Race, Gender, and Hiring Patterns: Evidence from a Large Service-Sector Employer

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Abstract: Using panel data from a large service sector employer, this study estimates the effect of manager race and gender on the race and gender composition of new hires. Data on consecutive managers at hundreds of stores makes it possible to control for all fixed attributes of the store, customers, and labor market. Estimates from probit and multinomial logit models with store fixed effects yield three main findings. First, nonblack (white, Hispanic, and Asian) managers hire significantly more whites and fewer blacks than do black managers. These nonblack-black differences are especially large in stores located in the South. Second, somewhat surprisingly, there appears to be little bias among whites, Hispanics, and Asians. Finally, this study finds no measurable biases with respect to gender. Additional tests suggest that the nonblack-black biases cannot be explained by the use of social networks in recruiting, nor do they appear to be motivated by productivity advantages of manager-employee similarity.

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Workplaces in the United States are strongly segregated by race, ethnicity, and gender. An important question is the extent to which this phenomenon is caused by biases in hiring that favor employees who share the race, ethnicity, or gender of those who employ them. The theoretical literature makes clear that workplace segregation could be caused by such discrimination (Becker 1957, Arrow 1973). However, previous empirical studies of workplace segregation have all encountered the same limitation—they could not securely identify such discrimination because they could not distinguish it from unobserved differences across workplaces or local labor markets that may also cause segregation.[1] Using a new panel data set that makes it possible to control for these differences, the present study can more accurately identify and measure same-race and same-gender biases in hiring.

The data are the complete personnel records of a large U.S. service-sector employer from February 1996-July 1998.[2] A crucial feature of this data set is that it contains hundreds of stores that have at least one change in the manager who is responsible for hiring, and these new managers often have demographics that differ from the managers they replace. This within-store variation in manager demographics makes it possible to control for all fixed attributes of the

[1] Though unable to control for unobserved differences, several studies of workplace segregation have produced results consistent with hiring discrimination. One set of studies shows that segregation cannot be fully explained by sorting of different demographic groups into specific industries and occupations. Rather, much of it occurs within industries and occupations at the individual-establishment level (for race, Bayard et al., 1999; Tomaskovic-Devey, 1993; for gender, Carrington & Troske, 1998; Petersen and Morgan, 1995; Groshen, 1991). A second set of studies uses cross-sectional data on manager-employee demographics: Bates (1994) and Raphael (2000) both find that establishments owned or managed by blacks are more likely to employ blacks, and Carrington & Troske (1998) find that women are more likely to be supervised by women in U.S. manufacturing establishments. To be sure, hiring discrimination has been approached from angles other than the analysis of workplace segregation. Bearing in mind their differences in method and context, as well as their limitations, these studies provide a benchmark for comparing the results of this study. Various "audit" studies have found evidence of discrimination against blacks (Turner et al., 1991; Bendick et al., 1994), Hispanics (Cross et al., 1990; Kenney and Wissoker, 1994) and women (Neumark, et al., 1996). However, this approach has been critiqued strongly (e.g. see Heckman and Siegelman, 1993). Goldin and Rouse (2000) use the adoption of blind auditions to test for sex-biased hiring in symphony orchestras, and find evidence of discrimination against women. And Holzer (1996), using cross-sectional data from establishments in four cities, finds that the ratio of hires to applicants is lowest for blacks and highest for whites, with Hispanics and Asians in between, which is suggestive of discrimination.

[2] The data used in this paper are confidential and unfortunately cannot be made available to other researchers. Also, in order to preserve the anonymity of the employer, I cannot disclose exact sample sizes.

stores, customers, and labor markets, and thereby to distinguish segregation caused by differences across workplaces from that caused by biases in hiring. Estimates from probit and multinomial logit models with store fixed effects yield three main findings. First, nonblack (white, Hispanic, and Asian) managers hire significantly more whites and fewer blacks than do black managers. These nonblack-black differences are especially large in stores located in the South. Second, somewhat surprisingly, there appears to be little discrimination among whites, Hispanics, and Asians. Finally, in contrast to much of the previous literature on gender discrimination, this study finds no measurable biases with respect to gender.

These results imply that the segregation of nonblacks and blacks in the workplace is not fully explained by differences among jobs and workplaces. Rather, it is attributable in part to systematic differences in hiring patterns between nonblacks and blacks. Additional tests suggest that these nonblack-black biases cannot be explained by the use of social networks in recruiting, nor do they appear to be motivated by productivity advantages of manager-employee similarity. Hence, these tendencies are likely to be driven by discrimination. However, because the composition of the applicant pool is unknown, interpretation of the results is still subject to two cautions. First, the results could indicate discrimination either by managers or by potential employees.[3] Second, the results measure only the relative likelihood of managers of each race to hire employees of each race, and thus the differences in hiring patterns between nonblack and black managers could indicate discrimination either by nonblacks against blacks or by blacks against nonblacks.

I. The Data

A. The Sample

[3] Even if the applicant pool were known, it might still be impossible to distinguish manager discrimination from applicant discrimination unless we had information on who received job offers. This is because individuals might submit applications without having met the manager, and then discriminate when accepting or declining job offers—after having met the manager.

The estimation sample consists of more than 1,500 managers, and more than 100,000 frontline employees who were hired at more than 700 workplaces between February 1, 1996 and July 31, 1998. While the workplaces are located throughout the United States, they are all very similar—they are all part of a national chain with uniform, highly centralized policies and procedures. A typical workplace has a full-time store manager, and between 25 and 50 part-time employees. All frontline employees at this company have similar job titles and descriptions; they rotate through several tasks in the workplace, spending some of their time dealing with customers and other time in support tasks. Like the majority of low-wage service-sector jobs, these jobs require only basic skills and employees receive little training.

Hiring is the responsibility of the store manager. On average, each store in the sample hires five new employees per month. The company's official hiring policy is neutral with respect to race and gender, and managers are given a small amount of training in fostering a diverse workforce. While telephone interviews are used for pre-screening applicants, the vast majority of hiring decisions are made only after a face-to-face interview with the manager.

Table 1 summarizes the demographics of the workplaces in the sample and of the communities surrounding these stores. The workforce of the average store is 57 percent white, 18 percent black, 14 percent Hispanic, and 10 percent Asian.[4] Roughly two-thirds of the employees are women. A typical store location has over 100,000 people within a two-mile radius of the store and a median household income of $40,000. The stores' workforces tend to be more racially diverse than the populations they serve.[5]

Table 2 shows the demographic composition of both frontline sales workers and managers in the sample (columns 1 and 2). The management force is more homogeneous than the frontline workforce: 84 percent of managers are white and 75 percent are female. It is worth

[4] The race and ethnicity codes are the company's coding, and they create a set of mutually exclusive and collectively exhaustive categories. Hispanics are classified by ethnicity and not by race. For brevity, I will often refer to these categories simply as "race".
[5] Community statistics are from the 1990 Census and are based on a two-mile radius from the center of each store's ZIP code.

noting that racial minorities are under-represented in management (the ratio of managers to employees is much lower for blacks, Hispanics, and Asians than it is for whites).

The estimation sample (summarized in the first two columns of Table 2) is restricted to new hires at stores that hire at least one employee of each race between Feb. 1, 1996 and July 31, 1998. This restriction is necessary to estimate a fixed effects multinomial logit model predicting the probabilities with which each of the four race groups is hired. Columns 3 and 4 of Table 2 show statistics from the total population of the company's U.S. retail establishments. The estimation sample is somewhat more diverse—with respect to both race and gender—than the wider population. However, a robustness test suggests that the sample restriction has little effect on the estimation results (see below and Appendix Table A1).

In all case studies, an important consideration is whether the sample can be considered representative of a particular population of interest. The racial composition of this employer resembles that of the retail sector in the U.S. (Table 2, columns 5 and 6), which accounts for roughly eighteen percent of all U.S. jobs. However, this company employs a relatively high share of women managers for the retail sector (81 percent compared to 50 percent nationwide), and both the workforce and management are relatively young.

To the extent that this company is not representative, one must be cautious about generalizing from the patterns in this study. First, the large share of women managers, the relative youth of the workforce, and the (admittedly modest) diversity training for managers may all affect the hiring biases at this company. Second, the sample period (1996-1998) was a time of historically low unemployment in the U.S. Because it was difficult to find qualified workers during these years, manager discrimination may well have been less prevalent than during periods of higher unemployment. Finally, generalizing to other sectors may be especially problematic. For example, turnover rates are relatively high at this company and in the retail sector generally; discriminatory preferences may matter much more where employment relationships last years rather than months.

B. Manager-Employee Similarity at this Company

Table 3, which is based on the estimation sample, shows that there is a strong correlation between the race of the hiring manager and the racial composition of new hires. Black, Hispanic and Asian employees in this chain are roughly twice as likely to be hired by managers of their own race as they are to be hired by managers of other races. Likewise, female employees are significantly more likely to be hired by women than by men, and (correspondingly) men are more likely to be hired by men.

