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Matchmaker, Matchmaker: The Effect of Old Boy Networks on Job Match Quality, Earnings, and Tenure

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Firms often view job applicant referrals from current employees as more informative than direct applications or referrals through formal labor market intermediaries such as placement firms. We argue that old boy networks reduce employers' uncertainty about worker productivity. Using Jovanovic's job matching model, we show that workers hired through the old boy network should (1) earn higher initial salaries, (2) experience lower subsequent wage growth on the job, and (3) stay on the job longer than otherwise comparable workers hired from outside the network. We find considerable support for this theory using data from the 1972 Survey of Natural and Social Scientists and Engineers.

I. Introduction

Business firms often view job applicant referrals from their current employees as "more informative and reliable than direct applications from prospective employees" (Holzer 1988). For example, unsolicited résumés

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seldom lead to a job interview, much less a job offer.[1] Indeed, employers often prefer to bypass formal job matching networks such as public employment services, private fee-charging agencies, and want ads in favor of job seekers referred by colleagues through the old boy network. One explanation for employers' reliance on old boy networks is favoritism. By favoritism, we mean that the managers of a firm, for whatever reason, may prefer some workers for reasons that are unrelated to productivity. It is not necessary, however, to resort to favoritism to explain employers' hiring practices. Albert Rees argued that employee referrals "usually provide good screening for employers who are satisfied with their present work force" (Rees 1966). There may be, therefore, important differences between job seekers referred by employees and colleagues, on the one hand, and job seekers hired through more formal sources, on the other.

We posit that knowledge of how a worker was placed into his job may embody important information about how well matched he is for that position.[2] Our priors lead us to reject favoritism in favor of an explanation based on imperfect and costly information as likely causes of employers' reliance on informal labor market channels.[3] Specifically, we argue that referees reduce employers' uncertainty regarding a worker's productivity. We use Jovanovic's model of job matching to derive propositions regarding the relationship between job match procedure, starting salaries, within-firm salary growth, and job tenure. We find considerable support in favor of the theory based on tests using data from the 1972 Survey of Natural and Social Scientists and Engineers.

II. Theory

In his study of job-seeking behavior among professional, managerial, and technical workers, Granovetter (1974) reported that workers finding jobs through employee referrals earned more than workers hired through more formal channels (want ads, etc.). Corcoran, Datcher, and Duncan (1980), using Panel Study of Income Dynamics (PSID) data, did not find

[1] According to a recent *Wall Street Journal* article that reported a flood of unsolicited résumés into executive search firms, "most of the jobs will go to candidates sought out by search firms, not those knocking on headhunter doors." Only "one in 300 unsolicited résumés is likely to be shown to a client," while "only one in 3,000 may get a job."

[2] After our article was accepted for publication, we learned of two related lines of research. Montgomery (1991) studies the interaction between social networks and labor market outcomes when the correlation of productive traits across acquaintances provides firms with a more precise evaluation of referred workers than those seeking employment through a formal market. Staiger (1990) develops a Jovanovic-type job matching model in which a personal contact provides a worker with up-front information about the match quality of a prospective job.

[3] For an example of a model of favoritism in the labor market, see Goldberg (1982).

large differences in wages conditional on hiring channel, but did find a small wage premium earned by referred workers that declined over time with tenure (p. 33). Corcoran, Datcher, and Duncan suggested that informal labor market contacts might be more efficient at matching jobs to people, but they did not develop a framework within which to test their contention. Although the pattern of initial earnings and subsequent earnings growth found by Corcoran, Datcher, and Duncan is not widely known, it tells an important story about the informational role of informal labor market networks.

It is possible to spin a tale in which favoritism accounts for the empirical findings of Corcoran, Datcher, and Duncan. Goldberg (1982), for example, developed a model in which an owner who has a taste for nepotism can successfully discriminate in the long run if she is willing to accept a lower rate of return. It is, however, necessary to assume that (the owners of) firms maximize utility rather than profit. Fortunately, a job matching story yields the same empirical implications (and more) under more conventional assumptions about the behavior of firms.

Objective criteria such as schooling, labor market experience, and the like communicate only imperfectly and incompletely the true attributes of job applicants. As Akerlof (1970) wrote, "An untrained worker may have valuable natural talents, but these talents must be certified" (p. 21). There is uncertainty not only about job seekers' overall ability but also about the fit of a particular individual within the firm, holding overall ability constant. Employers counteract uncertainty by gathering information about job seekers prior to hiring. The employer can screen out low-valued job matches by soliciting the subjective opinions of people who know the job seeker personally. Employers can more cheaply become informed about a job seeker who has an acquaintance in the firm than about a job seeker who responded to a want ad.

Job matching theories emphasize the incompleteness of information provided by criteria such as years of schooling and experience, grades, and letters of recommendation, especially for skilled positions (Barron and Bishop 1985; Barron, Bishop, and Dunkelberg 1985; and Barron, Black, and Loewenstein 1989). The opinions of third parties such as colleagues and friends are valuable to employers. Saloner (1985) studied the role of old boy networks in helping employers screen out low-quality workers by providing subjective personal opinions about the productivity of potential hirees. Such subjective opinions are not as readily obtained or are more difficult to interpret in formal labor markets.

A. The Matching Model

We posit that referees inside the old boy network are able to help employers recruit applicants by reducing uncertainty about an applicant's true productivity. Jovanovic's (1979) job matching model provides a natural framework within which to analyze old boy networks because he explicitly considers the role of employers' uncertainty about a given worker's productivity in determining initial wage offers, wage growth on the job, and job tenure.[4]

In the simplest job matching model there is no on-the-job training and worker productivity is assumed to be constant over a worker's tenure with a given employer. Although Jovanovic's model was developed in continuous time, Sargent's (1987) discrete-time exposition of it serves our purposes well. Consider a worker who enters the job market for the first time. Let θ be a random variable indicating the productivity of this worker with a given employer, where θ is $N(\mu, \sigma_\theta^2)$. Assume that the match parameter θ is firm specific, so that the current value of θ at a given firm conveys no information about the value of θ of the same worker to any alternative employer. That is, all draws of θ are independently and identically distributed by hypothesis. We postpone consideration of differences in overall ability until later in this section.

Suppose that firms do not initially observe a given applicant's productivity, θ. Instead, they observe a noise-ridden version of it: $\hat{\theta} = \theta + \varepsilon$, where ε is $N(0, \sigma_\varepsilon^2)$ and is independent of θ and of other draws of ε. Assume that the employer observes the employee's true θ after one period of work.

