

The Economic Returns to an MBA^{*}

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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. Controlling for individual fixed effects generally reduces the estimated returns to an MBA, and especially so for those in top programs. However, for full-time MBA students attending schools outside of the top-25 the estimated returns are higher when we control for individual fixed effects. 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. Rather, there is some evidence that those who take the GMAT but 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.

Keywords: Returns to education, ability bias, panel data

JEL: J3, I2, C23

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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 disentangling 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 selection. 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 ability. The treatment effect of an MBA on wages is thus identified from wages 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 individual fixed effects, a result consistent with the commonly expected positive correlation between ability and returns to schooling. This positive ability bias is also 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

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

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

³See, for example, Berhman and Taubman (1989), Berhman et al. (1994), Ashenfelter and Krueger (1994) and Ashenfelter and Rouse (1998).

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

recall of years of schooling than in whether or not one has received an MBA. Furthermore, MBA programs are geared more directly toward increasing wages or other career-related goals than other types of schooling which may have broader aims.⁵

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 often too small to obtain accurate estimates of the returns to college quality.⁶ A notable exception is Berhman et al. (1996), who find that, after controlling for family background characteristics using twins, there are significant returns to attending colleges of higher “quality” in several observable dimensions. By using data on pre-MBA wages, we are able to distinguish how the average effect on wages varies across the quality of programs.⁷ Controlling for selection via observables lowers 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, the somewhat surprising result is that 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 found higher returns to schooling than OLS estimates. However, many of the standard reasons given for the higher IV estimates do not hold here. As discussed in Card (1999, 2001), one explanation for higher IV estimates is that they mitigate the measurement error problem associated with misreported years of schooling. An alternative explanation applies to the likely case where the returns to schooling differ across individuals. Then, IV estimates some weighted average of the heterogeneous treatment effects, which is not directly

⁵It is also worth noting that in general the return to schooling literature focuses on the return to an additional year of schooling, while we measure the return to an MBA as the return to obtaining the degree which is typically 2 years of schooling. Note that this is a gross return rather than an actual return, and so neglects costs of schooling, taxes, etc. (see Heckman et al. (2006)).

⁶Researchers have attempted to estimate the return to college quality by controlling for selection with observables (Black et al., 2005; James et al., 1989; Loury and Garman, 1995), matching based upon similar application and acceptance sets (Dale and Krueger, 2002; Black and Smith, 2004), and structurally estimating the decision to attend particular colleges (Brewer et al. (1999), and Arcidiacono (2004, 2005).

⁷Programs may differ both in their treatment effects and in costs. Higher costs of top programs may in part explain their higher average effects, since individuals will only participate in a program if its benefits exceed its costs. Costs and projected benefits are considered more explicitly in Section 6.

comparable to the average treatment effect estimated by OLS.⁸ If the instrument affects a small subset of the sample with a higher marginal return to schooling, IV estimates will be biased upward relative to OLS estimates for the same sample. 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 toward zero.⁹ Further, both the OLS and fixed effect estimates are of the treatment effect on the treated 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.¹⁰ In particular, Ashenfelter (1978) documented the dip in earnings which took place before individuals enrolled in job training programs, something which may also occur when individuals go back to school.¹¹ 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. We also test for the possibility that individuals with higher returns to experience are selecting into business school and thus biasing our estimates of the returns to an MBA upwards.¹²

An alternative explanation is that additional schooling could compensate for low workplace skills. While those who attend full-time MBA programs outside of the top-25 have higher test scores and higher grades than those who take the GMAT but 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, we are able to show 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, thus providing evidence of negative selection into business school conditional on taking the GMAT and not attending a top-25 program.

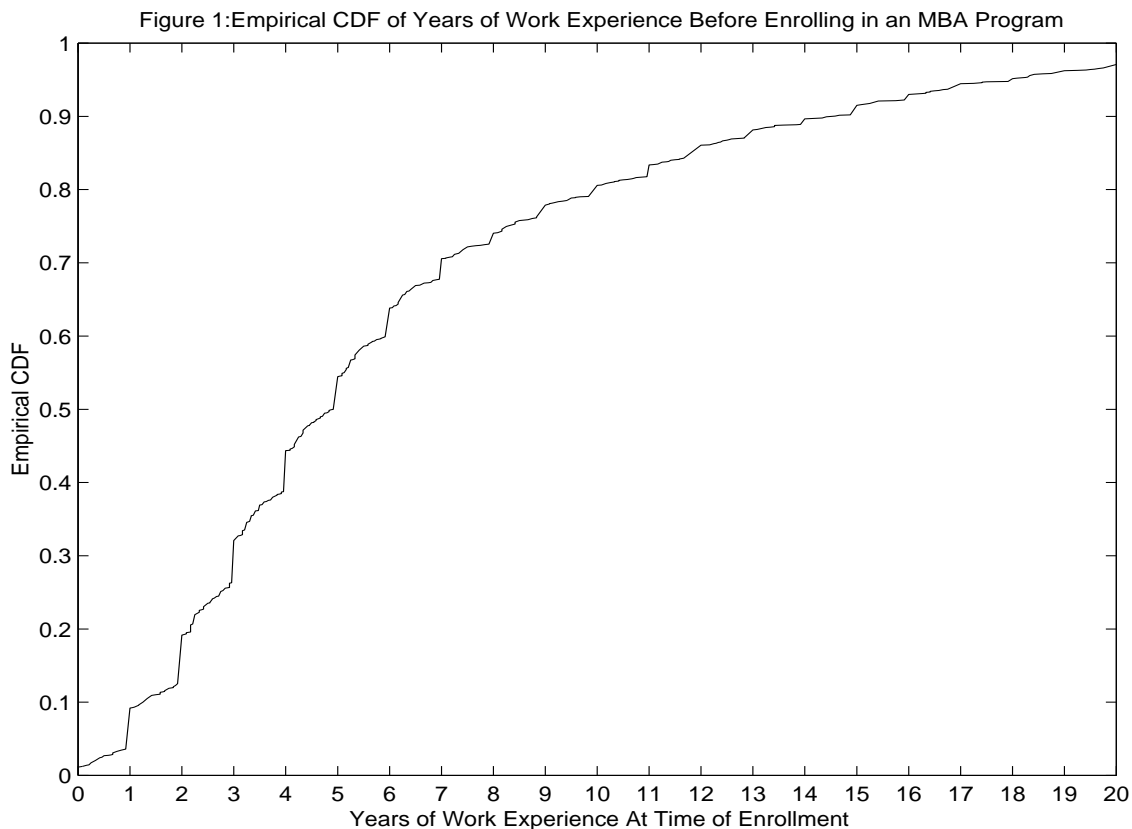
⁸Heckman and Vytlačil (2005) develop a unifying framework that clarifies the links between the parameters being estimated using these alternative estimators in the context of heterogeneous treatment effects.