These correlations between manager and employee demographics could be due to same-race and same-gender biases in hiring. But they could also reflect a number of differences across stores and locations (some of which are unobserved or imperfectly observed). One obvious suspect is differences in labor pool demographics. The correlations might reflect nothing more than the fact that Hispanic neighborhoods (for example) tend to have both Hispanic managers and a large share of Hispanic employees. Two other suspects are differences in the attributes of stores and differences in the skill requirements of stores. The correlations could be caused by such differences if the demographic groups themselves differ with respect to preferences or skills. In this sample, one might think such differences across stores would play a minimal role since all employees at every store have the same job description, sell the same products, and work for the same national firm. Nevertheless, even subtle attributes of a store and its location may make it especially attractive or unattractive to people of a particular demographic group. Or, if managers and employees who match their customers are more skilled at serving them, the relative productivity of different demographic groups may vary across stores.[6]

The goal of the regression analysis in the following sections is to unravel the causality underlying the correlations between manager and employee demographics. The key to this

[6] This may be due to customer discrimination (if customers prefer to be served by similar employees) or to improved communications among those of the same race, ethnicity or gender. Using the data set employed in the present study, Leonard and Levine (2002) find that employee-customer matching improves sales in enclaves of non-English speaking immigrants, but is otherwise not important.

analysis is the nature of manager turnover in the sample. Not only is turnover frequent—80 percent of the stores have at least one change in management during the 30-month sample period.[7] But, crucially, of these changes in management, 38 percent involve a change in manager sex and 30 percent involve a change in manager race. These changes in manager demographics make it possible to estimate models with workplace fixed effects. This study can thus identify the influence of manager demographics on hiring patterns, controlling for all differences—observed and unobserved—across stores.

II. Estimation Model and Methods

A. Model of Managers' Hiring Decisions

I estimate several equations that predict, as a function of manager race or gender, the probability that a new hire belongs to a given race or gender. These equations are derived from a simple model of the utility a manager receives from hiring a given type of worker. The model assumes that the decision to hire a new employee is made before the decision about which type of worker to hire, and is independent of the relative costs and benefits of hiring each type of worker. Thus, on the hiring date *t*, the manager must choose from among applicants belonging to different race or gender groups.

The basic model is a simple reduced-form equation expressing the utility, or net benefit, manager *i* in store *j* obtains from choosing an employee of a given race or gender at date *t*. This utility is a function of the manager's own race ($MgrBl_{ijt}$, etc.), characteristics of store *j* (S_j), characteristics of the store's surrounding community (C_j), the month in which the hire takes place (M_t), and an error term (ϵ_{ijt}^{w}). Equation 1 illustrates the model for the case where the new hire is white.

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[7] The median tenure of a manager is 13 months. Approximately 60% of the manager turnover events involve transfers to other company stores. The rest involve termination of employment with the company. Most terminations are voluntary, the most common reasons being "found better job/prefer other work" (41%), "personal" (13%), "moving", "dislike hours", "limited career growth", and "return to school". Roughly 13% of manager terminations are involuntary, due primarily to violations of company policy, substandard performance, or dishonesty.

$$(1) \qquad U_{ijt}(new\ hire\ is\ white) = b_0^{\,w} + MgrBl_{ijt}\, b_B^{\,w} + MgrHi_{ijt}\, b_H^{\,w} + MgrAs_{ijt}\, b_A^{\,w} + S_j\, b_S^{\,w} + C_j\, b_C^{\,w} + M_t\, b_M^{\,w} + \epsilon_{ijt}^{\,w}$$

The parameters of interest in this model are the coefficients $b_B^{\,w}$, $b_H^{\,w}$ and $b_A^{\,w}$ on the dummy variables indicating the race of the hiring manager. These represent the extent to which a manager's race affects the manager's utility if he or she chooses to hire a white employee. Negative coefficients on the dummy variables indicating that the manager is black, Hispanic or Asian would suggest that it is relatively costly for these managers to hire a white employee. Obviously, one source of such relative costs might be the discriminatory preferences of the managers. Hence, negative coefficients on nonwhite manager dummies could indicate that managers discriminate against employees who do not share their race. However, negative coefficients could also be caused by other things. For example, discriminatory tastes of potential white employees may make it harder and more costly for nonwhite managers to recruit whites. Again, if the social networks of managers are important for recruiting new hires and if these networks tend to run along racial lines, this could make it harder to recruit workers of a different race. Finally, if productivity benefits are associated with manager-employee similarity, a manager would sacrifice these benefits to hire a dissimilar employee. While I cannot distinguish manager preferences from employee preferences, I will examine the roles of networks and productivity in additional tests below.

Apart from the manager's race, other variables that determine the costs and benefits of recruiting a qualified white employee include the share of whites in the local labor pool, the particular needs of the store (e.g. the share of whites in the customer base, if matching the customers is important), and any attributes of the store that may influence whites' preferences for working at the store. By estimating the model with several measured characteristics of the store (S_j) and community (C_j), I can assess the extent to which these variables are responsible for the observed race and gender segregation across stores. Specifically, my controls include the share of each race in the local population, population density, median household income, and the location type (mall, street, etc.) of the store. The racial composition of the applicant pool might also be affected by changes over time in labor supply and demand. For example, whites may be more likely to work in low-wage retail jobs when labor markets are weak. Therefore, I also include a dummy variable for each of the 30 months in the sample (M_t) to control for national fluctuations in the labor market.

Despite the uniformity of jobs in the sample and the ability to control for several store and community characteristics, it is likely that the residual, ϵ_{ijt}^{w}, contains unobservable features of the store and community that are correlated with both the manager's race and his or her choice of whom to hire. For example, the exact racial composition of each store's potential applicant pool is not observed and the community demographics may provide only an imperfect proxy. Such omitted variables will result in biased estimates of the effect of manager race on hiring choices.

To the extent that the unobserved factors affecting both manager and employee demographics are fixed over time, I can control for them using store fixed effects. The fixed effects model is:

(2) $$U_{ijt}(\textit{new hire is white}) = b_0^{w} + MgrBl_{ijt}\, b_B^{w} + MgrHi_{ijt}\, b_H^{w} + MgrAs_{ijt}\, b_A^{w} + M_t\, b_M^{w} + \alpha_j^{w} + \varepsilon_{ijt}^{w}.$$

The workplace fixed effects, α_j^{w}, summarize the effect of any permanent differences across stores, communities, and labor markets that influence both the race of the manager and the costs and benefits of hiring a white employee.

If the omitted variables are not fixed over time, then even the fixed effects specification in equation (2) may produce biased estimates. For example, the fixed effects estimates would overstate the causal effect of manager race on hiring patterns if trends in local demographics led to parallel trends in the applicant pools of managers and employees. A similar bias could arise from a policy of promoting new managers from within the store or from deliberate attempts to hire new managers who match the evolving demographics of the workforce. Thus, to rule out local trends as a source of any correlations between manager race and hiring patterns, I also estimate equations that include store-specific trends in addition to store fixed effects.

B. Estimation Methods

Because only the manager's hiring choices (but not the underlying utilities) are observed, estimation of the model relies on the assumption that the manager chooses the option with the highest utility. When the choice is assumed to be binomial (as in the choice between white and nonwhite or male and female), the manager's choice of a white employee implies that $U_{ijt}(new\ hire\ is\ white) > U_{ijt}(new\ hire\ is\ nonwhite)$. The probability that manager *i* in store *j* is observed to choose a white employee at date *t* can therefore be written, based on the model in equation (1), as:

(3) $Prob\ (new\ hire\ is\ white |\ MgrBl_{ijt,}\ MgrHi_{ijt,}\ MgrAs_{ijt,}\ S_{j,,}\ C_j,\ M_t)_{ijt}$

$= Prob\ (U_{ijt}(new\ hire\ is\ white) > U_{ijt}(new\ hire\ is\ nonwhite))$

$= Prob\ ((b_0{}^{w} + MgrBl_{ijt}\, b_B{}^{w} + MgrHi_{ijt}\, b_H{}^{w} + MgrAs_{ijt}\, b_A{}^{w} + Sj\ b_S{}^{w} + C_j\, b_C{}^{w} + M_t\, b_M{}^{w} + \epsilon_{ijt}{}^{w}) >$
$(b_0{}^{nw} + MgrBl_{ijt}\, b_B{}^{nw} + MgrHi_{ijt}\, b_H{}^{nw} + MgrAs_{ijt}\, b_A{}^{nw} + Sj\ b_S{}^{nw} + C_j\, b_C{}^{nw} + M_t\, b_M{}^{nw} + \epsilon_{ijt}{}^{nw}))$

$= Prob\ ((b_0{}^{w} - b_0{}^{nw}) + MgrBl_{ijt}(b_B{}^{w} - b_B{}^{nw}) + MgrHi_{ijt}(b_H{}^{w} - b_H{}^{nw}) + MgrAs_{ijt}(b_A{}^{w} - b_A{}^{nw}) +$
$S_j(b_S{}^{w} - b_S{}^{nw}) + C_j(b_C{}^{w} - b_C{}^{nw}) + M_t\,(b_M{}^{w} - b_M{}^{nw}) + (\epsilon_{ijt}{}^{w} - \epsilon_{ijt}{}^{nw}) > 0)$

$= Prob\ (b_0 + MgrBl_{ijt}\, b_B + MgrHi_{ijt}\, b_H + MgrAs_{ijt}\, b_A + Sj\ b_S + C_j\, b_C + M_t\, b_M + \epsilon_{ijt} > 0)$

Here the coefficients b_B, b_H, and b_A represent the impact of the manager's race on the probability that a new hire is white. Similar equations can be written to express the probabilities that a new hire is black, Hispanic, Asian, and female.

