The Promise of the Tennessee Promise Program*

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This is an excerpt from my Master's thesis while I was at Columbia University. Please find the complete version of this work at bit.ly/3Yp7zTn.

Abstract

This paper evaluates the effects of a free-college promise program enacted by the state of Tennessee on college outcomes of enrollment and graduation rates. I employ a triple difference strategy to correct for endogeneity, thus exploiting variation by state, time, and type of institution. The model utilizes a control group of colleges in neighboring states and ineligible colleges within the state and examines both the immediate impact and the persistent effects up to four years after the program's implementation. I find that there are significant positive effects on enrollment, but graduation rates are unaffected. More interestingly, I find that the program does not impact the composition of the student population by race and gender. Finally, I find evidence of spillover effects on 4-year public colleges that are not persistent.

Keywords: Tennessee Promise, higher education, financial aid, community college
JEL codes: I12, C1

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1 Introduction

It is widely known that demand for higher education varies greatly with the price of education. An exogenous reduction in the cost of higher education can raise the demand for higher education when all else remains constant (Becker, 1994). Much of recent higher education policy has been focused on improving access to higher education, and tuition-free programs are becoming synonymous with this goal. Specifically, several such programs have been directed at public community colleges. The model of community colleges was first designed to serve as an easy-access option for students who wished to pursue post-secondary education (Rouse, 1995). It is, therefore, not surprising that "College Promise Programs" in community colleges have become immensely popular. [1]

The Tennessee Promise Program was one of the first state-wide policies implemented to provide last-dollar scholarships to high school students graduating from the state of Tennessee. Following the success of this program, and others more generally, states such as Arkansas, Kentucky, and New York launched their own Promise Programs in the following years. Thus, it becomes imperative that such programs' effects be studied thoroughly and the generalisability be evaluated carefully. It is also important to be mindful of the potential spillover effects of such a program on other types of colleges in the state.

In this paper, I estimate the effects of the Tennessee Promise Program on college outcomes of enrollment and graduation rates. The Tennessee program is novel owing to its universal eligibility for students and its last-dollar nature. It thus serves as an ideal setup for quasi-experimental design. I can estimate the effect immediately following the launch of the program and the sustained effect for up to three years later. I use aggregated institution-level data to study the general impact of this program. This is useful as the program is directed at the whole population of high school graduates and does not come with any eligibility criteria based on financial need or merit.

Studying such a program's impact is valuable for many reasons. First, it is unclear

[1] I use the phrases community college and 2-year public college interchangeably.

whether such a policy generates substantial externality effects on ineligible colleges. Second, the impact of aid programs on degree completion and graduation outcomes is understudied. Third, the universal nature of the program sets it apart from other well-studied merit or financial need-based aid policies. The universality also questions whether the program achieves the desired results regarding distributional and equitable effects.

I employ a triple-difference or a difference-in-difference-in-differences (DDD) strategy that exploits the eligibility criteria for colleges and the exogenous timing of the intervention, as well as similar outcomes observed in a set of neighboring states (Gruber (1994), Chetty, Looney, and Kroft (2009)). This approach is more suitable as the assumptions required for identification are weaker than those required for the regular DD estimate. I also perform sensitivity checks to assess the robustness of these estimates to various specifications and sub-samples.

The results indicate that there have been significant and persistent effects on overall enrollment numbers. There is also a rise in enrollment for students from minority racial categories. However, there is no evidence of any change in the composition of the student population by race. The program also appears to have had a small, negative effect on the number of graduates. Finally, I test for spillover effects this program may have on other public colleges. I find evidence of a small but non-persistent effect on enrollment whereby enrollment in 4-year public colleges experiences a decline in the year immediately following the program's launch.

There are five related themes of literature that are relevant to this work. The first theme concerns the role of aid programs in improving enrollment and college attendance. Theory predicts that as the cost of attending college falls, there will be an increase in college attendance and enrollment. With this backdrop, financial aid is likely to have an impact on college enrollment decisions. However, evidence from extensive existing research has been mixed. Much of this research focuses on the impact of need-based and merit-based programs, most of which are grant-based. These studies are often confounded by unobservable factors as aid is not randomly assigned in these contexts.

In the case of need-based programs, the Pell Grant is one of the most widely studied interventions. Kane and Rouse (1995) finds that the introduction of the Pell had no impact on the college enrollment rate for low-income students. Dynarski (2003) examines the elimination of a Social Security benefits program and concludes that college attendance for the targeted group fell by more than a third with the removal of the aid. Cornwell, B. Mustard, and J. Sridhar (2006) find that the HOPE scholarship raises college attendance by 4 to 6 percentage points per 1000$ in aid. Dynarski (2000) reports that Georgia's HOPE program increased enrollment by seven percentage points and that the impact is greater for white students. This is likely due to the merit-based nature of the scholarship favoring higher-income students. Dynarski (2004) found that scholarship programs in a dozen states have positively affected college attendance that favored nonwhites.

Deming and Dynarski (2009) conclude that the effects of financial aid programs are susceptible to the nature and design of the intervention and that there may be a trade-off between targeting and effectiveness.

The mixed evidence could be explained by imperfect information and credit constraints (Scott-Clayton, 2011). Students are more likely to learn about reductions in tuition fees and the cost of attendance than about the introduction of specific financial aid schemes. Further, complex eligibility criteria and application processes could be an obstacle for students to enroll even without borrowing constraints. Scott-Clayton (2011) also draw attention to the requirement of submitting a Free Application for Federal Student Aid (FAFSA). The complexity of this requirement has been discussed amply in Dynarski and Scott-Clayton (n.d.); it acts as a barrier for low-income students and disproportionately burdens them.

Second, the effect on completion outcomes has yet to be surveyed much. Despite large economic returns to completion, many students fail to complete degrees often due to high costs, and financial aid has the potential to boost these completion rates. According to Denning (2017), there are both intensive and extensive margins through which completion can be affected. Cohodes and Goodman (2014) study the impact of the merit-based Adams scholarship to find that college quality affects completion rates. They also find that the scholarship impacts enrollment but not graduation rates for the most disadvantaged students of the cohort. Scott-Clayton (2011) finds that the West Virginia Promise, a merit-based program, increased the probability of obtaining a bachelor's degree in four years. Bartik, Hershbein, and Lachowska (2017) survey the impact of the Kalamazoo Promise on enrollment and completion. They find that the Promise raised the percentage of students earning any credential by ten percentage points, three-fourths of which was due to students earning a bachelor's degree. Bozick, Gonzalez, and Engberg (2015) evaluated the effect of the Pittsburgh Promise and concluded that the program had no effect on enrollment but increased the probability of attending a 4-year college.

