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EdWorkingPaper No. 23-748

Funding Shocks and University Behavior: A synthetic control evaluation of Colorado's College Opportunity Fund

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Discussion of the rising price of higher education and associated student debt in America has been a key feature of political discourse in recent memory, with renewed interest sparked by the announcement of the student loan forgiveness plan. Federal student debt has increased by 756% since 1995, and total student debt tripled from 2007 to 2022. Concurrently, state support for public universities fell by 18% from 2000 to 2015. This phenomenon has drawn interest in the literature, with works by Jaquette and Curs (2015), Bound et al. (2016), Deming and Walters (2017), Webber (2017), and Mathias (2022) examining the effect of state disinvestment on higher education pricing and enrollment. This paper uses data from IPEDS to examine Colorado's College Opportunity Fund, which eliminated state appropriations to Colorado universities in 2006. I advance the literature by being the first to employ quasi-experimental methods, using a synthetic control identification strategy to measure the impact of this funding shock on enrollment and tuition revenue recuperation by Colorado universities. I find that Hispanic enrollment increased by 3 percentage points relative to the synthetic counterfactual, and that tuition revenue increased by 42% as a result of the policy. These results are robust to threats to identification, and placebo tests conrm the validity of the design. These findings provide robust evidence of the pitfalls of state disinvestment in higher education, and the consequences for students who are left to foot the bill.

VERSION: March 2023

Suggested citation: Mathias, Max R.W.. (2023). Funding Shocks and University Behavior: A synthetic control evaluation of Colorado's College Opportunity Fund. (EdWorkingPaper: 23-748). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/a51h-w985

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Max R.W. Mathias*

Abstract

Discussion of the rising price of higher education and associated student debt in America has been a key feature of political discourse in recent memory, with renewed interest sparked by the announcement of the student loan forgiveness plan. Federal student debt has increased by 756% since 1995, and total student debt tripled from 2007 to 2022. Concurrently, state support for public universities fell by 18% from 2000 to 2015. This phenomenon has drawn interest in the literature, with works by Jaquette and Curs (2015), Bound et al. (2016), Deming and Walters (2017), Webber (2017), and Mathias (2022) examining the effect of state disinvestment on higher education pricing and enrollment. This paper uses data from IPEDS to examine Colorado's College Opportunity Fund, which eliminated state appropriations to Colorado universities in 2006. I advance the literature by being the first to employ quasi-experimental methods, using a synthetic control identification strategy to measure the impact of this funding shock on enrollment and tuition revenue recuperation by Colorado universities. I find that Hispanic enrollment increased by 3 percentage points relative to the synthetic counterfactual, and that tuition revenue increased by 42% as a result of the policy. These results are robust to threats to identification, and placebo tests confirm the validity of the design. These findings provide robust evidence of the pitfalls of state disinvestment in higher education, and the consequences for students who are left to foot the bill.

The recent announcement of President Biden's student loan forgiveness plan has put student debt after college back into center-focus in the United States. Federal student debt has increased by 756% since 1995, and the past 15 years has seen the total student debt treble from $545 billion in 2007 to $1.75 trillion today.[1] This drastic increase in the price of higher education and associated

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[1]Federal Student Loan Portfolio. https://studentaid.gov/data-center/student/portfolio

student debt is unpalatable to both sides of the political aisle; Mitt Romney has said, "You can't continue to have higher education tuition grow at a multiple of the rate of inflation", and Ilhan Omar recently tweeted, "Going to college is one of the greatest tools for upward economic mobility we have. It shouldn't come with a 20-year debt sentence."

Whatever one's political leanings, it is hard to dispute that college education is an important educational landmark for many Americans, and that increasing prices are making it less beneficial to many. In 2016, 2/3 of the U.S. labor force over the age of 25 had at least some college, while around 40% had at least completed college. In 1992, only 27% of the labor force had completed college (Bureau of Labor Statistics). The U.S. labor market demands college-educated workers, and universities are enrolling and educating larger populations to meet demand. Public universities enroll a considerable number of these future workers. In 1995, degree-granting public universities in the United States enrolled 11.09 million students; in 2016, this number was 14.58 million, a 31% increase (2021 Digest of Education Statistics. NCES).

However, funding for public universities has not grown to accommodate this increased demand for higher education. From 2000 to 2015, state appropriations for higher education decreased from $84 billion to $69 billion in real 2019 dollars, a decrease of roughly 18% (2021 Digest of Education Statistics. NCES). This leaves universities fewer resources to serve an ever-growing student population. It is no surprise that they attempt to recoup lost revenue from the state through raising revenue from their student body, effectively passing through the burden of university funding from public to private dollars.

This response by universities to state disinvestment is being documented by a growing literature. Jaquette and Curs (2015), Bound et al. (2016), Deming and Walters (2017), Webber (2017), and Mathias (2022) have all examined different outcomes using variation in state appropriations for public universities. Jaquette and Curs (2015) examine nonresident enrollment, finding that universities enroll more nonresident students in response to state appropriation cuts, but their study is only correlational. Bound et al. (2016) use aggregated state appropriations as an IV for university-level funds, finding that foreign enrollment – particularly Chinese students – increases when state funding falls. Deming and Walters (2017) modify Bound et al. (2016)'s instrument by adding a share element that captures universities' dependence on appropriations as a revenue source, showing that overall enrollment grows when state appropriations decline. Webber (2017) use an IV almost identical to Deming and Walters (2017) and look at pass-through of costs by universities to students when appropriations fall. Mathias (2022) further modifies Bound et al. (2016)'s instrument, instead choosing a share that simulates a plausibly exogenous funding method, and examines tuition and enrollment outcomes for resident and nonresident students. He finds that a 10% cut to state appropriations increases resident tuition by 2% and foreign enrollment by 16%.

This paper uses data from the Integrated Postsecondary Education Data System (IPEDS), spanning the years 1992 through 2019, to examine how Colorado's College Opportunity Fund impacted enrollment and tuition revenue. The College Opportunity Fund (COF), passed in 2004 and implemented in 2006, shifted state funding away from universities in Colorado towards resident students. Beginning in 2006, state appropriations were reduced to almost zero for universities in Colorado and resident students received a stipend for attending a university in Colorado. This stipend was available to students for eight academic terms, and required authorization from the student when she was registering for classes in any given term. Details of the amount of the stipend can be found in table 1. This stipend was not meant to wholly offset the cost of attending college, but to make it more affordable for Colorado residents. Another stated goal of the COF was to increase enrollment of under-represented groups, as well as male students.

I use a synthetic control identification strategy to examine two main outcomes: did the COF achieve its goals of increasing diversity among public universities in Colorado? And, by what amount did the revenue shock caused by the policy – through cuts to state appropriations – induce Colorado universities to increase tuition revenue from their respective student bodies?

This work advances the literature by being the first to use quasi-experimental methods to examine a question of the effect of state funding on university behavior. This paper leverages a policy change by Colorado legislators, and the expanding literature on synthetic control implementation (Abadie and Gardeazabal (2003), Abadie, Diamond and Hainmueller (2010), Abadie, Diamond and Hainmueller (2015), Abadie and L'Hour (2021), Abadie (2021)), to provide robust answers to the question of university recuperation of lost revenue.

I find that Hispanic enrollment in Colorado increased by 3 percentage points relative to the counterfactual (a 36% increase compared to the pre-treatment mean), and that university tuition revenue increased by 0.353 log points, or 42%, relative to synthetic Colorado. Both of these estimates are significant at the 1% level. Decomposing tuition revenue into resident and nonresident sources finds that resident tuition revenue increased by 0.204 log points (23%) despite the effective tuition reduction of the COF stipend, which is significant at the 10% level. Nonresident tuition increased by 0.505 log points, or 66%, in relation to the synthetic counterfactual, which is significant at the 1% level.

Each of the above estimates are robust to identification threats like anticipatory behavior by Colorado universities, spillover effects of the treatment, and post-intervention shocks. Placebo tests assigning treatment to the other states in the sample produce less extreme effects than Colorado, confirming the validity of the design.

The rest of the paper is laid out as follows: section 1 provides information on the College Opportunity Fund and its stated aims, while section 2 reviews the literature on state funding shocks to public universities. Section 3 outlines the data used, and challenges posed by data limitations. Section 4 details the synthetic control methodology, how it is relevant in analyzing the College Opportunity Fund, and summarizes the threats to identification common to the synthetic control literature and those which are unique to my sample. Section 5 provides baseline results for enrollment and tuition revenue outcomes. The enrollment outcomes I examine are: male enrollment, White enrollment, Hispanic enrollment, and African American enrollment. Tuition revenue measures are: university-wide tuition revenue, which is provided directly by IPEDS, and my approximations for how this revenue is split among resident and nonresident students. Section 6 delves into robustness of results to identification threats, as well as an alternative specification where the COF stipend is universally adopted among Colorado residents. I also provide placebo tests for each outcome to examine the validity of the synthetic control design. Section 7 concludes and discusses further avenues for research.

1 The College Opportunity Fund

The College Opportunity Fund Act (SB 04-189) was passed May 10, 2004 with the intent of revamping higher education funding in Colorado. To quote at length from the bill:

> (1.3) ...
> (e) Based on these findings, the blue ribbon panel set as a goal increased access and opportunities for Coloradans by encouraging participation of groups who are currently under-represented, specifically low-income individuals, males, and minority groups ...
> ...
> (1.5) Therefore, it is the intent of the general assembly that:
> (a) This new funding system should encourage access and student enrollment for undergraduate students while maintaining distinctive missions of universities and colleges and encouraging geographical access;

(b) All Colorado residents should have access to the college opportunity fund; and

(c) Performance contracts should provide greater flexibility and a more focused accountability for institutions to students and the people of Colorado.

(1.6) It is the intent of the general assembly in enacting this act to fundamentally change the process by which the state finances postsecondary education from funding institutions to funding individuals.

The two primary goals of the COF were to increase enrollment of low-income students, males, and minority students, whom the state considered to be under-represented; and to move away from the classic model of funding institutions via appropriations to providing a per-credit hour stipend to Colorado residents who attended universities in state.[2]

The intent of the College Opportunity Fund was to provide an incentive for residents to attend a university in Colorado through a reduction in the price of attendance. The wording in 1.3.e. appears to explicitly target the extensive margin of college-going for the under-represented groups; the stipend make college affordable for groups who otherwise may not have not attended due to prohibitive tuition. It is also plausible that the lower price of higher education in Colorado could induce wealthier residents to stay through a substitution towards the now relatively cheaper option. This second mechanism is not testable in the data due to the lack of socioeconomic information for resident students; therefore the analysis will focus on the change in the observable populations of male, Hispanic, and Black students whom the law assumes lack access to higher education institutions.[3]

Table 1 provides per credit hour funding by year for the duration of the COF. My sample is comprised entirely of four-year public universities, so values shown in table 1 are for a student attending public institutions in Colorado – students attending private institutions receive half the rate shown per credit hour. What is clear from table 1 is that after the first two years of the program, funding dropped dramatically due to the Great Recession and did not recover in nominal terms until 2019.

Figure 1 visualizes effective in-state tuition for Colorado residents as a result of the policy. Prior to 2006, this is sticker price resident tuition adjusted into real 2019 dollars. From 2006 to 2019, I take

[2] For an in-depth description of state appropriation funding models, see Mathias (2022).

[3] These sub-groups may be induced to attend through the cost reduction despite means to enroll elsewhere; therefore this paper will not comment on the mechanisms that are causing any observed changes.

sticker price tuition and subtract the approximate full-time COF stipend, calculated by multiplying the real per credit hour rate in table 1 by thirty. The COF provided a dramatic reduction in effective tuition for residents in its first two years, with the 2005 tuition of approximately $6,500 falling to under $3,000 in 2006. However, by 2009 effective tuition was up to $6,000, and peaked at over $8,000 in 2017 and 2018. This is strong evidence that public universities passed through reductions in state funding to residents through higher tuition, mitigating the efficacy of the COF in making college more affordable for students.

Figure 2 displays how the COF impacted effective state funding per full-time equivalent enrollment (FTE) in Colorado relative to the rest of the country. For universities not in Colorado – the blue line – this is calculated by dividing state appropriations by the undergraduate population. For institutions in Colorado – the orange line – the calculation is a piece-wise function: prior to 2006, the calculation is state appropriations over the undergraduate population. Beginning in 2006, since state appropriations are zero, I multiply the COF funding for a full-time student (approximated by multiplying the per-credit hour rate by thirty) by the number of students who received it, which is then divided by the undergraduate population. It is clear that for both Colorado and the rest of the country, state appropriations per FTE were declining in real terms in the years prior to the COF. Other states see a slight uptick in funding after 2005 and prior to the Great Recession, while Colorado's COF continues the decline in state support by eliminating any funding for non-resident students that would have been present indirectly through state appropriations. To put these changes in perspective, Colorado's average effective funding per FTE declined by 67% after the COF relative to before, while the rest of the country's average effective funding declined by just 20%.

Figure 3 presents how the COF affected average total state funding for Colorado relative to all other states in the sample. Yearly statewide appropriations were averaged for all non-Colorado states, while Colorado's depiction on the graph is its aggregated funding. For both Colorado (orange) and all other states (blue) there are two lines which represent state appropriations plus the College Opportunity Fund (the solid line) and solely state appropriations (dashed line). Note that these lines are identical for all other states – the distinction in line type is meant to highlight Colorado's funding changes. The dashed line for Colorado shows that the implementation of the COF in 2006 dropped state appropriations to effectively zero, where it remains for the duration of the sample.[4] It is clear from figure 3 that the passage of the COF caused an over 200 million dollar

[4]State appropriations in Colorado are not exactly zero, they range from statewide totals of $3.87 million in 2006

disinvestment in higher education in Colorado from 2005 ($553 million) to 2006 ($334 million), even accounting for the stipend resident students receive.

The College Opportunity Fund has to be authorized by students for the academic term in which they are enrolling, and is available to them for eight terms. From the author's personal experience in using this stipend, it is straightforward to utilize for any term in which classes are taken. I do not have information on take-up of the COF, but it cannot be 100% for various reasons. Despite the simplicity of authorizing its usage for any given term, some students may not understand it or perhaps not use it due to taking a "light" term of classes. The eight term usage limit also implies that any student who graduates in more than four years would lose access to it their final term(s) after the initial eight. A student who graduate in four years, but used the stipend for a summer course, would also not be able to use it for their last semester in school. All of this is to say that the COF is not universally adopted among Colorado residents, and I detail in section 5 how I attempt to calculate take-up.

2 Review of Related Literature

The literature on the effect of state funding on universities is growing rapidly. The early theoretical work of Danziger (1990) transitioned into early empirical papers by Koshal and Koshal (2000), Curs and Singell (2002), Groen and White (2004), Rizzo and Ehrenberg (2004), and Adkisson and Peach (2008). These early empirical works found interesting – and perhaps counter-intuitive – results. Koshal and Koshal (2000) look at appropriations and tuition and find a negative relationship, but fail to distinguish between in-state and out-of-state tuition. Groen and White (2004) use the sample of colleges from the *College and Beyond* dataset to examine how public and private universities set admissions cutoffs for in-state and out-of-state students, but the data is limited to four public universities and the point estimates are imprecise. Rizzo and Ehrenberg (2004) use IPEDS data to look at how tuition determines enrollment composition at flagship state universities. The authors argue that these universities use resident students to increase revenue, while nonresident students are admitted solely to improve student quality. Adkisson and Peach (2008) employ IPEDS data to examine the relationship between nonresident tuition and nonresident enrollment at land grant universities. The authors find that there is a positive relationship between the two variables, however their instrument likely violates the exclusion restriction (they instrument with in-state

to a high of $28.78 million in 2010.

tuition). Curs and Singell (2002) use a unique dataset comprised of admissions and enrollment to the University of Oregon spanning from 1996-2000. The authors find that non-subsidized loans have differential impacts for resident and nonresident students: resident students are more likely to enroll as loans increase, while nonresident students are less likely to enroll.

Kerkvliet and Nowell (2014) is the most recent theoretical paper in this field, asserting that universities should manipulate their acceptance rates in response to tuition and state funding. More recent work by Jaquette and Curs (2015), Bound et al. (2016), Deming and Walters (2017), Webber (2017), Chakrabarti, Gorton and Lovenheim (2020), and Mathias (2022) have brought more technical heft to the field and provided more robust answers to the state funding literature.

The outcomes studied in each of these papers has varied: Jaquette and Curs (2015) examine nonresident enrollment in response to state appropriations shock, though their work is limited by employing only an OLS model. Bound et al. (2016) look at foreign enrollment, particularly students emigrating from China, instrumenting for state appropriations by aggregating them at the state level. Deming and Walters (2017) use tuition freezes and price caps to identify enrollment and cost outcomes, also instrumenting for state funding via a shift-share instrument closely related to that of Bound et al. (2016). Webber (2017) uses an IV very similar to Deming and Walters (2017)'s to look at university pass-through of costs to students when state funding is cut. Chakrabarti, Gorton and Lovenheim (2020) examine longer-run student outcomes like debt in response to state disinvestment in higher education. Mathias (2022) updates the shift-share instruments of Deming and Walters (2017) and Webber (2017), basing the IV on a plausibly exogenous model of state funding, and examines resident and nonresident tuition as well as resident and nonresident enrollment.

A main contribution of this paper to the field is that it is the first to use quasi-experimental methods to examine the behavior of universities in response to state funding shocks. All prior work has used observational data – mostly coming from IPEDS, as I do here – and has sought to establish causality through instrumental variables. The College Opportunity Fund provides a unique opportunity to examine school responses to state policy absent the endogenous relationship of how state legislatures allot money to individual universities.

3 Data

Data comes from the Integrated Postsecondary Education Data System (IPEDS), spanning the years 1992 through 2019. IPEDS is a publicly available database under the umbrella of the National

Center for Education Statistics (NCES), itself a part of the U.S. Department of Education. Each institution has a unique identifier, which allows for matching universities over time.

IPEDS is structured as surveys that all universities in the United States answer, though my sample is comprised solely of four-year public institutions. The rationale for the exclusion of two-year colleges, private institutions, tribal colleges, and religious seminaries from the sample is twofold: first, a bachelor's degree is the most relevant higher education attainment margin discussed in increasing the labor force skill set – the COF explicitly mentions the bachelor degree-holding population of Colorado in section 1.3.b. Second, public universities receive state appropriations, while private institutions do not. In viewing the COF as a state funding shock to the universities of Colorado, four-year institutions in other states are the most relevant control group with which to compare outcomes. After selecting on these characteristics, my sample consists of 270 four-year public universities from 44 states for the years 1992 through 2019 – for a total of 7,560 university-year observations.

The set of institutions in Colorado who are included in my sample are as follows (arranged by descending average undergraduate enrollment):

- UC Boulder
- CSU Fort Collins
- Metro State Denver
- University of Northern Colorado
- UC Denver
- UC Colorado Springs
- Mesa State
- CSU Pueblo
- Fort Lewis College
- Colorado School of Mines
- Western State
- Adams State

Institutional financial information is available through the "GASB 34/35" survey – named so after 2000, prior to this it is titled "Public 4 year and 2 year" – from this I collect information on university tuition revenue and state appropriations. Tuition revenue is only available at the aggregated level. I approximate tuition revenue coming from resident and nonresident students at the university level, which is described in more detail in section 5. Beginning in 2002, tuition revenue becomes a net, rather than gross calculation. That is, tuition allowances are included as a separate variable in the finance survey. There is no measure of these allowances prior to 2002, so I add back the allowances to the tuition revenue variable for 2002-2019 to create a consistent gross revenue measure through the sample.[5]

Enrollment data is drawn from two surveys: university enrollment, both aggregated and by demographic (male, White, students of color, etc.), comes from the "Race/ethnicity, gender, attendance status, and level of student" survey. To make the distinction between resident and nonresident students, I collect data from the "Residence and migration of first-time freshman" survey, which has a count of first-year students for each institution by sending state and an aggregated foreign element. I classify all students whose university location matches the sending state as residents, all other students are nonresident (including domestic but out-of-state students and foreign students). Prior to 2000, this residency data is available only in even-numbered years, and extends back only as far as 1992 (hence the first year of my sample). I linearly interpolate the values for 1993, 1995, 1997, and 1999 using the respective surrounding years where data is available.

Tuition rates, both for resident and nonresidents, come from the "Student Charges" survey, which is grouped under the Institutional Characteristics section. These are "sticker price" values, which realistically only a small fraction of students pay. Due to the lack of concrete tuition revenue by source, or resident enrollment status for non-first-year students, an effective tuition, i.e. sticker price tuition minus average aid, cannot be calculated. Prior to 1999, these tuition rates also included mandatory fees that students paid each semester. In 1999, sticker price tuition and fees were separated. Due to there being no information on what fees were prior to 1999, the tuition rates I report from 1999-2019 include the fees component to keep the measure consistent throughout the sample.

All dollar values are deflated via the Higher Education Price Index (HEPI), and are shown in terms of real 2019 dollars. I choose to use the HEPI over the CPI because its more accurately reflects the increase in prices and costs of higher education over time, which exceed standard

[5] Keeping tuition revenue as a "net" variable does not impact the results of the paper.

inflation calculations.