⁹See Hsiao (1986) for a discussion of measurement error in panel data models. See also Bertrand et al. (2004).

¹⁰See Heckman et al. (1999) for a review.

¹¹See Heckman and Smith (1999) for a more recent discussion of the Ashenfelter dip and its effects on longitudinal estimators of program impact.

¹²See Baker (1997). Furthermore, our results are robust to restricting the sample only to those who obtain MBAs.



The one study that uses fixed effects to estimate the returns to schooling – Angrist and Newey (1991) – finds that fixed effects estimates of the returns to schooling are higher than the corresponding OLS estimates. They suggest that individuals may make up for low workplace productivity by obtaining more schooling. However, the fixed effects coefficient is identified off of only those who have a break in schooling, a group which is less than twenty percent of the sample. This is in contrast to our sample where virtually everyone who obtains an MBA in the sample first obtains work experience.

While there is a broad literature on the returns to schooling, few studies have investigated the returns to an MBA. The value of an MBA degree is a concern to potential MBA students, and articles in the popular press and schools themselves often report average starting salaries of graduates as an indicator of program effectiveness without addressing issues of selection. The more rigorous attempts to determine the efficiency or value-added of MBA programs rely on aggregate data of student characteristics as reported by top-rated schools (Tracy and Waldfogel, 1997; Colbert et al., 2000; Ray and Jeon, 2003). The purpose of these studies is to rank MBA programs based on

their effectiveness after controlling for different observable measures of student quality. They rely primarily on post-MBA salary information to assess the quality of an MBA program and therefore cannot control for differences individual fixed effects. An important contribution of our paper, therefore, is applying individual-level data on student characteristics and pre- and post-MBA wages to estimate the returns to an MBA, which allows for a more careful treatment of selection first into attending business school and second into programs of varying types and qualities. Other studies that benefit from data on individual outcomes from attending business school have focused on a substantively different question, explaining the gender wage gap, rather than estimating the return to an MBA for various types of MBA programs.¹³

The rest of the paper proceeds as follows. Section 2 describes the data. A simple model of MBA attainment and the identification strategy are 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. In Section 6 we consider the net benefit of an MBA, after taking into account the varying costs of different types of MBA programs. Section 7 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 1998.¹⁴ 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 wages both before and after an individual receives an MBA. In Table 1 we show the distribution of the individuals across five activities and the four

¹³Graddy and Pistaferri (2000) analyze the extent of the gender wage gap comparing the starting salaries of graduates of London Business School. Montgomery and Powell (2003) look at changes in the gender wage gap due to MBA completion, using the same data as in the current study.

¹⁴The same survey has been used by Montgomery (2002) and Montgomery and Powell (2003).

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.

survey waves. A substantial portion of the sample have pre- and post-MBA wages, obtaining their MBA sometime between wave 2 and wave 3.¹⁵

Using the four waves, we construct hourly wages corresponding to the individual's job at the time of response to the survey indexed to the monthly level, spanning the years 1990 to 1998.¹⁶ We only include wages for full-time jobs (at least 35 hours a week). As shown in Table 2, the variation in enrollment across waves translates into considerable overlap in the pre- and post-MBA wages, particularly in the middle years, 1993 and 1994.

We also construct an experience measure based on the 4 waves, using as a starting point individuals' responses in Wave 1 to the question regarding the number of years in total worked full time (35 hours per week or more) for pay during at least one half of the year. In each wave, we have detailed

¹⁵Over 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.

¹⁶The survey allows for individuals to report either an hourly, weekly, biweekly, monthly or annual wage. They also report how many hours per week they work. When an hourly wage is not reported, we calculate it using the reported hours. We drop the bottom and top percentile of wages in order to eliminate the possibility of extreme outliers driving the results. The use of hourly wages allows for the more direct comparison to the returns to schooling literature and allows us to abstract from issues involving labor supply.

Table 2: Number of Wage Observations Pre- and Post-MBA by Year[†]

Year	Pre-MBA.	Post-MBA	Total	Wave
1990	529	0	529	1
1991	588	16	604	1 & 2
1992	503	14	517	2
1993	73	101	174	2 & 3
1994	293	499	792	3
1995	9	26	35	3
1996	0	0	0	-
1997	0	415	415	4
1998	0	576	576	4

[†]Includes only those who obtained an MBA by wave 4.

information on the individual’s employment, including beginning and ending dates. Based on these employment records, we assign experience to individuals at the monthly level, if they were working a full-time job (more than 35 hours) for some portion of that month. 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 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 similar to those who remained in the sample in terms of their gender, race, test scores, and labor market outcomes.¹⁷ 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

¹⁷An appendix characterizing the attrition results in more detail is available on request.

