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David Neumark

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ABSTRACT

This paper attempts to test whether information problems in labor markets can explain why minority or female workers are sometimes paid less than equally-qualified white male workers. In particular, the relationship between starting wages, current performance, and race and sex is studied. OLS regressions of starting wages on current performance--which is measured some time after the beginning of employment--indicate that minority workers are paid lower starting wages than white workers with the same eventual performance, among both men and women. This could reflect taste discrimination. However, if employers base starting wages on expected productivity or performance, and average performance is lower for minority workers (as it is in these data), then these estimated differentials could reflect simple statistical discrimination. A test of statistical versus taste discrimination, and a test of statistical discrimination versus pure measurement error, provide some evidence for both men and women that statistical discrimination is partly to blame for these differences in starting wages between minority and white workers, although the evidence is not very strong statistically. Average performance of women is if anything higher than that of men, so simple statistical discrimination cannot explain the lower starting wages that women receive. However, more complex models of statistical discrimination suggest that worse labor market information about a particular group can generate lower wages for that group. A test of the quality of labor market information suggests that employers have better information about male workers, which may explain the lower starting wages paid to women. Together, this evidence suggests that better labor market information might boost starting wages of minorities and women.

David Neumark
Department of Economics
Michigan State University
East Lansing, MI 48824
and NBER
dneumark@msu.edu

I. Introduction

Labor economists agree on the existence of persistent wage differentials by race and sex in the U.S. labor market, but disagree on the source of these differentials. There are two dominant explanations of these wage differentials that are the basis for most empirical research on this topic. The first is the employer discrimination hypothesis, developed in Becker's seminal work (1957), in which minorities or women are paid less because of employers' distastes for hiring from these groups. The second is that women or minorities come to the market with unobserved productivity shortfalls.[1]

An alternative model of discrimination that has not received nearly as much empirical attention is the statistical discrimination model, originally developed by Phelps (1972).[2] In a simple statistical discrimination model in which the distribution of productivity in sub-populations of white men, women, and minorities is the same, the inability of employers to accurately predict or measure an individual worker's productivity does not generate average wage differentials among these subgroups. As Cain (1986) emphasizes, average wage differentials only emerge if there are average productivity differentials, in which case the average wage differentials do not reflect "group discrimination." Thus, without extensions of the simple model, statistical discrimination is not thought to provide a compelling theory of wage discrimination against particular groups in the labor market.

However, there are extensions of statistical discrimination models that generate group discrimination. For example, Aigner and Cain (1977) show that if a minority group has a less

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[1]Examples of research on wage differences by race and sex include: Bergmann (1989); Neumark (1996); Oaxaca (1973); Fix and Struyk (1993); Becker (1985); Neal and Johnson (1996); and Smith and Ward (1989). An extensive compendium of this research is provided in Darity (1995).

[2]See Oettinger (1996) and Altonji and Pierret (1997) for recent applications of this model.

reliable signal, and employers are risk averse, then the minority group will earn a lower average wage despite identical average productivity. Rothschild and Stiglitz (1982) obtain a similar result by positing a production function in which productivity depends on the quality of the match of the worker to the job. Another line of research considers "self-fulfilling prophecies" in statistical discrimination models, in which initially incorrect prior beliefs of lower productivity for a group results in lower human capital investment among that group, hence rationalizing and perpetuating the prior beliefs (Farmer and Terrell, 1996).

Whether or not some form of statistical discrimination leads to group discrimination, in all of these models imperfect information implies that the most productive members of a group with lower actual or assumed average productivity (e.g., a minority group) will be paid less than equally-able members of the non-minority group. Even though the minority group as a whole may not be treated unfairly, there is an obvious sense in which this highly-productive individual suffers from discrimination--in that he or she is paid less by virtue of identification with that group--although the least-able members of this group (and any group) likely benefits from statistical discrimination. Of course, if there is group discrimination then the implications are even more apparent.

This paper assesses evidence on imperfections in employer information about new workers in the labor market, and their role in generating wage differentials by race and sex. The methods used borrow heavily from research by Foster and Rosenzweig (1993, hereafter FR) to study statistical versus taste discrimination in developing countries, applying their methods to data from the U.S. that might be informative with respect to some of the same questions they consider. In addition, the paper presents some innovations relative to their methods. The analysis proceeds in three steps. First, using FR's methods directly, evidence is presented on whether wage differences between apparently equally-productive white and minority workers are better characterized as reflecting taste discrimination or statistical discrimination.[3] As noted above, even in the simple statistical discrimination model, minority workers who are as productive as white workers will receive lower wages than comparable white workers if average productivity of minority workers is lower--where "comparability" is determined based on actual productivity, rather than expected productivity which determines starting wages in the statistical discrimination model. Thus, evidence that looks like taste discrimination may stem solely from statistical discrimination. Second, extending FR's methods, the empirical analysis attempts to distinguish between imperfect information on the part of employers and measurement error in the productivity proxies available to econometricians, which have identical empirical implications for the test of taste discrimination versus statistical discrimination, but quite different implications for modeling wage setting and for policy. Finally, evidence is presented on whether employers have better information about some groups of workers than others. As suggested by the Rothschild-Stiglitz model, worse information about a particular group could lead not only to the usual statistical discrimination result, but also to group discrimination, so that average wage differentials exceed average productivity differentials.

The policy implications of this analysis are potentially quite important. If taste discrimination accounts for the unexplained lower wages of women and minorities, then anti-discrimination legislation may be the only appropriate response. On the other hand, if statistical discrimination is important, especially in conjunction with some other factor that leads to group discrimination, then better means of assessing workers' productivity--including apprenticeships, skill certification, job testing, etc.--may contribute to the reduction of discrimination at the individual or group level.

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[3]This paper does not attempt to distinguish between taste discrimination and unobserved productivity differentials that generate evidence consistent with taste discrimination.

II. Empirical Methods

Statistical Versus Taste Discrimination

The empirical approach to testing for employer taste discrimination versus statistical discrimination that is the starting point for this paper was originally developed by FR. The discussion here is geared more closely towards the data used in the present paper, as discussed fully in Section III. Suppose that data are available on starting wages (w_s), race (R, a dummy variable defined as one for minorities and zero otherwise), and (marginal) productivity (P).[4] It is assumed that P is constant over the time horizon considered by the employer (ruling out human capital investment), and that there are no incentive considerations (as in Lazear, 1979) that lead wages to diverge from productivity. According to the simple statistical discrimination model, w_s is set equal to the expected value of P when the worker begins the job, denoted P_s^*, and defined as

(1) $$P_s^* = E(P|I_s) ,$$

where I_s is all information about the worker available to the employer when the starting wage is set.[5] Under the null hypothesis of no race discrimination, in the regression

(2) $$w_s = \alpha P_s^* + R\beta + \epsilon ,$$

we should find that $\beta = 0$. However, we do not have data on expected productivity, but only on actual productivity P, where because of equation (1),

(3) $$P = P_s^* + \eta_s, \eta_s \perp P_s^* .$$[6]

Thus, the estimated equation is

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[4]In the empirical implementation, a set of dummy variables for race and ethnicity is used.

[5]P does not have an 's' subscript, indicating that it is measured some time after the starting wage is set.

