

The Impact of Tax Credits on Higher Education Enrollment and URM Representation*

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Abstract

The Taxpayer Relief Act of 1997 introduced the Hope Tax Credit and the Lifetime Learning Tax Credit, establishing a major federal mechanism to subsidize higher education costs. Despite the substantial fiscal cost of these programs, prior research regarding their effectiveness is limited and has not reached a consensus on their ability to stimulate enrollment. This study examines the effect of these policies on college enrollment by exploiting variation in institutional exposure. I classify institutions based on the proportion of tuition covered by the capped tax credits, distinguishing between high-exposure institutions, where the credit covers the majority of tuition, and low-exposure institutions. Using the Synthetic Difference-in-Differences (SDID) estimator on institution-level enrollment data from 1992 to 2006, I find substantial evidence that the tax credits increased full-time enrollment at two-year and four-year institutions relative to a synthetic counterfactual constructed from low-exposure institutions. Furthermore, the analysis reveals that the policy led to significant increases in enrollment levels for underrepresented minority (URM) students in certain institutions and programs, resulting in an increase in their relative share.

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1. Introduction

The Taxpayer Relief Act of 1997 established one of the largest tax cuts in U.S. history, enacting significant changes to the federal tax code. Significantly, this legislation introduced higher education tax credits for the first time: the Hope Tax Credit (HTC) and the Lifetime Learning Tax Credit (LLTC). Designed to improve college affordability, both policies allowed taxpayers to claim a nonrefundable credit against their federal income tax liability for qualified postsecondary educational expenses. These credits saw immediate and widespread adoption, with approximately 47% of the total undergraduate population benefitting from a credit in the 1999-2000 academic year.

Structurally, these policies function by allowing households to reduce their federal income tax liability dollar-for-dollar based on qualified expenses, which are strictly limited to tuition and mandatory fees excluding costs such as room and board. The Hope Tax Credit (HTC) was targeted specifically at students in their first two years of postsecondary education who were enrolled at least half-time, offering a maximum benefit of \$1,500 per student. In contrast, the Lifetime Learning Tax Credit (LLTC) served a broader population, including part-time students and those beyond their second year, providing a credit of up to \$1,000 per tax return. However, a critical design feature limits the accessibility of these benefits for the most financially vulnerable. Because the credits are nonrefundable, families with little to no federal tax liability, often those in the lowest income quintiles, cannot utilize them to offset costs. Furthermore, federal regulations require the subtraction of non-taxable aid, such as Pell Grants, from qualified expenses, substantially reducing or eliminating the potential credit for low-income students who rely most heavily on need-based aid.

Despite the substantial fiscal magnitude of the program and its widespread utilization over the past decades, prior research has not reached a consensus regarding the policy's effectiveness or its impact on the racial enrollment gap. To address this, this study exam-

ines the effect of these policies on college enrollment by exploiting variation in institutional exposure.

In this study, I provide an evaluation of the first wave of tax-credit policies (HTC and LLTC) on college enrollment. Since the tax credits are capped at a specific dollar amount, they cover a significantly larger proportion of tuition at lower-cost institutions compared to higher-cost institutions. Therefore, I define the treatment indicator based on this variation in exposure intensity. Institutions are classified as *treated* (high exposure) if the maximum available tax credit covers at least 80% of their in-state tuition and fees. Conversely, institutions are classified as *controls* (low exposure) if the credit covers less than 20% of their tuition and fees.

The dataset comprises the universe of postsecondary institutions reporting to IPEDS from 1992 to 2006. To ensure a consistent set of comparison units, I restrict the analysis to a balanced panel of institutions that operated continuously throughout this period. Furthermore, I retain only those institutions with valid tuition and fee data in 1997, as this information is essential for defining the exposure-based treatment indicators used in the empirical strategy. The sample is subsequently refined by excluding private for-profit institutions, programs of less than two years, and institutions that are ineligible for Title IV federal financial aid or do not require a high school diploma. This process yields a final sample of 2,731 institutions. To estimate the causal effect, I employ the Synthetic Difference-in-Differences (SDID) estimator, which constructs a synthetic counterfactual from the low-exposure group to account for potential differences in pre-treatment trends.

The empirical analysis reveals statistically significant evidence of a positive effect on full-time enrollment at four-year institutions. Following the implementation of the policy, high-exposure four-year institutions experienced an average increase of 85.3 full-time students per institution relative to the synthetic control group. Given the pre-treatment mean, this coefficient implies an approximate 10.2% increase in enrollment. Furthermore, the analysis provides evidence of significant enrollment growth in the two-year sector,

where high-exposure institutions saw an average increase of 46.4 full-time students; this magnitude represents a relative increase of 10.1%, comparable to the growth observed in the four-year sector. In contrast, there is no evidence that aggregate part-time enrollment changed significantly at either two-year or four-year institutions as a result of the tax credits. Regarding distributional effects, the results indicate that enrollment gains for underrepresented minority (URM) students were concentrated in two-year full-time and four-year part-time programs. Consequently, the policy led to a statistically significant rise in the relative share of URM students within these specific sectors.

Prior research offers mixed evidence regarding the efficacy of these tax credits, often reflecting differences in data sources and identification strategies. Notably, Long (2004) evaluates the institutional responses and enrollment effects of the Taxpayer Relief Act of 1997, finding no discernible evidence of an aggregate enrollment response. However, this analysis is constrained by the use of Current Population Survey (CPS) data, which relies on broad categorical income groups that hinder precise eligibility definitions, and covers only the first three years following policy implementation. Similarly, Bulman and Hoxby (2015) leverage de-identified IRS tax records and a Regression Kink Design (RKD) to examine the policy, yielding no statistically significant effects. A critical drawback of this approach, however, is that the estimates are local to households near the income phase-out regions, meaning the findings cannot be extrapolated to lower- or middle-income families. In contrast, Turner (2011) provides evidence of a significant behavioral response using SIPP data, estimating that tax-based aid increased full-time enrollment by approximately 7 percent. Yet, similar to Long (2004), this analysis focuses on potential eligibility rather than actual exposure and employs a standard Difference-in-Differences (DiD) framework with a limited time horizon, relying on data from only two years prior to the policy and three years following its implementation.

This study contributes to the existing literature by employing an identification strategy that distinguishes itself in several ways. First, in contrast to Bulman and Hoxby

(2015), whose estimates are restricted to households near the income phase-out range, the strategy employed here exploits variation in institutional exposure to compare the most and least affected institutions. This approach captures the behavioral response at the margin where price sensitivity is likely highest. Second, this study benefits from an extended time horizon, utilizing data from 1992 to 2006 to observe trends six years prior to the policy and eight years following its implementation, thereby addressing concerns regarding the short post-treatment windows in earlier studies. Third, the analysis provides a granular perspective by explicitly disaggregating effects across full-time and part-time enrollment within both two-year and four-year institutions. Finally, distinct from prior policy evaluations which have generally overlooked heterogeneous effects across racial subgroups, this study explicitly examines distributional consequences to test the hypothesis that the policy's regressive structure might exacerbate racial disparities.

The remainder of this paper proceeds as follows. Section 2 provides background information on the Taxpayer Relief Act of 1997 and details the specific structures of the Hope Tax Credit and Lifetime Learning Tax Credit. Section 3 describes the data sources, sample selection criteria, and the classification of institutional exposure. Section 4 outlines the empirical strategy, detailing the implementation of the Synthetic Difference-in-Differences estimator. Section 5 presents the main results regarding aggregate enrollment effects and examines the heterogeneity of these impacts across URM and Non-URM student populations. Finally, Section 6 concludes by summarizing the key findings, and discussing the limitations of the study.

2. Background

The Taxpayer Relief Act of 1997 established one of the largest tax cuts in U.S. history. The act provided sweeping tax changes, including capital gains tax reductions, the creation of the Child Tax Credit, expanded retirement savings options, and estate tax relief. Significantly, the relief act also introduced higher education tax credits for the first time: the

Hope Tax Credit (HTC) and the Lifetime Learning Tax Credit (LLTC). Both policies allowed taxpayers to claim a nonrefundable credit against their federal income tax liability for qualified postsecondary educational expenses.

The Hope Tax Credit (HTC) was the primary credit in the relief act designed to make higher education more affordable¹. It provided a nonrefundable credit of up to \$1,500 per eligible student, calculated as 100% of the first \$1,000 in qualified expenses and 50% of the next \$1,000. The credit was subject to income limitations; in its first year (1998), it phased out for taxpayers with a Modified Adjusted Gross Income (MAGI) between \$40,000 and \$50,000 for single filers and \$80,000 to \$100,000 for joint filers. Student eligibility was restrictive, limiting the credit to students in their first two years of post-secondary education who were enrolled at least half-time in a degree program. Qualified expenses were limited to tuition and required fees, excluding costs such as books, room and board, and transportation (Internal Revenue Service, 2002).

