

RACE, RECESSION, AND SOCIAL CLOSURE IN THE LOW-WAGE LABOR MARKET: EXPERIMENTAL AND OBSERVATIONAL EVIDENCE

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ABSTRACT

This paper tests whether employers responded particularly negatively to African American job applicants during the deep U.S. recession that began in 2007. Theories of labor queuing and social closure posit that members of privileged groups will act to minimize labor market competition in times of economic turbulence, which could advantage Whites relative to African Americans. Although social closure should be weakest in the less desirable, low-wage job market, it may extend downward during recessions, pushing minority groups further down the labor queue and exacerbating racial inequalities in hiring. We consider two complementary data sources: (1) a field experiment with a randomized block design and (2) the nationally representative NLSY97 sample. Contrary to expectations, both analyses reveal a comparable recession-based decline in job prospects for White and African American male applicants, implying that hiring managers did not adapt new forms of social closure and demonstrating the durability of inequality even in times of structural change. Despite this proportionate drop, however, the recession left African Americans in an extremely disadvantaged position. Whites during the recession obtained favorable responses from employers at rates similar to African Americans prior to the recession. The combination

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of experimental methods and nationally representative longitudinal data yields strong evidence on how race and recession affect job prospects in the low-wage labor market.

Keywords: Recession; hiring; racial inequality; social closure; labor queues

INTRODUCTION

Evidence abounds that race remains a “master status” (Hughes, 1945, p. 357) in American society, associated with discrimination across a wide range of socioeconomic outcomes (Pager & Shepherd, 2008). Moreover, race often intersects with other factors that exacerbate disadvantage, such as a criminal record (Pager, 2003) and low education (Wilson, Tienda, & Wu, 1995). Racial minorities have also experienced worse job loss rates than whites in the presence of society-wide labor market shifts, such as deindustrialization (Hill & Negrey, 2010), deunionization (Bound & Freeman, 1992), and outsourcing (Johnson, Burchley, & Ghorm, 2008). In light of emerging research on the group-specific effects of the Great Recession (Grusky, Western, & Wimer, 2011), we ask whether turbulent economic times intensify racial discrimination in hiring in the low-wage, unskilled job market. We gain purchase on this question by analyzing both a field experiment in which young African American and White men applied for entry-level jobs before and during a recession and a nationally representative longitudinal survey of young people.

Weber’s ([1922] 1968) concept of “social closure” describes processes in which dominant groups safeguard desirable positions by restricting entry to a limited circle of eligible workers (Murphy, 1988; Parkin, 1979; Weeden, 2002). This effect has been documented in cases of race and gender discrimination (Loveman, 1999; Reskin & Roos, 1990; Roscigno, Garcia, & Bobbitt-Zeher, 2007), such that “superordinate groups preserve their advantage by tying access to jobs or other scarce goods to group characteristics” (Tomaskovic-Devey, 1993, p. 9). As jobs become scarcer during a recession, dominant groups may seek to extend social closure to less desirable jobs. If so, recessions would worsen racial inequalities in hiring, such that African Americans would be excluded from a broader range of employment opportunities.

Alternatively, there are also reasons to expect a more proportional decline in hiring behavior during recessions. Weberian closure theory generally concerns the efforts of workers, rather than employers. Although workers in desirable positions may wish to minimize labor market competition, employers and hiring authorities do not share these interests. If employers’ preferences remain stable during recessions, then we would expect fewer workers of all groups to

be hired during such periods. We will test these ideas by specifying race-by-recession interactions using unique experimental and observational data collected before and during the recession.

In 2007 and 2008, we conducted a field experiment of low-wage, entry-level hiring. The Great Recession began at about the midpoint of our data collection. This economic shift within the field experiment allows us to test whether employers select only racially privileged applicants – those at the very front of the labor queue – from the many available workers during recessionary periods. We then analyze National Longitudinal Survey of Youth 1997 (NLSY97) data to validate the findings in the field experiment, examining weekly employment arrays from the year prior to the recession and the first year of the recession.

In what follows, we first review theory and research on the socioeconomic effects of racial discrimination, particularly in how race interacts with recessions and other sources of disadvantage and develop a set of hypotheses based on this previous work. We then exploit the unanticipated environmental change during our field experiment to estimate the causal effect of applicant race during a major economic decline, formally testing recession-based differences in employer “callbacks” by race. Next, we analyze observational data from the NLSY97 to test for racial differences in job search success before and during the recession. The results will thus demonstrate whether disadvantage in hiring is proportionate or pronounced; that is, whether African American disadvantage is due to the main effects of racial discrimination and the recession or to a heightened deficit resulting from their combination. In contrast, the absence of differential effects on race groups would suggest that inequality operates similarly within different economic contexts, demonstrating the durability and resilience of racial inequality (Tilly, 1999) even in the face of structural change.

THEORETICAL BACKGROUND

To situate our questions and hypotheses, we first consider the literature on racial discrimination and on how economic incidents like recessions affect hiring. We use these literatures to develop hypotheses based on the main effects of both race and recession on hiring outcomes. We then theorize the interaction between these effects, drawing upon theories of labor queues and social closure, developing further hypotheses regarding the interaction of these effects.

Racial Discrimination and Sources of Employment Disadvantage

The persistence of racial discrimination is among the most consistent findings in U.S. sociology, particularly for socioeconomic outcomes. With regard to labor markets, field experiments have consistently documented discrimination

in hiring (Bertrand & Mullainathan, 2004; Pager, 2007a; Pager, Western, & Bonikowski, 2009). Observational data show that this discrimination contributes to lower wages (Huffman & Cohen, 2004; Tomaskovic-Devey, Thomas, & Johnson, 2005), decreased home equity (Krivo & Kaufman, 2004), and health inequalities (Nazroo, 2003). We here concentrate on the persistent effects of racial discrimination in employment among younger and less-educated African American men (Pager, 2007a; Holzer, 2009). Hiring discrimination is especially consequential for such workers because it decreases human capital, leading to persistent socioeconomic differentials over the life course (Tomaskovic-Devey et al., 2005).

Human capital differences in education, work experience, and job training only partially explain race differences in employment outcomes (Kaufman, 2002; Moss & Tilly, 2001; Turner, Fix, & Struyk, 1991). Theories of statistical discrimination and labor market queuing help explain these residual differences. Statistical discrimination models posit that employers use race to draw “quick and dirty” assumptions about group differences in productivity and other characteristics, particularly when they lack detailed information about applicants (Arrow, 1973; Bielby & Baron, 1986; Braddock & McPartland, 1987; Moss & Tilly, 1996; Phelps, 1972; Tomaskovic-Devey & Skaggs, 1999). By assuming racial differences in productivity, employers can rapidly categorize workers based on their group membership, giving less attention to personal qualifications (Pager & Shephard, 2008).

In terms of the match of applicants to positions, racial characteristics can thus determine applicants’ relative position in a labor queue that reserves better-quality employment for more favored groups (Aigner & Cain, 1977; Kaufman, 2002; Reskin & Roos, 1990). This phenomenon deepens the concentration of African American and White workers in different occupations, reinforcing racial inequalities (see review by Reskin, 2012; Smith, 2002). As race-specific attributes are attached to specific occupations, employers rank applicants based on the match between their race and the presumed racial appropriateness of a specific occupation. Considering them together, statistical discrimination (Arrow, 1973; Phelps, 1972) and labor market queuing theories (Reskin & Roos, 1990) contend that an applicant’s position in line is shaped by the combination of their race, the racial characteristics attached to an occupation, and employer stereotypes and observations of race-employment patterning (Kaufman, 2002). This process results in “homosocial reproduction,” such that predominantly White employers tend to select predominantly White workforces (Kanter, 1977).

Based upon the strong experimental evidence on racial discrimination in hiring and theories of statistical discrimination and labor queuing, we offer the following hypothesis on the main effect of race on hiring:

Hypothesis 1. Employers will respond less favorably to African American job applicants than to White applicants.

The Varying Effect of the Great Recession on Individuals

During the recent Great Recession, individuals and families experienced higher unemployment, more erratic work patterns, decreased home values, lowered income and net worth, and higher indebtedness and bankruptcy rates (Carruthers & Kim, 2011). By definition (NBER, 2008), economic opportunity declines during recessions. Indeed, the Great Recession affected a host of economic indicators at both the aggregate and individual levels (Carruthers & Kim, 2011; Grusky et al., 2011), leading to the following hypothesis regarding the main effect of the recession:

Hypothesis 2. Employers will respond less favorably to applicants during official recessionary periods than during non-recessionary periods.

The recent economic downturn has had particularly severe consequences for some social groups (Grusky et al., 2011). Some researchers have dubbed it a “mancession,” with men facing higher job separation, lower chances of finding positions, and higher unemployment rates, partially due to the concentration of male job loss in particular sectors (Berthoud & Sosa, 2011; Hout, Levanon, & Cumberworth, 2011; Sierminska & Takhtamanova, 2011). Within families, women increasingly shared the burden by entering the labor force or increasing work hours (Mattingly & Smith, 2010). Young adults and those with only a high school education also faced disproportionate economic disadvantage (Berthoud & Sosa, 2011; Hout et al., 2011; Vuolo, Mortimer, & Staff, 2016; Wolff, Owens, & Burak, 2011). These combined effects led to a recession-induced increase in poverty, especially among young, unskilled men (Smeeding, Thompson, Levanon, & Burak, 2011).