Assuming a normal distribution for ϵ_{ijt}, I use a probit model to estimate several variations of equation (3) for each race and gender group. The residuals are assumed to be identically distributed and independent across stores, but not necessarily within stores. I use Huber-White robust estimates of the standard errors that are corrected for within-store correlation of the error terms. To estimate the fixed effects version of the probit model (derived from equation 2), I include a dummy variable for each store in the model. A potential concern with this approach is that maximum likelihood estimation provides consistent estimates of the fixed effects only if the number of observations per group is large enough.[8] Fortunately, my data include an average of 140 employee hires per store, which is large by the standards of the current literature.[9]

The binomial choice model is restrictive in that it ignores the fact managers may choose simultaneously from among four possible race categories rather than choosing white vs. nonwhite, black vs. nonblack, etc. Therefore, in addition to the probit models, I estimate a multinomial logit model that incorporates the full set of possible choices with respect to race.[10] Because the fixed effects prove important in the probit estimations, I estimate a multinomial logit model with store fixed effects. Again, I estimate the fixed effects by including a dummy variable for each store. I do not control for store trends, however. This is because of computational limitations, and because the store trends prove to be relatively unimportant in the probit estimations.

The fixed effects multinomial logit model is derived from equation (2). Because the manager now has four possible choices instead of two, he or she will choose a member of race group *k* if $U_{ijt}(\textit{new hire is race k}) > U_{ijt}(\textit{new hire is race l})$, $\forall k \neq l$. Now, assuming that the residuals are distributed according to a Type I extreme value distribution, the probability that manager *i* in store *j* on date *t* chooses a new hire of race *k* can be written as:

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[8] The most common alternative method for estimating nonlinear binomial choice models with panel data is Chamberlain's (1980) conditional logit model, which bypasses estimation of the fixed effects by estimating the probability of a positive outcome conditional on the number of positive outcomes in the group. While estimating probit models with several hundred dummy variables is computationally cumbersome, it is even more impractical to estimate conditional logit models with well over 100 observations per group and large numbers of both positive and negative outcomes. However, I ran several tests on smaller subsets of the data in order to compare estimates from fixed effect probit and conditional logit models, and found the estimates to be very close. I also ran all binomial specifications using a fixed effects linear probability model and obtained results substantively similar to those obtained from the probit estimations (See Appendix Table A1).

[9] For example, Greene (2000) presents Monte Carlo evidence suggesting that the bias from estimating nonlinear models using maximum likelihood with fixed effects drops off rapidly as the number of observations per group increases above three and is substantially reduced even at 20 observations per group.

[10] There are too few Native Americans in my sample to obtain reliable estimates for this group. In all of the analyses reported in this paper, the small number of Native American and "other" race employees are treated as white. In the case of the probit equation predicting the probability that a new hire is white, I also calculated estimates treating Native Americans and others as nonwhite. The results were nearly identical.

$$(4)\ Prob(new\ \ hire\ is\ race\ k)_{ijt} = \frac{exp(b_0^k + MgrBl_{ijt} b_B^k + MgrHi_{ijt} b_H^k + MgrAs_{ijt} b_A^k + M_t b_M^k + \alpha_j^k + \varepsilon_{ijt}^k)}{\sum_{l=1}^{4} exp(b_0^l + MgrBl_{ijt} b_B^l + MgrHi_{ijt} b_H^l + MgrAs_{ijt} b_A^l + M_t^l b_M^l + \alpha_j^l + \varepsilon_{ijt}^l)}$$

where $k = 1, \ldots, 4$ represents the four race groups white, black, Hispanic, and Asian.

An important assumption of the multinomial logit model is that the odds ratio for any two alternatives is independent of the other alternatives. This implies, for example, that the ratio of the odds of choosing a white employee to the odds of choosing a black employee is not affected by the presence of Hispanic and Asian employees as other alternatives. To test the validity of this "independence of irrelevant alternatives" assumption, I apply a Hausman type specification test by comparing estimates from models with and without each of the four alternatives. The test provides no evidence against the model.[11]

III. Empirical Results

A. Probit Estimates of Race-Based Hiring Biases

Tables 4a-4d show the estimates from the probit models relating manager race to the race of a new hire. The dependent variable in Table 4a is a dummy variable equal to one if the new hire is white and zero otherwise. In Tables 4b-4d, the dependent variables are dummies indicating that a new hire is black, Hispanic, and Asian. For ease of interpretation, I report marginal effects instead of coefficients. In the case of dummy variable regressors, such as the manager race indicators, I report the effect of a discrete change from zero to one. In each regression, the omitted manager race category is the race for which the dependent variable is defined. Hence, negative estimates for the manager race variables are consistent with same-race biases.

[11] This application of Hausman's specification test is described by Hausman and McFadden (1984). The test is based on the test statistic $(b_r - b_f)'(V_r - V_f)(b_r - b_f)$, where b_r denotes the estimates of the restricted model in which one race alternative is omitted. These estimates are inefficient but still consistent under the null hypothesis of independence. b_f denotes estimates of the full model, which are efficient and consistent under the null. The statistic is distributed $\chi^2(>800)$ under the null hypothesis. The values of the test statistics are 36.12 (omitting white), 4.35 (black), 2.49 (Hispanic), and 2.24 (Asian), none of which provides evidence against the null.

Correlates of Manager-Employee Similarity in the Cross Section.—In the first three columns of Tables 4a-4d, one can see the effect of controlling for observable differences across stores. The pattern of results is similar for all four race groups. First, column 1 shows the effect of manager race on hiring outcomes without any controls. The estimated effects of racial dissimilarity are consistently negative and, except for the Asian manager indicator in Table 4a, all are significant at a confidence level of .1 percent. These results simply confirm what has already been seen in Table 3: Employees are much more likely to be hired by managers who share their race.

The specification in column 2 adds controls for the population share of each race in the community. Not surprisingly, community demographics explain a significant part of the correlation between manager race and employee race. The magnitudes of the manager race estimates are consistently reduced by at least one quarter from column 1 to column 2. However, most of them remain significantly different from zero. Finally, Column 3 adds more location variables, including population density, median household income and store location type. Although these variables are often statistically significant, they explain little of the correlation between manager race and employee race.

Column 4 may also be noted here. The inclusion of month dummies in this specification has little effect, suggesting that the similarities between managers and new hires are not explained by seasonal and national fluctuations in the labor market.

Store Fixed Effects Estimates.— After controlling for observed differences across stores (columns 2 and 3) and unobserved differences across time (column 4) , a substantial amount of the correlation between manager race and employee race still remains to be explained. Column 5 of Tables 4a-4d adds the fixed effects specifications of the probit models. The fixed effects are clearly important. In many cases, unobserved differences across stores and locations account for nearly all of the remaining correlation between manager race and the race of the new hire. In the equation predicting that a new hire is white (Table 4a), the Hispanic and Asian manager effects are very small and not significantly different from zero. In the equations predicting that a new hire is Hispanic (Table 4c) or Asian (Table 4d), none of the manager race effects are significantly different from zero at a 5 percent confidence level.[12]

However—even after controlling for fixed effects—a certain pattern of bias remains. There is a significant difference between the hiring patterns of black managers and nonblack (white, Hispanic, Asian) managers, and this difference lies mainly in the share of whites and blacks hired by these two groups of managers. First, the probability that a new hire is white is 4.4 percentage points lower under a black manager than it is under a white manager in the same store (Table 4a). Second, the probability that a new hire is black is 3.5-4.0 percentage points lower under a white, Hispanic, or Asian manager than it is under a black manager (Table 4b).[13] These estimates are all significant at a 1 percent level of confidence. The import of this pattern is explored in the sections below.

Store-Specific Trends.— If manager-employee similarities are driven by local demographic trends, the fixed effects estimates might overstate the causal effects of manager race on hiring patterns. The specification in column 6 of Tables 4a-4d addresses this concern by including store-specific trends as controls. The results of this specification are somewhat surprising. In almost all cases, the magnitudes of the manager race coefficients become *larger* rather than smaller, suggesting that if anything, the fixed effects estimates *understate* the causal effect of manager race. However, the differences between the estimates in columns 5 and 6 are generally quite small.

Figures 1a and 1b provide additional evidence that the estimates of the relationship between manager and employee race are not driven by trends in store demographics. These graphs are based on all stores for which I observe two consecutive managers for at least four

[12] It should be noted, though, that the effect of a white manager on the probability of a new hire being Asian (Table 4d) is negative and significant at the 6 percent confidence level.
[13] A Wald test indicates that coefficients on the three dummy variables (for white, Hispanic, and Asian managers) are not significantly different from one another.

months each. Figure 1a shows trends in the white share of new hires for cases where: 1) a white manager replaces a white manager, 2) a black manager replaces a white manager, and 3) a white manager replaces a black manager. Figure 1b shows similar trends in the black share of new hires. In all cases, the introduction of a new manager whose race differs from the outgoing manager results in either a clear intercept shift or a break in the previous trend.[14]

Robustness Test using Linear Probability Model.—The estimation sample is restricted to stores that hire at least one new employee belonging to each of the four main race groups. This sample restriction allows me to estimate all probit models on the same sample of stores, and is also necessary to estimate the multinomial logit model with store fixed effects. However, there is a concern that this restriction introduces some sample selection bias. By eliminating stores in which there is no change in the hiring probability for at least one race group, I may be dropping many stores in which a change in manager race has zero impact on hiring patterns. To examine the implications of the sample selection, I use a linear probability model to estimate all of the binomial choice equations on both the restricted and full samples. The results, reported in Appendix Table A1, are similar for both samples, and are substantively similar to the probit results.

