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The Importance of Signalling in Job Placement and Promotion

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Abstract

In a setting where training or promotion opportunity depends positively on expected initial ability, the effects from signalling initial skills on earnings may last well beyond the period when knowledge of a workers' skill set is fully known. This paper proposes extending recent tests for signalling to better accommodate training differences by using firm-level characteristics and apply these tests to a large sample of MBA and law graduates from different ranked schools. If training is greater at firms that hire workers with better expected-ability, earnings adjustments after controlling for initial firm should be correlated with new information about productivity, but not with initial signals of productivity. This is what the paper finds.

Keywords: signalling, job mobility

JEL Classifications: J79, J31, J00

I. Introduction

Employers often face the challenge of choosing a single person among hundreds of job applicants with very different levels of skill. They do not always choose correctly. According to a job matching survey by the National Federation of Independent Business (NFIB), one in every five workers turns out to be 20% less productive than managers expected, as rated after 6 months into a job on a productivity scale of 0 to 100 (Barron et al. 1985). When hiring, employers have strong incentives to turn to observable signals of productivity, such as education, previous job placements, or other outstanding features in deciding who gets selected for a particular job. But a candidate's observable characteristics convey only partial information about their real productivity, and as workers accumulate experience on the job, employers may acquire new information about performance and reevaluate initial earnings offers and hiring decisions.

In this paper, we develop a simple model to highlight the consequences from this type of signalling, and test whether such behaviour occurs for a large sample of MBAs and lawyers graduating from different ranked schools. The chief point from the model is that, in a setting where training or promotion opportunity depends positively on expected initial ability, the effects from signalling on earnings may extend well beyond the period when knowledge of a workers' skill set is fully known. This holds whether worker characteristics that correlate with ability are acquired (e.g., through education or certification), or innate, and whether workers know their true abilities or not.

Suppose, for example, a particularly talented MBA graduate was unable to afford a top graduate school. If firms assess initial productivity based on school rank, the worker may land a first job at a firm that provides worse career opportunities than those at a firm he or she would have started with if a better school had been attended. Even if employers soon realize the individual's talent, he or she never catches-up to full productivity (and earnings) potential. Long-run earnings may thus depend crucially on initial placement.[1]

A similar result occurs if women have less labour-force attachment than men. If labour-force attachment is initially unobservable to employers and firms that value long-term commitment train employees more than firms that do not, then an exceptionally career-oriented female would find it difficult to receive the same amount of training as a male with similar initial ability.

Farber and Gibbons (1996) and Altonji and Pierret (2001a) [hereinafter FG and AP respectively] provide a methodology for testing signalling behaviour. They note that when earnings growth is unrelated to initial placement, signalling and learning implies that relative earnings adjustments occur from new information about worker productivity. Changes in earnings should be correlated with worker characteristics not initially observable but related to new information and uncorrelated with characteristics observable at time of hire (such as education or gender).

If earnings growth depends positively on initial job placement, however, then relative earnings between workers with different signals, but similar ability could instead increase over time. If

1. This example suggests one reason behind the strong intergenerational correlation between parental and child earnings. A talented child from a modest income family may not be able to attend a prestigious university that offers superior career and network opportunities. Parents with poorer access to funds may lead children to have lower earnings than otherwise.

this occurs, identifying earnings changes due to updated productivity expectations becomes no longer possible. We modify the methodology to better account for earnings growth differences by adding firm-level information. First, we add initial-firm fixed effects, which absorb information about recent graduates' expected ability <u>and</u> average earnings growth across firms. Only the bias from within-firm earnings growth differences remains. We also measure changes in firm quality. If workers with expected high-ability match to high-earnings firms, we can use the same regressions as AP, but include changes in firm quality as the dependent variable instead of changes in earnings. Initial signal variables should predict what type of firm junior workers begin at, but should not be able to predict changes in firm type when switching jobs. A variable correlated with new information about workers (such as brother or father's earnings), however, should be able to predict whether they advance to higher or lower earnings firms.

These alternative tests provide a way to correct the upward bias when earnings growth depends on initial placement. More importantly, the results from both sets of tests, taken together, provide a first-attempt to examine empirically whether the presence of signalling can have long-run influences on labour market outcomes, even after performance becomes known.

The firm-adjusted tests are possible with a unique panel dataset of administrative data from Statistics Canada. We match a large sample of graduating male MBA and law students in the mid-1980s to tax records containing annual earnings and firm identifiers up to 1998. The graduates are also matched to their brothers and fathers. We look to see whether firms use the ranking of the school a graduate came from as a signal of their productivity. Using AP's methodology, we find the predictable power of school rank on earnings rises with experience, which is contrary to their signalling and learning model. When we examine movement across high-paying and low-paying firms, however, school rank is not correlated with firm quality, but brother and father's average earnings is correlated, consistent with this paper's model of signalling and learning behaviour. Taken together, the results suggest permanent effects from judging ability by school attended.

The next section introduces the model. We present AP's tests over two periods, and show how they are complicated by the presence of training, when training depends on expected initial ability. We show how including firm fixed effects helps control for both initial expected ability and earnings growth from training, and helps identify earnings growth from updating expectations. We then extend the model to contain job assignment. With job assignment, we show how looking at changes in firm quality make it easier to test signalling. The administrative datasets are discussed in detail in Section III. Section IV presents the results. We summarize and conclude in Section V.

II. A simple model of signalling and learning with endogenous firm-specific training

The main implications of our model may be shown using a two period, two ability level framework. For a more detailed exposition, the reader should consult AP and Altonji and Pierret (2001b).

Assume that employers use a single observable characteristic in first period, s, to derive productivity expectations because they cannot observe first period worker productivity, y, directly.[2] The signal is either high or low: $s \in \{s^H, s^L\}$. Competition among firms makes the first period earnings, w_1, equal to the conditional expectation of the worker's marginal output, $E(y \mid s)$. Let $E(y \mid s^H) > E(y \mid s^L)$. We use a one-dimensional definition of ability. High ability workers are more productive than low ability workers and so those with signal s^H are, on average, more able than workers with signal s^L.

Workers receive training between first and second period, $\eta(s)$, that causes their productivity to rise. Another interpretation for this earnings growth is from skills acquired through learning-by doing, or by promotion within a fast-track firm (see below, and O'Flaherty and Siow, (1995)). For simplification, assume $\eta(s)$ is acquired from learning on-the-job which is costless to both employers and employees. Workers at firms that hire graduates with high signals gain at least as much training as those at firms that hire graduates with low signal: $\eta(s^H) \geq \eta(s^L)$. This holds empirically, in U.S. for example, when s is years of schooling, gender, or race (Altonji and Spletzer (1991), Lillard and Tan (1986)).[3]

By the second period, productivity is known and market clearing earnings, w_2, are equal to workers' actual marginal products, $w_2 = y + \eta(s)$.[4]

The return from having a high signal, r, can be expressed as:

$$
\begin{aligned}
r &= T_1\left[(w_1 \mid s^H) - (w_1 \mid s^L)\right] + T_2\left[(w_2 \mid s^H) - (w_2 \mid s^L)\right], \\
&= T_1\left[E(y \mid s^H) - E(y \mid s^L)\right] + T_2\left[\eta(s^H) - \eta(s^L)\right]
\end{aligned}
\tag{1}
$$

where T_1 and T_2 are the relative weights placed on period 1 and period 2 respectively. Until employers learn workers' real productivity (by the end of period 1), they set earnings with only information from the signal. They perceive workers with a high signal to be more productive, on average, than those with a low signal. The first part of equation (1), $T_1\left[E(y \mid s^H) - E(y \mid s^L)\right]$, is the return from having a high signal under incomplete information. The quantitative significance of the signal increases the slower it takes for employers to learn more about actual productivity (when T_1 is large).