African Americans were hit particularly hard from late 2007 to 2009. The foreclosure crisis was highly racialized, with those residing in segregated African American neighborhoods most likely to have received subprime mortgages and to have faced foreclosure (Rugh & Massey, 2010). At the individual level, African Americans experienced greater losses in terms of mortgage delinquencies, home equity, foreclosures, and personal bankruptcies (Wolff et al., 2011). Further, when each of the demographic categories noted above (i.e., gender, education, age) are broken down by race, African Americans are typically worse off in terms of unemployment and assets than their similarly situated counterparts of other races, especially Whites (Jacobsen & Mather, 2011). For males, Black-White disparities in economic outcomes during recessions are particularly pronounced (Sierminska & Takhtamanova, 2011). Based on the main effects of economic contraction and racial discrimination, African Americans are clearly in a disadvantaged position, but it is less clear whether their losses are disproportionate – a critical question for theory and policy.

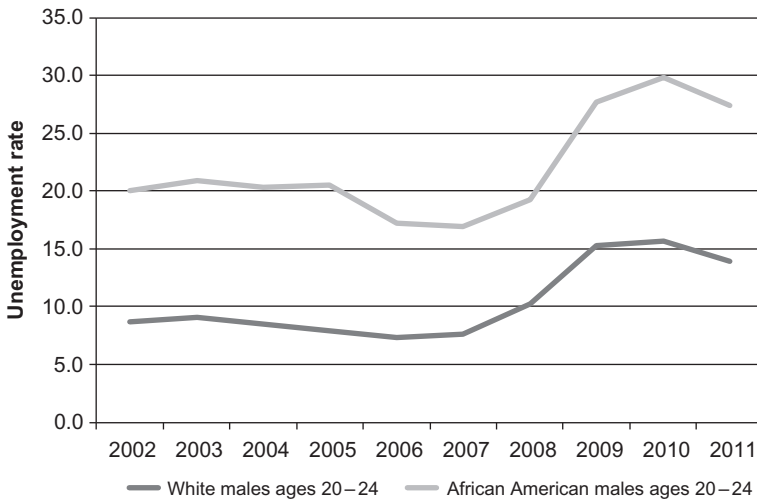


Fig. 1. Unemployment Rates for Males Ages 20–24 by Race. Source: U.S. Department of Labor, Current Population Survey, 2002–2011.

Some macro-level evidence can be brought to bear on this question. Fig. 1 shows the U.S. unemployment rate for males aged 20–24 by race. Although there is a distinct difference in levels, the trends for Whites and African Americans are quite similar. Using the difference between 2006 (the year before the recession began) and the 2010 peak, the White unemployment rate increased by a factor of 2.2 (from 7.3% to 15.7%). For African Americans, the rate increased by a factor of 1.7 (from 17.2% to 29.8%). Thus, the unemployment rate did not increase disproportionately for African Americans, although in absolute terms African American unemployment rose 12.6 percentage points compared to 8.4 percentage points for Whites.¹ Moreover, this measure obscures those who drop out of the labor force, which itself exhibits race differences (U.S. Bureau of Labor Statistics, 2012). Job loss, however, does not speak to individual-level hiring discrimination, a critical site for testing theories of labor queues and social closure.

Disproportionate Disadvantage: Labor Queues and Social Closure

The statistical discrimination literature suggests some characteristics that are particularly likely to interact with race. When combined with race, these stigmatizing characteristics push applicants even farther back in the labor queue by signaling individual differences in presumed productivity (Spence, 1973). For example, racial disparities are compounded by other bases of discrimination,

such as criminal records (Pager, 2007b; Pager et al., 2009) and gender (Greenman & Xie, 2008; Holzer, 2009), underscoring how disadvantage in other domains disproportionately affects African Americans. Yet, these individual-level characteristics do not speak to how recessions would affect labor queues.

Theories of social closure have been advanced to explain how members of a privileged group use categorical distinctions, such as race, to minimize the pool of potential rivals they must face (Reskin, 1988; Tilly, 1999; Tomaskovic-Devey, 1993; Weber, 1968). For example, there is considerable evidence that supervisory and management positions are dominated by Whites (Stainback & Tomaskovic-Devey, 2009), that White women are the dominant beneficiaries of diversity programs (Kalev, Kelly, & Dobbin, 2006), and that African Americans are relegated to the lowest-level supervisory positions, where the workforce is predominantly African American (Elliott & Smith, 2001; Stainback & Tomaskovic-Devey, 2009). While social closure has thus been applied largely to the professional and managerial job market, as well as those requiring proven vocational skills (Bol & Weeden, 2015), there is also reason to apply this logic to the unskilled, low-wage labor market, particularly when jobs are in short supply.

In the less desirable unskilled, low-wage labor market where racial integration has been highest (Tomaskovic-Devey et al., 2006), closure may operate differently (Reskin & Roos, 1990). Pressures for exclusion are weaker for less desirable positions, where Whites may not always gain preference over African Americans. In fact, in this segment of the job market, African Americans exhibit strong personal networks from which they can gain job referrals, though this does not always translate into hiring (Fernandez & Fernandez-Mateo, 2006; McDonald, Lin, & Ao, 2009). Even when Whites and African Americans are successful at gaining employment, job channeling can produce inequalities. In the unskilled job market, Whites are more likely to be channeled upward to a higher position than is advertised, while African Americans are more likely to be channeled downward (Pager et al., 2009).

How do recessions affect social closure? Social closure is both a conscious and unconscious process (Roscigno, 2007; Stainback, 2008; Weber, 1968), and active closure is strongest when status or class advantages are challenged (Chamberlain, Crowley, Tope, & Hodson, 2008; Hodson, Roscigno, & Lopez, 2006; Roscigno, Lopez & Hodson, 2009; Stainback, Tomaskovic-Devey, & Skaggs, 2010; Uggen & Blackstone, 2004). Some research finds that employment segregation declines as demand for workers rises (Szafran, 1982, 1984; see Reskin, 1993). Correspondingly, when many workers are available for few positions, employers will select only those most-preferred applicants at the front of the queue. By this logic, in a labor market flush with jobseekers, the combined effects of human capital and race may further displace younger and less-educated African American males while simultaneously extending White applicants' privileged position in the labor queue as they also begin to seek low-wage (or any) labor. Following Weber (1968), if the dominant status group clings more tightly

to the available positions during recessions, they will reserve even the least desirable jobs for in-group members. While “individualistic” characteristics such as educational credentials, trade union membership, and professional licensure can be used for social closure in the professional and skilled job market (Weeden, 2002), if employment becomes scarce even in the unskilled labor market, easily ascertained ascribed characteristics (such as race) may become a de facto differentiator for superordinate groups (Weber, 1968). In such a scenario, we expect that African American disadvantage in hiring will be further compounded during a recession as Whites displace African Americans in the low-wage labor queue (Reskin & Roos, 1990). This scenario leads to the following hypothesis:

Hypothesis 3_A. Race differences in hiring outcomes will be significantly greater during recessions than in non-recessionary periods.

Social closure usually considers *workers'* actions to exclude out-group members, but this hypothesis suggests that employers and managers would similarly act to exclude African American applicants as the labor market slackens. That is, the current regime of social closure would shift during recessions from reserving more desirable positions to reserving *all* positions for in-group members, but only if hiring authorities participate in this shift. If not, we would expect proportional losses during the recession.

Proportionate Disadvantage: Durability of Inequality

Broad structural changes, such as deindustrialization, deunionization, and outsourcing, often specifically disadvantage African Americans (Bound & Freeman, 1992; Hill & Negrey, 2010; Johnson et al., 2008; McKee, 2008; Sugrue, 2005). Phenomena such as these may bear little on statistical discrimination as they carry no signal regarding worker productivity, though they clearly affect the length of labor queues and the size of the labor pool. More generally, society-wide phenomena (like economic recessions) should be less stigmatizing than individual-level characteristics, such as criminal records and the lack of educational credentials.

Whether the Great Recession of 2007–2009 systematically disadvantaged African Americans remains an open question. Because researchers cannot foresee and manipulate recessions, the extant research has emphasized historical and observational analysis of job loss, greatly complicating efforts to draw causal inferences about racial discrimination in hiring. Beyond methodological barriers to studying structural changes like recessions, it may also be true that the “resilient nature of inequality” across time and various structural contexts will result in similar hiring trends by race in recessionary periods (Tilly, 1999; Wilson, Roscigno, & Huffman, 2015).

Indeed, Tilly's (1999) work on durable inequality provides theoretical expectations for a similar pattern of hiring during recessionary periods. Theories of social closure draw heavily on the actions of workers, rather than managers, to restrict outsiders to privileged positions through processes such as licensures. Tilly states, however, that these wider institutionalized forms of social closure legitimate staffing practices for organizations and their hiring managers. That is, organizational isomorphism results from widely disseminated routines and models. In fact, organizations would prefer to avoid costs that may result from experimenting with and developing new hiring structures that would require re-legitimation. Thus, even in the face of a recession, hiring managers would still default to their typical hiring practices regarding the likelihood of hiring those from subordinate subpopulations. If inequality remains durable regardless of the structural conditions due to prior legitimized hiring practices (Tilly, 1999), we expect African Americans to receive a proportionate amount of employment opportunities in the low-wage labor market both prior to and during a recession.

Indeed, there is no evidence in studies of past recessions that African Americans are the last hired in the queue of available positions (Couch & Fairlie, 2010). This would suggest equivalent losses for Whites and African Americans based on societal shifts (such as recessions) and disproportionately worse losses for racial minorities with individual-level stigmatizing characteristics (such as criminal records). This leads to the following null hypothesis:

Hypothesis 3₀. Race differences in hiring outcomes will not differ significantly during recessions relative to non-recessionary periods.

We offered hypotheses regarding three effects on hiring behavior: the main effect of race, the main effect of recession, and the interaction effect between race and recession. The latter, also known as a difference-in-difference (because it measures the difference between race categories in the recession-induced drop in hiring), considers whether race effects are more pronounced during a recessionary period. We note that the combined effects of Hypotheses 1 and 2 would result in considerable disadvantage for African American applicants during a recession. It is unknown, however, whether the recession would further disadvantage African American applicants beyond these main effects; that is, whether there is a significant difference-in-difference.