Colorado demographic variables – that is, calculations of racial makeup by year – come from the American Community Survery (ACS), accessed through IPUMS.

Information on counts of students who use the College Opportunity Fund at each Colorado university in each year of my sample comes from the Colorado Department of Higher Education (CDHE), particularly the work of COF Manager Peggy Hill. This measure is reported as the total number of students at each university in each year (broken down by semester) who receive the stipend.[6]

The CDHE is unable to disentangle this count by grade-level – i.e. first-year students, second-year, etc. – so this necessitates that I approximate university-level enrollment by residency status, the details of which are described in section 5. My calculations introduce measurement error into reports of tuition revenue coming from each residency group, so I use the CDHE numbers as a robustness check in section 6.

4 Methodology

I use the synthetic control (SC) estimator pioneered by Abadie and co-authors in a series of seminal papers (Abadie and Gardeazabal (2003), Abadie, Diamond and Hainmueller (2010), Abadie, Diamond and Hainmueller (2015), Abadie and L'Hour (2021)), which is modified to account for multiple treated units. This estimator is chosen due to lacking an explicit control set on which to test the efficacy of the COF, and the growing literature adapting SC methods for identifying casual estimates in non-experimental settings. I use Arkhangelsky et al. (2021)'s *synthdid* R-package as it is computationally faster than other available resources.

To borrow from the explanation in Arkhangelsky et al. (2021), the SC estimator solves the following equation:

$$(\hat{\tau}, \hat{\mu}, \hat{\beta}) = \underset{\tau,\mu,\beta}{argmin} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \right\} \tag{1}$$

where Y_{it} is the outcome for unit i in period t, μ is the intercept term, β_t is a time fixed effect, $W_{it} \in \{0, 1\}$ is a binary treatment equal to 1 if unit i in period t has received the treatment, τ is the average causal effect of exposure to the treatment, and $\hat{\omega}_i$ is the unit-specific weight.

[6] I average fall and spring semester usage as my measure in section 5.

Translating Arkhangelsky et al. (2021)'s terminology to my sample, the outcomes Y_{it} that I examine are: male enrollment, White enrollment, Hispanic enrollment, and African-American enrollment (all these enrollment values are percentages of the aggregate); aggregate tuition revenue, and tuition revenue decomposed by resident and nonresident populations (these are in millions of dollars, although I take the natural logarithm for ease of interpretation) for institution i in year t. $\beta_t = \{1992, 1993, ..., 2019\}$ is a vector of year fixed effects, while $W_{it} = 1$ if institution i is in Colorado and $t \geq 2006$, 0 otherwise.

The unit-specific weight vector $\hat{\omega}$ assign more weight to donors – schools not in Colorado – that share similar pre-treatment characteristics to the treated set. These weights are computed via a Franke-Wolfe exact-line search algorithm. This algorithm solves a ridge-penalized version of the canonical synthetic control optimization:

$$\hat{\omega} = \underset{\omega}{min} \; ||\mathbf{A}\omega - \mathbf{b}||^2 + \zeta^2 T_0 ||\omega||^2$$

where the typical constraints of $\omega_i \geq 0$ for all i and $\sum_{i=1}^{N_0} \omega_i = 1$ are present. $\mathbf{A}$ is the $T_0 \times N_0$ pre-treatment matrix of the donor pool – with T_0 denoting the fourteen (1992-2005) years of pre-treatment data, and N_0 denoting the 258 donor schools in the sample. ω is the $N_0 \times 1$ vector of weights, and $\mathbf{b}$ is the $T_0 \times 1$ vector of the averaged pre-treatment outcomes for Colorado universities. ζ is the ridge penalty parameter, calculated by taking the standard deviation of the $(T_0 - 1) \times N_0$ matrix of first-differences among the N_0 donor units. This penalizes noisier measures of pre-treatment outcomes, shrinking weights to zero.

The point estimate $\hat{\tau}$ that the SC estimator produces is calculated by averaging the "effect curve" – the vector of treatment effects by year post-treatment. The standard error of $\hat{\tau}$ is estimated via bootstrap.

Plausible threats to the validity of the synthetic control estimator include: poor fit of the control group due to the treated units being an outlier in any particular outcome (convex hull), anticipatory effects of the policy by the treated set, spillover effects between the treated and control groups, and heterogeneous shocks post-treatment all of which could confound the estimated treatment effect.

The convex hull assumption states that for the outcome of interest the treated set must be in the convex support of the control set – that is, the treated set is not an outlier that cannot be adequately approximated by weighting the donor pool. In a simple example, if no outcome for a unit in the treated group is in the support of the control group, then there do not exist any weights to approximate that treated unit, subject to the non-negativity constraint and that the weights sum to unity.

Another threat to identification would be anticipatory effects in response to the COF being passed by universities in Colorado (Abadie (2021) describes this phenomenon in detail, or see Ashenfelter (1978) for the "Ashenfelter dip" in cases where treatment is selected into). This can come in two forms: treated units can change behavior before the policy takes effect and thereby bias estimates of the treatment itself, or elements of the policy can be enacted prior to formal implementation, causing the outcome(s) of interest to systemically change prior to the treatment – or when the researcher believes the treatment occurred. The bias from this anticipatory behavior is ambiguous. If universities in Colorado begin to act as if the treatment occurred prior to its implementation – perhaps due to elements of it being enacted without the researcher's knowledge, then estimates of the causal impact of the policy would be biased towards zero. On the other hand, if the COF was enacted due to particular university behavior that the state wished to cease – say, revenue-seeking – universities could increase revenue-seeking behaviors in the years before treatment knowing the policy would disallow this. In this instance, causal estimates would be overstated due to universities intensifying non-desired activity in anticipation of the policy.

Spillover effects of treatment are an additional factor that would confound causal estimates of the COF. It is not difficult to imagine a scenario where changing enrollment composition in Colorado (for example, increased resident enrollment due to the cost reduction) affects neighboring states, impacting their respective enrollment profiles. In this instance, I am assuming a set of students who in the counterfactual would have attended university out of state now attend in Colorado, which has the effect of inflating both state's measures of resident enrollment. In this instance, causal estimates on resident enrollment would be biased to zero due to schools in the control set being correlated with universities in Colorado. It is also possible that states where a large share of students emigrate to Colorado could be affected by either a tightening or loosening of propensity to admit nonresidents by Colorado schools in response to the treatment.

Lastly, shocks that produce heterogeneous responses would impact estimates of the treatment effect. These shocks can happen in two ways: there could be a common shock experienced by all units that, due to the treatment, causes differential responses among the treated and control groups. Second, there can be a post-treatment shock that is unique to either group, again confounding estimates of the treatment effect. The main threat to identification in this work is the Great Recession, which dramatically contracted state budgets and subsequently reduced funding to higher education. While this shock can be considered common to all states in that the entire United States was affected, state and university responses varied widely. Colorado institutions could have been an outlier in their available choice set, with the state not slashing appropriations to them due to appropriations being effectively eliminated beginning in 2006. Colorado did cut effective funding by reducing the College Opportunity Fund (see table 1), but considering this analogous to the behavior of other state legislatures in regard to state appropriations is a dubious assumption.

Each of these threats to the validity of the assumptions of the SC estimator and the subsequent effect on the estimation of the treatment effect are investigated in greater detail in section 6.

A threat to identification unique to this study is that state funding for Colorado universities may not have declined. State appropriations are eliminated beginning in 2006, but if Colorado supplemented this decline through another channel then there is no revenue shock that would motivate a change in university behavior. Relevant alternative funding channels that IPEDS provides are state operating and non-operating grants. Non-operating grants are listed as zero for Colorado universities throughout the sample, while operating grants increase markedly from 2005 to 2006. As a test of there being a meaningful decline in funding from the state, I add together yearly revenue from state appropriations and state operating grants, and test if they are statistically different from each other in 2005 and 2006 via a paired t-test.

In 2005, average state appropriations for universities in Colorado was \$46.1 million and average operating grants were \$8.65 million (for a total of \$54.75 million). In 2006, state appropriations were effectively zero for universities, while operating grants were on average \$32.12 million. This means that the average Colorado university lost over \$22 million dollars due to the COF; the t-statistic on that difference is 4.48, indicating a statistically significant difference between 2005 and 2006 revenues. This confirms that there was a significant revenue shock for Colorado universities, although the state did blunt this to an extent by increasing operating grants.

5 Results

This section presents results for the main specification described in equation (1) in the previous section. Each set of results consists of a table where the top panel contains the point estimate of the treatment effect $\hat{\tau}$ – labeled "Average Treatment Effect on Treated" in the table – with its standard error in parentheses beneath it. The bottom panel contains the weights of the top ten contributing universities for each estimate.

Below each table is the associated figure plotting Colorado and its synthetic counterpart, so quality of fit of the pre-trend and any treatment effect dynamics may be viewed – although only the ATET is reported as described above.

Results are broken into two tranches: the first set examines how the COF affected enrollment in sub-populations it deemed to be underrepresented. Namely, male enrollment, White enrollment (this was considered to be overrepresented), Hispanic enrollment, and African-American enrollment. This tranche can be simplified to: did the COF achieve its aims in changing enrollment to become more diverse and more representative of the state population?

The second tranche analyzes tuition revenue, and how universities may have manipulated this in response to the state legislature cutting their appropriations to nearly zero. These results include: tuition revenue as it is reported by universities in IPEDS and my estimates of how this burden fell onto resident versus nonresident students – I will discuss how I construct these estimates below.[7] This second tranche can be summarized as: what were the unintended consequences of the COF, particularly with regard to university pass-through of revenue shortfalls? Universities are rational agents who respond to revenue shocks by pulling levers available to them, and tuition revenue has the effect of raising prices for students and increasing student debt.

Table 2 and figure 4 present results for male enrollment as a percentage of undergraduate enrollment. Looking at table 2, the treatment effect estimate is a 2.2 percentage point increase in male enrollment, although it is statistically insignificant. Figure 4 confirms that the pre-trend fit is good, and that male enrollment in Colorado after the treatment is fairly volatile, with an over one percentage point increase from 2007 to 2008, and a roughly two percentage point jump from 2015 to 2016. The synthetic Colorado curve is more stable, aside from a corresponding jump from 2015 to 2016. It appears that any impact on male enrollment that the COF is having is increasing over time, although the averaged estimate presented in table 2 is not statistically significant.

The bottom panel of table 2 finds that Louisiana Tech is the strongest donor to the construction of synthetic Colorado, assigning 19.1% of the weight. The majority of universities are located either in the Midwest (University of Wisconsin - Superior, IUPU - Fort Wayne) or the Eastern United States. Institutions like Coppin State College and North Carolina School of the Arts are small, with undergraduate enrollments of around 3600 and 815 students, respectively.[8]

[7]These decompositions are subject to measurement error, since I am imputing these values based on my own best approximations with available data. Therefore, I strongly encourage caution in interpretation of these point estimates as being exact – they are meant to suggest a relative change in tuition revenue burden.

[8]NC School of the Arts is an outlier in the sample in that its enrollment is so small, and was the first public arts

The results of table 2 and figure 4 beg the question: were men underrepresented in Colorado prior to the implementation of the COF, as the state legislature thought? It is clear from figure 4 that male enrollment share was declining from 1992 to 2003, after which enrollment share stabilized. I perform a series of t-tests to see if average male enrollment share was meaningfully different than the male gender composition in Colorado from 2000 through 2019.[9] Table 3 lists the weighted means of male enrollment composition, as well as the male population proportion for each year for Colorado (which comes from the ACS). For enrollment, observations are weighted by undergraduate enrollment for each year, hence the differing values from figure 4, which weights all treated units equally. The state-level male proportion is a random variable due to the sampling of the ACS, but for simplification I treat it as the population parameter in the t-tests. From 2000 to 2006, the means are statistically different at the 10% level; after 2006, all estimated differences are statistically insignificant.[10] This suggests that the state was correct in believing that men were underrepresented at universities in Colorado prior to passing the COF (albeit marginally), and that after 2007 there was enough variation in male enrollment among schools in Colorado that this difference was no longer significant. However, we should not attribute this change to the COF as it showed no statistical difference from the synthetic counterfactual estimates.

Table 4 and figure 5 examine enrollment percentages of White students. An explicit goal of the COF was to increase diversity in Colorado universities, which is more complicated than examining changes in underrepresented groups' enrollment. It is possible that increases in the enrollment of one underrepresented group could be coming at the expense of another, which would not increase diversity in Colorado. Therefore, decreases in White enrollment are a strong signal that diversity in Colorado is increasing in a meaningful way. Table 4 finds that White enrollment decreased by an average of 4.1 percentage points, which is significant at the 1% level. Figure 5 shows that prior to the COF, White enrollment was declining from around 82.5% in 1992 to 72.5% in 2005, and that the COF hastened this decline relative to the counterfactual of synthetic Colorado. By 2019, Colorado enrolled about 7.5 percentage points fewer White students than the synthetic counterfactual. This is strong evidence that Colorado's undergraduate diversity was increasing as a result of the COF.

The bottom panel of table 4 finds that Indiana University - Southeast is the strongest donor

conservatory in the United States.

[9] ACS data is not available yearly before 2000, so any measure of statewide gender/racial makeup prior to 2000 would be an interpolation between the 1990 and 2000 Census values.

[10] Unweighted averages for male enrollment in Colorado are statistically insignificant from the population estimate for all years.

in the composition of Synthetic Colorado, receiving roughly 18.5% of the weight. Aside from the IU - Southeast and the University of Utah – which contributes about 6% to the synthetic estimate – the rest of the donors are located in the Eastern US, primarily in New England.

Table 5 and figure 6 present results for Hispanic enrollment composition. The average treatment effect on the treated shown in the top panel of table 5 finds that on average Hispanic enrollment increased by 3 percentage points relative to the counterfactual, which is significant at the 1% level. Figure 6 shows that this relative increase in Hispanic enrollment is growing over time, with the gap between Colorado and synthetic Colorado's Hispanic enrollment reaching about 6 percentage points by 2019. Unlike the prior two figures showing male and White enrollment, respectively, Hispanic enrollment does not have a pronounced trend prior to the implementation of the COF. From 1992 to 2019, male enrollment declined by about 3 percentage points, and White enrollment declined by about 10 percentage points; Hispanic enrollment only increased by about 1.25 percentage points over that same period, compared to its near trebling after the COF was passed.

The bottom panel of table 5 shows the primary schools used in constructing the synthetic counterfactual for Hispanic enrollment. Jackson State University in Mississippi has by far the largest weight at 41.1%, followed by Texas A&M - Kingsville at 9.8%. North Carolina School of the Arts makes another appearance in the top donor pool, with a weight of 7.7% for Hispanic enrollment.

Table 6 finds that Hispanic students were still underrepresented despite the enrollment gains from the COF until 2019.[11] The increase in Hispanic enrollment due to the COF is substantial and statistically different from its synthetic counterfactual, but still falls short of adequate representation until well past its passage.

Last among the first set of results are African American enrollment statistics, contained in table 7 and figure 7. Table 7 finds no change in African American enrollment composition due to the COF: the point estimate is very close to zero and is statistically insignificant. Figure 7 shows that African American enrollment is a small share of enrollment at Colorado universities, remaining between 2% and 4% for the duration of the sample. The scale of the vertical axis should be noted relative to the other enrollment figures: each tick is 0.25 percentage points, so what appear to be large increases in enrollment such as from 2015 to 2016 are under half a percentage point. The null result from table 7 becomes clearer in figure 7; there was no change in African American

[11] State Hispanic compositions are derived from the simplified single-race variable available in IPUMS beginning in 2000.

enrollment relative to the counterfactual until 2010, at which point synthetic Colorado actually passed observed enrollment and remains higher through 2019.

The bottom panel of table 7 displays the primary control units used in the synthetic counterfactual. Castleton State College in Vermont contributes the most weight at 24.3%, followed by the University of Wisconsin - La Crosse at 17.4%. Aside from UW - La Crosse and the University of Nebraska - Kearney (whose weight is 11.2%), both of which are Midwestern universities, the remaining eight donors are located in the Northeast. Massachusetts contributes three of the ten donor schools shown, with Vermont adding another two institutions.

Table 8 shows that African Americans are underrepresented in public universities in Colorado throughout the sample. The state population of African Americans is already low, averaging 4.4% from 2000 to 2019, but undergraduate enrollment percentages are consistently below that by over one percentage point for the entire sample.

The prior set of results examined enrollment compositions of populations that the College Opportunity Fund stated were of interest to the state legislature. Of those populations, only Hispanic students saw a significant increase attributable to the COF, although these percentages still lagged behind state demographics until 2019. Males and African Americans saw no significant increase in enrollment due the COF, and the latter still remain statistically underrepresented per the findings of table 8. While male enrollment percentages converged to the state composition, those observed enrollments were not statistically different from the synthetic counterfactual, and therefore it cannot be confirmed that the COF played a role in that increase in enrollment.

Table 9 and figure 8 present results for tuition revenue, of which I take the natural logarithm. Tuition revenue is reported by universities in the "GASB 34/35" survey. As discussed in section 3, tuition revenue was calculated as a gross amount prior to 2002, after which the variable was reported as net of allowances. To keep the measure consistent, all measures here are yearly gross tuition revenue. Table 9 finds that tuition revenue increased by 0.353 log points relative to synthetic Colorado, which is statistically significant at the 1% level. This represents a 42% increase ($e^{0.353}-1$) in tuition revenue relative to the counterfactual. Figure 8 shows the marked increase in tuition revenue by Colorado universities immediately after implementation of the COF, and how this tuition revenue increase grows over the sample to around a 0.4 log point differential by 2019. The clear trend break in tuition revenue with the passage of the College Opportunity Fund in 2006 provides striking visual evidence that universities substituted tuition revenue for lost state appropriations as a result of the policy.

The bottom panel of table 9 displays the top donors for the tuition revenue synthetic counterfactual. Montana State - Northern, Texas A&M, and the University of Michigan are the top three contributors, with weights of 16.1%, 15.7%, and 14.5%, respectively. The Massachusetts Maritime Academy appears again as a top donor (it also featured in the African American enrollment specification), contributing 4% to the synthetic control.

The next two results decompose the tuition revenue findings above into resident and nonresident revenue components, but this is an imputation as this information is not available from IPEDS. To begin, I approximate university enrollment of residents and nonresidents in year t by summing available first-year enrollment data for years $t-3$ through t, which is then scaled to equal observed university enrollment. For 1992 through 1994, not all of these lagged measures are available. 1992 uses the 1992 first-year enrollment multiplied by four. 1993 uses three times the 1992 first-year enrollment, plus the 1993 first-year enrollment. Lastly, 1994 uses the 1992 first-year enrollment for years $t-3$ and $t-2$, 1993 for $t-1$, and then its own first-year enrollment for year t.

The reason for this intermediate step of approximating university enrollment in calculating tuition revenue is that data regarding COF student usage is available only at the university level. As mentioned earlier, student uptake of the stipend cannot be 100% due its term limit and students potentially failing to authorize its usage. Therefore, approximating university enrollment of resident and nonresident students can simulate the non-universal use of the stipend at schools in Colorado, although it does add potential measurement error in my results.

Define $E^j_{it} = e^j_{i,t-3} + e^j_{i,t-2} + e^j_{i,t-1} + e^j_{it}$ to be the sum of observed first-year enrollments of population $j \in \{Resident(R), Nonresident(NR)\}$ at university i in year t. In the absence of attrition or transfers, and assuming every student graduates in four years, then observed university enrollment of institution i in year t, UE_{it}, would equal $E^R_{it} + E^{NR}_{it}$. I assume proportional attrition and transfers among the two sub-populations and create the final imputation of each population, $\widehat{Enroll}^j_{it}$, by scaling so that their sum equals UE_{it}. That is,

$$\widehat{Enroll}^j_{it} = \frac{E^j_{it}}{E^R_{it} + E^{NR}_{it}} * UE_{it} \tag{2}$$

for each residency status j. An alternative view is that I approximate the percentage composition of residents and nonresidents through their observable four-year rolling enrollments, and assign that percentage to university enrollment.

Creating the resident and nonresident tuition revenue estimations builds on the enrollment imputation and follows the same logic of having intermediate imputations scaled so that their sum equals observed tuition revenue for the university. Define $TR^j_{it} = Tuition^j_{it} * \widehat{Enroll}^j_{it}$, where $Tuition$ is sticker-price tuition provided by IPEDS for institution i in year t, $\widehat{Enroll}$ is the enrollment imputation defined above, and j denotes residency status. It is expected that this tuition revenue estimate would overstate actual revenue since it assumes all students pay sticker-price tuition, which is not the case. Similar to the logic of scaling the rolling sum of student enrollment as per $\widehat{Enroll}$, I scale the tuition revenue imputations TR so their sum equals reported tuition revenue, $TuitionRev_{it}$. The final imputation of tuition revenue for resident group j is then:

$$\widehat{TR}^j_{it} = \frac{TR^j_{it}}{TR^R_{it} + TR^{NR}_{it}} * TuitionRev_{it} \tag{3}$$

for each j. This is the tuition revenue calculation used for all universities not in Colorado throughout the sample, and for Colorado universities prior to 2006.

