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The payoff to school selectivity: An application of Dale and Krueger's method to MBA programs

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ABSTRACT

Many studies find a notable return to college quality. Dale and Krueger (2002, 2011) only do until they address selection bias concerns by proxying for ambition and by matching students with similar admission outcomes but different matriculation decisions. Although we employ similar methodologies to Dale and Krueger, we find substantial returns to MBA program selectivity.

1. Introduction

Both because of the rising costs of higher education and increasing income inequality, the payoff to attending more selective schools has headlined many popular media stories and economic journal articles. Estimates of the economic returns to higher education and to school quality, though, are plagued by concerns about unobserved characteristics that affect student and college decisions and future earnings. To address possible omitted variable bias, researchers have sought either more information about applicants (via richer or sibling data sets) or to establish causality with a variety of techniques, such as regression discontinuity design, propensity score matching, fixed effects, and instrumental variables.[1] Although almost all studies find a notable return to college selectivity, Dale and Krueger (2002, 2011) do so only until they use two approaches to attempt to control for students' otherwise unobservable aspiration, ambition, and motivation: (1) matching students with similar sets of school acceptances and rejections by quality but who make different choices and (2) including a "self-revelation" specification of the number and quality of schools to which students apply.[2] Long (2008) reports strong returns to the quality of undergraduate institutions with OLS estimation but not when using Dale and Krueger's (2002) matching method. We contribute to this literature by using similar techniques to address the selection problem but do so for a post-baccalaureate degree, the Masters of Business Administration (MBA). In contrast, though, we find strong and significant returns to selectivity in all specifications.

2. Empirical strategy and data

Our analysis utilizes data derived from the GMAT Registrant Survey, four waves of panel data from 1990 to 1998, including Wave I individuals who registered to take the Graduate Management Admission Test (GMAT) and were surveyed prior to enrolling in an MBA program. We restrict our sample to those individuals who completed an MBA. Similar to Dale and Krueger (2002) but in

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1 For reviews of this literature, see Dale and Krueger (2011) and Long (2010).

2 However, Dale and Krueger (2002) find that net tuition has a positive effect on wages, even in the matched applicant and self-revelation models (see Table 8, page 1521).

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a panel data context, we assume that earnings are related to individual i's attributes in the following way:

$$\ln W_{it} = \beta_0 + \beta_1 \text{MBA}_{it} + \beta_2 \text{GMAT}_{j*} \times \text{MBA}_{it} + \beta_3 X_{1i} + \beta_4 X_{2i} + \varepsilon_i, \quad (1)$$

where *MBA* is an indicator variable representing whether or not the individual has completed an MBA degree by time t, and GMAT_{j*} is the average GMAT score of students at the MBA program (j) attended by student i. The average GMAT score of the enrolled students proxies for quality of MBA program. The term β_2, our coefficient of interest, allows the returns to an MBA to differ by program quality. Higher quality programs are also likely to have higher admission criteria. X_1 and X_2 are vectors of the characteristics used by MBA programs to determine whether to admit an individual, of which the former are observable variables and latter information is unobservable to the econometrician but was known to admission committees. In the context of MBA applicants, X_2 may contain information such as the quality of employment experience, communication with employers, confidence in interviews, etc. Generally, if the variables contained in X_2 are valued (or penalized) by both employers and admission committees, omitting these variables would lead to an upward biased estimate of β_2.

Our analysis involves running OLS regressions on Eq. (1). Initially, we control for a relatively rich set of observable variables (X_1), namely race (white, Asian, black, Hispanic), gender, undergraduate GPA, age, age squared, years of work experience (through Wave 1), tenure on the current job (Wave I), and whether the undergraduate institution was considered highly selective or moderately selective by Barron's *Profiles of American Colleges*. Since the data were linked to GMAT records, we also control for individual's actual verbal and quantitative GMAT scores. Finally, we know the number and quality of schools to which the individual had their GMAT scores sent upon registering for the test; we use this information to proxy for otherwise unobservable ambition or potentially self-perceived ability. Our dependent variable is the logarithm of hourly wage, calculated from survey questions regarding compensation and hours worked. Average GMAT scores of institutions were obtained from *Barron's Guide to Graduate Business Schools*.

