Why Is an Elite Undergraduate Education Valuable?

Evidence from Israel

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I. Introduction

Economists have become increasingly interested in the effect of peer groups on individual outcomes. It is, however, exceedingly difficult to distinguish true effects from characteristics that are common to both the individual and his peer group but are not observed by the researcher. It is even more difficult to distinguish among the various mechanisms through which peer groups might affect their members.

In this paper we compare students in Israel who attended one of the leading universities, Hebrew University (HU), with those who attended a professional undergraduate college, College of Management Academic Studies (COMAS). To understand why this comparison is interesting, we must consider the institutional setting. Entry into HU is largely formulaic. Each department sets a cutoff entry requirement based on the bagrut (high school exit exam) and a pychometric test similar to the SAT. All students above that cutoff are admitted to the program. There is a small range below the cutoff in which students may be admitted if space remains and a very small possibility for the head of the department or program to make exceptions.

As a result, the probability of being admitted to HU increases dramatically as the student's test scores go from slightly below the cutoff to slightly above it. Moreover, most students who are admitted to HU choose to attend rather than to go to COMAS. HU is both more prestigious and less expensive since COMAS is private while HU is heavily subsidized by government. At current exchange rates, the tuition at HU is about $2,400 compared with about $6,000 at COMAS.

COMAS has degrees in law, economics, accounting and management, all of which are also offered at HU. For each of these fields, we know each student's entry score. Therefore, it is possible to compare outcomes for COMAS and HU graduates while controlling for a polynomial in entry score.

If all we could learn from this study is whether HU students do better, controlling for initial characteristics, than do COMAS students, it would probably be of little interest outside of

Israel. However, one feature of the Israeli education system makes it unlikely that outcome differences reflect differences in the quality of instruction. Almost all of the courses at COMAS are taught either by university faculty or by COMAS faculty who also have lecturer appointments at a university. For a large fraction of the courses, the syllabus and exams are identical at the two institutions at which the instructor teaches. In some areas, COMAS waits until HU has announced its exam schedule and then schedules exams at the same time, knowing that the exams will be identical. This is emphasized in a recent report on the economics program at COMAS by the Committee for the Evaluation of Economics Study-Programs [in Israel]:

> … faculty members said that, while they would prefer to offer more applied courses, they were bound to follow the syllabi set by the research universities; to do otherwise would penalize their students. (Helpman et al, 2008, p. 6)

If there is no difference in quality of instruction, there are still at least three mechanisms by which the institution a student attends could affect outcomes:

1. Peer effects on learning: Students' learning could be affected by the quality of their peer group. University students might ask better questions or participate in better discussions in class or be more able to explain the material to a peer having difficulty with the material.

2. Sorting/statistical discrimination: Being part of a better peer group could raise the market's perception of the individual's unobserved ability as in sorting and statistical discrimination models in economics or labeling models in sociology. Thus, if the market does not have ready access to the test scores used to determine entry into HU, firms might pay more to otherwise comparable university graduates simply because they know that university students generally had better secondary school performance than did college graduates.

3. Networks: Finally, better peers could provide students with better networks. Students who go to HU may be more likely to have colleagues in good jobs who, in turn, offer them good jobs.

If the first of these effects is operative, we would expect that, conditional on their pre-college/university performance, university graduates would do better on external exams. We find no evidence to support this hypothesis. If anything, we observe the opposite which may reflect the smaller class size at COMAS.

To test the sorting model, we note, following Farber and Gibbons (1996) and Altonji and Pierret (2001), if university students benefit from being labeled as good quality students, we would expect that measures of pre-college/university performance would become more important over time in the market and that the university premium would decline with experience.

None of the explanations fits the data perfectly, and they are not mutually exclusive. However, our results are most consistent with a model in which employers have good information about the quality of HU graduates and pay them according to their ability, but in which the market has relatively little information about COMAS graduates so that their return to skill increases over time. Because COMAS graduates are, on average, weaker, they earn less, on average, than do HU graduates. Initially high skill COMAS graduates are treated as if they were the average COMAS graduate, but over time the market differentiates among them so that after several years of experience, COMAS and HU graduates with similar entry scores have similar earnings.

Our results are therefore consistent with the view that employers use education information to screen workers but that the market acquires information fairly rapidly (Lange, 2007). The results for HU are consistent with the general finding for the United States that the market can infer the pre-college skills of college graduates (Arcidiacono, Bayer and Hizmo, 2008). Since COMAS was relatively new at the time of our study, the market may have been less able to assess its graduates.

II. Literature Review

In some sense it is obvious that peer effects exist. Immigrant children of non-English speaking parents who are brought up in the United States are much more likely to learn English even before they enter school than are apparently similar children who are not brought up in the United States. While we cannot rule out selection as an explanation – the immigrant children are those who would have learned English anyway – we find this implausible.[1]

The real questions are when and how are peer effects operative, and how important are they in particular settings. This literature is extensive, and we cannot do it full justice in a brief review. Understandably much of the research on this topic focuses on school-age children, and the findings have been very mixed. Hoxby (2000), Hanushek, Kain and Rivkin (2002) and Ammermueller and Pischke (2006) use random variation in classroom composition and find large but complex peer effects. Hanushek et al (2003) use a value-added approach and find that students benefit from having more able peers. Consistent with Hoxby's finding of nonlinear effects, Angrist and Lang (2004) find negligible peer effects from a small integration program while Gould, Lavy and Paserman (forthcoming) find notable effects from a large scale immigration.[2]

The literature on peer effects in higher education is much less extensive. Arcidiacono and Nicholson (2005) use random variation in the quality of cohorts within medical schools and find evidence of at most very modest peer effects. In contrast, Sacerdote (2001) and Winston and Zimmerman (2003) find some evidence of effects of randomly assigned college roommates on each other's academic performance.

[1] For those inclined to be pedantic: we also recognize that four-year olds might realize that they are more likely to grow up and work in the United States and therefore invest more heavily in English language skills. We suspect that anyone who actually believes this has little experience of four-year olds. It is true that these children may find learning English more valuable because they want to communicate with other children on the playground, but this is a peer effect.

