The Promise of the Tennessee Promise Program*

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November 2022

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 insitution. The model utilises a control group of colleges in neighbouring states and ineligible colleges within the state, and examines both the immediate impact and the persistent effects up to five years since the program's implementation. The results are consistent with previous studies which find that aid-programs affect enrollment positively but not graduation rates. I also find that the program does not impact the composition of student population by race and gender. Finally, I find small 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

*I thank Professor Bernard Salanie for his continued guidance and support throughout my work on this project. vp2409@columbia.edu

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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 of 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 such 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. It thus becomes imperative that the effects of such programs be studied thoroughly and the generalisability be evaluated carefully. It is also important to be mindful of potential general equilibrium effects such a program may have on other types of colleges in the state.

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

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 am able to 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 complete population of high school graduates and does not come with any eligibility criteria based on financial need or merit.

It is valuable to study the impact of such a program for many reasons. First, it is unclear whether such a policy generates substantial externality effects on ineligible colleges. Second, there is relatively less literature related to the impact of aid programs on degree completion and graduation outcomes. Third, the universal nature of the program sets it apart from other well-studied merit or financial need-based aid policies. The universality also calls into question whether the program achieves the desired results in terms of 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 neighbouring states ((Gruber, 1994), (Chetty, Looney, and Kroft, 2009)). I find this approach more suitable as the assumptions required for identification are weaker than the assumptions 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 any 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 launch of the program.

There are five related themes of relevant literature. The first 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. A large portion of this research focuses on the impact of need-based programs and merit-based programs, of which most of them 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. **?** find that the HOPE scholarship raises college attendance by 4 to 6 percentage points per 1000$ in aid. **?** reports that Georgia's HOPE program increased enrollment by 7 percentage points and that the impact is greater for white students. This is likely due to the merit-based nature of the scholarship that favours higher-income students. Dynarski (2004) found that scholarship programs in a dozen states have had positive effects on college attendance that favoured nonwhites.

Deming and Dynarski (2009) conclude that the effects of financial aid programs are very sensitive 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 certain financial aid schemes. Further, complex eligibility criteria and application processes could be an obstacle for students to enroll even in the absence of borrowing constraints. They 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 it disproportionately burdens them.

Second, while the effect on enrollment rates is (a priori) reasonable, the effect on completion outcomes is less clear and has not been surveyed as much. Despite large economic returns to completion, many students fail to complete degrees often due to large 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. **?** analyses the Georgia and Arkansas merit scholarship programs to find that they increase degree completion rates by 3 to 4 percentage points. 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 has an impact on 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 10 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 is relatively less 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. **?** 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 that in other schools. Compared to four-year college students, community college students are more likely to be from disadvantaged families. 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 have an impact on 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 HOPE grant was often supplemented by the Pell Grant so 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 where students who enroll in community college 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 enrollling in a community college deters a student from completing their 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 are different from their counterparts in 4-year institutions. These differences are caused by observable factors, such as test scores, as well as unobservable factors, such as ability and motive. Doyle (2009) uses propensity score matching to address selection bias in the estimation of treatment effects and concludes that shifting to community colleges can indeed lower overall graduation rates. Leigh and Gill (2003) provide contradictory results. Using the information on the desired years of schooling of students, they find that community colleges increase educational attainment for those who wish to earn a bachelor's degree. In the context of such contradictory evidence, it is crucial to evaluate the spillover effects of a program such as the Tennessee Promise.

Finally, there is little literature on place-based scholarships such as the Tennessee Promise. These are programs that don't have eligibility criteria per se and are dependent 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 as well as enrollment. Such programs are different in that they are not targeted at certain subpopulations. On one hand, this makes the process of applying and enrollling will be simpler and more uniform thus encouraging participation. On the other hand, such a program may carry exclusion errors in that 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 generalised 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 organised 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 for the various analyses. Finally, I provide a discussion of 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 level of the institution, 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 categories of Black, White, Hispanic, and Asian. All enrollment figures correspond to the enrollment of full-time, first-time undergraduate students as the Tennessee Promise Program is targeted at this group. I use data for the years starting from 2010 up to 2018. This provides a fairly long period in order to explore the immediate treatment effects of the program as well as the medium-run effects of the program up to three years later.

