Taking a Chance on College: Is the Tennessee Education Lottery Scholarship Program a Winner?

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Abstract
Most policies seeking to improve high school achievement historically either provided incentives for educators or punished students. Since 1991, however, over a dozen states, comprising approximately a quarter of the nation's high school seniors, have implemented broad-based merit scholarship programs that reward students for their high school achievement with college financial aid. This paper analyzes one of these initiatives, the Tennessee Education Lottery Scholarships, using individual-level data from the ACT exams. The program did not achieve one of its stated goals, inducing more students to prefer to stay in Tennessee for college, but it did induce large increases in performance on the ACT. Policies that reward students for performance do affect behavior and may be an effective way to improve high school achievement.

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I. Introduction

Many policies implemented to improve American elementary and secondary education provide incentives to teachers and schools, not students. Those that do provide incentives to students typically do so by punishing students who perform poorly instead of rewarding those who do well. However, since 1991, more than a dozen states have enacted scholarship programs that award merit aid at in-state colleges to large fractions of the states' high school graduates for students' high school GPAs, scores on a standardized test, or both. This paper analyzes the effect of one of these programs, the Tennessee Education Lottery Scholarship (TELS), on high school achievement as measured by the ACT.

Approximately a quarter of high school seniors live in states offering these scholarship programs and these programs represent a large expense – Tennessee's program cost $68 million for just one class in its first year – understanding the effects of these programs is important in its own right. To this end, this paper also analyzes the effect of Tennessee's scholarship program on students' college preferences.

The main prong of the TELS, the HOPE Scholarship, rewarded Tennessee residents who (1) scored at least 19 on the ACT (or 890 on the SAT) or (2) had a final high school GPA of 3.0 or higher, including a 3.0 unweighted GPA in all 20 credits of college core and university track classes. Winners received a renewable $3,000 per year to attend any four-year Tennessee college or a renewable $1,500 per year to attend any two-year college in the state. Using micro data on students' ACT scores, the colleges to which students sent their scores, and a rich set of background characteristics, I analyze the effect of the TELS on Tennessee students' ACT scores.

This scholarship increases the return to scoring 19 or higher on the ACT for students who were unsure of their ability to qualify for the scholarship through their GPAs and who were considering attending an in-state college. Because it does not strongly affect the return to increasing the ACT score for students who would have already scored 19 or higher or for students who cannot reach 19, I expect to (and do) find that students increased their scores from below 19 to 19 or just above, but there was very little change in the rest of the test score distribution.

Secondly, I analyze the effect of the TELS on students' college preferences as measured by their stated preferences and the colleges to which they sent their ACT scores. The TELS decreases the cost of attending in-state relative to out-of-state colleges and four-year in-state relative to two-year in-state colleges. I find no effect of the TELS on college preferences. While there were small changes in preferences in Tennessee in 2004, I show that the changes occurred primarily for students ineligible for the TELS and thus are extremely unlikely to have resulted from the scholarship.

The paper is organized as follows: Section II provides background information and discusses the relevant literature. Section III describes the dataset used; Sections IV and V present the empirical results on changes in the test-score distribution and students' college preferences, respectively. Section VI concludes.

II. Background

Tennessee Education Lottery Scholarships

The TELS, funded by a newly-created state lottery, first began awarding scholarships in the fall of 2004 to college freshmen from the high school class of 2004 and college sophomores from the high school class of 2003. [1] Scholarships were available to Tennessee residents who enrolled in Tennessee post-secondary institutions and required no application except for the FASFA, which was required of all scholarship winners whether or not they were likely to be eligible for need-based aid.

The TELS is open to a larger percentage of students than many of the other state merit scholarship programs -- approximately 65% of high school graduates -- and, comparatively, is especially inclusive of African-Americans and low-income students (Ness and Noland 2004). It is the only program that allows students to qualify through their performance on a standardized test *or* through their grades.

Table 1 shows the size of the HOPE Scholarship. The state awarded 23,287 scholarships from the class of 2004, costing almost $68 million. Of those who qualified for and accepted the scholarship, 79% attended four-year colleges and the vast majority of these attended public colleges.

Tennessee Postsecondary Education

Even before the TELS was enacted, 85% of Tennesseans going straight to college attended one of the state's nine four-year public universities, 13 two-year public colleges, 35 independent institutions, or 27 technology centers.[2]

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[1] The TELS encompassed five programs, the largest of which was the HOPE Scholarship which accounted for over 99% of scholarship winners attending a two- or four-year college. The other programs provided supplements to the HOPE Scholarship for low-income and high-achieving students, provided a smaller award to low-income students who very slightly missed HOPE eligibility, and provided aid to students entering technical or trade schools.

[2] The statistics in this section are derived from the *Statistical Abstract of Tennessee Higher Education: 2003-04*, Dorie Turner's article "99% of UT Freshmen Qualify for Lottery Aid," the American College Survey, and personal correspondence with Robert Anderson at the Tennessee Higher Education Commission.

The four-year public universities were competitive enough that a student on the margin of HOPE eligibility would have found peers of similar ability in the university system, but Tennessee's "best and brightest" typically would not. Approximately 60% of 2004 Tennessee freshmen at Tennessee four-year public colleges received lottery scholarships. Academically, the four-year public colleges range from the historically black Tennessee State University, at which the middle 50% of students score between 16 and 21 on the ACT (equivalent to 760 to 1010 on the SAT) to the flagship, the University of Tennessee-Knoxville, at which the middle 50% of students score 20 to 27 on the ACT (equivalent to 940 to 1230 on the SAT).

While the size of the individual HOPE awards was comparable to many other states' scholarship programs, students planning to attend a public university could not have expected the HOPE Scholarship to cover their entire cost of attendance. In the year before the TELS was implemented, tuition and fees alone of the public four-year universities ranged from $500 to $1500 more than the value of the HOPE Scholarship. For two-year colleges tuition and fees ranged from $550 to $600 more than the value of the scholarship.

Research on Merit Aid in Other States

Many papers analyze the effects of broad-based merit scholarship programs on student behavior. Dynarski (2004) analyzes programs in seven Southern states (not including Tennessee's) using CPS data and finds large effects on college matriculation. In the aggregate, these programs increased the probability that students from these states would enroll in college by 4.7 percentage points, primarily by increasing the probability of enrolling at a public college.

Cornwell *et al.* (2005, 2006a, and 2006b) and Cornwell and Mustard (2006) find that the Georgia HOPE program increased students' desire to attend Georgia colleges, particularly elite colleges. There was a significant increase in the number of students attending Georgia colleges after the scholarship was implemented, specifically at four-year public and private Georgia colleges and HBCUs, while the acceptance rates of Georgia colleges and the yield rates of elite Georgia colleges decreased relative to control schools. They also find that to retain HOPE funding while in college, in-state University of Georgia students enrolled in fewer credits overall, withdrew from more classes, took fewer math and sciences classes, and switched to easier majors than their out-of-state peers.

Less attention has been paid to these scholarships' effects on high school achievement. However, Henry and Rubenstein (2002) do indirectly show that the Georgia HOPE program increased Georgia high school achievement. They argue that the HOPE program increased high school grades among Georgia students entering Georgia public colleges, but SAT scores did not decrease relative to grades for this group, so the increase in grades must have been a result of improved achievement, not grade inflation.

III. Data Description

The data for this analysis come from a unique set of individual records assembled from the administrative files of the ACT Corporation. The data include a broad cross section of observations on students who took the ACT and planned to graduate high school in 1996, 1998, 2000, and 2004: one out of every four Caucasians, one out of every two minorities, and every student who either listed their race as “multiracial” or “other” or failed to provide a race. I limit the sample to the 24 states[3] in which the flagship university received more ACT than SAT scores, because the students who take the ACT in states where the ACT is not the primary college entrance exam are not expected to be representative of the states’ potential college-goers. In the analysis of college preferences, I use only data from 1998, 2000, and 2004 since students graduating high school in 1996 could not send complimentary score reports to as many schools as in later years. Finally, in my analysis of score-sending, I omit the 13.6% of students who did not send their scores to any colleges.