The third theme is research specific to community colleges. There needs to be more research on the impact of aid programs on community college outcomes. Belfield and Bailey (2011) provide a review of literature that focuses on returns to a community college education. Jepsen, Troske, and Coomes (2014) find that associate's degrees have quarterly returns of about \$1500 for men and \$2400 for women, and for both women and men, certificates have a quarterly return of about \$300.

The community college student population is relatively more diverse than other schools. Community college students are more likely to be from disadvantaged families than four-year college students. Welch (2014) studies the merit-based HOPE program in Tennessee and its effects on community college student outcomes both during and after college. The paper finds that the reduction in the cost of community college does not impact persistence, completion, or expected earnings for marginally eligible students. Likely explanations for this finding were that scholarship programs usually came with renewal requirements which could cause some students to lose their scholarship, and that the Pell Grant often supplemented the HOPE grant. Hence, the average value of the HOPE scholarship was not substantial. Further, dropout rates are high in community colleges Deming and Dynarski (2009).

Fourth, there is a concern about the indirect effects of programs targeted at community colleges on other schools. Rouse (1995) talks of the “democratization” effect of community colleges. This refers to the effect of students enrolling in community college who would not otherwise have attended any post-secondary institution. On the other hand, there is concern that community colleges divert students. This debate has important policy implications. If enrolling in a community college deters a student from completing their 4-year degree, then the role of free community college aid programs has to be re-examined. This analysis is tricky because of the inherent selection bias. Students who begin in community colleges differ from their counterparts in 4-year institutions. Observable factors, such as test scores, and unobservable factors, such as ability and motive, cause these differences. Doyle (2009) uses propensity score matching to address selection bias in estimating treatment effects and concludes that shifting to community colleges can lower overall graduation rates. Using the information on students’ desired years of schooling, Leigh and Gill (2003) find that community colleges increase educational attainment for those who wish to earn a bachelor’s degree. In the context of such contradictory evidence, evaluating the spillover effects of a program such as the Tennessee Promise is crucial.

Finally, there needs to be more literature on place-based scholarships, such as the Tennessee Promise. These programs do not have eligibility criteria per se and depend on the state of residency or high-school graduation. The Kalamazoo Promise is one example. Carruthers and Fox (2016) study the role of Knox Archives, a free community college program in Tennessee before the Promise, and find large impacts on high school graduation and enrollment. Such programs are different in that they are not targeted at specific subpopulations. On the one hand, this makes applying and enrolling simpler and more uniform, thus encouraging participation. On the other hand, such a program may carry exclusion errors because it may fail to benefit the more disadvantaged groups. Several works report that the demand elasticity of higher education is greater for low-income families. (Manski and Wise (1983), Radner and Miller (1970)) The paper most closely related to mine (Nguyen, 2020) uses generalized synthetic control and a standard difference-in-difference estimation to find significant increases in enrollment due to the Tennessee Promise.

The rest of this paper is organized as follows. Section 2 provides background about the data used and details of the Tennessee Promise Program. Section 3 discusses the empirical strategy that I use in the paper. Section 4 presents the results of the various analyzes. Finally, I discuss the results and conclude the paper in section 5.

2 Data description

The data used in this paper come from the Integrated Postsecondary Education Data System maintained by the National Center for Education Statistics (IPEDS) within the United States Department of Education. The dataset provides information for several variables aggregated at the institution level based on surveys completed by participating colleges. The dependent variables of interest in my model are measures of enrollment, enrollment by divisions of gender and race, and graduation rates. For enrollment by race categories, I consider percentages of students belonging to each of the race categories of Black, White, Hispanic, and Asian. All enrollment figures correspond to full-time, first-time undergraduate students, as the Tennessee Promise Program targets this group. I use data for the years starting from 2010 up to 2018. This provides a reasonably long period to explore the program's immediate treatment effects and the medium-run effects up to three years later.

The sample includes all institutions in Tennessee and nine neighboring Southeast states, forming the control group. These are Alabama, Arkansas, Delaware, Georgia, Kentucky, Mississippi, Missouri, North Carolina, South Carolina, and Virginia. All colleges have been classified based on whether they are public or private and whether they offer bachelor's degrees. All colleges in the sample offer some degrees or certificates and enroll undergraduate students. Colleges with only above the baccalaureate level of study are excluded, as are those that do not offer any certification. The classification groups are labelled as "Public, 2-year", "Public, 4-year", "Private, 2-year", and "Private, 4-year". These labels are only for convenience, as students may take longer to obtain these degrees. In essence, 2-year colleges refer to technical or community colleges eligible for the Tennessee Promise program.

In 2014, the Tennessee General Assembly signed the Tennessee Promise Program into law. The program acts as a last-dollar scholarship that all recent high school graduates are eligible for in order to complete an associate's degree or certificate program at a public community college. The scholarship is also offered for select 2-year programs in a few private colleges. However, in this case, the scholarship is not last-dollar in that it will only cover amounts equivalent to that paid to public colleges on average. Students must be enrolled as full-time undergraduate students immediately after high school graduation, i.e., students who choose to defer postsecondary education for any period will not be eligible to apply. The "last-dollar" nature of the scholarship implies that the program finances any remaining tuition and other mandatory fees once students have received all other grants and aid. The procedure is such that the Pell Grant is applied first, followed by other state grants. Once these federal and state gifts are applied, the Tennessee Promise funds the balance. Thus the amount of assistance depends on the other tuition aid a student receives.

Interested high school graduates must fill out an online application and apply to the community colleges of their choice. They are also required to complete FAFSA verification. The application process assigns mentors to students for assistance with completing forms and selecting potential college choices.

