Returns to Schooling:

Results when the Counterfactual is Observed

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Abstract

Estimating the returns to education is difficult in part because we rarely observe the counterfactual of the wages without the education. One of the advantages of examining the returns to an MBA is that most programs require work experience before being admitted. These observations on wages allow us to see how productive people are before they actually receive an MBA and to identify and correct for potential bias in the estimated treatment effect. Our results show that unobserved ability is generally positively correlated with obtaining an MBA, and especially so for those in top programs. However, for full-time MBA students attending schools outside of the top-25 the correlation is *negative*, implying that when we properly control for ability the estimated returns increase. We show that this arises neither because of a dip in wages before enrolling nor because these individuals are weaker in observed ability measures than those who do not obtain an MBA. In fact, we show that those who do not obtain an MBA are stronger in dimensions such as workplace skills that are not easily measured. Including proxies for these skills substantially reduces the gap between the OLS and fixed effects estimates, suggesting that at some margins schooling and ability serve as substitutes.

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1 Introduction

While it is generally accepted that more education leads to an increase in wages, an extensive literature attempts to quantify this effect. The difficulty lies in untangling the effect of education on wages from the unobservable personal traits that are correlated with schooling. Because schooling is usually completed before entrance into the labor market, previous research has relied on instrumental variables, such as proximity to colleges or date of birth,[1] or exclusion restrictions in a structural model to identify the effect of schooling on wages.[2] Alternatively, several studies have used data on siblings or twins to identify the treatment of additional years of schooling, while controlling for some degree of innate ability and family environment.[3]

We use data on registrants for the Graduate Management Admissions Test (GMAT)–individuals who were considering obtaining an MBA–to estimate the returns to an MBA and show how these returns depend upon the method used to control for unobserved ability. Unlike most other schooling, MBA programs generally require work experience. Figure 1 plots the cumulative distribution function for post-collegiate work experience before enrolling. As shown in the figure, almost ninety percent of those who enroll in an MBA program have over two years of work experience. That individuals work before obtaining an MBA allows us to use panel data techniques both to estimate the returns to an MBA and to quantify the biases associated with not having good controls for unobserved ability. The treatment effect of an MBA is thus identified from earnings on the same individual before and after receiving an MBA.

When the return to an MBA is restricted to be the same across program types and qualities we estimate a return for males of 9.4%.[4] This coefficient falls by about a third when standard human capital measures (test scores, grades) are included, and falls by another third to 4.8% when we control for unobserved ability using fixed effects, a result consistent with the commonly expected positive correlation between ability and returns to schooling. This positive ability bias is higher than those reported by many of the studies using identical twins. However, comparisons across these studies is difficult as the samples are different and because there may be more measurement error present in retrospective recall of years of schooling than in whether or not one has received an

[1]See Angrist and Krueger(1991) and Kane and Rouse (1995) among many others.

[2]See Willis and Rosen (1979) and Keane and Wolpin (1997, 2000, 2001).

[3]See, for example, Ashenfelter and Krueger (1994) and Ashenfelter and Rouse (1998).

[4]Similar results are seen for females and are reported in section 4.

MBA.

While disentangling the returns to schooling from the returns to unobserved ability is difficult, estimating the returns to college quality is harder still. No good instruments have been found for college quality and the sample sizes of twins are too small to obtain accurate estimates of the returns to college quality.[5] By using data on pre-MBA earnings, we are able to distinguish how the treatment effect varies across programs quality. Controlling for selection via observables drops the return to attending a top-10 program over a program in the lowest tier from 33% to 25%. When fixed effects are included, the gap falls to 11%. This decline is due to both a drop in the returns to attending a top-10 program, and to an increase in the return to attending a program outside the top-25. In fact, our OLS estimates show virtually no return for those attending programs outside the top-25, while the fixed effects estimates are around eight percent.

Instrumental variable techniques have also shown higher returns to schooling than OLS estimates. However, many of the standard reasons given for the higher IV estimates do not hold here. As Card (1999, 2001) shows, the IV estimates 1) mitigate the measurement error problem associated with misreported years of schooling and 2) measure the marginal, rather than the average, treatment effect.[6] While both of these are potential reasons for the finding of higher IV estimates, neither applies to fixed effects. In contrast to IV estimates, using fixed effects tends to exacerbate measurement error, thus biasing estimates *downward*.[7] Further, both the OLS and fixed effect estimates are of the average treatment effect for a particular type of program.

Why are the fixed effects estimates higher for those who do not attend top-25 schools? While having wage observations both before and after schooling presents many advantages, it also introduces problems associated with the program evaluation literature.[8] In particular, Ashenfelter (1978) documented the dip in wages which took place before individuals enrolled in job training programs,

[5]Researchers have attempted to estimate the return to college quality by controlling for selection with observables (Black et al. 1997, James et al. 1989, and Loury and Garman 1995), matching based upon similar application and acceptance sets (Dale and Krueger 2000), and structurally estimating the decision to attend particular colleges (Brewer et al. 1999, and Arcidiacono 2004a, 2004b).

[6]The measurement of marginal, rather than average, treatment effects is also an issue for the one study that uses fixed effects to estimate the returns to schooling– Angrist and Newey (1991). Identification of the OLS coefficient comes from the full sample while identification of the fixed coefficient comes only from those who had a break in schooling, a group which is less than twenty percent of the sample.

[7]See Hsiao (1986) for a discussion of measurement error in panel data models.

[8]See Heckman, LaLonde, and Smith (1999) for a review.

something which may also occur when individuals go back to school. Such a dip would cause us to over-estimate the return to an MBA in a fixed effects framework. However, a similar dip in wages is *not* found in our data.[9]

An alternative explanation is that, at some margins, schooling and ability serve as substitutes. While those who attend full-time MBA programs outside of the top-25 have higher test scores and higher grades than those who do not attend, they may be weaker on other traits which are not easily observable but also important for labor market success. For example, obtaining an MBA may provide one with job contacts– something those who do not choose to obtain an MBA may already have. In fact, the richness of the data shows that those who do not obtain an MBA are actually stronger in areas not generally measured by standard survey data. Controlling for these factors explains much of the difference between the fixed effects and OLS estimates. Hence, ability and schooling may sometimes serve as substitutes.