B. Multinomial Logit Estimates of Race-Based Hiring Biases

The results from the fixed effects multinomial logit estimation are shown in Tables 5 and 6. They are remarkably similar to the probit results. The multinomial results facilitate the comparison of hiring patterns across all four manager races. First, Table 5 shows the coefficients of the model (columns 1-6), and a Wald test assessing the overall similarity between hiring patterns for each manager race pair (column 7). This table confirms that there are no significant hiring differences among white, Hispanic, and Asian managers, but that the hiring choices of each

[14] In three out of four cases involving a change in manager race, it appears that the manager change coincides with a reversal of the previous demographic trend in hiring. However, regressions based on six-month trends prior to a change in management showed that this relationship is not significant: In no case does the trend in hiring predict the change or direction of change in manager race.

nonblack group differ significantly from those of black managers. Specifically, these nonblack-black differences lie in the ratios of black hires to hires of other races, and mainly in the ratio of black hires to white hires.

Next, Table 6 presents the predicted probabilities for each manager-employee race combination.[15] Differences among whites, Hispanics, and Asians are very small (the largest being the 1.3 percentage point difference between whites and Asian managers in the share of Asians hired).[16] It is also notable that black managers differ very little from the three nonblack groups in the shares of Hispanics and Asians hired. Once again, the differences that stand out are those between nonblack managers and black managers in the shares of blacks and whites hired. The estimates imply that when a black manager is replaced by a white, Hispanic, or Asian manager in a typical store, the share of new hires that is black falls by 3.8-4.8 percentage points. In all cases, this decline is offset mainly by an increase in the share that is white.

C. How Large are the Effects of Manager Race on Hiring Patterns?

To interpret the magnitudes of these effects, consider (as an example) what happens to the racial composition of the average store when a white manager replaces a black manager. Employee turnover is high at this company—90 percent of all employees leave within one year of being hired. Thus, within a year of a change in management, almost all employees will have been hired by the new manager. The estimates in Table 6 therefore suggest that within a year, the employment share of blacks falls from 21 percent to 17 percent, and the share of whites rises from 60 percent to 64 percent. In a store of 25 employees, this change amounts to going from 5 blacks and 15 whites to 4 blacks and 16 whites.

Now, from the viewpoint of someone (such as a district manager) who is observing just a small sample of stores, this change might either go unnoticed or appear insignificant. However, the change appears much more significant from the point of view of job seekers—and especially

15 Predicted probabilities are calculated at the means of all store and month dummy variables.
16 The ratios of Asians to whites and of Asians to blacks are higher under an Asian manager than under a white manager, but these differences are significant only at 9 and 13 percent confidence levels.

black job seekers. Indeed, a 3.8 percentage point decline in the black share (from 20.9 to 17.1) amounts to an 18 percent decline in the number of blacks hired.

It is important to remember that because I lack applicant pool data, I cannot conclude that the nonblack-black biases I find are due to discrimination by nonblack managers against black employees. Rather, they could indicate discrimination either by managers or by applicants, and either by nonblacks or by blacks. Specifically, there are four possible patterns of discrimination: (1) black managers may discriminate against or discourage nonblack (especially white) applicants, (2) black applicants may avoid working for nonblack managers, (3) nonblack managers may discriminate against or discourage black applicants, or (4) nonblack (especially white) applicants may avoid working for black managers. It is unlikely that any of these groups is free of discriminatory tastes.[17]

Still, it is true that my results are consistent with the hypothesis that is the starting point for most studies of racial discrimination—that employers discriminate against blacks. Hence, to facilitate comparison with other studies, it is useful to consider what my estimates would imply in the extreme case that they were driven entirely by discrimination against black employees. If we assume that black managers in my study do not discriminate, then we can infer the share of blacks in the pool of qualified applicants from the share of blacks hired by black managers—21 percent. Under this assumption, my estimates imply that black applicants are roughly 25 percent less likely than white applicants to be hired.[18] Interestingly, this figure is comparable to what has been found in audit studies comparing hiring rates of white and black applicants. For example,

[17] Responses to questions on racial attitudes in the General Social Survey indicate that while attitudes of whites toward blacks are more negative than those of blacks toward whites, discriminatory attitudes nevertheless are found among both groups. For example: Between 1990 and 2000, 15.3% of all whites and 4.4% of all blacks favored laws prohibiting interracial marriage.

[18] If p is the probability that a given applicant is hired by a black (i.e., non-discriminating) manager, then a black applicant's probability of being hired when facing a non-black (i.e., discriminating) manager is (.17/.21) p or .81p, while for white applicants this probability is (.64/.60) p or 1.07p. Hence, a black applicant's probability of being hired is (1.07-0.81)/1.07=24.3% lower than that for white applicants.

Turner et al. (1991) find that black applicants are between 25 and 35 percent less likely than white applicants to be offered a job.

While my findings are consistent in this way with audit study findings for blacks, they stand in contrast to audit studies on Hispanics. The latter have tended to find discrimination against Hispanic applicants that is similar in magnitude to discrimination against blacks (e.g. Cross et al, 1990; Kenney and Wissoker, 1994).[19] However, other types of studies have found results that are similar to mine in suggesting discrimination against blacks but not against Hispanics. For example, Holzer (1996) examines differences across races in the ratios of hires to applicants, and finds that this ratio is lowest for blacks—particularly in sales jobs at service and retail firms. Consistent with my own results, his estimates imply that the ratio of hires to applicants is about 50% greater for Hispanics than it is for blacks.[20]

D. The South

Another germane question is whether the difference between nonblack and black hiring patterns is particularly strong in the South. There is evidence that racial bias remains more common in the South that in the rest of the U.S., particularly between whites and blacks. For example, the 1990-2000 General Social Survey found that among respondents in non-Southern states, 3.0% of blacks and 11.4% of whites favored laws against interracial marriage, while in the South, 5.7% of blacks and 23.0% of whites favored such laws.[21,22]

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[19] One possible explanation for the difference between these findings and my own is that the audit studies using Hispanics are biased due to use of testers who are different in respects not solely related to race. For example, Heckman and Siegelman (1993) point out that in the study by Cross et al., Hispanic testers all had facial hair and strong accents while white (Anglo) testers did not. It is also possible that my estimates are attenuated by some error in the Hispanic measure. Because an employee may be both white and Hispanic, but can be coded only one way in this dataset, it is possible that some Hispanics code themselves as white.

[20] The implication that racial discrimination appears to affect blacks more than recent immigrant groups is also consistent with some qualitative evidence comparing white-black relations to white-immigrant relations. For example, Moss and Tilly (1995) find that many nonblack employers stereotype immigrants as hard working, whereas they tend to stereotype blacks as lazy and irresponsible.

[21] Because most of the managers and employees in this data set under 40, it should be noted that survey respondents under 40 were less likely to favor these laws. Nevertheless, among whites in the under-40 sample, the nonSouth-South difference remains significant: 5.6% of nonsouthern whites vs. 13.4% of southern whites favor the laws. All statistics are the author's calculations based on sample of 8,351 from surveys between 1990 and 2000.

To compare hiring biases in the South to the biases in the rest of the country, I run separate fixed effect probit regressions on the two samples.[23] Table 7 shows the results of probit regressions predicting the probability that (1) a new hire is white and (2) a new hire is black. The key independent variable in both regressions is a dummy variable indicating whether the hiring manager is black.

The tendency of nonblack managers to hire more whites and fewer blacks than black managers is particularly strong in the South, and the differences between Southern and non-Southern states are quite significant. When a nonblack manager replaces a black manager in a non-Southern store, the share of black hires falls by 14.8% (from .169 to .144) and the share of whites rises by 5.0% (from .544 to .571). In Southern stores, the difference is much larger. The share of blacks falls by 27.6%, from (.293 to .212) while the share of whites increases by 18.1% (from .520 to .614).[24]

E. Probit Estimates of Gender-Based Hiring Biases

The results of the probit regressions analyzing the effect of a manager's sex on the probability that a new hire is female are shown in Table 8. The correlation between manager and employee sex is fully accounted for by observed and unobserved differences across store locations. Specifically, controlling for population density and store location reduces the estimated effect of manager sex by about 15 percent (column 3), and the inclusion of store fixed effects brings it very close to zero (columns 5 and 6). Hence, at this company, there are no systematic hiring biases based on differences in gender.

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[22] For another example, see Kuklinkski, Cobb, and Gilens (1997), who estimate that 10% of nonsouthern whites and 42% of southern whites react negatively to the idea of living next door to a black family.

[23] Here, the South is defined as: Arkansas, Alabama, Georgia, Florida, Louisiana, Mississippi, North Carolina, Tennessee, South Carolina, and Virginia.

[24] We may again consider the extreme assumption that the hiring biases are caused entirely by discrimination against black employees. My estimates imply that, whereas black applicants would be 19 percent less likely than whites to be hired by a white manager in the rest of the country, they would be 39 percent less likely to be hired by a white in the South.

The finding of zero gender discrimination stands in contrast to other current studies that find evidence of gender-based employment discrimination. Among these studies, only Carrington and Troske (1998) take the same approach as the present study; they, too, ask whether gender segregation is caused by discrimination. The fact that Carrington and Troske reach a different conclusion may partly reflect their inability to control for unobserved differences in workplaces. The present study finds that despite the similarity of the stores and jobs in the sample, store fixed effects are important; this demonstrates that even subtle differences across stores, locations, or labor markets can cause some workplaces to employ more women than others. However, both in the case of Carrington and Troske's study and in the case of two other studies as well, the contrast in results may reflect differences in samples. In the present sample, women make up a large majority of both the workforce and management. In contrast, the other studies all analyze industries traditionally dominated by men—namely, manufacturing (Carrington and Troske), orchestras (Goldin and Rouse, 2000), and high-priced restaurants (Neumark, et al., 1996). Gender-based discrimination may be prevalent in these three industries due to historical and social norms that cause certain occupations to be considered exclusively male. Clearly, this is not true of the jobs in my study.