MBA using before and after wages for those who received an MBA, i.e. the treatment effect on the treated.

Wave 1 sample characteristics are reported in Table 3 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.¹⁸ 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. We include in our analysis scores from both the quantitative and the verbal sections of the GMAT. Each of these scores range from 0 to 60, with a population average of around 30. In our sample, men performed better on the quantitative section of the GMAT than women, while women had higher average undergraduate grades. Both GMAT scores (quantitative and verbal) 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.¹⁹ Finally, it is interesting to note that black females in our sample are considerably more likely than black males to get an MBA.²⁰

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. Thus, 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

¹⁸While differences in experience suggest a lower labor force participation rate for women, the labor force participation rate for women in our sample is over 95%.

¹⁹The word "ability" is used here loosely. According to the GMAC: "The GMAT measures basic verbal, mathematical, and analytical writing skills that you have developed over a long period of time in your education and work. It does not measure: your knowledge of business, your job skills, specific content in your undergraduate or first university course work, your abilities in any other specific subject area, subjective qualities - such as motivation, creativity, and interpersonal skills." [www.mba.com]

²⁰The NCES Digest of Trends and Statistics also reports that black females make up a larger percentage of undergraduate degree recipients than black males. (<http://nces.ed.gov/programs/digest/d03/tables/dt264.asp>)

Table 3: Wave 1 Descriptive Statistics

	Male			Female			Female=
	No MBA	MBA [†]	p-value	No MBA	MBA	p-value	Male MBA p-value
Experience (years)	6.86 (6.00)	6.65 (5.79)	0.502	5.44 (4.71)	5.84 (5.33)	0.209	0.007
Hourly Wage (\$/hour)	15.72 (7.07)	15.96 (6.42)	0.505	13.42 (4.86)	14.14 (5.05)	0.023	0.000
Quantitative score	28.84 (8.98)	31.81 (8.22)	0.000	24.28 (7.76)	27.90 (8.07)	0.000	0.000
Verbal score	27.30 (8.23)	30.15 (7.42)	0.000	25.85 (7.65)	28.91 (7.97)	0.000	0.003
Undergrad. GPA	2.92 (0.43)	3.01 (0.41)	0.000	2.98 (0.42)	3.11 (0.43)	0.000	0.000
Married	0.4827	0.5657	0.002	0.3443	0.4181	0.017	0.000
Asian	0.1790	0.1262	0.006	0.1579	0.1525	0.819	0.164
Black	0.1363	0.0787	0.006	0.1922	0.1950	0.912	0.000
Hispanic	0.1724	0.1690	0.865	0.1533	0.1507	0.910	0.354
Other Adv. Degree	0.1099	0.0805	0.061	0.0495	0.0538	0.760	0.044
Observations	609	864		437	564		

[†]Defined by whether an individual enrolled in an MBA program sometime during the 4 waves. Standard deviations in parenthesis. The sample is restricted to individuals who report current wage observations in Wave 1. The 3rd and 6th columns present p-values from tests of equal means between MBAs and non-MBAs for males and females, respectively. The last column presents p-values from tests of equal means between male and female MBAs.

in school.

Table 4 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 and World Report, 1992). In particular, we distinguish between schools ranked in the top ten, the next fifteen, and outside the top-25.²¹ 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 sometimes offset by employers that are willing to pay a portion of the expenses. While we do not observe exactly how much the employer contributes toward the MBA, the survey does report whether an employer was the main source of financing (i.e., paid more than 50% of) the degree. 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.

3 Model and Identification

In order to clarify the assumptions underlying our empirical strategy, we present a model of wages as it relates to the decision to obtain an MBA. The decision to obtain an MBA is modelled similarly to the labor market program participation models discussed in Heckman et al. (1999). The purpose of the model is to clarify the assumptions under which the fixed effects estimate of the returns to an MBA can be appropriately interpreted as the treatment on the treated. Intuitively, our identification strategy revolves around the argument that pre-MBA wages serve as an appropriate counterfactual of wages without the MBA that allows us to control for a time invariant component

²¹Although more schools are now ranked by US News, for a long time (including during our sample period) only the top-25 schools were reported. Anecdotally, students, administrators and employers place much weight on the top-10, suggesting that for quality purposes this is the most relevant breakdown for potential MBA entrants during our sample period.

Table 4: Wave 1 Descriptive Statistics by Program Type[†]

	Part-time			Full-time			Executive		
	Male	Female	p-value	Male	Female	p-value	Male	Female	p-value
Experience	7.13 (6.02)	6.13 (5.43)	0.007	4.42 (4.50)	4.25 (4.33)	0.733	9.19 (5.40)	8.03 (6.05)	0.336
Hourly Wage	16.03 (5.97)	14.19 (5.02)	0.000	14.18 (6.00)	13.34 (4.55)	0.149	20.08 (8.26)	16.41 (6.39)	0.012
Quantitative score	30.63 (8.11)	27.16 (7.97)	0.000	34.83 (7.85)	30.22 (7.75)	0.000	31.83 (8.01)	28.50 (9.08)	0.068
Verbal score	29.16 (7.31)	28.46 (8.04)	0.164	32.37 (7.22)	30.33 (7.51)	0.016	31.04 (7.35)	29.21 (8.33)	0.270
Undergrad. GPA	2.99 (0.41)	3.09 (0.44)	0.000	3.08 (0.41)	3.20 (0.39)	0.008	2.96 (0.40)	3.08 (0.43)	0.167
Married	0.6098	0.4429	0.000	0.4131	0.2994	0.034	0.6679	0.5422	0.213
Asian	0.1119	0.1345	0.293	0.1743	0.2231	0.287	0.0964	0.1176	0.742
Black	0.0746	0.1760	0.000	0.0963	0.2727	0.000	0.0602	0.1471	0.195
Hispanic	0.1563	0.1491	0.758	0.2018	0.1322	0.091	0.1687	0.2353	0.429
Other Adv. Degree	0.0839	0.0442	0.011	0.0716	0.0800	0.781	0.0813	0.0760	0.923
Top 10	0.0142	0.0147	0.949	0.2064	0.1736	0.457	0.0723	0.0294	0.293
Top 11-25	0.0249	0.0367	0.262	0.2248	0.1240	0.015	0.0602	0.0294	0.432
Employer pay half	0.6377	0.6039	0.284	0.2064	0.2479	0.387	0.6988	0.4706	0.025
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 parentheses. The sample is restricted to individuals who report current wage observations in Wave 1. Columns 3, 6 and 9 include p-values from tests of equal means between males and females for each type of program.

