Essays on Statistical Discrimination and on the Payoff to Publishing in Economics Journals

By

Yariv Fadlon

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Approved:

Professor **Andrea Moro**

Professor **Myrna Wooders**

Professor **William J. Collins**

Professor **Jacob Sagi**

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Chapter I

Introduction

This dissertation is comprised of three essays entitled: "Statistical Discrimination and the Implications of Employer-Employee Racial Matches," "Gender Wage Gap: Does Employer's Gender Matter?," and "Ranking Journals: Show me the Money."

The first essay studies the empirical validity of a statistical discrimination model that incorporates employer's race. The theory of statistical discrimination states that, at hiring, an employer does not observe an employee's true productivity but instead uses the employee's average group productivity as an indicator of the individual employee's productivity. For example, if an employer has to decide between two identical potential employees and it just happens that one employee is white while the other is black, then the employer is more likely to bid a higher wage on the white employee just because the employer thinks that, on average, white employees are more productive.

Previous literature that empirically tested the implications of a statistical discrimination model focused mostly on the race of the employee only (Altonji and Pierret 2001, Oettinger 1996). In this essay, I build a model that takes into account the employer's race. I then test whether employers statistically discriminate less against employees that share the same racial background than they do against employees from a different race.[1] To incorporate the employer's race into a statistical discrimi-

[1] An employee statistically discriminates less against one group if the wage assignment correlates less with the employee's group average productivity.

nation model, I assume that communication is better between the employer and the employee if the two share the same racial background. This assumption is based on evidence from previous literature, especially from psychology.

From the theoretical model, I generate two predictions to test empirically. The first prediction states that the correlation between the employee's unobserved skill level and the wage is greater if the employee and the employer share the same racial background than if they do not. This prediction excludes *taste for discrimination* (Becker (1971)), since the effect of *taste for discrimination* shows in the average wage and not in the correlation between skill and wage. The second prediction states that, if there are enough minority employers, a high skill minority employee is more likely to select himself into a same race employer.

To test these predictions, I use data from the National Longitudinal Survey of Youth 1997 (NLSY97). The data contains information on the supervisor's race, which is used as a proxy for the employer. In addition, NLSY97 contains test scores that are used to calculate the Armed Force Qualification Test (AFQT). The AFQT is not observed directly by employers and is used in the analysis as a measure of skill.[2] In the empirical section, I regress log wage on AFQT and other controls and test whether the coefficient on AFQT for employees that share the same racial background as the employers is greater than the coefficient on AFQT for employees that are from a different race.

Given the wage schedules, there is a potential selection bias that should be ad-

[2] Neal and Johnson (1996) show that AFQT is a good proxy for pre-market skill level using data from NLSY79 for the years 1990-1991. Since in this Chapter I use a sample from NLSY97, which contains different responders, and the data is taken for the year 2007, I report the analysis from Neal and Johnson in this Chapter.

dressed in the estimations. That is, the selection of employees to employers is not random and it depends on the employees' skill level. Because a high skill employee is more likely to accept a higher wage offer from a same race employer that can better read his productivity, the selection of employees to employers is not random. Therefore, there is a sample selection bias which I address in the estimations by using the maximum likelihood Heckman sample selection technique.

My findings mainly support the hypothesis that employers statistically discriminate less against same race employees. I find evidence to support this prediction for both white and black employees. However, only 6% of white employees have a non-white employer, whereas 63% of the minority employees are employed by a different race employer. In addition, the results do not show a large selection of high skilled employees to a same race employer. Therefore, the results suggest that even though a high skill minority employee is more likely to receive a higher wage offer from the same race employer, there are not enough minority employers that offer high skill jobs to accommodate the supply. As a result, a high skill black employee might be forced to accept a lower wage offer from a white employer, because he cannot find a black employer.

These findings provide evidence that policy makers can reduce the racial wage gap by focusing on affirmative action programs that help minorities become employers and/or programs which provide cultural sensitivity training for employers to improve cross-race communication.

The second essay picks up on the findings from the first essay and tests whether the results from the racial wage gap apply to the gender wage gap. That is, I test whether the sex of the employer explains part of the gender wage gap.

The sample is collected from the NLSY97 with the same criteria as the sample used in the first essay. However, because the focus of this essay is on the gender wage gap, the sample is limited to white employees and employers.

The paper is mainly related to the literature that explains the gender wage gap due to differences between the sexes' returns to skill. That is, if an employer of one gender has more information about the employee's underlying skill level than an employer of another gender, then the return for skill is different between genders of employers. This difference might explain the differences in the return for skill between genders of employees if there are not enough female employers, as the sample suggests.

The main finding suggests that there is no evidence that an employer of one gender better observes an employee's underling skill level than an employer of the opposite sex. This result is a contrary to the findings in the first essay that tested the racial wage gap. The intuition for the results in the first essay is that communication is better when two parties share the same racial background than if they do not. In addition, because social networks are often racially stratified, an employer is more likely to have more information about a potential employee with the same racial background. Therefore, additional information might flow through the social network.

The motivations for the racial wage gap, however, are most likely not transferable to the gender wage gap. Social networks are not gender stratified in the way that they are racially stratified. In addition, communication may not vary based on whether or the not the two parties share the same sex. This does not mean that employers do not statistically discriminate against one gender, just that statistical discrimination does not vary based on the employer's gender.

The results have important implications for programs that focus on closing wage gaps. In particular, programs that help reduce the racial wage gap might not be as effective in reducing the gender wage gap. For example, a few studies suggest that female employees are less likely to be promoted relative to comparable male employees (see McDowell et al. (1999), Sampson and Moore (2008), Jellala et al. (2008), Arulampalam et al. (2007)). Affirmative action programs that provide incentives to promote female employees and minority employees are likely to yield greater reductions in the racial wage gap. As with minority average wage, female average wage will increase when more women are in higher level positions; however, female employees will not derive the additional benefit from improved communication with their employers that would be seen with minority employees

The third essay is a joint work with Myrna Wooders. In this essay we rank economics journals based on the salaries of those that have published in those journals. Previous literature that ranked economics journals used mainly citation rate, with some sporadic use of ranking according economists' opinions. We introduce a novel method to rank economics journals according to author salaries using a unique dataset of 597 economics faculty from research institution across the United States.

We estimate the expected wage conditional on the number of publications in each journal and other faculty characteristics. In addition, we introduce two methodologies to calculate a publication. First, a publication by n authors counts as $\frac{1}{n}$ publications. Second, a publication by one author counts as one publication, two authors counts as $\frac{2}{3}$ publications, and $n > 2$ authors counts as $\frac{1}{n-1}$ publications. Furthermore, we report results with two ways for treating older publications. First, a publication value is constant over time. Second, a publication depreciates according to the Consumer Price Index.

Our main finding suggests that the *Journal of Political Economics* and *Econometrica* are at the top of our ranking in almost all the estimations. The relative payoff to publication in those journals is worth an additional $16,900 to $18,800 above the average salary, after controlling for faculty characteristics.

Chapter II

Statistical Discrimination and the Implications of Employer-Employee Racial Matches

Introduction

In this paper, I test the empirical validity of a statistical discrimination model which incorporates the employer's race. The empirical literature consistently documents a racial wage gap that persists despite the passage of more than 40 years since the Civil Rights Act of 1964, which sought to eliminate racial discrimination in employment. Statistical discrimination theories for such persistence have emphasized explanations based on incomplete information about a worker's productivity.[3] In one version of the theory, the nature of the incomplete information problem relies on the difficulties an employer has in observing a minority worker's skill.

Textbook examples of statistical discrimination emphasize that one mechanism for obtaining group inequality is based on a race biased measure of skill observed by an employer. That is, it is assumed that the nature of the communication between an employer and an employee is such that a minority employee is at a disadvantage in communicating his skill to the employer. In this paper I test whether an employer statistically discriminates less against an employee that shares the same race (hereafter referred to as match) than an employee that does not share the same race (hereafter referred to as mismatch). If an employer is better equipped to evaluate the skill of

[3] See for example the seminal papers of Phelps (1972), and Arrow (1973).

an employee of the same race, then the theory provides us with a clean empirical prediction about the relationship between employee's race, employer's race and the relationship between wages and skill.

Understanding the nature of racial wage inequality is important not only for theoretical implications but also for policy implications. If an employer statistically discriminates less against a match employee, then policy makers can reduce the racial wage gap by focusing on affirmative action programs that help minorities become employers.[4] In addition, programs that provide cultural sensitivity training for employers to improve cross-race communication may also be useful.

To generate testable empirical predictions, I extend the basic model of statistical discrimination proposed by Phelps (1972). In the model, when an employer estimates a potential employee's productivity, he uses observable characteristics of productivity, such as education, and estimates the residual productivity using a signal. Therefore, an employer's wage offer is the weighted average between the group statistic and the signal after conditioning on the observable characteristics. If the signal is more accurate, then an employer puts more weight on the signal and less on the group statistic, and vice-versa. To this standard setup, I add the assumption that a match employer can communicate with an employee better than a mismatch employer. The next section describes evidence from several disciplines supporting this assumption. One of the goals of this paper is to test whether the theoretical implications of such assumption hold empirically using economic data.

[4] Affirmative action policy that focuses on the employee's side only can be expensive and inefficient (see Coate and Loury (1993), Moro and Norman (2003)).

From this theoretical model, I derive two testable predictions. First, a match employee's wage should correlate with measures of skill more than a mismatch employee's wage. Second, if the labor demand is perfectly elastic, then the probability of working for a match employer should be positively correlated with measures of skill. The intuition for these predictions is that a high skill employee prefers to be evaluated by his true skill level and, therefore, prefers a match employer that can better read his skill level. A low skill employee prefers to pool with his group rather than be evaluated by his own skill level. Therefore, he is more likely to receive a higher wage offer from a mismatch employer.

To test these predictions, I use data from the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 contains detailed information about the responder's employment. In particular, it contains information about the race/ethnicity of the responder's supervisor, who is used in this study as a proxy for the employer.[5] The predictions in this study rely on the information an employer has about his employee. Therefore, a supervisor is a good proxy because a supervisor usually interacts frequently with his employee and is directly involved in the evaluations. The data consists of male employees, age 22 to 28 from the year 2007.

I use the Armed Force Qualification Test (AFQT) score as a correlate-of-productivity.[6] Section 4 provides evidence showing that AFQT score is a good measurement of employee's productivity, since it explains much of the racial wage gap and it is not

[5] In contrast, the earlier NLSY79 does not contain information about the supervisor.

[6] Neal and Johnson (1996) show that AFQT is a racially unbiased measure of premarket skill level for responders in the earlier NLSY79. They argue that since the AFQT was composed of tests the responders have completed by the age 18, it is not biased by discrimination and better than postsecondary education which is endogenous and affected by discrimination.

affected by the employee's postsecondary education decision. Specifically, AFQT explains 43% of the racial wage gap for men and 100% of the racial wage gap for women.

To test the first prediction, I use the Maximum Likelihood Estimation (MLE) based on Heckman sample selection estimation procedure because I estimate the conditional wage by dividing the sample into match and mismatch subsamples.[7] The results show support for the first prediction. For white employees, the effect of AFQT on wage in the match subsample is positive and statistically significant and is greater than the effect of AFQT in the mismatch subsample, as the theory predicts. The effect of AFQT in the mismatch sample is small and statistically insignificant. For black employees, the effect of AFQT for the match subsample is 30% greater in magnitude than the effect of AFQT in the mismatch subsample.

To test the second prediction, I estimate the probability of working for a match employer using probit estimation. The results show support for some selection of employees in white-collar occupations based on AFQT, with the results driven by white employees only. An increase in one standard deviation in the AFQT increases the probability of working for a match employer by 4.9% more than an employee that is employed in a non-white-collar occupation. However, the results do not support a strong selection of employees with high AFQT score to match employers.

Putting together the results from the wage regressions and the probit estimations, I find evidence to support the prediction that a match employer better observes the underlying skill level, but there is little evidence that a high skill minority employee

[7] Since the selection of employees to employers is based on the employees' skill level, each subsample is not random. The Heckman correction technique corrects for such non-randomness.

selects himself to a match employer. These results imply that the demand of black employers for high skill employees is elastic. That is, employers statistically discriminate less against match employees, but black employees hardly select themselves based on the skill level because there are not enough minority employers.[8]

The rest of the paper is organized as follows. The next section summarizes the related literature. Section 3 presents the theory that underlies the empirical results. Section 4 describes the data. Section 5 presents the empirical methodology and results. Section 6 discuss the results and tests their robustness to firm size and union membership. Finally, section 7 concludes.

Related Literature

The theoretical model presented in this paper is built on Phelps (1972). In his seminal paper, Phelps argues that statistical discrimination can cause differences in the wage schedule across groups in two ways. First, actual or perceived average productivity across groups is different. Because employers base their wage assignment to individual employees on the group average productivity, the average wages across groups is different (see also Coate and Loury (1993) and Moro and Norman (2004)). Second, members from the disadvantage group are at a disadvantage in communicating their skill. Therefore, employers make more mistakes when trying to estimate the skill of employees from the disadvantage group, and as a result, employers base

[8] This implication is also supported by looking at the summary statistics. The vast majority of white employees (94%) work for white employers while only 30% of the minority employees are employed by match employers regardless of AFQT.

the wage assignment to members of the disadvantage group more heavily on the average productivity (statistical discrimination) and less on the individual employee's observed signal. This paper is mostly related to the second type of statistical discrimination.

The main assumption of this paper's model, that employers better observe employees' signals when their races match, relies on evidence from various disciplines documenting difficulties in communications among members of different racial groups. Lang (1986) summarizes a vast literature documenting evidence that miscommunication is more common for members of a different group (race, gender) than for members of the same group. Similarly, Hecht et al. (2003) examine a number of studies on racial differences in communication. These studies find that cultural differences, such as unique linguistic, rhetorical, and relational styles, contribute to lack of communication (reading 3.1 pages 105-107). In the medical literature, Cooper-Patrick et al. (1999) analyze physician-patient interactions. They find that white and black patients who saw physicians of the same race rated their physician's decision making style as more participatory and were more satisfied than were patients who saw physicians of a different race. In essence, physicians and patients communicate better if both parties share the same race. Finally, Calvo-Armengol and Jackson (2004) and Calvo-Armengol and Jackson (2007) summarize evidence in the social networking literature that workers find jobs through social networks. These networks are often racially stratified (McPherson et al., 2001). If blacks are more likely to be connected through a black social network, then a black social contact is more likely to refer a black worker to a black employer. This referral can include additional information about the worker's skill.

The model presented here relates to a theoretical paper by Cornell and Welch (1996). They make a similar assumption that an employer can better estimate an employee's productivity if the employee belongs to the same group. They show that even if an employer does not want to discriminate, noisy signals lead to hiring discrimination. Their model provides an implication similar to the first prediction presented in this paper. That is, in Cornell and Welch's model, a noisy signal leads to a lower probability of been hired whereas in this paper, it leads to a lower wage.