Considering only these 24 states, there is still a large sample: 997, 346 records for the four years and over 225,000 in each year. The sample from Tennessee is large – there are 58,595 observations in total, with over 14,400 individuals in each year – and representative of the state’s college-goers – over 87% of the state’s high school seniors took the ACT in 2004.

I observe each student’s composite ACT score the last time she took the exam,[4] stated preferences regarding aspects of her desired college, demographic and other background information, and up to six colleges to which she sent her score.[5] Students

[3] These are Alabama, Arkansas, Idaho, Illinois, Iowa, Kansas, Kentucky, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Mexico, North Dakota, Ohio, Oklahoma, South Dakota, Tennessee, Utah, Wisconsin, and Wyoming.

[4] Despite my best efforts to procure data on how many times students took the ACT, their scores on previous test sittings, and even aggregate information on retesting rates, I was only able to obtain a student’s score the last time she took the ACT.

[5] Observing only six colleges to which students sent their scores does not practically limit my knowledge of student preferences as over 98% of students who sent test scores sent five or fewer.

indicate the colleges they'd like their scores sent to when they register for the test[6] and indicate the state, number of years, institutional control, and maximum tuition of their desired college on test day. The background information is very detailed and includes gender, race, family income, classes taken, extracurricular activities, and out-of-class accomplishments.

The analysis in this paper differs from studies that analyze improvements in high school grades, which may confound changes in student achievement and incentives for grade inflation, because the ACT is an objective, nationally-administered test. The extremely rich background information allows me to conclude that my results are not due to a changing pool of test-takers. However, I cannot conclude that the increases in ACT scores reflect human capital accumulation. Students could have increased their score as a result of developing ACT-specific human capital, getting more sleep the night before the exam, or exploiting the randomness in test questions by retaking the test until receiving their desired score. I cannot rule out the first two explanations, but I do use data on the traditional gains from retesting to show that the effect is too large to plausibly be generated by increased retesting alone.

Card and Krueger (2005) suggest that score-sending data is a good proxy for application data. They find that the number of SAT scores sent to a particular college is very highly correlated with the number of applications it receives. Using score-sending data to measure college preferences has become common in the literature.[7] However, additionally analyzing students' stated college preferences allows me to detect any

[6] Students could send four score reports for free, with each additional report costing $6 in 1998 and 2000 and $7 in 2004.
[7] For examples, see Card and Krueger (2005), Abraham and Clark (2006), Pope and Pope (2006), Long (2004), and Pallais and Turner (2006).

changes in preferences over colleges within their application portfolios that would not be evident from examining score-sending or application data alone.

IV. Impact of the TELS on ACT Score

Tennessee students increased their ACT scores from below 19 to 19, 20, and 21 as a result of the TELS. This is evident in graphically comparing the distribution of test scores in Tennessee from 2004 with distributions in earlier years and in difference-in-difference regressions. The results are incredibly robust.

Graphical Analysis

Figures 1a, 1b, 1c, and 1d show the probability and cumulative distributions of test-scores in Tennessee and comparison states for each year. They show the 2004 distribution marks a clear departure from the other years in Tennessee, but not in the rest of the country.

The first analytic question is whether the change in the distribution of test scores captures changes in the performance of test-takers or changes in the composition of the pool of test-takers. To control for differential selection into test-taking in the different years, I use a procedure developed by Dinardo, Fortin, and Lemieux (DFL), reweighting the 1996, 1998, and 2004 distributions so that they represent the counterfactual test-score distributions that would have prevailed if the same pool of students had taken the ACT in those years as in 2000.[8] I construct the counterfactual distribution for each year separately, pooling the observations from 2000 and that year, running a probit regression

[8] This is very similar to the estimator proposed by Barsky *et al.* (2002) in their exploration of the black-white wealth gap.

of the year of the observation (2000 = 1) on control variables and reweighting that year's data by the ratio of the fitted value to the fitted value's additive inverse. Appendix A has a mathematical explanation of the reweighting procedure.

I use many control variables in addition to state and year dummies, including information on the students' academic and extracurricular activities and dummies for whether each of the control variables is missing. [9] Because students' academic and extracurricular records are potentially affected by the TELS, I reperform the analysis using only fully exogenous variables as controls. The results are very similar.

Figures 2a through 2d show how this reweighting procedure changes the test-score distributions for Tennessee and the rest of the country in 2004, while figures 3a through 3d are replications of figures 1a through 1d using the reweighted distributions. The graphs show that the change in the score distribution in 2004 was a result of actual score improvement and not solely a result of differential selection into test-taking. Figures 3a and 3b show that the only change in the Tennessee distribution from previous years was the change theory predicts: a shifting of mass from below 19 to 19 or just above. There is almost no change in the distribution of scores above 21.

To quantify the differences in the test-score distributions, I use Kullback and Leibler's (1951) divergence function:

$$D_{KL}(P \| Q) = \sum_i P(i) \times \log(\frac{P(i)}{Q(i)}), \qquad (1)$$

[9] Background variables included are: income dummies, race dummies, a dummy for U.S. citizenship, an indicator for English being the primary language spoken in the home, the number of siblings the student has under the age of 21, the size of the community the student lives in, and the student's gender. The variables on the student's academic career are whether she attends a private high school, is on a college preparatory track, has any college credit, the number of years of English and math classes she has taken, and dummies for whether she has taken honors English or math. Also included are dummy variables about the student's extracurricular activities that record whether she was ever elected to a student office, worked on the staff of a school paper or yearbook, earned a varsity letter for sports participation, and held a regular part-time job.

to calculate the divergence of the 2000 distribution (P) from the other years' actual distributions, reweighted distributions, and distributions reweighted using only exogenous controls (Q). The results are presented in Table 2. For states other than Tennessee, the divergences are small and fairly similar across years. For Tennessee, the divergences of the 2004 population from the 2000 population are more than double the divergences between 2000 and any other year.

Regression Analysis

Using an indicator variable for whether the student scored 19 or higher on the ACT as the dependent variable (y_{ist}), I estimate difference-in-difference regressions comparing the change in ACT scores in Tennessee in 2004 to the change in scores in other states. I estimate the effect using both an OLS and probit specification, estimating equations (2) and (3) respectively:

$$y_{ist} = \beta_0 + \beta_1 TELS_{st} + \beta_2 X_{ist} + \delta_s + \delta_t + \varepsilon_{ist} \qquad (2)$$

$$y_{ist} = \Phi(\beta_0 + \beta_1 TELS_{st} + \beta_2 X_{ist} + \delta_s + \delta_t) + \varepsilon_{ist}\,. \qquad (3)$$

Here $TELS_{st}$ is an indicator for whether the student lived in a state and year (Tennessee in 2004) when the TELS was in effect and Φ represents the standard normal cumulative distribution function. The coefficient of interest in both cases is β_1. The terms δ_s and δ_t are state and year fixed effects respectively and ε_{ist} is an idiosyncratic error term. I consider several different specifications for X_{ist}, the control variables. Standard errors are clustered at the state-year level.

Results from estimating equations (2) and (3) are presented in Tables 3 and 4 respectively. In Table 4, the first row in each cell contains the average of the marginal effects for each observation in the sample while the second and third rows in each cell are the estimated coefficient and standard error. The impact of the TELS is clear: every coefficient in the tables is positive and significant at the 5% level and over 95% are significant at the 1% level. As more controls are added, the measured impact of the program decreases, however it still remains large and significant. The coefficient also remains large and significant after the many robustness checks described in Appendix B. The results from these robustness checks are displayed in Appendix Table 1.

