ESSAYS IN EDUCATION ECONOMICS

by

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DISSERTATION ABSTRACT

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I empirically explore the potential post-secondary enrollment effects of Louisiana's legislation requiring high school students to file a FAFSA application, or opt-out, prior to graduation. FAFSA submissions increased significantly in Louisiana following the policy change, suggesting there may have been some follow through into post-secondary institutions. I use a synthetic control approach to estimate causal impacts of Louisiana's FAFSA policy on college enrollment and Pell Grant awards. I find suggestive evidence that students may have substituted away from public two-year institutions towards four- year institutions. Specifically, I find marginally significant effects on enrollment for Black students at large, public four-year universities.

In evaluating the effect of nutrition and academic achievement, I exploit variation in the timing of schools' participation in the USDA Fresh Fruits and Vegetables Program. Using a staggered difference-in-differences design, I estimate the effect of receiving FFVP grants on school-level academic achievement. Results suggest that FFVP participation reduces school-level average test scores, but I am unable to distinguish the effect from a null effect.

I then explore the effects of a reading intervention aimed young, school-aged children and academic achievement. Specifically, using student-level data, I evaluate the effect of Michigan's 2014 "Culture of Reading Program" on third grade standardized test scores. I consider the potential heterogeneity by how young students were when they received access to the program. I find significant, positive increases in achievement for students who received the program while enrolled in public Pre-K, but I find no such effects for students enrolled in kindergarten, first grade, or second grade.

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To Mom and Dad

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CHAPTER I

INTRODUCTION

Through applied econometric analysis, this work aims to inform and shape education policy to improve access and equity in education, improve student achievement, and address barriers to higher education for traditionally underrepresented groups.

In Chapter II, I consider the post-secondary enrollment effects of Louisiana's 2017 change in financial aid submission policy, when financial aid filing was made mandatory for high-school graduation. While not likely to have induced enrollment among those who would have already been inclined to complete a FAFSA application, this mandate may have impacted students who were on the margin of receiving aid, moving these students towards completing an application. Thus, we may expect such a mandate—where increases in FAFSA applications is evident—to expand access to higher education for students from historically disadvantaged backgrounds. I find suggestive evidence that students may have substituted away from public two-year institutions toward public four-year institutions–this is particularly true for Black students who substitute toward large, public four-year institutions.

In Chapter III, I contribute to the existing literature evaluating the effect of federally funded nurtition programs on student achievement. Specifically, I examine the impact of the USDA Fresh Fruits and Vegetables Program on academic achievement, attendance, and disiciplinary actions. I find suggestive evidence that FFVP participation reduces school-level average test scores, but the magnitude of the effect cannot be distinguished from a null effect. Further, I find no evidence of changes in attendance rates or suspensions that can be attributed to participation in the program.

Finally, in Chapter IV, I examine the effect of Michigan's "Culture of Reading Program" on student achievement. The program gave a storybook and family reading activites to students in selected classrooms; teachers in selected classrooms were also intended to use evidence-based reading instruction in the classroom. I find large and significant gains in third-grade performance among students who were given access to the reading program while enrolled in Pre-K. However, I find no such effects for students who received similar treatment even one year later in kindergarten, or in first or second grade.

CHAPTER II

MAKING FAFSA MANDATORY: AN EVALUATION OF LOUISIANA'S FINANCIAL AID SUBMISSION POLICY ON COLLEGE ENROLLMENT AND PELL GRANT AWARDS

II.1 Introduction

Each year, approximately 20 million students complete the Free Application for Federal Student Aid (FAFSA) application. However, FAFSA completion differs across various economic and socio-demographic groups—completion rates are higher among the higher performing students, among those with higher expected family contributions, and among among those of parents who attended college (Feeney and Heroff, 2013).[1]

With the objective of encouraging more students to complete a FAFSA form and enroll in college, in 2017, Louisiana enacted legislation requiring high-school students to submit a FAFSA application as a prerequisite for graduation. To take effect with the incoming Fall 2018 cohort, this policy was initiated with the 2017–2018 high school seniors.

While not likely to induce enrollment among those who would already have been inclined to complete the FAFSA absent the mandate, for those marginal students—around the margin of receiving aid—this may well move these students toward completing a financial aid application. In particular, then, we might expect such a mandate to expand access to higher education for students from historically disadvantaged backgrounds, where FASFA applications are lowest (Lowry, 2018) and college going is less the norm (Hussar et al., 2020). Moreover, given that the FAFSA submission will have students, at least for a time, be forward-looking in their college plans, we

[1] There is differences in the filing status across students' gender, race/ethnicity, income status, and pre-college academic experiences (McKinney and Novak, 2013, 2015).

might also expect that this information about financial aid would influence a student's decision to enroll (Dynarski, 2000; Cornwell et al., 2006), as well as the type and quality of the institution a student attends (Avery and Hoxby, 2004; Bruce and Carruthers, 2014). Understanding the extent to which mandatory FAFSA policies induce students to enroll in college or to substitute between types of post-secondary institutions is important, as students—particularly lower-skill students—are significantly more likely to earn a bachelor's degree if they start at a four-year school rather than a two-year college (Goodman et al., 2017).

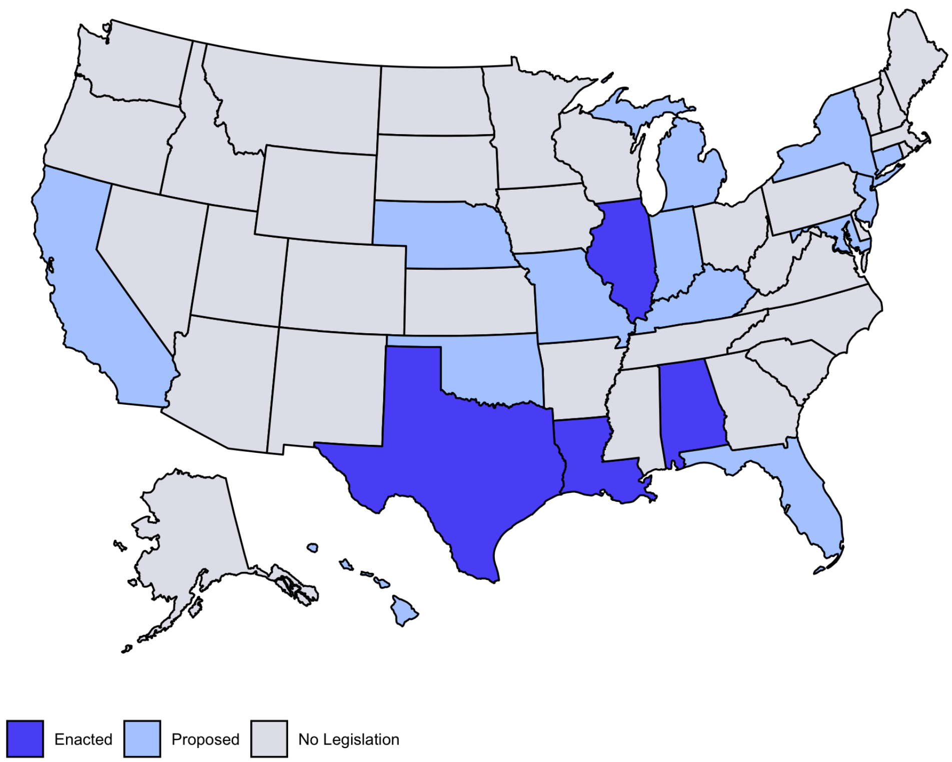
In the two years since the FAFSA mandate, Louisiana did see large increases in FAFSA submissions—approximately 25% increases relative to the 2016-2017 academic year, before the initiation of the mandate.[2] But at the same time, Louisiana had already been experiencing a steady increase in college enrollments (Louisiana Department of Education, 2019), making the interpretation of such unconditioned increases challenging. Moreover, increasing numbers of high-school graduates in Louisiana (since 2012) also implicate a potential confoundedness—reported results in Louisiana Department of Education (2019) imply that adjusting for the size of graduating high-school students, enrollment in Louisiana *decreased* by 0.34 percentage points between Fall 2017 and Fall 2018.

Given the raw increases in FAFSA submissions and college enrollment, Louisiana's policy has largely been viewed as a success—so much so that many states have either proposed similar legislation or are in the process of implementing similar mandates. Among other early adopters, for example, Illinois required FAFSA submission prior to high-school graduation beginning with the Fall 2021 college cohort, and Texas and Alabama will require FAFSA submission beginning with the Fall 2022 (i.e., the 2021–2022 graduating seniors). Now 13 other states have proposed mandatory FAFSA

[2] In the 2017-2018 academic year, Louisiana also saw a 31% increase the number of students eligible for the Taylor Opportunity Program for Students (TOPS) Scholarship, which provides state scholarships for students who are attending public two-year or public four-year institutions in Louisiana (DeBaun, 2019).

legislation.[3]

Figure II.1
Mandatory FAFSA Legislation by State



In this paper, I consider whether there are evident increases in college going that can be attributed to Louisiana's mandatory FAFSA submission policy. In order to do so, I compare outcomes in Louisiana—post-secondary enrollments and total Pell Grant awards—to outcomes in a "synthetic" Louisiana, before and after the policy change. To the extent outcomes in Louisiana increase relative to a control group that

[3] See Figure II.1 for a map of states where this policy has been proposed or enacted.

was *similar* to Louisiana in its outcomes prior to the innovation in policy, we will be inclined to attribute those increases to the mandated FAFSA submission.

Comparing Louisiana's outcomes in the post-treatment period to those in Louisiana's synthetic counterfactual, I find that relative changes in college enrollment and Pell Grant awards are small—I cannot rule out that the aggregate effect is zero. As the FAFSA mandate came with no real increase in funding for high schools, that there are no significant increases attributable to the policy is potentially unsurprising.[4] Moreover, its implementation may have put additional stress on understaffed schools, and to the extent high-school counselors play a role in the school's college-going rate (Hurwitz and Howell, 2014; Engberg and Gilbert, 2014; Mulhern, 2019), may have *lowered* access. Either way, our priors may be for the policy to have least benefited students who would have the most to gain from submitting a FAFSA application. Absent adequate information and assistance on financial aid and college pricing, these policies may not be enough to induce students on the margin into enrolling in college.

That said, there are margins at which there are economically meaningful changes, and in places that are consistent with the policy having influenced students more likely to be marginal in their enrollment decisions. For example, as the policy may have induced students to substitute between two-year and four-year institutions, I consider enrollments and financial aid separately across the type of institution. There, I find that suggestive evidence that students may have substituted away from public two-year schools into four-year schools. To the extent this is generally true, substitution between two-year and four-year schools does not appear in the aggregate—estimates are small and statistically indistinguishable from zero. That said, I will show that this is particularly true for Black students, who are disproportionately likely to be attending less-resourced schools with less college-related counseling (McDonough, 1997;

[4] The Louisiana Office of Student Financial Assistance did receive a grant from the National College Attainment Network for their FAFSA Completion Tracker, which monitors FAFSA completions at the state, district, and school levels.

Mcdonough, 2004). For all other students, there is no meaningful variation in two- and four-year enrollments that can be attributed to the policy change—estimates are estimated null effects.

To the extent that the FAFSA mandate induces students toward receiving more/better information about their financial eligibility, we might expect that the total value of all Pell grants awarded in Louisiana could respond to the policy, despite the lack of a significant enrollment response. In fact, it is at lower-cost, public four-year institutions and public two-year institutions where I find suggestive evidence that Pell Grant awards may have *increased* with the onset of the FAFSA mandate—this is consistent with students who would have otherwise not filed a FAFSA (absent the mandate) learning about Pell Grant eligibility through the mandate-induced FAFSA process and accepting a grant, despite not switching their enrollment behavior.

In Section 2 I discuss previous literature on financial aid and college enrollment. In Section 3 I outline the data used in my analyses. I provide details on my empirical strategy used to identify the causal impact of Louisiana's policy on enrollment and Pell Grant awards and present results in Section 5. I offer concluding comments in Section 6.

II.2 Financial Aid and College Enrollment

Becker's model of human capital predicts that an exogenous reduction in tuition would induce students on the margin into enrolling in college (Becker, 1964). Empirically, there is plenty of evidence that suggests financial aid can have large impacts on college enrollment (Dynarski, 2000, 2003; Seftor and Turner, 2002; Kane, 2003). However, many eligible students do not apply for financial aid and forgo large amounts of financial assistance (King, 2006). It has been argued that the complexity and inconvenience of the FAFSA form deters many students from applying for aid, thus contributing to college enrollment gaps between low-income and high-income students (Dynarski and Scott–Clayton, 2006). This is not because low-income students do not value a college education; low-income students with high valuations of college often fail to clear seemingly minor hurdles in the process of applying for college and financial aid (Avery and Kane, 2004).

There is a growing behavioral literature that studies the reasons for low take-up of financial aid. Perhaps the most relevant to this paper is the literature on default-options. That is, economic decisions are influenced by the individual's status-quo option (Samuelson and Zeckhauser, 1988). For example, changing employees' 401k forms to make participation the default option increased participation by 50 percentage points (Madrian and Shea, 2001), from a baseline of about 37%. Louisiana's FAFSA mandate changes the default option to participation, requiring that students opt-out of submitting the FAFSA rather than opt-in. As the default option is now to submit a FAFSA application, students who were on the margin of completing one because they were previously unwilling to do so or were discouraged from applying may be induced into applying for financial aid. This may help students pass a major barrier in the college application process and therefore may translate into increased college enrollments, as students may now be more informed about their financial aid options.

There have also been several studies imposing "nudges" on potential college enrollees by providing information or personal assistance to help with the FAFSA process. Using a randomized experiment, Bettinger et al. (2012) finds that students who receive assistance completing the FAFSA form are significantly more likely to complete two years of college, but students provided with only information and no assistance did not see improved outcomes. Text message programs that provide reminders about filing the FAFSA have also been effective in inducing students to complete a FAFSA application (Page et al., 2020) and to inducing students toward persisting in college (Castleman and Page, 2016). These findings suggest that using small nudges to simplify the FAFSA application process and providing students and parents with information about that process may result in increased rates of students who complete the application and qualify for aid. Though not mandated in Louisiana's policy, in putting the FAFSA filing into practice, the Louisiana Office of Student Financial Assistance used similar technologies that could be thought of as "nudges," in their attempts to encourage students to complete their application—phone calls and text messages were used to remind parents about FAFSA-related events and financial aid workshops, for example. Thus, to the extent movement in enrollment around the policy innovation are evident in Louisiana, I will be inclined to interpret them as inclusive of the use of such technologies.

II.3 Data

To examine changes in FAFSA submissions, I use data from Federal Student Aid, which provides school-level data on the number of submitted and completed FAFSA applications for each application cycle. I collect this data for the 2014-2015 academic year through the 2019-2020 academic year. I also use school-level data from the Louisiana Department of Education to calculate the percentage of minority students and the percentage of free/reduced priced lunch students enrolled at each school.