[2] There is also a notable literature that uses experimental and quasi-experimental techniques to examine the effect of neighborhood on outcomes (Katz, Kling and Liebman, 2001; Oreopoulos, 2003; Jacob, 2004, Sanbonmatsu et al, 2006).

The peer effects literature is largely divorced from the literatures on educational sorting and statistical discrimination. It is unlikely that potential employers have information on random variation in the quality of a worker's peers and therefore unlikely that it affects their wage offers. However, employers may have information about systematic variation in the peer group from which individuals are drawn (Phelps, 1972, formalized in Aigner and Cain, 1977). The evidence suggests that employers use educational attainment to screen workers (Lang, 1986; Bedard, 2001), but that the market learns about worker productivity over time (Farber and Gibbons, 1996; Altonji and Pierret, 2001) and possibly quite quickly (Lange, 2007).

Finally, peers may be a source of information about jobs (Granovetter, 1974) or about workers (Montgomery, 2001).[3] Holzer (1996) documents the extensive use of networks in hiring in low-skill labor markets. We will be examining high-skill labor markets where networks might be expected to play a lesser role, but it should be recalled that Granovetter's study focused on professional workers in a Boston suburb. Moreover, anyone who has participated in the academic labor market for new PhD economists is well aware of the role of networks even in a setting where potential employers should be able to judge quality fairly accurately and information about job openings is readily available.

III. Theory and Methods

As discussed in greater detail below, we have data on the performance of Hebrew University and College of Management Academic Sciences graduates before, during and after university/college. We use these data to examine whether which institution a student attends affects his or her outcomes and, if so, through what channel.

[3] See Ioannides and Loury (2004) for a more extensive review.

Peer effects on learning

Although we have argued that the formal education is similar at COMAS and HU, informal learning may be quite different. HU students may benefit more from discussions with classmates, study sessions, etc. On the other hand, classes at COMAS tend to be smaller, and this may benefit its students. Also, in contrast with HU, most core courses at COMAS have two hours of teaching assistant led discussion sections in addition to the four hours of lecture that are standard at both institutions. If we find that students at HU outperform those at COMAS, this will be strong evidence of peer effects on learning. If we find the opposite, then the effect of peers is insufficient to outweigh the benefits of smaller class size and more teaching assistant time.

We test for effects on learning in two ways. First, we have data on GPA. We estimate the equation

$$GPA = a + b\ HU + f(\text{entry score}) + XB + e \qquad (1)$$

where HU is a dummy variable for whether the individual attended HU and f is a polynomial of degree four in the entry score. The subscripts denoting individuals are dropped for convenience.

One concern about equation (1) is that, contrary to the claims of faculty at COMAS, they may use lower standards for COMAS than for HU students, or faculty who teach only at HU and not at COMAS may be tougher graders than those who teach at COMAS. Both law and accounting have professional exams that we can use to test this hypothesis. For these two fields we can test the "grade inflation" hypothesis by asking whether COMAS students have higher grades than would be expected on the basis of their performance on the professional exams. Since these exams are graded by a common set of examiners outside of the two institutions, they are not subject to the grade inflation argument.

To test for grade inflation, we therefore estimate

$$GPA = a + b\,HU + c\,\text{Prof. Exam Grade} + d\,\text{Attempts at Prof. Exam} + XB + e. \quad (2)$$

We choose this "reverse regression" approach because it provides a simple way to control simultaneously for the grade on the exam and the number of times the individual took the professional exam. In the case of accounting, it is easy to control simultaneously for both professional exams.

Equation (2) has no causal interpretation. It is merely a way to test for differences in grading between the two institutions. We can learn more about peer effects on learning by examining the direct effect of the institutions on performance on these exams. We therefore replace GPA as the dependent variable with

$$\text{Prof. Exam} = a + b\,HU + f(\text{entry score}) + XB + e \quad (3)$$

where the left hand side is a measure of performance such as the test score or the number of attempts.

Of course, we must recognize that there may reasons other than peers for differences in outcomes. For example, one of the institutions may put more emphasis on preparing students for the professional exams. However, given the importance of these exams, we find it unlikely that there is a dramatic difference between the two institutions in the centrality accorded to the exams. To the extent that the focus of the two institutions differs, the estimates address whether the peer effects are sufficient to outweigh them.

Labor Market Effects

If there are effects on learning, we would expect the choice of institution to affect the earnings of graduates. However, to preview the results somewhat, our evidence suggests that

COMAS students do as well as HU students on professional exams and have higher GPAs although much of the difference in GPA probably reflects differences in grading standards.

Nevertheless, there may still be differences in earnings between HU and COMAS students. If the market does not have information about the productivity of individual students but does know from which institution they graduated, it will treat individuals as representative of the institution from which they are drawn (Phelps, 1972). In this case, HU graduates will earn more than COMAS graduates do, and in each case, weaker students will benefit from being pooled with stronger students while stronger students will be penalized. Note that in each case, all students would prefer to graduate from HU.

However, we would expect the market to learn gradually about workers' true productivities. The now standard approach (Altonji and Pierret, 2001) to testing for such effects is to include additional information that may not be readily available to the market.[4] Thus we typically estimate something similar to

$$\ln w = XB + b\,HU + c\,\text{Entry Score} + d\,\text{Exp.} + f\,HU*\text{Exp.} + g\,\text{Entry Score}*\text{Exp.} \qquad (4)$$

If the market does not know or does not use the entry score information, then the estimate of c will be zero and b will capture the difference in average productivity between the two groups. After the market has had time to learn about workers' true productivities, the effect of HU on earnings should be 0 while entry score should have a positive effect. In practice, we probably want to allow the effect of experience to be nonlinear, but we abstract from this issue for the moment.

[4] Farber and Gibbons (1996) use a specification that ensures that the additional information is orthogonal to any variables that are known to the market.

If the effect of attending HU does not go to zero over time, then either firms are not profit maximizing or there must be a direct benefit of attending HU. Perhaps there is learning not captured by GPA or professional tests or HU students benefit from better networks.

