Title
Ascription at Work: Essays on Discrimination, Networks, and Employment Histories

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Publication Date
2017

Peer reviewed|Thesis/dissertation

Ascription at Work: Essays on Discrimination, Networks, and Employment Histories

By

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A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Sociology

in the

Graduate Division

of the

University of California, Berkeley

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Fall 2017

Abstract

Ascription at Work: Essays on Discrimination, Networks, and Employment Histories

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This dissertation consists of three essays examining ascriptive inequality in the labor market. In the first two essays, I draw on an original, two-wave experiment with a sample of white hiring agents to advance the sociological understanding of the determinants of employers' discriminatory behavior and the consequences of jobseekers' racially-segregated networks. First, I examine what motivates employers to discriminate. I find implicit (largely unconscious), but not explicit, racial attitudes predict employers' evaluations of white applicants, and of black applicants relative to white applicants. Thus, instead of deliberately rejecting black jobseekers, hiring agents' behavior appears to be driven by largely unconscious biases. Further, in open-ended responses, hiring agents justify their racially-motivated evaluations without invoking race, suggesting the ambiguity of the hiring process enables them to maintain and portray an egalitarian image. Second, I analyze how white hiring agents reward employee referrals. I find that in the most prevalent real-life conditions—black applicants referred by black employees, and white applicants referred by white employees—black applicants' referrals were significantly discounted relative to white applicants' referrals. Indeed, black applicants only benefited from having a referral when two conditions were met: the referring employee was white and the hiring agent was relatively low-prejudiced. Thus, in addition to their disadvantage in access to employee referrals, black jobseekers suffer from a disadvantage in returns to these referrals. In the third essay, I use the rich week-by-week measures of work experiences from the National Longitudinal Survey of Youth 1979 (NLSY79) to examine the role of labor market experiences—specifically, employment histories—in explaining the intergenerational transmission of economic status. I document a strong association between parental income and employment histories for men without a college degree. Among this group, men from higher-income families accumulate more work experience and tenure, and less unemployment, throughout their careers than men from lower-income families. In contrast, regardless of parental income, college graduates quickly settle into stable, long-term employment. Thus, a college degree appears to be a powerful resource that leaves little room for family background effects on employment histories. Consequently, for non-college graduates (but not for college graduates), employment histories mediate approximately one-third of the effect of parental income on earnings. Ultimately, these essays highlight the enduring effect of race and family background in the labor market.

To Andrei and my parents

ACKNOWLEDGEMENTS

I am tremendously grateful for the support, guidance, and feedback provided by my dissertation committee. Heather Haveman, my dissertation chair, has encouraged my work since we first met and always reminded me that I had something to contribute. She is also a perceptive and very careful reader, and has been a great advocate on my behalf. I also always appreciated having such a strong proponent of clear and logical writing as my chair. Kim Voss has advised me since my first days at Berkeley. In addition to being a wonderful co-author and engaged mentor, she has been caring and supportive during difficult moments in graduate school. Kim, thank you for being in my corner. Sameer Srivastava provided generous, incisive comments, and pushed me to be a more ambitious researcher. Outside of my committee, I am also very fortunate to have had Irene Bloemraad as a teacher, mentor, and co-author. Irene, thank you for encouraging me to write for a broader audience, for your kindness, for making it clear that it is okay to have a life outside of academia, and for asking direct questions even when I wanted to avoid thinking about my career or various graduate school milestones.

Additionally, I would like to thank Zawadi Rucks-Ahidiana, Dana Carney, Claude Fischer, and participants of UC Berkeley's Mathematical, Analytical, and Experimental Sociology (MAXSoc) and Race, Ethnicity, and Inequality working groups for excellent comments on components of this dissertation. I am also grateful financial support from the National Science Foundation Graduate Research Fellowship, the Dean's Normative Time Fellowship, and the Department of Sociology.

Thank you to my friends for cheering me on, putting up with me, and making life much more fun during my graduate school years. I especially want to thank Rebecca Dizon-Ross, Sophia Taula-Lieras, Lucas Weisendenger, Jessica De La Cruz, and Andrea Gadberry. Thanks also to Elena Boutylina and Grigori Nepomniachtchi for your encouragement and love, for welcoming me into your family, and for keeping me very well-fed. At UC Berkeley, I am especially lucky to have joined the wonderful community of MAXSoc. Thank you to Sanaz Mobasseri, Katherine Hood, Liana Prescott, Joe LaBriola, Pat Hastings, Matt Stimpson, Casey Homan, Lindsay Bayham, and William Welsh, for the camaraderie.

Finally, I would like to thank my parents, my brothers Mario and Alejandro, and my partner Andrei Boutyline for their love, support, encouragement, and patience. My parents provided an exceptional model of intellectual curiosity, hard work, and commitment to the public good. They fostered an incredibly stimulating household environment, and made painful sacrifices to provide their three children with the best opportunities possible. For this, and for their unconditional love, I am forever grateful. To Andrei, thank you for believing in me even when I felt certain I had given you no evidence for doing so; your leap of faith, early in graduate school, bolstered my confidence and courage at a crucial time. This dissertation benefited immensely from our conversations, from your very detailed feedback, and from your encouragement to make my writing bolder. I am so happy we shared this experience.

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CHAPTER 1
Introduction

This dissertation examines ascriptive inequality in the labor market. The three empirical essays seek to advance the sociological understanding of the labor market processes—specifically, employer discrimination and the differential accumulation of work experiences—that generate ascriptive inequality. While considerable research focuses on the effect of ascriptive characteristics on the skills and education individuals bring into the labor market, much inequality is produced within the labor market.

In chapter 2, I examine what *motivates* employers to discriminate. Despite the strong evidence that hiring discrimination against black jobseekers remains prevalent in the United States, we know relatively little about the causes of employers' discriminatory behavior. I draw on an original two-wave study with a sample of white hiring agents to examine whether respondents' explicit (conscious) and implicit (largely unconscious) racial attitudes predict their evaluations of white and black job applicants at a later date. Do hiring agents deliberately reject black jobseekers, perhaps due to anti-black affect or negative expectations about blacks' workplace productivity? Or do hiring decisions reflect largely unconscious "gut" instincts? Building on dual-process models of the attitude-behavior relationship (e.g., Fazio 1990), I theorize that the hiring process at many U.S. organizations—characterized by ambiguity, time pressure, and the legitimacy of emotions as a decision-making tool—encourages decision-making based on implicit rather than explicit cognition. These factors are expected to limit employers' awareness of their racial biases, restrict their ability to control implicit processes, and legitimize the use of implicit cognition in hiring. Consequently, they should reduce employers' motivation and opportunity to control their implicit biases.

Consistent with these expectations, I find implicit racial attitudes predict hiring agents' evaluations of black applicants relative to white applicants. In contrast, I find no significant effect of explicit racial attitudes—whether measured as affect or stereotypes—on hiring agents' relative evaluations of black and white applicants. Indeed, hiring agents who describe blacks as less competent, lazier, and more hostile than whites, or who report feeling more warmly to whites than blacks, do not evaluate black and white jobseekers significantly differently than hiring agents who espouse egalitarian views. Thus, instead of deliberately rejecting black jobseekers, hiring agents' behavior appears to be driven by largely unconscious biases.

The results also point to pro-white bias as an important determinant of discriminatory decision-making. Sociologists overwhelmingly conceptualize hiring discrimination as reflecting anti-black sentiment, and emphasize employers' negative perceptions of blacks (e.g. Bobo et al. 2002; Neckerman and Kirchenman 1991; Waldinger and Lichter 2003). Instead, I find white hiring agents' implicit racial bias predicts how favorably they evaluate *white* jobseekers, not only how negatively they evaluate black jobseekers *relative to* white jobseekers. Specifically, an increase in white hiring agents' implicit anti-black (or pro-white) bias is associated with more positive evaluations of white applicants. This suggests a central role for pro-white sentiment in theorizing discrimination. Indeed, disparate treatment can just as easily result from employers' positive intuitive reactions to white jobseekers (e.g. "I like him!"), as from their negative "gut" reactions to black jobseekers.

In addition to improving our understanding of the determinants of white hiring agents' racially-motivated behavior, this chapter provides insight into white hiring agents' interpretation of this behavior. This is important as implicit biases can influence hiring agents' behavior without their awareness, yet awareness of bias is a key determinant of individuals' motivation to control their intuitive responses (Devine 1989). In open-ended responses, I find that given the ambiguity of the hiring process, hiring agents can use varied strategies to interpret their racially-motivated decisions without invoking race. For instance, hiring agents can determine whether an experience is (or is not) relevant to the position, can interpret an experience favorably or negatively, and can decide whether it is preferable to select applicants based on relevant experience or on "gut" intuitions. Given strong egalitarian norms, hiring agents are likely motivated to interpret their behavior as non-discriminatory (Srivastava and Banaji 2011); the ambiguity of the selection process appears to provide them with the tools to do so. Together, the findings in chapter 2 demonstrate how employers can maintain and portray an egalitarian self-image while perpetuating racial disparities in employment.

In chapter 3, I integrate a focus on employer discrimination into the study of network-driven labor market inequality. Network scholars argue that, given segregated networks and black and white employees' unequal position in the labor market, employers' reliance on employee referrals reproduces black disadvantage (e.g., Lin 2001; Trimble and Kmec 2011). The focus of this research is on black jobseekers' disadvantage in accessing and mobilizing social resources: black jobseekers have access to lower-status, less-influential ties than white male jobseekers and their contacts are less likely to speak to employers on their behalf (McDonald 2011; Smith 2000). Yet, the strong evidence of employer discrimination suggests that even if white and black jobseekers accessed and mobilized equivalent social resources, employers may not equivalently reward their resources. Thus, it is important to distinguish between the resources applicants access and mobilize, and the returns to those resources. In this chapter, I focus on racial variation in the returns to having a recommendation from a current employee. By "return," I mean the difference in how employers respond to applicants with and without employee referrals, all else equal. Specifically, I examine whether employers' implicit anti-black prejudice affects how they reward black and white applicants' referrals, from black and white employees.

While, as seen in chapter 2, hiring agents' implicit anti-black prejudice is expected to decrease their likelihood of hiring non-referred black applicants relative to non-referred white applicants, whether it should increase or decrease black applicants' payoff to referrals remained unclear. In chapter 3, I draw on the social cognition literature on implicit prejudice and stereotypes, and on research on referral hiring, to develop competing predictions (e.g., Fernandez, Castilla, and Moore 2000; Hamilton, Sherman, and Ruvolo 1990; Fiske 1998). On the one hand, employee referrals could benefit black applicants by reducing employers' reliance on negative stereotypes. If an employer believes that a referral is a credible signal of applicant quality, she may give a referred black applicant a chance even if she is generally doubtful about blacks' work ethic. On the other hand, black applicants may be disadvantaged if prejudiced employers do not perceive their referrals as credible, or do not trust recommendations from black employees.

To test these predictions, I again drew on a two-wave study with a sample of white hiring agents. In the first wave, I measured hiring agents' implicit racial attitudes. In the second wave, the same hiring agents evaluated pairs of equally-qualified same-race job applicants. One applicant in each pair had a referral from either a black or white employee; the other had no referral. I found that in the most common real-life scenarios—black applicants referred by black employees, and white applicants referred by white employees—black applicants' referrals were significantly discounted relative to white applicants' referrals. In fact, black applicants *only* benefited from having a referral when two conditions were met: (1) the referring employee was white, and (2) they were evaluated by a less-prejudiced hiring agent. In contrast, white applicants overwhelmingly benefited from their same-race referrals, and benefited from black employees' recommendations as long as they were evaluated by relatively prejudiced hiring agents. These findings suggest that in addition to their disadvantage in accessing and mobilizing social resources, black jobseekers suffer from a disadvantage in returns to these resources. Moreover, given black and white jobseekers' overwhelming tendency to rely on same-race job contacts (Mouw 2002), this chapter identifies employers' differential response to black and white jobseekers' same-race referrals as a contributor to racial inequality in the labor market.

In the final empirical chapter, I turn to a different dimension of ascriptive inequality in the labor market: earnings disparities based on parental income. Parents pass on a substantial amount of their economic advantage to their children. For instance, approximately half of parental income differences are passed on to children (Mazumder 2005; Mitnik, Bryant, Weber, and Grusky 2015). While sociologists have long been interested in explaining how parents pass on their economic advantage (e.g., Blau and Duncan 1967; Sewell, Haller, and Portes 1969), they have overwhelmingly focused on the effect of family background on children's pre-labor market outcomes. In particular, an extensive literature documents the robust effect of family background on children's educational success (e.g., Gamoran 2001; Shavit and Blossfeld 1993). Yet, key pre-labor market factors—including educational attainment, school quality, and cognitive achievement—appear to account for at *most* half of the intergenerational transmission of income and earnings (Bowles, Gintis, and Osborne 2005: 4, 18).

Instead, drawing on rich week-by-week measures of work experiences from the National Longitudinal Survey of Youth 1979 (NLSY79), I examine the role of labor market experiences—specifically, employment histories—in explaining the intergenerational transmission of economic status. Employment histories are important determinants of earnings (e.g., Fuller 2008; Gangl 2006), as they affect the accumulation of on-the-job human capital and provide signals to prospective employers about workers' competence, motivation, and commitment. Indeed, while workers usually earn relatively low wages at the start of their career, earnings disparities increase as differences in work experience, job tenure, and unemployment spells accumulate (e.g., Cheng 2014; Tomaskovic-Devey, Thomas, and Johnson 2005). There are compelling reasons—including employer discrimination and differences in economic, social, and cultural resources—to expect individuals from more-advantaged backgrounds to attain more work experience and tenure, and less unemployment, over the course of their careers than their less-advantaged peers (e.g., Armstrong and Hamilton 2013; Lin 1981; Rivera and Tilcsik 2016).

I document a strong association between parental income and employment histories for men without a college degree. Among this group, men from higher-income families accumulate more work experience and tenure, and less unemployment, throughout their careers than men from lower-income families. Further, higher parental income is associated with a faster transition to stable employment for men with at most a high-school education, reducing the "churning" that characterizes the early labor market years of less-educated men (Klerman and Karoly 1994). Consequently, for non-college graduates, employment histories mediate approximately one-third of the effect of parental income on earnings after conditioning on pre-labor market factors.In contrast, regardless of parental income, college graduates quickly settle into stable, long-term employment. For the purposes of attaining stable employment, a college degree appears to be a powerful resource that leaves little room for family background effects. Overall, the three empirical chapters highlights the utility of examining how family background and race continue to affect individuals long after they enter labor force.

CHAPTER 2
Why do employers discriminate?
The role of implicit and explicit racial attitudes

Hiring discrimination against black jobseekers remains prevalent in the United States. Indeed, a meta-analysis of experimental field studies finds white applicants are 44% more likely to receive a callback or job offer than equally-qualified black applicants (Quillian et al. 2016). Employer racial discrimination persists across cities, occupations, and firms, and affects a wide range of applicants: male and female; with and without college degrees; with degrees from more or less selective colleges; with and without criminal convictions; from high poverty and low poverty neighborhoods; and those who apply in-person and online (Bertrand and Mullainathan 2004: Gaddis 2015; Kang et al. 2016; Pager 2003; Pager, Western, and Bonikowski 2009; Uggen et al. 2014).

Yet, despite widespread evidence of discrimination, we know little about the *causes* of employers' discriminatory behavior. Field experiments are informative about what employers do, but offer few insights about why employers do it. In this chapter, I ask a simple question: what motivates employers to discriminate? Internal motivations can be difficult to measure (Reskin 2003), but are essential to understanding the causes and potential solutions to discrimination. Specifically, I examine whether white hiring agents' explicit (conscious) or implicit (largely unconscious) racial attitudes predict their evaluations of black and white jobseekers. Do hiring agents deliberately reject black jobseekers, perhaps due to anti-black affect or concerns about black applicants' expected productivity? Or do hiring decisions reflect largely unconscious biases rather than deliberate avoidance?

To understand employers' motives, sociologists have conducted interviews and surveys (e.g., Bobo, Johnson, and Suh 2002; Kirschenman and Neckerman 1991; Moss and Tilly 2001). Employers express anti-black affect, as well as negative views of blacks' work ethic, attitude, and skills. As a retail store employer in New York City bluntly noted, "I will tell you the truth. African Americans don't want to work" (Pager and Karafin 2009: 78). The implication of these studies is that employers purposefully avoid hiring black applicants because they are concerned about their expected work performance, or because they simply dislike blacks. Indeed, it seems straightforward that an employer that states that African Americans "don't want to work" would be reticent to hire black jobseekers.

Yet, there are several reasons to refrain from concluding that employers' expressed anti-black attitudes affect their hiring behavior. First, a long line of research in sociology and psychology cautions against inferring that what people *say* necessarily predicts what they *do* (e.g. LaPiere 1934; Duckitt 1992; Jerolmack and Khan 2014). Second, evidence linking employers' racial attitudes to their behavior is scant, since individual studies typically examine employers' behavior or their attitudes, but not both.[1] Finally, psychological research suggests that implicit racial biases are prevalent. Such implicit biases often operate by shaping one's "gut" responses to others (Ranganath, Smith, and Nosek 2008; Vaisey 2009). These implicit biases, rather than explicit stereotypes or anti-black affect, could be driving employers' discriminatory behavior. Indeed, some scholars of culture and action argue implicit processes play a primary role in motivating action (Miles 2015; Vaisey 2009).

[1] In the "Previous Research" section, below, I discuss the limitations of the few existing studies that test the association between racial attitudes and jobseeker evaluations.

Building on dual-process models of the attitude-behavior relationship (e.g. Fazio 1990), I argue that the hiring process at many U.S. organizations—characterized by ambiguity, time pressure, and the legitimacy of emotions as a decision-making tool—encourages decision-making based on implicit rather than explicit cognition. These factors reduce employers' awareness of their racial biases, restrict their ability to exert the necessary effort to employ explicit cognition, and legitimize the use of implicit cognition in hiring. Indeed, if employers believe their "gut" feelings are an effective and legitimate decision-making tool, why should they engage in more effortful and time-consuming explicit reasoning? Consequently, I posit that employers' implicit anti-black bias should be more predictive of their hiring decisions than their explicit racial attitudes.

To test this proposition, I draw on an original two-wave study with a sample of white individuals with hiring responsibilities (i.e. hiring agents). In the first wave, I collected information on respondents' implicit and explicit racial attitudes. In the second wave, hiring agents evaluated white and black job applicants. The two-wave approach allows me to assess whether hiring agents' implicit and explicit attitudes predict their evaluations of black and white jobseekers at a later time. This is an important advance over the great majority of implicit attitude/behavior studies which measure attitudes and behaviors during the same session (see Fazio and Olson 2003; Greenwald et al. 2009), potentially biasing the estimated attitude-behavior associations.

Since individuals may be unaware of, and thus unable to report, their implicit attitudes, I use an indirect measurement tool from cognitive psychology—the Implicit Association Test—to measure implicit bias. To assess explicit racial attitudes, I developed four explicit racial attitude scales: anti-black affect, and stereotypes of blacks' work ethic, competence, and hostility. Even in the context of egalitarian norms limiting respondents' willingness to explicitly endorse anti-black views, the rich set of survey questions provided substantial variation on the explicit racial attitude scales. This is essential as I could not properly study the effect of racial attitudes on hiring agents' evaluations without sufficient variation on these attitudes.

To anticipate, I find that an increase in hiring agents' implicit anti-black bias is associated with more positive evaluations of white applicants, and more negative evaluations of black applicants relative to white applicants. Thus, implicit attitudes are not only associated with discrimination *against* black applicants relative to white applicants, but also with bias in *favor* of white applicants. In contrast, I find no significant effect of explicit racial attitudes—whether measured as affect or stereotypes—on hiring agents' evaluations of black and white applicants. This suggests hiring decisions reflect largely unconscious biases rather than deliberate avoidance. Further, in open-ended responses, hiring agents justify their racially-motivated evaluations in non-racial terms; the ambiguity of the selection process appears to enable them to interpret their decisions without invoking race. Together, these findings illustrate how employers can maintain and portray a color-blind self-image (Bonilla-Silva 2010) while perpetuating racial disparities in employment.

Dual-process models and employers' racial attitudes

Dual-process models posit that human cognition involves two basic processes—one slow, deliberate, effortful, and largely conscious, and one fast, automatic, effortless, and largely unconscious (Evans 2008). Explicit attitudes, typically measured through surveys and interviews, reflect the former process. They represent a more deliberate, well-considered assessment of the target object. For instance, an employer may reflect on whether she thinks white employees have a better work ethic than black employees. In contrast, implicit attitudes—habitual cultural associations which can be activated without effort, intention, or awareness—reflect the latter process (Dovidio and Gaertner 2010). Thus, an employer may have a negative "gut" feeling about a black applicant or doubts about his work ethic, without realizing these responses are racially-motivated. Below, I detail what we know about employers' explicit and implicit racial attitudes, and how these attitudes may affect hiring behavior.

Employers' explicit racial attitudes

Traditionally, sociologists emphasized the role of explicit cognition in shaping employer behavior. In-depth interviews indicate many employers articulate negative views of black workers (Kirschenman and Neckerman 1991; Moss and Tilly 2001; Pager and Karafin 2009; Shih 2002; Waldinger and Lichter 2003; Wilson 1996). Most frequently, employers criticize blacks' work ethic, competence, and hostility. For instance, the chairman of a car transport company in Chicago described blacks as "the laziest of the bunch," (Wilson 1996: 112), an Atlanta grocery store manager argued back applicants have "very limited skills and very limited training. They really don't have anything to offer," (Moss and Tilly 2001: 100), and a New York City retail employer noted that blacks seem "more intimidating" than whites (Pager and Karafin 2009: 82). More infrequently, employers express anti-black affect. For example, a white male manufacturer in Los Angeles shared that "I have a difficult time dealing with the black man…And that probably is a part of my own social upbringing, because my mom was very much a racist person." (Waldinger and Lichter 2003 : 170). In close-ended surveys, employers express similarly negative views: they rate blacks as having weaker English skills, greater welfare dependency, lower intelligence, and being harder to get along with than whites (Bobo et al. 2002). Overall, these studies suggest employers deliberately avoid hiring black applicants, either because they have concerns about black applicants' expected workplace productivity or because of their explicit anti-black affect. Indeed, Kirschenman and Neckerman (1991: 204) conclude that "race is an important factor in hiring decisions."

Employers' implicit racial attitudes

More recently, sociologists have argued implicit processes are likely drivers of employers' discriminatory behavior (e.g. Quillian et al. 2016; Reskin 2003; Stainback, Tomaskovic-Devey, and Skaggs 2010). Given the salience of race in the U.S., Americans are likely to spontaneously categorize others based on race (Brewer 1988). In turn, these racial categories automatically activate associated stereotypes and emotional responses (Dovidio and Gaertner 2010). Further, once activated, stereotypes can bias individuals' interpretation of ambiguous evidence (Hamilton et al. 1990). For example, whites with greater implicit anti-black bias, who are more likely to activate anti-black stereotypes, were quicker to perceive anger in ambiguously hostile black faces than less-biased whites (Hugenberg and Bodenhausen 2003).

Thus, instead of intentionally rejecting black jobseekers, employers' hiring decisions may reflect their largely unconscious biases. For instance, an employer may believe her "gut" instincts are a useful way of assessing whether an applicant is trustworthy, without recognizing her gut is racially-biased. Similarly, an employer who rejects a black applicant with a history of unemployment may believe unemployment is a racially-neutral signal of poor work ethic. Yet, the same employer may have offered a more charitable interpretation of a white applicant's unemployment spell, perhaps reasoning the applicant simply had back luck. Nevertheless, while sociologists have begun to incorporate implicit attitudes into their theorizing of employers' behavior, they largely have not included measures of these attitudes in empirical studies (for an exception, in the context of Moroccan discrimination in the Netherlands, see Blommaert, Tubergen, and Coenders 2012).

Dual-process models: *Which* type of attitudes predict behavior *when*?

Which of these attitudes—explicit or implicit—should we expect to guide employers' behavior? That is, do hiring decisions reflect deliberate avoidance or largely unconscious biases? I build on the MODE dual-process model of the attitude-behavior relationship (Fazio 1990) to argue that the American hiring process encourages decision-making based on implicit rather than explicit cognition.[2] MODE is an acronym for *m*otivation and *o*pportunity as *de*terminants of whether the attitude-to-behavior process is primarily implicit or explicit. Explicit, deliberate reasoning requires *effort*: thus, the MODE model posits that individuals need motivation and opportunity to engage in such reasoning. When people have the opportunity and motivation to deliberate, explicit attitudes are the main driver of behavior. In the absence of either condition, implicit attitudes are the main driver.

What determines individuals' motivation and opportunity to deliberate? Frequently, individuals are motivated to deliberate because they are interested in making high-quality decisions (Fazio and Olson 2014). If an individual believes deliberation leads to better decisions, and is invested in the outcome of a decision, then she is likely motivated to deliberate. Further, in the domain of racial bias, *awareness of bias* is a key determinant of motivation to deliberate (Devine 1989). Given widespread egalitarian norms, many whites have developed conscious, non-prejudiced self-images, while retaining unconscious anti-black attitudes (Dovidio and Gaertner 2010). They do not want to discriminate, or at least do not want to think of themselves as discriminatory. Thus, they will be motivated to control their implicit reactions when it is obvious that expressing such reactions reflects racial bias (Dovidio and Gaertner 2004).

Opportunity refers to the ability to control implicit processes and engage in deliberate reasoning. Certain behaviors, such as spontaneous eye blinking, are difficult to control by their very nature (Dovidio et al. 1997). Further, even if a behavior is controllable, people need time and cognitive resources to deliberate; thus, if an individual is time-constrained or under heavy cognitive load, she may be unable to deliberate (Fazio and Olson 2014). Thus, aspects of the behavior (i.e. controllable versus spontaneous) and of the decision-making context (i.e. time, cognitive load) determine opportunity to deliberate.

The focus on opportunity to deliberate has led many psychologists to emphasize implicit attitudes' ability to predict spontaneous behaviors, such as nonverbal reactions, which are difficult to control (e.g., Dovidio et al. 1997; Dovidio, Kawakami, and Gaertner 2002; McConnell and Leibold 2001). In contrast, sociologists have emphasized the importance of

[2] For a recent review of the MODE model, including empirical support for its propositions, please see Fazio and Olson (2014).

implicit processes in predicting controllable behavior, such as skipping class or contributing monetary resources to others (e.g. Miles 2015; Stepanikova, Triplett, and Simpson 2011; Vaisey 2009). Yet, perhaps because of their focus on highlighting implicit processes—generally the domain of psychologists—to a sociological audience, sociologists have spent less time theorizing *when* we might expect explicit and implicit processes to matter for controllable action. Below, I argue that common aspects of the American hiring process encourage the use of implicit processes in hiring, even though hiring decisions are controllable.

The Hiring Process: Gut instincts, ambiguity, and time pressure

I argue three aspects of the American hiring process encourage decision-making based on implicit rather than explicit processes: the legitimacy of gut instincts in hiring, ambiguity about how to assess applicants' qualities, and time pressure. These factors legitimize the use of implicit cognition in hiring, reduce employers' awareness of their racial biases, and restrict employers' ability to exert the necessary effort to employ explicit cognition. Together, they reduce employers' motivation and opportunity to deliberate.

Intuitive hiring

First, employers report making hiring decisions based on their *gut instincts* (Godart and Mears 2009; Lageson, Vuolo, and Uggen 2015; Miller and Rosenbaum 1997; Neckerman and Kirchenman 1991; Shih 2002). For instance, employers of low-wage workers in Los Angeles noted they frequently relied on their intuitions to select new hires: "'I go by gut feeling,' 'So much of it is just a gut feel,' 'Basically, it comes down to an extra sense.'" (Waldinger and Lichter 2003: 136). Further, relying on gut instincts is frequently seen as a legitimate and effective approach to making personnel decisions. For instance, Rivera (2015a: 1375-6) finds evaluators in elite professional services firms frequently ask each other to report on their gut feelings about job candidates (e.g. "What was your *feel*?"), and argue for or against candidates based explicitly on their gut instincts (e.g., "I just wasn't feeling it."). Indeed, hiring agents even argue that relying on their intuitions is *better* than relying on "paper" qualifications alone. For example, an investment banker noted that, "I think I can pick out great people…You shouldn't shun someone based on what's on paper. There's [*sic*] plenty of people I've interviewed [that]…don't have the [right work] experience, but I've just gotten good gut feelings about them." (Rivera 2015a: 1353). Since relying on instincts is a legitimate approach to decision-making—respected and even encouraged by other hiring agents—hiring agents have limited *motivation* to employ explicit cognition. Indeed, if employers believe their intuitions are an effective and legitimate approach to decision-making, why should they engage in effortful and time-consuming deliberate reasoning?

Ambiguity

Second, hiring decisions are characterized by *ambiguity* (Bishop 1993; Kirschenman and Neckerman 1991; Miller and Rosenbaum 1997; Waldinger and Lichter 2003). Employers rarely follow strict guidelines regarding how to assess or weigh applicants' attributes (Dipboye, Macan, and Shahani-Denning 2012; Lageson et al. 2015). Further, while employers may know which qualities they seek in workers, they are typically less certain about which previous experiences or observable skills best predict these qualities. Indeed, in a large-scale survey of small and medium sized firms, hiring managers reported they were only moderately able to predict the work habits, people skills, ability to learn new skills, and leadership ability of new hires (Bishop 1993). As a Chicago employer noted, "You really don't know how they're gonna turn out." (Rosenbaum and Miller 1997: 512).

Given this ambiguity, employers' implicit racial biases can color their perceptions of applicants' suitability for the job. As discussed above, research in social psychology finds implicit biases shape how people interpret ambiguous information (Hamilton et al. 1990). Further, ambiguity allows employers to justify—even to themselves—their racially-motivated decisions on non-racial terms. For example, when evaluating black and white university applicants with mixed records (i.e., strong SAT scores but weak grades or vice versa), prejudiced white college students were more likely to recommend the white applicant (Hodson, Dovidio, and Gaertner 2002). They were also more likely to identify the credential (i.e. scores or grades) white applicants were stronger in relative to black applicants as being the better predictor of college success. Thus, more prejudiced respondents were more likely to indicate that SAT scores (grades) better predicted college success if the white applicant had stronger scores (grades). In contrast, respondents' evaluations of black and white applicants did not differ if the applicants had unambiguously strong or weak records. This suggests the ambiguity of the selection process allowed respondents to discriminate without recognizing it.