The rest of the paper proceeds as follows. Section 2 describes the data. The identification strategy is discussed in section 3. Estimates of the treatment effects are presented in section 4. Section 5 examines possible explanations for the higher fixed effect estimates for those who attended institutions outside the top-25. Section 5 concludes.

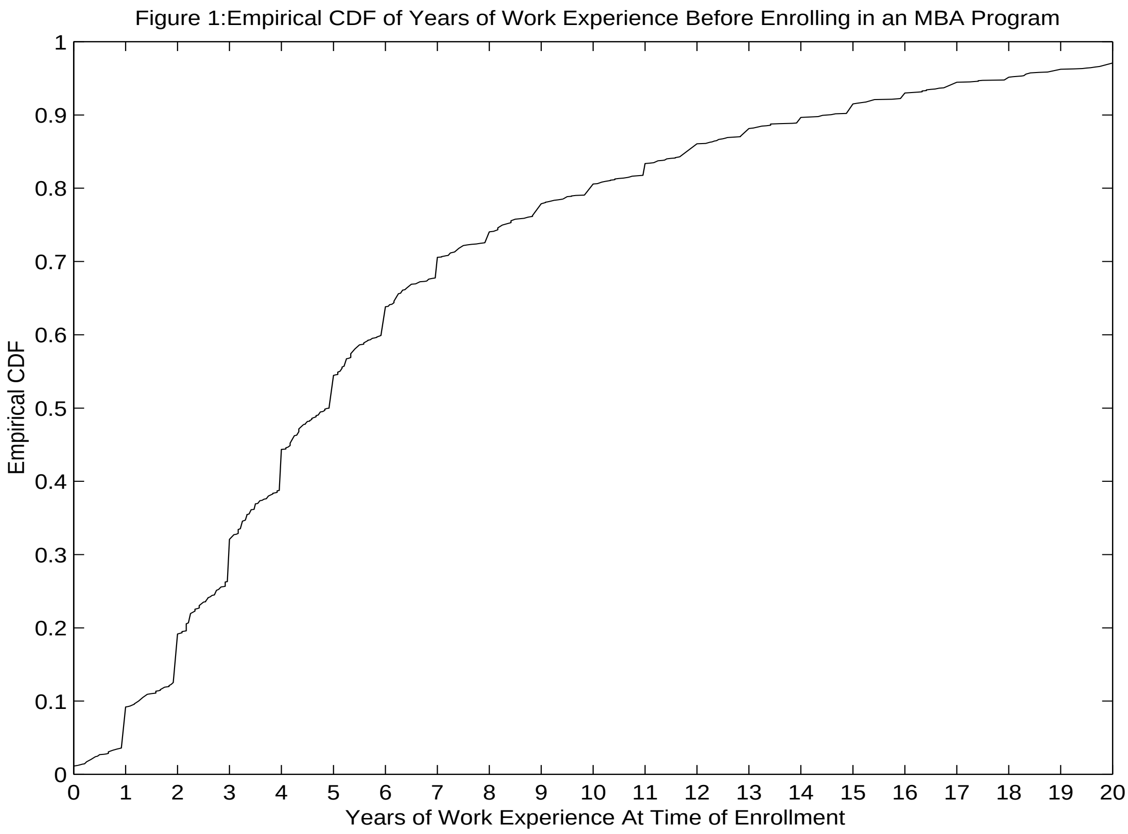
2 Data

We utilize a longitudinal survey of registrants for the Graduate Management Admissions Test (GMAT) to estimate the economic returns to an MBA. The GMAT exam, an admissions requirement for most MBA programs, is similar to the SAT for undergraduates without the competition from the ACT. The survey, sponsored by the Graduate Management Admissions Council (GMAC), was administered in four waves, beginning in 1990 and ending in 1998.[10] In addition, survey responses were linked to GMAC's registration and test data, which includes personal background information and GMAT scores. The initial sample size surveyed in wave 1 was 7006, of which 5602 actually took the test. We focus our analysis on the sample of test takers.

The key feature of the data is that we observe earnings both before and after an individual

[9]We test a number of other possible explanations, including allowing individuals to have different returns to experience (Baker 1997) and allowing the treatment effect of an MBA to vary at the individual level. The average treatment effect is consistently around 8% for those individuals who receive MBA's at an institution outside the top-25.

[10]The same survey has been used by Montgomery (2002) and Montgomery and Powell (2003).



receives an MBA. In Table 1 we show the distribution of the individuals across five activities and the four survey waves. A substantial portion of the sample have pre- and post-MBA wages, obtaining their MBA sometime between wave 2 and wave 3.[11]

Using the four waves, we construct current hourly wages spanning the years 1990 to 1998. Of the 15,715 observations across the four waves, 10,612 reported full-time jobs and the corresponding wage. The difference between the two numbers can largely be explained by individuals being in school. Of the 4,103 observations where no full-time job or wage was reported, 1806 were either full-time undergraduates, full-time MBA's, or in some other professional program.

Note that the 15,715 observations is a selected sample, as the total number of possible replies to the survey would be 22,408 had no attrition occurred among the test takers. Those who dropped out of the sample were substantially less likely to have entered into an MBA program, which is not

[11]Over twenty percent of those who respond in all four waves are still at their undergraduate institution despite the work requirements associated with MBA programs. This is explained by GMAT scores being valid at most institutions up to five years after the individual took the test.

Table 1: Distribution of Students Across School and Work†

	Wave 1	Wave 2	Wave 3	Wave 4
Working, No MBA	81.9%	80.8%	68.4%	55.1%
Working, Have MBA	0.0%	2.3%	24.5%	42.3%
Business School	0.0%	13.3%	4.5%	0.2%
Other Grad. School	1.1%	2.8%	2.6%	2.4%
4-year Institution	17.0%	0.7%	0.0%	0.0%
First Survey Response	Jan. 1990	Sept. 1991	Jan. 1993	Jan. 1997
Last Survey Response	Dec. 1991	Jan. 1993	Nov. 1995	Nov. 1998

†Sample is those who responded to all four surveys (N=3244). For the purposes of this table, part-time and executive students who had full-time wage observations while in business school are treated as being in the labor market.

surprising given that the survey was clearly geared towards finding out information about MBA's. However, conditional on obtaining or not obtaining an MBA, those who attrit look very much like those who remained in the sample in terms of their gender, race, test scores, and labor market outcomes. Within our sample MBA's may also have different characteristics than non-MBA's, again emphasizing the importance of our preferred estimation strategy: identifying the effect of an MBA using before and after earnings for those who received an MBA, i.e. the treatment effect on the treated.