The Lifetime Learning Tax Credit (LLTC) targets a broader range of educational pursuits than the Hope Scholarship, covering undergraduate, graduate, and professional development coursework. The LLTC provides a nonrefundable credit of up to \$1,000 per tax return, calculated as 20 percent of the first \$5,000 in qualified expenses (a cap that increased to \$2,000 in 2003). In its original implementation, the income phase-out ranges mirrored those of the Hope Credit: \$40,000 to \$50,000 for single filers and \$80,000 to \$100,000 for joint filers. Unlike the Hope Credit, the LLTC is not restricted by the student's year in school or enrollment intensity (e.g., half-time status), though qualified expenses are similarly limited to tuition and mandatory fees (Internal Revenue Service, 2025).

Given the structural differences between the two policies, families typically select a credit based on the student's academic progress and enrollment intensity. The HTC is generally the optimal choice for eligible students in their first two years of postsecondary

¹In 2009, the HTC was replaced by the American Opportunity Tax Credit (AOTC), a more generous policy that provided a refundable credit of up to \$2,500 per student.

education, as it offers a higher maximum benefit calculated on a per-student basis (\$1,500) compared to the LLTC's per-return cap (\$1,000). Consequently, the LLTC serves primarily as an alternative for students ineligible for the HTC, specifically those beyond their second year, enrolled less than half-time, or pursuing non-degree coursework. Furthermore, while families may claim the HTC for each eligible student on a single tax return, the LLTC is limited to one claim per return regardless of the number of students. Crucially, taxpayers are prohibited from claiming both credits for the same student in the same tax year (Internal Revenue Service, 2002). Table 1 provides a comparison of U.S. federal higher education tax credits in the Taxpayer Relief Act of 1997.

The HTC and LLTC policies saw widespread adoption, with approximately 47% of the total undergraduate population benefitting from a credit in the 1999-2000 academic year. This estimate encompasses students at both two-year and four-year institutions, regardless of whether they filed independently or were claimed as dependents by their parents (United States General Accounting Office, 2002). However, utilization was not evenly distributed across income levels. While some students were ineligible due to high family income, the nonrefundable nature of the credits presented a significant barrier for low-income households. These families typically possessed minimal or no federal income tax liability, preventing them from benefitting from a credit designed solely to offset taxes owed. Data from the Congressional Budget Office regarding the 1999 tax year highlight this structural limitation. Households in the lowest income quintile, which had an average pretax income of \$16,800, faced an effective individual income tax rate of -5.2%. Similarly, households in the second quintile, with an average pretax income of \$37,100, faced an effective individual income tax rate of just 1.7%. Consequently, families in the lowest bracket possessed no tax liability to offset, while those in the second bracket often lacked sufficient liability to fully utilize the available credits (Congressional Budget Office, 2004).

This pattern is detailed in Figure 1, which shows the percentage of undergraduates

who received the HTC and LLTC, disaggregated by parental income. Utilization was lowest among the lowest-income families (\$0-\$19,999), where only 4% of students claimed the HTC and 5% claimed the LLTC. Conversely, participation was significantly higher in the middle and upper-middle-income brackets; in the \$20,000-\$79,999 range, on average about 30% of students claimed the HTC and the LLTC.

Figure 2 reveals a similar trend regarding the portion of net tuition and fees covered by the credits. For this analysis, net tuition is defined as total tuition and fees charged minus all private, institutional, state, and federal grant aid received. The HTC covered approximately 31% of net tuition for students from middle-income families (those earning between \$20,000 and \$79,999), but provided significantly lower coverage for students in both the low-income (12%) and phase-out (15%) ranges. A comparable pattern is evident for the LLTC, which also provided its greatest relative coverage to students in these same middle-income brackets. This disparity is largely driven by federal regulations, which require tax filers to subtract nontaxable aid, such as Pell Grants, from their qualified educational expenses (United States General Accounting Office, 2002).

Beyond the uneven eligibility for higher education tax credits, awareness of these policies is significantly stratified by socioeconomic status. Table 2 reports tax credit awareness in 1999 using data from the National Household Education Survey (NHES). As shown, awareness increases monotonically with household income; only 19.3% of the lowest-income households were aware of either credit, compared to 39.9% of households earning more than \$75,000. Racial and educational disparities are similarly evident. Only 22.5% of Hispanic parents and 28.3% of Black parents had heard of either tax credit, compared to 32.8% of white parents. Furthermore, awareness is positively correlated with parental education; only 20.8% of parents with less than a high school education knew of the credits, whereas this figure rises to 37.4% for those with a college degree.

The combination of low awareness and the policy's regressive structure creates a differential impact on the net price of higher education. While the policy effectively lowers

the cost of attendance for middle- and upper-income families who can fully utilize the nonrefundable credits, low-income households often remain ineligible due to insufficient tax liability or the subtraction of grant aid. This reduction in net price creates a specific financial incentive for wealthier families to increase enrollment or select more expensive institutions, while low-income families experience no comparable change in their effective price. Consequently, there is a theoretical concern that this asymmetry could widen the college attendance gap between URM and Non-URM students, as well as between low- and high-income groups (Dynarski, 2000).

However, this disparity in utilization does not necessarily imply that low-income students face a large disadvantage in total aid received. As illustrated in Figure 3, students from low-income families rely primarily on need-based financial assistance, such as Pell Grants and federal loans, to finance their postsecondary education. Because federal regulations require non-taxable grants to be subtracted from qualified educational expenses, the receipt of significant need-based aid often reduces a student's eligible expenses and their potential tax credit to zero. Therefore, the limited uptake of tax credits among low-income families may not represent a critical gap in affordability, as these students are already served by alternative federal subsidies designed specifically to target their financial need.

3. Data

This study relies on institution-level data drawn from the Integrated Postsecondary Education Data System (IPEDS) to examine the effect of tax credit policies on college enrollment. Conducted annually by the National Center for Education Statistics (NCES), IPEDS is a primary source of data on colleges, universities, and technical and vocational postsecondary institutions in the United States. The system consists of a series of interrelated surveys that collect data from every institution that participates in federal student financial aid programs (Title IV). These surveys capture comprehensive information regarding

institutional characteristics, including enrollment counts, program completions, faculty and staff, institutional finances, and pricing, though the availability of specific variables varies in earlier years.

My dataset comprises the universe of postsecondary institutions reporting to IPEDS from 1992 to 2006. I selected 1992 as the starting year to ensure data quality and institutional coverage, as reporting consistency is lower in years prior to this date. Furthermore, this starting point establishes a six-year pre-treatment window prior to the implementation of the Taxpayer Relief Act of 1997. Conversely, I restrict the analysis to 2006 to allow for a sufficient post-treatment period while avoiding the confounding economic shocks associated with the Great Recession of 2007. Table 3 details the characteristics of all 10,095 institutions present during the 1998 policy year.

To ensure a consistent set of comparison units over time, the population is restricted to institutions that operated continuously from 1992 to 2006. This balanced panel approach is utilized to eliminate potential composition bias arising from the entry and exit of institutions during the study period. Furthermore, institutions missing valid tuition data are excluded, as this information is critical for constructing the empirical strategy. The panel is further refined to create the final analytical sample by excluding private for-profit institutions, those offering programs of less than two years, institutions ineligible for Title IV federal financial aid, and those that do not require a high school diploma (or equivalent) for admission. This process yields a final "Refined Sample" of 2,731 institutions.

The filtering process substantially alters the sample composition, resulting in a shift toward traditional academic institutions. Consequently, the refined sample consists primarily of public and private non-profit institutions that offer at least two-year degrees and maintain standard admission criteria, such as universal high school diploma requirements and a higher prevalence of standardized testing (see Table 3). While these restrictions significantly reduce the total number of institutions, the final sample retains the vast majority of the relevant student population. Based on data from 1998, the 12-month

enrollment of first-time, first-year, degree-seeking students in this sample (1,928,066) represents approximately 78% of the total enrollment for all institutions.