Firms choose a pay strategy that maximizes expected profit subject to a constraint of zero expected profits, implying that the expected stream of wages paid to a worker over his tenure with the firm equals his expected value of marginal product. Although the equilibrium pay strategy is not unique, Jovanovic demonstrates that the following is one such strategy. In the first period, the worker receives a wage equal to his expected productivity *conditional* on the firm's error-ridden prediction of it, that is, $m = E(\theta|\hat{\theta})$, and then receives a wage equal to actual productivity, θ, thereafter. Jovanovic shows that the first-period offer, m, is then given by the mean of the posterior of θ conditional on $\hat{\theta}$,

$$m = \mu + \sigma_1^2(\hat{\theta} - \mu), \tag{1}$$

where $\sigma_1^2 = (1/\sigma_\theta^2 + 1/\sigma_\varepsilon^2)^{-1}$ is the variance of θ conditional on $\hat{\theta}$. In turn, m is $N[\mu, \sigma_m^2 = \sigma_\theta^4/(\sigma_\theta^2 + \sigma_\varepsilon^2)]$.

The worker faces a dynamic programming problem of maximizing the expected present value of the stream of income. The optimal decisions of workers currently employed and workers currently unemployed are considered in turn.

At the end of his first period of employment, an employed worker receives an offer of θ from his current employer, his true productivity, and

[4] See Polachek and Yoon (1987) for an alternative model of job matching and the effect of employer and employee ignorance on earnings.

must decide to stay or quit. Only unemployed workers may draw new offers, that is, new values of θ and ε.[5] Let $J(\theta)$ be the expected present value of the decision to remain one more period and behave optimally thereafter, and let Q be the expected present value of quitting to become unemployed in order to draw one more offer, that is, to search for a new job. The employed worker will establish an on-the-job reservation wage θ^r such that he stays if $\theta > \theta^r$ and quits to search otherwise. Then $J(\theta)$ is defined recursively as

$$J(\theta) = \begin{cases} \theta + \beta J(\theta) = \theta/(1-\beta) & \text{for } \theta \geq \theta^r \\ \beta Q & \text{for } \theta \leq \theta^r, \end{cases} \tag{2}$$

where β is the discount factor. Thus, θ^r satisfies $\theta^r/(1-\beta) = \beta Q$.

Consider now the search problem of a worker who is currently unemployed. The value of an initial offer, m, is equal to

$$V(m) = \max\{m + \beta E[J(\theta)], \beta Q\}, \tag{3}$$

where $E[J(\theta)]$ denotes the expected present value of future earnings (which are implicitly conditional on m and σ_1^2). Notice that the expected value of accepting the offer (the first term in curly brackets) incorporates both the value of the option of staying with the firm beyond the first period and the option of quitting to search again. The expected value of rejecting the offer, βQ, is simply the expected value to an unemployed worker of seeking one more offer. Because the first term in brackets increases with m and βQ is constant for a given worker, there exists a unique reservation wage, m^r, that separates acceptable offers from unacceptable ones.

B. What Do Old Boy Networks Do?

Assume that individuals receive two types of offers. Offers of type R (for referred) are received through the old boy network, while offers of type N (for nonreferred) are received outside the old boy network. We posit that referees inside the old boy network help employers by reducing their uncertainty about an applicant's true productivity, so that $\sigma^2(\varepsilon_N) > \sigma^2(\varepsilon_R)$.

We assume that each individual receives an offer through the old boy network with (exogeneous) probability p and receives an offer outside the old boy network with probability $1 - p$. Let the value of the game for a currently unemployed worker with an offer m_R received through the old

[5] Jovanovic's 1984 paper permits job-to-job switches as well as quitting to become unemployed. Although it would be more realistic to incorporate job-to-job switches in the model discussed here, the predictions of the more complicated model would not be qualitatively different from those presented here.

boy network be $V(m_R)$ and the value of an offer received outside the old boy network be $V(m_N)$. If the worker rejects an offer, he obtains the right to draw a new offer next period, the value of which is Q. The variable Q, in turn, is

$$\begin{aligned} p \int V(m_R)\, dG[m_R \mid \mu, \sigma^2(m_R)] \\ + (1-p) \int V(m_N)\, dG[m_N \mid \mu, \sigma^2(m_N)], \end{aligned} \tag{4}$$

where $dG[m_R | \mu, \sigma^2(m_R)]$ is the probability density of offers received through the old boy network and $dG[m_N | \mu, \sigma^2(m_N)]$ is the density of offers received outside the old boy network. Notice that, given p, the value of quitting is independent of the source of the current job. Finally, the value of an offer in hand of a worker who has been employed at least 1 period is just $J(\theta)$, defined in equation (2) above.

1. *Reservation Wage for Remaining on the Job*

We have noted that Q is independent of the source of the current job (if employed). Note that, because $J(\theta)$ is solely a function of productivity on the current job (θ) and Q, it, too, is independent of the job source. The reservation wage for remaining on a job (θ^r), implicitly defined by equation (2), is therefore also independent of the job source. The job source does, however, have important effects on initial wages, wage growth on the job, and tenure.

2. *Distribution of Initial Wage Offers and Reservation Wages*

Sargent (1987) shows that the initial reservation wage, m^r, must satisfy the following:

$$V(m^r) = m^r + \beta \int J(\theta)\, dF(\theta \mid m^r, \sigma_1^2) = \beta Q. \tag{5}$$

But Sargent also shows that the solution for the reservation wage for initially accepting an offer is related to the reservation wage for staying on the job, θ^r, by

$$m^r = \theta^r - \frac{\beta}{1-\beta} \int_{\theta^r}^{\infty} (\theta - \theta^r)\, dF(\theta \mid m^r, \sigma_1^2). \tag{6}$$

Because the second term is positive, workers set lower reservation wages for initially accepting a job than for remaining on the job. Because firms cannot perfectly determine a worker's productivity prior to hire, part of the worker's return to accepting an initial offer is the gamble that his actual productivity, θ, will exceed the firm's error-ridden prediction of it, $\hat{\theta}$.

Recall that θ^r is the same for jobs received inside and outside the old boy network. For a given θ^r, the second term on the right-hand side is a positive function of σ_1^2, which in turn is a positive function of σ_ε^2. Hence, m^r must be smaller, the larger is σ_ε^2. Because $\sigma^2(\varepsilon_N) > \sigma^2(\varepsilon_R)$, $m_R^r > m_N^r$.

LIMITING CASES. In one case, $\sigma_\varepsilon^2 = 0$. In this case, $dF(\theta|m, \sigma_1^2)$ is degenerate, as $\sigma_1^2 = 0$. Then, $m^r = \theta^r$. The reservation wage for initially accepting the job is the same as the reservation wage for staying on the job because actual productivity is observed from the start. Because neither firm nor worker learns anything new about worker productivity, there is no wage growth on the job.