Wave 1 sample characteristics are reported in Table 2 by sex and by whether the individual enrolled in an MBA program by wave 4. The first row gives the years of full-time experience since the age of 21. At over 6.5 years, men report one year more experience than women. Interestingly, women who eventually enroll in MBA programs have more experience at wave 1 than those who do not, but the reverse holds for men. This one year gap between men and women is also reflected in their ages, with an average age of close to 29 for men and 28 for women. Little difference in wave 1 wages are seen for men across future MBA enrollment status, though women who enrolled in an MBA program had wages that were five percent higher than those who did not obtain an MBA.

Differences in test scores and undergraduate grade point average emerge across both sex and future MBA status. Men performed better on the quantitative section of the GMAT than women, while women had higher average undergraduate grades. Both the GMAT score and undergraduate grades are higher for those who enrolled in an MBA program than those who did not, suggesting higher ability in the MBA sample.

MBA programs often offer a number of different paths to completing an MBA. The three major paths are full-time, part-time, and executive. The typical full-time program takes two years to complete. While the first two paths are fairly common in higher education, the third is unique to MBA's. Executive MBA's are usually offered on a one day per week or an alternating weekend basis, generally taking two years to complete. One feature of these executive programs is their substantially higher tuition. Tuition at the Fuqua School of Business, for example, is $79,500 for their executive program during the 2003-04 academic year. However, the opportunity cost of these programs, as well as part-time programs, is generally lower as they allow individuals to continue working full-time while in school.

Table 3 presents descriptive statistics by sex and type of program conditional on enrollment by wave 4. Substantial differences exist in the characteristics of the individuals across the different types of programs. Younger individuals with less experience are generally found in the full-time programs, with older, more experienced workers in the executive programs. Consistent with this, those who eventually obtain an MBA in a full-time program have the lowest wave 1 wage and lower marriage rates.

Conditional on program type, MBA programs may still differ in quality. We use 1992 rankings of U.S. News & World Report as our quality measure (U.S. News 1992). In particular, we distinguish between schools ranked in the top ten, the next fifteen, and outside the top-25. In general, men are more likely to attend the top schools.

While little need-based aid is offered to MBA's, the high costs are somewhat offset by the fact that employers often pay a portion of the expenses. We constructed a variable that indicates whether or not an employer was paying for at least half of the individual's tuition. Since part-time and executive enrollees are typically working during the week and are therefore more likely to have strong ties to a particular company, it is perhaps not surprising that these groups are more likely to be backed by employers than those in full-time programs.

Table 2: Wave 1 Descriptive Statistics

	Male		Female	
	No MBA[†]	MBA	No MBA	MBA
Experience	6.86	6.65	5.44	5.84
(years)	(6.00)	(5.79)	(4.71)	(5.33)
Hourly Wage	15.72	15.96	13.42	14.14
($/hour)	(7.07)	(6.42)	(4.86)	(5.05)
Quantitative score	28.84	31.81	24.28	27.90
	(8.98)	(8.22)	(7.76)	(8.07)
Verbal score	27.30	30.15	25.85	28.91
	(8.23)	(7.42)	(7.65)	(7.97)
Undergrad. GPA	2.92	3.01	2.98	3.11
	(0.43)	(0.41)	(0.42)	(0.43)
Married	0.4827	0.5657	0.3443	0.4181
Asian	0.1790	0.1262	0.1579	0.1525
Black	0.1363	0.0787	0.1922	0.1950
Hispanic	0.1724	0.1690	0.1533	0.1507
Other Adv. Degree	0.1099	0.0805	0.0495	0.0538
Observations	609	864	437	564

[†]Defined by whether an individual enrolled in an MBA program sometime during the 4 waves. Standard deviations in parenthesis.

Table 3: Wave 1 Descriptive Statistics by Program Type†

	Part-time		Full-time		Executive	
	Male	Female	Male	Female	Male	Female
Experience	7.13	6.13	4.42	4.25	9.19	8.03
	(6.02)	(5.43)	(4.50)	(4.33)	(5.40)	(6.05)
Hourly Wage	16.03	14.19	14.18	13.34	20.08	16.41
	(5.97)	(5.02)	(6.00)	(4.55)	(8.26)	(6.39)
Quantitative score	30.63	27.16	34.83	30.22	31.83	28.50
	(8.11)	(7.97)	(7.85)	(7.75)	(8.01)	(9.08)
Verbal score	29.16	28.46	32.37	30.33	31.04	29.21
	(7.31)	(8.04)	(7.22)	(7.51)	(7.35)	(8.33)
Undergrad. GPA	2.99	3.09	3.08	3.20	2.96	3.08
	(0.41)	(0.44)	(0.41)	(0.39)	(0.40)	(0.43)
Married	0.6098	0.4429	0.4131	0.2994	0.6679	0.5422
Asian	0.1119	0.1345	0.1743	0.2231	0.0964	0.1176
Black	0.0746	0.1760	0.0963	0.2727	0.0602	0.1471
Hispanic	0.1563	0.1491	0.2018	0.1322	0.1687	0.2353
Other Adv. Degree	0.0839	0.0442	0.0716	0.0800	0.0813	0.0760
Top 10	0.0142	0.0147	0.2064	0.1736	0.0723	0.0294
Top 11-25	0.0249	0.0367	0.2248	0.1240	0.0602	0.0294
Employer pay half	0.6377	0.6039	0.2064	0.2479	0.6988	0.4706
Observations	563	409	218	121	83	34

†The sample is limited to those who enrolled in an MBA program sometime during the 4 waves. Standard deviations are in parenthesis.

3 Identification

We now turn to the identification of the treatment effect of an MBA on wages. We first consider a baseline model that allows for neither heterogeneity across the quality of MBA programs, nor in the individual returns to an MBA. In the baseline model, we assume that log wages for the ith individual at time t follow:

$$\ln W_{it} = X_{it}\beta_1 + MBA_{it}\beta_2 + A_i\beta_3 + \epsilon_{it} \tag{1}$$

where X_{it} refer to both time-invariant individual characteristics, such as race, and time-varying characteristics such as experience. The indicator variable MBA_{it} takes on a value of one if individual i has an MBA at time t, and A_i represents the abilities of the individual. The transitory shock to wages, ϵ_{it}, is assumed to be independent from the other variables in the equation and independent over time as well.