F. Gender Differences in Race Discrimination

The fact that my sample is largely female may have implications not only for the results on gender discrimination but also for the results on race discrimination. First, some studies have suggested that women are less likely than men to discriminate on the basis of race (Kenney and Wissoker, 1994; Yinger, 1986). Second, other studies have suggested that black men are more likely than black women to be discriminated against (e.g. Holzer, 1996; Moss and Tilly, 1995; Kirschenman and Neckerman, 1991).

I examine whether the nonblack-black hiring biases in my sample differ by either the sex of the manager (Table 9), or the sex of the employee (Table 10). Table 9 shows the results of a fixed effects probit regression predicting that a new hire is black as a function of a dummy variable indicating that the manager is black, another dummy variable indicating that the manager is female, and the interaction of these two dummy variables. With respect to nonblack men and women, I find that there is no difference in the share of blacks they hire. However, with respect to black men and women, the negative interaction term (while not highly significant, $p<.14$) suggests that the tendency to hire black employees is stronger for black male managers than for black female managers. There are two possible causes of this result. One is that black managers—and in particular black male managers—discriminate against nonblack employees. The other is that nonblack applicants avoid working for black managers, and especially black male managers.

Table 10 shows the results of separate probit regressions predicting (1) that a new hire is a black male, and (2) that a new hire is a black female. Both male and female black employees are less likely to be hired by nonblack managers than by black managers. Specifically, when a nonblack manager replaces a black manager, the probability that the next hire is a black male falls by about 18 percent, from .055 to .045, and the probability that the next hire is a black female falls 16.5 percent, from .109 to .091. Hence, the hiring biases affect the employment of black males and females more or less equally.

IV. Alternate Explanations: Social Networks or Productivity Effects

While my findings suggest that nonblack managers hire fewer blacks than do black managers, this difference in hiring patterns could be caused by factors other than discrimination. The recent literature suggests two possibilities. Managers might hire same-race employees either because they use their own social networks in recruitment, or because they want to exploit productivity advantages inherent in manager-employee similarity.

A. Neighborhood Segregation and Networks

If managers use their own social networks in recruitment, and if social networks tend to be segregated by race, then these networks may lead managers to hire same-race employees.[25,26] Thus social segregation could be driving the nonblack-black differences in hiring.

To explore this possibility, I use data on the residential ZIP codes of managers and employees. Although ZIP codes are only an imperfect proxy for one's network of acquaintances, nevertheless residential areas and social networks do tend to coincide. Hence if the social networks of managers are driving my results, I should find that managers tend to hire employees who live in the manager's ZIP code.

Instead, I find that the share of employees living in the same ZIP code as the manager is quite small—roughly 4.7 percent. Indeed, employees are much more likely to live near the store than they are to live near the manager—with 13 percent sharing the store's ZIP code. Moreover, Table 11 presents evidence that the slight tendency of managers and new hires to live in the same ZIP is spurious, and not caused by managers hiring their neighbors. Specifically, the probability that a new hire lives in a particular ZIP is only .1 to .2 percentage points higher if the manager also lives that ZIP.[27] This effect is extremely small and not statistically significant.[28]

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[25] Previous studies have argued that informal networks play an important role in hiring (e.g. Granovetter, 1995; Holzer, 1996); that such networks tend to be segregated by race (Marsden ,1987); and that minorities tend to lack access to hiring networks (Petersen et al., 2000; Moss and Tilly 2001).

[26] Much of the network literature emphasizes the role of employee referrals rather than managers' personal networks, and some have found that employees tend to refer similar others (e.g. Fernandez et al., 2000; Muow, 2002 on race). Employee referrals alone cannot cause manager-employee similarity, but if there is some tendency for managers to hire same-race employees, then employee referrals could amplify this tendency. In my data, however, regression analysis revealed that the share of employees of race *k* at the time of hire is negatively related to the probability that the next hire is race *k*. This is the opposite of what would be expected if employee referrals were an important source of workplace segregation.

[27] These estimates are based on fixed effects probit regressions predicting the probability that a new hire resides in a given ZIP code as a function of a dummy variable indicating that the manager also resides in that ZIP code. The estimation sample is restricted to stores that have at least two managers who live in different ZIP codes, and to the two managers with the longest tenures in each of these stores. Thus, the regressions examine whether a manager is more likely to hire employees in his or her own ZIP code than is another manager in the same store but from a different ZIP code.

[28] Since I find bigger nonblack-black hiring biases in the South, I also look to see if the tendency to hire from within one's own ZIP code is stronger in the South. The estimates for this sample are slightly larger—.3 to .5 percentage points—but still are not significantly different from zero.

B. Productivity Effects of Manager-Employee Similarity

Managers might hire same-race employees because same-race relationships are more productive. For example, racial diversity may raise transactions costs or make communication difficult. In this study, lower levels of mutual trust, respect, or cooperation between nonblacks and blacks could make nonblack-black relationships particularly unproductive.[29] And even among native English speakers, racial diversity could make communication difficult.[30] Still, it remains true that language difficulties are likely less important for blacks than for recent immigrant groups (Hispanics and Asians), and thus if "language discrimination" were driving the nonblack-black results, we would expect to see similar biases against these immigrant groups.

To test the hypothesis that black employees are more productive when they are managed by blacks, I use data on store monthly sales. Table 12 reports the results of a linear fixed effects regression of log monthly sales on manager race, employment shares of each race, and the interactions of these variables. The coefficient on the interaction of the black manager indicator and the black share of employment is positive, suggesting that the relative productivity of black employees to white employees is slightly higher under a black a manager than under a nonblack manager. However, this difference is not significant. Hence, these results do not indicate that productivity is improved significantly by manager-employee similarity, or that productivity considerations are driving my results.[31]

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[29] This hypothesis is suggested by the organizational behavior literature on racial "mismatch". Typical findings are that subordinates whose manager is a different race have lower perceptions of procedural justice and lower job satisfaction (Wesolowski and Mossholder, 1997), that white subordinates with black supervisors report high role ambiguity and role conflict (Tsui and O'Reilly, 1989), and that same-race mentoring relationships last longer and provide more psychosocial support than do cross-race relationships (Thomas, 1990). Using the data set employed in the present paper, Levine, Leonard and Giuliano (2003) also find that rates of quits and dismissals are higher for employees whose manager is a different race. Of course, differential manager-employee compatibility may itself result from manager prejudice and discrimination in the treatment of employees.

[30] Lang (1986) emphasizes this point in his model of "language discrimination".

[31] Similar regressions that allow the effects to differ in the South (not reported in table) indicate that even in the South, sales are unaffected by manager-employee racial differences.

V. Discrimination and Sales: A Test of the Becker Hypothesis

Theoretically, the regression results in Table 12 could also help to distinguish whether it is blacks or nonblacks that are doing the discriminating. A key insight of Becker's (1957) theory of employer discrimination is that employers who discriminate must forego profits to do so, since their hiring decisions are based on tastes and not solely on productivity considerations.[32] This implies, for example, that if black managers are discriminating by giving preference to less qualified black employees over qualified white employees at the same wage, then black managers—especially those who hire very few whites—should have lower sales.[33] Similarly, if it is non-black managers who are discriminating, then we should observe lower sales under non-black managers.

However, the estimates from the regression of sales on the interactions of manager race and employee race shares suggests that manager-employee similarity has no effect on sales. Because I find no significant relationship between sales and hiring biases, I learn little about who is exercising discriminatory preferences. Nevertheless, the lack of a sales effect is interesting. It suggests that managers in this company can wield some latitude in hiring decisions and still maintain sales, and therefore that they have some freedom to exercise discriminatory tastes.

VI. Conclusion

This study has used a new panel data set to examine whether segregation in the workplace is caused by same-race and same-gender biases in hiring. Data on consecutive managers at hundreds of stores makes it possible to estimate models with store fixed effects—

[32] This insight is the focus of a few recent studies; for example, Hellerstein et al. (1997) find a positive cross-section relationship between firm profitability and the share of women in the workforce, which is consistent with discrimination against women. Related studies comparing wages to marginal products include Hellerstein et al. (1996) and several studies of performance and salaries in sports, surveyed by Kahn (1991). Leonard (1984) examines the productivity consequences of employment composition shifts resulting from compliance with federal contract regulations.

[33] Wage regressions controlling for observable employee characteristics and store and month dummies revealed very small differences (one to three cents per hour) in starting wages by race. These differences may be due to some unmeasured differences in experience (including previous work with the company). In any case, they are too small to affect profits in any significant way and thus should not affect hiring decisions. There are also no significant differences in wage differentials by manager race.

something previous studies of segregation have been unable to do. The present study can thus identify more accurately the effect of manager demographics on hiring choices. The main findings are: (1) In the case of men vs. women, discrimination in hiring plays no role; (2) among nonblacks (whites, Asians, and Hispanics), discrimination plays a small to negligible role; and (3) in the case of nonblacks vs. blacks, discrimination is a likely culprit. Specifically, all nonblack managers all hire substantially more whites and fewer blacks than do black managers. Moreover, these nonblack-black biases are especially strong in the South.