In the other case, $\sigma_\varepsilon^2 = \infty$. Then, the posterior probability density function is simply equal to the prior (unconditional) probability density function, that is, $dF(\theta|m, \sigma_1^2) = dF(\theta)$. In this case, all jobs look alike to workers and all workers look alike to firms a priori. Everyone is paid μ in the first period, and expected wage growth will, therefore, be positive.

It is helpful to think of old boy networks in terms of these limiting cases. Suppose, for example, that $\sigma^2(\varepsilon_R) = 0$ for offers received through the old boy network, while $\sigma^2(\varepsilon_N)$ is strictly positive for nonreferred offers. The theory predicts that (1) workers have higher reservation wages for offers received through the old boy network than for offers received outside the network, implying higher initial wages for workers who actually accept offers inside the old boy network than for workers who accept offers from outside the network, but (2) referred workers will have lower wage growth on the job because of the employer's greater initial certainty about their productivity.

REMARK. One might argue that referred workers have higher starting wages simply because more productive workers have higher values of p, that is, are more likely to receive an offer through the old boy network. Specifically, it is possible that $\mu_R > \mu_N$, but that $\sigma^2(\varepsilon_N) = \sigma^2(\varepsilon_R)$. In this case, however, we predict equal on-the-job wage growth for referred and nonreferred workers.[6] A crucial test of our hypothesis, then, is whether wage growth is higher for nonreferred workers.

3. *Tenure*

In our simple model, the probability that a worker will quit in the second period is given by

[6] Indeed, if there is training on the job and training on the job is directly related to overall ability, we might expect higher wage growth on the job among referred workers.

$$\int_{-\infty}^{\theta^r} dF(\theta | m, \sigma_1^2) = \Phi\left(\frac{\theta^r - m}{\sigma_1}\right), \tag{7}$$

where $\Phi(\cdot)$ denotes the standard normal cumulative distribution function. In the case where $\sigma_\varepsilon^2 = 0$ and $dF(\theta | m, \sigma_1^2)$ is degenerate, the quit probability is zero. Since $m = \theta \geq m^r = \theta^r$, each worker is optimally placed to begin with and has no incentive to quit to search. The probability of a quit is an increasing function of σ_ε^2, with

$$\lim_{\sigma_\varepsilon^2 \to \infty} \int_{-\infty}^{\theta^r} dF(\theta) = \Phi\left(\frac{\theta^r - \mu}{\sigma_\theta}\right). \tag{8}$$

Logically, less initial certainty about productivity creates speculation that a job match will turn out to be better than it initially appears. Inevitably, such speculation will often end in disappointment. To the extent that referees reduce initial uncertainty about worker productivity, referred workers should display lower turnover than nonreferred workers.[7]

III. Data

We test our contentions on a sample of individuals from the 1972 Survey of Natural and Social Scientists and Engineers. The survey asked detailed questions about each individual's current and two previous jobs. Most important, as discussed in greater detail in Section IV, the survey asked about how the individual located his current and previous two jobs.

Of the 50,093 individuals in the survey, we extracted all males between the ages of 25 and 55 (as of 1972) who answered the job history questionnaire (PMS-1) and who worked full-time as of the survey. The resulting sample contained 18,594 individuals. Individuals who reported leaving the labor force in order to retire temporarily, for military service, to return to school, or as a result of illness were deleted (352). Next, individuals who failed to supply the relevant data on any full-time job they reported were deleted (current job, 3,178; second-to-last job, 1,115; third-to-last job, 799). Finally, certain criteria were imposed in order to delete extreme and obviously wrong data, leaving 9,603 individuals in the sample.[8]

[7] Jovanovic (1984) permits job-to-job as well as job-to-unemployment and unemployment-to-job transitions. In this more realistic model, all jobs are destined to end. Nevertheless, fewer transitions will occur, and completed job tenures will be longer on average, the smaller is σ_1^2.

[8] Specifically, individuals were deleted if (i) age at receipt of final degree was not between 18 and 55; (ii) age at start of first professional job was not between 18 and 35; (iii) they received their final degree before 1933; or (iv) they reported a college degree, but not the year of its receipt.

The unit of observation was the job, by which we mean a spell of employment with a particular employer.[9] We collected data on starting and final (or, in the case of the job in progress at the time of the survey, current) salary, tenure, labor market experience, prior main activity, and most important, the job match procedure used for each job.

The remaining 9,603 individuals were sorted into current, and if applicable, second-to-last, and third-to-last jobs. Only full-time jobs in the for-profit private sector among those who were not self-employed were included,[10] yielding 9,541 possible jobs. Jobs were deleted if they were preprofessional or in academia, or if work began before age 18 or started more than 1 year after a prior job, leaving 8,106 jobs in the sample.

IV. Search Method, Referral, and Employer Information

The ideal test of our theory requires data on the earnings and labor market histories of individuals and the employers' source of information about the respondents prior to hire. We could then examine directly differences in starting salaries, salary growth, and job tenures between referred and nonreferred workers. Unfortunately, we do not have data on the source of *employer* information. We do, however, have data on the procedure by which the respondent found out about his job. The information on individuals' job match procedures can be used to infer the quality of information possessed by employers prior to hire.

Definitions and the relative frequencies of job match procedure appear in table 1 for three subsamples: all jobs, first jobs, and subsequent jobs. Focusing first on all jobs (col. 1), RECRUIT matches were most frequent (21.3%), followed by INSIDER (19.9%), COLLEGE PLACEMENT (13.0%), WANT AD (12.5%), and OTHER (10.9%) matches. A considerably different ranking emerges, however, in the first job and subsequent job subsamples. The most frequent method in first jobs is COLLEGE PLACEMENT, but it is the next-to-least frequent in subsequent jobs. The decline in COLLEGE PLACEMENT was offset by marked increases in INSIDER, OUTSIDER, and PRIVATE AGENCY methods.

[9] The survey defined a job differently than we do here. The survey asked respondents about their current and previous two jobs, where respondents were told to report any substantial change in supervisory responsibility or work-related activities as a job change. It was, however, possible to construct a sample of jobs defined on the basis of a spell with a given employer using the information provided about how a job was located. One of the choices was "promotion." We assumed that individuals who reported being promoted into a position did not change employer and that a change in employer occurred otherwise. Each respondent can, therefore, contribute from one to three observations in our sample.

[10] To restrict the sample to the non-self-employed, those who reported "began own business or professional practice" or "purchased business or professional practice" as the method of locating a job were deleted from the sample.