However, networks are often modeled as a source of information about workers. If HU students have better networks than COMAS students do, we would expect HU students to receive greater rewards initially for their own productivity while COMAS students would be treated more like the average of their group. This suggests a more general specification

$$\ln w = XB + b_1 H + b_2 E + b_3 S + b_4 H*E + b_5 E*S + b_6 H*S + b_7 H*E*S + e \qquad (5)$$

where H refers to HU graduates, E is experience, S is the entry score. This specification nests a large number of models. If learning is unimportant, than the coefficients on all the interaction terms should be zero. If learning is similarly important for HU and COMAS graduates, then the coefficients on the last two interactions should be zero. In the extreme case, where HU graduates had good networks that fully revealed their productivity and the market has no information about recent COMAS graduates, b_3 and b_6 would be zero. In practice, when we use the full sample, we include experience squared and experience squared interacted with being an HU graduate.

IV. Data

COMAS graduates students in four major areas: accounting, management, economics and law. In 1999 both institutions graciously agreed to send questionnaires to a random sample of their graduates. We provided questionnaires in sealed envelopes, and administrative assistants at each institution were asked to select students at random based on an algorithm we provided. The sample was supposed to be limited to students who graduated between 1993 and 1998. We allowed replacement sampling for the modest number of envelopes that were returned as undeliverable.

Unfortunately, we learned from the administrative assistant at COMAS that a new secretary had misunderstood the instructions and sent some of the surveys to students who graduated in earlier years. Fortunately, we made this discovery sufficiently early that we were able to ask the HU administrative assistant to alter the sampling procedure to match that of COMAS. As a result, for both institutions we have a small number of respondents who graduated in the late 1980s and early 1990s. For reasons we do not understand but which may be related to the same secretarial error, COMAS graduates from 1997 are noticeably over-represented in the sample. With this exception, the distribution of respondents across years is similar for HU and COMAS. A total of 600 surveys were returned for a response rate of roughly 38 percent of those for whom the letter was not returned as undeliverable.

In 2001, we sent a further 600 surveys to students who graduated between 1996 and 1999. Of these, 220 were returned. Four report graduating in 1995 and eleven in 2000. Otherwise, the results are consistent with proper sampling. We do not know whether the "illegal responses" represent reporting error or inappropriate selection of the sample, but the distribution of the samples across graduation years are the same for COMAS and HU, suggesting that the issue is not secretarial error.

COMAS provided us with information on average scores on the bagrut and psychometric test by major for 1993 - 1998. This allows us to compare our sample with the student universe. Unfortunately, we were not able to obtain similar information from HU. In addition, we used administrative records from COMAS and HU to determine the average GPA and approximate number of graduates by major at each institution for this period.

Our survey asked respondents about their pre-admission records (school-leaving exams know as the bagrut and psychometric test scores), their performance at university (GPA), their performance on professional exams, if any, and their earnings at various stages of their career, most importantly their current salary. In addition, the survey collected basic demographic information.

In order to verify whether our sample is representative, we compare (see Table 1) reported student performance (before, during and after college or university) with the means in the administrative records. For accounting and management, the confidence intervals for the sample means include the means in the administrative records for all three measures at COMAS and the single measure at HU. However, at both COMAS and HU, reported grades and pre-entry scores for economists and lawyers are somewhat higher for the sample than recorded in administrative records. We do not know whether this reflects non-random response, changes in student quality over time or a tendency for economists and lawyers to exaggerate their performance. While statistically significant, none of the discrepancies is large. We take this as some evidence that we do not have significant sample selection bias either from nonrandom response or from the difficulties in the drawing of our sample.

Table 2 summarizes the data. The sample is roughly evenly divided among the four fields and between students who attended COMAS and those who attended HU. Respondents who attended the College report lower entry credentials (65 points on the aptitude test and .72 points on the end-of-school exams). They also report lower scores on the professional exams and lower earnings.

It is worth noting that there is considerable variation across the fields in pre-university or pre-college performance. Law students show much higher entry scores than students in the other fields while economics students have the weakest entry credentials.

V. Results

Entry Score and University Attendance

From our discussions with relevant individuals, we understand that each year each department at the university and college selects target entry requirements based on the bagrut and psychometric exam. These requirements are, at first blush, complex combinations of results for the two exams. For example, in a recent year, Hebrew University published the following

requirements for entry into the economics program – at least 448 on the psychometric exam and 11.00 on the bagrut or at least 502 on the psychometric exam and at least 10.50 on the bagrut or at least 556 on the psychometric exam and at least 10.00 on the bagrut or at least 582 on the psychometric exam and at least 9.75 on the bagrut, or at least 771 on the psychometric exam and at least 8.00 on the bagrut.

As published, the cutoffs seem to be discrete approximations to what are essentially linear formulas. In the example above, the cutoffs in the table satisfy the formula 108 * bagrut + psychometric $\geq$ 1558. For other years and other majors, the linear relation is not perfect, but it is generally very close. For the period for which we have data, the entry requirement is always well approximated by the rule 100 * bagrut + psychometric is greater than some cutoff. Strikingly, in recent years this approximation has changed. Some fields, such as economics, use a tradeoff of 108 points on the psychometric exam for 1 point on the bagrut. Others, including management, accounting and law use a tradeoff of 46 or 47 points.

Our approach depends on the existence of a relatively sharp cutoff above which students are admitted to and attend the University and below which they attend the College. In fact, all 126 students with scores above 1693 attended the University. Similarly, almost all students with low scores attend the College. Of the 131 members of our sample with scores below 1490, only five attended the University.

Of course, there is still a significant range of overlap in which students attend both institutions. However, some of this reflects different standards for entry into the four fields. Figures 1a-1d show the relation by field. Except in economics, the division between students is quite sharp. For the fields other than economics, the overlap between the lowest entry score for an HU graduate and the highest score for a COMAS graduate is less than one hundred points, with the exception of one HU accounting graduate who reports a very low entry score. Although we have described entry as formulaic and argued that students admitted to both institutions will attend HU, there is still some overlap in the test scores of entrants. This reflects a number of

factors. First, the admissions cutoffs change over time. Second, HU departments may admit students up to sixty points below the cutoff. Third, COMAS provides some scholarship money in an attempt to attract stronger students. Fourth, some students might have failed to qualify for another university and attended COMAS instead.[5] And, fifth, respondents may have misreported their scores.