The sample includes all institutions in Tennessee and nine neighbouring states in the

Southeast which form 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 or not. All colleges included in the sample offer some form of degree or certificates and enroll undergraduate students. Colleges with only above the baccalaureate level of study are excluded, as are those that do not offer either any form of certification. The classification groups are labelled as "Public, 2-year", "Public, 4-year", "Private, 2-year", and "Private, 4-year". It is worth noting that these labels are only for the purpose of convenience as students may obtain an associate's degree in a period not equal to 2 years and students may obtain a bachelor's degree within a period not equal to 4 years. In essence, 2-year colleges refer to technical or community colleges that are eligible for the Tennessee Promise program.[2]

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 handful of 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 that a student receives.

Interested high school graduates are required to fill 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 the completion of forms and selection of potential college choices.

If selected for the scholarship, students can receive Promise funds either until they earn their degree or certificate, or for a period of five semesters, whichever occurs earliest. In

[2] One-third of Medicare patients opt out of the traditional version of Medicare, where care is paid directly by the government, in favor of a private insurance scheme ("Medicare Advantage"). In these private schemes, the government pays the insurer a fixed amount per patient and the insurers are responsible for the patient's care. Because Medicare does not pay claim-level bills in these private insurance schemes, the availability and quality of data for the privately insured patients is lower. We exclude these patients from our analysis.

order to remain eligible to receive the scholarship in subsequent years, they are required to renew FAFSA, maintain good academic standing with a GPA of at least 2.0, and complete 8 hours of community service each semester.

Figure 1 depicts trends in state and local grants made available to public, 2-year colleges in Tennessee and nearby states. The plot indicates a rise in state grants in colleges in Tennessee in the year 2015 while they have remained stable in the other states. Since no other program was rolled out during this time, the increase in aid in eligible colleges is largely driven by the Tennessee Promise. A similar trend emerges in figure 2 which shows the level of grants in all colleges in Tennessee, by category of classification. It is important to note here that the increase in funds in public, 2-year colleges is accompanied by a decline in funds in public, 4-year colleges suggesting that there may have been some transfer of state funds out of 4-year colleges which were then channelled into 2-year colleges.

3 Empirical Strategy

The main 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. The straightforward approach would be to subtract 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 outcomes 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.

Therefore, it's appropriate to use a basic difference-in-differences strategy to evaluate the impact of the program 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.

$$Y_{it} = \gamma_i + \alpha_t + \delta_2(treated_i * 2015_t) + \beta * x_{it} + \delta_2(treated_i * 2016\_2018_t) + Const + \varepsilon_{it} \quad (1)$$

where Y stands for the different outcomes of interest for college i in year t. 2015 refers to a short-run effect dummy variable that takes the value of 1 for the year 2015 and 2016_2018_t refers to a dummy variable that takes 1 for the years 2016 through 2018. The dummy variable

$treated_i$ 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; 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 loans and aid which are the percentage of students receiving federal aid and the percentage of students receiving federal loans. These are time-varying factors that could potentially affect 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 first control group comprises three mutually exclusive groups of ineligible colleges within Tennessee. The key threat to identification here is the potential effect that the program may have had on ineligible institutions, i.e. externalities may bias the impact of the program on public community colleges. Figure A1 suggests that there may have 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.

A second control group comprises colleges in neighbouring states around Tennessee. 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, as a result of 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, as observed in figure A2. However, it is still unclear whether there may have been other programs launched in control states that could have affected college outcomes.

I then 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 regular 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 at the same time in Tennessee, I believe that this condition is likely to be satisfied.

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

$$
\begin{aligned}
Y_{it} = {} & \gamma_i + \alpha_t + \beta * x_{it} + \delta_1(TN_i * 2015_t * Treated_i) + \delta_2(TN_i * 2015_t) + \\
& \delta_3(2015_t * Treated_i) + \delta_4(TN_i * Treated_i) + \delta_5(TN_i) + \delta_6(2015_t) + \\
& \delta_7(Treated_i) + +\mu_1(TN_i * 2016\_2018_t * Treated_i) + \mu_2(TN_i * 2016\_2018_t) + \\
& \mu_3(2016\_2018_t * Treated_i) + \mu_4(TN_i * Treated_i) + \mu_5(TN_i) + \\
& \mu_6(2016\_2018_t) + \mu_7(Treated_i) + \varepsilon_{it}
\end{aligned} \tag{2}
$$

The set of δ coefficients capture 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. A concern in the difference-in-difference analysis is that serial correlation can potentially produce incorrect standard errors (Bertrand et al. 2004). Therefore, I choose to cluster my standard errors at the level of the college to adjust for potential serial correlation within the college.