If selected for the scholarship, students can receive Promise funds either until they earn their degree or certificate or for five semesters, whichever occurs earliest. In order to remain eligible to receive the scholarship in subsequent years, they are required to renew their FAFSA, maintain good academic standing with a GPA of at least 2.0, and complete 8 hours of community service each semester.

3 Empirical Strategy

The primary empirical strategy used in this paper exploits the exogenous variation driven by the introduction of the Tennessee Promise Program. The ideal natural experiment would be to observe potential outcomes with and without the treatment for eligible public colleges. Then one could difference the pre-treatment and post-treatment outcomes for treated colleges. Here, the post-treatment period begins in 2015. This simple difference, however, will be confounded by overall time trends in factors that affected all colleges in Tennessee. Similarly, a difference in post-treatment outcomes between treated colleges in Tennessee and colleges in other states will be biased by unobservable systematic variations across states.

First, I use a basic difference-in-differences strategy to evaluate the program's impact on the desired outcome variables. I examine changes in enrollment and graduation rates in public community colleges in Tennessee across time and survey the potential effects of the program.

$$Y_{it} = \gamma_i + \alpha_t + \delta_1(\mathrm{TT}_i \times (D_t = 2015)) + \beta X_{it} + \delta_2(\mathrm{TT}_i \times \mathrm{Post}_t) + \varepsilon_{it} \tag{1}$$

where Y is the outcome of interest for college i in year t. D_t refers to an indicator that takes the value of 1 for 2015, and Post_t refers to an indicator variable for the years 2016 through 2018. TT_i indicates whether the college is a 2-year public college in Tennessee. δ_1 captures the short-term effect of the program in the year immediately after the announcement, and δ_2 captures the medium-term effects of the program from 2016 to 2018. I include a vector of college-fixed effects γ_i and year-fixed effects α_t. I also include a set of controls related to federal assistance in the form of total loan amounts awarded to students in a given year. These time-varying factors could potentially explain some of the variation in outcomes. The underlying assumption is that they are not affected by the intervention itself. This is likely to be true as the program is designed such that the scholarship depends on federal grants received first.

The control group comprises three mutually exclusive groups of ineligible colleges within Tennessee- 4-year public colleges and 2-year and 4-year private colleges. The key threat to identification here is the potential effect the program may have had on ineligible institutions, i.e., externalities may bias the program's impact on public community colleges. Figure 4 suggests that there may have been substitution within public colleges where students who would have otherwise chosen to enroll in 4-year public colleges decided to enroll in 2-year public colleges instead.

To use the simple DD framework, I could use a second control group comprising colleges in neighboring Tennessee states. I use eight states as control states. This specification would alleviate the externality problem from within state substitution. Since state residency is one of the key criteria for eligibility in the program, the potential migration of students from other states into Tennessee community colleges due to the intervention is not a source of concern. However, the parallel trends assumption is crucial here. The assumption requires that the trends in outcomes should have been the same in the absence of the treatment. I note that this assumption is not testable. Using pre-treatment trends provides partial support to this assumption. However, it is still unclear whether other programs may have been launched in control states that could have affected college outcomes.

Therefore, I choose to employ an alternate strategy that relies on weaker assumptions and potentially corrects for endogeneity problems that may arise from using the two control groups above. Following Gruber (1994) and Chetty et al. (2009), I implement a difference-in-difference-in-differences (DDD) strategy, also known as a "triple difference" strategy. I compare treatment colleges in Tennessee to a set of control colleges in Tennessee and measure the change in relative outcomes relative to the control states. The underlying assumption for identification here is weaker than the usual parallel trends assumption. It requires that there was no shock during the intervention that affected outcomes of only public community colleges in Tennessee differentially. Since no other targeted policies were launched simultaneously in Tennessee, I believe this condition is likely to be satisfied.

I examine the robustness of the DDD estimate by estimating a series of regression models with different sets of covariates and specifications. The following linear model presents the basic structure of the linear model:

$$\begin{aligned} Y_{it} = \gamma_i + \alpha_t + \beta x_{it} + \delta_1(\mathrm{TN}_i \times (D_t = 2015) \times \mathrm{TT}_i) + \delta_2(\mathrm{TN}_i \times (D_t = 2015)) + \\ \delta_3(\mathrm{TT}_i \times (D_t = 2015)) + \delta_4(\mathrm{TN}_i \times \mathrm{TT}_i) + \delta_5 \mathrm{TN}_i + \delta_6(D_t = 2015) + \\ \delta_7 \mathrm{TT}_i + \mu_1(\mathrm{TN}_i \times \mathrm{Post}_t \times \mathrm{TT}_i) + \mu_2(\mathrm{TN}_i \times \mathrm{Post}_t) + \\ \mu_3(\mathrm{Post}_t \times \mathrm{TT}_i) + \mu_4(\mathrm{TN}_i \times \mathrm{TT}_i) + \mu_6(\mathrm{Post}_t) + \varepsilon_{it} \end{aligned} \tag{2}$$

The set of δ coefficients captures short-term effects, and μ coefficients capture the medium-term effects. The third-level interactions δ_1 and μ_1 are the DDD estimates when there are no additional covariates in the model. To adjust for any potential serial correlation between standard errors within the college, I cluster my standard errors at the college level.

Table 2 presents the regression estimates. Column 1 of Table 2 estimates the regression of total enrollment on the set of indicators, including year and college fixed effects. In column 2, I estimate the analogous model using log enrollment. Column 3 adds covariates of federal loan amounts. The estimate does not change significantly. Finally, column 4 restricts the sample to 2-year public colleges only and compares enrollment across time and states. The estimate is close to the DDD estimate from column 3. In addition, I perform several robustness checks using different specifications and present the results in the appendix.

Next, I estimate the same equation with enrollment outcomes by race and gender. The percentages are calculated as the number of full-time, first-time undergraduate students belonging to the race or gender group of interest divided by the total enrollment of full-time, first-time undergraduate students. I include the set of covariates related to federal aid and college and year-fixed effects in all specifications. I estimate the effects for four race categories- Black, White, Hispanic, and Asian. I study these categories as they form the dominant share of total enrollment. I estimate both the immediate as well as medium-run effects of the program on these outcomes.