The COF changes how I calculate resident tuition revenue – and indirectly nonresident tuition revenue – for Colorado after 2005. I have the stipend a full-time student would receive (calculated by multiplying the real per-hour amount in table 1 by thirty) each year, and the total number of resident students who received the stipend at each institution each year. Define the full-time per-student stipend as COF_{it}, and the number of students who received it as N^R_{it}, where R defines a Colorado resident per the notation above. Ideally, the imputed measure $\widehat{Enroll}^R_{it}$ is larger than N^R_{it} for all universities in Colorado for all periods, but there are 16 instances (out of 168 university-year observations in Colorado after 2005) where this is not true. In these instances, I assume universal uptake of the COF. Based on the imputations for university-level resident enrollment, I calculate that the average uptake of the College Opportunity Fund is 80.35%, which appears reasonable based on the ease of applying for and receiving the COF, as well as its limitations in usage.

For those N^R_{it} students, their "effective" tuition is $(Tuition^R_{it} - COF_{it})$. I assume the remaining $(\widehat{Enroll}^R_{it} - N^R_{it})$ students pay the sticker-price tuition $Tuition^R_{it}$. Therefore, the new intermediate resident tuition revenue calculation for Colorado universities after 2005 is:

$$\overline{TR}^R_{it} = (Tuition^R_{it} - COF_{it}) * N^R_{it} + Tuition^R_{it} * (\widehat{Enroll}^R_{it} - N^R_{it}), \quad \forall i \in \text{Colorado}, \ t > 2005$$

which is then scaled to equal tuition revenue. The final tuition revenue measures used for Colorado after 2005, $\widetilde{TR}$, are:

$$\begin{cases} \widetilde{TR}_{it}^{R} = \frac{\overline{TR}_{it}^{R}}{\overline{TR}_{it}^{R}+TR_{it}^{NR}} * TuitionRev_{it} \\ \\ \widetilde{TR}_{it}^{NR} = \frac{TR_{it}^{NR}}{\overline{TR}_{it}^{R}+TR_{it}^{NR}} * TuitionRev_{it} \end{cases} \quad \forall i \in \text{Colorado},\ t > 2005 \tag{4}$$

where TR_{it}^{NR} is sticker-price nonresident tuition multiplied by $\widehat{Enroll}_{it}^{NR}$ per the definition.

It is clear from (4) that this new calculation of tuition revenue for Colorado schools assigns a larger share of tuition revenue to nonresident students relative to (3), all else equal. Therefore it is expected that the increase in tuition revenue found in table 9 and figure 8 comes primarily through increased nonresident revenue. The change in resident tuition revenue is ambiguous, it could be increasing through a dramatic increase in sticker-price tuition after 2005 which would negate the COF stipend, or it could be decreasing through the lower effective tuition residents pay.

Table 10 and figure 9 present results for resident tuition revenue. Table 10 finds that on average resident tuition increased by 0.204 log points, and this result is significant at the 10% level. This translates to a roughly 23% increase in resident revenue relative to the synthetic counterfactual. Figure 9 shows the dynamics of the treatment over time, which are increasing until 2017. There is also a noticeable increase in resident tuition revenue from 2001 to 2005, suggesting there may have been an anticipation of the implementation of the COF by Colorado universities – this is discussed in section 6.

The bottom panel of table 10 presents the top ten universities used in the donor pool to construct Synthetic Colorado. Texas A&M contributes the most in the construction of the synthetic counterfactual, with a weight of 17%. Virginia State University is the next largest donor, with a weight of 12.8%. The set of donor institutions has fairly broad geographic representation, with schools in Texas, Ohio, Oklahoma, North Carolina, North Dakota, and Connecticut.

Table 11 and figure 10 estimate the effect of the COF on nonresident tuition revenue. The top panel of table 11 finds that the average increase in nonresident tuition revenue was 0.505 log points relative to the counterfactual, and this result is significant at the 1% level. This is a 66% increase ($e^{0.505} - 1$) compared to synthetic Colorado. Figure 10 plots Colorado and its synthetic counterfactual. It is clear that the COF increased tuition revenue coming from nonresident immediately after its passage, and aside from a slight decline during the recession years, continues to increase throughout the sample. By 2019, the difference in tuition revenue is over 0.5 log points.

The bottom panel of table 11 contains the primary control schools to create the synthetic counterfactual. Utah State University is the top contributor, with a weight of 21%. The University of Michigan at Ann Arbor is the next largest donor, with a weight of 11.4%. Massachusetts Maritime Academy appears again as a leading donor, with a weight of 4.1%.

The prior results tranche examined how universities in Colorado increased tuition revenue in response to the revenue shock caused by the passage of the College Opportunity Fund. University tuition revenue increased by 0.353 log points (42%) in relation to synthetic Colorado. This 42% increase in tuition revenue was comprised of 23% increase in resident tuition (0.201 log points), which was significant at the 10% level; and a 66% increase in nonresident tuition (0.505 log points), which was significant at the 1% level.

The point estimates from the resident and nonresident decompositions are based on my best estimates imputing data on resident and nonresident enrollment to account for non-universal take-up of the College Opportunity Fund by Colorado residents, and therefore should be viewed as suggestive of the relative burden borne by residents and nonresident students, not as concrete causal estimates.

The results section has examined the effects of the College Opportunity Fund on Colorado universities from two lenses: the first analyzed how the COF impacted enrollment that the law itself deemed vital; male enrollment, Hispanic enrollment, and African American enrollment. Hispanic enrollment increased by on average 3 percentage points after 2005, and the dynamics shown in figure 6 suggest that this enrollment boost increased over time relative to the synthetic counterfactual.

The second lens is grounded in the growing literature on university responses to state disinvestment: how did the state legislators' decision to slash state appropriations (with minimal buffering via increased operating grants) impact how universities collect revenue from their students? On average, tuition revenue increased by 42% relative to the synthetic counterfactual, with the increase being driven by increased nonresident tuition revenue (66% increase). Perhaps surprisingly, resident tuition revenue increased by about 23% on average, suggesting that the COF stipend did little to offset increasing costs of higher education for Colorado residents.

6 Robustness Checks

This section will revisit the main specifications from section 5, testing their robustness to the identification threats mentioned in section 4. These threats are: failure for the treated units to lie within the convex hull of the donor pool, anticipatory effects that alter the data-generating process prior to the treatment, spillover effects of the treatment into the donor pool that would confound estimates of the average treatment effect, and shocks that are either unique to the treated or control group – shocks unrelated to the treatment, or common shocks that due to the nature of treatment produce heterogeneous responses.

This section addresses each of these issues in turn, as well as further tests the validity of the results by conducting placebo tests for each specification.

Figures 12 and 11 show Colorado (in orange) relative to all other states (translucent blue) for each outcome. In each instance, Colorado resides within the convex hull formed by the other states. Colorado is above the median in male and Hispanic enrollment in figure 11, but otherwise resides near the median in all other measures.

Anticipatory effects prior to the implementation of the treatment come in two broad forms: first, elements of the legislation could be implemented prior to its formal passage, thereby changing the latent data generating process. Second, treated units may have prior knowledge of the planned policy, and begin to change behavior before the treatment is active. The bias from the first source – elements of treatment implemented prior to formal assignment – would bias estimates of the treatment effect towards zero. However, the COF was passed in May 2004 – with implementation beginning in 2006, with no provisions taking effect before then. Therefore, that facet of anticipatory effects is not applicable.

The second aspect of anticipatory effects, university knowledge of the upcoming policy despite their being no elements actually implemented yet, could bias estimates away from zero. The treatment effect would be overstated if the treatment was enacted to discourage a particular university behavior – say, men being underrepresented, as the law suggests. Universities could intensify this undesirable behavior knowing that the COF would disincentivize this, increasing the treatment effect in absolute terms due to universities preferentially enrolling women "while they still can".

It is expected that in the absence of any anticipatory behavior, Colorado would not meaningfully deviate from the synthetic counterfactual, therefore moving estimates of any treatment effect to zero due to the averaging of the effect curve.

To test if universities in Colorado are anticipating the passage of the COF, I run trend regressions on each outcome, allowing a trend break to occur in years leading up to 2006. The COF was passed in May 2004, and due to the timing of admissions and enrollment decisions by universities and students (typically January through April), this effectively leaves 2005 as the only year in which universities could alter behavior in anticipation of losing funding from the state in 2006. It is unclear when deliberation on the COF began, so I allow anticipatory effects to extend back to as early as 2002. I test if Colorado's trend break – if one exists at all – is statistically different from the rest of the country, which could indicate an anticipation of the COF. Formally, the regression I run is:

$$y_{it} = \alpha + \beta Trend_{it} + \Gamma Trend_{it} * CO_i + \Omega Trend_{it} * Break_t + \Theta Trend_{it} * Break_t * CO_i + \phi_i + u_{it}$$

where CO_i is a dummy variable equal to 1 if a university i is in Colorado, 0 otherwise. The variable $Break_t$ equals 1 if $t \geq \tau \in \{2002, 2003, 2004\}$, 0 otherwise. Θ is the coefficient of interest, particularly if it is statistically different than 0, which would indicate that the trend break Colorado experienced prior to 2006 is different than that of the rest of the country (due to the Colorado trend break incorporating both Ω and Θ). ϕ is an institution fixed effect, capturing time-invariant differences between the universities. Standard errors are clustered at the university level to allow for correlation between observations within a university. The data is subset to the years 1992-2005 for these regressions.

For measures that do have significantly different pre-trends ($\Theta \neq 0$), I re-run the synthetic control assigning treatment to that year – as suggested by Abadie (2021) – and comment on any changes in the estimated average treatment effects.

Tables 12 and 13 provide estimates from all pre-trend regressions. Table 12 contains the trend regressions for all enrollment specifications. Each column within each panel is a separate regression, allowing for a differential trend in the labeled "trend break year". Standard errors are below each point estimate, and clustered at the institution level to allow for correlation between observations within each university. The coefficient of interest within each specification is the Trend*Break*CO row, which would signal a differing trend for Colorado relative to all other states beginning in "break year" t. Among the two tables, only tuition revenue (top panel of table 13) in 2004 and all three nonresident tuition trends (right half of bottom panel of table 13) are statistically significant. Interestingly, all of these estimates are negative, indicating that Colorado was actually decreasing tuition revenue relative to the rest of the country in the lead up to 2006. This perhaps provides evidence that the COF was not enacted due to revenue-seeking behavior by universities, for they lagged behind the country in doing so! For the years where this Colorado trend break was significant – tuition revenue in 2004, nonresident tuition revenue in 2002, 2003, and 2004 – I re-estimate the synthetic control assigning treatment to that year (I also assign treatment to 2005 for both variables for completeness). These results are presented below.

Table 14 and figure 13 provide results assigning treatment to 2004 and 2005. Both estimates are lower than the baseline specification in table 9, but are statistically significant (5% for 2004 treatment, 1% for 2005 treatment). This lower point estimate in each case is driven by the synthetic counterfactual being greater than the observed revenue prior to and including 2005, which was suggested by the negative coefficient in table 13. It is clear that beginning in 2006 Colorado's tuition revenue markedly increases (due to the COF), and the visual evidence in figure 13 confirms that Colorado universities were not increasing revenue in anticipation of the coming funding shock. This is strong evidence that Colorado universities were not changing their behavior in anticipation of a policy that would strip them of funding.

Table 15 and figure 14 present results for nonresident tuition revenue. The story here is similar to aggregate tuition revenue above: all points estimates in table 15 are lower than the baseline specification in table 11, and all are significant at the 5% level. Figure 14 shows that these lower point estimates are due to synthetic Colorado being greater than Colorado for years up to and including 2005 – which was what table 13 found. The tuition revenue increase from 2005 to 2006 is partially due to how I construct tuition revenue once I have COF usage information in 2006, but all nonresident tuition revenue measures prior to that are identically constructed – I am not simulating COF uptake or a stipend in any year prior to 2006.[12] This appears to be strong evidence once again that Colorado was not anticipating the COF by extracting more revenue from nonresident students.

Spillover effects of the College Opportunity Fund would entail changing behavior either by Colorado residents or Colorado universities impacting other states around them. Colorado residents deciding to attend school in Colorado due to the stipend could negatively affect a state like Wyoming's ability to enroll nonresident students. This could bias an estimate of nonresident tuition revenue towards zero due to resident tuition revenue increasing in Colorado through more residents deciding to attend school in state (indirectly decreasing nonresident tuition revenue in Colorado), and a direct decrease in nonresident tuition revenue in Wyoming due to lack of demand from Colorado residents.

On the other hand, a change in behavior by Colorado universities could percolate to other states. If Colorado universities begin to favor nonresident students to recoup the lost revenue from

[12]See equation (3) in section 4 for details on tuition revenue construction by residency status.

the state, this could impact the available resident pool in a neighboring state which is now sending more students to Colorado than they otherwise would have. In both states, nonresident tuition revenue would increase – albeit for different reasons, biasing estimates towards zero.

To address these concerns of spillover effects, I re-run the synthetic control for all measures under two different specifications: first, I eliminate the top five states to which Colorado sends students. In the second model, I drop the top five states which send students to Colorado. These measures are computed aggregating first-year enrollment counts across the years 2006 through 2009 to measure initial responses of the policy.

Tables 16 and 17 – and their associated figures 15 and 16, respectively – drop the top five states that enroll the most Coloradans. These states are: Wyoming (who enrolls the most by a large margin), Arizona, Kansas, Nebraska, and Washington. This suggests that Coloradans do not move too far from home when deciding to enroll out of state.

The point estimates in table 16 are nearly identical to their baseline values, the only differences being that the Hispanic enrollment estimate is no longer significant at the 1% level and that the White enrollment estimate changed by 0.1 percentage points. The results in table 17 are remarkably similar, but not identical, to the baseline coefficients. The resident tuition revenue coefficient in the middle column decreases to 0.189 log points (from 0.204 in table 10), but the other estimates are almost identical to their baseline coefficients.

The robustness of the results of tables 16 and 17 are indicative of two possible phenomena, which are not mutually exclusive: first, any contamination of the average treatment effect estimates due to spillover of Colorado residents systematically changing where they choose to enroll is minimal. Second, the states where Coloradans primarily choose to enroll if they decide to leave Colorado are different than Colorado, making them poor fits to include in the construction of synthetic counterfactuals. For all measures, Nebraska is the only excluded state that was present in the top ten contributors, with the University of Nebraska - Kearney contributing 11.2% to the African American enrollment synthetic counterfactual (see table 7). Aside from this instance, states where Colorado sends the most students to are omitted from the estimates, making concerns of spillover effects negligible.

Tables 18 and 19, and their respective figures 17 and 18, drop the top five states that send the most students to Colorado – measured by Colorado's enrollment count of these students. These states are: Texas, Illinois, New Mexico (about 66% the enrollment of Illinois), Arizona, and Minnesota.

Table 18 presents results from the enrollment outcomes. The male and African American point estimates are nearly identical to their respective baseline coefficients, while the White enrollment estimate is larger in absolute terms than the result of table 4 – -0.045 percentage points here relative to the -0.041 percentage points earlier. The Hispanic enrollment estimate is very close to its baseline value, decreasing to 0.028 percentage points, but now is only significant at the 10% level. This relative imprecision could be attributed to losing Texas and Minnesota, the second and third largest contributors to the Hispanic enrollment synthetic counterfactual, and also the loss of over one thousand university-year observations by dropping those five states. However, the results of table 18 are broadly consistent with the main specifications, signaling that any spillover effects to states whose students emigrate to Colorado are minimal.

Table 19 contains the estimates for the tuition revenue measures. Aggregate tuition revenue – column 1 – is lower than the main result (0.308 log points), while resident tuition revenue – column 2 – is larger (0.243 log points) and more precisely estimated. The nonresident tuition revenue value in column 3 is about 20% lower than its baseline coefficient, 0.415 log points relative to 0.505 log points in table 11. These results are more sensitive to the exclusion of the biggest sending states, particularly Texas, as it has schools that contribute to the counterfactual for each revenue measure. Aggregate tuition revenue loses both Texas A&M and UT - Dallas, resident tuition revenue loses Texas A&M, and nonresident tuition revenue loses Stephen F Austin State, Texas A&M, Tarleton State University, and UT - Permian Basin. Considering the loss of these large donors to the respective synthetic counterfactuals, the tuition revenue estimates – particularly aggregate tuition revenue – are quite robust in that they are not markedly different from the main results, and all are still precisely estimated.

The results of tables 18 and 19 are more sensitive to the exclusion of the states who send the most students to Colorado, but whether this is due to spillover effects or the loss of over 10% of the sample in omitting these states is unclear. The enrollment estimates were largely unchanged, while the tuition revenue estimates did move, but this can be attributed to losing Texas, their primary donor. The tuition revenue results still confirm that Colorado increased tuition revenue as a result of the COF, although not to the degree as the main specification would suggest (a 36% increase relative to the counterfactual as opposed to a 42% increase in table 9).

The last threat to identification that will be discussed is the existence of shocks post-treatment. These shocks can be unique to either the treated or control group, or a common shock that due to the treatment produces heterogeneous responses. To the best of my knowledge, no other state passed legislation similar to the College Opportunity Fund after 2006 – that is, no other state has appropriations eliminated anywhere in my sample. There is also no evidence of any addenda to the COF or subsequent legislation that either further cut funding to Colorado universities, or provided additional funds to the universities. This is not to say that no shocks exist that are either treatment or control specific, just that I do not detect any anomalies that would suggest a change in the underlying data generating process.

The remaining threat to identification would be a common shock to all universities, notably The Great Recession, and the adverse effect this had on state funding of higher education. Among the control set, state responses varied widely. Most states decreased funding to universities, but states like Connecticut, Michigan, North Carolina, and North Dakota either kept appropriations comparable to their pre-recession levels or even increased them. Assuming the average university lost appropriations during the recession years, the literature suggests that they would substitute to extracting revenue from their student body (Jaquette and Curs (2015), Bound et al. (2016), Mathias (2022)). All else equal, this would bias estimates of tuition revenue increases from the COF downwards. The implementation of the COF in 2006 meant that state appropriations were not a viable revenue source for the state to cut during the recession, so quantifying a change in the propensity of Colorado universities to extract revenue from students due to the recession is difficult to differentiate from their response to the COF. Colorado did cut the stipend amount given to students during these years – see table 1 – so a change in student behavior due to decreased funding could lead to university enrollment and revenue characteristics changing.

To address this issue of The Great Recession potentially causing heterogeneous responses among the treated and control sets, I re-run the synthetic control for each outcome omitting the years 2008 through 2011. This means that the average treatment effect is estimated for 2006, 2007, and 2012 through 2019 – graphically the 2007 and 2012 estimates are connected with a straight line. The intent behind dropping these years is that by 2012 it is expected that state funding to universities has stabilized, eliminating potential volatility in university behavior due to funding uncertainty.[13]

Table 20 and figure 19 present the enrollment results dropping 2008 through 2011. The male and African American enrollment results are very close to their original specifications, with no change in statistical significance. Both the White and Hispanic enrollment estimates are larger in absolute terms, indicating that the donor schools increased enrollment of both demographics during the recession years relative to Colorado. If the recession years are biasing estimates of changes in

[13]Dropping through 2012 produces nearly identical results to dropping through 2011.

enrollment composition, it appears to be towards zero as Colorado's respective enrollment changes increase in absolute value with the omission of those years.

Table 21 and figure 20 examine tuition revenue changes when the recession years are dropped. All three estimates are close to their respective baseline specifications, but are slightly larger. This indicates that the control group was increasing tuition revenue at a higher rate than Colorado during the recession years, likely due to the funding cuts described above. While Colorado continued to increase tuition revenue, it does not appear that the recession provided universities with an opportunity to further leverage students above what the COF was already inducing them to do.

The results of tables 20 and 21 provide evidence that the recession years did not provide means for Colorado universities to increase tuition revenue or alter their enrollment meaningfully, relative to the counterfactual. If anything, their enrollment composition and revenue generation were steadier than other universities which were reacting to disinvestment by their respective legislatures. This solidifies that universities pull available levers to recoup lost funding – Colorado did it immediately after the COF was implemented, while other universities did so during the height of the recession.

As a last robustness check, I re-run the resident and nonresident tuition revenue synthetic control specifications assuming COF take-up is 100%. I discussed in section 5 that my approximations of resident and nonresident enrollment at the university level were best guesses with data available to me, and that measurement error there could cascade to impact the tuition revenue measures. This approximation of enrollment by residency status was necessary due to COF stipend usage being available only at the university level, and that take-up cannot be 100%. The stipend is available to students for only eight semesters, and requires explicit authorization from the student that she would like to use the stipend for the term in which she is enrolling. Any student who authorizes the stipend for a summer course, or does not graduate in four years, or either wishes to not use it during a "light" academic term (or forgets to authorize it) would necessarily move usage away from 100%. My approximation of 80% take-up appears reasonable, but if this percentage is too low it would overstate revenue coming from resident students.