Because the tax credits are capped at a maximum (\$1,500 if a household uses the HTC program), they cover a smaller proportion of costs at expensive institutions and a larger proportion at lower tuition institutions. Consequently, I stratify our sample into three groups central to our empirical strategy: low-exposure institutions (where the credit covers less than 20% of in-state tuition), high-exposure institutions (coverage exceeding 80%), and an intermediate group falling between these thresholds. Table 4 presents summary statistics for these categories. High-exposure institutions are generally larger than low-exposure institutions, with average 12-month enrollments of 798 and 428, respectively. Additionally, the high-exposure group consists predominantly of public and two-year institutions, whereas the low-exposure group is comprised mainly of private and four-year institutions. We leverage this variation in exposure to construct our identification strategy as discussed in the following section.

4. Empirical Strategy

To examine the causal effect of federal tax credit policies on higher education enrollment, I employ the Synthetic Difference-in-Differences (SDID) estimator proposed by Arkhangelsky et al. (2021). This method combines the advantages of the standard Difference-in-Differences (DiD) estimator with the flexibility of the Synthetic Control (SC) method.

The identification strategy exploits variation in institutional exposure to the tax credit policies introduced by the Taxpayer Relief Act of 1997. Since the tax credits are capped at a specific dollar amount, they cover a significantly larger proportion of tuition at lower-cost institutions compared to higher-cost institutions. I define the treatment indicator based on this variation in exposure intensity. Institutions are classified as *treated* (high exposure) if the maximum available tax credit covers at least 80% of their in-state tuition and fees. Conversely, institutions are classified as *controls* (low exposure) if the credit

covers less than 20% of their tuition and fees. As detailed in the Data Section, Table 4 presents summary statistics for institutions in each category. The policy shock occurs in 1998, separating the sample into a pre-treatment period ($t < 1998$) and a post-treatment period ($t \geq 1998$).

While the standard DiD estimator is widely used in policy evaluation, it relies on the parallel trends assumption. In this research context, high-exposure institutions and low-exposure institutions may possess distinct underlying enrollment trends driven by unobserved factors. This potentially violates the parallel trends assumption. Furthermore, due to limited information about institutional characteristics before the 1998 treatment year, I am unable to include sufficient covariates to adjust for these pre-treatment differences.

Synthetic Control (SC) offers an alternative approach by constructing a weighted combination of untreated units that best approximates the pre-treatment behavior of the treated group. However, this method is not suitable for this context. SC is primarily designed for comparative case studies where only a few aggregate units receive treatment (Abadie et al., 2010; Abadie, 2021). In settings where a large number of units receive treatment, the standard SC estimator can be sensitive to extrapolation and exhibits instability.

I employ the SDID estimator because it relaxes the strict parallel trends assumption while avoiding the limitations of SC in this context. By combining these features, SDID effectively localizes the DiD regression, ensuring that I compare the treated institutions only to the most comparable control institutions during the most relevant time periods. Furthermore, Arkhangelsky et al. (2021) demonstrate that SDID possesses a "double robustness" property, meaning the estimator remains consistent if either the unit weights *or* the time weights succeed in removing the bias from unobserved systematic time-varying factors.

4.1 Estimation Procedure

Following Arkhangelsky et al. (2021), the estimation of the causal effect of tax credits on college enrollment proceeds in two steps: calculating unit weights to construct a synthetic control group, and calculating time weights to balance pre- and post-treatment periods.

Unit Reweighting (Synthetic Control) Unlike standard DiD, which weights all control units equally, SDID constructs a synthetic control group by reweighting low-exposure institutions. The goal of unit weights is to match the trends of the treated units during the pre-treatment period. The unit weights are chosen to minimize the discrepancy between the average outcome of the treated units and the weighted average of the control units during the pre-treatment period, subject to a ridge penalty for regularization.

Let N_{co} be the number of control units and N_{tr} be the number of treated units. Let T_{pre} denote the pre-treatment periods. In this method, we should solve for the intercept ω_0 and weights ω :

$$(\hat{\omega}_0, \hat{\omega}) = \underset{\omega_0, \omega}{\operatorname{argmin}} \left\{ \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{j=N_{co}+1}^N Y_{jt} \right)^2 + \zeta^2 T_{pre} \|\omega\|_2^2 \right\} \quad (1)$$

subject to the constraints:

$$\omega_i \geq 0 \quad \forall i \in \{1, \dots, N_{co}\}, \quad \text{and} \quad \sum_{i=1}^{N_{co}} \omega_i = 1 \quad (2)$$

where ζ is a regularization parameter that prevents overfitting by penalizing extreme weights on noisy units. The intercept ω_0 allows the synthetic control to differ from the treated unit by a constant level, focusing the matching on trends rather than levels.

Time Reweighting The time weights are calculated to balance the pre-treatment and post-treatment periods for the control units. I seek weights λ such that the weighted average of pre-treatment outcomes matches the average post-treatment outcome for control units.

Let T_{post} be the number of post-treatment periods. I solve for the intercept λ_0 and weights λ :

$$(\hat{\lambda}_0, \hat{\lambda}) = \underset{\lambda_0, \lambda}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N_{co}} \left(\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t Y_{it} - \frac{1}{T_{post}} \sum_{s=T_{pre}+1}^T Y_{is} \right)^2 \right\} \quad (3)$$

subject to the constraints:

$$\lambda_t \geq 0 \quad \forall t \in \{1, \dots, T_{pre}\}, \quad \text{and} \quad \sum_{t=1}^{T_{pre}} \lambda_t = 1 \quad (4)$$

Unlike the unit weights, the time weights optimization generally does not include a regularization penalty.

SDID Estimator ($\hat{\tau}^{sdid}$) With the estimated weights $\hat{\omega}$ and $\hat{\lambda}$, the SDID treatment effect estimator, $\hat{\tau}^{sdid}$, is obtained from a weighted two-way fixed effects regression:

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau, \mu, \alpha, \beta}{\operatorname{argmin}} \sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t \quad (5)$$

where W_{it} is the treatment indicator, $\hat{\omega}_i = 1/N_{tr}$ for treated units, and $\hat{\lambda}_t = 1/T_{post}$ for post-treatment periods.

Here, the weights $\hat{\omega}_i$ and $\hat{\lambda}_t$ align the trends of the high-exposure and low-exposure institutions. The resulting coefficient $\hat{\tau}^{sdid}$ represents the estimated impact of the Taxpayer Relief Act tax credits on enrollment for the high-exposure institutions, relative to the synthetic counterfactual constructed from the low-exposure group.

The validity of the SDID estimator relies on two primary assumptions. First, it replaces

the standard parallel trends assumption with a “weighted” parallel trends assumption. This implies that, while raw trends may differ, the counterfactual trajectory of the treated group would have evolved in parallel with the reweighted synthetic control group (constructed via $\hat{\omega}$ and $\hat{\lambda}$) in the absence of treatment. Second, the identification assumes the Stable Unit Treatment Value Assumption (SUTVA), which requires that the treatment status of one institution does not affect the outcomes of another. In the context of this study, SUTVA implies that the tax credits did not induce a substitution effect where students transferred from low-exposure (control) to high-exposure (treated) institutions, as such spillovers would contaminate the control group and potentially inflate the estimated treatment effects.

5. Results

Table 5 presents the baseline estimates of the Average Treatment Effect on the Treated (ATT) for college enrollment. It is important to note that each row corresponds to a separate SDID regression estimated for a specific subsample, stratified by institution type (four-year vs. two-year) and enrollment intensity (full-time vs. part-time). The “ATT” column reports the primary coefficient of interest ($\hat{\tau}^{sdid}$), which measures the average deviation in enrollment counts for high-exposure institutions relative to their synthetic counterfactuals in the post-treatment period. The results indicate that the tax credits had a substantial and statistically significant impact on full-time enrollment at four-year institutions. As shown in Panel A of Table 5, following the adoption of the policy, four-year institutions experienced an average increase of 85.3 full-time students per institution relative to the synthetic control group. Given the pre-treatment mean of 840 students, this coefficient implies an approximate 10.2% increase in enrollment relative to the baseline. Furthermore, the analysis provides evidence of significant enrollment growth in the two-year sector, where high-exposure institutions saw an average increase of 46.4 full-time students. This magnitude represents a relative increase of approximately 10.1%, which

is comparable to the growth observed in the four-year sector. In contrast, there is no evidence that aggregate part-time enrollment changed significantly at either two-year or four-year institutions as a result of the tax credits (see Panel B of Table 5).