Finally, I present a regression analysis of graduation rates. For this, I restrict my sample to 2-year colleges in Tennessee and the control states and perform a DDD analysis. The primary reason I do this is that the graduation rate for students seeking associate's degrees is not comparable to those seeking a bachelor's degree. The entering cohorts of bachelor's degree-seeking and associate's degree-seeking students will be different due to the differences in the average time for completion. For 2-year colleges, I use graduation rates for the entering cohort of 2015, the first cohort after the program launch, and consider this cohort to be the "treated" group. However, the data for the 2015 cohort of bachelor's degree-seeking students is unavailable. I use graduation data from the four previous cohorts for the pre-treatment periods. In the appendix, I present results from robustness checks with additional pre-treatment periods.

$$GR_{it} = \gamma_i + \alpha_t + \delta_1(\mathrm{TN}_i \times (D_t = 2015)) + \delta_2(\mathrm{TN}_i \times \mathrm{Post}_t) + \beta X_{it} + \varepsilon_{it} \tag{3}$$

Graduation rates are available for students who graduate within 100%, 150%, and 200% of the normal time for completion, which is two years in this case. Table 5 presents the estimates from this analysis. I evaluate the effect on the total number of graduates and the number of graduates as a percentage of the cohort size. Standard errors are clustered at the college level, and the models include fixed effects for the year and college.

A key identifying assumption for this specification is the usual parallel trends assumption. To substantiate this, I replicate the regression specification with an indicator for the two cohorts before the 2015 cohort. I also include the second-level interaction of this pre-treatment indicator in the estimation. This acts as a placebo test. The estimates for the cohorts entering before the cohort of 2015 are close to zero and not statistically significant, thus validating the parallel trends identification assumption.

4 Results

4.1 Descriptive Statistics

Table 1 presents summary statistics on total enrollment, enrollment by race, and enrollment by gender across both control states and Tennessee, in both eligible and ineligible colleges. The statistics are means computed across years for each category of colleges in both treatment and control states. On average, 2-year public colleges have higher enrollment than other control institutions in both sets of states. It must be noted here that these are unweighted means, and the higher means for eligible colleges could be due to the smaller sample sizes.

Panel B presents statistics of enrollment by race. Whites form a majority of the student population across all groups. The population of White students is almost three times that of the next largest subpopulation of Black students. Hispanic and Asian students form a very small part of the total population. The disparities in racial representation in colleges appear consistent across all four groups.

Panel C presents total enrollment by gender. These figures appear consistent with national averages. Females form the majority in all four groups. The gender gap is larger (around 30 %) in ineligible colleges in both Tennessee and the neighboring control states.

4.2 Enrollment

Table 2 presents results from the regression equation (2). Column 1 shows coefficients on total enrollment. The effect of the treatment given by the short-term DDD estimate is of the order 267.5 and is statistically significant. Column 2 uses the log transformation of total enrollment as the outcome variable and is the better form. The result shows that the treatment has led to a rise in total enrollment by around 38%. Adding covariates increases the coefficient marginally, but it remains significant. All three specifications show that the program's introduction resulted in an immediate increase in enrollment in public community colleges in Tennessee of just around 40%. The delayed effect is also significant across specifications, with point estimates close to that of the immediate effect.

In column 4, I restrict the sample to 2-year public colleges only. The resulting DD estimate is similar to the DDD estimate. This suggests that the placebo effect is zero, and the estimate is likely unbiased.

Next, I present results from the same regression analysis studying the effect on the composition of enrolled students by race and gender. The specifications in Table 3 include regressions with both enrollments in total and as a percentage of the student population as outcome variables. There is no significant change in the percentage of enrollment by race categories for any of the four races. There is a significant increase in raw enrollment counts for White, Black, and Asian students. The highest short-term increase is for Whites. The percentage increase for Whites is positive but insignificant, while the same is negative for the other three races.

In the following years, percentage change remains near zero while enrollment numbers increase significantly. The delayed effects are larger for Black and Hispanic students than the short-run effects. Overall, the program has not affected the racial composition of the student population. However, the increase in students enrolling in public community colleges is significant for all four race categories. It has been persistent even for years after the program's immediate launch.

Table 4 contains estimates of the regression analysis for enrollment by gender. Once again, the results are significant for enrollment numbers but not for percentages. The effect of the program is more prominent for males than for females. There is an increase in the number of male students enrolled of the order of 55$ compared to 45% for female students. The effect is persistent even in the years following 2015. The effect for female students is, however, smaller. Regression of composition by gender yields estimates close to zero.

Robustness Checks I also perform a series of robustness checks and provide the results in the Appendix. Table A.1 contains results from the same regression specifications as in Table 2, and I include a set of pre-treatment dummies for three years before the program's launch. I also add their second-order and third-order interactions. The reported coefficient estimates are the third-level interactions for three years before, the same year, and three years after the program's launch. Consistent with the results in Table 2, the coefficient from specification (3) is 0.374, which is very close to that of 0.358 in Table 2. The corresponding "placebo" estimates for the pre-treatment periods are close to zero in all four columns, indicating that the rise in enrollment coincides with the program's launch.

I conduct similar tests for the robustness of regressions of gender and racial enrollment shares. Table A.2 describes the results from two different robustness checks. Columns 1-4 are DD estimates from the regression of the log of enrollment by race, with the sample restricted to eligible colleges only. I include year-fixed effects, covariates, and college-fixed effects. These are the basic DD estimates without the third unit of difference, i.e., the type of college. The estimates are similar to those obtained in Table 3, indicating that the placebo effect is close to zero. The DD estimates are similar to the DDD estimates. I also perform the same pre-treatment placebo tests and report the estimates in columns 5 - 9.

For gender, I rerun the same robustness tests with the treated group alone and the pre-treatment indicators. The results are similar to those in Table 4. This provides supporting evidence that the placebo effects are not significant.

4.3 Graduation Rates

In Table 5, I present the results from equation (3). This is a DDD model with only 2-year colleges in the sample. The control group comprises community colleges in control states. I examine the program's effect on the number of graduates and the graduation rate. For this analysis, I restrict the sample to the entering cohort of 2015 across all colleges. I use three measures of graduation rates that are dependent on the time taken to graduate. Given that these are associate's degrees/certificates, the expected time to complete the course is two years. Thus within 100% is a maximum period of 2 years, within 150% is a maximum period of three years, and within 200% is a maximum period of four years.

