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Quantile Treatment Effects of College Quality on Earnings: Evidence from Administrative Data in Texas

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Abstract

This paper uses administrative data on schooling and earnings from Texas to estimate the effect of college quality on the distribution of earnings. We proxy for college quality using the college sector from which students graduate and focus on identifying how graduating from UT-Austin, Texas A&M or a community college affects the distribution of earnings relative to graduating from a non-flagship university in Texas. Our methodological approach uses the rich set of observable student academic ability and background char- acteristics in the data to adjust the earnings distributions across college sectors for the fact that college sector quality is correlated with factors that also affect earnings. Although our mean earnings estimates are similar to previous work in this area, we find evidence of substantial heterogeneity in the returns to college quality. At UT-Austin, the returns increase across the earnings distribution, while at Texas A&M they tend to decline with one's place in the distribution. For community college graduates, the returns relative to non-flagship four-year graduates are negative across most of the distribution of earnings, but they approach zero and become positive for higher earners. Our data also allow us to estimate effects separately by race and ethnicity, and we find that historically under-represented minorities experience the highest returns in the upper tails of the earnings distribution, particularly among UT-Austin and community college graduates. While we focus on graduates, we also show our estimates are robust to examining college attendees as well as to many other changes in the sample and to the estimation strategy. Overall, these estimates provide the first direct evidence of the extent of heterogeneity in the effect of college quality on subsequent earnings, and our estimates point to the need to consider such heterogeneity in human capital models that incorporate college quality.

1. Introduction

A large body of work in economics shows that college-educated workers earn a sizable premium in the labor market (Card, 1999; Goldin and Katz, 2008; Autor, Katz and Kearney, 2008), but less has research has been done examining whether this premium depends on college quality. The US higher education system is characterized by a large and growing amount of heterogeneity (Hoxby, 2009), which produces significant differences in per-student resources and peer quality but also in costs across institutions. Although the average college earnings premium is high, those students who attend a higher-quality postsecondary school may experience the largest returns to collegiate attainment. Such a difference in returns would be consistent with the fact that higher-quality universities typically are more expensive to attend. Since US colleges receive a significant amount of public support from federal and state governments, who obtains the returns to college and how these returns vary across school types and students has implications for the efficient allocation of that support. Furthermore, due to the high opportunity cost of attendance, in terms of tuition, fees, and forgone earnings, understanding how the choices of college students along dimensions of postsecondary quality affect future earnings is of primary importance.

The motivations for examining how college quality affects earnings point to the importance of understanding the impact on the entire distribution of earnings, not just the mean. The social welfare implications of any positive mean quality effect differ conditional on whether the whole earnings distribution shifts outward or whether the gains are concentrated at the top or the bottom of the distribution. For example, if higher-quality schools only shift out the top of the earnings distribution, then relatively few students would obtain the return to investing in such schools while all students would bear the costs. Although the mean return to college quality would be positive in such a circumstance, the investment would include substantial risk for students because these returns are experienced by a potentially small number of students. Which part of the earnings distribution is affected by college quality also has important impli- cations for whether public financing for higher-quality schools exacerbates or reduces income inequality, even among college graduates. Given recent

growth in income inequality in the US (Piketty and Saez, 2003), understanding how public investments in higher education affect the distribution of income is of high policy importance. Finally, examining the effect of college quality on the earnings distribution yields insight into the efficiency of the college selection process. College choices are made under much uncertainty, both with respect to the resources available at each school but also with respect to the quality of peers and the academic fit. This uncertainty could lead students to choose colleges that lead to lower returns than if they had selected another college in their feasible choice set (i.e., mismatch). Estimating the extent of heterogeneity in the returns to college quality is a first step in determining whether certain types of students are choosing colleges that do not maximize their return to college, as measured by earnings.

Despite the importance of understanding how college quality impacts the distribution of earnings, there has been no previous work attempting to answer this question. The previous literature on the returns to college quality focuses solely on mean effects, finding mixed re- sults. The balance of the evidence, however, indicates that students who attend or graduate from higher-quality schools earn more on average in the labor market. A series of papers us- ing "selection-on-observables" techniques show that students who attend schools with higher observed quality earn significantly more post-graduation (Brewer, Eide and Ehrenberg, 1999; Black and Smith, 2004, 2006), and Hoekstra (2009) finds similar effects exploiting an admissions cutoff rule at a large flagship state university. In contrast, Dale and Krueger (2002, 2011) employ a matching estimator that compares earnings among students who applied and got into the same set of schools but who attended schools of different quality and find no average return to college quality. However, they do find evidence of substantial heterogeneity, with lower-income and minority students experiencing positive average returns. At a minimum, this work suggests that the average effects estimated in previous work paint a misleading picture of the relationship between college quality and labor market returns. That mean effects miss important aspects of how college quality impacts earnings also is consistent with analyses of other large government programs, such as welfare reform and job training, that show

much heterogeneity in program effects on different parts of the earnings distribution (Bitler, Gelbach and Hoynes, 2006; Heckman, Smith and Clements, 1997).

This paper represents the first comprehensive study of the effects of college quality on the distribution of earnings. A key component of our contribution is the employment of extremely detailed administrative data that link high school records with college records and up to three years of earnings data derived from unemployment insurance records. Since these data come from Texas, a large and diverse state that has a large and heterogeneous public higher education system, we are able to generate precise estimates that are relatively rare in studies that estimate distributional effects. The administrative high school data include very rich information about student pre-collegiate academic ability, such as standardized test scores, information about students' academic track, and the high school from which they graduated. Thus, this data set is unique in both the size of the sample and the scope of relevant background characteristics we observe about each individual.

Our methodological approach follows the unconditional quantile treatment effects (QTE) methods outlined in DiNardo, Fortin and Lemieux (1996) and Firpo (2007). DiNardo, Fortin and Lemieux (1996) show that, if one has a set of observable characteristics with which to plausibly control for selection, one can construct counterfactual outcome distributions by reweighting the control group earnings distribution by the odds ratio of treatment generated from a regression of the probability of treatment on the observables. As described in Firpo (2007), the quantile treatment effects[1] can be estimated by taking the difference between the actual treated distribution and the counterfactual untreated distribution at a given quantile. The main assumption under which our QTE estimates identify the effect of college quality on the distribution of earnings is that our detailed academic and student background data are sufficient to control for the fact that students of high ability select into and graduate from more elite schools and also have higher earnings potential

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[1] Firpo (2007) distinguishes between the "quantile treatment effect," which is the quantile analog to the average treatment effect and the "quantile treatment on the treated" that is the quantile analog to the average treatment effect on the treated. We will use the term quantile treatment effect to refer to the quantile treatment effect on the treated, as that is the parameter we are able to identify.

absent their school choice. The data include a comprehensive set of controls that we argue account for such selection; the pre-collegiate academic information we observe about students is more detailed than has been used in previous "selection-on-observables" analyses of the effect of college quality on earnings, and we show extensive evidence of the robustness of our results to alternative specifications and to potential biases stemming from unobserved aspects of student ability.

We proxy for college quality by partitioning the Texas higher education system into four groups: University of Texas at Austin (UT-Austin), Texas A&M University at College Station (TAMU), all other four-year public universities, and all public two year colleges. The first two groups represent the two flagship schools in the state of Texas, and we split them up because UT-Austin is typically higher-ranked and because TAMU is highly focused on engineering and agriculture. Four-year public universities outside of UT-Austin and TAMU represent our control group, and we also examine how community college graduates' earnings compare to this control group.

While our mean estimates are similar to those from previous work, the quantile treatment effects show evidence of large amounts of heterogeneity in the returns to college quality that vary across school type. The findings suggest that the variance in the expected return to college is not a simple function of college quality. For UT-Austin graduates, the earnings premia mostly are increasing across the earnings distribution, from a low of 2.7% to a high of 31.7%, while for Texas A&M graduates there is much less heterogeneity in returns. We present evidence that differences in college majors between UT and TAMU graduates is a plausible explanation for the differences in returns experienced by these students. For community college graduates, the returns are mostly negative and tend to increase with one's place in the earnings distribution. Notably, for about 15% of the distribution, the estimated returns to community college versus non-flagship, four-year graduation are close to zero in magnitude and are not statistically different from zero at the 5% level. Given the large cost differences between two- year and four-year schools, these results suggest that community colleges may be optimal for a significant subset of students who are relatively high potential earners and who are choosing between a less-selective four-year school and a community college.

We also examine the effect of college quality on the distribution of earnings separately by race and ethnicity. This is among the first evidence that demonstrates that the returns to college quality vary by race, because most data sets used in previous work lack minority samples of sufficient size to estimate such parameters with any precision.[2] Our main findings show that the top and bottom of the earnings distributions of historically under-represented minorities are most affected by UT-Austin graduation, but at Texas A&M these groups experience a large level shift in earnings across the entire distribution. Among historically under-represented minority groups at community colleges, the top of the earnings distribution shifts out substantially, leading the higher earners to make more than their counterparts who graduated from a non- flagship public university.

We show evidence of extensive heterogeneity in the returns to graduating from postsecondary institutions of different quality. Our estimates demonstrate that mean impacts poorly charac- terize the breadth of returns a given student can expect as a function of college quality. These results underscore the need to understand how the variance of expected returns, rather than just the mean, affect student postsecondary attendance decisions across the quality spectrum. That college quality does not impact the distribution of earnings uniformly also has distribu- tional implications that should be taken into account when making higher education funding decisions, as our results suggest funding certain types of schools will have uneven effects on the distribution of earnings that may not be intended by policy-makers.

2. Data

The data we use in this study come from three sources: administrative data from the Texas Education Agency (TEA), administrative data from the Texas Higher Education Coordinating Board (THECB), and quarterly earnings data from the Texas Workforce Commission (TWC). The data are housed at the Texas Schools Project, a University of Texas at Dallas Education Research Center (ERC). These

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[2] Dale and Krueger (2011) estimate average returns by race and ethnicity and find that African American and Hispanic students experience higher returns to college quality than other students on average.

data allow one to follow a Texas student from Pre-Kindergarten through college and into the workforce, provided individuals remain in Texas.

Because college quality is difficult to measure with a single variable or set of variables (Black and Smith, 2006), we follow much of the previous literature in proxying college quality by college sector (Brewer, Eide and Ehrenberg, 1999; Hoekstra, 2009; Bound, Lovenheim and Turner, 2010; Bound, Lovenheim and Turner, forthcoming; Lovenheim and Reynolds, forthcoming).[3] Due to data availability constraints, we focus only on public university graduates, and we split them into four comprehensive and mutually exclusive sectors: UT-Austin, Texas A&M at College Station,[4] other four-year public universities (i.e., non-flagship public universities) and community colleges. We examine UT-Austin and Texas A&M separately because they are the flagship universities of the State of Texas.

Table 1 shows the observable characteristics of the universities across these sectors. Both UT-Austin and Texas A&M have higher resources and quality measures than the other four-year and community college sectors. They both have much higher SAT scores and faculty-student ratios as well as spending per student that is twice the amount spent in the non-flagship universities. The two flagship universities also graduate over twice the proportion of students as the other four-year colleges. However, in-state tuition (unadjusted for financial aid) is about $1,000 more per year to attend the flagship schools. Community colleges are cheaper to attend than four- year schools as well, but they have far fewer resources than the public four-year sector. Thus, our four sectors have large differences in resources and measurable college quality associated with them, and they also define the relevant college choices for most students in Texas due to the dominance of public universities in that state.[5]

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[3] Our analysis therefore will focus on analyzing the effect of college quality as it relates to the type of sectoral choices that are faced by most students, rather than identifying the effect of altering a specific college characteristic or educational input level.

[4] Hereafter, we will refer to Texas A&M at College Station as "Texas A&M" or "TAMU." This university is to be distinguished from the other Texas A&M campuses, which are part of the the other four-year sector.

[5] Unfortunately, we do not have information about students who graduate from private universities. However, public postsecondary schools dominate the higher education market in Texas. In the National Longitudinal Study of 1988, only 9.6% of Texas high school graduates who went to college attended a

We focus on male graduates from Texas' public colleges and universities who graduated from high school during the years 1996–2002. The total sample size includes 94,071 male graduates, with 9,837 graduates from the University of Texas at Austin, 13,436 graduates from Texas A&M University-College Station, 47,935 graduates from Texas' other four-year public colleges and universities, and 22,863 graduates from Texas's community colleges. We only include males in the analysis because of the concern that many female college graduates are endogenously missing from the sample due to fertility decisions. The sample includes males who meet the following restrictions: 1) the student must graduate before the age of 25, 2) the graduate's earnings for a given year are included only if he worked for four consecutive quarters in the year, with the exception of 2009 where the requirement for inclusion is three consecutive quarters as we only have three quarters of available earnings data for 2009, and 3) The student must not be currently enrolled in graduate school when the earnings are measured.[6] These restrictions are meant to isolate the earnings of full-time working males, and they are similar to the sample restrictions imposed by Hoekstra (2009).

We obtained records of each individual's quarterly earnings from the TWC and examine earnings data for the years 2007–2009. Because these students graduated from high school between 1996 and 2002, they will be between 23 and 31 post-graduation when we observe their earnings. Examining earnings of graduates in their early 20s may be problematic if college quality increases the returns to experience. In such a circumstance, we will understate the earnings of college graduates from higher-quality schools relative to lower-quality schools. However, in Section 4.5 we estimate effects for the older sample who graduated from high school in 1996-1998. The results using this sample are similar to the estimates for the sample as a whole, which is suggestive that the relative inexperience of our sample is not driving our results.

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private college. Thus, our focus on public schools is appropriate given the small proportion of students who enroll in private universities in Texas.

[6] Students who earn a graduate degree are included. The fourth restriction ignores earnings while students are enrolled in graduate school because they likely are not reflective of the student's permanent earnings.

The use of data from 2007-2009 also is potentially problematic because of the large increase in unemployment rates during this period in the US. Since our data only include full-time workers, the recession may cause us to overstate quality premia to the extent that unemployment increases were inversely proportional to college quality. However, the recession in Texas was relatively mild: the average unemployment rate was 5.4% between 2004-2006 and was 5.6% between 2007-2009. In contrast, for the US as a whole, the unemployment rate increased from 5.1% to 6.6% across these two periods. Furthermore, we show below that our results and conclusion are robust to including all earnings observations, not just those from full-time workers. These estimates suggest that recession-driven unemployment is not generating a misleading picture of earnings distributions in Texas.

We observe more than one quarter of earnings for all sample members. In order to generate one earnings estimate for each respondent, we stack log quarterly earnings observations and regress them on year, quarter-of-year, and high school graduating cohort indicators. We use the within-graduate average of the residuals from this regression as the earnings measure in our empirical models. This method isolates the constant component of earnings for each individual over the period for which we observe his earnings and allows us to control for time- and cohort- specific shocks as well as for seasonality.

The data from the TEA consist of both individual- and high school-level information. The individual-level data include variables such as race/ethnicity, an indicator for whether the stu- dent has a college plan, participation in Title 1, whether the child receives free or reduced price meals, and the scores from the 11th grade reading, writing, and mathematics sections of the Texas Assessment of Academic Skills (TAAS). The high school-level data include enrollment, the ethnic composition of the school, and the percentage of the school that participates in talented and gifted programs. We obtain graduation status and timing from the THECB for each student as well.

The odd columns of Table 2 present summary statistics of individual characteristics for our analysis sample, separately by school type. As expected, the UT-Austin and TAMU graduates have higher high school test scores in every subject, and the community college students have the lowest

average high school test scores. The flagship university graduates also are more likely to be in the top decile of their school in each of these tests and have fewer black, Hispanic and economically disadvantaged students than non-flagship universities and community colleges. Overall, Table 2 demonstrates that students attending these different school types differ on important observable characteristics that are likely to affect earnings. Our empirical strategy described below seeks to eliminate the differences in the earnings distributions across sectors that are due to the differences in these observable characteristics.

A main limitation of the data we use is the fact that individuals only are in our sample if they graduated both from a Texas high school and a public Texas college. They also need to have at least three quarters of complete earnings data in Texas, which could be a limiting factor if students are in graduate school, if they leave the state or if they do not work. Because both UT-Austin and Texas A&M have more of a national profile than other universities in Texas, if these graduates are more likely to take a job in another state or if they are more likely to be attending graduate school, then our earnings distributions will be biased. Especially if the most high-skilled students are those who leave the state, the Texas A&M and UT-Austin earnings distributions will be biased downward. Furthermore, if college quality has an effect on the extensive margin of labor supply, it could create endogenous sample biases in our estimates.

Table 3 shows the characteristics of those included and excluded from our analysis sample among graduates of each school type. Excluded observations are graduates for whom we never observe full-time work. As the table demonstrates, those excluded from the sample are very similar to those included. Those who are in the top 10 percent of their high school class in reading and writing are slightly more likely to be excluded, but the difference is only 3 percentage points and this difference is present in all school sectors. Within school type there are few differences in the observable characteristics of graduates included and excluded from the sample, and comparing the flagship and community college sectors to the non-flagship sector, there are no discernible differential patterns of exclusion. The fact that our sample is balanced with respect to whether earnings are

present is summarized with the mean of predicted log earnings in Table 3. The predicted earnings are calculated by estimating a regression of log earnings residuals on all observable characteristics, separately by sector. We then predict log earnings using the resulting coefficients for the samples with and without earnings data in each sector. As the estimates in Table 3 indicate, these means are extremely similar, which suggests that the sample of men for whom we observe earnings is not systematically different from the sample of men for whom we do not observe earnings.

Finally, at the bottom of Table 3, we show the number and proportion of students included and those excluded because they attend graduate school. While the proportion included declines across the table (and thus with observable college quality), particularly within the four-year sectors, the differences are not large. As the rest of the table shows, these different inclusion rates are uncorrelated with the rich set of observable characteristics in our data. The percentage of students excluded due to graduate school attendance also is very similar for UT-Austin and Texas A&M, and it actually is slightly higher in the non-flagship sector.[7] The sum total of the evidence in Table 3 indicates that the sample restrictions we make are unlikely to create systematic biases in our earnings distributions for each school type. Tabulations by race and ethnicity, which are available upon request, show that observable characteristics among included and excluded students by school type do not differ substantially either for any of these groups.

The critical missing variable in Table 3 is earnings; because our earnings data only cover Texas residents, we are unable to determine whether students who move out of Texas after graduating are higher (or lower) earners. This problem is particularly salient for the state flagships, where the most capable students may compete in a more lucrative national job market. In order to generate some information on earners among those who stay in Texas (and thus who are in our sample) and those who leave, we examine log earnings in the 2000 US Census among BA

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[7] This result may be due, in part, to the fact that we only observe graduate school attendance if it is within the state of Texas. More graduates from non-flagship universities who attend graduate schools probably do so in-state.

recipients who report having lived in Texas five years prior and who would have been of college age at the time (18-21).