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2. For most of the analysis, we suppress the individual i subscript.
3. Note that part of the relationship between race and gender may reflect biased inferences by employers or other forms of signalling that have nothing to do with productivity.
4. We assume productivity is known to all firms. A number of authors have considered asymmetric learning: only the employer that hires in the first period gains additional information about a worker. See, for example, O'Flaherty and Siow (1995), Demougin and Siow (1994), Gibbons and Katz (1991), and Waldman (1990). We hope to analyze the implications of asymmetric learning more in future research.

The return from having a high signal may still be large even when employers can observe skill after a short period of time, or even when the expected initial productivity differential between high and low signal workers is small. This arises because the amount of training acquired in the first period may depend also on employers' initial expectations of worker ability. If training and expected ability are positively related, the training portion of the returns to the high signal, $T_2\left[\eta(s^H)-\eta(s^L)\right]$ will be positive and permanent after period 1.

For acquired signals, such as education, individuals weigh expected returns from obtaining a signal from its cost. Spence (1973) pointed out in his original article that such a signal would not effectively distinguish one job applicant from another unless the costs of signalling are negatively correlated with ability. High ability workers obtain signals correlated to their abilities more often than low ability workers when signal acquisition is more costly for low ability workers. Incomplete knowledge of applicants' capabilities is costly in the sense that unnecessary resources are spent on obtaining signals, but conditional on signals' returns, workers make rational choices whether to acquire them.

Of more concern is if individuals with similar ability face disparate constraints on obtaining signals, or if some signals are innate. For example, consider two equally able workers, k, and l. Worker k is born with signal s^H, while worker l is born with s^L. Although l would like to let employers know he is equally productive as k, he cannot. Until employers learn workers' real capabilities, k receives the premium $\left[(w_1\,|\,s^H)-(w_1\,|\,s^L)\right]$. By the time employers realize that the two employees were initially the same, k has gained more training than l. Even after the original innate signal conveys no additional information, worker k could still receive higher earnings than l. Of course, the opposite is true if $E(y\,|\,s)>y$. Workers with higher expected ability than actual ability benefit, in the long-run, from employers believing they are more productive and providing them additional training.

II.1 Previous tests for signalling and learning behaviour

i) Test 1A – Regress w_2-w_1 on s

For the empirical analysis, assume that s is continuous, and the correlation between s and y positive. The difference between earnings in first and second period is

(2) $$w_2-w_1=\left[y-E(y\,|\,s)\right]+\eta(s).$$

The simplest version of AP's test for signalling and learning (which was first introduced by FG) considers regressing w_2-w_1 on s. If training is independent from $E(y\,|\,s)$, then changes in earnings between the two periods reflect the part of productivity that is unrelated with the signal. Clearly, in this special case, the value of the coefficient on the single variable OLS equation equals zero. Otherwise, from the omitted variables bias formula, the coefficient is $\frac{\text{cov}(s,\eta(s))}{\text{var}(s)}$.

The predictions from this model are obscured by the acquisition of training that depends on workers' initial characteristics. Adding controls in equation (2) for training would reduce the omitted variables bias, but may not remove the bias completely. Information on training acquired through learning by doing, for example, would be difficult to obtain.

ii) Test 1B – Regress $w_2 - w_1$ on b

AP's second test for signalling considers regressing $w_2 - w_1$ on a variable, b, that is related to productivity, but not used as a signal in the first period: $\text{cov}(b, y) > 0$, and $\text{cov}(b, s) = 0$. In our empirical analysis, we use brother's and father's average earnings for b. Ideally, the most interesting variables to use would be the new information itself. By using on-the-job performance measures, we could measure directly the value from this information on earnings adjustments. From the omitted variables bias formula, the coefficient from b is $\frac{\text{cov}(b, E(y \mid s) - y)}{\text{var}(b)} + \frac{\text{cov}(b, \eta(s))}{\text{var}(b)}$. If training is unrelated to the signal, the coefficient estimate captures the part of the earnings adjustment due to learning. If no learning takes place between the first and second period, earnings do not change and the coefficient would be zero. Similarly, if employers have full knowledge of worker productivity before the first period, the coefficient would also be zero, because earnings in the first period already reflect workers' productivity. Thus, the models' predictions should hold only when signalling and learning occurs between the first and second period.

II.2 Tests for signalling with initial firm fixed effects

To reduce the bias from training, depending on workers' initial signals, we use firm-level information. Why would firm-level information help identify $\eta(s)$? In a setting with constant-returns-to scale technology and perfect substitutability of human capital for production, it does not—firms supply on-the-job training elastically at a constant price, and are indifferent to whom they hire. Under these assumptions, firms play no active role, and thus, mobility across and within firms cannot be addressed. Job assignment models that relax these conditions, however, may do a better job in explaining the importance of the firm by describing market behaviour that is not commonly explicable using the predominant model. Market behaviour described by assignment models in the past include career mobility, internal labour markets, seniority wage premiums, and fast-track jobs (for example, see Sattinger (1993), Gibbons and Katz (1991), and Hartog (1980)). Fast-track jobs are entry-level positions in which a junior employee is given particular training and preparation for senior jobs. The employee is either promoted to a more senior position ahead of the cohort that entered the labour market with him or else he is moved laterally to another position within the firm, or is dismissed from the firm altogether. The prototypical example of a fast-track job is a clerkship with a law firm. The firm decides whether to promote a junior employee partner at the end of a short term if the existing partners believe the junior has an adequate expected set of skills. Otherwise, they dismiss him, or place him into a less promising career-path. The existence of fast-track jobs would exacerbate the effects of signalling if fast-track jobs went to entry workers with high-ability signals.

We define fast-track firms for this paper as firms that provide more training and opportunity for promotion than other firms. Oi and Idson (1999) and Gibbons and Waldman (1999) note that a common empirical finding is that firms sort by various employee characteristics, such as education and AFQT scores. Spurr (1990) finds evidence of matching into law firms by school and graduate ranking, and we find evidence below of matching by graduating school for MBAs and lawyers. Including initial firm fixed effects should, therefore, control for average training and career opportunities for all MBAs and lawyers in the firm.

i) Test 2A – Regress $w_2 - w_1$ on s with initial firm fixed effects.

We can decompose equation (2) further to get:

(3) $$w_2 - w_1 = \left[y - E(y \mid s)\right] + \eta^{F}(s) + \eta'(s),$$

where η^{F} is the initial firm-specific training given to employees in firm F, and η' is the remaining within-firm training component for employees. The coefficient on s from regressing $w_2 - w_1$ on s with initial firm fixed effects is the coefficient from the auxiliary regression of $\eta'(s)$ on s. The value is lower than the value from test 1A by the portion of the earnings change due to training acquired from fast-track firms.

ii) Test 2B – Regress $w_2 - w_1$ on b with initial firm fixed effects.

Similarly, we can reduce training differences that are correlated with initial observable characteristics by regressing $w_2 - w_1$ on b with initial firm fixed effects. If all training differences were due to initial firm placement, the model predicts that the coefficient from test 2A on s is zero, while the coefficient from test 2B on b is the coefficient from the auxiliary regression of $\left[y - E(y \mid s)\right]$ on b.