There are significant effects of the program on both the total number of graduates and the graduation rate. There is an increase in the number of students who graduate within

150% of the expected time of around 48% relative to the control group. The effect is larger for longer graduation periods but remains negative. In terms of graduation rates, the effect is negative but insignificant for all, and the point estimates are close to zero.

Table A.4 presents robustness results from the analysis of graduation rates. Once again, I include a set of pre-treatment indicators. The point estimates are similar to those in the main specification. The results are, therefore, robust.

4.4 Potential Spillovers

Figure 4 motivates one to think about the externalities that this program may have had. In particular, students may have been deterred from enrolling in 4-year public colleges and driven to enroll in 2-year colleges instead. Such a spillover effect may have considerable effects on college outcomes.

I use a basic DD framework here with a sample of the only public, 4-year colleges. 4-year public colleges in the control states form the control group. The effects are not highly significant. The number of students enrolled in Tennessee community colleges reduced significantly in absolute terms by around 199 points. In terms of percentage change, the point estimates are negative and significant at the 10% level, but they are small. Moreover, this effect is not persistent. Analyzing the coefficient on the long-term indicator, the point estimates are too small with very large standard errors.

5 Discussion & Conclusion

I find sizeable gains in enrollment following the launch of the Tennessee Promise. Enrollment increased by around 40% in 2015 and around 38% in the years after, relative to the years before 2015. The immediate effect can be attributed to the announcement made in 2014 and the immediate availability of funds in 2-year public colleges. Further, the program was promoted extensively to garner adequate attention in 2014. This is a reasonable explanation for the remarkable increase in enrollment numbers. Another critical aspect is the mentoring program. Given that filing FAFSA is a requirement, the mentorship program has potentially helped students deal with the complex process and fostered greater participation. The effect remains persistent for up to three years later as well.

One concern about the design of a universal program such as this is the distributional effect. My results show that enrollment of Black, Hispanic, and Asian students did increase. However, there was no significant effect on the population's racial composition. The effect on enrollment was disproportionately greater for White students. The result holds even in the following years. This is consistent with concerns that the least disadvantaged groups may not be the ones benefiting from such a program. Moreover, this program is a last-dollar aid program. Thus most of the gains are likely to be made by the marginal student. Students from the most disadvantaged backgrounds are likely to be someone other than the ones who are moved to attend college due to such a program. I need more granular data to carry out this analysis, but there is much scope to expand research in this direction.

The program has had a greater impact on males than on females. This differs from other results that look at differential gendered impacts of college aid (Dynarski and Scott-Clayton, n.d.). I attribute this to the universal nature of the scholarship. Merit scholarships are likely to favor female students due to their better learning outcomes as measured by grades. Similarly, previous evidence on the relative elasticity of male and female college attendance shows higher elasticity for females (Card and Lemieux, 2001). However, these are mainly in the context of need-based scholarships, which target the most disadvantaged students. Bartik, Hershbein, and Lachowska (2017) also find stronger effects of the Kalamazoo Program on women. The Kalamazoo Promise was a first-dollar scholarship. Once again, I emphasize the last-dollar nature of the Tennessee Promise that separates this program from the others commonly surveyed in the literature. Perhaps this means that more male students are on the margin of their decision to attend college. However, I cannot test this.

I also document that graduation rates were not affected significantly, but they had negative small point estimates. This is consistent with findings from Doyle (2009). However, the test does not give me adequate power to validate the finding. I also do not have adequate data to evaluate the effects for later cohorts.

A final concern is that there appears to be some evidence for substitution. In particular, the reduction in enrollment in 4-year public colleges is significant in the immediate year after the intervention but not in the years after. This is likely due to the large-scale promotion of the program in 2014, which might have yet to be sustained. Figure 2 shows that enrollment eventually picked up in 2016. It may also be possible that there were no new undergraduate entrants, but there was merely a substitution within colleges in Tennessee. Using the DDD framework helps uncover that this was not the case. Figure 2 also shows that total enrollment figures across all colleges in Tennessee did increase, indicating that the Tennessee promise boosted total enrollment in the state.

The Tennessee Promise does indeed look promising, with tremendous gains in enrollment. The result that the racial composition of the population is unaffected is surprising. Since Black students are more likely to come from lower-income backgrounds, one may have expected higher enrollment gains for Black students. On the other hand, Black students are less likely to go to college, given worse high school completion rates. My results suggest that the latter effect dominates the former. This may substantiate the fear that such a program with universal eligibility may not have intended distributional consequences and could drive larger inequalities and disparities in the higher education system. Future research could also look at the composition of the student population by income distribution to see which income groups are most effectively targeted. Another interesting line of research could look at the program's impact on the distribution of admission test scores of admitted students to evaluate the program's impact on cohort composition by ability. While the IPEDS dataset does contain some preliminary data on test scores, more is needed for testing empirically. Finally, once more data is available, it would be helpful to look at the program's effect on college persistence and completion to understand the promise that such promise programs hold fully.

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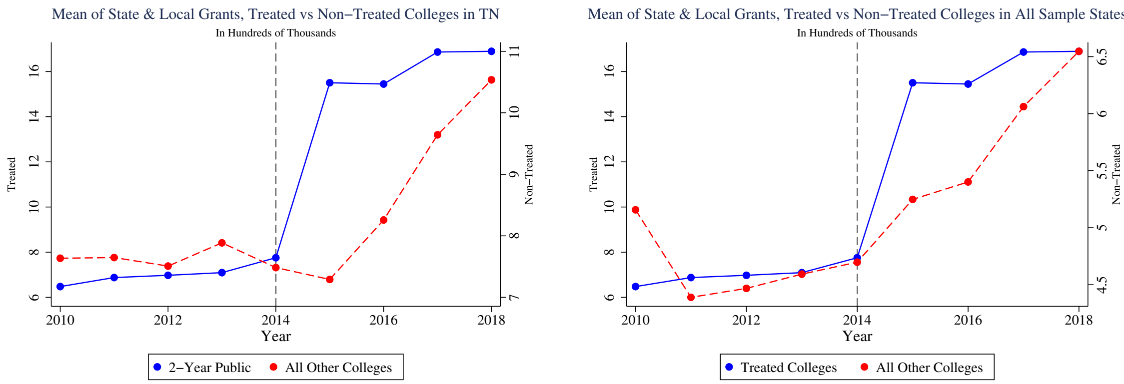
Table 1: Summary Statistics