Two problems arise when estimating equation (1). First, true ability is not observed. Second, enrolling in an MBA program is a choice. This second problem is compounded by the first if the choice to enroll also depends upon A_i.

3.1 Selection on Observables

As a first step to estimating β_2, we can attempt to control for A_i using observed ability measures, A_i^*. Substituting in for A_i in equation (1) yields:

$$\ln W_{it} = X_{it}\beta_1^* + MBA_{it}\beta_2^* + A_i^*\beta_3^* + \epsilon_{it}^*. \tag{2}$$

Consistent estimates of β_2 can be obtained if MBA_{it} and ϵ_{it}^* are actually uncorrelated. Identification is then achieved by comparing wages for those who do not obtain an MBA with those who do. However, as stated above, since the decision to obtain an MBA may be influenced by true ability, inconsistent estimates will result from our noisy ability proxies. We still use this method to show the extent of the bias when one pursues this estimation strategy.

3.2 Using Before and After Wages to Remove Unobserved Ability

Given the selection problem, much of the literature on returns to education has turned to instrumental variables in order to obtain consistent estimates of the returns to schooling. This is in part because individuals usually enroll in school continuously before pursuing full-time work beyond summer jobs. Hence, few observations on wages are available before one's final year of education. For MBA's this is not the case as many programs require work experience to obtain admittance. This makes it possible to remove the effect of A_i.

In particular, differencing equation (1) yields:

$$\ln(W_{it}) - \ln(W_{it-1}) = (X_{it} - X_{it-1})\beta_1 + (MBA_{it} - MBA_{it-1})\beta_2 + \epsilon_{it} - \epsilon_{it-1} \tag{3}$$

with identification of β_2 coming no longer from comparing non-entrants to those who obtain an MBA but rather from data on the same individual over time. Analogously, we can use fixed effects to remove the unobserved ability. Our estimating equation is then:

$$\ln(\tilde{W}_{it}) = \tilde{X}_{it}\beta_1 + M\tilde{B}A_{it}\beta_2 + \tilde{\epsilon}_{it} \tag{4}$$

where the tildes refer to the differences relative to the mean value for the individual over the sample period. Since we have multiple observations on pre- and post-MBA wages, we use the fixed effects estimator.

By either differencing or controlling for individual effects we remove the effect of unobserved ability. However, the decision of when to enter an MBA program is still endogenous. This problem is frequently encountered in the job training literature. Namely, individuals may enter training programs when their earnings are abnormally low— the Ashenfelter dip. For example, suppose that an individual does not have an MBA at $t-1$. In the extreme case, the decision to enter an MBA program at t may depend solely on wages at $t-1$:

$$MBA_{it} = \begin{cases} 1 & \text{if } W_{it-1} < C \\ 0 & \text{otherwise} \end{cases}$$

In this case, mean wages will be artificially low for the individual and the effect of an MBA on wages will then be overstated.

We test for the Ashenfelter dip by examining the residuals from the log wage regressions. If there is a dip in wages before enrollment, then:

$$E(\epsilon_{it-1}|MBA_{it-1} = 0, MBA_{it} = 1) < 0$$

Regressing wage residuals on indicator variables for one, two, and three years before individuals enroll in an MBA program should yield a negative coefficient on the year before enrollment if an

Ashenfelter dip is present.[12] However, if the coefficients on these variables are indistinguishable from zero, past wage shocks are uncorrelated with enrollment.

3.3 Heterogeneity in Treatment Effects

The selection problem is even more acute when one allows for the effect of the program to differ by program quality. We also estimate models which allow the returns to an MBA to differ based upon program type (full-time, part-time, and executive) as well as by program quality. The estimating equation is then:

$$\ln(\tilde{W}_{it}) = \tilde{X}_{it}\beta_1 + M\tilde{B}A_{ijkt}\beta_{2jk} + \tilde{\epsilon}_{it} \tag{5}$$

where j and k indicate program type and quality respectively. Identification of the effect of the different types of programs is then achieved by examining before and after earnings for the same individuals. Note that there still may be problems with using this approach to identify β_2 because of the Ashenfelter dip. Again, we can test for a dip in earnings before enrollment by examining the earnings residuals in the years immediately prior to enrollment.

Finally, treatment effects may vary from individual to individual. If this is the case, since the treatment is a choice, individuals who receive the treatment may be expected to receive larger treatment effects than those who do not. We can estimate the full distribution of the treatment effects on the treated by allowing the returns to an MBA to be individual specific. The estimation equation is then:

$$\ln(\tilde{W}_{it}) = \tilde{X}_{it}\beta_1 + M\tilde{B}A_{it}\beta_{2i} + \tilde{\epsilon}_{it} \tag{6}$$

with the β_2's now having individual subscripts. Averaging the treatment effects for particular subgroups then allows us to test how well the restricted models perform.

4 Results

Our first set of results do not allow the effect of an MBA to vary across the three types of programs or with program quality. Table 4 presents the results for men. The OLS results without ability controls yield an estimate of a 9.4% return for obtaining an MBA. The return falls to 6.3% when GMAT scores and undergraduate grades are included in the regression. As in Arcidiacono (2004a),

[12]This test is also a test for individuals who enroll in MBA programs having different growth rates on earnings than their non-MBA counterparts. We discuss this in detail in section 5.

there is a positive and significant return to math ability but no return to verbal ability. For males, one standard deviation increase in math ability, 8.66 points, yields an 8% increase in wages. A one standard deviation increase in undergraduate grade point average, 0.42 points, increases wages by 2.4%. Adding individual fixed effects further reduces the return to an MBA, with the return now estimated to be close to 5%.

Table 4: Estimates of the Return to an MBA for Males†

	No Ability Controls		Observed Abil. Controls		Fixed Effects	
Variable	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
MBA	0.0941*	(0.0162)	0.0628*	(0.0158)	0.0484*	(0.0127)
Other Adv Deg	0.1569*	(0.0228)	0.1013*	(0.0217)	-0.0863*	(0.0316)
Married	0.0650*	(0.0142)	0.0682*	(0.0137)	0.0171	(0.0121)
Asian	0.0765*	(0.0186)	0.0645*	(0.0183)		
Black	-0.0799*	(0.0252)	0.0037	(0.0247)		
Hispanic	-0.0268	(0.0191)	0.0190	(0.0191)		
Undergrad GPA			0.0579*	(0.0174)		
GMAT Verbal			0.0011	(0.0011)		
GMAT Quant			0.0092*	(0.0010)		
R^2	0.3546		0.3939		0.7641	

†Dependent variable is log wages. Estimated on 5759 observations from 2248 individuals. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significant from zero at the 5% level.