Figure 4 illustrates the dynamic evolution of treatment effects by plotting enrollment trends for treated institutions (solid line) against their estimated synthetic counterfactuals (dashed line). Figure 4a specifically displays the results for full-time enrollment at four-year institutions. The pre-treatment trends (1992–1997) overlap nearly perfectly, providing strong evidence that the synthetic control serves as a valid counterfactual. Following policy implementation in 1998 (indicated by the vertical dotted line), a sharp and sustained divergence emerges, with the treated group experiencing significantly higher enrollment growth than the counterfactual trajectory. Similarly, Figure 5a shows a comparable divergence for full-time enrollment at two-year institutions, consistent with the positive and significant estimate reported in Table 5. Conversely, Figures 4b and 5b, which display the results for part-time enrollment, indicate no systematic divergence between the treated and synthetic control groups after treatment, consistent with the statistically insignificant estimates.

These findings stand in contrast to much of the existing literature regarding the first wave of federal tax credits. Notably, Bulman and Hoxby (2015) and Long (2004) find no discernible causal effects on college attendance across any sector, including four-year versus two-year or full-time versus part-time enrollment. However, our results align more closely with Turner (2011), who provides evidence that tax-based aid increases full-time enrollment in the first two years of college. It is worth noting that Turner (2011) considers the aggregate impact of all tax-based federal aid (including tuition deductions). By focusing specifically on institutions where the credit covers a substantial portion of tuition rather than an average effect across all colleges, this paper’s estimates likely capture the behavioral response of the most sensitive margin of students, potentially explaining the stronger positive effect observed in this analysis compared to prior estimates.

As discussed in Section 2, the regressive structure of the HTC and LLTC, coupled with socioeconomic disparities in policy awareness, raised concerns that these policies might exacerbate the enrollment gap between low- and middle-income families by effectively reducing the cost of higher education primarily for the latter. Furthermore, given the disproportionate representation of underrepresented minority (URM) students within low-income households, there was a parallel concern that these policies could widen the racial gap between URM and Non-URM groups. Distinct from prior literature, which has largely overlooked the distributional consequences of these policies across racial subgroups, this study explicitly tests the hypothesis that the tax credits might exacerbate the enrollment gap between URM students (defined here as those identified as Black, Hispanic, or Native American) and Non-URM students. Table 6 reports the SDID estimates of treatment effects on enrollment for both groups. Results are presented for both the absolute level of enrollment and the share of the student body comprised of each group.

As shown in Panel B of Table 6, the tax credit policy is associated with significant increases in URM enrollment levels, though these gains are concentrated in specific sectors rather than being uniform across all institution types. Specifically, high-exposure two-year institutions experienced a statistically significant average increase of 18.4 full-time URM students per institution relative to the synthetic control group. Additionally, significant gains are observed in four-year part-time tracks, where URM enrollment increased by 6.4 students. In contrast, estimates for Non-URM students (Panel A) show no statistically significant changes in enrollment levels across either two-year or four-year institutions. This divergence in behavioral response led to a significant shift in the composition of the student body within these sectors. As reported in Panel B, the share of URM students in two-year full-time programs increased by 1 percentage point, and the share in four-year part-time programs rose by 2 percentage points. Furthermore, Panels C and D reveal that while both genders contributed to these gains, the magnitude of the effect was generally larger for URM women; for instance, in two-year full-time programs,

enrollment increased by 12.7 for women compared to 5.4 for men. These dynamic trends are visually corroborated in Figures 6 through 11, which plot the observed enrollment trajectories of treated institutions (solid line) against their estimated synthetic counterfactuals (dashed line).

Consequently, the empirical results provide evidence that the policy may have narrowed the racial gap in college access within specific sectors, leading to a statistically significant increase in the relative share of URM students in two-year full-time and four-year part-time programs. This finding challenges the hypothesis that the regressive structure of nonrefundable credits would uniformly exacerbate racial disparities. However, it is important to note that these findings are limited to enrollment levels and do not capture potential divergences in institutional quality. While access increased for URM students in the two-year and part-time sectors, it remains plausible that Non-URM students may have utilized the financial benefits to attend more selective or higher-resourced institutions. Furthermore, given the limitations of the data, no conclusions can be drawn regarding the policy's impact on the income gap between enrolling families.

6. Conclusion

This study examined the effect of the Hope Tax Credit and Lifetime Learning Tax Credit on higher education enrollment following their introduction in the Taxpayer Relief Act of 1997. While these programs represented a major federal mechanism designed to improve college affordability, prior research regarding their effectiveness is limited and has not reached a consensus. This research contributes to the existing literature by addressing some methodological limitations of previous studies, specifically through the use of an extended time horizon and an identification strategy that is not restricted to families near the income phase-out range. The analysis employed the Synthetic Difference-in-Differences (SDID) estimator, an approach that constructs a data-driven synthetic counterfactual to account for pre-treatment trend differences. My identification strategy ex-

ploited the variation in institutional exposure to the capped tax credits by comparing high-exposure institutions, where the credits covered the majority of tuition, to a control group of low-exposure institutions.

The empirical analysis yields substantial evidence that the tax credits increased full-time enrollment at both four-year and two-year institutions. Specifically, high-exposure four-year institutions experienced an average increase of 85.3 full-time students, representing an approximate 10.2% increase relative to the synthetic counterfactual. Similarly, the analysis reveals significant enrollment growth in the two-year sector, where high-exposure institutions saw an average increase of 46.4 full-time students. This corresponds to a relative increase of approximately 10.1%, indicating that the policy stimulated demand with comparable magnitude across both segments of higher education. Beyond these aggregate effects, a central concern regarding the policies was that their nonrefundable structure and the disparity in program awareness might widen the enrollment gap between racial groups. However, the results indicate that the policy actually led to a statistically significant increase in the relative share of URM students within two-year full-time and four-year part-time programs, suggesting that the credits provided a meaningful wedge for URM access in these specific sectors.

Finally, several limitations should be considered when interpreting these findings. First, the results measure the change in enrollment at high-exposure institutions relative to low-exposure institutions. Consequently, these estimates capture the differential impact of the policy based on tuition coverage rather than the general effect of the policy on the entire higher education sector. In addition, these results are susceptible to substitution effects; if students shifted their enrollment decisions from low-exposure to high-exposure institutions, the observed increases may reflect a change in institutional choice rather than net enrollment growth induced by the tax credit policy. Second, while the findings indicate that the share of URM students increased in the two-year and part-time sectors, this analysis relies on enrollment levels and does not capture potential divergences in insti-

tutional quality. It remains plausible that while access expanded for URM students at the margin, Non-URM students may have utilized the financial benefits to attend more selective or higher-resourced institutions, maintaining stratification in degree value. Furthermore, given the limitations of the data, no conclusions can be drawn regarding the policy's impact on the income gap between enrolling families.

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Table 1: Comparison of Higher Education Tax Credits in The Relief Act of 1997

Feature	Hope Tax Credit (HTC)	Lifetime Learning Credit (LLC)
Max Credit	\$1,500 per student. Calculated as 100% of the first \$1,000 + 50% of the next \$1,000 in expenses.	\$1,000 per tax return.^a Calculated as 20% of the first \$5,000 in expenses.
Refundable	No	No
Years Eligible	First 2 years of postsecondary education.	Unlimited.
Enrollment	At least half-time in a degree or credential program.	No minimum enrollment required for any course.
Expenses	Tuition and required fees.	Tuition and required fees.
Period Active	1998 – 2008	1998 – Present
Income Eligibility	Final Year: Phased out range for single filers from \$40k-\$50k.	Current: Phased out range for single filers from \$40k-\$50k
Status	Expired; replaced by AOTC in 2009.	Active.

^a In 2003, the maximum credit increased to \$2,000 per tax filer (20 percent of qualified higher education expenses up to \$10,000).

Table 2: Percent of Parents Who Have Heard of the Tax Credits, 1999

	Either Tax Credit	Hope Tax Credit	Lifetime Learning Credit	Number of Observations
Mean	33.3	21.5	18.7	8552
<i>Race</i>				
White	32.8	22.5	21.4	5355
Black	28.3	22.9	12.9	1326
Hispanic	22.5	16.2	13.7	1392
All other races	30.9	23.2	17.8	479
<i>Household income</i>				
\$10,000 or less	19.3	14.6	10.4	540
\$10,001–20,000	22.7	17.2	11.3	851
\$20,001–30,000	24.6	18.5	12.2	1202
\$30,001–40,000	27.9	20.5	14.9	1253
\$40,001–50,000	29.0	20.6	18.5	1023
\$50,001–75,000	33.2	23.1	21.6	1704
More than \$75,000	39.9	26.9	27.9	1979
<i>Parent's education</i>				
Less than high school	20.8	15.6	11.1	665
High school degree	22.7	17.5	10.7	2105
Vocational or some college	30.0	20.9	17.0	2657
College degree	37.4	26.1	25.0	1465
Graduate degree	38.6	26.0	28.8	1660

Source: Long (2005). Calculations based on data from the 1999 National Household Education Survey (NHES), Parent Interview.