Table 22 and figure 21 provide estimates for resident and nonresident tuition revenue assuming COF take-up was 100% among resident students.[14] These results are presented with the understanding that resident tuition revenue will be understated, and therefore nonresident tuition revenue will be overstated. This is clear from table 22, where resident tuition is much smaller (0.071 log

[14] Aggregate tuition revenue is unaffected since it is a variable IPEDS provides.

points) and statistically insignificant. Nonresident tuition is much larger at 0.682 log points, which is to say that nonresident tuition increased by 98% relative to the counterfactual. What is clear from the left pane of figure 21 is that even under the best case scenario, the decrease in resident tuition revenue was fleeting – falling dramatically in 2006 before becoming greater than the counterfactual in 2009. While my baseline estimates of resident tuition revenue may be overstated due to not precisely measuring COF usage, assuming universal up-take of the stipend still finds that resident students paid no less to Colorado universities than they would have had the COF not been enacted.

Figures 22 and 23 show results from placebo tests where I assign the treatment to every other state in the sample. Each line on each graph represents a state's average treatment effect on the treated, measured by its observed value minus the synthetic counterfactual for each year. For outcomes where a significant effect was detected – White enrollment, Hispanic enrollment, the tuition revenue measures – it is expected that Colorado's effect curve (orange in each graph) would be an outlier relative to the other states' simulated treatment effects (translucent blue). If this is not the case, i.e. some states have more extreme outcomes than Colorado despite no treatment, this would cast doubt on the identifying assumption that the College Opportunity Fund changed the data generating process Colorado universities.

I calculate the root mean-squared error (RMSE) of the yearly deviation between each "treated" state and its synthetic counterfactual in the 1992-2005 pre-period, dropping states whose RMSE lies above the 40^{th} percentile.[15] This process eliminates states whose pre-trend fit is noisy, which could lead to large post-treatment effects which are purely a function of poor pre-treatment fit.

Figure 22 displays the placebo test results for the four enrollment outcomes. The "Percent White" and "Percent Hispanic" tests are of particular interest, since the baseline synthetic control results were both economically and statistically significant.

The top right pane of figure 22 shows Colorado's average treatment effect on the treated for White enrollment composition relative to placebo treatment of other states with sufficiently good pre-treatment fits. There are four states who treatment effects dip below that of Colorado by the end of the sample: Connecticut, Nevada, Texas, and Washington. Nevada's placebo line finishes at -0.08, but is as high as 0.03 earlier in the sample. Running the synthetic control with Nevada as treated yields an average treatment effect of -2 percentage points, which is statistically insignificant, so it can be discarded as a more extreme outcome than Colorado. However, Connecticut, Texas, and

[15] This is chosen to leave about 20 states with good pre-trend fits.

Washington are statistically significant and have larger average treatment effects than Colorado. This is because Colorado's marked decline in White enrollment composition – 73% in 2005 to 57% in 2019 – is similar to the White enrollment decreases in Connecticut, Texas, and Washington. Connecticut's White enrollment percent was 75% in 2005 and fell to 58% in 2019; Texas's enrollment dropped from 58% in 2005 to 40% in 2019; and Washington's White enrollment composition moved from 69% in 2005 to 53% in 2019. That is to say, Colorado's enrollment after 2005 became more diverse, but was not unique among general enrollment trends in the country. The COF may have pushed Colorado universities to enroll fewer White students, but attributing the decline entirely to its passage ignores broader enrollment trends in the United States.

The bottom left pane of figure 22 presents Colorado and the placebo states for the Hispanic enrollment outcome. Colorado's treatment effect is much larger than all other states, finishing at over 6 percentage points in 2019. The placebo states have a reasonably tight dispersion, with no treatment effect finishing over 3 percentage points. This is indicative that the College Opportunity Fund changed Colorado's propensity to enroll Hispanic students beyond the general enrollment trends of other states in the US.

Figure 23 presents placebo test results for the three tuition revenue measures. For each measure – particularly aggregate tuition revenue (top pane) and nonresident tuition revenue (bottom pane) – Colorado's increase in tuition revenue is an outlier compared to the rest of the country. For aggregate tuition revenue, there is a clear increase increase in the treatment effect which persists until 2019. Resident tuition revenue (middle pane) does not increase above all states until 2011, where it stays through the rest of the sample. Nonresident tuition revenue increases sharply in 2006 and remains higher than the other placebo states until 2017, when it is passed by Indiana. Indiana's average treatment effect is 0.337 log points, which is a noticeable increase, but far lower than the 0.505 log points average treatment effect of Colorado.

This section has tested the robustness of the main synthetic control results to different specifications which could bias the estimates. This included showing that Colorado was squarely within the convex hull of the donor set, testing if anticipatory effects existed, examining spillover effects between Colorado and other states, and testing if the Great Recession produced heterogeneous responses which could bias in estimation of the average treatment effect. In each instance, the baseline results were robust to these potential confounders. The placebo tests for each outcome found that Hispanic enrollment and the tuition revenue measures were more extreme than random assignment of the treatment to other states, confirming their validity.

7 Conclusion

This paper examined the effect of Colorado's College Opportunity Fund on enrollment outcomes and tuition revenue. I use data from IPEDS spanning the years 1992 through 2019 comprising 270 four-year public universities, 12 of which are located in Colorado. Synthetic control estimates find Hispanic enrollment increased by 3.1 percentage points relative to the counterfactual, and this result is statistically significant at the 1% level. Tuition revenue increases by 0.353 log points, or 42%, relative to synthetic Colorado (significant at the 1% level). I approximate the relative share of this increase coming from resident and nonresident students. Despite the reduction in tuition paid with the stipend of the COF, resident enrollment increases by 0.203 log points (22.5%) over the synthetic counterfactual. This result is significant at the 10% level. Nonresident enrollment increases by 0.505 log points, or 66%, and is significant at the 1% level. Placebo tests assigning treatment to other states confirms that Colorado's treatment effect in these outcomes is an outlier compared to the rest of the country, supporting the validity of the design.

This paper is the first to examine state funding and its affect on university behavior through a quasi-experimental design. Prior work has sought to create exogenous variation in state funding through instrumental variable designs, which are subject to how well the author argues that their respective instrument – usually aggregated state funding – does not violate the exclusion restriction. The author hopes that this work will spur others in the field to find more state policies which will provide more opportunities to employ quasi-experimental designs.

There is still much fruitful research to be done in this area. A push towards more granular data can elucidate questions that this paper and others cannot answer. Something as simple as clear measures of how many total resident and nonresident students a university enrolls – not just at the first-year level – would be invaluable to this work and others in answering enrollment questions. Better defined measures of effective tuition rates paid by students could answer questions of pass-through of revenue shocks to universities and give a clearer answer to how much more Colorado residents actually paid in tuition despite the COF stipend.

This paper is policy-relevant in that it provides evidence of the unintended consequences of well-intentioned state policies. The COF has been in effect for seventeen years, and it is clear that it did not achieve all it set out to accomplish. Hispanic enrollment in Colorado increased markedly, which is a real victory for the College Opportunity Fund and for diversity in Colorado higher education – but African Americans are still woefully underrepresented, and students are paying more in tuition to universities than they otherwise would have. Providing education stipends for Colorado residents is admirable, but the larger issue of dis-investing in higher education incentivizes universities to recoup revenue from students, residents included. This work and the literature before it have established that universities are rational economic agents, and have the will and means to self-fund from students should the need arise. Giving money to universities through state appropriations appears to make that need arise less frequently. If legislators desire that public universities better serve residents and do so in a cost-effective manner, policies like the College Opportunity Fund need adequate funding to achieve their stated goals.

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Table 1: COF Funding per Credit Hour

Year	Nominal	Real
2006	\$86	\$117.53
2007	\$89	\$118.27
2008	\$68	\$86.10
2009	\$44	\$54.49
2010	\$62	\$76.10
2011	\$62	\$74.36
2012	\$62	\$73.14
2013	\$64	\$74.34
2014	\$75	\$84.59
2015	\$75	\$82.91
2016	\$75	\$81.81
2017	\$77	\$81.25
2018	\$85	\$87.14
2019	\$94	\$94

Real values are deflated to 2019 dollars using the Higher Education Price Index (HEPI), described in the data section.

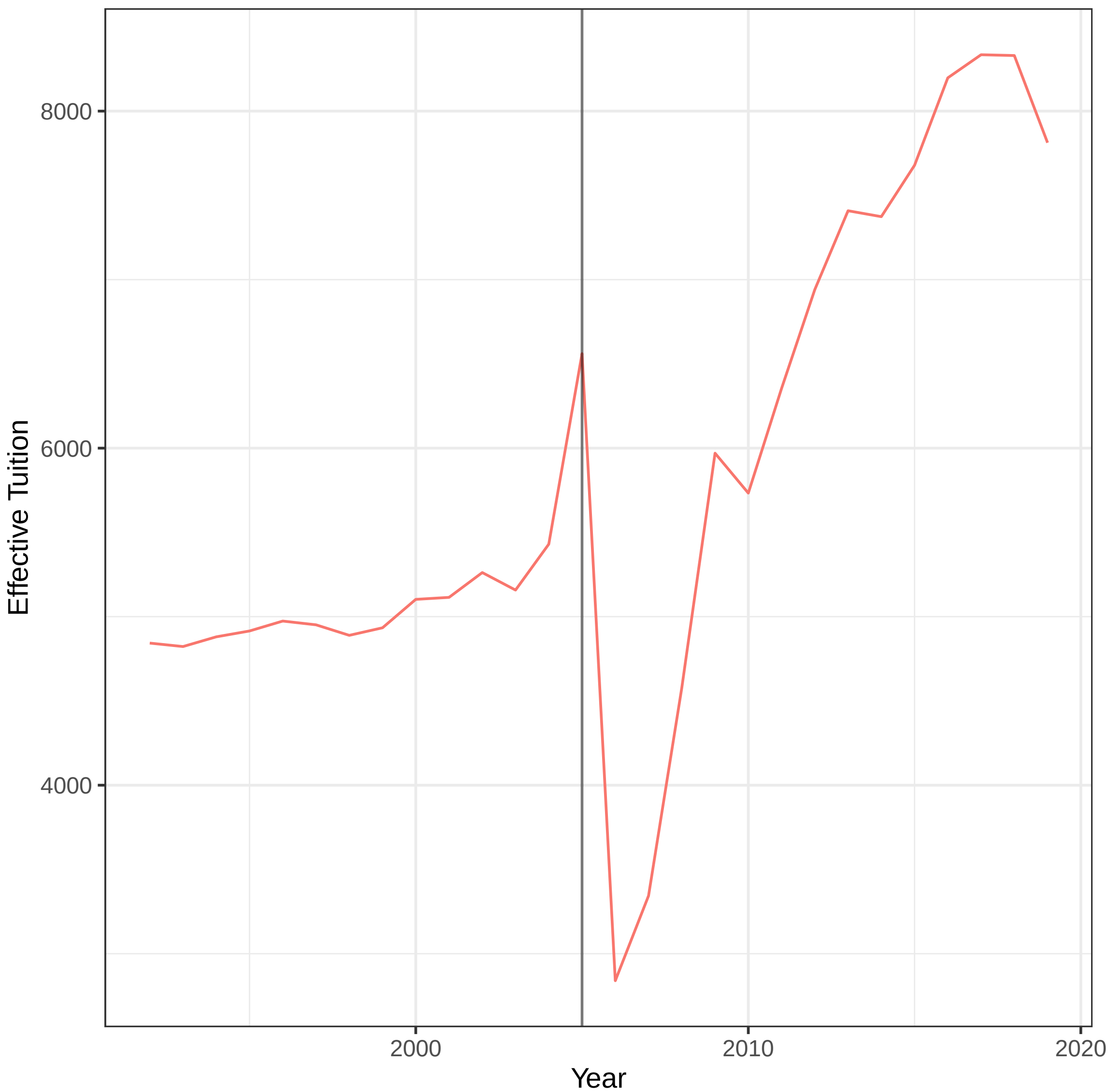


Figure 1: Colorado Effective Resident Tuition

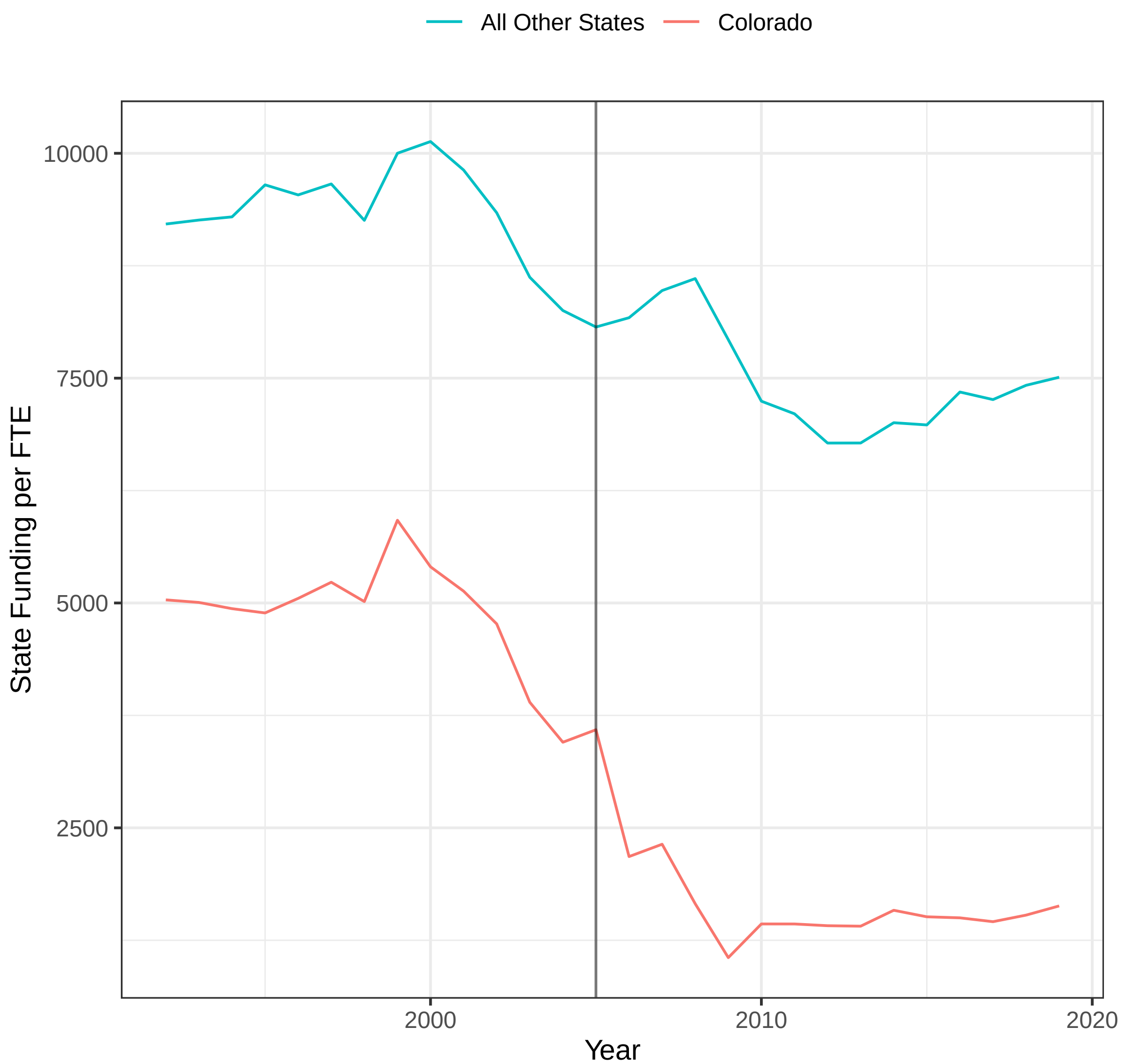


Figure 2: Effective State Funding per FTE

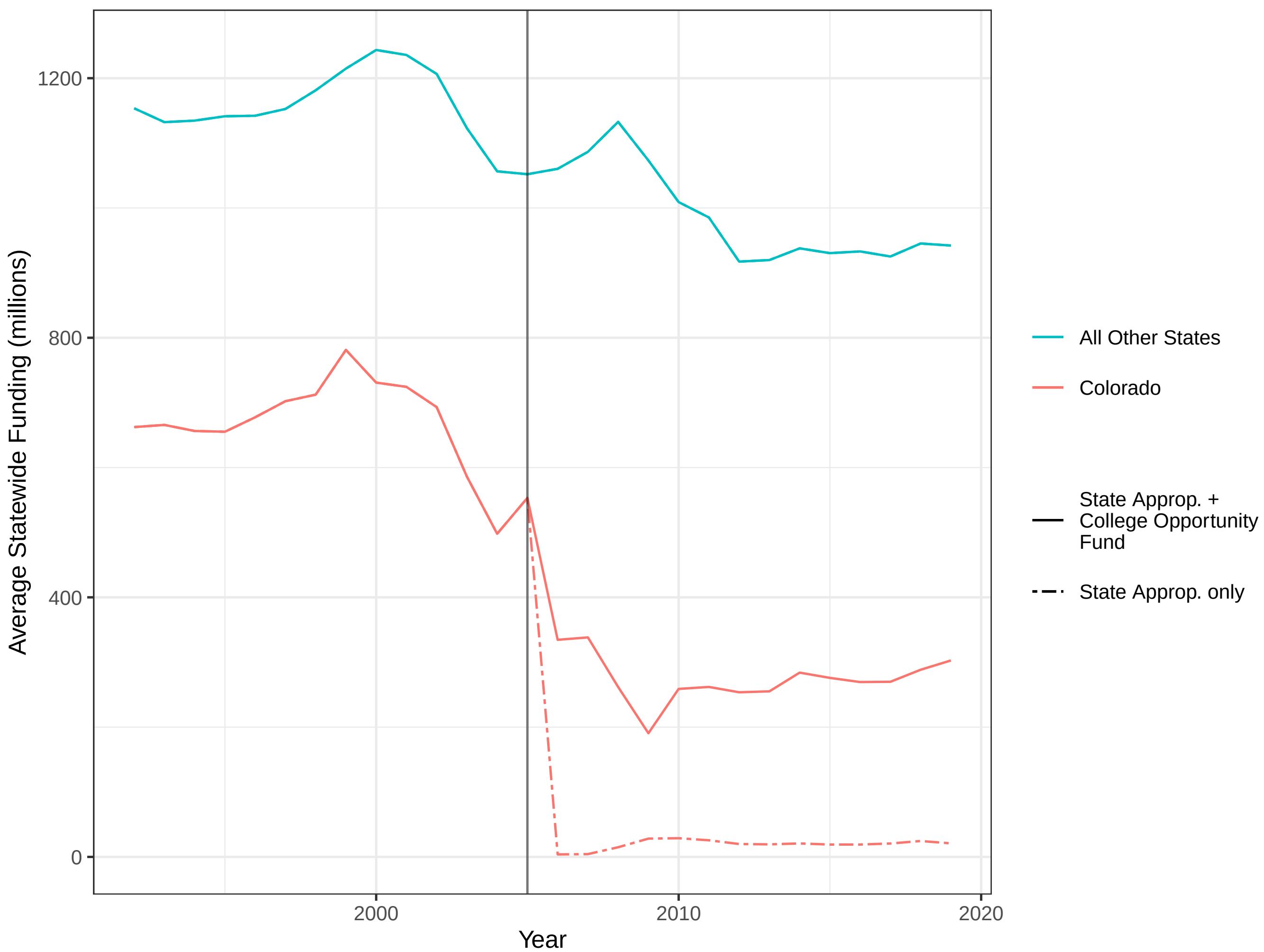


Figure 3: Average Statewide Funding

Table 2: Male Enrollment (%)

Average Treatment Effect on Treated			0.022
			(0.015)
Top 10 Donor Unit Weights			
Louisiana Tech	0.191	Coppin State College (MD)	0.057
Valdosta State (GA)	0.091	IUPU-Fort Wayne	0.043
Univ. Wisconsin-Superior	0.090	NC School of the Arts	0.041
Virginia Military Ins.	0.087	Univ. of North Alabama	0.039
Lock Haven Univ. (PA)	0.076	Longwood University (VA)	0.030