of worker productivity. Because we have multiple pre- and post-MBA wage observations for many individuals, we can further test whether the assumptions that yield the treatment on the treated are valid.

We assume that log wages for individual i at time t follow:²²

$$\ln W_{it} = \alpha_i + D_{it}\beta_i + f(\text{exp}_{it})\gamma_i + \epsilon_{it}$$

where α_i represents a time invariant worker productivity and D_{it} is an indicator variable denoting whether or not the individual has an MBA at time t . The return to an MBA is captured by β_i and γ_i represents the return to a non-linear function of experience, $f(\text{exp}_{it})$. Finally, ϵ_{it} is a time-varying determinant of wages that is unobserved to the econometrician and is assumed to be distributed $N(0, \sigma)$.

We begin with the simplifying assumption that the individual has only one opportunity to enter business school at period $t = k$.²³ We further focus attention on full-time students. At $t = k - 1$, an individual chooses whether or not to enter business school in the next period k . There is a cost to attending business school, $c_i + \eta_{ik}$, which is assumed to be observed by the individual at the time of the decision. The first term, c_i , denotes the individual-specific costs that can be measured by the econometrician (i.e., a person who has demonstrated low academic performance in the past may find business school more difficult and thus more costly). The term η_{ik} captures a component to cost that is unobserved to the econometrician.

We assume that individuals not enrolled in school work a fixed amount of hours h . Denote Y_{it}^0 as earnings at time t without an MBA and Y_{it}^1 as earnings with an MBA where $Y_{it}^j = hW_{it}^j$. If an individual enters business school, he foregoes earnings Y_{ik}^0 in period k (the period he is in school) and acquires earnings Y_{it}^1 from period $k + 1$ onward. If he does not enter business school, then he continues to earn Y_{it}^0 for all t . Assuming a terminal date of employment T , an individual i then

²²In practice, we have time-varying X 's—including time—that also affect wages. We ignore these for the moment for ease of notation.

²³In reality, when individuals forego entry into business school in a given period, they still have the option of obtaining an MBA in the future. This could be particularly important when there are heterogeneous returns to an MBA that affect the timing of the decision and consequently the number of pre- and post-MBA wages we observe. We do test for whether the returns to an MBA are correlated with the time of enrollment and find no significant correlation.

chooses to enter business school when:

$$\sum_{j=k+1}^T \frac{E(Y_{ij}^1|I_{ik-1})}{(1+r)^{j-k+1}} - \sum_{j=k}^T \frac{E(Y_{ij}^0|I_{ik-1})}{(1+r)^{j-k+1}} - c_i \geq \eta_{ik}, \quad (1)$$

where $I_{ik-1} \equiv (\alpha_i, \beta_i, c_i, \eta_{ik}, \text{exp}_{i0}, \dots, \text{exp}_{iT}, \epsilon_{i0}, \dots, \epsilon_{ik-1})$ denotes an individual's information set at the time of making his decision.²⁴ In words, an individual knows the costs of business school, the return to an MBA and can predict earnings for future periods up to the unobserved time-varying shock on wages, ϵ_{it} . Then, the probability an individual obtains an MBA can be expressed as follows:

$$\begin{aligned} Pr(D = 1) &= Pr \left(\sum_{j=k+1}^T \frac{E(Y_{ij}^1|I_{ik-1})}{(1+r)^{j-k+1}} - \sum_{j=k}^T \frac{E(Y_{ij}^0|I_{ik-1})}{(1+r)^{j-k+1}} - c_i \geq \eta_{ik} \right) \\ &= \Phi \left(\sum_{j=k+1}^T \frac{E(Y_{ij}^1|I_{ik-1})}{(1+r)^{j-k+1}} - \sum_{j=k}^T \frac{E(Y_{ij}^0|I_{ik-1})}{(1+r)^{j-k+1}} - c_i \right), \end{aligned} \quad (2)$$

where $\Phi(\cdot)$ denotes the distribution of η_{ik} conditional on the observables.

Our parameter of interest is the treated on the treated, β_{TT} , where β_{TT} is given by the average treatment effect for those who obtain the treatment, i.e. $E(\beta_i|D = 1)$. Given the decision rule above, we can now lay out sufficient conditions for the fixed effects estimator to yield consistent estimates of β_{TT} . Given that we observe both pre- and post-MBA wage observations²⁵ for individuals with various degrees of experience and other background characteristics but not c_i , η_{it} , or ϵ_{it} , fixed effects will yield consistent estimates of β_{TT} when:

1. The ϵ_{it} 's are independent over time or are uncorrelated with the decision to obtain an MBA.²⁶
2. $\gamma_i = \gamma$ holds for all individuals or the decision to obtain an MBA is independent of γ_i .
3. One of the following holds:
 - (a) $\beta_i = \beta$ for all individuals or
 - (b) We have the same number of post-MBA wage observations for each MBA recipient or

²⁴Note that this implicitly assumes perfect credit markets so that an individual can borrow and lend freely at rate r .