In line with our hypotheses, we next examine two specific interactions of race and the recession: (1) the likelihood that applicants receive employer callbacks in a field experiment; and, (2) the likelihood that unemployed jobseekers find work in a nationally representative survey. The field experiment examines high school educated young men applying to unskilled entry-level positions in a single metropolitan area, thus holding other potential sources of disadvantage during the recession constant (male, unskilled, non-college youth). Because it straddled the official start of the recession, this study adds new experimental evidence to a literature that has thus far relied solely on observational analysis

of survey data. We test the robustness of our experimental findings using the NLSY97 survey, analyzing the odds that unemployed high school educated young men gain employment. We take advantage of weekly employment arrays to assess job acquisition in the year before the recession and first year of the recession, mirroring our experiment to as great a degree as possible, while adjusting for the varying qualifications of respondents. Consistent findings in both analyses would lend greater support to our hypothesis tests.

METHODS

Field Experiment

From August 2007 to June 2008, we conducted a field experiment on the effect of low-level criminal records and race on receiving a callback for entry-level positions from employers in the Minneapolis-St. Paul-Bloomington metropolitan area. The Great Recession officially began during the course of the study, providing a rare opportunity to experimentally assess race differentials in recession effects. In the original study design, pairs of applicant “testers” of the same race were sent to job sites to apply for employment, with one presenting a fabricated criminal record. For this study, we use only the applicant in the pair that presented no criminal record, so that our design conforms to a randomized complete block design (Cox, 1958). That is, race of the applicant, as the experimental treatment, is randomly assigned to each employer in our sample. Because the recession cannot be experimentally manipulated and there is random assignment of race to employers pre-recession and during the recession, the recession effect represents a “block.”²

As in our original design, many field experiments on racial discrimination employ a “matched” approach (Pager, 2007a), which we now lack. The matched approach, however, is not necessary unless one wishes to account for employer effects (e.g., by including employer-specific intercepts for employers who call back both or neither testers (see Vuolo, Uggen, & Lageson, 2016; Agresti, 2002, pp. 410–411, 467–468, 493–501)).³ Such approaches are preferable when race cannot be randomly assigned (Pager, 2007a, p. 123), as is the case when the unit of analysis is applicants. With the employer as the unit of analysis, however, race is randomly assigned to the *employer being sampled*.⁴ In this randomized complete block design, as in other experimental approaches, the randomization process should ensure no systematic bias in that assignment (Cox, 1958).⁵

Sampling and Experimental Procedures

Our experimental unit (to which applicant race is randomly assigned) is the employer, with effects blocked by the recession. The research team directed

testers to apply to positions, based on available job postings. Each Sunday, the job classifieds from five print sources (*Minneapolis Star Tribune*, *St. Paul Pioneer Press*, *Employment News*, *Employment Guide*, and *JobDig*) and one online source (*Craigslist*) were reviewed. All entry-level advertisements were included in the sample, as long as they had no special skill requirements (such as a specialized license to operate machinery), instructed applicants to apply in-person, and were located in the seven-county, Twin Cities metropolitan area.⁶ Eight male college students in their early twenties were selected as testers.⁷ We fabricated tester biographies to be similar in every regard, except for race identifiers (for instance, African American resumes indicated participation in African American student groups, helping to convey race to employers). These biographies presented similar working-class education, employment, and personal backgrounds.⁸ Simple randomization was used to allocate the jobs among eight applicant testers, each of whom applied for an average of seven jobs per week. Upon arriving at the application site, testers completed applications, submitted their resumes, and attempted to maximize personal contact by asking to speak to the manager. The latter further ensured that the race of the tester was conveyed to the hiring authority. Our total sample includes 605 employers, with each of the eight testers completing approximately 75 tests.

Variables: Dependent (Callback), Experimental (Race), and Blocking Effects (Recession)

The primary dependent variable in our experimental analysis is an employer “callback.” A callback represents a tangible positive response from an employer: an on-site job offer, an on-site offer for an interview, a job offer through voicemail, an offer for an interview through voicemail, or a call from an employer for something beyond a reference or request for basic information. Callbacks were tracked for four weeks after each tester left the field.

Our main experimental treatment is the randomly assigned race of the applicant. For the recession block, we constructed an indicator variable for the block in which the test occurred. According to the U.S. National Bureau of Economic Research (the official arbiter of U.S. recessions), the Great Recession began in December 2007 (NBER, 2008) and lasted until June 2009 (NBER, 2010). Formally, NBER defines a recession as “a significant decline in the economic activity spread across the economy, lasting more than a few months, normally visible in production, employment, real income, and other indicators” (2008, p. 1). For our purposes, all tests that occurred from August 2007 to November 2007 were coded as pre-recession, and all tests from December 2007 to June 2008 were coded as occurring during the recession.

Though the precise timing of the recession varied to some extent across regions, Fig. 2 shows that Minnesota generally tracked the nation in entering the recession (Grunewald & Madden, 2009). After a long period of relative prosperity, unemployment began rising at the start of the recession in December

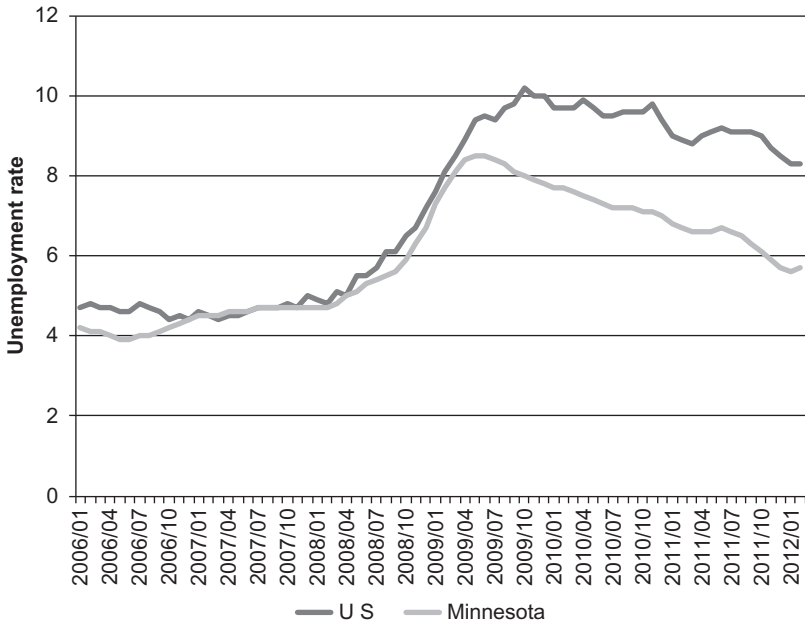


Fig. 2. Unemployment Rate in Minnesota and the United States, 2006–2012. *Source:* Minnesota Department of Employment and Economic Development, 2006–2012.

2007, in the midst of our experiment. Minnesota and its Federal Reserve District may have recovered more quickly than other areas (Grunewald & Madden, 2009), although Minnesota followed national trends through at least mid-2009.

For a randomized complete block experimental design, the method of analysis is typically analysis of variance and linear regression. Because our outcome is dichotomous, we instead use standard logistic regression models. We employ several alternative coding schemes for race and recession effects in order to assess the magnitude and significance of both difference-in-differences and between-group differences. We present models with control variables to adjust for the possibility of incomplete randomization and to assess the strength of the treatment and block effects. These include the source of the advertisement (online vs. paper), whether the test occurred in Minneapolis-St. Paul or a suburb, the industry, whether testers made contact with a hiring authority, and whether they observed persons of color working in the establishment. Our randomization procedure leads us to expect no relationship between the control variables and the experimental and blocking effects.

Survey Data: National Longitudinal Survey of Youth 1997 (NLSY97)

The NLSY97 is a nationally representative survey of U.S. residents born from 1980 to 1984; that is, ages 12–16 in 1997 (U.S. Department of Labor, 2003). Conducted by the U.S. Bureau of Labor Statistics, the survey's primary goal is to collect information on labor market behavior and educational experiences. The survey began in 1997 with a sample of 8,984 respondents. We examine weekly and monthly arrays and individual questions from the annual surveys taken before and during the Great Recession (from 2005 to 2008). Across those survey years, retention rates varied from 81.8% to 83.3%. In 2007, the year the recession began, respondents were between the ages of 21 and 26. We selected a subset of respondents to closely resemble the biographies presented by our testers in the field experiment. Thus, our analyses included only those unemployed in either the pre-recession or recession periods, males, Whites and African Americans, and those with no more than a high school education.⁹

Given our interest in the two periods, we reorganized the data as a two-observation panel data structure. To track unemployment¹⁰ and job acquisition, we use the NLSY97's weekly arrays of employment data. We define the pre-recessionary period as the 52-week array occurring prior to December 2007, with the subsequent 52-week array defined as during the recession.¹¹ Within our subset, there are 772 unique individuals and 976 observations.¹²

Variables and Statistical Analyses

The dependent variable in the NLSY97 analysis is job acquisition. A respondent was coded as acquiring a job if they were coded in the employment array as unemployed in a given week and then coded as having a job in a subsequent week in the period. Unlike our experiment, survey respondents apply for jobs with widely varying work histories, many of which are related to both race and recession. We therefore include control variables, such as the weeks worked in the year prior to the start of each period, again based on the weekly employment arrays. Given the non-normal and non-uniform distribution of this predictor, we use four categories evenly dividing the 52-week period. We also adjust for the industry and occupation of the last job held, using the socioeconomic index (SEI) for males developed by Hauser and Warren (1997),¹³ reported income in the preceding year,¹⁴ receipt of unemployment insurance, and two measures of educational performance and participation. For the latter, these include self-reported high school academic performance¹⁵ and a dummy variable indicating any college attendance.¹⁶ Finally, we include an indicator of whether the respondent reported being convicted of any crime in the last year, which is known to affect job prospects (Pager, 2007b).