This paper also belongs to a literature aimed at testing implications of statistical discrimination models in the reduced form. Dickinson and Oaxaca (2009) report results from controlled laboratory experiments. They find support for statistical discrimination in a labor market where employers are risk averse. Oettinger (1996) develops a dynamic model of statistical discrimination based on Phelps (1972), which he then tests using data from the NLSY79. In Oettinger's model, as experience accumulates, black employees are less able to pursue new job offers that would result in a higher wage. That is because black employees suffer from statistical discrimination at hiring and by moving to a new employer, they are more likely to receive a smaller wage offer than the wage paid by the current employer that learned their productivity. His empirical findings mainly support this theoretical prediction. Oettinger, however, is primarily interested in the dynamics of statistical discrimination and does not control for employers' race. In addition, Oettinger does not control for unobserved indicators of productivity, such as AFQT, so his implications are different than the ones pursued in this paper.

Altonji and Pierret (2001) incorporate unobservable indicators of productivity into a model of statistical discrimination, as I do here. In their model, employers statistically discriminate at hiring, basing decisions on easily observable, biased measurements of skill such as education and group statistics. As employers learn a worker's true productivity, the wage should be more correlated with hard to observe measurements of skill, and the gap in wages should fade away. Altonji and Pierret find some support for this proposition using data from the NLSY79. However, they assume that information is common across firms and, just like Oettinger, do not control for employer's race. To my knowledge, this is the first paper analyzing the relationship between the employer's race, the worker's race, measures of productivity and wages.

A Model of Statistical Discrimination with Employer's Race

To illustrate the theoretical basis for the empirical work presented in this paper, I extend Phelps (1972) basic model of statistical discrimination by incorporating the employer's race. There is a continuum of employees and n employers.[9] Each employee and each employer belong to one group (race/ethnicity). A couple (employee, employer) either belongs to the same group (s), or belongs to different groups (d).

Each employee has an unobserved (by employers) skill level, μ, drawn from a normal distribution, which is identical for both groups i.e., $\mu \sim N(m, \gamma^2)$. This

[9] The assumption of final number of employers can be relaxed. However, in the second part of the theoretical model, I test the implications of the model when n is smaller than the number of employees.

assumption can easily be changed, and in the empirical section I control for groups' indicators. An employer does not observe the employee's skill, but instead observes a noisy signal of skill (θ) for each worker. This signal may contain impressions from the interview, chemistry and letters of recommendation, for example. Let $\theta_i = \mu + \varepsilon_i$ where $i \in \{s\ ,\ d\}$, and $\varepsilon_i \sim N(0, \sigma_i^2)$, is independent of skill, and for simplicity, ε_s is independent of ε_d.

Based on the employee's skill level (μ), a match employer and a mismatch employer draw a different signal (θ_s and θ_d).[10] I assume that a match employer better observes the signal than a mismatch employer, or $\sigma_d^2 > \sigma_s^2$. This assumption is essential to get differences in the wage schedule between match employers and mismatch employers. The previous section provides evidence from multiple disciplines including economics, psychology and medicine to support this assumption.

Employers are risk neutral, compete in the labor market for workers in Bertrand competition, and simultaneously announce wage offers as a function of the observed signal and employee's race. Thus, an employer wage strategy is a mapping $w(\theta_i) : \mathbb{R}$ x $\{s, d\} \rightarrow \mathbb{R}_+$. Each employee observes wage offers, and for this part I assume the offers come from match and mismatch employers, and then the employee accepts one offer and rejects the others. Thus, an employee's strategy is a mapping $\mathbb{R}_+^n \rightarrow \{s, d\}$. I assume that an employee chooses to work for an employer that offers the highest wage.

[10] The number of offers is not crucial for determining the wage schedule. However, it might effect the selection of employees to employers based on the skill level as I discuss bellow.

Therefore, the wage schedule is:

$$w(\theta_i) = E(\mu|\theta_i) = \left(\frac{\sigma_i^2}{\sigma_i^2 + \gamma^2}\right) m + \left(\frac{\gamma^2}{\sigma_i^2 + \gamma^2}\right) \theta_i \qquad (1)$$

$$i \in \{s, d\}.$$

Appendix A contains a detailed derivation of equation (1). From equation (1), I conclude that the wage offer is the weighted average between the unconditional expected group skill (m) and the individual employee's observed signal (θ). Thus, if the signal is more accurate to a match employer, then a match employer puts more weight on the signal and less on the group average statistics.[11]

Note that equation (1) can also be written as:

$$w(\theta_i) = (1 - \rho_i^2)m + \rho_i^2(\mu + \varepsilon_i) = (1 - \rho_i^2)m + \rho_i^2\mu + \rho_i^2\varepsilon_i \qquad (2)$$

$$i \in \{s, d\}$$

where $\rho_i^2 = \frac{\gamma^2}{\sigma_i^2+\gamma^2}$ is the square correlation between the signal and the true skill (productivity).

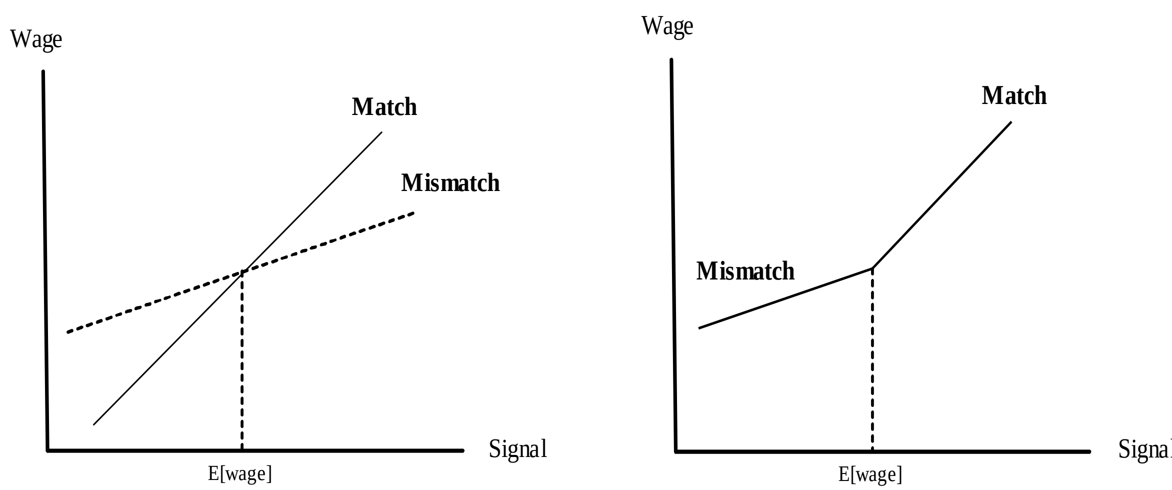
From equation (2) I conclude the following proposition:

Proposition 1 *If employers can better read signals of productivity from employees that share the same race as the employer, then the effect of true productivity on the wage correlates more for a match employee than for a mismatch employee.*

[11] Using the law of iterated expectations, one can easily show that the unconditional expected wage for a match employee is equal to the unconditional expected wage for a mismatch employee ($E[w(\theta_s)] = E[w(\theta_d)]$). I discuss below why this unrealistic result does not change the empirical implications.

Proposition 1 suggests that if a match employer statistically discriminates less than a mismatch employer (by putting less weight on the group statistic), then the weight of true productivity on the wage offer is higher for a match employer than for a mismatch employer. This is shown in Figure 1(a). In the figure, the dashed (solid) line represents the wage offer as a function of the signal observed by a mismatch (match) employer. Employees observe the wage offers and choose which offer to accept. Since, by assumption an employee's utility is equal to the wage, an employee accepts the higher wage. Figure 1(b) presents the accepted wage with a solid line.

Figure 1: The Effect of Signal on Wage: Theory



(a) Wage Offer

(b) Wage Accept

Therefore, an employee chooses to work for a match employer if $w(\theta_s) \geq w(\theta_d)$. After substituting (2) and reorganizing, the inequality is:

$$\rho_d^2 \varepsilon_d - \rho_s^2 \varepsilon_s \leq (\rho_d^2 - \rho_s^2)p + (\rho_s^2 - \rho_d^2)\mu.$$

Since ε_s and ε_d are assumed to be independent, it follows that:

$$\rho_d^2\varepsilon_d - \rho_s^2\varepsilon_s \sim N(0, \rho_d^4\sigma_d^2 + \rho_s^4\sigma_s^2). \tag{3}$$

Therefore, the probability that the wage offer made by a match employer is greater than the wage offer made by a mismatch employer is:

$$\begin{aligned} \Pr[match|\mu] &= \Pr[w(\theta_s) \geq w(\theta_d)|\mu] = \qquad (4) \\ \Pr[(1-\rho_s^2)m + \rho_s^2\mu + \rho_s^2\varepsilon_s &\geq (1-\rho_d^2)m + \rho_d^2\mu + \rho_d^2\varepsilon_d] \end{aligned}$$

From the result obtained in (3), I conclude the following probability:

$$\Pr[match|\mu] = \Phi\left(\frac{(\rho_d^2 - \rho_s^2)p + (\rho_s^2 - \rho_d^2)\mu}{\sqrt{\rho_d^4\sigma_d^2 + \rho_s^4\sigma_s^2}}\right). \tag{5}$$

From equation (5) I conclude the following proposition:

Proposition 2 *If employers can better read signals of productivity from employees that share the same race as the employer and labor demand is completely elastic, then a highly productive worker is more likely to work for an employer of the same race.*

Proof. The proof is straightforward. Notice that the derivative of equation (5) with respect to μ is:

$$\frac{\partial \Pr[match|\mu]}{\partial\mu} = \frac{(\rho_s^2 - \rho_d^2)}{\sqrt{\rho_d^4\sigma_d^2 + \rho_s^4\sigma_s^2}}\phi\left(\frac{(\rho_d^2 - \rho_s^2)p + (\rho_s^2 - \rho_d^2)\mu}{\sqrt{\rho_d^4\sigma_d^2 + \rho_s^4\sigma_s^2}}\right)$$

Since $\sigma_d^2 > \sigma_s^2$, it is easy to show that $\rho_s^2 - \rho_d^2 > 0$. In addition, the second term in the derivative, $\phi(\cdot)$, is non-negative since it is the normal probability distribution function. Therefore, the derivative is non-negative. ■

There are at least three aspects of the model which are unrealistic. First, groups with the same average productivity receive the same average wage. Various papers obtained average group difference by adding another dimension into the employees' strategy and making the human capital decision endogenous (see Lundberg and Startz (1983), Coate and Loury (1993), Moro and Norman (2004) and Aigner and Cain (1977)). However, extending the model in this direction would not change the qualitative implications, which focus on the correlation between wages and signals and not on average wages. Second, the model implies extreme segregation among high signal employees and low signal employees. That is, employees with high (low) signal are more likely choose to work for the same (different) race employers. However, one could add heterogeneity into the employees' preferences. For example, if an employee's utility also depends on a random variable (not observed by the employer) measuring the preferences for working for a same race employer and workers are heterogeneous in this dimension, then the model would display more heterogeneity in the racial assignment of workers to employers, but the basic testable implications will still be valid.

Finally, in the model I have assumed completely elastic labor demand. In the data, however, there appears to be a low share of minority employers which might affect the selection of employees to employers based on their skill level (Proposition 2). A simple extension of the model can account for such fact.

Suppose the employee draws two employers.[12] Each employer can be either a match or a mismatch employer. The probability of drawing a match employer is π, and a mismatch employer is $1-\pi$. Therefore, the probability that the employee works for a match employer is equal to the probability of drawing two s employers or drawing one s employer and one d employer, and the wage offer from the s employer is greater than the wage offer from the mismatch employer. This probability is calculated as follows:

$$\begin{aligned} \Pr(match|\mu) &= \pi^2 + 2\pi(1-\pi)\Pr[w(\theta_s) > w(\theta_d)|\mu] \\ &= \pi^2 + 2\pi(1-\pi)\Phi\left(\frac{(\rho_d^2-\rho_s^2)p + (\rho_s^2-\rho_d^2)\mu}{\sqrt{\rho_d^4\sigma_d^2+\rho_s^4\sigma_s^2}}\right). \end{aligned} \tag{6}$$

From (6) I derive the following proposition:

Proposition 3 *If employers can better read signals of productivity from employees that share the same race as the employer, then the selection of employees to employers based on skill level is non-negative and gets smaller as $\pi \to 1$ or $\pi \to 0$.*

The marginal effect of skill level on the probability of match as a function of π is highest at $\pi = 0.5$ and gets lower as π goes up or down. The extreme cases are when $\pi = 0$ or $\pi = 1$. In these two cases, there is no selection of employees to employers based on skill. In the former, all employers are mismatch employers, whereas in the latter all employers are match employers. If π is small, a high skill employee is looking for a match employer, but the probability of receiving a wage offer from a

[12] This analysis corresponds to a case in which the employee is selected by a human resource agency and the employer is the one determining the wage.

match employer is low. As a result, the employee is forced to accept the wage offer from a mismatch employer. If π is high, a low skill employee wants to work for a mismatch employer but is less likely to find one. As a result, the employee is forced to work for a match employer, which suggests that the effect of skill on the probability of working for a match employer is low.

Propositions 2 and 3 look at the selection of employees based on their skill and the probability of receiving a wage offer from an employer. Specifically, proposition 2 suggests that if an employee can always receive wage offers from match employers and mismatch employers, then a highly productive employee selects himself into a match employer. Proposition 3 suggests that if the probability of receiving a wage offer from either a match or a mismatch employer is low, then the selection of employees to employers based on skill is low. In particular, if the proportion of black employers is too small to accommodate the demand for black employers, then the selection is small for both white and black employees.

Data

The data is taken from the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 is a nationally representative sample of 6,748 youths from the U.S. population who were 12 to 16 years old in December 31, 1996. I also use the supplemental over-sample of 2,236 Hispanic and black of the same age group. Since 1997, the responders have been interviewed annually.

The sample used in this study is from the interview results from the most recent year available to date: 2007. In the data, all responders are between 22 and 28 years of age. In addition, the sample is limited to male responders who are employed full time, who work at least 30 hours per week and who are not enrolled in school.[13] Therefore, the sample represents young employees, the majority of whom have completed their education and are fully engaged in the job market.

The NLSY97 is more detailed than the previous NLSY79 and includes information about the supervisor's race together with the respondent's race. With this information I build a variable, *match*, that indicates whether or not the worker has the same race as the supervisor (proxy for employer). In addition, I observe the race of the responder's spouse (defined as wife, husband, lover, or dating partner). I use this variable to instrument for the selection of employees into a match or mismatch employer in the Heckman sample selection model.[14] Table 1 presents summary statistics of the sample used in the analysis.

The implications extracted from the theoretical model are based on information the employer has about the employee's skill level. Following Farber and Gibbons (1996) and Altonji and Pierret (2001), I use the Armed Forces Qualification Test (AFQT) score as a proxy for the employee's skill level.[15] Unlike postsecondary education, which is not compulsory and is obviously endogenous since it is determined

[13] Appendix B provides more detailed variable descriptions.
[14] See further discussion in the next section.
[15] About 80% of the individuals in the sample took the Armed Services Vocational Aptitude Battery (ASVAB). The ASVAB is a set of 10 tests of which four are used to calculate the AFQT: Arithmetic Reasoning, Word Knowledge, Paragraph Comprehension and Mathematics Knowledge. I first sum these four test scores and then standardize the row scores to the responder's age at the test in cohorts of three months as in Neal and Johnson (1996).