²⁵Note that in order to separately identify time dummies from the returns to an MBA we would need not only pre- and post-MBA wage observations, but also need differences in when individuals received their MBA's.

²⁶Note that the ϵ_{it} 's being independent over time is necessary to be consistent with the model. Without independent ϵ_{it} 's, past values of ϵ_{it} 's should be correlated with the decision to obtain an MBA.

(c) The number of post-MBA wage observations is uncorrelated with β .

It is useful to compare these assumptions with those required by OLS. Let X_i indicate a set of observed characteristics for individual i where:

$$\alpha_i = X_i\delta + u_i$$

In order to obtain consistent estimates of β_{TT} , OLS requires the assumptions above plus one additional:

$$4. E(u|D, X) = 0.$$

While we generally expect that $E(u_i|D = 1)$ may be higher than $E(u_i|D = 0)$, the standard selection problem, this need not be the case. In particular, even if those who obtain an MBA are stronger on observable dimensions such as test scores they may be weaker on unobserved dimensions such as the ability to form networks. For these reasons, using within variation is likely to be very useful for obtaining consistent estimates of β_{TT} .

That said, the assumptions listed above that are needed for the FE estimates of β_{TT} to be consistent are still strong and have been shown not to hold in a variety of contexts. We discuss below what happens when these assumptions are violated as well as what trends we should expect to see in the data should the assumptions be violated. We pay particular attention to the implications of individuals deciding to obtain an MBA in response to the ϵ 's or in response to differential returns to experience.

3.1 Selection on Time-Varying Unobservables

Suppose that Assumption 1 is violated, and the ϵ 's are serially correlated. Taking the partial derivative of (2) with respect to ϵ_{ik-1} , we have:

$$\frac{\partial\Phi(\cdot)}{\partial\epsilon_{ik-1}} = \sum_{j=k+1}^T \frac{1}{(1+r)^{j-k+1}} \frac{\partial\Phi(\cdot)}{\partial EY_{ij}} \left(\frac{\partial EY_{ij}^1}{\partial\epsilon_{ik-1}} - \frac{\partial EY_{ij}^0}{\partial\epsilon_{ik-1}} \right) - \frac{1}{1+r} \frac{\partial\Phi(\cdot)}{\partial EY_{ik}} \frac{\partial EY_{ik}^0}{\partial\epsilon_{ik-1}},$$

where $\partial\Phi(\cdot)/\partial EY_{ij} \equiv \partial\Phi(\cdot)/\partial EY_{ij}^1 = -\partial\Phi(\cdot)/\partial EY_{ij}^0 > 0$. Intuitively, individuals who receive higher wage draws in $k-1$ will predict higher values of foregone earnings in k , thus decreasing their probability of attending business school, so the second term, inclusive of the minus sign, is negative. The first term, however, is likely to be positive since the marginal effect of ϵ in the post-MBA wage is

augmented by the additional return to MBA component, β .²⁷ With individuals maximizing earnings and earnings distributed log normal, there is an interaction between ϵ and MBA and earnings that is not present for log earnings. An MBA then becomes more valuable when the ϵ 's are expected to be high. Thus, the marginal effect of shocks to pre-MBA earnings is ambiguous.

If the second term dominates, i.e., individuals who receive low wage draws predict relatively low foregone earnings in the next period, then $\partial\Phi/\partial\epsilon_{ik-1} < 0$ and we have the case of the Ashenfelter Dip. This would suggest that individuals with low wages in period $k-1$ will be selecting into business school, and thus we may overstate the return to an MBA. Because we have multiple pre-MBA wage observations, we can directly test whether this is the case by looking at residuals before enrollment in business school. If individuals with lower wage draws are selecting into business school, we should see a dip in wages prior to enrollment. However, if the first term dominates and individuals with higher wage draws are selecting into business school we should see a bump up in wages prior to enrollment.²⁸

3.2 Heterogeneous Returns to Experience

Now, suppose there are heterogeneous returns to experience, and individuals select into business school based on these returns, causing Assumption 2 to be violated. Intuitively, the problem arises that individuals with higher growth rates in wages (i.e., higher γ_i) may be more likely to select into business school. Taking the partial derivative of the probability of attending with respect to γ yields:

$$\frac{\partial\Phi(\cdot)}{\partial\gamma_i} = \sum_{j=k+1}^T \frac{1}{(1+r)^{j-k+1}} \frac{\partial\Phi(\cdot)}{\partial EY_{ij}} \left(\frac{\partial EY_{ij}^1}{\partial\gamma_i} - \frac{\partial EY_{ij}^0}{\partial\gamma_i} \right) - \frac{1}{1+r} \frac{\partial\Phi(\cdot)}{\partial EY_{ik}} \frac{\partial EY_{ik}^0}{\partial\gamma_i}.$$

Again, we expect the first term to be positive as the first term inside the brackets is expected to dominate the second. The second term is again negative. However, unlike the case with the Ashenfelter dip, we may expect the first term to dominate. This is because log normal wages lead

²⁷This term would disappear if individuals were to maximize log earnings rather than earnings or if earnings were normally distributed rather than log earnings.