If an employer were to evaluate two identical applicants who differed *only* in their race, it would be obvious that a negative gut reaction to the black applicant was racially-motivated. In this case, if the employer wants to maintain a color-blind self-image, she would be *motivated* to deliberate rather than rely on her intuitions. As I noted, individuals' awareness of their implicit racial bias is an important determinant of their motivation to deliberate (Devine 1989). However, even very similar applicants will almost always differ in some way, and implicit biases can shape the interpretation of these differences. Further, given strong egalitarian norms, employers are likely motivated to interpret their behavior as non-discriminatory. Indeed, as Srivastava and Banaji (2011) argue, strong norms can constrain the tools available to people to make sense of and justify their behavior, leading them to interpret their behavior as consistent with prevailing norms. Thus, in the absence of unambiguous hiring criteria, employers are likely to interpret their intuitive decisions as non-discriminatory. This reduces their motivation to employ explicit cognition.

Time pressure and distractions

Finally, employers making hiring decisions—especially those screening resumes—frequently face *time pressure* and *distractions* (Chugh 2004; Rivera 2015b). They may have to quickly sort through thick stacks of resumes or applications, even as they face interruptions and other demands on their attention. For example, in her study of professionals in elite services firms, Rivera (2015b) found evaluators reported spending ten seconds to four minutes per resume, and typically bypassed cover letters. Further, these evaluators frequently screened resumes while they commuted, ate, or engaged in other activities.

Such "messy, pressured, and distracting" environments (Chugh 2004: 219) impair evaluators' ability to engage in deliberate reasoning, but leave implicit processes relatively unaffected (Govorun and Payne 2006; Miles 2015). Consequently, evaluators are more likely to rely on implicit processes under these circumstances because they do not have the *opportunity* to deliberate. For instance, in a study of interracial interaction, Italian participants' implicit anti-African bias was more predictive of their behavior towards an African interviewer relative to an Italian interviewer when they had to engage in a memory task during their interaction (Hofmann et al. 2008). The memory task reduced respondents' ability to deliberate, increasing their reliance on implicit cognition.

Together, intuitive hiring, ambiguity, and time pressure reduce employers' motivation and opportunity to deliberate. Thus, I expect implicit attitudes to be stronger predictors of hiring agents' evaluations of black and white jobseekers than explicit attitudes. Further, given egalitarian norms and an ambiguous selection process, I expect hiring agents to justify their evaluations in non-racial terms.

Previous research

Only a few studies have examined the relationship between respondents' implicit and explicit racial/ethnic attitudes and their evaluations of jobseekers (Blommaert et al. 2012; Derous, Nguyen, and Ryan 2009; Derous, Ryan, and Serlie 2015; Rooth 2010; Son Hing et al. 2008; Ziegert and Hanges 2005). Given this study's focus on understanding discrimination against black jobseekers in the United States, the studies have several important limitations. First, all but two studies were conducted outside of the U.S. Both organizational practices and egalitarian norms vary cross-nationally, which could affect the relevance of these studies for the U.S. context. For example, 44% of Swedish hiring agents in Rooth (2010) stated they prefer hiring Swedish males over Arab-Muslim males; thus, Swedish hiring agents may be more willing to act on their explicit racial attitudes than American hiring agents. Second, in all but one study, the implicit and/or explicit attitude measures were collected in the same session as the applicant evaluation task, potentially biasing the estimated association between the attitudinal measures and the applicant evaluation task. For instance, in Blommaert et al. (2012), Dutch students reported how warmly or coldly they felt towards Moroccans immediately after evaluating Dutch and Moroccan job applicants. It seems plausible respondents considered their evaluations of Moroccan job applicants in reporting their feelings of warmth. Further, in no study were the implicit and explicit attitudinal measures collected prior to and in a separate session from the jobseeker evaluation task, as would be consistent with the theorized causal order. Finally, these studies mostly rely on samples of college students. College students have not developed a habit of evaluating jobseekers, potentially decreasing their reliance on implicit cognition, and may have a different understanding than hiring agents about the legitimacy of intuitive decision-making. In this chapter, I address these concerns: I rely on a U.S. sample of individuals with hiring responsibilities in their workplace, and the attitudinal measures were collected prior to and in a separate session from the jobseeker evaluations.

To my knowledge, only one previous study has examined the effect of individuals' implicit and explicit racial attitudes on their evaluations of black and white jobseekers in the U.S. Ziegert and Hanges (2005) find college undergraduates' implicit anti-black bias—measured immediately after the jobseeker evaluations—predicts their likelihood of discriminating against black jobseekers, but only if they are in a "climate of racial bias" condition. They find no evidence that explicit racial attitudes, measured through the Modern Racism Scale and the Attitudes Towards Blacks Scale, predict evaluations of jobseekers in either experimental condition. In the "climate of racial bias" condition, study participants were shown a memo from the president of the hiring company stating that "given that the vast majority of our workforce is white, it is essential that we put a White person in the VP position." While an interesting finding, the implication for real-world hiring is unclear: I expect hiring managers in the U.S. are unlikely to receive such explicit instructions to discriminate.

The Study

To examine the relationship between employers' attitudes and their behavior, I conducted a two-wave study with 339 white, non-Hispanic workers with hiring responsibilities in their workplaces (see Table 2.1 for descriptive statistics).[3] In the first wave, conducted between June 7, 2014 and August 16, 2014, I collected information on respondents' implicit and explicit racial attitudes. In the second wave, conducted between September 8, 2014 and November 3, 2014, qualified participants evaluated white and black job applicants.

The two-wave approach allows me to assess whether hiring agents' implicit and explicit racial attitudes predict their evaluations of black and white jobseekers at a later date. This is in contrast with qualitative studies of employers' attitudes, which generally do not assess how these attitudes affect employers' behavior (e.g, Kirschenman and Neckerman 1991; Waldinger and Lichter 2003).[4] It is also in contrast with experimental field studies that measure employers' discriminatory behavior but do not measure their attitudes (e.g. Pager 2003; Gaddis 2015). Further, it improves upon the great majority of IAT attitude/behavior studies—including those outside the hiring context—which measure implicit attitudes and behavior during the same session, potentially biasing the estimated associations between the IAT and the measured behavior (see Fazio and Olson 2003; Greenwald et al. 2009).

Further, while not a probability sample of white hiring agents, the study sample is more representative than most employment-focused survey and lab experiments, which rely on student or convenience samples (e.g., Benard and Correll 2010; Blommaert et al. 2012; Munsch 2016). The sample is diverse with respect to age, gender, education, region, income, and establishment size. Importantly, as I describe below, it is also diverse with respect to implicit and explicit racial attitudes. This is essential as I could not properly study the effect of implicit and explicit racial attitudes on hiring agents' evaluations of jobseekers without sufficient variation on these attitudes.

Recruiting participants

I recruited participants through Amazon's Mechanical Turk. Mechanical Turk is an

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[3] I restricted participation to white, non-Hispanic respondents as I was unable to obtain a sufficient sample of non-white hiring agents, and the effect of racial attitudes could differ for non-white respondents. The focus on whites is warranted given their overrepresentation among U.S. hiring agents (Smith 2002; Wodtke 2015).

[4] To my knowledge, only one employer interview study examined how hiring agents' racial attitudes affected their hiring decisions. Surprisingly, Moss and Tilly (2001: 151) found companies where one or a plurality of managers criticized blacks' hard skills or interaction skills were *more* likely to hire a black man for their most recent hire than companies where none of the managers made similar criticisms. As the researchers were unable to control for the racial composition of the applicant pool or job applicant characteristics, this relationship should not be understood as causal. Nevertheless, it suggests we should not simply assume employers who express anti-black views are less likely to hire black applicants.

online platform for recruiting and paying individuals to perform tasks. Using Mechanical Turk samples, researchers have successfully replicated experiments in sociology, political science, psychology, and economics conducted in laboratory settings or with population-based samples (Berinsky, Huber, and Lenz 2012; Horton, Rand, Zeckhauser 2011; Paolacci, Chandler, Ipeirotis 2010; Weinberg, Freese, and McElhattan 2014). Administering the survey online reduces social desirability, important for a study interested in race (Chang and Krosnick 2009; Kreuter, Presser, and Tourangeau 2008). More generally, online respondents provide high-quality answers: compared to telephone respondents, their answers are more reliable, less susceptible to satisficing, and have higher concurrent and predictive validity (Chang and Krosnick 2009).

For the initial survey, I recruited 8,462 individuals. Study participants were told they would be asked about their attitudes and demographics. Following the survey, I sent invitations to 1,009 qualified respondents to participate in an applicant evaluation study. The invitation did not mention the title or content of the original survey. Qualified respondents lived in the U.S., identified as white and non-Hispanic, and had hiring responsibilities in their current workplace.[5] Of these, 727 individuals responded: 340 were randomly assigned to participate in the applicant evaluation task discussed in this chapter.[6] To prevent participants from drawing a connection between the survey and the applicant evaluation task, I waited a minimum of 60 days before contacting them.[7]

The applicant evaluation task

Study participants were asked to evaluate two applicants—fictitious, but presented as real—for the position of Assistant Store Manager in a leading national retail company. [8] They were told that the company would like to use the "wisdom of crowds" to improve its hiring practices. To increase task orientation, and following Correll, Benard, and Paik (2007), participants were told their input would be incorporated with other information the company collected and could affect actual hiring decisions. Participants were first shown a brief job description. Then, they were shown the resume of the first job applicant and asked to evaluate the applicant. Immediately following, participants were shown the resume of the second job applicant, and asked to evaluate him. To reflect the ambiguity of many U.S. hiring contexts (Dipboye et al. 2012), respondents were *not* told how to assess or weigh applicants' attributes. Next, participants compared the two applicants. Respondents proceeded to answer questions meant to gauge whether they were suspicious of the experimental setup and whether the experimental manipulations were successful, as well as several demographic questions. Finally,

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[5] I consider participants to have hiring responsibilities if they answered yes to the following: "As part of your job, do you make (or help make) decisions regarding whether or not to hire job applicants? Answer yes if you have input in the decision-making process, such as looking at résumés to decide who to interview, or interviewing candidates and making recommendations."

[6] I arrived at the final sample by removing one respondent who did not consent to let me use his or her data.

[7] Respondents were unable to access the names of studies they completed through Mechanical Turk more than 45 days prior; thus, they would have been unable to identify the previous study in their account.

[8] I use deception in order to increase experimental realism and decrease social desirability. This may be particularly important in settings where race is salient. I believe these benefits outweigh the relatively small costs of the brief (approximately ten minutes) deception. Furthermore, I cannot identify whether any individual respondent engaged in discriminatory behavior, since the applicants were not identical. Respondents were debriefed at the end of the survey.

respondents were debriefed, given an opportunity to comment, and asked for permission to use their data.[9]

Seventy-five respondents evaluated both a black jobseeker and a white jobseeker. They were randomly assigned to evaluate the black or white jobseeker first. This within-respondent cross-race comparison allows me to increase statistical power by accounting for idiosyncratic differences in how individuals evaluate jobseekers (see Aronson et al. 1990). For instance, perhaps due to differences in their interpretation of response scales or in the hiring selectivity of their workplaces, respondents may differ in how positively or negatively they evaluate jobseekers in general (black or white). Since each of the 75 respondents evaluates both a black and a white jobseeker, I can focus on respondents' *relative* evaluations of black and white jobseekers, and eliminate the random error introduced by these individual differences. Further, the comparative aspect of this design reflects the real-world task of evaluating resumes: evaluators typically compare applicants, asking "which applicant is more qualified?" rather than "how qualified is this applicant?"

Nevertheless, despite its strengths, the direct cross-race comparison raises concerns about social desirability (see "Is social desirability driving these results?" section for a broader discussion of this point). Consequently, as a robustness check, I analyzed whether respondents' implicit and explicit racial attitudes affect their evaluation of the *first* applicant they see. At the point respondents evaluate the first applicant, they did not know anything about the traits (including the race) of the second applicant. Thus, this approach is equivalent to studies where respondents evaluate a single black or white applicant to reduce social desirability pressures (e.g. Pedulla 2014). For this robustness check, I analyzed the evaluations of an additional 264 respondents who evaluated either a black or white jobseeker first (for a total of 339 respondents). While the 264 respondents were randomly assigned to a separate study, their task is identical to that of the 75 original respondents through the first applicant evaluation.[10] The full set of 339 respondents were randomly assigned to evaluate either a black or white jobseeker first: 170 participants evaluated a white applicant, and 169 participants evaluated a black applicant.

Race manipulation

I used racially-distinct names to indicate applicant race (see Bertrand and Mullainathan 2004). In order to select suitable names, I pre-tested eighteen names used in previous studies (Bertrand and Mullainathan 2004; Fryer and Levitt 2004; Gaddis 2015; Milkman, Akinola, and Chugh 2015). From this pretest, I chose six names for the experiment that successfully indicated the intended race while minimizing perceived class differences among the white and black names.[11] The distinctly white names I chose were Charlie, Greg, and Jake, and the distinctly

[9] One respondent did not grant permission to use his or her data.
[10] The task differs beginning with the second applicant.
[11] Eighty-seven white respondents, recruited through Mechanical Turk, evaluated the eighteen names between July 27, 2014 and July 29, 2014. The eighteen names were: Lamar, Terrell, Darnell, Tyrone, Jamal, Leroy, Jermaine, Jalen, DeShawn, Charlie, Brad, Steven, Greg, Todd, Matthew, Jay, Jake, and Connor. I removed three names from consideration due to relatively low identification rates. Only 54% of respondents identified Leroy and Jalen as black men, and 78% of respondents identified Jay as a white man. Then, I chose names that minimized differences in perceived class background among the black and white names. Specifically, I asked participants to report their first impressions of an individual, based on his name. I evaluated class background by asking respondents to guess the mother and father's highest education level, parents' social class, and household income when the individual was

black names were Jermaine, Lamar, and Terrell. The manipulation checks indicate respondents were successfully exposed to the treatment conditions: 88% of respondents correctly identified the race of the black applicant, and 91% of the white applicant. As I rely on male names to indicate race, a scope condition of this study is that it is limited to male applicants. While audit studies suggest racial discrimination is comparable for men and women (see Quillian et al. 2016), and the theory I outlined does not suggest employers' reliance on implicit cognition will vary by applicant gender, it remains possible that implicit and explicit racial attitudes would differentially affect the evaluations of male and female applicants.

Job position and resumes

I chose the position of Assistant Store Manager at a large retail store because I anticipated most study participants would have some familiarity with this position. I expect this to increase their comfort and engagement with the task. For similar reasons, I chose an entry level position, as appropriate resumes for such a position can be relatively concise and unspecialized.

To increase realism, the resumes were based on actual resumes of jobseekers for similar jobs, and of individuals who are currently assistant store managers in large retail stores. The two resumes indicated that the job applicants were recent college graduates from similarly-selective public universities in Massachusetts. Both had retail experience, but neither had direct experience managing employees. As neither applicant was unambiguously more qualified than the other, I expect respondents' implicit biases to shape their interpretation of applicant differences. I pre-tested the two resumes to assess whether they were of equivalent quality. Eighty-six respondents, recruited through Mechanical Turk, evaluated the two resumes for the same position and on the same criteria as I use in this experiment. I found no statistically significant difference in any of the outcomes. Resume order was randomly assigned.

Dependent variables

The primary dependent variable is the *hiring score.* The hiring score is a composite of four items that assess how respondents view applicants' suitability for the job. Specifically, for each applicant, respondents (1) reported whether they recommend that the company interview him (five-point scale from "Do not recommend" to "Very strongly recommend"), (2) estimated the likelihood he would be promoted if hired (seven-point scale from "Extremely unlikely" to "Extremely likely"), and (3) suggested a salary in case of hire (six-point scale from "\$35,000-\$39,999" to \$60,000-\$65,000"). Additionally, after evaluating both applicants, respondents chose one applicant to recommend for an interview. Then, they indicated how strongly they felt about their choice (five-point scale from "Not at all strongly" to "Extremely strongly.") I combined the last two questions into (4) a ten-point strength of choice index. A score of one

sixteen. Even among distinctly black names and distinctly white names, I found important distinctions in perceived class background. For example, compared to Lamar, the names DeShawn and Tyrone are associated with lower household income, parents' social class, and parents' education. My goal was not to eliminate all differences in class background between black and white names, since African-Americans overall come from more disadvantaged backgrounds than whites. However, I sought to minimize concerns that distinctly black names signal socioeconomic status, above and beyond the socioeconomic status that is signaled by race (Fryer and Levitt 2004).

indicates the applicant was not chosen and the respondent feels extremely strongly about this choice. A ten indicates the applicant was chosen and the respondent feels extremely strongly about this choice.

Additionally, for the analysis of respondents' evaluation of the *first* applicant, the dependent variable is the *evaluation score* rather than the hiring score. The evaluation score is restricted to the first three items in the hiring score, which are asked immediately after respondents are shown the first resume. It excludes the last item which focuses on a comparison of the two jobseekers, and which is asked after respondents have seen the resumes of the two applicants.

I used exploratory factor analysis to construct the hiring score and the evaluation score from the survey items. As these items are ordinal, I used a polychoric correlation matrix. For both composite measures, the analysis strongly suggests the individual items belong to the same factor. The retained factor is the only positive factor, and has an eigenvalue of 2.2 for the hiring score and 1.5 for the evaluation score. Further, the minimum factor loading is 0.6 in both cases. I standardized both measures so that a zero represents the mean value and the standard deviation is one.

Explicit anti-black bias

To assess respondents' explicit anti-black bias, I measured their endorsement of negative stereotypes about blacks' workplace suitability, as well as their affect towards blacks relative to whites. While stereotypes tap *beliefs* about the traits associated with racial groups, affect taps individuals' *emotions* towards members of these groups. These two components—stereotypes and affect—have long been central to theorizing employers' discriminatory motives (e.g. Allport 1954; Becker 1957; Phelps 1972).

Specifically, based on several questions in the original survey, I created three scales that capture respondents' views of black and whites' work ethic, competence, and hostility, and a fourth scale that captures respondents' affect towards blacks and whites. By relying on composite scales rather than individual items, I can better capture variation among respondents. Recall that, in in-depth interviews, employers frequently criticized blacks' work ethic, competence, and hostility. Further, these traits reflect enduring stereotypes about blacks (Devine and Elliott 1995).

The work ethic scale, adapted from Peffley, Hurwitz, and Sniderman (1997), is based on respondents' ratings of blacks and whites' work ethic (seven-point scale from "Very lazy" to "Very hard working"), reliability (five-point scale from "Not at all reliable" to "Extremely reliable"), discipline (five-point scale from "Not at all disciplined" to "Extremely disciplined"), and determination to succeed (five-point scale from "Not at all determined to succeed" to "Extremely determined to succeed.")

The competence scale, adapted from Fiske et al. (2002), is based on respondents' ratings of blacks and whites' intelligence (seven-point scale from "Very unintelligent" to "Very intelligent"), capability (five-point scale from "Not at all capable" to "Extremely capable"), skill (five-point scale from "Not at all skilled" to "Extremely skilled"), and efficiency (five-point scale from "Not at all efficient" to "Extremely efficient.")

The hostility scale is based on respondents' ratings of blacks and whites' hostility ("Not at all hostile" to "Extremely hostile"), violence (five-point scale from "Not at all violence prone" to "Extremely violence prone"), aggression (five-point scale from "Not at all aggressive" to "Extremely aggressive"), and criminality (seven-point scale from "Very law-abiding" to "Very criminal.") These items are adapted from Devine and Elliott (1995) and Bobo and Kluegel (1993). For the three stereotype scales, and following the language used to measure stereotypes in the General Social Survey (GSS), ratings are based on respondents' views of whites and blacks "in general."

Finally, the affect scale is based on respondents' assessment of their feelings of warmth towards (seven-point scale from "Extremely cold" to "Extremely warm"), comfort with (seven-point scale "Extremely uncomfortable" to "Extremely comfortable"), frequency of sympathy towards (five-point scale from "Never" to "Always"), liking of (seven point scale from "Dislike a great deal" to "Like a great deal"), and closeness towards (five-point scale from "Not at all close" to "Extremely close") blacks and whites. These items tap into, and add to, measures of affect included in the GSS (see Bobo et al. 2012) and the American National Election Study (ANES).

For all the measures, I subtracted respondents' evaluations of blacks from their evaluation of whites. Thus, I examine the *difference* in how respondents evaluate blacks and whites. Do respondents describe blacks as generally lazier, less competent, and more hostile than whites? Do they report feeling warmer towards whites than blacks? Examining the difference is important as I am interested in predicting employers' differential treatment of blacks and whites. If employers believe both blacks and white are equally lazy, it is hard to see how this would drive their differential treatment of black and white jobseekers. Further, previous research on racial attitudes finds modern stereotypes have a gradational (e.g. blacks are lazier than whites) rather than categorical (e.g. blacks are lazy) character (Bobo et al. 2012).

After subtracting respondents' evaluations of blacks from their evaluations of whites, I used exploratory factor analysis to create the four scales. As the items are ordinal, I used a polychoric correlation matrix. For the three stereotype scales, the retained factor is the only positive factor and has an eigenvalue above 2. Further, the minimum factor loading is 0.67 (for criminality). For the affect scale, the retained factor has an eigenvalue of 2.4 and is the only retained factor with an eigenvalue above 1. The minimum factor loading is 0.52 (for sympathy). I reverse-coded the hostility scale so that, for all the scales, a positive value implies anti-black attitudes. That is, higher values imply respondents feel more warmly towards whites than blacks, and believe blacks are generally less competent, have poorer work-ethic, and are more hostile than whites. For the purposes of predicting respondents' evaluations of black and white applicants, I standardized the scales so that a zero represents their mean value in the sample, and the standard deviation is one.

Implicit anti-black bias

While implicit bias is discussed in sociological studies of workplace inequality (e.g, Pager et al. 2009; Reskin 2003; Stainback 2008), it is almost never measured (for an exception, see Blommaert et al. 2012). I used the race Implicit Association Test (IAT) to measure respondents' implicit anti-black bias (Greenwald et al. 1998). The IAT is the most widely used measure of implicit attitudes (for an overview of the IAT's reliability and validity, see Nosek, Greenwald, and Banaji 2007).[12] The race IAT predicts a wide range of behavior, including whites' monetary generosity towards black partners (Stepanikova et al. 2011), physicians' treatment recommendations for black patients (Green et al. 2007), and individuals' decision to trust black partners, relative to white partners, in an economic trust game (Stanley et al. 2011).

The version of the IAT I used measures the strength of the association between the racial categories "African-American" and "Caucasian," and the evaluative categories "positive" and "negative." Specifically, it measures how quickly respondents associate the racial categories with the evaluative categories. The logic is that the faster the responses to category pairings (e.g. "African-American" and "Negative"), the stronger these categories are associated in respondents' minds.[13] In this case, faster responses to the pairings of Caucasian/positive and African-American/negative than to the pairings of Caucasian/negative and African-American/positive indicate implicit anti-black bias. I scored the test using the recommended *D* algorithm (Greenwald, Nosek, and Banaji 2003). Higher scores indicate greater implicit anti-black bias. For the analyses, I standardized the IAT score to have a mean of zero and a standard deviation of one.

Control variables

Since I do not experimentally manipulate hiring agents' racial attitudes, I control for respondents' age, education, gender, and region in multivariate analyses. Prior research indicates these attributes predict implicit and/or explicit racial attitudes (Nosek et al. 2007; Quillian 1996; Wodtke 2016), and they could plausibly affect the relative evaluations of black and white jobseekers.[14] I operationalize age as years, education as years of completed education, gender as a dummy variable (1 for female, 0 for male), and region as a series of dummy variables indicating residence in the North, Midwest, West, and South (the excluded category). I mean-centered age and education, so that the baseline respondent in the relevant analyses has the average years of completed education (15.5) and age (36.8) for the sample. Only two respondents had missing values on any of the control variables; I exclude these respondents from the relevant analyses.

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[12] Some critics have expressed skepticism about the construct and predictive validity of the IAT (e.g. Arkes and Tetlock 2004; Tetlock and Mitchell 2009). While a detailed review of the methodological debate is outside the scope of this chapter and more research is certainly needed, I believe many of these critiques have been satisfactorily addressed (for responses to critiques, see Jost et al. 2009 and Quillian 2008; for a review of the debate, see Tinkler 2012). For instance, Arkes and Tetlock (2004) posit that the IAT may capture cultural knowledge of racial stereotypes rather than personal attitudes. However, studies have shown a relationship between the IAT and discriminatory behavior (e.g. Agerström and Rooth 2011; Rooth 2010), which we would not expect if the IAT *only* captured cultural knowledge. Further, in a large-scale investigation of the relationship between the IAT, explicit attitudes, and cultural knowledge, Nosek and Hansen (2008) found explicit attitudes account for the (weak and inconsistent) relationship between implicit attitudes and cultural knowledge. That is, cultural knowledge had little to no independent relationship with the IAT.

[13] A race IAT is available online at http://www.implicit.harvard.edu. Taking the test is a useful way to gain intuition about the test's measurement of implicit bias.

[14] Education plausibly mediates, in addition to confounding, the effect of racial attitudes on relative evaluations of black and white jobseekers. Since controlling for mediators may bias effect estimates (Elwert 2013), I also estimate all the models without education as a control variable. The effects of implicit and explicit racial attitudes reported in this chapter are robust to excluding education from the models, in terms of both statistical and substantive significance.

Analytic strategy

First, I describe white hiring agents' implicit and explicit racial attitudes. This allows me to address potential concerns about limited variation in the explicit racial attitude scales, due to widespread egalitarian norms. Such norms likely affect how respondents see themselves and portray themselves when answering survey questions (Srivastava and Banaji 2011). Second, I assess the effect of implicit and explicit racial attitudes on respondents' relative evaluations of black and white jobseekers, among respondents who evaluated both a black and a white jobseeker. Since the dependent variable is continuous, I use OLS regressions for this analysis. Further, since each respondent evaluated two applicants, I account for the nonindependence of observations by clustering the standard errors at the level of the respondent. Third, as a robustness check, I analyze whether respondents' implicit and explicit racial attitudes affect their evaluation of the *first* applicant they see among the full sample of 339 respondents. This lets me assess whether the previous findings are robust to mitigating social desirability and self-presentation pressures resulting from the direct cross-race comparison. I use OLS regressions for this analysis. Finally, I analyze respondents' open-ended justifications of their hiring recommendations.

Results

Description: Racial attitudes

Table 2.2 presents descriptive statistics of hiring agents' implicit and explicit racial attitudes. Specifically, I present the distribution of respondents' implicit anti-black bias and of their explicit evaluations of blacks and whites on the 17 dimensions—related to affect, work ethic, competence, and hostility—discussed above. Unsurprisingly, respondents are more likely to express implicit anti-black bias than to explicitly report negative views of blacks relative to whites. Indeed, consistent with previous IAT studies (e.g. Nosek et al. 2007), the great majority of respondents (83%) exhibit implicit anti-black bias. For two-thirds of the sample (66%), this bias is at least moderate in strength. Only 12% of respondents demonstrated no implicit preference for either blacks or whites. In contrast, and largely consistent with comparable measures in the GSS, most respondents espoused the neutral option (i.e. rated blacks and whites equally) in all but one of the explicit attitude items.[15] Thus, I find evidence of limited variation of explicit responses, consistent with strong egalitarian norms.

Nevertheless, for almost all the explicit items, a large minority of respondents rated blacks more negatively than whites. For example, 44% of respondents rated blacks as more violence-prone than whites, 20% described blacks as less intelligent than whites, 33% rated blacks as lazier than whites, and 31% reported feeling more warmly towards whites than blacks.

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[15] Most white non-Hispanics respondents in the GSS rated blacks as equally intelligent (73%; 2014 GSS) and hard-working (60%; 2014 GSS) as whites, and felt equally close (53%; 2014 GSS) and warm (58%; 2002 GSS) toward blacks and whites. Further, while 45% of white non-Hispanics (2000 GSS) rate black and whites as equally "violence-prone," the percentage reaches a majority if the sample is restricted to better-educated respondents (at least 14 years of education) to better match this study's sample. (Author's analyses of GSS data, using survey weights and the latest year of available data for each variable).

This is also consistent with comparable measures in the GSS.[16] Furthermore, at least half of the respondents rated blacks more negatively than whites in at least one of the items pertaining to each of the four attitude dimensions: affect (70%), work ethic (50%), competence (50%), and hostility (58%). This means that, for instance, 58% of respondents rated blacks more negatively than whites in at least one of the four hostility items: violence, aggression, criminality, and hostility. Thus, even given egalitarian norms, many respondents are willing to express negative views of blacks relative to whites.

Finally, Table 2.3 presents correlations among the racial attitudes scales. All the pairwise correlations are highly statistically significant ($p < .001$). Further, the magnitudes of the correlations among the explicit attitudes are very large, ranging from $r = .58$ to $r = .86$. In contrast, the IAT is only moderately correlated with the explicit measures, with the magnitude of the correlations ranging from $r = .23$ to $r = .27$. These moderate implicit/explicit attitude correlations are consistent with previous research (Nosek et al. 2007), and suggest that implicit attitudes are related to but distinct from explicit attitudes.

Effect of hiring agents' racial attitudes on their evaluations of black and white job applicants

Tables 4 and 5 present results of regressions of white hiring agents' implicit and explicit racial attitudes on their evaluations of black and white jobseekers. I limit these analyses to the 75 respondents who evaluated both a black applicant and a white applicant. Given the high correlations among the explicit racial attitude scales, I only include one explicit racial attitude scale at a time in the regressions. I estimate both the separate (Table 2.4) and joint (Table 2.5) effect of explicit and implicit racial attitudes on applicants' hiring score. The latter analysis allows me to assess whether there is an additive effect of the implicit and explicit attitudes. That is, after controlling for implicit attitudes, do explicit attitudes increase our ability to predict hiring agents' relative evaluations of black and white jobseekers? The key predictors in all the regressions are the interactions between the racial attitude measures and applicant race. These interactions indicate whether racial attitudes differentially affect the evaluations of black and white applicants. A negative interaction coefficient implies that an increase in anti-black bias is associated with a decrease in hiring agents' evaluations of black applicants relative to white applicants.