Table 1: Summary Statistics by Employee's Race

Variable	White	Black	Hispanic
Education	13.41	12.322	12.284
Age	24.891	24.913	25.072
AFQT (age adjusted and normalized)	0.306	-0.693	-0.425
	(0.92)	(0.92)	(0.96)
Hours worked (weekly)	42.264	40.712	41.025
	(8.31)	(5.68)	(5.60)
Wage per hour	16.282	13.066	14.541
	(7.88)	(7.59)	(7.21)
# of employees in the firm	760	642	360
Big firm (# employees> 50)	44%	54%	54%
Union member	11%	20%	15%
Racial match	94.1%	37.2%	22.4%
White-collar occupation	37%	19%	25%
Partner match	89.1%	67.6%	68.1%
N	723	309	277

Standard errors in parentheses.

by ability, the AFQT score is composed of tests responders took between 12.5 and 18 years of age (i.e., before entering the job market) and therefore, is less affected by labor market discrimination.

Table 2 provides evidence showing that AFQT is a good measure of premarket skill level by regressing log wage on AFQT. The analysis in Table 2 replicates the main results obtained in Neal and Johnson (1996), but with the newer sample. Neal and Johnson use a sample from the earlier NLSY79 for the years 1990-1991. They argue that AFQT can explain about 70% of the racial wage gap for men and the entire racial wage gap for women.[16] My results using the more recent sample show that when I control for race/ethnicity indicators and age (column 1 for men and column 4

[16] Since the sample used in this paper is from NLSY97, which consists of a different set of responders, I perform separate analyses for a sample of men defined using the criteria described above and in more details in the appendix and an additional sample of women defined using the same criteria established for the men.

Table 2: Log Wage Regressions by Sex

	Men (N=1,309)			Women (N=1,168)		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Black	-0.221***	-0.175***	-0.125***	-0.156***	-0.0868***	-0.0136
	(0.0342)	(0.0336)	(0.0367)	(0.0363)	(0.0327)	(0.037)
Hispanic	-0.104***	-0.0592**	-0.0367	-0.0356	0.0751**	0.0647*
	(0.03)	(0.0287)	(0.0304)	(0.0365)	(0.0348)	(0.0359)
Age	0.0567***	0.0526***	0.0535***	0.0493***	0.0345***	0.0412***
	(0.00956)	(0.0091)	(0.00924)	(0.0108)	(0.00962)	(0.0104)
AFQT			0.107***			0.172***
			(0.0139)			(0.0154)
$AFQT^2$			0.0136			0.00585
			(0.0103)			(0.0139)
Education		0.0476***			0.0876***	
		(0.0054)			(0.00579)	
Constant	1.284***	0.749***	1.318***	1.325***	0.435*	1.438***
	(0.237)	(0.232)	(0.23)	(0.267)	(0.241)	(0.257)
R^2	0.063	0.132	0.112	0.035	0.237	0.121

Note- The data is taken from NLSY97 and includes the oversample of black and Hispanic. The sample for women was collected using the same criteria as for men. AFQT is normalized and age adjusted in 3-month cohorts. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

for women), the racial wage gap is high for men (-0.221) and women (-0.156). After controlling for education (column 2 for men and column 5 for women), which is a biased proxy for skill, the racial wage gap drops, but is still present and statistically significant (-0.175 for men and -0.0868 for women). When I control for AFQT score instead (column 3 for men and column 6 for women), the racial wage gap drops in the set of men (-0.125) and almost disappears in the set of women (-0.0136). Therefore, in this sense AFQT explains almost the entire racial wage gap among women and about 43% of the racial wage gap among men.

A comparison with the results in Neal and Johnson (1996) reveals that after 16 years, the racial wage gap still persists. Although AFQT explains less of the racial wage gap for men in NLSY97 than in NLSY79, it is still a good measure of premarket skill level.

Empirical Methodology and Results

The empirical results are divided into two parts. In the first part, I test the empirical implications of the model regarding wages as suggested in Proposition 1. In the second part, I look at the impact of employer's race and employee's race on the selection of employees to employers based on AFQT, and compare the results to Propositions 2 and 3.

Implication I: Worker and Employer Racial Match Affects the Relationship between Wages and Skills.

I divide the sample into two subsamples: match employees and mismatch employees. For each subsample, I compare the estimated marginal effect of AFQT on log hourly wage. The objective is to test whether the effect of AFQT on log(wage) is greater in the match subsample than in the mismatch subsample, as Proposition 1 suggests.

When I divide the sample into two subsamples, however, a self-selection problem might bias the results. Specifically, high ability employees are self selected into match employers. Since only a correlate-of-ability (AFQT) is observed, not true ability, the selection is not random, resulting in inconsistent estimations. Therefore, I use a maximum likelihood Heckman sample selection correction technique to account for the endogeneity of the racial match between employer and employee.[17]

To estimate the Heckman MLE, I define the following model with two latent variables:

$$\begin{aligned} wage_i &= \alpha_0 + \alpha_1 AFQT_i + \boldsymbol{\alpha}_2' \mathbf{X}_i + u_i \qquad (7) \\ d_i &= \boldsymbol{\gamma}' \mathbf{Z}_i + v_i \end{aligned}$$

[17] The variable that causes the sample selection is *match*. If I use, for example, the Instrumental Variable (IV) technique to instrument for the binary variable *match*, then the difference between the match sample and the mismatch sample is only in the intercept (because *match* is a binary variable). However, the model predicts that the two samples differ in the slope as well (i.e., the effect of AFQT in the match sample is different than that in the mismatch sample). One could use IV estimation by instrumenting for $match * AFQT$. However, this would not show differences in the other coefficients between match and mismatch.

where $\mathbf{X}_i$ and $\mathbf{Z}_i$ are vectors of individual characteristics, α and γ are parameter vectors to be estimated. u_i and v_i are jointly normally distributed, independently of X, Z and AFQT, with zero expectations. d_i is the variable defining the selection of employees to employers. If $d_i \geq 0$, then the observed wage is the wage offer made by a match employer while the wage offer from the mismatch employer is not observed. If $d_i < 0$, then the observed wage is the wage offer made by a mismatch employer while the wage offer from the match employer is not observed. The variable d_i is not observed, but only an employee's decision to work for match or mismatch employer is observed. The vector $\mathbf{Z}$ includes all of the controls in $\mathbf{X}$ together with AFQT and an additional variable that explains the selection ($match$).

The additional variable included in $\mathbf{Z}$ is a dummy variable that indicates whether or not the worker's spouse is of the same race as the worker. This variable should not be correlated with the error terms, but is correlated with the worker's decision to work for a match employer or a mismatch employer. If a worker's spouse is of a different race, then this worker has a link to the social network of the other race. This connection is likely to increases the number of jobs opportunities from mismatch employers.

From specification (10), the conditional expected wage to be estimated is:

$$E[wage_i|j, \mathbf{Z}, AFQT] = \alpha_{0j} + \alpha_{1j} AFQT_i + \boldsymbol{\alpha}'_{2j}\mathbf{X}_i + \rho_j \sigma_j \lambda(\boldsymbol{\gamma}'_j \mathbf{Z}_i) \tag{8}$$

$$j \in \{match, mismatch\}$$

where ρ is the correlation between u_i and v_i, and σ is the standard deviation of u_i.

$\lambda(\boldsymbol{\gamma}'\mathbf{Z}_i) = \frac{\phi(\boldsymbol{\gamma}'\mathbf{Z}_i)}{\Phi(\boldsymbol{\gamma}'\mathbf{Z}_i)}$ is the Inverse Mills Ratio and $\phi(\cdot)$ and $\Phi(\cdot)$ are the PDF and CDF of a standard normal distribution, respectively. Therefore, if the coefficient on λ is zero, OLS regression provides unbiased estimates. However, when the coefficient on λ is not zero, OLS estimates are biased

From (10) it is clear that the mapping from $\mathbf{Z}$ to $wage_i$ is nonlinear. Therefore, the excluded restriction imposed on the additional variable in $\mathbf{Z}$ in an IV estimation does not binding in the Heckman. That means also that $\mathbf{Z}$ and $\mathbf{X}$ could be identical. However, to improve identification it is better to include the additional variable in $\mathbf{Z}$ that explains selection (Vella and Verbeek (1999)).

Spouse Race as an Instrument for Selection

Table 3 reports the marginal effect of a spouse's race on the probability of match. For white employees, the coefficient on the *white spouse* indicator is almost zero and statistically insignificant. The coefficient on the *Hispanic spouse* indicator is statistically significant at the 10% level when I control for spouse race indicators only (column 1), but is statistically insignificant when I control for individual characteristics (column 2). However, white employees rarely work for mismatch employers (only 6%). Therefore, the selection is small.

The results for black employees indicate that a black employee with Hispanic or white spouse is about 30% less likely to work for a black employer. The marginal effect of the *black spouse* indicator is negative and statistically insignificant. This result may be attributed to a low number of black employers in the market. For

Table 3: Marginal Effect of Spouse's race on Pr(Match)

	White Employees		Black Employees	
Variable	(1)	(2)	(3)	(4)
White partner	-0.00103	0.00562	-0.267***	-0.292***
	(0.0227)	(0.019)	(0.0682)	(0.0658)
Hispanic partner	-0.121*	-0.0683	-0.328***	-0.346***
	(0.0727)	(0.0585)	(0.0551)	(0.0493)
Black partner			-0.0562	-0.0527
			(0.065)	(0.0653)
AFQT	0.00167	0.00549	-0.0154	-0.0263
	(0.0096)	(0.01)	(0.0335)	(0.036)
Age		0.00561		-0.0296
		(0.0056)		(0.0236)
High school		-0.0239		-0.112*
		(0.0211)		(0.0614)
College		-0.0284		0.00564
		(0.0256)		(0.106)
Experience		0.00707		0.0379
		(0.0096)		(0.0403)
Experience^2		-5.74E-05		-0.00345
		(0.0009)		(0.0047)
Urban		-0.0456***		-0.0131
		(0.0152)		(0.0702)
Observations	723	723	309	309

Note- The dependent variable is match- indicating whether the employee shares the same racial background as the supervisor. Partner is defined as the husband, wife, lover or dating partner. ***p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses.

example, if a black employee is connected to the black social network only, and there are only few black employers and many white employers, he is likely to work for a white employer. At the same time, that employee is more likely to work for a black employer than a black employee whom is connected to the white social network. This is evidence that for black employees, the spouse race correlates with *match*.

Analysis of the Wage Regression

Throughout the paper, I report two specifications for each subsample: the short specification, which includes only variables that are clearly exogenous, and the long specification, which controls for education, experience and an urban indicator. I prefer the short specification over the long specification since it should provide the cleanest approach and it does not contain variables that could be contaminated by discrimination, such as postsecondary education.

Table 4 presents the ML Heckman results for the entire sample. In the short specification, the coefficient of AFQT in the match subsample is positive and statistically significant (0.119) and is 56% greater in magnitude than for the mismatch sample (0.0762).[18] In the long specification, the effect of AFQT in the match subsample is positive and statistically significant, but is lower in magnitude (0.0825) than the effect in the short specification. In the mismatch subsample, the effect of AFQT is small

[18] In the test of the hypothesis that the marginal effects of AFQT are equal for both subsamples, the test statistic is $F(1, 1308) = 1.82$, which suggests that the null hypothesis cannot be rejected with the 5% or 10% certainty.

and is statistically insignificant.[19] This suggests that the wage in the match sample is explained by an unobserved correlate-of-productivity (AFQT) and observed variables of productivity (education), whereas in the mismatch sample, the wage is explained primarily by observed variables of productivity.

[19] In the test of the hypothesis that the marginal effects of AFQT are equal for both subsamples, the test statistic is $F(1, 1308) = 3.00$, which suggests that the effects are different at the 10% significance level.

Table 4: The Effect of AFQT on Log(Wage); Entire Sample

	Short		Long	
Variable	Match	Mismatch	Match	Mismatch
AFQT	0.119***	0.0762***	0.0825***	0.0267
	(0.0165)	(0.0262)	(0.0176)	(0.028)
Black	0.105	-0.185	0.0829	-0.0601
	(0.0806)	(0.2)	(0.0873)	(0.257)
Hispanic	0.235**	-0.08	0.217*	0.054
	(0.119)	(0.221)	(0.131)	(0.293)
Age	0.0527***	0.0547***	0.0465***	0.0322**
	(0.0109)	(0.0185)	(0.0110)	(0.0163)
High School			0.0696**	0.0169
			(0.0330)	(0.0427)
College			0.181***	0.329***
			(0.0438)	(0.067)
Experience			0.054***	0.0672
			(0.0131)	(0.047)
Experience^2			-0.0047***	-0.0023
			(0.0009)	(0.007)
Urban			0.0436	-0.053
			(0.0316)	(0.0609)
Constant	1.373***	1.344***	1.339***	1.582***
	(0.272)	(0.507)	(0.267)	(0.508)
λ	-0.226	-0.0067	-0.2039	0.062
	(0.0767)	(0.1264)	(0.0877)	(0.1698)
Uncensored N	857	452	857	452

Note- The data is taken from NLSY97 and includes the oversample of black and Hispanic and contains 1,309 individuals. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. All results are obtained using ML Heckman Sample selection correction technique. AFQT is normalized and age adjusted in 3-month cohorts. Robust standard errors in parentheses. λ is the coefficient on the inverse Mills Ratio.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4 reveals two additional interesting findings. The first is the difference in the returns to having a college degree between match and mismatch employees. The magnitude of the marginal effect of the dummy variable *college degree* is greater in the mismatch sample than in the match sample.[20] This supports the conclusion that the wage paid to mismatch employees is explained more by observable indicators of productivity relative to match employees.

The second interesting result relates to the argument made by Neal and Johnson (1996). When I do not control for employer type, as in Neal and Johnson's analysis, AFQT explains only 43% of the racial wage gap (Table 2). When I examine the match subsample alone, however, the coefficient on *black* dummy is positive and insignificant and the coefficient on *Hispanic* dummy is positive and significant. This suggests that AFQT explains the entire racial wage gap when employees are of the same race as their employers.

Analysis of the Wage Regression by Employee's Race

The results in Table (4) assume that the marginal effect of AFQT is the same for black and white employees. In Table (5), I instead look at the wage regression by race. Specifically, I regress log wage on AFQT and other controls for white employees and black employees, separately.

[20] In the test of the hypothesis that the coefficients of college education are equal for both subsamples, the test statistic is $F(1,1308) = 3.50$, which suggests that the effects are different at the 6% significance level.

White employees (Panel 1) - The marginal effect of AFQT is positive and statistically significant for match employees (0.118 in the short specification and 0.0846 in the long specification). For mismatch white employees, however, the marginal effect is close to zero in the short specification, is negative in the long specification and is statistically insignificant in both specifications.[21] These results partially support the theoretical model. As the theory predicts, the marginal effect of AFQT is greater in the match subsample than in the mismatch subsample. However, AFQT does not have a significant effect on the wage for mismatch white employees. One explanation could be that mismatch white employees are employed largely in low skill jobs. For low skill jobs, the employee's skill level is not as crucial as for high skill jobs. Therefore, the wage for a white employee that selects himself into a mismatch employer is explained mainly by observable variables of productivity.