²⁸Note that a similar story would follow if we were to reinterpret the wage residual as worker "effort." If business school admittance is based in part on labor market performance, then individuals might be induced to work harder prior to entry into business school and it would look like those with higher wage residuals were selecting into business school. In this case, we would underestimate the return to an MBA. Again, we can directly test whether there is a bump up in wages prior to enrollment.

to an interaction between the MBA and the returns to experience. Those with high returns to experience see even higher monetary gains to an MBA because of this interaction. The response to higher returns to experience by the individual may also be compounded by the schools themselves, which have incentives to enroll individuals with high γ_i 's in order to be associated with graduates who have high future earnings. A testable implication of this is that pre-MBA wages should be increasing as we move closer to the enrollment date.

4 Results

Our first set of results does not allow the effect of an MBA to vary across the three types of programs or with program quality. Table 5 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. There is a positive and significant return to math ability but no return to verbal ability.²⁹ 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%.³⁰

The results for women are presented in Table 6. 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. This finding is consistent with the twins' literature that finds that OLS estimates of the return to schooling are biased upward compared to twin fixed effects (See Card (1999) for a review).

We performed a number of specification tests to enhance the credibility of our results. In particular, under the fixed effects estimation those individuals who only have one wage observation

²⁹This is comparable to the results in Arcidiacono (2004) and Paglin and Rufolo (1990) that suggest a positive return to math but not verbal ability in the context of undergraduates.

³⁰A more flexible regression with ability controls was carried out, which, in addition to the current variables, also included interaction terms between the observed variables. The estimated coefficients on the current variables were virtually identical to those reported here.

Table 5: Estimates of the Return to an MBA for Males[†]

Variable	No Ability		Observed		Fixed	
	Controls		Abil. Controls		Effects	
	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. The sample is restricted to individuals who report a wage for the job at which they are currently employed at the time of the survey. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significantly different from zero at the 5% level.

Table 6: Estimates of the Return to an MBA for Females[†]

Variable	No Ability		Observed		Fixed	
	Controls		Abil. Controls		Effects	
	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. The sample is restricted to individuals who report a wage for the job at which they are currently employed at the time of the survey. Regression also included a quartic in time and experience. Standard errors are clustered at the individual level. *Statistically significantly different from zero at the 5% level.

will be predicted perfectly. Removing those individuals had no effect on either the OLS or fixed effects results. In addition, since the MBA coefficients in the fixed effects specifications are identified solely off of those individuals that ultimately received an MBA, the regressions were also carried out using this sub-sample. No significant differences in the estimates were found.³¹ We also try including year fixed effects rather than a time trend to capture potential shocks to wages in a given period, but our results do not change. 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.³²

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 7 and 8 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 for those who obtain their MBA.³³

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.³⁴

Without controlling for individual fixed effects, the returns to attending a program in the top-10

³¹Since this specification check removed individuals who start but do not finish business school, it also suggests that the inclusion of this type of partial treatment in the non-MBA group is not biasing our estimates.

³²We 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.

³³As 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 does not affect the results.

³⁴One 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, in that less able individuals are not able to get into the top programs. Further, not only do MBA programs have substantially different time and monetary costs, but there may be non-pecuniary benefits associated with the degree, such as increased ability to move between types of jobs or industries, which are not directly picked up in the wage returns.

Table 7: Estimates of the Return to an MBA for Males by Program Type[†]

Variable	No Ability		Observed		Fixed	
	Controls		Abil. Controls		Effects	
	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 significantly different from zero at the 5% level.

Table 8: Estimates of the Return to an MBA for Females by Program Type [†]

Variable	No Ability		Observed		Fixed	
	Controls		Abil. Controls		Effects	
	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 significantly different from zero at the 5% level.

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.³⁵ These coefficients fall to 25% and 20% when observed ability measures 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 contrasts with 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 for the premium for attending an executive MBA program. This too would be expected, as no controls for previous occupation were implemented and executives have demonstrated themselves to be strong on the unobservables. 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.³⁶

The most 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

³⁵Given the emphasis placed on attending a top-10 school, it is somewhat surprising that we do not find that the returns differ significantly between a top-10 vs. a top 11-25 program for males. This finding is robust to small changes in the number of schools included in these two groups or including the actual rank in addition to the quality dummies.

³⁶A concern with this specification is that part-time and executive MBAs generally work during school. Using wages while in school could bias down our results for this sample if there are returns to partial completion of an MBA. However, we find that dropping the wages of an individual while in school from our sample does not affect our estimated return to executive or part-time programs, suggesting more of a signalling story.

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 individual fixed effects increases the returns for these programs in the next section.³⁷

Without individual fixed effects, those who have employers paying for a substantial portion of their MBA program receive considerably higher wages than those who do not. In particular, the return to an MBA is estimated to be 3.8% higher for men and 12% higher for women who have employers paying for at least half of the MBA when ability is controlled for using observed measures. These returns fall to -4% once individual fixed effects are included. This coefficient may be picking up a number of different effects, not only related to particular individuals or firms, but to the structure of compensation. To the extent that obtaining an MBA represents the acquisition of general human capital, employees may have to compensate employers for payment of tuition by accepting lower initial (pre-MBA) wages (see Becker (1964)). In this case, the use of fixed effects would overestimate the economic return an MBA (as compared to the return the individual would have observed by working at another firm and paying for the degree on their own). There is some evidence, however, that even in the case of general training, few costs appear to be passed on to the worker in the form of a lower wages prior to training completion (see, for example, Loewenstein and Spletzer (1998)). Employers may then instead write contracts such that workers must return to the firm or pay back the costs of the MBA. Consistent with our results, the estimated return to the MBA would then be lower for those whose employers pay their way.

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

³⁷The observed selection effects (and inadequacy of the OLS specifications) were corroborated by the implementation of a “pre-program test”, similar to that performed by Heckman and Hotz (1989). This test involves using only pre-MBA observations in an OLS regression and coding the MBA variable to represent eventual MBA status. Other than full-time programs outside the top-25, which resulted in a negative and significant coefficient, the MBA coefficients were positive and significant for each program type and quality.