For the whole population, the OLS coefficient is 0.061 when all controls are included and 0.079 when only the strictly exogenous controls are included, signifying that 6.1% and 7.9% of Tennessee students increased their score to 19 or higher as a result of the TELS, respectively. These estimates are similar in magnitude to the actual 7.2% increase in the number of Tennessee students who scored 19 or higher before and after the TELS was implemented.

Assessing the Magnitude of the Effect

A 6.1 percentage point increase in the number of students scoring 19 or higher is a large increase in test scores. [10] Since only 42.1% of Tennessee test-takers scored below 19 in 2000, this implies that one out of every seven Tennessee students who could have increased their score to 19 or higher did so. This may even understate the change in achievement as some students may have increased their scores but not all the way to 19

[10] The magnitude of this effect is roughly comparable to estimated magnitudes of effects of the Georgia HOPE program. Henry and Rubenstein (2004) find the percentage of Georgia high school students earning a GPA of "B" or better increased by 2.9 percentage points and Dynarski (2004) finds that college matriculation of Georgia students increased by 4.7 percentage points as a result of the Georgia HOPE program. These effects are approximately ½ and ¾ of the size of the effect of the TELS respectively. Cornwell *et al.* (2005) find that GPAs among University of Georgia freshmen increased by 0.18 standard deviations which is 50% larger than the 0.12 standard deviation increase in ACT scores caused by the TELS.

and some students who would have scored 19 or higher even without the TELS worked to increase their scores because of uncertainty over how they would perform.

Further analysis suggests that despite the decrease in mass at scores as low as 12 and 13 in Figure 3a, only students who would have scored 15 to 18 without the TELS increased their scores to 19 or higher. Some students who would have scored below 15 without the TELS did increase their scores, but fell short of 19.

To determine whether students who would have earned low ACT scores increased their scores to 19 or higher, I first predict the ACT score students would have received without the TELS using data from 1996, 1998, and 2000 and all of the control variables. Then I estimate equation (2) separately for students predicted to have different ACT scores. Table 5 shows that the coefficients for students predicted to score below 15 are small and insignificant, but students predicted to score 15 to 17 and 17 to 19 saw 5.3 and 7.6 percentage point increases in the probability of scoring 19 or higher respectively. Column 3, which attempts to account for the prediction error, suggests that 6.7% and 12.6% of students who would have scored in these ranges increased their scores above the threshold.

Though no information is available on changes in retesting rates, these effects are too large to be a function of students simply retaking the test. An ACT Research Report from 1993 found that 36% of students retook the ACT nationally. Eleven percent of those scoring 15 or 16 and 43% of those scoring 17 or 18 increased their score to 19 or higher on their second attempt. A back-of-the-envelope calculation shows that to get effects as large as seen here, there would need to be a 61 percentage point increase in retesting among students who scored 15 to 16 on their first attempt and a 29 percentage point increase in retesting among those who scored 17 to 18. Assuming the fraction of students retaking the ACT was constant across ACT scores, 97% of students scoring 15 and 16 would have to retake the ACT to see effects this large. This is too large to be plausible.[11] Since students often study between test dates, even if this effect could be explained only by retesting, the gains would still have probably partially been a result of increased human capital.

Impact of the TELS on Different Subgroups

Tables 3 and 4 also display the results from restricting the estimation of equations (2) and (3) to different subgroups. They show that African-Americans were significantly less responsive to the TELS than Asians and Caucasians, and males were slightly more responsive than females.

The results in Table 3 suggest an African-American who would have scored below 19 without the TELS was over five times less likely than an Asian and seven times less likely than a Caucasian to increase her score to 19 or higher: the point estimate for blacks is smaller than for other groups and the fraction scoring below 19 is larger. Part, but not all, of this disparity in apparent responsiveness is due to the fact that African-Americans scoring below 19 scored, on average, lower than the other racial groups, so they would have had to increase their score by more to reach the cutoff of 19. However, a higher percentage of Tennessee blacks score between 15 and 18 than Tennessee whites, so if blacks and whites were equally affected by the TELS, the point estimate for blacks

[11] In fact, the fraction that would need to retake the test to see effects this large would probably need to be even greater. This calculation assumes that students retaking the ACT as a result of the TELS would see score increases as large as those who retook the ACT without it. But this is unlikely as there was a reason some students chose to retake the ACT initially.

should be higher than the point estimate for whites. Repeating the analysis in Table 5 separately for blacks and whites suggests African-Americans were at least three times less responsive to the TELS than Caucasians.

Males were slightly more responsive to this program than females. The coefficients of different specifications of equation (2) are 25% to 37% higher for males than females. In pooled data the interaction term between being male and the presence of the TELS is positive and significant in every specification while the score distributions of males and females before the TELS were very similar. This result is interesting in light of several papers that find larger effects of financial incentives on educational attainment and performance for females (e.g. Angrist et al., 2006; Angrist and Lavy, 2002; and Dynarski, 2005).

V. Impact of the TELS on College Applications

The TELS did not affect where students sent their ACT scores or students' stated college preferences. I analyze students' preferences for in-state versus out-of-state, four-year versus two-year, and four-year in-state versus two-year in-state colleges as well as the tuition students were willing to pay and find no robust effect of the TELS.

For each type of school listed above, I estimate equation (2) separately using the total number of scores the student sent to that type of college, a dummy variable for whether the student sent any score to that type of college, and the student's preference for that type of college as dependent variables. I also use the maximum tuition the student reports being willing to pay and the average tuition of colleges she sent scores to as dependent variables. [12] Tables 6 and 7 present the results for the whole sample, students who scored 19 or higher on the ACT, students who scored below 19, and students who scored below 19 and reported a GPA below 3.0. I include all of the control variables and cluster standard errors at the state-year level.

While these subgroups are endogenous, if the results are due to the TELS, we would expect students scoring 19 or higher to be much more responsive than students who are likely ineligible. The results are not driven by the endogeneity of the subgroups; they are the same when ACT score and GPA are predicted using data before the TELS and the subgroups restricted based on those variables.

Panel A of Table 6 shows that the number of scores sent did not change significantly either in the aggregate or for any of the subgroups examined, allowing us to more easily interpret changes (or the lack thereof) in score-sending as changes (or lack thereof) in preferences. Panel B shows that the TELS did not induce students to prefer in-state colleges more strongly. In fact, while not all significant, the point estimates all indicate students were less likely to prefer in-state as compared to out-of-state colleges: students sent fewer scores to in-state colleges, more scores to out-of-state colleges, and were less likely to say they wanted to attend college in-state. This is not due to preferences of students who were ineligible for the scholarship: the point estimates for students scoring 19 or higher are all signed in the “wrong” direction as well.

[12] Average tuition is calculated using data from the IPEDs. I use tuition in 2004-2005 regardless of when the student graduated high school to pick up changes in preferences over types of schools as opposed to changes in tuition of more or less popular schools. For schools that have different in-state and out-of-state tuitions, I use in-state tuition if the student lives in the same state as the school and the out-of-state tuition if she doesn’t. It is interesting to note that the maximum tuition students indicate being willing to pay is less than half of the average tuition of the colleges they send their scores to. This most likely indicates that students are poorly informed about the cost of college. As long as the nature of their misinformation is not changing parallel to the TELS, evaluating their stated preferences over college tuition is still instructive.

While results from the entire sample indicate that Tennessee students increased their preference for four-year as opposed to two-year schools, the point estimates for students scoring 19 or higher, while not significant, indicate these students sent fewer scores to four-year colleges. It was only students scoring below 19 (including those with GPAs below 3.0) who sent scores to more four-year colleges in 2004. Moreover, while students scoring 19 or higher did realize decreases in both the total number of two-year colleges they sent scores to and their preferences for four-year colleges, these changes were only about half and one-third as large, respectively, as those realized by students with ACT scores below 19 and GPAs below 3.0.