[6]Note that this is different from what is typically assumed in statistical discrimination models, in which the observed signal available to the employer is equal to true productivity plus orthogonal noise (Cain, 1986). Rather, starting wages are set based on P_s^*, but P (which is true productivity) is a noisy signal of P_s^*.

(4) $w_s = \alpha P + R\beta + \epsilon - \alpha\eta_s$.

Clearly, the OLS estimate of α (a_{OLS}) will be biased downward. In particular, the attenuation bias in the OLS estimate of α is given by

(5) $\text{plim}(a_{OLS}) = \alpha \{Var(P_s^*)/Var(P)\}$,

where $Var(P_s^*)/Var(P)$ is the reliability of the information available on new hires.

Now suppose that P and R are negatively correlated, which will occur if minorities are on average less productive. In this case, the OLS estimate of β (b_{OLS}) will also be biased downward. As a result, OLS estimates of equation (4) may lead to evidence that taste discrimination generates race differences in wages, because controlling for productivity, minorities are paid lower starting wages. But the downward bias in the estimate of β implies that β could nonetheless equal zero, with starting wages conditional on *expected* productivity not reflecting any race differential.

What is required to correct the estimates of equation (4) for the bias from using P instead of P_s^* is a variable that is correlated with productivity but uncorrelated with η_s, and that does not appear in equation (2). Because η_s is orthogonal to the information set I_s, any variable that is in I_s and correlated with productivity satisfies the first two criteria. However, to satisfy the third criteria, this variable has to be unrelated to starting wages conditional on expected productivity. Given that the null hypothesis is that there is taste discrimination, only variables that measure characteristics not subject to taste discrimination are valid instruments. The instruments considered include education, age, and training or experience. Age is potentially objectionable, given that there may well be age discrimination in the workplace (e.g., Johnson and Neumark, forthcoming). However, this is unlikely to be an issue in the present context, both because the sample consists of relatively young workers, and because most age discrimination claims concern discharges, layoffs, and hiring, rather than wage discrimination.[7]

Under statistical discrimination, a minority worker will earn less than an equally-productive white worker, as long as average productivity for minority workers is lower. But a minority and white worker with identical *expected* productivity will earn the same wage. Taste discrimination, on the other hand, implies that even with equal expected productivity, the minority worker will earn less. Therefore, at the one extreme of pure taste discrimination, the IV and OLS estimates of β will be identical, while at the other extreme of pure statistical discrimination, the IV estimate of β (b_{IV}) will fall in absolute value to zero. Thus, a statistical test of whether race differences in wages reflect taste or statistical discrimination is obtained from a Hausman test for bias in the OLS estimate of the coefficient of R, which is a test of the null hypothesis of pure taste discrimination.[8] A statistical test of the broader question of whether employers have accurate information about workers on which they base starting wages is obtained from a Hausman test for the "exogeneity" of P in equation (4), which is a test of the null hypothesis of complete information (under which the IV estimate of α (a_{IV}) equals a_{OLS}).

An additional important component of the empirical analysis, which is described following the discussion of the results testing for taste versus statistical discrimination, concerns whether the differences between a_{OLS} and a_{IV} indicated by the data stem from imperfect information, or simply from measurement error in the performance rating available in the data set as a measure of true,

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[7]For example, in 1994 out of 19,571 total charges under the Age Discrimination in Employment Act filed with the EEOC, in 5.7 percent of cases wages were in issue, versus 9.1 percent for race discrimination charges and 10.2 percent for sex discrimination charges (United States Equal Employment Opportunity Commission, 1994).

[8]One could imagine trying to distinguish between taste discrimination and statistical discrimination based on the statistical significance of the IV estimates of β; this is equivalent to treating the null as statistical discrimination, and testing for evidence against it. But a small reduction in the absolute value of the estimate of β from instrumenting could be enough to make the estimate insignificant, while representing little change from the OLS estimate of β. In addition, given the focus in the literature on taste discrimination, it seems appropriate to treat this hypothesis as the null in research testing for alternative forms of discrimination.

known productivity. This is potentially important because the latter type of measurement error could generate evidence of statistical discrimination, despite employers (but not econometricians) having perfect information about workers.[9]

Is Labor Market Information Better for Some Demographic Groups?

An additional issue relating to labor market information and race and sex differences in wages is whether employers have better information about white workers than about minority or female workers, possibly because of word-of-mouth references, better communication (Lang, 1986), or more difficulty in discerning true signals.[10] The tests described above may not indicate that imperfect information gives rise to statistical discrimination, which in turn leads to evidence looking like taste discrimination, if expected productivity is the same across groups. Nonetheless, information problems that are more severe for minority or female workers could help to explain their lower wages. For example, in the Rothschild-Stiglitz model of statistical discrimination, less accurate information about minorities can lead to group discrimination--i.e., an average wage gap that is larger than the average productivity gap (which could equal zero). Thus, this model explains why groups of workers with identical *expected* productivity could earn different wages, and therefore provides yet another reason why labor market information may generate wage differences between similar workers. This is perhaps most pertinent for sex differences in wages, because women do not, on average, receive lower performance ratings than men. While simple statistical discrimination therefore cannot explain women's lower wages, such extensions of the statistical discrimination model, coupled with worse information about women, can generate group

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[9]FR do not consider this issue.

[10]For example, Cain (1986) notes that women may have difficulty signalling their long-term commitment to the labor market.

discrimination against women.

To this point, a single reliability ratio for information regarding new hires is estimated. The question of differential information, however, hinges on whether the reliability ratio differs across groups. This question can be addressed by estimating the regression

(6) $w_s = \alpha P + \epsilon$

for each subgroup, obtaining both OLS and IV estimates.

The reliability of information on new workers can be estimated as the ratio of the OLS estimate of α (a_{OLS}) to the IV estimate (a_{IV}), since

(7) $\text{plim}(a_{OLS}/a_{IV}) = \text{Var}(P^*)/\text{Var}(P)$.[11]

To carry out a statistical test for differences in this estimated ratio across demographic groups, an estimate of the variance of this ratio is required. A first-order Taylor-series expansion yields an approximation for the variance

(8) $\text{Var}(a_{OLS}/a_{IV}) \approx (1/\alpha_{IV}^2)\text{Var}(a_{OLS}) + (\alpha_{OLS}^2/\alpha_{IV}^4)\text{Var}(a_{IV}) - 2\cdot(\alpha_{OLS}/\alpha_{IV}^3)\text{Cov}(a_{IV},a_{OLS})$.

The covariance term in equation (8) is straightforward to estimate, since both a_{IV} and a_{OLS} are linear estimators. In particular,

(9) $\text{Cov}(a_{IV},a_{OLS}) = \text{Cov}(e_{OLS},e_{IV})(P'P)^{-1} = \text{Var}(e_{OLS})(P'P)^{-1}$,

where e_{OLS} and e_{IV} are the OLS and IV residuals. In fact, equation (6) ends up being estimated for more than one demographic group, in which case P in equation (9) is simply the matrix including the performance rating as well as the dummy variables for demographic subgroups. The estimates from each demographic group (or set of groups) can be treated as coming from independent samples, making it easy to test for differences in the estimated ratios a_{OLS}/a_{IV} across groups.