Panel A of Figure 1 shows the distribution of earnings in 2000 among BA recipients who are 23-26 and who lived in the Austin, TX MSA in 1995. The earnings distributions for those in Texas in 2000 versus those not in Texas in 2000 are virtually identical. Although there is a slight divergence at the top of the distribution, earnings are higher among those currently residing in Texas. A similar pattern holds for 23-26 year old BA recipients who lived in the College Station, TX MSA in 1995 (Panel B). While out-of-state workers earned more at the bottom 20% of the distribution,[8] the rest of the earnings distributions are very similar. We show below that the returns for Texas A&M are highest for lower earners. Panel B of Figure 1 suggests we may understate these returns slightly because we are unable to observe earnings from Texas A&M graduates who leave Texas. Finally, in Panel C, the earnings distributions for those who were in other areas of Texas are the same regardless of whether they currently live in Texas.[9] Overall, Figure 1 presents little evidence of systematic attrition by higher or lower earning students, which suggests our inability to observe earnings for non-Texas-resident workers is not driving our results.

3. Methodology

The goal of this analysis is to estimate unconditional quantile treatment effects of college quality on earnings. This method differs from the conditional quantile treatment effects literature (Koenker and Bassett, 1978; Abadie, Angrist and Imbens, 2002; Chernozhukov and Hansen, 2005) in the

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[8] The difference at the bottom of the distribution could be due to the inclusion of Blinn College students. Blinn College is a two-year school with a very high four-year transfer rate. If those students are lower earners and are more likely to remain in-state, the presence of these students in this sample could cause a divergence in earnings at the bottom of the distribution.

[9] We also do not find evidence of differences in the likelihood of having any earnings in the Census by school type. For example, the difference in the likelihood of having any earnings between UT Austin and other four-year graduates is -0.008 percentage points. For Texas A&M, this difference is larger at about 4 percentage points, but this difference still is small relative to the large returns we estimate for Texas A&M graduation. In addition, these differences are unadjusted for covariates, and if we were able to control for the observables in our administrative data, we suspect there would be even smaller differences in labor force participation by school type.

examination of treatment effects for each quantile of the marginal earnings distributions rather than the quantiles conditional on the covariates. The conditional quantiles are more difficult to interpret for policy purposes because they are unobserved, and thus conditional quantile treatment effects cannot be mapped simply into unconditional quantile treatment ef- fects. We estimate the latter since we are interested in understanding how college quality affects the observed distribution of earnings.[10]

We estimate quantile treatment effects associated with graduating from each college sector relative to the public non-flagship four-year sector. We focus on graduation rather than on attendance because graduation is the outcome most likely to be observed (and rewarded) by the labor market. Furthermore, estimates of the effect of college quality attended on earnings is complicated by the fact that many dropouts do not accumulate a lot of credits. So, even at high-quality schools, they will be relatively untreated by the university's quality. Since college quality is associated with higher rates of graduation, even conditional on student background characteristics and preparation for college (Bound, Lovenheim and Turner, 2010; Rouse, 1995), college graduates may constitute an endogenous sample. In Section 4.5, we show our results are robust to analyzing college attendees rather than graduates, particularly among four-year schools, but our main analysis focuses on graduates because we believe college graduation to be a more relevant post-secondary outcome for the labor market.

To estimate the quantile treatment effects of graduating from a particular college sector on earnings, first consider a two-sector higher education system, where students choose UT-Austin (T=1) or a non-flagship, four-year university (T=0). As described in Firpo (2007), the quantile treatment effect on the treated for quantile τ can be written:

$$QTT = q_{1,\tau}|_{T=1} - q_{0,\tau}|_{T=1}. \tag{1}$$

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[10] See Angrist, Chernozhukov and Fernandez-Val (2006) for conditional quantile treatment effects of education on wages. Carneiro, Hanson and Heckman (2003) also show substantial heterogeneity and uncertainty in the returns to attending college.

The inference problem faced in this analysis is that the counterfactual quantile for the treated sample, $q_{0,\tau}|T=1$, is unobserved. In order to estimate $q_{0,\tau}|T=1$, we generate counterfactual earnings distributions that show what the earnings distribution would be among the untreated group if the distribution of their observable characteristics were the same as in the treated group using the propensity score re-weighting method proposed by DiNardo, Fortin and Lemieux (1996). The approach consists of estimating a "selection" equation, which yields the estimated probabil- ity of treatment as a function of the observables, and then re-weighting the control distribution by the predicted odds ratio of treatment. In order to show how this method can be used to estimate quantile treatment effects, we begin with the original derivation of the re-weighting estimator as shown in DiNardo, Fortin and Lemieux (1996) and then embed the counterfactual earnings distribution in the QTE estimator proposed by Firpo (2007).

Each graduate can be described by earnings, w, a vector of observable characteristics, x, and a treatment status, T. The joint distribution of earnings and observables conditional on treatment status is given by:

$$F(w, x|T = t). \tag{2}$$

The density of earnings at UT-Austin can be calculated by integrating the earnings distribution over the distribution of observable characteristics for UT-Austin students:

$$
\begin{aligned}
f(w|T = 1) &= \int_x dF(w, x|T = 1) \\
&= \int_x f(w, x|T = 1)dF(x|T = 1) \\
&\equiv f(w|X = x_1, T = 1),
\end{aligned}
\tag{3}
$$

where x_1 is the distribution of observable characteristics among the treated (i.e., $x_1 = g(x|T = 1)$). We want to estimate $f(w|X = x_1, T = 0)$, which is the counterfactual earnings distribution among

those who were not treated that we would expect if their observable characteristics were identical to the observable characteristics of the treated group. DiNardo, Fortin and Lemieux (1996) show that:

$$
\begin{aligned}
f(w|X = x_1, T = 0) &= \int f(w, x|T = 0)dF(x|T = 1) \\
&= \int f(w, x|T = 0)\psi(x)dF(x|T = 0),
\end{aligned}
\tag{4}
$$

where

$$
\psi(x) = \frac{dF(x|T = 1)}{dF(x|T = 0)}.
\tag{5}
$$

Applying Bayes' rule, equation (5) can be written:

$$
\psi(x) = \frac{P(T = 1|x)}{P(T = 0|x)} * \frac{P(T = 0)}{P(T = 1)}.
\tag{6}
$$

Because $p(T = 1|x) = 1 - p(T = 0|x)$, equation (6) is the odds ratio of the conditional likelihood of treatment and $\psi(x)$ are weights that we will estimate using our rich set of observable student characteristics. We then apply these weights to equation (4) to generate a counterfactual distribution of earnings that would have been expected if the observable characteristics of students who graduated from non-flagship public universities in Texas were distributed the same as the observables of UT-Austin graduates.[11] This method is akin to the "aggregate decomposition" described in Firpo, Fortin, and Lemieux (2010). To our knowledge, this is the first analysis to use this propensity score reweighting method to estimate quantile treatment effects.[12]

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[11] This interpretation of this counterfactual earnings distribution also relies on the treated and untreated groups facing the same potential earnings structure. Given that these graduates all are working in the same state in the same time period, we believe this assumption is reasonable.

[12] Firpo (2007) discusses the feasibility of this approach for estimating quantile treatment effects, but he does not use this method.

The estimated quantile treatment effect then can be written as:

$$QTT = \{inf_q P[f(w|X = x_1, T = 1) \leq q] \geq \tau\} - \{inf_q P[f(w|X = x_1, T = 0) \leq q] \geq \tau . \quad (7)$$

Equation (7) is simply the difference between the unconditional quantiles of two marginal distributions: the observed treated distribution and the counterfactual untreated distribution. This difference identifies the quantile treatment effect for quantile τ under the assumption that the observable characteristics in our re-weighting function given by equation (6) are sufficient to control for the fact that the earnings distribution among UT-Austin graduates differs from the distribution among non-flagship graduates because they have different characteristics that are rewarded by the labor market.[13]

In order to adjust the non-flagship public university earnings distribution for the fact that students who graduate from these universities differ systematically from UT-Austin graduates in ways that affect future earnings, we leverage the extensive information in our administrative data on student backgrounds. The propensity score model that we use to generate the weights given by equation (5) is a logistic regression of the probability a student graduates from a school in sector j ($j \in \{$UT-Austin, TAMU, Community College$\}$) relative to a non-flagship four-year university:

$$I(j)_i = \alpha + \gamma X_i + \theta T_i + \phi E_i + \delta HS_i + \epsilon_i, \quad (8)$$

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[13] Much of the QTE literature discusses the role of the rank-permanence assumption as well, which requires that the treatment must not change individuals' relative places in the earnings distribution (Doskum, 1974; Lehmannn, 1974; Firpo, 2007; Bitler, Gelbach and Hoynes, 2006). This assumption is necessary in order to interpret QTE estimates as identifying the distribution of treatment effects rather than the effect of the treatment on the distribution of outcomes. That is, our estimates identify the effect of a given school type on median earnings, but in order to interpret them as the median treatment effect rank permanence is needed. Our analysis focuses on estimating how college quality affects the distribution of earnings, which is what is most relevant for welfare analysis and which does not require rank permanence to hold.

where X is a vector of individual background characteristics, T is a vector of high school test score controls, E is a set of high school education variables, and HS contains observed high school characteristics in the year the student graduated. The variables in X are student ethnicity/race (white, black, Asian, Hispanic), Title I status, English proficiency, and free and reduced price lunch status.[14] We include flexible controls for high school test scores, including quartics of student scores on the Texas state math, reading and writing standardized exams all students take in high school. Using the high school students attend, we also control for each student's relative rank within his or her school on each exam. Because we cannot observe GPA or class rank in our data, the relative rank variables control for the fact that higher-ranked students in each high school, conditional on test scores, are more likely to be admitted to higher- quality schools. The vector E contains information on high school educational programs, such as enrollment in gifted programs, special education, career and technology courses, whether the student had a college plan, and whether he was at risk of dropping out. Finally, we control for high school variables that measure the educational environment from which students came: school ethnic composition, the percentage of students in each economic status group, of gifted students, of students at risk, of title I eligible students, as well as total school enrollment.[15]

While the above description pertains to UT-Austin and non-flagship universities, we use the same methodology for each of the three "treatment" school types: UT-Austin, Texas A&M and community colleges. For each treatment sector, we estimate a separate version of equation (8) that includes the same independent variables but that uses separate indicator variables for

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[14] Notably, we cannot observe parental education and income for many students. These variables come from college application material, and only the more selective schools asked students for this information. In Section 4.5, we show our estimates are robust to using these variables, suggesting that the detailed demographic characteristics in our model are sufficient to control for selection based on family background characteristics.

[15] We also have included high school fixed effects in the propensity score model, which are potentially powerful in controlling for unobserved student ability that is correlated with college sector and earnings. These estimates are very similar to our baseline results and are shown in Section 4.5.

whether a student graduated from a school in a given sector relative to a non-flagship public school. All versions of equation (8) are estimated using logit models, and the predicted values from these logit models are used to construct the weights shown in equation (6).[16]

In the even columns of Table 2, we show descriptive statistics for the non-flagship group that are weighted by the relative odds of graduating from each sector in Texas. Because our propensity score model contains a large number of variables and because we assume a logistic functional form, it is not assured that the methodology described above will balance each observable across treatment and control sectors. Comparing the even and odd columns in Table 2, however, shows that our method fully balances the covariates in each sector; in no case is there a large or statistically significant difference between the observed mean and the re-weighted control group mean. At the bottom of the table, we calculate predicted log earnings in each sector using the observed graduates from that sector and all observed characteristics. We then predict log earnings for the treatment and control groups using the sector-specific coefficients. One can interpret these predicted log earnings as summary measures of the difference between the characteristics of the treatment and control groups as they relate to earnings. As the table demonstrates, not only are the means identical among the treated and weighted control groups, but the distributions are the same as well. Thus, our propensity score model produces the desired result: there is no predicted difference in the distribution of earnings across treatment and control groups. Any observed difference in the distribution of earnings must thus be due to school quality or to unobserved factors that are uncorrelated with the observables in our model.

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[16] Because students make one decision over a set of schools, it may be more appropriate to estimate the weighting function using a multinomial logit model of the choice between all college sectors. We have implemented this model for the choice among our three four-year public school sectors and the estimates, which are available upon request, are indistinguishable from those shown below. We present results that use weights generated from treating each schooling decision as a binary choice between a given sector and the non-flagship public sector in order to be consistent across all "treatments" examined in this analysis, but our estimates are unchanged if we treat college sector choice as a multivalued treatment.

Table 2 shows that there would be limited value to using a more flexible propensity score model that includes more interactions or to using non-parametric methods to estimate the weights given by equation (6); the weights obtained from estimating logistic regressions of equation (8) balance the distribution of observables across school types as they relate to earnings.

The variables we include in our model represent powerful controls for the fact that students of higher academic ability are more likely to graduate from a higher-quality school and to earn more. Although "student ability" is very difficult to account for perfectly, our flexible controls for standardized test scores in three different subjects, each student's place in their high school's test score distribution for each exam, and a detailed set of information about each student's high school composition and academic track are much stronger ability controls than have been used previously.[17] Conditional on these observable characteristics, the critical remaining question is what drives the residual selection of students into colleges of differing quality. If these choices are based on unobservables that also are correlated with earnings potential, it will bias our estimates. Currently, there is little understanding of why students select different school types. Although students are sensitive to quality differences among schools (Long, 2004; Avery and Hoxby, 2004), this is not the only factor that matters. Choices also rely on idiosyncratic preferences (such as whether a relative attended the school, the quality of the campus visit, the quality of the sports teams), the behavior of one's peers (e.g., whether many peers attend this school) and, in our data, the change in access to flagship universities that accompanied the

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[17] Black and Smith (2004, 2006) use the 1979 National Longitudinal Survey of Youth (NLSY79) for their analyses. The main ability measure in those data are the Armed Forces Qualifying Test. Brewer, Eide and Ehrenberg use the National Longitudinal Survey of the High School Class of 1972 and High School and Beyond in their analysis, which contain exams developed by the National Center for Education Statistics. While each of these data sets contain strong measures of student academic aptitude at the end of high school, our data contain a larger battery of exams as well as within-school rank on each exam and high school level demographic information that is not present in these other data sets.

implementation of the Top Ten Percent Rule in 1998.[18] Due to the detailed set of controls for

student academic ability and background we include in our model, we argue these three sources of

variation are the predominant residual determinants of college sector selection. Crucially, these

factors all are unlikely to be correlated with underlying earnings potential.

Variation in particular from the Top Ten Percent Rule is important to consider because

it had a large change on the admissions regime in Texas. Unfortunately, our data do not

include class rank, so we cannot exploit this change directly. Post-1998, the data include an

indicator of whether students are admitted to a given university under the Top Ten Percent

Rule, which on its own is highly predictive of attending UT-Austin and Texas A&M. However,

conditional on the relative test score rank controls in our model, this variable loses its predictive

power, suggesting that controlling for relative rank on standardized tests is sufficient to account for

the top 10% rule and for the effects of student relative rank on flagship admission. This result

also suggests that our controls are indeed highly correlated with college sector choices of

students. Furthermore, if we control for whether one is admitted through the Top Ten Percent Rule

as well as interactions of all variables with a post-1998 indicator variable, our estimates are

unchanged.[19] Thus, there is little evidence in our data that the imposition of the Top Ten

Percent Rule biases our estimates by altering the relationship between student ability and college

choice in a manner that we cannot measure.

In addition to the factors discussed above, it is possible that unobserved factors such

as "student motivation" or "non-cognitive" skills that are related to earnings also drive some of

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[18] The Top Ten Percent Rule – Texas House Bill 588 – is an admissions algorithm that grants admission at any public university in Texas to Texas high school graduates who finish in the top decile of their graduating class and apply to college within two years of finishing high school. It also permits each college or university to decide, on an annual basis, whether or not to offer automatic admission to students who finish in the top quartile. Prior to 1998, admissions included affirmative action considerations, which allowed colleges to consider factors such as the applicant's race, academic performance, class rank, curriculum, and standardized test scores. To our knowledge, there were no strict formulas relating these factors to admission decisions.

[19] These estimates are available from the authors upon request.

the college quality variation. We believe our estimates are inconsistent with the existence of biases from these omitted variables because of the small effect of our observables on earnings distributions,[20] because of the different shapes of the quantile treatment effects across school types, and because of the robustness of our estimates to controlling for factors that are likely to be correlated with these unobservables, such as parental income and education as well as high school fixed effects. With observational data, it is not possible to control directly for these difficult-to-measure student attributes, which highlights the importance of studies that use natural experiments with exogenous school quality variation (e.g., Hoekstra, 2009). Such natural experiments are exceedingly rare, however, which necessitates using observational data with extensive student background controls. That our mean estimates are similar to, if somewhat smaller than, the estimates from previous work suggests these variables are sufficient to control for the selection of students with higher earnings power into high-quality colleges.

As implied by equation (6), the estimated propensity scores must be less than one because the weights are not defined for those who are predicted with certainty to attend a given type of college.[21] Our propensity score models generate predicted probabilities of less than one for every individual in our sample. Similar to matching estimators, there also must be overlap of the propensity score distributions among the treated and control observations.[22] Without overlap of the propensity scores, there will be individuals in the treated group for whom there are no observably equivalent individuals in the control group. Thus, it would not be

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[20] One could argue that the small effect of the observables on the earnings distribution reflects the fact that our observable characteristics are poor controls for student academic ability. We stress, however, that the battery of test scores, relative rank and student background controls we have are likely to be very highly correlated with student academic preparation for college and that these ability measures are more extensive than the ones available in other data sets used to study college quality.

[21] Note as well that those with zero predicted likelihood of attending each college type will receive a weight of zero. These observations are effectively excluded from the analysis.

[22] See Smith and Todd (2005) for a detailed discussion of this issue with respect to matching estimators.

possible to construct a counterfactual earnings distribution that would occur if the distribution of observables in the control group was the same as in the treated group because there would be parts of the joint distribution of observable characteristics in the treated group that are absent in the control group. The presence of such non-overlap in observable characteristics will cause a bias in the estimation of the counterfactual earnings distributions.