While preferences for four-year colleges could theoretically decrease if students began to prefer two-year in-state colleges over four-year out-of-state colleges (an admittedly very unusual response), the predictions indicate unambiguously that the TELS should increase the preference for four-year in-state colleges. However, the point estimates for students scoring 19 or higher indicate that these students were less likely to send scores or express preferences for attending four-year in-state colleges.

Finally, there was no effect of the TELS on the tuition students were willing to pay. In the aggregate, Tennessee students both said they were willing to pay higher tuition and sent their scores to more expensive schools in 2004. However, students scoring 19 or higher were the only group not to see a significant increase in the actual tuition of colleges scores were sent to and the point estimate for this group is approximately one third of those for the other two groups. They also did not report larger increases in the tuition they were willing to pay than the other groups.

While students could have changed their college preferences later in the application process, this analysis provides strong evidence that students had not changed their college preferences as a result of the TELS by the time they took the ACT. This section shows the value of using micro data with detailed background characteristics to analysis the difference in score-sending.

VI. Conclusion

The Tennessee Education Lottery Scholarship Program increased the return to scoring 19 or above on the ACT for students who unsure of their ability to win the scholarship based on their grades and were interested in attending a Tennessee college. It also decreased the cost of attending in-state as compared to out-of-state and four-year in-state as compared to two-year in-state colleges for scholarship winners.

The TELS did not induce scholarship winners to change their college preferences. Students did respond to the scholarship, however. Graphically, it is clear that students' scores increased sharply around the threshold of 19 while difference-in-difference regressions show that the probability a given student would score 19 or higher on the ACT increased by 6% to 8% in the first year of the program. This effect is extremely robust. It is also a very large increase, implying that one out of every seven students who could have increased their scores to 19 or higher did so as a result of the TELS. African-Americans responded very little to the scholarship incentive while males were more responsive than females.

The large increase in ACT scores induced by the TELS show that policies that reward students for their academic performance can potentially generate large improvements in high school achievement. The fact that this performance improvement is too large to result from students simply retaking the test suggests that it may likely indicate true human capital accumulation. However, it remains to be seen whether this is ACT-specific human capital, such as learning the directions for the test, or whether this is human capital that will positively affect other outcomes.

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Appendix A: Mathematical Explanation of the Multivariate Reweighting Procedure

Let f(ACT| z, t, TELS) be the distribution of ACT scores for students with a set of observable characteristics z, in year t, in a state of the world where the TELS is in place (TELS=1) or a state of the world where it is not (TELS=0). Let dF(z|t) be the distribution of attributes z in the pool of ACT-takers in year t. Then, the actual distribution of scores in 2004 is

$$\int f(ACT \mid z, t = 2004, TELS = 1) \times dF(z \mid t = 2004) \quad (1)$$

while the distribution of scores that would have prevailed in 2004 if the population of test-takers was the same in 2004 as it was in 2000, is

$$\int f(ACT \mid z, t = 2004, TELS = 1) \times dF(z \mid t = 2000) \quad (2)$$

I define the reweighting function

$$\varphi_z(z) = \frac{dF(z \mid t = 2000)}{dF(z \mid t = 2004)} \quad (3)$$

so that $\varphi_z(z)$ essentially measures how much more frequently a student with characteristics z is in the 2000 pool of test-takers than in the 2004 pool. This allows me to calculate the counterfactual density:

$$\begin{aligned} &\int f(ACT \mid z, t = 2004, TELS = 1) \times dF(z \mid t = 2000) = \\ &\int f(ACT \mid z, t = 2004, TELS = 1) \times dF(z \mid t = 2004) \times \varphi_z(z). \end{aligned} \quad (4)$$

which is practically estimated by reweighting each observation in the 2004 pool by the relevant value of $\hat{\varphi}_z(z)$, estimated as described in the text.

Appendix B: Robustness Checks for the Effect of the TELS on ACT Scores

The difference-in-difference results are extremely robust in terms of both the magnitude and significance of the effect. Tables 3 and 4 show that the results are not sensitive to the specific controls or specification chosen. In Appendix Table 1, I show they are not due to serial correlation of ACT scores within states over time or the fact that I only analyze the behavior resulting from one state's policy change.

Serial correlation is not as likely to be a problem in my analysis as many other differences-in-differences papers because I use a short time series with only four periods (see Bertrand *et al.* 2004). Moreover, regressing the mean residuals within a state for a given year on their lagged value produces negative coefficients, suggesting that in fact the reported standard errors may be too high.

The coefficient is of similar magnitude and still significant when state-specific linear time trends are added and standard errors are clustered at the state level. It remains large and significant when the data are collapsed down to the state-year level and even, in all but one specification, when data are collapsed to the state-year level when state-specific linear time trends are added.[13] Even collapsing the data into two observations by state, one before and one after the TELS was implemented, yields a highly significant estimate which indicates a 7.6 percentage point increase in Tennessee students scoring 19 or higher as a result of the TELS.

Conley and Taber (2006) show that if the number of states whose policy changes stays fixed even when the number of states used as controls and the number of students in a state approaches infinity, the program effect estimated by a difference-in-difference

[13] The only coefficient that isn't significant is a coefficient in a regression that estimates 89 coefficients with 96 observations.

regression is not consistent. However, the last two sets of rows in Appendix Table 1 show that this is not driving my results. I use the consistent estimator of p-values that they suggest on both the individual and state-year data and the p-values are all below 0.05. [14] The procedure for constructing this estimator is as follows.

Conley and Taber start with the following model of data at the state-year level:

$$\tilde{Y}_{jt} = \alpha\tilde{d}_{jt} + \tilde{X}'_{jt}\beta + \tilde{\eta}_{jt}. \tag{1}$$

Here j indexes the state, t indexes the time period and tildes denote variables which are projections onto group and time indicators. For any variable Z, $\tilde{Z}_{jt} = Z_{jt} - \bar{Z}_j - \bar{Z}_t - \bar{Z}$, where $\bar{Z}_j$, $\bar{Z}_t$, and $\bar{Z}$ are the state, year, and overall mean of Z respectively. The variables X_{jt} are control variables that vary at the state-year level and d_{jt} is the indicator for whether the policy was in effect at the time; η_{jt} is the idiosyncratic error.

When the number of states who do change their policy (N_0) is finite, but the number of states who do not (N_1) grows large, the differences-in-differences estimator $\hat{\alpha}$ converges in probability to α + W, where

$$W = \frac{\sum_{j=1}^{N_0}\sum_{t=1}^{T}(d_{jt} - \bar{d}_j)(\eta_{jt} - \eta_j)}{\sum_{j=1}^{N_0}\sum_{t=1}^{T}(d_{jt} - \bar{d}_j)^2} \tag{2}$$

The states that change their policies are indexed by j equal to 1 to N_0; states which do not realize a policy change are indexed by j equal to $N_0 + 1$ to N_1.

[14] Because the procedure involves using data from the 23 states that did not change their policy to estimate the error structure for Tennessee, the p-values must all be multiples of 1/23. This explains why so many of the specifications have exactly the same p-value.

Assuming that the state-year errors are independent of any regressors and identically distributed across state-year cells, a consistent analog estimator of the conditional cumulative distribution function of W given the entire set of d's is

$$\hat{\Gamma}(w) = \left(\frac{1}{N_1}\right)^{N_o} \sum_{l_1=N_0+1}^{N_0+N_1} \cdots \sum_{l_{N_0}=N_0+1}^{N_0+N_1} 1\left(\frac{\sum_{j=1}^{N_0}\sum_{t=1}^{T}(d_{jt}-\bar{d}_j)(\widetilde{Y}_{l_j t}-\widetilde{X}'_{l_j t}\hat{\beta})}{\sum_{j=1}^{N_0}\sum_{t=1}^{T}(d_{jt}-\bar{d}_j)^2} < w\right) \tag{3}$$

where 1(.) is an indicator function.