Wave II of the GMAT Registrant Survey asks respondents to indicate their top two choices of MBA programs, as well as whether or not they have applied, and whether or not they have been admitted to those programs. Furthermore, by default we have admission information on a third school, if an individual reports receiving an MBA from a school not among those top two choices.

Following Dale and Krueger (2002), we attempt to control for unobservable factors in admission (X_2) with the inclusion of a large number of dummy variables representing all of the alternative admitted/rejected sets present in our sample. In particular, we divided the MBA programs into four tiers based on quartile of average GMAT score (less than 510, 510–540, 540–575, and greater than 575).[3] Only matched individuals were included in the sample, meaning that at least two individuals had the same profile of acceptances and/or rejections from the same tiers of schools. Identification of the quality premium is based on differences in average GMAT of the school attended, among those individuals with the same admission and rejection set. For example, two individuals may have the same admission/rejection set, of having been rejected by a top tier school and accepted by both a top tier school and a second tier school, but one of them attended the top tier school while the other attended the second tier school.

Table 1 displays descriptive statistics of the overall sample, as well as the limited sample based on only those individuals who had comparable matches.

3. Results and discussion

Table 2 shows the results from estimating Eq. (1) and column (i) suggests that an increase in 100 points of the average GMAT score of enrolled students (e.g., MIT versus Florida State University) results in a 16% increase in earnings of graduates, a substantial wage premium. While holding constant a number of individual characteristics, including individual quantitative and verbal GMAT scores, this result does not control for differences in individual application and admission decisions.[4]

In column (ii) we control for the number of schools to which individuals sent their GMAT scores at the time of GMAT registration and the average GMAT scores of students at those schools, an attempt to control for differences in ambition or self-perceived ability, comparable to Dale and Krueger's (2002 and 2011) "self-revelation" model. In contrast to their results, though, our estimate of the quality premium remains statistically unchanged (though the sample size and point estimate are slightly diminished). Before using matching sets of admission outcomes, in column (iii) we control for similar combinations of application and admission decisions by each of four tiers of school quality (as described in Section 2), omitting individuals without relevant matches. Column (iii), then, lists results using specification (i) for our reduced sample of individuals with matched admission sets. While the estimated quality premium is smaller than with the full sample, it remains positive and significant. In column (iv) we add acceptance/rejection dummies, allowing people with different admission/rejection sets to have different levels of earnings (throughout the panel, both prior to and after the MBA). The column (iv) point estimate of the selectivity premium actually increased from the estimate found in column (iii), though the two estimates are not statistically different. Finally, in column (v) we include both the full set of admission/rejection dummy variable controls as well as their interactions with the MBA variable; thus, this specification allows the acceptance/rejection set to affect the general level of earnings and the returns to the MBA. In this specification, the most robust to selection, the coefficient representing the quality premium actually increases in magnitude (though it is not statistically different from the estimate in column (iv)).

4. Conclusion

While the effect of school quality in the undergraduate context remains under debate, we offer evidence that program selectivity yields large individual returns to the graduate business degree. While we can only speculate, it seems reasonable that more selective MBA programs offer more value in the form of curriculum, job recruitment services, alumni networks, or simply

3 Dale and Krueger (2002) utilized a more refined matching method with schools separated into ranges by 25 SAT points, since their data set contained substantially more observations than does our data set. Our analysis allows for 288 potential admission sets. This includes 4 possibilities for the school attended, 8 possibilities for the second observed school (accepted from any of 4 GMAT groups or rejected from any of those groups), and 9 possibilities for a third school (including a missing value). Only 54 of these potential admission sets are actually observed in our final sample. However, the inclusion of this still substantial number of dummy variables should go a long way towards controlling for selection into programs of higher versus lower quality.