Academic Achievement

We begin by examining the grades of HU and COMAS graduates. The average in the sample ranges from 65 to 98 with a mean of 82 and a standard deviation of 5. If grading policies were truly consistent across institutions and there were no peer effects, we would expect average grades to be similar conditional on entry score and higher at HU not conditional on the entry score.

Unconditionally, grades are slightly higher at HU, by an average of 1.0 points. However, as shown in Table 3, conditional on a quartic in entry score and other controls, they are 4.4 points higher at COMAS. The difference, conditional on entry score, is highest in accounting (5.3 points) and lowest in economics (4.1 points). The results (not shown) are similar if we restrict the sample to the overlap range (entry score 1420-1693) and control for entry score linearly.

There are, of course, at least two interpretations of these results. First, the grade difference may be real in the sense that COMAS students may learn more relative to HU students of similar quality. Or, it may be misleading. Contrary to policy, there may be grade inflation at COMAS relative to HU or instruction at COMAS may focus more on test preparation. HU students may learn more material that is not covered on the test. Carrell and West (2008) find

[5] COMAS is closer than HU to Tel Aviv, for example. Students who want to live in or near Tel Aviv might have applied to Tel Aviv University and COMAS but not to HU.

that students who take principles courses from teachers who produce high test grades on a common test actually receive lower scores in subsequent classes.

We can cast some light on this issue by looking at the relation between grades and scores on the professional exams in law and accounting. In Table 4 we will look at the more intuitive direct regression of test scores on course grades. However, this regression is complicated by the fact that individuals may take the test more than once. In the second column of Table 3, we therefore look at the reverse regression. For law we regress average grade on the bar exam score, the number of times the respondent attempted the bar exam, and the other controls except for the entry score. The coefficient on the university dummy is slightly negative, indicating that grades at COMAS are higher than would be predicted on the basis of subsequent bar exam performance, but statistically insignificant. For accounting we follow a similar procedure but include scores and number of attempts for both exams. The coefficient on HU attendance is negative but again insignificant.

However, when we restrict our estimates to the overlap sample (and thus to a linear control for entry score), we do find significantly lower grades among HU students conditional on professional exam performance. Thus there is some evidence of grade inflation at COMAS.

The first column of Table 4 presents the direct regression of test score on university attendance, grades and the other controls. The results differ from Table 3 in part because we do not control for number of times the individual took the exam and because in accounting Table 4 examines one test at a time while in the reverse regression in Table 3, we can control for both accounting test scores at once.

The results tell a clear story. The performance of COMAS students on professional exams is weaker than would be anticipated on the basis of their grades if grades were consistent between the two institutions. On all three professional exams, when we control for GPA but not entry scores, HU students significantly outperform COMAS students. Moreover, if we divide the coefficient on the university dummy by the coefficient on GPA (not shown) to get an

approximate measure of grade inflation, the results for the three exams are 3.64, 4.14 and 4.06. The first number should be compared with the 4.66 point gap in GPA among law graduate and the second and third with the 5.30 GPA gap among accounting graduates, shown in the first column of Table 3. The difference between the two gaps is consistent with the small (and statistically insignificant) gaps in the second column of Table 3 when we control for performance on the professional exams. In sum, it appears that the higher GPAs at COMAS (conditional on entrys scores) are mostly, and possibly entirely, due to different grading standards.

Because the comparability of grades between institutions is suspect, in column 2 of Table 4 we focus on the relation between performance on the professional exams and entry score. In this column we compare test scores controlling for entry score (quartic), graduation year and demographic characteristics but not number of attempts. Two of the three coefficients are statistically insignificant although the coefficient on auditing is over one-fifth of a standard deviation and therefore not small. However, for the bar exam, our results show that COMAS students do significantly (at the .1 level) better than their equivalent counterparts at HU. The coefficient is large, over a third of a standard deviation. Since the results in column (3) show that HU students are ten percentage points more likely to retake the bar exam, Table 4 suggests that, at least in law, COMAS students outperform HU students on the professional exam.

The situation in accounting is somewhat different. All four coefficients suggest that COMAS students do somewhat worse, but none of the coefficients remotely approaches statistical significance at conventional levels.

In sum, controlling for pre-entry performance, COMAS and HU students perform similarly at least as measured by professional tests and GPA, even allowing for some grade inflation. If there is a university wage premium, it cannot be ascribed to the sorts of skills measured directly by exams.

Earnings

Table 5 shows the results of regressing the individual's current wage on a dummy for attending HU and additional controls. The first column shows the coefficient on the HU dummy when we control only for experience and the year of the survey, and when we pool the majors, dummies for three of the majors. Not surprisingly HU graduates earn higher wages overall and within each major. The differences are large and highly significant. Column (2) adds controls for demographics but has little effect on the results.

When we control for a quartic in entry score, the university premium is dramatically reduced overall and is eliminated in law. Although somewhat surprisingly it increases in accounting, the change is not statistically significant at conventional levels. Nevertheless, this somewhat anomalous result raises the concern that the quartic is not appropriately adjusting for pre-entry performance. We therefore replicated the results limiting the sample to those in the overlap range (1420-1693) and controlling only for entry score only linearly. The results are similar to those shown in column (3) of Table 5.

Although the results in column (3) are consistent with no difference in wages between COMAS and HU students and a large differential for accounting students, the sampling variation is sufficiently large that we should not draw this conclusion. Instead, we cannot reject the specification that combines all four majors ($F(51,527)=1.02$).

Column (4) adds the professional tests to the specification in column (3). We continue to find a large differential in accounting. The differential in law is now positive but modest and statistically insignificant at conventional levels. The differential in accounting remains large.

To explore the HU/COMAS wage differential in greater detail, we turn to estimation of equation (5) above. This specification allows the differential to vary over time and to depend on the entry score of the graduate. Although we have heretofore specified the dummy variable as "university," for reasons that will become apparent, in one of the interactions, it is easier to

specify it as "college." It should be recalled that when the equation is unrestricted, this is just a linear transformation.

The results of estimation of equation (5) are presented in the first column of Table 6. This specification constrains the bagrut and psychometric scores to predict wages in relation to their importance in the entry score. Thus wherever the bagrut/psychometric score enters the equation, it is replaced by the entry score or psychometric score + 100*bagrut.