Figure 2 presents estimated propensity scores from equation (8), estimated for each school type separately with the non-flagship universities as the control group. Each panel shows the number of individuals in each grouping of estimated propensity scores among treated and control observations. Due to confidentiality concerns, we are unable to present means calculated with fewer than 10 individuals, so these propensity score groups are the smallest equal-sized bins we could construct between 0 and 1. For no school types are there gaps in the estimated propensity scores, and even for those who have very high and low estimated probabilities of attending each school type there are those in the same propensity score range in both the treated and control groups. Although our propensity score models are based on a large number of observable characteristics that are designed to control for student selection into different school quality types, we have sufficient overlap of the predicted likelihood of treatment among treated and control groups to estimate equation (7).[23]

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[23] Flores and Mitnik (2009) present a method for generating common support among treatment and control groups when there are multiple treatments. They emphasize that in order to compare estimates for different treatments, the same sample with general common support needs to be used. Their method is to delete observations sequentially that are not in the common support for each treatment, such that the final sample is the sample for which there is support with respect to each treatment individually. Since we do not exclude any observations for any treatment due to the lack of common support among treatment and controls, this sequential support method leaves exactly the same analysis sample as we use in the analysis. Because of this feature of our data, comparisons of the quantile treatment effects across treatment sectors are valid.

4. Results

4.1 Mean Effects

Before estimating the quantile effects, it is instructive to examine mean effects of college quality on earnings as a point of comparison with the rest of the literature. Table 4 shows estimated mean impacts, regressing log earnings residuals on treatment indicators while adding in controls sequentially. We add in the controls in this manner in order to show how correlated they are with school type and with earnings. We first show estimates from bivariate regressions, and the mean effects all are large and statistically significantly different from zero at the 1% level. When we add in our test score controls, the estimates become smaller in absolute value. We then add in student demographic characteristics and school experience controls (i.e., limited English proficiency, gifted and talented, at risk for dropping out) and finally include school average demographic characteristics. These variables further attenuate the estimates. However, conditional on the test score controls, the other variables do not have large impacts on the estimates. We interpret this as evidence that our test score measures are powerful controls for selection into school types, because the demographic, school experience and school composition variables are all correlated with college sector and with future earnings. This highlights the value of our set of test controls in this setting.

Focusing on the estimates with a full set of controls, the mean effect for UT-Austin is 11.5%. This estimate is somewhat smaller than in previous work that estimates the returns to attending an elite public university,[24] however it is still positive, statistically different from zero at the 1% level,

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[24] Hoekstra (2009) estimates the earnings effect of attending an un-named flagship state university is 24%. Brewer, Eide and Ehrenberg (1999) estimate a mean return of 26%. Note that both of these papers focus on attendance, while we examine the effect of graduation. This difference may cause our estimates to be smaller if part of the return to attending a flagship university is increasing the likelihood of finishing. Furthermore, in the Hoekstra (2009) analysis, the counterfactual for those not attending the flagship is a mix of lower-ranked four-year attendance, community college attendance, and non-college attendance. This counterfactual may cause the estimated earnings returns to be higher than if a counterfactual of non-flagship four-year universities is used.

and sizable in magnitude. The mean effect for Texas A&M relative to non-flagship public universities is much higher, at 21.2%. This estimate is similar to the results from the existing literature. Finally, for community colleges we estimate a mean effect of -10.0% on earnings, which is somewhat larger in magnitude relative to previous findings of the effects of community college enrollment on earnings.[25] Overall, the similarity of our mean estimates to prior work is inconsistent with the existence of large biases due to unobserved student attributes in our results. But, we emphasize that this conclusion is only suggestive due to both the sample differences and the differences in counterfactuals that make cross-study comparisons difficult.

4.2 Earnings Distributions

The purpose of this analysis is to determine the impact of college quality on the distribution of earnings. A value of our methodological approach is that it allows us to show both quantile treatment effects as well as the underlying earnings distributions that generate those estimates. One can interpret QTE results as a way to describe these distributional shifts, so it is instructive as well to examine these distributions directly. Figure 3 shows the observed earnings distributions for each treatment-control grouping as well as the counterfactual earnings distributions if the observable characteristics in the non-flagship schools were the same as in each of the treatment sectors. As shown in Panel A, the UT-Austin distribution is above the counterfactual non-flagship distribution, and this difference grows as one moves higher up in the earnings distribution. Thus, under the assumption that our observable characteristics are sufficient to control for selection of different-skilled individuals into higher quality universities,

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Similarly, in Brewer, Eide and Ehrenberg (1999), the counterfactual is ''bottom public'' universities, which are likely to be of lower quality on average than the non-flagship public universities in Texas. This will serve to increase the estimated earnings returns.

[25] Reynolds (2009) estimates a decline in earnings of about 5% due to community college relative to four-year attendance. Kane and Rouse (1995) find that community college and four-year credits are equally valued by the labor market but that communitycollege students earn less than four-year students because they earn fewer college credits.

Panel A of Figure 3 indicates that graduating from UT-Austin shifts out the earnings distribution and that this shift is larger higher up in the distribution.

In Panel B, we show similar distributions among Texas A&M graduates and non-flagship public university graduates. As with UT-Austin, the Texas A&M earnings distribution is above the counterfactual control group distribution, but here, the outward shift is more uniform. To the extent there is change in the shape of the distribution, it is a flattening of the distribution due to larger returns at the lower end than at the upper end.

Graduating from a community college rather than a four-year non-flagship public university has a substantial effect on the shape of the earnings distribution. For nearly the entire distribution of earnings, earnings for community college graduates are below those of non-flagship public university graduates. However, the estimates steadily asymptote towards zero as we move across the earnings distribution. Graduating from a community college is particularly deleterious for the bottom of the distribution. These estimates are suggestive that the previous literature that estimates negative effects of community college attendance or graduation on earnings is driven by the lower part of the earnings distribution (Reynolds, 2009; Kane and Rouse, 1995).

In all three panels of Figure 3, the counterfactual earnings distributions are quite similar to the observed other 4-year distributions. The difference between these distributions can be interpreted as the contribution of observable differences among students in each schooling sector to the differences in earnings across sectors.[26] That the controls do not explain the majority of the observed earnings differences across school sectors is suggestive that the differences in

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[26] This decomposition interpretation assumes that the effect of each observable is the same in each school type. For example, the effect of a 1 standard deviation change in math test scores is assumed to be the same for a student who graduates from UT-Austin as for a student who graduates from a non-flagship 4-year university.

earnings across school types is not being driven by selection on unobserved components of student ability. Given the comprehensive set of covariates included in the propensity score models that provide extensive measurement of academic skill as of the end of high school, one would expect these variables to explain a large proportion of the observed earnings differentials across school types if the returns to college quality were small and the ability/skill differences were driving the observed earnings differences. Indeed, a pooled regression of log earnings on our full set of non-school-type controls for all Texas graduates in our sample yields an R^2 of 0.06. The observable characteristics in our models thus explain a non-trivial proportion of the earnings variation.[27] Unobserved factors that are correlated with both college quality and earnings, such as non-cognitive skills and student academic ability, would have to be uncorrelated with our extensive set of controls and highly correlated both with school sector choice and subsequent earnings to be significantly biasing our estimates. But, these unobservables are likely to be correlated with the background, school environment and test score measures in the propensity score models. That our observable variables explain little of the observed earnings differences across school types is suggestive that it is the college environment that is causing the shift in earnings distributions shown in Figure 3 rather than unobserved factors correlated with both college quality and earnings.

4.3 Quantile Treatment Effect Estimates

We now turn to quantile treatment effects in Figure 4 that illustrate more clearly how col- lege quality affects the distribution of earnings. In each panel, the solid line is comprised of 99 quantile treatment effects for each percentile between 1 and 99 that are the difference between the observed UT-Austin/TAMU/community college distributions and the associated

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[27] This R^2 is similar to those reported by Brewer, Eide and Ehrenberg (1999) and Dale and Krueger (2002) that *include* school quality controls.

counterfactual other 4-year earnings distributions. The dashed lines show the bounds of the 95% confidence intervals that are estimated by bootstrapping the entire estimation procedure and plotting the 2.5th and 97.5th percentiles of the bootstrapped earnings differences at each percentile. Quantile treatment effects for each 5th percentile of the distribution together with bootstrapped confidence intervals are shown in column (i) of Appendix Tables A-1 through A-3 for UT-Austin, Texas A&M and community colleges, respectively. The horizontal dotted lines are the 95% confidence intervals of the mean estimates from the "All Controls" specification shown in Table 4.

As shown in Panel A of Figure 4, the mean estimates do a poor job of characterizing the earnings premia experienced by most UT-Austin graduates: the quantile treatment effects are outside the mean confidence interval for over 70% of the distribution. The effect of UT-Austin graduation relative to non-flagship graduation is decreasing in the first decile, from 12.0% to 3.4%. However, these estimates are not statistically different from zero at the 5% level. After the 10th percentile, the premium for UT-Austin graduation increases dramatically across the earnings distribution. From the 10th percentile premium of 3.4%, it increases to 6.3% at the 25th percentile, to 12.1% at the median, to 16.8% at the 75th percentile, and are over 19% above the 90th percentile. The effects are largest at the 97th percentile, at 31.6%. Thus, UT-Austin graduation increases earnings more for those higher in the earnings distribution, suggesting that this university is particularly lucrative for top earners.

The shape of Texas A&M estimates in Panel B is much different from that of UT-Austin. For Texas A&M, the largest earnings effects are at the bottom of the distribution, with an estimate of 36.4% at the 1st percentile. While the coefficients at the lowest part of the distribution are not very precisely estimated, the lower bound of the 95% confidence interval is 25.4% at the

1st percentile and the bottom 20% of the distribution is larger than the upper confidence interval bound of the mean. The estimated effects decline until the 30th percentile, where they are 20.6%. They remain fairly stable between the 28th and 90th percentiles, ranging between 17.6 and 21.8 percent, after which they increase to 22.8% at the 99th percentile. The impact of TAMU graduation on the distribution of earnings clearly is more stable across the distribution than for UT-Austin graduation. The standard deviation of the estimates across quantiles in Panel B is 0.037, while in Panel A it is 0.066 (a 78% difference).

That the slope of the earnings premia is opposite for Texas A&M relative to UT-Austin is further evidence against biases from omitted student characteristics. For example, it might be the case that those at the top of the earnings distribution are more motivated, attend higher-quality schools, and earn more. While such a story would explain the upward sloping returns at UT-Austin, it cannot explain why the returns largely decline with earnings at Texas A&M, especially because students at the two flagship schools are similar on observables (see Table 2). Similarly, if student motivation is a substitute for college quality, it should produce the pattern of returns for TAMU but not the one observed at UT-Austin. In short, we view the different shapes of earnings premia across the two flagship schools as evidence against large biases from omitted variables, because these omitted variables are likely to be distributed similarly across the income distribution in the two schools, which would force the earnings patterns to be similar.

A question of interest then is why the Texas A&M estimates are different than those for UT-Austin. As Tables 1–3 show, the observable characteristics of these two flagship schools and the students who graduate from them are similar. However, students who graduate from these schools major in very different subjects, as shown in Figure 5. For example, over 44% of the undergraduate degrees awarded at Texas A&M are in engineering and agriculture, whereas

only 19% of the degrees awarded to undergraduates at UT-Austin are in those subjects. In contrast, 62% of the degrees at UT-Austin are in liberal arts, social sciences, communication, math/computer science and science, while only 36% of the undergraduate degrees awarded by TAMU are in these subjects.

Using the same log earnings residuals used in the quantile treatment effect estimates for all male college graduates in Texas, Table 5 shows that agriculture and engineering majors earn more on average but have a lower variance of earnings than those in the majors favored by UT-Austin graduates. Consequently, when we control for college major dummies in the weighting logits, the differences in the QTE estimates between Texas A&M and UT-Austin largely disappear. These estimates are shown in Figure 6 along with the associated mean confidence interval bands. While the Texas A&M distribution is very similar to the one shown in Figure 4, the bottom of the UT-Austin distribution has been shifted upward significantly.

Although the earnings premia at the top of the UT-Austin distribution still are higher than at Texas A&M, from an accounting standpoint, the majority of the differences across the flagship universities can be explained by differences in college majors, with UT-Austin students majoring in areas that have lower average earnings and higher earnings variance. Of course, major selection is itself endogenous, and we are hesitant to draw too strong a conclusion from Figure 6 because our controls may not be sufficient to account for the selection of students into majors across schools.[28] But, these estimates are suggestive that both quality of the school as well as major are very important in driving college quality premia. That the returns to both

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[28] Arcidiacono (2004) finds large differences in returns to different majors and argues that much of the ability sorting is due to student preferences rather than monetary returns. However, Arcidiacono, Hotz and Kang (2011) and Wiswall and Zafar (2011) both find evidence from direct surveys of students about subjective beliefs regarding returns to and preferences for majors that expected returns are a driver of student preferences for majors. These findings highlight the difficulty in interpreting the estimates in Figure 6 as causal, but they still are interesting as descriptive evidence of the importance of college majors for the distribution of earnings differentials across college sectors.

flagships are universally positive and are almost universally statistically different from zero at the 5% level suggests that all students graduating from either school can expect positive gross earnings returns to the investment. However, the size and variance of the expected premium is a function of one's major as well.

In Panel C of Figure 4, we show the effect of graduating from a community college relative to graduating from a non-flagship public university on the distribution of earnings. The estimated effect of community college graduation increases across the earnings distribution. The estimates are negative and statistically significant below the 84th percentile, ranging from -21% to -1%. From the 85th through the 91st percentiles, the returns continue to be negative, but they are small in magnitude and are not statistically differentiable from zero. The estimates for the 92nd through the 97th percentiles are positive, with the 95th and 96th percentiles effects being statistically different from zero at the 5% level. While these positive estimates are relatively small in magnitude, the results in Panel C suggest that the earnings penalty from community college versus non-flagship four-year graduation only applies to the lower part of the earnings distribution.[29] In addition, the mean estimate of -10.0% is a very imprecise estimate of the expected return for a randomly selected community college graduate. Those lower in the earnings distribution experience a much higher penalty, and those at the top experience similar earnings to non-flagship graduates. Given the large price differences between the two-year sector and the four-year sector, which the tuition estimates in Table 1 likely understate due to the higher prevalence of commuter students at community colleges, the results in Panel C of

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[29] It is unlikely that the estimates at the top of the distribution for community colleges are due to transferring. Recall that these estimates are for graduates, so any transfers to four-year schools who obtain a BA are counted as four-year graduates in our analysis. If some of the community college graduates transfer to a four-year school but do not obtain a BA, their earnings could increase. But, they still likely would earn less than comparable BA recipients, which is at odds with the equivalent earnings of non-flagship four-year and community college graduates at the top of the earnings distribution shown in Figure 4.

Figure 4 suggest that it may be optimal for a non-trivial proportion of students to attend a community college in Texas rather than a four-year non-flagship university in Texas.[30]

An important question in seeking to understand who benefits from graduating from a more elite college is whether the earnings premium varies across the socioeconomic distribution. Although we do not observe parental income for a proportion of our sample, we can observe student race and ethnicity, which is correlated with socioeconomic status. Due to the large disparities in higher education attainment for African American and Hispanic students relative to white and Asian students, the differential returns to college quality faced by these groups are of much interest. Such differences could be driven by mismatch,[31] differences in academic preparation for college, differences in major selection, and labor market discrimination. Most previous work has not been able to identify how college quality earnings premiums vary across racial and ethnic backgrounds because of data limitations. Despite the fact that the proportion of black and Hispanic students at UT-Austin and Texas A&M is low (see Table 2), we are able to present some of the first evidence of college quality returns for these groups.

4.4 Quantile Treatment Effect by Race/Ethnicity

Table 4 presents mean effects of college sector on earnings for white, black, Asian and Hispanic students. At UT-Austin, white and Asian students have the highest mean returns, at about 12.9% and 13.8%, respectively. Black and Hispanic students experience the lowest earnings premiums – 3.4% for black graduates and 3.5% for Hispanic graduates. The mean return for Hispanic students is statistically different from zero at the 5% level, while the mean

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[30] Note that controlling for majors in the community college estimates is difficult, because many community college degrees are in areas that are not offered in four-year schools. Thus, we do not estimate community college models with major controls as we do for the public flagships.

[31] Arcidiacono et al. (2011) shows evidence from Duke University that affirmative action leads to mismatch based on private information that the university has about students. Cortes (2010), however, finds little evidence that the Texas Top Ten Percent Rule led to changes in minority graduation rates and persistence due to mismatch. Rothstein and Yoo (2008) also find little evidence of affirmative action-based mismatch in law school.

for black graduates is statistically indistinguishable from zero. At Texas A&M, however, the mean returns are both higher and more similar across groups, ranging from 17.9% to 21.9%. The differences between the earnings premia for black and Hispanic students across UT-Austin and TAMU mirror those for the whole sample show in Section 4.1. Among white, black and Hispanic community college graduates, the returns are between -8.5% and -12.3%. However, for Asian students, the average earnings penalty for community college graduation relative to non-flagship public four-year graduation is -27.1%.

These mean estimates are suggestive of a large amount of heterogeneity across sector and student groups in the returns to college quality. We now present quantile treatment effects for each sector and student race/ethnic group to examine how well these mean effects describe the earnings distribution effects for these students. Estimates for each decile along with boot-strapped 95% confidence intervals are shown in Appendix Table A-4.[32]

Figure 7 shows QTE estimates for UT-Austin separately for white, black, Asian and Hispanic graduates. The distribution of returns for white students is very similar to the overall sample. For black graduates, who are shown in Panel B, the estimates are generally higher than for white graduates below the 20th percentile. Although the standard errors are large, the point estimates for the 1st to the 4th percentiles range from 44.0% to 73.7%. The returns for black UT graduates are much lower for the remainder of the distribution and generally are indistinguishable from both the mean and from zero: between the 11th and the 90th percentile, the estimates range from about -4% to 10.5%. While the returns rise for the top decile, the estimates still are

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[32] We also have generated propensity score distributions akin to Figure 2 separately by race and school type. These distributions show that there is common support between treatment and control across all school types and race and ethnic groups. These graphs are available from the authors upon request.

imprecisely estimated. These results are suggestive that the top and bottom of the earnings

distribution for black students are most impacted by UT-Austin graduation.[33]

The returns to UT-Austin are universally high for Asian graduates, ranging from a minimum

of 8.5% to a high of 42.3%. As with black students, the bottom and top of the earnings distribution

shift the most. Unlike black graduates, however, the estimated quantile treatment effects are—

for the most part—statistically different from zero. In contrast, earnings premiums for the

lowest Hispanic earners are small, often with negative coefficients that are not statistically

distinguishable from zero. The estimates then rise; they become consistently positive above the

30th percentile and become statistically significant at the 50th percentile. The estimates take on a

value of 4.5% at the median and reach a high of 33.2% percent at the 99th percentile. Especially

for white and Hispanic students, the mean estimates do a poor job of characterizing how the

earnings distribution is affected by college quality. And, for historically under-represented

minorities, while the average returns are rather low, for portions of the earnings distribution

they are quite large.

