (see National Association of College and University Business Officers 2011) and research suggests that per student expenditures on instruction, academic support, student services, and institutional support are higher at more selective universities by a magnitude of 1.5x to
2.0x (Gansemer-Topf and Schuh 2006). Higher levels of spending likely lead to more resources focused towards career assistance and the labor market transition. Institutions provide different levels of access to connections and networking opportunities (social capital) that may help their students obtain jobs (Katchadourian and Boli 1994; Rivera 2011; Useem and Karabel 1990). Elite firms spend more time and money at elite colleges (Cook and Frank 1993; Rivera 2011). Thus, access to social capital varies among different types of colleges and is likely correlated with selectivity. Researchers have overlooked this critical aspect of the effect of college selectivity and studies typically do not differentiate between any of the different ways discussed above that a college degree affects labor market outcomes (Gerber and Cheung 2008; Ishida, Spilerman, and Su 1997).

Finally, although social capital often has been overlooked in the higher education literature, a few studies provide some interesting findings. Lee and Brinton (1996) examined the effect of social capital in South Korean elite institutions and found that graduation from selective institutions led to an increase in the use of networks in obtaining a job as well as a higher likelihood of placement in a large firm. In a study of Canadian institutions, Grayson (2004) found no effect of social capital on job satisfaction, income, or security, although the author did not examine social capital in terms of selectivity or institution type. In the U.S., Martin (2009) found some modest effects of social capital on graduating with honors, graduate school attendance, and occupational aspirations at one elite private university. Finally, one qualitative study found that students at an elite private university recognized and mentioned the importance of institutional networks in securing employment (Mullen 2010). Although
these studies provide some limited insights on the connection between college selectivity, social capital, and labor market outcomes, the literature is sparse and no study examines the effects of social capital among institutions of different selectivity levels in the U.S.

*Race, Gender, and Social Capital*

Race and gender is also important in the labor market due to availability and uses of social capital. As I suggested earlier, social capital is a wealth of resources individuals can access through their networks (Lin 1999; Lin, Ensel, and Vaughn 1981; Portes 1998). Researchers find that a referral from a current employee has a positive effect on the likelihood of obtaining a job (Fernandez, Castilla, and Moore 2000; Petersen, Saporta, and Seidel 2000). However, as Mouw (2003) highlights, a thorough look at existing research leads one to the conclusion that simply using contacts is not enough. Instead, social capital research suggests that the status and resources of contacts in an individual’s network has positive effects on occupational status and wages (see Lin 1999 for a review).

In the labor market, these resources can be information about job openings or relationships with individuals who themselves have personal relationships with key actors (e.g. human resource personnel or managers) in a firm. The level of racial and gender segregation across firms and occupations (see Reskin, McBrier, and Kmec 1999; Leicht 2008 for reviews), then, plays a role in this inequality as well. If firms and occupations are structured in a way that whites and men particularly have occupations of higher status and wages, individual networks, connections, and the information these resources provide have important implications for labor market outcomes. Historical factors, such as labor market participation and discrimination, affect the distribution of these resources and
serve to benefit white males (Braddock and McPartland 1987; Green, Tigges, and Diaz 1999; Kanter 1977; Lin 2000; Marsden and Gorman 2001; Saloner 1985; Stainback 2008). Also, some scholars suggest that non-whites and women not only have fewer network resources but use the resources they do have less often than their white and male peers (Mau and Kopischke 2001; also see Trimble and Kmec 2011 for a review).

Much of the research focuses on the differences in individual contact networks between race and gender but directs little attention to more micro-level processes such as information flow and use (Elliott 2000; Elliott and Sims 2001; Kmec 2007; Petersen, Saporta, and Seidel 2000; Smith 2000). Scholars demonstrate that comparable amounts of social capital can still lead to differences in outcomes due to the nature of those networks (McDonald, Lin, and Ao 2009; Mouw 2002; Royster 2003). Additionally, the racial composition of a firm can impact the likelihood of hire and wages when referrals and contacts are used (Mouw 2002; Kmec and Trimble 2009). Thus, there are a number of factors that may influence race and gendered differential access to and uses of social capital, which in turn affect labor market outcomes.

CHAPTER 3 APPENDIX.

Social Class and Names: Consequences and Concerns

Since at least the 1970s the U.S. has developed a distinctive racialized pattern between black and white parents’ naming practices (Fryer and Levitt 2004; Lieberson 2000; Lieberson and Mikelson 1995). This pattern stems from black parents choosing unique and nearly unique names for their children at rates much higher than whites, while simultaneously often not using many popular white names (Fryer and Levitt 2004). An
important factor in determining uniqueness in choice of a child’s name is SES. Fryer and Levitt (2004) found that “[b]lacker names are associated with lower-income zip codes [and] lower levels of parental education” (p 786). Moreover, although there are fewer instances of unique naming among white parents, there are SES patterns among whites. Thus, there are both race and class components to parental naming practices (also see Aura and Hess 2010).