	Control States		Tennessee	
	(1)	(2)	(3)	(4)
	Ineligible	2-year Public	Ineligible	2-year Public
A. Overall				
Total Enrollment	419.8	507.0	332.8	650.3
	(851.14)	(598.63)	(666.45)	(532.08)
B. Enrollment by Race				
Black	91.0	124.9	62.7	84.9
	(190.52)	(201.72)	(152.07)	(193.40)
Hispanic	21.5	31.0	13.1	21.3
	(56.23)	(97.59)	(31.96)	(34.70)
White	236.5	302.9	194.0	362.4
	(601.31)	(313.13)	(472.23)	(431.20)
Asian	15.3	11.1	7.24	5.49
	(65.79)	(62.04)	(25.67)	(9.68)
C. Enrollment by Gender				
Male	174.7	236.4	131.1	229.6
	(390.43)	(292.20)	(307.27)	(256.72)
Female	228.0	266.1	171.3	269.1
	(474.59)	(303.36)	(353.21)	(332.07)
Observations	6424	2093	1082	351

NOTE: This table reports summart statistics of enrollment data in IPEDS. All numbers reported are mean enrollment for the relevant samplepooled across all years. Standard deviations in parentheses.

Figure 1: State Grants

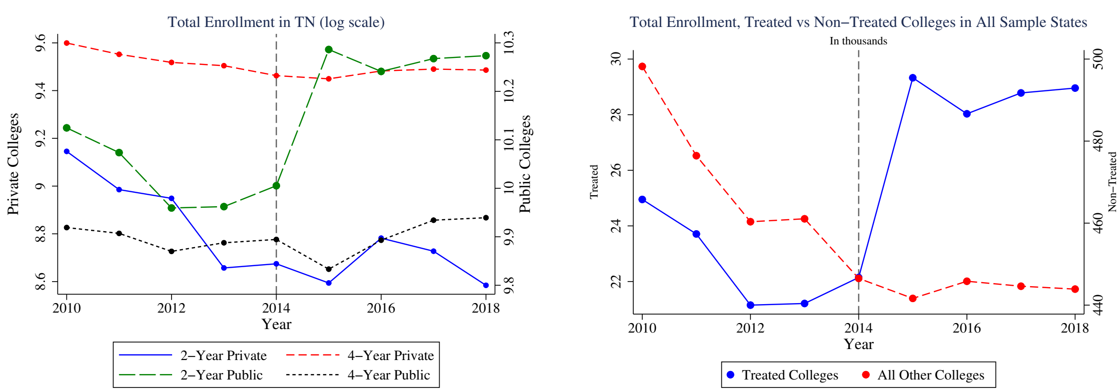
(a) Grants (b) Grants



Note: These diagrams illustrate time trends in amount of state grants in different types of colleges in both the treatment and control groups. Panel (a) shows mean amount of state grants in treated colleges vs. non-treated colleges in Tennessee. Non-treated colleges include all degree-awarding colleges that are not 2-year public colleges. Panel (b) shows mean amount of state grants in treated colleges in Tennessee vs. non-treated colleges in the full sample that includes neighboring states.

Figure 2: Enrollment

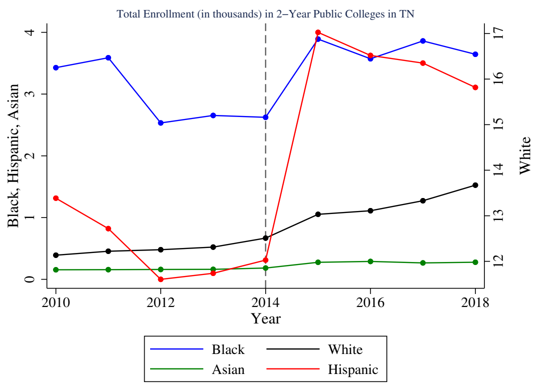
(a) Tennessee (b) Including control states



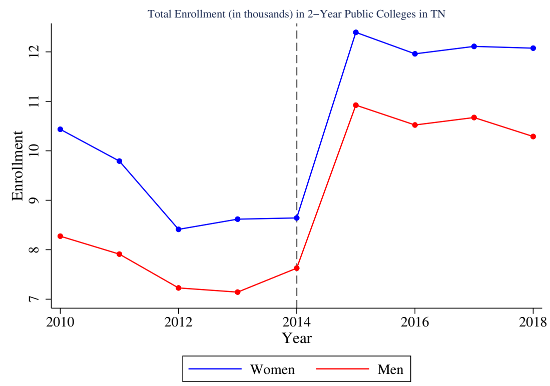
Note: These diagrams illustrate time trends in total enrollment in different types of colleges in both the treatment and control groups. Panel (a) shows total enrollment (log scale) in each type of college in Tennessee. Non-treated colleges include all degree-awarding colleges that are not 2-year public colleges. Panel (b) shows total enrollment treated colleges in Tennessee vs. non-treated colleges in the full sample that includes neighboring states.

Figure 3: Demographic Composition

(a) By Race

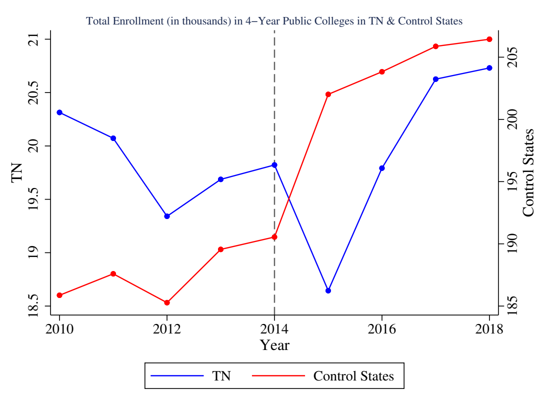


(b) By Gender



Note: These diagrams illustrate time trends in total enrollment by race and gender in the treated colleges, i.e. in 2-year public colleges in Tennessee. Panel (a) shows total enrollment by race. Panel (b) shows total enrollment by gender.