[11] FR also estimate this reliability ratio (although differently, given their data), but do not compare it across groups to compare reliability of labor market information.

There is no way to determine how much differences in labor market information would shift average wages for a group. Nonetheless, evidence of differences would suggest that better information about particular groups of workers could raise their average wages.

III. The Data

These questions are studied using an employer data set stemming from the Multi-City Study of Urban Inequality (MCSUI).[12] This data set contains information on starting and current wages, worker characteristics, and employers' ratings of employees. The information available in the MCSUI conforms quite closely to the data required to implement the tests described in the previous section, with some exceptions discussed below.

The data are from a survey that was administered to about 800 establishments in each of four metropolitan areas: Atlanta, Boston, Detroit, and Los Angeles. The survey was administered between June of 1992 and May of 1994. It was administered over the phone, and averaged roughly 35 minutes in length. The sample of establishments was drawn from two sources: 1) a listing of establishments and their phone numbers provided by Survey Sampling, Inc. (SSI), which is drawn primarily from local phone directories and supplemented by other sources; and 2) the establishments of employment of respondents in household surveys that were also administered in each of these four metropolitan areas. For the establishments in the SSI part of the sample, the main respondent to the survey is the person who is responsible for hiring non-college workers. The interviews for this part of the sample focused only on hiring for jobs not requiring a college degree. For the sample drawn from the household survey, the respondent is the person responsible for hiring into

[12]These data were generously supplied by Harry Holzer. The employer data set from the MCSUI has many parallels to the earlier Employment Opportunity Pilot Project (EOPP). Although the MCSUI includes other data sets, in this paper the employer data set is referred to as the MCSUI data.

the occupation of the household respondent.[13] The sample was constructed to permit pooling data from these two sources, as both were designed to generate employee-weighted samples of establishments, when sample weights are used. The overall response rate for the survey was 67 percent for establishments that were successfully screened.[14] This response rate compares favorably with other phone surveys of employers (e.g., Kling, 1995). Additional discussion of the data set is provided in Holzer (1995, 1996a, and 1996b).

Respondents are asked about the last worker hired, whether or not that worker is still with the employer. The recorded characteristics of the last worker hired include race/ethnicity, sex, age, educational attainment, starting and current wages, and job requirements. In addition, a supervisor's performance rating of the worker is also provided, measured on a scale of one to 100.[15] These ratings are used to measure productivity (P).

There are both conceptual and measurement issues that arise with respect to the performance ratings. First, the performance ratings do not provide an explicit productivity measure. In contrast, FR used time-rate pay as their measure of the wage, and piece-rate pay for the same worker to measure productivity. Nonetheless, it seems reasonable to assume that rated performance is monotonically positively related to productivity. We therefore use alternative positive monotonic transformations of the performance rating, specifically linear and log forms.

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[13]Other than education, most characteristics of workers and jobs do not differ significantly across the samples of establishments generated by the two data sources.

[14]Successfully-screened establishments were those where the correct establishment and the person responsible for new hiring into the relevant types of positions were contacted.

[15]A similar variable is used in the EOPP Survey (e.g., Barron, et al., 1989) and a more recent, similar survey of members of the National Federation of Independent Businesses (Bishop, 1993). Since the main survey respondent was the person responsible for hiring, in small- and medium-sized companies the performance rating was typically elicited from this respondent, who was likely to be a manager or owner, and who should therefore be able to speak knowledgeably about a worker's job performance. In large companies, these functions are more likely to be separated. As a result, in these cases the interviewer generally elicited the performance rating from a supervisor.

Second, if the performance ratings were the product of a formal evaluation procedure used to set wages and determine promotions, the ratings might be influenced by discrimination in the same way as are data on wages (as employers might feel constrained to manipulate performance ratings to back up their wage decisions). In this case, performance ratings might "explain" wage differentials by race or sex, but not because they reflect true differences in productivity. However, these ratings are informal and not explicitly related to actual pay and promotion decisions. In addition, survey respondents were promised full confidentiality. Therefore, the ratings seem likely to provide an unbiased measure of a worker's true job performance.[16]

Finally, the performance ratings pose some pure measurement problems, because they may vary for reasons other than the worker's actual performance. In particular, the ratings that particular respondents provide may vary for random reasons, with some tending to give higher and some lower ratings for equally-productive workers.[17,18] This case may be interpreted as one of pure measurement error in the performance rating. Unfortunately, in this case the instrumental variables procedure may be correcting for this pure measurement error, rather than that which arises in the imperfect information story because of discrepancies between current and expected productivity. Because the IV procedure may simply correct for standard measurement error bias, it could lead to spurious evidence in favor of the statistical discrimination model, with a_{IV} larger in absolute value

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[16]The piece-rate data used by FR as a proxy for productivity are not immune to the influence of discrimination. For example, tasks, equipment, or work sites may be allocated in such as way as to affect the output of specific groups (such as men and women).

[17]The scale should be the same, however, as respondents are instructed to regard a rating of 50 as average, although employers' perceptions of "average" are likely to vary.

[18]In other work using these data (Holzer and Neumark, 1996), the performance rating standardized by the supervisor's rating of the typical new hire into the job was used. However, the analysis in that paper concerned within-job differences in performance. In the present paper, in contrast, the performance rating should distinguish between a highly-productive worker in a demanding job and a less-productive worker in an undemanding job, when both have fairly typical performance for workers in those jobs, by assigning a higher rating to the former.

than a_{OLS}, and b_{IV} falling in absolute value relative to b_{OLS}.[19] Nonetheless, if there is taste discrimination, b_{IV} should still be significantly different from zero, as opposed to the case of pure statistical discrimination. While this implies that a test of whether b_{IV} differs from zero provides a test for taste discrimination, a significant difference between b_{IV} and b_{OLS} would not necessarily imply that there is statistical discrimination. As a result, considerable effort is devoted to distinguishing between the statistical discrimination and pure measurement error interpretations of the findings, an issue FR did not consider.

Aside from these potential problems with the productivity proxy, the MCSUI data offer some advantages for this study. First, the wage measure is a starting wage, which, according to the statistical discrimination model, is the wage that should be set equal to expected productivity. On the other hand, the performance rating is current, and workers in this data set have average tenure of about two to three months, so that we would expect actual productivity to differ from expected productivity. Thus, the time frame to which the data refer are precisely what is required for the tests this paper considers.

The data set includes three types of variables that can be potentially be used as predictors of productivity that do not themselves (i.e., independently of productivity) affect wages: age, education, and job requirements.[20] The information on job requirements comes from survey questions asking the employer whether specific experience, general experience, and vocational education or formal training are "absolutely required, strongly preferred, mildly preferred, or does not matter." It seems reasonable to suppose that if these are absolutely required of a hire, then that

[19]Moreover, as long as there is some pure measurement error of this variety, the results are biased in the direction of rejecting the null of pure taste discrimination.