Table 2 presents the regression estimates. Specification 1 of Table 2 estimates the regression of total enrollment on the set of indicators including year and college fixed effects. In specification 2, I estimate the analogous model using the log of enrollment. Specification 3 adds covariates related to federal aid. The estimate does not change drastically. Finally, specification 4 restricts the sample to 2-year public colleges only and compares enrollment across time and states. The estimate is quite similar to the one in specification 3 (DDD). The result is also similar to the DD estimate obtained when the sample is restricted to Tennessee alone. In addition, I also perform several robustness checks using different sample specifications and present the results in the appendix.

Next, I estimate the same equation with outcomes of enrollment in numbers and percentages 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 divide by the total enrollment of full-time, first-time undergraduate students. I include the set of covariates related to federal aid as well as college and year fixed effects in all specifications. I estimate the effects for four race categories- Black, White, Hispanic, and Asian. I choose to study these categories specifically 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 also present a regression estimation of graduation rates. For this, I restrict my sample to the set of 2-year colleges across Tennessee and the control states, and perform a DDD analysis. The primary reason I do this is because the graduation rate for students seeking associate's degrees is not comparable to that for students seeking a bachelor's degree. In order to perform that comparison, the entering cohorts of bachelor's degree-seeking students and associate's degree-seeking students will be different due to the difference in the average time for completion. For 2-year colleges, I use graduation rates for the entering cohort of 2015 which is the first cohort after the launch of the program and consider this cohort to be the "treated" group. The data for the 2015 cohort of bachelor's degree-seeking students, however, is not available. For the pre-treatment periods, I use graduation data for the four previous cohorts. In the appendix, I present results from robustness checks with additional pre-treatment periods.

$$GR_{it} = \gamma_i + \alpha_t + \delta_1(TN_i * 2015_t) + \delta_2(TN_i * 2016\_2018_t) + \beta * x_{it} + Const + \varepsilon_{it} \quad (3)$$

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 both 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 as well as the college.

A key identifying assumption for this specification is the usual parallel trends assumption. Figure A3 provides some evidence that this assumption could be valid. 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 enrolment, enrolment by race, and enrolment 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 cntrol states. On average, 2-year public colleges have higher enrolment 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 enrolment by race. Whites form a majority of the student population across all groups. The population of Whites is almost three times that of the next largest subpopulation of Blacks. Whites and Asians 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 enrolment 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 neighbouring control states.

4.2 Enrolment

Table 2 presents results from the regression equation (2). Column 1 shows coefficients on total enrolment. The effect of the treatment given by the short-term DDD estimate is of the order 267.5 and it is statistically significant. Column 2 uses the log transformation of total enrolment as the outcome variables and is perhaps the better form. The result shows that the treatment has led to a rise in total enrolment by around 38%. Adding covariates increases the coefficient marginally but it remains significant. All three specifications show that the introduction of the program resulted in an immediate increase in enrolment 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 potentially indicates that the placebo effect is indeed zero and the estimate is likely to be unbiased.

Next, I present results from the same regression analysis studying the effect on composition of enrolled students by race and gender. The specifications in Table 3 include regressions with both enrolment in total and as a percentage of the student population as outcome variables. There is no significant change in the percentage of enrolment by race categories for any of the four races. Increase in absolute terms, however, is significant for Whites, Blacks, and Asians. The highest short-term increase is for Whites. The percentage increase of Whites, despite being insignificant, is positive while the same is negative for the other three races.

In the following years, percentage change remains close to zero, while increases in enrolment numbers are significant. The delayed effects are larger for Blacks and Hispanics, relative to the short-run effects.

Overall, it appears that 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 and has been persistent even during years after the immediate launch of the program.

Table 4 contains estimates of the regression analysis for enrolment by gender. Once again, the results are significant for enrolment numbers but not for percentages. The effect of the program is larger 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 A1 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 launch of the program. I also add their second-order and third-order interactions. The reported coefficient estimates are the third-level interactions for the period of three years before, the same year, and three years after the launch of the program. Consistent with 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 enrolment coincides with the launch of the program.

I conduct similar tests for robustness for outcomes variables related to enrolment by race and gender. Table A2 describes results from two distinct robustness checks. Columns 1- 4 are DD estimates from the regression of log of enrolment 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 effect of the program on both the number of graduates and the graduation rate. 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 normal time taken 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 as well as the graduation rate. There is an increase in the number of students who graduate within 150% of normal time of around 48% relative to the control group. The effect is bigger 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.