I find that an increase in implicit anti-black bias is associated with a decrease in black applicants' hiring score, relative to white applicants' hiring score ($B = -.45$, $p < .05$; Model 1 in Table 2.4). Specifically, a one standard deviation increase in implicit anti-black bias is associated with a .45 standard deviation decrease in black applicants' hiring score relative to white applicants' hiring score. The effect of implicit anti-black bias is robust to including controls in the regression ($B = -.41$, $p < .05$; Model 2 in Table 2.4), and to including the explicit racial attitude interactions in the model (Table 2.5). Indeed, the coefficient for the implicit bias/applicant race interaction remains fairly stable across the models, ranging from -.38 to -.45.

In contrast, I do not find that explicit racial attitudes—whether measured as affect or

[16] A large minority of white non-Hispanic respondents in the GSS rated blacks as less intelligent (23%; 2014 GSS), lazier (34%; 2014 GSS), and more violence-prone (47%; 2000 GSS) than whites, and felt closer (42%; 2014 GSS) and more warmly (37%; 2002 GSS) toward whites than blacks. (Author's analyses of GSS data, using survey weights and the latest year of available data for each variable).

stereotypes—significantly predict hiring agents' evaluations of black and white jobseekers. Although a few of the estimates are moderately sized, none of the interacted explicit racial attitudes reach marginal statistically significant (Table 2.4 and Table 2.5). Additionally, after controlling for implicit bias, two of the coefficients are positive rather than in the expected negative direction (Models 1 and 5, Table 2.5). Further, using both the Bayesian Information Criterion (BIC) and Akaike Information Criterion (AIC) to assess model fit, I find models that include *only* the implicit attitude variables, with or without controls, are preferred over equivalent models that include only the explicit attitude variables, or models that include both the implicit and explicit attitude variables. Overall, these findings support the expectation that, compared to explicit racial attitudes, implicit racial attitudes are better predictors of hiring agents' relative evaluations of black and white jobseekers.

Additionally, these analyses identify an interesting main effect of implicit bias: an increase in implicit anti-black bias is associated with more positive evaluations of white applicants. Indeed, I find a one standard deviation increase in implicit anti-black bias is associated with a .34 standard deviation increase in white applicants' hiring score ($p < .01$; Model 1 in Table 2.4). This effect is robust to including controls in the regression ($B = .29$, $p < .05$; Model 2 in Table 2.4), and to including the explicit racial attitude interactions in the model (Table 2.5). I expect this main effect occurs because the IAT can be conceptualized as a measure of pro-white bias in addition to anti-black bias (DiTomaso 2015); since the IAT is a relative measure, high IAT scores simply imply respondents have more positive and/or less negative associations with whites than with blacks. Overall, this finding suggests implicit attitudes are not only associated with discrimination *against* black applicants relative to white applicants, but also with a bias in *favor* of white applicants. In contrast, none of the explicit racial attitude measures have a robust significant main effect on white jobseekers' hiring score.[17]

Is social desirability driving these results?

In the analysis reported above, white hiring agents evaluated both a black and a white jobseeker. By highlighting the cross-race comparison, this design could exacerbate self-presentation concerns, such as social desirability. Indeed, while I found a negative significant interaction between applicant race and implicit prejudice in the direct cross-race comparison, I did not find a significant negative main effect of applicant race (black vs. white) on applicants' evaluations (Table 2.4). It is plausible respondents modified their evaluations because the direct cross-race comparison heightened their awareness of their implicit bias (motivating them to deliberate) or because they were concerned they would be perceived as prejudiced if they evaluated a black applicant more negatively than a white applicant.

Yet, while the direct cross-race comparison may affect hiring agents' evaluations, it is unlikely it would *increase* the effect of hiring agents' implicit anti-black bias on their relative evaluations of black and white jobseekers. Indeed, the direct cross-race comparison plausibly *mitigates* the effect of implicit bias on applicant evaluations, by increasing the likelihood that

[17] While the main effect of hostility is moderately sized and marginally statistically significant in one model specification ($p = .08$; Model 10 in Table 2.4), the estimated effect is smaller and statistically insignificant in the other specifications (Model 9 in Table 2.4; Models 7-8 in Table 2.5). None of the other explicit racial attitudes have a statistically significant effect, and in six of the models the effects are negative rather than in the expected positive direction.

respondents recognize (and thus become motivated to control) their implicit biases. For instance, a hiring agent may modify her evaluations after reflecting on why she perceives the black jobseeker to be less competent than the white jobseeker. On the aggregate, such modifications would lessen the effect of hiring agents' implicit bias on their relative evaluations of black and white jobseekers.

Nevertheless, to examine the robustness of the previous analysis to the cross-race design, I analyze whether respondents' implicit and explicit racial attitudes affect their evaluation of the *first* applicant they evaluate. Table 2.6 presents OLS regressions of the effect of respondents' implicit and explicit racial attitudes, together and separately, on their evaluations of black and white jobseekers.[18] This analysis includes all 339 respondents. Again, the key predictors in the regressions are the interactions between racial attitudes and applicant race. A negative interaction coefficient implies that an increase in anti-black sentiment is associated with a decrease in hiring agents' evaluations of black applicants relative to white applicants.

Consistent with the previous results, I find an increase in implicit anti-black bias is associated with a significant decrease in black applicants' evaluation score, relative to white applicants' evaluation score. Specifically, across the five models, a one standard deviation increase in implicit anti-black bias is associated with a .23 to .24 standard deviation decrease in black applicants' evaluation score relative to white applicants' evaluation score ($p < .05$). Further, I find no evidence that explicit racial attitudes predict hiring agents' evaluations of black jobseekers relative to white jobseekers. None of the coefficients are statistically significant, they are all small in magnitude, and one is positive rather than in the expected negative direction. Consequently, the finding that implicit (but not explicit) racial attitudes predict hiring agents' evaluations of black jobseekers relative to white jobseekers is not a consequence of the direct cross-race comparison.

Additionally, and also consistent with the previous results, I find an increase in implicit anti-black bias is associated with more positive evaluations of white applicants. Indeed, across the five models, a one standard deviation increase in implicit anti-black bias is associated with a .15 to .17 standard deviation increase in white applicants' evaluation score ($p < .05$ for models 5; $p < .06$ for the remaining models). Thus, whether evaluating a single applicant, or comparing a black and a white applicant, hiring agents' implicit anti-black (or pro-white) bias is positively associated with white jobseekers' evaluations. In contrast, the coefficients for the main effect for the explicit racial attitude scales are statistically insignificant, small in magnitude, and six out of eight are negative instead of in the expected positive direction.

Finally, the finding of a significant main effect of implicit anti-black bias offers further evidence that hiring agents' self-presentation concerns are not driving the implicit bias effects. Recall this finding is based on hiring agents' evaluation of the *first* applicant they saw—in this case, a white applicant. Thus, white hiring agents' implicit anti-black (or pro-white) bias predicts how they evaluate a single white applicant: more implicitly pro-white respondents evaluate the

[18] While I present the models including the controls, I also estimated the models without the controls. The findings are unaffected by the exclusion of the control variables. In these analyses, a one standard deviation increase in implicit anti-black bias is associated with a .15 to .17 standard deviation increase in white applicants' hiring score ($p < .05$), and .25 to .27 standard deviation decrease in the hiring score of black jobseekers relative to white jobseekers ($p < .05$). Additionally, the coefficients of the main effect of the explicit racial attitude scales, and of the interaction between the scales and applicant race, are statistically insignificant, small in magnitude, and often in the opposite direction than would be expected.

single white applicant more positively than less implicitly pro-white respondents. It seems very unlikely that white hiring agents would be concerned about appearing prejudiced, or motivated to control their implicit racial biases, upon encountering a single applicant named Charlie, Greg, or Jake.

How do hiring agents justify their decisions?

Above, I demonstrated that white hiring agents' implicit anti-black bias affects their relative evaluations of black and white applicants. In this section, I explore how white hiring agents *justify* these evaluations. Do hiring agents recognize the influence of their implicit racial biases on their decision-making? This is important as awareness of bias can motivate hiring agents to control their intuitive responses (Devine 1989), yet implicit racial biases can influence behavior without awareness.

After respondents indicated which one of the two applicants they recommend for an interview, they were asked to explain *why* they made this choice. I analyze hiring agents' responses to this open-ended question. Specifically, I coded these responses by inductively identifying different categories of reasons for and against recommending the jobseekers. Since the justification question came after respondents had seen both applicants, I focus on the 75 respondents who evaluated both a black applicant and a white applicant. Two of the respondents did not answer this question, leaving an effective sample of 73 respondents for this analysis.

As discussed, the two jobseekers in this study were recent college graduates from similarly-selective public universities in Massachusetts; they had retail experience, but neither had direct experience managing employees. Thus, both jobseekers were plausible candidates for an Assistant Store Manager position in a large retail company, but neither was unambiguously more qualified than the other. Nevertheless, as would occur in a real-life hiring situation, there were differences between the applicants. For instance, Applicant A was an academically-oriented business major: he listed his G.P.A. (3.54) on his resume and had worked as a teaching assistant in college. In contrast, Applicant B graduated as an Economics major and had more directly relevant industry experience: an internship at Target.

Given the ambiguity of this selection process, I expected hiring agents to primarily justify their recommendations on non-racial terms. Indeed, I found all 73 hiring agents pointed to non-racial reasons to justify their hiring recommendations. Most commonly (80% of respondents), hiring agents argued the recommended applicant had more suitable experience for the Assistant Store Manager position. Interestingly, hiring agents were just as likely to use this justification on behalf of Applicant A as Applicant B. This suggests hiring agents exerted considerable discretion in determining the relevance of previous experiences.

What enabled hiring agents to exert this discretion?[19] Hiring agents frequently exerted discretion regarding *which* resume attributes they highlighted as relevant to the position. That is,

[19] Approximately half of the respondents did not clarify why one jobseeker had more suitable experience than another, beyond broadly referencing retail or leadership/management experience (e.g., "Jake appears to have held higher positions of leadership in his previous jobs than Terrell. I would recommend someone with leadership experience for an Assistant Manager position.") The remainder wrote more detailed accounts, enabling me to examine which experiences were (and were not) considered relevant to the position, and the reasons for this determination. The remainder of this section is based on these more detailed accounts.

respondents that recommended Applicant A simply identified different resume attributes as relevant than those that recommended Applicant B. For instance, seven respondents who recommended Applicant A praised his business degree and four praised his experience balancing cash drawers. In contrast, six respondents who recommended Applicant B praised his Target internship. This is consistent with an extensive literature in hiring that finds wide discretion in how employers weigh resume attributes (Dipboye et al. 2012).

Hiring agents also exerted discretion regarding their *interpretation* of resume attributes: they often pointed to the same experiences to justify different decisions, in ways consistent with their implicit bias. To illustrate, I focus on respondents' interpretation of Applicant A's experience as a teaching assistant for a Management and Organizational Behavior course. Is this experience irrelevant to the "real-world" work of management, or does it reflect leadership and relevant domain expertise? Five respondents discussed Applicant A's teaching experience: three interpreted the experience favorably, while two dismissed it. Implicit bias patterned these interpretations: the two respondents with at least moderate anti-black bias interpreted the experience favorably to explain their recommendation of the white jobseeker, while the three respondents with either pro-black bias or only slight anti-black bias referenced the experience to justify their recommendation of the black jobseeker. For instance, a moderately anti-black biased respondent argued that "Charlie is a much better candidate due to his teaching experience. He knows how to get people to listen and has more in depth experience [in management]." In contrast, a moderately *pro*-black biased respondent argued that "[Charlie's] only real leadership experience is as a TA in school, not in any of his employment areas. As a hiring manager myself I have found that 9 times out of 10 experience in a field versus education has given me a better quality of employee." Thus, respondents' interpretation of Applicant A's teaching experience differed in a manner consistent with their implicit racial attitudes.[20]

Finally, hiring agents sometimes exerted discretion by dismissing relevant experience as the appropriate selection criterion. In these cases, respondents typically emphasized applicants' personal characteristics over their experiences. For instance, respondents argued their preferred applicant seemed like a "go getter," more motivated, or like someone who could "think outside the box." Some respondents explicitly noted their recommended applicant had less suitable experience, but argued other criteria were more relevant. For example, a respondent wrote that "Terrell doesn't have as good of job experience, but he seems like he does well where he is at and stays for a while. I have the gut feeling that he will stay with a new organization and climb the corporate ladder." This respondent pointed to his "gut," rather than his assessment of the applicant's experience, to justify his recommendation. Thus, in addition to exerting discretion in assessing relevant experience, hiring agents also exert discretion by dismissing relevant experience as the appropriate selection criterion.

To be clear, I cannot conclude that any *individual* respondent's implicit anti-black bias causes her to value a Target internship more than a business education, to discount teaching experience, or even to prioritize her "gut feelings" about an applicant over her assessment of his

[20] To be clear, I am not suggesting that *any* experience can be interpreted as an asset or as a disadvantage. For instance, while respondents who recommended Applicant A frequently praised his business degree, no supporter of Applicant B praised his economics degree. Further, a couple of respondents who recommended Applicant B noted that Applicant A's business degree was an advantage. This suggests there is consensus among respondents that a business degree is more appropriate for the position of an Assistant Store Manager than an economics degree. However, I do expect respondents' implicit biases shape how they interpret more ambiguous experiences.

experience. These hiring agents may already have had different beliefs about the value of a business degree relative to an internship, about the usefulness of teaching experience, or about the predictive validity of personality over experience. However, from the quantitative data, we know that on the *aggregate* respondents' implicit anti-black bias predicts their relative evaluations of black and white jobseekers. Given this aggregate pattern, hiring agents' justifications suggest the ambiguity of the selection process enables them to interpret their behavior as color-blind. Hiring agents are likely motivated to justify and interpret their behavior as non-discriminatory due to strong egalitarian norms (Srivastava and Banaji 2011). The ambiguity of the selection process provides hiring agents with varied strategies—like determining whether a previous employment experience is (or is not) relevant to the position, or interpreting an experience favorably or negatively—to explain their decisions without invoking race. Thus, hiring agents appear able to maintain a color-blind self-image even when they engage in racially-motivated behavior.

Discussion

Why do employers discriminate? Despite strong evidence that hiring discrimination against black jobseekers remains prevalent in the United States (Quillian et al. 2016), we know relatively little about the *causes* of employers' discriminatory behavior. Well-known interview studies have documented employers' negative attitudes towards black jobseekers (e.g. Kirschenman and Neckerman 1991), but have not assessed whether these attitudes predict employers' hiring decisions. Indeed, sociological studies of workplace discrimination tend to examine employers' behavior (i.e. field experiments) or their racial attitudes (i.e. in-depth employer interviews), but not both (e.g. Gaddis 2015; Waldinger and Lichter 2003).

In this chapter, I draw on an original two-wave study with a sample of white hiring agents to examine whether respondents' explicit (conscious) and implicit (largely unconscious) racial attitudes predict their evaluations of white and black job applicants at a later date. Do hiring agents deliberately reject black jobseekers, perhaps due to anti-black affect or negative expectations about blacks' workplace productivity? Or do hiring decisions reflect largely unconscious biases? I find implicit racial attitudes predict hiring agents' evaluations of black applicants relative to white applicants. In contrast, I find no significant effect of explicit racial attitudes—whether measured as affect or stereotypes—on hiring agents' relative evaluations of black and white applicants. Indeed, hiring agents who describe blacks as less competent, lazier, and more hostile than whites, or who report feeling more warmly to whites than blacks, do not evaluate black and white jobseekers significantly differently than hiring agents who espouse egalitarian views. Thus, instead of deliberately rejecting black jobseekers, hiring agents' behavior appears to be driven by largely unconscious biases.

These findings are consistent with the theoretical predictions I derived from the MODE dual-process model of the attitude-behavior relationship (Fazio 1990). Indeed, I argued the hiring process at many U.S. organizations—characterized by ambiguity, time pressure, and the legitimacy of intuitive decision-making—encourages the use of implicit rather than explicit cognition. These factors limit employers' awareness of their racial biases, restrict their ability to control implicit processes, and legitimize the use of implicit cognition in hiring. Consequently, they reduce employers' motivation and opportunity to control their "gut" instincts. Overall, I highlight the role of social context in determining whether employers rely primarily on intuitive or deliberate decision-making. Future research should test whether varying aspects of the hiring process—such as discouraging intuitive decision-making or specifying clear selection criteria—decreases hiring agents' reliance on implicit cognition.

This study also points to pro-white bias as an important determinant of discriminatory decision-making. Sociologists overwhelmingly conceptualize hiring discrimination as reflecting anti-black sentiment, and emphasize employers' negative perceptions of blacks (e.g. Bobo et al. 2002; Neckerman and Kirchenman 1991; Waldinger and Lichter 2003). Instead, I find white hiring agents' implicit racial bias predicts how favorably they evaluate *white* jobseekers, not only how negatively they evaluate black jobseekers *relative to* white jobseekers. Specifically, an increase in white hiring agents' implicit anti-black (or pro-white) bias is associated with more positive evaluations of white applicants. This suggests a central role for pro-white sentiment in theorizing discrimination. Indeed, disparate treatment can just as easily result from employers' positive intuitive reactions to white jobseekers (e.g. "I like him!"), as from their negative "gut" reactions to black jobseekers. Future research should investigate the determinants and consequences of pro-white bias and in-group favoritism (see DiTomaso 2015). For instance, I expect white employers to be less likely to monitor their behavior for evidence of racial bias in their treatment of white jobseekers than black jobseekers. If true, this suggests hiring agents are more likely to remain unaware of discriminatory behavior that is driven by pro-white rather than anti-black sentiment.

In addition to improving our understanding of the determinants of white hiring agents' racially-motivated behavior, this study provides insight into white hiring agents' interpretation of this behavior. This is important as implicit biases can influence hiring agents' behavior without their awareness, yet awareness of bias is a key determinant of individuals' motivation to control their intuitive responses (Devine 1989). I find hiring agents *do not* recognize the effect of their implicit biases on their decision-making. Indeed, in open-ended responses, all hiring agents offered non-racial justifications of their hiring recommendations. These justifications suggest that, given the ambiguity of the selection process, hiring agents can utilize varied strategies to interpret their decisions without invoking race. For instance, hiring agents can determine whether an experience is (or is not) relevant to the position, can interpret an experience favorably or negatively, and can decide whether it is preferable to select applicants based on relevant experience or on "gut" intuitions. Given strong egalitarian norms, hiring agents are likely motivated to interpret their behavior as non-discriminatory; an ambiguous selection process provides them with the tools to do so.

Further, this study has implications for the sociological understanding of the determinants of attitude-behavior correspondence. When do employers' attitudes predict their behavior? While sociologists interested in race have long recognized that what employers does not necessarily predict what they do (Merton 1949; Pager et al. 2009; Waldinger and Lichter 2003), they have emphasized the *complexity* of the hiring process as the key determinant of attitude-behavior correspondence. From this perspective, racial attitudes weakly predict employers' hiring decisions when other factors overwhelm their influence. For instance, an employer with a preference for white applicants, but an even stronger preference for graduates from highly-selective colleges, may hire a black Harvard graduate over a white graduate from a non-selective college if those are his only choices. Indeed, beyond employers' racial attitudes, employers' non-racial applicant preferences, the preferences of other evaluators and current employees, and the composition of the applicant pool can all influence hiring decisions.

Yet, while the complexity of real-world hiring decisions inevitably affects attitude-behavior correspondence, it cannot account for the negligible effect of explicit racial attitudes in this study. In fact, this study represents a best-case scenario to observe the effect of hiring agents' racial attitudes. Respondents evaluated comparably qualified black and white applicants, and black and white names were randomly assigned to resume templates. Further, respondents were solely responsible for their evaluations. Thus, on the aggregate, respondents' non-racial applicant preferences, the applicant pool, and the preferences of others should not overwhelm the effect of respondents' explicit racial attitudes. Instead of complexity, I argue the implicit/explicit attitude distinction best accounts for the negligible effect of explicit racial attitudes in this study: instead of deliberating about the relative merits of black and white workers, respondents appear to act on their "gut" intuitions. Overall, these results imply that sociological theorizing on attitude-behavior correspondence should consider whether the behavioral context promotes reliance on implicit and/or explicit cognition.

This study has several scope conditions that future research should build upon. First, I analyzed the effect of implicit and explicit racial attitudes on hiring agents' evaluations at the resume screening stage. While the best evidence of hiring discrimination comes from this stage (Bertrand and Mullainathan 2004; Gaddis 2015), hiring agents may have stronger motivation or opportunity to deliberate in other stages of the hiring processes. For instance, some employers engage in selective recruitment practices, such as advertising in suburban newspapers, that reduce the number of black jobseekers in the applicant pool (Moss and Tilly; Wilson 1996). Employers report carefully deliberating about their choice of recruitment practices (Kirschenman and Neckerman 1991), suggesting that explicit cognition could play a stronger role in this stage of the hiring process. Second, like most U.S. hiring agents (Smith 2002; Wodtke 2015), hiring agents in this study were white. It seems plausible that non-white hiring agents, particularly black hiring agents, would be more motivated to deliberate given their greater awareness about hiring discrimination.[21] Third, the applicants were college-educated men, and the job opening was for an assistant store manager. While audit studies suggest racial discrimination is comparable across categories of gender, education, and occupation (Quillian et al. 2016), it is unclear whether implicit and explicit racial attitudes would similarly affect hiring decisions across these contexts. For instance, it is conceivable that employers rely more heavily on their intuitions when hiring for positions that emphasize "soft skills" or client interaction. Finally, while I limited study participation to hiring agents and told respondents they were evaluating real applicants for an existing job opening, future research should investigate the effect of implicit and explicit racial attitudes in real workplaces. I expect hiring agents are less likely to monitor their behavior for bias when screening resumes as part of their routine hiring process, suggesting an even stronger effect of implicit attitudes in these settings.

Overall, this study illustrates how employers can maintain and portray a non-prejudiced self-image while perpetuating racial disparities in employment. This offers insight into previous research that shows that, despite strong evidence of employer discrimination, most employers dispute or minimize the role of discrimination in hiring (Pager and Karafin 2009; Wilson 1996:

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[21] While I am unaware of representative studies of black and white hiring agents' perceptions of discrimination, population data suggests blacks are much more likely than whites to recognize racial discrimination in employment. For instance, 74% of whites (but only 40% of blacks) believe blacks have as good a chance as whites to get any kind of job for which they are qualified (Gallup 2014). Similarly, 64% of blacks, but only 22% of whites, believe that blacks in the U.S. are treated less fairly than whites in the workplace (Pew Research Center 2016).

127). I find that hiring decisions reflect implicit bias, which can affect behavior without awareness. Further, when interpreting their behavior, hiring agents do not invoke race: indeed, given egalitarian norms and the ambiguity of the selection process, hiring agents have the motivation and tools to interpret their behavior as color-blind. Thus, implicit processes, egalitarian norms, and hiring ambiguity together enable hiring agents to retain an egalitarian self-image while engaging in racially-motivated behavior.

Table 2.1 Characteristics of Respondents (N=339)

% white, non-Hispanic	100.0
% hiring responsibility	100.0
% working full-time	76.4
% supervisor	64
% self-employed	11.8
% female	59.3
% foreign-born	2.1
(Mean, SD) Years of age	(35.8, 11.0)
(Mean, SD) Years of education	(15.5, 1.8)
Individual earnings	
% Under $20,000	16.5
% $20,000 to $34,999	24.5
% $35,000 to $49,999	22.7
% $50,000 to $74,999	23.3
% $75,000 to $99,999	6.8
% $100,000 or above	6.2
Establishment size (# of employees)	
% Under 25	51.0
% 25-99	18.3
% 100-499	11.8
% 500 or more	18.9
Region	
% South	37.2
% Northeast	19.8
% Midwest	25.4
% West	17.7

Table 2.2 Implicit and Explicit Racial Attitudes

Implicit Association Test d-score (N = 332)	%
Strong black preference ($d \leq -.65$)	0.6
Moderate black preference ($-.65 < d \leq -.35$)	1.5
Slight black preference ($-.35 < d \leq -.15$)	3.6
No preference ($-.15 < d < .15$)	11.8
Slight white preference ($.15 \leq d < .35$)	16.6
Moderate white preference ($.35 \leq d < .65$)	36.5
Strong white preference ($d \geq .65$)	29.5

Explicit Racial Attitudes (N=339)	Race Difference (White - Black)		Whites > Blacks	White = Black	Blacks > Whites
Affect	Mean	SD	%	%	%
Warmth	0.3	1.3	31.0	57.8	11.2
Comfortable	0.7	1.6	41.9	49.9	8.3
Sympathy	-0.2	0.9	12.4	61.7	26.0
Close	1.1	1.3	59.9	36.2	3.9
Like	0.2	0.9	17.4	76.7	5.9
Work ethic					
Reliable	0.3	0.7	27.4	69.0	3.5
Hard-working	0.5	1.3	33.3	58.7	8.0
Disciplined	0.4	0.9	34.8	60.5	4.7
Determined to succeed	0.4	0.9	35.4	55.8	8.9
Competence					
Efficient	0.4	0.8	26.8	70.5	2.7
Skilled	0.4	0.7	31.0	67.6	1.5
Intelligent	0.4	1.0	19.5	79.7	0.9
Capable	0.4	0.9	34.8	60.5	4.7
Hostility	(Black - White)				
Violence-prone	0.6	0.9	2.7	53.4	44.0
Aggressive	0.4	0.9	5.9	59.9	34.2
Criminal	0.8	1.4	2.9	58.7	38.4
Hostile	0.4	0.8	5.0	60.2	34.8

Note: N=337 for the comfortable evaluations.

Table 2.3 Correlations among racial attitudes

	Implicit	Affect	Work Ethic	Competence	Hostility
Implicit	1				
Affect	0.27***	1			
Work Ethic	0.23***	0.61***	1		
Competence	0.23***	0.62***	0.86***	1	
Hostility	0.26***	0.58***	0.74***	0.76***	1

*** $p < .001$

Table 2.4 Effect of hiring agents' implicit and explicit racial attitudes on their evaluations of black and white applicants

	DV: Hiring Score									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Implicit anti-black bias										
IAT x Black	-0.45*	-0.41*								
	(0.21)	(0.21)								
IAT	0.34**	0.29*								
	(0.11)	(0.11)								
Explicit anti-black attitudes										
Affect x Black			-0.09	-0.35						
			(0.32)	(0.33)						
Affect			-0.08	0.19						
			(0.20)	(0.20)						
Work Ethic x Black					-0.18	-0.29				
					(0.30)	(0.31)				
Work Ethic					0.07	0.22				
					(0.22)	(0.21)				
Competence x Black							-0.03	-0.10		
							(0.36)	(0.36)		
Competence							-0.02	0.12		
							(0.28)	(0.24)		
Hostility x Black									-0.27	-0.36
									(0.26)	(0.26)
Hostility									0.20	0.31+
									(0.21)	(0.17)
Controls										
Female × Black		-0.06		-0.03		-0.13		-0.01		-0.13
		(0.40)		(0.40)		(0.43)		(0.48)		(0.44)
Female		0.15		0.12		0.21		0.15		0.23
		(0.23)		(0.25)		(0.26)		(0.29)		(0.26)
Education (years) × Black		0.03		0.06		0.06		0.07		0.04
		(0.08)		(0.08)		(0.09)		(0.08)		(0.09)
Education (years)		-0.04		-0.07		-0.07		-0.07		-0.05
		(0.06)		(0.06)		(0.06)		(0.06)		(0.06)
Age (years) × Black		-0.05*		-0.06*		-0.05*		-0.05*		-0.05**
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Age (years)		0.04**		0.04**		0.04**		0.04**		0.04**
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Northeast × Black		0.45		0.51		0.37		0.42		0.37
		(0.45)		(0.44)		(0.43)		(0.44)		(0.41)
Midwest × Black		0.14		0.42		0.32		0.28		0.34
		(0.50)		(0.54)		(0.50)		(0.52)		(0.51)

(continued)

Table 2.4 (continued)

West × Black		-0.06		0.05		-0.00		0.02		0.03
		(0.50)		(0.49)		(0.51)		(0.51)		(0.48)
Northeast		-0.50		-0.52+		-0.44		-0.48		-0.44
		(0.31)		(0.30)		(0.31)		(0.31)		(0.29)
Midwest		-0.47		-0.64+		-0.60+		-0.59+		-0.63*
		(0.31)		(0.33)		(0.31)		(0.32)		(0.32)
West		-0.11		-0.18		-0.15		-0.16		-0.18
		(0.32)		(0.30)		(0.31)		(0.31)		(0.28)
Black	0.26	0.17	0.23	-0.01	0.22	0.12	0.23	0.05	0.20	0.08
	(0.17)	(0.42)	(0.18)	(0.45)	(0.19)	(0.44)	(0.20)	(0.45)	(0.19)	(0.43)
Constant	-0.14	0.02	-0.12	0.14	-0.11	0.05	-0.12	0.09	-0.09	0.07
	(0.12)	(0.25)	(0.13)	(0.27)	(0.13)	(0.27)	(0.14)	(0.27)	(0.14)	(0.26)
Respondents (N)	75	73	75	73	75	73	75	73	75	73
Observations (N)	150	146	150	146	150	146	150	146	150	146
R^2	0.061	0.176	0.021	0.153	0.019	0.153	0.015	0.143	0.027	0.165

Note: The baseline respondent has the average level of anti-black bias for the sample, as measured by the IAT or explicit variable included in each model. Additionally, in the models with controls, the baseline respondent is from the South and has the average age and years of completed education for the sample.
* p < .05; + p < .1 (two-tailed tests).