The main caveat for these results is that the differences in hiring patterns between nonblacks and blacks could reflect bias either by managers or by applicants, and either by nonblacks or by blacks. Nevertheless, regardless of whose preferences are driving them, it is important to emphasize that these nonblack-black biases are significant for black job seekers. Because blacks are underrepresented in management, such hiring biases will ensure inferior employment outcomes for blacks. Clearly, one solution to improving job opportunities for blacks is to get more blacks into positions of management and ownership.

With regard to bias among nonblacks and bias between the sexes, the present study is much more encouraging than certain previous studies. The latter have found evidence of discrimination against both women and Hispanics. To be sure, it is important to remember that (as in all case studies) the context should be considered when interpreting the results. In this study, several factors may mitigate against discrimination: the company's anti-discrimination policy; the high share of women and the relative youth of workforce; the tight labor market during the sample period; and the high rates of manager and employee turnover. However, the results of this study do suggest that in the right historical and institutional context, there is very little discrimination in hiring between men and women, and among whites, Hispanics, and Asians in the U.S.

Unfortunately, these results suggest a less optimistic conclusion regarding nonblack-black relations, particularly in the South. The presence of significant nonblack-black biases is especially discouraging in light of the potential mitigating factors; indeed, these biases occur in a context where hiring bias is absent along other dimensions. While the persistence of such racial bias will not come as a surprise to many, it has nevertheless been difficult to prove that such bias systematically affects the labor market. This study provides rare statistical evidence that the problem of race relations in United States has real effects on employment—especially in the South.

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TABLE 1. WORKPLACE AND COMMUNITY DEMOGRAPHICS

	Workplace		Community	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**
Race/Ethnicity				
White	56.7%	21.6%	73.4%	19.1%
Black	18.0%	14.3%	8.9%	10.9%
Hispanic*	13.7%	12.7%	7.5%	9.7%
Asian/Pacific Isle.	10.0%	9.0%	6.3%	7.7%
Native American	0.4%	0.7%	0.3%	0.4%
Other/Unknown	1.1%	1.4%	3.6%	5.2%
Gender				
Male	31.55%	10.0%	48.5%	3.7%
Female	68.45%	10.0%	51.5%	3.7%
Population			114,325	123,616
Median household income			$40,000	$15,500

Notes: Community statistics are from the 1990 Census and are based on all Census tracts within two miles of the center of each store's ZIP code. *In the Census, respondents can categorize themselves as both black and Hispanic or as both white and Hispanic, whereas the employer has mutually exclusive codes of white, black and Hispanic. In this table, the Census figures for whites and blacks refer to non-Hispanics, while the Hispanic figures refer to Hispanics of all races.

TABLE 2. DEMOGRAPHIC COMPOSITION OF WORKFORCE AND MANAGEMENT, 1996-1998

	Company employees: Estimation sample		Company employees: All retail stores		All Retail (CPS)	
	Sales workers	**Managers**	**Sales workers**	**Managers**	**Sales workers**	**Managers**
Race/Ethnicity:						
White	56.6%	84.0%	68.4%	85.9%	72.6%	81.0%
Black	19.1%	6.5%	13.0%	5.8%	12.7%	6.6%
Hispanic*	13.6%	6.5%	10.1%	5.2%	10.0%	6.9%
Asian/Pacific Isle.**	9.2%	2.9%	6.9%	2.6%	4.1%	5.0%
Native American	0.4%	0.2%	0.3%	0.1%	0.7%	0.6%
Other/Unknown	1.0%	1.0%	1.2%	0.5%	--	--
Gender:						
Male	32.6%	24.5%	25.4%	19.8%	33.7%	49.8%
Female	67.4%	75.5%	74.6%	81.2%	66.3%	50.1%
Average Age:	22.2	30.0	22.1	30.3	32.5	39.4

Notes: Company statistics based on all individuals employed at least one day between February 1, 1996 and July 31, 1998.
*In the CPS, respondents can categorize themselves as both black and Hispanic or as both white and Hispanic, whereas the employer has mutually exclusive codes of white, black and Hispanic. In this table, the CPS figures for whites and blacks refer to non-Hispanics, while the Hispanic figures refer to Hispanics of all races.
**Unlike our employer, the CPS lumps "other" races together with Asians and Pacific Islanders.

TABLE 3. AVERAGE DEMOGRAPHICS OF NEW HIRES BY MANAGER RACE AND GENDER

	Manager Race				
Employee Race	White	Black	Hispanic	Asian	All Managers
White	**58.3%**	44.8%	42.1%	48.6%	56.3%
Black	18.5%	**30.9%**	20.9%	16.2%	19.3%
Hispanic	12.7%	13.5%	**27.0%**	13.7%	13.6%
Asian	9.0%	9.3%	8.7%	**19.4%**	9.3%
Native Amer./other	1.5%	1.5%	1.3%	2.1%	1.5%

	Manager Sex		
Employee Sex	Female	Male	All Managers
Female	**67.8%**	65.8%	67.3%
Male	32.2%	**34.2%**	32.7%

Notes: Statistics based on estimation sample: All individuals hired between February 1, 1996 and July 31, 1998 at stores that hired at least one new employee of each race during this sample period. N>100,000 new hires.

TABLE 4A. PROBIT ESTIMATES OF THE EFFECT OF MANAGER RACE ON THE PROBABILITY THAT A NEW HIRE IS WHITE

	(1)	(2)	(3)	(4)	(5)	(6)
Hiring manager is black[1]	**-0.135****	**-0.103****	**-0.108****	**-0.105****	**-0.044****	**-0.052****
	(0.027)	(0.021)	(0.020)	(0.020)	(0.012)	(0.013)
Hiring manager is Hispanic[1]	**-0.162****	**-0.056***	**-0.062****	**-0.056***	**-0.005**	**-0.014**
	(0.034)	(0.028)	(0.024)	(0.024)	(0.012)	(0.014)
Hiring manager is Asian[1]	**-0.098***	**0.021**	**-0.004**	**0.001**	**-0.007**	**-0.012**
	(0.041)	(0.031)	(0.025)	(0.025)	(0.014)	(0.020)
Population % black		-0.691**	-0.669**	-0.675**		
		(0.082)	(0.076)	(0.076)		
Population % Hispanic		-1.219**	-0.964**	-0.978**		
		(0.144)	(0.097)	(0.096)		
Population % Asian		-1.278**	-0.959**	-0.960**		
		(0.168)	(0.090)	(0.090)		
Population % other		-0.656**	-0.584**	-0.580**		
		(0.190)	(0.166)	(0.166)		
Median household income (in $10,000)			-0.023**	-0.023**		
			(0.004)	(0.004)		
Population within 2 miles (in 100,000's)			-0.073**	-0.073**		
			(0.008)	(0.008)		
Location==Open Mall[1]			0.047*	0.045		
			(0.023)	(0.023)		
Location==Street[1]			0.043*	0.042*		
			(0.020)	(0.020)		
Location==Strip[1]			0.058**	0.058**		
			(0.016)	(0.016)		
Month dummies	--	--	--	Yes	Yes	Yes
Store dummies	--	--	--	--	Yes	Yes
Store-specific trends	--	--	--	--	--	Yes
Number of hires	>100,000	>100,000	>100,000	>100,000	>100,000	>100,000
Number of stores	>700	>700	>700	>700	>700	>700

Notes: Table reports marginal effects. Parentheses contain robust standard errors, adjusted for clustering on store. [1] Table reports change in probability that a new hire is white for a discrete change of dummy variable from 0 to 1. Omitted manager race is white. Omitted location type is mall. * significant at 5%; ** significant at 1%.

Table 4B. Probit Estimates of the Effect of Manager Race on the Probability that a New Hire is Black

	(1)	(2)	(3)	(4)	(5)	(6)
Hiring manager is white[1]	**-0.121****	**-0.089****	**-0.091****	**-0.088****	**-0.035****	**-0.048****
	(0.021)	(0.018)	(0.018)	(0.017)	(0.008)	(0.011)
Hiring manager is Hispanic[1]	**-0.075****	**-0.046***	**-0.041***	**-0.041***	**-0.039****	**-0.046****
	(0.020)	(0.022)	(0.019)	(0.019)	(0.009)	(0.013)
Hiring manager is Asian[1]	**-0.106****	**-0.093****	**-0.081****	**-0.081****	**-0.040****	**-0.049****
	(0.017)	(0.016)	(0.015)	(0.015)	(0.013)	(0.016)
Population % white		-0.592**	-0.618**	-0.621**		
		(0.067)	(0.068)	(0.068)		
Population % Hispanic		-0.705**	-0.880**	-0.872**		
		(0.106)	(0.103)	(0.103)		
Population % Asian		-0.458**	-0.621**	-0.630**		
		(0.112)	(0.085)	(0.085)		
Population % other		-0.513**	-0.563**	-0.573**		
		(0.132)	(0.121)	(0.120)		
Median household income (in $10,000)			0.019**	0.019**		
			(0.004)	(0.004)		
Population within 2 miles (in 100,000's)			0.033**	0.033**		
			(0.005)	(0.005)		
Location==Open Mall[1]			-0.054**	-0.053**		
			(0.014)	(0.014)		
Location==Street[1]			-0.003	-0.003		
			(0.018)	(0.018)		
Location==Strip[1]			-0.027	-0.028		
			(0.014)	(0.014)		
Month dummies	--	--	--	Yes	Yes	Yes
Store dummies	--	--	--	--	Yes	Yes
Store-specific trends	--	--	--	--	--	Yes
Number of hires	>100,000	>100,000	>100,000	>100,000	>100,000	>100,000
Number of stores	>700	>700	>700	>700	>700	>700

Notes: Table reports marginal effects. Parentheses contain robust standard errors, adjusted for clustering on store. [1] Table reports change in probability that a new hire is black for a discrete change of dummy variable from 0 to 1. Omitted manager race is black. Omitted location type is mall. * significant at 5%; ** significant at 1%. Wald test of equality of column (5) coefficients: $chi^2(2)= 1.53$; $Pr(>chi^2) = 0.464$.