Black employees (Panel 2) - In the short specification, the marginal effect of AFQT for the match subsample is positive and statistically significant (0.146) and is greater in magnitude than the marginal effect for the mismatch subsample (0.113), which is also statistically significant. However, in a hypothesis test, I cannot reject the hypothesis that the AFQT coefficient for the match subsample is equal to that in the mismatch subsample. The results from the long specification do not show a significant marginal effect of AFQT in either subsample: match or mismatch.

When I compare the results for black and white employees in each subsample separately, I find that the marginal effect of AFQT for match employees is statistically

[21] In the test of the hypothesis that the marginal effects of AFQT are equal for both subsamples, the test statistic is $F(1, 722) = 4.21$, which suggests that the effects are different at the 5% significance level.

Table 5: The Effect of AFQT on Log(Wage) by Employee's Race

Panel 1: White Employees (N=723)

	Short		Long	
Variable	match	mismatch	match	mismatch
AFQT	0.118***	0.0287	0.0846***	-0.107
	(0.018)	(0.0668)	(0.0196)	(0.103)
Age	0.0494***	0.0848	0.0445***	-0.00884
	(0.0122)	(0.059)	(0.0122)	(0.0564)
High School			0.0972**	0.106
			(0.0389)	(0.185)
College			0.181***	0.487**
			(0.0484)	(0.191)
Experience			0.0505***	0.263**
			(0.0143)	(0.122)
Experience^2			-0.00463***	-0.0354**
			(0.001)	(0.0166)
Urban			0.0434	0.0416
			(0.0337)	(0.216)
Constant	1.463***	0.955	1.395***	1.345
	(0.304)	(1.374)	(0.296)	(1.087)
λ	-0.2915	-0.1791	-0.2528	-0.1207
	(0.0628)	(0.1726)	(0.0779)	(0.2057)
Uncensored N	680	43	680	43

Panel 2: Black Employees (N=309)

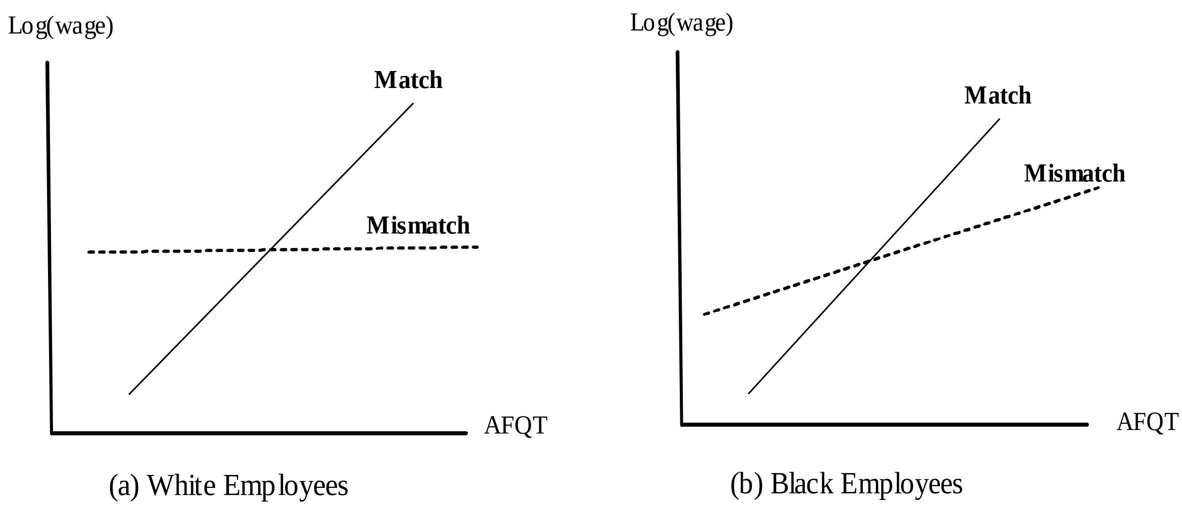
	Short		Long	
Variable	match	mismatch	match	mismatch
AFQT	0.146**	0.113**	0.0755	0.053
	(0.0636)	(0.0443)	(0.0565)	(0.0459)
Age	0.0666**	0.0969***	0.0327	0.0570**
	(0.0293)	(0.0294)	(0.0292)	(0.0275)
High School			0.0462	0.051
			(0.0914)	(0.0755)
College			0.421***	0.421***
			(0.13)	(0.131)
Experience			-0.0318	0.0223
			(0.044)	(0.0626)
Experience^2			0.0079**	0.00451
			(0.004)	(0.0081)
Urban			0.273***	-0.107
			(0.0918)	(0.0762)
Constant	1.049	0.341	1.571**	1.235*
	(0.703)	(0.73)	(0.68)	(0.699)
λ	0.4119	-0.3739	0.4453	-0.387
	(0.1933)	(0.1485)	(0.1806)	(0.1099)
Uncensored N	115	194	115	194

Note- The data is taken from NLSY97 and includes the oversample of black and Hispanic. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. AFQT is normalized and age adjusted in 3-month cohorts. Panel 1 is restricted to white employees and Panel 2 is restricted to black employees. All results are obtained using ML Heckman Sample selection correction technique. λ is the coefficient on the inverse Mills Ratio. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

equivalent for black and white (the statistic is $F(1, 1031) = 0.23$). However, the marginal effect of AFQT for blacks in the mismatch sample is statistically different from whites at the 10% significance level (the statistic is $F(1, 1031) = 2.83$).

To conclude, the results suggest that the marginal effect of AFQT is positive for both black and white employees. In the mismatch sample for black employees, the marginal effect of AFQT is positive and lower in magnitude than the match sample. In the mismatch sample for white employees, AFQT has no significant effect on the wage. I draw the results in Figure 2.

Figure 2: The Effect of AFQT on Log(wage) by Match



Implication II: Employee Skill Predicts Employer-Employee Racial Match

To test the second prediction, I use a probit regression to estimate the probability that an employee and an employer share the same race, where the independent variables are AFQT, education, urban dummy and experience. The objective is to observe whether employees select themselves into employers based on AFQT, as Proposition 2 suggests, if there are enough match and mismatch employers to accommodate the labor supply.

Table 6 presents the average marginal effects using a probit regression. The estimated marginal effect of AFQT is almost zero and insignificant (column 1). However, statistical discrimination maybe more likely to occur in occupations where the skill level is important but hard to observe. Therefore, in column 2, I estimate the effect of AFQT for employees in white-collar occupations[22] using a difference-in-difference technique.[23] The results suggest that for an employee that is employed in a white-collar occupation, an increase of one standard deviation in the AFQT score increases the probability of working for a match employer by 4.9% more than for an employee that is employed in a non-white-collar occupation. In columns 3,4 and 5, I estimate the probit regression for each race. The estimated marginal effect of AFQT is statistically insignificant for either race, even for employees in the white-collar occupation.

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[22] This is not to say that the signal is not important in blue-collar occupations, but that the effect of statistical discrimination might be more substantial in white-collar occupations.

[23] As proposed by Ai and Norton (2003), the marginal effect of the interaction term White-Collar*AFQT was calculated according to the following:

$$\begin{aligned} F(match) &= \Phi\left(\beta_1 X_1 + \beta_2 X_2 + \beta_{12} X_1 X_2 + \beta X\right) \\ \frac{\Delta \frac{\partial F(match)}{\partial X_1}}{\Delta X_2} &= (\beta_1 + \beta_{12})\phi\left((\beta_1 + \beta_{12})X_1 + \beta_2 + \beta X\right) - \beta_1 \phi\left(\beta_1 X_1 + \beta X\right) \end{aligned}$$

Where $X_1 - AFQT$, $X_2 - White\text{-}Collar$ and $X = other\ controls$

Table 6: Probit Model: Marginal Effect of AFQT on the Probability of Match.

	All		White	Black	Hispanic
Variables	(1)	(2)	(3)	(4)	(5)
AFQT	0.000799	-0.0135	-0.00415	-0.0339	-0.00884
	(0.0153)	(0.0158)	(0.0113)	(0.0389)	(0.0297)
White-Collar	-0.00491	-0.0248	-0.00877	-0.088	-0.110*
	(0.0386)	(0.0408)	(0.0253)	(0.103)	(0.0607)
White-Collar		0.0488*	0.0462	-0.0542	-0.004
x AFQT		(0.0276)	(0.0356)	(0.115)	(0.0559)
High School	-0.0557*	-0.0522*	-0.025	-0.123**	-0.0231
	(0.0286)	(0.0281)	(0.0205)	(0.0619)	(0.0508)
College	-0.0299	-0.0455	-0.0298	0.0714	-0.0285
	(0.0382)	(0.0394)	(0.0244)	(0.122)	(0.0873)
Urban	-0.0708***	-0.0702***	-0.0459***	-0.0387	0.0656
	(0.0257)	(0.0255)	(0.0145)	(0.0731)	(0.0644)
Black	-0.645***	-0.648***			
	(0.0339)	(0.0341)			
Hispanic	-0.774***	-0.774***			
	(0.0256)	(0.0257)			
Observations	1,309	1,309	723	309	277

Note- The data is taken from NLSY97 and includes the oversample of black and Hispanic. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. All equations control for blue collar occupation, age and experience in quadratic form. AFQT is normalized and age adjusted in 3-month cohorts. Standard errors in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

When estimating the marginal effect of a high school degree, a variable that is easily observed by all employers, the marginal effect is negative and statistically significant. This suggests that employees select themselves based on easily observable characteristics of productivity. An educated employee is more likely to work for a mismatch employer, which implies that the level of skill in the jobs offered by match and mismatch employers is different. In addition, the results suggest that a black or Hispanic employee is more likely to work for a mismatch employer regardless of his AFQT. These results are not surprising, since in the sample, 94% of the white are match employees, while only 30% of the black and Hispanic are match employees.

The results from the probit regressions suggest a small selection based on AFQT for an employee in the white-collar occupation, driven by the white employees. However, as noted above, a black or Hispanic employee is more likely to work for a mismatch employer and a white employee is more likely to work for a white employer, regardless of AFQT. Putting together the results from section IV.1 and IV.2, I conclude that an employer statistically discriminates less against a match employee, but employees do not select themselves into employers based on AFQT. This suggests that a high skill minority employee works for a mismatch employer even thought the return from a match employer is higher, just because he cannot find a match employer.

Discussion

In this section, I first discuss differences between the theory and the empirical evidence and then discuss alternative explanations. Subsequently, in section V.1 I test the robustness of the results to firm size and union membership.

To compare the theoretical predictions to the empirical evidence, one can simply compare Figure 1 and Figure 2. That is, the slope of AFQT for match employees is bigger than the slope of AFQT for mismatch employees for both white and black employees. However, the slope of AFQT for mismatch white employees is zero, and the estimated coefficients on AFQT for match and mismatch black employees are noisy. One might argue that the results are driven by a "taste for discrimination" Becker (1971) rather than statistical discrimination. However, if white employers have a taste for discrimination, then the effect of AFQT on wage for match white employees should be identical to that for mismatch white employees and the only difference would be in the intercept, which is not observed in the analysis.

The probit model provides additional evidence to explain the differences between the theory and the empirical results. From the probit results and the summary statistics in the appendix, one can note that white employees rarely work for mismatch employers, while black and Hispanic employees are more likely to work for mismatch employers, regardless of AFQT.[24] Using the notation in the theoretical model, this implies that, in some places, $\pi_{white} \rightarrow 1$ and $\pi_{black} \rightarrow 0$. That is, minority employees

[24] Robb and Fairlie (2007) provide evidence that blacks have substantially lower levels of start-up capital, which is correlated with less business formation. In addition, Blanchflower et al. (2003) report evidence that black owned small businesses are about twice as likely to be denied credit. Both papers provide evidence that blacks have fewer opportunities for becoming employers.

with high AFQT scores hardly receive wage offers from match employers. Therefore, they have to accept a low wage offer from a mismatch employer. Even if a black employer is better able to assess a high skill black employee's signal, which is supported by the fact that the magnitude of the coefficient of AFQT is 30% greater for match employees, there are not enough black employers offering high skill jobs to accommodate all of the black workers. The effect of AFQT in the mismatch black group reflects high skill black workers' employment in higher paying jobs than low skill black workers, mitigating the difference in the marginal effect of AFQT between the match and mismatch workers.

The shortage of black employers also explains why the effect of AFQT on wage for mismatch white employees is statistically zero. The high skill jobs offered by black employers are more likely to be filled by black employees. A low skill white employee looks for a black employer that cannot accurately read his skill level. However, the low probability of receiving a wage offer from a black employer, force the low skill white employee to accept a lower wage offer from a white employer. This is shown in the low number of white employees that are employed by black employers (about 3%). Therefore, the group of mismatch white employees is too small to make any statistical inference (which is also supported by the high standard error of the coefficient of AFQT for mismatch white employees).

Robustness to Firm Size and Union Effect

In this section, I test the sensitivity of the results to two effects that might bias the results. The first potential problem occurs if the supervisor does not take part in the wage/hiring decision. This might occur in big firms, where the direct supervisor might have little influence. Therefore, I test whether the results are robust to firm size.

In the NLSY97, each responder has been asked to provide information about the work place, including the number of employees working for the firm. I use this information to create a variable *big*, which indicates whether the number of employees in the firm is greater than the median number of employees in a firm in the entire sample (50 employees). Then I estimate the same set of regressions presented in table 4 using a difference-in-difference approach for AFQT and *big*. This specification separates the effect of firm size from the effect of employee skill level on the employee's decision to work for a match or a mismatch employer.

Table 7 presents the results. For the match subsample, firm size has no significant effect on the estimated wage. In addition, the effect of AFQT does not vary between big and small firms. For the mismatch subsample, the effect of AFQT in big firms is not significant. However, the dummy *big* is significant at the 10% level in the short specification. Therefore, the results are robust to firm size, because in big and small firms, the effect of AFQT is bigger in the match subsample than in the mismatch subsample.

The second potential problem is the influence of unions. A few studies have emphasized the effect unions have on productivity (see Hirsch (2004) for a summary). For example, Card (1990) argues that for most union members, wages are predetermined and, therefore, do not change as a result of a higher signal. Since high signal employees are more likely to be hired in the first place, however, AFQT is expected to have a positive marginal effect on the probability of match. To test whether the wage results are sensitive to union membership, I control for an indicator variable *union* and the interaction between *union* and AFQT together, with the independent variables as shown in Table 4.

Table 8 presents the results. A union member's wage is about 20% greater than a non-union member's wage regardless of the supervisor's race and AFQT. However, the marginal effect of AFQT is not significantly different for union members. That is, the effect of AFQT for union and non-union employees is bigger in the match subsample than in the mismatch subsample. Therefore, the results are robust to union members.