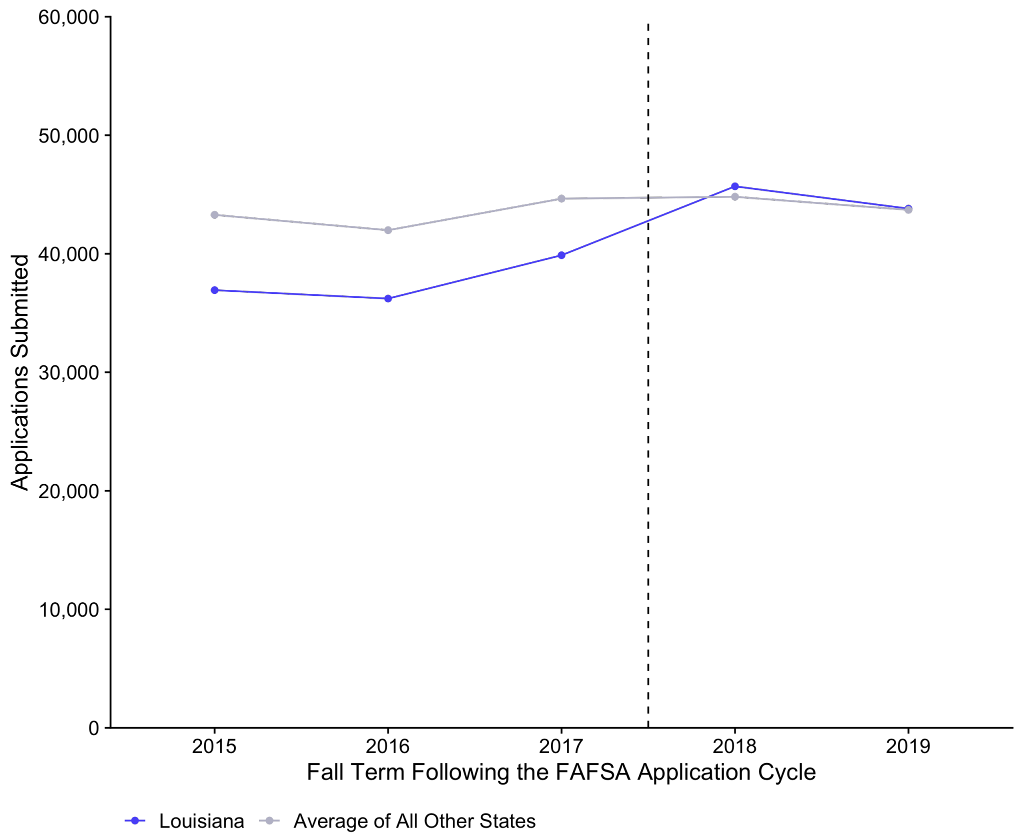
To examine the impact of the FAFSA policy on post-secondary outcomes, I use data from the Integrated Post-Secondary Education System (IPEDS), an intergration of surveys of post-secondary institutions conducted by the US Department of Education. I restrict my sample of institutions to degree-granting institutions that have full-time undergraduate students enrolled during the fall semesters of 2010-2019. The enrollment outcome of interest is full-time, first-time degree/certificate-seeking (FTF) undergraduate enrollment. I also collect data on Pell Grant awards for FTF students, defined as the total amount of Pell Grant aid awarded to full-time first-time undergraduates, and the price of tuition for each institution. Data on financial aid is only available through 2018, giving one year of post-treatment information.

II.4 Empirics

II.4.1 FAFSA Submissions

In Figure II.2 I plot FAFSA application submissions for Louisiana and the United States average. Coincident with the policy introduction, there is an evident increase the number of FAFSA applications submitted in Louisiana. In 2019, Louisiana's submissions fall slightly.

Figure II.2
First-Time FAFSA Applications Submitted in Louisiana Relative to US Average



To estimate the causal impact of Louisiana's policy on FAFSA submissions, I use a simple difference-in-differences design. I compare FAFSA submission in Louisiana to FAFSA submissions in the rest of the United States, before and after the policy change. Specifically, I estimate

$$Y_{st} = \beta \text{Treated}_{st} + \alpha_s + \delta_t + \varepsilon_{st}$$

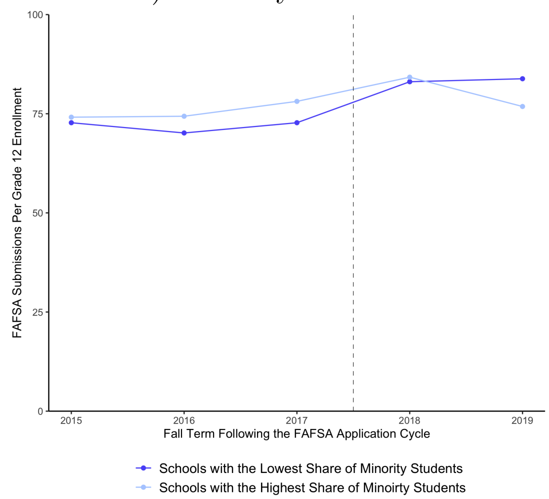
where Y_{st} is the number of FAFSA applications submitted in state s in academic year t, Treated_{st} is an indicator variable equal to one for Louisiana in the academic years 2017-2018 and 2018-2019. I include a state fixed effect, α_s, and a time fixed effect, δ_t, and cluster my standard errors at the state level. Results indicate that FAFSA submissions in Louisiana increased by an average of 4913 applications ($p < .001$) in the post-treatment years, relative to the rest of the United States. I also perform this analysis at the school-level, where I estimate that FAFSA submissions in Louisiana increased by an average of nine applications ($p < .001$) per school in the post-treatment period, relative to other high schools in the United States.

While FAFSA submissions increased statewide, we should also consider how factors such as income status or race/ethnicity affect FAFSA submission, as these are the margins in which we could imagine this policy having the greatest impact. In Figure II.3 Panel A I plot the FAFSA submission rate for schools in the top 10 percent and bottom 10 percent for minority student enrollment. Schools with the highest share minority students have a minority student enrollment of 96% or greater and schools with the lowest share minority students have a minorty student enrollment of 10% or less.

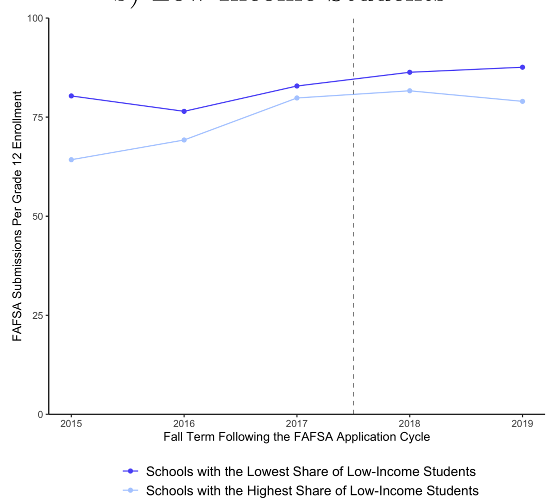
Prior to the policy introduction, the submission gap between high-share and low-share minority schools was roughly the same with high-share minority schools having slightly higher submission rates; the gap is -4.07 percentage points in 2017 ($p = 0.20$). While the submission rate increased from 2017 to 2018 for both groups, the submission rate for low-share minority schools increased more than the rate for high-share minority schools, closing the submission gap to -1.1 pecentage points ($p = 0.73$).

Figure II.3
FAFSA Submission by Share of Minority Students and Low-Income Students

a) Minority Students



b) Low-Income Students



High-share minority schools see a decrease in the submission rate in the second year policy, making the submission gap roughly 7.0 percentage points ($p = 0.13$).

I perform a similar exercise for income, using free/reduced priced lunch status as proxy for income status. Schools with highest share of low-income students have more than 90% of students on free/reduced priced lunch and schools with the lowest share of low-income students have less than 45% of students on free/reduced priced lunch. In Figure II.3 Panel B we can see there exists a gap in the submission rate between schools with many low-income students and schools with few low-income students, prior to the policy introduction. This gap is about 7.62 percentage points in 2017 ($p = 0.04$). FAFSA submission rates increase for both both groups after the policy change and the submission rate gap decreased slightly to 5.47 percentage points in 2018 ($p = 0.08$). However, we can see that submission rates fall in 2019 for schools with many low-income students, thus the increasing the submission gap to 11.8 percentage points ($p = 0.004$)–larger than the gap prior to the policy introduction.

Given this, there is little evidence that mandatory FAFSA submission affected gaps in filing between low-income and high-income schools or between low-share and high-share minority enrollment schools. Students in relatively affluent schools are probably more likely to have access to the types of resources it takes to get more students to submit the FAFSA, which could explain why we see slightly larger movements in the submission rates at higher income and low-share minority school relative to low-income and high-share minority schools. We should also consider that Louisiana's policy requires *submission*, not *completion*. Submitted applications reflect all FAFSA forms submitted by students at a high school but these applications can be rejected if they are missing key pieces of information. Students who submit a FAFSA application but do not complete it are not eligible for financial aid. Requiring submission, not completion, may miss these students who are trying to become eligible for aid but are failing to clear hurdles in completing the financial aid application and thus preventing them from gaining necessary information about the costs of college attendance.

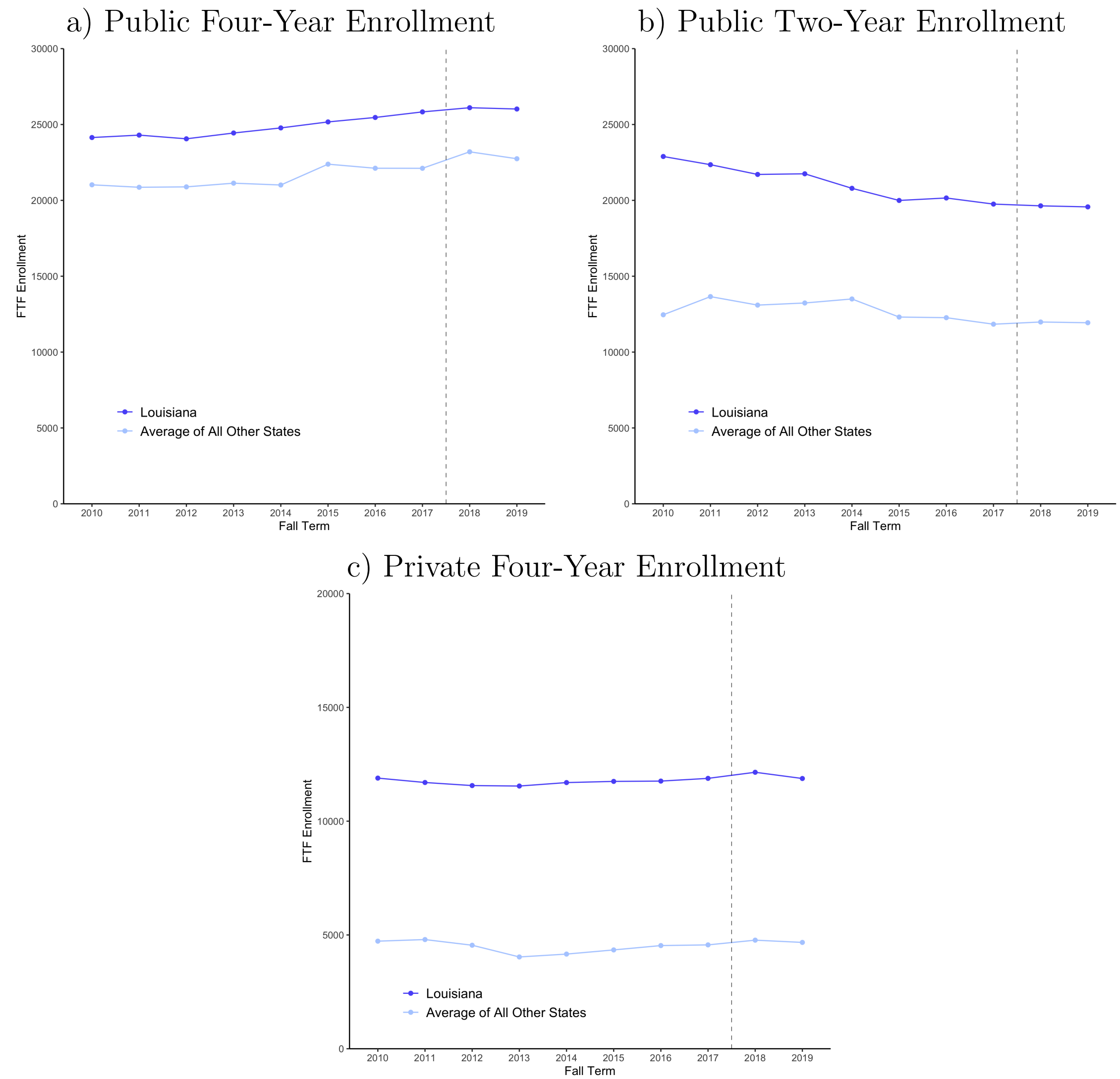
II.4.2 Enrollment and Pell Grant Awards

In Figure II.4 I plot the trend in first-time undergraduate fall enrollment for Louisiana and the United States average. Based on these figures, it does appear that college enrollments increased in Louisiana after the policy introduction for public four-year and private four-year institutions. Ideally, to identify causal impacts of Louisiana's FAFSA policy on enrollment and Pell Grant awards, we would use a difference-in-differences design to compare outcomes in Louisiana to outcomes in a set of control states, before and after the policy change. However, finding an appropriate control group to perform this analysis proves difficult. To use a difference-in-differences design, it is required that the states in the control group trend in the same way as Louisiana prior to the policy change. Figure II.4 suggests that Louisiana's enrollments do not trend in the same way as the United States average prior to the policy introduction. This is particularly true when examining enrollment by race/ethnicity.[5] This suggests that the parallel trends assumption required for difference-in-differences is likely violated. One solution to this violation may be to narrow the control group down to include only other southern states or states that border Louisiana. Doing so may also be problematic because Louisiana's FAFSA policy may have led to increases in enrollments in surrounding states, as colleges in these states are also competing for students from Louisiana. Approximately 11% of Louisiana FTF students enrolled in out-of-state colleges in Fall 2018 (Louisiana Department of Education, 2019), so it is possible that states near Louisiana saw an increase in enrollment due to Louisiana's FAFSA policy change. Therefore, surrounding states cannot serve as a reasonable control group for Louisiana because institutions in these states may have also been affected by the policy change. Without a reasonable control group, difference-in-

[5] See Appendix for corresponding figures.

differences would yield biased estimates of the impact of mandatory FAFSA legislation on enrollment and Pell Grant awards.

Figure II.4
First-Time Fall Enrollment in Louisiana and Other States



To estimate the causal impact of Louisiana's FAFSA policy on enrollment and Pell Grant awards, I use the synthetic control method. The synthetic control method (Abadie et al., 2010) relaxes the parallel trends assumption and constructs a control unit as counterfactual for the treated unit by weighting each control unit using the pre-treatment periods. The weights are chosen such that, prior to the policy change, the treated unit and its synthetic control have similarly trending outcomes. Using synthetic control, I am able to estimate the effect of the policy change by comparing post-treatment outcomes in Louisiana to post-treatment outcomes for the synthetic counterpart. In this paper, I use the Generalized Synthetic Control Method developed by Xu (2017). To avoid biasing my estimates, I use a relatively long pre-treatment window of 2010-2017 and match only on outcomes.[6]

II.4.2.1 Enrollment Results

In Figure II.5 I plot synthetic control results for each institution type. The outcome variable of interest is FTF enrollment. In each figure, actual FTF enrollment for Louisiana is represented by the black line and the synthetic control unit is represented by the dashed line. Panel A includes all states as potential donors to the synthetic control, Panel B excludes neighboring states to Louisiana, and Panel C excludes any state with a school in the Southeastern Conference for NCAA athletics. I exclude the neighboring states as these states may have been influenced by the FAFSA policy due to the possibility of Louisiana high school students enrolling in out-of-state colleges in nearby states.[7] I exclude the states with an SEC school because students who are considering large, public universities in Louisiana may also be considering large, public universities in other states within the same athletic conference. Including these states in the potential donor pool may lead to double counting, as the policy could have induced students who would have otherwised enrolled in an out-of-state school to enroll in a school in Louisiana, thus decreasing enrollment in the other state and increasing enrollment in Louisiana.[8]

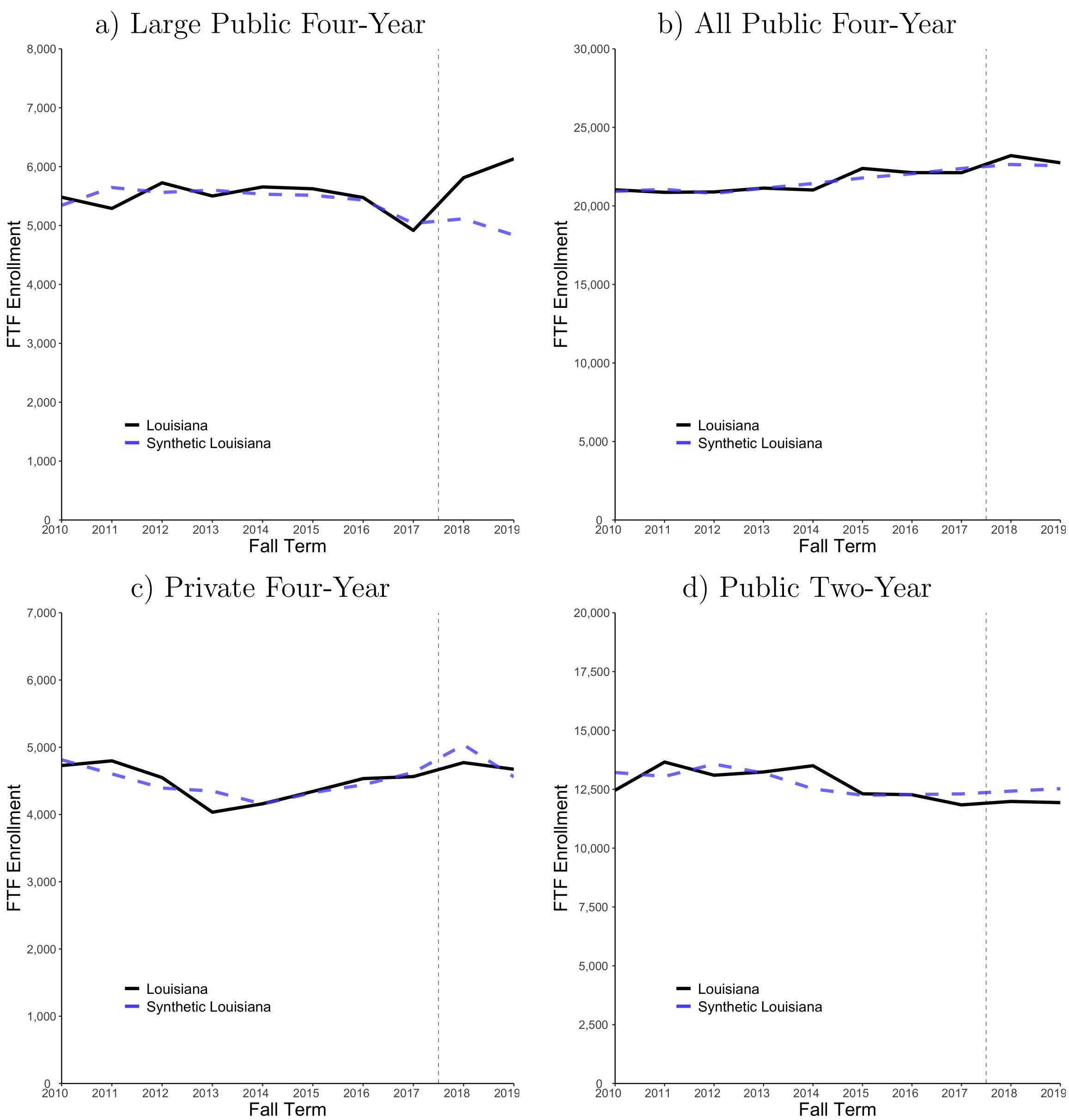
The synthetic control unit matches Louisiana closely in the pre-treatment years.