Robustness Checks :

Table A4 presents robustness results for the analysis with graduation rates. I include a set of pre-treatment indicators as I did in the previous analyses. The point estimates are similar to those in the main specification. The results are therefore robust.

4.4 Potential Spillovers

Figure 2 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 enrol in 2-year colleges instead. Such a spillover effect may have considerable effects on college outcomes.

I use a basic DD framework here with the sample of only public, 4-year colleges. The control group is formed by 4-year public colleges in the control states. 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. Analysing the coefficient on the long-term indicator, the point estimates are too small with very large standard errors.

5 Discussion & Conclusion

My findings are consistent with those obtained by Nguyen (2020). enrollment increases by around 40% in 2015 and around 38% in the years after, relative to the years before 2015. This is a fairly sizeable gain in enrollment. 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 perhaps a reasonable explanation for the remarkable increase in enrollment numbers. Another key aspect is the mentoring program. Given that filing FAFSA is a necessary 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. But there was no significant effect on the racial composition of the population. In fact, 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 from the marginal student. Students from the most disadvantaged backgrounds are not likely to be the ones who are moved to attend college due to such a program. I do not have data that is granular enough 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 for females. This is different from other results that look at differential gendered impacts of college aid (Dynarski 2008). I attribute this to the universal nature of the scholarship. Merit scholarships are likely to favour 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 et al. (2015) also find stronger effects of the Kalamazoo Program on women. The Kalamazoo Promise was a first-dollar scholarship. Once again, I lay emphasis on the last-dollar nature of the Tennessee Promise that separates this program from the others that are commonly surveyed in the literature. Perhaps this means that more male students are on the margin with respect to 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 esitmates. This is consistent with findings from Doyle (2009). But the test does not give me adeqaute 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 that. This is likely due to the large-scale promotion of the program in 2014 which might not have been sustained. Figure A2 shows that enrollment did eventually pick 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. I believe that using the DDD framework helps uncover that this was not the case. Figure A3 in the appendix 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 prmising with tremendous gains in enrollment. However, the disproportionate impact on Whites is something to be mindful of. This may confirm the fears that such a program with universal eligiblity may not have intended consequences and could drive larger inequalities and disparities in the higher education system. Future research could also look at the composition of student population by income distribution to see which income-groups are most effectively targeted. Another interesting line of research could look at the impact of the program on distribution of admission test scores of admitted students to evaluate the impact of the program on cohort composition by ability. While the IPEDS dataset does contain some preliminary data on test scores, it is not adequate for testing empirically. Finally, once more data is available, t would be useful to look at the effect of the program on college persistence and completion to fully understand the promise that such promise prgrams hold.

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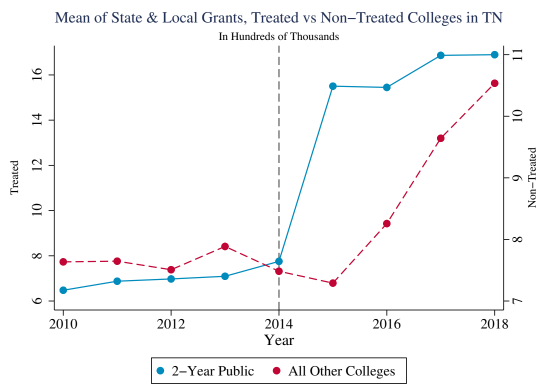
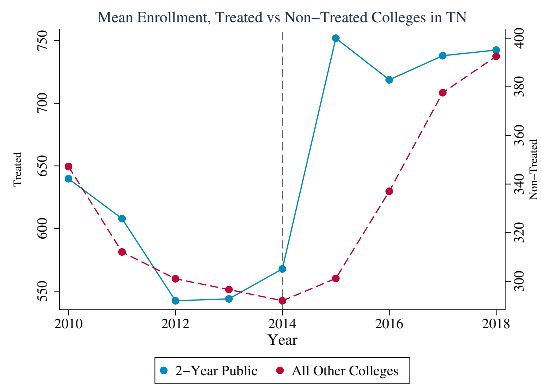
Figure 1: Time Trends