The point estimate suggests that HU graduates with no experience and a zero entry score earn about 332 log points less than COMAS graduates. However, the slope of HU earnings with respect to the entry score is significantly higher for HU students. Each point on the entry score is worth .00239 log points more in current earnings for HU graduates than for COMAS graduates. Together the estimates imply that initially COMAS graduates earn more than HU if their entry score was at least 1388, which is the case for all HU graduates.

In contrast, the coefficient on the entry tests is negative (-0.006) and statistically insignificant for COMAS graduates. In other words, when dealing with COMAS graduates with no experience, the market does not seem to have (or use) information about graduates' skill as measured by their entry score. As in Phelps's model of statistical discrimination, COMAS graduates are treated as indistinguishable from each other and are paid less because on average they are less productive. The greater understanding of differences among HU students than among COMAS students results in lower starting wages for COMAS graduates regardless of ability.

However, over time, the market learns to distinguish among COMAS graduates. The three-way interaction of the entry score information, experience and the college dummy is positive and statistically significant. In contrast, the interaction between the entry score information and experience is negative and statistically insignificant. In other words, we cannot reject the hypothesis that the market has full knowledge of the information contained in the entry score for HU graduates entering the market. However, the market takes more time to learn this

information for COMAS graduates. Note that as it does, the market wage for lower ability COMAS graduates will tend to fall, which explains the otherwise somewhat surprising negative coefficients on experience and its square.

Because of the many interaction terms in column (1), it is somewhat difficult to understand how the HU/COMAS differential evolves over time. Figures 2a and 2b present this information visually. After four years of experience, the entry score has a positive effect on earnings for COMAS graduates although the slope remains lower than for HU graduates. At low entry scores, earnings are actually higher for COMAS graduates although this difference is only statistically significant for those at the lowest end of the HU entry score distribution. After seven years, the slopes of the two lines are similar. The point estimates suggest that the earnings of COMAS graduates are nontrivially higher but the difference is not statistically significant.

The second column restricts the model so that all information contained in the entry score of HU graduates is immediately available to the market and none of this information is immediately available for COMAS graduates. This restriction is not rejected by the data. Not surprisingly, the results are similar to those obtained in the first column. We again find that all predicted wages are initially higher for HU than for COMAS graduates, regardless of entry score. Over time the market gains information about the COMAS graduates so that after about ten years, the coefficients on entry score are similar for the two groups.

It is not obvious that the market and educational institutions should put the same relative weight on pre-entry tests. The third and fourth columns of Table 6 relax this assumption. In equation (5) we replace entry score with psychometric test + γ*bagrut and estimate γ along with other parameters by nonlinear least squares. The estimating equation in column (3) is otherwise identical to that in column (1). Our estimate of γ is 69 and is statistically insignificantly different from 100. Not surprisingly the results are similar to those in the first column. The point estimates continue to suggest that the market has good information about HU graduates but little or no information about recent COMAS graduates but acquires information comparable to its initial

information about HU graduates within seven years. Again we cannot reject the hypothesis that the market has no knowledge of the information contained in the COMAS graduates' entry scores but has complete knowledge about this information for HU graduates.

The restricted model (column (4)) gives similar results. However, the estimate of γ is lower (60) and falls just short of being significantly different from 100 at the .05 level. Although this does not change the interpretation of the results, it is striking that the educational institutions have been putting less weight on the bagrut in recent years.

VI. Conclusions

There seems to be little difference in educational outcomes between students who graduate from HU and those who graduate from COMAS. Performance on professional exams is comparable. While students at COMAS have higher grades than would be expected on the basis of their pre-entry performance, much, and perhaps all, of this difference is attributable to differences in grading standards. Thus our findings are consistent with Helpman et al (2008) who find that the strengths and weaknesses of the education provided by COMAS are in many respects similar to those found at the research universities.

Except for those with a strong interest in higher education in Israel, by itself, this finding would not be of great interest. However, coupled with our results regarding wages, it suggests a great deal about the role of higher education in the labor market. We find that the market knows a great deal about HU graduates entering the market at least in the sense that pre-entry information is captured in their wages. This is consistent with the Arcidiacono, Bayer and Hizmo (2008) finding that the U.S. labor market "knows" the AFQT of college graduates. However, it takes the market about seven years to fully acquire this information about COMAS graduates.

There are at least two explanations for the U.S. result. The first is that college admissions reflect much of the information captured by AFQT. The market can therefore infer much of this information from where an individual attended college. The second is that there are strong

networks between colleges/universities and the market that allowed such information to be conveyed. In all probability both mechanisms are at work.

The Israel experience suggests that it takes time for the market to learn how to judge graduates from a relatively new institution. Either the market had insufficient information to judge COMAS transcripts or it lacked networks to capture information about COMAS graduates.

Moreover, because we can condition on where the individual graduated, our results suggest that the market is not merely inferring ability from where the individual graduated but is gathering information about the student from other channels. Thus an elite education is not merely a signal. This is important because a standing criticism of sorting models is that education is an expensive signal. Therefore, critics argue, if the sole role of attending a good university is to signal ability, universities should go into the business of identifying good students, and then those students should enter the labor market directly. Our findings do not speak to whether what students learn in higher education institutions is productive, nor can we prove that not all of HU's information about students is immediately available to it from the application, but they do at least suggest to us that time spent at HU provides valuable information to the market. We suspect that with time, the market will acquire information about COMAS graduates more quickly.

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TABLE 1
COMPARISON OF SURVEY AND ADMINISTRATIVE DATA

	Undergrad GPA	Bagrut	Psychometric	Bar Exam
College of Management				
Law	*	*	*	
Survey	82.4	9.80	630	77.0
Administrative	81.0	9.65	623	78.0
Economics	*	*	*	
Survey	82.0	8.96	582	
Administrative	81.0	8.60	570	
Accounting				
Survey	81.0	8.93	621	
Administrative	81.2	8.88	615	
Management				
Survey	82.3	8.97	610	
Administrative	83.0	8.95	610	
Hebrew University				
Law	*			
Survey	83.3			80.7
Administrative	82.0			80.6
Economics	*			
Survey	83.0			
Administrative	81.0			
Accounting				
Survey	82.1			
Administrative	82.0			
Management				
Survey	83.3			
Administrative	82.5			

* Indicates survey and administrative results are statistically significantly different at the .05 level.