Table 3: Characteristics of Postsecondary Institutions in 1998: Full, Balanced Panel, and Refined Sample

	All Institutions		Balanced Panel (1992–2006) ^a		Refined Sample	
	Count	%	Count	%	Count	%
<i>Sector</i>						
Public	2,357	24%	1,682	50%	1,425	52%
Private, nonprofit	2,833	29%	1,332	39%	1,306	48%
Private, for-profit	4,554	47%	380	11%	0	0%
<i>Program Length</i>						
4 or more	2,957	30%	1,865	55%	1,685	62%
At least 2 less than 4	2,802	29%	1,465	43%	1,046	38%
Less than 2 years	3,985	41%	64	2%	0	0%
<i>Title IV Eligibility</i>						
Eligible	6,604	68%	3,376	99%	2,731	100%
Not Eligible	3,140	32%	18	1%	0	0%
<i>Admission Req.</i>						
HS diploma	7,272	75%	3,115	92%	2,731	100%
Test scores (general)	4,284	44%	2,351	69%	2,082	76%
SAT test score	2,003	21%	1,699	50%	1,644	50%
ACT test score	2,163	22%	1,797	53%	1,750	64%
Other test score	2,456	25%	794	23%	543	20%
12 Month Enrollment ^b	2,487,641		2,249,824		1,928,066	
Number of Institutions	10,095		3,394		2,731	

^a The balanced panel sample includes institutions that operated continuously from 1992 to 2006 and had valid tuition.

^b Only includes first-year, first-time, and degree seeking enrollment

Table 4: Summary Statistics by In-state Tuition Coverage (Robustness Check)

Variable	In-state Tuition Coverage		
	$\leq 20\%$	Between	$\geq 80\%$
12 Month Enrollment (Mean)	428	989	798
<i>Charges (Mean)</i>			
In district charges	13,174	3,548	1,025
In state charges	13,176	3,734	1,061
Out of state charges	13,198	6,372	3,428
<i>Sector (Count)</i>			
Public	3	758	664
Private, nonprofit	904	379	23
Private, for-profit	0	0	0
<i>Program Length (Count)</i>			
Four or more years	857	754	74
At least 2 but less than 4 years	50	383	613
Less than 2 years (below associate)	–	–	–
<i>Admission Req. (%)</i>			
HS diploma	100%	100%	100%
Test scores (general)	94%	74%	57%
SAT test score	89%	57%	28%
ACT test score	87%	61%	40%
Other test score	13%	18%	33%
Total Obs	907	1,137	687

Table 5: Estimated Average Treatment Effect on College Enrollment (Robustness Check)

Outcome Variable	Pre-Treatment Mean	ATT	Std. Error	P-value
Panel A: 4-Year Institutions				
Full Time Enrollment	840	85.3**	42.4	0.04
Part Time Enrollment	192	–37.4	75.1	0.62
Panel B: 2-Year Institutions				
Full Time Enrollment	460	46.4***	15.0	0.00
Part Time Enrollment	465	33.4	35.4	0.35

Note: ATT is the Average Treatment Effect on the Treated. The Pre-Treatment Mean includes only institutions in the treatment group. Significance levels: * 10%, ** 5%, *** 1%.

Table 6: SDID Estimates of Treatment Effects on URM and Non-URM Enrollment

Group	Level (Count)			Share (Proportion)		
	Mean	ATT	SE	Mean	ATT	SE
Panel A: Non-URM Students						
2-Year Full Time	333.7	-5.3	11.3	0.74	-0.01***	0.00
2-Year Part Time	313.7	-12.5	17.4	0.75	-0.01	0.00
4-Year Full Time	484.0	40.0	29.0	0.58	0.00	0.01
4-Year Part Time	92.2	-26.2	37.6	0.65	-0.02*	0.01
Panel B: All URM Students						
2-Year Full Time	110.8	18.4***	5.6	0.26	0.01***	0.00
2-Year Part Time	113.1	1.8	6.9	0.25	0.01	0.00
4-Year Full Time	342.6	1.7	19.2	0.42	0.00	0.01
4-Year Part Time	25.3	6.4**	2.9	0.35	0.02*	0.01
Panel C: URM Women						
2-Year Full Time	62.2	12.7***	3.8	0.27	0.01**	0.00
2-Year Part Time	62.7	3.9	3.7	0.26	0.00	0.01
4-Year Full Time	204.8	-2.6	12.3	0.43	0.00	0.01
4-Year Part Time	14.9	3.8**	1.9	0.37	0.02	0.01
Panel D: URM Men						
2-Year Full Time	48.6	5.4**	2.2	0.24	0.01***	0.00
2-Year Part Time	50.4	-1.6	3.3	0.24	0.00	0.01
4-Year Full Time	137.8	6.1	7.0	0.41	0.01	0.01
4-Year Part Time	10.4	2.6**	1.1	0.34	0.02*	0.01

Notes: URM (Underrepresented Minority) includes students identified as Black, Hispanic, or Native American.

Level refers to the total enrollment count; Share refers to the proportion of the specific group relative to total enrollment.

Mean reports the pre-treatment average outcome for the treated group.

Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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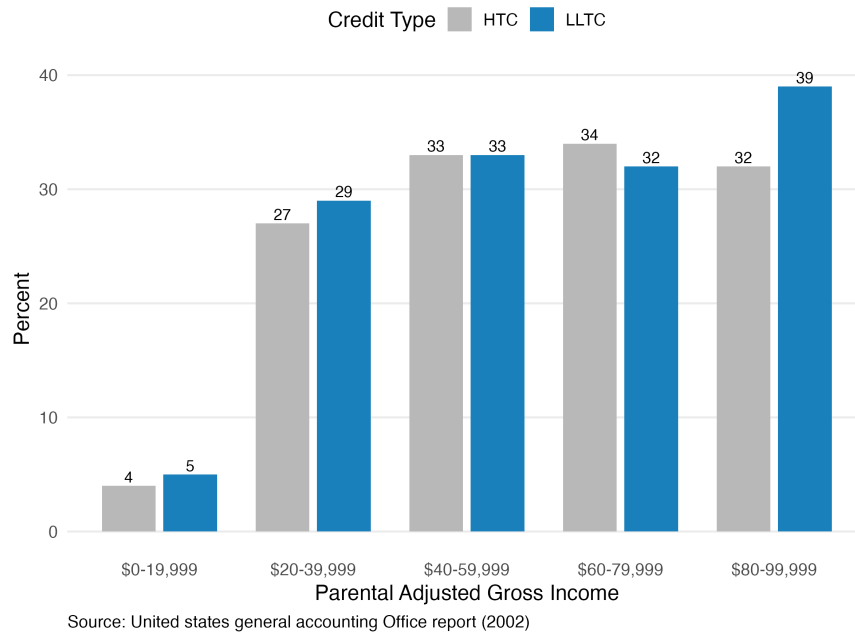


Figure 1: Percent of All Undergraduate Students Receiving HTC and LLTC 1999

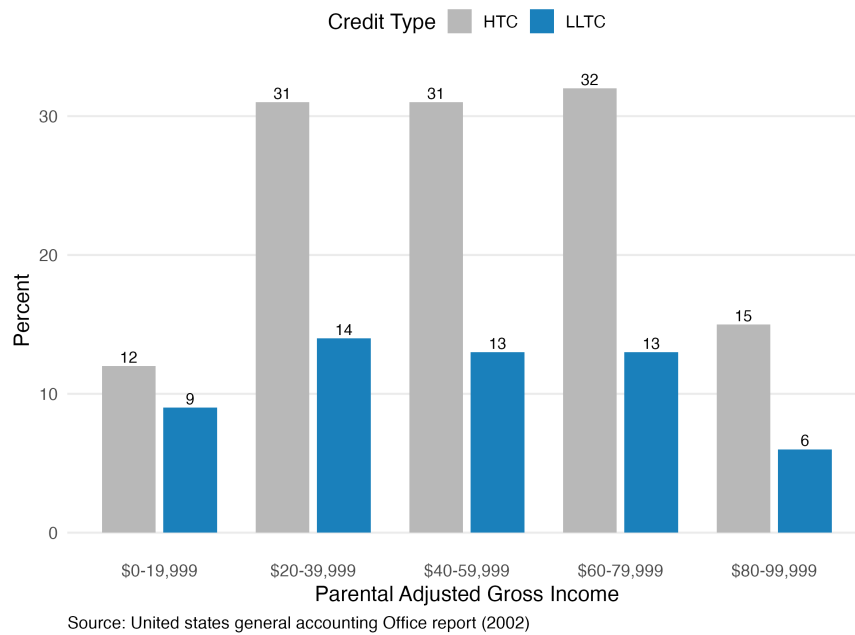


Figure 2: Credit as a Percent of Net Tuition and Fees Paid by Dependent Students 1999