[20]The MCSUI offers little else in the way of potential instruments for the performance rating, since most of the questions included in the survey relate to firm characteristics or hiring and recruiting procedures, rather than workers.

hire must possess these qualifications, and the data are used in this manner. In fact, this supposition could be checked using another question on whether a high school diploma was required and corresponding information on the reported actual education of the worker hired; only 1.4 percent of those hires for which a high school diploma was absolutely required (27 percent of the hires in the sample) did not actually have a high school diploma.[21,22]

Finally, because of measurement problems attention is restricted to the bulk of the sample (about 70 percent) paid hourly wages.[23] The most important problem is that the only hours information comes from a question regarding how many hours per week are usually worked, with no distinction between the time periods referring to the starting wage and the current wage. Consequently, there is no way to accurately construct an hourly starting wage and hourly current wage for those paid on a non-hourly basis. This is likely to be further complicated by differences between hourly and non-hourly workers in the value of non-wage compensation.[24]

III. Results

Statistical Versus Taste Discrimination

Table 1 reports descriptive statistics on log starting wages, performance ratings, and log performance ratings.[25] The wage differences between race and ethnic groups are similar for men

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[21] In contrast, the percentage was 4.8 when a high school diploma was strongly preferred, 14.1 when it was mildly preferred, and 24.9 when it did not matter.

[22] However, some measurement error may be introduced because some individuals in jobs in which these qualifications are not absolutely required may still possess them; of course, in jobs in which these qualifications are unimportant, workers who possess them may not be more productive.

[23] About seven percent are paid a weekly or monthly rate, and 23 percent an annual salary.

[24] In the data, a considerably higher fraction of non-hourly workers receive health, dental, and pension benefits (the differences in the proportions of non-hourly and hourly workers receiving these benefits are .23, .22, and .26, respectively).

[25] Because of the sampling scheme described in the previous section, all estimates are weighted.

and women, with whites earning about 19 percent more than blacks, and four to eight percent more than Hispanics.[26] The difference in starting wages between men and women is about 10 to 14 percent, toward the lower figure for Hispanics. These sex-related differentials are somewhat small compared with representative samples of the U.S. work force, but the data here refer to starting wages of relatively young workers (29.5 years old, on average); existing work with other data sets documents the lower sex differences in wages for workers early in their careers (e.g., Light and Ureta, 1995).

The performance ratings reveal that women in each race or ethnic group receive higher scores than men, on average, using either levels or logs. On the other hand, within sexes, whites generally receive higher ratings than blacks or Hispanics. The possibility that statistical discrimination generates evidence that looks like taste discrimination--which is the motivation for the test of statistical versus taste discrimination--requires lower average productivity of the lower-paid group. Since this does not apply to male-female differentials, the test for statistical versus taste discrimination is carried out only for race/ethnic differences for each sex considered separately. However, the second test regarding the quality of information about each demographic group is still pertinent; women could have higher average productivity, but if labor market information about them is worse, and mismatches costly, they could receive lower wages.

Tables 2 and 3 report the results for the test of statistical versus taste discrimination, for men and women respectively. The first column in Panel A of each table reports OLS estimates of a standard log wage regression (for starting wages) without any information on performance ratings, with controls for education, age, job requirements, and race/ethnicity. In both the male and female

[26]The MCSUI does not include separate race and ethnicity variables. Rather, the survey elicits the "racial or ethnic background" of the new employee, which is then coded as either white, black, Hispanic, Asian, or other. (Individuals in the latter two categories are dropped from the analysis.)

samples, wages of blacks are significantly lower by about 14 percent, while wages of Hispanics are not significantly lower (with the point estimates indicating wage gaps of one to four percent).[27] These results are not fully consistent with other estimates of race and ethnic wage differentials, where it is more common to find a smaller race difference among women than among men (Blau and Beller, 1992), and Hispanic-white differences are often larger than black-white differences (Reimers, 1983). However, this sample is somewhat unique in covering four specific metropolitan areas, and the wage measure studied here is the starting wage. The starting wage differentials associated with schooling appear relatively similar to those observed in other data sets for contemporaneous wages, although the considerably higher wage premium for male college graduates compared with female college graduates is unusual.[28] The relationship between age and the starting wages also parallels the usual relationship. Among the job requirements, both specific experience and training are associated with significantly higher wages, while general experience is not.

Column (2) of Tables 2 and 3 reports OLS estimates of regressions of log wages on performance ratings (in Panel A) and log performance ratings (in Panel B). For men, the estimated coefficients of the performance ratings variables are positive and statistically significant. Using the standard deviations from Table 1, a one standard deviation increase in performance ratings (a weighted average across the demographic groups of 13.90 for the linear variable, and .22 for the log variable) is associated with a six percent increase in the wage, which is about one-sixth of the standard deviation of log wages. Thus, the estimated coefficients on the productivity variables

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[27]Unless otherwise specified, statements about statistical significance refer to the five-percent level.

[28]The educational classification available in the MCSUI is actually somewhat more detailed. But the results for these specifications as well as the IV estimations reported below were qualitatively similar using the richer classification, so the simpler results are reported here.

appear quite small. For women, the estimated coefficients in column (2) are even smaller, and not statistically significant.

Before proceeding to the IV estimation, a decision had to be made regarding which instrumental variables to use, choosing among the age, education, and job requirements variables. The maintained assumption is that at least one set of these variables can be excluded from the starting wage equation. The question that can be addressed empirically, however, is which set of instruments provides the most predictive power for the performance rating in the first-stage regression. To assess this, the first-stage regression was estimated using each set of instruments separately. For men, only the age variables were jointly significant in the first-stage regression; as reported in column (3), for the levels specification the p-value for the test of joint significance was .03 for the age variables, .58 for the education variables, and .47 for the job requirement variables, with qualitatively similar results for the log specification. Thus, for men the first set of instruments considered is the age variables. For women, only the education coefficients were jointly significant, with p-values of .00 in both the levels and log specifications. However, the p-values for the age variables are also relatively low (.17 and .21 in the two specifications); consequently, specifications using education and age as instruments are also reported for women.[29]

Turning first to the results for men, columns (3) and (4) therefore report OLS estimates and IV estimates of the wage equation, using age and its square as instruments, in this case including the other variables (education and job requirements) in the wage equation. The OLS estimates of α, the coefficient of the productivity proxy, are similar to those in column (2). The IV estimate rises to .009 in the levels specification, but with the increased standard error becomes insignificant; in the

[29]These results are similar if only one set of possible instruments at a time is included in the first-stage regression; the only difference is that the p-values for the age variables for women were lower, although again not as low as those for the education variables.

log specification the IV estimate of α actually falls, also becoming insignificant. However, although the education and job requirements variables enter significantly in both the OLS and IV estimations, this model may be misspecified, and these variables may simply capture productivity differentials that would otherwise be captured in the performance rating.