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

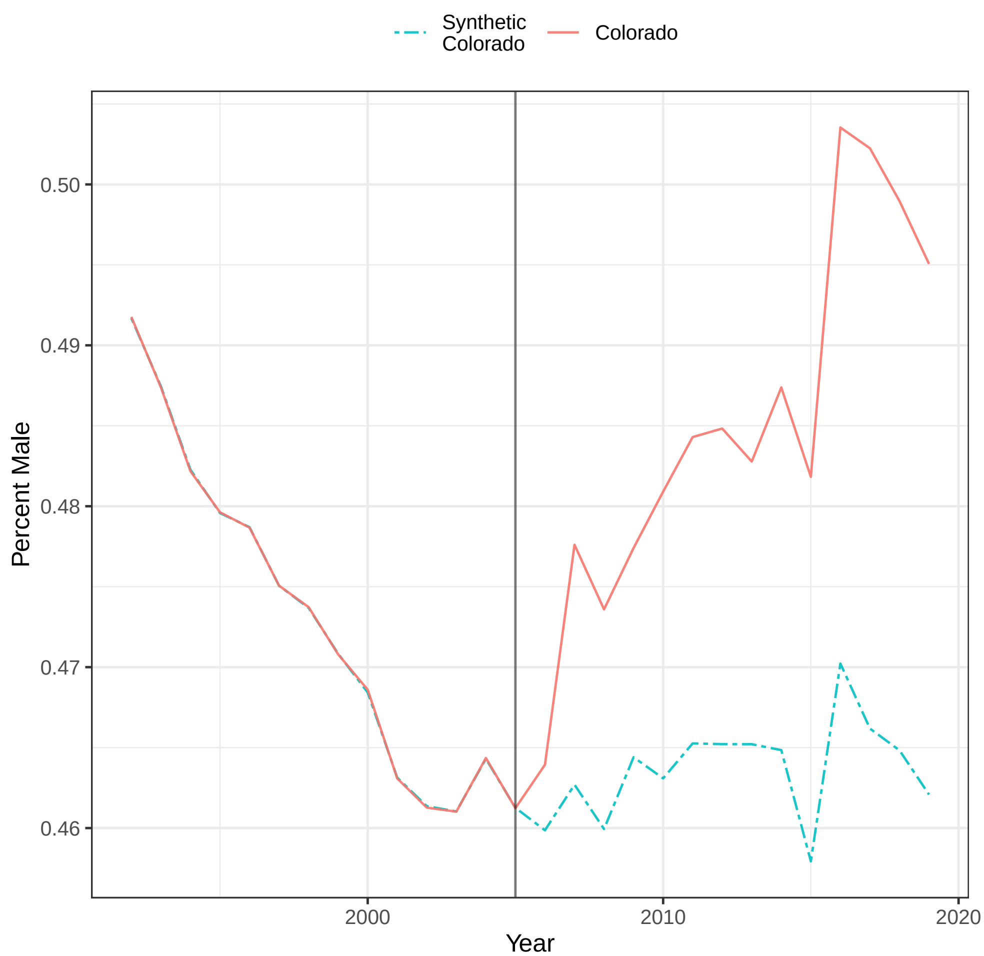


Figure 4: Male Enrollment (%)

Table 3: Male Enrollment Differences

Year	Undergrad Composition	State Composition	p-value
2000	0.458	0.503	0.084
2001	0.454	0.499	0.088
2002	0.454	0.500	0.080
2003	0.455	0.503	0.079
2004	0.456	0.502	0.093
2005	0.451	0.501	0.079
2006	0.454	0.503	0.086
2007	0.466	0.504	0.127
2008	0.465	0.504	0.104
2009	0.468	0.503	0.124
2010	0.473	0.502	0.200
2011	0.476	0.502	0.253
2012	0.478	0.502	0.295
2013	0.478	0.503	0.279
2014	0.482	0.502	0.380
2015	0.479	0.503	0.318
2016	0.490	0.504	0.481
2017	0.486	0.503	0.413
2018	0.485	0.503	0.402
2019	0.485	0.505	0.322

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Undergraduate enrollment compositions are weighted by university size. The state composition comes from the ACS and is treated as the true population parameter in the t-tests.

Table 4: White Enrollment (%)

Average Treatment Effect on Treated			-0.041***
			(0.015)
Top 10 Donor Unit Weights			
Indiana Univ.-Southeast	0.184	Mass. College of Art	0.055
Calif. Univ. of Penn.	0.087	Morgan State Univ. (MD)	0.050
Univ. of Southern Maine	0.087	Johnson State College (VT)	0.039
Plymouth State College (NH)	0.077	Maine Maritime Acad.	0.038
University of Utah	0.061	Univ. of Virginia	0.037

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

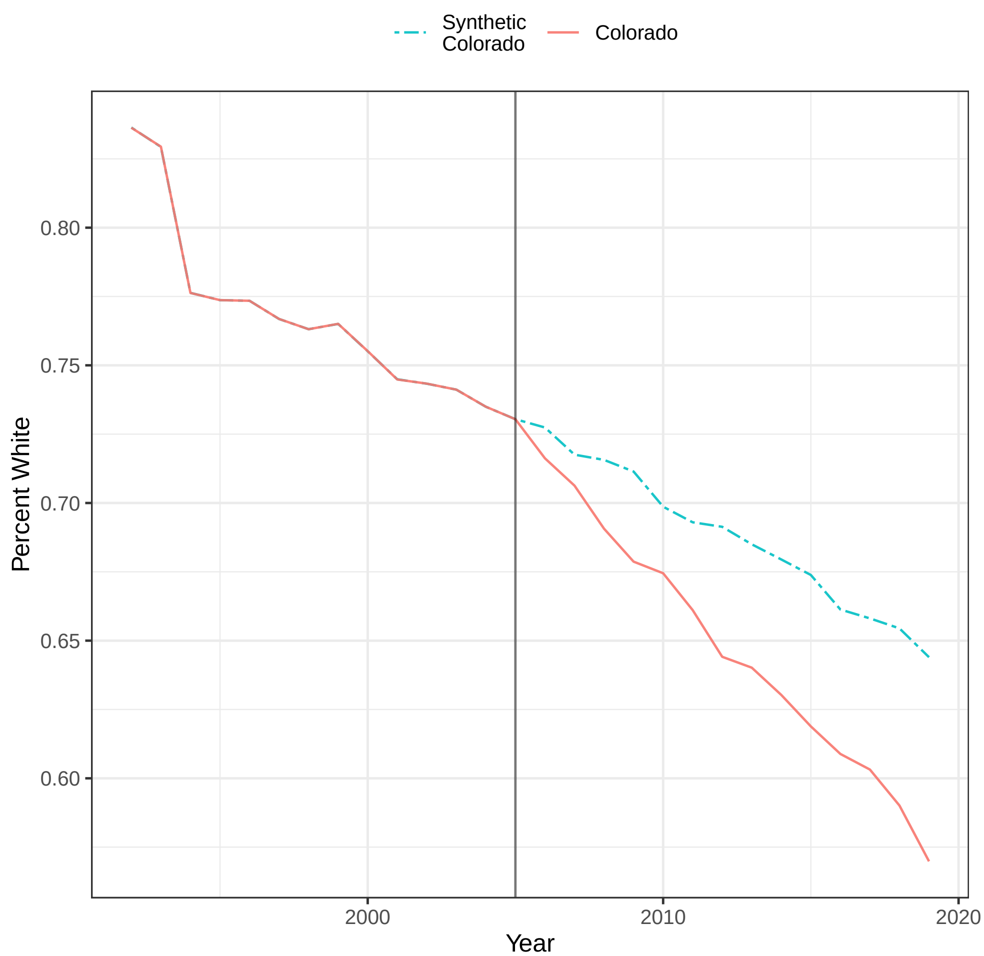


Figure 5: White Enrollment (%)

Table 5: Hispanic Enrollment (%)

Average Treatment Effect on Treated			0.030***
			(0.011)
Top 10 Donor Unit Weights			
Jackson State Univ. (MS)	0.411	Valley City State Univ. (ND)	0.053
Texas A&M-Kingsville	0.098	Troy State University (AL)	0.050
Univ. of Minn.-Morris	0.090	Louisiana State-Shreveport	0.041
NC School of the Arts	0.077	Framingham State Coll. (MA)	0.038
Georgia State Univ.	0.064		

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

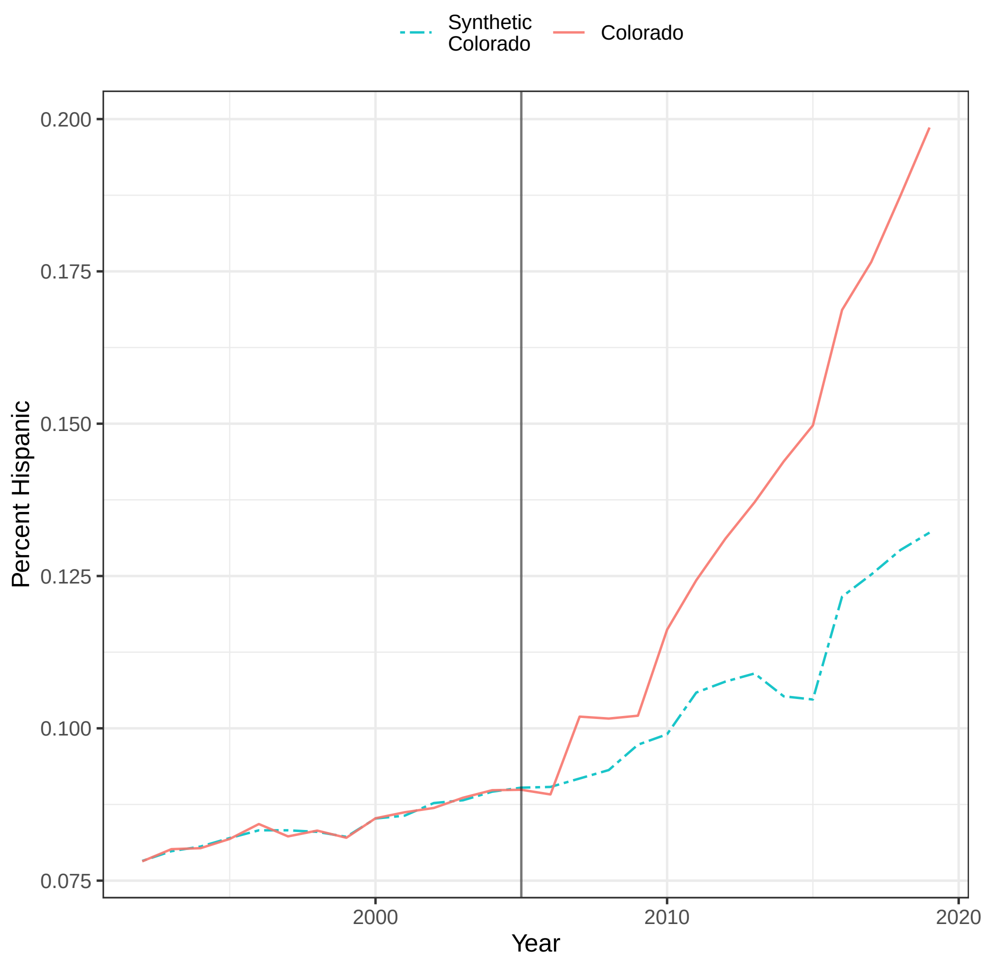


Figure 6: Hispanic Enrollment (%)

Table 6: Hispanic Enrollment Differences

Year	Undergrad Composition	State Composition	p-value
2000	0.080	0.174	<0.01
2001	0.079	0.176	<0.01
2002	0.080	0.182	<0.01
2003	0.082	0.187	<0.01
2004	0.084	0.192	<0.01
2005	0.085	0.197	<0.01
2006	0.085	0.195	<0.01
2007	0.088	0.199	<0.01
2008	0.089	0.201	<0.01
2009	0.090	0.202	<0.01
2010	0.105	0.208	<0.01
2011	0.114	0.209	<0.01
2012	0.121	0.210	<0.01
2013	0.128	0.210	<0.01
2014	0.134	0.212	<0.01
2015	0.142	0.214	<0.01
2016	0.161	0.213	0.011
2017	0.169	0.215	0.025
2018	0.181	0.217	0.086
2019	0.191	0.218	0.207

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Undergraduate enrollment compositions are weighted by university size. The state composition comes from the ACS and is treated as the true population parameter in the t-tests.

Table 7: African American Enrollment (%)

Average Treatment Effect on Treated			-0.002
			(0.003)
Top 10 Donor Unit Weights			
Castleton State College (VT)	0.243	Maine Maritime Academy	0.045
Univ. of Wisconsin-La Crosse	0.174	Plymouth State College (NH)	0.043
Univ. of Nebraska-Kearney	0.112	Lock Haven Univ. of Penn.	0.043
Mass. College of Liberal Arts	0.056	Massachusetts College of Art	0.026
Univ. of Vermont & State Ag. Coll.	0.049	Massachusetts Maritime Acad.	0.026

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

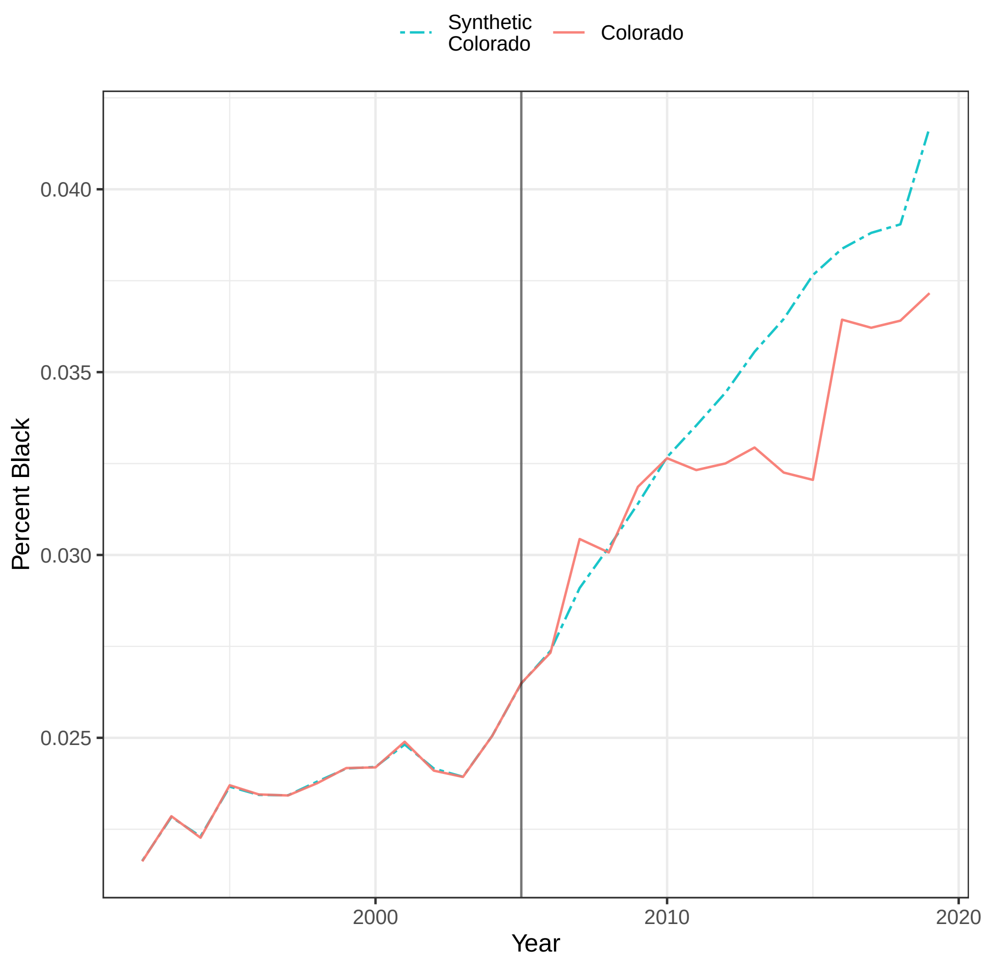


Figure 7: African American Enrollment (%)

Table 8: African American Enrollment Differences

Year	Undergrad Composition	State Composition	p-value
2000	0.026	0.043	0.004
2001	0.026	0.036	0.043
2002	0.026	0.039	0.011
2003	0.026	0.042	0.004
2004	0.027	0.041	0.008
2005	0.028	0.040	0.026
2006	0.028	0.042	0.010
2007	0.030	0.042	0.021
2008	0.030	0.041	0.041
2009	0.032	0.044	0.032
2010	0.032	0.045	0.045
2011	0.032	0.045	0.032
2012	0.032	0.045	0.030
2013	0.032	0.047	0.011
2014	0.032	0.047	0.007
2015	0.032	0.047	0.006
2016	0.036	0.048	0.048
2017	0.036	0.047	0.062
2018	0.037	0.049	0.045
2019	0.036	0.047	0.076

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Undergraduate enrollment compositions are weighted by university size. The state composition comes from the ACS and is treated as the true population parameter in the t-tests.

Table 9: Log(Tuition Revenue)

Average Treatment Effect on Treated			0.353^{***}
			(0.081)
Top 10 Donor Unit Weights			
Montana State-Northern	0.161	Univ. of Wisconsin-River Falls	0.060
Texas A&M University	0.157	Central State University (OH)	0.056
Univ. of Mich.-Ann Arbor	0.145	University of Texas-Dallas	0.052
Georgia Southwestern State	0.072	Univ. of Maryland-Baltimore Cnty.	0.042
Univ. of Science and Arts of OK	0.065	Massachusetts Maritime Academy	0.040

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

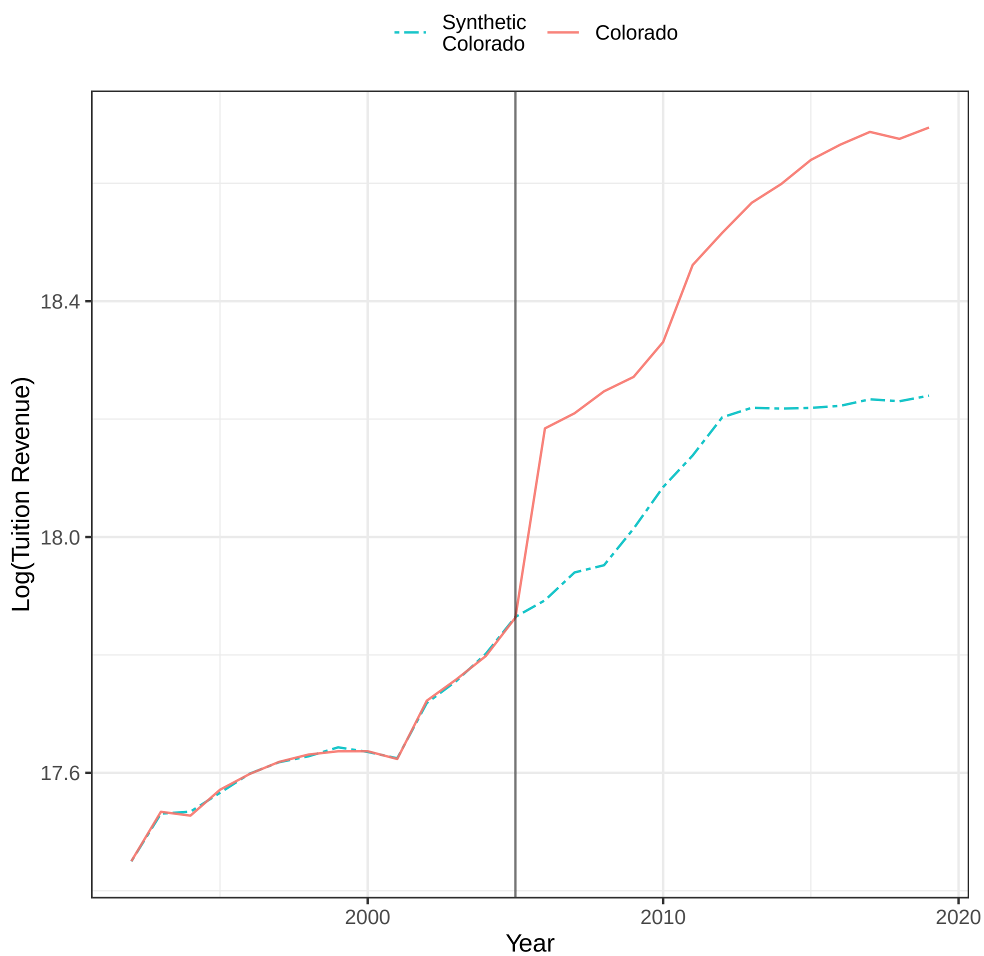


Figure 8: Tuition Revenue

Table 10: Log(Resident Tuition Revenue)

Average Treatment Effect on Treated			0.204*
			(0.118)
Top 10 Donor Unit Weights			
Texas A&M University	0.17	Central State University (OH)	0.049
Virginia State University	0.128	Univ. of Arts and Sciences of OK	0.045
Ohio State University	0.113	NC School of the Arts	0.044
North Carolina Central Univ.	0.10	Valley City State Univ. (ND)	0.040
Univ. of Arkansas-Pine Bluff	0.071	Western Connecticut State	0.040