Since the effect of the recession varies regionally, we include indicator variables for U.S. Census Region, as well as an indicator for urban location, relative to rural. We also include several life course indicators that may affect

gaining employment, including migration to a different county within the last year, parenthood (differentiated by child's residential status), and marital status. As a measure of socioeconomic background of origin, we control for parents' highest education, coded from years of education reported by the parents themselves. Finally, we include age as a control due to the five-year age range included in the NLSY97.

With partially dependent data structured with two observations, we use the "xtgee" procedure in Stata 14.0 to estimate Generalized Estimating Equations (GEE) with a logit link and robust standard errors.¹⁷ We use population averaged models because we wish to compare two like individuals of different races (i.e., an "average" White applicant and an "average" African American applicant). The same logic applies to the recession effect.¹⁸

FIELD EXPERIMENT RESULTS

Main Effects and Difference-in-Difference

Model 1 in Table 1 shows the main effects of the experimental treatment of race and the block effect of the recession. Both are statistically significant. Consistent with research on racial discrimination in hiring and supporting Hypothesis 1, Whites are 85% more likely to receive an employer callback than African Americans ($p < .01$). Supporting Hypothesis 2, there is also a substantial recession effect; applicants are 53% less likely to receive a callback during the recession than they are in the four months preceding the recession ($p < .001$).

Model 2 shows that these effects are robust to several control measures. The only significant new predictor is contact with the hiring authority. Testers who made contact with the hiring authority were 3.2 times as likely as others to receive a callback.¹⁹ Even with the control measures, however, the magnitude and significance of the race and recession effects are little changed. This stability in the coefficients and standard errors is due to the small amount of covariation between the controls and the race and recession effects, further attesting to successful randomization. Fig. 3 shows the predicted probabilities derived from Model 1, which mirror the raw callback rate. Prior to the recession, Whites received callbacks 44% of the time, compared to 30% for African Americans. During the recession, employers called back Whites 27% of the time, compared to 17% for African Americans. The recession-era White callback rate is thus nearly equivalent to the pre-recession African American rate.

Before explicating these between-group differences, we consider Hypothesis 3, testing whether the declining rate of callbacks during the recession was of greater magnitude for African Americans than for Whites. For African Americans, the callback rate dropped 13 percentage points or 44% $((29.5 - 16.6)/29.5 = 43.7)$, relative to a 17 percentage point or 38% decrease

Table 1. Experiment Logistic Regressions for Callback Received Testing Main Effects and Difference-in-Difference.

	Model 1		Model 2		Model 3		Model 4	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
(Intercept)	-0.870*** (0.202)		-1.170*** (0.302)		-0.801** (0.257)		-1.140*** (0.349)	
White (vs. African American)	0.618** (0.206)	1.854	0.610** (0.214)	1.841	0.528 (0.294)	1.695	0.573 (0.307)	1.773
Recession (vs. pre-recession)	-0.744*** (0.203)	0.475	-0.798*** (0.219)	0.450	-0.845** (0.312)	0.430	-0.839** (0.327)	0.432
White × recession					0.173 (0.409)	1.189	0.073 (0.428)	1.076
Contact with hiring authority			1.124*** (0.204)	3.076			1.123*** (0.204)	3.074
Online source (vs. paper)			0.024 (0.228)	1.024			0.023 (0.228)	1.023
In MSP (vs. suburbs)			-0.056 (0.210)	0.946			-0.059 (0.210)	0.946
Observed minority employees			0.111 (0.202)	1.118			0.110 (0.202)	1.116
<i>Industry (vs. restaurant)</i>								
Office work			-0.614 (0.475)	0.541			-0.613 (0.475)	0.542
Retail			-0.195 (0.280)	0.823			-0.198 (0.280)	0.820

Table 1. (Continued)

	Model 1		Model 2		Model 3		Model 4	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
Healthcare			0.569 (0.730)	1.767			0.568 (0.730)	1.764
Warehouse/labor			-0.361 (0.292)	0.697			-0.359 (0.292)	0.698
Hotel			0.176 (0.327)	1.193			0.175 (0.327)	1.191
Driver			0.169 (0.492)	1.182			0.164 (0.493)	1.179
Security			-0.525 (0.814)	0.592			-0.529 (0.814)	0.589
Log-likelihood	-343.42		-321.94	-343.33			-321.90	
Model χ^2 (df)	39.16*** (2)		81.48*** (13)	39.34*** (3)			81.51*** (13)	

Note: Industry differences are non-significant regardless of baseline.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

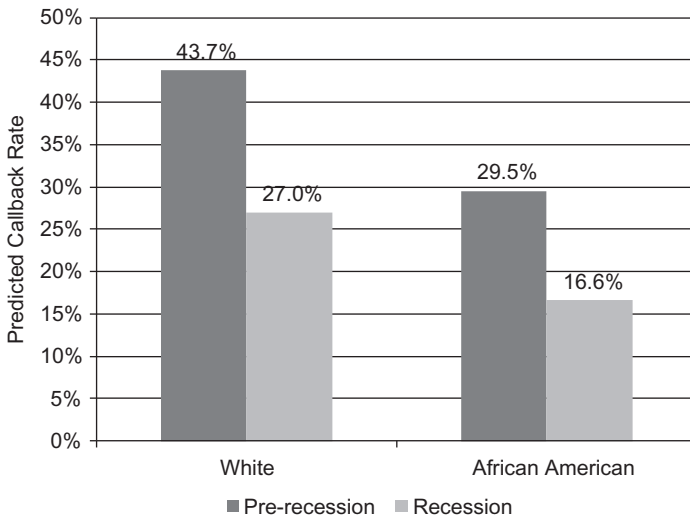


Fig. 3. Experiment Predicted Callback Rate by Race and Recession (Derived from Table 1, Model 1).

for Whites ($((43.7 - 27.0)/43.7 = 38.2)$). Model 3 of Table 1 formally tests this difference-in-difference, indicating no significant racial difference in the callback decline between the two periods. We therefore cannot reject the null hypothesis (Hypothesis 3₀) that the recession affected young African American and White men equally, at least in terms of their ability to garner a favorable employer response.²⁰ Model 4 demonstrates that inclusion of our control measures does not alter this conclusion, with a coefficient for the difference-in-difference close to zero. Of course, the pre-recession starting point was much lower for African Americans than for Whites. We next explore such between-group differences.

Between-Group Differences

In Table 2, we modify the coding of race and recession effects to test whether the differences between each of the four groups in Fig. 3 are statistically significant. Model 5 thus contains the same information as Model 3 in Table 1 (as evidenced by the equivalent log-likelihood and model chi-squared), but uses “simple” coding (see Cohen, 1968) against a new baseline – that of African American testers during the recession period. This model confirms that all other race/recession groups are significantly more likely to receive a callback than are African Americans during the recession. Whites in the recession are twice as likely to receive a callback ($p < .05$); African Americans before the recession are 2.3 times as likely to receive a callback ($p < .01$); and, Whites before the

Table 2. Experiment Logistic Regressions for Callback Received Testing between-Group Differences by Race and Recession.

	Model 5 (No Controls)		Model 6 (Controls)	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
(Intercept)	-1.646*** (0.177)		-1.980*** (0.300)	
<i>Race by recession (vs. African American, during recession)</i>				
White, during recession	0.701* (0.286)	2.016	0.646* (0.298)	1.908
African American, pre-recession	0.845** (0.312)	2.328	0.839** (0.328)	2.315
White, pre-recession	1.373*** (0.228)	3.946	1.412*** (0.245)	4.105

Note: Since these models are only a change in coding of race and recession, the log-likelihood, Model χ^2 , and estimates of the controls for Model 6 are the same as Model 4. See Table 1, Model 4 for those values.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

recession are almost 4 times as likely to receive a callback ($p < .001$), all relative to African American applicants during the recession. Again, these estimates are little changed by the addition of covariates in Model 6.

Table 3 shows each of the possible comparisons between the groups in our field experiment. The first row is described above. When the baseline is shifted to Whites during the recession, Whites pre-recession are about twice as likely to receive a callback ($p < .05$). As evidenced in Fig. 3, the difference between Whites *during* the recession and African Americans *pre*-recession is small and not statistically significant, indicating near equality in their viability as job candidates. Finally, Whites pre-recession are about 70% more likely to receive a callback than African Americans pre-recession, though this effect is only marginally significant ($p = .07$).²¹

Three main conclusions emerge from the models. First, the recession appears to have had a comparable effect on White and African American applicants, with prospects for young men of both races dimming by approximately equal amounts as would be expected by theories of durable inequality. Both before and during the recession, however, these prospects were much lower for African Americans than for Whites, leading to our second conclusion: that African Americans *prior* to the recession had approximately the same probability of getting a favorable employer response as Whites *during* the recession. Finally, African Americans during the recession are clearly in the most

Table 3. Odds Ratios for All Between-Group Differences by Race and Recession from Field Experiment and NLSY97.

	Comparison		
	White, Recession	African American, Pre-recession	White, Pre-recession
<i>A. Field experiment (callbacks)</i>			
Baseline			
African American, recession	2.02*	2.33**	3.95***
White, recession		1.15	1.96*
African American, pre-recession			1.70 [#]
<i>B. NLSY97 Survey (job acquisition)</i>			
Baseline			
African American, recession	1.82*	1.66**	3.23***
White, recession		0.91	1.77*
African American, pre-recession			1.94*

Note: Field experiment odds ratios come from Table 2, Model 5. NLSY97 survey odds ratios come from Table 5, Model 12.

[#] $p = .07$.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

disadvantaged position due to the main effects of both race and recession. Compared to all other groups, such applicants have much lower odds of receiving callbacks from employers. Relative to all other race/recession combinations, African Americans during the recession were 63% less likely to receive a callback ($p < .001$).²² In short, the estimated recession effect is constant by race. Nevertheless, racial differences in hiring that are independent of the recession cause African Americans to be highly disadvantaged during periods of economic contraction.