Table 1
Frequency Distribution of Job Match Procedures

	Frequency by Subsample		
Variables	All Jobs (N = 8,106)	First Jobs (N = 3,024)	Subsequent Jobs (N = 5,082)
1. Contacted by recruiter or personnel officer of firm via letter or personal visit (RECRUIT)	.213	.287	.169
2. Told of position by acquaintance in organization (INSIDER)	.199	.120	.246
3. Found position through college placement office (COLLEGE PLACEMENT)	.130	.308	.025
4. Told of position by acquaintance not in organization (OUTSIDER)	.092	.068	.106
5. Answered want ad (WANT AD)	.125	.056	.167
6. Found position at professional society meeting (MEETING)	.010	.009	.010
7. Found position through private employment agency (PRIVATE AGENCY)	.091	.046	.118
8. Found position through public employment agency (PUBLIC AGENCY)	.030	.026	.032
9. Other (OTHER)	.109	.081	.126

NOTE.—Columns may not sum to one due to rounding errors.

The essential question is how much prior information employers possess about the (firm-specific) productivity of job seekers who arrive through different labor market networks. Consider the case of an INSIDER, who learned about his job through an acquaintance at the current firm. It is likely that the personnel officer at the current firm would contact this mutual acquaintance in order to acquire information about the applicant. We conclude, therefore, that information about INSIDERs is superior to information about other types of matches such as WANT AD and PUBLIC PLACEMENT.

Consider next the case of RECRUITs, who were contacted by a recruiter or personnel officer of the firm via letter or personal visit. Presumably, the recruiting firm must have possessed some information about the potential hiree that caused it to contact him in the first place. Perhaps RECRUITs tend to be of higher overall ability than average, which attracts the initial attention of recruiters. In the process of recruiting, however, the recruiter will acquire at least some information about the firm-specific productivity of the potential hiree. We conclude, therefore, that the quality of information about RECRUITs is probably comparable to information about INSIDERs.

Other researchers have long speculated that a firm might recruit employees from specific colleges, departments, and even faculty members because their evaluations in the past have turned out to be good predictors of how well a hiree will perform. The information transmitted by a college placement office to the firm is similar to that transmitted by an employee at the firm. Consequently, COLLEGE PLACEMENT matches, in which individuals found their position through a college placement office, are likely to be well informed compared to other sources, at least in first jobs (MacDonald 1980). Use of a COLLEGE PLACEMENT agency to obtain subsequent jobs may indicate lack of worker information about employers and a corresponding lack of employer information about the worker.[11]

We call individuals hired via INSIDER, RECRUIT, and—in the case of first jobs—COLLEGE PLACEMENT match procedures referred workers. In each case, it is likely that a third party has provided information that reduces hiring firms' uncertainty about the productivity of the worker.

We call workers who used other match procedures nonreferred workers. Clearly, workers matched via WANT AD or PUBLIC AGENCY are nonreferred. There is, however, some ambiguity about PRIVATE PLACEMENT, OUTSIDER, and MEETING matches. Although firms may successfully recruit workers from private placement agencies, it seems unlikely that a firm is as informed about hirees from a private placement agency as about hirees who are referred. The OUTSIDERs were told of their position by an acquaintance outside the organization; it is not clear whether the personnel officer of the hiring firm would know this acquaintance. The MEETING matches are also difficult to interpret because in some cases interviews at a meeting are set up precisely because the recruiting firm knew the potential hiree's professor or another acquaintance. It is unclear, then, how the survey respondent would classify the source of such a job. The empirical results below suggest that MEETING and OUTSIDER matches are best interpreted as nonreferred.

V. Regression Results

A. Starting Salaries and Job Match Procedure

Our theory predicts that referred workers should earn higher starting salaries than nonreferred workers because the superior information possessed by employers about referred workers (a lower σ_ε^2) increases referred workers' reservation wages. Starting salary regressions were estimated in

[11] Indeed, as we explain below, this concern led us to divide the sample of jobs into first jobs and subsequent jobs.

which job match procedure dummy variables were included as regressors. The variable EXPERIENCE is the number of potential years of professional experience since the receipt of the highest degree earned (as of 1972). The variable PREDEGREE EXPERIENCE is equal to the number of years since the start of professional work prior to receiving one's high degree and is zero if work started only after the receipt of high degree. The variable YEARS BREAK is the number of years between the receipt of high degree and the start of work and is zero otherwise.[12] Finally, we controlled for school enrollment. If an individual reported attending school as his main previous activity, we set EX FULL-TIME STUDENT equal to one and equal to zero otherwise. If an individual received his terminal degree (as of 1972) after beginning the current job, CURRENTLY ENROLLED is set equal to one and equal to zero otherwise.

One further remark is in order before examining the econometric evidence. We have already discussed the possibility that more productive workers might be more likely to receive referred offers. One explanation, due to Saloner, is that referees gain greater utility by referring higher-ability job applicants to employers, other things equal.[13] If job match procedure is correlated with a job seeker's overall ability, the estimated coefficients in the salary regressions will reflect not only differences in reservation wages that result from differences in employer information (σ_ε^2) but also differences in overall ability (μ). If this is generally the case, referred workers will be of higher overall ability and will, therefore, earn higher salaries than nonreferred workers not only at the outset but also throughout their careers. As discussed in the theory, however, a test of the pure ability hypothesis against the information hypothesis is possible by examining within-firm wage growth, which we perform in the next section.

Starting salary regressions are reported in table 2 for the full sample of 8,106 jobs and two subsamples: all first jobs (3,024) and all subsequent

[12] The computation of PREDEGREE EXPERIENCE and BREAK follows that of Weiss and Lillard (1978). Let BYR denote the year in which the job began, EYR denote the year in which the job ended, YR1STJOB the year of the first professional job, EXPERIENCE the potential number of years of post-degree experience at the start of the job, and DEGYEAR the year in which the high degree was received. If BYR > DEGYEAR, then EXPERIENCE = BYR − DEGYEAR, PREDEGREE EXPERIENCE = max(DEGYEAR − YR1STJOB,0) and BREAK = max(YR1STJOB − DEGYEAR, 0). If BYR < DEGYEAR, then EXPERIENCE = 0, PREDEGREE EXPERIENCE = BYR − YR1STJOB, and BREAK = 0.