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

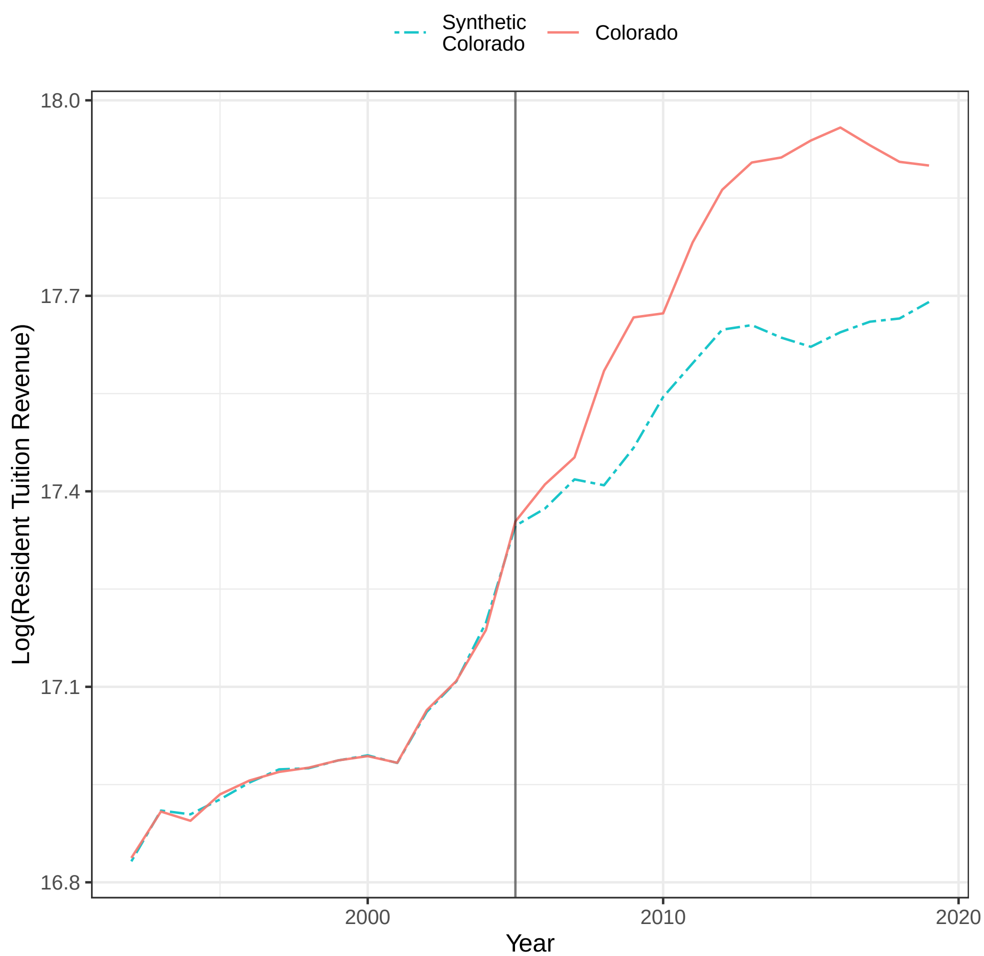


Figure 9: Resident Tuition Revenue

Table 11: Log(Nonresident Tuition Revenue)

Average Treatment Effect on Treated			0.505***
			(0.157)
Top 10 Donor Unit Weights			
Utah State University	0.21	Tarleton State Univ. (TX)	0.064
Univ. of Mich.-Ann Arbor	0.114	Western Carolina Univ.	0.052
Stephen F Austin State	0.095	NC State Univ.-Raleigh	0.047
Texas A&M University	0.080	Univ. of Texas-Permian Basin	0.042
Appalachian State Univ.	0.069	Massachusetts Maritime Acad.	0.041

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

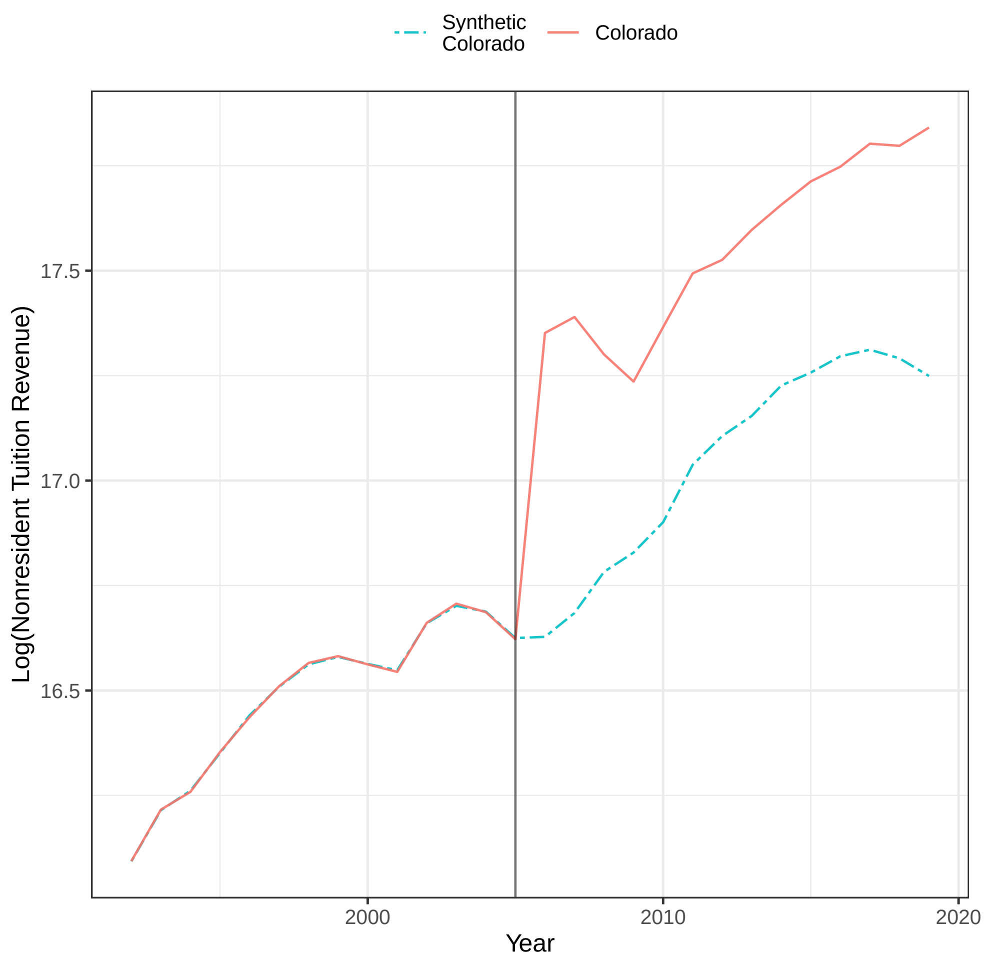


Figure 10: Nonresident Tuition Revenue

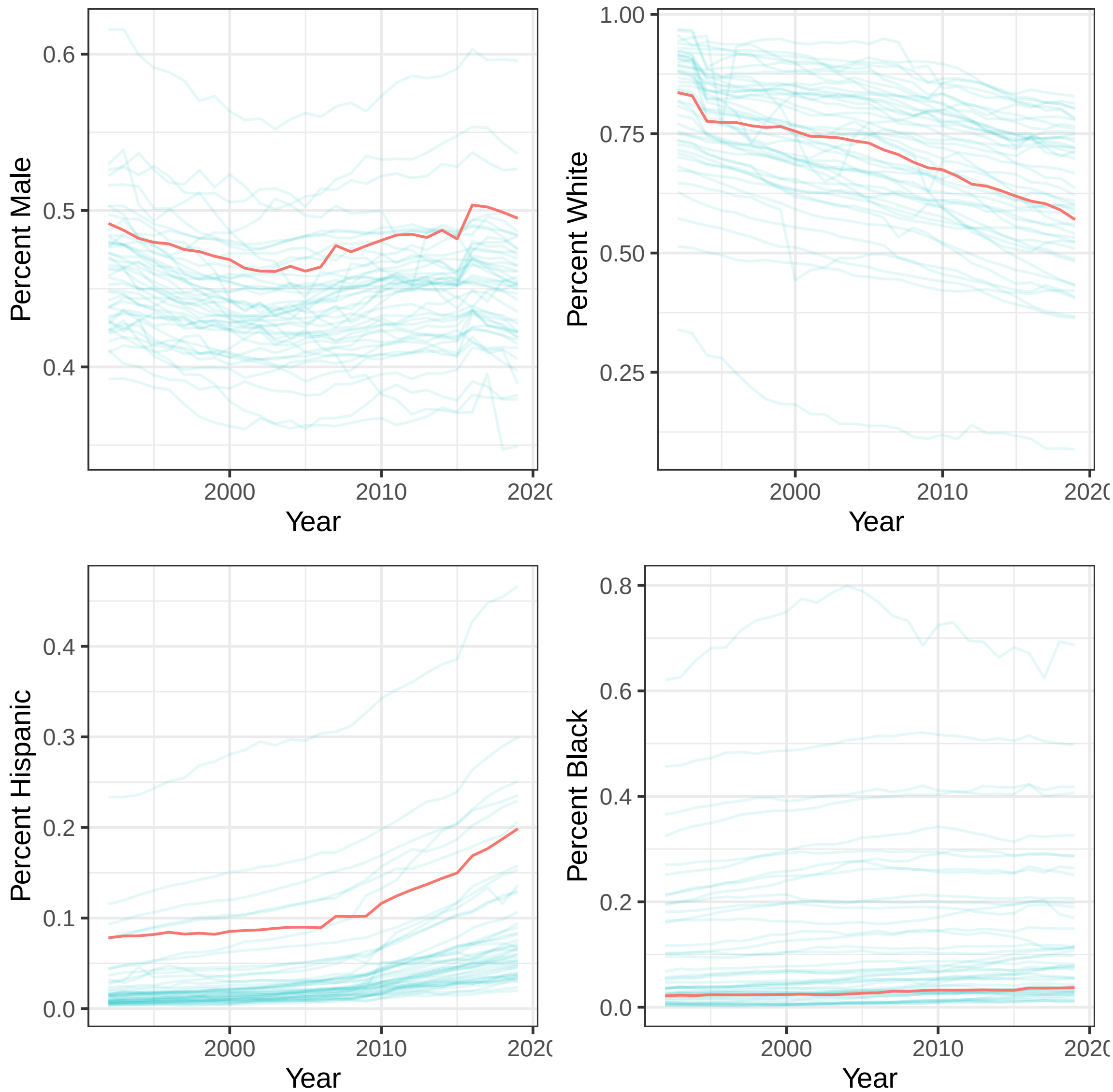


Figure 11: Enrollment Measures

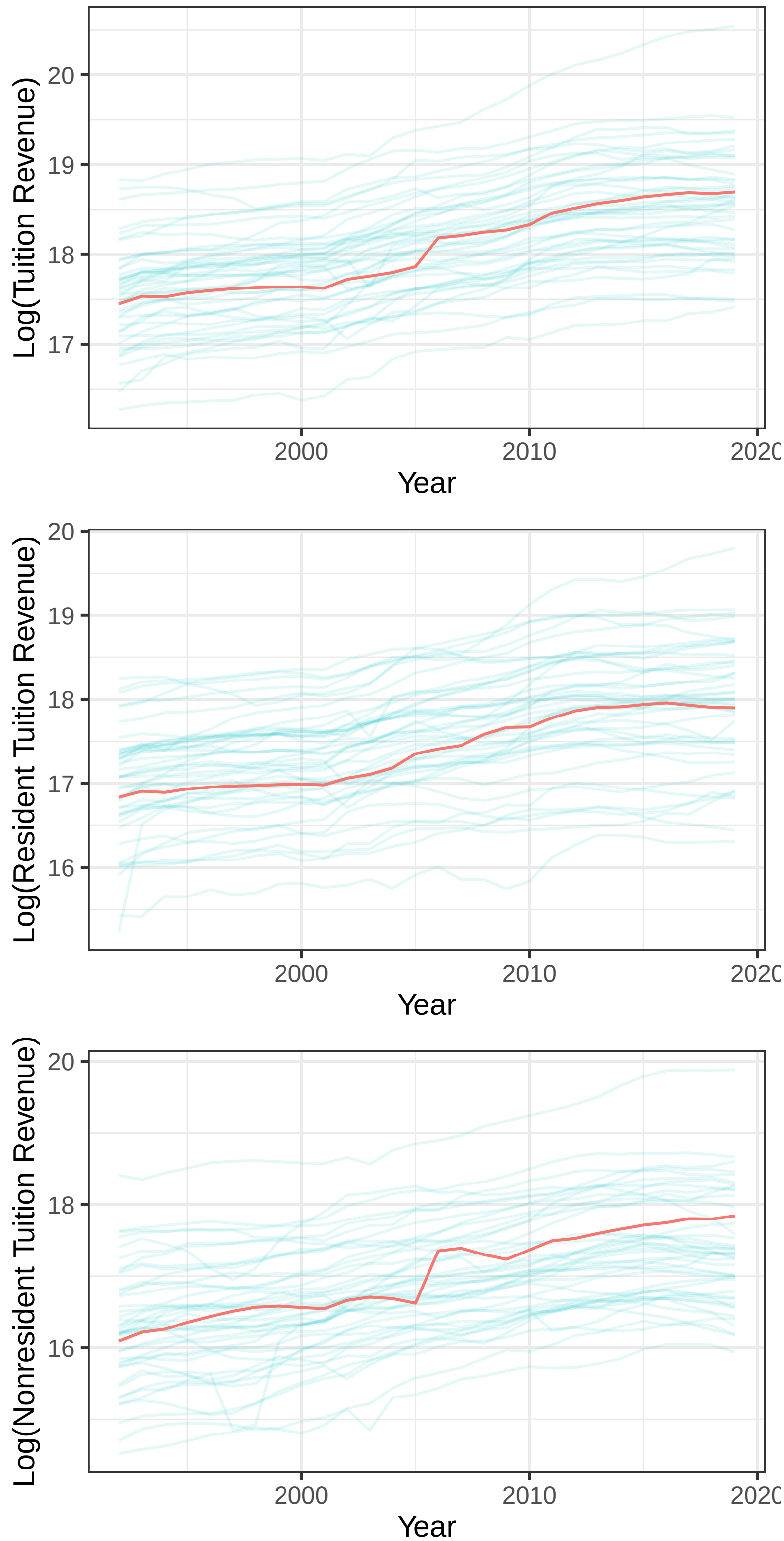


Figure 12: Tuition Revenue Measures

Table 12: Enrollment Anticipatory Effects

	Male Enrollment				White Enrollment		
Trend Break Year	2002	2003	2004	**Trend Break Year**	2002	2003	2004
Trend	-0.0025***	-0.0024***	-0.0022***	Trend	-0.0079***	-0.0076***	-0.0073***
	(0.00017)	(0.00017)	(0.00016)		(0.00049)	(0.00046)	(0.00043)
Trend*CO	-0.00015	-0.00043	-0.00055	Trend*CO	-0.00062	-0.00050	-0.00029
	(0.00069)	(0.00062)	(0.00058)		(0.0017)	(0.0015)	(0.0013)
Trend*Break	0.00069***	0.00062***	0.00057***	Trend*Break	0.0011***	0.00093***	0.00075***
	(0.000083)	(0.000074)	(0.000068)		(0.00025)	(0.00022)	(0.00020)
Trend*Break*CO	-0.0042	-0.0013	0.00051	Trend*Break*CO	0.0061	0.0058	0.0039
	(0.0044)	(0.0040)	(0.0038)		(0.010)	(0.010)	(0.010)
	Hispanic Enrollment				African American Enrollment		
Trend Break Year	2002	2003	2004	**Trend Break Year**	2002	2003	2004
Trend	0.0011***	0.0010***	0.0010***	Trend	0.0020***	0.0020***	0.0019***
	(0.00014)	(0.00013)	(0.00013)		(0.00024)	(0.00024)	(0.00023)
Trend*CO	-0.00033	-0.00031	-0.00024	Trend*CO	-0.0017***	-0.0017***	-0.0017***
	(0.00054)	(0.00046)	(0.00042)		(0.00030)	(0.00029)	(0.00028)
Trend*Break	-0.0000010	0.000018	0.000045	Trend*Break	-0.00023***	-0.00019**	-0.00011*
	(0.000032)	(0.000032)	(0.000030)		(0.000086)	(0.000076)	(0.000067)
Trend*Break*CO	0.0013	0.0012	0.00050	Trend*Break*CO	0.0018	0.0020	0.0022
	(0.0029)	(0.0022)	(0.0021)		(0.0013)	(0.0014)	(0.0013)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and clustered at the institution level.

Table 13: Tuition Revenue Anticipatory Effects

	Log(Tuition Revenue)		
Trend Break Year	2002	2003	2004
Trend	0.030***	0.030***	0.032***
	(0.0017)	(0.0017)	(0.0017)
Trend*CO	-0.011***	-0.0093**	-0.0098*
	(0.0036)	(0.0041)	(0.0050)
Trend*Break (Ω)	0.0079***	0.0095***	0.0099***
	(0.0011)	(0.00085)	(0.00072)
Trend*Break*CO (Θ)	-0.024	-0.046	-0.055**
	(0.041)	(0.032)	(0.026)

	Log(Resident Tuition Revenue)				Log(Nonresident Tuition Revenue)		
Trend Break Year	2002	2003	2004	**Trend Break Year**	2002	2003	2004
Trend	0.033***	0.031***	0.032***	Trend	0.037***	0.042***	0.045***
	(0.0048)	(0.0041)	(0.0036)		(0.0040)	(0.0038)	(0.0037)
Trend*CO	-0.014**	-0.012**	-0.012**	Trend*CO	0.011	0.0080	0.0043
	(0.0063)	(0.0058)	(0.0055)		(0.018)	(0.018)	(0.018)
Trend*Break	0.0044*	0.0077***	0.0091***	Trend*Break	0.014***	0.011***	0.0093***
	(0.15)	(0.19)	(0.25)		(0.0029)	(0.0022)	(0.0017)
Trend*Break*CO	0.047	0.039	0.053	Trend*Break*CO	-0.24***	-0.24***	-0.27***
	(0.052)	(0.051)	(0.049)		(0.069)	(0.066)	(0.067)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and clustered at the institution level.

Table 14: Tuition Revenue Anticipation – SC Estimates

	Log(Tuition Revenue)	
"Treated Year"	2004	2005
ATET	0.166**	0.280***
	(0.084)	(0.091)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

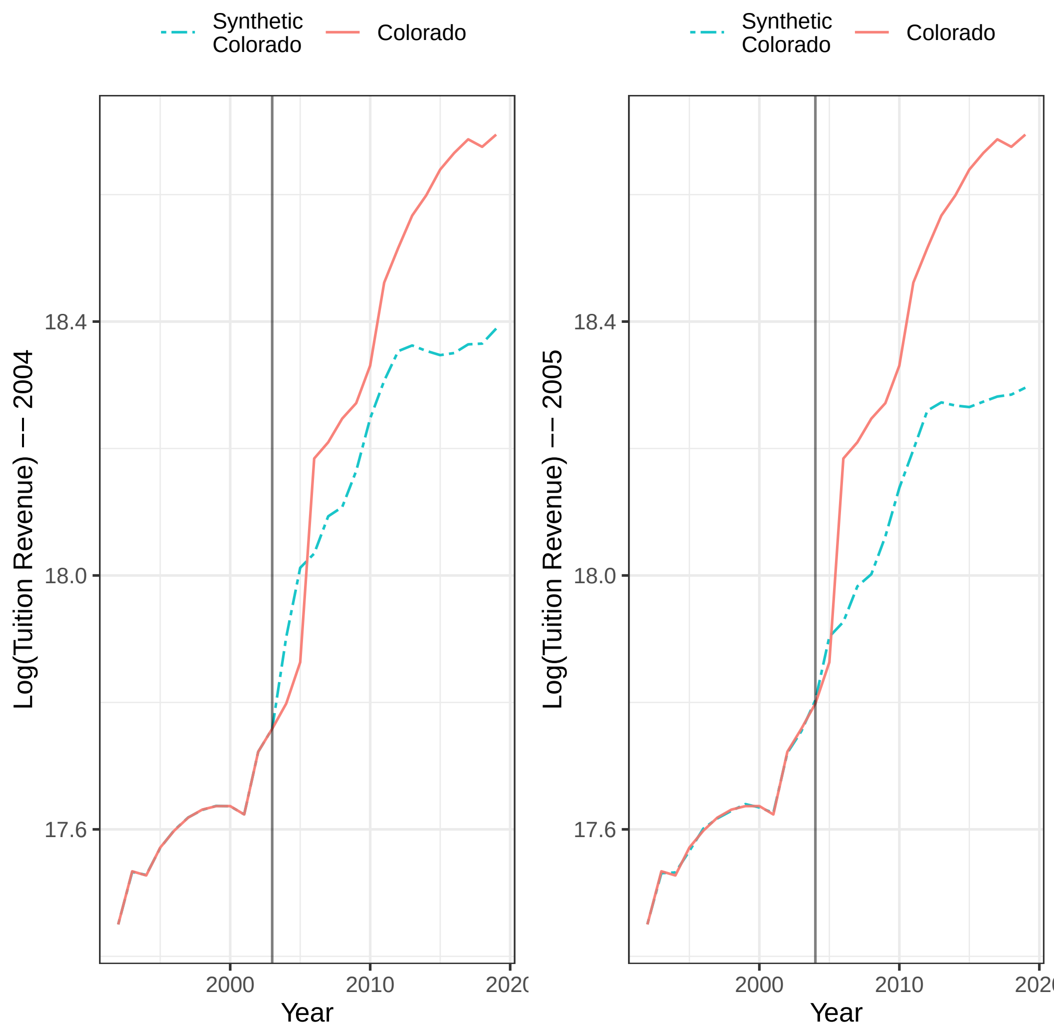


Figure 13: Tuition Revenue Anticipation

Table 15: Nonresident Tuition Revenue Anticipation – SC Estimates

	Log(Nonresident Tuition Revenue)			
"Treated Year"	2002	2003	2004	2005
ATET	0.294**	0.294**	0.313**	0.324**
	(0.128)	(0.137)	(0.144)	(0.163)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