Table 2.5 Effect of hiring agents' implicit and explicit racial attitudes on their evaluations of black and white applicants (implicit + explicit)

	DV: Hiring Score							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implicit anti-black bias								
IAT x Black	-0.45*	-0.38+	-0.44*	-0.38+	-0.47*	-0.41+	-0.42+	-0.38+
	(0.22)	(0.21)	(0.21)	(0.21)	(0.21)	(0.22)	(0.22)	(0.22)
IAT	0.35**	0.27*	0.34**	0.26*	0.35**	0.28*	0.31**	0.26*
	(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Explicit anti-black attitudes								
Affect x Black	0.00	-0.26						
	(0.30)	(0.32)						
Affect	-0.15	0.13						
	(0.19)	(0.19)						
Work Ethic x Black			-0.07	-0.20				
			(0.29)	(0.29)				
Work Ethic			-0.01	0.16				
			(0.20)	(0.19)				
Competence x Black					0.09	-0.00		
					(0.33)	(0.33)		
Competence					-0.12	0.05		
					(0.24)	(0.21)		
Hostility x Black							-0.19	-0.30
							(0.26)	(0.25)
Hostility							0.13	0.27
							(0.20)	(0.17)
Controls								
Female × Black		-0.11		-0.19		-0.06		-0.21
		(0.39)		(0.41)		(0.44)		(0.41)
Female		0.18		0.25		0.19		0.29
		(0.24)		(0.24)		(0.27)		(0.24)
Education × Black		0.02		0.03		0.03		0.01
		(0.08)		(0.08)		(0.08)		(0.09)
Education		-0.04		-0.04		-0.04		-0.02
		(0.06)		(0.06)		(0.06)		(0.06)
Age × Black		-0.05*		-0.05*		-0.05*		-0.05*
		(0.02)		(0.02)		(0.02)		(0.02)
Age		0.04**		0.04**		0.04**		0.04**
		(0.01)		(0.01)		(0.01)		(0.01)
Northeast × Black		0.52		0.42		0.45		0.42
		(0.45)		(0.44)		(0.45)		(0.42)
Midwest × Black		0.28		0.19		0.14		0.22
		(0.53)		(0.49)		(0.50)		(0.50)

(continued)

Table 2.5 (continued)

West × Black		-0.04		-0.07		-0.06		-0.05
		(0.50)		(0.51)		(0.51)		(0.49)
Northeast		-0.53+		-0.47		-0.50		-0.47
		(0.30)		(0.31)		(0.31)		(0.29)
Midwest		-0.54+		-0.52+		-0.49		-0.55+
		(0.32)		(0.30)		(0.31)		(0.31)
West		-0.12		-0.10		-0.11		-0.12
		(0.31)		(0.32)		(0.32)		(0.29)
Black	0.26	0.14	0.25	0.23	0.27	0.17	0.23	0.21
	(0.17)	(0.42)	(0.18)	(0.42)	(0.19)	(0.42)	(0.19)	(0.41)
Constant	-0.14	0.03	-0.14	-0.03	-0.15	0.01	-0.12	-0.01
	(0.12)	(0.24)	(0.12)	(0.25)	(0.13)	(0.25)	(0.13)	(0.24)
Respondents (N)	75	73	75	73	75	73	75	73
Observations (N)	150	146	150	146	150	146	150	146
R^2	0.07	0.183	0.063	0.182	0.065	0.177	0.067	0.194

Note: The baseline respondent has average level of implicit and explicit anti-black bias for the sample, as measured by the IAT and the explicit attitude measure included in each model. Additionally, in the models with controls, the baseline respondent is from the South and has the average age and years of completed education for the sample.
* p < .05; + p < .1 (two-tailed tests).

Table 2.6 Effect of hiring agents' implicit and explicit racial attitudes on their evaluations of black and white applicants, OLS models of first-applicant evaluations

	DV: Evaluation Score								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Implicit anti-black bias									
IAT x Black	-0.24*		-0.24*		-0.24*		-0.23*		-0.23*
	(0.11)		(0.11)		(0.11)		(0.11)		(0.11)
IAT	0.15+		0.16+		0.17*		0.16+		0.16+
	(0.08)		(0.08)		(0.08)		(0.08)		(0.08)
Explicit anti-black attitudes									
Affect x Black		-0.09	-0.02						
		(0.11)	(0.11)						
Affect		0.01	-0.02						
		(0.08)	(0.08)						
Work Ethic x Black				-0.00	0.05				
				(0.11)	(0.11)				
Work Ethic				-0.06	-0.08				
				(0.08)	(0.08)				
Competence x Black						-0.06	-0.00		
						(0.11)	(0.11)		
Competence						-0.02	-0.04		
						(0.08)	(0.08)		
Hostility x Black								-0.10	-0.04
								(0.11)	(0.11)
Hostility								0.01	-0.02
								(0.08)	(0.08)
Controls									
Female × Black	0.43+	0.55*	0.46*	0.53*	0.45+	0.50*	0.43+	0.49*	0.42+
	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)
Female	-0.11	-0.17	-0.11	-0.20	-0.14	-0.17	-0.12	-0.17	-0.11
	(0.16)	(0.16)	(0.16)	(0.16)	(0.17)	(0.16)	(0.16)	(0.16)	(0.16)
Education × Black	-0.01	0.01	-0.01	0.01	-0.00	0.01	-0.00	0.00	-0.01
	(0.07)	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)	(0.07)
Education	0.04	0.02	0.04	0.01	0.03	0.02	0.03	0.02	0.03
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Age × Black	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Northeast × Black	-0.14	-0.13	-0.17	-0.08	-0.12	-0.09	-0.13	-0.11	-0.14
	(0.31)	(0.31)	(0.31)	(0.31)	(0.31)	(0.30)	(0.31)	(0.31)	(0.31)
Midwest × Black	0.01	0.03	-0.02	0.05	0.00	0.05	0.01	0.03	0.00
	(0.28)	(0.28)	(0.29)	(0.28)	(0.29)	(0.28)	(0.29)	(0.28)	(0.29)

(continued)

Table 2.6 (continued)

West × Black	-0.57+	-0.55+	-0.60+	-0.49	-0.55+	-0.51	-0.57+	-0.53+	-0.58+
	(0.32)	(0.32)	(0.32)	(0.32)	(0.32)	(0.31)	(0.32)	(0.31)	(0.32)
Northeast	0.30	0.30	0.30	0.29	0.28	0.30	0.30	0.31	0.30
	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)
Midwest	-0.06	-0.06	-0.06	-0.05	-0.04	-0.05	-0.05	-0.06	-0.06
	(0.20)	(0.19)	(0.20)	(0.19)	(0.20)	(0.19)	(0.20)	(0.19)	(0.20)
West	0.25	0.21	0.24	0.19	0.23	0.21	0.25	0.21	0.24
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
Black	0.04	-0.03	0.05	-0.05	0.02	-0.03	0.04	-0.00	0.05
	(0.23)	(0.22)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)	(0.23)
Constant	-0.14	-0.10	-0.14	-0.08	-0.12	-0.10	-0.14	-0.11	-0.14
	(0.16)	(0.16)	(0.16)	(0.16)	(0.17)	(0.16)	(0.16)	(0.16)	(0.16)
Observations (N)	330	335	328	337	330	337	330	337	330
R^2	0.07	0.06	0.07	0.06	0.07	0.06	0.07	0.06	0.07

Note: The baseline respondent is from the South, has the average age and years of completed education in the sample, and has the average level of implicit and/or explicit anti-black bias for the sample (as measured by the IAT and/or explicit attitude measures included in each model.)
* $p < .05$; + $p < .1$ (two-tailed tests).

CHAPTER 3
The Strength of Whites’ Ties:
How employers reward the referrals of black and white jobseekers

To explain racial inequality in employment, sociologists routinely point to jobseekers' racially-segregated networks and employers' discriminatory behavior. Network scholars argue that, given segregated networks and black and white employees' unequal positions in the labor market, employers' reliance on employee referrals reproduces black disadvantage (e.g., Lin 2001; Trimble and Kmec 2011). Scholars of discrimination focus instead on employers' unequal treatment of equally-qualified black and white applicants (e.g., Gaddis 2015; Pager 2003). This discriminatory behavior is commonly attributed to employers' racial prejudice—that is, to their negative feelings or beliefs about blacks relative to whites (Quillian 2006, 300-301).

In this chapter, I bridge these literatures by examining whether employers' racial prejudice affects how they reward the referrals of black and white applicants, from black and white employees. While convincing evidence shows that employers respond differently to black and white applicants *without* referrals (Quillian et al. 2016), we largely do not know how employers respond to black and white *referred* applicants. Further, black and white applicants generally rely on same-race job contacts (Stainback 2008), but it is unclear whether employers similarly reward the recommendations of their black and white employees. These omissions are important: approximately half of U.S. workers find employment through personal contacts, and these contacts usually work at the hiring firm (Granovetter 1995; Mouw 2002).

While employers' racial prejudice is expected to decrease their likelihood of hiring non-referred black applicants, it is unclear whether it should increase or decrease black applicants' payoff to referrals. I draw on the social cognition literature on implicit prejudice and stereotypes, and on research on referral hiring, to develop competing predictions (e.g., Fernandez, Castilla, and Moore 2000; Hamilton, Sherman, and Ruvolo 1990; Fiske 1998). On the one hand, employee referrals could benefit black applicants by reducing employers' reliance on negative stereotypes. If an employer believes that a referral is a credible signal of applicant quality, she may give a referred black applicant a chance even if she is generally doubtful about blacks' work ethic. On the other hand, black applicants may be disadvantaged if prejudiced employers do not perceive their referrals as credible, or do not trust recommendations from black employees.

To test these predictions, I conducted an original two-wave study with a sample of white individuals with hiring responsibilities in their workplaces (hereafter, hiring agents). In the first wave, I used the Implicit Association Test (IAT) to measure hiring agents' implicit anti-black prejudice (Greenwald, McGhee, and Schwartz 1998). In the second wave, the hiring agents evaluated pairs of equally-qualified same-race job applicants. One applicant in each pair had a referral from either a black or white employee; the other had no referral. I found that in the most common real-life scenarios—black applicants referred by black employees, and white applicants referred by white employees—black applicants' referrals were significantly discounted relative to white applicants' referrals. In fact, black applicants *only* benefited from having a referral when two conditions were met: (1) the referring employee was white, and (2) they were evaluated by a less-prejudiced hiring agent.

Network research emphasizes black jobseekers' *social capital* disadvantage: black jobseekers have access to lower-status, less-influential ties than white male jobseekers and their contacts are less likely to speak to employers on their behalf (McDonald 2011; Smith 2000). By integrating employers' racial bias into this account, I find black jobseekers also face a *return* disadvantage: employers are often less willing to reward the equivalent referrals of black jobseekers than white jobseekers. This limits black jobseekers' ability to benefit from a key network resource: referrers' influence over hiring agents' decision-making.

Unequal social capital & disparate impact

Social capital theory (SCT) posits that individuals with better social capital achieve better outcomes, all else equal (Lin 2001; Portes 1998; Small 2009). Following Lin (2001, 25), I conceptualize social capital as accessed and mobilized resources embedded in social networks. In the labor market, jobseekers derive two primary resources from their networks: information and influence (Trimble and Kmec 2011). For instance, job contacts can tell jobseekers about job openings and can exert influence on jobseekers' behalf by vouching for them to hiring agents. Since high-status individuals are expected to be more knowledgeable about employment opportunities and more influential in the workplace than lower-status individuals, SCT further posits that high-status contacts are especially valuable to jobseekers (Lin 2001, 61).

Following SCT, network scholars argue that black jobseekers' disadvantage in *access* to social resources contributes to their labor market disadvantage (e.g., McDonald and Day 2010; Trimble and Kmec 2011). Given racially-segregated networks, jobseekers largely rely on same-race job contacts (Brown et al. 2016; Son and Lin 2012; Stainback 2008); for example, 86 percent of black and white workers who found their latest job through a personal contact in Boston, Los Angeles, and Atlanta, used a same-race contact (Mouw 2002). Since black employees have lower-status jobs than white employees, segregated networks are expected to lead to unequal access to high-status contacts for black and white jobseekers (Lin 2001). Indeed, compared to white men, blacks have lower-status networks and less-influential job contacts (McDonald 2011; Smith 2000).

Network scholarship also points to black jobseekers' difficulty *mobilizing* their job contacts' resources (e.g., Royster 2003; Smith 2005). Indeed, simply because a jobseeker's friend could vouch for him does not mean that the friend will do so (Smith 2005). For example, Royster (2003) documented that while black and white vocational students had access to the same white teachers, the teachers only referred white students to employers they knew. Further, black jobseekers may be disadvantaged by black employees' fear that recommending their job-seeking ties could damage their workplace reputation (Smith 2005). Overall, black jobseekers' contacts are less likely to speak to employers on their behalf than the contacts of white male jobseekers (McDonald 2011).

Employer prejudice & disparate treatment

Instead of highlighting black and white jobseekers' unequal social capital, discrimination scholars emphasize employers' unequal treatment of equally-qualified black and white jobseekers. Indeed, a meta-analysis of experimental field studies finds white applicants are 44% more likely to receive a callback or job offer than equally-qualified black applicants (Quillian et al. 2016). Employer racial discrimination persists across cities and occupations, and affects a wide range of applicants: male and female; with and without college degrees; with and without criminal convictions; from high-poverty and low-poverty neighborhoods; and those who apply in-person and online (e.g., Bertrand and Mullainathan 2004; Gaddis 2015; Pager 2003; Pager, Western, and Bonikowski 2009).

While field experiments generally cannot examine employers' motives, scholarship in psychology, sociology, and economics points to the importance of *implicit prejudice* in driving employer discrimination (e.g., Jost et al. 2009; Reskin 2000; Rooth 2010). I conceptualize prejudice as representing a negative affective (e.g., dislike) and/or cognitive (e.g., stereotypes) response to others based on their group membership, relative to the response to members of other groups (see Dovidio and Gaertner 2010; Quillian 2006, 300). Social cognition research finds this response can be activated implicitly—without effort, intention, or awareness (Greenwald and Banaji 1995). Thus, an employer may have a negative "gut" feeling about an applicant, and doubts about his work ethic, without being aware these responses are due to the applicant's race.

Moreover, once activated, stereotypes can bias individuals' interpretation of ambiguous evidence in stereotype-confirming ways (Hamilton et al. 1990). For example, whites with greater implicit anti-black prejudice, who are more likely to activate negative stereotypes, were quicker to perceive anger in ambiguously hostile black faces than less-prejudiced whites (Hugenberg and Bodenhausen 2003). Since these responses can influence hiring decisions (Rooth 2010), more implicitly-prejudiced employers are expected to penalize non-referred black applicants, relative to non-referred white applicants, more than less-prejudiced employers.

Theorizing returns to referrals

Returns to referrals

The strong evidence of employer discrimination suggests that even if white and black jobseekers accessed and mobilized equivalent social resources, employers may not equivalently reward their resources. Thus, it is important to distinguish between the resources applicants access and mobilize, and the *returns* to those resources. In this chapter, I focus on racial variation in the returns to having a recommendation from a current employee. By "return," I mean the difference in how employers respond to applicants with and without employee referrals, all else equal. Specifically, I examine whether employers' implicit anti-black prejudice affects how they reward black and white applicants' referrals, from black and white employees.

To be clear, network scholars have recognized that employers may differentially reward black and white jobseekers' referrals (e.g., Fernandez and Greenberg 2013, 94; McDonald 2011, 329; Son and Lin 2012, 603). Most prominently, in his theoretical account of social capital inequality, Lin (2001, 101-102) argues that groups with equivalent access to social resources may differentially benefit from those resources if they differ in their ability to mobilize their resources, or if employers respond differently to their mobilized resources. Yet, while black and white jobseekers' differential access to, and mobilization of, social resources has been well-studied (e.g., Royster 2003; Smith 2000), employers' differential response to mobilized social resources has yet to be systematically examined or theorized.

How do employers interpret referrals?

To theorize the effect of employer prejudice on returns to referrals, it is instructive to consider whether employers interpret employee referrals as credible, positive signals of applicant quality.[22] A large body of theoretical and empirical research on referral hiring suggests employers do interpret referrals as credible (see Fernandez and Greenberg 2013, 85). Indeed,

[22] While I focus on referral credibility, employers may reward referrals for other reasons. For instance, hiring managers may give an "extra look" to referred applicants out of courtesy to referring employees (Fernandez and Galperin 2014, 454).

employees are likely motivated to recommend well-qualified applicants to protect their workplace reputation, and are well-positioned to identify appropriate applicants given their familiarity with both the job and their job-seeking friends (Marin 2012; Rees 1966; Smith 2005). Yet, employers may also have reason to be skeptical of employees' recommendations: employees may be motivated to help their job-seeking friends, or to earn a bonus for providing a referral, even if they have doubts or limited information about their friends' workplace suitability (Fernandez et al. 2000, 1333; Kim and Fernandez 2017; Smith 2005). Thus, in hypothesizing returns to referrals, I consider two possibilities: employers may interpret referrals as highly credible, or as more ambiguous, signals of applicant quality.

Employer prejudice and applicant race

How might employer prejudice affect returns to referrals for black and white applicants? If employers interpret referrals as highly credible, then I would expect referrals to reduce employers' reliance on group-based stereotypes by providing positive *individuating* information about applicants (Fiske 1998, 386). Indeed, the social cognition literature on stereotyping suggests that when evaluators encounter information about an individual that is credible and clearly contradicts the group stereotype, they rely less on the stereotype in evaluating the individual (Fiske, Lin, and Neuberg 1999; Macrae, Shepherd, and Milne 1992). Thus, an employer may expect a referred black applicant to be hard-working, even if she is generally doubtful about blacks' work ethic. Since more-prejudiced employers activate more negative stereotypes about black workers than less-prejudiced employers, referrals should be especially helpful to black jobseekers evaluated by more-prejudiced employers. This suggests:

> Hypothesis 1a*:* More-prejudiced employers reward black applicants' referrals *more* than white applicants' referrals, relative to less-prejudiced employers.

If, in contrast, employers interpret referrals as more ambiguous signals of applicant quality, I would expect more-prejudiced employers to be more likely to discount black applicants' referrals than less-prejudiced employers. Recall that activated stereotypes can bias individuals' interpretation of ambiguous evidence (Hamilton et al. 1990). Indeed, an employer with negative expectations about an applicant's work ethic may be skeptical of a referral attesting to the applicant's positive attributes, perhaps reasoning that the referring employee must have felt obligated to help a friend in need. This implies:

> Hypothesis 1b*:* More-prejudiced employers reward black applicants' referrals *less* than white applicants' referrals, relative to less-prejudiced employers.

Employer prejudice and referrer race

In terms of referrer race, I expect more-prejudiced employers to be more likely to discount recommendations from black employees, relative to white employees, than less-prejudiced employers. Indeed, I expect more-prejudiced employers to perceive black employees as less trustworthy and as less capable of determining applicants' suitability for the job than less-prejudiced employers.[23] Consistent with this expectation, Stanley and colleagues (2011) found more implicitly-prejudiced individuals trust blacks less, relative to whites, than less-prejudiced individuals. This discussion suggests:

> Hypothesis 2: More-prejudiced employers reward referrals from black employees *less* than referrals from white employees, relative to less-prejudiced employers.

For referrer race, I expect referral ambiguity to affect the strength, but not the direction, of the hypothesized employer prejudice effect. Specifically, I expect anti-black prejudice to have a stronger effect when employers interpret referrals as more ambiguous signals.[24]

Methodological limitations of previous empirical research

Despite an extensive literature on referrals in the labor market, the designs of previous empirical studies do not allow careful examination of racial variation in the labor market. Thus, we currently know very little about how employer racial prejudice, applicant race, and referring employee race affect returns to referrals. *Jobseeker studies* compare the employment outcomes of workers who obtained their jobs with and without job contacts, or with different types of job contacts (e.g., black contact versus a white contact) (e.g., Green, Tigges, and Diaz 1999; Kmec and Trimble 2009; Stainback 2008). Although findings are mixed, several studies suggest black jobseekers experience poor payoffs to job contacts (for a review, see McDonald et al. 2013). Yet, jobseeker studies cannot (and do not seek to) isolate how employers reward referrals. For example, consider the finding that jobseekers who use female contacts obtain lower-paying jobs than jobseekers who use male contacts (Smith 2000). This finding *may* imply that employers reward referrals from men more than referrals from women. However, the pay difference could also reflect differences in the jobseekers who rely on male and female contacts, in the jobs male and female contacts provide information about, or in the job-finding assistance offered by male and female contacts.

In contrast to jobseeker studies, *firm studies* compare the outcomes of applicants with and without referrals. These studies generally find referred applicants are more likely to be offered a job than non-referred applicants, even after controlling for observed differences (e.g., Brown et al. 2016; Burks et al. 2015; Fernandez et al. 2000; Fernandez and Weinberg 1997; Petersen, Saporta, and Seidel 2000). Yet, to my knowledge, no firm study examines whether employers differentially reward referrals from black and white employees, or measures employer prejudice. Further, only two studies, with dissimilar findings, assess the effect of applicant race on returns to employee referrals. The first study found no effect of referrals on hiring likelihood for *any* racial group and no evidence of discrimination against non-referred black applicants—two

[23] Relatedly, Kmec and Trimble (2009) speculate that—given employer racial biases—black contacts without direct contact with employers may be more useful to job applicants than those with direct contact.

[24] While I do not hypothesize or test the three-way interaction between employer prejudice, applicant race, and referrer race, I do examine the effect of employer prejudice on returns to referrals for all the applicant race/referrer race conditions: (1) black applicants, black referrer, (2) black applicants, white referrer, (3) white applicants, black referrer, or (4) white applicants, white referrer (see Table 3.4). Given the scarcity of research on how individuals interpret same-race versus cross-race relationships, and an unexpected finding regarding the effect of employer prejudice on returns to referrals by referrer race (see Table 3.3), I decided to inductively interpret (rather than deductively hypothesize) the effect of employer prejudice on returns to referrals for the applicant race/referrer race conditions.

anomalous results (Fernandez and Fernandez-Mateo 2006). The second study found that both black and white applicants received job offers at higher rates if they were referred, and that black applicants' hiring disadvantage among non-referred applicants disappeared among referred applicants (Fernandez and Greenberg 2013). While this suggests black applicants benefited more than white applicants from referrals, the interaction between applicant race and referral status was insignificant. Moreover, one hiring agent evaluated most (87%) applicants, limiting generalizability.

Moreover, firm studies are unable to establish the causal effect of referrals on employers' hiring decisions: referred applicants may differ from non-referred applicants in ways unobservable to the analyst (Fernandez et al. 2000). The unobserved referred/non-referred difference could vary systematically with applicant race, making it difficult to establish whether employers differentially reward black and white applicants' referrals. For example, if employees are more reluctant to recommend black applicants than white applicants, referred black applicants may be a more selective group than referred white applicants.

As I detail below, I address these methodological concerns by using an experimental approach: referred and non-referred applicants, and black and white applicants, had equivalent (and randomly assigned) résumés and applied to the same job opening. Black and white referrers had the same position and wrote identical recommendation letters. Consequently, I am able to establish the causal effect of referrals on hiring agents' evaluations of applicants. This is important to establish because examining the effect of employer racial prejudice on returns to referrals would be fruitless if employers did not reward referrals at all.

The Study

I conducted a two-wave study with 226 white, non-Hispanic workers with hiring responsibilities in their workplaces.[25] In the first wave, conducted between June 7, 2014 and August 16, 2014, I measured respondents' implicit anti-black prejudice and demographic characteristics. In the second wave, conducted between September 8, 2014 and November 3, 2014, I conducted a survey experiment with qualified respondents to assess how they reward the referrals of black and white jobseekers, from black and white employees. I rely on a survey experiment, rather than a field experiment, since manipulating whether employees recommend applicants—as well as the race of employees—in a real-world setting is infeasible.

The two-wave approach improves upon the great majority of IAT studies, which measure implicit prejudice and behavior during the same session, potentially biasing the estimated associations between the IAT score and the measured behavior (see Fazio and Olson 2003; Greenwald et al. 2009a). For example, in studies where the IAT is administered prior to the measured behavior, taking the IAT may make respondents aware of (or concerned about) their racial bias, affecting their subsequent behavior.

[25] I restricted participation to white, non-Hispanic respondents as I was unable to obtain a sufficient sample of non-white hiring agents, and the effect of anti-black prejudice could differ for non-white respondents. The focus on whites is warranted given their overrepresentation among U.S. hiring agents (Smith 2002; Wodtke 2015).

Recruiting participants

I recruited participants through Amazon's Mechanical Turk, an online platform for recruiting and paying individuals to perform tasks. Using Mechanical Turk samples, researchers have successfully replicated social scientific experiments conducted in laboratory settings or with population-based samples (e.g., Berinsky, Huber, and Lenz 2012; Paolacci, Chandler, Ipeirotis 2010; Weinberg, Freese, and McElhattan 2014). Administering the survey online reduces social desirability, important for a study interested in race (Chang and Krosnick 2009). More generally, online respondents provide high-quality answers: compared to telephone respondents, their answers are more reliable, less susceptible to satisficing, and have higher concurrent and predictive validity (Chang and Krosnick 2009). Relative to participants recruited through other internet platforms, Mechanical Turk participants are equally or more attentive (Berinsky et al. 2012; Paolacci et al. 2010; Weinberg et al. 2014).

For the initial survey, I recruited 8,462 individuals. Following the survey, I sent invitations to 1,009 qualified individuals to participate in an applicant evaluation study. The invitation did not mention the title or content of the original survey. Qualified individuals lived in the U.S., identified as white and non-Hispanic, and had hiring responsibilities in their current workplace.[26] Of these, 727 individuals responded: 228 were randomly assigned to the experiment; the remainder were assigned to a separate study.[27] To prevent respondents from drawing a connection between the IAT and the applicant evaluation task, I waited a minimum of 60 days before contacting them.[28]

Table 3.1 presents descriptive statistics about the study participants. While not a probability sample of white hiring agents, the sample is more representative than most employment-focused survey and lab experiments, which rely on student or convenience samples (e.g., Benard and Correll 2010; Blommaert, Tubergen, and Coenders 2012; Munsch 2016). Two-thirds of the participants are supervisors, a fifth work in firms with 500 or more employees, most are college graduates, and slightly more than half (57%) are women. The sample is also diverse with respect to age, earnings, geography, and implicit prejudice.

Applicant evaluation task: Experimental design

For the survey experiment, study participants were asked to evaluate two equally-qualified job applicants—fictitious, but presented as real—for the position of Assistant Store Manager in the Boston store of a leading national retail company.[29] To reduce personal and

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[26] I consider participants to have hiring responsibilities if they answered yes to the following: "As part of your job, do you make (or help make) decisions regarding whether or not to hire job applicants? Answer yes if you have input in the decision-making process, such as looking at résumés to decide who to interview, or interviewing candidates and making recommendations."

[27] I arrived at the final sample by removing one respondent who did not consent to let me use his or her data, and one respondent who did not evaluate either applicant.

[28] Respondents were unable to access the names of studies they completed through Mechanical Turk more than 45 days prior; thus, they would have been unable to identify the previous study in their account.

[29] I used deception to increase realism and decrease social desirability bias. This is important for studying referrals, since the hypothesized effect is a result of having a real employee risk her reputation by vouching for an applicant, and for a study interested in race. I believe these benefits outweigh the costs of the brief (approximately ten minutes) deception.

social desirability bias, each respondent evaluated two same-race applicants: white or black.[30] One of the two applicants had a referral from a black or white employee; the other had no referral. Thus, participants were randomly assigned to one of four experimental conditions: (1) black applicants, black referrer, (2) black applicants, white referrer, (3) white applicants, black referrer, or (4) white applicants, white referrer. Overall, the design consisted of three between-subjects factors (applicant race, referring employee race, and respondent prejudice) and one within-subjects factor (referral status). Since my goal was to examine variation in returns to referrals, it was important to vary referral status within subject, as within-subject comparisons are more efficient than between-subjects comparisons (Cohen 1988). For details on the job position, résumés, and employee referral form, see Appendix A.

Applicant Evaluation Task: Procedure

Participants were first introduced to the applicant evaluation task and told the retail company sought their opinion because it was interested in using the "wisdom of crowds" to improve its hiring practices. To increase task orientation (see Correll et al. [2007]), participants were also told their input would be incorporated with other information the company collected and could affect actual hiring decisions. Participants then read a brief job description, the résumé of the first applicant, and an employee referral form for the first applicant (if applicable). I counterbalanced whether participants first saw the referred or non-referred applicant by experimental condition.[31] After evaluating the first applicant, they reviewed the second applicant's résumé and employee referral form (if applicable), evaluated the second applicant, and compared the two. Respondents then answered questions designed to gauge suspicions about the experimental setup and the success of the manipulation, as well as demographic questions. Finally, respondents were debriefed and asked for permission to use their data.[32]

Race manipulation

I indicated applicant and referrer race by using racially-distinct names in the résumé and employee referral forms (see Bertrand and Mullainathan 2004). The distinctly white names I used were Charlie, Greg, and Jake, and the distinctly black names were Jermaine, Lamar, and Terrell. To select suitable names, I pre-tested eighteen names used in previous studies (Bertrand and Mullainathan 2004; Fryer and Levitt 2004; Gaddis 2015; Milkman, Akinola, and Chugh 2015). In the pre-test, 87 white respondents recruited through Mechanical Turk evaluated the eighteen names, for a total of 1565 evaluations. From this pre-test, I chose names that successfully signaled the intended race while minimizing perceived class differences among the

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[30] This approach follows previous hiring-focused experiments (Correll et al. 2007; Pedulla 2016), whose participants also compared two same-race and same-gender applicants. Given my focus on implicit prejudice, it was important to minimize respondents' awareness of the race manipulation: I sought to capture biased behavior even among respondents who would not knowingly penalize black applicants' referrals or recommendations from black employees. Minimizing respondents' awareness of the race manipulation also decreases social desirability bias.

[31] All results are robust to including controls for referred/non-referred applicant order, by experimental condition.

[32] Prior to the debrief, one respondent noted the study may be interested in race and another that "the information might be put together just for the study, but it looked like it was real." After the debrief, two additional participants reported suspicions. I included these four respondents in the analyses; results remain the same if I exclude them.

white and black names (see Appendix B for details). Job applicants and referring employees were randomly assigned to names that matched the experimental condition.

The post-experiment manipulation checks indicate the names successfully signaled race. Respondents correctly identified applicants' race at high rates: 88 percent for black applicants and 91 percent for white applicants. Additionally, 77 percent of respondents correctly identified black referrers' race and 87 percent correctly identified white referrers' race. As I rely on male names to indicate race, an important scope condition of this study is that it is limited to male referrers and applicants. While audit studies suggest racial discrimination is similar for men and women (Quillian et al. 2016), it is unclear whether gender affects racial variation in returns to referrals.

Dependent variables

The primary dependent variable in this study is the *return to employee referral*, operationalized as the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. The evaluation score is a composite of four items that assess respondents' view of applicants' suitability for the job.