[13] The assumption is justifiable on several grounds. First, workers' marginal products are often interdependent, as, e.g., in the coauthoring of scholarly papers. Productive recruits increase the productivity of all individuals within an organization, including that of the recruiter. Second, recruiting successful matches is valuable in and of itself and will, therefore, be rewarded. Finally, as Rees (1966) pointed out, recognizing genius in others may indicate one's own abilities. One can therefore enhance one's reputation by recruiting a particularly productive individual.

jobs (5,082). A key assumption of the job matching model is that there is a firm-specific component of productivity that is independent across jobs and only imperfectly ascertained at the time of hire. If this is the case, the predictions of the job matching theory regarding starting salary, salary growth, and tenure should be borne out in both subsamples. Alternatively, it might be the case that new workers' productivities are not known with certainty but become general information over time, in which case the tests of the job matching model would hold only in first jobs.

We focus initially on the results for the total sample, which appear in column 1 of table 2. The estimated coefficients on the match procedure variables (the omitted category was OTHER) support the job matching theory. Referred workers (RECRUITs, INSIDERs, and COLLEGE PLACEMENTs) earned significantly higher starting salaries than nonreferred workers (PUBLIC AGENCY, WANT AD). Workers matched through a PRIVATE AGENCY, who probably fall between referred and nonreferred workers in terms of information, earned higher starting salaries than other nonreferred workers, but lower starting salaries than referred workers.

We argued above that COLLEGE PLACEMENT matches should be classified as referred matches because employers tend to recruit from sources that have supplied successful applicants in the past, but that this would be true only of first jobs. We provide the evidence for this contention by separating first jobs from subsequent jobs. We expected a positive premium to be earned by COLLEGE PLACEMENTs only in first jobs and not in subsequent jobs because job seekers who must resort to the college placement office after their first job will probably be perceived by employers to be of lower productivity. There is a large positive premium for COLLEGE PLACEMENT matches on first jobs (col. 2) but a sizeable negative premium on subsequent jobs (col. 3). These results also indicate that workers on their first job who resort to non–COLLEGE PLACEMENT channels are perceived by employers to be of lower productivity.[14]

The number of jobs held since the start of a career is another dimension of job experience and is empirically distinguishable from years of potential job experience. There is presumably greater uncertainty surrounding individuals on their first job out of school than on subsequent jobs. Consequently, we expected workers on their first job to earn lower starting salaries. Note, however, that the survey asked individuals only about their current and, if applicable, two previous jobs. This means that we do not always see an individual's complete career history. In our sample, we have first job information for only 3,024 individuals, of whom 1,452 were still

[14] It is not surprising that the positive effect of COLLEGE PLACEMENT dominates in the subsample of all jobs because few such matches occur in subsequent jobs.

Table 2
Starting Salary Regressions

	All Jobs (1)	First Jobs (2)	Subsequent Jobs (3)
RECRUIT	.074	.108	.051
	(5.9)	(4.8)	(3.3)
INSIDER	.034	.016	.033
	(2.7)	(.6)	(2.3)
COLLEGE PLACEMENT	.087	.138	−.075
	(5.9)	(6.2)	(2.6)
OUTSIDER	−.035	−.035	−.039
	(2.4)	(1.2)	(2.3)
WANT AD	−.062	−.023	−.080
	(4.5)	(.8)	(5.2)
MEETING	−.000	.054	−.017
	(.0)	(.8)	(.4)
PRIVATE AGENCY	.009	−.006	.005
	(.6)	(.2)	(.3)
PUBLIC AGENCY	−.085	−.030	−.107
	(3.9)	(.8)	(4.2)
FIRST JOB	−.159	*	†
	(13.2)		
EXPERIENCE	.039	.016	.041
	(19.0)	(1.4)	(18.2)
EXPERIENCE^2	−.0005	.00017	−.0006
	(5.6)	(.1)	(6.2)
YEARS COLLEGE (1972)	.081	.099	.068
	(32.8)	(24.4)	(21.9)
PREDEGREE EXPERIENCE	.019	−.004	.023
	(18.4)	(1.1)	(20.5)
YEARS BREAK	−.018	‡	−.017
	(5.7)		(4.5)
EX FULL-TIME STUDENT	−.028	−.030	−.087
	(2.4)	(2.1)	(3.5)
CURRENTLY ENROLLED	−.104	−.105	−.083
	(11.1)	(7.2)	(6.1)
MARRIED (1972)	−.008	−.039	.013
	(.7)	(2.4)	(1.0)
CONSTANT	8.852	8.616	8.896
Adjusted R^2	.406	.201	.312
No. of observations	8,106	3,024	5,082

NOTE.—*t*-statistics are in parentheses.
* By construction, this variable is equal to one for all observations in the subsample.
† By construction, this variable is equal to zero for all observations in the subsample.
‡ By construction, this variable is equal to EXPERIENCE for all observations in the subsample.

on their first job (see table A1, col. 4–6). It is possible, therefore, that individuals for whom we have first job information found good matches early in their careers. Table A1, for example, indicates that average (uncompleted) tenure for individuals in first jobs still in progress (col. 6) was 9.4 years, while tenure for individuals in subsequent jobs still in progress (col. 9) was only about 6 years. Note, though, that, even if presence of first job information indicated the finding of a good match early on in a career, we should still find lower initial wages and higher wage growth on such jobs unless the employer possessed superior information about such applicants. The negative coefficient on FIRST JOB suggests that the uncertainty surrounding such individuals overwhelms the potential effects of this sample selection bias.[15]

B. Within-Firm Salary Growth

Our theory predicts that within-firm salary growth will be relatively higher for nonreferred workers, who were hired under greater initial uncertainty than referred workers. This prediction, however, is conditional on the job match being successful. Although employers and employees learn about the true value of productivity after 1 period in our model, learning may, in reality, take years. We may fail to find support for our theory if we aggregate successful and unsuccessful matches together.[16] In order to address this bias, we divided each of our three samples (all, first jobs, and subsequent jobs) into jobs completed and jobs still in progress.

Our analysis of within-firm wage growth is contained in table 3. The dependent variable was the difference between the logarithms of final (or current) and starting salaries. We entered both experience and tenure as continuous variables augmented with four linear splines in order to more accurately fit the salary distribution. The intervals were [5, 10), [10, 15), [15, 20), and [20, ∞).[17] We estimated the effect of job match procedure on wage growth by interacting each of the job match procedure dummy variables (linearly) with TENURE. We also interacted TENURE with EXPERIENCE and FIRST JOB.

We make one more observation before turning to the econometric evidence. In our theoretical discussion, we assumed that earnings would grow (or diminish) with tenure only as a result of learning about worker productivity. In reality, of course, on-the-job training, implicit contracts,

[15] We examined how the existence of first job information was correlated with the wage in 1972 by creating a dummy variable, FJINFO, equal to one if there was first job information for the individual (i.e., either the third-to-last, second-to-last, or current job was a first job) in the data, and zero otherwise. We held constant all the regressors in table 2. The estimated coefficient on FJINFO was significantly *negative* (−.025 with TENURE and TENURE^2 constant and −.034 without). One explanation consistent with these results is Lazear's (1986) theory of raids and matching offers. In this theory, workers of low ability are less likely to receive outside offers, and, hence, less likely to switch jobs than workers of high ability. Raises, in turn, occur when an employer attempts to match an outside offer.