II.3 Signalling with job assignment

Including initial firm fixed effects in the regressions above reduces the precision of the coefficient estimates on the signal and on father's earnings when there are few MBAs and lawyers from different schools matched to the same initial firm. To increase the degrees of freedom for estimation, we introduce a variation of this approach that uses changes in firm quality, identified through movement across firms, as the dependent variable instead of using changes in earnings. Suppose that MBA and law firms differentiate themselves by average worker productivity. The following simple assignment model motivates how, in such a setting, we may use estimates of changes in firm quality that arise from the individual moving between firms to identify the coefficients of interest in the model.

Suppose workers are assigned to firms based on their expected ability and there are only two initial ability levels among workers.[5] High ability workers produce y^H, low ability workers produce y^L, and $y^H > y^L$.[6] There are also 2 firms, $F1$ and $F2$, with different qualities.

5. For reviews on assignment models, see Sattinger (1993) and Gibbons and Waldman (1999).
6. The model presented here is similar to one presented by Gibbons and Katz (1991), who assume employer signalling and learning in a two period model to examine whether inter-industry earnings differentials can be explained by unmeasured ability. The main differences between their model and ours are that we include endogenous training and achieve sorting of high ability workers into high earnings firms not because of comparative advantage, but because of a scale of operations effect: workers with same ability are more productive in some firms than others, and available positions are limited.

Firm quality is defined as any attribute that workers with high and low expected ability sort on. Specifically, let y_i be individual i's productivity, s_i be i's signal value, and Q_i^F be the firm quality for firm F, where i is working. Firm quality is defined as any attribute that achieves sorting such that $Q_k^{F2} > Q_j^{F1}$ when $E\,[(y_k \mid s_k) + \eta(s_k)] > E\,[(y_j \mid s_j) + \eta(s_j)]$.

Whenever a worker's expected productivity (including any training) is higher than another's, that worker is also at a firm with greater quality. Notice that sorting on quality and expected ability is relative to which period a worker is in. First period workers sort on initial ability. Second period workers sort on initial ability plus training. Possibilities for firm quality variables include firm size, average earnings by firm, high paying industries, and high paying firms, adjusted for employer productivity.

Suppose firm F has the following production technology, Y^F:

(4) $$\mathrm{Y}^F = \left(Q^F \sum_i y_{F,i} \right)^{\alpha},$$

where Q^F is the firm's quality, $Q^{F2} > Q^{F1}$, $y_{F,i}$ is the productivity of worker i in firm F, and $0 < \alpha < 1$. The marginal product for all workers is higher at firm $F2$ than at firm $F1$ if initially $y_{F2,i} = y^H, \forall\, \mathrm{i}$, and $y_{F1,i} = y^L, \forall\, \mathrm{i}$, and $\frac{Q^{F2}}{Q^{F1}} > \frac{y_{F2,i}}{y_{F1,i}}, \forall\, \mathrm{i}$. While these conditions hold, firm $F2$ could increase profits, $\Pi^{F2} = \left(Q^{F2} \sum_i y_{F2,i} \right)^{\alpha} - \sum_i E(y_j \mid s)$ by hiring any additional worker, given their higher firm quality than $F1$. But suppose firms are restricted by the number of workers they can hire in the first and second period. This assumption is common for assignment models.

Firm $F2$ first prefers to hire workers with expected high ability (to maximize expected profit). If the number of workers Firm $F2$ can hire in first period equals the number of workers with signal s^H, then perfect sorting occurs: all those with the high signal go to the second, more productive firm and all those with the low signal end up at firm $F1$. In the second period, also assume that the number of available jobs at $F2$ equals the number of workers with y^H and that not all high ability workers have the high signal. If training acquired at $F2$ by low ability workers is not enough to make their second period productivity greater than the productivity of high ability workers that begin work with $F1$, $y^H + \eta(s^L) > y^L + \eta(s^H)$, then Firm $F2$ now hires all workers with productivity, y^H, and $F1$ is left with all workers with $F1$. With this assumption (which can be relaxed, but makes the exposition much more complicated), relative differences in training are not important. Only the order between highest and lowest expected ability matters. Training alters relative productivity differences, but does not affect the order of hiring preferences. Sorting by firms occurs only by $E(y \mid s)$, regardless of whether in the first or second period.

II.4 Testing for signalling with changes in firm quality

We defined firm quality above as any attribute that achieves sorting such that $Q_k^{F2} > Q_j^{F1}$ when, $E(y_k \mid s) > E(y_j \mid s)$. Motivated by having many firms, many ability levels, and limits on the number of first and second period workers that can be hired, assume that workers with the same expected ability match exactly to one particular firm, so that $Q_i^F = E[(y_i \mid s_i) + \eta(s_i)]$. Experience acquired from high quality firms is at least as great as experience from low quality firms: $\eta(s_k) \geq \eta(s_j) \ \forall \ j \neq k$. Assume that the ratio of first and second period workers hired at each firms is one to one. Then training will not affect the relative order of a firm's worker preferences. We can thus define $Q^{F,1}$, worker's firm quality in period t as $Q^{F,1} = E[(y \mid s) + \eta(s)]$. Adding the training parameter does not affect the relative order by which firms select workers. For period 2, firm quality is the same, but firms now sort based on actual ability, not expected: $Q^{F,2} = y + \eta(s)$.

Test 3A – Regress $Q^{F,2} - Q^{F,1}$ on s

The difference, $Q^{F,2} - Q^{F,1}$, equals $y - E(y \mid s)$. Under the model's assumptions, movement across firms is only driven by new information. Controlling for initial firm absorbs information about employees' expected initial ability and earnings growth from training in first period. This approach is similar to adding firm fixed effects. The coefficient from regressing $Q^{F,2} - Q^{F,1}$ on s should be zero: information from the signal has already been used to decide which firm a worker is initially assigned to in the first period. [7] Unlike AP's Test 1, the regression results for Test 3 do not depend on training. Since all workers at better quality firms receive at least as much training as worse quality firms, training plays no role in determining subsequent movement.

Test 3B – Regress $Q^{F,2} - Q^{F,1}$ on b

The main empirical implication of the model is that variables that firms cannot observe in the first period and are correlated with new information used in the second help predict movement across firms. If we observe a variable correlated with new information, b, we can regress $Q^{F,2} - Q^{F,1}$ on b. The coefficient from this regression is $\dfrac{\operatorname{cov}(b, E(y \mid s) - y)}{\operatorname{var}(b)}$. As before, if b is positively related with productivity adjustments, the coefficient should be positive. But here, the coefficients are not affected if training depends on initial characteristics, since differences in training do not affect movement across firms.

In summary, the null hypothesis that firms statistically discriminate, under the model's assumptions, implies that changes in earnings and changes in firm quality should be uncorrelated

7. This prediction might not hold for the lowest or highest quality firms, since workers from the firms with the lowest quality in first period can only move up and vice versa. The junior to senior worker ratio assumption is violated if some workers' expected ability turns out to be higher in the second period than in the first. The violation becomes less important as the number of firms becomes large.

with s, but correlated with b. If training depends on initial characteristics, changes in earnings are no longer uncorrelated with s. But the predictions arising when using changes in firm quality are the same.

III. Data

Only a few variables are required for applying tests 1, 2, and 3. Most of them, however, are difficult to obtain. The regressions require a panel on workers with earnings and firm-level information each period, signal variables that we are interested in testing for signalling, and variables correlated with productivity that are initially unobservable to the firm, but available to the researcher.