NLSY97 SURVEY RESULTS

Main Effects and Difference-in Difference

To test the results of our field experiment on a nationally representative sample, we next analyze NLSY97 data for those unemployed males with only a high school education seeking employment in the year prior to and during the

recession. Irrespective of race, the percentage acquiring employment is higher in the year prior to the recession (76%) than during the recession (67%). There are, however, considerable differences by race. Prior to the recession, 83% of unemployed Whites gained employment, while only 67% of African Americans did so. During the recession, this number decreases to 77% for Whites and 56% for African Americans. These probabilities mirror those from a logistic regression with no controls, as shown in Model 7 of Table 4. Here, Whites are 2.6 times as likely as African Americans to acquire employment ($p < .001$) and jobseekers during the recession are 33% less likely to find work than those in the pre-recession period ($p < .01$). Model 8 confirms that the difference-in-difference is non-significant. Unlike the field experiment, White and African American applicants, and potentially those applying before and during a recession, present varying qualifications to employers. We therefore use covariate-adjustment to better estimate the difference between White and African American males of similar qualifications, and to better understand the difference-in-difference between the two periods.²³ Indeed, the race effect decreases when controls are added to the model, as some of the unadjusted race effect is due to differences on the controls.

Models 9 and 10 in Table 4 depict the covariate-adjusted models. The results for the main effects of race and recession in Model 9 again lend support to Hypotheses 1 and 2. Net of all controls, White males are 88% more likely to acquire a job than African American males ($p < .01$), while those applying during the recession are 41% less likely to gain employment ($p < .01$). Among the significant control variables, those respondents working more weeks in the previous year are more likely to acquire a job, as are those with higher incomes. Those who migrated in the last year between counties were 76% more likely to get a job, while those convicted of a crime were 54% less likely to find employment.²⁴

Fig. 4 shows the predicted probability of job acquisition from Model 9 while holding the control variables constant at their respective means – a comparison of equally qualified candidates of each race in both periods. In the year prior to the recession, unemployed White males acquired a job 85% of the time, compared to 75% for African Americans. During the recession, Whites acquired employment 77% of the time, compared to 64% for African Americans. As with the callback rate in the experiment, rates for Whites during the recession were nearly equivalent to those of African Americans prior to the recession.

Model 10 presents the difference-in-difference. For African Americans, the callback rate dropped 8 percentage points or 15% $((75.0 - 63.8)/75.0 = 15.0)$, relative to an 11 percentage point or 10% decrease for Whites $((84.9 - 76.8)/84.9 = 9.6)$. The coefficient for the race-by-recession interaction in Model 10 testing this difference-in-difference is not statistically significant and close to zero in magnitude (and in a direction opposite to our hypothesis, as demonstrated by the higher percentage point reduction for Whites). As in the

Table 4. NLSY97 GEE Logit for Acquired Employment Testing Main Effects and Difference-in-Difference with Robust SEs.

	Model 7		Model 8		Model 9		Model 10	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
(Intercept)	0.690*** (0.122)		0.706*** (0.138)		1.331 (1.599)		1.313 (1.606)	
White (vs. African American)	0.963*** (0.153)	2.619	0.923*** (0.217)	2.517	0.630** (0.202)	1.877	0.665* (0.270)	1.944
Recession (vs. pre-recession)	-0.405** (0.144)	0.667	-0.436* (0.189)	0.647	-0.534** (0.181)	0.586	-0.507* (0.229)	0.602
White × recession			0.074 (0.291)	1.077			-0.066 (0.336)	0.936
<i>Weeks worked in previous year (vs. 0–13)</i>								
14–26					0.576* (0.267)	1.779	0.577* (0.267)	1.781
27–39					0.946*** (0.276)	2.575	0.947*** (0.276)	2.577
39–52					0.858*** (0.230)	2.357	0.858*** (0.229)	2.358
Received unemployment insurance					0.654 (0.426)	1.922	0.653 (0.426)	1.921

Table 4. (Continued)

	Model 7		Model 8		Model 9		Model 10	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
<i>Industry of last job (vs. none in two years)</i>								
Construction					0.153 (0.414)	1.165	0.154 (0.414)	1.167
Manufacturing					-0.023 (0.413)	0.977	-0.023 (0.413)	0.977
Retail trade					0.063 (0.418)	1.066	0.068 (0.419)	1.070
Transportation and warehousing					0.070 (0.504)	1.072	0.076 (0.504)	1.079
Professional and related services					-0.095 (0.403)	0.909	-0.094 (0.402)	0.910
Entertainment, accommodations, & food services					0.022 (0.380)	1.023	0.023 (0.380)	1.023
Other					0.076 (0.391)	1.079	0.074 (0.392)	1.077
Hauser & Warren SEI Male					0.015 (0.010)	1.015	0.015 (0.010)	1.015
Income (thousands)					0.030*** (0.009)	1.030	0.030*** (0.009)	1.030
High school grades					-0.032 (0.052)	0.969	-0.032 (0.052)	0.969

Ever attended college	0.052 (0.195)	1.053	0.052 (0.195)	1.053
Convicted of crime in last year	-0.778** (0.293)	0.459	-0.779** (0.294)	0.459
Age	-0.085 (0.061)	0.918	-0.085 (0.061)	0.918
<i>Census region (vs. Northeast)</i>				
North Central	0.179 (0.263)	1.196	0.178 (0.263)	1.194
South	0.118 (0.232)	1.126	0.118 (0.232)	1.125
West	0.097 (0.299)	1.101	0.097 (0.300)	1.102
Urban (vs. rural)	0.239 (0.195)	1.270	0.240 (0.195)	1.271
Migrated between counties	0.563* (0.226)	1.756	0.562* (0.226)	1.754
<i>Children (vs. none)</i>				
Non-residential	0.059 (0.195)	1.061	0.061 (0.195)	1.063
Residential	0.343 (0.257)	1.410	0.344 (0.257)	1.410
Married	0.024 (0.245)	1.024	0.023 (0.244)	1.023

Table 4. (Continued)

	Model 7		Model 8		Model 9		Model 10	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
<i>Parents' education (vs. HS or less)</i>								
Some college or associates					-0.030 (0.197)	0.970	-0.029 (0.197)	0.971
Bachelors or higher					0.303 (0.280)	1.354	0.303 (0.280)	1.353
No parent information					0.269 (0.305)	1.309	0.267 (0.305)	1.306
Model chi-squared	46.66*** (2)		47.29*** (3)		118.22*** (30)		118.09*** (31)	

* $p < .05$.** $p < .01$.*** $p < .001$.

experiment, we again cannot reject the null hypothesis (3₀) of an equal reduction in job prospects between the two periods by race.

Between-Group Differences

We again repeated the analyses using simple coding to compare the race by recession groups, as shown in Table 5. We concentrate on the covariate-adjusted estimates in Model 12. Relative to the baseline of African Americans during the recession, Whites during the recession were 82% more likely to find work ($p < .05$). African Americans prior to the recession were 66% more likely to acquire a job compared to their counterparts during the recession ($p < .05$). Finally, Whites before the recession were 3.2 times more likely to find work than African Americans during the recession ($p < .001$).

Table 3 shows all possible NLSY97 race by recession group comparisons. The first row repeats the results of Model 12. The next row changes the baseline to Whites during the recession, showing that pre-recession Whites were 77% more likely to find jobs ($p < .05$). The contrast between recession-period Whites and pre-recession African Americans is not statistically significant, with an odds ratio close to 1. Again, this effect size supports the idea that African

Table 5. NLSY97 GEE Logit for Acquired Employment Testing between-Group Differences with Robust SEs.

	Model 11 (No Controls)		Model 12 (Controls)	
	Coefficient (St. Error.)	Odds Ratio	Coefficient (St. Error.)	Odds Ratio
(Intercept)	0.271* (0.132)		0.806 (1.653)	
<i>Race by recession (vs. African American, during recession)</i>				
White, during recession	0.997*** (0.205)	2.711	0.599* (0.256)	1.820
African American, pre-recession	0.436* (0.189)	1.546	0.507* (0.229)	1.661
White, pre-recession	1.358*** (0.213)	3.890	1.172*** (0.272)	3.229

Note: Since these models are only a change in coding of race and recession, the Model χ^2 and estimates of the controls for Model 12 are the same as Model 10. See Table 4, Model 10 for those values.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

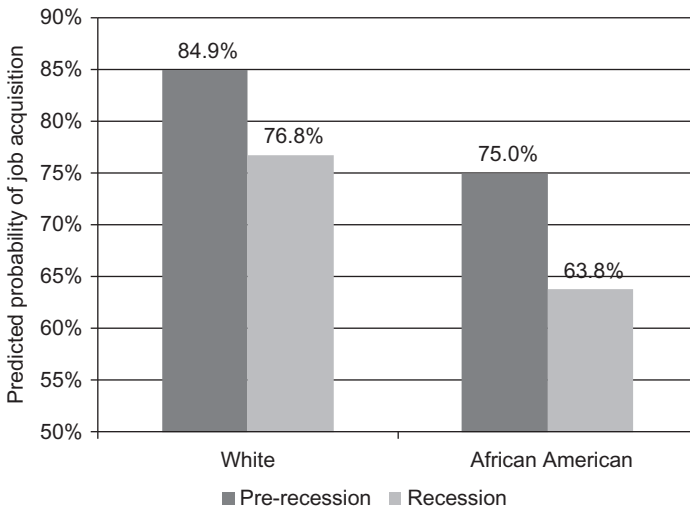


Fig. 4. NLSY97 Predicted Probability of Job Acquisition (Derived from Table 4, Model 9).