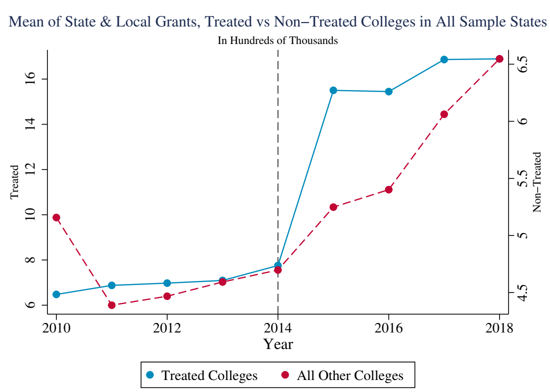
(a) Grants



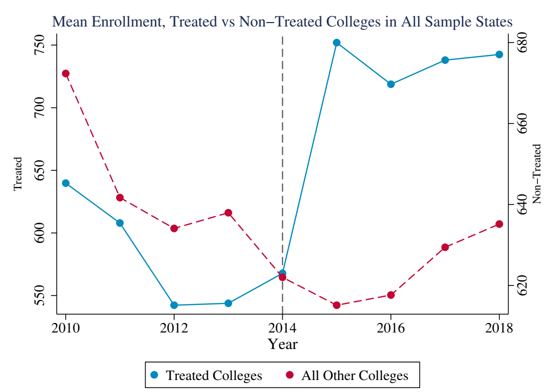
(b) Enrollment



(c) Grants



(d) Enrollment



Note: These diagrams illustrate demand and isocost curves in quantity and quality space. Prices are fixed and supply exhibits local increasing returns to scale. Panel (a) shows the autarkic (no trade) equilibrium in two regions. Greater demand in the big region means that in equilibrium higher quality is produced for the same average cost. Panel (b) introduces trade. Some small-region patients import higher-quality care from the big region. This further increases the quality gap between the locations. Panel (c) contrasts rare and common procedures. Because the isocost curve is concave, market-size effects are more pronounced for rare than for common procedures. In the case illustrated, the small region does not even produce the rare procedure in autarky. Panel (d) introduces trade in both procedures. Trade flows are larger in the rare procedure, as small-region patients import higher-quality care.

Table 1: Gravity regression: aggregate medical services exhibit a strong home-market effect

Note: This table reports estimates of equation (**??**), which estimates the presence of gross and/or net home market effects. The sample is all HRR pairs ($N = 306^2$), and the dependent variable in all regressions is the value of trade computed by assigning each procedure its national average price. The independent variables are patient- and provider-market log population, log distance between HRRs, and an indicator for same-HRR observations ($i = j$). The positive coefficient on provider-market log population implies a weak home-market effect, and the fact that this coefficient exceeds that on patient-market population implies a strong home-market effect. Column 2 makes the distance coefficient more flexible by adding a control for the square of log distance. Column 3 replaces parametric distance specifications with fixed effects for each decile of the distance distribution. Column 4 uses the provider-market and patient-market log populations in 1940 as instruments for the contemporaneous log populations when estimating by generalized method of moments. Trade data are computed from the Medicare 20 percent carrier Research Identifiable File. HRR definitions are from the Dartmouth Atlas Project. Standard errors (in parentheses) are two-way clustered by patient market and provider market.

Table 2: Summary Statistics

	Control States		Tennessee	
	(1)	(2)	(3)	(4)
	Ineligible	2-year Public	Ineligible	2-year Public
A. Overall				
Total Enrolment	419.8	507.0	332.8	650.3
	(851.14)	(598.63)	(666.45)	(532.08)
B. Enrolment 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. Enrolment 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

Table 3: Fixed-Effects Regression of Enrolment

	(1) Enrolment	(2) Log Enrolment	(3) Log Enrolment	(4) Log Enrolment (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	No	No	Yes	Yes
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 4: Estimates of Regression of Enrolment by Race

	Black		Hispanic		White		Asian	
	(1) Enrolment	(2) Percent	(3) Enrolment	(4) Percent	(5) Enrolment	(6) Percent	(7) Enrolment	(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 5: Estimates of Regression of Enrolment by Gender

	Male		Female	
	(1) Log Enrolment	(2) Percentage	(3) Log Enrolment	(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
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 6: Regression of Graduation Rates

	Within 100%		Within 150%		Within 200%	
	(1) Log Graduates	(2) Graduation Rate	(3) Log Graduates	(4) Graduation Rate	(5) Log Graduates	(6) Graduation Rate
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.

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

Table 7: Effects on 4-Year Public Colleges

	(1) Enrolment	(2) Log Enrolment	(3) Log Enrolment
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.

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