[6] I implement the generalized synthetic control method using the "gsynth" package in R (Xu and Liu, 2018).

[7] The states excluded from this sample are: Alabama, Arkansas, Mississippi, Missouri, Oklahoma, Tennessee, and Texas.

[8] Figures provided in the Appendix

Figure II.5
Post-Secondary Enrollment in Louisiana, by Institution Type

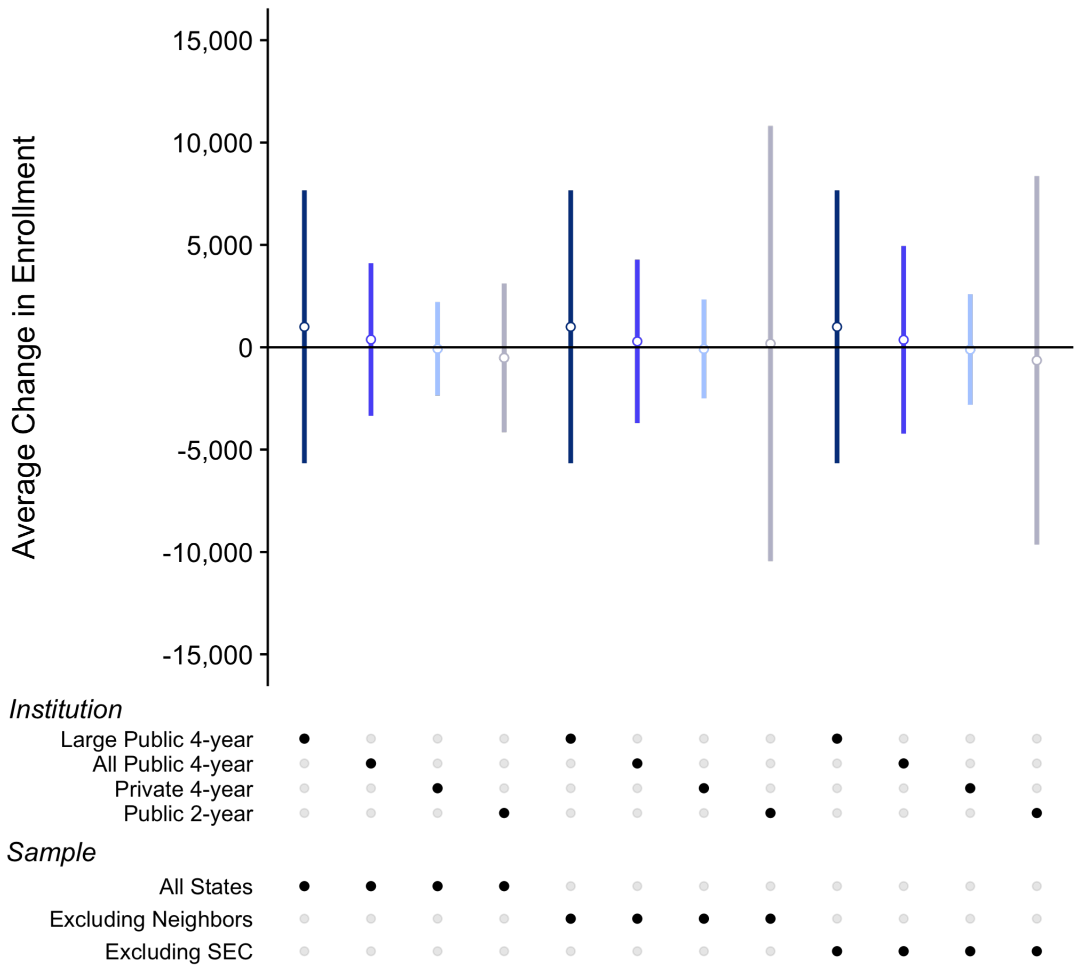


In the two years following the introduction of the policy, it appears that there is a divergence between enrollment at large, public four-year institutions in Lousiana and large, public four-years in other states. However, for other institution types, there does not appear to be any meaningful difference between Louisiana's enrollment and the synthetic control enrollment. In Figure II.6 I report estimates of the average treatment effect on the treated (ATT), the estimate of the difference between the observed enrollment in Louisiana and the synthetic counterfactual enrollment after the intervention, along with the 95% confidence interval. The point estimates are quite small and are not statistically significant, so I cannot rule out that these estimates are zero. However, as the confidence intervals are large, I cannot rule out that this program did induce large changes in enrollment. For example, the 95% confidence interval for total enrollment at all public four-year universities is -3,212 to 3,968. Additionally, there does not appear to be much evidence of substitution between public two-year and four-year schools in the aggregate.

As the policy may have differentially impacted students of color, I then test for potential heterogeneity in enrollments by race/ethnicity. Enrollment for Louisiana and the synthetic control unit is given in Figures II.7-II.10.[9] For white students, the synthetic control follows Louisiana's enrollment closely in the pre-treatment years. White student enrollment at public two-years is slightly above the synthetic control enrollment in the post-treatment years and enrollment in large public four-years is below the synthetic control enrollment in Fall 2019. For Black students, visual evidence suggests that Louisiana's enrollments have increased in four-year institutions, particularly large public four-years, and have decreased slightly in public two-years. However, the synthetic control does not match quite as well on public two-years in the pre-treatment years, and the difference between Louisiana and the synthetic unit does not appear to be outside of normal year-to-year deviations.

[9] Additional figures for synethic control estimation excluding neighboring states and SEC states are given in the appendix.

Figure II.6
The Effect of Mandatory FAFSA Legislation on Enrollment, by Institution Type



Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for FTF enrollment by institution type. The synthetic control matches on pre-treatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R.