[16] For example, although we assumed in our theory that all workers could choose to draw a new offer every period, the arrival of offers might be random, and the probability of arrival might differ systematically across workers. If this were the case, then unsuccessful matches among some workers would tend to last longer than unsuccessful matches among others.

[17] Our results are robust to the particular specification of the EXPERIENCE and TENURE variables. For an analysis of the biases that may result when a quadratic earnings function is assumed, see Murphy and Welch (1990).

and other factors may cause wage offers to rise over time for any given worker. As long as these other effects of tenure are unrelated to job match procedure, our predictions will still hold.[18] In the end, however, our learning story will stand or fall on the evidence below.

Consider first the estimated coefficients on TENURE for the sample of all completed jobs (see table 3, col. 1). The estimated coefficient on TENURE indicates that, on average, real salaries grew at about 5% per year (depending on the level of tenure) for an individual who was matched to his job via the omitted category, OTHER. The estimated coefficients on the job match procedure interactions with TENURE indicate the difference between annual wage growth for an individual in a given match procedure category and the omitted category, OTHER. Interpretation of the TENURE-match procedure interaction terms is straightforward. For example, the estimated coefficient on the TENURE interaction with RECRUIT is .0066, indicating that salaries of RECRUITs grew about .7 percentage points per year more than salaries of OTHERs.

We argued above that employers probably possess more accurate information about workers on subsequent jobs than on first jobs, holding potential experience constant. We found in table 2 that workers on first jobs earned lower starting salaries, other things equal. A consequence of this greater uncertainty is that average wage growth should be higher on the first job than in subsequent jobs. The positive estimated coefficients on FIRST JOB and its interaction with TENURE indicate that this is, indeed, the case.[19]

The parameter estimates in table 3 generally support our hypothesis that wage growth should be higher for nonreferred workers. For instance, the estimated coefficient on the TENURE job match procedure interactions are highest for WANT ADs in columns 1 and 5 and for MEETINGs in columns 2–4 and 6. This pattern is consistent with our theory but warrants more formal testing.

We performed *F*-tests for the equality of the estimated coefficients on the TENURE match coefficients between the groups we classified as re-

[18] It is not difficult, however, to construct stories in which the effect of tenure on earnings (beyond learning) is related to job match procedure. We already considered one such story in n. 6 above.

[19] The estimated coefficient on the FIRST JOB dummy for jobs still in progress is more than double that for completed jobs, while the estimated coefficient on the *interaction* between FIRST JOB and TENURE is significant only for completed jobs. A possible explanation is that the effects of uncertainty on wage growth last only for the first few years of the job. Table A1 shows that completed first jobs lasted an average of only 4 years (col. 5), compared with an average (uncompleted) tenure of 9 years for first jobs still in progress (col. 6). The effect of uncertainty on wage growth for an individual with a successful first-job match may have been limited to only the first few years, resulting in a weaker interaction but a stronger intercept for first jobs in progress.

Table 3
Within-Firm Salary Growth Regressions, by Subsample

			First Jobs		Subsequent Jobs	
	Completed (1)	In Progress (2)	Completed (3)	In Progress (4)	Completed (5)	In Progress (6)
TENURE*	.0499	.0408	.0520	.0497	.0486	.0434
	(18.3)	(13.7)	(11.7)	(6.3)	(15.2)	(14.8)
TENURE × RECRUIT	.0066	−.0014	.0160	.0030	.0042	−.0058
	(3.2)	(1.3)	(4.8)	(1.7)	(1.5)	(4.1)
TENURE × INSIDER	.0053	−.0009	.0144	.0010	.0028	−.0027
	(2.6)	(.8)	(4.0)	(.5)	(1.2)	(2.0)
TENURE × COLLEGE PLACEMENT	.0058	.0006	.0147	.0032	−.0017	.0010
	(2.5)	(.5)	(4.2)	(1.7)	(.4)	(.4)
TENURE × OUTSIDER	.0065	.0031	.0191	.0074	.0019	.0002
	(2.8)	(2.3)	(4.6)	(3.1)	(.7)	(.2)
TENURE × WANT AD	.0105	.0015	.0174	.0051	.0094	−.0013
	(4.6)	(1.1)	(3.8)	(1.6)	(3.6)	(.9)
TENURE × MEETING	.0079	.0118	.0397	.0165	−.0053	.0085
	(1.5)	(3.0)	(4.0)	(2.1)	(.9)	(2.0)
TENURE × PRIVATE AGENCY	.00081	−.0006	.0205	.0053	.0044	−.0037
	(2.8)	(.3)	(3.6)	(1.4)	(1.3)	(2.0)

TENURE × PUBLIC AGENCY	.0048	.0026	.0120	.0014	.0071	.0026
	(1.4)	(1.3)	(1.9)	(.4)	(1.7)	(1.1)
FIRST JOB	.0319	.0762	†	†	‡	‡
	(3.5)	(6.1)				
FIRST JOB × TENURE	.0065	.0013	. . .	. . .	. . .	. . .
	(3.5)	(1.0)	. . .	. . .	. . .	. . .
EXPERIENCE AT JOB START*	.0007	.0038	.0080	−.0205	.0007	.0030
	(.3)	(1.6)	(1.0)	(1.9)	(.3)	(1.3)
EXPERIENCE AT JOB START × TENURE	−.0021	−.0013	−.0039	.0011	−.0019	−.0013
	(9.2)	(9.1)	(3.8)	(1.4)	(8.6)	(10.2)
YEARS COLLEGE (1972)	−.0034	−.0158	−.0072	−.0214	−.0009	−.0127
	(1.9)	(7.2)	(2.4)	(5.0)	(.4)	(5.3)
MARRIED (1972)	.0158	.0227	.0184	.0290	.0134	.0133
	(2.0)	(2.4)	(1.4)	(1.6)	(1.4)	(1.3)
CONSTANT	−.0258	.0128	.0110	.0619	−.0284	.0109
	(1.7)	(.7)	(.5)	(1.5)	(1.6)	(.6)
Adjusted R^2	.5349	.7091	.5598	.6671	.4582	.6775
No. of observations	3,915	4,191	1,572	1,452	2,343	2,739

NOTE.—*t*-statistics are in parentheses.
* TENURE and EXPERIENCE were entered linearly, augmented with four linear splines to capture nonlinearities (see text). We report only the linear effects to reduce clutter.
† By construction, this variable is equal to one for all observations in the subsample.
‡ By construction, this variable is equal to zero for all observations in the subsample.