I use this estimated cumulative distribution function and report $\Pr(\hat{\alpha}+W<0)=\Pr(W<-\hat{\alpha})=\Gamma(-\hat{\alpha})$ as the p-value directly for the regressions done at the state-year level as Conley and Taber suggest. For the individual level regressions, presented in the line above, I also follow the suggestion of Conley and Taber. The regression model

$$Y_i = \alpha d_{jt} + X'_{jt}\beta + \theta_j + \gamma_t + Z'_i\delta + \mu_i, \tag{4}$$

where Z_i are controls that vary at the individual level and μ_i are idiosyncratic errors, can be estimated as

$$Y_i = \lambda_{j(i)t} + Z'_i\delta + \varepsilon_i \tag{5}$$

$$\lambda_{jt} = \alpha d_{jt} + X'_{jt}\beta + \theta_j + \gamma_t + \eta_{jt} \tag{6}$$

where λ_{jt} are the individual state-year fixed effects and η_{jt} are errors. I estimate (5), use the estimates for λ_{jt} as the dependent variables in equation (6) and then calculate the p-value for each specification as in (3).

As a final robustness check, I compare the response of students who sent their test scores to at least one in-state college with those who did not. Sending a test score to an in-state college is endogenous (though as Section V shows, the TELS does not affect this outcome much). While the TELS may cause students who do not send their scores to any in-state colleges to increase their ACT score as a result of spillovers or uncertainty over whether they will want to attend an in-state college in the future, students who prefer to attend college in-state should have a larger response. This is the case. The coefficient from estimating (7) is 60% larger for students sending scores to in-state colleges. Adjusting for the distribution of test scores before the TELS shows even stronger results: the coefficients from the regressions restricted to students in predicted ACT ranges are four times larger for students sending scores in-state. For these students, the Kullback-Leibler divergences for 2004 are ten times larger than those for any other year, while for students who did not send any scores in-state, the divergences for 2004 are only 1.4 times larger than those in other years.

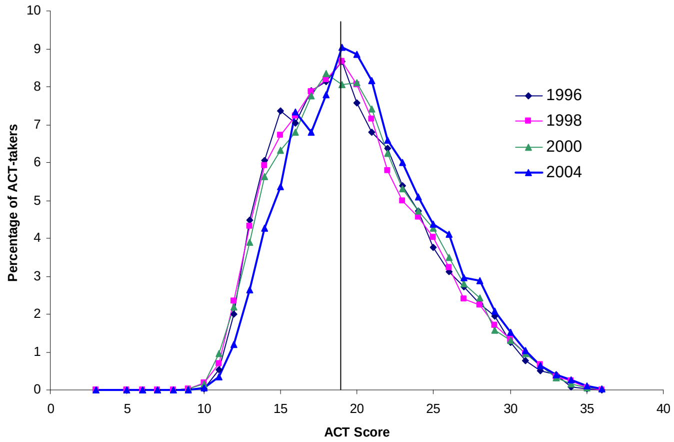
Table 1. College Choices of HOPE Scholarship Winners

Type of Institution	Number of Awards	Total Value
Two-Year Private	64	$119,000
Two-Year Public	4,827	$8,737,515
Four-Year Private	4,066	$12,799,297
Four-Year Public	14,330	$46,273,678
Total	23,287	$67,929,490

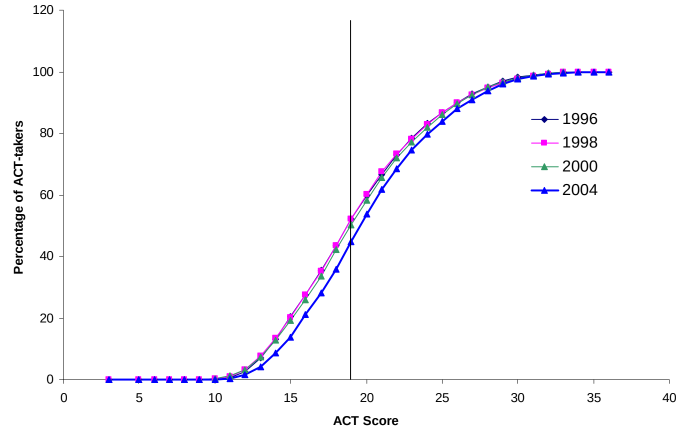
These figures only include students from the class of 2004. The data come from personal correspondence with Robert Anderson at the Tennessee Higher Education Commission.

Figure 1. Actual Distributions of ACT Scores

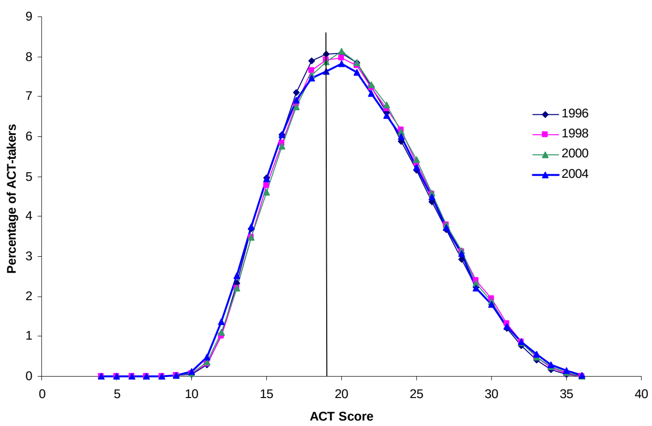
1a. Probability Distribution Function of ACT Scores in Tennessee



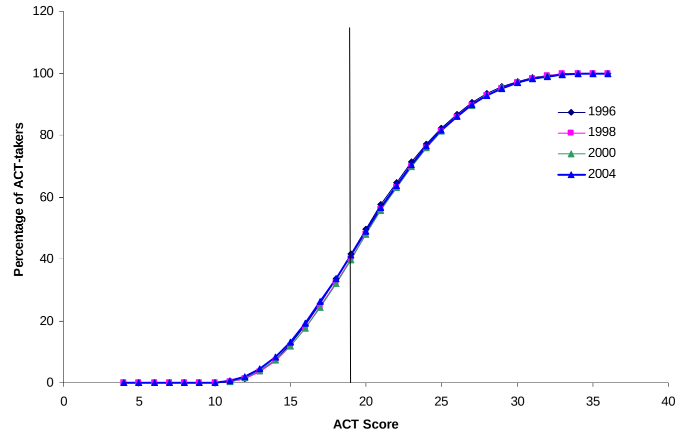
1b. Cumulative Distribution Function of ACT Scores in Tennessee



1c. Probability Distribution Function of ACT Scores in States Other than Tennessee



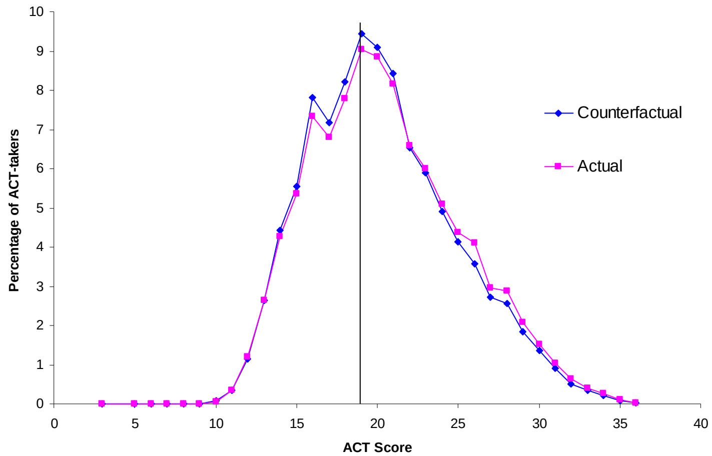
1d. Cumulative Distribution Function of ACT Scores in States Other Than Tennessee



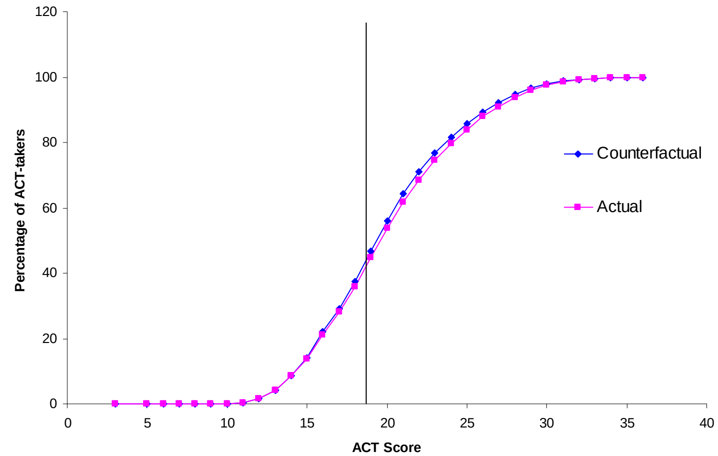
Data come from the ACT database. Vertical lines mark scores of 19.