Data comes from a sample of lawyers and MBA graduates from Canada. Lawyers and MBAs are ideal to look at in our model of signalling that supposes workers sort by firm and face different earnings paths depending on initial placement. Lawyers, for example, often begin their careers as associates. The key promotion at a law firm is from associate to partner, and partners earn much higher incomes than associates. Law firms often have reputations for handling claims of particular size and character. Some firms employ (or are run by) hundreds of lawyers while other firms are comprised of only three or four.[8] Many MBA graduates work in managerial positions that require superior etiquette and communication skills. These skills are difficult to observe externally. Firms that hire MBA graduates often promote from within.

Four datasets were joined to carry out the regressions. We describe each below.

III.1 The T1 Family File (T1FF)

The Small Area and Administrative Data Division of Statistics Canada maintains a file, called the T1FF, containing all individual tax records filed by Canadians, and grouped by family household. Children and spouses of tax filers are linked using social insurance numbers, names, and address information. Cook and Demnati (2000) describe how families are grouped using the T1FF. The data contain, among other variables, income from all sources including market and transfer income sources, the residential address of the tax filer, and marital status. The T1FF tracks tax filers and their annual returns records from 1982 to 1999.

Our dataset includes only MBAs and lawyers matched with at least one brother or father. A match more likely occurs for filers at earlier ages. This reduces the number of MBAs and lawyers with long employment histories. For example, if a family link is established for a lawyer to his father at age 17 in 1983, and graduates from law school at age 26, we only have data since graduation for 7 years. Fortunately, many graduates were linked to a brother or father when older, either because graduates were living at home, living with a brother, or filing from an old address while living elsewhere. The total main sample contains 3,064 MBA graduates and 1,576 law graduates.

8. For additional research about the labour market for lawyers, see Spurr (1987, 1990), O'Flaherty and Siow (1995), Landers et al. (1996), and Murphy et al. (1991).

III.2 The University Student Information System (USIS)

The USIS contains information on student participation and graduation from Canadian degree granting institutions obtained from administrative records provided at the individual level. USIS data is available annually, from 1974 to 1998. Most universities are covered in the USIS, but some schools never report. Of the 19 MBA schools in Canada, 14 are included in the USIS, and 14 of the 17 Canadian law schools are included. The missing schools are not related to rank, but to geography. Most schools in Quebec, for example, did not provide administrative student records to USIS. The exclusion of some schools does not violate the empirical analysis outlined in the last section, but leads to results conditional on graduating from a university that reports. Systematic exclusion of high ranked or low ranked schools might prevent precise estimates from regressing school quality on earnings, but such exclusion does not appear to be going on.

III.3 The Longitudinal Employment Analysis Program Database (LEAP)

The LEAP database is a longitudinal file, from 1978 to present, with Canadian business records at the firm level. The database includes any business remitting taxes on behalf of employees through Canada Revenue Agency's payroll deduction accounts. Employee identifiers allow linkage of the LEAP to the T1FF. Self-employed are not usually included in the LEAP files. However, even though partners at law firms receive self-employed income, they are usually included in the LEAP because law firms forward employment tax records on behalf of the partners.

III.4 School ranking data

We use average Graduate Management Admissions Test (GMAT) and Law School Admission Test (LSAT) percentile scores to rank business and law schools.[9] Average GMAT scores are from Canadian Business Magazine (2000) and average LSAT scores are from MacLean's Magazine (1997). It seems unlikely that rankings changed significantly over the 10 to 15 year period. Average entry GMAT percentile scores range from 52 to 92, with a mean of 0.824 and standard error of 0.081. Average entry LSAT percentile scores range from 74 to 93, with a mean of 0.772 and standard error of 0.075.

The matched dataset includes all full-time male MBAs and lawyers who graduated between 1974 and 1998, born between 1961 and 1973, and linked to at least one brother or father. We also restrict the sample to those who filed each year since graduation for at least seven years. We drop self-employed workers by removing any individual not matched to the LEAP data and remove any MBA graduate with reported self-employment earnings greater than payroll earnings.

All dollar amounts are converted to 2001 Canadian dollars using the Consumer Price Index. Observations with annual earnings under $6,000 are excluded. Earnings include payroll wages, self-employment income (mostly to partners), and other sources of income, including bonuses. Capital gains, interest, and rent income are excluded. Truncating everyone with earnings above the 99th percentile does not affect the results, so we do not present them here.

9. We also used an indicator variable for whether an MBA or lawyer graduated from a top-3 school. The results were quite similar and consistent with the results presented here using average percentile scores.

Basic descriptive statistics for the sample of MBAs and lawyers are presented in Table 1. For both groups, earnings exhibit substantial gains in the first eight years after graduation. Median lawyer earnings grow by a factor of 1.7 over the 8-year period. The median MBA graduate, 8 years after leaving school, earned $93,861 while the median lawyer earned $82,134. Earnings also vary greatly among workers with same levels of experience. Almost every lawyer and MBA graduate works within a Metropolitan Area, and more than 50% of the sample works in one of the three largest cities: Montreal, Toronto, or Vancouver.

We measure firm quality as the adjusted average earnings for workers with 7 or more years experience. For the sample of MBAs and lawyers with at least seven years of experience, we regress log earnings on a cubic in experience, a full set of year indicators, province indicators, Metropolitan Area indicators, a lawyer indicator interacted with experience, and a full set of firm dummies. The regression uses 38,043 observations and 4,983 firms. The estimated firm effects are used for the firm quality measure.

The variables used that correlate with worker productivity but are assumed initially unobservable by the firm are fathers' and brothers' adjusted average earnings. Variables for worker performance, efficiency, or actual productivity are unfortunately not available. But Solon (1999) finds substantial correlation in earnings and general ability measures among fathers and sons, and brothers. We take the full sample of fathers and brothers between ages 25 and 55 and regress log earnings on aquartic in age, a set of year indicators, province indicators, and major city indicators. The mean residual is used as the variable correlated with unobserved productivity, and is merged back to the MBA and lawyer samples.

A potential concern arises in using these variables if fathers or brothers with superior earnings also have better connections to help graduates obtain jobs at fast-track firms. For example, previous research estimates relatively high rates of occupational inheritance among lawyers. Laband and Lentz (1992), for example, find 11% of lawyers in a sample of California were sons of lawyers. It seems more likely, however, that relatives might help with initial placement rather than later. In that case, the results are biased against concluding signalling occurs, since graduates with better family connections obtain initial jobs superior to those corresponding just with their initial expected ability. The coefficient on father or brother earnings would fall, not rise over time, as the model suggests.

IV. Results

IV.1 Altonji and Pierret's Tests 1A and 1B using father's and brother's earnings for b

Table 2 carries out Tests 1A and 1B on MBA graduates. We regress log earnings on experience, interacted with school rank, and brother's or father's earnings. Each regression also includes a cubic in experience, year fixed effects, and an indicator for whether residing in Montreal, Toronto, or Vancouver in 1985 before graduation (not shown). All reported standard errors and test statistics are based on Huber-White standard errors, clustered by individual. The sample includes MBAs who reported positive earnings in each year since graduation for at least seven years.

AP's model of signalling predicts that the coefficient on school rank should not change with experience, but the coefficient on brother's or father's earnings, being correlated with new information about worker productivity, should increase with experience. This prediction does not hold in Table 2. Column 1 shows the average effect of school rank on log earnings. A 10-point increase in the average entry percentile score of the MBA's graduate school (about a one standard error increase) raises earnings by about 9.3%. Both school rank and father's earnings have large positive correlations with MBA earnings. However, the coefficient on school rank rises considerably with experience, a finding inconsistent with the signalling model when training and initial expected ability is independent. The positive coefficient is even inconsistent without signalling. If firms know worker productivity from the start, the effect from both school rank and father's earnings should remain constant with experience.