TABLE 2
SAMPLE CHARACTERISTICS

	Law	Economics	Management	Accounting	University	College
University	0.48 (0.50)	0.55 (0.50)	0.49 (0.50)	0.56 (0.50)	1.00	0.00
Psychometric Test	662 (48)	617 (59)	645 (54)	658 (51)	676 (47)	611 (44)
Bagrut	9.97 (0.41)	9.37 (0.63)	9.43 (0.72)	9.46 (0.80)	9.89 (0.46)	9.17 (0.73)
College/Univ. GPA	82.9 (4.5)	82.6 (5.0)	82.8 (5.0)	81.6 (5.3)	82.9 (4.9)	81.9 (5.0)
CPA/Bar Exam Score	78.8 (8.4)	-		69.1 (6.8)	75.0 (9.2)	72.4 (8.6)
Auditing Exam Score	-	-		67.1 (7.2)	68.5 (8.0)	65.4 (5.7)
ln Current Wage	9.30 (0.47)	9.16 (0.39)	9.39 (0.35)	9.52 (0.44)	9.47 (0.42)	9.20 (0.41)
Age	30.7 (3.0)	29.3 (3.2)	31.0 (3.4)	31.9 (3.7)	30.9 (3.1)	30.5 (3.8)
Male	0.57 (0.50)	0.63 (0.48)	0.65 (0.48)	0.69 (0.46)	0.65 (0.48)	0.62 (0.49)
Native	0.89 (0.32)	0.85 (0.35)	0.88 (0.32)	0.88 (0.33)	0.86 (0.35)	0.89 (0.31)
Russian Father	0.06 (0.24)	0.05 (0.22)	0.03 (0.16)	0.05 (0.22)	0.04 (0.20)	0.06 (0.24)
Sephardic Father	0.21 (0.41)	0.30 (0.46)	0.23 (0.42)	0.31 (0.46)	0.22 (0.42)	0.31 (0.46)
Ashkenazi Father	0.26 (0.44)	0.18 (0.38)	0.16 (0.37)	0.21 (0.41)	0.22 (0.41)	0.18 (0.38)
N	195	226	190	209	428	392

TABLE 3
AVERAGE GRADE: UNIVERSITY RELATIVE TO COLLEGE

	Controlling for Quartic in Entry Score	Controlling for Performance on Professional Exams
All	-4.36 (0.44)	
Law	-4.66 (0.80)	-0.93 (0.62)
Economics	-4.14 (0.99)	
Accounting	-5.30 (1.07)	-0.86 (0.73)
Management	-4.93 (1.14)	

All regressions control for year of graduation (dummies), born in Israel, Sephardic, father born in Russia, father Ashkenazi, age, a dummy for the 2001 sample, and sex. "All" also controls for field of graduation.

TABLE 4
TEST PERFORMANCE AND UNIVERSITY/COLLEGE ATTENDANCE
(Coefficient on Attended University)

	Grade		**Number of Attempts**
	Controls for GPA	**Controls for Entry Score Quartic**	**Controls for Entry Score Quartic**
Bar Exam	3.29 (1.12)	-2.86 (1.70)	0.10 (0.05)
Financial Accounting	2.44 (0.88)	-0.63 (1.45)	-0.18 (0.17)
Auditing	1.95 (0.91)	-1.52 (1.38)	-0.08 (0.20)

All regressions control for year of graduation (dummies), born in Israel, Sephardic, father born in Russia, father Ashkenazi, age and sex.

TABLE 5
RELATION BETWEEN UNIVERSITY ATTENDANCE AND EARNINGS

	(1)	(2)	(3)	(4)
All	0.224 (0.026)	0.209 (0.025)	0.091 (0.037)	
Law	0.284 (0.060)	0.231 (0.060)	-0.017 (0.082)	0.058 (0.077)
Economics	0.274 (0.048)	0.282 (0.046)	0.119 (0.063)	
Management	0.150 (0.048)	0.151 (0.049)	0.110 (0.082)	
Accounting	0.170 (0.061)	0.146 (0.061)	0.208 (0.088)	0.216 (0.079)
Experience controls	yes	yes	yes	yes
Demographic controls		yes	yes	yes
Quartic in Entry Score			yes	yes
Test score controls				yes

Estimates for "all" also control for field of graduation. Experience controls are years since graduation and its square. Demographic controls are born in Israel, Sephardic, father born in Russia, father Ashkenazi, age, sex, married, married interacted with sex, government employment, public employment, and whether the individual has a second degree.

TABLE 6 LN WAGES, EXPERIENCE AND TEST SCORES				
	Entry Score		**Bagrut & Psychometric Test Separately**	
	(1)	(2)	(3)	(4)
University	-3.315 (0.108)	-1.996 (0.421)	-2.994 (1.07)	-1.785 (0.417)
Experience	-0.012 (0.003)	-0.055 (0.086)	-0.012 (0.003)	-0.012 (0.003)
Experience Squared	-0.007 (0.039)	-0.012 (0.039)	-0.007 (0.039)	-0.007 (0.039)
University * Experience	0.325 (0.247)	0.025 (0.091)	0.343 (0.245)	0.097 (0.098)
University * Experience Squared	0.017 (0.004)	0.018 (0.004)	0.017 (0.004)	0.018 (0.004)
Bagrut/Psychometric Test /100	-0.006 (0.038)	*	-0.016 (0.048)	*
Bagrut/Psychometric Test * Experience/100	-0.017 (0.012)	*	-0.016 (0.014)	*
Bagrut/Psychometric * University/100	0.239 (0.066)	0.160 (0.025)	0.270 (0.084)	0.192 (0.036)
Bagrut/Psychometric * College * Experience/100	0.033 (0.015)	0.016 (0.005)	0.043 (0.019)	0.026 (0.009)
Coefficient on bagrut in entry score	100	100	69.12 (22.60)	60.75 (20.25)

Also controls for field of graduation, born in Israel, Sephardic, father born in Russia, father Ashkenazi, age, sex, married and married interacted with sex, government employee, public sector employee, second degree and 2001 sample..