Similar to the estimates in Figure 4, the estimates across all four groups exhibit less

variability for Texas A&M than for UT-Austin, as shown in Figure 8. Again, the white distribution

is very similar to the overall distribution shown in Figure 4, which is not surprising given that

Texas A&M is over 88% white. For black graduates, the returns at the bottom of the

distribution are very high. They decline across the distribution of earnings, going below 30% at

the 16th percentile and remaining between 10.8% and 27.8% for the remainder of the

distribution. Earnings premia for Asian graduates of Texas A&M are high in the lower tail of the

the distributions as well. The maximum estimate is at the 4th percentile, with a value of

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[33] Note that the distribution of majors is remarkably similar across race and ethnic groups, such that differences in majors across groups are unlikely to account for the differences in estimated returns. Major distributions are available from the authors upon request.

approximately 50%. The estimates decline below 20% at the 12th percentile, remain between 13.2% and 19.3% for the 13th through the 89th percentiles and climb above 20% as we move into the top decile. The treatment effects for Hispanic graduates from TAMU follow a pattern similar to the returns for Asian graduates. The largest shift occurs at the 2nd percentile, at 36.7%. Earnings then drop below 20% at the 11th percentile, remain between 15.2% and 19.6% for the 12th through the 95th percentiles, and exceed 20% for the 96th through the 99th percentiles.

Figure 9 shows earnings penalties from finishing at a community college rather than finish- ing at a four-year non-flagship university stratified by race and ethnicity. We obtain negative estimates of the returns to graduating from a community college for whites from the 1st per- centile to the 93rd percentile, with the estimates from the 3rd percentile to the 78th percentile being statistically significantly different from zero. The returns rise as we move across the distributions of earnings, become positive but statistically insignificant at the 93rd percentile, and fall below zero at the 96th percentile. The distribution of returns for black community college graduates relative to their four-year non-flagship counterparts follow a similar pattern to that of the returns white community college graduates experience. Black community college graduates experience negative returns from the 1st percentile to the 91st percentile, with the estimates from the 7th percentile to the 86th percentile being statistically significant. Relative earnings rise as we move across the distributions of earnings; they are positive, rising, and statistically insignificant between the 91st and 96th percentiles and become statistically significant for the 97th percentile and above. The positive returns at the top of the distribution are large, reaching 15.5% at the 99th percentile. Thus, while most black community college graduates earn less than their counterparts at four-year non-flagship universities, for the very top of the earnings distribution this relationship is reversed. Adding in the lower time and

direct costs of community colleges, the net returns to community college graduation are likely positive for much of the upper portion of the earnings distribution for black college graduates.

The community college returns are almost universally negative for Asian students, and al- though the confidence intervals are large, the negative returns are more pronounced in the lower part of the earnings distribution. As with whites, the low mean community college returns are driven by the lowest earners. The distribution of returns for Hispanic students, however, are quite similar to those for black students. The returns are negative below the 84th percentile and then are positive and statically significantly different from zero at the 96th percentile and above. For the top decile of the distribution, the returns to community college graduation are between 2% and 6%. While typically lower than the returns for the African American sample, these results point to positive gross (and thus higher net) returns to community college enrollment for high-earning Hispanic students. The finding that mean returns are negative but are positive for the upper portion of the distribution of earnings, particularly for black and Hispanic students, highlights the importance of examining the effect of college quality on the entire distribution of earnings rather than just focusing on the mean. In particular, the lower mean effects for black and Hispanic students relative to white students shown in Table 4 mask the larger negative shift at the bottom of the distribution and the bigger positive shift at the top.

4.5 Robustness Checks

As discussed in Section 3, the validity of our estimates rests on the ability of the observable characteristics in our model to control for the selection of students with higher underlying earnings power into higher-quality schools. In this section, we assess the robustness of our estimates to several different modeling assumptions. These estimates all are shown in Figure 10. For parsimony, we have not included confidence intervals in this figure, but they are shown in Appendix Tables A-1 through A-3. First, we include high school fixed effects in

the propensity score model. Because one's high school likely is correlated with unobserved ability, motivation and non-cognitive skills, these results will lend insight into remaining selection bias in our preferred estimates. The drawback of this model is that we lose many control group observations because there are numerous high schools in Texas that send no students to Texas A&M and/or UT-Austin. For all three school types, the estimates are virtually identical to those without high school fixed effects.

While our data contain a rich set of covariates with which to control for selection, we do not include parental income and education in our baseline estimates. This omission stems from the large volume of missing data for these variables due to the fact that only students who apply to elite schools in Texas are required to supply such information. We show estimates for the sample of students that have parental income and education data but excluding these variables and then for this sample including these variables. We are unable to do this robustness check for community college students because too few students provide parental background information. However, as Figure 10 and Appendix Tables A-1 and A-2 show, the estimates for the sample of students who provide this information are virtually unchanged when parental income and education are included or excluded. Furthermore, despite the endogeneity of the reporting of these variables, the estimates for this sample are very similar to the baseline estimates. Given the strong correlation between family background, student ability, schooling and earnings, that adding parental education and income does not influence the quantile treatment effect estimates suggests that the set of controls in our baseline model, which we use for the entire sample, suggests does a good job of accounting for the influence of student academic ability.

The earnings data we use for this analysis come from earnings when graduates in our sample are in their mid 20s and early 30s. However, if college quality affects the returns to

experience, examining earnings differences for recent graduates may yield misleading estimates of the effect of college quality on long-run earnings. In order to examine whether our estimates are sensitive to the timing of when earnings are measured, Figure 10 shows quantile treatment effects using the oldest cohorts: those who graduate between 1996 and 1998 and thus who are between 27 and 31 years old in 2007-2009 when earnings are measured. These students also are unaffected by the Texas Top Ten Percent Rule, so these estimates provide a check that this admissions regime change does not seriously bias our estimates. For UT-Austin and Texas A&M, the estimates using the older sample match the estimates from the whole sample very closely above the 40th percentile of the earnings distribution. Below the 40th percentile, the older workers experience slightly lower returns, suggesting that our baseline sample understates the amount of heterogeneity in returns. For UT-Austin, the estimates are negative below the 15th percentile; however, these estimates are statistically insignificant. Aside from the estimates below the 15th percentile for UT-Austin, the quantile treatment effects of UT-Austin and Texas A&M graduation are similar for the 1996-1998 sample and for the full sample.

Among community college graduates, the differences are somewhat larger between the two samples, as shown in Panel C of Figure 10. As with the estimates for TAMU and UT-Austin, the estimates for the 1996-1998 sample are below those for the full sample at the bottom of the distribution. Between the 10th and 65th percentiles, the estimates are very similar. However, above the 65th percentile, the early cohort estimates approach zero and then become more negative at the very top, while the full sample estimates approach zero higher up in the distribution. Despite these differences, these estimates are qualitatively similar, and the differences in magnitudes of the returns at the top of the earnings distribution are small. Using the early cohort versus the full sample does not alter the main conclusions drawn from the community college results that for the upper portion of the earnings distribution the returns

to community college graduation relative to four-year non-flagship graduation are close to zero

and likely are positive once one accounts for the cost differences across school types.

We also examine the effect of college attendance rather than college graduation on

subsequent earnings. On the one hand, college attendance is a compelling margin to examine

because college sector can affect the likelihood of graduation (Bound, Lovenheim and Turner,

2010; Rouse, 1995). On the other hand, college graduation is more appropriate because this is

the more salient signal for employers and because examining only enrollment means the

intensity of treatment varies significantly across individuals based on how many credits they

receive.

Furthermore, for community college students, the graduate sample is likely to be much

more similar to BA recipients than is the attendee sample, as many community college students

who drop out have loose attachments to the postsecondary sector and do not intend to obtain a

BA. Figure 10 shows results as well when we use all college attendees and assign school types

based on the institution in which each individual earned the most credits.[34] For UT-Austin and

Texas A&M graduates, the results using attendees are similar to those using graduates. The

main differences come at the bottom of the distributions, where the returns for attendees are

around 3-5 percentage points higher. But, the main qualitative and quantitative results

remain, with the returns for UT-Austin students increasing across the income distribution and the

returns for Texas A&M students declining.[35]

For community college attendees, there is more of a divergence from the baseline

estimates. Below the 40th percentile, the returns for community college attendees lie above the

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[34] Estimates are very similar when we assign students based on the first college attended after high school.

[35] Appendix Table A-5 presents estimates by race and ethnicity for the attendee sample. The same differences apply to these samples: returns are higher at the lower end of the distribution among attendees at UT-Austin and Texas A&M. The only substantive difference comes from black UT-Austin attendees, who experience returns of over 10% below the 30th percentile.

returns for college graduates, while above the 45th percentile the returns for community college attendees are lower than the returns for two-year graduates. However, near the top of the distribution, the returns among attendees approaches zero, with returns higher than -5% above the 90th percentile of the earnings distribution. The differences in these results most likely can be attributed to the fact that completion rates at community colleges are very low.[36] Thus, graduates and non-completers are likely to differ substantially with respect to future earnings, with the inclusion of community college dropouts shifting the community college earnings distribution downward. The low completion rate at community colleges also makes the selection problem more difficult to solve among the attendee sample, as the community college dropouts are probably less similar to four-year college attendees on observable characteristics. These estimates show that for community college students there are differences in returns between graduates and attendees. While we find evidence that the returns to community college graduation may be positive for the upper part of the earnings distribution, this finding is weaker when one examines all attendees. Although the top of the attendee distribution approaches zero and the estimates become statistically indistinguishable from zero, the attendee returns approach zero higher in the distribution than in the graduate sample and never become positive. One implication of this finding is that there may be substantial returns to increasing completion rates among community college students. This issue deserves more attention in future research.

Finally, we include in our analysis all earnings for individuals who are not contemporaneously enrolled in graduate school. This robustness check relaxes the constraint that

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[36] In the National Educational Longitudinal Study of 1988 (NELS:88), only 20% of community college attendees earned an AA among those who attended within two years of high school graduation. In our sample, only 14% percent of attendees who started at a community college finished with any type of degree, be it from a community college or otherwise. The analogous completion rates for UT Austin and TAMU are 77% and 82%, respectively.

an individual must work for three to four consecutive quarters for their earnings to be included. We do not favor this method of measuring earnings because we want to measure lifetime earnings differences as best we can with our data. Including earnings of those who may work very few hours part time or who may work part time and then leave the state or the labor market may yield a misleading picture of permanent earnings differences by school type. Using all earnings reduces the returns among UT-Austin graduates at the bottom of the distribution. Thus, we are understating the amount of heterogeneity in returns by examining only full-time earnings. The Texas A&M returns are extremely similar to the baseline estimates, and for community colleges the returns are less negative at the bottom of the distribution. However, we still find that the returns become positive at the top of the distribution for community college graduates in this sample. These estimates demonstrate that our main results and conclusions are not being driven by analyzing earnings from full-time workers only.[37]

5. Conclusion

This paper estimates quantile treatment effects of college quality on earnings using administrative data on schooling and earnings from the state of Texas. We measure quality using public college sector in Texas, examining the effects of UT-Austin, Texas A&M and community college graduation on the distribution of earnings relative to earnings for non-flagship public four-year university graduates. While our mean estimates are consistent with previous work in this area, our quantile estimates demonstrate a large amount of heterogeneity in the earnings premium from college quality. At UT-Austin, the premia are roughly increasing with earnings, while the opposite pattern is exhibited among Texas A&M graduates. We argue that differences in the courses of study across these schools is a potential explanation for this difference, but

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[37] We have generated estimates using all non-graduate school earnings for the attendee sample as well, and those estimates are similarly robust to the use of this alternative earnings measure. These results are available upon request from the authors.

these re- sults indicate that much work remains in understanding how the characteristics of a particular university map into the distribution of earnings for graduates. At community colleges, we find an overall negative effect on earnings but show that there is significant heterogeneity in the returns and uncover evidence of positive returns at the very top of the distribution.

Our data also allow us to examine returns separately by race and ethnicity, which previous work has not been able to do because of data limitations. Particularly for black and Hispanic students, who are historically under-represented in higher education in general and at high- quality universities in particular, earnings premia are low for UT-Austin graduates except at the very top of the earnings distribution, but they are consistently high among Texas A&M graduates. The returns to community college graduation is negative on average; however, for black and Hispanic community college graduates we find large and positive returns relative to non-flagship public graduates of the same race and ethnicity at the top of the earnings distribution.

In drawing attention to the large amount of heterogeneity in college quality earnings premia and the differences in the quantile treatment effects across school types, this paper demon- strates the importance of considering more than the average treatment effect of college quality on earnings. Even if educational choices made by students are based on such averages, our estimates suggest that these averages mask significant uncertainty of the returns for any given student. The main policy implications of this work are twofold. First, policies seeking to induce students to attend four-year universities and more selective colleges should pay attention to the distribution of returns, not simply the average, in order to target students who will benefit most from changing their attendance behavior. Second, the large public subsidies to higher education do not benefit all student equally, even within school quality tiers. Future work that focus on identifying which students face the largest predicted returns to

attending different school types is needed help guide how to best fund public universities to support human capital development as well as to develop policies that facilitate the efficient sorting of students into different schools.

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Table 1: Means of Texas Public College Resource and Quality Measures by Higher Education Sector

	UT Austin	Texas A&M College Station	Other Public Four-year	Community College
25th Percentile Math SAT	535	520	440	
75th Percentile Math SAT	650	630	549	
Faculty-Student Ratio	0.045	0.041	0.034	0.023
Expenditures Per Student	25081	27449	10981	5756
Instructional Expenditures Per Student	6900	8931	3648	2317
Graduation Rate	0.710	0.750	0.338	
In-state Tuition	3212	3187	2001	1217

[1] Source: 1997-2003 IPEDS data. All monetary figures are in real $2007 and are weighted by total undergraduate enrollment. All per-student means are per total enrollment. Graduation rates are for BA degrees within six years of initial enrollment.

[2] SAT scores and graduation rates are reported for a small percentage of two-year schools. Because of the open-admission mandate of community colleges and the fact that many students do not intend to obtain a BA, we do not report means for SAT scores and graduation rates for these schools.

Table 2: Summary Statistics of Analysis Variables

	UT Austin (i)	Weighted Non-Flagship (ii)	TAMU College Station (iii)	Weighted Non-Flagship (iv)	Community College (v)	Weighted Non-Flagship (vi)	Un-Weighted Non-Flagship (vii)
Log Quarterly Earnings	0.167	0.045	0.248	0.032	-0.168	-0.075	-0.023
	(0.657)	(0.595)	(0.546)	(0.574)	(0.603)	(0.564)	(0.564)
TAAS Math Score	56.084	56.015	52.429	55.415	48.505	49.039	52.831
	(5.836)	(6.096)	(5.712)	(5.734)	(11.283)	(10.293)	(6.680)
TAAS Reading Score	45.022	44.964	44.623	44.615	39.670	40.138	43.031
	(4.304)	(4.550)	(4.381)	(4.413)	(8.950)	(8.075)	(4.978)
TAAS Writing Score	37.046	37.014	36.393	36.383	32.138	32.518	34.999
	(3.834)	(3.974)	(3.978)	(3.984)	(7.630)	(6.965)	(4.499)
Age at Graduation	21.829	22.242	22.098	22.286	21.304	22.632	22.438
	(0.934)	(1.036)	(0.860)	(1.013)	(1.743)	(1.008)	(1.0083)
Tabulations (Percentage):							
Math Rank							
Top 10 Percentile	51.22	51.31	45.31	45.24	15.34	15.68	27.15
70th-90th Percentile	31.33	31.28	33.40	33.37	27.24	27.83	32.93
Below 70th Percentile	17.44	17.41	21.29	21.39	57.41	56.49	39.91
Reading Rank							
Top 10 Percentile	43.40	43.94	39.21	39.18	13.81	14.24	25.16
70th-90th Percentile	31.86	31.49	33.63	33.67	25.42	25.95	31.01
Below 70th Percentile	24.74	24.57	27.16	27.15	60.77	59.81	43.83
Writing Rank							
Top 10 Percentile	40.55	41.09	34.18	34.14	12.56	13.03	21.91
70th-90th Percentile	32.78	32.52	32.88	32.83	23.25	23.99	29.72
Below 70th Percentile	26.66	26.39	32.93	33.04	64.20	62.98	48.37
Ethnic							
White	68.82	67.65	88.59	88.62	60.68	61.15	67.03
Hispanic	12.36	12.49	7.14	7.09	29.56	29.29	19.45
Afr. American	2.56	2.60	1.50	1.49	6.40	7.00	8.33
Asian	16.01	16.98	2.58	2.69	2.02	2.04	4.99
Gifted (Yes)	42.39	43.36	32.84	32.99	7.90	8.21	18.55
At Risk (Yes)	4.74	4.96	5.03	5.12	32.28	31.73	14.89
Not Disadvantaged	93.77	93.53	87.08	96.69	77.05	77.10	87.08
Predicted Log Earnings:							
Mean	0.167	0.167	0.248	0.248	-0.168	-0.168	
10th Percentile	-0.025	-0.019	0.127	0.127	-0.356	-0.352	
25th Percentile	0.080	0.082	0.192	0.193	-0.265	-0.264	
50th Percentile	0.184	0.182	0.257	0.257	-0.143	-0.143	
75th Percentile	0.269	0.266	0.315	0.314	-0.065	-0.067	
90th Percentile	0.334	0.329	0.361	0.360	-0.012	-0.015	
95th Percentile	0.367	0.367	0.386	0.385	0.016	0.012	
Observations	9,837	47,935	13,436	47,935	22,863	47,935	47,935

Source: Authors' tabulations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Standard deviations are in parentheses. 48

Table 3: Summary Statistics of Analysis Variables for Included and Excluded Observations Among Texas College Graduates

	UT Austin		TAMU College Station		Other Non-Flagship		Community College	
	Excluded	Included	Excluded	Included	Excluded	Included	Excluded	Included
TAAS Math Score	56.19	56.08	55.64	55.43	53.13	52.83	48.59	48.51
TAAS Reading Score	45.17	45.02	44.90	44.62	43.28	43.03	39.66	39.67
TAAS Writing Score	37.18	37.05	36.64	36.39	35.20	35.00	32.08	32.14
Math Rank								
Top 10 Percent	51.00	51.22	46.82	45.31	29.30	27.15	17.19	15.34
70th-90th Percentile	31.42	31.33	34.15	33.40	33.37	32.93	27.29	27.24
Below 70th Percentile	17.58	17.44	19.04	21.29	37.33	39.91	55.52	57.41
Reading Rank								
Top 10 Percent	45.96	43.40	43.37	39.21	28.54	25.16	16.32	13.81
70th-90th Percentile	30.99	31.86	33.18	33.63	31.47	31.01	24.55	25.42
Below 70th Percentile	23.04	24.74	23.45	27.16	39.99	43.83	59.14	60.77
Writing Rank								
Top 10 Percent	42.28	40.55	37.11	34.18	24.33	21.91	13.33	12.56
70th-90th Percentile	32.07	32.78	32.15	32.88	29.30	29.72	23.24	23.25
Below 70th Percentile	25.65	26.66	30.74	32.93	46.36	48.37	63.43	64.20
Ethnic								
White	68.36	68.82	87.98	88.59	67.34	67.03	59.43	60.68
Hispanic	9.90	12.36	6.72	7.14	18.55	19.46	28.60	29.56
Afr. American	1.98	2.56	1.60	1.50	6.91	8.33	7.46	6.40
Asian	19.51	16.01	3.47	2.58	6.97	4.99	3.25	2.02
Gifted (Yes)	43.46	42.39	36.65	32.84	1.14	18.55	8.61	7.90
At Risk (Yes)	4.45	4.74	4.35	5.03	14.11	14.89	32.36	32.28
Economic Status (Not Disadvantaged)	95.75	93.77	96.97	96.69	87.60	87.08	75.93	77.05
Predicted Log Earnings	0.181	0.167	0.255	0.248	-0.018	-0.023	-0.124	-0.168
Observations	5598	9837	4759	13436	14511	47935	5733	22863
Observations Included (Percentage)		63.73		73.84		76.76		79.98

Observations are excluded if they never have 3-4 consecutive quarters of earnings in Texas when not enrolled in graduate school. This could occur because one does not work, because one leaves the state, or because one is enrolled in graduate school during all periods of observation.