Specifically, for each applicant, respondents (1) reported whether they recommend that the company interview him (five-point scale from "Do not recommend" to "Very strongly recommend"), (2) estimated the likelihood he would be promoted if hired (seven-point scale from "Extremely unlikely" to "Extremely likely"), and (3) suggested a salary in case of hire (six-point scale from "$35,000-$39,999" to $60,000-$65,000"). Additionally, after evaluating both applicants, respondents chose <u>one</u> applicant to recommend for an interview. Then, they indicated how strongly they felt about their choice (five-point scale from "Not at all strongly" to "Extremely strongly.") I combined the last two questions into (4) a ten-point "strength of choice" index. A score of one indicates the applicant was not chosen and the respondent feels extremely strongly about this choice. A ten indicates the applicant was chosen and the respondent feels extremely strongly about this choice.

I used exploratory factor analysis to construct the evaluation score from these four variables. As the variables are ordinal, I used a polychoric correlation matrix. The analysis strongly suggests the four variables belong to the same factor: the retained factor has an eigenvalue of 2.2 and is the only positive factor, and the minimum factor loading is .58. A positive return to employee referral indicates the referred applicant has a higher evaluation score than the non-referred applicant. I standardized the evaluation score to have a mean of zero and a standard deviation of one. Results are robust to using the individual variables instead of the composite evaluation score (see Appendix C).

Anti-black prejudice

While implicit prejudice is commonly discussed in sociological studies of workplace inequality (e.g, Pager et al. 2009; Reskin 2000; Stainback 2008), it is rarely measured (for an exception, see Blommaert et al. 2012). I used the race Implicit Association Test (IAT) to measure respondents' implicit anti-black prejudice (Greenwald et al. 1998).[33] The IAT is reliable

[33] A race IAT is available online at http://www.implicit.harvard.edu.

and widely used within psychology (for an overview of the IAT's reliability and validity, see Nosek, Banaji, and Greenwald 2007). The race IAT has been shown to predict a wide range of behavior, including whites' monetary generosity towards black partners (Stepanikova, Triplett, and Simpson 2011), vote choice in the 2008 election (Greenwald et al. 2009b), and individuals' decision to trust black partners, relative to white partners, in an economic trust game (Stanley et al. 2011).

The IAT measures the strength of associations between categories by capturing response times in a categorization task. The version of the IAT I used measures the strength of the association between the racial categories "African-American" and "Caucasian," and the evaluative categories "Positive" and "Negative." Specifically, respondents were told to rapidly sort positive (e.g., "happy," "joy," "wonderful") and negative (e.g., "awful," "terrible," "horrible") words, and images of racially-distinctive black and white faces, into one of two category pairings. Each respondent sorted the words and images into stereotype-consistent category pairings (i.e., African-American/Negative and Caucasian/Positive) and stereotype-inconsistent category pairings (i.e., African-American/Positive and Caucasian/Negative). The logic is that the faster the responses to category pairings, the stronger these categories are associated in respondents' minds. In this case, faster responses to the stereotype-consistent pairings of Caucasian/Positive and African-American/Negative than to the stereotype-inconsistent pairings of Caucasian/Negative and African-American/Positive indicate implicit anti-black prejudice.

I scored the test using the recommended D algorithm, in which the difference in response times between the stereotype-consistent and stereotype-inconsistent pairings is divided by their pooled standard deviation (Greenwald, Nosek, and Banaji 2003). To assess the strength of implicit anti-black prejudice, researchers typically use the following cutoffs: no anti-black prejudice ($D < .15$), slight anti-black prejudice ($.15 \leq D < .35$), moderate anti-black prejudice ($.35 \leq D < .65$), and strong anti-black prejudice ($D \geq .65$).[34] In the study sample, D scores ranged from -.64 to 1.4 ($M = .46$, $SD = .35$). Thus, while the average respondent exhibited moderate anti-black prejudice and the great majority of the sample (80%) exhibited at least some anti-black prejudice (see Table 3.1), there is considerable variation in the extent of implicit anti-black prejudice in the sample. For the analyses, I standardized the IAT D score to have a mean of zero and a standard deviation of one.

An important advantage of the IAT, as opposed to explicit measures of prejudice, is that it is resistant to personal and social desirability bias (Fazio and Olson 2003). Even subjects asked to fake their response to the race IAT were unable to do so unless explicitly instructed on how to do so (Kim 2003). Further, the IAT can capture biases among people who genuinely believe they are unbiased. Given widespread egalitarian norms, many whites have developed conscious, non-prejudiced self-images while retaining unconscious negative feelings towards blacks (Dovidio and Gaertner 2010). Thus, individuals may interpret their actions, even to themselves, in non-prejudiced ways; this may reduce the ability of explicit measures of prejudice to predict discrimination (Srivastava and Banaji 2011). Indeed, in 32 samples of black-white interracial behavior, the predictive validity of the IAT significantly exceeded that of explicit measures of prejudice (Greenwald et al. 2009a). Further, two Swedish studies found that the IAT, but not

[34] These cutoffs are reported in the Project Implicit website (https://implicit.harvard.edu/implicit/demo/background/raceinfo.html) and adapted from the cutoffs for small, medium, and large effect sizes for Cohen's (1977) *d* measure (Greenwald et al. 2003, 199).

explicit measures, reliably predicted hiring discrimination against Arab-Muslim men and against obese individuals in real workplaces (Agerström and Rooth 2011; Rooth 2010).[35]

Finally, recall I conceptualized prejudice as having cognitive (e.g., stereotypes) and affective (e.g., dislike) components. Consistent with this, respondents' IAT score is significantly correlated with their explicit views of blacks' work ethic and hostility, as well as feelings of dislike or discomfort with blacks relative to whites.[36]

Control variables

Since I did not experimentally manipulate respondents' anti-black prejudice, I controlled for respondents' age (years) and gender (1= male) in analyses that include prejudice as a predictor. Prior research indicates gender and age predict implicit anti-black prejudice (Nosek et al. 2007). Results are robust to including additional control variables, including region and education (see Appendix D).

Analytic approach

To estimate the causal effect of referrals on applicants' evaluation score in each of the four experimental conditions, I used linear regressions of referral status on the evaluation score. Standard errors were clustered at the respondent level, since each respondent evaluated two applicants. I did not include controls, since respondents were randomly assigned to the experimental conditions, and applicants were randomly assigned to referrals.

Then, to examine the effect of respondents' *prejudice* on returns to referrals, I estimated linear regressions of respondents' prejudice, gender, and age, on returns to referrals. I estimated these regressions separately for black and white applicants, black and white employees, and for each of the four experimental conditions. Additionally, to examine whether respondents' prejudice has a differential effect across these characteristics (i.e., applicant race, referring employee race, and experimental condition), I estimated single models that include each predictor (prejudice, gender, and age), the characteristic of interest, and the interaction of each predictor with the characteristic. The interactions allow me to test for differences in the coefficients by the characteristic of interest.

Throughout, I illustrate the substantive effect of respondents' prejudice on returns to referrals by estimating the *predicted* returns to referrals ($\Delta\hat{y}$) of evaluators with the mean age (37 years old) and gender (57 percent women) of the sample, and varying IAT scores. In the text, I focus on the predicted responses of "highly-prejudiced" and "unprejudiced" evaluators. Highly-prejudiced evaluators have an IAT score one standard deviation above the mean and unprejudiced evaluators have an IAT score one standard deviation below the mean. These correspond to unstandardized IAT D scores of .8 and .1, indicating strong anti-black prejudice

[35] Since many IAT studies rely on lab experiments, critics have expressed skepticism of the test's ability to predict real hiring decisions (Tetlock and Mitchell 2009); although more research is needed, these studies provide strong evidence of the IAT's predictive validity in real workplaces. For other IAT critiques, see Tetlock and Mitchell (2009); for responses to these critiques, see Jost et al. (2009) and Quillian (2008).

[36] Based on questions in the initial survey, I created three scales: (1) stereotypes of hard work/competence, (2) hostility, and (3) affect. These scales were substantively ($r = .22, .28, .29$, respectively) and statistically ($p < .001$) significantly correlated with the IAT.

and no anti-black prejudice, respectively. I use the delta method to calculate the standard error of the predicted responses.

Results

Rewards to referrals, by experimental condition

Do hiring agents reward employee referrals? Figure 1 illustrates the mean difference in the evaluation score of referred and non-referred applicants, by experimental condition. In three out of the four experimental conditions, referrals had a large, positive, and statistically significant effect on applicants' evaluation scores. On average, white applicants' evaluation scores increased .67 standard deviations if they were referred by white employees ($p < .001$), and .53 standard deviations if they were referred by black employees ($p < .05$). Similarly, black applicants' evaluation score increased an average of .59 standard deviations if they were referred by white employees ($p < .01$). In contrast, black applicants' evaluation score only increased an insignificant .11 standard deviations if they were referred by black employees.

Thus, while hiring agents generally rewarded employee referrals, they did not reward black applicants' recommendations from black employees. As actual black applicants largely rely on same-race contacts (Mouw 2002), this finding identifies an important disadvantage faced by black jobseekers.

Respondent prejudice & applicant race

How does respondents' implicit anti-black prejudice shape these patterns? First, I assess whether respondents' prejudice differentially affects returns to employee referrals for black and white applicants. Hypothesis 1a states that more-prejudiced employers reward black applicants' referrals more than white applicants' referrals, relative to less-prejudiced employers. Hypothesis 1b states that more-prejudiced employers reward black applicants' referrals less than white applicants' referrals, relative to less-prejudiced employers.

Table 3.2 present results of linear regressions of respondents' prejudice, gender, and age, on returns to employee referrals, by applicant race. The right-most column presents the p-value of tests of differences in coefficients by applicant race. Hypothesis 1a implies the coefficient for respondents' prejudice should be significantly more positive for black applicants than for white applicants, while Hypothesis 1b implies it should be significantly more negative.

As respondents' anti-black prejudice increased, they rewarded black applicants' referrals *less* ($p < .05$) and white applicants' referrals *more* ($p < .1$). Prejudice differentially affected black and white applicants' returns to referrals ($p < .01$).[37] These results support Hypothesis 1b: more-prejudiced respondents are likelier to discount the referrals of black applicants, relative to white applicants, than less-prejudiced respondents.

The predicted returns to referrals indicate the effect of anti-black prejudice is substantively important. Highly-prejudiced evaluators strongly reward white applicants' referrals ($\Delta\hat{y} = .83;\ p < .001$), but do not reward black applicants' referrals ($\Delta\hat{y} = .05;\ p\ = .78$; p-

[37] Although not this study's focus, I found men rewarded the referrals of black applicants, relative to white applicants, more than women.

value of difference$< .01$). In contrast, unprejudiced evaluators reward black applicants' referrals more than white applicants' referrals ($\Delta\hat{y} = .65$ compared to $\Delta\hat{y} = .30$), but the difference is statistically insignificant ($p = .24$).

Respondent prejudice & referring employee race

How does respondents' prejudice affect how they reward referrals from black and white employees? Hypothesis 2 states that more-prejudiced employers reward referrals from black employees *less* than referrals from white employees, relative to less-prejudiced employers. Table 3.3 presents results of linear regressions of returns to employee referrals on anti-black prejudice, gender, and age, by referrer race. The right-most column presents the p-value of tests of differences in coefficients by referrer race. Hypothesis 2 implies the coefficient for respondents' prejudice should be significantly more negative for black employees than for white employees.

As respondents' anti-black prejudice increased, they rewarded white employees' recommendations *less* ($p < .05$) and insignificantly rewarded black employees' recommendations more; thus, referrals from black employees were increasingly rewarded relative to those from white employees (p-value of difference$< .05$).[38] This race difference is in the opposite direction of what I predicted. I investigate this finding in the next section.

To be clear, most of the sample rewarded referrals from white employees more than referrals from black employees. However, contrary to Hypothesis 2, white referrers' advantage is most pronounced among the *least* prejudiced respondents. Indeed, predicted returns to referrals indicate that unprejudiced evaluators strongly and significantly reward referrals from white employees ($\Delta\hat{y} = .95;\ p < .001$), but do not significantly reward referrals from black employees ($\Delta\hat{y} = .14;\ p = .5$; p-value of difference$< .01$). In contrast, highly-prejudiced evaluators do not significantly differentiate between referrals from black and white employees ($p = .3$).

Respondent prejudice & experimental condition

To investigate this unexpected result, I separately estimate the effect of anti-black prejudice on returns to referrals by experimental condition. This lets me assess whether the effect of respondents' prejudice on returns to the referrals from white and black employees is contingent on applicant race. Indeed, respondents may evaluate same-race referrals differently than cross-race referrals. Table 3.4 presents results of linear regressions of returns to employee referrals on anti-black prejudice, gender, and age, by experimental condition.

Respondents' prejudice had a negligible effect on returns to same-race referrals, but strongly affected returns to cross-race referrals. Relative to less-prejudiced respondents, more-prejudiced respondents rewarded black applicants' cross-race referrals *less* ($p < .05$) and white applicants' cross-race referrals *more* ($p < .05$).

Figure 2, which illustrates the predicted returns to referrals, demonstrates a few striking patterns. First, regardless of their prejudice, evaluators do not reward black applicants' same-race referrals (upper-left panel). Even when evaluated by the *least* prejudiced evaluators, black

[38] Additionally, relative to women, men rewarded white employees' recommendations more and black employees' recommendations less.

applicants do not significantly benefit from same-race referrals. Second, white applicants benefit from same–race referrals regardless of evaluators' prejudice (lower-right panel). Both unprejudiced ($\Delta\hat{y} = .72;\ p < .05$) and highly-prejudiced ($\Delta\hat{y} = .66;\ p < .05$) evaluators strongly reward white applicants' same-race referrals. This points to a widely shared bias among white evaluators against rewarding black applicants' same-race referrals, and is consistent with widespread skepticism about the credibility of black employees' recommendations on behalf of black applicants.[39]

Third, instead of the widely shared bias that characterizes white evaluators' response to same-race referrals, unprejudiced and highly-prejudiced evaluators respond differently to cross-race referrals (upper-right and lower-left panels). [40] While highly-prejudiced evaluators strongly reward white applicants' cross-race referrals ($\Delta\hat{y} = 1.18;\ p < .01$), they do not reward black applicants' cross-race referrals ($\Delta\hat{y} = -.06;\ p = .83$; p-value of difference$< .01$). In contrast, unprejudiced evaluators strongly reward black applicants' cross-race referrals ($\Delta\hat{y} = 1.01;\ p < .001$), but do not reward white applicants' cross-race referrals ($\Delta\hat{y} = .06;\ p = .86$; p-value of difference$< .05$). Consequently, black applicants only benefit from having a referral when two conditions are met: (1) the referrer is white and (2) the evaluator is relatively less-prejudiced. In contrast, white applicants overwhelmingly benefit from same-race referrals, and benefit from cross-race referrals as long as the evaluator is relatively prejudiced. [41]

How can we explain the differential effect of prejudice on returns to same-race and cross-race referrals? Overall, the strong effect of anti-black prejudice on returns to cross-race referrals, and its negligible effect on returns to same-race referrals, suggests respondents treat cross-race referrals as more ambiguous signals of applicant quality than same-race referrals. As cross-race referrals are rare, it is perhaps unsurprising there is less agreement about their interpretation. Should employers treat cross-race referrals as exceptionally strong, positive signals of applicant quality, since they may be more difficult for jobseekers to obtain? Or, should employers treat cross-race referrals as weak signals of applicant quality, since referring employees may be relatively uninformed about different-race applicants?[42]

I argue that respondents' attitudes towards the *jobseeker* shaped how they resolved this ambiguity: the more positively respondents felt towards a jobseeker based on his race, the more likely they were to interpret his cross-race referral as a credible signal of applicant quality. This argument relies on the fact that the IAT can be conceptualized as a measure of pro-white bias in addition to anti-black prejudice (DiTomaso 2015): since the IAT is a relative measure, high IAT

[39] Indeed, supplementary analyses indicate white applicants—but not black applicants—with same-race referrals were perceived as more hard-working ($p < .01$), capable ($p < .01$), and skilled ($p < .05$) than their non-referred counterparts. This suggests respondents interpret white applicants' (but not black applicants') same-race referrals as credible quality signals.
[40] Similarly, men and women differentially rewarded cross-race referrals. Compared to women, men rewarded black applicants' cross-race referrals more and white applicants' cross-race referrals less.
[41] The predicted returns to referrals imply that evaluators with anti-black prejudice up to .1 standard deviations above the mean significantly reward black applicants' cross-race referral, while evaluators with anti-black prejudice at or above .2 standard deviations below the mean significantly reward white applicants' cross-race referrals.
[42] Both interpretations are plausible. On the one hand, employees may have higher standards for recommending different-race ties, since people tend to be closer to—and thus more motivated to help (Kim and Fernandez 2017)—their same-race ties. On the other hand, since employees are generally better-informed about their stronger ties than their weaker ties (Marin 2012), cross-race referrals may be relatively uninformative about applicant quality (Brown et al. 2015, 163).

scores simply imply respondents have more positive and/or less negative associations with whites than with blacks. Thus, as respondents' pro-white bias (or anti-black prejudice) increased, they rewarded white applicants' cross-race referrals more and black applicants' cross-race referrals less.

Consistent with this argument, in supplementary analyses, I found respondents' perception of referred applicant quality (relative to non-referred applicant quality) largely mediated the effect of respondents' prejudice on returns to cross-race referrals.[43] The more positive associations respondents had with whites (blacks), the more they perceived white applicants (black applicants) with cross-race referrals to be more hard-working and competent than their non-referred counterparts. This suggests respondents' implicit prejudice indeed influenced their perception of the quality signal provided by cross-race referrals.

Finally, these results help explain why more-prejudiced respondents rewarded referrals from black employees, relative to white employees, more than less-prejudiced respondents. As anti-black prejudice had no effect on returns to same-race referrals, the unexpected finding was driven by cross-race referrals. Indeed, while I reported that referrals from white employees were discounted as respondents' prejudice increased, these results indicate that only white employees' recommendations *on behalf of black applicants* were discounted as respondents' prejudice increased. Similarly, only black employees' recommendations *on behalf of white applicants* were increasingly rewarded as respondents' prejudice increased. Thus, the unexpected finding is due to more-prejudiced respondents rewarding black applicants' cross-race referrals less, and white applicants' cross-race referrals more, than less-prejudiced respondents.

Limitations and Future Research

In interpreting the study findings, it is useful to remember that I focus exclusively on referring employees' *influence* over hiring agents' decisions. Thus, while I find black employees' recommendations do not influence hiring agents' evaluations of black jobseekers, black jobseekers may still obtain useful information—such as the best time to submit an application—from their same-race contacts. Additionally, I experimentally manipulated referral status, referrer race, and applicant race, but did not manipulate implicit prejudice. Thus, while I improve upon the great majority of IAT studies by measuring implicit prejudice prior to and separately from the evaluation task (see Fazio and Olson 2003), I cannot conclude that implicit prejudice has a *causal* effect on returns to referrals. Instead, I examined how implicit prejudice predicts returns to referrals at a later date, after the inclusion of control variables. By directly manipulating implicit prejudice (Todd et al. 2011), future research could establish the causal effect of implicit prejudice on returns to referrals.

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[43] To measure perceived applicant quality, I asked respondents to indicate on five-point scales (1 = not at all, 5 = extremely) the extent to which they expected the applicants to be hard-working, skilled, competent, and disciplined. I used exploratory factor analysis to construct the applicant quality scale; the retained factor has an eigenvalue of 2.7 and the minimum factor loading was .81. The within-respondent difference in the perceived quality of the referred applicant and the non-referred applicant largely mediated the effect of prejudice on returns to cross-race referrals, controlling for age and gender: it mediated 92% of the effect for black applicants' cross-race referrals ($p < .05$) and 60% of the effect for white applicants' cross-race referrals ($p < .1$). A limitation of this analysis is that respondents' assessments of applicant quality could represent a justification of—rather than a motivation for—their applicant evaluations. However, if justification fully accounted for the mediation, applicant quality should similarly mediate the effect of other respondent characteristics on returns to referrals, but this is not the case (analyses not shown).

Future research should also test the scope conditions of this study's findings, particularly with respect to black applicants' same-race referrals. Four scope conditions seem plausible. First, hiring agents in this study did not have relationships with the referring employees. This is consistent with the screening practices of medium and large firms with professional personnel departments (e.g., Fernandez et al. 2000; Fernandez and Fernandez-Mateo 2006), but is unlikely to be consistent with small firms' practices. It may be harder for hiring agents to dismiss recommendations of employees they know personally. Second, like most U.S. hiring agents (Smith 2002; Wodtke 2015), hiring agents in this study were white. Given their greater likelihood of hiring black applicants (Stoll, Raphael, and Holzer 2014), black hiring agents may be more likely to reward black applicants' same-race referrals. Finally, the applicants and referrers were men, and the job opening was for an assistant store manager. While audit studies suggest racial discrimination is comparable across occupations and for men and women (Quillian et al. 2016), these factors could possibly affect racial variation in returns to referrals.

While the latter three conditions could be tested with experiments, it seems more prudent to test the first condition with firm data than to attempt to manipulate the strength of the relationship between respondents and hypothetical referrers. Additionally, firm data would allow researchers to assess the extent to which this study's findings hold in the context of real workplaces. I limited study participation to individuals with hiring responsibilities in their workplace, and told participants they were evaluating real applicants for an existing job opening. Nevertheless, hiring agents' behavior could differ when they evaluate applicants as part of their routine hiring process, and could be affected by organizational practices. Admittedly, finding adequate observational data will be challenging, as even the best firm studies have limited information about firms' use of referrals (e.g., Fernandez et al. 2000; Petersen et al. 2000).[44]

Discussion and Conclusion

Instead of focusing on differences in black and white applicants' accessed and mobilized network resources, which has been well-studied in the literature (e.g., McDonald 2011; Royster 2003), I focus on differences in how employers reward these resources. Drawing on an original experiment with a sample of white hiring agents in the United States, I find that even black applicants who successfully access and mobilize equivalent network resources to whites face a *return disadvantage*. In the most prevalent real-life conditions—black applicants referred by black employees, and white applicants referred by white employees—black applicants' referrals were significantly discounted relative to white applicants' referrals. Indeed, black applicants *only* benefited from having a referral when two conditions were met: the referring employee was white and the hiring agent was relatively low-prejudiced. In contrast, white applicants overwhelmingly benefited from their same-race referrals, and benefited from black employees' recommendations as long as they were evaluated by relatively prejudiced hiring agents. Given black jobseekers' tendency to rely on same-race job contacts (Mouw 2002), these findings suggest black jobseekers are frequently unable to benefit from a key network resource: referrers' influence over hiring agents' decision-making. Thus, this study identifies employers' differential response to black and white jobseekers' same-race referrals as a contributor to racial inequality in the labor market.

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[44] For instance, only one study has collected data on the race of both applicants and referrers (Fernandez and Fernandez-Mateo 2006).

These findings also point to a *widely shared bias* among white hiring agents against rewarding black applicants' same-race referrals: both unprejudiced and highly-prejudiced evaluators strongly reward whites' same-race referrals, but do not reward blacks' same-race referrals. In contrast, highly-prejudiced and unprejudiced hiring agents differ in their evaluations of cross-race referrals, suggesting cross-race referrals are perceived as more ambiguous signals of applicant quality. Respondents' attitudes towards the applicants, based on the applicants' race, appear to determine how they resolve this ambiguity. Indeed, as respondents' anti-black prejudice (or pro-white bias) increased, they increasingly discounted black applicants' cross-race referrals and increasingly rewarded white applicants' cross-race referrals. While some approaches to racial bias focus on shared cultural beliefs (e.g., Correll and Ridgeway 2003), and others focus on individual variation in bias (e.g., Stepanikova et al. 2011), these findings highlight the utility of combining these approaches.

To my knowledge, this is the first study to experimentally establish that hiring agents reward employee referrals. Despite important methodological advances, firm studies have not established the causal effect of employee recommendations on hiring decisions. Fernandez and Galperin (2014) improve upon prior firm studies by focusing on repeat applicants to the same firm: the same applicants were more likely to be interviewed or offered a job when they applied with a referral, than without a referral. This alleviates concerns about unobserved, time-invariant differences between non-referred and referred applicants. Yet, as the authors note, it is unclear whether the findings reflect referrers' influence over employers' decision-making or information referring employees provided jobseekers (e.g., which job opening is most appropriate). This is also the first study to examine the role of referring employee race and employer prejudice on returns to referrals.

This study has important implications for understanding the causes and consequences of social capital inequality. Social capital scholarship has emphasized black jobseekers' limited access to high-status contacts, who are expected to be more knowledgeable and influential than lower-status contacts (Lin 2001). From this perspective, unequal access to white contacts matters because white employees occupy higher-status positions than black employees, not because contact race is itself consequential (Son and Lin 2012, 602). This study contradicts this reasoning: although black and white employees were identical except for their name, white hiring agents only rewarded black applicants' recommendations from white employees. This provides compelling evidence that for black jobseekers, the race of their referrer is not simply *associated* with workplace resources, but serves as a resource *in itself*. This suggests racially-segregated networks contribute to social capital inequality by leading to unequal access to white contacts, in addition to unequal access to high-status contacts.

This study also implies that while black jobseekers' difficulty mobilizing their social resources may limit their social capital, this limitation may be less detrimental to their employment outcomes than previously thought. Black jobseekers' mobilization difficulties are most pronounced for proactive assistance, such as obtaining a referral (Smith 2010). Indeed, black workers receive just as many job leads from their networks as white male workers, but their job contacts are less likely to "put in a good word" for them (McDonald 2011). Yet, while it seems straightforward to assume that vouching for a jobseeker helps the jobseeker, this is only the case if employers respond positively to the "good word" put on jobseekers' behalf. Thus, the finding that employers do not reward black applicants' same-race referrals suggests that black jobseekers' difficulties mobilizing their same-race contacts may have limited effect on their employment outcomes.

Finally, this study suggests employers' differential response to jobseekers' social resources should be central to the study of social capital inequality, and identifies three factors that together influence this response: applicant race, referrer race, and employer racial prejudice. Prior to this study, employers' differential response to black and white jobseekers' social resources had been recognized as possible (Lin 2001), but never demonstrated. Further, I argue that that to theorize variation in employers' response to referrals, we need to first theorize variation in employers' perception of the credibility of referrals. Ultimately, given employers' racial biases, understanding jobseekers' ability to access and mobilize social resources is not enough; we need to understand how employers *respond* to those resources.

Table 3.1 Characteristics of Respondents (N = 226)

% white, non-Hispanic	100.0
% hiring responsibility	100.0
% working full-time	77.4
% supervisor	67.3
% self-employed	13.3
% female	56.6
% foreign-born	4.0
(Mean, SD) Years of age	(36.6, 11.1)
(Mean, SD) Years of education	(15.6, 1.8)
Anti-black prejudice (Implicit Association Test D-score)	
% None ($D < .15$)	20.4
% Slight ($-.15 \leq D < .35$)	14.5
% Moderate ($.35 \leq D < .65$)	35.8
% Strong ($D \geq .65$)	29.4
Individual earnings	
% Under $20,000	16.4
% $20,000 to $34,999	20.4
% $35,000 to $49,999	23.0
% $50,000 to $74,999	27.0
% $75,000 to $99,999	7.1
% $100,000 or above	6.2
Region	
% Northeast	20.8
% Midwest	26.6
% South	38.1
% West	14.6
Establishment size	
% Under 25	46.0
% 25-99	22.1
% 100-499	12.4
% 500 or more	19.5

Note: There were 221 respondents for the IAT.

Table 3.2 Return to employee referrals, by applicant race (OLS regressions)

	Black applicants	White applicants	P-Value of difference
Anti-black prejudice	-0.30*	0.27+	0.01
	(0.14)	(0.16)	
Age (years)	0.01	0.01	0.82
	(0.01)	(0.01)	
Male	0.46	-0.45	0.03
	(0.30)	(0.30)	
Constant	0.16	0.75**	
	(0.20)	(0.18)	
Respondents (N)	117	104	
R^2	0.05	0.05	

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. The left and middle column present OLS coefficients with standard errors in parentheses. These regressions are estimated separately for black and white applicants. The right-most column presents the p-value of tests of difference of each coefficient, by applicant race, estimated using a single model with the three predictors (prejudice, age, gender), applicant race, and the interaction of each predictor with applicant race. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.
$+ p < .1; * p < .05; ** p < .01$ (two-tailed tests).

Table 3.3 Return to employee referrals, by referrer race (OLS regressions)

	Black referrer	White referrer	P-Value of difference
Anti-black prejudice	0.16	-0.32*	0.03
	(0.14)	(0.15)	
Age (years)	0.00	0.02	0.37
	(0.01)	(0.01)	
Male	-0.65*	0.62*	0.00
	(0.30)	(0.29)	
Constant	0.57**	0.37*	
	(0.20)	(0.18)	
Respondents (N)	94	127	
R^2	0.06	0.07	

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. The left and middle column present OLS coefficients with standard errors in parentheses. These regressions are estimated separately for black and white referrers. The right-most column presents the p-value of tests of difference of each coefficient, by referrer race, estimated using a single model with the three predictors (prejudice, age, gender), referrer race, and the interaction of each predictor with referrer race. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.
$+ p < .1$; $* p < .05$; $** p < .01$ (two-tailed tests).

Table 3.4 Return to employee referrals, by experimental condition (OLS regressions)

	Black applicants		White applicants	
	Black referrer	White referrer	Black referrer	White referrer
Anti-black prejudice	-0.07[ab]	-0.54*[bcd]	0.56*[acd]	-0.03[ab]
	(0.19)	(0.21)	(0.22)	(0.23)
Age (years)	0.01	0.01	-0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Male	-0.49[a]	1.12**[bcd]	-0.72+[a]	-0.01[a]
	(0.42)	(0.40)	(0.42)	(0.44)
Constant	0.31	0.00	0.93**	0.68**
	(0.27)	(0.28)	(0.27)	(0.24)
Respondents (N)	52	65	42	62
R^2	0.03	0.18	0.22	0.03

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. OLS coefficients with standard errors in parentheses. The regressions are estimated separately for each experimental condition. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.
[a] $p < .1$; coefficient is significantly different from the black applicant/white referrer coefficient (two-tailed tests).
[b] $p < .1$; coefficient is significantly different from the white applicant/black referrer coefficient (two-tailed tests).
[c] $p < .1$; coefficient is significantly different from the white applicant/white referrer coefficient (two-tailed tests).
[d] $p < .1$; coefficient is significantly different from the black applicant/black referrer coefficient (two-tailed tests).
$+p < .1$; $*\ p < .05$; $**\ p < .01$ (two-tailed tests).