Figure II.7
Enrollment for White Students in Louisiana, by Insitution Type

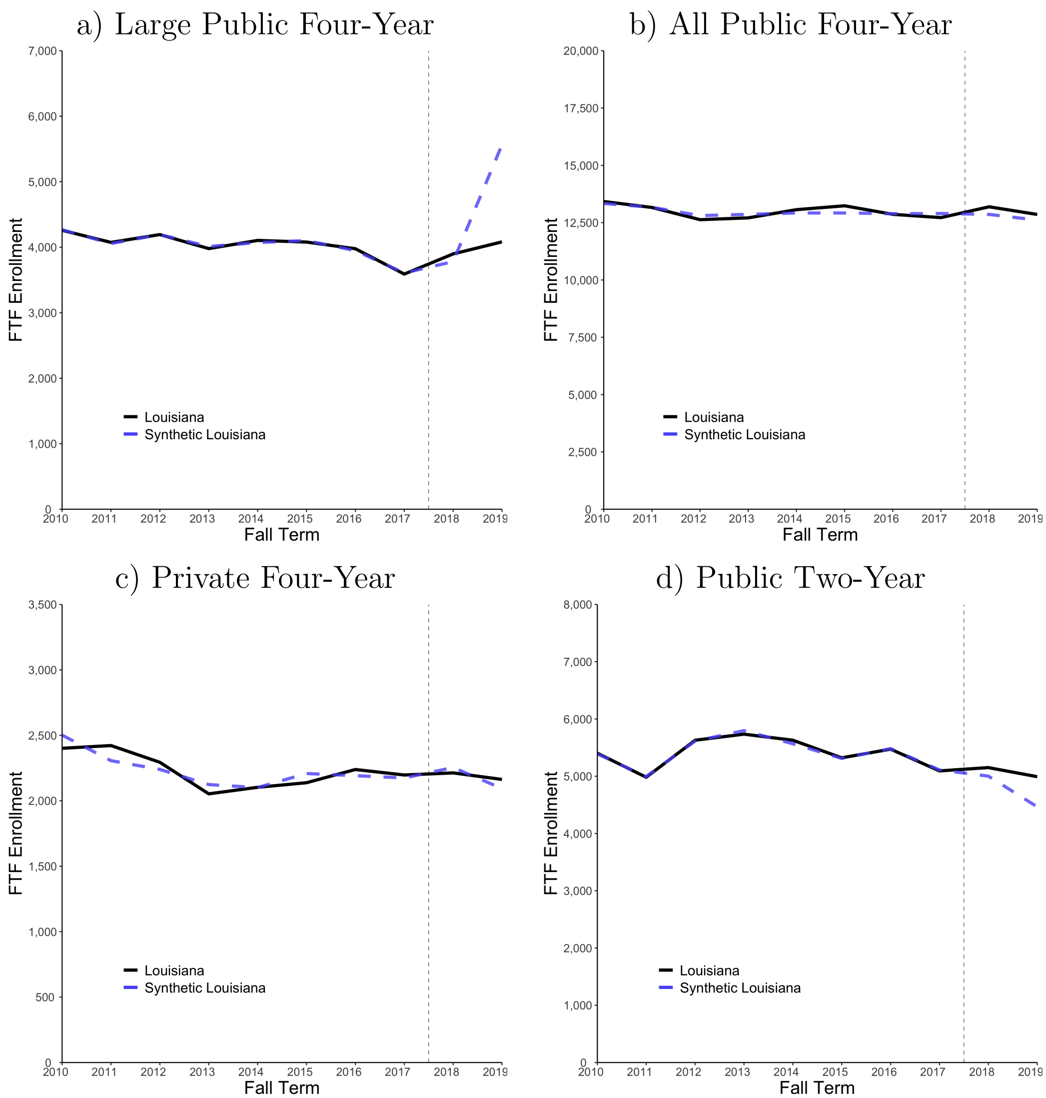


Figure II.8
Enrollment for Black Students in Louisiana, by Insitution Type

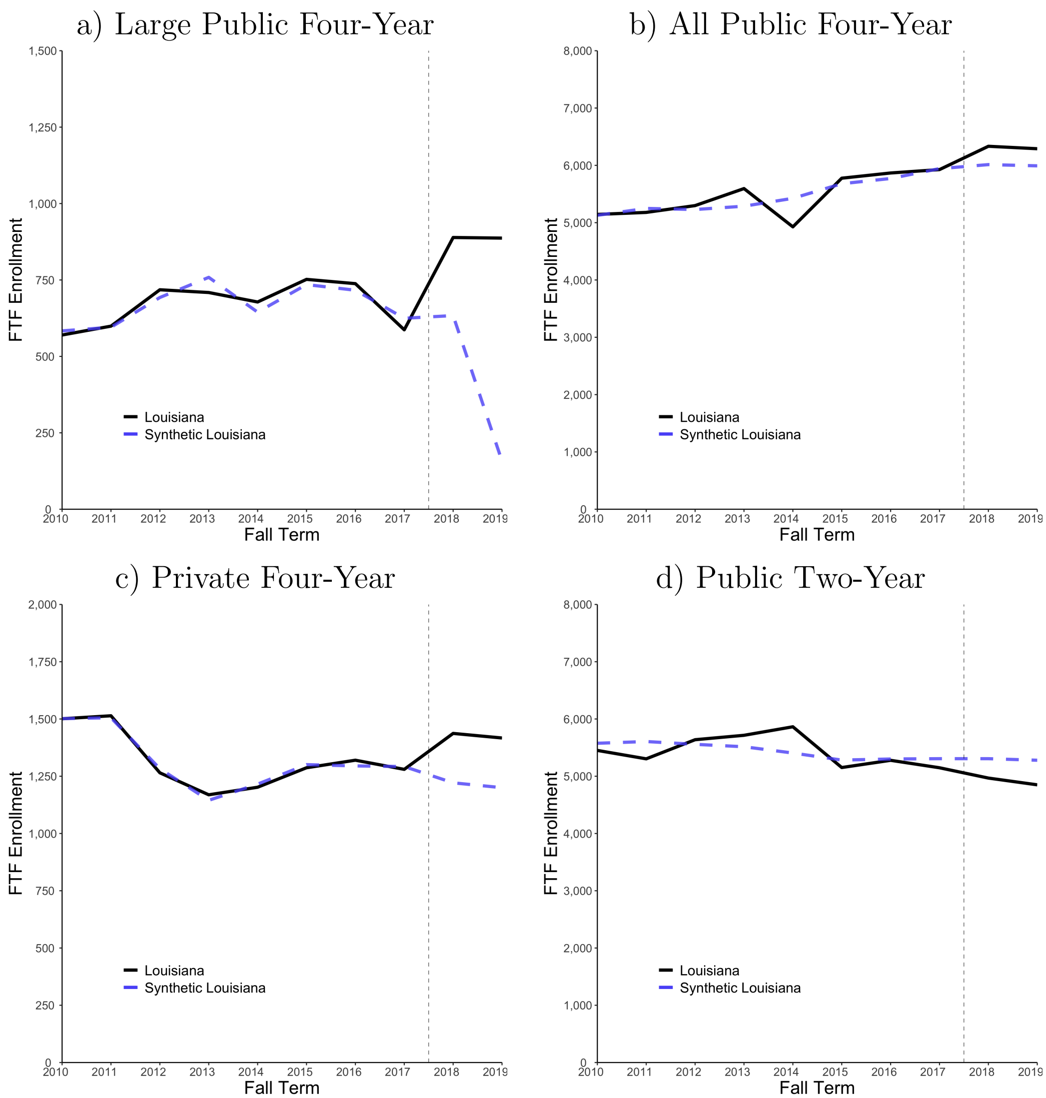


Figure II.9
Enrollment for Hispanic Students in Louisiana, by Insitution Type

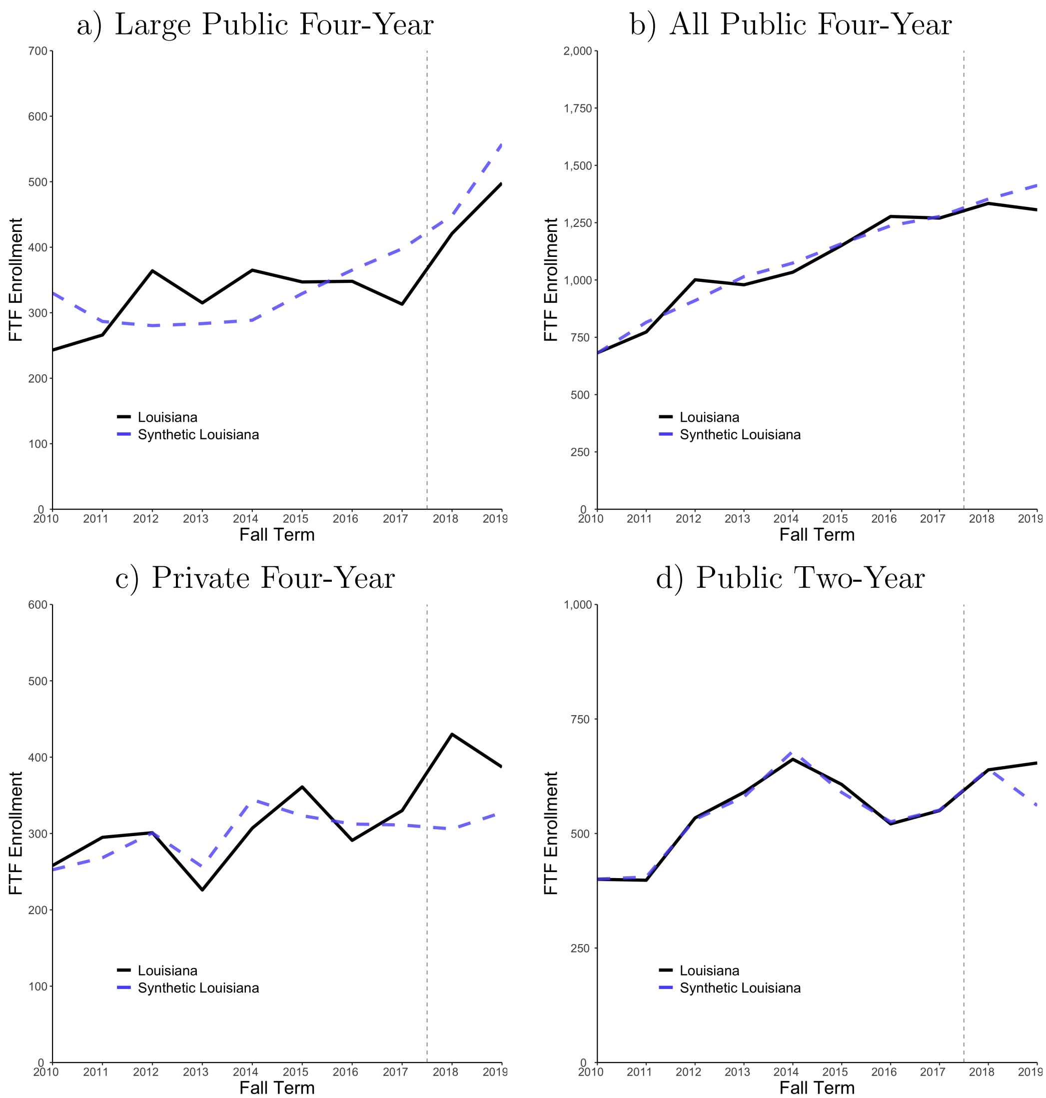
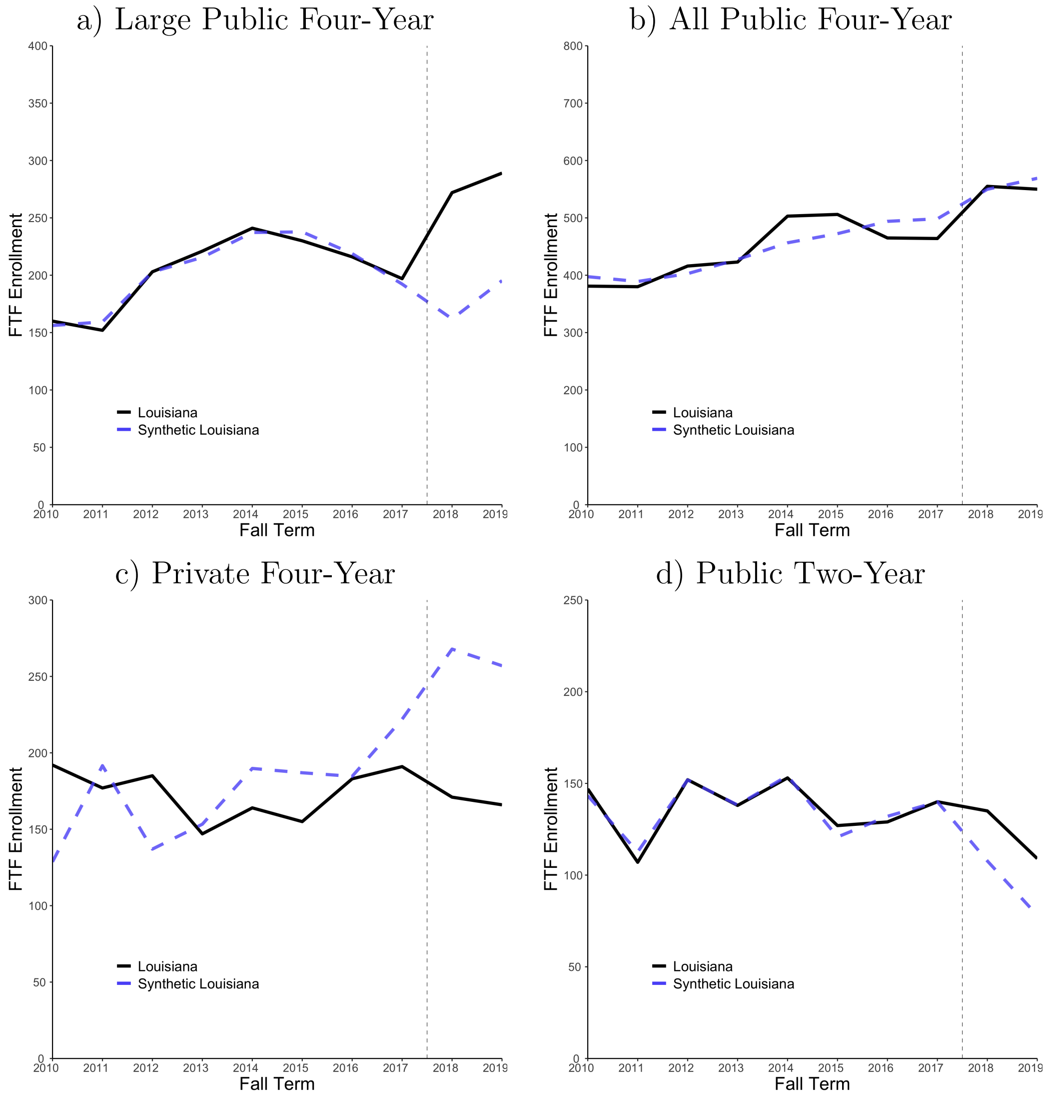


Figure II.10
Enrollment for Asian Students in Louisiana, by Insitution Type



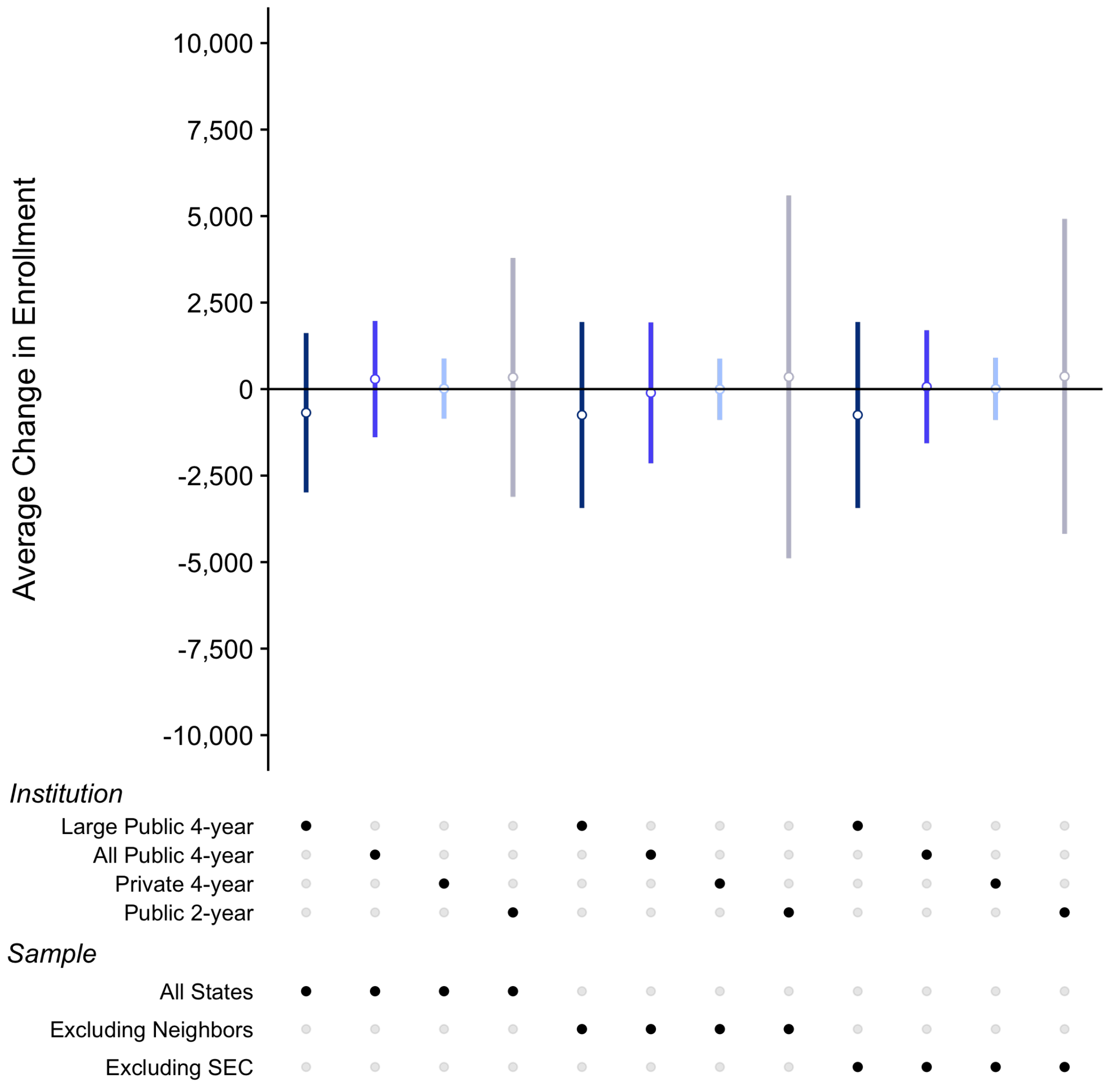
Point estimates from the synthetic control estimation are provided in Figures II.11-II.14. For white, Hispanic/Latino, and Asian students, there is no evidence of enrollment changes due to the policy. The point estimates are small and not statistically significant; the estimates are not affected by the states included in the potential donor pool for the synthetic control. For Black students, I find suggestive evidence of substitution between public two-year shcools and four-year schools. While I cannot rule out that the effect is zero, public two-year enrollment seems to have fallen after the policy introduction and enrollment in four-year institutions appears to have increased. This is particularly plausible for large, public four-year institutions. The ATT at large public four-year institutions is 523 using all states as potential donors to the synthetic control and 689 restricting neighboring states and SEC states from the potential donors. The 95% confidence interval for the estimates obtained with the donor pool restricted is (-32, 1077) students, suggesting that the actual effect of the policy is likely positive.[10] Thus, it is plausible that the FAFSA policy induced Black students to substitute away from community colleges towards larger, more selective four-year institutions.

II.4.2.2 Financial Aid Results

As the FAFSA submission policy may have impacted historically disadvantaged students who were previously deterred from filing a FAFSA, I also examine the effect on financial aid. Specifically, I use synthetic control to estimate the impact of Louisiana's policy on the total amount of Pell Grants awarded to FTF undergraduate students. I use the total Pell Grant amount awarded because using the percentage of students enrolled who have Pell Grants is potentially problematic. If the policy impacted enrollment and it also impacted the number of students on Pell Grants,

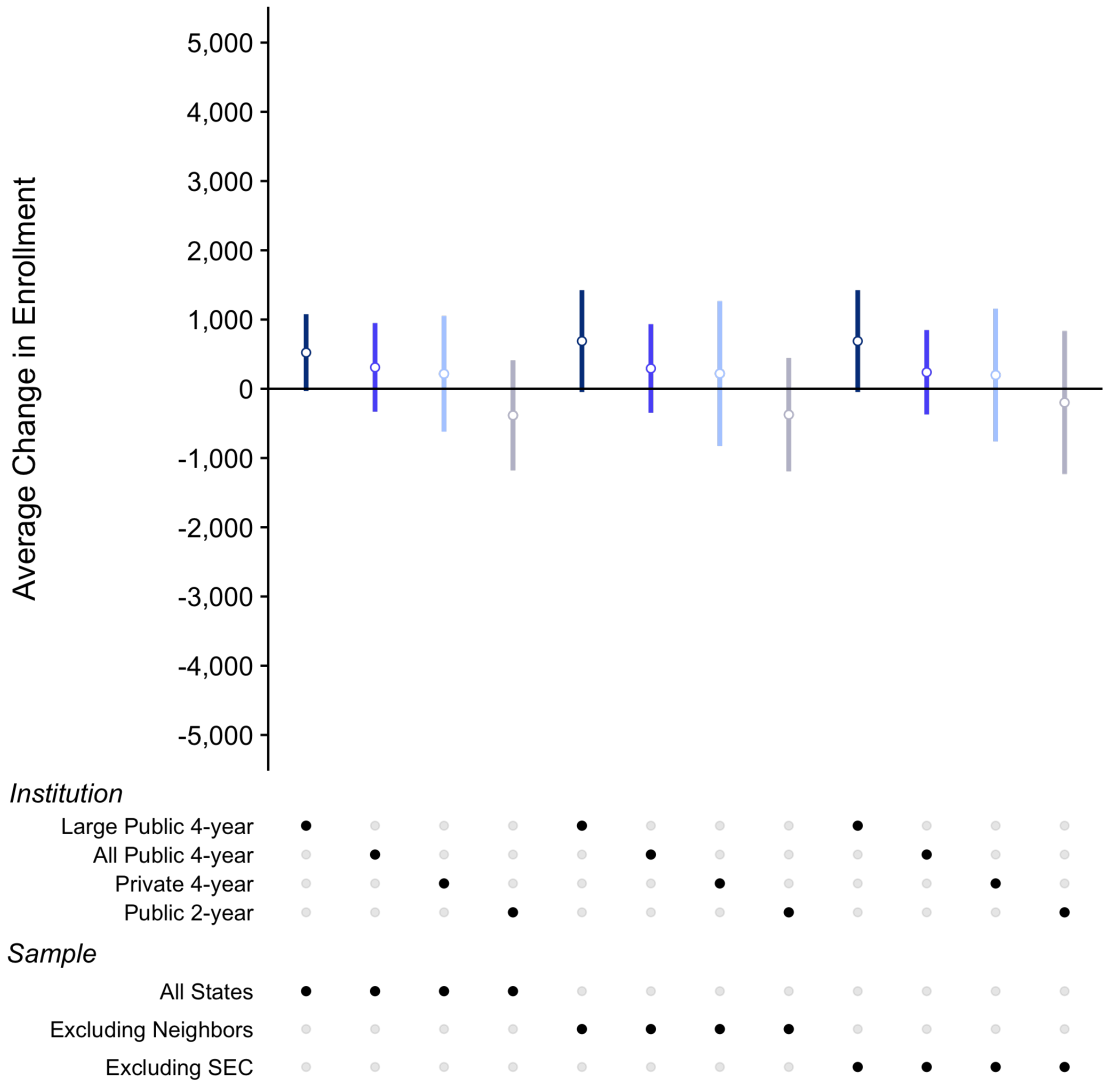
[10] The 90% confidence interval for enrollment at large public four-years obatined from the synthetic control estimated on same sample of potential donors is (105, 1273), suggesting that this estimate is marginally significant.