Thus, columns (5) and (6) omit the education and job requirement controls, retaining only the productivity proxy and the race/ethnicity variables that may, because of taste discrimination, affect wages independently of productivity. This seems the most appropriate specification of the starting wage equation with which to test for statistical versus taste discrimination. In the OLS regression, the R^2 is considerably lower than in column (1) or (3). Of course, this may be partly attributable to the discrepancy between the performance rating P and expected productivity P_s^*. Note that the F-statistic for the joint significance of the instruments in the first-stage regression is reasonably high (3.81 in the levels specification, and 2.92 in the log specification), indicating that small sample biases towards the OLS estimates are unlikely to be severe. The IV estimates of α are considerably higher than the OLS estimates, rising by a factor of eight or nine, and in both the linear and log specifications these estimates are statistically significant. As reported in the table, for both the levels and log specification the null hypothesis of no bias in the estimated coefficient of the actual performance rating is rejected (at the five-percent level in the levels specification, and the ten-percent level in logs). For the linear specification, a one-standard deviation in the performance rating is associated with an increase of .49 in the log wage, a bit higher than one standard deviation of the log wage. For the log specification, a one-standard deviation in the performance rating is associated with a .44 increase in the log wage, approximately the same result. Thus, the IV estimates appear to generate estimated coefficients of the productivity proxy that map into wage differentials relatively well.

Turning to the question of substantive interest, the estimated wage shortfalls for blacks and Hispanics in the OLS estimates of the levels specification are -.178 and -.066 respectively, with the latter only significant at the ten-percent level. However, instrumenting for the performance rating causes the differential for blacks to fall to -.062, and that for Hispanics to change sign; both estimated differentials become statistically insignificant. Qualitatively, these reductions in the wage shortfalls for black and Hispanic men are consistent with a substantial part of these shortfalls being attributable to statistical discrimination rather than taste discrimination. The results are similar for the log specification. Hausman tests to gauge the statistical significance of the differences are reported in the last two rows of each panel. In the levels specification, the p-values for the test of the null of pure taste discrimination--implying no bias in the OLS estimates of the race/ethnicity wage differentials--are .11 for blacks and .07 for Hispanics. In the log specification, the corresponding p-values are .14 and .13. Thus, there is some evidence against the lower wages paid to blacks and Hispanics reflecting solely taste discrimination, although it is not overwhelming.[30]

Finally, column (7) repeats the IV estimation, but now using education and job requirements as instruments as well. The estimates are more precise as we would expect, leading to lower p-values from the tests of the null of taste discrimination (in the .06-.09 range). However, the overidentifying restrictions are rejected, and the full set of instruments has relatively weak predictive power for the performance rating, as reflected in the F-statistics of 1.50 or 1.51. Thus, this column probably does not provide very reliable evidence.

Having discussed the various specifications and analyses for men in detail, the results for

[30]As noted earlier, one could test the null hypothesis of statistical discrimination based on whether the IV estimates of the coefficients of the race/ethnicity dummy variables are significantly different from zero. The estimates in column (6) indicate a failure to reject this null, and the same result occurs for women, discussed below. However, as mentioned earlier, it is probably preferable to treat taste discrimination as the null hypothesis.

women can be discussed more succinctly. In the levels specification in column (3) of Table 3, the OLS estimate of α is actually negative (-.0001) but insignificant. The estimates of the race/ethnicity differentials are similar to those excluding the performance rating. Instrumenting for the performance rating with the education variables, in column (4), causes the estimate of α to rise to .027 and become statistically significant. A similar result holds for the log specification. Columns (5) and (6) instead use the age variables along with education as instruments, with similar results. The overidentifying restrictions are not rejected for this specification, with p-values of .75 in the levels specification, and .52 in logs. Thus, these estimates are preferable. Note also that the F-statistics for the instruments in the first-stage regression are high for all of the specifications discussed so far. As for men, the model in columns (5) and (6) may be misspecified by including the job requirements variables, so columns (7) and (8) report specifications excluding these variables. The results are little changed. Finally, in column (9) the job requirements variables are also used as instruments. The overidentifying restrictions are not rejected, although the p-values are relatively low (.20 for the levels specification, and .12 for the log specification). Thus, the specifications in columns (7) and (8) are the preferred ones, and the remaining results are discussed in reference to them; nonetheless, the qualitative conclusions are the same for the other specifications.

The IV estimate of α is considerably higher than the OLS estimate in both the linear and log specifications, and the IV estimates are statistically significant. For both the levels and log specification the null hypothesis of no bias in the estimated coefficient of the performance rating is rejected, with p-values of .00. For the linear specification, a one-standard deviation in the performance rating is associated with an increase of .49 in the log wage. For the log specification, a one-standard deviation in the performance rating is associated with a .55 increase in the log wage.

Again the IV estimates appear to generate estimated coefficients of the productivity proxy that map into wage differentials relatively well.

Turning to the question of most interest, the estimated wage shortfalls for blacks and Hispanics in the OLS estimates of the levels specification are -.185 and -.041 respectively, with the latter insignificant. Instrumenting for the performance rating causes the differential for blacks to fall by more than half, to -.081, and that for Hispanics to fall to zero. Similar results occur for the log specifications, although the changes are a bit smaller. Like for men, these reductions in the wage shortfalls for black and Hispanic women are consistent with a substantial part of these shortfalls being attributable to statistical discrimination rather than taste discrimination. The Hausman tests indicate that the change for Hispanic women is not statistically significant, while for black women the p-values for the test of the null of pure taste discrimination are .11 in the levels specification, and .14 in the log specification.

Overall, then, for black and Hispanic men and for black women there is some evidence that imperfect information is partly responsible for the lower starting wages they receive, compared with white workers with identical performance ratings. The point estimates of the shortfalls in starting wages experienced by black and Hispanic workers fall substantially once account is taken of statistical discrimination via an instrumental variables procedure, generally by more than half for blacks, and disappearing altogether for Hispanic men. The null hypothesis that these starting wage differentials are solely attributable to taste discrimination is rejected--for the specifications that fit the data--at the .07-.14 significance level, evidence that is not overly strong, but which nonetheless suggests that the evidence against the null of pure taste discrimination should not be dismissed.[31]

[31]This range of p-values refers to the tests for bias in the OLS estimates of the race/ethnicity differentials in the preferred specifications in Table 2 (columns (5) and (6)) and Table 3 (columns (7) and (8)).

Statistical Discrimination Versus Pure Measurement Error in the Performance Rating

The evidence to this point is consistent with statistical discrimination being partly responsible for the lower starting wages of minority workers relative to equally-productive white workers. However, as discussed earlier, some caution is in order because the same contrast between the OLS and IV estimates could arise if there is no information problem--i.e., employers know a worker's productivity when the worker is first hired--but the performance rating measures this known productivity with error. In this scenario, the results would have no implications with respect to labor market information, but would nonetheless tend to weaken evidence from studies claiming discrimination based on results in which race or sex differences in wages persist when error-ridden proxies for productivity are included in wage regressions.

There is some information that can be used to assess these alternative interpretations of the data. Specifically, the MCSUI data includes information on whether there was a probation period for the most recently-hired employee. Presumably, a probationary period is used when it is difficult to assess the worker's productivity prior to hiring. Thus, using the same reasoning as discussed in Section II in relation to the test for different quality of labor market information about different demographic groups, the ratio a_{OLS}/a_{IV} can be compared for the subsample subject to a probationary period and the subsample not subject to one. If in fact labor market information plays a role in driving down the OLS estimate of α relative to the IV estimate, this ratio should be lower for those who are subject to a probationary period.[32]

Results for the non-probationary and probationary subsamples are reported in Table 4.