IV estimates of the returns to schooling are higher than OLS estimates are measurement error and that the IV estimates do not yield the treatment on the treated but rather the average treatment effect for those affected by the instrument. Fixed effect estimates generally exacerbate measurement error and therefore bias the coefficient on return to schooling towards zero. While there is likely to be less measurement error associated with the MBA and quality of MBA variables than there is in studies using retrospective recall of years of schooling, to the degree that measurement error does exist, it should bias our results in the direction of having lower fixed effects estimates than OLS estimates. Further, unlike IV estimates which in the context of heterogeneous returns may overweight the high-return population, we are estimating the treatment on the treated 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, if a dip in wages prior to enrolling motivates individuals to go to business school.³⁸ As discussed in Section 3.1, we can test for an Ashenfelter dip by controlling for proximity to MBA enrollment in the regression. Indicator variables for the year before enrolling, two years before enrolling, and three years before enrolling (for those who obtained an MBA) were constructed and were included in our fixed effects specifications. These estimates are given in Table 9. Though most of the indicator variables are negative, for both men and women none are statistically significant. There is no evidence of an Ashenfelter dip.³⁹

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. As before, including indicator variables for the year before they expected to enroll, two years before, and three years before yielded very small and insignificant coefficients.

³⁸Recall, however, from Section 3.1 that the actual effect of a dip in wages prior to enrolling on the decision to attend business school is actually ambiguous.

³⁹A potential reason for this is that the Ashenfelter dip is frequently associated with people losing their jobs and unemployment is not very relevant in our sample.

Table 9: Is There a Dip in Wages Before Enrollment?[†]

Variable	Men		Women	
	Coef.	Std. Err.	Coef.	Std. Err.
Year Prior to Enrollment	-0.0069	(0.0168)	-0.0115	(0.0200)
Two Years Prior	-0.0127	(0.0238)	0.0185	(0.0310)
Three Years Prior	-0.0295	(0.0331)	-0.0396	(0.0406)
R ²				
Total Observations	5756		4049	
Number of Individuals	2248		1606	

[†]Table reports estimated fixed effects regression coefficients on dummy variables representing between 0 and 1 year, between 1 and 2 years, and between 2 and 3 years prior to enrollment in an MBA program by wave 4. The dependent variable is log wages, and the regression also included all of the variables shown in the fixed effects specifications of Tables 7 & 8.

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, as discussed in Section 3.2. If MBA's have higher returns to experience our fixed effects estimator may falsely attribute wage growth to the effect of receiving an MBA rather than to differential returns to experience. If this were the case, the wage residuals would be on average higher as proximity to enrollment decreases, and controlling for time before enrollment could significantly change our estimates. As shown in Table 9, while there is a modest increasing trend for men in the point estimates of the coefficients on these variables, none of the coefficients are statistically significant. Furthermore, the estimates of the MBA coefficients were not significantly different from those reported in Tables 7 & 8. Therefore, we do not find evidence to support a random growth model.⁴⁰

⁴⁰We 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 pre-enrollment effects, suggesting that the treatment effect of an MBA does not depend upon initial experience. Note that since we do not have a long enough panel to conduct a similar test on post-MBA wages, we are not able to test for selection based on post-MBA gradients.

We also directly tested for differential returns to experience by interacting experience with a dummy for the subsample of individuals who eventually receive an MBA in our fixed effects specifications in Tables 5-8. 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, and estimates of the return to an MBA were unchanged. That said, if the individuals who do not get an MBA do indeed have lower returns to experience, then using them as a control group in our analysis could bias our estimates of the return to an MBA. However, as mentioned in Section 4, the MBA coefficients are the same when we remove non-MBAs from the analysis, further suggesting that our results are not driven by differential returns to experience.

5.3 Negative Selection

One final possible alternative explanation for the 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 could be used to make up for poor skills in other dimensions. Angrist and Newey (1991) find that OLS estimates of the return to schooling are biased upward next to fixed effects estimates in their sample, 18-26 year old men who have some break in employment to return to school. In their conclusion, they discuss the possibilities of this being attributable to the estimation strategy, i.e., that they are potentially picking off a subpopulation with a higher marginal return than those in the overall sample, or that it really is evidence that ability and schooling serve as substitutes.⁴¹ Looking solely at the observable ability measures for our sample of MBAs, this would not appear to be the case. For both men and women, full-time students outside the top-25 have significantly higher math GMAT scores, higher verbal GMAT scores and 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. The model in Section 3 could then be adapted to explicitly allow for the costs of an MBA to include the cost of effort. Those who are more 'book-smart,' i.e., those with high GMAT scores, can make up for their poor skills in other

⁴¹A recent evaluation of Teach for America also suggests that ability and schooling may be substitutes (see Decker et al. (2004)). Teach for America teachers have less schooling, less experience, and are much less likely to be certified in teaching, but have higher test scores and a stronger influence on student math scores.

dimensions by obtaining an MBA. Further, they are willing to do so because higher GMAT scores are associated with lower effort costs for obtaining an MBA.

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 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.⁴² 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.

If these variables are measuring unobserved workplace skill, 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

⁴²An alternative explanation for why these answers are correlated with MBA attainment may be that the treatment effect is different depending upon these answers. The differential responses to these questions may also be the result of different requirements for promotion across different firms. Controlling for these answers in the OLS specification may lead to a closing of the gap between the OLS and fixed effects results if these answers were correlated with pre-MBA job characteristics that were in turn correlated with wages.