Figure 2. Actual Distributions of ACT Scores Compared to Distributions Generated by Multivariate Reweighting

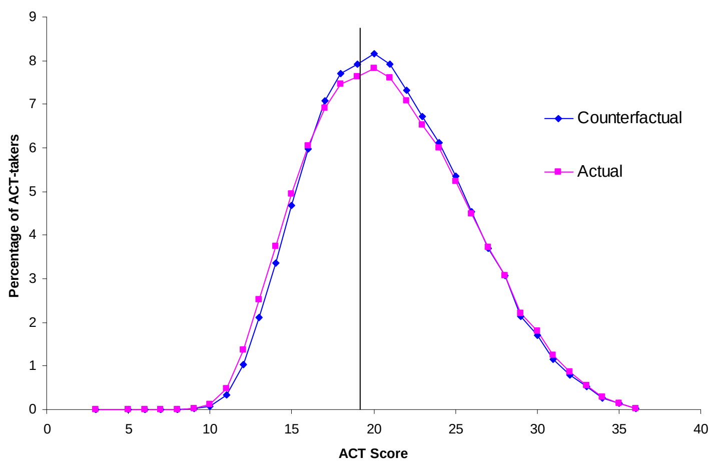
2a. Probability Distribution Function of ACT Scores in Tennessee



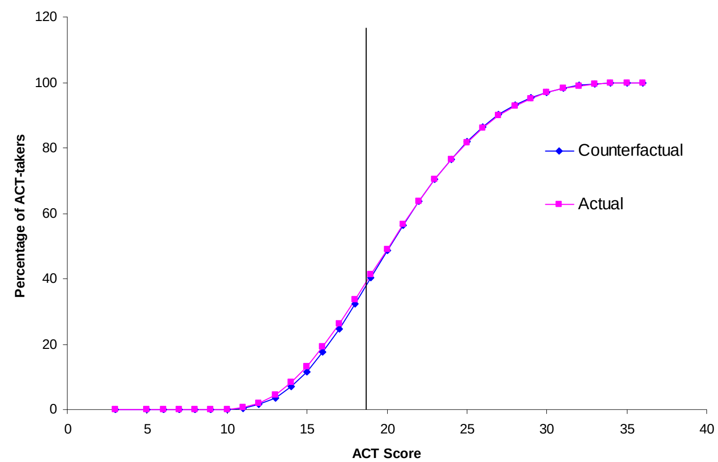
2b. Cumulative Distribution Function of ACT Scores in Tennessee



2c. Probability Distribution Function of ACT Scores in States Other than Tennessee



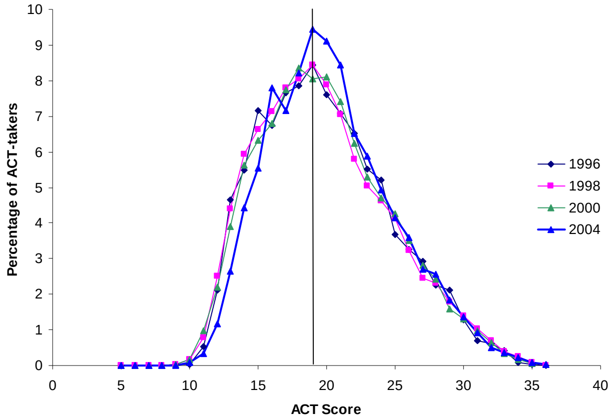
2d. Cumulative Distribution Function of ACT Scores in States Other Than Tennessee



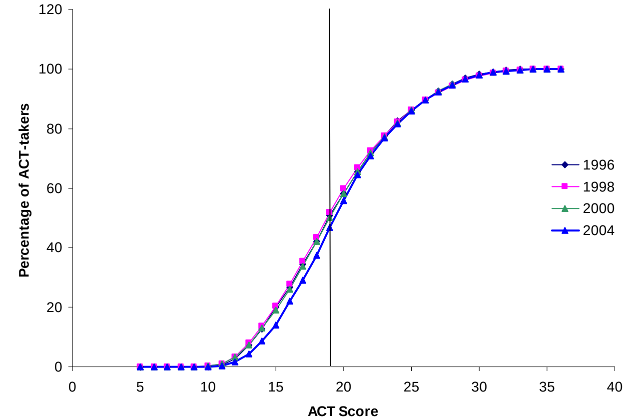
Data come from the ACT database. Vertical lines mark scores of 19. Reweighted distributions are produced using all of the controls described in footnote 9, state dummies (when applicable), and year dummies.

Figure 3. Distributions of ACT Scores Generated by Multivariate Reweighting

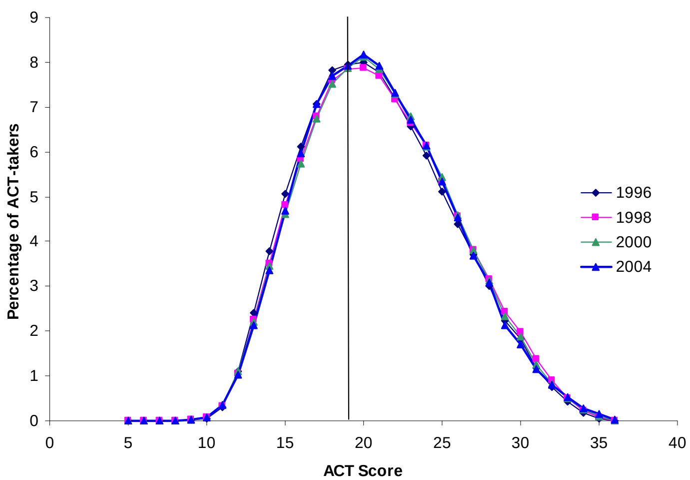
3a. Probability Distribution Function of ACT Scores in Tennessee



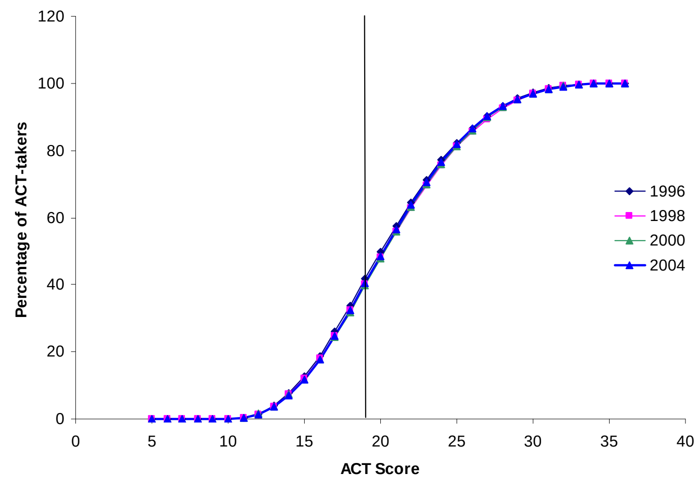
3b. Cumulative Distribution Function of ACT Scores in Tennessee



3c. Probability Distribution Function of ACT Scores in States Other than Tennessee



3d. Cumulative Distribution Function of ACT Scores in States Other Than Tennessee



Data come from the ACT database. Vertical lines mark scores of 19. Distributions are produced using all the controls described in footnote 9, state dummies (where applicable) and year dummies.