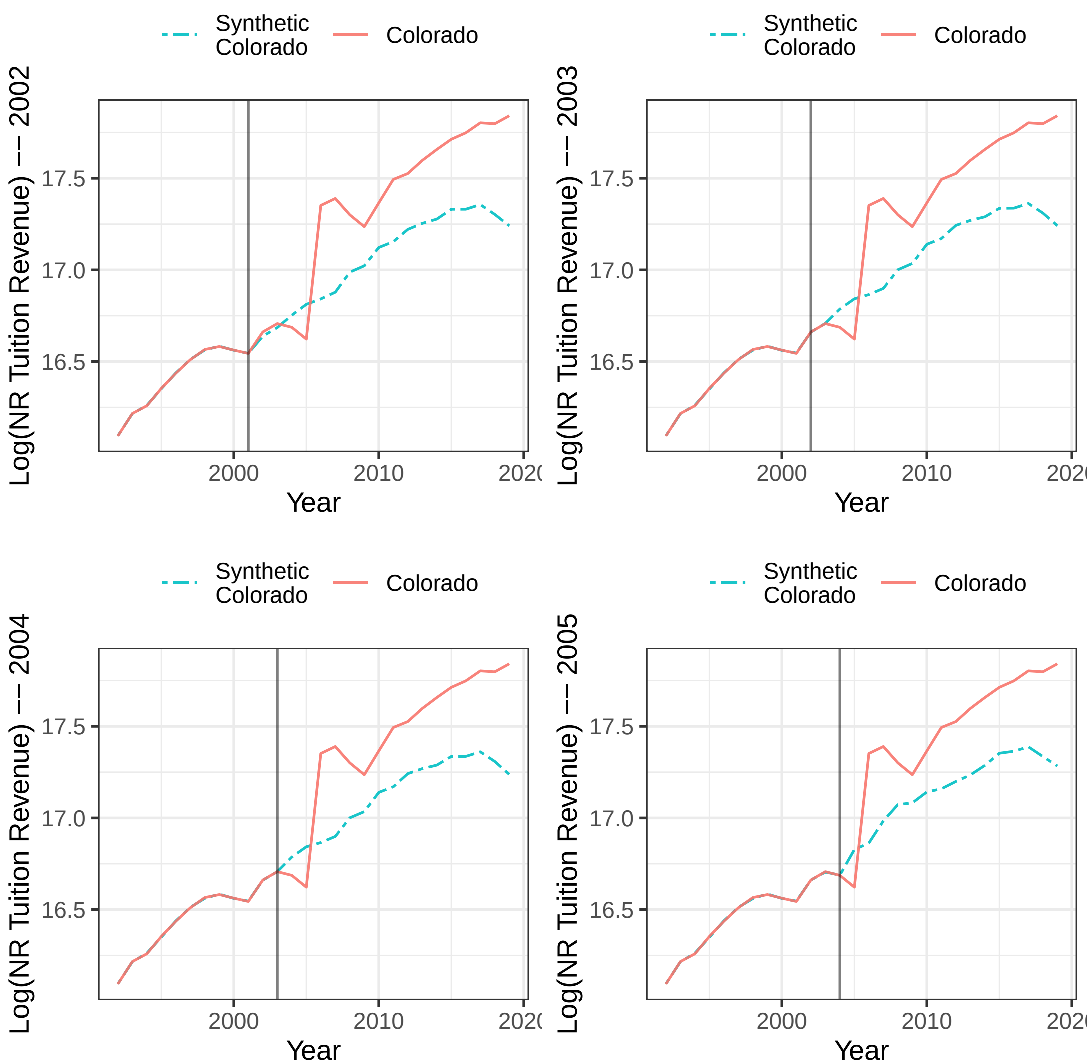


Figure 14: Nonresident Tuition Revenue Anticipation

Table 16: Enrollment Robustness – Dropping States That Enroll Coloradans

	Male	White	Hispanic	African American
ATET	0.021	-0.040**	0.030**	-0.002
	(0.016)	(0.017)	(0.012)	(0.003)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

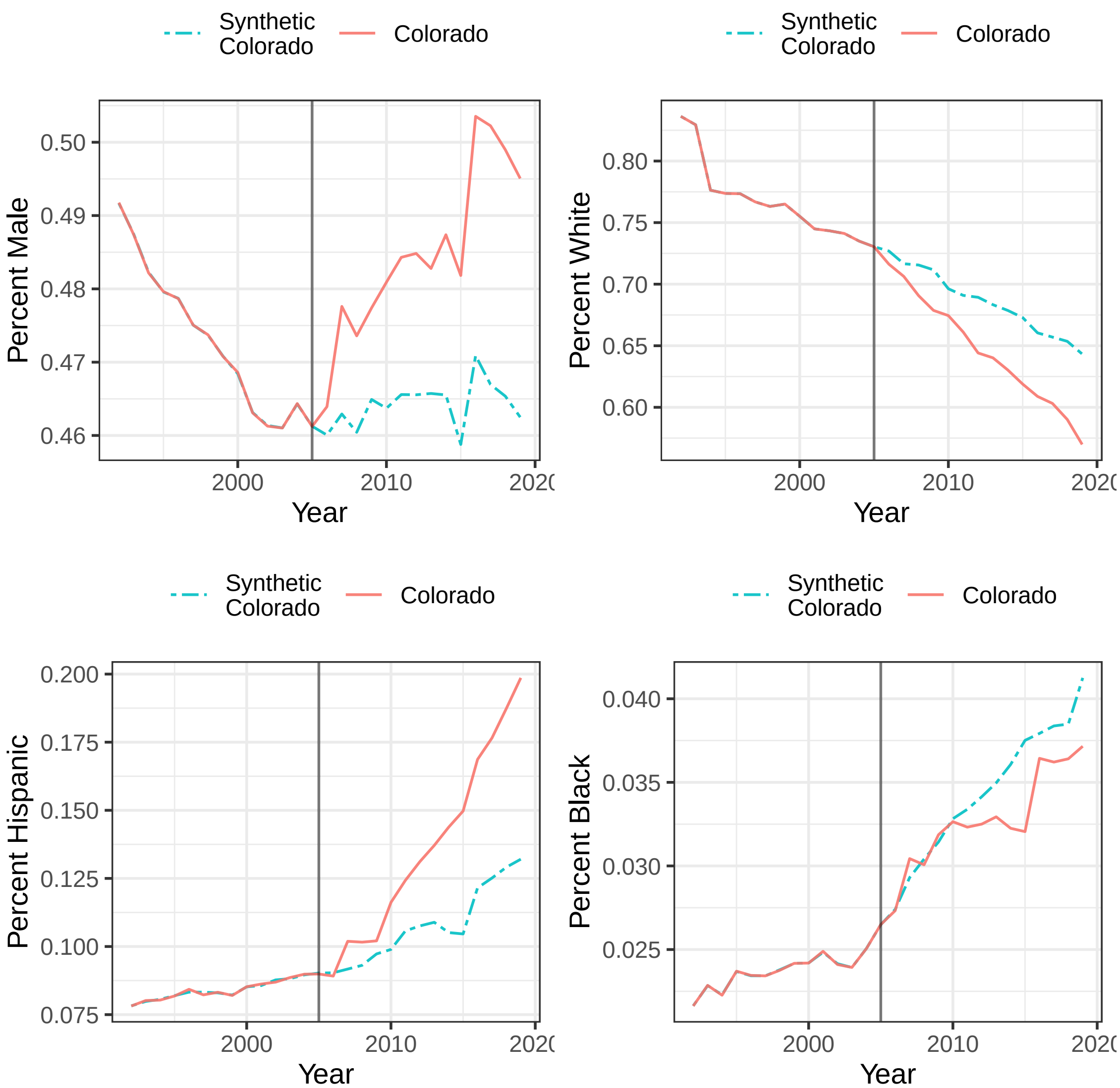


Figure 15: Enrollment Robustness – Dropping States That Enroll Coloradans

Table 17: Tuition Revenue Robustness – Dropping States That Enroll Coloradans

	Tuition Revenue	Resident Tuition Revenue	Nonresident Tuition Revenue
ATET	0.349***	0.189*	0.503***
	(0.074)	(0.114)	(0.156)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

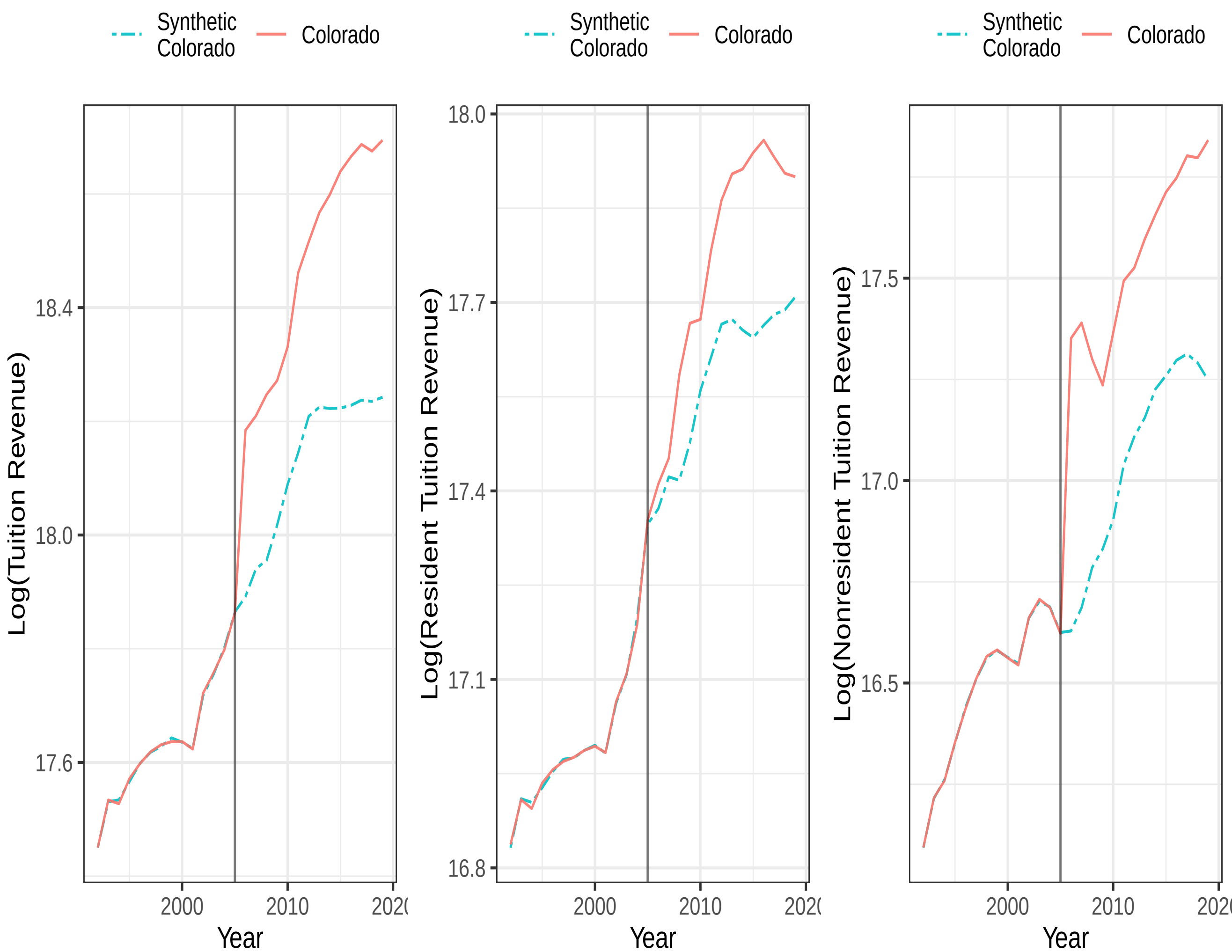


Figure 16: Tuition Revenue Robustness – Dropping States That Enroll Coloradans

Table 18: Enrollment Robustness – Dropping States That Colorado Enrolls

	Male	White	Hispanic	African American
ATET	0.022	-0.045***	0.028*	-0.002
	(0.014)	(0.015)	(0.017)	(0.003)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

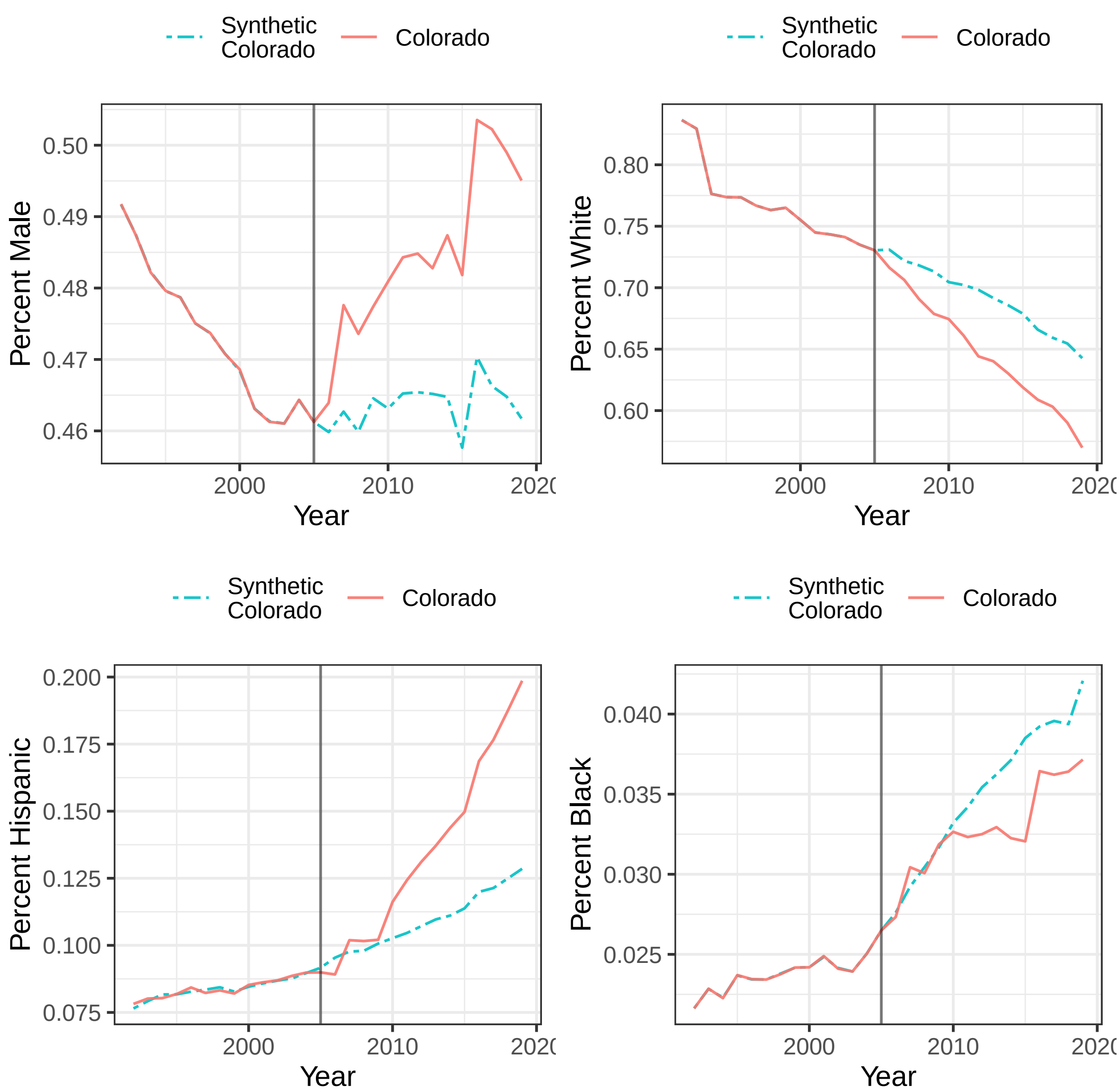


Figure 17: Enrollment Robustness – Dropping States That Colorado Enrolls

Table 19: Tuition Revenue Robustness – Dropping States That Colorado Enrolls

	Tuition Revenue	Resident Tuition Revenue	Nonresident Tuition Revenue
ATET	0.308***	0.243**	0.415***
	(0.071)	(0.113)	(0.141)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

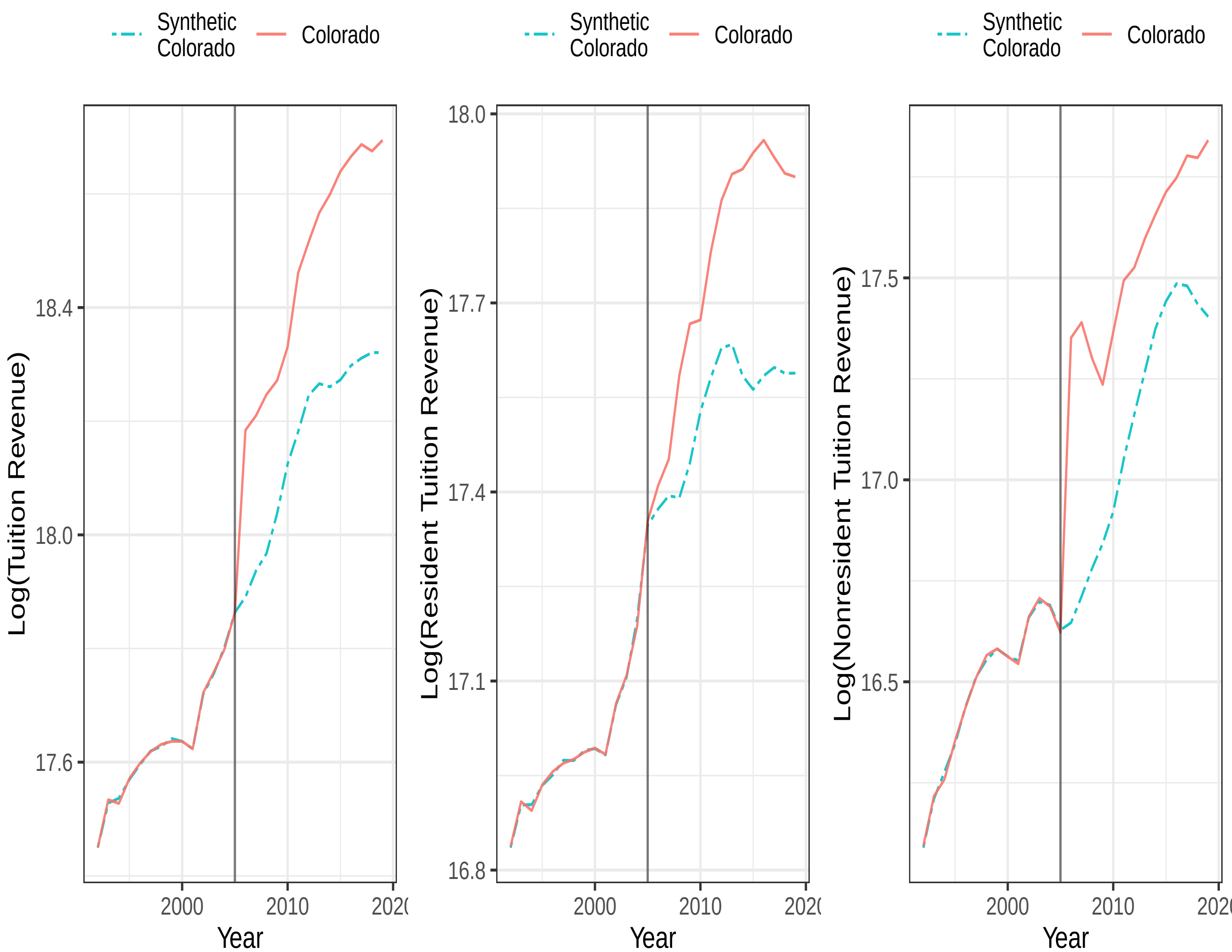


Figure 18: Tuition Revenue Robustness – Dropping States That Colorado Enrolls

Table 20: Enrollment Robustness – Dropping 2008-2011

	Male	White	Hispanic	African American
ATET	0.024	-0.046***	0.037***	-0.002
	(0.016)	(0.016)	(0.011)	(0.003)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