Figure II.11
The Effect of Mandatory FAFSA Legislation on White Student Enrollment, by Institution Type



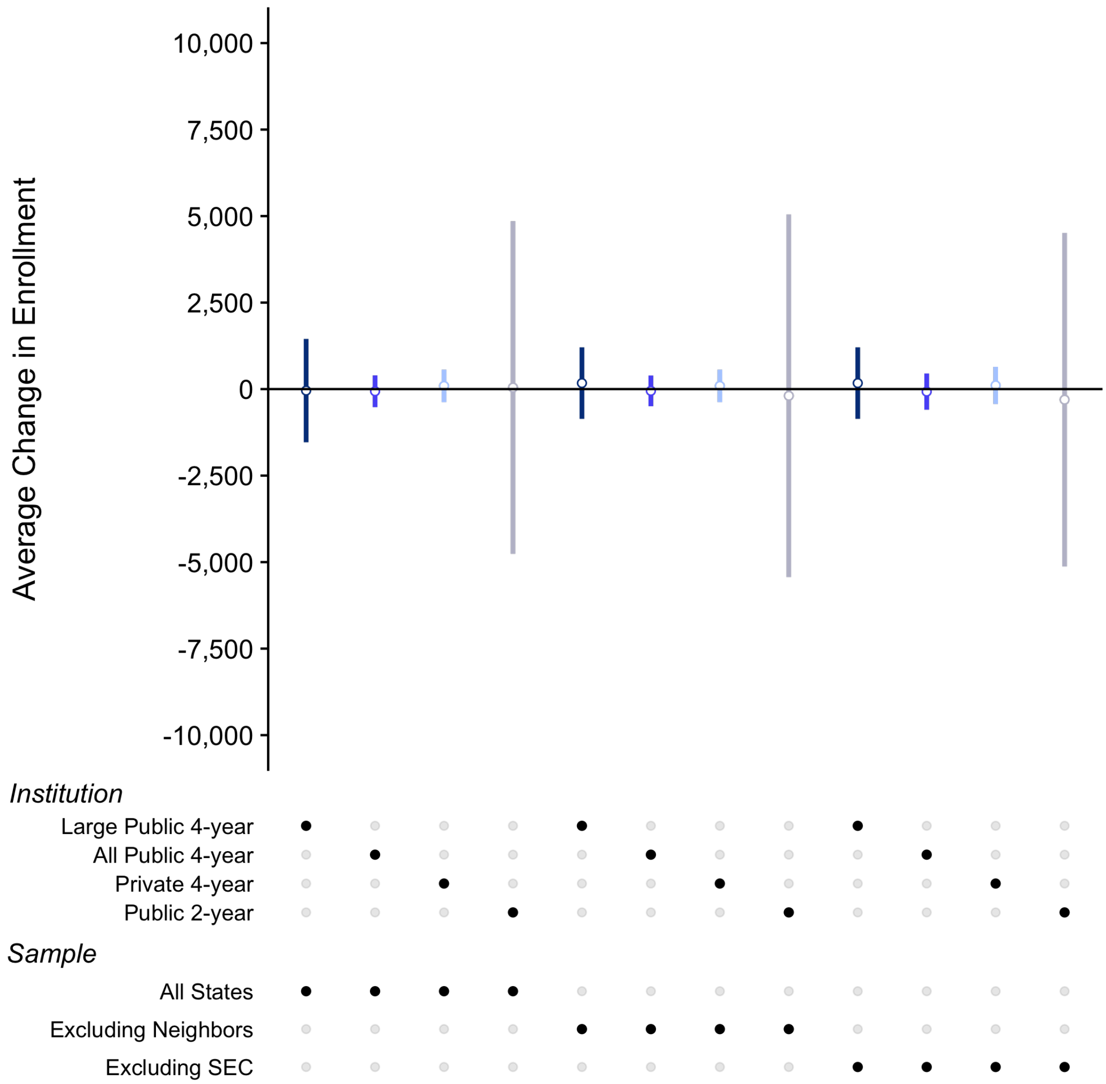
Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for FTF enrollment for white students by institution type. The synthetic control matches on pre-treatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R.

Figure II.12
The Effect of Mandatory FAFSA Legislation on Black Student Enrollment, by Institution Type



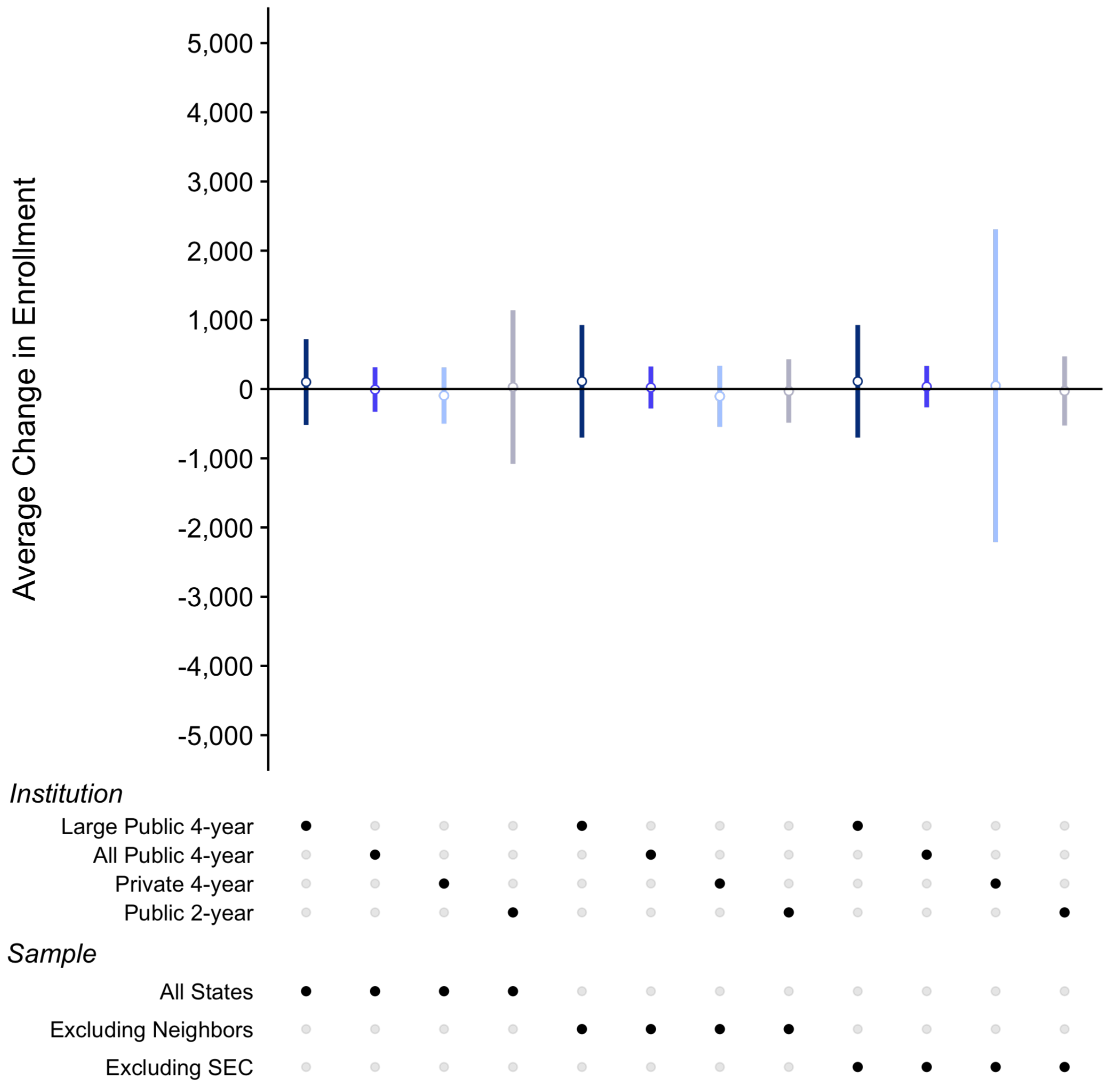
Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for FTF enrollment for Black students by institution type. The synthetic control matches on pre-treatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R. The 95% confidence interval for enrollment at large public 4-year institutions is [-32, 1077] using all states as potential donors.

Figure II.13
The Effect of Mandatory FAFSA Legislation on Hispanic Student Enrollment, by Institution Type



Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for FTF enrollment for Hispanic/Latino students by institution type. The synthetic control matches on pre-treatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R.

Figure II.14
The Effect of Mandatory FAFSA Legislation on Asian Student Enrollment, by Institution Type



Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for FTF enrollment for Asian students by institution type. The synthetic control matches on pre-treatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R.

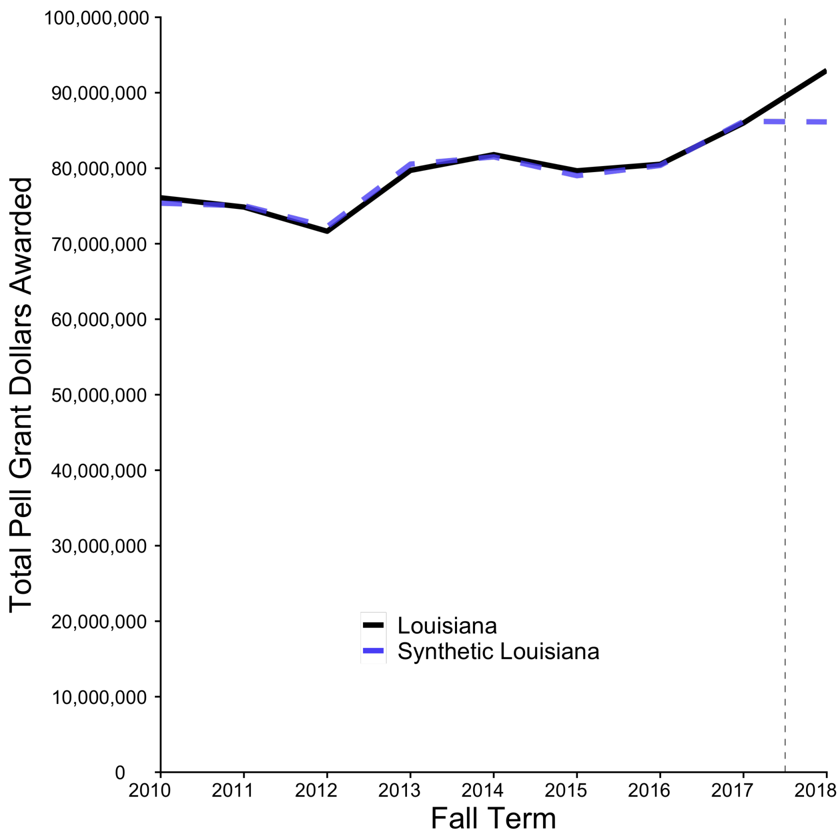
both the numerator and the demonimator in that statistic is moving, thus not giving us a good measure of how the policy impacted Pell Grants alone.

The total amount of Pell Grant dollars disbursed to students in Louisiana increased by just under $7 million from 2017 to 2018. An increase in Pell Grant awards in Lousiana does not necessarily mean that the FAFSA policy had a causal effect on the amount of financial aid disbursed. We need to compare Pell Grant awards in Louisiana to Pell Grant awards in a suitable control group, before and after the policy change. Thus, I again use the synthetic method to estimate the causal impact of the FAFSA policy on total Pell Grant dollars awarded. In Figure II.15 I plot the total dollar amount of Pell Grants at all institutions in Louisiana and its synthetic control.[11] In 2018, there is a small divergence in Lousiana's Pell Grant awards from the synthetic control unit, suggesting that there may have been some increase in Pell Grants due to the FAFSA mandate.

I examine Pell Grant aid by tuition as the FAFSA policy may have induced students who were previously uninformed about the price of college to attend different types of institutions after learning about Pell Grant eligibility. Pell Grant eligibility may have induced students to attend more expensive institutions, so I begin by restricting the sample to schools above 90th percentile for tuition and fees in a given state and year. This gives a total of 415 schools, of which 404 are private four-year, ten are private two-year and one public four-year. The average tuition at these instititutions was $42,572 for the 2018-2019 academic year. I also restrict the sample to public four-years and examine higher-cost public four-year institutions, in the 90th percentile or higer for tuition, separately from other public four-years. I also consider Pell Grant awards at all public two-year institutions, as we could imagine that students who are eligible for a Pell Grant may be more likely to use their financial aid at a community college. Visualizations of Pell Grants by tuition in Louisiana

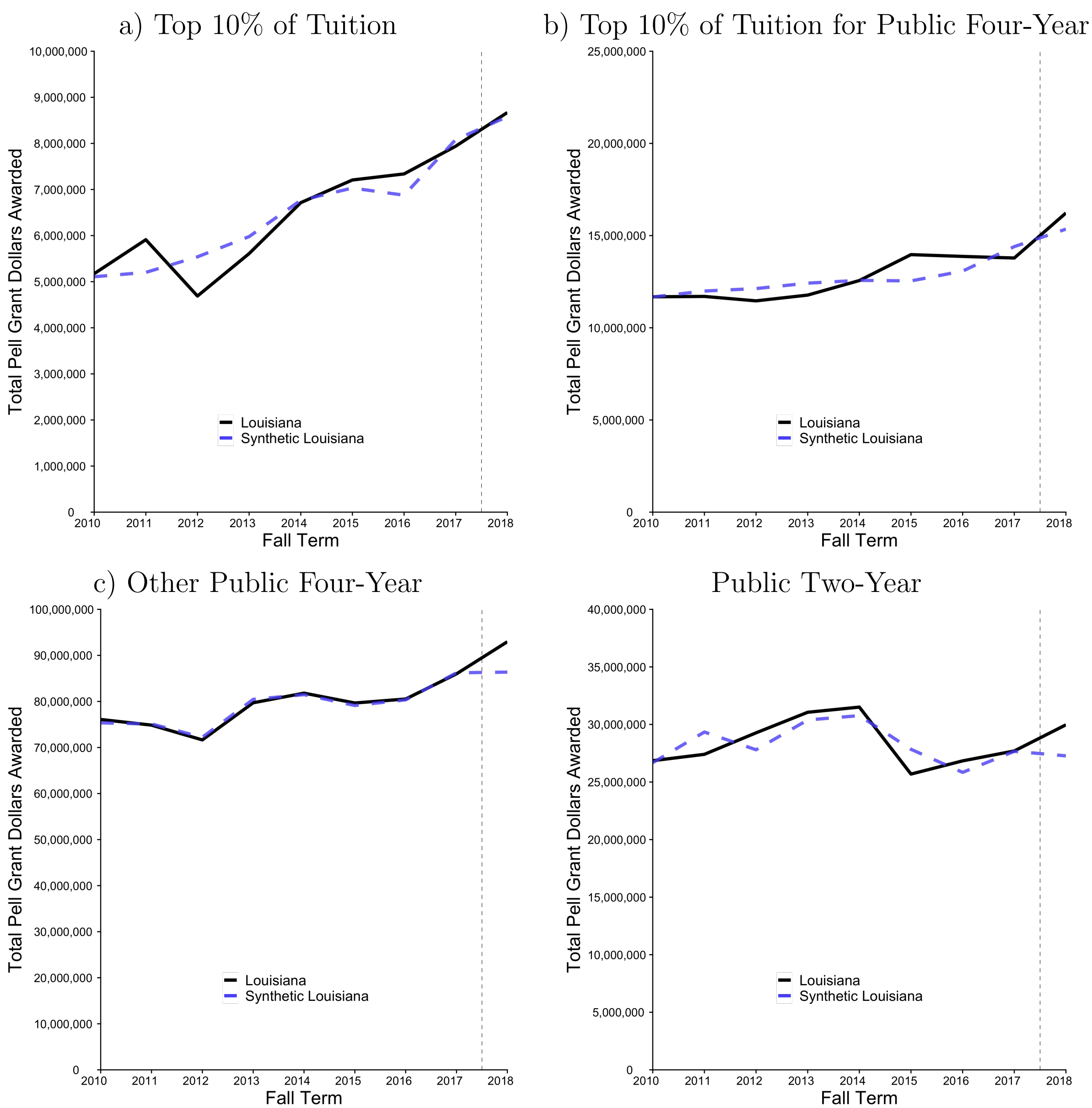
[11] Additional figures are provided in the appendix.

Figure II.15
Total Pell Grant Awards for Lousiana



and the synthetic counterpart are given in Figure II.16. The synthetic control unit does not match well for several of the models, with the exception of lower-cost public four-years displayed in Panel C. Here, we can see a small increase in Louisiana Pell Grant dollars relative to the synthetic control, similar to the small increase displayed in Figure II.15.