The results for women are presented in Table 5. Unlike Montgomery and Powell (2003), the estimated returns to an MBA are consistently lower for women. The return to an MBA for women is estimated to be 10.4% with no ability controls and falls to 6.7% with ability controls. The fixed effect estimate is a little under 4%. The return to math ability is higher for women than for men, with again no return to verbal ability.

For both men and women, including fixed effects resulted in a significant decrease in the return

Table 5: Estimates of the Return to an MBA for Females†

	No Ability Controls		Observed Abil. Controls		Fixed Effects	
Variable	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
MBA	0.1044*	(0.0198)	0.0669*	(0.0195)	0.0378*	(0.0153)
Other Adv Deg	0.0998*	(0.0309)	0.0697*	(0.0311)	0.0111	(0.0328)
Married	0.0156	(0.0151)	0.0130	(0.0146)	0.0068	(0.0131)
Asian	0.0968*	(0.0237)	0.0742*	(0.0241)		
Black	-0.0617*	(0.0216)	0.0360	(0.0225)		
Hispanic	-0.0495*	(0.0226)	0.0030	(0.0220)		
Undergrad GPA			0.0392*	(0.0196)		
GMAT Verbal			-0.0001	(0.0014)		
GMAT Quant			0.0118*	(0.0013)		
R^2	0.3269		0.3742		0.7586	

†Dependent variable is log wages. Estimated on 4053 observations from 1607 individuals. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significant from zero at the 5% level.

to an MBA. The fifty percent drop between the base OLS regression and the fixed effect estimates is higher than the 10-15% drop found in the twins literature (See Card 1999 for a review). However, our sample is different from that used in the twins studies, and there may be more measurement error in retrospective recall of years of education than in a survey of GMAT registrants asking whether they obtained an MBA.

We performed a number of specification tests to ensure the veracity of our results. In particular, under the fixed effects estimation those individuals who only have one wage observation will be predicted perfectly. Removing those individuals had no effect on either the OLS or fixed effects results. Note also that most studies of the returns to education focus on wage observations after completing one's education. We can perform a similar OLS analysis here by removing pre-MBA wages for those individuals who eventually received an MBA. Again, the OLS results were unaffected by the specification change.[13]

These estimated returns constrain the return to an MBA to be constant across the different types of MBA programs and across different school qualities. In Tables 6 and 7 we relax these assumptions for men and women respectively. In particular, the returns are allowed to vary by the three types of programs (full-time, part-time, and executive) as well as by whether the program was in the top 10 or the top-25 according to 1992 U.S. News & World Report rankings. We also allow for the returns to vary by whether or not the individual's employer paid for over half of the tuition of the program.

The treatment effect of an MBA varies substantially across programs and schools. For males, the base returns for attending a school outside of the top-25 are 3.2%, 2.5%, and 14% for full-time, part-time, and executive programs respectively, with only the last statistically significant. These returns essentially become zero for full-time and part-time programs once ability controls are added.[14]

Without controlling for individual fixed effects, the returns to attending a program in the top-10 or in the top-25 are substantially higher than the base case. For men, the premiums over attending a school outside of the top-25 are 33% and 27% for schools in the top-10 and schools in the 11 to 25 range, respectively. These coefficients fall to 25% and 20% when observed ability measures

[13] We cannot use a fixed effects specification in this case as both pre- and post-MBA observations are needed to provide the identification of the coefficient on MBA.

[14] One may ask why anyone would choose to attend programs with lower returns. For both top-25 and executive programs, there are substantial supply side constraints. Further, MBA programs have substantially different time and monetary costs.

Table 6: Estimates of the Return to an MBA for Males by Program Type†

	No Ability Controls		Observed Abil. Controls		Fixed Effects	
Variable	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
MBA	0.0322	(0.0224)	0.0113	(0.0228)	0.0867*	(0.0245)
Part-time MBA	-0.0071	(0.0292)	-0.0026	(0.0288)	-0.0561*	(0.0275)
Executive MBA	0.1119*	(0.0471)	0.1189*	(0.0469)	-0.0209	(0.0397)
Top 10 MBA	0.3298*	(0.0387)	0.2476*	(0.0372)	0.1089*	(0.0405)
Top 11-25 MBA	0.2673*	(0.0453)	0.2046*	(0.0491)	0.1088*	(0.0417)
Other Adv Deg	0.1661*	(0.0247)	0.1104*	(0.0237)	-0.0794*	(0.0326)
Adv Deg×MBA	-0.0507	(0.0434)	-0.0345	(0.0420)	-0.0331	(0.0409)
Employer Pay Half	0.0445	(0.0285)	0.0378	(0.0279)	-0.0404	(0.0229)
Married	0.0640*	(0.0140)	0.0671*	(0.0136)	0.0176	(0.0120)
Asian	0.0752*	(0.0183)	0.0636*	(0.0181)		
Black	-0.0824*	(0.0248)	-0.0033	(0.0245)		
Hispanic	-0.0298	(0.0191)	0.0143	(0.0192)		
Undergraduate GPA			0.0553*	(0.0173)		
GMAT Verbal			0.0009	(0.0011)		
GMAT Quantitative			0.0088*	(0.0010)		
R^2	0.3666		0.4011		0.7665	

†Dependent variable is log wages. Estimated on 5756 observations from 2248 individuals. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significant from zero at the 5% level.