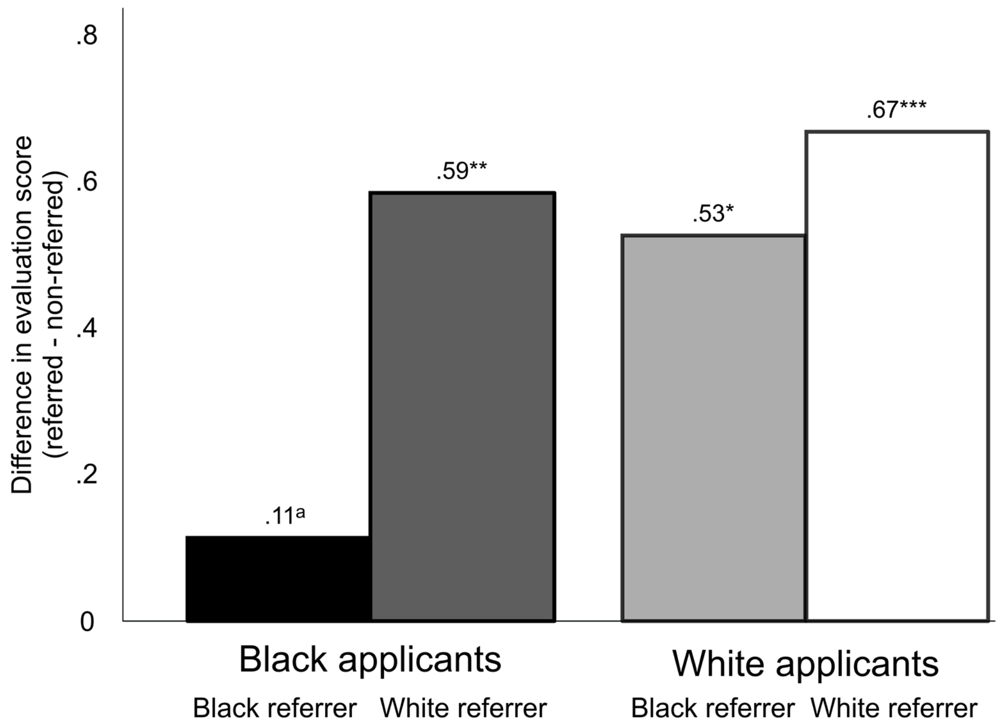

Figure 3.1 Effect of referral status on evaluation score, by experimental condition.

Note: Statistical tests based on linear regressions of referral status on the evaluation store, with standard errors clustered at the respondent level. The evaluation score is standardized.
[a] $p < .05$; regression coefficient is significantly different from the white applicant/white referrer coefficient (two-tailed tests).
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

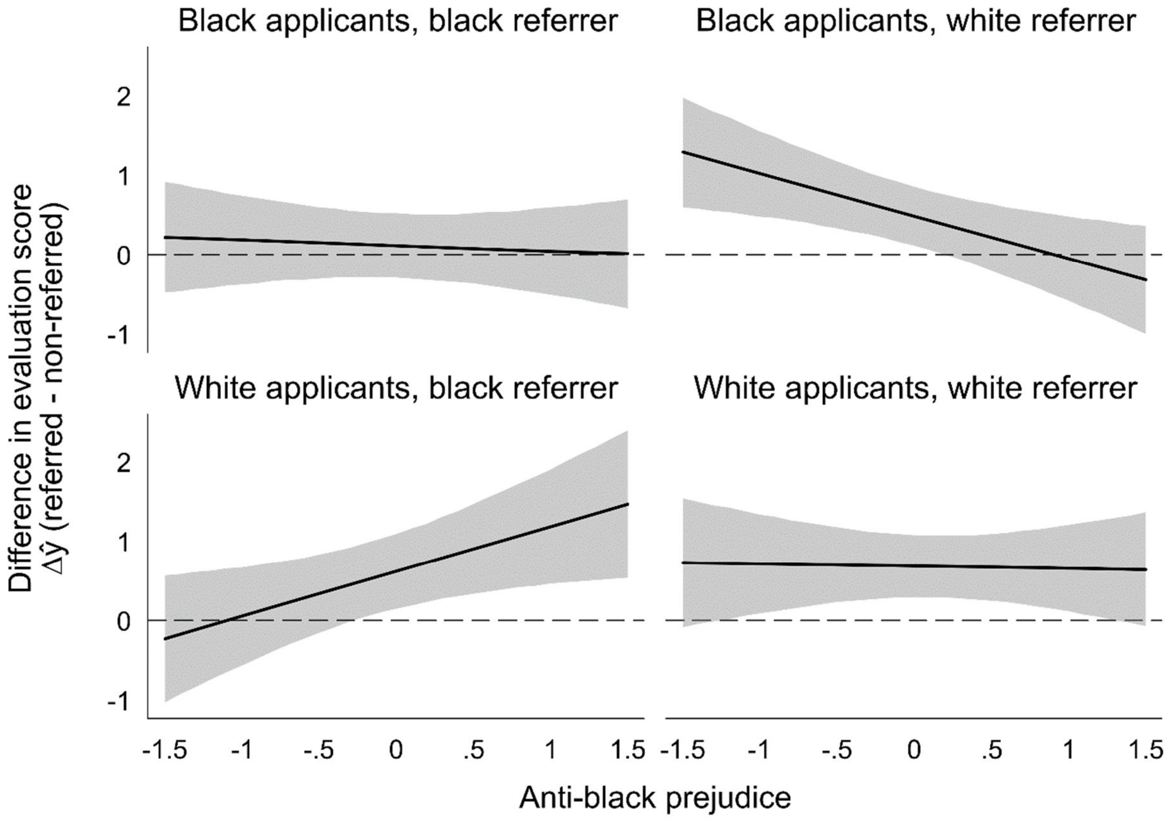

Figure 3.2 Effect of evaluators' anti-black prejudice on returns to employee referrals, by experimental condition.

Note: The solid black line is the predicted return to employee referral ($\Delta\hat{y}$) and the shaded region is the 95% confidence interval, calculated using the delta method. A zero on the y-axis indicates there is no difference in the predicted evaluation score of the referred and the non-referred applicant. Both the evaluation score and the anti-black prejudice measure are standardized. I keep age and gender at their mean values in the sample. Estimates derived from a single model with three predictors (prejudice, age, gender), experimental condition dummies, and the interaction of each predictor with the experimental condition dummies. See Table 3.4.

CHAPTER 4
Generating labor market inequalities:
Family background, employment histories, and earnings disparities

In the United States, parents pass on a substantial amount of their economic advantage to their children. For instance, approximately half of parental income differences are passed on to children (Mazumder 2005; Mitnik, Bryant, Weber, and Grusky 2015). Understanding the mechanisms behind this intergenerational transmission of economic status is essential to identifying the barriers to adult economic success faced by people from less-advantaged backgrounds. How does family background constrain and enable economic success?

While sociologists have long been interested in explaining how parents pass on their economic advantage (e.g., Blau and Duncan 1967; Sewell, Haller, and Portes 1969), they have overwhelmingly focused on the effect of family background on children's pre-labor market outcomes. In particular, an extensive literature documents the robust effect of family background on children's educational success (e.g., Gamoran 2001; Shavit and Blossfeld 1993). More-advantaged children attain higher test scores, take more advanced courses, attend higher-quality high-schools and more selective colleges, and graduate from college at higher rates than their less-advantaged peers (Campbell, Hombo, and Mazzeo 2000; Lucas 2001; Bailey and Dynarski 2011). Yet, key pre-labor market factors—including educational attainment, school quality, and cognitive achievement—appear to account for at *most* half of the intergenerational transmission of income and earnings (Bowles, Gintis, and Osborne 2005: 4, 18).

Instead, I examine the role of *labor market* processes—specifically, employment histories—in explaining the intergenerational transmission of economic status. While workers usually earn relatively low wages at the start of their career, earnings disparities increase as differences in work experience, job tenure, and unemployment spells accumulate (e.g., Cheng 2014; Tomaskovic-Devey, Thomas, and Johnson 2005). Employment histories are important determinants of earnings (e.g., Fuller 2008; Gangl 2006), as they affect the accumulation of on-the-job human capital and provide signals to prospective employers about workers' competence, motivation, and commitment.

To my knowledge, no study has systematically examined the effect of employment histories in explaining the intergenerational transmission of economic status in the U.S. This is surprising since there are compelling reasons—including employer discrimination and differences in economic, social, and cultural resources—to expect individuals from more-advantaged backgrounds to attain more work experience and tenure, and less unemployment, over the course of their careers than their less-advantaged peers (e.g., Armstrong and Hamilton 2013; Lin 1981; Rivera and Tilcsik 2016). Further, in the domains of gender and race, a robust literature finds employment histories indeed play an important role in explaining earnings disparities (e.g., Antecol and Bedard 2004; Tienda and Stier 1996).

Drawing on rich panel data from the National Longitudinal Survey of Youth 1979 (NLSY79), including detailed week-by-week measures of work experiences, I examine two main research questions: 1) How does parental income shape men's employment histories? and 2) To what extent do employment histories mediate the association between parental income and earnings? I examine these questions separately by level of educational attainment, because previous research finds the intergenerational transmission of economic status (Hout 1988; Torche 2011) and the stability of employment histories (Alon and Haberfeld 2007; Klerman and Karoly 1994) vary by education level. I restrict the sample to men due to my interest in labor market processes. For women, non-labor market factors—especially marriage and spouse's earnings conditional on marriage—account for much of intergenerational transmission of economic advantage (Harding et al. 2005; Mitnik et al. 2015).[45] I focus on parental income and earnings since broader measures—such as occupational status—obscure much of the intergenerational transmission of economic status (Laurison and Friedman 2016; Witteveen and Attewell 2017).

I find parental income has a strong effect on the employment histories of men without a college degree. Among this group, men from higher-income families accumulate more work experience and tenure, and less unemployment, throughout their careers than men from lower-income families. Further, higher parental income is associated with a faster transition to stable employment for men with at most a high-school education, thus reducing the "churning" that characterizes the early labor market years of less-educated men (Klerman and Karoly 1994). In contrast, regardless of parental income, college graduates quickly settle into stable, long-term employment. Thus, for the purposes of quickly finding employment, especially long-term employment, a college degree appears to be a powerful resource that leaves little room for family background effects.

Consequently, for non-college graduates (but not for college graduates), employment histories significantly mediate the association between parental income and earnings. Conditioning on pre-labor market factors, employment histories reduce the association between parental income and earnings by 28% for men with at most a high-school education and 36% for men with some college education. Overall, employment histories appear to play a critical role in explaining the intergenerational transmission of economic advantage among non-college graduates.

Employment histories, earnings disparities, and family background

There are compelling theoretical reasons to expect employment histories to affect earnings. From the perspective of standard human capital theory (Becker 1993), work experience contributes to workers' general and specific human capital. General human capital represents capabilities that are broadly useful to employers. These capabilities can be acquired through training prior to labor force entry, such as through formal education, but also through workplace experience. For instance, workers that frequently coordinate with others in their workplace may improve their coordination skills. Specific human capital refers to capabilities that are useful to a specific employer, occupation, or industry, such as knowledge of firm-specific practices or products. Given a competitive labor market, employers are expected to financially reward workers' human capital. Consequently, periods of nonemployment can depress earnings as individuals do not accumulate additional human capital and as their existing human capital depreciates. Additionally, since prospective employers are not expected to reward human capital that is specific to a previous employer, workers with extensive firm-specific human capital may suffer considerable earnings losses from unemployment (Topel 1991). Furthermore, individuals experiencing long-term unemployment may broaden their job search to seek employment in occupations or industries where they lack experience—this implies a further loss of occupation and industry-specific human capital (Jacobson, LaLonde, and Sullivan 1993; Neal 1995).

[45] Indeed, while the intergenerational transmission of income is comparable for men and women, the intergenerational transmission of earnings and occupational status is much weaker for women than for men (Mitknik et al. 2015; Torche 2011).

From the perspective of labor market signaling (Spence 1973, 2002), employment histories are also expected to affect earnings. In particular, employers may view jobseekers with unstable work histories or long periods of unemployment as lacking competence, motivation and commitment to the workplace. Consequently, applicants with stable work histories are likely to receive more job offers and to be offered higher starting wages. Furthermore, if employers expect applicants with stable work histories to be more loyal and committed to the firm, they will be more likely to invest in them and to hire them for jobs with the potential for higher earnings growth. This suggests that differences in accumulated work experiences can lead to cumulative earnings disadvantage, as workers' future career prospects with a given employer depend on their starting position in a company (e.g., DiPrete and Eirich 2006; Rosenfeld 1992).

Consequently, if family background shapes employment histories, employment histories may play an important role in explaining the intergenerational transmission of economic status. There are several reasons to expect people from more socioeconomically advantaged backgrounds to achieve more stable, long-term employment histories than their less-advantaged counterparts. First, individuals from more-advantaged backgrounds are likely to have more material resources, in the form of parental gifts or loans, than their less-advantaged peers (Armstrong and Hamilton 2013; Hout 1984). Material resources, such as a car, can enable jobseekers to search for employment more intensively and can also facilitate a move to a new location to seek job opportunities. Second, jobseekers may benefit from their parents' social networks (Armstrong and Hamilton 2013; Lin et al. 1981). Indeed, approximately half of U.S. workers find employment through job contacts (Granovetter 1995; Marsden and Gorman 2001). In addition to being higher in status, the networks of higher-status people are expected to have greater breadth (Lin 2002), suggesting that higher-status parents will be able to provide their children with contacts that are higher-status *and* more relevant than lower-status parents.

Third, employers may be more likely to hire candidates from more-advantaged backgrounds. In particular, employers may prefer specific interactional styles, manners of speaking and dressing, and extracurricular activities associated with higher-class backgrounds (e.g., Erikson and Goldthorpe 2002; Neckerman and Kirschenman 1991; Rivera 2015b). These employer preferences are often discussed in the context of high-status employment (Laurison and Friedman 2016; Rivera 2015b), where individuals from higher-class backgrounds are perceived to better "fit" the elite organizational culture (Rivera and Tilcsik 2016). Yet hiring agents in the broader labor market also express preferences for interactional styles associated with more-advantaged backgrounds, such as speaking expressively and articulately (Kennelly 1999; Neckerman and Kirschenman 1991: 441). Moreover, people from more-advantaged backgrounds are generally perceived to be more competent than less-advantaged individuals (Fiske et al. 2012). Together, these arguments suggest more-advantaged individuals are more likely to experience shorter unemployment spells and to find suitable long-term employment quickly, than their-less advantaged peers.

Data, Measures, and Methods

The National Longitudinal Survey of Youth (NLSY79)

To study the effect of parental income on employment histories and earnings, I analyzed data from the 1979 to 2014 waves of the National Longitudinal Survey of Youth 1979 (hereafter, NLSY79). The NLSY79 is a nationally representative panel study of 12,686 young men and women, aged 14-22 years old when they were first surveyed in 1979. The sample was interviewed annually through 1994, and biennially thereafter. The NLSY79 is well-suited to this study's purposes, since it includes detailed week-by-week information about respondents' employment experiences. Thus, instead of solely relying on an age-education proxy of potential work experience, I can precisely measure respondents' *actual* work experiences. NLSY79 also includes a parental income measure, answered by the parents, and a measure of cognitive skill. Further, the NLSY79's long duration allows me to track workers far into their career: respondents in my sample are 50-55 years old in the most recent wave. The NLSY79 is frequently used to examine intergenerational transmission of economic status (e.g., Osborne Groves 2005; Torche 2011), to characterize employment histories (e.g., Damaske and Frech 2016; Tomaskovic-Devey et al. 2005), and to study earnings disparities (e.g., Cheng 2016; Western 2002).

Sample restrictions. As discussed above, this study is limited to men. I exclude observations prior to adult labor market entry and from full-time students, because the corresponding wage reports are poor predictors of respondents' long-term earnings potential (Killewald and Lundberg 2017). Since entry into the labor market varies by educational attainment, I operationalize adult labor market entry as follows: 17 for respondents with a high school education or less, 18 for respondents with a high school education, 21 for respondents with some college education, 23 for respondents with a college education, and 25 for respondents with more than a college education. I also restrict the sample to respondents living in the parental home in 1979, since parental income is only measured for these respondents. Further, since many young people leave their parents' home after completing high school, older respondents who live in the parental home are a selected sample. To avoid this selectivity bias, I limit the sample to respondents who were 18 years old or younger in 1979. This age restriction also minimizes problems caused by left censoring of labor force participation (see Neal and Johnson 1996). After these sample restrictions and listwise deletion, the analytic samples for earnings models contain 25,502 person-year observations for 1,995 men with at most a high-school degree, 6,554 person-year observations for 670 men with some college education, and 5,442 person-year observations for 476 men with at least a college degree.

Measures

Table 4.1 presents descriptive statistics for the sample and Table 4.2 presents correlations for the study variables, by level of educational attainment. The measures in the table are described below.

Parental income. Parental income refers to total family income during 1978, reported in 1979. It is only measured for respondents living in the parental home during the first wave of the NLSY79. To increase the accuracy of the report, parental income is reported by the respondent's parent. To protect respondents' privacy, the NLSY79 truncates parental income at $75,000; this is equivalent to $272,000 in 2014 dollars and affects less than one percent of the parental income observations.

Hourly wages. The dependent variable in the earnings models is the log of hourly wages in the respondent's current job.[46] I adjusted all wages to 2014 dollars using the Bureau of Labor Statistics Consumer Price Index. To reduce the effect of outliers, I bottom-coded wages at $1 and top-coded wages at $100. This recoding affected approximately one percent of the hourly wage observations.

Employment histories. I use several variables to capture employment histories. These variables are the dependent variables in the models of employment histories, and predictors in the earnings models. *Cumulative work experience* is the cumulative number of weeks a respondent has worked since entering the adult labor market. *Job tenure* is the number of weeks a respondent has worked in his current job. *Recent unemployment* is the number of weeks a respondent spent unemployed in the previous calendar year. *Cumulative unemployment* is the cumulative number of weeks a respondent has spent unemployed since entering the adult labor market, minus the previous year unemployment. I include both recent and cumulative unemployment since the direct effect of unemployment stigma on hiring is limited to recent unemployment spells (Eriksson and Rooth 2014). Cumulative unemployment captures the well-established long-term effect of previous unemployment on earnings, likely reflecting both the long-term indirect effect of unemployment stigma and the loss of firm-specific human capital (Gangl 2006). In the earnings models, I further include square terms for cumulative experience and tenure, as these measures have a declining effect on earnings at higher levels (Gangl and Ziefle 2009). Since I am including their square terms, I also mean-center cumulative experience and tenure in these models to reduce multicollinearity.

Additional measures. In all models, I control for potential experience and its square term. *Potential experience* is operationalized as age minus adult labor market entry. I center potential experience (at 12 years) to reduce multicollinearity. Potential experience differs from cumulative work experience in that it measures the length of time since entry into the adult labor market, regardless of the actual amount of time a respondent has worked since labor market entry. I measure *race/ethnicity* as classified by the NLSY79 during household screening: non-Hispanic white or other, non-Hispanic black, and Hispanic. To account for pre-labor market skills, I control for *cognitive skill* and *education.* Cognitive skill is the age-adjusted percentile on the Armed Forces Qualification Test (AFQT). It is measured prior to adult labor market entry, and consequently does not reflect skills learned in the adult labor market. Since I conduct analyses separately by education group—high school or less, some college, college graduate or more—I only control for high school degree (in the first group) and post-graduate education (in the third group). I also include indicators for marital status (*married*) and for whether a respondent is currently enrolled in school (*enrolled*). To account for geographic differences in earnings and employment opportunities, I control for region and urban location. *Region* is a series of dummy variables indicating residence in the North, Midwest, West, and South. *Urban* is a dummy variable indicating residence in an urban (versus rural) location. Finally, I control for occupational earnings in the earnings analysis. *Occupational earnings* is the percentage of

[46] To be precise, by "current job" I mean the current or most recent job since the last interview. For respondents with more than one job, the "current job" is the job in which the respondent worked the most hours during the survey week. In the case of a tie, I selected the job in which the respondent had the longest tenure.

workers in occupations with lower standardized median earnings than the respondent's occupation. The NLSY79 codes occupation using the 1970 census codes until 2000, and the 2000 census codes thereafter. To code occupational earnings, I used the IPUMS crosswalk to convert these into 1990 census codes, and then constructed measures of occupational earnings based on the 1990 codes. In preliminary analysis, I established that the occupational earnings measure better fits the data than two standard measures of occupational status: Duncan's (1961) Socioeconomic Index and Hauser and Warren's (1997) occupational education formulation. The mediating effect of employment histories on the association between parental income and adult child earnings is not affected by the choice of occupational measure.

Analytic approach

Modeling employment histories

I examined the effect of parental income on four measures of employment history: cumulative work experience, cumulative unemployment, job tenure, and recent unemployment. Cumulative work experience, cumulative unemployment, and job tenure are necessarily accumulated over time. Regardless of parental income, individuals do not enter the labor market with extensive work experiences, histories of unemployment, or long tenure with an employer. Thus, I am interested in how parental income affects the *growth* of these outcomes over time. Does an additional year of potential work experience translate into more actual work experience for individuals with higher parental income? To examine how parental income is associated with the growth of cumulative work experience, cumulative unemployment, and job tenure, I use fixed-effects models and interact parental income with potential experience and its square term. The interactions allow me to test for differences in the effect of potential experience by parental income. By using fixed-effects models, I am able to compare the extent of work experience, unemployment, and job tenure of the same individual at different points in time.

In contrast, the overall level of recent unemployment—not simply its change over time—is essential to understanding employment histories. Compare a worker who experiences two months of recent unemployment every year to a worker who experiences a week of recent unemployment every year. Clearly, while neither of these workers experience a change in recent unemployment over time, the difference in their overall level of recent unemployment is likely relevant to explaining differences in their earnings. Thus, I examine the main effect of parental income on recent unemployment, as well as its effect on change in recent unemployment over time. Since I am interested in the main effect of parental income on recent unemployment, and parental income does not vary within-respondent, I cannot estimate fixed-effects models. Instead, I use linear regressions to model recent unemployment (since it is an interval variable). In addition to the main effect of parental income, I include the interaction of parental income with potential experience and its square term to examine how parental income affects change in recent unemployment over time.

In all the employment history models, I include controls for potential experience and its square term. I also include controls for the interaction of the race/ethnicity indicators (i.e., black, Hispanic) and potential experience and its square term; and the interaction of cognitive skill with potential experience and its square term. Additionally, I control for the time-varying control variables: marital status, school enrollment, education level, geographic region, and urban location. In the recent unemployment models, I additionally control for the main effect of race and cognitive skill. Since respondents contribute multiple observations, I clustered standard errors at the respondent level in all the analyses.

Modeling earnings

To model log hourly wages, I use linear regressions with standard errors clustered at the respondent level. For each education category, I present four nested linear regressions. To test whether the addition of variables to each successive model significantly changes the parental income coefficient, I use the results of seemingly unrelated regressions (SUR) with bootstrapped standard errors. SUR allows for a correlated error structure across the models.

Model 1 includes parental income, race/ethnicity indicators, potential experience and its square term, cognitive skill, education, and survey year indicators as predictors. Since cognitive skill was measured in high school, these represent pre-labor market characteristics and help me establish a baseline association between parental income and hourly wages after accounting for the pre-labor market characteristics that have been the focus of much previous research. Model 2 adds contextual controls: marital status, school enrollment, geographic region, and urban location. Model 3 adds employment histories: cumulative work experience and its square term, cumulative unemployment, recent unemployment, and job tenure and its square term. Model 4 adds the occupational earnings measure. I add the occupational earnings measure to assess an additional labor market process—allocative inequality—that may account for the intergenerational transmission of economic status. Allocative inequality refers to differences in the allocation of groups to higher-paying or higher-status occupations (Petersen and Saporta 2004). This lets me compare the relative effect of employment histories and allocative inequality, as well as to examine whether the effect of employment histories is spurious and instead reflects occupation allocation (e.g., perhaps individuals remain longer in jobs at higher-paying occupations). Finally, I do not include interactions between parental income and the quadratic of potential experience in any of the earnings models, as I am interested in assessing the change in the *overall* association between parental income and earnings due to the inclusion of employment history variables.

Results

Models of employment histories

Table 4.3 presents estimates from models of employment histories for each of the three education categories: high school or less, some college, and college or more. Because the models include interaction terms and squared terms, interpreting the regression coefficients requires some care. The baseline individual in these models has the sample mean years of potential experience (12 years), cognitive skill (42nd percentile), and parental income ($58,994), and is non-black/non-Hispanic. Consequently, the sum of the coefficients for potential experience and its square term indicates the predicted change in the outcome associated with a one-year increase in potential experience for baseline individuals. Additionally, for men with the mean sample potential experience, the sum of the coefficients of the parental income × potential experience interactions is the *difference* in the predicted effect of a one-year increase in potential experience associated with a 1 SD increase in parental income. Thus, for instance, for baseline individuals with at most a high-school degree, a one-year increase in potential experience is associated with an additional 3.7 weeks of unemployment ($3.777 - .100 = 3.677$). Further, for these individuals, a 1 SD increase in parental income reduces the predicted effect of one year of potential experience on unemployment by .7 weeks, from 3.7 weeks to 3 weeks ($-.718 + .025 = -.693$).

As expected, the fixed-effects models indicate that as men gain potential experience, they accumulate additional weeks worked, weeks unemployed, and tenure with the same employer. More interestingly, potential experience translates into more actual experience for higher-educated individuals. For instance, among men with 12 years of potential experience, college-educated men are predicted to work 5 more weeks, spend 2 fewer weeks unemployed, and gain 6 more weeks of job tenure in the following year relative to men with at most a high-school degree. This education difference is most pronounced upon labor market entry: college-educated men are predicted to work 6 more weeks, spend 4 fewer weeks unemployed, and gain 9 additional weeks of job tenure in their first year in the labor market than men with at most a high-school degree.

The greater disparity upon labor market entry occurs because male college graduates steadily accumulate work experience, unemployment, and job tenure over time, but the rate of acquisition for less-educated men changes over time. Specifically, as indicated by the coefficients for the squared potential experience term, men with at most a high-school degree accumulate more work experience, less unemployment, and more job tenure per year at higher levels of potential experience. Similarly, men with only some college accumulate less unemployment per year at higher levels of potential experience. In contrast, as indicated by the substantively and statistically insignificant coefficients for the squared potential experience terms for college graduates, college graduates accumulate work experience, unemployment, and tenure at similar rates throughout their careers. The findings for recent unemployment reveals a similar pattern: for men without a college degree, the length of recent unemployment declines (at a slowing rate), while the length of recent unemployment spells remains constant for college graduates.

Together, these findings point to a period of *transition* to stable, long-term employment for men with a high-school education or less, and to elevated unemployment upon labor market entry for men without college degrees. In contrast, college graduates appear to quickly settle into stable, long-term employment upon labor market entry. For instance, white male college graduates with the mean cognitive skill and parental income in the sample are predicted to accumulate 48 weeks of work, only 2 weeks of unemployment, and 22 weeks of tenure their first year in the labor market.[47] Furthermore, even after acquiring extensive labor market experience, less-educated are still predicted to accumulate less work experience, more unemployment, and less tenure than college graduates per additional year of potential experience. Overall, these findings suggest that relative to college graduates, less-educated men face important barriers to achieving stable, long-term employment histories. Consequently, I expect family background—operationalized as parental income—to be more influential in helping non-college graduates than college graduates achieve stable, long-term employment histories.

How does parental income affect employment histories? Among non-college graduates, I find that potential experience translates into more actual experience for men with higher parental

[47] While the exact predictions vary by race and cognitive skill, overall all predictions point to high rates of stable, long-term employment upon labor market entry for male college graduates.

income. For men with at most a high-school degree, parental income significantly moderates the joint effect of linear and squared potential experience on cumulative work experience ($F(1, 2057) = 13.46, p < .001$), cumulative unemployment ($F(1, 2057) = 24.47, p < .001$), and job tenure ($F(1, 2023) = 5.62, p < .05$); and has a significant negative main effect on recent unemployment ($B = -.44, p < .001$). Further, parental income most strongly affects the accumulation of unemployment and weeks worked at labor market entry, but has a steady effect on tenure throughout the career. For instance, a 1 SD increase in parental income is associated with an additional 2.1 weeks worked and 1.3 fewer weeks unemployed for men with at most a high-school education their first year in the labor market, but only 1 additional week worked and .1 fewer weeks unemployed their 20th year in the labor market. Thus, among men with a high-school education or less, higher parental income is associated with a faster transition to stable employment, as well as longer-term employment throughout their careers.

For men with some college, parental income also significantly moderates the joint effect of linear and squared potential experience on cumulative unemployment ($F(1,689) = 5.70, p < .05$) and has a significant negative main effect on recent unemployment ($B = -.42, p < .05$). Further, while parental income does not significantly moderate the joint effect of potential experience on cumulative work experience ($F(1, 689) = 1.95, p = .16$) or job tenure ($F(1,671) = 2.69, p = .10$) at conventional levels of statistical significance, the moderating effect for job tenure is marginally significant and substantively important. I do not find that the moderating effect of parental income changes over time: higher parental income is similarly associated with less unemployment and longer job tenure throughout the careers of men with some college.

In contrast, for college graduates, parental income does not significantly moderate the joint effect of linear and squared potential experience on cumulative work experience ($F(1, 485) = .10, p = .76$), cumulative unemployment ($F(1,485) = .13, p = .72$), or job tenure ($F(1,483) = .62, p = .43$); and does not have a significant main effect on recent unemployment ($B = -.01, p = .89$). This is consistent with male college graduates facing relatively weak barriers to achieving stable, long-term employment histories.