Table 7: ML Heckman Sample Selection Model: The Effect of Firm Size

	Short (N=1,309)		Long (N=1,309)	
Variables	Match	Mismatch	Match	Mismatch
Black	0.0943	-0.429	0.0811	-0.117
	(0.0818)	(0.458)	(0.0849)	(0.491)
Hispanic	0.228*	-0.378	0.219*	-0.0157
	(0.120)	(0.521)	(0.127)	(0.570)
Age	0.0521***	0.0580***	0.0453***	0.0333**
	(0.0109)	(0.0196)	(0.0110)	(0.0165)
AFQT	0.112***	0.0341	0.0779***	-0.0102
	(0.0234)	(0.0388)	(0.0244)	(0.0354)
AFQT x Big	0.0121	0.0587	0.00746	0.0574
	(0.0309)	(0.0443)	(0.0306)	(0.0410)
Big Firm (# employees>50)	0.0455	0.0871*	0.0365	0.0745
	(0.0310)	(0.0512)	(0.0307)	(0.0540)
High School			0.0716**	0.0214
			(0.0329)	(0.0426)
College			0.183***	0.325***
			(0.0437)	(0.0658)
Experience			0.0550***	0.0598
			(0.0130)	(0.0471)
Experience^2			-0.00464***	-0.00133
			(0.000930)	(0.00704)
Urban			0.0435	-0.0589
			(0.0318)	(0.0814)
λ	-0.2234	-0.182	-0.208	0.0228
	(0.0785)	(0.3067)	(0.0842)	(0.3427)
Uncensored N	857	452	857	452

Note- Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

λ is the coefficient on the inverse Mills Ratio.

Table 8: ML Heckman Sample Selection Model: The effect of union members employees

	Short (N=1,309)		Long (N=1,309)	
Variables	Match	Mismatch	Match	Mismatch
Black	0.0870	-0.218	0.0711	-0.0570
	(0.0827)	(0.317)	(0.0861)	(0.352)
Hispanic	0.248**	-0.112	0.236*	0.0646
	(0.124)	(0.362)	(0.130)	(0.406)
Age	0.0533***	0.0493***	0.0470***	0.0284*
	(0.0108)	(0.0181)	(0.0109)	(0.0166)
AFQT	0.121***	0.0758***	0.0831***	0.0262
	(0.0173)	(0.0279)	(0.0187)	(0.0288)
AFQT x Union Member	0.0384	-0.0167	0.0414	-0.0232
	(0.0597)	(0.0603)	(0.0587)	(0.0492)
Union Member	0.222***	0.209***	0.211***	0.192***
	(0.0541)	(0.0627)	(0.0537)	(0.0581)
High School			0.0589*	0.0210
			(0.0326)	(0.0418)
College			0.178***	0.334***
			(0.0436)	(0.0670)
Experience			0.0554***	0.0596
			(0.0131)	(0.0444)
Experience^2			-0.00469***	-0.00145
			(0.000941)	(0.00642)
Urban			0.0397	-0.0649
			(0.0314)	(0.0660)
λ	-0.2324	-0.0387	-0.2171	0.0535
	(0.0804)	(0.2131)	(0.0861)	(0.2383)
Uncensored N	857	452	857	452

Note- Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

λ is the coefficient on the inverse Mills Ratio.

Conclusion

This paper tests whether match employers statistically discriminate less against match employees based on the premise that match employers are better able to communicate with match employees. I first derive empirical predictions using a basic model of statistical discrimination. Then, I estimate the wage regression using the Heckman sample selection technique and find some evidence that employers statistically discriminate less against match employees. I then test the effect of AFQT on the selection of employees to match or mismatch employers using a probit regression. From the probit regressions, I conclude that there is a small selection of employees into match employers driven only by white employees.

Putting together the two main conclusions, I find evidence that not all employers behave the same. In particular, match employers put more weight on the correlate-of-productivity than mismatch employers. In addition, I find evidence that the shortage of black employers contributes to the racial wage gap for men. Therefore, policies that are meant to reduce inequality across race, such as affirmative action, should consider the race of the employer.

Appendix

A. Proof of Equation (1)

Proof. We know $\theta_i = \mu + \varepsilon_i$, where $\mu \sim N(m, \gamma^2)$ and $\varepsilon_i \sim N(o, \sigma_i^2)$.

The employer estimates the worker's ability based on the observed signal. Denote $\hat{\mu}_i = \widehat{E[\mu|\theta_i]}$ as the estimated ability, conditional on the worker's signal and the match type $i \in \{s, d\}$. Therefore,

$$\hat{\mu}_i - m = E[\mu - m|\theta_i] + u = \alpha_i(\theta_i - m) + u,$$

where u is the error term from the employer's estimation. The coefficient α_i can be written as

$$\alpha_i = \frac{cov(\mu - m, s_i - m)}{var(s_i - m)} = \frac{cov(\mu, s_i)}{Var(s_i)} = \frac{\gamma^2}{\gamma^2 + \sigma_i^2}.$$

Therefore,

$$\hat{\mu}_i - m = \frac{\gamma_i^2}{\gamma^2 + \sigma_i^2}(\theta_i - m)$$

and

$$\hat{\mu}_i = \frac{\gamma^2}{\gamma^2 + \sigma_i^2}\theta_i + \frac{\sigma_i^2}{\gamma^2 + \sigma_i^2}m.$$

■

B. Data Collection

The sample consists of 4,599 men responders from 2007, the most recent year available to date. To be included in the sample, responders must be either white, black or Hispanic, but not biracial (208 fewer). I drop all responders who did not take the ASVAB tests used to calculate the $AFQT$ score (964 fewer). In addition, responders must not be enrolled in school (419 fewer).

I consider only current employment data. Wages used are hourly rates of pay, either calculated or reported. I consider only responders that have reported a supervisor that is not biracial and is either white, black or Hispanic (1,577 fewer). I consider only full time employment, so workers must work at least 30 hours per week (80 fewer). If, in a particular year, a responder reported more than one job, I consider only the job with the highest hours worked per week. If two jobs had identical hours, I considered the most recent job. In addition, wage per hour must be calculable (80 fewer). To eliminate outliers, I consider only wages between \$2 and \$100 per hour (33 fewer). In addition, experience must be reported (13 fewer). Using these criteria, I am left with a cross-sectional sample of 1,309. The means, standard deviations and frequencies are reported in Table 1.

To produce a proper representation of the US labor market, I use sampling weights to account for differences in probability of being selected into the sample. Sampling weights are necessary in this study because I use the oversample of blacks and Hispanics.

Table 9: Data Description

Variable	Description
Wage	Real hourly wage.
Supervisor race	The reported supervisor's race. If not reported, use the reported supervisor race from previous year if employer ID is the same.
Partner match	Dummy indicator, equal to 1 if the responder share the same race as his spouse (husband, wife, lover or dating partner).
Hours worked	Number of hours worked per week
AFQT	Age adjusted and normalized AFQT.
Match	Dummy indicator, equal to 1 if the supervisor's race is the same as the worker's race.
High school	Dummy indicator, equal to 1 if the highest grade completed by the year 2007 is 12.
College	Dummy indicator, equal to 1 if the highest grade completed by the year 2007 is 16 or bigger.
Experience	Number of weeks worked full time divided by 50.

Table 10: Means, Standard Deviations and Frequencies of Real Hourly Wage

		Supervisor			
		White	Black	Hispanic	Total
White Employee	Mean	16.29	16.76	15.66	16.28
	S.D.	(7.91)	(7.95)	(7.03)	(7.88)
	N	680	22	21	723
Black Employee	Mean	13.17	12.68	14.31	13.07
	S.D.	(7.82)	(7.23)	(7.71)	(7.58)
	N	173	115	21	309
Hispanic Employee	Mean	14.83	14.25	13.69	14.54
	S.D.	(7.50)	(5.94)	(6.56)	(7.21)
	N	199	16	62	277
Total Employee	Mean	15.50	13.43	14.21	15.15
	S.D.	(7.90)	(7.32)	(6.87)	(7.78)
	N	1,052	153	104	1,309

Chapter III

Gender Wage Gap: Does Employer's Gender Matter?

Introduction

Over the past several decades, the gender wage gap has shrunk substantially in the United States (Blau (1998), Even and Macpherson (1993)). Although women have come a great distance toward achieving a wage equivalent to men, the gender wage gap is still present and substantial. According to the U.S. Bureau of Labor Statistics, in 2009, women in all occupations who usually worked full time have earned a median income of 80.2 percent of the median for men.[25] Previous theories used to explain the gender wage gap include: discrimination against women, differences in skills between genders, and lower workforce attachment of women. This paper tests whether the gender of the employer influences the gender wage gap.

Empirical results from Fadlon (2010) show that the race of the employer is important to understand the racial wage gap. In particular, Fadlon uses data from the National Longitudinal Survey of Youth 1997 (NLSY97) to show that employees that share the same racial background as their employers are paid more based on their underlying skill level. The intuition is that communication is better between employer and employee if the two share the same racial background than if they do not. In this paper, I test whether Fadlon's results can be extended to the gender wage gap.

[25] See also Blau and Kahn (2006) for a recent study documenting the slow down of the gender wage convergence.

Specifically, I test whether employers who share the same sex as the employee can better read the employee's underlying skill level than employers of the opposite sex.

The data used in this paper is taken from NLSY97. NLSY97 contains detailed information about employment including information about the supervisor's gender, which is used as a proxy for the employer's gender. In addition, I use the Armed Force Qualification Test (AFQT) score as a proxy for the employee's underlying skill level. To exclude bias due to racial discrimination, the sample used in the analysis includes white employees and white supervisors only.

In the empirical section, I divide the sample into two subsamples: employees that share the same sex as the employer and employees whose sex is opposite that of their employers. Then, I regress log wage on AFQT and other controls in each subsample to test whether the coefficient on AFQT is different between the two subsamples.

The main result from the analysis suggests that there is no evidence that employers of a matching gender can read an employee's underlying skill level better than employers of an opposite gender. Unlike the results reported in Fadlon (2010), which finds that the race of the employer partially explains the racial wage gap, the gender wage gap is not affected by the employer's gender.

A potential explanation for the disparate results is that, unlike the racial wage gap, social networks are probably not gender stratified. This means that employers of one gender do not gain better information from the social networks about employees of the same gender than they do about employees of the opposite gender. Another explanation is that employer-employee communication is not actually better when it occurs within a gender than when it is occurs across genders.

The results from this paper have important implications. In particular, certain affirmative action policies that are effective to reduce the racial wage gap might not be as effective in the gender wage gap.

The next subsection discusses related research. Section 2 describes the data. Section 3 presents the estimation methodology and the results. Finally, Section 4 concludes.

Related Literature

The analysis in this paper focuses on how the correlation between a measure of skill and wage varies by employer-employee gender matches. A few other studies have looked at the gender differences in the return to skill and its effect on closing the gender wage gap. Black and Juhn (2000) argue that females have better responded to the rising skill demand in the 1980's and 1990's in the US. In the same notion, Black and Spitz-Oener (2010) show that women experienced a larger increase in non-routine interactive tasks and non-routine analytical tasks at the workplace, relative to men. Other studies looked at the supply side and documented a relative increase in supply of skilled females relative to males (see for example, Brown and Corcoran (1997) and Blau and Kahn (1997)). In addition, the existing literature suggests that relative improvements in female skills will continue to close the gender wage gap (see Shannon and Kidd (2003) for a projection of the US gender wage gap at 2000-2040). However, these papers do not consider the employer's gender as a potential source for the lower return to skill for women.

This paper is also incorporates the concept of the *glass ceiling*. In this phenomenon, women are less likely than men to hold supervisory positions (Ichino and Filippin (2005), Mitra (2003)). If, as proposed in my model, female employers observe female employees' underlysing skill level better than male employers, fewer female supervisors will mean a lower return to skill for female employees and a gap in wages.

Few studies have used a sample of employer-employee pairs. Bayard et al. (2003) matched employees to employers using the New Worker-Establishment Characteristics Database (NWECD), a part of the census. They find that women employees are segregated in lower paying occupations and industries. In addition, Korkeamaki and Kyyra (2006) do a similar analysis with a dataset of Finnish employees. Although these studies have information about the employer, they do not analyze the importance of the employer's gender, as I do in this paper, and instead focus on the differences in the occupations and industries in which men and women are employed (occupational segregation).

Data

The data is taken from the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 consists of a nationally representative sample of the United States and was collected annually from the same responders staring 1997. In this study, I use the most recent year available to date, 2007. The data was collected using the same criteria described in Fadlon (2010). In this paper, however, I include both genders and restrict the sample to only white employees and supervisors to reduce the bias from racial discrimination.[26] All regressions are adjusted using sampling weight to produce a proper representation of the US labor market. The sample used is for the year 2007 and includes 1,268 individuals, of which 680 are male and 588 are females. Table 11 represents the summary statistics for the sample used in this paper.

The average AFQT in the sample is about 0.4 standard deviations above the average AFQT in the entire sample because the sample is restricted to white responders. Women are more educated than men (14.4 vs. 13.4 years of education) and score higher, on average, in the AFQT (0.48 vs. 0.315 standard deviations above the mean). However, men earn more on average ($16.3 vs. $14.4 per hour). About 10% of the employees are union members. Most responders (75%) live in urban areas.

Table 12 presents the means, standard deviations and frequencies of wage by employer-employee gender matches. Most employees (71%) are employed by a same gender employer. Females employees are more likely to be employed by a different gender employer. This result is consistent with the literature on glass ceiling. That is, females are employed in the lower rungs of the hierarchy within a firm, but they are less likely than men to get promoted into management. Therefore, female employees are more likely to be employed by male employers because most employers are males. In addition, the wage paid to employees, male or female, is greater, on average, if the employer is a male. This may be because women, even when they have been promoted to supervisors, are more likely to be lower level supervisors of lower wages

Table 11: Summary Statistics

Variables	Men Mean	Men Std. Dev.	Women Mean	Women Std. Dev.
Education	13.39	(2.50)	14.40	(2.44)
Age	24.91	(1.42)	24.95	(1.38)
AFQT	0.306	(0.915)	0.484	(0.786)
Hours worked (weekly)	42.3	(8.4)	41.2	(6.9)
Real Hourly Wage	16.29	(7.91)	14.34	(7.32)
Experience (weeks)	138	(121)	118	(109)
Supervisor Gender	0.14	(0.347)	0.548	(0.498)
# of Employees	796	(7,064)	553	(4,500)
Union Member	0.098	(0.297)	0.1	(0.3)
Urban	0.731	(0.444)	0.75	(0.433)
N	680		588	

Note- Standard errors in parentheses.

Table 12: Means, Standard Deviations and Frequencies of Real Hourly Wage

		Supervisor Gender: Male	Female	Total
Male Employee	Mean	16.52	14.78	16.29
	S.D.	(8.17)	(5.89)	(7.91)
	N	585	95	680
Female Employee	Mean	14.98	13.81	14.34
	S.D.	(8.14)	(6.52)	(7.32)
	N	266	322	588
Total	Mean	16.04	14.04	15.38
	S.D.	(8.19)	(6.39)	(7.7)
	N	851	417	1,268

employees.

The gender wage gap is very apparent in the sample used in this paper. Figure 5 represents the wage distribution of male employees and female employees separately. Figure 4, for male employees, and Figure 3, for female employees, represent the sample distributions of AFQT by employer's gender.