Table 7: Estimates of the Return to an MBA for Females by Program Type †

	No Ability Controls		Observed Abil. Controls		Fixed Effects	
Variable	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
MBA	0.0351	(0.0287)	0.0013	(0.0277)	0.0767*	(0.0282)
Part-time MBA	-0.0223	(0.0361)	-0.0182	(0.0358)	-0.0245	(0.0316)
Executive MBA	0.0949	(0.0764)	0.1074	(0.0685)	-0.0144	(0.0560)
Top 10 MBA	0.4279*	(0.0749)	0.3394*	(0.0680)	0.0911	(0.0585)
Top 11-25 MBA	0.1436*	(0.0600)	0.0967	(0.0580)	0.0053	(0.0518)
Other Adv Deg	0.1121*	(0.0344)	0.0733*	(0.0341)	0.0307	(0.0343)
Adv Deg×MBA	-0.0759	(0.0581)	-0.0411	(0.0549)	-0.1077	(0.0568)
Employer Pay Half	0.1258*	(0.0345)	0.1218*	(0.0337)	-0.0444	(0.0277)
Married	0.0168	(0.0149)	0.0141	(0.0144)	0.0074	(0.0131)
Asian	0.0878*	(0.0234)	0.0682*	(0.0239)		
Black	-0.0666*	(0.0212)	0.0307	(0.0222)		
Hispanic	-0.0534*	(0.0222)	-0.0007	(0.0216)		
Undergraduate GPA			0.0397*	(0.0195)		
GMAT Verbal			-0.0002	(0.0013)		
GMAT Quantitative			0.0115*	(0.0013)		
R^2	0.3306		0.3822		0.7583	

†Dependent variable is log wages. Estimated on 4049 observations from 1606 individuals. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significant from zero at the 5% level.

are included. Women see steeper returns to the quality of the program with the corresponding premiums at 43% and 14% without observed ability measures and 34% and 9.6% with observed ability measures.

These differential returns across program type and program quality change dramatically once individual fixed effects are included. The largest drops in returns relative to OLS come from the groups where we would expect the greatest unobserved abilities: graduates of the top-25 schools. The total effect of attending a school in the top-25 (be it top-10 or in the next set) falls to 19% for men, where total effects include both the MBA premium and the quality premium. This compares to total effects of 26% and 22% for top-10 and the next fifteen respectively when we included observed ability measures but no individual fixed effects. Similar drops in the returns to quality were observed for women, with total premiums falling to 17% and 8% for top-10 and the next fifteen, respectively. The comparable numbers when we controlled for ability using observables were 34% and 10%.

A drop is also observed on the premium for attending an executive MBA program. This too would be expected, as those who are executives have shown themselves to be strong on the unobservables and no controls for previous occupation were implemented. The returns for executive MBA programs at institutions outside of the top-25 fall to less than 7% for both men and women. The return for part-time MBA programs for men does not change when individual effects are included, remaining indistinguishable from zero.

The surprising results come from the changes in the returns to full-time programs outside of the top-25 for both men and women and, to a lesser extent, the returns to part-time programs for women. For these three cases, the returns to an MBA *increase* once individual fixed effects are included. The returns for these three groups increase from essentially zero to over 8.7%, 7.7%, and 5.2% for full-time males, full-time females, and part-time females respectively. We explore why controlling for unobserved ability increases the returns for these programs in the next section.[15]

Without individual fixed effects, those who have employers paying for a substantial portion of their MBA program receive considerably higher wages than those who are paying their own way. In particular, these returns are estimated at 3.8% for men and 12% for women when ability is controlled for using observed measures. These returns fall to -4% once individual fixed effects are included.

[15] As with the results where the effect of an MBA was constrained to be the same across program type, restricting the data set to those individuals who had more than one observation or had completed their education had no effect on the results.

For an employer to pay for an individual's MBA, the employer must be certain of the quality and commitment of the worker. The employer must also have assurances that the worker will continue to stay at the firm once the MBA is completed. Indeed, tuition payments by employers generally require the individual to come back to work for the firm for a number of years. This increases the firm's bargaining power in setting wages. While the worker quality effect dominates without fixed effects, the bargaining effect emerges when individual fixed effects are included.

5 Why are the Fixed Effects Estimates Higher for Programs Outside the Top-25?

We now turn to possible explanations for the higher estimated returns for full-time programs outside the top-25 once fixed effects are implemented. Note first that the fixed effect estimates are *not* higher for the reasons that IV estimates are higher than OLS estimates. Two primary explanations for why IV estimates of the returns to schooling are higher than OLS estimates are measurement error and that the IV estimates yield the marginal, rather than the average, effect of an additional year of schooling. Fixed effect estimates generally exacerbate measurement error and therefore bias the coefficient on return to schooling towards zero. Further, unlike IV estimates, we are estimating the average treatment effect for different programs using both the fixed effects and OLS techniques.

5.1 Testing for the Ashenfelter Dip

Estimates of the returns to an MBA may be upward biased, however, because of the endogeneity of obtaining an MBA. In particular, enrolling in an MBA program may be viewed as enrolling in a training program. Individuals may find it optimal to time their enrollment such that they enter school when their wages are low. This feature of a dip in wages before enrolling in a job training program was first noticed by Ashenfelter (1978), hence the "Ashenfelter dip." To see if wages are lower in years immediately before enrolling in an MBA program, we regress the residuals of the fixed effects regressions on indicator variables for the year before enrolling, two years before enrolling, and three before enrolling for those who obtained an MBA.[16] These estimates are given in Table 8. Note that for both men and women none of the indicator variables are either economically or statistically

[16] Similar results were found when examining the OLS residuals.

significant, with extremely low R^2's for both regressions.[17] There is no evidence of an Ashenfelter dip.

Note that a similar issue may arise in reverse for those who do not obtain an MBA. In particular, these individuals may receive positive wage shocks and then respond to these shocks by not enrolling. All individuals were asked in wave 1 when they expected to enroll in an MBA program. We then tested to see if those who did not obtain an MBA received substantially higher wages in the years before they expected to enroll. Regressing fixed effect residuals on indicator variables for the year before they expected to enroll, two years before, and three years before yielded very small coefficients, none of which were significant.

Table 8: Is There a Dip in Wages Before Enrollment?†

	Men		Women	
Variable	Coef.	Std. Err.	Coef.	Std. Err.
Year Prior to Enrollment	0.0033	(0.0075)	-0.0071	(0.0096)
Two Years Prior	0.0121	(0.0106)	0.01982	(0.0143)
Three Years Prior	-0.0022	0.0113	-0.0214	0.0180
R^2	0.0005		0.0020	
Total Observations	3598		2339	
Number of Individuals	1329		876	

†Dependent variables are the fixed effects residuals calculated from Tables 5 & 6. Years prior to enrollment are only for those who enrolled in an MBA program by wave 4.