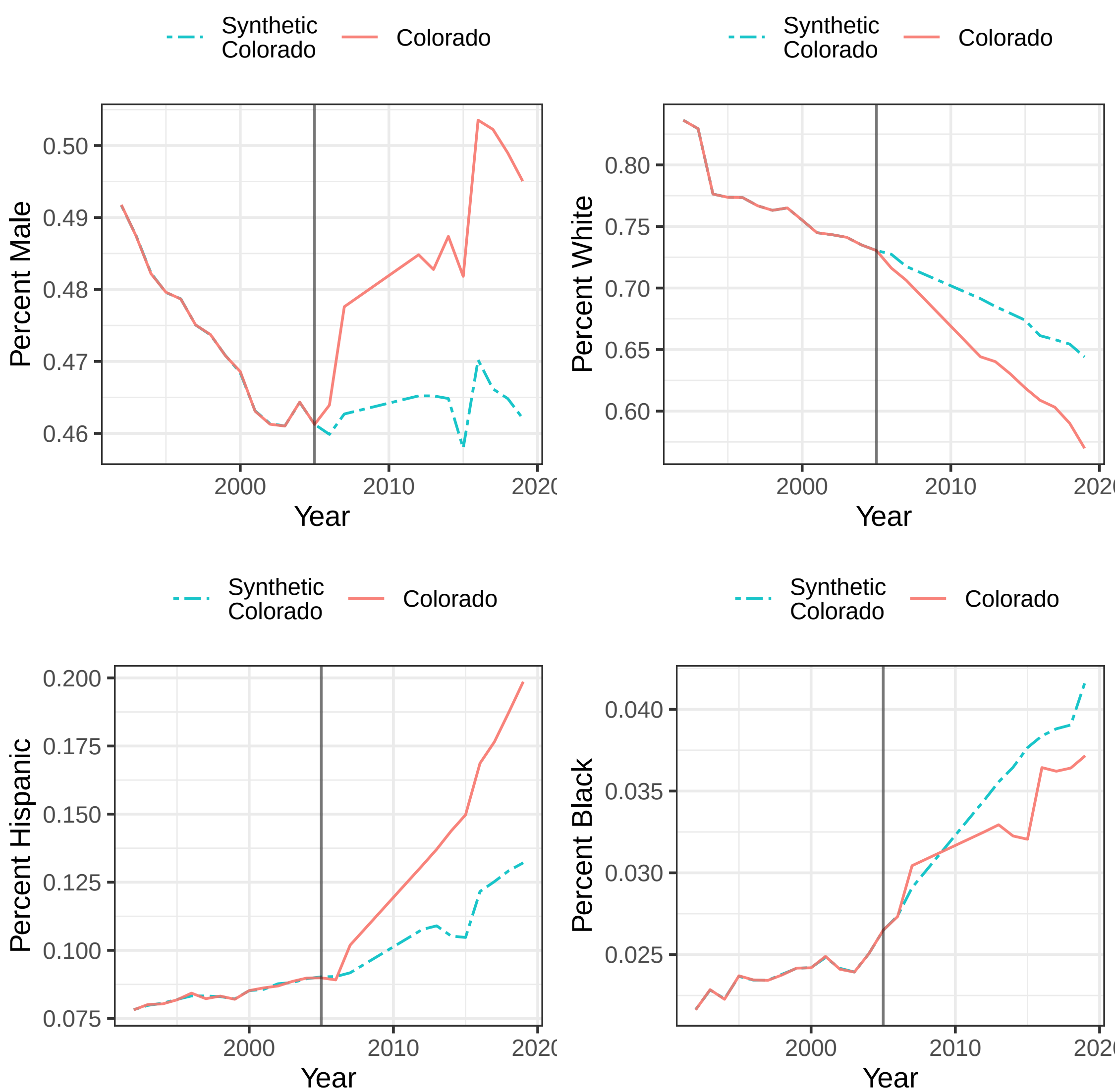


Figure 19: Enrollment Robustness – Dropping 2008-2011

Table 21: Tuition Revenue Robustness – Dropping 2008-2011

	Tuition Revenue	Resident Tuition Revenue	Nonresident Tuition Revenue
ATET	0.382***	0.216*	0.522***
	(0.089)	(0.124)	(0.184)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

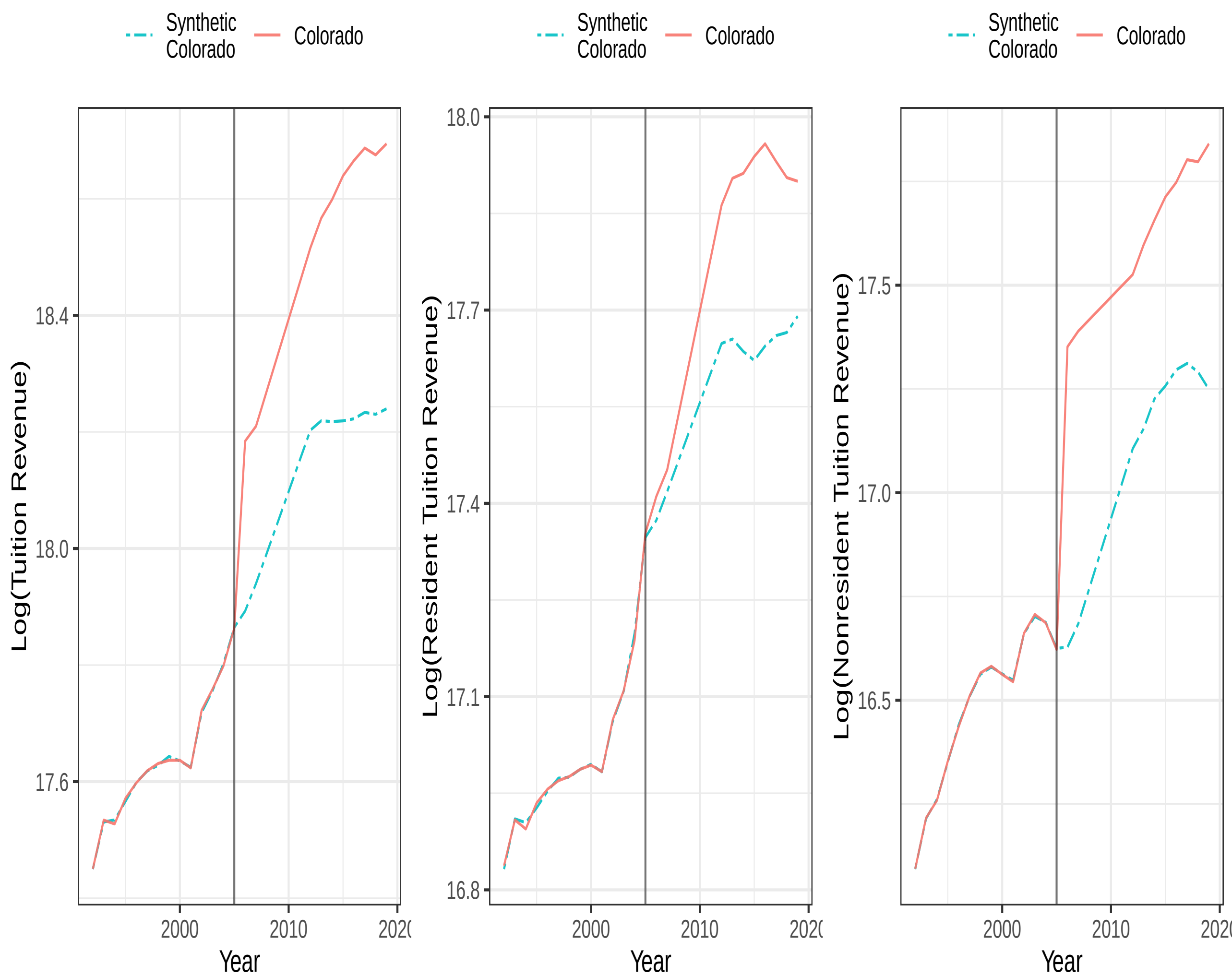


Figure 20: Tuition Revenue Robustness – Dropping 2008-2011

Table 22: Tuition Revenue – 100% COF Usage

	Resident Tuition Revenue	Nonresident Tuition Revenue
ATET	0.071	0.682***
	(0.121)	(0.182)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors are in parentheses and obtained via 200 bootstrap repetitions.

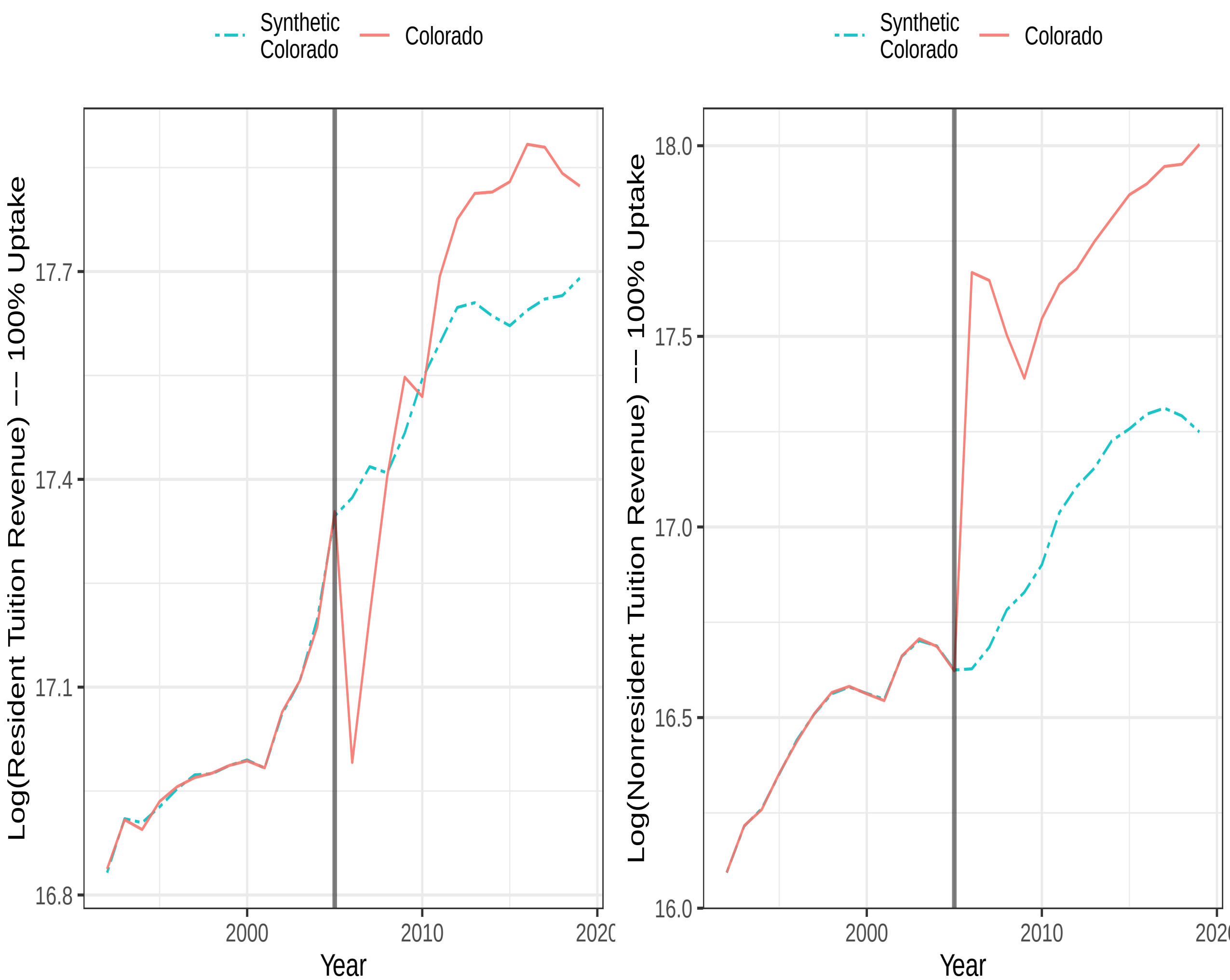


Figure 21: Tuition Revenue – 100% COF Usage

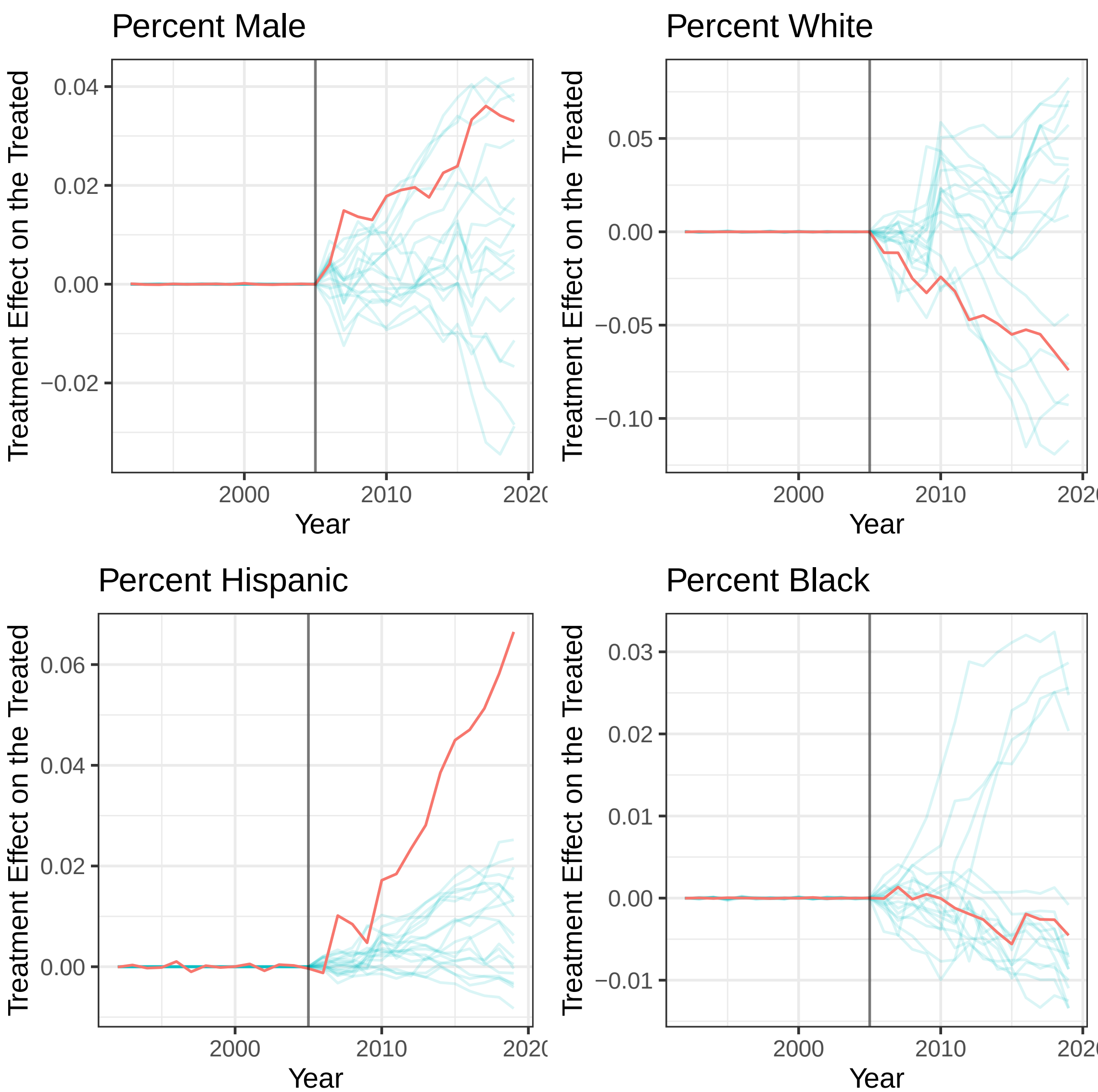
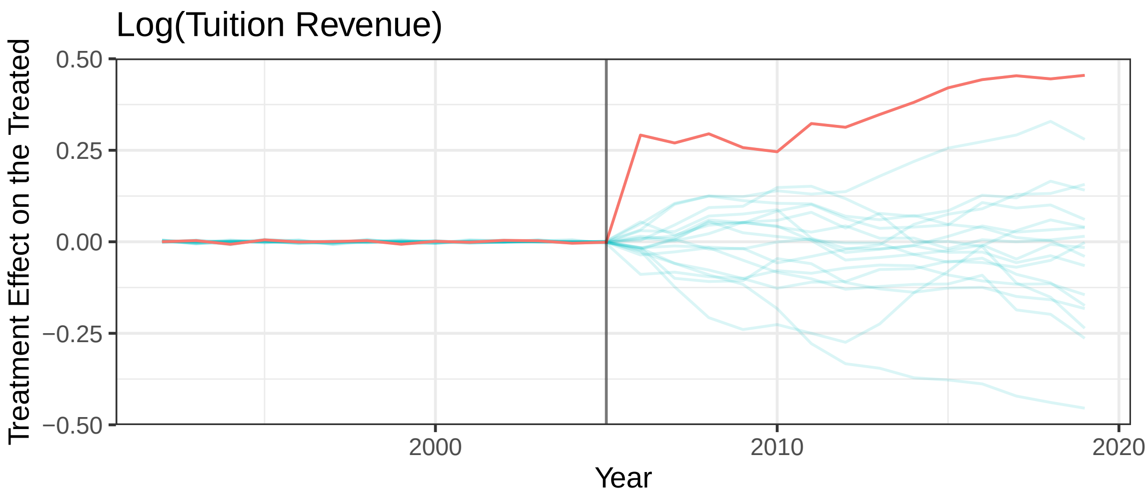
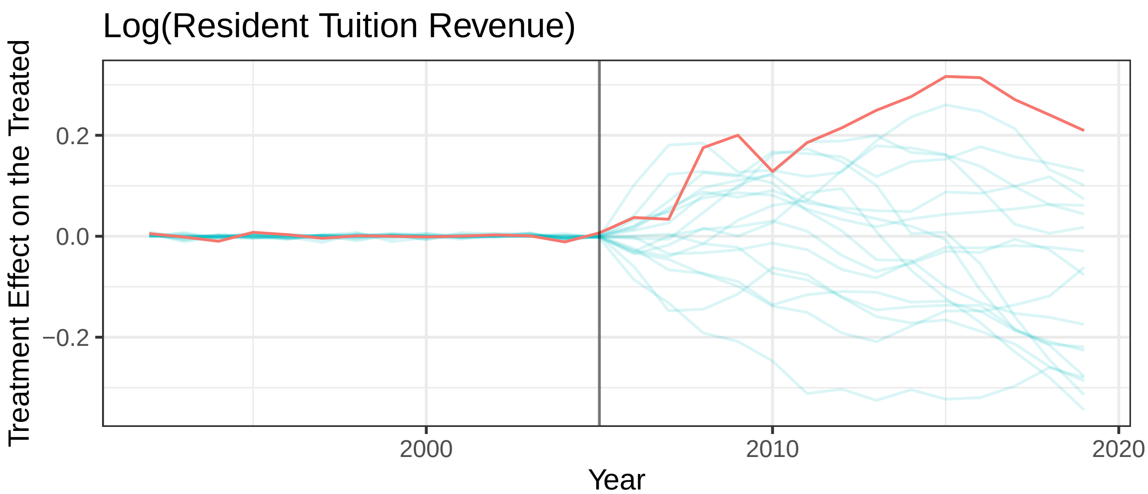


Figure 22: Enrollment Placebo Tests





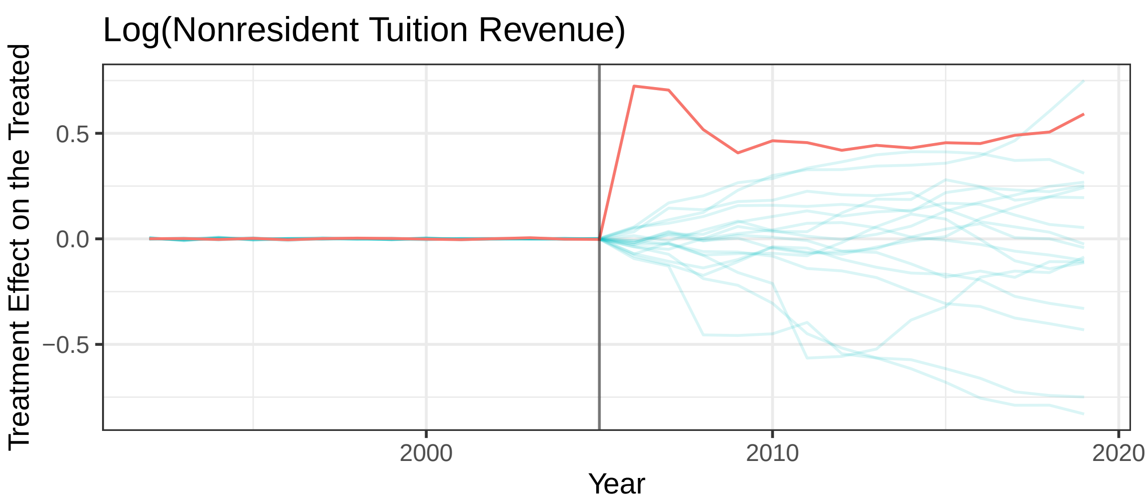


Figure 23: Tuition Revenue Placebo Tests