In Table 3, the school rank measure is the graduate school's average percentile LSAT score. The effect on earnings from father's and brother's log earnings rises with experience, as it does for MBAs. The school rank coefficient also rises with experience, which is again contrary to our model's predictions without training. A one standard deviation difference in school rank (0.08 for lawyers) corresponds with a 0.102-point difference in log earnings for lawyers in their first year and a 0.150-point difference in the tenth year after graduation.

IV.2 Tests 2A and 2B using initial firm fixed effects and father's and brother's earnings for b

We show the same analysis but add initial firm fixed effects in Tables 4 and 5. Only cases where there are at least two workers in an initial firm are included. Adding initial firm fixed effects controls for average earnings differences and earnings growth differences across firms, and reduces the bias towards estimating a positive coefficient on school rank with experience.

The relation with school rank on earnings for MBAs drops only about 15% in Table 4, suggesting large within-firm earnings differences for MBAs. The rise in the coefficient with experience, however, disappears after controlling for initial firm. Earnings growth differences across MBAs are therefore mostly attributable to across firm earnings growth differences. The point estimates of father's and brother's earnings interacted with experience are positive, but measured imprecisely.

Including initial firm fixed effects reduces the importance of school rank for lawyers. The coefficient on percentile LSAT score on earnings for workers with one-year experience falls from 1.7 to .36, and no longer increases with experience. The relationship between brother's earnings and lawyer's earnings does, however, rise with experience. The point estimates are consistent with our model, but estimated imprecisely. With only two or three workers in some firms, measuring relative earnings changes across workers who started with the same firm is difficult. We are not able to derive decisive conclusions from the firm fixed-effects results. But they do reinforce the results below that use changes in firm quality for the dependent variable.

IV.3 Tests 3A and 3B using firm quality and father's and brother's earnings for b

Table 6 reports the least squares estimates with changes in firm quality as the dependent variable instead of changes in earnings. Firm quality is measured as the mean log earnings of workers with at least seven years experience by firm, adjusted for age and year.
The school rank effect on firm quality remains about constant over time (column 4), but the effect from father's earnings rises (column 5). The coefficient on father's log earnings is about zero for workers with one year experience, but climbs to 0.130 after 10 years experience. The coefficient on brother's earnings also rises with experience, although the overall effect on firm quality is not as strong as that from father's earnings. School rank on firm quality does not change with experience. The estimated outcome of a one standard deviation increase in school rank is to raise firm quality by 3.8%. This influence remains about the same over time.

Table 7 shows the same analysis for lawyers. School rank, in columns 1 and 3, has a strong effect on average firm quality. The school rank effect does not change with experience (in columns 4, 6, 9, and 11). The effect of father's earnings and brother's earnings, however, does (columns 5, 6, 10, and 11).

In column 6 for both tables, our model predicts the school rank effect should fall with experience simultaneously with a rise in the effect from father's or brother's earnings (in column 6 or 11), if the two variables are positively correlated. The product of $-\operatorname{cov}(s_i, b_i) / \operatorname{var}(s_i)$ —the negative of the coefficient of the regression of b_i on s_i —times the coefficient on the interaction between father's earnings and experience should equal the coefficient on the interaction between school rank and experience. For the MBA sample, the coefficient from regressing father's earnings on experience is 0.816 (with a standard error of 0.142), so the predicted coefficient on school rank times experience / 100 is -0.001. The confidence range around our estimate of this term in column 6 is clearly within this range. The small correlation between school rank and family earnings explains why adding family earnings variables do little to change the effect of school rank over time.

IV.4 Year-to-year changes

Figures 1 to 4 summarize the empirical results with the observable and unobservable signal effects graphed over 10 years of experience. Figure 1 shows the effects from school rank and father's earnings, interacted with each year of experience from 0 to 9. The estimates use the combined lawyer and MBA samples. The regression equation that generated these estimates is the same as that used in Table 3 and Table 4, column (6), but includes individual experience interactions with the signal variables instead of a linear specification. The effects from both variables are graphed on different scales in the figure, with standard error bars indicated.

In both Figures 1 and 2, the positive interaction with school rank and log earnings rises with experience. The correlation with school rank and graduate's earnings jumps considerably between the lawyer's third and fifth year after graduation. Possibly this increase is related to lawyers becoming partners around this time, but the jump occurs earlier than we would expect for this to be case: associates tend to become partners after their sixth or seventh year. The significant rise in the coefficient for school rank can only be explained in our model by a positive relationship between training and expected initial ability.

Figures 3 and 4 show the school rank and family earnings effects on firm quality. A worker's father's or brother's earnings becomes an increasingly good predictor of his firm quality over experience. The effect of father's and brother's log earnings on firm quality rises steadily over time. School rank, however, does not predict whether a worker moves to a higher or lower ranked firm. In both figures, the school rank coefficient remains about the same over experience. Taken together, the results support our model in which employers use the school rank of incoming MBA and law graduates to predict worker productivity.

V. Summary and discussion

Altonji and Pierret (2001a) suggest testing for signalling by examining whether earnings over time are uncorrelated with observable employer characteristics but are correlated with unobservable productivity measures. Intuitively, employers' expectations of future worker productivity based on observable signals should correspond with actual worker productivity if earnings growth does not depend on initial expectations. Earnings adjust only by updating productivity expectations from new information. Initially observable characteristics should be unrelated to earnings adjustments, hiring, and firing decisions. But variables correlated with new information about productivity, such as father's earnings, should be correlated with these adjustments. This is what AP find using education and race for observable characteristics and AFQT scores for unobservable variables correlated with ability.

When we apply this methodology to MBA and law graduates to examine if employers use school rank for predicting ability, the school rank effect on earnings rises with experience. This finding cannot be explained by AP's model, whether we allow for signalling or not. Many studies, however, conclude that it is unrealistic to assume earnings growth is uncorrelated with school rank. For example, training relates positively with AFQT scores, education, and gender.

The effects from signalling extend beyond the point when workers' abilities are known when earnings growth and career opportunity depend significantly on initial placement. Workers whose ability deviates from expected ability may end up with earnings quite different from those if productivity was known when entering the labour market. We extend AP's test for signalling to account for this possibility by utilizing information about a worker's initial placement. Controlling for workers' initial firm controls for the average amount of training or promotion opportunity within a firm. This helps identify earnings adjustments from updated productivity expectations. If high-earnings firms tend to train junior employees more, we can also use changes in firm quality as the dependent variable. In a setting where high ability workers sort into high paying firms, updated information about worker productivity should relate to whether workers move to higher or lower paying firms. Old information should not be correlated with firm movement.

As predicted by this model, adding initial firm fixed effects to the regression of school rank and father's earnings on changes in firm quality leads to a zero coefficient on school rank and a positive coefficient on brother's earnings. Moving to a high-paying firm is also positively correlated with brother's and father's earnings but uncorrelated with school rank. These findings provide evidence that signalling occurs among entry MBAs and lawyers and that initial placement plays an important role in determining future relative earnings growth. Controlling for initial firm, for example, removes most of the entry premium and future premium associated with obtaining a law degree from a top school.