Results

To test whether employer's gender is a significant contributor to the gender wage gap, I repeat the exercise from Fadlon (2010), but now I focus on the employer's gender. Equation (9) specifies the conditional expected wage used in the analysis below. The objective is to compare the coefficient on AFQT for employees that share the same gender as the employer to that of an employee with a different gender than the employer.

$$E[wage_i|X_i, AFQT_i] = \alpha_1 + \alpha_2 AFQT_i + \boldsymbol{\alpha}_3^{'}\mathbf{X}_i \tag{9}$$

I divide the sample into two subsamples: same and different employer-employee gender match.[27] Next, I compare the estimated α_2 in the same-gender subsample to that on the different-gender subsample. I first estimate the conditional expected wage for the entire sample and then I limit the sample to men and female employees only. In both cases I estimate the conditional expected wage in two specifications: a) short

[26] According to the U.S. Bureau of Labor Statistics, the female-to-male earnings ratios in 2009 were higher among blacks (93.7 percent) and Hispanics (89.5 percent) than among whites (79.2 percent) or Asians (81.8 percent).

[27] The results presented in this essay are from the OLS estimation. The results from the Heckman estimation do not change the conclusions and are available from the author upon request.

specification, where **X** is a vector containing age and a gender dummy, and b) long specification, where **X** also contains education, experience and an urban dummy.

Table 13 represents the results for the entire sample divided into two subsamples: employer-employee with the same gender (match) and employer-employee with opposite gender (mismatch). The results in the short specification indicate that the coefficient on AFQT in the match subsample is smaller than that in the mismatch sample. However, in the long specification, the results flip and the coefficient on AFQT in the mismatch subsample is greater, in magnitude only, than that in the mismatch subsample.[28]

The results when the entire sample is used do not provide a concrete conclusion. Therefore, unlike the results for the employer's race, when I use the entire sample, the gender wage gap cannot be explained by differences in the abilities of match and mismatch employers to estimate a hard to observe measure-of-productivity. In addition, the long specification reveals another interesting result. The return for a college degree is greater for an employee that shares the same sex as the employer than for an employee of the opposite sex.[29]

[28] In the test of the hypothesis that the marginal effects of AFQT are equal for both subsamples, the test statistic is $F(1, 1267) = 0.26$, which suggests that we cannot reject statistically the hyphothesis at the 10% significance level.

[29] In the test of the hypothesis that the coefficients of College are equal for both subsamples, the test statistic is $F(1, 1267) = 4.00$, which suggests that the coefficients are different at the 5% significance level.

Table 13: Log(Wage) by Employee's Supervisor

	Short		Long	
Variables	Match	Mismatch	Match	Mismatch
AFQT	0.130***	0.180***	0.0727***	0.0551*
	(0.0162)	(0.0267)	(0.0177)	(0.0297)
female	-0.207***	-0.00311	-0.229***	-0.0276
	(0.0301)	(0.0528)	(0.0296)	(0.0514)
Age	0.0497***	0.0491***	0.0426***	0.0430**
	(0.0102)	(0.0183)	(0.0104)	(0.0171)
AFQT	0.130***	0.180***	0.0727***	0.0551*
	(0.0162)	(0.0267)	(0.0177)	(0.0297)
High School			0.0543*	-0.00832
			(0.0329)	(0.0644)
College			0.254***	0.398***
			(0.0398)	(0.0608)
Experience			0.0594***	0.0662**
			(0.0126)	(0.0267)
Experience^2			-0.00520***	-0.00539**
			(0.000962)	(0.00219)
Urban			0.0343	0.0884*
			(0.0297)	(0.0532)
Constant	1.439***	1.263***	1.422***	1.142***
	(0.254)	(0.455)	(0.250)	(0.428)
Observations	907	361	907	361
R^2	0.124	0.121	0.185	0.255

Note- The data is taken from NLSY97. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. AFQT is normalized and age adjusted in 3-month cohorts. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Analysis of the Wage Regression by Employee's Gender

In this subsection, I estimate the same set of regressions as in Table 13, but I derive separate estimates for male employees and female employees. Just as in the previous section, the objective is to observe whether the coefficient on AFQT in the match-gender subsample differs from that in the mismatch-gender subsample.

Table 14 represents the results for male employees. In both the short specification and the long specification, the coefficient on AFQT in the match subsample is smaller in magnitude than that in the mismatch sample. This is in contrast to the racial wage gap. In addition, the coefficient on *college education* in the match sample is statistically greater than that in the mismatch sample.[30] These results suggest that the wage of a male employee that is employed by either male or female employer is explained by a hard to observe correlate of productivity (AFQT) and easy to observe correlate of productivity. There is no evidence, however, that for male employees, an employer of one gender can better observe the employee's underlying skill level than an employer of another gender.

Table 15 represents the results for female employees. In the short specification, the coefficient on AFQT is smaller in the match subsample than in the mismatch subsample. In the long specification, the coefficients on AFQT in the match subsample is almost identical to that in the mismatch subsample and in both subsamples, the

[30] In the test of the hypothesis that the coefficients of College are equal for both subsamples, the test statistic is $F(1, 679) = 3.07$, which suggests that the coefficients are different at the 10% significance level.

Table 14: Log(Wage) by Employee's Supervisor Male

	Short		Long	
Variables	Match	Mismatch	Match	Mismatch
AFQT	0.123***	0.156***	0.0991***	0.101**
	(0.0191)	(0.0384)	(0.0210)	(0.0470)
Age	0.0511***	0.0666**	0.0468***	0.0438
	(0.0127)	(0.0302)	(0.0131)	(0.0268)
High School			0.0810**	0.145
			(0.0397)	(0.102)
College			0.140***	0.333***
			(0.0521)	(0.101)
Experience			0.0537***	0.0739
			(0.0148)	(0.0687)
Experience^2			-0.00477***	-0.00838
			(0.000993)	(0.00713)
Urban			0.0298	0.0430
			(0.0359)	(0.107)
Constant	1.407***	0.840	1.345***	1.130*
	(0.316)	(0.754)	(0.316)	(0.657)
Observations	585	95	585	95
R^2	0.092	0.148	0.126	0.246

Note- The data is taken from NLSY97. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. AFQT is normalized and age adjusted in 3-month cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

coefficients are statistically insignificant. Therefore, just as in the male employees' sample, the results for female employees do not suggest differences in the return to AFQT based on employer's gender.

Probit Results

In this subsection, I test the selection of employees to match-gender or mismatch-gender employers based on the underlying skill level (AFQT). I use the probit estimation to estimate the marginal effect of AFQT on the probability that an employee works for a same gender employer. If a match-gender employer can better observe an employee's underlining skill level than a mismatch-gender employer, I should observe a positive coefficient on AFQT.

Table 16 presents the marginal effects from the probit regressions. The coefficient on AFQT is negative in all specifications. When I control for AFQT only (column 1), the coefficient is negative and very significant. Even when I control for easy to observe correlates of productivity (column 2) education and experience, the coefficient on AFQT is negative and significant. However, when I regress the probability of match gender for each employee gender separately, the coefficient on AFQT is insignificant for female employees and significant for male employees. These results suggest that there is no evidence that a high skill employee selects into a same gender employer just because the return for skill is greater.

Table 15: Log(Wage) by Employee's Supervisor Female

	Short		Long	
Variables	Match	Mismatch	Match	Mismatch
AFQT	0.149***	0.191***	0.0355	0.0409
	(0.0304)	(0.0344)	(0.0327)	(0.0391)
Age	0.0467***	0.0425*	0.0326*	0.0399*
	(0.0171)	(0.0226)	(0.0167)	(0.0214)
High School			-0.0373	-0.0681
			(0.0567)	(0.0817)
College			0.380***	0.420***
			(0.0600)	(0.0745)
Experience			0.0785***	0.0727**
			(0.0250)	(0.0298)
Experience^2			-0.00714***	-0.00556**
			(0.00236)	(0.00239)
Urban			0.0663	0.0911
			(0.0496)	(0.0629)
Constant	1.297***	1.418**	1.371***	1.186**
	(0.423)	(0.558)	(0.398)	(0.533)
Observations	322	266	322	266
R^2	0.091	0.116	0.261	0.268

Note- The data is taken from NLSY97.In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. AFQT is normalized and age adjusted in 3-month cohorts. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Marginal Effect of AFQT on the Probability of Match Gender.

	Entire sample			
Variable	(1)	(2)	Male	Female
AFQT	-0.0645***	-0.0424**	-0.0516***	-0.0141
	(0.0155)	(0.0178)	(0.0179)	(0.0306)
High School		0.0354	-0.00320	0.0386
		(0.0332)	(0.0324)	(0.0586)
College		-0.0535	-0.0318	0.00543
		(0.0343)	(0.0386)	(0.0523)
Experience		-0.00192	0.00931	-0.0336
		(0.0138)	(0.0153)	(0.0258)
Experience^2		0.00108	0.000391	0.00327
		(0.00138)	(0.00164)	(0.00282)
Urban		-0.00904	-0.0517*	0.0334
		(0.0298)	(0.0270)	(0.0484)
Observations	1,268	1,268	680	588

Note- The data is taken from NLSY97. In all regressions, I use sampling weights to account for differences in probability of being selected into the sample. AFQT is normalized and age adjusted in 3-month cohorts. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Putting together the results from the wage regressions and the probit regressions, I conclude that the results do not support the proposition that employer's gender matters to explain the gender wage gap in the same way that the employer's race explains the racial wage gap. However, the motivation for the importance of the employer's race to explain the racial wage gap is driven by the differences in communication and stratified social networks. Therefore, it seems as if the gender wage gap is not driven by differences in information between gender groups.

Conclusion

This paper aimed to test whether the gender wage gap is explained by asymmetric information between an employer-employee couple that shares the same sex and an employer-employee couple that does not, as the results in the racial wage gap suggest. I obtain this result by testing whether the correlation between a measure of skill which is not directly observed by employers and wage differs if the employer and the employee have the same gender than if they have opposite genders. The main result from the empirical analysis suggests that there is no evidence that an employer of one gender has more information about an employee's underlying skill level than an employer of another gender.

The results have an important implication for programs that aim to close wage gaps. In particular, programs that might work for closing the racial wage gap might not be as effective in closing the gender wage gap.

Appendix

C. Wage and AFQT Graphs

Figure 3: Kernel Density of AFQT by Employee's Gender Match: Female Employees

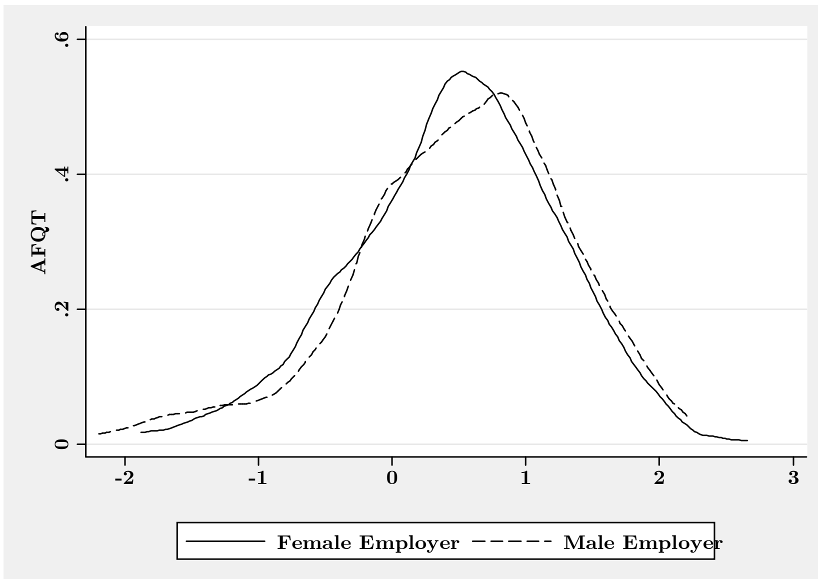


Figure 4: Kernel Density of AFQT by Employee's Gender Match: Male Employees

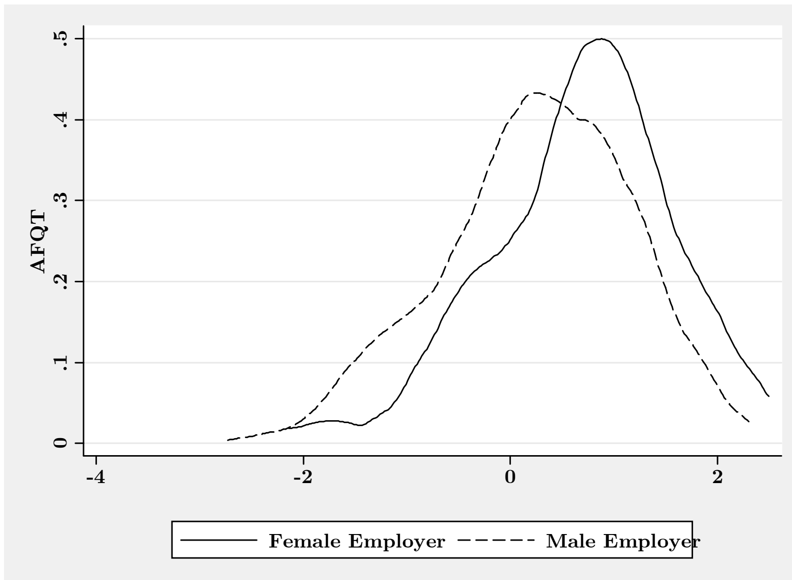
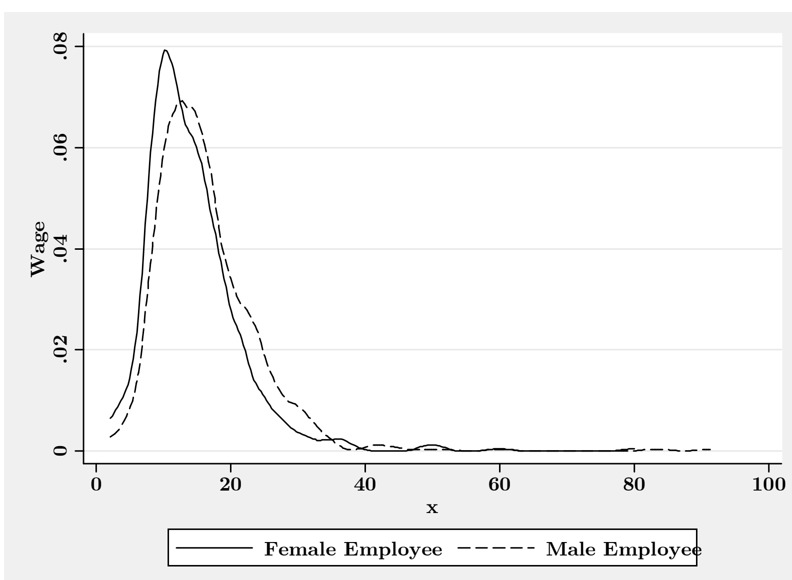


Figure 5: Kernel Density of Wage by Employee's Gender



Chapter IV

Ranking Journals; Show Me the Money

(with Myrna Wooders)

Introduction

Journal ranking plays an important role in evaluating institutions where economics is taught and also in evaluating the quality of a candidate's scholarship for hiring, tenure, and salary decisions. Different methods for ranking journals have been used in previous literature, including opinions of economists (Hawkins et al. (1973), Malouin et al. (1987), Axarloglou and Theoharakis (2003)) and citation frequency (Liebowitz and Palmer (1984), Laband and Piette (1994), Liner and Amin (2004)). In economics departments, citation based rankings are frequently used, even though they can be biased (see Palacios-Huerta and Volij (2004)). No ranking, however, has considered the economic payoff of publishing in a particular journal.