American job opportunities in good times approximate those of Whites in hard times. Conversely, in the pre-recession period, Whites were 94% more likely to acquire employment than African Americans ($p < .05$).

All three main experimental findings are thus replicated in the nationally representative survey. First, the prospects of gaining employment decreased during the recession by similar amounts for both White and African American unskilled job applicants. Second, Whites *during* a recession (77%) gain favorable employer responses about as often as African Americans *prior* to a recession (75%). Finally, although the drop across the periods is roughly proportional, the main effects of race and recession combine to put African Americans in the most disadvantaged position. African Americans' lower odds of finding work will likely translate into a diminished socioeconomic future and greater difficulty recovering from the recent deep recession.

LIMITATIONS AND STRENGTHS

This multi-method analysis has limitations that merit discussion, though the limitations of one approach are often the strengths of the other approach. First, we use a simple indicator variable to mark the recession, though recessions represent complex combinations of economic variables over long periods of time (NBER, 2008). Yet, our indicator approach has the advantage of conforming to an established experimental design, which we then mimic in the

survey data. People also identify and experience recessions as a qualitative state (yes/no) rather than a continuum and this label has its own implications for behavior, with consequences for employers and applicants. Nevertheless, as shown in Table 6, we obtain similar results when we block on specific months in the experiment rather than the recession dichotomy.

Table 6 shows that the drop in callback rates begins immediately in December with the official start of the recession and remains consistent in magnitude across the recession months. Model comparisons show, however, that the more complex model with month blocks does not fit the data better than the dichotomous recession indicator ($\chi^2 = 6.03$, $df = 9$, $p = 0.74$). By most indicators, the recession deepened throughout 2008 and much of 2009, so a longer observation period may have altered our results, particularly if dominant status groups feel increasingly threatened as the recession progressed and if hiring practices alter to incorporate such social closure. Although Figs. 1 and 2 show the unemployment rate increasing substantially after our experiment ended, we emphasize that unemployment is just one measure of the presence of a recession. We also emphasize that our analyses focus on *hiring outcomes* rather than job loss, which captures the immediate effects of the recession. Indeed, we observed strong and immediate recession effects on hiring, with callback rates dropping precipitously in the first month of December 2007. While we are limited by the end date of our experiment, we tested this idea by analyzing the second year of the recession as a third time point in the NLSY97 panel models, which again supports a proportionate decrease for both White and African American young men.²⁵

Second, our experimental findings pertain to a single metropolitan area. Given the high ratio of African American-to-White unemployment in the Twin Cities (Austin, 2011, 2012), however, this area provides an important site for testing racial disproportionality in employment, and the nationally representative survey results affirm the findings of this single location.²⁶

Third, our outcomes are not identical in the experiment and the survey. The experiment measured the first step in the hiring process (an employer callback) and the NLSY97 measured actual job acquisition. Survey respondents may have received invitations for interviews and even unrequited job offers (analogous to callbacks in the experiment) while unemployed, but they would not be coded affirmatively on our response variable until they actually accept work. Relatedly, while our testers only used formal means to apply for positions, survey respondents could use any means to acquire employment and were selected on the basis of unemployment, which omits employed jobseekers. While we do not directly address informal job search and networks in gaining entry-level employment, for which a complex race picture emerges (see, e.g., Moss & Tilly, 2001; Fernandez & Fernandez-Mateo, 2006; Stainback, 2008), similar conclusions regarding the low-wage labor queue emerge from both of our methodologies.

Fourth, while we held the other noted sources of disadvantage constant at their lowest levels (young age, high school education, unskilled) in the

Table 6. Experiment Logistic Regression for Callback Received with Month Blocks.

	Model 13	
	Coefficient (St. Error.)	Odds Ratio
(Intercept)	-0.642* (0.256)	
White (vs. African American)	0.496* (0.224)	1.642
<i>Month (vs. Pre-recession: October)</i>		
Pre-recession: August	0.369 (0.518)	1.446
Pre-recession: September	-0.196 (0.295)	0.822
Pre-recession: November	-0.473 (0.349)	0.623
Recession: December	-0.779 [#] (0.458)	0.459
Recession: January	-0.885* (0.375)	0.425
Recession: February	-0.713 [#] (0.381)	0.490
Recession: March	-0.886* (0.377)	0.412
Recession: April	-0.880* (0.406)	0.415
Recession: May	-1.661** (0.854)	0.190
Recession: June	-1.204 [#] (0.672)	0.300
Log-likelihood	-340.406	
Model χ^2 (df)	45.18*** (11)	

[#] $p < .10$.* $p < .05$.** $p < .01$.*** $p < .001$.

experiment, the NLSY97 analysis represents observational data, such that our estimate of the race effect before and during the recession must be adjusted by characteristics that affect candidate viability. While we attempted to control for as many confounders as possible, the estimate may still be subject to omitted variable bias. For example, while our experimental testers were trained for uniform presentation, no such uniformity necessarily exists in observational data and is not easily controlled.

Finally, while losses and disadvantage should be most pronounced among the subset we analyzed, a shared limitation of both the experiment and the survey is that we do not address the experiences of older men, Latinos, women, or more skilled workers. Latinos constitute an increasing percentage of the low-wage labor markets, with [Pager et al. \(2009\)](#) finding that Latino men receive a similar callback rate to Whites, but significantly higher than African Americans. Although an analysis of women could be conducted with the NLSY97,²⁷ we lack the corresponding experimental data needed for causal inferences. In our view, analysis of gender, race, and recession is an important topic for subsequent research. We are also limited in our ability to draw conclusions about the skilled labor market, given the low numbers of unemployed college-educated men in the NLSY97.²⁸ In our experiment, we might even expect our applicants to be favored for low-wage positions, as employers might assume skilled workers will leave such positions as soon as they were able. Yet, our applicants still experience the noted decreases in callbacks. Even with these caveats, the comparison examined here is still of critical theoretical and policy concern, even among this specific segment of the low-wage labor market. Through a rigorous field experiment and validation via a nationally representative sample, we can speak confidently to the role of labor queues and social closure in the low-wage labor market for young White and African American men.

DISCUSSION: RACE AND RECESSION

For decades, social scientists have clearly documented racial discrimination in employment opportunities and socioeconomic outcomes ([Pager & Shepherd, 2008](#)). In predicting such outcomes, race appears to interact with many other sources of disadvantage or stigma. We here asked whether the recent Great Recession, considered the worst economic downturn since the Great Depression, compounded racial discrimination in employment opportunities, or if racial inequality in the labor market is truly durable and stable throughout recessionary and non-recessionary periods. We examined this question using a field experiment in which the effect of applicant race was varied randomly, with other sources of disadvantage held constant (these include sex, age, education, job skills, and work experience). We then buttressed these findings with an analysis of nationally representative survey data that closely mimicked the

experiment. Of course, our results may not be directly comparable to past recessions given historical changes in job prospects and occupational segregation by race. Nevertheless, the processes we identify likely offer generalizable conclusions about employer behavior in times of economic contraction.

Regarding our hypotheses, we found strong support for both the main effect of race (Hypothesis 1) and the recession (Hypothesis 2), but we could not reject the null hypothesis of proportionate disadvantage in hiring during the recession by race (Hypothesis 3₀). Regarding the latter, not only could we not reject the null, but the magnitude of the difference-in-difference in both the experiment and the NLSY97 were both close to zero, indicating that our non-rejection was not simply a function of sample size. After drawing upon theoretical perspectives on labor markets and race (Reskin & Roos, 1990; Tomaskovic-Devey, 1993; Weber, 1968), we did not find that the recession altered the low-wage labor queue disproportionately for White or African American men. Although job opportunities were reduced across racial boundaries, social closure did not appear to extend to the unskilled labor market during the Great Recession. While we cannot speak to supervisory positions, it appears that African Americans still have similar access to less desirable, low-wage positions, albeit at lower rates than Whites. In this segment of the labor market, it appears rates of racial inequality remain similarly disparate.

We did not find support for our alternative hypothesis (3_A) regarding greater social closure in low-wage positions during the recession. Although more advantaged *workers* may wish to close ranks during recessions, it appears these preferences did not extend to employers and managers, whose racial preferences remained stable. Social closure is most likely when worker interests are threatened and status advantages are challenged (see Stainback et al., 2010), so it appears that the recession did not present a *race-based* threat in terms of low-wage, unskilled hiring.

Although our conclusions regarding the unskilled labor market were supported in both experimental and observational analyses, it is possible that social closure increases rather than remains proportional in the *skilled* labor market. We might expect such a result given that social closure is more readily apparent in the skilled labor market and that such positions would be particularly coveted by dominant status groups during difficult economic times. Similarly, the labor market for young, unskilled African American men has continued to deteriorate (Holzer, 2009), such that the recession-era experiences of young women may differ. Thus, we caution that our results are only applicable to the low-wage, unskilled labor market for young White and African American men. Nonetheless, we are confident that our experimental approach, corroborated through nationally representative survey data, validates our basic conclusions for this segment of the labor market.

Why do we find no additional threat to dominant status groups during a recession when other work finds strong race interactions with other characteristics? The other forms of disadvantage examined in field experiments, such as

education and criminal records, represent individual characteristics. These are also subject to statistical discrimination (Arrow, 1973), as they signal information on the presumed productivity of workers (Spence, 1973). For example, a felony criminal record may signal secondary characteristics to employers, such as unreliability, lack of trustworthiness, or liability issues. Employers thus use criminal records to make inferences about individual characteristics. Given this interaction (Pager, 2003, 2007b, p. 115), employers not only discriminate against both “felons” and “African Americans,” but they combine the characteristics and discriminate even further based upon the label “African American felon.” Such an interaction is consistent with processes of social closure. If dominant groups are seeking to maintain status and a source of disadvantage is more prevalent among minority applicants, as is the case for low education and criminal records, hiring authorities can use those disqualifying characteristics to increase social closure in a manner that appears race-neutral.