Table 2. Kullback-Leibler Divergences Between ACT Distributions

Year	Actual Distribution	DFL Distribution	DFL Distribution with Only Exogenous Controls
		A. Tennessee	
1996	0.007	0.008	0.007
1998	0.003	0.003	0.002
2004	0.019	0.017	0.017
		B. Other States	
Year	Actual Distribution	DFL Distribution	DFL Distribution with Only Exogenous Controls
1996	0.001	0.002	0.001
1998	0.000	0.000	0.001
2004	0.002	0.001	0.001

Each cell in Panel A gives the Kullback-Leibler divergence between the actual Tennessee 2000 distribution of ACT scores and the either the actual Tennessee distribution for the year indicated, the Tennessee distribution created using the DFL technique and all the control variables, or the Tennessee distribution created using the DFL technique and only the fully exogenous controls. Panel B is the same for all states other than Tennessee. Exogenous controls are state and year fixed effects, income dummies, race dummies, and five background variables. The additional controls are eight aspects of the student's academic history and four aspects of the student's extracurricular participation. The specific controls are listed in footnote 9. The data come from the ACT database.

Table 3. The Effect of the TELS on Scoring 19 or Higher on the ACT: OLS Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	% Scoring < 19 in 2000
			A. Aggregate Results				
Everyone	0.086 **	0.086 **	0.078 **	0.063 **	0.072 **	0.061 **	42%
	(0.005)	(0.011)	(0.008)	(0.007)	(0.004)	(0.006)	
			B. Race				
Black	0.045 **	0.045 **	0.033 **	0.021 **	0.027 **	0.018 *	75%
	(0.009)	(0.006)	(0.006)	(0.008)	(0.007)	(0.008)	
Asian	0.075 **	0.075 **	0.066 **	0.048 **	0.063 **	0.048 **	38%
	(0.029)	(0.021)	(0.022)	(0.015)	(0.019)	(0.015)	
White	0.081 **	0.081 **	0.080 **	0.064 **	0.074 **	0.062 **	34%
	(0.006)	(0.008)	(0.007)	(0.007)	(0.004)	(0.006)	
			C. Gender				
Male	0.100 **	0.100 **	0.092 **	0.071 **	0.085 **	0.069 **	42%
	(0.008)	(0.014)	(0.010)	(0.008)	(0.004)	(0.005)	
Female	0.075 **	0.075 **	0.068 **	0.056 **	0.062 **	0.055 **	42%
	(0.007)	(0.010)	(0.007)	(0.007)	(0.004)	(0.006)	
Clustered SEs	No	Yes	Yes	Yes	Yes	Yes	
Income, Race, and Background	No	No	Yes	Yes	Yes	Yes	
Academic	No	No	No	Yes	No	Yes	
Extracurricular	No	No	No	No	Yes	Yes	

Each cell in the first six columns of data gives the coefficient and standard error of a separate regression limited to the individuals indicated by the leftmost column. The dependent variable is an indicator for whether the student scored 19 or higher on the ACT. The bottom rows indicate the control variables included in the regression and whether standard errors are clustered. When standard errors are clustered, it is at the state-year level and when they are not, they are White's robust standard errors. All regressions include state and year dummies. The specific control variables corresponding to each category are listed in footnote 9. One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. The right-most column gives the percentage of test-takers in each subgroup scoring below 19 in 2000. Data come from the ACT database.

Table 4. The Effect of the TELS on Scoring 19 or Higher on the ACT: Probit Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	% Scoring < 19 in 2000
			A. Aggregate Results				
Everyone	0.080 **	0.080 **	0.070 **	0.060 **	0.070 **	0.060 **	42%
	0.228	0.228	0.228	0.208	0.213	0.205	
	(0.013)	(0.032)	(0.025)	(0.024)	(0.012)	(0.019)	
			B. Race				
Black	0.050 **	0.050 **	0.040 **	0.030 **	0.030 **	0.020 **	75%
	0.135	0.135	0.107	0.086	0.088	0.077	
	(0.027)	(0.018)	(0.019)	(0.026)	(0.021)	(0.027)	
Asian	0.070 *	0.070 **	0.060 **	0.050 **	0.060 **	0.050 **	38%
	0.209	0.209	0.210	0.172	0.205	0.174	
	(0.084)	(0.058)	(0.067)	(0.052)	(0.057)	(0.048)	
White	0.070 **	0.070 **	0.070 **	0.060 **	0.070 **	0.060 **	34%
	0.231	0.231	0.233	0.210	0.215	0.204	
	(0.018)	(0.025)	(0.022)	(0.022)	(0.010)	(0.018)	
			C. Gender				
Male	0.090 **	0.090 **	0.090 **	0.070 **	0.080 **	0.060 **	42%
	0.268	0.268	0.271	0.238	0.254	0.234	
	(0.020)	(0.038)	(0.031)	(0.026)	(0.013)	(0.019)	
Female	0.070 **	0.070 **	0.060 **	0.050 **	0.060 **	0.050 **	42%
	0.197	0.197	0.196	0.185	0.182	0.184	
	(0.018)	(0.027)	(0.020)	(0.022)	(0.012)	(0.020)	
Clustered SEs	No	Yes	Yes	Yes	Yes	Yes	
Income, Race, and Background	No	No	Yes	Yes	Yes	Yes	
Academic	No	No	No	Yes	No	Yes	
Extracurricular	No	No	No	No	Yes	Yes	

Each cell in the first six columns of data gives the average marginal effect, coefficient, and standard error of a separate regression limited to the individuals indicated by the leftmost column. The dependent variable is an indicator for whether the student scored 19 or higher on the ACT. The bottom rows indicate the control variables included in the regression and whether standard errors are clustered. When standard errors are clustered, it is at the state-year level and when they are not, they are robust standard errors. All regressions include state and year dummies. The specific control variables corresponding to each category are listed in footnote 9. One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. The right-most column gives the percentage of test-takers in each subgroup scoring below 19 in 2000. Data come from the ACT database.

Table 5. Effect of TELS on Students with Different Predicted Academic Ability

Predicted Score	Coefficient (s.e)		% Scoring < 19 in 2000	Coefficient/Fraction Scoring < 19	% Accurately Predicted
< 11	0.032 (0.016)	*	97.3%	3.290	8.2%
11 ≤ x < 13	0.006 (0.008)		98.0%	0.612	30.3%
13 ≤ x < 15	0.010 (0.006)		90.1%	1.110	34.3%
15 ≤ x < 17	0.053 (0.007)	**	78.7%	0.067	25.8%
17 ≤ x < 19	0.076 (0.012)	**	60.2%	0.126	22.2%

ACT scores are predicted by an OLS regression using observations from 1996, 1998, and 2000, all the controls listed in footnote 16, and state and year dummies. The results in the four data columns are limited to students predicted to score in the range indicated in the leftmost column. The first data column gives the coefficient and standard error (clustered at the state-year level) from an OLS regression. The dependent variable is an indicator for whether the student scored 19 or higher on the ACT; all controls listed in footnote 9 plus state and year dummies are included. The far right-hand column indicates the percentage of students predicted to be in that range in 1996, 1998, and 2000 who did score in the range (e.g. for students predicted to be in the 11 to 13 range, the percentage of students scoring 11 or 12). One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. Data come from the ACT database.