Table 4: OLS Estimates of the Effect of College Sector on Earnings

	UT Austin	Texas A&M College Station	Community College
No Controls:			
Full Sample	0.190**	0.272**	-0.145**
	(0.006)	(0.005)	(0.005)
Test Score Controls:			
Full Sample	0.141**	0.237**	-0.106**
	(0.007)	(0.006)	(0.005)
Test Score, Demographic, School Experience Controls:			
Full Sample	0.128**	0.217**	-0.099**
	(0.007)	(0.006)	(0.005)
All Controls:			
Full Sample	0.115**	0.212**	-0.100**
	(0.007)	(0.006)	(0.005)
All Controls, By Race and Ethnicity:			
White Sample	0.129**	0.219**	-0.085**
	(0.008)	(0.006)	(0.006)
Black Sample	0.034	0.206**	-0.107**
	(0.040)	(0.043)	(0.019)
Asian Sample	0.138**	0.179**	-0.271**
	(0.022)	(0.034)	(0.034)
Hispanic Sample	0.035**	0.180**	-0.123**
	(0.018)	(0.019)	(0.010)

[1] Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each cell represents a separate regression, and each coefficient shows the mean adjusted earnings difference between the students graduating from each school type and the students graduating from a non-flagship public university.

[2] Test score controls are quartics in the state math, reading and writing tests as well as each student's relative rank in their high school on each exam. Demographic and school experience controls include student ethnicity/race, Title I status, English proficiency, free and reduced price lunch status, enrollment in gifted programs, special education, career and technology courses, whether the student had a college plan, and whether he was at risk of dropping out. All controls add high school variables in the year of graduation: the ethnic composition of the high school, the percentage of students in each economic status group, the percentage of gifted students and students at risk, the percentage of title I eligible students, and total school enrollment.

[3] Robust standard errors are in parentheses: ** indicates statistical significance at the 5% level and * indicates statistical significance at the 10% level.

Table 5: Average Residual Log Earnings by Major from Male College Graduates in Texas

Major	Mean	Std. Deviation
Agriculture	0.008	0.514
Liberal Arts	-0.293	0.642
Social Science	-0.105	0.622
Communication	-0.242	0.632
Math/Computer Science	0.091	0.569
Science	-0.020	0.650
Business	0.112	0.545
Engineering	0.234	0.529

The table shows the mean and standard deviation of residualized log earnings among male college graduates from all schools in Texas. The log earnings residuals are the same as those used to generate the quantile treatment effects.

Figure 1: Distribution of Log Earnings in the 2000 Census Among BA Recipients of College Age 5 Years Prior by Location of Residence 5 Years Prior and by State of Current Residence
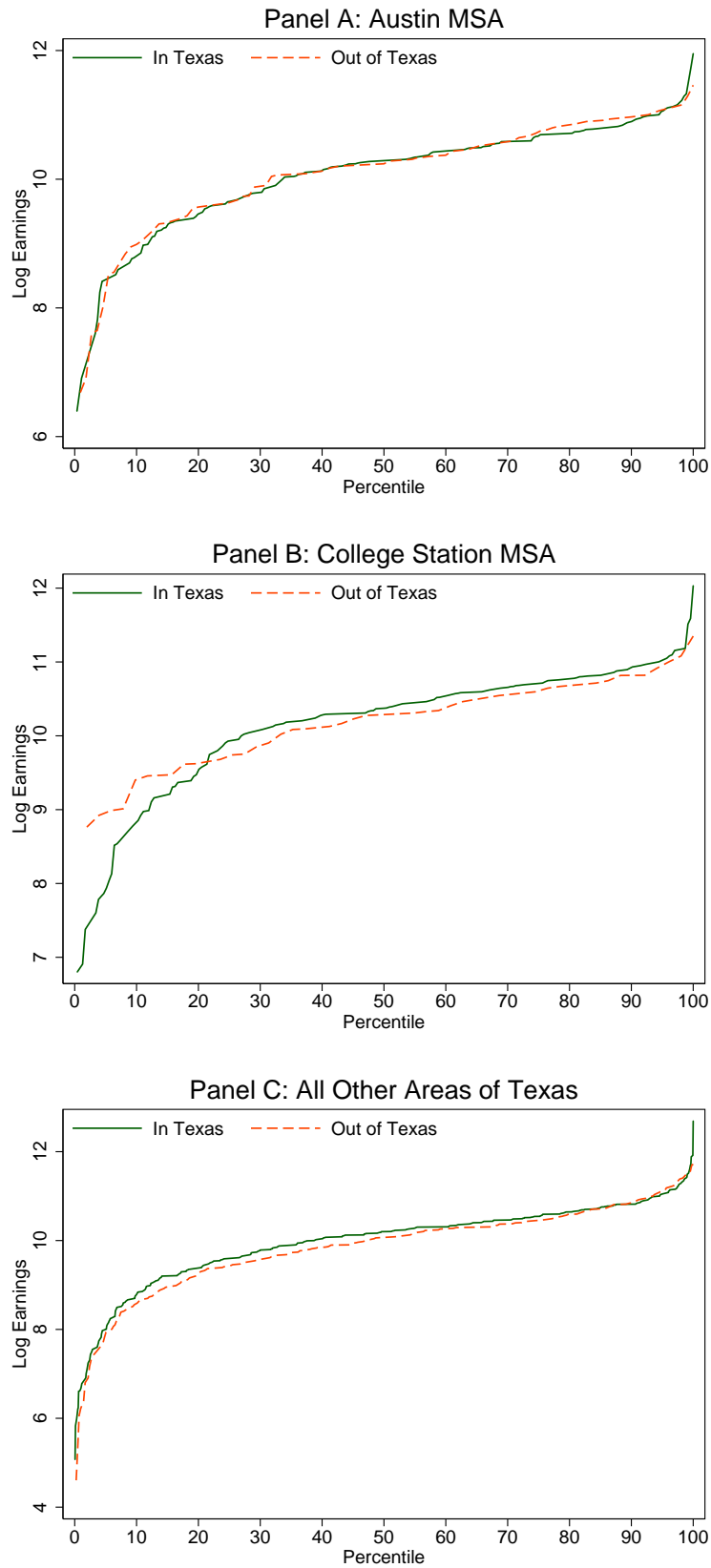
Each panel shows log earnings distributions for respondents 23-26 years old with a BA in the 2000 Census who reported living in the Austin MSA (Panel A), the College Station MSA (Panel B) or in any other part of Texas (Panel C) in 1995. Among those who reported living in each area in 1995, the figures show log earnings distributions by whether the respondent lives in Texas in 2000 or not.

Figure 2: Distribution of Propensity Scores Among Treated and Control Observations by School Treatment Type
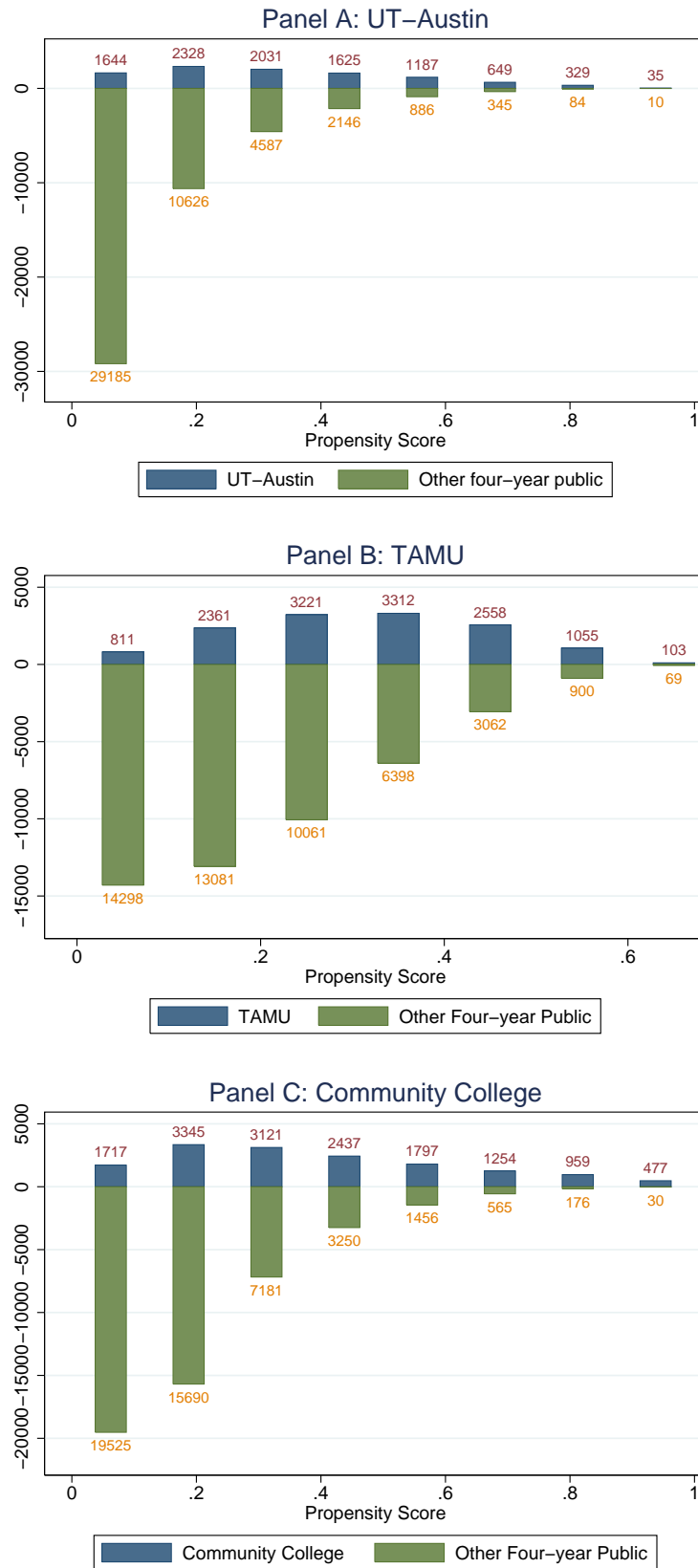
Each panel shows propensity score distributions from estimation of equation (8) in the text for each treatment school type and non-flagship universities. Each set of bars show the estimated probability of graduating from the treatment school relative to non-flagship schools, separately by the type of school from which the individual actually graduated. The range of the bars in each panel are constrained by the confidentiality agreement we signed for data use that restricts any reported results to be based on 10 or more observations.

Figure 3: Observed and Counterfactual Earnings Distributions, by School Type
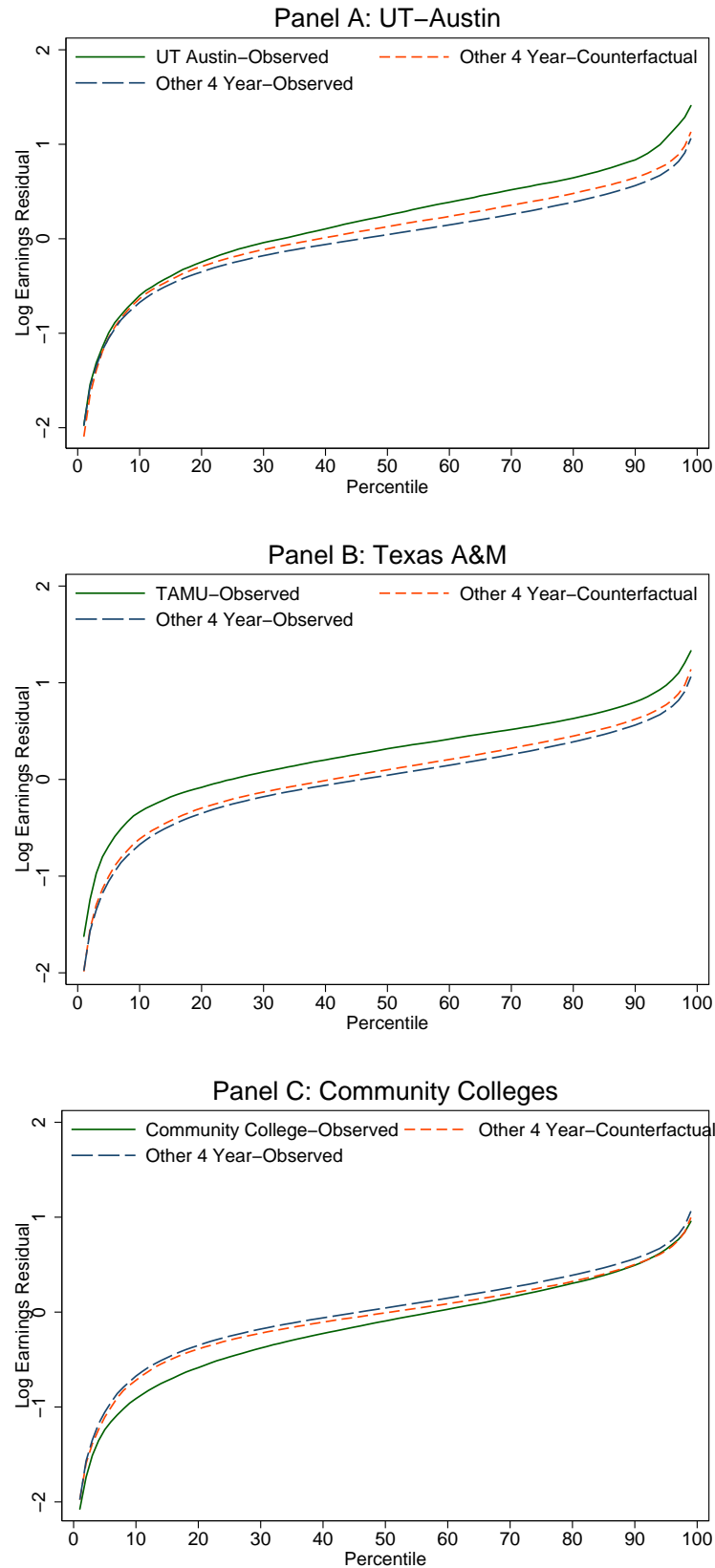
Panel A: UT–Austin

Legend: UT Austin–Observed; Other 4 Year–Counterfactual; Other 4 Year–Observed

Panel B: Texas A&M

Legend: TAMU–Observed; Other 4 Year–Counterfactual; Other 4 Year–Observed

Panel C: Community Colleges

Legend: Community College–Observed; Other 4 Year–Counterfactual; Other 4 Year–Observed

Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each earnings distribution consists of 99 percentile cut-points from the empirical cumulative distribution. The other 4 year counterfactual distribution is the re-weighted other 4 year distribution, where the weights are estimated from equation (8) in the text.

Figure 4: Quantile Treatment Effects of College Sector on Earnings
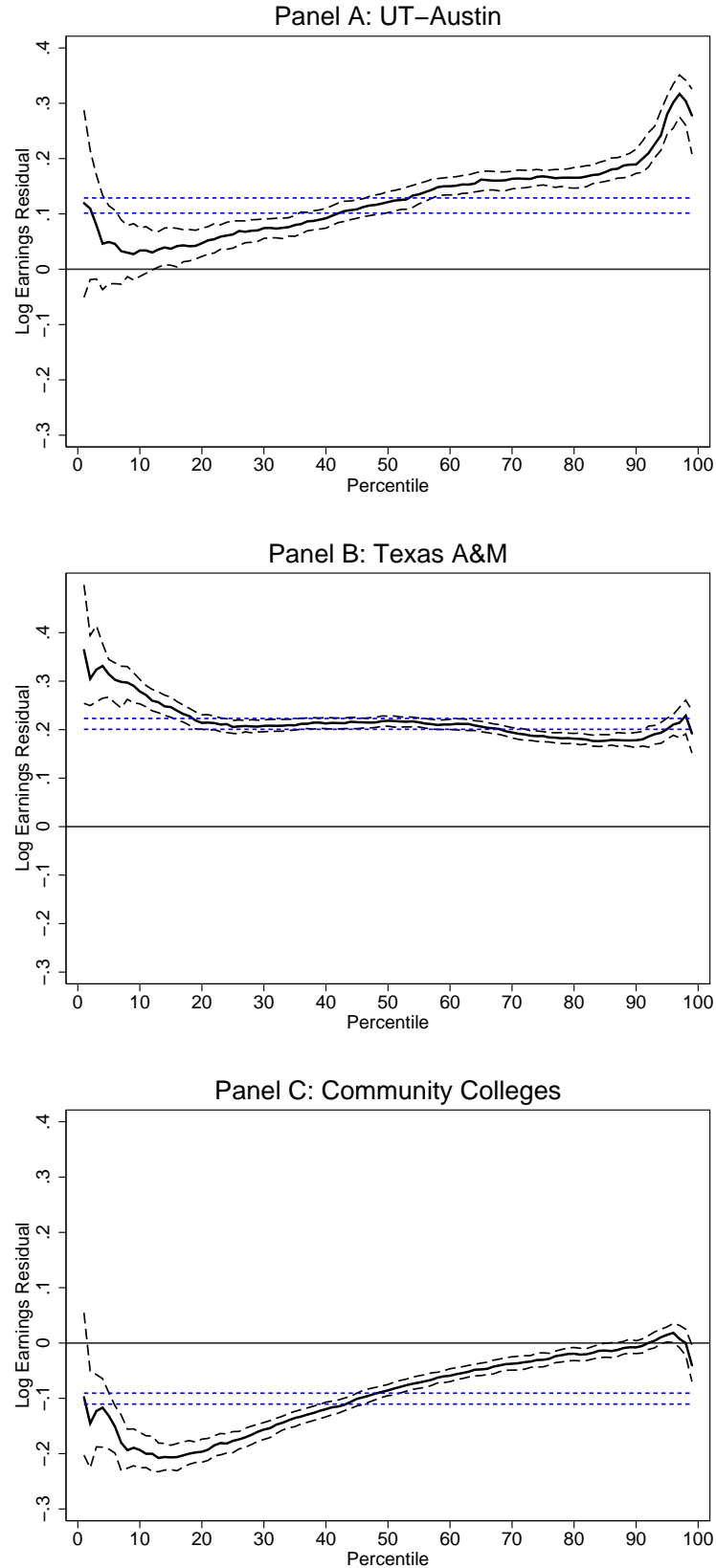
Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for UT-Austin (Panel A), Texas A&M (Panel B) and community colleges (Panel C) and the associated earnings at that percentile from the counterfactual earnings distribution. The dotted lines show the bounds of the 95% confidence intervals for each percentile point. The horizontal dashed lines show the 95% confidence interval of the mean effect from the "All Controls" specification in Table 4.