4 In addition to individual GMAT scores, including average GMAT at the school attended results in substantively similar findings. The coefficient on this added variable is statistically insignificant in all specifications, while the estimates of the effect of quality remain statistically significant (though slightly lower in magnitude).

Table 1
Summary statistics of regression samples, Wave 1.

	Full sample	Matched sample
Avg. GMAT of school attended	549.08 (51.07)	568.73 (52.09)
Avg. GMAT of schools where scores were sent	556.744 (42.01)	570.18 (39.38)
Asian	0.14	0.17
Black	0.10	0.11
Hispanic	0.16	0.15
Female	0.37	0.35
Verbal GMAT	30.34 (7.17)	31.34 (7.43)
Quant GMAT	31.37 (7.90)	32.35 (8.00)
Undergrad. GPA	3.07 (0.40)	3.08 (0.40)
Hourly wage ($)	15.23 (6.43)	14.95 (6.16)
Experience <1 year	0.20	0.18
Experience 1–3 years	0.26	0.31
Experience 3–5 years	0.19	0.20
Experience 7+years	0.24	0.21
Tenure (years)	3.12 (3.53)	2.77 (2.84)
Age (years)	28.13 (5.67)	27.38 (4.92)
Undergrad moderately selective	0.28	0.27
Undergrad highly selective	0.25	0.33
Number of schools sent scores	3.25 (1.86)	3.61 (1.81)
# Individuals	1165	364

Note: Reported are means (and standard deviations) of non-missing samples of Wave 1 respondents to GMAT Registrant Survey who went on to complete an MBA prior to the final survey.

Table 2
Earnings premiums due to MBA program selectivity using admission decisions and outcomes to reveal unobservable characteristics.

	Full sample	Controlling for # and avg. GMAT of schools sent scores	(i) With matched sample	Similar school-GMAT quartile admission matches	
				Accepted/rejected set dummies	Accepted/rejected dummies + MBA interactions
	(i)	(ii)	(iii)	(iv)	(v)
MBA *average GMAT score/100	0.160	0.150	0.109	0.138	0.206
Std. error	(0.026)	(0.026)	(0.052)	(0.058)	(0.087)
Adjusted R^2	0.409	0.413	0.402	0.495	0.519
Observations	3289	3006	1016	1016	1016
Individuals	1165	1058	364	364	364

Notes: Dependent variable is log(hourly wage). In addition to the variables indicated in this table for each specification, all regressions included the individual demographic and background variables found in Table 1, plus a quadratic in time and an indicator variable for MBA completion. Errors clustered at the individual level.

the opportunity to associate with other highly able and motivated students. An advantage of our data set is that it broadly reflects the wide range of MBA programs in the United States. Long (2008) notes that the main Dale and Krueger (2002) finding is restricted to students at highly elite schools which have a very narrow range of variation in quality (589). Perhaps, then, our findings of strong returns to program selectivity reflect the starker differences in the range of program quality available in our data. It should also be emphasized that Dale and Krueger, due to a substantially larger sample size, are able to utilize more refined matching on the basis of smaller SAT ranges. Thus, the lack of a significant difference in our estimates when we control for matching could be due to our having proxies for unobserved ability/ambition that are cruder than Dale and Krueger's proxies.

As with other studies of returns to selectivity, the methodologies used here do not presume to eliminate the endogeneity problem since school attendance is not randomly assigned, and individuals may still differ in their specific returns to different MBA programs, even within the same school choice set. Our aim in controlling for similar choice sets is to reduce the otherwise large influence of selection. To the extent that individuals positively select (in terms of their ability or ambition) into schools of higher quality, attempting to control for at least part of this selection should reduce estimates of the quality premium. Nonetheless, we find that the quality premium remains strong and significant in all specifications.

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