Researchers find that there are consequences to having a race- or class-based distinctive name in school. Even after controlling for important background factors, students with low-SES names have lower test scores, are less likely to be labeled as gifted, and more likely to be retained, perhaps due to teacher expectations (Figlio 2005).

A recent psychology study uses an experiment to examine whether teachers respond to student essays in different ways based on the name on a paper (Harber et al. 2012). Although each fabricated student essay was designed to be a C paper, teachers gave more critical feedback to essays written by students with white sounding names than black or Hispanic sounding names. This may serve to harm the self-esteem of minority students — if a child realizes that a teacher is pandering to them — and almost certainly reduces their ability to learn from writing exercises through the lack of critical feedback. Beyond school, race- and class-based names also have negative consequences on the labor market (Bertrand and Mullainathan 2004; Cotton, O’Neill, and Griffin 2008; Watson, Appiah, and Thornton 2011) and in access to political power (Butler and Broockman 2011). Moreover, recent media reports suggest that some job-seekers alter the name on their resume in an attempt to secure more callbacks (Luo 2009a, 2009b).
As discussed earlier in Chapter 3, I selected various distinctive race- and class-based names from New York State birth records (also see Table 3.9). Due to my ability to control for the social class portion of a name separately and the potential that the social class indicator of a name has its own separate effects, I pose the following secondary research questions:

(1) Does having a lower class name, rather than an upper or middle class name, have negative effects on (a) the likelihood of receiving an employer response, (b) the salary range of jobs, and (c) the type of job?

(2) Does having both a black name and a lower class name alter any effects from above?

CHAPTER 4 APPENDIX.

The Combination of Gender and Other Characteristics

When I examine gender differences within social class I find no significant differences (see Figure 4A.1). Class differences within gender are statistically significant for both male and female candidates (p<0.05). Male candidates with an upper/middle class name receive the highest response rate (13.8%), followed by female candidates with an upper/middle class name (13.6%), male candidates with a lower class name (9.4%), and then female candidates with a lower class name (8.4%). In a logistic regression model that includes an interaction effect of female candidate and lower-class name, no gender or class variables are statistically significant (see Table 4.4 model 3).
The results for gender differences within region show an interesting, but not statistically significant pattern (see Figure 4A.2). Male and female candidates receive similar response rates in the Southeast (10.9% vs. 10.1% respectively; p-value=0.74), but female candidates receive higher response rates than male candidates in the Northeast (16.0% vs. 13.4%; p-value=0.34) and male candidates receive higher response rates in the West (12.7% vs. 9.4%; p-value=0.19). The regional differences within gender have one significant results: female candidates in the Northeast are more likely to receive any type of employer response than female candidates in either of the other two regions (p<0.05).

In a logistic regression model that includes an interaction effect of female candidate and region, no gender or region variables are statistically significant (see Table 4.4 model 4).

**The Combination of College Selectivity and Other Characteristics**

The difference between a degree from an elite college and a degree from a less selective college, shown in Figure 4A.3, is statistically significant for both candidates with economics degrees and candidates with psychology degrees (p<0.01). However college major differences within college selectivity are not statistically significant. A candidate with a degree from an elite college has similar response rates whether that degree is in economics (16.1%) or psychology (14.4%). Likewise, a candidate with a degree from a less selective college has similar response rates whether that degree is in economics (9.6%) or psychology (8.2%).

An examination of regional differences in the effect of college selectivity sheds some light on local labor market conditions and the relative value of educational credentials within a region. Figure 4A.4 shows that the response rate ratios of a degree from an elite college vs. a degree from a less selective college are relatively similar
across all three regions (1.7 to 1.8) and all differences across region are not statistically significant.

**A Methodological Note on Three-Way Interactions**

Although an examination of three-way interactions potentially would be useful to see further combinations of the effects of educational credentials and social background characteristics, the sample size simply does not support this type of analysis. In most cases, response rates in individual cells are too small to produce reliable estimates. The original power analysis conducted after the second pilot data collection round was done so with two-way interactions in mind and because three variables were not used in the pilot data collection, it was impossible to predict what type of sample size would be necessary.
Figure 4A.1. Employer Responses by Gender and Social Class

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4A.2. Employer Responses by Gender and Region

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4A.3. Employer Responses by College Selectivity and College Major

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
Figure 4A.4. Employer Responses by College Selectivity and Region

Note: The outer lines represent the 95% confidence interval and the inner lines represent the 90% confidence interval, both using a two-tailed Welch’s t-test.
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