In this study, we report a novel method of rating economics journals based on salaries of economists as a function of publications. Specifically, we estimate an economist's salary, controlling for individual characteristics and indicator variables, for 36 economic journals. This method allows us to estimate the average marginal effect on conditional expected salary of publishing a paper in a particular economics journal. A higher average marginal effect may be viewed as an indicator of higher market value of the publication.

Our data comes from public universities from U.S. states that publish state employees' salaries as part of public records. We were able to collect data from 26 universities that represent different tiers and geographic regions in the United States. We restrict the sample to faculty members for whom salary is likely to be affected by numbers of publications and the journals in which these publications appear. (We exclude data from universities that do not offer a PhD program in economics, for example.)

We report on two different methods to weight a joint publication. In the first method, called *per capita weighting*, a paper that was published by n authors counts as $\frac{1}{n}th$ of a publication. In the second method, called *upweighting*, a paper by one author counts as one publication, a paper by two authors counts as $\frac{2}{3}$'s of a publication for each author and a paper with $n > 2$ authors counts as $\frac{1}{n-1}$ publications for each author.[31] We also report on two different ways to take into account the age of a publication. In the first method, called *constant value*, all publications in a given journal are treated equally, regardless of their dates of publication. In the second method, called *decreasing value*, we assume that the value of a publication decreases at the rate of inflation.[32] At the top end of our ranking, the second method leads to larger reported current worth of publications while at the bottom end, it leads to lower current worths.[33] Our ordinal rankings of journals are little affected by which

[31] This particular scheme of taking co-authored papers into account has been proposed to us by several colleagues at various institutions. It is, of course, ad hoc. With longitudinal data we could estimate the effects of numbers of co-authors on expected marginal contributions of publications to earnings.

[32] It would be desirable to estimate the rate at which the real value of a publication is affected by time, which would be possible with longitudinal data.

[33] It would be desirable to estimate the rate at which the real value of a publication is affected by time but, at this point, we do not have sufficient data.

weighting is used.[34]

Using the decreasing value method, with both per capita weighting and upweighting of co-authorships, in our ranking the top two journals are *Journal of Political Economy* and *Econometrica,* with worths between \$16,900 and \$18,800 above average salary. Using the same weighing, the bottom two journals in our sample are *Economica* and *Southern Economic Journal*, with current worths of approximately \$-6,900 and \$ -13,500. It should be kept in mind that these worths are contributions to average earnings of an economist in the sample and *should not* be interpreted as assigning negative worths to publications; that is, a faculty member that publishes mainly in these journals earns, on average, less than the average salary in the sample. As we discuss later, our ranking correlates well with citation rankings.

We focus on ranking journals but, essentially, we estimate the average monetary worth to a publication in a particular journal as it may be perceived by employers (in our case, universities). Thus our paper also relates to a number of articles that estimated the monetary value to a publication. Older literature that used a national survey of faculty conducted between 1972 and 1973 by the American Council of Education (ACE), use a limited sample that does not distinguish between journal qualities (Tuckman and Leahey (1975)). A more recent literature estimated the return to a citation (Diamond Jr (1986), Baser and Pema (2003)). However, estimating the return to a citation does not distinguish between journals, but between articles.

[34] We have also, in an unreported study, taken into account fields within economics. Again, this leaves our journal ranking relatively unaffected.

Data

The data for our study consists of 597 economics faculty from 26 economics departments across the U.S. The data was collected from three different sources and contains information about faculty members' characteristics, publications and annual salary. Individual characteristics such as the school of appointment, the date in which the faculty member earned his PhD, position, gender and fields of interest were collected from economics departments' websites. Detailed individual faculty publications, including date, number of authors and journals' names were collected from the website Econlit. Salary was collected from states that publish state employees' salaries as part of revealing financial information.[35] As a result, we focus on public universities.[36]

The salary used in this study is gross pay for the academic year 2008-2009. We restrict the sample to faculty members for whom publications are likely to have major effects on salary. We cannot directly control for the influence of teaching responsibilities on salary since we do not have a good measurement of the monetary rewards to teaching. However, we restrict the sample to faculty members who do not hold lecturer positions and we consider only universities with a PhD program (research schools). About one third of the faculty members in the sample are employed in schools that are ranked, according to the National Research Council (NRC) 1995, in the top 20 universities in the United States.

[35] We did not take into account more than one year since information was not publically available for all universities in the sample for the prior year(s).

[36] It would of course be desirable to have data from private universities, but we do not. Nor does it seem likely that we will be able to obtain data

The salary immediately after receiving a PhD is primarily a function of the potential to publish and other individual characteristics such as the school in which the faculty member earned his PhD. For this reason, we exclude faculty who received a PhD after 2006. In addition, the Econlit database only contains publications since 1969. Therefore, our sample includes faculty members who earned a PhD in economics between the years 1969 and 2006. To eliminate outliers, only individuals earning at least $60,000 per year were considered.[37] In addition, since the salary is for the academic year 2008-2009, we include only papers published through 2008. In addition, we excluded faculty who hold a dean position.

Table 17 represents the summary statistics for the sample used in this study. A more detailed summary statistics table is available in the appendix. Gross salary varies widely across faculty members with mean of around one hundred and fifty-one thousand dollars and standard deviation of around sixty-two thousand dollars. The majority of the sample consists of full professors (58%) mainly because most assistant professors do not pass the criteria discussed above. The average number of publications is twenty-one with a standard deviation of twenty, where twenty-eight faculty members in the sample have zero publications. These faculty members are all assistant professors and earned a PhD between the years 2004 and 2006.

[37]■ We lose 8 faculty members. The reason we eliminate only faculty from the lower tail of the wage distribution is that for those faculty members, publications are less likely to determined salary.

Table 17: Summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
9-month salary	151,630	(62,010)	60,685	482,666
year received PhD	1989	(11.377)	1969	2006
# of publications	21.22	(20.272)	0	146
full professor	57.8%	(0.494)	0	1
associate professor	17.6%	(0.381)	0	1
assistant professor	24.6%	(0.431)	0	1
experience	18.48	(11.38)	2	39
chair	0.04	(0.197)	0	1
N		597		

Methodology and Results

We rank journals in which at least 7% (40 individuals) of the faculty in the sample published. This criterion allows for variations within a journal. We then use a simple OLS method according to the following specification[38]:

$$wage_i = \beta_0 + \beta_1 X_i + \sum_j \beta_j Publication_{ij} + \varepsilon_i \qquad (10)$$

where $wage_i$ is the annual gross salary for the academic year 2008/2009, X_i - is an individual characteristics vector that includes experience in quadratic form, an indicators for chair, a gender dummy variable, indicators for rank (associate, full professor) and the rank of school in which the faculty member earned his PhD according to the NRC 1995, divided into 5 tiers.

[38] This method is essentially the hedonic pricing method, since we observe the salary but not the marginal contribution of a publication to the salary.

$Publication_{ij}$ is the sum of faculty i publications in journal j. We report two different weight methods for a publication: *per capita weighting* and *upweighting*. We prefer the second method, since we found that faculty members, with whom we discussed the paper, tend to evaluate a publication according to the second method.[39]

We take account of the age of a publication by assuming that the return to a publication in journal j is depreciating according to the consumer price index (CPI). For example, if a publication in *AER* is worth $10,000 today, then the increase in salary to a ten-year-old publication in AER is the value of $10,000 today evaluated ten years earlier according to the CPI. This means that a publication that is t years old, accounts for $\frac{1}{1+\pi(t)} Publication_{ij}$ where $\pi(t)$ accounts for the inflation during the past t^{th} years.[40]

We do not control for the number of pages in an article, as has been done in previous literature that ranked journals based on citation frequency. The reason is that multiplying by the number of pages on $Publication_{ij}$ would imply that the return to a ten page publication in a journal X, for example, is twice as much as the return to a five page publication in the same journal. We see no reason to suppose that this is the case[41].

Tables 2 and 3 present the results, where the main method, *upweighting*, is in

[39] These faculty members are all professors and have experience on committees evaluating the performance of other faculty members in their own departments.

[40] Roughly, the situation we have in mind is when a faculty member publishes a paper, he either receives a merit increase in salary or a job offer from another university, leading to a either a raise in salary at his current job or a higher salary at the competing institution. This, of course, is only "on average". It may take several publications before the faculty member receives a merit increase or a job offer. Or universities may go through different budgetary climates, leading to money available for merit increases in some years than in others. To the extent the universities implement across-the-board percentage-based salary increases to make cost-of-living increases, the decreasing-value approach will over-estimate the effects of inflation and lead to more variation in values of publications in journals.

[41] It seems to us that one can argue either way.

Table 2 and the second method, *per capita weighting*, is in Table 3. Notice that the coefficient for some journals is negative and the constant term is relatively high at more than one hundred thousand dollars. We stress that a negative sign does not mean that the return to an additional publication in these journals is negative but, instead, that faculty members who publish mainly in these journals earn less on average than faculty members who publish mainly in journals with positive coefficients, after controlling for other variables as described in equation (10). In addition, some coefficients are statistically insignificant. However, this is not a problem for the purpose of our study since we care only about the average marginal effect; when it occurs, the statistical insignificance is due to high standard deviations which suggests wide variation in marginal values of contributions to some journals (*Quarterly Journal of Economics*, for example).

Of the top ten journals, five journals do not surprise us: *Econometrica, Journal of Political Economy, American Economic Review, Review of Economics and Statistics* and *Review of Economic Studies.* One journal that placed unexpectedly low on the list at number 13, is *Quarterly Journal of Economics.* However, for this journal the standard error is large, which suggests that, even though the average marginal effect is relatively low, the marginal effect varies substantially across faculty members in our sample.

The top two journals in both weighting methods for co-authorships are *Econometrica and Journal of Political Economy.* In these two journals the return to a publication is between $16,900 and $18,800 above the average salary, holding all other characteristics the same. A publication in the journal *American Economic Re-*

view adds around \$9,000 above the average salary, holding all other characteristics constant. In this respect, our ranking differs from others which place *American Economic Review* at the top of the ranking (Liebowitz and Palmer 1984, Kalaitzidakis et al. 2003, Hawkins et al. 1973).

Table 4 presents the results for the same specification as in Tables 2 and 3, but without adjustment to CPI. This specification is accurate if the salary increment is constant over time; that is, if a publication ten years ago worths the same as a publication in the same journal today. In most universities, that is not the case. In some institutions, however, salary is linked to the CPI, so the salary increment for an older publication may be constant over time in real value.[42]

Publications might be rewarded with tenure appointments. That is, a faculty member who does not have a tenure appointment is more likely to accept a lower salary for a tenure appointment, either in the current placement or in a new institution. To address this concern, we restrict the sample to faculty members who are professors or associate professors. We find that this hardly changes the ranking. Specifically, among the top ten journals in the restricted sample, nine are included in the top ten journals in the full sample. In addition the rank correlation between the full sample to the sample of associate and full professors is 86%.

The ranking is fairly consistent with previous ranking based on citations, even though the methods are very different. The rank correlation between our ranking and the ranking reported in Palacios-Huerta and Volij (2004) is 75% and with the

[42] With a larger, longitudinal data set we could estimate the effects of time on the value of a publication. We plan to do such estimation with another data set.

Ritzberger (2008) ranking it is 61%. This suggests that journals that are ranked high in citation based ranking are likely to be high in our ranking and vice-versa.

Conclusions

Some considerations may be important in interpreting the data and our results. Our research aims to estimate the marginal work of a publication in a particular journal in terms of *the market* for academic economists. Probably the first mission of an most academic economists is to earn a living. After some "reasonable" income (or tenure) is assured, many researchers may pursue their interests without regard to whether they are working in a fashionable area of economics. Some work may be deep, difficult, and in an area only appreciated by a small percentage of economists. This work may not have a high market value, but may still be highly regarded by eminent researchers in economics. The market value of a publication is just that; it should not be viewed as representing the intrinsic scientific worth of a publication. (In our view, the same can be said of citations. Some papers appear to be widely cited because they are much attacked, for example.) And we agree with those who think that, in evaluating a publication, especially in ones own area, there is no substitute to reading it oneself.

Our approach to ranking economics journals would benefit from more data and we plan to continue to collect data as it becomes available. In principle, with enough years in the sample, we could estimate the average effects of age on the value of a publication in each journal. Another avenue, which we intend to pursue, is to estimate the effects of quantity of publications on earnings. We could, for example, derive some one-dimensional measure of publications and estimate a relationship between the numbers of publications and the income (salary) distribution of academic economists.[43]

[43] This possibility was inspired by Albarrán, Ortuño, and Ruiz-Castillo (2009).

Table 18: Journal Ranking, Decreasing Value: Upweighting

Rank	Journal	Coef.	Std. Err.	# Faculty
1	Econometrica	18,712	(5,692)	174
2	Journal of Political Economy	18,381	(5,657)	165
3	Journal of Monetary Economics	10,414	(3,853)	109
4	Review of Economic Studies	9,177	(5,621)	147
5	American Economic Review	9,167	(2,553)	290
6	RAND	8,451	(4,453)	75
7	Journal of Human Resources	8,441	(4,006)	64
8	Review of Economics and Statistics	8,396	(4,787)	158
9	Journal of Development Economics	7,546	(4,863)	55
10	Journal of Urban Economics	6,027	(3,000)	41
11	Economic Theory	5,772	(4,454)	92
12	Journal of Econometrics	5,636	(2,343)	99
13	Quarterly Journal of Economics	4,795	(4,867)	127
14	J of Economic Literature	4,060	(8,852)	55
15	J of Economic Theory	4,057	(3,663)	145
16	J of Business and Economic Statistics	3,943	(7,840)	55
17	J of Economic Perspectives	3,795	(5,017)	79
18	J of Applied Econometrics	3,279	(6,791)	51
19	European Economic Review	2,675	(6,164)	75
20	Journal of Public Economics	2,012	(2,296)	119
21	Journal of Money, Credit, and Banking	349	(5,226)	76
22	Economic Journal	-255	(6,273)	92
23	Games and Economic Behavior	-266	(3,230)	55
24	Canadian Journal of Economics	-995	(6,331)	56
25	Journal of Mathematical Economics	-1,176	(5,643)	46
26	J of Economic Behavior and Organization	-1,565	(2,831)	68
27	Economics Letters	-1,976	(4,862)	139
28	International Economic Review	-3,406	(5,965)	158
29	Journal of International Economics	-3,483	(2,717)	72
30	J of Economic Dynamics and Control	-3,769	(5,345)	75
31	Public Choice	-3,889	(1,776)	48
32	Journal of Labor Economics	-4,679	(3,857)	58
33	International J of Industrial Organization	-5,368	(3,574)	49
34	Economic Inquiry	-6,015	(4,728)	86
35	Economica	-6,843	(9,851)	44
36	Southern Economic Journal	-9,657	(5,219)	77
	Not Ranked	795	(365)	525
	Chair	26,921	(7,075)	
	Experience	-2,068	(1,195)	
	Experience^2	22.50	(26.16)	
	Female	-2,623	(4,821)	
	Constant	102,410	(5,046)	
	Observations	597		
	R^2	0.60		

Note - Robust standard errors in parentheses. The column # Faculty reports the number of faculty who published in the journal. A paper by one author counts as one publications, a paper by two authors counts as $\frac{2}{3}'$s of publication for each author and a paper with $n > 2$ authors counts as $\frac{1}{n-1}$ publications for each author. The regression controls for experience in quadratic form, chair position, gender, associate and professor dummies, university fixed effects, and PhD university rank, divided into five tiers, fixed effects.