Table 4

***F*-Tests (Prob Values) of Equal Salary Growth between Referred and Nonreferred Workers, All Jobs, by Subsample**

	OUTSIDER	WANT AD	MEETING	PRIVATE AGENCY	PUBLIC AGENCY
A. All completed jobs:					
RECRUIT	$.00^-$	4.09^+	$.07^+$	0.33^-	$.30^+$
	(.97)	(.04)	(.80)	(.56)	(.58)
INSIDER	$.38^+$	7.50^+	$.25^+$	1.11^+	$.03^-$
	(.54)	(.01)	(.62)	(.29)	(.87)
COLLEGE PLACEMENT	$.10^+$	4.51^+	$.16^+$	$.66^+$	$.08^-$
	(.75)	(.03)	(.69)	(.42)	(.77)
B. All jobs-in-progress:					
RECRUIT	14.58^+	5.48^+	11.34^+	$.26^+$	4.26^+
	(.00)	(.02)	(.00)	(.61)	(.04)
INSIDER	10.87^+	3.77^+	10.52^+	$.05^+$	3.23^+
	(.00)	(.05)	(.00)	(.82)	(.07)
COLLEGE PLACEMENT	3.48^+	$.39^+$	7.97^+	$.46^-$	$.96^+$
	(.06)	(.53)	(.00)	(.50)	(.33)

NOTE.—Prob values are in parentheses. A superscript "+" denotes that the sign of the difference was correct, i.e., that growth for nonreferred workers was higher than for referred workers; a superscript "−" indicates that the sign was wrong.

Table 5
***F*-Tests (Prob Values) of Equal Salary Growth between Referred and Nonreferred Workers, First and Subsequent Jobs, by Subsample**

	OUTSIDER	WANT AD	MEETING	PRIVATE AGENCY	PUBLIC AGENCY
A. First jobs completed:					
RECRUIT	$.97^{+}$	$.14^{+}$	6.12^{+}	$.82^{+}$	$.48^{-}$
	(.32)	(.70)	(.01)	(.36)	(.49)
INSIDER	1.86^{+}	$.57^{+}$	6.81^{+}	1.40^{+}	$.17^{-}$
	(.17)	(.45)	(.01)	(.24)	(.68)
COLLEGE PLACEMENT	1.81^{+}	$.50^{+}$	6.72^{+}	1.32^{+}	$.22^{-}$
	(.18)	(.48)	(.01)	(.25)	(.64)
B. First jobs-in-progress:					
RECRUIT	4.65^{+}	$.51^{+}$	2.97^{+}	$.41^{+}$	$.24^{+}$
	(.03)	(.48)	(.08)	(.52)	(.62)
INSIDER	7.33^{+}	1.66^{+}	3.83^{+}	1.25^{+}	$.01^{+}$
	(.01)	(.20)	(.05)	(.26)	(.92)
COLLEGE PLACEMENT	3.83^{+}	$.38^{+}$	2.85^{+}	$.32^{+}$	$.32^{-}$
	(.05)	(.54)	(.09)	(.57)	(.57)
C. Subsequent jobs completed:					
RECRUIT	$.77^{-}$	4.72^{+}	2.50^{-}	$.00^{+}$	$.49^{+}$
	(.38)	(.03)	(.11)	(.94)	(.48)
INSIDER	$.14^{-}$	10.02^{+}	1.86^{-}	$.31^{+}$	1.18^{+}
	(.71)	(.00)	(.17)	(.58)	(.28)
COLLEGE PLACEMENT	$.65^{?}$	$6.29^{?}$	$.25^{?}$	$1.56^{?}$	$2.52^{?}$
	(.42)	(.01)	(.62)	(.21)	(.11)
D. Subsequent jobs-in-progress:					
RECRUIT	17.39^{+}	11.48^{+}	11.34^{+}	1.59^{+}	12.47^{+}
	(.00)	(.00)	(.00)	(.21)	(.00)
INSIDER	4.68^{+}	1.30^{+}	7.03^{+}	$.36^{-}$	5.19^{+}
	(.03)	(.26)	(.01)	(.55)	(.02)
COLLEGE PLACEMENT	$.10^{?}$	$.91^{?}$	$2.56^{?}$	$3.16^{?}$	$.27^{?}$
	(.75)	(.34)	(.11)	(.08)	(.60)

NOTE.—Prob values are in parentheses. A superscript "+" denotes that the sign of the difference was correct, i.e., that growth for nonreferred workers was higher than for referred workers; a superscript "−" indicates that the sign was wrong; "?" indicates that the predicted sign is ambiguous.

ferred and nonreferred. These tests are reported by subsample in tables 4 and 5. We report the F-ratio and its significance level in parentheses and whether wage growth was higher (denoted "+") or lower (denoted "−") for nonreferred workers. Fifteen tests are reported for each subsample of first jobs and 10 tests for each subsample of subsequent jobs.

Table 4 contains the F-tests for the all completed (panel A) and all jobs-in-progress (panel B) subsamples. Looking first at completed jobs, 11 of the 15 comparisons are in the right direction, but only the three comparisons of referred workers with WANT ADs are significant (at the 5% level). This lack of statistical significance is not altogether surprising. As we pointed out, our theory applies to successful matches, which are less likely to be among the completed jobs subsample. Support for this contention is found in the all jobs-in-progress sample (panel B); 14 of 15 comparisons are in the right direction, 8 are statistically significant at the 5% level or better, and 2 at the 10% level.

Table 5 contains the F-tests for the first jobs and subsequent jobs subsamples. We focus on the jobs-in-progress subsamples (panels B and D).[20] In panel B, 14 of the 15 comparisons are in the right direction, with four statistically significant at the 5% level and two at the 10% level. In panel D, there are only 10 comparisons because we do not consider COLLEGE PLACEMENTs to be referred workers in subsequent jobs. Of the 10 comparisons, nine are in the right direction and seven are significant at the 5% level or better.[21]

Taken as a whole, the results are broadly consistent with our theory.

C. Job Tenure

Referred workers, with higher signal-to-noise ratios, should have higher reservation wages, making them better matched from the start. Referred workers should, therefore, be less likely to switch jobs than nonreferred workers. We estimated Weibull TENURE equations for our three subsamples: all jobs, first jobs, and subsequent jobs. The results appear in table 6.