The effect of parental income on the employment histories of non-college graduates is substantively important. Among men with 12 years of potential experience and at most a high school degree, a 1 SD increase in parental income is associated with an additional 1.6 weeks worked, .7 fewer weeks spent unemployed, and 1.2 additional weeks of job tenure in the following year. For men with equivalent experience and some college education, a 1 SD increase in parental income is associated with .6 additional weeks worked, .5 fewer weeks spent unemployed, and 1.9 additional weeks of job tenure in the ensuing year. Over time, these yearly differences lead to wide gaps in the accumulation of work experience by parental income. For instance, a 1 SD increase in parental income is associated with a total of 39 additional weeks worked, 17 fewer weeks of unemployment, and 30 additional weeks of job tenure for men with a high-school education or less 25 years after labor market entry. For men with at least some college, a 1 SD increase in parental income is associated with 14 additional weeks worked, 12 fewer weeks of unemployment, and 48 additional weeks of job tenure after 25 years in the labor market. Unsurprisingly, the effect of parental income on the accumulation of work experience for college graduates is much smaller: 2 additional weeks of work experience, .7 fewer weeks of unemployment, and 17 *fewer* weeks of job tenure after 25 years in the labor market.

As a further comparison, I find the effect of being black (rather than white/other) is stronger than the parental income effect among men with at most a high-school education, but that the effects are comparable among men with some college education. For instance, for men with at least some college, being black (rather than white/other) is associated 18 (compared to 12) fewer weeks of unemployment, and 38 (compared to 48) additional weeks of job tenure after 25 years in the labor market. This is interesting since differences in employment histories are understood to be a key determinant of racial earnings disparities (e.g., Alon and Haberfeld 2007; Tienda and Stier 1996).

Models of earnings

Table 4.4 presents estimates from earnings models for each of the three education categories: high school or less, some college, and college graduates. For each education category, I present four nested linear regressions. In all the models, I centered potential experience, cumulative work experience, and tenure. Thus, for instance, the sum of exponentiated coefficients for cumulative work experience and its square term indicates the predicted change in hourly wage associated with a one-year increase in work experience, for men with the mean work experience (i.e., 7.7 years of work experience). The same logic applies to interpreting the effects of potential experience and tenure. In describing the results below, I refer to the predicted percentage change in hourly wage associated with a one-unit change in the relevant independent variable, obtained by exponentiating the relevant regression coefficient.

Model 1 indicates that, after accounting for pre-labor market characteristics, parental income is associated with a substantial and statistically significant earnings advantage for men in all three education categories ($p < .001$). A 1 SD increase in parental income is associated with an 8% increase in the hourly wage of non-college graduates and a 7% increase for college graduates. These are large effects. As a comparison, Model 1 indicates that relative to white/other men, black men earn 7% less among men with at most a high-school degree, 10% less among men with some college, and an insignificant 2% less among college graduates (not shown). Further, a 1 SD increase in cognitive skill is associated with a 12% increase in earnings for men with at most a high-school education, 7% increase for men with some college, and 15% increase for male college graduates (not shown). Adding contextual factors (Model 2) implies less than a 10% reduction in the effect size of parental income in all three education categories.

Model 3 adds the employment history variables. Overall, employment histories are strongly associated with earnings in all three education categories. For men in all three categories, cumulative unemployment, recent unemployment, and the quadratic of job tenure have a highly significant association with hourly wages ($p < .001$). Further, for non-college graduates, the quadratic of work experience also has a highly significant association with hourly wages ($p < .001$). The only exception is the quadratic of work experience for college graduates ($F(1{,}475) = 0.11, p = .74$). This null result appears to be due to the greater collinearity of potential experience, cumulative work experience, and survey year among college graduates.[48] Since, as discussed above, college graduates work steadily throughout their careers, potential experience is more highly correlated with cumulative work experience for college graduates than

[48] Survey year indicators are collinear with potential experience and cumulative work experience in the NLSY79 because it is a longitudinal cohort sample. Thus, necessarily, in later survey years the sample will be older and have acquired more experience.

for less-educated workers. In supplementary models that exclude potential experience and survey year indicators, I find that the association between the quadratic of work experience and earnings is highly significant ($F(1{,}475) = 56.34, p < .001$) for college graduates, and similar in magnitude to that of less-educated workers.

Additionally, the coefficients in Model 3 indicate that the employment history variables are substantively important. For non-college graduates with the mean level of experience, an additional year of work experience is associated with approximately a 2.3% increase in their hourly wage. For men with the mean level of job tenure, an additional year of tenure is associated with approximately a 2% increase in hourly wages at every education level. Interestingly, the penalties for length of unemployment appear to be strongest among more highly-educated men. For instance, a 1-year increase in cumulative unemployment is associated with a 2.9% hourly wage decrease for men with at most a high-school degree, a 7.5% hourly wage decrease for men with some college education, and an 8.8% hourly wage decrease for male college graduates. Further, a full-year of unemployment the previous calendar year (relative to continuous employment) is associated with an 8.9% hourly wage decrease for men with at most a high-school degree, an 8.6% hourly wage decrease for men with some college education, and a 22% hourly wage decrease for male college graduates. This is consistent with greater stigma and/or greater loss of firm-specific human capital as a result of unemployment for college graduates, as well as stronger long-term effects of lengthy unemployment spells for men with at least some college education.

How does the inclusion of employment history variables affect the association between parental income and earnings? Comparing Model 2 to Model 3, I find that the employment history variables mediate a significant 28% of the effect of parental income on earnings for men with at most a high-school degree, a significant 36% of the effect for men with some college education, and a non-significant 6% of the effect for college graduates.[49] This is consistent with the weak effect of parental income on college graduates' employment histories, and its relatively strong effect on the employment histories of less-educated men.

Finally, Model 4 adds the occupational earnings measure. Unsurprisingly, the occupational earnings measure is highly significant ($p < .001$) in all the education categories: men who work in higher-paying occupations indeed earn higher hourly wages. A 1 SD increase in occupational earnings is associated with a 15% hourly wage increase for men with at most a high-school education, a 14% hourly wage increase for men with some college education, and a 26% hourly wage increase for college graduates. Nevertheless, adding the occupational earnings measure to Model 3 has little effect on the size of the parental income coefficient. Comparing Model 3 to Model 4, I find that the occupational earnings measure mediates 10% of the effect of parental income on earnings for men with at most a high-school degree and 9% of the effect for college graduates. It does not mediate the effect of parental income on earnings for men with some college education.

[49] Since the contextual variables I added in Model 2 (especially marriage) plausibly mediate the effect of parental income on employment histories, I also estimated the change in the association of parental income and earnings from adding the employment history variables to Model 1 (not shown). The percentage reduction in the effect size of parental income for all three education categories, and the statistical significance of the change in coefficients, is essentially unchanged if I exclude the contextual variables from the models.

More importantly for this study, the inclusion of the occupational earnings measure has relatively small, statistically insignificant effect on the coefficients of the employment history variables. The only exception is the effect of recent unemployment on the earnings of college graduates: the decrease in the hourly wage from a full-year of unemployment in the previous calendar year (relative to continuous employment) drops from 22% to 14% with the inclusion of the occupational earnings measure. Thus, the effect of employment histories does not appear to simply reflect occupational allocative inequality.

Furthermore, comparing Model 2 to Model 4, I assess the relative effect of the employment history variables and the occupational earnings measure on the association between parental income and earnings. Among college graduates, neither employment histories nor occupational earnings strongly mediate the effect of parental income on earnings: employment histories mediate 6% of the effect and the occupational earnings measure mediates 10% of the effect. In contrast, among men without a college degree, employment histories mediate a larger (and substantial) portion of the effect of parental income on earnings than the occupational earnings measure. For men with at most a high-school degree, employment histories mediate 28% of the parental income effect and occupational earnings mediates 13% of the effect. For men with some college education, employment histories mediate 31% of the parental income effect and occupational earnings mediates only 3% of the effect. Thus, among men without a college degree, I find that employment histories play a much stronger role in explaining the association between parental income and hourly wages than occupational earnings.

Conclusion

How do parents pass on their economic advantage to their children? In this chapter, I draw on detailed panel data from the 1979 to 2014 waves of the National Longitudinal Survey of Youth 1979 to examine the role of labor market processes—specifically, employment histories—in explaining the intergenerational transmission of economic status. I document a strong association between parental income and employment histories for men without a college degree. Among men with at most a high-school degree, men from higher-income families transition more quickly to stable employment; and accumulate more work experience and tenure, and less unemployment, throughout their careers than men from lower-income families. For men with some college education, higher parental income is associated with less unemployment and longer job tenure throughout their careers. Consequently, for non-college graduates, employment histories significantly mediate the association between parental income and earnings. Even after conditioning on key pre-labor market factors—including cognitive skill, race, and education—the employment history variables account for 28% (for men with at most a high-school degree) and 36% (for men with some college education) of the association between parental income and earnings.

In contrast, parental income does not affect the employment histories of college graduates. In fact, regardless of parental income, college graduates quickly settle into stable, long-term employment. To be clear, I do not find that a college degree is "the great equalizer," erasing the effect of family background on economic well-being (Hout 1988; Torche 2011). Instead, consistent with more recent studies (Manzoni and Streib 2016; Witteveen and Attewell 2017), I find that parental income is significantly associated with earnings among college graduates. However, I do find a college degree "erases" the effect of family background on the attainment of stable, long-term employment histories. For the purposes of quickly finding employment, especially long-term employment, a college degree appears to be a powerful resource that leaves little room for family background effects.

It is important to clarify that I do not identify the causal effect of parental income on employment histories, or of employment histories on earnings. Instead, I identify robust patterns between employment histories, parental income, and earnings; as well as barriers to economic success for men from lower-income backgrounds. For instance, I find that lower-income men with at most a high-school degree experience slow transitions to stable employment. This pattern holds, and invites further examination of school-to-work linkages for these men, even if parental income does not have a causal effect on the speed of the transition to stable employment. Moreover, as I use parental income as a *proxy* for family background, I do not expect that parental income on its own has a causal effect on employment outcomes. Instead, I posited that individuals from higher-income families receive certain economic, social, and cultural resources which in turn affect their employment histories.

Ultimately, this chapter highlights the utility of studying how family background continues to affect individuals after they enter the labor force. Indeed, I find lingering effects of parental income on the accumulation of work experiences decades after men enter the labor force. Unlike in the domain of family background, sociologists have thoroughly examined the effect of labor market processes—including employment histories and allocative inequality—in the domains of race and gender. The sociological understanding of the intergenerational transmission of economic status would likely benefit from adapting theories and methods used to study gender and racial labor market inequality.

Table 4.1 Descriptive statistics of sample, by educational attainment

	High School or Less	Some college	College graduates
Parental income (1978$)	14,815.8	18,779.1	25,943.4
	(10,591.1)	(12,097.9)	(15,861.4)
Cognitive skill (AFQT)	30.3	51.6	74.5
	(23.9)	(26.0)	(21.9)
Race			
% Black	29.3	27.3	17.1
% Hispanic	18.9	19.6	9.6
% White/other	51.9	53.1	73.3
Potential experience	12.5	12.5	11.5
	(9.4)	(9.3)	(8.8)
Work experience	549.1	642.1	661.6
	(442.3)	(455.1)	(448.2)
Cumulative unemployment	61.2	47.7	29.2
	(76.5)	(63.1)	(40.6)
Recent unemployment	4.2	2.1	1.0
	(9.7)	(7.0)	(4.7)
Job tenure	219.3	274.8	312.1
	(289.2)	(315.8)	(324.4)
Occupational earnings	47.2	57.3	73.5
	(23.6)	(25.9)	(23.9)
% Married	44.1	50.9	64.4
% Enrolled	0.9	6.5	4.5
% Urban	75.2	81.8	84.4
Region			
% South	40.2	36.7	34.5
% Northeast	16.7	16.6	84.4
% Midwest	24.2	21.5	27.4
% West	18.9	25.3	17.8
Hourly wage (2014$)	16.8	22.4	33.4
	(10.3)	(14.0)	(21.0)
Respondents (N)	1,995	670	476
Observations (N)	25,502	6,554	5,442

Table 4.2 Correlations among study variables, by educational attainment

	Sample: High School or Less																	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(1) Parental income	**1.00**																	
(2) Potential exp	**0.03**	**1.00**																
(3) Black	**-0.23**	**0.06**	**1.00**															
(4) Hispanic	**-0.08**	**0.03**	**-0.32**	**1.00**														
(5) Cognitive skill	**0.32**	**-0.03**	**-0.36**	**-0.09**	**1.00**													
(6) Married	**0.06**	**0.25**	**-0.18**	**0.07**	**0.08**	**1.00**												
(7) Enrolled	**0.05**	**-0.08**	**-0.02**	**0.02**	**0.06**	**-0.04**	**1.00**											
(8) Urban	**0.05**	**-0.02**	**0.11**	**0.19**	**-0.02**	**-0.10**	**0.03**	**1.00**										
(9) Midwest	**0.10**	0.00	**-0.12**	**-0.19**	**0.12**	**0.02**	**-0.01**	**-0.04**	**1.00**									
(10) South	**-0.13**	**0.02**	**0.25**	**-0.10**	**-0.18**	**0.02**	**-0.02**	**-0.19**	**-0.45**	**1.00**								
(11) West	**0.08**	**0.00**	**-0.21**	**0.34**	**0.08**	**0.01**	**0.04**	**0.14**	**-0.26**	**-0.39**	**1.00**							
(12) HS degree	**0.21**	**0.13**	**0.05**	**-0.11**	**0.34**	**0.06**	**0.05**	0.00	**0.07**	**-0.04**	**-0.02**	**1.00**						
(13) Work exp	**0.10**	**0.93**	**-0.02**	**0.02**	**0.05**	**0.31**	**-0.07**	**-0.05**	0.00	**0.01**	**0.00**	**0.17**	**1.00**					
(14) Cum. unemploy	**-0.16**	**0.35**	**0.21**	**-0.01**	**-0.25**	**-0.07**	**-0.05**	**0.02**	**0.07**	**0.02**	**-0.07**	**-0.09**	**0.13**	**1.00**				
(15) Recent unemploy	**-0.10**	**-0.14**	**0.09**	**-0.02**	**-0.11**	**-0.15**	**-0.01**	**0.01**	**0.03**	**0.00**	**-0.02**	**-0.11**	**-0.22**	**0.31**	**1.00**			
(16) Tenure	**0.11**	**0.52**	**-0.05**	0.00	**0.10**	**0.24**	**-0.03**	**-0.07**	**0.05**	**-0.03**	**-0.04**	**0.14**	**0.62**	**-0.11**	**-0.25**	**1.00**		
(17) Occ. earnings	**0.14**	**0.03**	**-0.17**	**-0.01**	**0.23**	**0.18**	**-0.01**	**-0.02**	0.00	**-0.01**	**0.02**	**0.10**	**0.09**	**-0.14**	**-0.13**	**0.11**	**1.00**	
(18) Log hourly wage	**0.20**	**0.25**	**-0.14**	**0.02**	**0.23**	**0.26**	**-0.03**	**0.04**	-0.01	**-0.10**	**0.07**	**0.16**	**0.33**	**-0.15**	**-0.22**	**0.31**	**0.33**	**1.00**

(continued)

Table 4.2 (continued)

	Sample: Some College																
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1) Parental income	**1.00**																
(2) Potential exp	**0.00**	**1.00**															
(3) Black	**-0.28**	**0.04**	**1.00**														
(4) Hispanic	**-0.13**	**0.04**	**-0.31**	**1.00**													
(5) Cognitive skill	**0.33**	**-0.08**	**-0.43**	**-0.11**	**1.00**												
(6) Married	**0.06**	**0.17**	**-0.17**	**0.04**	**0.09**	**1.00**											
(7) Enrolled	**0.02**	**-0.20**	**-0.05**	0.01	**0.10**	**-0.07**	**1.00**										
(8) Urban	**0.02**	**-0.05**	**0.11**	**0.12**	**-0.05**	**-0.11**	**0.07**	**1.00**									
(9) Midwest	**0.14**	**0.02**	**-0.02**	**-0.21**	**0.09**	0.00	0.00	**-0.08**	**1.00**								
(10) South	**-0.13**	**0.03**	**0.20**	**-0.06**	**-0.12**	0.01	**-0.03**	**-0.06**	**-0.40**	**1.00**							
(11) West	**0.02**	0.00	**-0.17**	**0.27**	0.00	**0.02**	**0.02**	**0.10**	**-0.31**	**-0.45**	**1.00**						
(12) Work exp	**0.05**	**0.89**	**-0.07**	**0.05**	**-0.02**	**0.22**	**-0.18**	**-0.07**	0.01	**0.02**	0.00	**1.00**					
(13) Cum. unemploy	**-0.18**	**0.33**	**0.21**	**-0.02**	**-0.22**	**-0.14**	**-0.09**	**0.05**	**0.12**	**-0.06**	**-0.04**	**0.12**	**1.00**				
(14) Recent unemploy	**-0.09**	**-0.03**	**0.11**	-0.01	**-0.08**	**-0.13**	0.00	**0.03**	**0.05**	**-0.02**	**-0.02**	**-0.10**	**0.35**	**1.00**			
(15) Tenure	**0.10**	**0.50**	**-0.04**	**0.02**	0.02	**0.23**	**-0.10**	**-0.09**	**0.02**	**-0.03**	0.00	**0.59**	**-0.10**	**-0.20**	**1.00**		
(16) Occ. earnings	**0.10**	**0.02**	**-0.17**	0.01	**0.20**	**0.19**	**-0.02**	-0.01	**-0.03**	**-0.01**	**0.02**	**0.07**	**-0.18**	**-0.12**	**0.13**	**1.00**	
(17) Log hourly wage	**0.18**	**0.23**	**-0.15**	**0.02**	**0.14**	**0.26**	**-0.06**	0.01	**-0.03**	**-0.10**	**0.09**	**0.30**	**-0.20**	**-0.17**	**0.33**	**0.34**	**1.00**

(continued)

Table 4.2 (continued)

	Sample: College Graduates																	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
(1) Parental income	**1.00**																	
(2) Potential exp	-0.02	**1.00**																
(3) Black	**-0.31**	**0.06**	**1.00**															
(4) Hispanic	**-0.12**	**0.03**	**-0.15**	**1.00**														
(5) Cognitive skill	**0.32**	**-0.07**	**-0.43**	**-0.14**	**1.00**													
(6) Married	0.01	**0.24**	**-0.05**	0.01	**0.06**	**1.00**												
(7) Enrolled	**-0.03**	**-0.16**	0.00	0.00	**0.04**	**-0.06**	**1.00**											
(8) Urban	**0.07**	**-0.06**	**0.07**	**0.03**	-0.01	**-0.12**	0.02	**1.00**										
(9) Midwest	0.01	0.00	**-0.07**	**-0.16**	**0.09**	**0.05**	0.00	**-0.04**	**1.00**									
(10) South	**-0.12**	**0.03**	**0.19**	**0.04**	**-0.13**	**0.03**	-0.01	**-0.08**	**-0.45**	**1.00**								
(11) West	**-0.06**	**0.02**	**-0.09**	**0.16**	-0.01	-0.01	0.00	**0.10**	**-0.29**	**-0.35**	**1.00**							
(12) Post-graduate	0.02	**0.06**	**-0.06**	**0.02**	**0.15**	**0.12**	**0.06**	-0.01	-0.01	-0.01	**0.03**	**1.00**						
(13) Work exp	**-0.02**	**0.94**	**0.03**	**0.04**	**-0.06**	**0.24**	**-0.15**	**-0.06**	**0.02**	-0.01	**0.03**	**0.08**	**1.00**					
(14) Cum. unemploy	**-0.15**	**0.25**	**0.18**	**0.02**	**-0.21**	**-0.11**	**-0.04**	0.00	**-0.02**	0.01	**0.03**	-0.01	**0.15**	**1.00**				
(15) Recent unemploy	**-0.05**	**-0.02**	**0.07**	-0.01	**-0.06**	**-0.13**	0.00	0.01	-0.02	**0.02**	-0.02	**-0.03**	**-0.07**	**0.36**	**1.00**			
(16) Tenure	0.00	**0.51**	**-0.03**	0.01	0.01	**0.21**	**-0.07**	**-0.06**	**0.08**	**-0.04**	0.01	**0.04**	**0.54**	**-0.09**	**-0.16**	**1.00**		
(17) Occ. earnings	**0.11**	0.00	**-0.10**	0.01	**0.22**	**0.14**	**-0.06**	**0.02**	0.01	0.02	-0.01	**0.13**	0.01	**-0.14**	**-0.09**	**0.04**	**1.00**	
(18) Log hourly wage	**0.18**	**0.26**	**-0.08**	**-0.03**	**0.19**	**0.21**	**-0.08**	**0.04**	**-0.02**	**-0.03**	**0.03**	**0.11**	**0.28**	**-0.14**	**-0.12**	**0.25**	**0.36**	**1.00**

Note: Pairwise correlations. Bold indicates correlation is statistically significant at $p < .05$, two-tailed tests.

Table 4.3 Association between parental income and work experience measures, by educational attainment

	Work experience	Cumulative unemployment	Job tenure	Recent unemployment
	FE	FE	FE	Linear
		High School or Less		
Parental income				-0.438***
				(0.137)
Parental income x Potential exp	1.575***	-0.718***	1.202*	0.066***
	(0.425)	(0.142)	(0.512)	(0.014)
Parental income x Potential exp^2	-0.024	0.025***	-0.002	-0.002*
	(0.016)	(0.007)	(0.042)	(0.001)
Potential exp	43.277***	3.777***	15.529***	-0.269***
	(0.407)	(0.171)	(0.636)	(0.017)
Potential exp^2	0.079***	-0.100***	0.128***	0.014***
	(0.018)	(0.008)	(0.045)	(0.001)
Black				2.043***
				(0.348)
Black x Potential exp	-4.672***	1.777***	-2.437*	0.092***
	(0.893)	(0.369)	(1.029)	(0.035)
Black x Potential exp^2	-0.083*	0.042*	0.130	-0.001
	(0.040)	(0.016)	(0.069)	(0.002)
Hispanic				0.928***
				(0.332)
Hispanic x Potential exp	-1.667	0.077	-1.218	0.135***
	(0.865)	(0.327)	(1.095)	(0.032)
Hispanic x Potential exp^2	-0.064	0.036*	-0.008	-0.007***
	(0.040)	(0.015)	(0.077)	(0.002)
Cognitive skill				-0.593***
				(0.158)
Cognitive skill x Potential exp	2.834***	-1.246***	1.619***	0.065***
	(0.421)	(0.168)	(0.571)	(0.016)
Cognitive skill x Potential exp^2	-0.014	0.034***	0.031	-0.002*
	(0.019)	(0.008)	(0.041)	(0.001)
Married	32.363***	-8.610***	20.398***	-1.807***
	(4.091)	(1.662)	(5.937)	(0.192)
Enrolled	-7.706	5.701	12.119	-0.839
	(6.314)	(3.118)	(10.008)	(0.593)
Urban	-7.295	4.756*	-6.937	-0.235
	(5.813)	(2.276)	(9.393)	(0.257)
Midwest	34.072	-3.896	-71.716*	1.441***
	(20.485)	(8.007)	(30.808)	(0.354)
South	21.411	3.713	-82.999***	-0.320
	(17.318)	(6.586)	(23.581)	(0.304)
West	12.329	-2.160	-76.990***	-0.072
	(24.356)	(8.045)	(26.692)	(0.336)
High school degree	-20.107	11.797***	-27.488*	-1.556***
	(12.862)	(4.478)	(13.844)	(0.283)
Constant	450.064***	73.243***	257.491***	4.651***
	(18.264)	(6.213)	(24.017)	(0.419)
Respondents (N)	2058	2058	2024	2047
Observations (N)	31264	31264	27767	30258
R^2	0.91	0.45	0.36	0.07

(continued)

Table 4.3 (continued)

	Work experience	Cumulative unemployment	Job tenure	Recent unemployment
	FE	FE	FE	Linear
		Some College		
Parental income				-0.419*
				(0.163)
Parental income x Potential exp	0.541	-0.491*	1.923	0.006
	(0.408)	(0.210)	(1.185)	(0.014)
Parental income x Potential exp^2	0.037	-0.003	0.008	-0.001
	(0.024)	(0.009)	(0.064)	(0.001)
Potential exp	46.458***	2.204***	17.294***	-0.048
	(0.726)	(0.300)	(1.723)	(0.028)
Potential exp^2	-0.057	-0.032*	0.149	0.005*
	(0.039)	(0.013)	(0.113)	(0.002)
Black				0.763
				(0.430)
Black x Potential exp	-2.839*	0.707	-1.439	-0.089
	(1.361)	(0.471)	(2.576)	(0.047)
Black x Potential exp^2	0.061	0.011	-0.071	0.008
	(0.062)	(0.024)	(0.163)	(0.005)
Hispanic				0.246
				(0.409)
Hispanic x Potential exp	-0.687	0.012	0.853	0.010
	(1.023)	(0.508)	(2.918)	(0.043)
Hispanic x Potential exp^2	0.049	-0.001	0.151	0.002
	(0.054)	(0.021)	(0.174)	(0.004)
Cognitive skill				-0.365
				(0.201)
Cognitive skill x Potential exp	1.225*	-0.353	0.533	0.034
	(0.580)	(0.243)	(1.324)	(0.020)
Cognitive skill x Potential exp^2	-0.001	0.026*	0.037	0.001
	(0.027)	(0.012)	(0.084)	(0.002)
Married	18.578***	-3.824	31.320*	-1.484***
	(7.058)	(2.727)	(14.423)	(0.260)
Enrolled	-0.785	0.373	-8.378	-0.340
	(6.463)	(2.165)	(15.221)	(0.398)
Urban	3.056	2.668	-25.519	-0.062
	(8.387)	(2.531)	(16.889)	(0.311)
Midwest	46.749	-8.587	-10.789	0.392
	(36.493)	(9.560)	(62.694)	(0.567)
South	31.770	-11.648	-121.492***	-1.335***
	(23.215)	(8.080)	(44.121)	(0.489)
West	56.960*	-11.181	-71.520	-0.704
	(28.097)	(8.098)	(53.754)	(0.523)
Constant	534.497***	54.990***	308.941***	3.087***
	(21.245)	(6.648)	(39.010)	(0.566)
Respondents (N)	690	690	672	688
Observations (N)	7345	7345	6923	7206
R^2	0.95	0.27	0.39	0.04

(continued)

Table 4.3 (continued)

	Work experience	Cumulative unemployment	Job tenure	Recent unemployment
	FE	FE	FE	Linear
		College Graduates		
Parental income				-0.014
				(0.101)
Parental income x Potential exp	0.086	-0.030	-0.567	0.008
	(0.190)	(0.073)	(0.854)	(0.009)
Parental income x Potential exp^2	-0.026	0.003	-0.103	-0.001
	(0.018)	(0.007)	(0.064)	(0.001)
Potential exp	48.531***	1.552***	21.950***	-0.036
	(0.958)	(0.414)	(2.563)	(0.034)
Potential exp^2	0.036	-0.037	0.001	0.003
	(0.053)	(0.027)	(0.192)	(0.002)
Black				0.747
				(0.612)
Black x Potential exp	-0.232	0.369	-3.034	-0.017
	(0.955)	(0.464)	(3.194)	(0.036)
Black x Potential exp^2	-0.089	0.031	-0.084	-0.001
	(0.058)	(0.027)	(0.272)	(0.005)
Hispanic				-0.025
				(0.321)
Hispanic x Potential exp	0.355	-0.498	-5.580	0.036
	(0.712)	(0.288)	(3.258)	(0.029)
Hispanic x Potential exp^2	0.010	0.022	0.681***	-0.004
	(0.048)	(0.022)	(0.218)	(0.003)
Cognitive skill				-0.266
				(0.192)
Cognitive skill x Potential exp	1.041*	-0.533*	-0.445	0.007
	(0.482)	(0.226)	(1.483)	(0.019)
Cognitive skill x Potential exp^2	-0.029	0.020	0.021	0.001
	(0.033)	(0.015)	(0.123)	(0.001)
Married	-0.653	0.921	13.297	-1.132***
	(3.775)	(2.199)	(16.785)	(0.255)
Enrolled	2.092	1.796	0.406	-0.093
	(3.355)	(1.543)	(16.356)	(0.393)
Urban	2.871	1.274	-8.369	0.091
	(4.065)	(1.481)	(20.376)	(0.237)
Midwest	18.031	-0.742	29.951	0.117
	(10.114)	(4.275)	(55.739)	(0.335)
South	17.714	3.482	-15.638	0.070
	(12.558)	(4.457)	(45.439)	(0.371)
West	4.118	6.280	-36.296	-0.136
	(10.751)	(5.015)	(54.150)	(0.321)
Post-graduate education	87.940***	1.971	7.783	-0.265
	(5.505)	(2.452)	(26.304)	(0.189)
Constant	599.177***	18.042***	305.116***	1.751***
	(9.791)	(3.961)	(43.199)	(0.526)
Respondents (N)	486	486	484	485
Observations (N)	5950	5950	5767	5878
R^2	0.99	0.17	0.39	0.03

(continued)

Table 4.3 (continued)

Note: Results presented are coefficients with clustered standard errors in parentheses. Parental income and cognitive skill is standardized. Baseline respondent is white, and has the sample mean parental income, cognitive skill, and potential experience.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests).