Figure II.16
Total Pell Grant Awards for Lousiana by Tuition

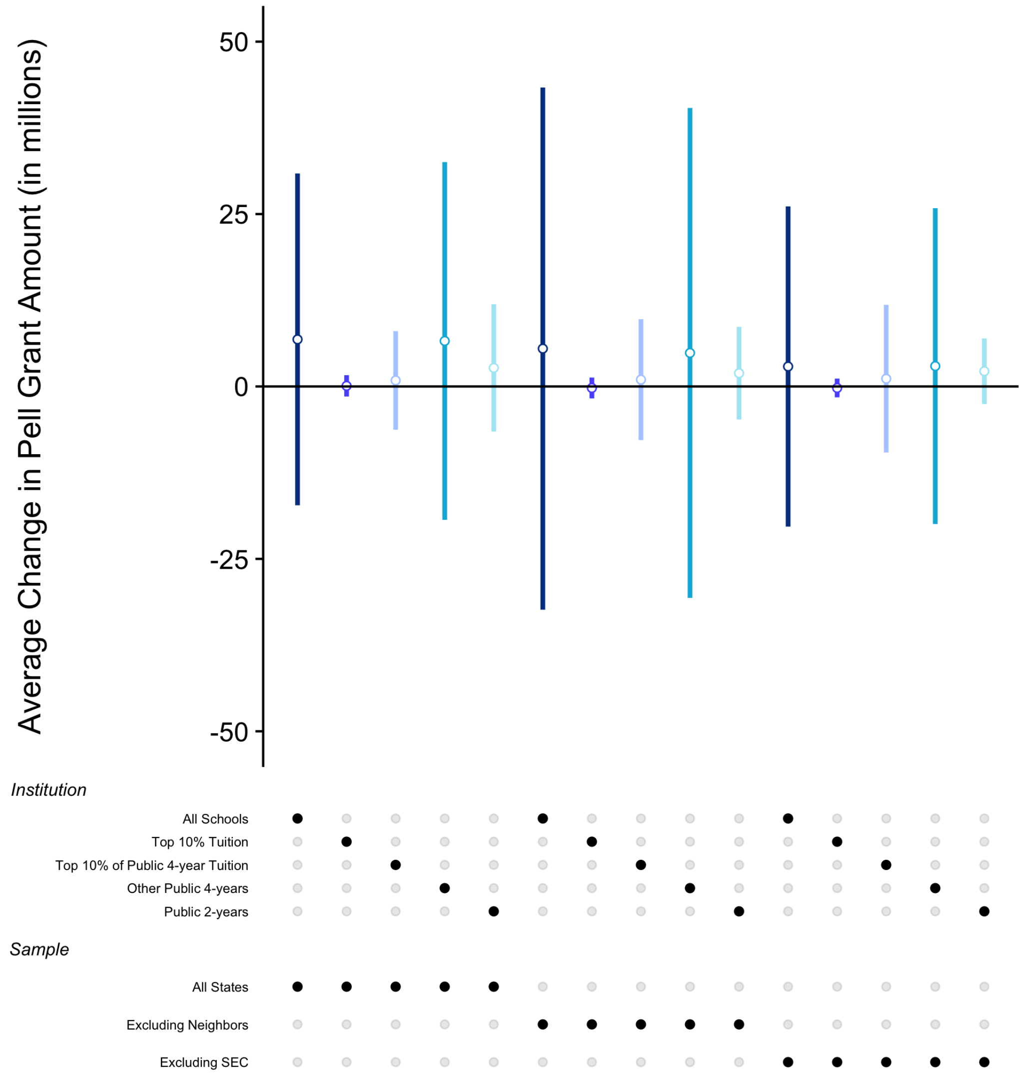


The point estimates and 95% confidence intervals obtained from the synthetic control method are presented in Figure II.17. Lower-cost public four-year and public two-year schools have a postive point estimate. It is possible that the FAFSA mandate informed students–who would have otherwise not completed a FAFSA–about Pell Grant eligibility but did not change their college enrollment plans, thus increasing Pell Grant awards at lower-cost institutions. However, the standard errors on these estimates are large and I cannot rule out that the effect of Louisiana's policy on Pell Grant awards at lower-cost instituitons is zero. The more expensive institutions have small point estimates and are also not significant, suggesting that the policy did not have an effect on the total amount of Pell Grant awards disbursed at these institutions.

II.5 Conclusion

Starting in the 2017-2018 academic year, Louisiana has required high school students to submit a FAFSA application, or opt-out, prior to high school graduation. The state has since garnered support as a pioneer in its attempt to increase the number of students submitting a financial aid application. In this paper, I provide early evidence on whether Louisiana's policy to require FAFSA submission prior to high school graduation translates into increased college enrollments. Using a Generalized Synthetic Control approach, I find suggestive evidence that this policy may have led to a substitution between four-year schools and public two-year schools—particularly for Black students, where I find marginally significant effects on enrollment at large, public four-year institutions. However, I cannot rule out that the aggregate effect of the policy is zero; I also cannot rule out that the policy created large increases or even small decreases in enrollment. I find little evidence that Pell Grant awards increased due to the policy change. This makes sense conceptually, as the FAFSA mandate likely only affected students who were on the margin of enrolling college or students

Figure II.17
The Effect of Mandatory FAFSA Legislation on Pell Grant Awards, by Tuition



Notes: This figure provides point estimates and the 95% confidence interval to the synthetic control estimates for the total amount of Pell Grant dollars awarded, separated by tuition. The synthetic control matches on pretreatment outcomes only. Confidence intervals are computed using bootstrapped standard errors with 5000 iterations. Synthetic control estimation was done using the gsynth package in R.

who would otherwise not have filed a FAFSA form. Louisiana's policy is likely to increase the probability of attendance among these marginal students, especially those for who are most influenced by the cost of college attendance.

It is still too early to declare Louisiana's FAFSA policy a success and more research is needed to determine the effect that mandatory FAFSA submission policies have on college enrollment. Encouragingly, FAFSA submission rates have increased since the policy introduction but perhaps less encouraging is that the policy failed to close the gap in submissions between high-share minority and low-share minority schools or between high-income and low-income schools. The results of this paper also indicate that the policy did not have much effect on FTF enrollment at the state-level, suggesting that more efforts may be needed to encourage students to complete a financial aid application and enroll in college. It is likely that allowing schools time to adjust to the policy change, especially under-resourced and under-staffed schools, may increase their effectiveness at helping students file a FAFSA and prepare for college enrollment. Thus, it would be helpful to study the effects of Louisiana's policy on enrollment using a longer post-treatment period. Unfortunately, the period in which these policies have been implemented now overlaps with the Covid-19 pandemic and the resulting changes colleges and universities have faced. This makes studying long-term effects of the FAFSA policy in Louisiana difficult. It also complicates the policies implemented recently in Texas and Illinois, as the policy introduction in these states directly overlap with the Covid-19 pandemic.

CHAPTER III

DID THE USDA FRESH FRUITS AND VEGETABLES PROGRAM IMPROVE STUDENT PERFORMANCE? EVIDENCE FROM ILLINOIS ELEMENTARY SCHOOLS

III.1 Introduction

Many school-age children do not eat the recommended amount fruits and vegetables in their diets. According to a study by the Center for Disease Control and Prevention, about 60% of children do not eat enough fruit and 93% do not eat enough vegetables to meet dietary benchmarks. Given that fruit and vegetable consumption is asscoiated with increased cognitive development (see Bell et al. (2015) and Lamport et al. (2014) for reviews of related medical literature), eating more of these types of food could also be associated with improved academic outcomes.

The USDA Fresh Fruit and Vegetable Program (FFVP) aims to increase fresh fruit and vegetable consumption among students in elementary schools with a high percentage of students qualifying for free or reduced priced lunch. While the program does not dictate a planned nutrition curriculum, the goals of the program are "to introduce children to fresh fruits and vegetables, to include new and different varieties, and to increase overall acceptance and consumption of fresh, unprocessed produce among children" (U.S. Department of Agriculture, 2017).

While the literature specific to FFVP is small, there is evidence that FFVP has been successful in accomplishing these goals. Several studies find that participation in FFVP increases fruit and vegetable intake among children and adolescents (Jamelske et al., 2008; Coyle et al., 2009; Davis et al., 2009). However, other studies note that this increase in consumption is mostly due to the foods directly provided by the program (Olsho et al., 2015). Bartlett et al. (2013) finds that while fruit and vegetable consumption increases for students in FFVP schools, there is no increase in total energy intake and no change in consumption of other types of food, suggesting that fruit and vegetable consumption was in addition to, rather than in place of, other foods.

There is also evidence that FFVP participation changes behaviors outside of the classroom and has benefits beyond the immediate effects of the food provided. For example, FFVP participation is associated with asking parents for more fruits and vegetables at home or on shopping trips (Bica and Jamelske, 2012; Ohri-Vachaspati et al., 2018)

While not a stated objective of FFVP, the program may have academic impacts. Currently, to the best of my knowledge, there is no causal evidence examining the role of FFVP in academic achievement. Most of the current literature on federally-funded nutrition programs relates to the School Breakfast Program (SBP) and the National School Lunch Program (NSLP). The evidence of the causal effect of school-based nutritional assistance on test scores is mixed. Many studies find positive effects of meal provision on test scores (Figlio and Winicki, 2005; Imberman and Kugler, 2014; Frisvold, 2015), while others find no increases in achievement (Leos-Urbel et al., 2013; Anzman-Frasca et al., 2015; Cuadros-Meñaca et al., 2022). Modest negative effects on test scores have been found in evaluations of Breakfast in the Classroom programs for some groups of students (Cuadros-Meñaca et al., 2022). Previous studies of SBP and NSLP generally find no meaningful effects of these programs on school attendance (Leos-Urbel et al., 2013; Imberman and Kugler, 2014; Frisvold, 2015).

An empirical challenge in evaluating federally-funded nutrition programs is that these programs are often determined by family income and therefore eligibility is non-random. Given the associations between family income and academic performance and family income and health, directly comparing academic outcomes of participants and non-participants would underestimate benefits of school nutritional programs.

In this paper, I consider whether there are measurable academic benefits to food provision using variation in access to a school nutrition program that is unrelated to family income. FFVP is unique in that the program targets low income students, but funding is determined through the schools' characteristics, and students are able to receive produce through FFVP regardless of their family income.

Using a sample of public elementary schools in Illinois, I leverage variation in FFVP participation to estimate the effect of FFVP funding on school-level standardized test scores, attendance rates, and disciplinary referrals. Importantly, not all schools receive FFVP grants at the same time. The program funding expanded since FFVP's inception in 2008, leading to more schools participating in the program. I exploit this staggered rollout of FFVP participation to identify the causal effect of FFVP funding on academic outcomes.

There are several mechanisms through which FFVP participation could effect academic achievement. First, as there is evidence that nutrition is important for cognitive functioning and academic success (Wesnes et al., 2003; Geier et al., 2007), FFVP may directly impact achievement through improved nutrition. Second, students who attend school more regularly would be expected to perform better on standardized tests. With more time spent in the classroom, students gain valuable instructional time that may translate into higher scores on standardized tests. But also, as FFVP is commonly served in the classroom, accompanied by nutrition curriculum, FFVP may take up instructional time that could be used for other instruction. Thus, to the extent that FFVP is disruptive or occupies time that would otherwise be used for instruction towards the test, academic performance may suffer at schools participating in FFVP despite more children having access to nutritional snacks at school.

I find little evidence of school-level achievement impacts in Illinois elementary schools participating in FFVP. Specifically, I find that receiving FFVP funds reduces school-level ELA scores by 0.04 to 0.09 standard deviations, or 0.02 to 0.04

student-level standard deviations. I estimate the effect on mathematics scores to be 0.07 to 0.14 school-level standard deviations, or 0.03 to 0.07 student-level standard deviations. However, the magnitudes are not distinguishable from a null effect. Additionally, I find no meaningful effects of FFVP on attendance rates or the percentage of students taking the standardized test. Given this, the null findings on school-average test scores cannot be explained by increased attendance or more students taking the test.

In Section 2 I provide more information about FFVP in Illinois. I describe my data and empirical methodology in Section 3. In Section 4 I report the results for the analysis of FFVP on academic achievement and explore potential mechanisms. In Section 5 I draw conclusions.

III.2 Background

The USDA Fresh Fruit and Vegetable Program began as a pilot program in 2002 and was expanded in 2004 and 2006. The program became nationwide in 2008. The program aims to create a healthier school environment and identify best practices for increasing students' consumption of fresh foods. The USDA allocates funds to each state; school funding for FFVP is then determined by the state agency administering the program, based on the amount of state funding and student enrollment. Elementary schools participating in FFVP receive \$50 to \$75 per student for the academic year and at least 50% of the grant funds must be spent directly on fresh produce. In Illinois, priority is given to elementary schools with 50% or more of students qualifying for free or reduced priced lunch. However, all enrolled students in eigth grade or below at the FFVP school can receive the fresh fruits and vegetables provided through FFVP, regardless of the their free or reduced priced lunch eligibility (Illinois State Board of Education, 2022).

FFVP must begin within the first week of the school year and continue for the duration of the academic year. Schools are required to serve students a fresh snack at least twice per week—the food may be served at various times during the school day but cannot be served during the federally-funded SBP or NSLP periods. Snacks are most commonly served inside the classroom in Illinois (Illinois State Board of Education, 2022).

III.3 Methodology

III.3.1 Data

I use publicly available data provided by the Illinois State Board of Education. The data contains yearly records of school-level average standardized test scores, attendance rates, student disciplinary referrals, and school characteristics. The Illinois State Board of Education also reports schools that receive FFVP funds for each academic year.[1] The data set covers academic years 2008-2009 through 2018-2019. The full sample is a panel of 2,492 public elementary schools. In this sample, there are 543 elementary schools that received FFVP funds at least once over the sample period and there are 1,949 schools that never participated in FFVP.

I measure school-level achievement using the schools' average English-language Arts (ELA) and mathematics standardized test scores from the Illinois' state standardized test.[2] I standardize the scale scores within subject and year to a mean of zero and a standard deviation of one. Thus, the estimated effects are in school-level standard deviations and can be interpreted as an effect size. As most education research reports student-level effects, I use the ratio of school-level to student-level standard deviations to convert the school-level effects. Using values reported in 2016, student-level standard deviations on the PARCC test are 40.58 for ELA and 36.55

[1]For a complete list of schools receiving FFVP funds for a given academic year, see https://www.isbe.net/Pages/Fresh-Fruit-and-Vegetable-Program.aspx.

[2]From the 2008-2009 academic year through the 2013-2014 academic year, the Illinois elementary standardized test was the ISAT. Starting with the 2015-2016 academic year, the PARCC became the statewide standardized test in Illinois.

for mathematics.[3] School-level standard deviations are calculated from the sample as 17.47 for ELA and 17.01 for mathematics scaled scores.

Descriptive statistics of school-level variables and outcomes are reported in III.1. Column (2) represents the sample of schools that participated in FFVP at least once between 2008 and 2019; column (3) represents the sample of schools that never participated in FFVP over the same time frame. There are some notable differences between treated schools and never treated schools. First, schools that received an FFVP grant at least once had lower average test scores in comparison to schools that had never received FFVP funding. Additionally, as priority for FFVP funds is given to elementary schools based on free and reduced eligibility, it is unsurprising that FFVP participating schools have a higher percentage of students on free/reduced priced lunch. Schools receiving FFVP grants also have a higher share of minority students than non-FFVP schools.

Table III.1
Descriptive Statistics

	Full Sample	FFVP Schools	Non-FFVP Schools
Enrollment	419	410	422
Percent FRPL	57.73	82.31	50.00
Percent White	52.13	23.67	60.26
Percent Black	20.60	44.44	12.90
Percent Hispanic	22.39	31.32	19.83
(Standardized) ELA Score	0.01	-0.78	0.26
(Standardized) Math Score	0.01	-0.74	0.24
Attendance Rate	95.13	94.35	95.38
Number of Schools	2547	543	1,949

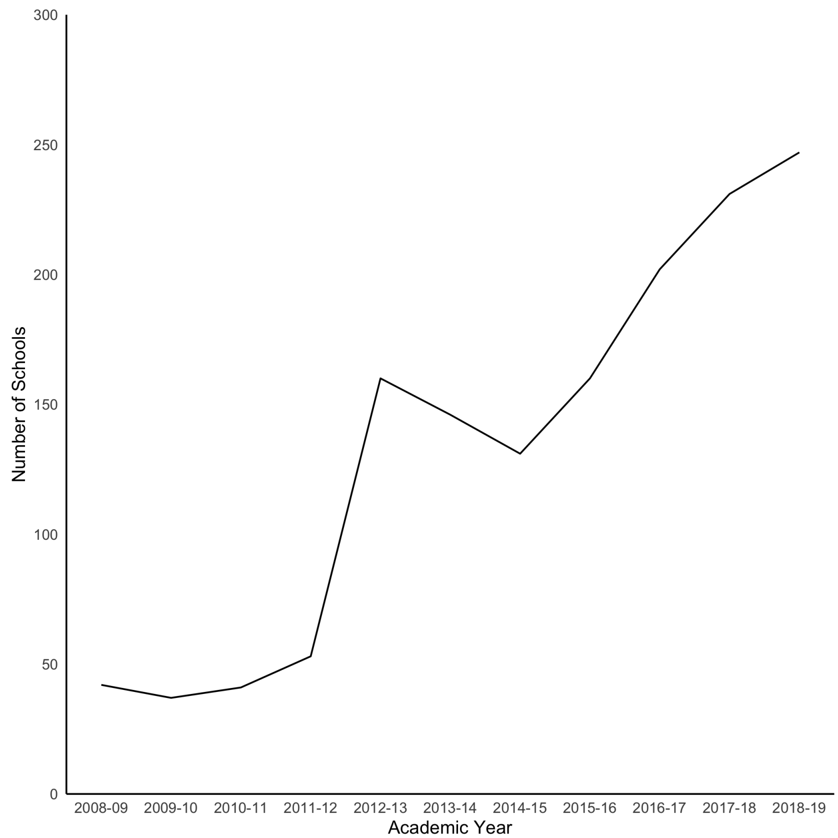
III.3.2 Empirical Strategy

As can be seen in Figure III.1, Illinois began the FFVP program in 2008 and participation has grown over the decade. At the start of the program in 2008-2009, 42 schools were participating in FFVP and 247 schools received FFVP funding in

[3]Student-level standard deviations are reported in PARCC technical reports.