Figure 5: Distribution of Majors at UT-Austin and Texas A&M
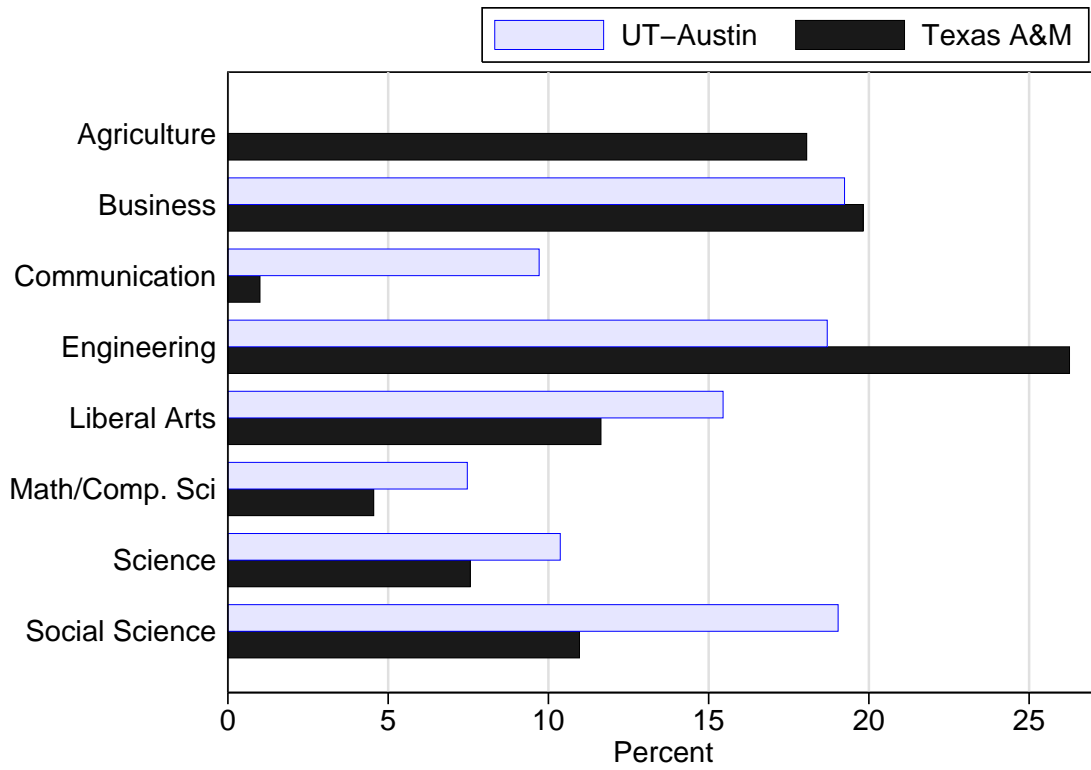
Source: Authors' calculations from the University of Texas at Dallas Education Research Center data.

Figure 6: Quantile Treatment Effects of Four-year College Sector on Earnings, Including College Major Controls
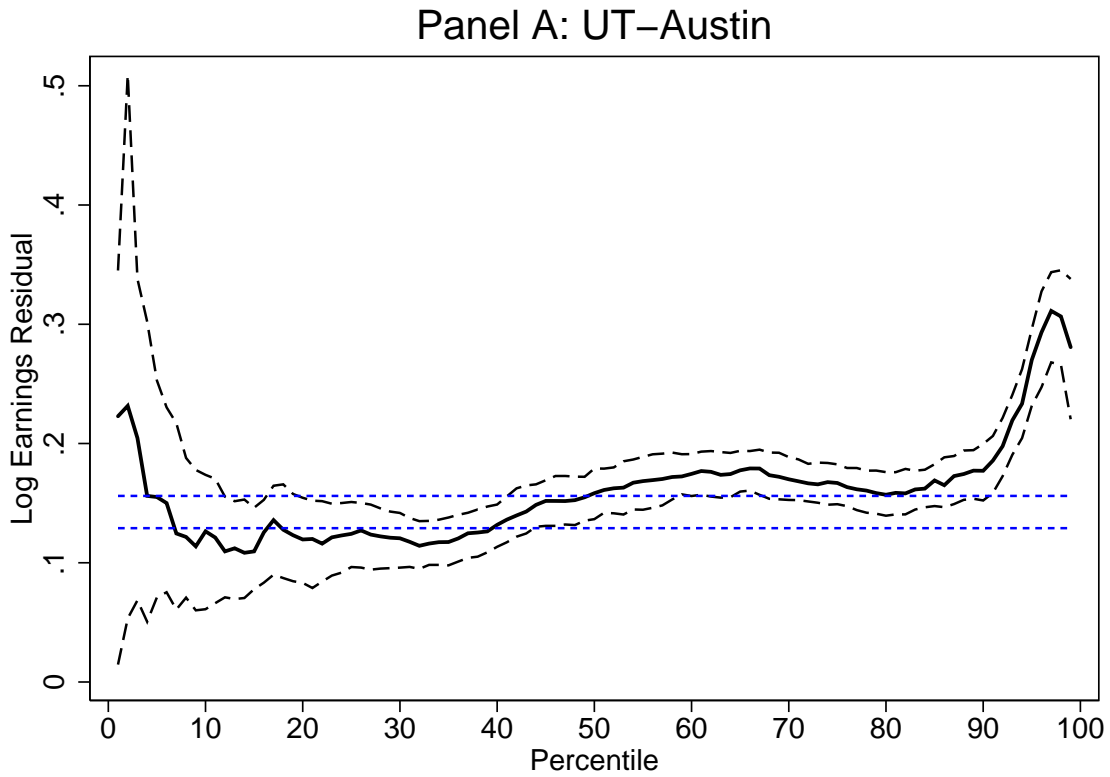
Panel A: UT–Austin

Panel B: Texas A&M

Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for UT-Austin (Panel A) and Texas A&M (Panel B) and the associated earnings at that percentile from the counterfactual earnings distribution. The dotted lines show the bounds of the 95% confidence intervals for each percentile point. The horizontal dashed lines show the 95% confidence interval of the mean effect, calculated using all observables as well as college major controls.

Figure 7: Quantile Treatment Effects of Graduating from UT-Austin on Earnings by Race
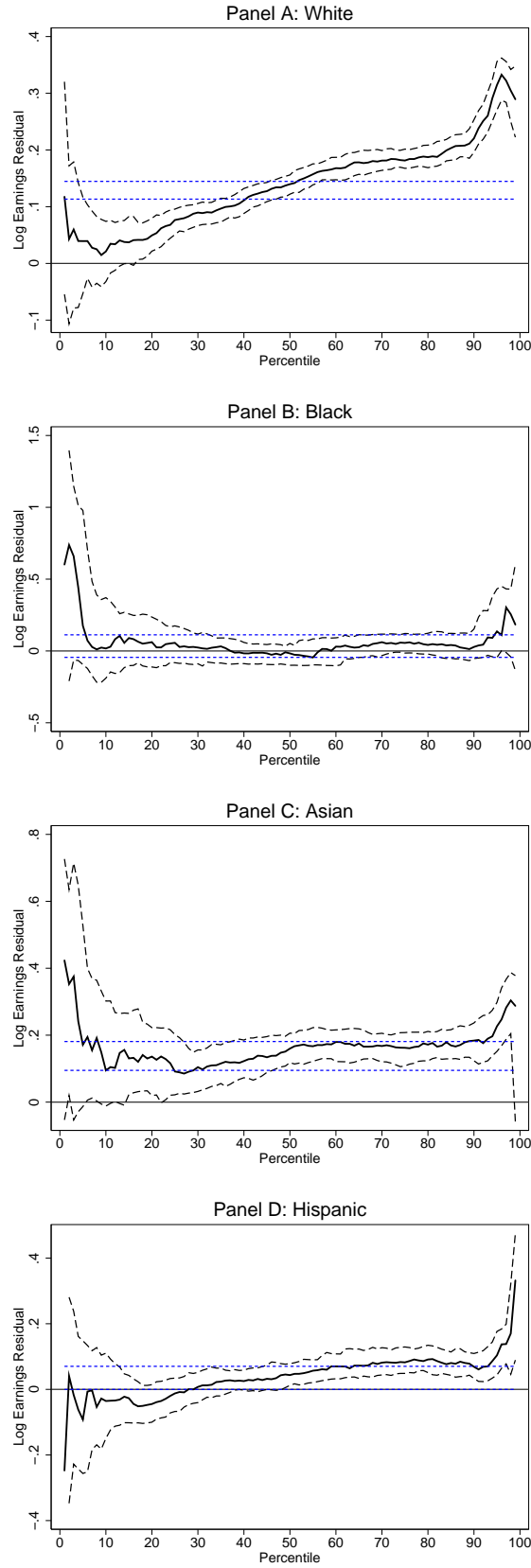
Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for UT-Austin and the associated earnings at that percentile from the counterfactual earnings distribution. The dotted lines show the bounds of the 95% confidence intervals for each percentile point. The horizontal dashed lines show the 95% confidence interval of the mean effect from Table 4.

Figure 8: Quantile Treatment Effects of Graduating from Texas A&M - College Station on Earnings by Race
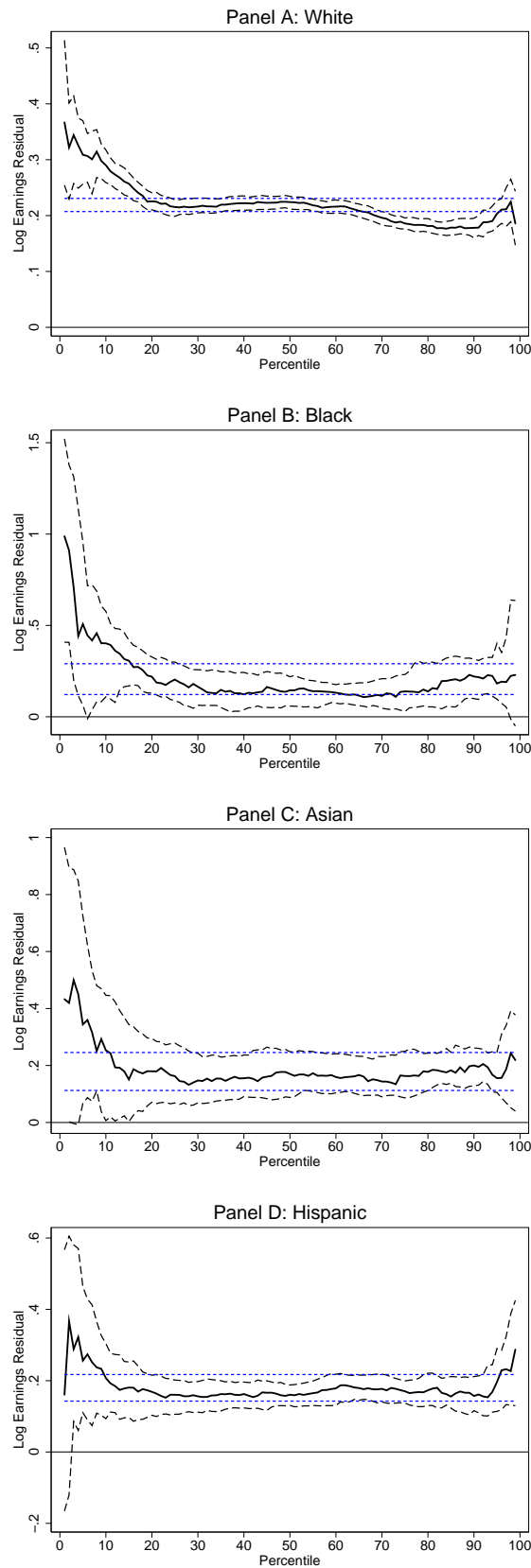
Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for Texas A&M and the associated earnings at that percentile from the counterfactual earnings distribution. The dotted lines show the bounds of the 95% confidence intervals for each percentile point. The horizontal dashed lines show the 95% confidence interval of the mean effect from Table 4.

Figure 9: Quantile Treatment Effects of Graduating from a Community College on Earnings by Race
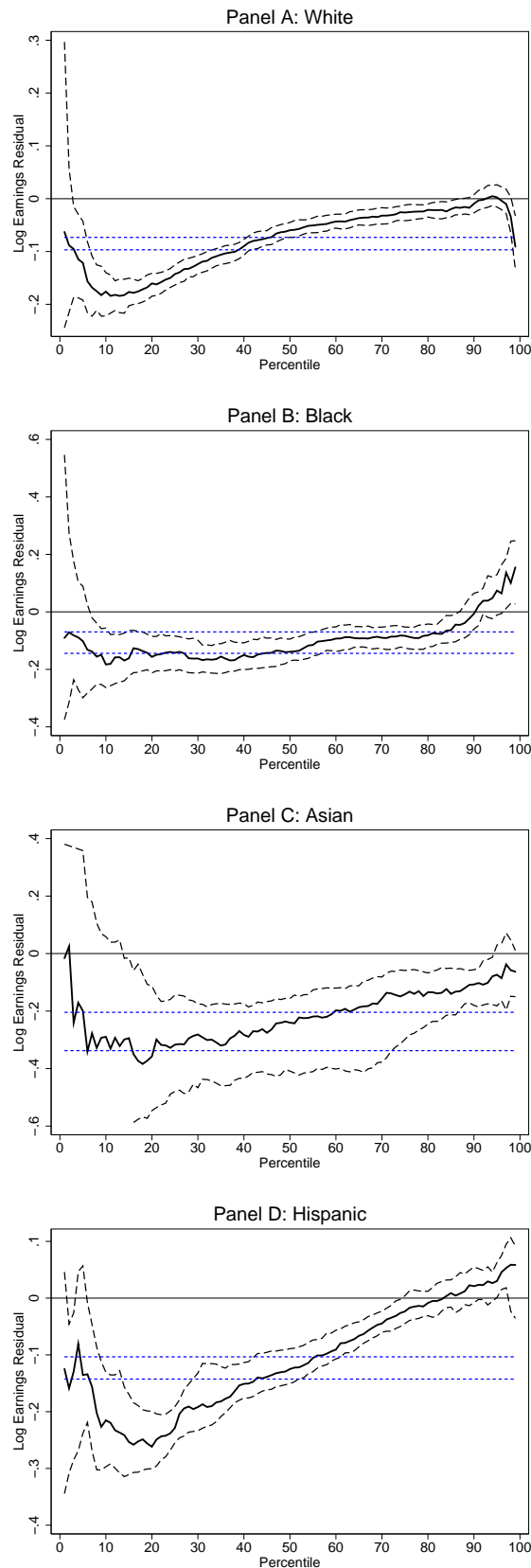
Panel A: White

Panel B: Black

Panel C: Asian

Panel D: Hispanic

Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for community colleges and the associated earnings at that percentile from the counterfactual earnings distribution. The dotted lines show the bounds of the 95% confidence intervals for each percentile point. The horizontal dashed lines show the 95% confidence interval of the mean effect from Table 4.