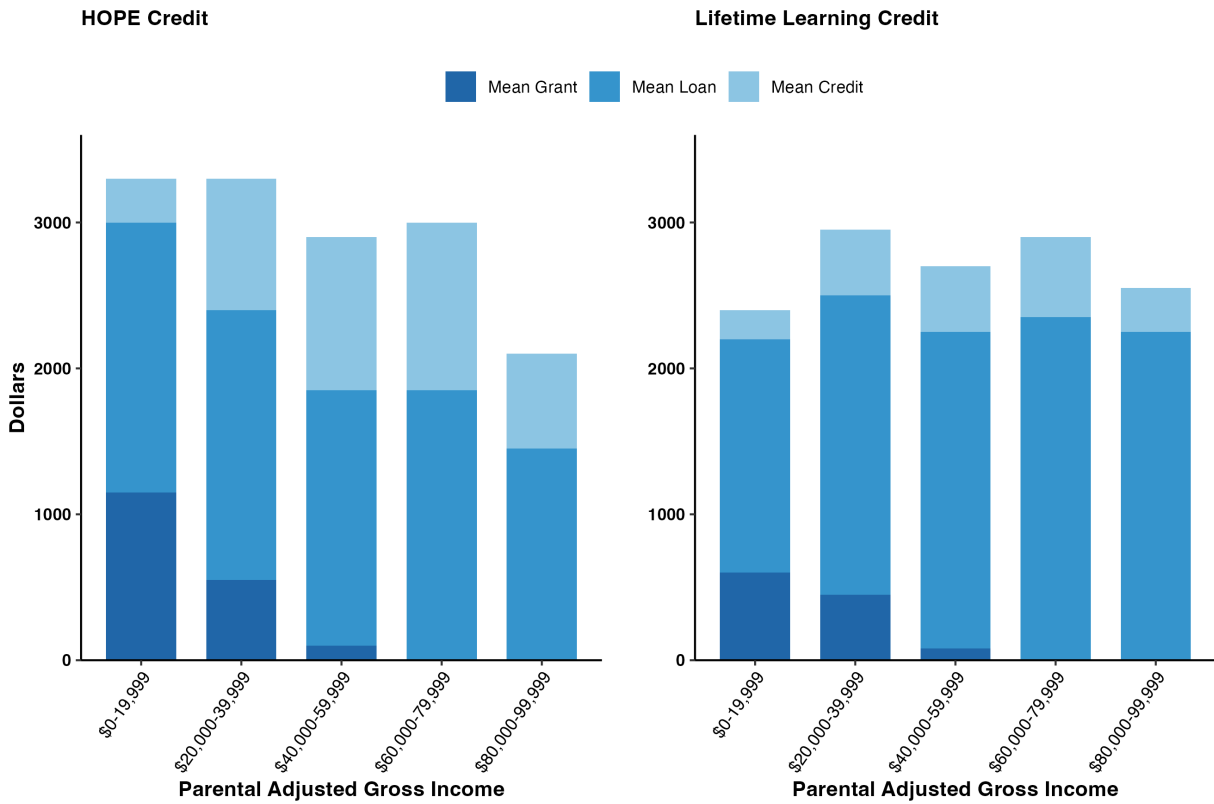


Figure 3: Composition of Federal Financial Aid by Family Income

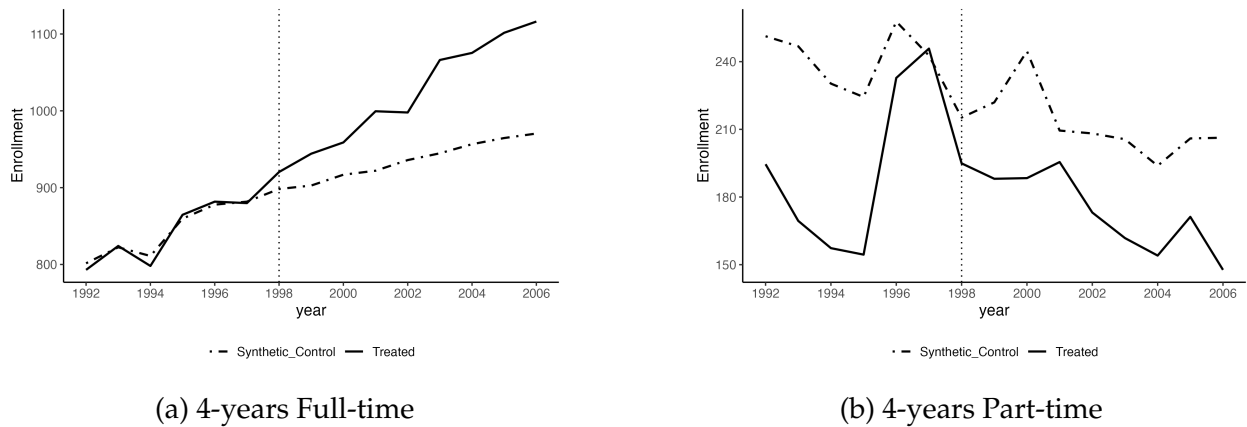
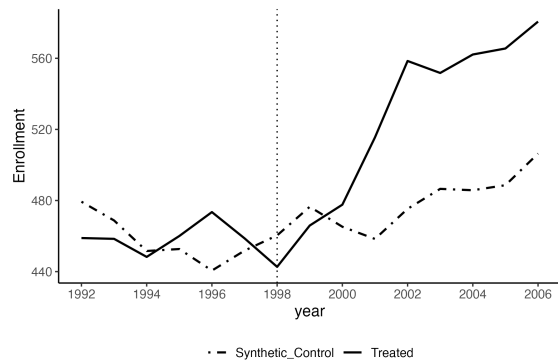
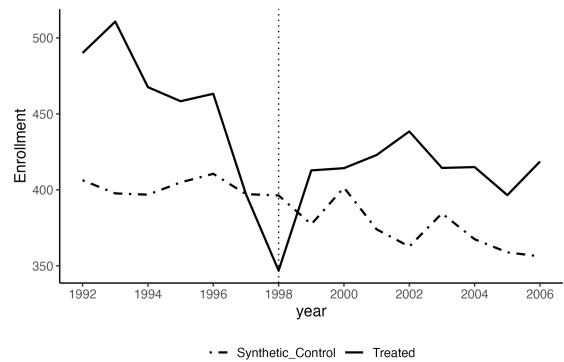


Figure 4: Synthetic difference in difference outcome trend for enrollment in four-year institutions

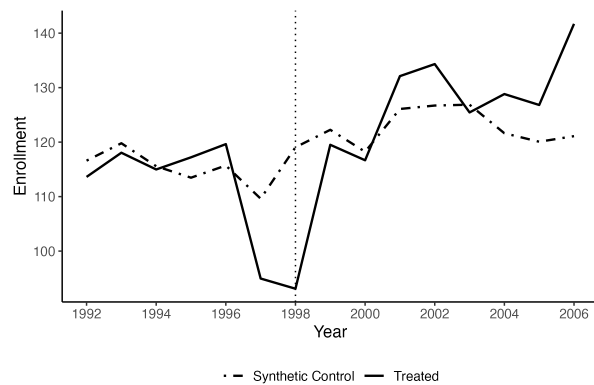


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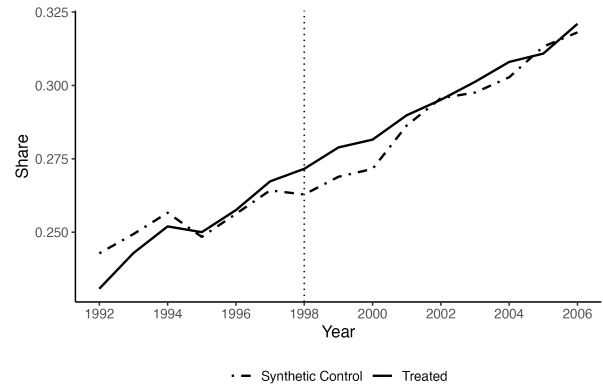