TABLE 4C. PROBIT ESTIMATES OF THE EFFECT OF MANAGER RACE ON THE PROBABILITY THAT A NEW HIRE IS HISPANIC

	(1)	(2)	(3)	(4)	(5)	(6)
Hiring manager is white[1]	**-0.137****	**-0.032****	**-0.028****	**-0.026****	**-0.005**	**-0.004**
	(0.023)	(0.012)	(0.010)	(0.010)	(0.007)	(0.010)
Hiring manager is black[1]	**-0.082****	**-0.037****	**-0.029****	**-0.028****	**-0.008**	**-0.005**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.008)	(0.011)
Hiring manager is Asian[1]	**-0.079****	**-0.040****	**-0.031****	**-0.031****	**-0.005**	**-0.006**
	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.015)
Population % white		-0.741**	-0.676**	-0.674**		
		(0.081)	(0.068)	(0.067)		
Population % black		-0.746**	-0.750**	-0.748**		
		(0.100)	(0.092)	(0.090)		
Population % Asian		-0.607**	-0.700**	-0.700**		
		(0.103)	(0.079)	(0.078)		
Population % other		-0.267	-0.313*	-0.308*		
		(0.145)	(0.123)	(0.121)		
Median household income (in $10,000)			0.001	0.002		
			(0.002)	(0.002)		
Population within 2 miles (in 100,000's)			0.031**	0.031**		
			(0.003)	(0.003)		
Location==Open Mall[1]			-0.001	0.000		
			(0.015)	(0.015)		
Location==Street[1]			-0.019*	-0.019*		
			(0.008)	(0.008)		
Location==Strip[1]			-0.005	-0.005		
			(0.007)	(0.007)		
Month dummies	--	--	--	Yes	Yes	Yes
Store dummies	--	--	--	--	Yes	Yes
Store-specific trends	--	--	--	--	--	Yes
Number of hires	>100,000	>100,000	>100,000	>100,000	>100,000	>100,000
Number of stores	>700	>700	>700	>700	>700	>700

Notes: Table reports marginal effects. Parentheses contain robust standard errors, adjusted for clustering on store. [1] Table reports change in probability that a new hire is Hispanic for a discrete change of dummy variable from 0 to 1. Omitted manager race is Hispanic. Omitted location type is mall. * significant at 5%; ** significant at 1% .

TABLE 4D. PROBIT ESTIMATES OF THE EFFECT OF MANAGER RACE ON THE PROBABILITY THAT A NEW HIRE IS ASIAN

	(1)	(2)	(3)	(4)	(5)	(6)
Hiring manager is white[1]	**-0.097****	**-0.028**	**-0.029**	**-0.028**	**-0.015**‡	**-0.022****
	(0.024)	(0.018)	(0.017)	(0.016)	(0.008)	(0.007)
Hiring manager is black[1]	**-0.057****	**-0.017**	**-0.019**	**-0.019**	**-0.012**	**-0.015**
	(0.010)	(0.014)	(0.014)	(0.013)	(0.008)	(0.007)
Hiring manager is Hispanic[1]	**-0.061****	**-0.030***	**-0.029***	**-0.031****	**-0.007**	**-0.005**
	(0.010)	(0.013)	(0.012)	(0.012)	(0.008)	(0.008)
Population % white		-0.514**	-0.519**	-0.519**		
		(0.043)	(0.048)	(0.047)		
Population % black		-0.590**	-0.567**	-0.565**		
		(0.050)	(0.053)	(0.052)		
Population % Hispanic		-0.558**	-0.556**	-0.554**		
		(0.054)	(0.058)	(0.057)		
Population % other		-0.290**	-0.273*	-0.274*		
		(0.101)	(0.107)	(0.107)		
Median household income (in $10,000)			0.004*	0.004*		
			(0.001)	(0.001)		
Population within 2 miles (in 100,000's)			-0.004	-0.004		
			(0.003)	(0.003)		
Location==Open Mall[1]			0.010	0.010		
			(0.012)	(0.012)		
Location==Street[1]			-0.005	-0.004		
			(0.007)	(0.007)		
Location==Strip[1]			-0.028**	-0.027**		
			(0.005)	(0.005)		
Month dummies	--	--	--	Yes	Yes	Yes
Store dummies	--	--	--	--	Yes	Yes
Store-specific trends	--	--	--	--	--	Yes
Number of hires	>100,000	>100,000	>100,000	>100,000	>100,000	>100,000
Number of stores	>700	>700	>700	>700	>700	>700

Notes: Table reports marginal effects. Parentheses contain robust standard errors, adjusted for clustering on store. [1]Table reports change in probability that a new hire is Asian for a discrete change of dummy variable from 0 to 1. Omitted manager race is Asian. Omitted location type is mall. ‡ significant at 10%; * significant at 5%; ** significant at 1%. Wald test for joint significance of column (5) coefficients: $chi^2(3) = 5.30$; $Pr(>chi^2) = 0.151$.

TABLE 5. MULTINOMIAL LOGIT ESTIMATES OF THE EFFECTS OF MANAGER RACE ON THE RACE OF NEW HIRES

Change in race of hiring manager:	White vs. Black	White vs. Hispanic	White vs. Asian	Black vs. Hispanic	Black vs. Asian	Hispanic vs. Asian	Wald Test Chi2 (Pr>chi2)
	Change in log odds that new hire is race 1 vs. race 2						
White to Black	-0.274**	-0.095	-0.112	0.179*	0.162*	-0.018	21.82**
	(0.059)	(0.072)	(0.074)	(0.075)	(0.073)	(0.079)	(0.000)
White to Hispanic	0.039	-0.013	-0.096	-0.053	-0.136	-0.083	2.26
	(0.068)	(0.067)	(0.077)	(0.089)	(0.093)	(0.088)	(0.520)
White to Asian	0.053	-0.009	-0.163‡	-0.044	-0.217	-0.173	3.33
	(0.094)	(0.106)	(0.095)	(0.121)	(0.142)	(0.120)	(0.343)
Black to Hispanic	0.313**	0.081	0.016	-0.232*	-0.297**	-0.065	13.89**
	(0.086)	(0.089)	(0.101)	(0.106)	(0.115)	(0.109)	(0.003)
Black to Asian	0.327**	0.104	-0.051	-0.223*	0.378*	-0.155	9.24*
	(0.110)	(0.127)	(0.118)	(0.139)	(0.157)	(0.143)	(0.026)
Hispanic to Asian	0.014	0.023	-0.067	0.009	-0.081	-0.089	0.46
	(0.115)	(0.126)	(0.118)	(0.150)	(0.165)	(0.145)	(0.093)

Notes: Based on multinomial logit regressions predicting the race of a new hire. Controls include month dummies and store fixed effects. Parentheses contain robust standard errors, adjusted for clustering on store. Final column reports Wald test of H_o: No change in hiring pattern. N > 100,000 new hires. ‡ significant at 10%; * significant at 5%; ** significant at 1%

TABLE 6. PREDICTED PROBABILITIES FOR NEW HIRES OF EACH RACE BY MANAGER RACE

	Race of Manager			
Race of New Hire	White	Black	Hispanic	Asian
White	.641	.597	.639	.638
Black	.171	.209	.163	.161
Hispanic	.111	.114	.112	.109
Asian	.078	.081	.086	.091

Notes: Based on multinomial logit predicting the race of a new hire. Controls include month dummies and store fixed effects. N > 100,000 new hires.

TABLE 7. DIFFERENTIAL BLACK-NONBLACK HIRING BIASES IN SOUTHERN VS. NON-SOUTHERN STATES

	Dependent variable is dummy variable = 1 if new hire is:			
	(1) White		**(2) Black**	
	South	**Non-South**	**South**	**Non-South**
Change in probability for a change in manager race from nonblack to black	-0.094**	-0.027*	0.081**	0.026**
	(0.020)	(0.013)	(0.014)	(0.009)
Predicted Probabilities for:				
Nonblack Manager	0.614	0.571	0.212	0.144
Black Manager	0.520	0.544	0.293	0.169

Notes: Based on probit regressions predicting probability that a new hire is white (black). Regressions include store and month dummies. Parentheses contain robust standard errors, adjusted for clustering on store. N > 100,000. South is defined here as states that were part of the Confederacy, except Texas. These states are: Arkansas, Alabama, Georgia, Florida, Louisiana, Mississippi, North Carolina, Tennessee, South Carolina, and Virginia. * significant at 5%; ** significant at 1%.