Figure 10: Robustness Checks
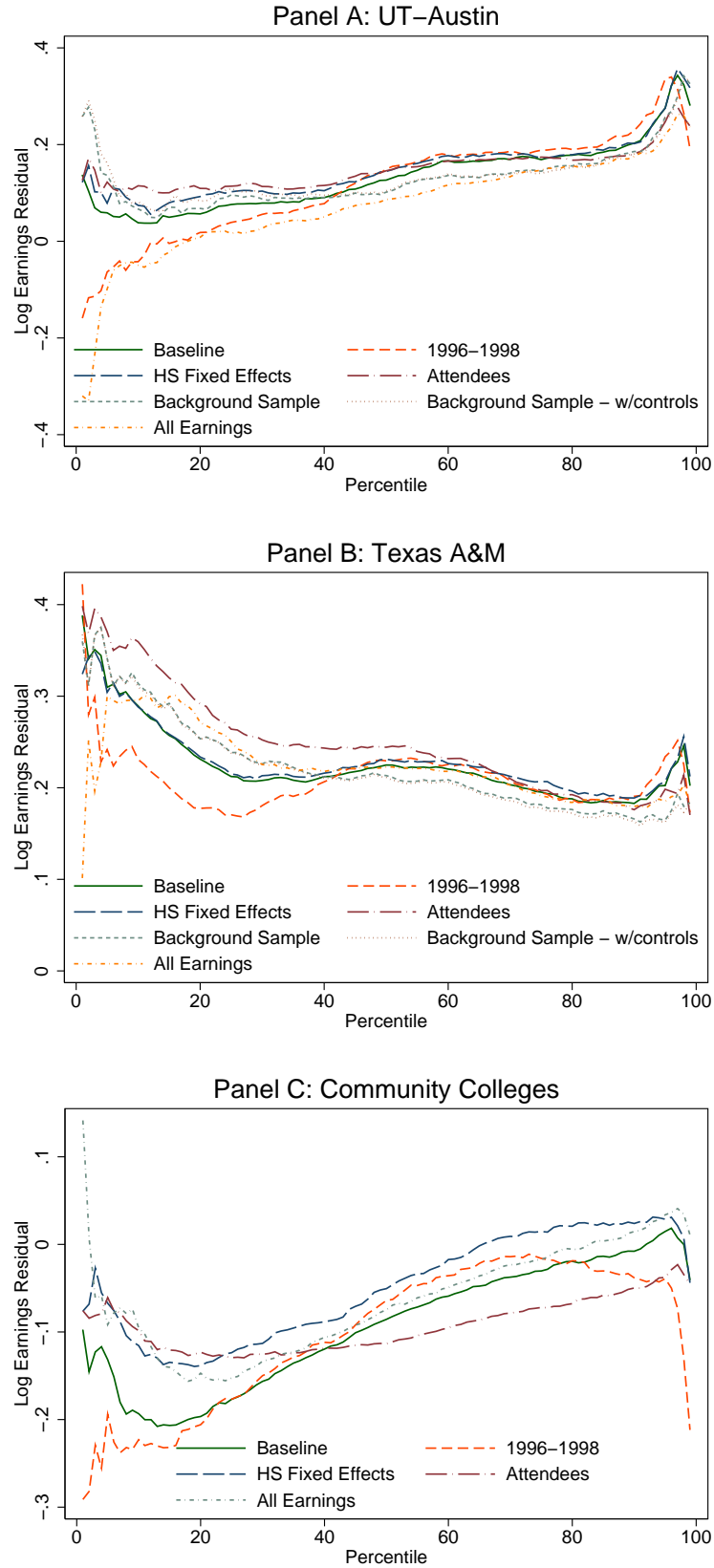
Panel A: UT–Austin

Legend:
Baseline — HS Fixed Effects — Background Sample — All Earnings — 1996–1998 — Attendees — Background Sample – w/controls

Panel B: Texas A&M

Legend:
Baseline — HS Fixed Effects — Background Sample — All Earnings — 1996–1998 — Attendees — Background Sample – w/controls

Panel C: Community Colleges

Legend:
Baseline — HS Fixed Effects — All Earnings — 1996–1998 — Attendees
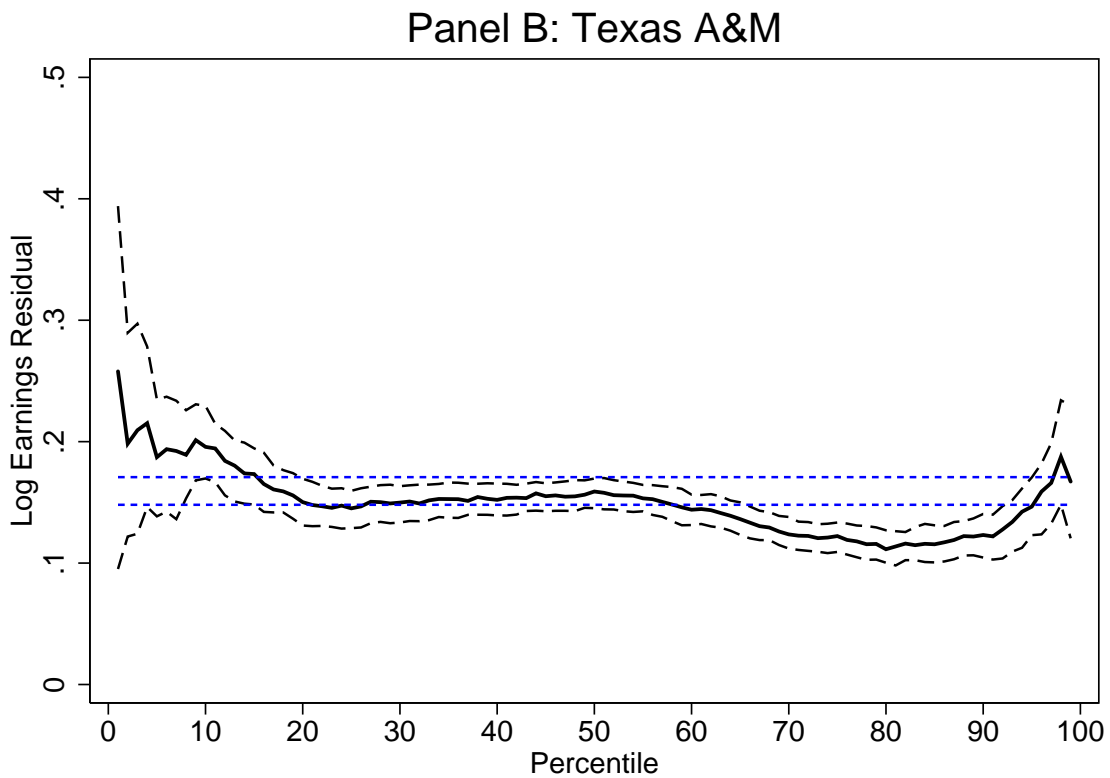
Source: Authors' calculations from the University of Texas at Dallas Education Research Center data and administrative earnings records as described in the text. Each estimated point is the difference between the observed earnings at each percentile for UT-Austin (Panel A), Texas A&M (Panel B) and community colleges (Panel C) and the associated earnings at that percentile from the counterfactual earnings distribution. Estimates and 95% confidence intervals are shown in Appendix Tables A-1 through A-3. The "Background Sample" is the sample that contains parental income and education information. The "Background Sample – w/ controls" is a specification that uses this sample and includes these parental controls.

Online Appendix

Not for Publication

Table A-1: Quantile Treatment Effects of College Sector on Earnings - UT Austin

Quantile	Baseline (i)	1996-1998 Cohort (ii)	HS Fixed Effects (iii)	Family Background Sample — Without Background Variables (iv)	Family Background Sample — With Background Variables (v)	College Attendees (vi)	All Earnings (vii)
1	0.120	-0.159	0.117	0.258	0.258	0.126	-0.320
	[-0.051, 0.288]	[-0.349, 0.136]	[-0.036, 0.251]	[-0.070, 0.476]	[-0.051, 0.480]	[-0.031, 0.310]	[-0.583, -0.137]
5	0.049	-0.063	0.075	0.136	0.162	0.122	-0.099
	[-0.026, 0.115]	[-0.184, 0.039]	[-0.019, 0.193]	[-0.002, 0.294]	[0.015, 0.325]	[0.052, 0.193]	[-0.213, -0.006]
10	0.034	-0.042	0.073	0.067	0.081	0.115	-0.050
	[-0.013, 0.075]	[-0.099, 0.008]	[0.017, 0.137]	[-0.026, 0.158]	[-0.016, 0.172]	[0.070, 0.162]	[-0.112, 0.013]
15	0.037	-0.004	0.077	0.071	0.083	0.100	-0.023
	[0.007, 0.075]	[-0.040, 0.041]	[0.039, 0.115]	[0.014, 0.125]	[0.019, 0.133]	[0.073, 0.139]	[-0.083, 0.029]
20	0.047	0.018	0.085	0.068	0.083	0.109	0.008
	[0.023, 0.074]	[-0.018, 0.051]	[0.054, 0.119]	[0.018, 0.118]	[0.036, 0.135]	[0.086, 0.142]	[-0.028, 0.048]
25	0.063	0.039	0.099	0.096	0.109	0.114	0.016
	[0.038, 0.087]	[0.006, 0.066]	[0.063, 0.126]	[0.050, 0.130]	[0.060, 0.145]	[0.092, 0.140]	[-0.020, 0.052]
30	0.074	0.056	0.100	0.087	0.097	0.113	0.027
	[0.056, 0.091]	[0.031, 0.074]	[0.071, 0.121]	[0.063, 0.116]	[0.067, 0.130]	[0.096, 0.136]	[-0.009, 0.059]
35	0.080	0.063	0.097	0.088	0.097	0.108	0.044
	[0.060, 0.097]	[0.037, 0.084]	[0.074, 0.117]	[0.063, 0.117]	[0.064, 0.123]	[0.089, 0.127]	[0.012, 0.064]
40	0.092	0.078	0.107	0.090	0.095	0.115	0.050
	[0.074, 0.110]	[0.057, 0.110]	[0.085, 0.127]	[0.060, 0.119]	[0.067, 0.124]	[0.096, 0.139]	[0.023, 0.071]
45	0.108	0.120	0.126	0.100	0.102	0.135	0.074
	[0.092, 0.125]	[0.090, 0.151]	[0.105, 0.145]	[0.072, 0.122]	[0.072, 0.125]	[0.117, 0.156]	[0.050, 0.092]
50	0.121	0.149	0.144	0.102	0.105	0.146	0.087
	[0.102, 0.141]	[0.115, 0.175]	[0.121, 0.163]	[0.074, 0.131]	[0.075, 0.136]	[0.127, 0.163]	[0.066, 0.103]
55	0.135	0.164	0.157	0.125	0.127	0.154	0.096
	[0.118, 0.155]	[0.135, 0.189]	[0.135, 0.173]	[0.096, 0.151]	[0.100, 0.154]	[0.139, 0.174]	[0.078, 0.115]
60	0.150	0.178	0.170	0.136	0.139	0.166	0.116
	[0.134, 0.167]	[0.154, 0.200]	[0.147, 0.191]	[0.109, 0.162]	[0.111, 0.165]	[0.152, 0.181]	[0.095, 0.135]
65	0.162	0.179	0.178	0.132	0.132	0.169	0.123
	[0.142, 0.177]	[0.157, 0.209]	[0.154, 0.197]	[0.111, 0.159]	[0.112, 0.161]	[0.152, 0.185]	[0.105, 0.138]
70	0.164	0.186	0.177	0.141	0.139	0.170	0.135
	[0.145, 0.177]	[0.155, 0.215]	[0.154, 0.195]	[0.122, 0.162]	[0.115, 0.165]	[0.156, 0.187]	[0.114, 0.154]
75	0.168	0.188	0.171	0.145	0.141	0.174	0.147
	[0.152, 0.179]	[0.163, 0.213]	[0.148, 0.193]	[0.128, 0.168]	[0.122, 0.162]	[0.160, 0.190]	[0.128, 0.167]
80	0.165	0.190	0.175	0.156	0.153	0.168	0.156
	[0.147, 0.184]	[0.162, 0.217]	[0.149, 0.198]	[0.140, 0.178]	[0.134, 0.174]	[0.154, 0.186]	[0.135, 0.172]
85	0.176	0.201	0.186	0.168	0.166	0.173	0.161
	[0.159, 0.196]	[0.167, 0.229]	[0.166, 0.202]	[0.148, 0.189]	[0.142, 0.186]	[0.157, 0.191]	[0.140, 0.177]
90	0.189	0.229	0.194	0.186	0.181	0.181	0.175
	[0.173, 0.217]	[0.187, 0.268]	[0.162, 0.226]	[0.155, 0.219]	[0.150, 0.217]	[0.161, 0.204]	[0.154, 0.197]
95	0.280	0.336	0.278	0.257	0.255	0.247	0.221
	[0.244, 0.313]	[0.273, 0.386]	[0.236, 0.314]	[0.197, 0.304]	[0.185, 0.299]	[0.210, 0.283]	[0.182, 0.247]
99	0.278	0.191	0.302	0.326	0.337	0.238	0.236
	[0.208, 0.326]	[0.058, 0.332]	[0.258, 0.357]	[0.269, 0.409]	[0.270, 0.415]	[0.135, 0.287]	[0.173, 0.288]

The table shows quantile treatment effect estimates with the bounds of bootstrapped 95% confidence intervals in brackets. All models control for quartics in the state math, reading and writing tests as well as each student's relative rank in their high school on each exam, student ethnicity/race, Title I status, English proficiency, free and reduced price lunch status, enrollment in gifted programs, special education, career and technology courses, whether the student had a college plan, and whether he was at risk of dropping out. The estimates also control for high school characteristics in the year of graduation: the ethnic composition of the high school, the percentage of students in each economic status group, the percentage of gifted students and students at risk, the percentage of title I eligible students, and total school enrollment.

Table A-2: Quantile Treatment Effects of College Sector on Earnings - Texas A&M

Quantile	Baseline (i)	1996-1998 Cohort (ii)	HS Fixed Effects (iii)	Family Background Sample Without Background Variables (iv)	With Background Variables (v)	College Attendees (vi)	All Earnings (vii)
1	0.364	0.422	0.315	0.360	0.367	0.398	0.101
	[0.254, 0.499]	[0.234, 0.642]	[0.214, 0.452]	[0.143, 0.505]	[0.154, 0.515]	[0.299, 0.516]	[-0.120, 0.346]
5	0.314	0.242	0.300	0.341	0.341	0.370	0.300
	[0.267, 0.345]	[0.185, 0.306]	[0.250, 0.368]	[0.262, 0.411]	[0.262, 0.415]	[0.319, 0.413]	[0.210, 0.382]
10	0.279	0.231	0.285	0.314	0.310	0.359	0.295
	[0.253, 0.304]	[0.203, 0.265]	[0.253, 0.325]	[0.256, 0.363]	[0.251, 0.357]	[0.328, 0.394]	[0.240, 0.351]
15	0.247	0.200	0.254	0.292	0.291	0.320	0.300
	[0.226, 0.267]	[0.172, 0.225]	[0.229, 0.277]	[0.245, 0.318]	[0.244, 0.317]	[0.299, 0.340]	[0.257, 0.332]
20	0.214	0.178	0.227	0.253	0.253	0.292	0.271
	[0.201, 0.231]	[0.153, 0.199]	[0.211, 0.246]	[0.228, 0.279]	[0.228, 0.278]	[0.276, 0.311]	[0.250, 0.292]
25	0.205	0.171	0.211	0.238	0.238	0.264	0.251
	[0.192, 0.219]	[0.151, 0.187]	[0.197, 0.227]	[0.214, 0.265]	[0.214, 0.265]	[0.251, 0.283]	[0.234, 0.269]
30	0.208	0.181	0.209	0.226	0.225	0.252	0.225
	[0.196, 0.220]	[0.162, 0.197]	[0.195, 0.224]	[0.205, 0.251]	[0.204, 0.250]	[0.241, 0.266]	[0.211, 0.243]
35	0.209	0.191	0.212	0.222	0.221	0.247	0.220
	[0.199, 0.222]	[0.176, 0.205]	[0.202, 0.227]	[0.203, 0.238]	[0.202, 0.238]	[0.235, 0.260]	[0.206, 0.233]
40	0.213	0.207	0.215	0.212	0.211	0.243	0.218
	[0.202, 0.225]	[0.190, 0.220]	[0.203, 0.230]	[0.196, 0.231]	[0.194, 0.230]	[0.231, 0.254]	[0.205, 0.231]
45	0.215	0.221	0.223	0.211	0.209	0.243	0.221
	[0.202, 0.225]	[0.206, 0.237]	[0.210, 0.239]	[0.194, 0.228]	[0.192, 0.227]	[0.232, 0.258]	[0.211, 0.235]
50	0.218	0.229	0.228	0.213	0.211	0.244	0.223
	[0.207, 0.228]	[0.215, 0.246]	[0.217, 0.241]	[0.199, 0.227]	[0.195, 0.226]	[0.234, 0.257]	[0.209, 0.234]
55	0.215	0.231	0.223	0.207	0.205	0.240	0.221
	[0.206, 0.224]	[0.216, 0.247]	[0.213, 0.237]	[0.192, 0.221]	[0.189, 0.219]	[0.231, 0.250]	[0.208, 0.233]
60	0.211	0.226	0.223	0.209	0.206	0.232	0.218
	[0.200, 0.221]	[0.209, 0.241]	[0.213, 0.237]	[0.192, 0.223]	[0.190, 0.221]	[0.221, 0.244]	[0.206, 0.229]
65	0.206	0.219	0.218	0.196	0.193	0.226	0.212
	[0.196, 0.217]	[0.203, 0.233]	[0.209, 0.231]	[0.182, 0.213]	[0.180, 0.209]	[0.216, 0.237]	[0.200, 0.229]
70	0.194	0.208	0.210	0.189	0.186	0.210	0.203
	[0.183, 0.204]	[0.190, 0.224]	[0.200, 0.221]	[0.175, 0.204]	[0.173, 0.202]	[0.201, 0.220]	[0.192, 0.213]
75	0.187	0.195	0.203	0.182	0.178	0.197	0.191
	[0.175, 0.196]	[0.178, 0.211]	[0.192, 0.213]	[0.166, 0.195]	[0.162, 0.193]	[0.188, 0.206]	[0.180, 0.202]
80	0.181	0.187	0.195	0.176	0.172	0.193	0.184
	[0.172, 0.192]	[0.167, 0.205]	[0.183, 0.205]	[0.162, 0.191]	[0.158, 0.188]	[0.182, 0.204]	[0.174, 0.197]
85	0.177	0.186	0.189	0.175	0.170	0.184	0.186
	[0.166, 0.190]	[0.161, 0.205]	[0.176, 0.202]	[0.154, 0.190]	[0.150, 0.187]	[0.175, 0.198]	[0.173, 0.201]
90	0.178	0.191	0.187	0.166	0.163	0.176	0.180
	[0.163, 0.194]	[0.166, 0.213]	[0.169, 0.202]	[0.146, 0.186]	[0.141, 0.183]	[0.161, 0.191]	[0.165, 0.196]
95	0.201	0.234	0.207	0.165	0.163	0.198	0.185
	[0.180, 0.224]	[0.194, 0.265]	[0.182, 0.235]	[0.140, 0.201]	[0.136, 0.198]	[0.175, 0.223]	[0.170, 0.202]
99	0.192	0.173	0.206	0.184	0.182	0.170	0.182
	[0.152, 0.240]	[0.086, 0.258]	[0.166, 0.255]	[0.134, 0.247]	[0.134, 0.241]	[0.138, 0.206]	[0.143, 0.228]

The table shows quantile treatment effect estimates with the bounds of bootstrapped 95% confidence intervals in brackets. All models control for quartics in the state math, reading and writing tests as well as each student's relative rank in their high school on each exam, student ethnicity/race, Title I status, English proficiency, free and reduced price lunch status, enrollment in gifted programs, special education, career and technology courses, whether the student had a college plan, and whether he was at risk of dropping out. The estimates also control for high school characteristics in the year of graduation: the ethnic composition of the high school, the percentage of students in each economic status group, the percentage of gifted students and students at risk, the percentage of title I eligible students, and total school enrollment.