Figure 4: Potential Spillovers



Note: This figure illustrate the potential spillover effects of the Promise Program on public 4-year colleges in Tennessee. There's a drop in enrollment in bachelor's degree awarding colleges in Tennessee that is not observed in neighboring states. There is also evidence that parallel trends assumption is satisifed pre-treatment.

Table 2: Fixed-Effects Regression of Enrollment

	(1) Enrollment	(2) Log Enrollment	(3) Log Enrollment	(4) Log Enrollment (2-Year Public)
Treatment 2015	267.5***	0.324***	0.358***	0.311***
	(47.42)	(0.0656)	(0.0641)	(0.0453)
Treatment 2016-2018	228.6***	0.317***	0.311***	0.348***
	(42.75)	(0.0750)	(0.0713)	(0.0465)
Covariates			x	x
Observations	10290	10290	9962	2446

Standard errors in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Estimates of Regression of Enrollment by Race

	Black		Hispanic		White		Asian	
	(1) Enrollment	(2) Percent	(3) Enrollment	(4) Percent	(5) Enrollment	(6) Percent	(7) Enrollment	(8) Percent
Treatment 2015	0.342***	-0.00515	0.205	-0.0108	0.433***	0.0272	0.228*	-0.00194
	(0.0944)	(0.0149)	(0.124)	(0.0104)	(0.0813)	(0.0154)	(0.0889)	(0.00331)
Treatment 2016-2018	0.417***	0.00561	0.304**	-0.00414	0.422***	0.0208	0.194**	0.00111
	(0.0914)	(0.0125)	(0.109)	(0.00581)	(0.0855)	(0.0143)	(0.0752)	(0.00370)
Covariates	x	x	x	x	x	x	x	x
College, Year FE	x	x	x	x	x	x	x	x
Observations	9929	9929	9929	9929	9929	9929	9929	9929

Standard errors are in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Estimates of Regression of Enrollment by Gender

	Male		Female	
	(1) Log Enrollment	(2) Percentage	(3) Log Enrollment	(4) Percentage
Treatment 2015	0.438***	0.0265	0.371***	-0.0265
	(0.0847)	(0.0184)	(0.0825)	(0.0184)
Treatment 2016-2018	0.462***	0.0273	0.268**	-0.0273
	(0.0928)	(0.0168)	(0.0978)	(0.0168)
Covariates	x	x	x	x
College, Year FE	x	x	x	x
Observations	9929	9929	9929	9929

Standard errors are in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Regression of Graduation Outcomes

	Within 100%		Within 150%		Within 200%	
	(1) Log Graduates	(2) GR	(3) Log Graduates	(4) GR	(5) Log Graduates	(6) GR
year=2016	-0.00790	-0.00659	0.0137	0.00363	0.00408	0.00260
	(0.0332)	(0.00768)	(0.0227)	(0.00650)	(0.0228)	(0.00688)
year=2017	0.00314	-0.00759	0.0558	0.00788	0.0309	0.00332
	(0.0372)	(0.00863)	(0.0287)	(0.00724)	(0.0287)	(0.00745)
year=2018	0.00916	0.00239	0.0330	0.0191*	-0.00244	0.0139
	(0.0346)	(0.00847)	(0.0257)	(0.00751)	(0.0255)	(0.00776)
TN x Cohort 2015	-0.0364	0.0208	-0.191	-0.0153	-0.199	-0.0227
	(0.170)	(0.0429)	(0.125)	(0.0263)	(0.127)	(0.0269)
2-Year Public x Cohort 2015	0.108	0.0218	0.235***	0.0908***	0.189***	0.0884***
	(0.0750)	(0.0169)	(0.0502)	(0.0116)	(0.0500)	(0.0119)
Treatment	0.191	-0.0497	0.469***	-0.00970	0.487***	-0.00471
	(0.184)	(0.0457)	(0.142)	(0.0293)	(0.142)	(0.0300)
Constant	3.108***	0.315***	3.842***	0.495***	3.954***	0.528***
	(0.0234)	(0.00559)	(0.0180)	(0.00478)	(0.0180)	(0.00501)
College, Year FE	x	x	x	x	x	x
Observations	2967	2967	2967	2967	2967	2967

Standard errors are in parentheses and are clustered at the level of the college.

GR stands for Graduation Rate defined as the number of student who graduate divided by total strength of the cohort.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Spillover Effects on 4-Year Public Colleges

	(1) Enrollment	(2) Log Enrollment	(3) Log Enrollment
Year=2012	22.59	-0.00658	-0.00842
	(22.27)	(0.0153)	(0.0160)
Year=2013	10.79	-0.0250	-0.0267
	(24.63)	(0.0169)	(0.0176)
Year=2014	37.41	-0.0362	-0.0384
	(29.87)	(0.0235)	(0.0242)
Year=2015	96.19**	-0.00330	-0.00323
	(35.90)	(0.0242)	(0.0247)
Year=2016	104.4**	0.0123	0.0123
	(39.07)	(0.0243)	(0.0248)
Year=2017	137.7**	0.0284	0.0285
	(43.41)	(0.0216)	(0.0220)
Year=2018	143.3**	0.0325	0.0345
	(43.93)	(0.0240)	(0.0244)
TN × 2015	-198.8*	-0.0790	-0.0908
	(96.36)	(0.0459)	(0.0467)
TN × 2016-2018	-57.17	-0.0254	-0.0274
	(115.8)	(0.0541)	(0.0549)
Constant	1705.1***	7.017***	7.142***
	(24.21)	(0.0145)	(0.0531)
Covariates			x
College, Year FE	x	x	x
Observations	1097	1097	1070

Standard errors are in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A Appendix

Table A.1: Fixed-Effects Regression of Enrollment

	(1) Enrollment	(2) Log Enrollment	(3) Log Enrollment	(4) Log Enrollment (2-Year Public)
Treatment 2015	260.5***	0.326***	0.374***	0.285***
	(49.51)	(0.0863)	(0.0838)	(0.0557)
Treatment 2016-2018	221.5***	0.318***	0.326***	0.322***
	(45.68)	(0.0888)	(0.0853)	(0.0563)
Pre-Treatment 2012-2014	-12.50	0.00144	0.0239	-0.0444
	(22.00)	(0.0635)	(0.0609)	(0.0321)
Covariates			x	x
Observations	10290	10290	9962	2446