Table 4.4 Effect of parental income on men's log hourly wage, by educational attainment

	Model 1	Model 2	Model 3	Model 4
	Pre-labor mkt controls	+ contextual controls	+ employment histories	+ occupational earnings
	High School or Less			
Parental income	0.074***	0.069***	0.050***	0.045***
	(0.012)	(0.012)	(0.011)	(0.010)
Potential exp	0.023***	0.017**	-0.001	-0.003
	(0.006)	(0.005)	(0.006)	(0.005)
Potential exp^2	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.073***	-0.053*	-0.017	0.008
	(0.021)	(0.021)	(0.020)	(0.018)
Hispanic	0.037	-0.015	-0.010	-0.000
	(0.024)	(0.023)	(0.022)	(0.020)
Cognitive skill	0.109***	0.102***	0.082***	0.061***
	(0.011)	(0.011)	(0.011)	(0.010)
Married		0.190***	0.131***	0.108***
		(0.013)	(0.013)	(0.012)
Enrolled		-0.097*	-0.093*	-0.080*
		(0.042)	(0.040)	(0.039)
Urban		0.067***	0.078***	0.072***
		(0.015)	(0.015)	(0.014)
Midwest		-0.142***	-0.113***	-0.107***
		(0.023)	(0.022)	(0.020)
South		-0.111***	-0.104***	-0.114***
		(0.021)	(0.019)	(0.018)
West		-0.041	-0.021	-0.021
		(0.026)	(0.024)	(0.022)
High school degree	0.063**	0.062***	0.027	0.022
	(0.019)	(0.018)	(0.018)	(0.017)
Work exp			0.023***	0.021***
			(0.003)	(0.003)
Work exp^2			-0.000*	-0.000*
			(0.000)	(0.000)
Cumulative unemployment			-0.029***	-0.024***
			(0.006)	(0.005)
Recent unemployment			-0.093***	-0.081***
			(0.020)	(0.019)
Job tenure			0.019***	0.017***
			(0.002)	(0.002)
Job tenure^2			-0.001***	-0.001***
			(0.000)	(0.000)
Occupational earnings				0.140***
				(0.007)
Constant	2.873***	2.819***	2.836***	2.830***
	(0.076)	(0.076)	(0.073)	(0.068)
Respondents (N)	1,995	1,995	1,995	1,995
Observations (N)	25,502	25,502	25,502	25,502
R^2	0.16	0.20	0.25	0.29

(continued)

Table 4.4 (continued)

	Model 1	Model 2	Model 3	Model 4
	Pre-labor mkt controls	+ contextual controls	+ employment histories	+ occupational earnings
		Some College		
Parental income	0.080***	0.074***	0.048*	0.050**
	(0.021)	(0.020)	(0.020)	(0.018)
Potential exp	0.035**	0.026*	0.009	0.011
	(0.012)	(0.011)	(0.012)	(0.011)
Potential exp^2	-0.001**	-0.001**	-0.001*	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.097*	-0.064	-0.015	0.002
	(0.046)	(0.046)	(0.042)	(0.039)
Hispanic	0.014	-0.040	-0.048	-0.045
	(0.046)	(0.047)	(0.043)	(0.040)
Cognitive skill	0.063**	0.061**	0.049*	0.029
	(0.022)	(0.021)	(0.020)	(0.018)
Married		0.210***	0.129***	0.094***
		(0.028)	(0.027)	(0.024)
Enrolled		-0.025	-0.047	-0.044
		(0.037)	(0.034)	(0.032)
Urban		0.072*	0.089**	0.077**
		(0.031)	(0.029)	(0.026)
Midwest		-0.132**	-0.102*	-0.090*
		(0.051)	(0.045)	(0.042)
South		-0.084	-0.105*	-0.104**
		(0.047)	(0.041)	(0.039)
West		0.048	0.027	0.017
		(0.054)	(0.047)	(0.044)
Work exp			0.023***	0.021***
			(0.007)	(0.006)
Work exp^2			-0.000	-0.000
			(0.000)	(0.000)
Cumulative unemployment			-0.078***	-0.068***
			(0.014)	(0.013)
Recent unemployment			-0.090	-0.068
			(0.056)	(0.055)
Job tenure			0.016***	0.014***
			(0.004)	(0.003)
Job tenure^2			-0.000	-0.000
			(0.000)	(0.000)
Occupational earnings				0.135***
				(0.012)
Constant	3.152***	2.991***	3.009***	3.016***
	(0.199)	(0.196)	(0.194)	(0.176)
Respondents (N)	670	670	670	670
Observations (N)	6,554	6,554	6,554	6,554
R^2	0.15	0.19	0.27	0.31

(continued)

Table 4.4 (continued)

	Model 1	Model 2	Model 3	Model 4
	Pre-labor mkt controls	+ contextual controls	+ employment histories	+ occupational earnings
		College Graduates		
Parental income	0.071***	0.064***	0.060**	0.055***
	(0.019)	(0.019)	(0.018)	(0.016)
Potential exp	0.016	0.009	0.001	0.002
	(0.017)	(0.016)	(0.018)	(0.015)
Potential exp^2	-0.002**	-0.001*	-0.001*	-0.001*
	(0.000)	(0.000)	(0.001)	(0.001)
Black	-0.017	-0.018	0.014	0.019
	(0.059)	(0.058)	(0.056)	(0.047)
Hispanic	-0.010	-0.034	-0.037	-0.063
	(0.080)	(0.078)	(0.075)	(0.066)
Cognitive skill	0.139***	0.139***	0.130***	0.069**
	(0.033)	(0.031)	(0.030)	(0.027)
Married		0.162***	0.122***	0.081**
		(0.033)	(0.033)	(0.029)
Enrolled		-0.154**	-0.146**	-0.112*
		(0.050)	(0.049)	(0.047)
Urban		0.072	0.080	0.084*
		(0.041)	(0.041)	(0.038)
Midwest		-0.130*	-0.136**	-0.141**
		(0.053)	(0.052)	(0.044)
South		-0.088	-0.090	-0.107*
		(0.057)	(0.054)	(0.046)
West		-0.027	-0.025	-0.023
		(0.064)	(0.061)	(0.051)
Post-graduate	0.026	0.013	-0.001	-0.022
	(0.056)	(0.054)	(0.054)	(0.047)
Work exp			0.002	0.002
			(0.008)	(0.008)
Work exp^2			0.001	0.000
			(0.000)	(0.000)
Cumulative unemployment			-0.092***	-0.085***
			(0.025)	(0.022)
Recent unemployment			-0.251*	-0.146
			(0.114)	(0.103)
Job tenure			0.015***	0.015***
			(0.004)	(0.004)
Job $tenure^2$			-0.000	-0.000
			(0.000)	(0.000)
Occupational earnings				0.230***
				(0.017)
Constant	3.178***	2.997***	3.003***	2.742***
	(0.186)	(0.186)	(0.197)	(0.176)
Respondents (N)	476	476	476	476
Observations (N)	5,442	5,442	5,442	5,442
R^2	0.16	0.18	0.21	0.29

(continued)

Table 4.4 (continued)

Note: Coefficients from linear regressions with clustered standard errors in parentheses. Parental income, occupational earnings, and cognitive skill are standardized. Employment history variables and potential experience measured in years, and work experience, potential experience, and tenure are centered. All models include year fixed effects.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

CHAPTER 5
Conclusion

In this dissertation, I examined labor market processes generating ascriptive inequality in employment outcomes. In the first two empirical chapters, I drew on an original, two-wave experiment with a sample of white hiring agents to advance the sociological understanding of the determinants of employers' discriminatory behavior and the consequences of jobseekers' racially-segregated networks. First, I investigated *why* employers discriminate. I found implicit (largely unconscious), but not explicit, racial attitudes predict employers' evaluations of white applicants, and of black applicants relative to white applicants. Thus, instead of deliberately rejecting black jobseekers, hiring agents' behavior appears to be driven by largely unconscious biases. Further, the findings suggest implicit attitudes are not only associated with discrimination against black applicants, but also with bias in favor of white applicants. Indeed, I found white hiring agents' implicit pro-white bias predicts how favorably they evaluate white jobseekers, not only how negatively they evaluate black jobseekers relative to white jobseekers. This suggests a central role for pro-white sentiment in theorizing discrimination. Lastly, I find that given the ambiguity of the hiring process, hiring agents can use varied strategies to interpret their racially-motivated decisions without invoking race. Overall, this chapter illustrates how employers can maintain and portray a non-prejudiced self-image while perpetuating racial disparities in employment.

These findings are consistent with the theoretical predictions I derived from the MODE dual-process model of the attitude-behavior relationship (Fazio 1990). Indeed, I argued the hiring process at many U.S. organizations—characterized by ambiguity, time pressure, and the legitimacy of intuitive decision-making—encourages the use of implicit rather than explicit cognition. In the future, I plan to test whether varying aspects of the hiring process—such as discouraging intuitive decision-making or specifying clear selection criteria—decreases hiring agents' reliance on implicit cognition. Future research should also investigate the determinants and consequences of pro-white bias and in-group favoritism (see DiTomaso 2015). For instance, I expect white employers to be less likely to monitor their behavior for evidence of racial bias in their treatment of white jobseekers than black jobseekers. If true, this suggests hiring agents are more likely to remain unaware of discriminatory behavior that is driven by pro-white rather than anti-black sentiment.

Second, I bridged the literatures on networks and discrimination by examining how white hiring reward the employee referrals of black and white jobseekers, from black and white employees. I found that in the most prevalent real-life conditions—black applicants referred by black employees, and white applicants referred by white employees—black applicants' referrals were significantly discounted relative to white applicants' referrals. Indeed, black applicants only benefited from having a referral when two conditions were met: the referring employee was white and the hiring agent was relatively low-prejudiced. Thus, in addition to their disadvantage in access to employee referrals, black jobseekers suffer from a disadvantage in returns to these referrals. To my knowledge, this is also the first study to experimentally establish that—at least for some jobseekers—employers do reward employee referrals.

I plan to extend this work to examine why race affects how employers reward referrals. On the one hand, race could affect employers' perception of referral credibility, since referrals are an ambiguous signal of applicant quality. Indeed, from the employers' perspective, it is not clear how they should interpret referrals: while employees may want to protect their reputation by only recommending appropriate applicants, they may also be motivated to help a friend, even if they have doubts about their friend's workplace suitability. On the other hand, employers' perceived racial group interests could also affect how they reward referrals. Group position theory (Blumer 1958) posits dominant group members feel entitled to certain resources, and threatened by the belief that subordinate groups wish to encroach on those resources. Thus, a highly prejudiced white hiring agent may dismiss a white employee's recommendation of a black applicant because she sees it as a betrayal of her group interest, rather than because she does not find it credible. To assess the relative explanatory power of the credibility and group position perspectives, I plan to experimentally manipulate referral credibility and the level of perceived threat invoked by the recommendation.

In the final empirical chapter, I use the rich week-by-week measures of work experiences from the National Longitudinal Survey of Youth 1979 (NLSY79) to examine the role of labor market experiences—specifically, employment histories—in explaining the intergenerational transmission of economic status. I document a strong association between parental income and employment histories for men without a college degree. Among this group, men from higher-income families accumulate more work experience and tenure, and less unemployment, throughout their careers than men from lower-income families. In contrast, regardless of parental income, college graduates quickly settle into stable, long-term employment. Thus, a college degree appears to be a powerful resource that leaves little room for family background effects on employment histories. Consequently, for non-college graduates (but not for college graduates), employment histories mediate approximately one-third of the effect of parental income on earnings.

This chapter highlights the utility of studying how family background continues to affect individuals after they enter the labor force. Indeed, I find lingering effects of parental income on the accumulation of work experiences decades after men enter the labor force. Unlike in the domain of family background, sociologists have thoroughly examined the effect of labor market processes—including employment histories and allocative inequality—in the domains of race and gender. The sociological understanding of the intergenerational transmission of economic status would likely benefit from adapting theories and methods used to study gender and racial labor market inequality. Ultimately, this dissertation highlights the enduring effect of race and family background in the labor market.

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APPENDICES

Appendix A

Job position, résumés, and employee referral forms for Chapters 2 and 3

I chose the position of Assistant Store Manager at a large retail store because I anticipated most study participants would already have some familiarity with this position, increasing their comfort and engagement with the task. Further, appropriate résumés for this position can be relatively concise and unspecialized.

To increase realism, the résumés were based on actual résumés of jobseekers for similar jobs, and of individuals who are currently assistant store managers in large retail stores; the employee referral form was based on actual company templates. The two résumés indicated that the job applicants were recent college graduates from similarly-selective public universities in Massachusetts. Neither had management experience. Prior to the experiment, I pre-tested the two résumés to assess whether they were of equivalent quality. Eighty-six respondents, recruited through Mechanical Turk, evaluated the two résumés for the same position and on the same criteria as I use in this experiment. I found no statistically significant difference in any of the outcomes. Applicants were randomly assigned to the résumés.

The employee referral form, designed to be filled in by hand by the referring employee, included a small space for employees to explain why they think the applicant is qualified for the position. The referring employee states that "[Applicant name] would be a great addition to [redacted company name]. He is a people person with extensive experience in customer service and an effective problem solver. He is also smart, enthusiastic, and very hard-working." The employee referral form and résumés were hand-redacted of personally-identifying information, such as addresses, to decrease the likelihood of prompting respondent suspicion.

Appendix B

Selecting racially-distinct names for Chapters 2 and 3

I manipulated race, for the applicants and referring employee, by using racially-distinct names in the résumé and employee referral forms. To select suitable names, I pre-tested eighteen names used in previous studies focused on racially-distinct names (Bertrand and Mullainathan 2004; Fryer and Levitt 2004; Gaddis 2015; Milkman, Akinola, and Chugh 2015). My goals were to identify names that signaled the intended race, and reduced perceived class origin differences among the black and white names. The eighteen names were: Lamar, Terrell, Darnell, Tyrone, Jamal, Leroy, Jermaine, Jalen, DeShawn, Charlie, Brad, Steven, Greg, Todd, Matthew, Jay, Jake, and Connor.

The pretest took place from July 27, 2014 to July 29, 2014. Eighty-seven white respondents recruited through Mechanical Turk evaluated the 18 names, for a total of 1565 evaluations. I asked participants to imagine an individual with the given name, and to report their first impressions of this individual. To test if the names correctly signaled the intended race, I asked respondents to guess which racial/ethnic category the individual belonged to ("White or Caucasian," "Black or African-American," "Latino or Hispanic," "Asian," "Pacific Islander," and "Other"). To identify the perceived class of the imagined individual, I asked respondents to guess the highest level of education of the individual's mother and father (five-point scale from

“Less than a high school degree or equivalent” to “Graduate or professional degree”), parental social class (five-point sale from “Lower class” to “Upper class”), and household income when the individual was 16 years old (nine-point scale from “Under $10,000” to “150,0000 and above.”) I used exploratory factor analysis to create a class index based on these four items; the retained factor has an eigenvalue of 3.3, and the minimum factor leading is .87. Higher scores on the index indicate a higher-class background. I randomized the order in which the 18 names were shown.

I disqualified three names (Leroy, Jalen, and Jay) from consideration because they signaled race relatively poorly (See Table B1). Then, I chose names that minimized differences in class background among the black and white names, as measured by the class index. Consequently, I chose the distinctly-white names Charlie, Jake, and Greg, and the distinctly-black names Terrell, Lamar, and Jermaine.

Even among distinctly-black names and distinctly-white names, I found important differences in perceived class background. For example, compared to the name Terrell, the names Tyrone and DeShawn have a significantly ($p < .05$) lower score on the class index. Similarly, compared to the name Charlie, the names Brad, Steven, and Connor have a significantly ($p < .001$) higher score on the class index. To illustrate, while only 13% of respondents guessed Terrell’s mother had less than a high–school education, the comparable figures were much higher for Tyrone and DeShawn (30% and 39%, respectively). Among distinctly-white names, while 26% of respondents believed Charlie’s mother had at least a college degree, the comparable figures were 45%, 48%, and 51% for the mothers of Brad, Steven, and Connor.

My goal was not to eliminate all differences in perceived class background among black and white names. Instead, I wanted to minimize concerns that distinctly-black names and distinctly-white names signal class origin, beyond the class origin that is signaled by race (see Fryer and Levitt 2004).

Table B1. Perceptions of individuals, by their name

Name	Race: White	Race: Black	Class Index
	%	*%*	*Mean*
DeShawn	1%	94%	-0.9
Tyrone	5%	93%	-0.8
Jamal	0%	93%	-0.6
Leroy	39%	54%	-0.5
Darnell	7%	86%	-0.5
Jermaine	7%	89%	-0.5
Terrell	1%	92%	-0.5
Lamar	8%	82%	-0.5
Jalen	23%	54%	-0.2
Jay	78%	8%	0.2
Charlie	90%	1%	0.3
Jake	91%	1%	0.3
Greg	90%	2%	0.4
Todd	93%	2%	0.5
Connor	93%	2%	0.8
Brad	92%	2%	0.8
Steven	88%	5%	0.8
Matthew	94%	1%	0.8

Note: Number of evaluators: 87. Number of evaluations: 1565. Class index is standardized.

Appendix C

Robustness checks for Chapter 3: Individual variables

The evaluation score is a composite of four individual variables: interview recommendation, promotion likelihood, salary recommendation, and the strength of choice scale. The results presented in the main text are robust to using the four individual variables, as well as the binary choice variable (i.e. choose <u>one</u> applicant to recommend for an interview), instead of the composite evaluation score.

Table C1 presents logistic regressions of referral status on the binary choice variable, and ordered logistic regressions of referral status on the remaining four variables, by experimental condition. I used logistic regressions for the binary choice variable since it is binary, and ordered logistic regressions for the remaining variables since they are ordinal. I did not include controls since respondents were randomly assigned to the experimental conditions. I clustered the standard errors by respondent, as each respondent evaluated two applicants. Consistent with the analysis presented in Figure 1 of the main text, respondents did not reward black applicants' same-race referrals for any of the five variables. Further, and also consistent with the analysis presented in Figure 1, respondents significantly rewarded the referrals for four out of the five variables in each of the remaining three experimental conditions. I also found support for Hypothesis 1b ($p < .05$) using each variable, and did not find support for Hypothesis 2 for any of the variables (analyses available upon request).

Table C2 presents regressions of returns to employee referrals on anti-black prejudice, gender, and age, by experimental condition. For the variables interview recommendation, promotion likelihood, and salary recommendation, I operationalized returns to employee referrals as the within-respondent difference in the evaluations of referred applicant and the non-referred applicant. Since the strength of choice scale and the binary choice variable are comparative measures by design (e.g. if the referred applicant is recommended in the binary choice, the non-referred applicant cannot be recommended), I restrict these analyses to the evaluations of the referred applicants. I used logistic regressions for the binary choice variable, and ordered logistic regressions for the remaining variables. Consistent with the analysis presented in Table 4 of the main text, I found that relative to less-prejudiced respondents, more-prejudiced respondents rewarded white applicants' cross-race referrals more ($p < .1$ for promotion likelihood, $p < .05$ for the remaining variables), and black applicants' cross-race referrals less ($p < .1$ for interview likelihood, $p < .05$ for the remaining variables). Further, as in the analysis presented in the main text, respondents' anti-black prejudice did not predict returns to same-race referrals for black or white applicants.

Table C1. Effect of referral status on interview recommendations, promotion likelihood, salary recommendation, binary choice, and strength of choice scale, by experimental condition

	Interview	Salary	Promotion	Binary choice	Strength of choice
Black applicants, black referrer					
Has referral	0.20	0.13	0.24	0.30	-0.02
	(0.32)	(0.24)	(0.37)	(0.55)	(0.48)
Pseudo R^2	0.00	0.00	0.00	0.00	0.00
Black applicants, white referrer					
Has referral	1.20**[a]	0.79*[a]	0.64*	0.86+	0.65
	(0.33)	(0.32)	(0.32)	(0.51)	(0.44)
Pseudo R^2	0.04	0.02	0.01	0.03	0.01
White applicants, black referrer					
Has referral	0.95**[a]	0.64*	0.48	1.46*	1.33*[a]
	(0.36)	(0.26)	(0.34)	(0.66)	(0.57)
Pseudo R^2	0.02	0.01	0.01	0.09	0.03
White applicants, white referrer					
Has referral	1.05**[a]	0.74**[a]	1.17**[a]	0.71	1.24**[a]
	(0.32)	(0.25)	(0.28)	(0.52)	(0.46)
Pseudo R^2	0.03	0.02	0.04	0.02	0.03

Note: Respondents per condition: 54 (Black applicants, black referrer), 66 (black applicants, white referrer), 43 (white applicants, white referrer), and 63 (white applicants, white referrer). Each respondent evaluates two applicants. The binary choice models are estimated with a logistic regression. The remaining models are estimated with ordered logistic regressions. Robust standard errors in parentheses, clustered by respondent. Cut points and constant omitted.

[a] $p < 0.1$; coefficient is significantly different than the black applicant/black referrer coefficient

$+ p < .1$; $* p < .05$; $** p < .01$ (two-tailed tests).

Table C2. Regressions of interview recommendation, salary recommendation, promotion likelihood, binary choice, and strength of choice, by experimental condition

	Interview	Salary	Promotion	Binary choice	Strength of choice
Black applicants, black referrer					
Anti-black prejudice	-0.08[c]	-0.09[bc]	-0.20[c]	-0.10[c]	0.03[b]
	(0.24)	(0.24)	(0.23)	(0.26)	(0.24)
Age (years)	0.01	-0.01[b]	0.01	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Male	-0.58[b]	-0.22	-0.65[b]	-0.75[b]	-0.58[b]
	(0.53)	(0.53)	(0.53)	(0.58)	(0.52)
Pseudo R^2	0.01	0.01	0.02	0.03	0.01
Black applicants, white referrer					
Anti-black prejudice	-0.46+[c]	-0.92**[acd]	-0.60*[cd]	-0.70*[cd]	-0.56*[acd]
	(0.26)	(0.28)	(0.26)	(0.34)	(0.25)
Age (years)	0.01	0.04*[a]	0.01	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Male	0.95*[acd]	0.51	1.16*[acd]	1.83**[acd]	1.51**[acd]
	(0.46)	(0.47)	(0.48)	(0.60)	(0.48)
Pseudo R^2	0.04	0.08	0.05	0.16	0.06
White applicants, black referrer					
Anti-black prejudice	0.69*[abd]	0.81*[abd]	0.58+[abd]	0.93*[ab]	0.64*[b]
	(0.34)	(0.36)	(0.31)	(0.45)	(0.30)
Age (years)	-0.02	0.02	-0.01	0.02	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Male	-0.71[b]	-0.53	-0.78[b]	-1.44+[b]	-1.36*[b]
	(0.60)	(0.63)	(0.57)	(0.78)	(0.61)
Pseudo R^2	0.06	0.07	0.04	0.18	0.06
White applicants, white referrer					
Anti-black prejudice	-0.10[c]	-0.19[bc]	-0.06[bc]	0.30[b]	0.10[b]
	(0.27)	(0.29)	(0.28)	(0.31)	(0.26)
Age (years)	0.03	0.01	0.04+	0.04	0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Male	-0.05[b]	0.53	-0.15[b]	-0.28[b]	-0.01[b]
	(0.52)	(0.54)	(0.55)	(0.60)	(0.55)
Pseudo R^2	0.01	0.01	0.02	0.04	0.01

Note: Respondents per condition: 52 (Black appl., black ref.), 65 (black appl., white ref.), 42 (white appl., white ref.), and 62 (white appl., white ref.). Each respondent evaluated two applicants. Binary choice models estimated with logistic regression. Remaining models estimated with ordered logistic regressions. Standard errors in parentheses. Cut points and constant omitted.

[a] $p < .1$; coefficient is significantly different than the black applicants/black referrer coefficient
[b] $p < .1$; coefficient is significantly different than the black applicants/white referrer coefficient
[c] $p < .1$; coefficient is significantly different than the white applicants/black referrer coefficient
[d] $p < .1$; coefficient is significantly different than the white applicants/white referrer coefficient
$+ p < .1$; $* \ p < .05$; $** \ p < .01$ (two-tailed tests).

Appendix D

Robustness checks for Chapter 3: Additional control variables

The results presented in the main text are robust to the inclusion of additional control variables. Tables D1, D2, and D3 present the analyses presented in Tables 2, 3, and 4, respectively, with the inclusion of education and region as additional control variables. I operationalize region as a series of dummy variables indicating residence in the North, Midwest, West, and South (the excluded category), and education as years of completed education.

Table D1 presents results of linear regressions of returns to employee referrals on anti-black prejudice, gender, age, region, and education, for black and white applicants. I find that as respondents' anti-black prejudice increased, they rewarded black applicants' referrals *less* ($p < .05$) and insignificantly reward white applicants' referrals more. Prejudice differentially affected black and white applicants' returns to referrals ($p < .05$). Like the results presented in the main text, these results support Hypothesis 1b.

Table D2 presents results of linear regressions of returns to employee referrals on anti-black prejudice, gender, age, region, and education, for black and white referrers. As respondents' anti-black prejudice increased, they rewarded white employees' recommendations *less* ($p < .05$) and insignificantly rewarded black employees' recommendations more; thus, referrals from black employees were increasingly *rewarded* relative to those from white employees (p-value of difference$< .1$). Thus, and consistent with results presented in the main text, I find support against Hypothesis 2.

Table D3 presents results of linear regressions of returns to employee referrals on anti-black prejudice, gender, age, region, and education, by experimental condition. Like in the main analysis presented in Table 4, I found that relative to less-prejudiced respondents, more-prejudiced respondents rewarded black applicants' cross-race referrals *less* ($p < .05$) and white applicants' cross-race referrals *more* ($p < .05$). Further, respondents' prejudice had a negligible effect on returns to black and whites' same-race referrals.

I also estimate the predicted returns to referrals with the addition of region and education as control variables. Again, I confirm that black applicants do not significantly benefit from their same-race referrals, but that both unprejudiced ($\Delta\hat{y} = .82;\ p < .05$) and highly-prejudiced ($\Delta\hat{y} = .65;\ p < .05$) evaluators strongly reward whites' same-race referrals. Further, white applicants' significantly benefit from their cross-race referrals as long as they are evaluated by a relatively prejudiced respondent, and black applicants' benefit from cross-race referrals as long as they are evaluated by a relatively less-prejudiced evaluator. Specifically, evaluators with anti-black prejudice at or above .2 standard deviations below the mean significantly reward white applicants' cross-race referrals, and evaluators with anti-black prejudice up to .1 standard deviations above the mean significantly reward black applicants' cross-race referrals.

Finally, I examined whether other respondent characteristics (i.e. individual earnings, full-time employment status, supervisorial authority, self-employment, firm size, birthplace) confound the effect of IAT on returns to referrals. I did not expect these variables to confound the effect of IAT on returns to referrals, since I am unaware of research postulating a relationship between these characteristics and the IAT. Consistent with these expectations, all the study findings are robust to including these control variables in addition to age, gender, region, and education: I still find support for Hypothesis 1b ($p < .05$), against Hypothesis 2 ($p < .05$), and implicit prejudice continues to have a negligible effect on returns to same-race referrals. Further, again I find that relative to less-prejudiced respondents, more-prejudiced respondents reward black applicants' cross race referrals *less* ($p < .05$) and white applicants' cross-race referrals *more* ($p = .06$).

Table D1. Return to employee referrals, by applicant race (OLS regressions)

	Black applicants	White applicants	P-Value of difference
Anti-black prejudice	-0.32*	0.24	0.01
	(0.14)	(0.16)	
Age (years)	0.01	0.01	0.90
	(0.01)	(0.01)	
Male	0.53+	-0.47	0.02
	(0.31)	(0.30)	
Northeast	-0.17	0.09	0.64
	(0.39)	(0.41)	
Midwest	-0.46	0.82*	0.02
	(0.39)	(0.36)	
West	-0.87+	-0.05	0.20
	(0.47)	(0.42)	
Education (years)	-0.02	-0.08	0.63
	(0.08)	(0.08)	
Constant	0.41	0.54*	
	(0.29)	(0.24)	
Respondents	117	104	
R^2	0.09	0.12	

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. The left and middle column present OLS coefficients with standard errors in parentheses. These regressions are estimated separately for black and white applicants. The right-most column presents the p-value of tests of difference of each coefficient, by applicant race, estimated using a single model with the three predictors (prejudice, age, gender), applicant race, and the interaction of each predictor with applicant race. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.
$+ p < .1; * p < .05$ (two-tailed tests).

Table D2. Return to employee referrals, by referring employee race (OLS regressions)

	Black referrers	White referrers	P-Value of difference
Anti-black prejudice	0.08	-0.31*	0.07
	(0.15)	(0.15)	
Age (years)	0.00	0.02	0.41
	(0.01)	(0.01)	
Male	-0.65*	0.53+	0.01
	(0.30)	(0.29)	
Northeast	-0.75+	0.44	0.03
	(0.40)	(0.38)	
Midwest	-0.61	0.63+	0.02
	(0.39)	(0.35)	
West	-0.97*	-0.04	0.14
	(0.43)	(0.45)	
Education (years)	-0.07	-0.07	0.98
	(0.08)	(0.08)	
Constant	1.06**	0.15	
	(0.28)	(0.24)	
Respondents	94	127	
R^2	0.13	0.11	

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. The left and middle column present OLS coefficients with standard errors in parentheses. These regressions are estimated separately for black and white referrers. The right-most column presents the p-value of tests of difference of each coefficient, by referrer race, estimated using a single model with the three predictors (prejudice, age, gender), referrer race, and the interaction of each predictor with referrer race. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.

$+ p < .1$; $* p < .05$; $** p < .01$ (two-tailed tests).

Table D3. Return to employee referrals, by experimental condition (OLS regressions)

	Black applicants		White applicants	
	Black referrer	White referrer	Black referrer	White referrer
Anti-black prejudice	-0.15[c]	-0.52*[c]	0.46*[abd]	-0.08[c]
	(0.18)	(0.22)	(0.21)	(0.22)
Age (years)	0.02	0.01	-0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Male	-0.48[b]	1.13*[acd]	-0.53[b]	-0.06[b]
	(0.42)	(0.43)	(0.42)	(0.44)
Northeast	-0.56[d]	0.06	-1.10+[d]	1.07+[ac]
	(0.51)	(0.54)	(0.59)	(0.55)
Midwest	-1.60**[bcd]	0.11[a]	0.54[a]	0.96*[a]
	(0.52)	(0.53)	(0.52)	(0.47)
West	-1.54*[d]	-0.12	-0.38	0.07[a]
	(0.57)	(0.71)	(0.61)	(0.57)
Education (years)	-0.06	-0.11	-0.13	-0.02
	(0.11)	(0.12)	(0.11)	(0.12)
Constant	1.10**	-0.03	0.97*	0.27
	(0.39)	(0.39)	(0.36)	(0.32)
Respondents	52	65	42	62
R^2	0.25	0.19	0.37	0.14

Note: The return to employee referral is the within-respondent difference in the evaluation score of the referred applicant and the non-referred applicant. OLS coefficients with standard errors in parentheses. The regressions are estimated separately for each experimental condition. The evaluation score and anti-black prejudice measure are standardized. The baseline respondent is a woman of average age and anti-black prejudice for the sample.
[a] $p < .1$; coefficient is significantly different than black applicant/black referrer coefficient (two-tailed tests).
[b] $p < .1$; coefficient is significantly different than black applicant/white referrer coefficient (two-tailed tests).
[c] $p < .1$; coefficient is significantly different than white applicant/black referrer coefficient (two-tailed tests).
[d] $p < .1$; coefficient is significantly different than white applicant/white referrer coefficient (two-tailed tests).
+ $p < .1$; * $p < .05$; ** $p < .01$ (two-tailed tests).