Remember that the coefficients on the job match procedure dummy variables estimate the effect on tenure relative to the omitted category,

[20] It is important to note, however, that we found support for the theory even without separating out jobs completed from jobs-in-progress. Separating the jobs in this fashion merely points out that it might be difficult to find evidence of our theory for short jobs.

[21] A more direct way of disaggregating good and bad matches might be to partition the sample by tenure. In results not reported here, we divided the sample into two groups, short tenure and long tenure. We chose various cutoff levels between 3 and 10 years, defining short jobs as those lasting less than the cutoff and long jobs as those that lasted longer than the cutoff. We tended to find support for our hypothesis for long jobs only until cutoffs of around 8–10 years. One problem with this procedure, already noted above in n. 15, is that bad matches may tend to last longer for some individuals than others.

Table 6
Weibull Tenure Regressions

	All Jobs (1)	First Jobs (2)	Subsequent Jobs (3)
RECRUIT	.0005	.0947	−.1110
	(.0)	(.8)	(1.2)
INSIDER	−.0997	−.1818	−.0764
	(1.4)	(1.4)	(.9)
COLLEGE PLACEMENT	−.1120	−.1537	.1107
	(1.4)	(1.3)	(.6)
OUTSIDER	−.1278	−.1248	−.1282
	(1.5)	(.8)	(1.3)
WANT AD	−.3999	−.5598	−.3519
	(5.2)	(3.6)	(4.0)
MEETING	−.3659	−.4925	−.2580
	(2.0)	(1.6)	(1.2)
PRIVATE AGENCY	−.4804	−.4729	−.4775
	(5.7)	(2.8)	(4.9)
PUBLIC AGENCY	−.2002	−.1246	−.2636
	(1.7)	(.6)	(1.8)
EXPERIENCE	−.0124	.0296	−.0090
	(2.8)	(.9)	(1.8)
YEARS COLLEGE	−.0503	−.0468	−.0438
	(3.7)	(2.1)	(2.5)
EX FULL-TIME STUDENT	.1036	.1038	−.3664
	(2.1)	(1.4)	(2.9)
CURRENTLY ENROLLED	−.3950	−.4344	−.3273
	(8.7)	(6.4)	(5.1)
INTERCEPT	2.9944	3.0326	2.9036
	(31.8)	(19.6)	(23.7)
Duration dependence parameter	1.1509	1.1810	1.1268
	(77.2)	(47.9)	(60.6)
Log likelihood	9,952	3,984	5,949

NOTE.—Asymptotic *t*-ratios are in parentheses.

OTHER. The WANT AD and PRIVATE AGENCY matches were least durable, followed by MEETING matches. By comparison with these match categories, RECRUIT and INSIDER matches were more durable. If job matches of longer duration are interpreted as better firm-specific matches, our estimates agree well with our theory: referred matches are the most enduring.

VI. Conclusion

Researchers have long speculated why firms place such great reliance on informal labor market channels, that is, old boy networks, when hiring workers. Because the existence of favoritism is not consistent with efficient factor markets, job matching is a more compelling explanation. Because the information that firms have about worker productivity is limited, they have an incentive to use the subjective opinions of third parties to determine who is of high productivity. Referees in the labor market provide employers with valuable information that can be used to screen out poor job matches. Because workers who use the old boy network are comparatively well matched initially, salary growth on the job is lower than for workers who were hired outside the network but who prove to be good matches. Job stayers from outside the network who have low match value ex post move on when better firm-specific matches are found, while those with high match-specific value ex post stay. The result is higher salary growth for stayers hired from outside the old boy network. Finally, referred workers display lower rates of turnover than nonreferred workers. Tests using data from the Survey of Natural and Social Scientists and Engineers supported the job matching hypothesis.

Although the job matching hypothesis provides an attractive alternative to the favoritism hypothesis, the research here did not test between them. Our goal was more modest: to show that the job matching model was just as compelling as the favoritism model. One possible strategy to distinguish the two would possibly be to compare the earnings and turnover of workers in government and private firms. Presumably, the forces of competition are less strong in the government sector. If so, favoritism should be more prevalent in the government sector. Devising a test that distinguishes between favoritism and job matching provides a fruitful area for future research.

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Appendix

Table A1
Summary Statistics: Means and Standard Deviations

	All Jobs			First Jobs			Subsequent Jobs		
	All (1)	Completed (2)	In Progress (3)	All (4)	Completed (5)	In Progress (6)	All (7)	Completed (8)	In Progress (9)
Ln (STARTING SALARY)	9.343	9.263	9.417	9.116	9.039	9.199	9.478	9.414	9.533
	(.389)	(.397)	(.366)	(.343)	(.344)	(.321)	(.351)	(.358)	(.335)
WAGE GROWTH (within-firm)	.224	.162	.282	.341	.232	.459	.154	.115	.188
	(.311)	(.236)	(.358)	(.356)	(.266)	(.401)	(.257)	(.201)	(.292)
YEARS COLLEGE	5.149	5.164	5.135	5.255	5.249	5.262	5.086	5.108	5.068
	(1.459)	(1.481)	(1.438)	(1.497)	(1.510)	(1.483)	(1.432)	(1.459)	(1.409)
EXPERIENCE	3.698	3.086	4.271	.326	.307	.346	5.705	4.950	6.352
	(5.613)	(5.012)	(6.067)	(1.109)	(1.121)	(1.096)	(6.224)	(5.700)	(6.572)
TENURE	5.412	3.529	7.172	6.585	3.951	9.437	4.715	3.246	5.971
	(5.467)	(3.446)	(6.348)	(6.241)	(3.819)	(7.050)	(4.816)	(3.141)	(5.834)
PREDEGREE EXPERIENCE	2.117	2.146	2.090	.756	.884	.617	2.927	2.992	2.872
	(3.970)	(3.809)	(4.114)	(2.004)	(1.841)	(2.160)	(4.581)	(4.494)	(4.655)
YEARS BREAK	.312	.303	.320	.326	.307	.346	.304	.301	.306
	(1.122)	(1.125)	(1.120)	(1.109)	(1.121)	(1.096)	(1.130)	(1.127)	(1.133)
Dummy variables:									
FIRST JOB	.373	.402	.346	1.000	1.000	1.000	.000	.000	.000
CURRENTLY ENROLLED	.277	.330	.227	.381	.437	.321	.215	.259	.177
EX FULL-TIME STUDENT	.287	.306	.269	.716	.702	.732	.031	.040	.023
MARRIED	.883	.882	.884	.871	.877	.864	.890	.885	.894
No. of observations	8,106	3,915	4,191	3,024	1,572	1,452	5,082	2,343	2,739

NOTE.—Standard deviations are in parentheses.