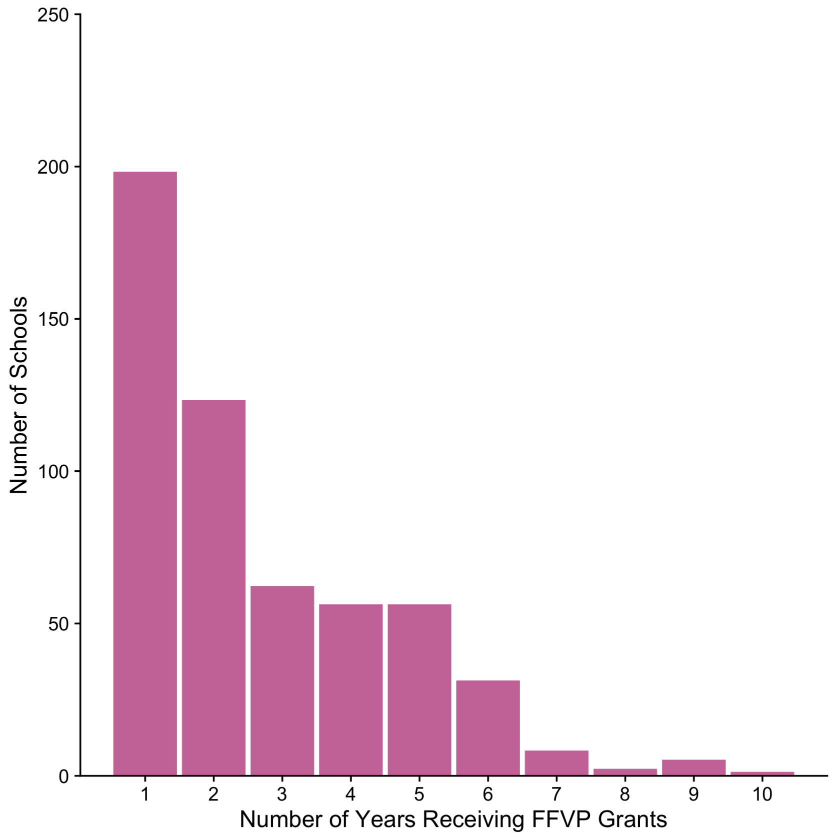
2018-2019. Unlike other staggered adoption designs, schools need not stay "treated". That is, a school may receive FFVP funding for one school year in the sample and does not receive it for the other years. There are also schools who move in and out treatment. For example, a school may have received FFVP funds in 2008-2009 and 2011-2012 but did not receive funds in other school years. As shown in Figure III.2, most of the schools in the sample receive FFVP funding for one or two academic years during the sample period.

Figure III.1
Number of Illinois Public Elementary Schools Participating in FFVP per Academic Year



The identification strategy relies on a two-way fixed effect difference-in-differences approach. The design compares treated schools (schools that received an FFVP grant in a given academic year) to a group of control schools (schools that did not receive an FFVP grant that year).

Figure III.2
Times Schools Receive FFVP Grants over Sample Period



I estimate the model

$$Y_{st} = \beta 1(Treated_{st}) + X_{st}\gamma + \alpha_s + \delta_t + \varepsilon_{st}$$

where Y_{st} is the outcome of interest for school s in year t. The treatment variable, $1(Treated_{st})$, is an indicator equal to one if school s received FFVP funding in year t. The coefficient on the $1(Treated_{st})$ indicator represents"intent-to-treat" effects, as some students may not eat the snacks provided through FFVP. In X_{st}, I include school-level controls (i.e. enrollment, the percent of students who are white, Black or Hispanic/Latino, and the percent who receive free or reduced-price lunch). The model includes school (α_s) and year (δ_t) fixed effects.

The identifying assumption is that a school's receipt of FFVP funding is uncorrelated with other school-specific factors that would affect test performance, after controlling for time-invariant school factors and time-varying school-level characteristics. Then, we can interpret the estimate for β as the causal effect of FFVP funding on school-level achievement. In some models, I restrict the sample to include only schools that ever received an FFVP grant, thus the estimates rely on the variation in the timing of FFVP participation. Here, we need to assume that the timing of treatment was as good as random–that is, schools did not select into treatment in systematic, unobservable ways. For example, schools cannot systematically select into receiving treatment after experiencing lower test scores or lower attendance rates.

Recent research has shown that two-way fixed effects models may not provide unbiased estimates in environemnts with staggered treatment timing (De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Athey and Imbens, 2022). To test the robustness of my two-way fixed effect results, I implement the alternative estimator developed by De Chaisemartin and D'Haultfœuille (2020). This method restructures regression models to use comparison units that are "not-yet-switchers" as a control group and allows for treated units to move in and out of treatment multiple times.[4]

III.4 Results

III.4.1 Test Scores

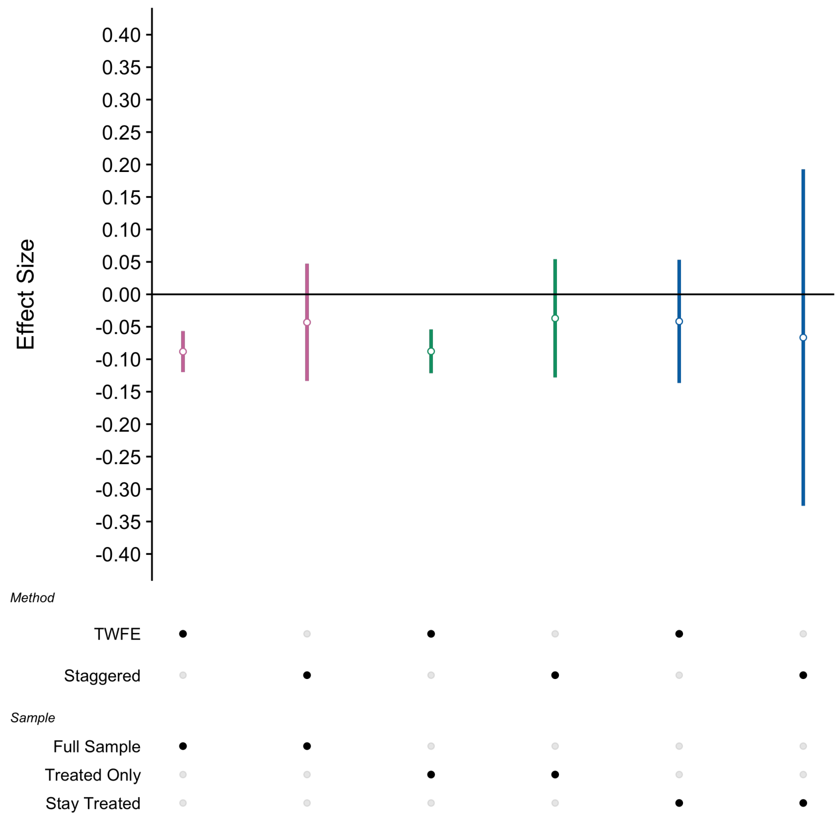
Results are summarized in Figure III.3 and Figure III.4. Using the TWFE approach described in Section 3.2, I estimate that FFVP participation decreases school-level average ELA test scores by 0.088 standard deviations and mathematics test scores by 0.144 standard deviations. Restricting the sample to include only schools that continued to receive FFVP funding after the initial receipt, I estimate the effect on ELA scores to be -0.034 standard deviations and the effect on mathematic scores to be -0.064 standard deviations, but these effects are not statistically significant.

However, note that the TWFE models assume homogeneous treatment effects, which can yield biased estimates when treatment varies over time, thus the variation in FFVP adoption calls for caution when interpreting these estimates. Estimates derived using De Chaisemartin and D'Haultfœuille (2020) are negative, but smaller in magnitude than the TWFE estimates. In these models, the effect size is -0.04 standard deviations for ELA scores and -0.07 standard deviations for mathematics scores. The 95% confidence intervals on these estimates include zero, however, so I am unable to conclude that there is a statistically significant effect of FFVP on ELA or mathematics scores.

One pathway in which test scores may decrease due to FFVP is through the number of students taking the standardized test. If lower-performing students are induced into attending school more frequently and are thus more likely to take the test,

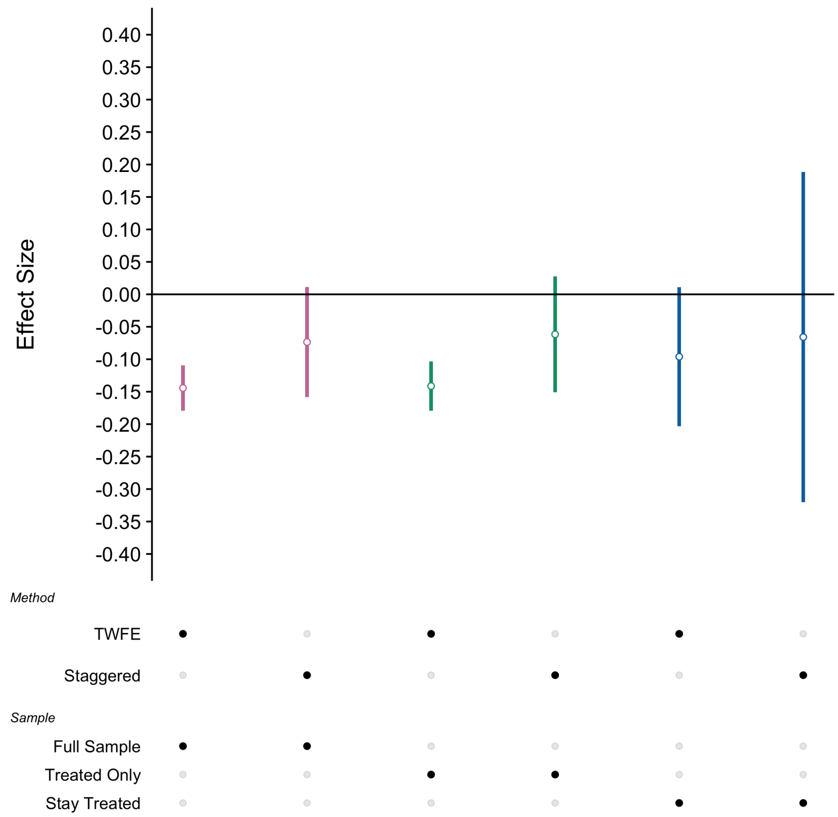
[4]This method relies on an assumption that is a variation on the standard parallel trends assumption for two-way fixed effect difference-in-differences models. To check for pre-trends, I use "placebo" tests as described in De Chaisemartin and D'Haultfœuille (2020). The placebo estimators should not significantly differ from zero. The results of these placebo tests can be found in Appendix A.

Figure III.3
Estimated Effect of FFVP on School-Level ELA Scores



Note: Each bar represents the average effect of FFVP participation on school-level ELA test scores. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,040 school by year observations; the treated only sample contains 3,843 observations; the stay treated sample contains 1,004 observations. Standard errors are clustered at the school-level.

Figure III.4
Estimated Effect of FFVP on School-Level Mathematics Scores



Note: Each bar represents the average effect of FFVP participation on school-level mathematics test scores. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,040 school by year observations; the treated only sample contains 3,843 observations; the stay treated sample contains 1,004 observations. Standard errors are clustered at the school-level.

standardized test scores at the school-level may fall. I repeat the analysis described in Section 3.2 using the percentage of students not taking the ELA and mathematics test as an outcome. I find no evidence of changes in the percentage of students taking the standardized test that can be attributed to FFVP participation. Figures III.5 and III.6 report the results.

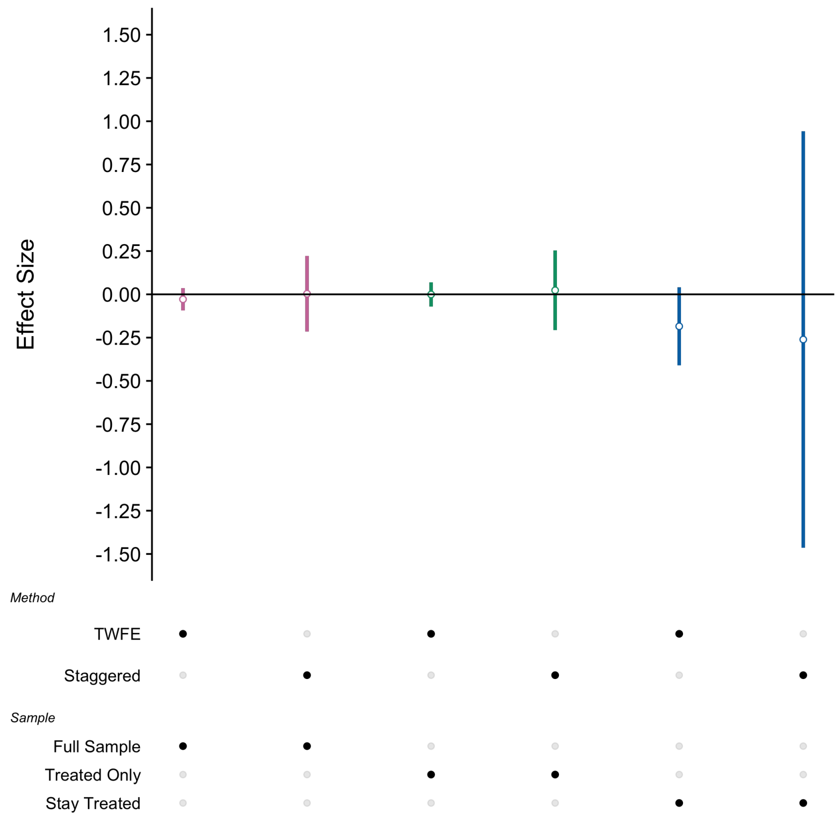
A cautious interpretation of these results would be that schools participating in FFVP do not see changes in school-level standardized test scores that can be attributed to program participation. While the point estimates are consistently negative, I am unable to conclude that FFVP funding causally impacts standardized test scores. We should note that the unit of observation is the school and school-level estimates may be masking heterogeneous effects. The benefits associated with increased nutrition through FFVP may vary differentially by income, as FFVP is likely to have greater impacts for the most food insecure students, who are more likely to be lower-income and lower-performing students (Alaimo et al., 2001; Case et al., 2005). Additionally, the outcome variable is a school-level average of the scaled scores for every grade tested. With the data available, I am unable to estimate the effect of FFVP on standardized test scores by grade level, masking any heterogeneity by grade that may be present.

III.4.2 Attendance

FFVP participation may increase attendance among students for whom the healthy food is an incentive to attend. Additionally, if a healthier diet promotes wellness, improvements in nutrition may lead to increased attendance due to reductions in illnesses or other factors.