Standard errors in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Robustness Check- Log Enrollment by Race

	2-Year Public Colleges Only				Pre-Treatment Placebo			
	(1) Black	(2) Hispanic	(3) White	(4) Asian	(5) Black	(6) Hispanic	(7) White	(8) Asian
Year=2012	-0.155***	0.117**	-0.122***	-0.0559	-0.203***	-0.00252	-0.254***	-0.0644**
	(0.0317)	(0.0401)	(0.0257)	(0.0399)	(0.0274)	(0.0239)	(0.0270)	(0.0213)
Year=2013	-0.141***	0.251***	-0.165***	0.0281	-0.157***	0.0735**	-0.216***	-0.0254
	(0.0326)	(0.0388)	(0.0250)	(0.0441)	(0.0297)	(0.0255)	(0.0277)	(0.0219)
Year=2014	-0.243***	0.285***	-0.231***	0.0236	-0.205***	0.104***	-0.286***	-0.0123
	(0.0352)	(0.0393)	(0.0285)	(0.0421)	(0.0300)	(0.0261)	(0.0286)	(0.0220)
Year=2015	-0.340***	0.326***	-0.302***	0.0165	-0.267***	0.0885**	-0.366***	-0.0157
	(0.0363)	(0.0411)	(0.0305)	(0.0443)	(0.0334)	(0.0307)	(0.0334)	(0.0238)
Year=2016	-0.399***	0.398***	-0.329***	0.0462	-0.263***	0.155***	-0.371***	0.00114
	(0.0407)	(0.0412)	(0.0344)	(0.0438)	(0.0348)	(0.0316)	(0.0335)	(0.0246)
Year=2017	-0.405***	0.401***	-0.374***	-0.00234	-0.226***	0.189***	-0.409***	-0.0233
	(0.0408)	(0.0484)	(0.0331)	(0.0437)	(0.0364)	(0.0339)	(0.0361)	(0.0247)
Year=2018	-0.434***	0.491***	-0.387***	0.0330	-0.203***	0.223***	-0.405***	0.00347
	(0.0402)	(0.0467)	(0.0337)	(0.0450)	(0.0363)	(0.0352)	(0.0358)	(0.0261)
Treatment 2015	0.320***	0.288**	0.414***	0.189*	-0.358**	0.197	0.468***	0.170
	(0.0784)	(0.109)	(0.0591)	(0.0807)	(0.118)	(0.145)	(0.0978)	(0.106)
Treatment 2016-2018	0.462***	0.380***	0.449***	0.211**	0.433***	0.296*	0.457***	0.137
	(0.0624)	(0.0911)	(0.0667)	(0.0654)	(0.114)	(0.137)	(0.0966)	(0.0949)
Pre-treatment 2012-2014					0.0261	-0.0111	0.0580	-0.0959
					(0.0859)	(0.104)	(0.0712)	(0.0747)
Covariates	x	x	x	x	x	x	x	x
College, Year FE	x	x	x	x	x	x	x	x
Observations	2440	2440	2440	2440	9929	9929	9929	9929

Standard errors in are in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3: Robustness Check - Enrollment by Gender

	Male		Female	
	(1) Log Enrollment 2-Year Public Only	(2) Log Enrollment Pre-Treatment Placebo	(3) Log Enrollment 2-Year Public Only	(4) Log Enrollment Pre-Treatment Placebo
Treatment 2015	0.430***	0.475***	0.348***	0.395***
	(0.0694)	(0.0927)	(0.0635)	(0.0969)
Treatment 2016-2018	0.471***	0.498***	0.370***	0.289*
	(0.0734)	(0.101)	(0.0797)	(0.112)
Pre-Treatment 2012-2014		0.0904		0.0577
		(0.0643)		(0.0682)
Covariates	x	x	x	x
College, Year FE	x	x	x	x
Observations	2440	9929	2440	9929

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.4: Pre-Treatment Placebo: Graduation Outcomes

	Within 100%		Within 150%		Within 200%	
	(1) Log Graduates	(2) GR	(3) Log Graduates	(4) GR	(5) Log Graduates	(6) GR
Year=2016	-0.00985	-0.00696	0.0125	0.00316	0.00306	0.00210
	(0.0333)	(0.00769)	(0.0228)	(0.00651)	(0.0229)	(0.00689)
Year=2017	-0.135**	-0.0340**	-0.0347	-0.0260*	-0.0440	-0.0327**
	(0.0511)	(0.0130)	(0.0427)	(0.0107)	(0.0431)	(0.0109)
Year=2018	-0.135**	-0.0251	-0.0606	-0.0159	-0.0796*	-0.0234*
	(0.0503)	(0.0132)	(0.0401)	(0.0112)	(0.0403)	(0.0113)
TN x Cohort 2015	-0.0253	0.0249	-0.156	-0.00406	-0.155	-0.00848
	(0.176)	(0.0463)	(0.134)	(0.0322)	(0.136)	(0.0322)
2-Year Public x Cohrot 2015	0.263**	0.0517**	0.334***	0.127***	0.267***	0.127***
	(0.0837)	(0.0193)	(0.0598)	(0.0136)	(0.0594)	(0.0141)
Treatment	0.145	-0.0653	0.397*	-0.0314	0.421**	-0.0290
	(0.200)	(0.0490)	(0.157)	(0.0352)	(0.157)	(0.0357)
Pre-treatment	-0.0994	-0.0324	-0.143	-0.0435	-0.128	-0.0482
	(0.127)	(0.0307)	(0.0960)	(0.0274)	(0.0933)	(0.0258)
Constant	3.111***	0.315***	3.844***	0.496***	3.955***	0.528***
	(0.0231)	(0.00557)	(0.0179)	(0.00471)	(0.0180)	(0.00492)
College, Year FE	x	x	x	x	x	x
Observations	2967	2967	2967	2967	2967	2967

Standard errors are in parentheses and are clustered at the level of the college.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$