5.2 Differential Returns to Experience

Another possible explanation for the higher fixed effects estimates is that individuals who enroll in MBA programs have higher returns to experience. Conditional on receiving an MBA, an MBA is positively correlated with experience. If MBA's have higher returns to experience our fixed effects

[17] We have also estimated the specifications in Tables 4 and 5 with an indicator variable for year before enrolling, finding that the coefficient on the year before enrolling variable was indistinguishable from zero and its inclusion had no effect on the estimates of the other coefficients.

estimator may falsely attribute growth earnings to receiving an MBA rather than to different returns to experience.

If the returns to experience are different for the types of people who obtain MBA's, we should see this pattern in the pre-MBA earnings. Let γ be the estimated coefficient on experience which is individual specific. Equation (5) is then:

$$\ln(\tilde{W}_{it}) = \tilde{X}_{it}\beta_1 + M\tilde{B}A_{ijkt}\beta_{2jk} + \gamma_i E\tilde{x}p_{it} + \tilde{\epsilon}_{it} \tag{7}$$

which can be rewritten as:

$$\ln(\tilde{W}_{it}) = \tilde{X}_{it}\beta_1 + M\tilde{B}A_{ijkt}\beta_{2jk} + \gamma E\tilde{x}p_{it} + (\gamma_i - \gamma)E\tilde{x}p_{it} + \tilde{\epsilon}_{it} \tag{8}$$

Estimating the model as though the returns to experience were not heterogeneous means that the residual has the following additional term:

$$(\gamma_i - \gamma)(E\tilde{x}p_{it})$$

If, for example, those who obtained an MBA had steeper returns to experience ($\gamma_i > \gamma$), the coefficients on the year before enrolling indicator variables would have been *rising* as we approached the date of entry into the MBA program. For example, the wage residuals would be on average higher for one year before enrolling than two years before enrolling. Since none of the coefficients on these variables are statistically significant, we can rule out the random growth model as an explanation.[18]

We also directly tested for differential returns to experience by allowing for interactions on the experience variables for those who eventually received an MBA. None of the MBA coefficients changed and an F-test showed that we could not reject the hypothesis that the returns to experience were the same for those who did or did not eventually receive an MBA. Consistent with these results, the MBA coefficients were unaffected by removing those who never received an MBA from the analysis.

[18] We also split the sample into two groups: high experience and low experience. Performing the same tests on the two groups separately again showed no significant patterns. Further, the mean residuals for pre-MBA earnings of both groups were not distinguishable from zero, suggesting that the treatment effect of an MBA does not depend upon initial experience.

5.3 Individual-Specific Treatment Effects

We next test to see if the results are robust to the treatment effect of an MBA varying at the individual level. We estimated the model with individual-specific treatment effects and then regressed these treatment effects on our program-type and quality variables to see how these effects compare to those in Tables 6 and 7.[19] The first column in Table 9 displays the results for men and the second for women.

Overall, the results are quite similar to those in Tables 6 and 7, particularly for men. Significant premiums exist for top-10 programs as well as for receiving an MBA in general. The one change is that the estimated treatment effect for attending a top-25, but not a top-10, program for women is now negative. However, the sample size for this group is quite small. The surprising result that the fixed effects estimates are higher for those who attended schools outside the top-25 remains.

Table 9: Averaging Over the Individual-specific Treatment Effects†

	Men		Women	
Variable	Coef.	Std. Err.	Coef.	Std. Err.
MBA	0.0777*	(0.0256)	0.0975*	(0.0321)
Part-time MBA	-0.0480	(0.0303)	-0.0263	(0.0382)
Executive MBA	0.0002	(0.0460)	-0.0307	(0.0690)
Top 10 MBA	0.1389*	(0.0458)	0.0958	(0.0690)
Top 11-25 MBA	0.0654	(0.0422)	-0.1147	(0.0618)
Other Adv Deg	-0.0414	(0.0454)	-0.1436*	(0.0657)
Employer Pay Half	-0.0344	(0.0266)	-0.0574	(0.0344)

†Dependent variable is the individual-specific treatment effects. Standard errors are clustered at the individual level.

*Statistically significant from zero at the 5% level.

[19] Allowing the treatment effects to vary increased the R^2 to 0.778 for males and 0.777 for females. Our estimates with returns to MBA constrained to be the same conditional on program type and quality yielded R^2's only slightly lower: 0.767 for males and 0.758 for females.

5.4 Ability and Schooling as Substitutes

Our final possibility for higher returns in the fixed effect estimates is that those who obtain MBA's in full-time programs at institutions outside of the top-25 have lower abilities than GMAT registrants who do not obtain an MBA. This would suggest that schooling and ability could be substitutes (see Angrist and Newey 1991). Looking solely at the observable ability measures, this would not appear to be the case. For both men and women, full-time students outside the top-25 have significantly higher math and verbal GMAT scores as well as higher undergraduate grade point averages than those who do not obtain an MBA. However, those who do not obtain an MBA may be stronger in other areas not easily measured. For example, they may have stronger social skills or better connections in the workplace. Indeed, this is exactly what we find.

Wave 1 of the GMAT registrant survey takes place before attending business school, and asks many questions regarding individuals' assessments of their own skills as well as the perceived benefits of obtaining an MBA. Answers to two of the questions in particular show that, indeed, the GMAT registrants who do not ultimately obtain an MBA may be stronger in dimensions not typically measured in standard survey data. These two questions asked were whether a graduate management education will:

1. "Give me a chance to gain valuable experience before entering the labor market."
2. "Not be that important because I have the credentials I need to do well in my career."

For each of these questions, individuals were asked to circle a number between -3 and +3 with +3 being more true.

The differences in responses between those who did not obtain an MBA and those who eventually obtained full-time MBA's at non-top-25 institutions are given in Table 10. Note that those who did not obtain an MBA seem to have workplace skills that the other group is lacking. In fact, the cumulative distribution for agreeing with the MBA not being important because the individual already has credentials shows that those who do not obtain an MBA think this statement is more true no matter what the cutoff for 'true' is. In contrast, those who obtain an MBA are more likely to think the statement is true that an MBA will help them gain valuable experience regardless of the cutoff value. Admittedly, these variables were chosen because of the differential responses across the two groups. However, the distributions for these variables show that there are dimensions in which those who do not obtain an MBA perceive themselves to be stronger than those who do.