TABLE 8. PROBIT ESTIMATES OF THE EFFECT OF MANAGER SEX ON THE PROBABILITY THAT A NEW HIRE IS FEMALE

	(1)	(2)	(3)	(4)	(5)	(6)
Hiring manager is female[1]	**0.020****	**0.020****	**0.017****	**0.017****	**0.002**	**-0.006**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Population % female		-0.138	-0.087	-0.084		
		(0.089)	(0.089)	(0.087)		
Median household income (in $10,000)			0.004	0.004		
			(0.002)	(0.002)		
Population within 2 miles (in 100,000's)			-0.020**	-0.021**		
			(0.003)	(0.003)		
Location==Open Mall[1]			-0.010	-0.010		
			(0.019)	(0.019)		
Location==Street[1]			-0.014	-0.013		
			(0.012)	(0.012)		
Location==Strip[1]			0.021**	0.023**		
			(0.007)	(0.007)		
Month dummies	--	--	--	Yes	Yes	Yes
Store dummies	--	--	--	--	Yes	Yes
Store-specific trends	--	--	--	--	--	Yes
Number of hires	>100,000	>100,000	>100,000	>100,000	>100,000	>100,000
Number of stores	>700	>700	>700	>700	>700	>700

Notes: Table reports marginal effects. Parentheses contain robust standard errors, which are adjusted for clustering on store. Omitted location type is mall.
[1] Table reports change in probability that a new hire is female for a discrete change of dummy variable from 0 to 1. * significant at 5%; ** significant at 1%.

TABLE 9. RELATIONSHIP BETWEEN MANAGER'S GENDER AND BIASES IN HIRING BLACKS

Hiring manager is female[1]	**-0.004**
	(0.004)
Hiring manager is black[1]	**0.051****
	(0.012)
Hiring manager is both female and black[1]	**-0.020**
	(0.013)

Notes: Based on probit regressions predicting whether a new hire is black. Regressions include store and month dummies. Parentheses contain robust standard errors, which are adjusted for clustering on store. N > 100,000.
[1] Table reports change in probability that a new hire is black for a discrete change of dummy variable from 0 to 1.
* significant at 5%; ** significant at 1%

TABLE 10. RELATIONSHIP BETWEEN EMPLOYEE GENDER AND BIASES IN HIRING BLACKS

	Dependent variable is dummy variable = 1 if new hire is:	
	(1) Black Male	**(2) Black Female**
Manager is white[1]	-0.010**	-0.018**
	(0.004)	(0.006)
Manager is Hispanic[1]	-0.013*	-0.019**
	(0.005)	(0.007)
Manager is Asian[1]	-0.018**	-0.015*
	(0.005)	(0.010)
	Predicted Probabilities:	
	Black Male	**Black Female**
Nonblack Manager	0.045	0.091
Black Manager	0.055	0.109

Notes: Based on probit regressions predicting whether a new hire is black male (black female). Regressions include store and month dummies. Parentheses contain robust standard errors, adjusted for clustering on store. N > 100,000. [1] Table reports change in probability that a new hire is black for a discrete change of dummy variable from 0 to 1. * significant at 5%; ** significant at 1%.

TABLE 11. PROBIT ESTIMATES OF THE PROBABILITY THAT AN EMPLOYEE LIVES IN A GIVEN ZIP CODE AS A FUNCTION OF WHETHER HIRING MANAGER ALSO LIVES IN THE ZIP CODE.

	employee lives in zip code 1	employee lives in zip code 2
	(1)	**(2)**
Employee hired by manager who lives in zip code 1	0.0018	
	(0.0018)	
Employee hired by manager who lives in zip code 2		0.0011
		(0.0017)
Constant term	0.0347	0.0361
	(0.0009)	(0.0008)

Notes: Based on sample of the two managers with the largest number of hires in each store, for stores with at least two managers in which the second manager lives in a different zip code from the first manager. Regressions include store fixed effects. N>50,000 new hires; >600 stores.

TABLE 12. LINEAR REGRESSION ESTIMATES OF THE IMPACT OF EMPLOYMENT BIASES ON SALES

Manager is Black	-0.029
	(0.017)
Manager is Hispanic	-0.017
	(0.010)
Manager is Asian	-0.025
	(0.013)
% Employees who are black	0.023
	(0.030)
% Employees who are Hispanic	0.013
	(0.036)
% Employees who are Asian	0.106**
	(0.034)
Mgr. Black * % Employees black	0.047
	(0.050)
Mgr. Black * % Employees Hispanic	0.066
	(0.044)
Mgr. Black * % Employees Asian	-0.054
	(0.096)

Notes: Dependent variable is log of monthly sales. Controls include manager age and sex, manager experience, a dummy for manager of "other" race and % hires who are "other", a dummy indicating if the manager is an assistant manager, share of new hires with no company experience, total monthly employment, a dummy variable for each of the 30 months in the sample, a dummy variable for each store in the sample, and a trend variable for each store in the sample. Omitted manager race and employee race category is white. Parentheses contain robust standard errors, adjusted for clustering on store. N > 20,000 store-months.

FIGURE 1A. TRENDS IN WHITE SHARE OF NEW HIRES

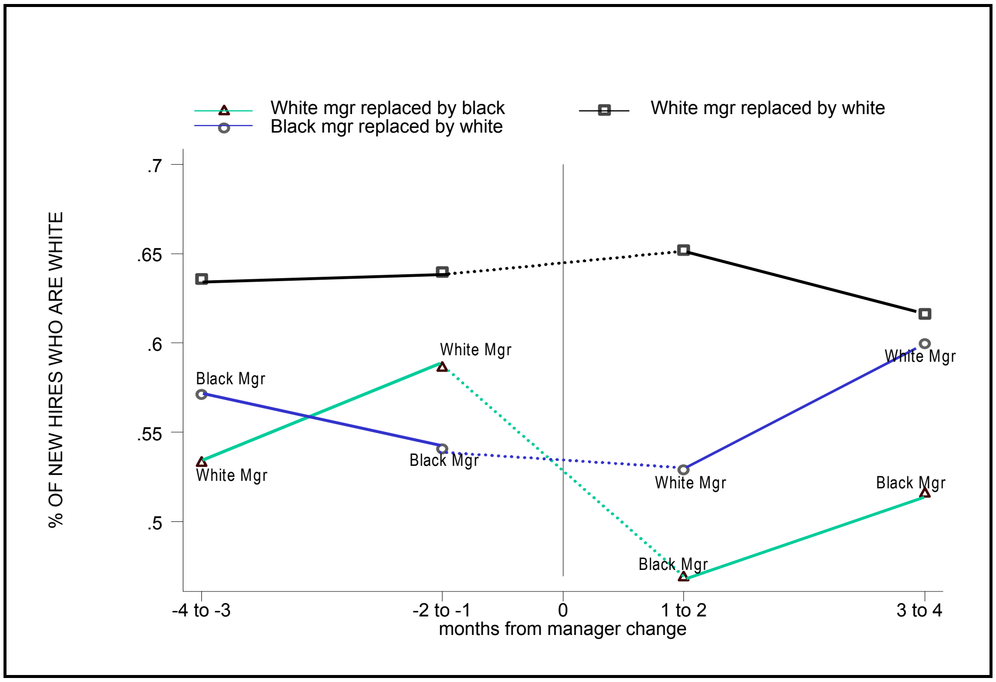
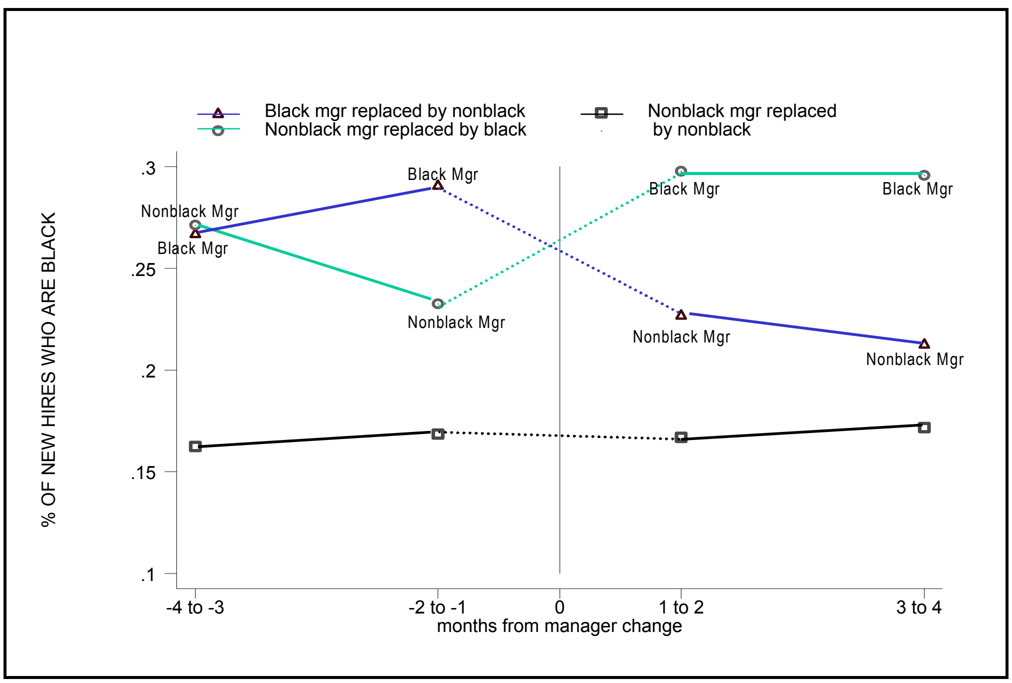


FIGURE 1B. TRENDS IN BLACK SHARE OF NEW HIRES



Notes: Based on sample of all manager turnover events with at least one hire every two months from four months before a manager change to four months after the change. Cases where a black manager is replaced by a black manager are not graphed due to small sample size.