Table 10: Selection on Workplace Skills (CDFs)[†]

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 or equal to	0.2780*	0.3417	0.5964	0.5480*
-1	0.4350*	0.6248	0.8520	0.7745*
0	0.5919	0.7658*	0.9596	0.9117*
1	0.8117	0.8887*	0.9821	0.9731*
2				
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.

* Statistically different from those who obtain an MBA at the 5% level.

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 7 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 Measuring the Net Monetary Benefits

Until this point, we have followed the convention in the literature on returns to schooling by focusing on the effect of obtaining an MBA on wages. We now give some consideration to the costs of MBA attainment. While the estimated average return for top-ranked programs substantially exceeds that of lower-ranked programs, it is also the case that the tuition costs tend to be significantly higher in top-ranked programs.⁴³ The additional costs borne by students at top programs may be even higher when foregone earnings are also taken into account. Thus, in this section we estimate the net benefit of obtaining an MBA at different quality programs. In particular, lifetime earnings forecasts are projected using fixed effects estimates of the average return to programs in the top-10, 11-25 and outside the top-25. We focus specifically on full-time MBA's and the effects of MBA attainment versus non-MBA attainment on the discounted lifetime earnings of a white male of average age and experience whose employer does not pay for schooling. By necessity we ignore the nonmonetary benefits or costs that may be incurred with MBA attainment and focus on measuring a net monetary benefit. For comparison purposes, we assume that the estimated treatment on the treated for each group would also apply to those who chose a different treatment. We then discuss what the differences in expected earnings across program types implies about the existence of heterogeneous treatment effects and private costs of obtaining an MBA.

⁴³The estimated average out-of-state tuition for the 1994-95 year was \$17,066 for top-10 programs. For programs ranked 11 through 25 and programs outside the top-25, average tuition of schools in the sample was \$12,750 and \$5,562, respectively. Data on tuition was obtained from *Barron's Guide to Graduate Business Schools* (Miller, 1994).

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 significantly different from zero at the 5% level.

Table 12 presents estimates of the lifetime net monetary benefit of attending each type of full-time MBA program. The numbers represent the expected additional discounted earnings stream from MBA attainment versus no MBA attainment, net of two years of tuition cost and foregone earnings. The rows correspond to the observed treatment, while the columns correspond to counterfactual treatments. Thus, the diagonal shows the expected benefits for each type of MBA program for those who actually completed that program. The off-diagonal elements show counterfactual expected returns if the individual had enrolled in a different program and for the case of the non-MBAs, the expected benefits of having completed any of the MBA programs.⁴⁴ As seen in the table, despite substantial costs of tuition and foregone earnings, a fairly large monetary benefit of attending full-time MBA programs remains, especially for highly-ranked programs. The expected lifetime monetary benefit of individuals having attended top-10 programs is estimated to be over \$113,000 (1994 dollars). Individuals attending programs immediately outside the top-10 are almost as well off, with an estimated net benefit of \$93,805 for the top 11-25 programs. The relatively small benefit of attending programs outside the top-25 of \$17,321 (compared to the expected benefit for the same individual attending a top ranked program) can be explained by supply side constraints—admission into the top programs is limited and highly competitive. To further describe differences in returns across programs, we also calculate the internal rate of return, the discount rate that equates expected benefits with expected costs, which includes tuition and foregone earnings. Assuming the same individual characteristics as was done in Table 12, we estimate internal rates of return of 11.3%, 11.2% and 7.7%, for programs in the top 10, top 11-25, and programs outside the top 25, respectively.

Note that the higher predicted treatment effect for those who did not receive an MBA versus those who received an MBA outside of the top-25 is consistent with either heterogeneous treatment effects (in which case the forecasted earnings do not apply) or lower effort costs for those who attend institutions outside the top-25, as discussed in the previous section. Finally, it is worth noting that the comparable OLS estimates of the net benefits predict substantial losses for attending programs outside the top-25, underscoring the importance of adequately controlling for selection into these programs.

⁴⁴For the counterfactual estimates, individuals retain the average fixed effect from their observed status and acquire the β_{TT} associated with the counterfactual program type.

Table 12: Predicted average lifetime net benefit of full-time MBA, by observed and potential program quality (in 1994 dollars)

		Expected Benefit		
		Top 10	Top 11-25	26+
Observed	Top 10	113303	108469	27428
	Top 11-25	97169	93805	23211
	26+	74636	73324	17321
	No MBA	94436	90411	22235

[†]Derived from fixed effects estimates using CPI adjusted wage data. Predictions based on a white, unmarried, 27 year-old male with the average estimated fixed effect for white males within each actual MBA category. Reported figures are equal to discounted earnings projected from Fall of 1994 until age 65 (assuming continuous employment) net of average tuition cost (projected to 1994-95) by MBA category and predicted foregone earnings. Calculated using the sample average work week of 47 hours, 50 weeks of work per year, and a discount factor of 0.94.

7 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 admittance. 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 time-invariant productivity is generally positively correlated with obtaining an MBA implying that the better the productivity controls the smaller the estimated returns to an MBA. The returns to attending programs in the top-10 and next fifteen for males fall from 25% and 20% when only controlling for observed ability measures to 19% with fixed effects. 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 effects. This does not arise because of a dip in wages before enrollment, and 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. Including proxies for workplace skills substantially decreases the gap between the OLS and fixed effects estimates for males. This provides evidence of negative selection into lower ranked business school conditional on taking the GMAT and not attending a top-25 program.

MBA programs clearly have different goals than those associated with undergraduate education. However, at the margin those who find school easy may choose to compensate for poor skills in other dimensions by obtaining more human capital through schooling. Our results suggest that analyzing the selection into schools and the workplace on the basis of non-cognitive skills may be important in obtaining accurate estimates of the returns to schooling.

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