A deep economic recession, in contrast, is a *structural* rather than an *individual* characteristic, carrying no signal regarding productivity. Hence, employers have no such label on which to further discriminate against African Americans, above and beyond the main effect of racial discrimination; “recession-era African American” is simply a less meaningful and consequential label. That is, unlike low education and criminal records, the presence of a recession is not the sort of applicant-specific characteristic that can be mobilized to increase social closure. As this label carries no particular new threat to the dominant status group, social closure continues to operate during the recession as it did before, as hiring managers have no new information on which to alter long-standing hiring practices. Where we might expect disproportionality, we instead find an effect during the recession that reproduces the inequality in hiring as existed prior to the recession. As theorized by Tilly (1999), inequality is remarkably resilient in the face of structural change, such that hierarchies reproduce themselves across time and contexts. Our results demonstrate this rigidity within these differing economic contexts.

This result suggests further study of race effects in other societal labor market shifts, such as deindustrialization (Hill & Negrey, 2010), deunionization (Bound & Freeman, 1992), and outsourcing (Johnson et al., 2008). Examinations of these phenomena could address the postulate of equivalent losses by race for societal shifts and disproportionate losses by race for individual-level characteristics, though rigorous field experiments of the former would be difficult to execute. Nonetheless, this study adds to the growing literature on the differential effects of the recession. Young unskilled African American men during recessions are greatly disadvantaged in hiring relative to their White counterparts. Even with proportionate recession-based disadvantage, the lower African American baseline implies greater cumulative losses over time (see Oreopoulos, von Wachter, & Heisz, 2012). To the extent that African Americans must wait longer in the labor queue, they sustain heavier losses in human, economic, and social capital (Tomaskovic-Devey et al., 2005). Whites,

in contrast, will be hired more quickly than African Americans, but less quickly than Whites prior to the recession. The recovery process thus remains racialized.

Despite the limitations noted above, the evidence presented here is bolstered by the major advantages of an experimental approach. Random assignment of applicant race to employers removes the influence of other factors, allowing for a more precise measurement of the causal effect of race during the recession. While racial discrimination in hiring is well-documented (Bertrand & Mullainathan, 2004; Pager, 2007a; Pager & Shepherd, 2008; Pager et al., 2009), researchers rarely have access to experimental data on the effect of pronounced economic contractions. As we gain greater perspective on the most recent Great Recession, much evidence is establishing differential effects on people in the least advantaged positions, including those subject to racial discrimination (Berthoud & Sosa, 2011; Hout et al., 2011; Sierminska & Takhtamanova, 2011; Wolff et al., 2011). While our difference-in-difference test detected no significant racial differences in the effects of the recession on hiring, African Americans entered and left the recession in a greatly disadvantaged position relative to Whites.

In sum, the odds of positive employer responses are greatly depressed during deep recessions for both Whites and African Americans and we observe a comparable drop in the employment prospects of both groups. Nevertheless, in an absolute sense, African Americans were far more disadvantaged before and during the recession. In noting that “a rising tide lifts all the boats,” John F. Kennedy was usually quick to add that “a dropping tide drops all the boats” (Kennedy, 1960). For those in precarious positions already, this drop is especially consequential.

NOTES

1. In Minneapolis-St. Paul, the site of our field experiment, race effects may be particularly pronounced, as the metropolitan area consistently ranks among the highest in terms of the ratio of African American to White unemployment (Austin, 2011, 2012). Within the metropolitan area in the 2010 census, 78.0% of residents identified as non-Hispanic White and 7.7% identified as Black or African American.

2. For an analogy of a randomized complete block design, one can think of two plots of land and two types of seeds for a given plant. One of the two plots of land has been treated with pesticides and one has not. Then each plot is split up into 300 equal squares and one of the two types of seeds is randomly assigned to each square. Thus, each plot is the block and the two types of seeds are the experimental condition being tested. The researcher then measures whether the plant grows to full maturity as the outcome. Even within the two plots of land, each of the 300 blocks might contain subtle differences in the soil that would affect growth to maturity, but randomly assigning the seeds to the squares within each plot should remove any systematic bias that could falsely be attributed to the difference in the seeds. Other sources of bias may be present in social experiments, as we discuss below.

3. In his influential critique of field experiments, Heckman (1998) similarly argues that use of matched pairs is not necessarily preferable to sending random pairs of African Americans and Whites to *different* job sites.

4. While we might have paired one White tester and one African American tester and sent them to the same employers, our interest in the effect of criminal records precluded this approach. We cannot pair on recession, of course, as there is no guarantee that a job will be open during both periods, and we did not foresee whether and when a recession might occur.

5. Because the race of applicants is randomly assigned to employers, systematic differences between employers should be controlled (or rendered ignorable) by the randomization process (Quillian, 2006). To identify significant departures from randomization, we compare the industry and location of job sites by race of applicants in appendix Table A1. Across all categories, there is a great degree of symmetry by race and a chi-squared test of independence is non-significant for both industry ($\chi^2 = 1.65$, $df = 7$, $p = .977$) and suburban or central city location ($\chi^2 = 0.79$, $df = 1$, $p = .373$). As for the block represented by the recession, it is important that the sample within the two blocks (pre-recession and during-recession) differs only by the effect of interest, since exogenous processes may cause a systematic difference that could affect the comparison. For example, the group of employers hiring during a recession may be different to the extent that the recession had unequal effects across industries or locations in the low-wage market within the metropolitan area. Any observed differences in outcomes could thus be due to industry or location effects, rather than race (Heckman, 1998). Appendix Table A1 shows the recession effect by industry and location. Relative to the marginal distribution (we do not have a balanced number of tests within the recession blocks, as 55.4% of tests occurred during the recession), we observe no systematic differences across either industry ($\chi^2 = 2.60$, $df = 7$, $p = .919$) or location ($\chi^2 = 1.45$, $df = 1$, $p = .229$). Despite this symmetry, our models will adjust for the effects of these and other covariates, described below, to attest to the success of the randomization.

6. Advertisements were cross-referenced with a database of completed job sites to ensure that no location was tested more than once.

7. In terms of data quality and tester comparability, 40 hours of tester training was provided to ensure data quality, addressing topics such as research ethics, physical presentation, answering questions concerning work experience, and handling unexpected situations in the field. All compiled in a Tester Training Manual referred to throughout the field experiment. Training also included videotaped mock interviews, in which differences between testers were discussed and addressed, and practice tests at real job sites. Before leaving a job site, testers were instructed to request an extra application to give to a friend or a business card. These were used to document that the testers had in fact applied to the appropriate location. After submitting their applications, testers immediately completed a four-page Tester Response Form detailing their experience, followed by a daily debriefing interview with research staff. Data quality was also ensured through constant contact with research team staff, primarily by mobile phone, with testers calling before and after each test. To ensure that testers were following protocol both before and during the recession, we sent them to apply to sites with research accomplices, who reported on their performance to the research team.

8. More specifically, the resumes reported high school education, steady employment in service industry and labor positions, and no special training or certifications. To construct work records for the biographies, the research team reviewed entry-level job advertisements and compiled a list of all the industries that were represented in the sample. We then created employment histories that touched on the primary job types (e.g., restaurant, hotel, and warehouse work). This ensured that our testers would have relevant work experience that reflected the local and current low-wage job market, maximizing the population of jobs to which they could reasonably apply.

9. Educational attainment is coded within the NLSY97 as attainment at the start of an academic year. We use the attainment at the end of our two periods to limit the sample to those with a high school education.

10. Unemployment in the NLSY97 is coded by survey administrators using the Bureau of Labor Statistics definition: the individual does not have a job, is currently available for work, and has actively looked for work in the prior four weeks (U.S. Department of Labor, 2003). Thus, the final coding as unemployed is a combination of several date-specific questions asked within the survey, including work status, availability, and job search behavior.

11. In each period, we include any respondent who was coded as unemployed in weeks 1–40, leaving the remaining three months to examine whether they secured employment by the end of the period.

12. Therefore, 26.4% of respondents contribute to both periods. Among those only unemployed in one period, 38.5% are in the pre-recession period and 35.1% are in the recession period. Except when derived from more exact survey items based on monthly or weekly arrays as noted, the variables for the pre-recession period and recession period are taken from the 2006 and 2007 annual surveys, respectively. In models with controls, missing data reduces the analytic sample to 725 unique individuals and 916 observations.

13. Given that Hauser and Warren's (1997) calculations were based on the 1990 Census occupation codes, we use the calculations developed by Frederick (2010), which applies Hauser and Warren's formula to the 2002 occupation codes used in the NLSY97. Those lacking employment within the last two years serve as the reference category for the industry variables and are coded at the minimum on SEI.

14. In this relatively low-wage group, the log of income is not necessarily a better choice for inclusion, based upon the distributions of both codings. Either coding produces similar outcomes.

15. The variable includes eight response categories ranging from "Mostly below Ds" to "Mostly As," with higher categories indicating better grades. The NLSY97 also includes GPA from high school transcripts, but this measure contains a considerable amount of missing data such that we chose to include self-reported grades.

16. We also tested a three category college attendance variable to reflect recent attendance, with categories of never attended, attended but prior to preceding year to the given period, and attended within the last year prior to the given period. This coding also did not produce significant results nor alter the results presented.

17. The correlation matrix was defined as unstructured, which imposes no structure on the residual covariance matrix and estimates the variance at each occasion and the covariance between each pair of occasions freely, and is an appropriate choice for few occasions (Rabe-Hesketh & Skrondal, 2012, pp. 298, 332).

18. The alternative subject-specific random effects specification produces nearly identical results. Similarly, if we include only those who are unemployed in a single period (reducing the model to typical logistic regression), we get nearly identical results.