Table 19: Journal Ranking, Decreasing Value: Per Capita Weighting

Rank	Journal	Coef.	Std. Err.
1	Econometrica	17,113	(5,957)
2	Journal of Political Economy	16,938	(6,170)
3	Journal of Monetary Economics	11,308	(4,139)
4	Review of Economic Studies	4,663	(6,658)
5	American Economic Review	8,573	(2,695)
6	RAND	6,696	(4,357)
7	Journal of Human Resources	16,450	(4,477)
8	Review of Economics and Statistics	8,488	(4,968)
9	Journal of Development Economics	5,735	(4,537)
10	Journal of Urban Economics	6,772	(3,939)
11	Economic Theory	8,096	(5,366)
12	Journal of Econometrics	7,109	(2,320)
13	Quarterly Journal of Economics	-4,015	(5,897)
14	J of Economic Literature	-2,713	(9,515)
15	J of Economic Theory	3,025	(3,868)
16	J of Business and Economic Statistics	4,736	(7,868)
17	J of Economic Perspectives	5,576	(5,471)
18	J of Applied Econometrics	5,437	(7,930)
19	European Economic Review	3,264	(6,625)
20	Journal of Public Economics	5,564	(2,663)
21	Journal of Money, Credit, and Banking	-26	(5,327)
22	Economic Journal	4,482	(7,403)
23	Games and Economic Behavior	1,668	(3,468)
24	Canadian Journal of Economics	3,062	(6,623)
25	Journal of Mathematical Economics	-2,091	(6,315)
26	J of Economic Behavior and Organization	2,033	(3,297)
27	Economics Letters	-3,029	(4,574)
28	International Economic Review	1,403	(5,813)
29	Journal of International Economics	-1,142	(2,967)
30	J of Economic Dynamics and Control	-3,062	(6,299)
31	Public Choice	-2,532	(2,177)
32	Journal of Labor Economics	-3,246	(3,736)
33	International J of Industrial Organization	-4,539	(4,037)
34	Economic Inquiry	-5,412	(4,716)
35	Economica	-13,469	(10,026)
36	Southern Economic Journal	-6,249	(5,235)
	Not Ranked	581	(398)
	Chair	28,838	(5,545)
	Experience	-840.0	(1,052)
	Experience^2	-0.91	(23)
	Female	-6,327	(4,386)
	Constant	97,946	(7,928)
	Observations	597	
	R^2	0.67	

Note - Robust standard errors in parentheses. A paper that was published by n authors counts as $\frac{1}{n}$th of a publication. The regression controls for experience in quadratic form, chair position, gender, associate and professor dummies, university fixed effects, and PhD university rank, divided into five tiers, fixed effects.

Table 20: Journal Ranking: Constant Value: Per Capita Weighting

Rank	Journal	Coef.	Std. Err.
1	Journal of Human Resources	9,678	(2,787)
2	Journal of Political Economy	9,243	(4,110)
3	Journal of Monetary Economics	9,067	(2,933)
4	Econometrica	8,880	(3,822)
5	Economic Theory	7,026	(4,084)
6	Economic Journal	6,797	(5,702)
7	Review of Economics and Statistics	6,530	(3,428)
8	Journal of Applied Econometrics	5,357	(6,247)
9	Journal of Public Economics	4,695	(1,946)
10	American Economic Review	4,455	(1,855)
11	Journal of Economic Perspectives	4,037	(4,076)
12	Journal of Econometrics	4,008	(1,776)
13	J. of Business and Economic Statistics	3,580	(5,784)
14	Canadian Journal of Economics	3,550	(4,847)
15	Journal of Urban Economics	2,980	(2,385)
16	RAND	2,905	(3,004)
17	Journal of Development Economics	2,264	(3,331)
18	Games and Economic Behavior	2,132	(2,713)
19	Journal of Economic Theory	1,560	(2,578)
20	Journal of Money, Credit, and Banking	1,059	(4,365)
21	J. of Economic Behavior and Organization	894	(2,581)
22	Journal of Economic Literature	463	(7,626)
23	Review of Economic Studies	113	(4,608)
24	European Economic Review	64	(4,480)
25	International Economic Review	-688	(3,974)
26	Journal of Labor Economics	-1,101	(2,603)
27	Journal of International Economics	-1,171	(2,218)
28	Economics Letters	-1,250	(3,525)
29	Public Choice	-1,554	(1,471)
30	Journal of Mathematical Economics	-2,688	(4,468)
31	Quarterly Journal of Economics	-2,926	(3,517)
32	International J. of Industrial Organization	-3,054	(3,456)
33	J. of Economic Dynamics and Control	-3,097	(4,773)
34	Southern Economic Journal	-3,709	(3,191)
35	Economic Inquiry	-5,940	(3,281)
36	Economica	-10,676	(6,293)
	Not Ranked	404	(308)
	Chair	26,941	(5,238)
	Experience	-744	(1,083)
	Experience^2	-12.9	(24)
	Female	-7,183	(4,498)
	Constant	103,035	(7,747)
	Observations	597	
	R^2	0.65	

authors counts as ((1/n))th of a publication. The regression controls for experience in quadratic form, chair position, gender, associate and professor dummies, Note - Robust standard errors in parentheses. A paper that was published by n university fixed effects, and PhD university rank, divided into five tiers, fixed effects.

Table 21: Journal Ranking: Constant Value and Upweighting

Rank	Journal	Coef.	Std. Err.
1	Journal of Political Economy	11,272	(4,002)
2	Econometrica	9,969	(3,819)
3	Journal of Monetary Economics	8,642	(2,875)
4	Review of Economics and Statistics	6,868	(3,285)
5	Journal of Economic Literature	6,451	(7,251)
6	American Economic Review	5,414	(1,810)
7	Journal of Human Resources	5,046	(2,624)
8	RAND	4,765	(3,083)
9	Economic Theory	4,728	(3,653)
10	Journal of Development Economics	4,119	(3,484)
11	Journal of Econometrics	3,609	(1,835)
12	J. of Business and Economic Statistics	3,291	(5,710)
13	Journal of Economic Perspectives	3,094	(3,814)
14	Journal of Economic Theory	2,862	(2,493)
15	Journal of Urban Economics	2,792	(2,004)
16	Journal of Applied Econometrics	2,792	(5,609)
17	Journal of Public Economics	2,546	(1,752)
18	Review of Economic Studies	2,500	(4,209)
19	Journal of Money, Credit, and Banking	2,033	(4,337)
20	Economic Journal	1,758	(4,782)
21	European Economic Review	1,646	(3,989)
22	Quarterly Journal of Economics	1,596	(3,350)
23	Games and Economic Behavior	1,190	(2,521)
24	Canadian Journal of Economics	470	(4,602)
25	Journal of Mathematical Economics	(637)	(4,161)
26	Economics Letters	(775)	(3,830)
27	Journal of Labor Economics	(2,294)	(2,663)
28	J. of Economic Behavior and Organization	(2,435)	(2,196)
29	Public Choice	(2,684)	(1,185)
30	Journal of International Economics	(2,785)	(2,011)
31	J. of Economic Dynamics and Control	(3,625)	(4,326)
32	International J. of Industrial Organization	(4,257)	(3,063)
33	International Economic Review	(4,375)	(3,963)
34	Economic Inquiry	(6,416)	(3,391)
35	Southern Economic Journal	(6,664)	(2,937)
36	Economica	(6,760)	(6,146)
	Not Ranked	500	(272)
	Chair	24,190	(6,797)
	Experience	-2,128	(1,246)
	Experience^2	10.7	(27.6)
	Female	-3,987	(5,057)
	Constant	105,485	(5,190)
	Observations	597	
	R^2	0.57	

Note - Robust standard errors in parentheses. The column # Faculty reports the number of faculty who published in the journal. A paper by one author counts as one publications, a paper by two authors counts as $\frac{2}{3}'$s of publication for each author and a paper with $n > 2$ authors counts as $\frac{1}{n-1}$ publications for each author. The regression controls for experience in quadratic form, chair position, gender, associate and professor dummies, university fixed effects, and PhD university rank, divided into five tiers, fixed effects.

Appendix

D. Data

In this appendix, I report the sample used in this study in more detail. Figure 6 represents the average salary by university rank. For full Professors, the trend line is downward slopping, as expected. For Associate Professors and Assistant Professors, it does not seem that there is a trend line. Table 22 represents the frequency of the faculty in the sample by university and faculty rank. Table 23 represents the mean and standard deviation of the salary by university and faculty rank.

Figure 6: Average Salary by University Rank

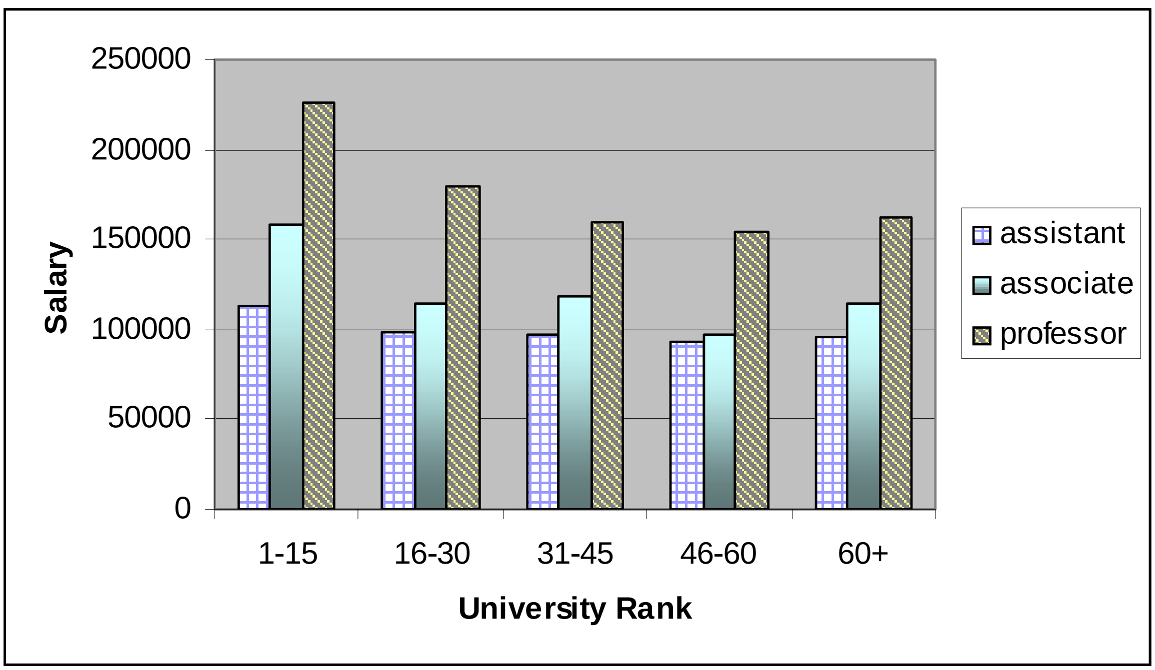


Table 22: Frequency of Faculty Members by School and Rank.

school	assistant	associate	professor	Total
Arizona	2	5	10	17
Arizona State	3	5	15	23
Berkeley	4	7	26	37
DAVIS	5	10	11	26
George Mason	4	4	13	21
IRVINE	11	5	7	23
Illinois	5	4	13	22
Indiana	9	1	12	22
Iowa	4	2	11	17
Maryland	6	6	15	27
Michigan	9	3	20	32
Minnesota	5	3	13	21
North Carolina	7	5	10	22
Ohio State	6	4	18	28
Purdue	5	3	8	16
Riverside	5	2	13	20
Rutgers	1	6	17	24
Texas	5	6	13	24
Texas AM	4	4	13	21
UCLA	12	7	18	37
UCSB	5	1	15	21
UCSC	7	1	12	20
UCSD	7	4	19	30
Virginia	8	4	8	20
Wisconsin	8	3	15	26
Total	147	105	345	597

Table 23: Mean and Standard Error of Salary by School and Rank.

School	Assistant		Associate		Professor		Total	
Arizona	94,090	(1,287)	136,400	(37,179)	153,800	(32,819)	141,658	(36,540)
Arizona State	105,330	(4,056)	128,396	(29,279)	177,987	(65,094)	157,729	(60,857)
Berkeley	107,040	(25,842)	189,717	(36,096)	234,743	(63,691)	212,419	(69,165)
DAVIS	97,126	(8,945)	121,224	(18,222)	150,634	(45,810)	129,032	(37,484)
George Mason	85,801	(9,308)	88,972	(18,303)	138,902	(30,715)	119,277	(35,881)
IRVINE	99,145	(9,793)	107,460	(21,336)	165,105	(45,721)	121,027	(39,948)
Illinois	97,582	(4,840)	106,515	(3,256)	150,171	(34,560)	130,282	(35,994)
Indiana	97,781	(16,812)	84,695	-	153,910	(43,548)	127,802	(44,332)
Iowa	110,832	(6,140)	126,213	(30,316)	187,529	(52,813)	162,269	(55,411)
Maryland	104,995	(8,663)	115,001	(23,952)	185,653	(51,702)	152,028	(55,166)
Michigan	107,085	(6,208)	117,960	(9,602)	191,585	(40,512)	160,917	(51,463)
Minnesota	124,949	(22,177)	189,215	(17,797)	245,796	(70,022)	208,940	(76,005)
North Carolina	110,150	(21,648)	130,250	(22,219)	179,037	(69,165)	146,031	(57,318)
Ohio State	102,084	(10,430)	106,689	(38,835)	173,784	(57,466)	148,835	(58,573)
Purdue	95,246	(11,183)	95,667	(19,034)	153,335	(29,711)	124,370	(37,264)
Riverside	87,073	(6,662)	118,678	(32,779)	163,852	(46,013)	140,140	(50,767)
Rutgers	86,177	-	110,070	(18,194)	157,804	(33,460)	142,886	(37,895)
Texas	98,400	(7,326)	116,857	(31,783)	166,978	(38,811)	140,161	(44,069)
Texas AM	87,426	(6,552)	113,402	(24,209)	151,453	(50,556)	132,010	(48,378)
UCLA	120,526	(31,281)	143,035	(53,161)	280,286	(91,848)	202,506	(103,472)
UCSB	96,127	(17,097)	136,620	-	167,918	(53,181)	149,335	(54,891)
UCSC	95,571	(14,586)	92,200	-	142,325	(43,568)	123,455	(41,580)
UCSD	90,007	(27,238)	115,999	(40,256)	201,569	(58,896)	164,128	(70,981)
Virginia	87,225	(2,548)	97,650	(9,126)	147,813	(32,601)	113,545	(35,305)
Wisconsin	101,323	(6,955)	133,841	(50,839)	177,585	(36,939)	149,072	(47,219)
Total	100,945	(17,802)	124,151	(36,376)	181,591	(62,443)	151,631	(62,011)

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