More generally, a key point we make is that signalling can affect career opportunity and long-run earnings when earnings growth relates to initial placement. Models that derive the value of a productivity signal do not typically include this characteristic. Our tests presented here cannot determine the magnitude of the impact from this behaviour, since signalling is identified by examining earnings and firm quality changes over experience, not the actual amounts. Ideally, we would like to use an exogenously allocated signal that varies between two groups with similar ability. Tracking earnings outcomes over time would then determine the signal's long-run effect. We believe this area of research merits further investigation.

Table 1
Descriptive statistics for male MBAs and lawyers

Variable	Means	
	MBAs	Lawyers
Age at graduation	28.4	27.0
	(2.4)	(2.3)
Year of graduation	87.7	87.6
	(2.1)	(2.0)
Graduated from top 3 ranked school	0.341	0.131
	0.474	0.338
GMAT/LSAT average score by school	0.824	0.772
	0.081	0.075
Work in Montreal, Toronto, or Vancouver	0.597	0.577
	0.480	0.494
	Medians	
Earnings, 1 year experience	75,488	48,375
Earnings, 2 year experience	79,084	60,308
Earnings, 3 year experience	81,910	65,825
Earnings, 4 year experience	84,159	70,922
Earnings, 5 year experience	87,094	75,612
Earnings, 6 year experience	89,130	78,076
Earnings, 7 year experience	91,856	80,622
Earnings, 8 year experience	93,861	82,134
Sample size	3,064	1,576

Notes: Standard deviations are in parentheses. All earnings are annual, converted to 2001 Canadian dollars.

Table 2
Effects of school rank, father's wage, and brother's wage on MBA graduates' earnings

Dependent variable: log earnings; school rank measure: average entry GMAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	0.933		1.07	0.754	0.803	0.803		1.289	0.852	1.289	0.862
	(0.053)***		(0.096)***	(0.110)***	(0.110)***	(0.110)***		(0.136)***	(0.154)***	(0.136)***	(0.155)***
School rank * experience / 100				0.073		0.062			0.082		0.08
				(0.028)***		(0.028)**			(0.029)***		(0.030)***
Log of father's earnings		0.077	0.066	0.066	0.008	0.008					
		(0.013)***	(0.014)***	(0.014)***	(0.015)	(0.015)					
Log of father's earnings * experience					0.0174	0.0183					
					(0.0056)***	(0.0055)***					
Log of brother's earnings							0.052	0.039	0.039	0.016	0.02
							(0.020)***	(0.021)*	(0.021)*	(0.024)	(0.024)
Log of brother's earnings * experience										0.004	0.003
										(0.004)	(0.004)
Observations	12,502	12,502	12,502	12,502	12,502	12,502	11,087	11,087	11,087	11,087	11,087
Number of individuals	1,438	1,438	1,438	1,438	1,438	1,438	1,260	1,260	1,260	1,260	1,260
R-squared	0.16	0.17	0.18	0.19	0.19	0.19	0.19	0.23	0.23	0.23	0.23

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09. The sample includes only males.

Table 3
Effects of school rank, father's wage, and brother's wage on lawyers' earnings

Dependent variable: log earnings; school rank measure: average entry LSAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	1.607		1.745	1.336	1.748	1.367		1.897	1.479	1.896	1.515
	(0.094)***		(0.159)***	(0.176)***	(0.159)***	(0.176)***		(0.168)***	(0.203)***	(0.168)***	(0.202)***
School rank *				0.066		0.062			0.065		0.06
experience / 100				(0.030)**		(0.030)**			(0.034)*		(0.034)*
Log of father's earnings		0.095	0.082	0.082	0.048	0.052					
		(0.016)***	(0.018)***	(0.018)***	(0.023)**	(0.023)**					
Log of father's earnings *					0.0078	0.0066					
experience					(0.0038)**	(0.0039)*					
Log of brother's earnings							0.128	0.109	0.109	0.07	0.078
							(0.022)***	(0.023)***	(0.023)***	(0.028)**	(0.028)***
Log of brother's earnings *										0.006	0.005
experience										(0.004)	(0.005)
Observations	11,572	11,572	11,572	11,572	11,572	11,572	9,054	9,054	9,054	9,054	9,054
Number of Individuals	1,232	1,232	1,232	1,232	1,232	1,232	974	974	974	974	974
R-squared	0.28	0.31	0.32	0.32	0.32	0.32	0.3	0.32	0.32	0.32	0.32

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09. The sample includes only males.

Table 4
Effects of school rank, father's wage, and brother's wage on MBA graduates' wages with initial firm fixed effects

Dependent variable: log earnings; school rank measure: average entry GMAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	0.793		0.77	0.831	0.77	0.848		0.909	0.911	.875	0.956
	(0.099)***		(0.230)***	(0.197)***	(0.230)***	(0.197)***		(0.229)***	(0.223)***	(0.323)***	(0.338)***
School rank *				-0.012		-0.016			0.004		-0.015
experience / 100				(0.048)		(0.049)			(0.055)		(0.085)
Log of father's wage		0.032	0.044	0.044	0.018	0.018					
		(0.024)	(0.023)*	(0.023)*	(0.022)	(0.022)					
Log of father's wage *					0.005	0.005					
experience					(0.005)	(0.005)					
Log of brother's wage							0.061	0.06	0.059	0.042	0.041
							(0.048)*	(0.057)	(0.057)	(0.057)	(0.057)
Log of brother's wage *										0.002	0.002
experience										(0.009)	(0.009)
Observations	6,038	6,038	6,038	6,038	6,038	6,038	5,368	5,368	5,368	5,368	5,368
Number of Individuals	676	676	676	676	676	676	608	608	608	608	608
R-squared	0.41	0.45	0.47	0.47	0.47	0.47	0.43	0.45	0.45	0.56	0.56

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09. The sample includes only males.

Table 5
Effects of school rank, father's earnings and brother's earnings on lawyers' earnings with initial firm fixed effects

Dependent variable: log earnings; school rank measure: average entry LSAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	0.262		0.226	0.376	0.226	0.362		-0.043	-0.004	-0.043	0.054
	(0.121)**		(0.268)	(0.271)	(0.268)	(0.269)		(0.274)	(0.343)	(0.274)	(0.338)
School rank *				-0.030		-0.027			-0.008		-0.019
experience / 100				(0.054)		(0.053)			(0.062)		(0.062)
Log of father's earnings		0.014	0.009	0.009	0.036	0.035					
		(0.022)	(0.024)	(0.024)	(0.034)	(0.033)					
Log of father's earnings *					-0.005	-0.005					
experience					(0.006)	(0.006)					
Log of brother's earnings							0.108	0.124	0.124	0.043	0.041
							(0.032)***	(0.037)***	(0.037)***	-0.050	-0.050
Log of brother's earnings *										0.016	0.017
experience										(0.009)*	(0.009)*
Observations	4,967	4,967	4,967	4,967	4,967	4,967	4,103	4,103	4,103	4,103	4,103
Number of Individuals	578	578	578	578	578	578	489	489	489	489	489
R-squared	0.16	0.17	0.18	0.19	0.19	0.19	0.19	0.23	0.23	0.23	0.23

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09, The sample includes only males.