[32]This test is based on the assumption that the bias from pure measurement error would be no different for the probationary and non-probationary samples; this assumption seems reasonable given that the measurement error would be attributable solely to errors in the reported performance rating relative to productivity that is known to the employer.

Columns (1) and (2) present results for men and women combined, using age and education as instruments, and columns (3) and (4) present results for men only, using age as the instrument.[33] The proportion subject to probation is testing is .79, with the proportions very close among men and women.[34] The results in Table 4 suggest that labor market information drives the differences between the OLS and IV estimates of α. For both samples, the ratio a_{OLS}/a_{IV} is considerably higher for the non-probationary sample for which initial labor market information should be better, although this ratio is estimated imprecisely for the smaller non-probationary subsamples.

Thus, based on the distinctions between probationary and non-probationary workers, the OLS and IV estimates of α correspond to what would be expected if there is imperfect information in labor markets. Although the evidence is not statistically strong, it suggests that the changes in estimates of α that result from instrumenting reflect errors of measurement on the part of employers in the reported performance rating, relative to initial expected productivity, rather than perfect information on the part of employers, with the measurement error reflecting only errors in the reported (to the researcher) performance rating relative to true, known productivity.

Another way this result should manifest itself is in the changes in the estimated race gaps in wages for the alternative subsamples. In particular, when statistical discrimination is likely to be more important (i.e., for the probationary workers) the estimates should indicate that a larger fraction of estimated race gaps in wages are attributable to statistical discrimination. Table 5 reports

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[33]In unreported estimates for men and women combined using only the age instruments, the IV estimates of the coefficients of the productivity scores in the non-probationary sample were small and insignificant, and the F-statistics for the instruments in the first-stage regression were much lower than when age and education were used as instruments. In unreported estimates for women only using age and education as instruments, the OLS estimates of the coefficients of the productivity scores in the non-probationary sample were negative and insignificant.

[34]This is relevant because, below, results are presented indicating that the ratio a_{OLS}/a_{IV} is smaller for women than for men. The similar demographic compositions of the non-probationary and probationary groups implies that this sex difference does not drive the testing results reported in Table 4.

these results, providing the estimated race coefficients corresponding to the specifications in Table 4 for the male-only sample.[35] The results for both blacks and Hispanics are consistent with expectations. Simply using the point estimates, the estimated proportion of the wage gap attributable to statistical discrimination is higher for the probationary workers. Indeed, for the probationary workers the IV estimates of the race/ethnicity gaps in wages are non-negative, whereas for the non-probationary workers the IV estimates of these gaps are about three-fourths as large as the OLS estimates. In results for women not reported in the table, the same conclusion emerged. For black-white differences (there are essentially no Hispanic-white differences for women, as shown in Table 3), the estimated proportion of the wage gap due to statistical discrimination was .06-.13 for the non-probationary sample, compared with .43-.51 for the probationary sample. Thus, in general, the findings for tests of statistical versus taste discrimination are consistent with the IV results being driven by imperfect information on the part of employers.

Is Labor Market Information Better for Some Demographic Groups?

The evidence from the preceding sections suggests that labor market information problems may partially account for the lower wages paid to minority workers, among both women and men. This subsection turns to the question of whether employer information is better about some demographic groups than others. If mismatches lower productivity, then worse information about women or minorities can lower that group's average wage, providing another channel for labor market information to lead to lower wages for such groups. In addition, even if the type of test from the preceding section does not point to information problems as a source of unexplained wage differences between equally productive workers in different demographic groups, the test in this

[35]As explained earlier, the samples pooling men and women are not suitable for testing statistical versus taste discrimination.

subsection can. This is potentially most pertinent to sex differences in wages, which cannot be explained as stemming from simple statistical discrimination, given that women's performance ratings are on average at least as high as men's.

The analysis initially proceeded by estimating the wage equation separately for each demographic group. However, for some of the smaller groups the estimates of α (particularly the OLS estimates, but one IV estimate as well) were negative. Consequently, the estimates are computed for two comparison groups, men versus women and whites versus non-whites. Since the latter results in pooling men and women, for whom different instruments appeared to perform well, estimates for this comparison are reported using first just the age variables, and then the age and education variables as instruments. The results are reported in Table 6.

Columns (1) and (2) look at sex differences, grouping workers of each race and ethnicity, of the same sex, together, and including race and ethnicity dummy variables in the regression. For both the linear and log specifications, the ratio a_{OLS}/a_{IV} is considerably higher for men (.12-.13) than for women (.02-.03). The lower estimate of α_{OLS} for women implies that women's starting wages are much more weakly related to their performance rating--which is measured after they have accumulated some time with the employer--than are men's. On the other hand, the estimates of α_{IV} are if anything higher for women, implying that their starting wages are at least as strongly related to *expected* productivity as are men's. Together, this evidence suggests that employers have considerably worse information about new female employees than about new male employees. However, the standard errors of the estimated ratios of α_{OLS}/α_{IV} are relatively large, so the t-statistics for testing the null hypothesis of equality of these ratios for men and women are in the 1.4-1.5 range, implying that the evidence of a lower ratio for women is not statistically strong.

Turning to the results for whites versus non-whites, there is no evidence that employers have better information on white workers. In particular, in all of the specifications the estimate of the ratio α_{OLS}/α_{IV} is a bit lower for white than for minority workers, indicating if anything slightly worse information about white workers, although the differences are nowhere near significant.[36]

IV. Conclusions

This paper attempts to test whether information problems in labor markets help to explain why minority or female workers are sometimes paid less than equally-qualified white male workers. In particular, the relationship between starting wages, current performance, and race and sex is studied. OLS regressions of starting wages on current performance--which is measured some time after the beginning of employment--indicate that minority workers are paid lower starting wages than white workers with the same eventual performance, among both men and women. This could reflect taste discrimination. However, if employers base starting wages on expected productivity or performance, and average performance is lower for minority workers (as it is in these data), then these estimated differentials could reflect simple statistical discrimination. Minority workers and white workers may each receive average starting wages equal to average performance, but a minority worker who turns out to be a high performer will end up getting a lower starting wage than a white worker who turns out to be a low performer, even if these workers turn out to have the same performance. A test of statistical versus taste discrimination, and a test of statistical discrimination versus pure measurement error, provide some evidence for both men and women that statistical discrimination is partly to blame for these differences in starting wages between minority and white workers, although the evidence is not very strong statistically.

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[36]This is true looking at the ratio α_{OLS}/α_{IV} or the difference between α_{OLS} and α_{IV}. The ratio is more relevant to the test, because a given difference between a_{OLS} and a_{IV} could be consistent with ratios close to one (e.g., a_{OLS} = .8 and a_{IV} = .9) and close to zero (e.g., a_{OLS} =.02 and a_{IV} = .12), and hence consistent with widely varying reliability of the employer's labor market information.

Average performance of women in the sample studied in this paper is if anything higher than that of men, so simple statistical discrimination cannot explain the lower starting wages that women receive. However, more complex models of statistical discrimination suggest that worse labor market information about a particular group can lead to lower demand for that group (even conditional on the same average performance or productivity), and hence generate wage differentials. A test of the quality of labor market information regarding male and female employees suggests that employers have better information about male workers, which may explain the lower starting wages paid to women, although again the evidence is not strong.