Figure 1a

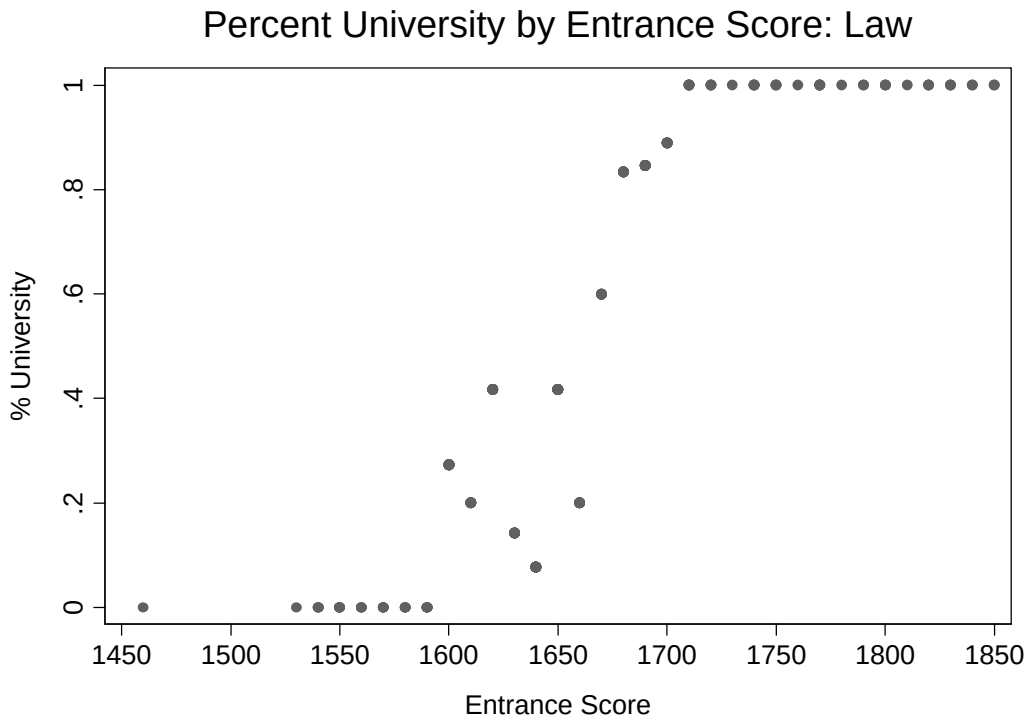


Figure 1b

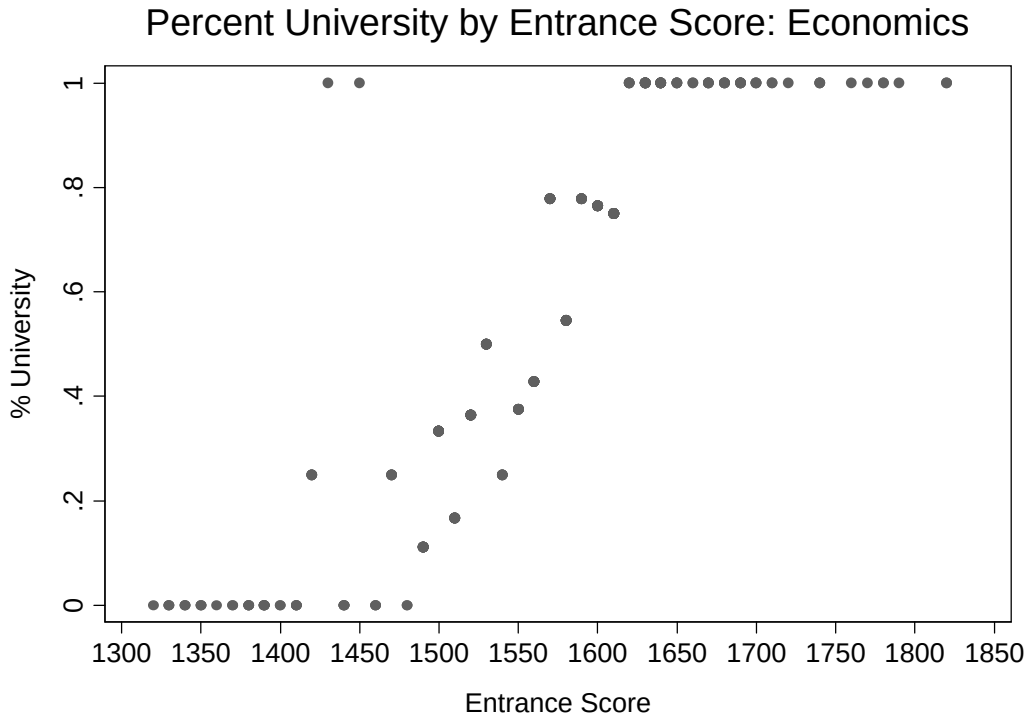
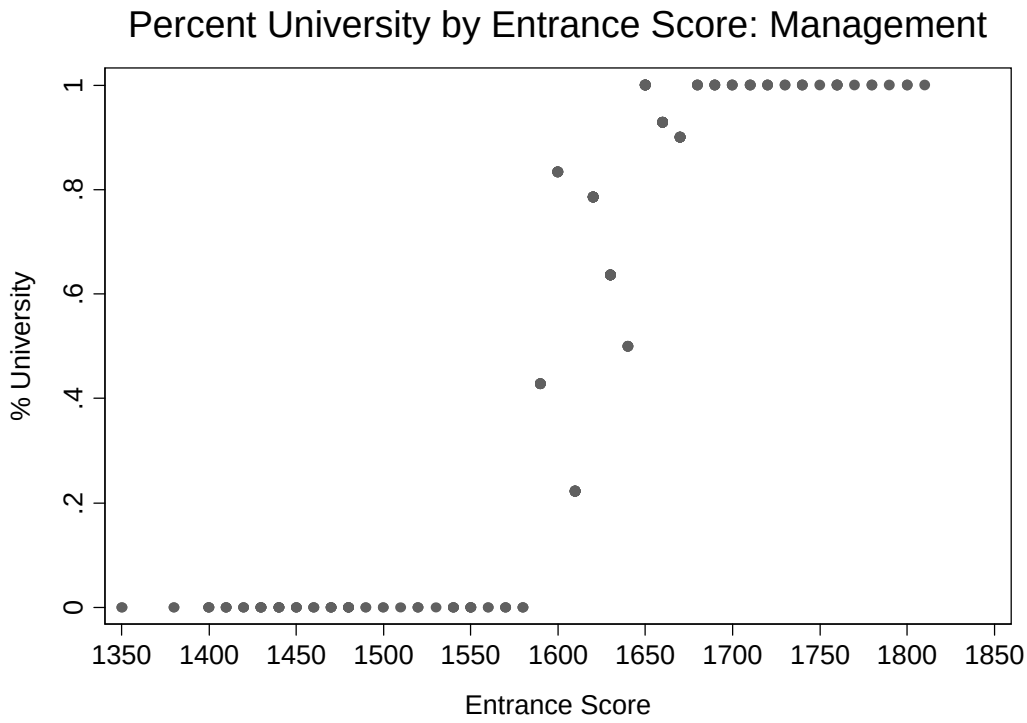