Table 6
Effects of school rank, father's earnings and brother's earnings on MBA graduate's firm quality

Dependent variable: adjusted log mean wage within firm; school rank measure: average entry GMAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	0.6508		0.4688	0.2780	0.4537	0.3858		0.4685	0.5794	0.4839	0.4556
	(0.1186)***		(0.1784)***	(0.2144)	(0.1009)***	(0.1180)***		(0.1166)***	(0.1600)***	(0.1032)***	(0.1247)***
School rank *				0.0365		0.0158			-0.0221		0.0069
experience / 100				(0.0408)		(0.0360)			(0.0380)		(0.0304)
Log of father's earnings		0.1066	0.1046	0.1047	0.0086	0.0094					
		(0.0317)***	(0.0330)***	(0.0330)***	(0.0149)	(0.0150)					
Log of father's earnings *					0.0130	0.0128					
experience					(0.0052)**	(0.0052)**					
Log of brother's earnings							0.0872	0.0608	0.0611	0.0258	0.0262
							(0.0332)***	(0.0231)***	(0.0231)***	(0.0215)	(0.0216)
Log of brother's earnings *										0.0093	0.0092
experience										(0.0049)*	(0.0049)*
Observations	10,186	10,186	10,186	10,186	10,186	10,186	8,764	8,764	8,764	8,764	8,764
R-squared	0.02	0.03	0.04	0.04	0.03	0.03	0.02	0.02	0.02	0.02	0.02

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09, The sample includes only males.

Table 7
Effects of school rank, father's wage, and brother's wage on lawyers' firm quality

Dependent variable: adjusted log mean wage within firm; school rank measure: average entry LSAT percentile score
OLS estimates (robust standard errors)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
School rank / 100	0.8560		0.9422	0.8858	0.9414	0.8906	0.8560	0.9207	0.8011	0.9189	0.8164
	(0.0900)***		(0.1165)***	(0.1360)***	(0.1164)***	(0.1365)***	(0.0900)***	(0.1301)***	(0.1592)***	(0.1303)***	(0.1589)***
School rank *				0.0138		0.0124			0.0265		0.0227
experience / 100				(0.0313)		(0.0313)			(0.0334)		(0.0334)
Log of father's wage		0.0581	0.0555	0.0554	0.0193	0.0199					
		(0.0144)***	(0.0158)***	(0.0158)***	(0.0136)	(0.0137)					
Log of father's wage *					0.0140	0.0130					
experience					(0.004)***	(0.004)***					
Log of brother's wage								0.0544	0.0543	0.0156	0.0165
								(0.0185)***	(0.0185)***	(0.0205)	(0.0205)
Log of brother's wage *										0.0084	0.0082
experience										(0.0049)*	(0.0049)*
Observations	9,768	9,768	9,768	9,768	9,768	9,768	7,668.00	7,665.00	7,662.00	7,659.00	7,656.00
R-squared	0.12	0.14	0.14	0.14	0.14	0.14	0.12	0.13	0.13	0.13	0.13

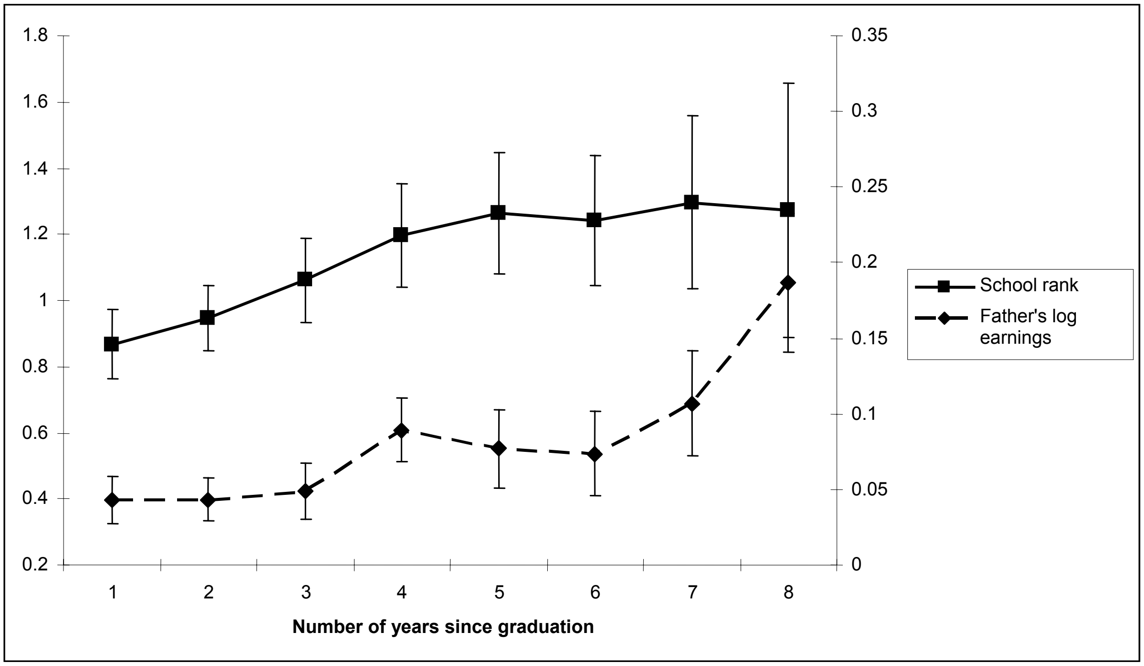
* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All equations control for a cubic in experience, year effects, and an indicator for whether working in Montreal, Toronto, or Vancouver. Standard errors are Huber/White standard errors computed accounting for the fact that there are multiple observations for each worker. The standard error for school rank is 0.09, The sample includes only males.

Figure 1
Effects of school rank and father's log earnings on graduate's log earnings and 1 to 8 years of experience

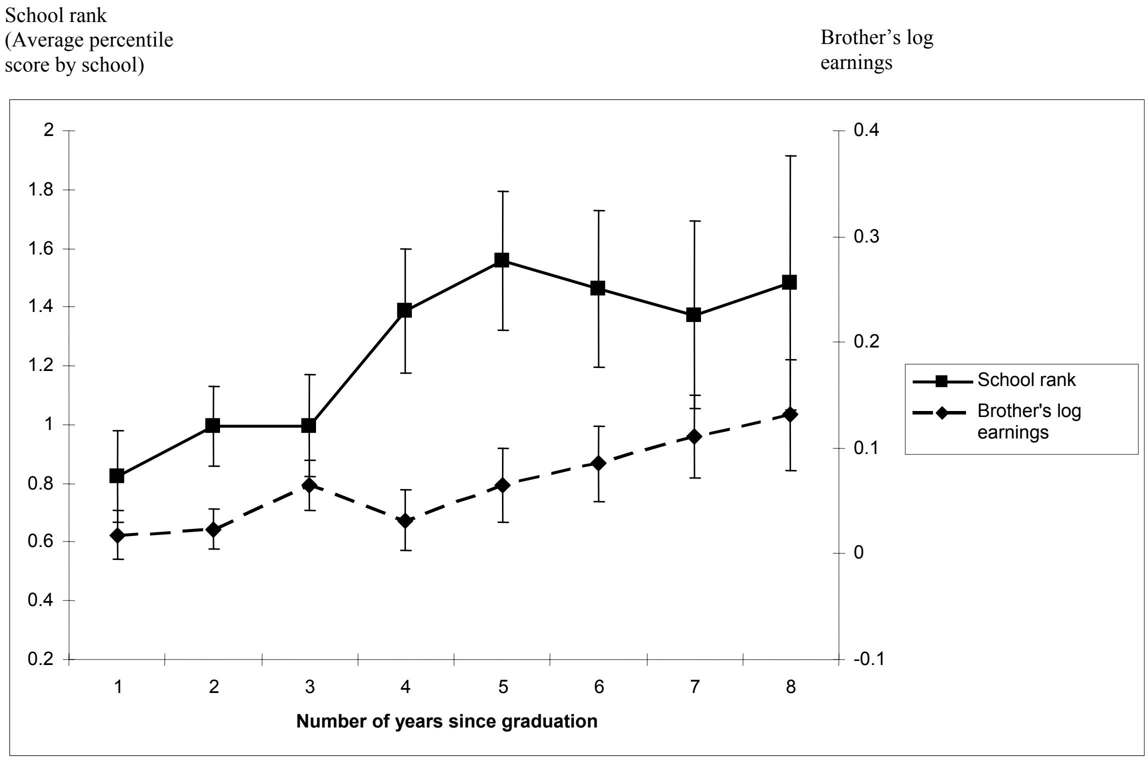
School rank
(Average percentile score by school)