Together, these findings provide some reasons to believe that better labor market information about minority or female employees (and, in fact, all employees) might help to boost starting wages of minorities and women. However, these conclusions should be treated cautiously for four reasons. First, the evidence reported in the paper is not overwhelmingly strong from a statistical standpoint. Second, it is difficult to distinguish fully between the statistical discrimination hypothesis and the hypothesis that the performance ratings studied in this paper are simply prone to classical measurement error, although some evidence reported in the paper suggests that the problem of labor market information is real. Third, the empirical methods rely on identifying assumptions that are obviously open to debate; alternative identifying assumptions that can be pursued using other data sets would clearly be of interest. Finally, relatively little is known about how to convey useful information about employees to potential employers. Direct examination of the consequences of using alternative methods (skills certification, job testing, etc.) would also be necessary to evaluate whether wage differentials by race and sex can be partly addressed via better labor market information.

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Table 1: Sample Means for Hourly Wages and Productivity Proxies

	Men			Women		
	White	Black	Hispanic	White	Black	Hispanic
Log starting wage	2.12	1.93	2.04	1.98	1.79	1.94
	(.42)	(.33)	(.36)	(.41)	(.28)	(.36)
Performance rating	78.10	74.43	73.48	79.29	76.66	78.37
	(13.24)	(16.16)	(13.08)	(14.58)	(15.23)	(12.48)
Log performance rating	4.34	4.28	4.28	4.35	4.31	4.35
	(.19)	(.30)	(.20)	(.23)	(.24)	(.17)
N	345	158	155	359	164	110

Standard deviations are reported in parentheses. Sample is restricted to those earning hourly wages. Estimates are weighted.

Table 2: OLS and IV Estimates of Log Starting Hourly Wage Regressions, Men

	OLS (1)	OLS (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	IV (7)
A. Using linear performance rating							
Performance rating	...	.0044	.0035	.009	.0038	.035	.027
		(.0011)	(.0009)	(.009)	(.001)	(.015)	(.011)
Black	-.143	...	-.126	-.106	-.178	-.062	-.091
	(.032)		(.033)	(.047)	(.029)	(.081)	(.064)
Hispanic	-.039	...	-.006	.020	-.066	.080	.043
	(.030)		(.031)	(.053)	(.034)	(.089)	(.067)
Schooling = 12	.120	...	.187	.184	...	...	...
	(.042)		(.042)	(.044)			
Schooling = 13-15	.093	...	.152	.142	...	...	...
	(.049)		(.050)	(.054)			
Schooling = 16+	.436	...	.549	.533	...	...	...
	(.054)		(.053)	(.060)			
Age	.050	...	...	...	...	...	...
	(.007)						
$Age^2 \times 10^{-2}$	-.061	...	...	...	...	...	...
	(.010)						
General experience required	.026	...	.043	.051	...	...	...
	(.031)		(.032)	(.035)			
Specific experience required	.122	...	.158	.147	...	...	...
	(.034)		(.035)	(.040)			
Vocational education/training required	.264	...	.292	.303	...	...	...
	(.039)		(.040)	(.045)			
R^2	.384	.024	.347	...	.054	...	...
P-value from F-test of instruments in first-stage regression:							
Age variables only	...	...	.03	...	...	...	...
Education variables only	...	...	.58	...	...	...	...
Job requirement variables only	...	...	.47	...	...	...	...
Instruments	...	...	...	Age	...	Age	Age, education, job requirements
F-statistic on instruments in first-stage regression:	...	...	...	3.39	...	3.81	1.50
Overidentifying restrictions, p-value:	...	...	...	...	...	...	.00
Bias in OLS estimates, p-value from Hausman test:							
Performance rating	...	...	...	.55	...	.04	.03
Black	...	...	...	.56	...	.11	.09
Hispanic	...	...	...	.56	...	.07	.06

Table 2 (continued)

	OLS (1)	OLS (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	IV (7)
<u>B. Using log performance rating</u>							
Performance rating	...	.289	.220	.034	.252	2.012	1.899
		(.068)	(.057)	(.657)	(.068)	(1.025)	(.689)
Black	-.143	...	-.124	-.136	-.175	-.059	-.067
	(.032)		(.033)	(.053)	(.039)	(.088)	(.070)
Hispanic	-.039	...	-.008	-.020	-.068	.043	.036
	(.030)		(.030)	(.052)	(.036)	(.082)	(.065)
R^2	.384	.027	.347	...	.056	...	...
P-value from F-test of instruments in first-stage regression:							
Age variables only	...	...	.09	...	...	...	...
Education variables only	...	...	.32	...	...	...	...
Job requirement variables only	...	...	.40	...	...	...	...
Instruments	...	...	...	Age	...	Age	Age, education, job requirements
F-statistic on instruments in first-stage regression:	...	...	...	2.47	...	2.92	1.51
Overidentifying restrictions, p-value:	...	...	...	...	...	...	.00
Bias in OLS estimates, p-value from Hausman test:							
Performance rating	...	...	...	.78	...	.09	.02
Black	...	...	...	.78	...	.14	.06
Hispanic	...	...	...	.78	...	.13	.06

There are 658 observations. Standard errors of estimates are reported in parentheses. Hausman tests are calculated based on individual coefficient estimates. Specifications in Panel B correspond to those in Panel A, although only selected coefficient estimates are reported. All estimates are weighted. Choice of instruments used in column (4) are based on F-statistics from first-stage regression for the productivity score, as reported in the table.

Table 3: OLS and IV Estimates of Log Starting Hourly Wage Regressions, Women

	OLS (1)	OLS (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)	IV (9)
A. Using linear performance rating									
Performance rating	...	.0014	-.0001	.027	.0004	.034	.0010	.041	.047
		(.0011)	(.0010)	(.009)	(.0010)	(.010)	(.0010)	(.010)	(.011)
Black	-.136	...	-.156	-.101	-.172	-.089	-.185	-.081	-.065
	(.036)		(.036)	(.057)	(.037)	(.066)	(.039)	(.075)	(.083)
Hispanic	-.010	...	-.042	-.026	-.062	-.026	-.041	-.004	.001
	(.037)		(.037)	(.056)	(.038)	(.065)	(.040)	(.073)	(.081)
Schooling = 12	.090	...	...	...	...	...	...	...	...
	(.056)								
Schooling = 13-15	.173	...	...	...	...	...	...	...	...
	(.059)								
Schooling = 16+	.227	...	...	...	...	...	...	...	...
	(.066)								
Age	.035	...	.039	.037	...	...	...	...	...
	(.009)		(.009)	(.013)					
$Age^2 \times 10^{-2}$	-.040	...	-.044	-.047	...	...	...	...	...
	(.012)		(.012)	(.019)					
General experience required	-.036	...	-.033	-.015	-.030	-.006	...	...	...
	(.039)		(.039)	(.058)	(.040)	(.068)			
Specific experience required	.132	...	.135	.091	.181	.103	...	...	...
	(.042)		(.043)	(.066)	(.043)	(.077)			
Vocational education/training required	.233	...	.247	.207	.255	.205	...	...	...
	(.044)		(.044)	(.068)	(.046)	(.079)			
R^2	.228	.003	.204	...	.148	...	.037	...	...
P-value from F-test of instruments in first-stage regression:									
Age variables only	...	...	.17	...	...	...	...	...	...
Education variables only	...	...	.00	...	...	...	...	...	...
Job requirement variables only	...	...	.69	...	...	...	...	...	...
Instruments	...	...	...	Educ.	...	Educ., age	...	Educ., age	Educ., age, job requirements
F-statistic on instruments in first-stage regression:	...	...	...	5.00	...	3.98	...	4.44	2.95
Overidentifying restrictions, p-value:	...	...	...	...	...	.75	...	.95	.20
Bias in OLS estimates, p-value from Hausman test:									
Performance rating	...	...	...	.00	...	.00	...	.00	.00
Black	...	...	...	.21	...	.13	...	.11	.10
Hispanic	...	...	...	.70	...	.50	...	.55	.55