Figure 1c



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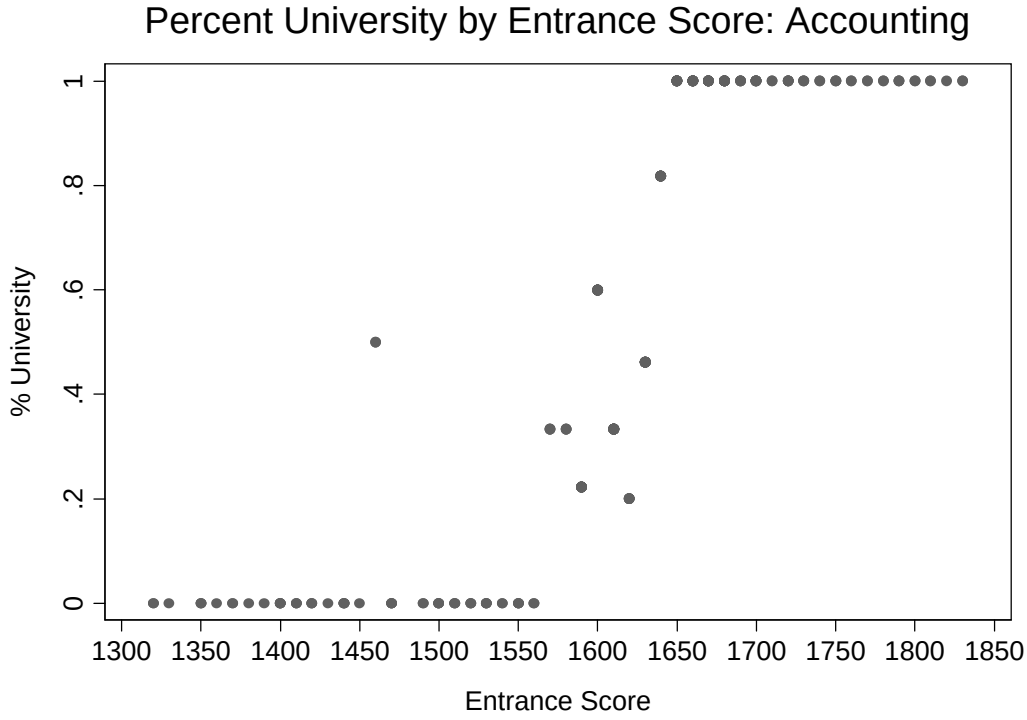

Figure 2a

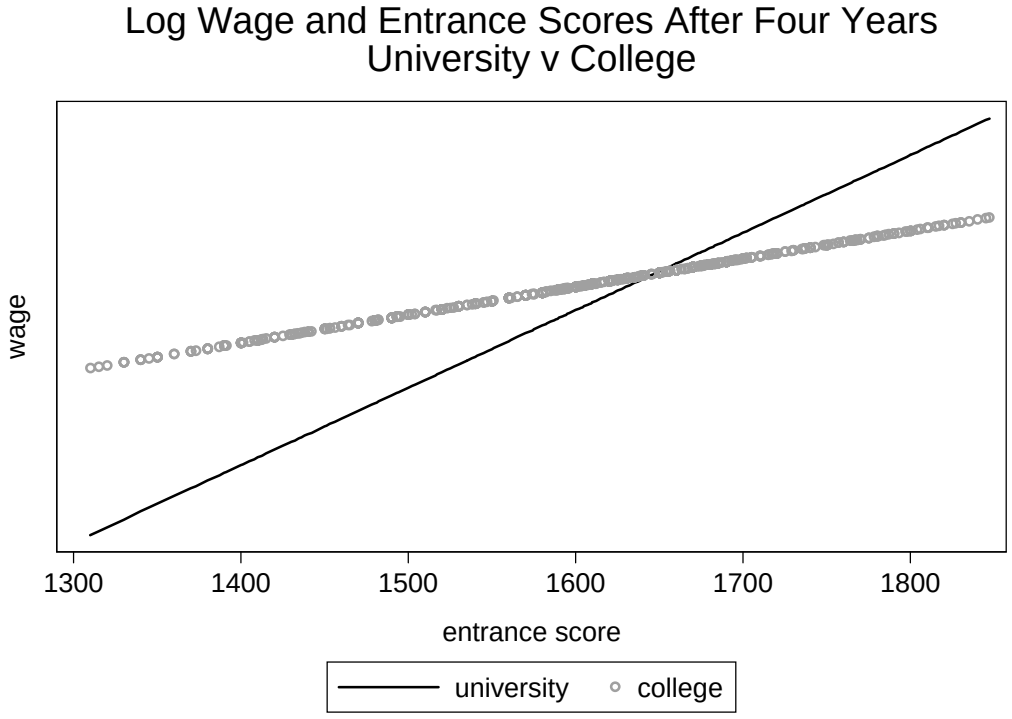

Figure 2b