Table 10: Selection on Workplace Skills†

On a scale of -3 to 3, How true or false are the following statements?		MBA will help me gain valuable experience		MBA not important– already have credentials	
		MBA	No MBA	MBA	No MBA
	-3	0.1345	0.2083	0.2870	0.2121
	-2	0.2377	0.2697	0.4126	0.3647
Less than	-1	0.2780	0.3417	0.5964	0.5480
or equal to	0	0.4350	0.6248	0.8520	0.7745
	1	0.5919	0.7658	0.9596	0.9117
	2	0.8117	0.8887	0.9821	0.9731
Mean		0.5112	-0.0988	-1.0897	-0.7841
Observations		223	1042	223	1042

†MBA refers to receiving an MBA an institution outside of the top-25 from a full-time program. The answers to the question are from the Wave 1 survey.

If these variables are measuring unobserved workplace skill then they should influence wages as well. We next test if the inclusion of these variables can explain the gap between the fixed effects and OLS estimates in the returns to full-time programs outside of the top-25. These results are displayed in Table 11, as well as the OLS results with controls for observed ability but not for the measures of workplace skills. Consistent with these variables proxying for workplace skills, those who say they have the credentials they need and that an MBA will not help them gain valuable experience have higher wages. While the full-time return to an MBA outside the top-25 is not significantly different from zero for women, the inclusion of these workplace skill variables yields a positive and significant return to a full-time MBA at an institution outside the top-25 for men. The estimated return of 4.8% is higher than the OLS estimates in Table 6 when *no* controls for observed ability are implemented. While the estimated return is still lower than the fixed effect estimate of 8%, this is not surprising given the imperfect measures we use for workplace skills.

6 Conclusion

Estimating the returns to education is difficult in part because we rarely observe the counterfactual of the wages without the education. One of the advantages of examining the returns to an MBA is that most programs require work experience before being admitted. These observations on wages allow us to see how productive people are before they actually receive an MBA. Hence, whereas fixed effects for the returns to years of schooling can only be identified off the small number of individuals with breaks in their schooling, we can control for fixed effects for virtually all those who obtain an MBA.

Our results show that unobserved ability is generally positively correlated with obtaining an MBA. For males, the returns to attending a top-10 and the next fifteen for males both fall from 25% and 20% when only controlling for observed ability measures to 19% with fixed effects. Similarly, examining the return to an MBA for those whose employers pay for over half the tuition shows a higher return for this group when no fixed effects are implemented but a negative return with fixed effects; those who have employers who are willing to pay for their MBA have high values of unobserved ability.

There is, however, one exception. The estimated returns for full-time MBA students outside of the top-25 increase from essentially zero without fixed effects to around eight percent with fixed

Table 11: Estimates of the Returns to an MBA With Controls for Workplace Skills[†]

	Males				Females			
	w/o Workplace Skills		w/ Workplace Skills		w/o Workplace Skills		w/ Workplace Skills	
MBA	0.0113	(0.0228)	0.0480*	(0.0235)	0.0013	(0.0277)	0.0350	(0.0280)
Part-time MBA	-0.0026	(0.0288)	-0.0152	(0.0294)	-0.0182	(0.0358)	-0.0286	(0.0354)
Executive MBA	0.1189*	(0.0469)	0.0849	(0.0454)	0.1074	(0.0685)	0.0901	(0.0695)
Top 10 MBA	0.2476*	(0.0372)	0.2403*	(0.0371)	0.3394*	(0.0680)	0.2949*	(0.0688)
Top 11-25 MBA	0.2046*	(0.0491)	0.2054*	(0.0494)	0.0967	(0.0580)	0.0911	(0.0611)
Other Adv Deg	0.1104*	(0.0237)	0.1057*	(0.0237)	0.0733*	(0.0341)	0.0755*	(0.0327)
Adv Deg x MBA	-0.0345	(0.0420)	-0.0715	(0.0402)	-0.0411	(0.0549)	-0.0402	(0.0527)
Employer Pay Half	0.0378	(0.0279)	0.0003	(0.0282)	0.1218*	(0.0337)	0.0763*	(0.0344)
Married	0.0671*	(0.0136)	0.0552*	(0.0133)	0.0141	(0.0144)	0.0129	(0.0141)
Undergraduate GPA	0.0553*	(0.0173)	0.0553*	(0.0168)	0.0397*	(0.0195)	0.0307	(0.0183)
GMAT Verbal	0.0009	(0.0011)	0.0005	(0.0011)	-0.0002	(0.0013)	-0.0005	(0.0013)
GMAT Quantitative	0.0088*	(0.0010)	0.0079*	(0.0010)	0.0115*	(0.0013)	0.0104*	(0.0013)
Have Credentials=-2			0.0236	(0.0211)			-0.0148	(0.0231)
Have Credentials=-1			0.0645*	(0.0213)			0.0009	(0.0217)
Have Credentials=0			0.0896*	(0.0197)			0.0757*	(0.0222)
Have Credentials=1			0.1412*	(0.0242)			0.0752*	(0.0263)
Have Credentials=2			0.1427*	(0.0296)			0.1087*	(0.0316)
Have Credentials=3			0.1075	(0.0665)			0.0354	(0.0495)
Gain Experience=-2			-0.0672*	(0.0256)			0.0026	(0.0348)
Gain Experience=-1			-0.1265*	(0.0303)			-0.0522	(0.0353)
Gain Experience=0			-0.0645*	(0.0197)			-0.0394	(0.0226)
Gain Experience=1			-0.1552*	(0.0231)			-0.1309*	(0.0262)
Gain Experience=2			-0.1572*	(0.0228)			-0.1351*	(.0281)
Gain Experience=3			-0.1859*	(0.0263)			-0.1462*	(0.0261)
R^2	0.4011		0.4339		0.3822		0.4135	
Observations	5756		5713		4049		4021	

[†]Dependent variable is log wages. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significant from zero at the 5% level.

effects. This does not arise because of a dip in wages before enrollment, and those full-time students outside the top-25 are actually stronger in observed ability than their non-MBA counterparts. However, they are weaker in areas such as 'workplace skills' that are not easily measured. The richness of our survey data shows that including proxies for workplace skills substantially decreases the gap between the OLS and fixed effects estimates for males. This suggests that, at least on some dimensions, ability and schooling are serving as substitutes.

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