(b) 2-years Part-time

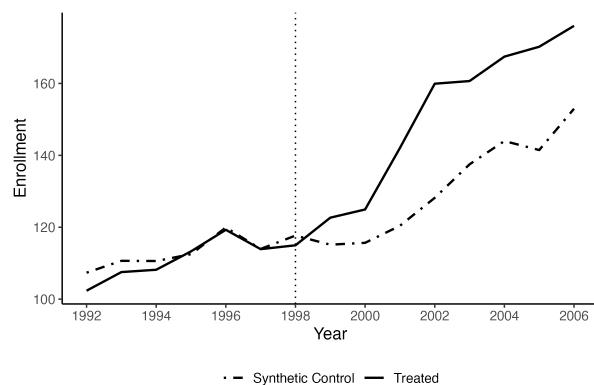
Figure 5: Synthetic difference in difference outcome trend for enrollment in two-year institutions



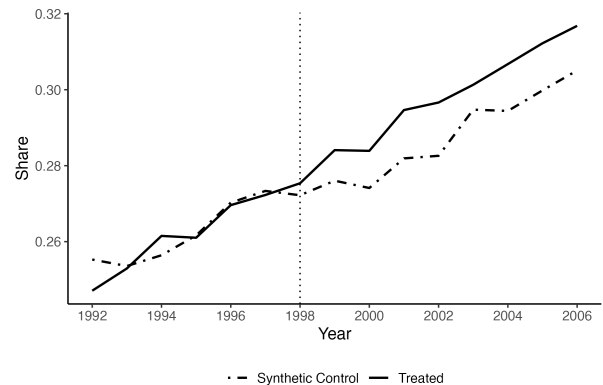
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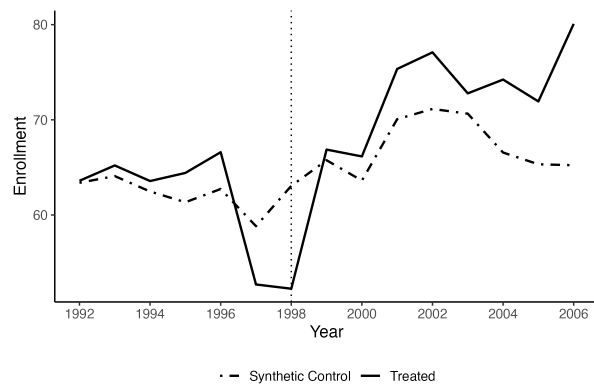


(c) 2-years Full-time

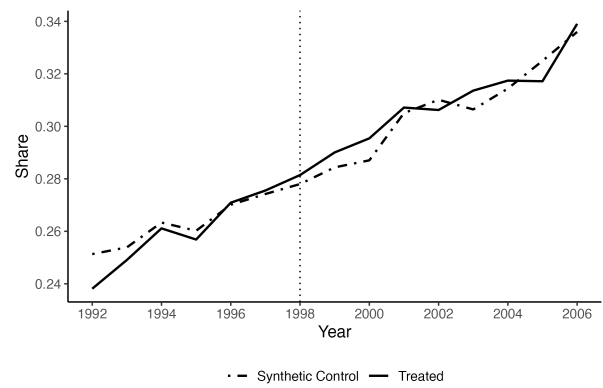


(d) 2-years Full-time

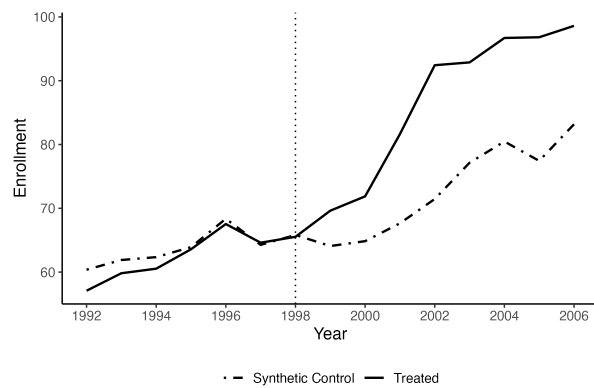
Figure 6: Synthetic difference in difference outcome trend for **URM enrollment** in **two-year institutions**



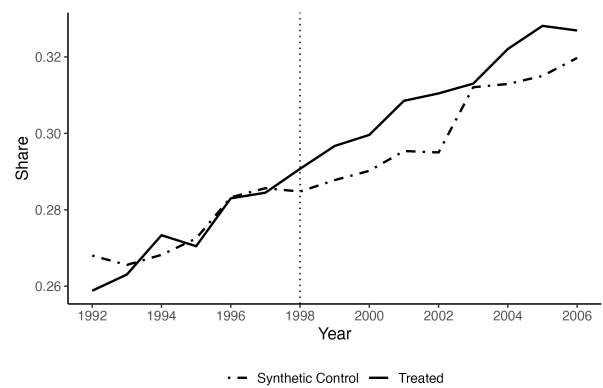
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(b) 2-years Part-time

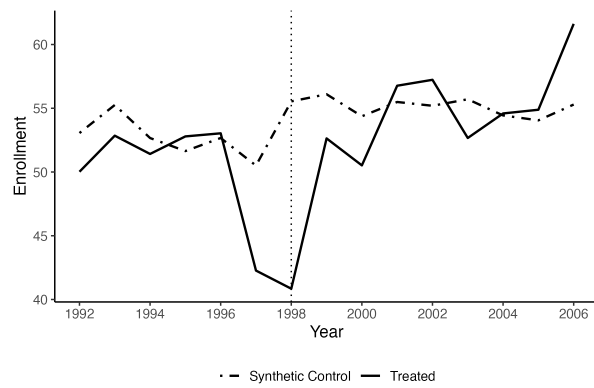


(c) 2-years Full-time

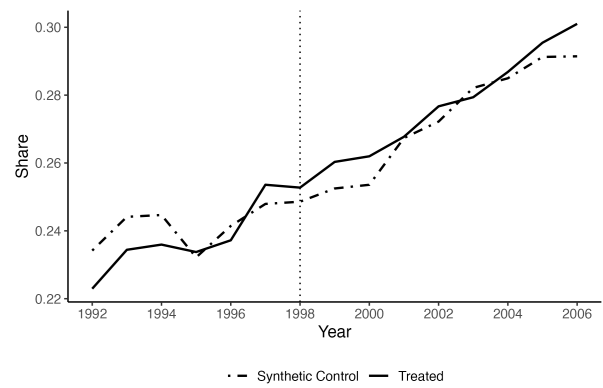


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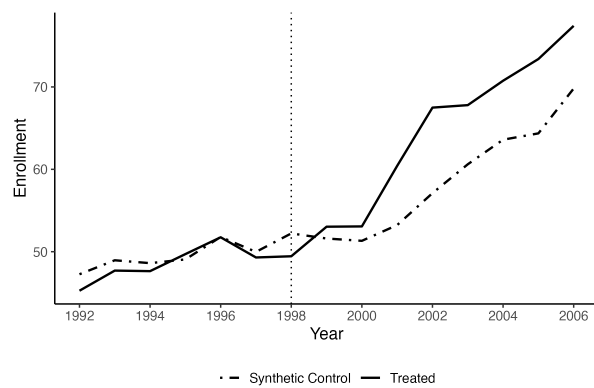
Figure 7: Synthetic difference in difference outcome trend for **URM women enrollment in two-year institutions**



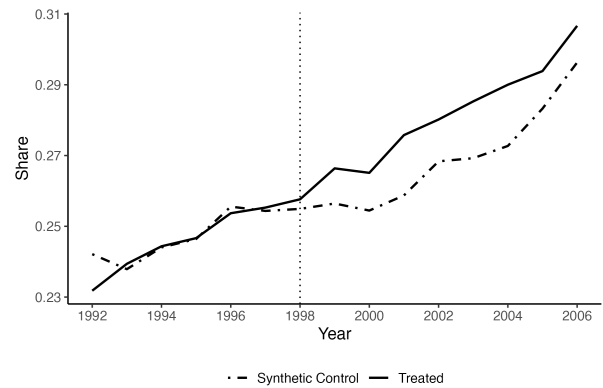
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(b) 2-years Part-time