Father's log earnings



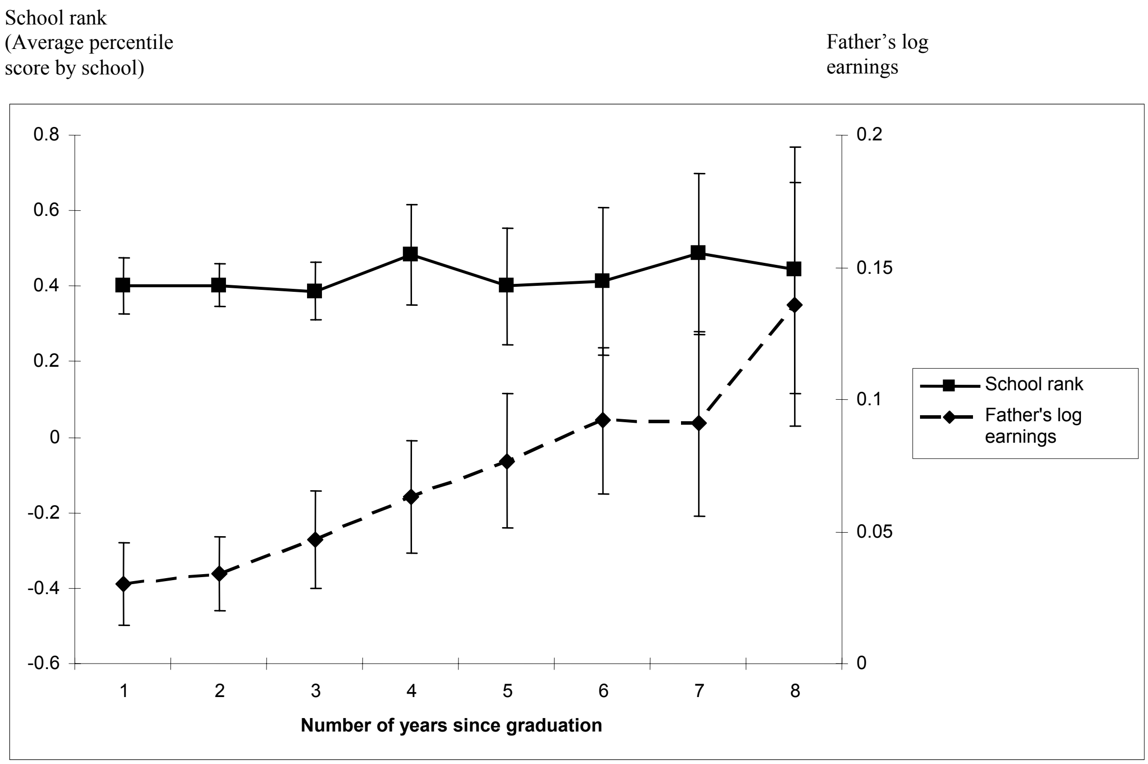
Note: Table displays coefficient estimates (and +/- 1 standard error bands) from regression of earnings at a given number of years since graduation on the rank of the school attended and the standardized log of father's earnings.

Figure 2
Effects of school rank and brother's log earnings on graduate's log earnings and 1 to 8 years of experience



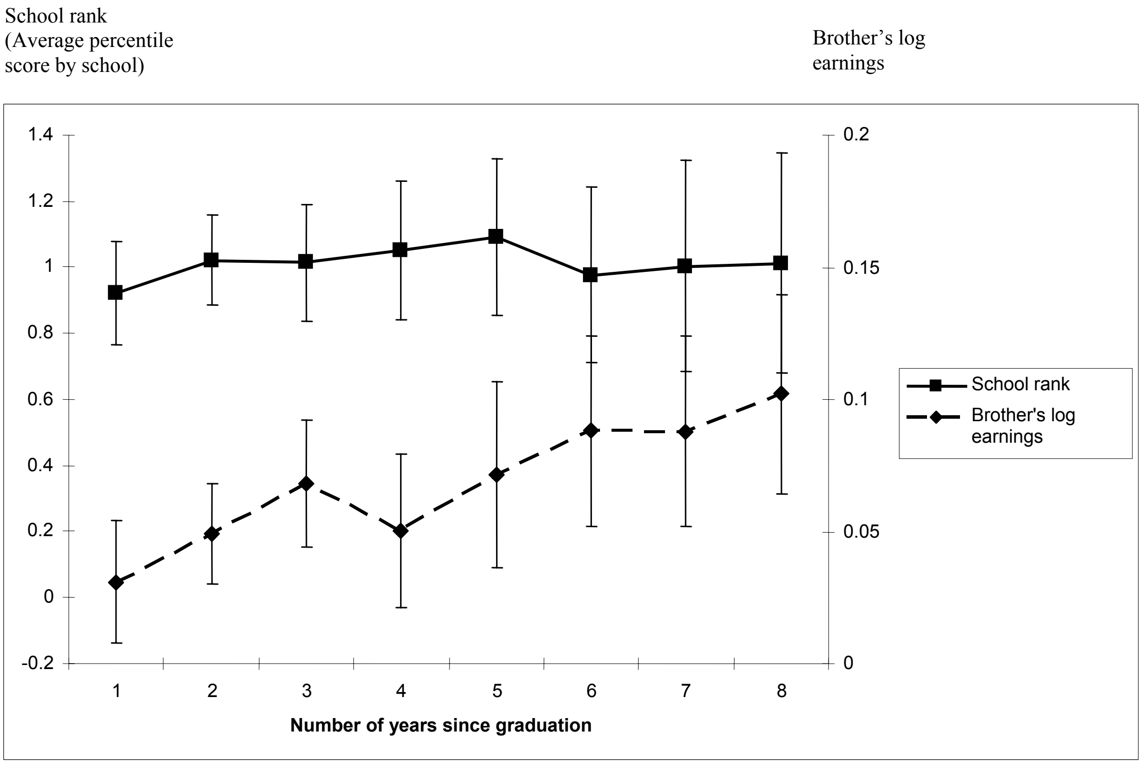
Note: Table displays coefficient estimates (and +/- 1 standard error bands) from regression of earnings at a given number of years since graduation on the rank of the school attended and the standardized log of brother's earnings.

Figure 3
Effects of school rank and father's log earnings on firm quality and 1 to 8 years of experience



Note: Table displays coefficient estimates (and +/- 1 standard error bands) from regression of firm quality at a given number of years since graduation on the rank of the school attended and the standardized log of father's earnings.

Figure 4
Effects of school rank and brother's log earnings on firm quality and 1 to 8 years of experience



Note: Table displays coefficient estimates (and +/- 1 standard error bands) from regression of firm quality at a given number of years since graduation on the rank of the school attended and the standardized log of brother's earnings.

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