Table A-3: Quantile Treatment Effects of College Sector on Earnings - Community College

Quantile	Baseline (i)	1996-1998 Cohort (ii)	HS Fixed Effects (iii)	College Attendees (iv)	All Earnings (v)
1	-0.098	-0.291	-0.113	-0.076	0.142
	[-0.203, 0.055]	[-0.455, -0.114]	[-0.228, 0.120]	[-0.192, 0.035]	[-0.089, 0.319]
5	-0.132	-0.194	-0.130	-0.061	-0.092
	[-0.192, -0.090]	[-0.307, -0.106]	[-0.180, -0.068]	[-0.109, -0.032]	[-0.158, -0.011]
10	-0.193	-0.223	-0.197	-0.099	-0.095
	[-0.226, -0.162]	[-0.288, -0.139]	[-0.233, -0.155]	[-0.125, -0.064]	[-0.138, -0.054]
15	-0.207	-0.232	-0.206	-0.121	-0.140
	[-0.229, -0.185]	[-0.263, -0.182]	[-0.233, -0.181]	[-0.141, -0.101]	[-0.173, -0.108]
20	-0.197	-0.206	-0.205	-0.123	-0.147
	[-0.216, -0.174]	[-0.227, -0.168]	[-0.222, -0.182]	[-0.145, -0.098]	[-0.179, -0.124]
25	-0.177	-0.177	-0.185	-0.129	-0.153
	[-0.198, -0.160]	[-0.202, -0.147]	[-0.200, -0.164]	[-0.146, -0.096]	[-0.174, -0.129]
30	-0.156	-0.150	-0.165	-0.126	-0.133
	[-0.174, -0.144]	[-0.170, -0.127]	[-0.177, -0.148]	[-0.142, -0.101]	[-0.153, -0.118]
35	-0.136	-0.129	-0.141	-0.124	-0.123
	[-0.151, -0.124]	[-0.147, -0.106]	[-0.155, -0.124]	[-0.138, -0.108]	[-0.138, -0.107]
40	-0.120	-0.112	-0.128	-0.119	-0.106
	[-0.133, -0.107]	[-0.127, -0.091]	[-0.140, -0.113]	[-0.130, -0.101]	[-0.120, -0.089]
45	-0.101	-0.092	-0.112	-0.115	-0.092
	[-0.114, -0.091]	[-0.112, -0.070]	[-0.127, -0.100]	[-0.125, -0.099]	[-0.105, -0.079]
50	-0.086	-0.064	-0.092	-0.113	-0.075
	[-0.095, -0.075]	[-0.082, -0.045]	[-0.104, -0.081]	[-0.123, -0.099]	[-0.086, -0.061]
55	-0.072	-0.047	-0.076	-0.104	-0.061
	[-0.082, -0.059]	[-0.062, -0.026]	[-0.090, -0.063]	[-0.112, -0.092]	[-0.073, -0.049]
60	-0.059	-0.036	-0.064	-0.095	-0.049
	[-0.070, -0.047]	[-0.051, -0.018]	[-0.076, -0.053]	[-0.104, -0.085]	[-0.060, -0.037]
65	-0.048	-0.022	-0.050	-0.086	-0.036
	[-0.059, -0.036]	[-0.039, -0.003]	[-0.064, -0.037]	[-0.096, -0.075]	[-0.047, -0.027]
70	-0.037	-0.014	-0.039	-0.078	-0.023
	[-0.049, -0.025]	[-0.030, 0.008]	[-0.053, -0.024]	[-0.090, -0.068]	[-0.035, -0.014]
75	-0.030	-0.016	-0.028	-0.072	-0.017
	[-0.043, -0.018]	[-0.034, 0.007]	[-0.041, -0.016]	[-0.082, -0.062]	[-0.029, -0.004
80	-0.019	-0.019	-0.020	-0.068	-0.005
	[-0.032, -0.008]	[-0.037, 0.002]	[-0.036, -0.007]	[-0.077, -0.058]	[-0.017, 0.006]
85	-0.014	-0.030	-0.017	-0.058	0.004
	[-0.026, 0.000]	[-0.049, -0.007]	[-0.032, -0.003]	[-0.068, -0.049]	[-0.009, 0.017]
90	-0.008	-0.038	-0.011	-0.050	0.015
	[-0.019, 0.004]	[-0.062, -0.008]	[-0.028, 0.001]	[-0.060, -0.038]	[0.001, 0.027]
95	0.015	-0.039	0.012	-0.038	0.034
	[0.002, 0.029]	[-0.063, -0.013]	[-0.006, 0.030]	[-0.051, -0.025]	[0.015, 0.051]
99	-0.040	-0.212	-0.043	-0.045	0.011
	[-0.070, -0.004]	[-0.289, -0.138]	[-0.080, -0.008]	[-0.072, -0.013]	[-0.024, 0.044]

The table shows the quantile treatment effects for each school type with the bounds of the 95% confidence intervals that are calculated using 250 bootstrap replications in brackets. All models control for quartics in the state math, reading and writing tests as well as each student's relative rank in their high school on each exam, student ethnicity/race, Title I status, English proficiency, free and reduced price lunch status, enrollment in gifted programs, special education, career and technology courses, whether the student had a college plan, and whether he was at risk of dropping out. The estimates also control for high school characteristics in the year of graduation: the ethnic composition of the high school, the percentage of students in each economic status group, the percentage of gifted students and students at risk, the percentage of title I eligible students, and total school enrollment.

Table A-4: Quantile Treatment Effects of College Sector on Earnings by Race/Ethnicity – Graduates

	Panel A: UT-Austin			
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	0.118	0.601	0.423	-0.247
	[-0.054, 0.320]	[-0.304, 1.385]	[-0.053, 0.727]	[-0.762, 0.033]
5	0.039	0.175	0.171	-0.092
	[-0.053, 0.117]	[-0.094, 0.978]	[-0.013, 0.529]	[-0.256, 0.148]
10	0.021	0.018	0.095	-0.035
	[-0.032, 0.074]	[-0.189, 0.371]	[-0.012, 0.302]	[-0.149, 0.110]
20	0.049	0.061	0.136	-0.045
	[0.022, 0.079]	[-0.112, 0.236]	[0.020, 0.223]	[-0.100, 0.014]
30	0.090	0.024	0.105	0.007
	[0.067, 0.106]	[-0.089, 0.117]	[0.032, 0.156]	[-0.040, 0.052]
40	0.111	-0.017	0.123	0.025
	[0.088, 0.132]	[-0.088, 0.060]	[0.073, 0.185]	[-0.002, 0.059]
50	0.140	-0.017	0.160	0.044
	[0.122, 0.156]	[-0.100, 0.052]	[0.118, 0.206]	[0.012, 0.078]
60	0.168	0.030	0.180	0.070
	[0.148, 0.187]	[-0.104, 0.091]	[0.121, 0.216]	[0.032, 0.108]
70	0.181	0.061	0.168	0.081
	[0.164, 0.199]	[-0.032, 0.117]	[0.121, 0.206]	[0.044, 0.127]
80	0.187	0.042	0.172	0.091
	[0.169, 0.209]	[-0.021, 0.123]	[0.123, 0.211]	[0.051, 0.134]
90	0.220	0.028	0.184	0.069
	[0.196, 0.253]	[-0.053, 0.152]	[0.124, 0.237]	[0.033, 0.109]
95	0.315	0.136	0.227	0.104
	[0.270, 0.356]	[-0.045, 0.432]	[0.152, 0.298]	[0.044, 0.177]
99	0.289	0.184	0.288	0.332
	[0.222, 0.348]	[-0.136, 0.604]	[-0.058, 0.378]	[0.089, 0.476]

	Panel B: Texas A&M - College Station			
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	0.367	0.988	0.433	0.161
	[0.255, 0.514]	[0.408, 1.521]	[-1.674, 0.966]	[-0.166, 0.567]
5	0.309	0.507	0.344	0.256
	[0.256, 0.370]	[0.063, 0.957]	[0.067, 0.725]	[0.110, 0.463]
10	0.290	0.402	0.253	0.207
	[0.259, 0.317]	[0.109, 0.577]	[0.007, 0.446]	[0.094, 0.304]
20	0.225	0.219	0.180	0.169
	[0.210, 0.241]	[0.130, 0.327]	[0.071, 0.295]	[0.103, 0.215]
30	0.216	0.164	0.147	0.156
	[0.205, 0.231]	[0.064, 0.257]	[0.069, 0.243]	[0.112, 0.198]
40	0.222	0.123	0.155	0.163
	[0.210, 0.235]	[0.039, 0.243]	[0.091, 0.237]	[0.124, 0.196]
50	0.224	0.145	0.170	0.161
	[0.212, 0.234]	[0.059, 0.219]	[0.088, 0.248]	[0.131, 0.188]
60	0.216	0.131	0.159	0.179
	[0.204, 0.228]	[0.074, 0.176]	[0.104, 0.241]	[0.131, 0.218]
70	0.196	0.114	0.144	0.177
	[0.183, 0.207]	[0.047, 0.208]	[0.090, 0.231]	[0.141, 0.218]
80	0.181	0.139	0.178	0.173
	[0.171, 0.195]	[0.055, 0.296]	[0.112, 0.243]	[0.131, 0.221]
90	0.178	0.220	0.200	0.157

Quantile	White	Black	Asian	Hispanic
	[0.160, 0.195]	[0.099, 0.316]	[0.129, 0.262]	[0.116, 0.211]
95	0.203	0.182	0.156	0.196
	[0.179, 0.227]	[0.097, 0.402]	[0.105, 0.247]	[0.114, 0.290]
99	0.186	0.229	0.219	0.287
	[0.147, 0.244]	[-0.051, 0.637]	[0.040, 0.377]	[0.130, 0.425]

	Panel C: Community College			
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	-0.063	-0.091	-0.016	-0.125
	[-0.244, 0.297]	[-0.375, 0.546]	[-1.474, 0.380]	[-0.344, 0.046]
5	-0.121	-0.102	-0.199	-0.135
	[-0.192, -0.043]	[-0.299, 0.090]	[-0.723, 0.358]	[-0.238, 0.057]
10	-0.176	-0.183	-0.290	-0.215
	[-0.222, -0.140]	[-0.263, -0.056]	[-0.649, 0.058]	[-0.297, -0.131]
20	-0.160	-0.157	-0.358	-0.262
	[-0.185, -0.141]	[-0.212, -0.085]	[-0.547, -0.113]	[-0.301, -0.199]
30	-0.124	-0.162	-0.281	-0.192
	[-0.142, -0.107]	[-0.213, -0.098]	[-0.467, -0.175]	[-0.234, -0.132]
40	-0.090	-0.150	-0.283	-0.151
	[-0.107, -0.077]	[-0.201, -0.108]	[-0.434, -0.175]	[-0.176, -0.114]
50	-0.060	-0.139	-0.240	-0.125
	[-0.073, -0.045]	[-0.170, -0.095]	[-0.409, -0.156]	[-0.151, -0.089]
60	-0.043	-0.093	-0.198	-0.091
	[-0.056, -0.029]	[-0.138, -0.052]	[-0.402, -0.120]	[-0.107, -0.057]
70	-0.032	-0.090	-0.157	-0.045
	[-0.048, -0.017]	[-0.128, -0.053]	[-0.377, -0.082]	[-0.060, -0.023]
80	-0.021	-0.082	-0.134	-0.009
	[-0.035, -0.010]	[-0.121, -0.042]	[-0.245, -0.067]	[-0.031, 0.012]
90	-0.010	-0.006	-0.107	0.021
	[-0.027, 0.008]	[-0.059, 0.065]	[-0.180, -0.056]	[-0.008, 0.055]
95	0.002	0.074	-0.074	0.030
	[-0.016, 0.026]	[-0.011, 0.130]	[-0.182, 0.030]	[0.001, 0.063]
99	-0.091	0.155	-0.063	0.058
	[-0.133, -0.033]	[0.028, 0.248]	[-0.150, 0.011]	[-0.036, 0.092]

The table shows the quantile treatment effects for each school type by race/ethnic group with the bounds of the 95% confidence intervals that are calculated using 250 bootstrap replications in brackets.

Table A-5: Quantile Treatment Effects of College Sector on Earnings by Race/Ethnicity – Attendees

	Panel A: UT-Austin			
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	0.096	0.203	0.663	-0.123
	[-0.076, 0.327]	[-0.719, 1.140]	[0.182, 1.084]	[-0.898, 0.178]
5	0.091	0.388	0.351	-0.012
	[0.012, 0.125]	[0.084, 0.691]	[0.075, 0.606]	[-0.176, 0.189]
10	0.097	0.254	0.135	0.111
	[0.039, 0.148]	[0.029, 0.477]	[0.033, 0.342]	[0.011, 0.211]
20	0.110	0.241	0.130	0.063
	[0.086, 0.142]	[0.114, 0.355]	[0.054, 0.232]	[0.003, 0.144]
30	0.125	0.129	0.112	0.077
	[0.104, 0.150]	[0.033, 0.246]	[0.060, 0.174]	[0.026, 0.137]
40	0.131	0.108	0.132	0.080
	[0.111, 0.160]	[0.027, 0.186]	[0.079, 0.204]	[0.046, 0.121]
50	0.166	0.041	0.153	0.082
	[0.148, 0.190]	[-0.007, 0.124]	[0.104, 0.200]	[0.038, 0.122]
60	0.186	-0.009	0.159	0.094
	[0.170, 0.206]	[-0.054, 0.123]	[0.118, 0.199]	[0.057, 0.132]
70	0.190	0.055	0.154	0.111
	[0.176, 0.209]	[-0.040, 0.162]	[0.115, 0.191]	[0.073, 0.139]
80	0.193	0.077	0.141	0.109
	[0.176, 0.214]	[-0.003, 0.133]	[0.102, 0.182]	[0.076, 0.140]
90	0.216	0.027	0.121	0.073
	[0.191, 0.245]	[-0.078, 0.123]	[0.064, 0.194]	[0.048, 0.125]
95	0.297	0.083	0.147	0.120
	[0.257, 0.345]	[-0.047, 0.248]	[0.078, 0.239]	[0.055, 0.183]
99	0.290	0.313	-0.055	0.235
	[0.220, 0.331]	[-0.018, 2.301]	[-0.110, 0.185]	[0.024, 0.389]

	Panel B: Texas A&M - College Station			
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	0.409	0.599	0.695	0.229
	[0.309, 0.547]	[-2.349, 1.181]	[0.226, 1.116]	[-0.128, 0.575]
5	0.355	0.256	0.381	0.287
	[0.311, 0.414]	[-0.014, 0.611]	[0.097, 0.661]	[0.113, 0.528]
10	0.365	0.364	0.258	0.307
	[0.332, 0.393]	[0.081, 0.594]	[0.056, 0.450]	[0.211, 0.431]
20	0.295	0.373	0.212	0.267
	[0.278, 0.314]	[0.214, 0.485]	[0.090, 0.270]	[0.191, 0.326]
30	0.259	0.253	0.135	0.232
	[0.244, 0.273]	[0.183, 0.362]	[0.054, 0.230]	[0.184, 0.274]
40	0.250	0.214	0.173	0.199
	[0.238, 0.261]	[0.133, 0.317]	[0.070, 0.227]	[0.157, 0.233]
50	0.253	0.221	0.149	0.201
	[0.242, 0.266]	[0.121, 0.300]	[0.078, 0.198]	[0.172, 0.235]
60	0.238	0.198	0.115	0.192
	[0.227, 0.251]	[0.112, 0.260]	[0.061, 0.185]	[0.154, 0.235]
70	0.211	0.156	0.115	0.193
	[0.202, 0.223]	[0.112, 0.251]	[0.054, 0.182]	[0.160, 0.230]
80	0.194	0.182	0.132	0.173
	[0.184, 0.203]	[0.086, 0.299]	[0.055, 0.196]	[0.140, 0.211]
90	0.180	0.232	0.129	0.162

	[0.164, 0.195]	[0.098, 0.353]	[0.057, 0.191]	[0.121, 0.192]
95	0.203	0.242	0.074	0.179
	[0.176, 0.227]	[0.086, 0.357]	[-0.014, 0.201]	[0.101, 0.240]
99	0.179	0.205	-0.082	0.224
	[0.143, 0.213]	[-0.192, 0.655]	[-0.269, 0.182]	[0.055, 0.367]

Panel C: Community College				
	White	Black	Asian	Hispanic
Quantile	(i)	(ii)	(iii)	(iv)
1	-0.165	0.025	-0.143	-0.118
	[-0.249, -0.052]	[-0.230, 0.232]	[-0.552, 0.254]	[-0.295, 0.148]
5	-0.167	-0.108	-0.068	-0.024
	[-0.199, -0.103]	[-0.243, 0.041]	[-0.317, 0.163]	[-0.093, 0.036]
10	-0.192	-0.074	-0.173	-0.021
	[-0.208, -0.118]	[-0.152, 0.014]	[-0.297, -0.019]	[-0.078, 0.023]
20	-0.174	-0.088	-0.159	-0.079
	[-0.193, -0.154]	[-0.013, -0.022]	[-0.285, 0.074]	[-0.110, -0.042]
30	-0.147	-0.083	-0.208	-0.107
	[-0.163, -0.136]	[-0.118, -0.041]	[-0.290, -0.059]	[-0.134, -0.078]
40	-0.131	-0.089	-0.190	-0.100
	[-0.144, -0.118]	[-0.128, -0.062]	[-0.264, -0.117]	[-0.122, -0.076]
50	-0.102	-0.100	-0.208	-0.100
	[-0.113, -0.092]	[-0.133, -0.075]	[-0.254, -0.141]	[-0.124, -0.077]
60	-0.080	-0.095	-0.199	-0.110
	[-0.091, -0.069]	[-0.119, -0.073]	[-0.240, -0.140]	[-0.128, -0.089]
70	-0.069	-0.093	-0.173	-0.099
	[-0.081, -0.060]	[-0.116, -0.070]	[-0.227, -0.117]	[-0.113, -0.081]
80	-0.055	-0.098	-0.181	-0.080
	[-0.069, -0.046]	[-0.125, -0.072]	[-0.235, -0.128]	[-0.097, -0.062]
90	-0.042	-0.075	-0.135	-0.069
	[-0.055, -0.027]	[-0.092, -0.057]	[-0.209, -0.087]	[-0.100, -0.044]
95	-0.026	-0.037	-0.143	-0.061
	[-0.041, -0.009]	[-0.066, -0.005]	[-0.205, -0.035]	[-0.085, -0.031]
99	-0.053	0.013	-0.218	-0.057
	[-0.092, -0.008]	[-0.069, 0.086]	[-0.416, 0.102]	[-0.083, -0.018]

The table shows the quantile treatment effects for each school type by race/ethnic group with the bounds of the 95% confidence intervals that are calculated using 250 bootstrap replications in brackets.