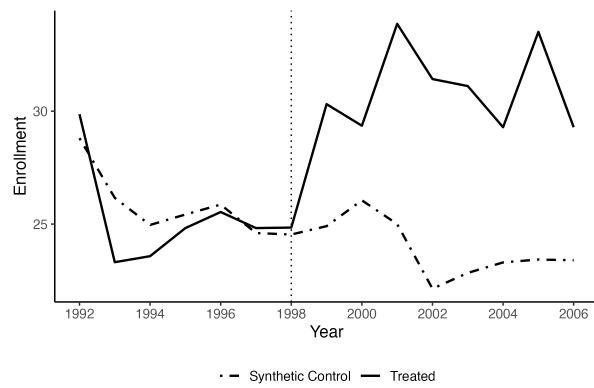


(c) 2-years Full-time

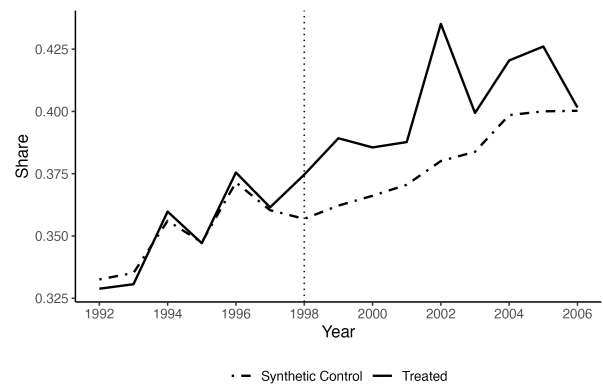


(d) 2-years Full-time

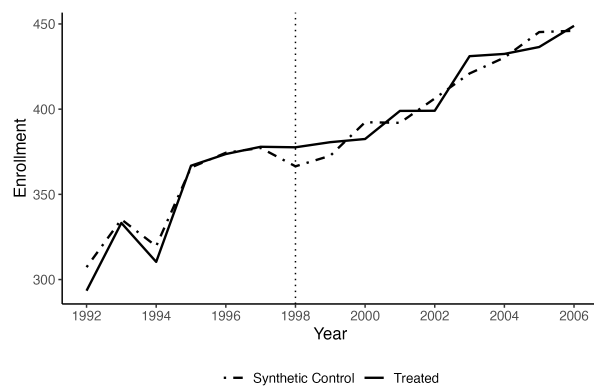
Figure 8: Synthetic difference in difference outcome trend for **URM men enrollment in two-year institutions**



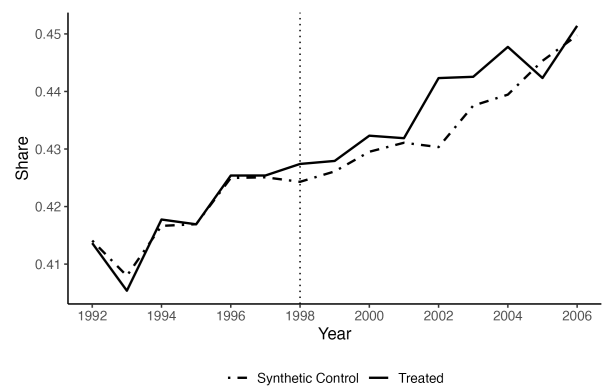
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(b) 4-years Part-time

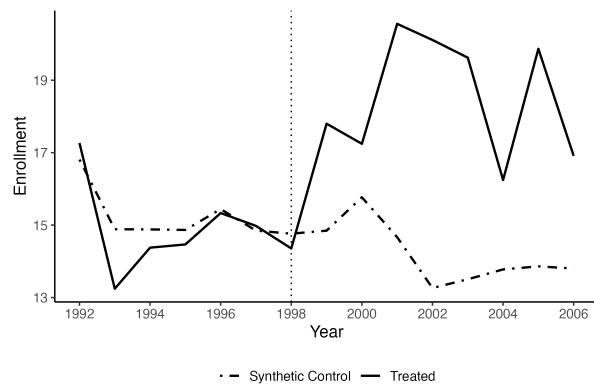


(c) 4-years Full-time



(d) 4-years Full-time

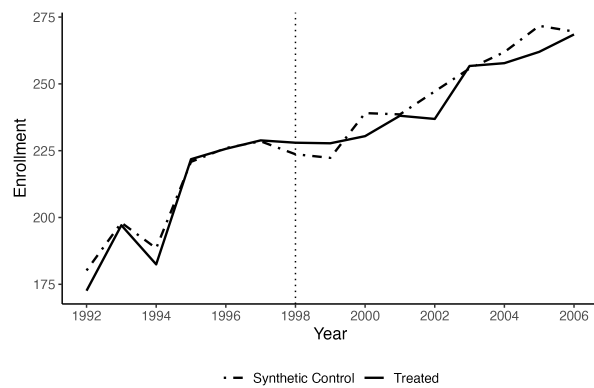
Figure 9: Synthetic difference in difference outcome trend for **URM enrollment in four-year institutions**



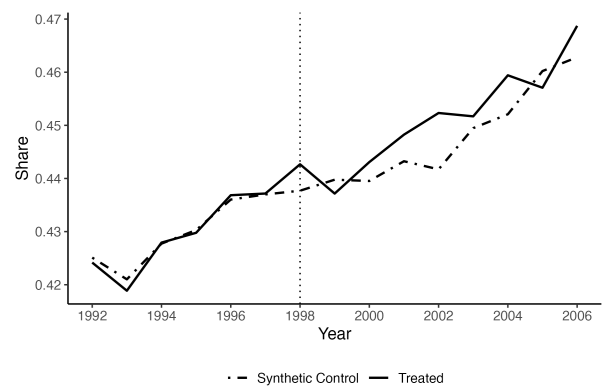
(a) 4-years Part-time



(b) 4-years Part-time

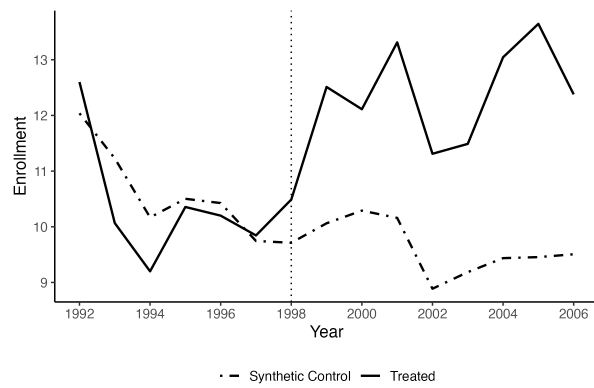


(c) 4-years Full-time

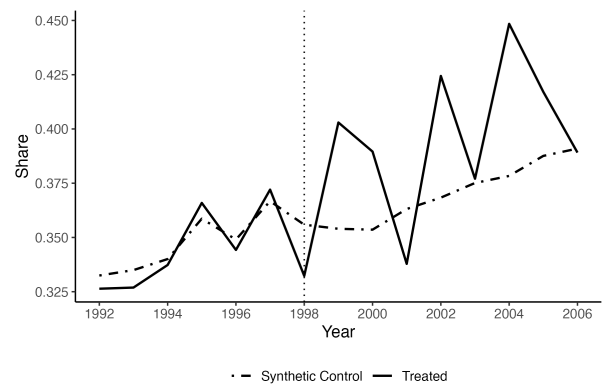


(d) 4-years Full-time

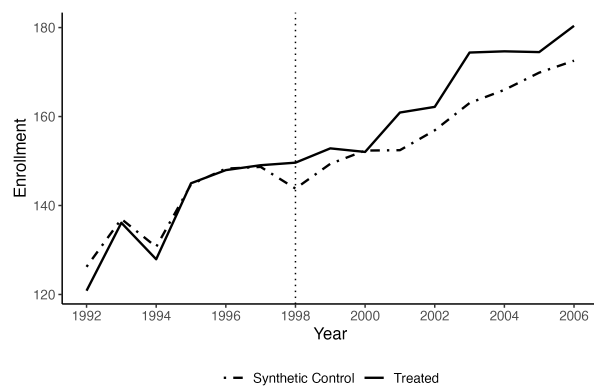
Figure 10: Synthetic difference in difference outcome trend for **URM women enrollment in four-year institutions**



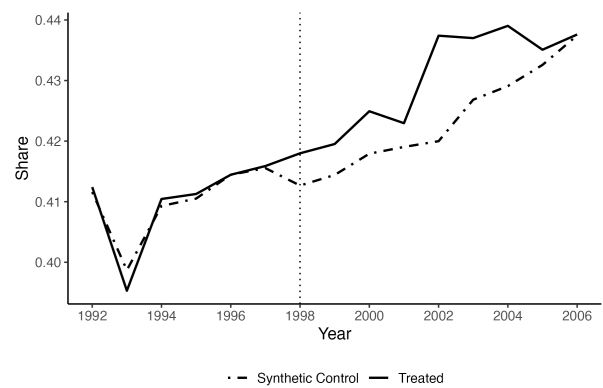
(a) 4-years Part-time



(b) 4-years Part-time



(c) 4-years Full-time



(d) 4-years Full-time

Figure 11: Synthetic difference in difference outcome trend for **URM men enrollment in four-year institutions**