Table 6. Effect of the TELS on College Preferences and ACT Score-Sending:
Robustness Check, In-State vs. Out-of-State Colleges and Two-Year vs. Four-Year Colleges

	Aggregate	ACT ≥ 19	ACT < 19	ACT < 19 & GPA < 3.0	Mean in 2000 TN
	A. Robustness Check				
Total Scores Sent	-0.043	-0.081	-0.002	0.017	2.99
	(0.047)	(0.053)	(0.032)	(0.035)	
	B. In-State vs. Out-of-State Colleges				
Total In-State Colleges	-0.084	-0.097	-0.031	0.005	2.08
	(0.052)	(0.057)	(0.039)	(0.042)	
Any In-State College	-0.005 *	-0.004	0.000	0.002	0.79
	(0.002)	(0.003)	(0.002)	(0.002)	
Total Out-of-State Colleges	0.041 **	0.015	0.029 *	0.012	0.91
	(0.014)	(0.015)	(0.014)	(0.016)	
Any Out-of-State College	0.023 **	0.013	0.018 *	0.011	0.45
	(0.008)	(0.008)	(0.009)	(0.008)	
Prefer to Attend College In-State	-0.012	-0.021 *	0.001	0.011	0.72
	(0.007)	(0.010)	(0.006)	(0.006)	
	C. Four-Year vs. Two-Year Colleges				
Total 4-year Colleges	0.029	-0.033	0.064 **	0.098 **	2.58
	(0.034)	(0.043)	(0.021)	(0.023)	
Any 4-year College	0.002 *	0.000	-0.001	-0.003	0.83
	(0.001)	(0.000)	(0.002)	(0.003)	
Total 2-year Colleges	-0.073 **	-0.048 **	-0.066 **	-0.081 **	0.41
	(0.016)	(0.012)	(0.019)	(0.023)	
Any 2-year College	-0.039 **	-0.030 **	-0.020	-0.017	0.32
	(0.011)	(0.009)	(0.012)	(0.015)	
Prefer to Attend 4-year College	0.033 **	0.013 **	0.029 **	0.041 **	0.84
	(0.007)	(0.004)	(0.011)	(0.014)	

Each cell in the first four columns of data gives the coefficient and standard error of a separate regression on the dependent variable listed in the left-hand column. Each regression is limited to the individuals indicated by the column heading. The first four specifications in Panels B and C relate to scores sent whereas the last relates to stated preferences. All of the controls listed in footnote 9 as well as state and year dummies are included and standard errors are clustered at the state-year level. One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. The right-most column gives the mean of each variable for the sample of 2000 Tennessee test-takers. Data on score-sending, preferences, GPA, and ACT score come from the ACT database and data on the level and location of the colleges comes from the IPEDs.

Table 7. Effect of the TELS on College Preferences and ACT Score-Sending:
Four-Year In-state vs. Two-Year In-State Colleges and Tuition Preferences

	Aggregate		ACT ≥ 19		ACT < 19		ACT < 19 & GPA < 3.0		Mean in 2000 TN
A. Four-Year In-State vs. Two-Year In-State Colleges									
Total 4-year In-State Colleges	-0.016		-0.053		0.033		0.084	**	1.72
	(0.038)		(0.048)		(0.024)		(0.024)		
Any 4-year In-State College	-0.003		-0.006	*	0.000		0.003		0.76
	(0.002)		(0.003)		(0.002)		(0.003)		
Total 2-year In-State Colleges	-0.068	**	-0.043	**	-0.064	**	-0.080	**	0.36
	(0.015)		(0.010)		(0.019)		(0.023)		
Any 2-year In-State College	-0.037	**	-0.029	**	-0.018		-0.014		0.29
	(0.011)		(0.008)		(0.013)		(0.015)		
Prefer 4-year In-State College	0.012	**	-0.009		0.015	**	0.033	**	0.58
	(0.003)		(0.007)		(0.005)		(0.008)		
B. Tuition									
Preferred Maximum Tuition	251.8	**	109.1		129.7	*	53.5		$ 4,016.97
	(31.6)		(61.0)		(56.6)		(54.5)		
Average Tuition of Colleges Scores Sent To	235.8	**	76.0		227.3	**	266.0	**	$9,141.88
	(62.9)		(42.7)		(46.1)		(52.2)		

Each cell in the first four columns of data gives the coefficient and standard error of a separate regression on the dependent variable listed in the left-hand column. Each regression is limited to the individuals indicated by the column heading. The first four specifications in Panel A relate to scores sent whereas the last relates to stated preferences. All of the controls listed in footnote 9 as well as state and year dummies are included and standard errors are clustered at the state-year level. One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. The right-most column gives the mean of each variable for the sample of 2000 Tennessee test-takers. Data on score-sending, preferences, GPA, and ACT score come from the ACT database and data on the level, location, and tuition of the colleges comes from IPEDs. Tuition is the in-state tuition in 2004-05 if the student lived in the same state as the college and the out-of-state tuition in 2004-05 if the student lived in a different state.

Appendix Table 1. Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Residual Regressions			
Basic Specification	-0.270	-0.270	-0.254 **	-0.298	-0.269	-0.295
	(0.344)	(0.344)	(0.000)	(0.193)	(0.173)	(0.173)
Including State Time Trends	-0.403 **	-0.403 **	-0.427 **	-0.496 **	-0.443 **	-0.502 **
	(0.075)	(0.075)	(0.082)	(0.081)	(0.088)	(0.083)
			B. Individual-Level Regressions			
Including State Time Trends	0.085 **	0.085 **	0.075 **	0.074 **	0.070 **	0.074 **
	(0.011)	(0.014)	(0.011)	(0.013)	(0.010)	(0.013)
Clustering at State Level	0.092 **	0.092 **	0.078 **	0.063 **	0.072 **	0.061 **
	(0.021)	(0.021)	(0.011)	(0.008)	(0.005)	(0.005)
			C. State-Year Regressions			
Basic Specification	0.076 **	0.076 **	0.049 **	0.042 **	0.054 **	0.048 *
	(0.006)	(0.006)	(0.008)	(0.015)	(0.008)	(0.019)
Including State Time Trends	0.078 **	0.078 **	0.075 **	0.059	0.053 *	
	(0.013)	(0.013)	(0.018)	(0.035)	(0.025)	
			D. State Pre/Postperiod Regressions			
Basic Specification	0.076 **					
	(0.005)					
			E. Conley-Taber P-values			
Individual-Level Regressions	0.043	0.043	0.043	0.000	0.000	0.000
State-Year Regressions	0.043	0.000	0.000	0.000	0.000	0.000
Clustered SE's	No	Yes	Yes	Yes	Yes	Yes
Income, Race, and Background	No	No	Yes	Yes	Yes	Yes
Academic	No	No	No	Yes	No	Yes
Extracurricular	No	No	No	No	Yes	Yes

Each cell in the first four panels gives the coefficient and standard error of a separate regression, the specification of which is indicated by the leftmost column. In panels B, C, and D the dependent variable is an indicator for whether the student scored 19 or higher on the ACT. In Panel C the data is collapsed to the state-year level whereas in Panel D the data is collapsed to a pre-TELS and post-TELS observation for each state. Cells in Panel C and D are left empty when the model is not identified. In, Panel A, the dependent variable is the average state residual from estimating equation (7) using the controls indicated by the column and the independent variables are the average residual from the previous year and a constant. Panel E computes Conley-Taber.(2006) p-values which correct for the fact that I only analyze one policy change. Their construction is explained in Appendix B. The bottom rows indicate the control variables included in the regression and whether standard errors are clustered. When standard errors are clustered, it is at the state-year level and when they are not, they are White's robust standard errors. The specific control variables corresponding to each category are listed in footnote 9. One asterisk indicates the result is significant at the 5% level and two asterisks indicate the result is significant at the 1% level. Data come from the ACT database.