Table 3 (continued)

	OLS (1)	OLS (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)	IV (9)
B. Using log performance rating									
Performance rating	...	.062	-.021	1.623	.006	2.184	.041	2.627	3.028
		(.068)	(.062)	(.613)	(.063)	(.642)	(.067)	(.704)	(.764)
Black	-.136	...	-.156	-.108	-.173	-.096	-.186	-.090	-.075
	(.036)		(.036)	(.055)	(.037)	(.066)	(.039)	(.076)	(.084)
Hispanic	-.010	...	-.042	-.044	-.062	-.051	-.042	-.033	-.031
	(.037)		(.037)	(.054)	(.038)	(.065)	(.040)	(.073)	(.081)
R^2	.228	.001	.204	...	.148	...	.037	...	...
P-value from F-test of instruments in first-stage regression:									
Age variables only	...	...	.21	...	...	...	...	...	...
Education variables only	...	...	.00	...	...	...	...	...	...
Job requirement variables only	...	...	.82	...	...	...	...	...	...
Instruments	...	...	...	Educ.	...	Educ., age	...	Educ., age	Educ., age, job requirements
F-statistic on instruments in first-stage regression:	...	...	...	4.58	...	3.60	...	3.94	2.56
Overidentifying restrictions, p-value:	...	...	...	...	...	.52	...	.84	.12
Bias in OLS estimates, p-value from Hausman test:									
Performance rating	...	...	...	.01	...	.00	...	.00	.00
Black	...	...	...	.25	...	.16	...	.14	.13
Hispanic	...	...	...	.96	...	.82	...	.88	.88

There are 633 observations. Standard errors of estimates are reported in parentheses. Hausman tests are calculated based on individual coefficient estimates. Specifications in Panel B correspond to those in Panel A, although only selected coefficient estimates are reported. Choice of instruments used in column (4) are based on F-statistics from first-stage regression for the productivity score, as reported in the table.

Table 4: Estimates of Quality of Labor Market Information for Non-Probationary and Probationary Workers

	Men and women combined		Men only	
	Non-probationary (1)	Probationary (2)	Non-probationary (3)	Probationary (4)
A. Using linear performance rating				
a_{OLS}	.0017	.0026	.0062	.0032
	(.0018)	(.0008)	(.0025)	(.0012)
a_{IV}	.020	.079	.021	.050
	(.008)	(.023)	(.020)	(.029)
a_{OLS}/a_{IV}	.089	.033	.294	.065
	(.093)	(.014)	(.295)	(.043)
Instruments	Age, education		Age	
First-stage regression				
F-statistic on instruments	3.80	2.46	1.31	1.79
p-value	.00	.03	.27	.17
N	274	1021	135	524
B. Using log performance rating				
a_{OLS}	.128	.153	.434	.219
	(.124)	(.051)	(.181)	(.072)
a_{IV}	1.349	4.744	1.349	2.579
	(.591)	(1.459)	(1.423)	(1.783)
a_{OLS}/a_{IV}	.095	.032	.322	.085
	(.093)	(.014)	(.349)	(.064)
Instruments	Age, education		Age	
First-stage regression				
F-statistic on instruments	3.37	2.24	1.28	1.31
p-value	.01	.05	.28	.27

Standard errors of estimates are reported in parentheses. All regressions exclude age, education, and job requirement variables from the wage equation.

Table 5: Estimates of Degree of Statistical Discrimination
Depending on Quality of Labor Market Information, Men

	Non-probationary (1)	Probationary (2)
A. Using linear performance rating		
OLS Estimates:		
Black	-.283	-.148
	(.093)	(.043)
Hispanic	-.194	-.039
	(.097)	(.039)
IV Estimates:		
Black	-.213	.010
	(.142)	(.129)
Hispanic	-.149	.167
	(.125)	(.148)
Proportion of wage gap due to statistical discrimination		
Black	.25	1.0
Hispanic	.23	1.0
B. Using log performance rating		
OLS Estimates:		
Black	-.287	-.144
	(.093)	(.043)
Hispanic	-.192	-.040
	(.098)	(.038)
IV Estimates:		
Black	-.233	.014
	(.132)	(.141)
Hispanic	-.147	.099
	(.127)	(.125)
Proportion of wage gap due to statistical discrimination		
Black	.19	1.0
Hispanic	.23	1.0

Estimates in columns (1) and (2) are from the specifications in columns (3) and (4) of Table 4. See notes to Tables 2-4 for details. The proportion of the wage gap due to statistical discrimination is recorded as 1.0 when the IV estimated is greater than or equal to zero, consistent with statistical discrimination explaining all of the wage gap.

Table 6: Estimates of Quality of Labor Market Information for Different Demographic Groups

	Men (1)	Women (2)	White (3)	Non-white (4)	White (5)	Non-White (6)
A. Using linear performance rating						
a_{OLS}	.0039	.0010	.0017	.0034	.0017	.0034
	(.0011)	(.0010)	(.0011)	(.0010)	(.0011)	(.0010)
a_{IV}	.034	.041	.046	.081	.045	.068
	(.015)	(.010)	(.017)	(.048)	(.013)	(.021)
a_{OLS}/a_{IV}	.115	.025	.037	.042	.038	.049
	(.058)	(.026)	(.027)	(.027)	(.026)	(.020)
Instruments	Age	Age, education	Age		Age, education	
First-stage regression						
F-statistic on instruments	3.82	4.36	4.87	1.43	3.18	2.16
p-value	.02	.00	.01	.24	.01	.06
B. Using log performance rating						
a_{OLS}	.253	.041	.093	.212	.093	.212
	(.067)	(.066)	(.073)	(.058)	(.073)	(.058)
a_{IV}	1.886	2.631	2.779	5.815	2.481	4.401
	(.985)	(.712)	(1.167)	(3.842)	(.791)	(1.417)
a_{OLS}/a_{IV}	.134	.016	.034	.036	.038	.048
	(.076)	(.025)	(.029)	(.026)	(.031)	(.020)
Instruments	Age	Age, education	Age		Age, education	
First-stage regression						
F-statistic on instruments	2.93	3.84	4.06	1.13	3.06	1.96
p-value	.05	.00	.02	.32	.01	.08

Standard errors of estimates are reported in parentheses. All regressions exclude age, education, and job requirement variables from the wage equation.