19. We also tested for interaction effects, finding that the effect of contact did not vary by race ($b = 0.056$, $p = 0.889$) or the recession ($b = -0.017$, $p = 0.965$). As essentially no covariation is observed between contact and race or recession due to randomization, this lack of effect should not be surprising.

20. Examination of both marginal and conditional effects leads to the same inferential conclusion.

21. Pager (2007a) argues that careful selection and training can minimize biases due to tester effects, though we recognize that variation in testers could theoretically alter an experiment's conclusions by affecting estimates of *variation*, though not the mean (Heckman, 1998). Returning to the seed analogy of footnote 2, if one of the two seeds has greater variation and more of its distributional tail is above the threshold for growing to maturity, it will surpass the threshold related to the subtle differences in the square

to which it was randomly assigned more often. Thus, it will appear to be a better seed, despite there being no mean differences in the two seeds' actual distributions. Taken to its extremes, this is an argument against all randomized experiments. We remain confident that our testers presented themselves across and within race as uniformly as possible as a result of our careful selection criteria, intensive training, and rigorous quality control. Further, having several testers of the same race should help eliminate any individual-level differences that could be attributed to race. To formally control for possible variation across testers, we further added a random effect for tester to our covariate models in Tables 2 and 3 (similar to the approach in Pager et al., 2009). All results were virtually identical (available upon request).

22. This effect and significance test is obtained using Helmert coding (see Cohen, 1968) for race and recession effects.

23. Appendix Table A2 shows descriptive statistics for each of the variables in our analysis, cross-classified by the recessionary period and race. Among this subset, race is about equally divided in both periods. Among the covariates, there are expected differences observed between Whites and African Americans on several measures (e.g., weeks worked, region, parenthood, income, SEI, college attendance, marriage, parent's education). Although to a much lesser extent, there are also some differences between the pre-recessionary and recessionary period (e.g., weeks worked, income). These differences stress the need for covariate adjustment for our main variables of interest.

24. Unlike in the experiment where randomization eliminates covariation with the controls, it is conceivable that the effects of the covariates vary by race or recession in the survey data. We tested for all such interactions, finding no significant interactions by race. The recession effect, however, varied significantly by urbanicity and marital status. Prior to the recession, there is an advantage for those in urban areas in terms of job acquisition, which disappears during the recession. Married individuals were less likely to acquire employment prior to the recession and more likely to find work during the recession. Importantly, these interactions do not alter the main effect of race, the main effect of year (as estimated through the margins command in the presence of an interaction), and the predicted probabilities for the race and recession combinations. Because they do not affect the results, we exclude them in order to maintain symmetry with the experiment. These models are available upon request.

25. In analogous models that add the subsequent annual employment array (December 2008 to November 2009) when the recession reached its peak ($N=1,438$; unique individuals = 961), we still find no difference-in-difference (both with and without controls). From an analogous model to Table 4, Model 10, the predicted chance of acquiring a job in 2007, 2008, and 2009 is 86%, 77%, and 63%, respectively, for Whites, and 73%, 61%, and 57%, respectively, for African Americans. As with Model 10, the coefficients testing the difference-in-difference, though non-significant, are actually in the opposite direction to our hypothesis. Whites experienced a 22.6 percentage point drop in employment acquisition from 2007 before the recession to its peak in 2009, while African Americans experienced a 16.2 percentage point drop over the same period. Thus, we again find no grounds to reject the null hypothesis of proportionate decreases, but caution that we cannot confirm this result experimentally. With regard to between-group differences, all groups have higher odds of acquiring jobs relative to African Americans during year 2 of the recession, though the comparisons to Whites during year 2 of the recession and African Americans during year 1 of the recession are not statistically significant. Results are available upon request.

26. We also conducted an in-depth analysis of geographic context within the metropolitan area by collecting suburb and neighborhood (for jobs in Minneapolis or St. Paul) information based on the location of each jobsite, totaling 110 unique geographic units in our dataset. Measures examined include the percentage under 18, African American, residing in the same home as a year ago, single-headed households, that speak English

less than very well, with a Bachelor's degree over age 25, below the poverty line, of adults employed, and voting Democrat, as well as the mean household income and Index I crime rate. As with the audit-level covariates discussed above, the race and recession coefficients remain identical when the geographic area characteristics are included (utilizing cluster-corrected standard errors). Thus, our randomization was very robust to location characteristics as well. None of these measures were statistically significant, either singly or as a whole. It would appear, then, that employers are more responsive to the characteristics of the applicant (i.e., race, personality as assessed through contact) than those of the area in which the jobsite is located. Results are available upon request.

27. In an analogous set of models for women, the results again suggest a lack of difference-in-difference (both with and without controls), though the race effect is less straightforward. While the recession effect remains significant in all models, the race effect is marginally significant without controls and non-significant with controls. Alternatively, when considering the between-group effects, all groups are significantly more likely to acquire work relative to recession-era African American women. Thus, unlike the men, the two forms of coding do not produce consistent results in the predicted probabilities (specifically, African American women experience a drop in job acquisition during the recession regardless of coding, while White women's decrease is less dramatic in the simple coding). Without a field experiment where causality is directly addressed, we urge caution in interpreting these results, and urge future research into the experiences of women during the recession by race. Results are available upon request.

28. Among those White or African American men who reported unemployment, very few respondents had achieved either an associate (pre-recession: $n = 28$, or 4.6%; recession: $n = 22$, or 3.9%) or a bachelor degree or higher (pre-recession: $n = 78$, 12.9%; recession: $n = 75$, 13.1%). When cross-classified by race and employment acquisition, the cell counts are too low for meaningful analysis.

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APPENDIX

Table A1. Experiment Industry and Location of Job Sites by Race and Recession.

	Cases	African American	White	χ^2 (df)	Pre-Recession	During Recession	χ^2 (df)
<i>Industry</i>				1.65 (7)			2.60 (7)
Office work	39	56.4%	43.6%		43.6%	56.4%	
Retail	115	51.3	48.7		41.7	58.3	
Healthcare	9	55.6	44.4		33.3	66.7	
Restaurant	217	55.6	44.4		44.2	55.8	
Warehouse/labor	120	52.5	47.5		45.8	54.2	
Hotel	68	50.0	50.0		51.5	48.5	
Driver	25	52.0	48.0		40.0	60.0	
Security	12	41.7	58.3		50.0	50.0	
<i>Location</i>				0.79 (1)			1.45 (1)
Suburbs	401	51.9	48.1		42.9	57.1	
Minneapolis-St. Paul	204	48.0	52.0		48.0	52.0	
<i>Total</i>	605	50.6	49.4		44.6	55.4	

Table A2. NLSY97 Descriptive Statistics.

	Pre-Recession			Recession		
	Total	White	African American	Total	White	African American
<i>Race</i>						
White	47.5			49.3		
African American	52.5			50.7		
Acquired employment	75.5	83.3	66.8	67.0	77.2	56.4
<i>Weeks worked in previous year</i>						
0–13	23.0	16.7	29.8	25.7	19.5	32.1
14–26	16.2	14.8	17.7	10.5	9.5	11.5
27–39	17.6	17.9	17.2	13.3	14.5	12.0
39–52	43.3	50.6	35.3	50.5	56.4	44.4
<i>Industry of last job</i>						
No report in last two years	7.5	4.9	10.5	8.7	5.0	12.5
Construction	14.1	18.3	9.2	17.2	23.4	10.8
Manufacturing	11.6	12.6	10.5	11.9	11.3	12.5
Retail trade	13.7	11.4	16.2	12.7	15.1	10.3
Transportation and warehousing	5.3	2.3	8.8	4.3	4.2	4.3
Professional and related services	15.3	13.7	17.1	14.0	12.1	16.0
Entertainment, accommodations, and food services	14.9	14.8	14.9	14.7	12.6	16.8
Other	17.7	22.1	12.7	16.6	16.3	16.8
Hauser & Warren SEI Male	24.3 (10.2)	25.8 (10.8)	22.6 (9.1)	23.6 (10.0)	25.2 (9.5)	22.0 (10.3)
Income (thousands)	12.0 (12.2)	14.5 (12.9)	9.2 (10.7)	14.2 (14.7)	18.4 (16.9)	9.9 (10.6)
High school grades (range 1–8)	4.7 (1.6)	4.8 (1.7)	4.5 (1.4)	4.7 (1.6)	4.8 (1.7)	4.6 (1.4)
Ever attended college	31.5	33.8	29.0	32.4	35.3	29.5

Convicted of crime in last year	10.2	9.9	10.5	6.3	5.8	6.8
Received unemployment insurance in last year	5.8	6.5	5.0	7.0	7.5	6.4
Age	23.7 (1.4)	23.6 (1.5)	23.7 (1.4)	24.6 (1.4)	24.6 (1.4)	24.7 (1.4)
<i>Census region</i>						
Northeast	15.2	16.4	13.9	13.7	16.3	11.1
North Central	28.1	33.1	22.7	25.6	29.3	21.8
South	43.9	30.4	58.8	48.6	32.6	65.0
West	12.8	20.2	4.6	12.1	21.8	2.1
Urban (vs. rural)	77.6	72.6	83.2	76.1	74.5	77.8
Migrated between counties	23.6	29.7	16.8	19.6	24.1	15.0
<i>Children</i>						
None	59.2	68.8	48.5	51.5	60.2	42.5
Non-residential	24.0	12.2	37.1	27.2	17.8	36.9
Residential	16.8	19.0	14.4	21.3	22.0	20.6
Married	14.8	19.0	10.1	17.9	22.4	13.3
<i>Parents' highest education</i>						
HS or less	52.8	46.0	60.2	53.0	46.8	59.6
Some college or associates	22.0	24.6	19.3	21.9	24.1	19.5
Bachelors or higher	16.4	24.1	8.3	16.6	23.9	8.7
No parent information	8.7	5.4	12.2	8.5	5.2	12.1

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