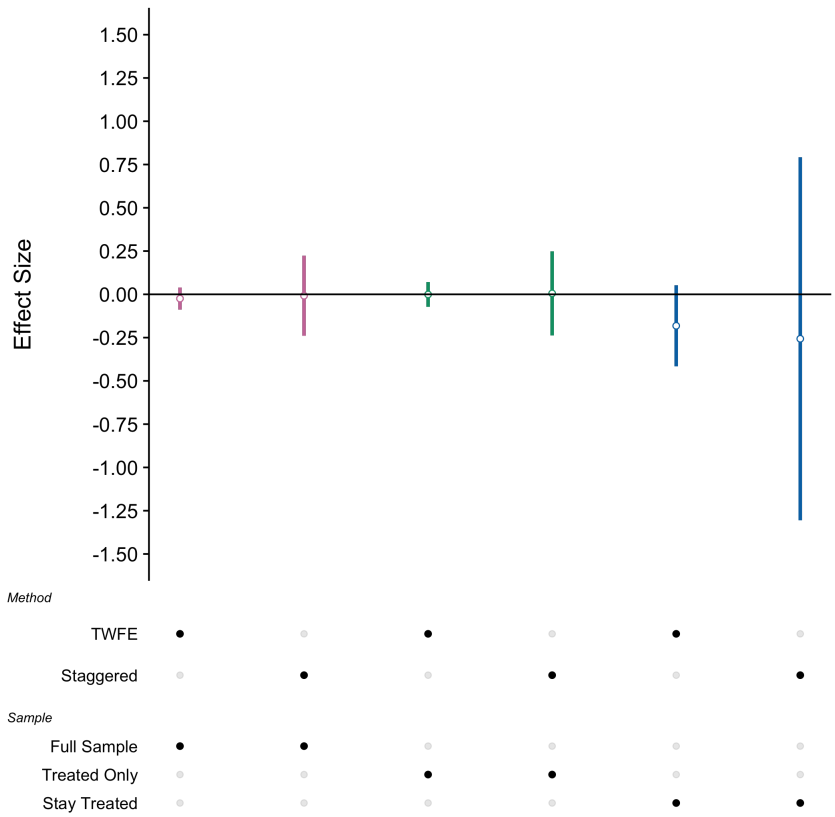
To estimate the effect of FFVP grants on attendance, I repeat the analysis described in Section 3.2 using school-level attendance rate as the outcome. Results are reported in Figure III.7. In the full sample of schools and treated school sam-

Figure III.5
Estimated Effect of FFVP on the Percentage of Students Not Taking ELA Test



Note: Each bar represents the average effect of FFVP participation on the percentage of students in the school that did not take the ELA standardized test. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,040 school by year observations; the treated only sample contains 3,843 observations; the stay treated sample contains 1,004 observations. Standard errors are clustered at the school-level.

Figure III.6
Estimated Effect of FFVP on the Percentage of Students Not Taking Mathematics Test



Note: Each bar represents the average effect of FFVP participation on the percentage of students in the school that did not take the mathematics standardized test. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,040 school by year observations; the treated only sample contains 3,843 observations; the stay treated sample contains 1,004 observations. Standard errors are clustered at the school-level.

ple, I estimate an increase in the school attendance rate of 0.09 standard deviations ($p < 0.05$) for schools participating in FFVP, using a TWFE approach.[5] Attendance rates in Illinois elementary schools are already high (94% in schools that received an FFVP grant at least once)–thus an effect of 0.09 standard deviations translates to approximately one quarter of a school day in a 180 day school year. However, using the staggered differences-in-differences methodology, the estimated effect goes to zero. In the sample of schools that remain treated after first receiving FFVP, I estimate negative, insiginificant effects on school attendance. Given this, I am unable to conclude that FFVP participation has meaningful effects on school attendance.

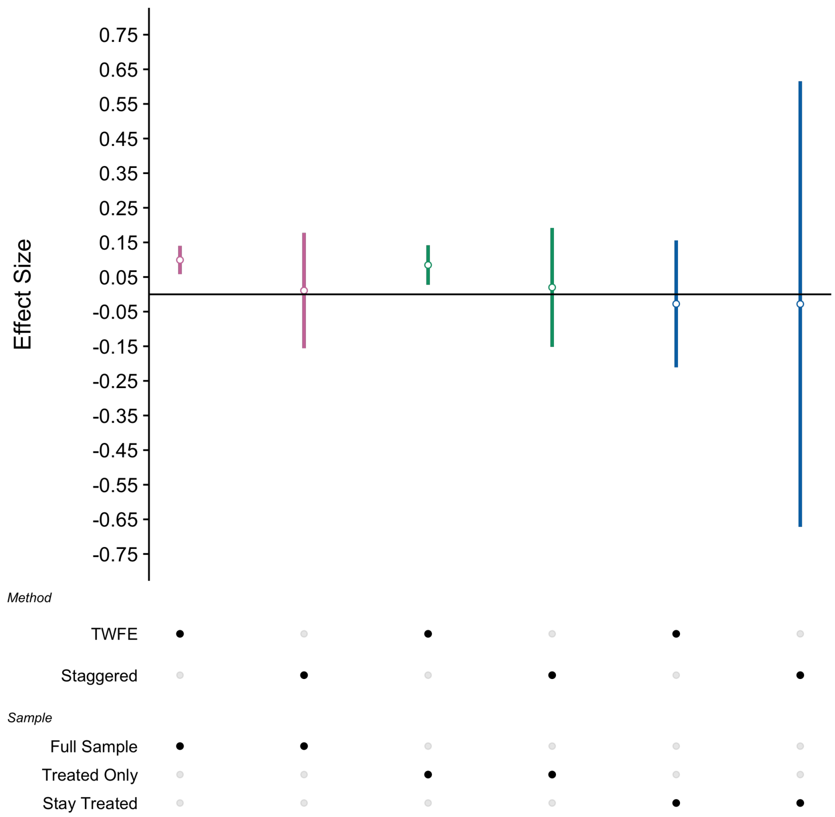
III.4.3 Disciplinary Actions

Overall, the results on disciplinary actions are inconclusive. For in-school suspensions, I estimate preicse null effects for the full sample of schools and the sample of schools that had ever received FFVP. When restricting the sample to schools that remain treated after initial FFVP receipt, there is a marginally significant, negative effect of 0.12 standard deviations using the TWFE approach. This effect size translates to about four fewer in-school suspensions for an elementary school in one academic year. However, note that using the De Chaisemartin and D'Haultfœuille (2020) methodology, I am unable to distinguish the magnitude from a null effect.

For out-of-school suspensions, I estimate negative effects in the full sample and in the FFVP schools sample. In the TWFE models, the effect is -0.11 standard deviations ($p < 0.05$). The effects estimated with the staggered difference-in-differences methodology are indistinguishable from zero. Last, in the sample of schools that remain treated after receiving FFVP funding, the effect of FFVP funding on suspensions is positive, but insiginificant.

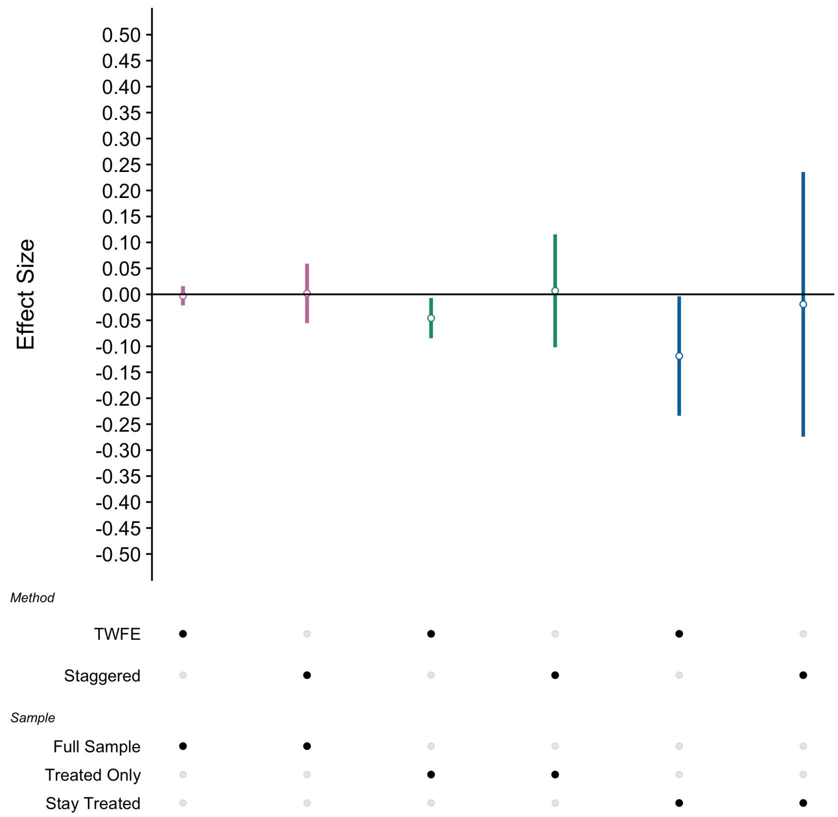
[5]An effect size of 0.09 standard deviations corresponds to a 0.14 percentage point increase in the school-level attendance rate.

Figure III.7
Estimated Effect of FFVP on School-Level Attendance Rate



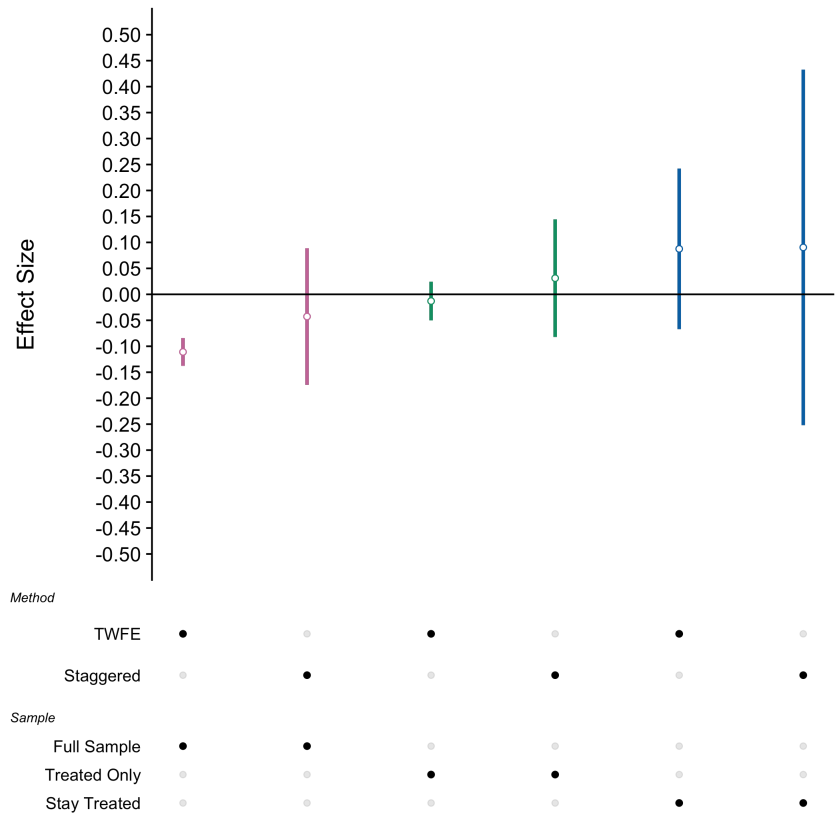
Note: Each bar represents the average effect of FFVP participation on school-level attendance rates. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,040 school by year observations; the treated only sample contains 3,843 observations; the stay treated sample contains 1,004 observations. Standard errors are clustered at the school-level.

Figure III.8
Estimated Effect of FFVP on In-School Suspensions



Note: Each bar represents the average effect of FFVP participation on school-level in-school suspensions. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,079 school by year observations; the treated only sample contains 1,724 observations; the stay treated sample contains 967 observations. Standard errors are clustered at the school-level.

Figure III.9
Estimated Effect of FFVP on Out-of-School Suspensions



Note: Each bar represents the average effect of FFVP participation on school-level in-school suspensions. TWFE effects are estimated using a linear regression as described in Section 3.2. Staggered difference-in-differences effects are estimated using the methodology in De Chaisemartin and D'Haultfœuille (2020). Each regression controls for school-level characteristics. The full sample includes 17,079 school by year observations; the treated only sample contains 1,335 observations; the stay treated sample contains 964 observations. Standard errors are clustered at the school-level.

III.5 Conclusion

Using a difference-in-differences design with multiple time periods and variation in treatment timing, I examine the effect of FFVP on academic performance in Illinois elementary schools. I find suggestive evidence of a negative effect of FFVP participation on school-level ELA and mathematics scores. However, I cannot rule out that these are null effects. I also find no meaningful effects on attendance rates or suspensions.

The analysis uses school-level average test scores as an outcome, which may be masking underlying heterogeneity. As FFVP funds are targeted at schools with the majority of students receiving free or reduced priced lunch, there could be differential effects of FFVP by income status. Additionally, the current data does not allow for examining the effect on test scores by grade or by how young children were when first exposed to FFVP. Further research on these dimensions would be beneficial to determine causal effects of FFVP participation on student achievement.

Despite the lack of evidence in support of achievement gains due to FFVP, the program could have unobserved effects, such as increased caloric intake on days where fresh fruits and vegetables are served, that do not show up in the long-term. Additionally, FFVP could improve other areas of development that are not assessed via standardized testing.

CHAPTER IV

READING RESOURCES AND STUDENT ACHIEVEMENT: EVIDENCE FROM THE MICHIGAN CULTURE OF READING PROGRAM

IV.1 Introduction

There is a growing literature that uses experimental or quasi-experimental methods to examine the effect of school resources on student achievement—achievement gains associated with reducing student-teacher ratios (Angrist and Lavy, 1999), with increasing instructional time in the classroom (Hansen, 2007; Marcotte, 2007), and with higher teacher quality (Rivkin et al., 2005), for example.

In terms of books, in particular, there are a few studies in the economics literature that identify the role of books on reading outcomes.[1] First, Holden (2016) finds significant increases in reading test scores around the implementation of a one-time textbook provision in California, leveraging the school-level qualification for resources in an environment that had sufficient monitoring to be confident that the resources actually increases textbook provision for treated students. Second, Guryan et al. (2016) examines the effect of incentives on reading through a summer reading program that mailed books to elementary students. One group of students were also given rewards based on how many books they read. Guryan et al. (2016) finds that more motivated readers, who received books that were well-matched with their reading level, had significant gains in reading comprehension test scores. Third, and perhaps

[1] There are several papers that consider related questions with a focus on the developing world. Hanushek (1995) argues that the provision of capital-related resources can have an effect on achievement in the right settings but schools often use these resources inefficiently. For example, Glewwe et al. (2009) finds negative effects on achievement in a sample of primary schools in Kenya, but in the region where the study took place, the textbooks provided were above the reading levels of the average child. A similar study conducted in Sierra Leone also finds no effect of reading resources on test scores (Sabarwal et al., 2014), but notes that many of the textbooks did not reach students in the program.

more relevant given that it also considers early readers, Bennett (2020) studies the effect of Dolly Parton's Imagination Library, a program that mails free books to young children, on elementary achievement. Bennett (2020) finds null effects on third- and fourth-grade achievement, suggesting that small capital investments, such as books, are not sufficient to increase achievement on their own.

There are also several studies examining reading programs that offer more than access to books. For example, Kim and White (2008) finds that teacher and parent involvement can enhance the effectiveness of reading books during summer vacation. Specifically, priming students to use reading strategies and to read aloud to their parents was found to increase reading achievement, but students who only received books saw no gains in reading scores. In another summer reading program, fourth-grade children received eight books to read during summer vacation, and were encouraged by their teachers to practice oral reading at home with a family member. This program increased students' reading achievement, with larger effects for disadvantaged students (Kim, 2007). Given these results, perhaps more actions are necessary to positvely impact achievement than just increased acess to books.

In this paper, I estimate the effect of a reading program aimed at young children on their reading abilities in third grade—their performance on the third-grade student achievement test. Specifically, I analyze Michigan's "Culture of Reading" program, which was implemented in 2014. The state of Michigan received 740 applications for the program. Awards were allocated on the basis of meeting grant criteria, including a commitment to provide the children with evidence-based reading instruction and family engagement activities focused on literacy.[2] The Culture of Reading program gave a storybook and reading instructions for the storybook to students in eligible classrooms; additionally, the teachers in the selected classrooms were intended to

[2] Evidence-based reading instruction refers to instructional practices that have been proven by peer-reviewed research to lead to predictable gains in reading achievement.

use evidence-based reading instruction in their reading curriculum.[3] In total, 3,000 books were given to 115 classrooms—24 early childhood programs, 23 kindergarten classrooms, 23 first-grade classrooms, 21 second-grade classrooms, and 24 third-grade classrooms.

Due to the multifaceted approach of the Culture of Reading program, there are several potential mechanisms through which the storybooks could influence reading achievement. First, having an additional book at home may lead to increased student achievement. For example, Evans et al. (2010) finds that having many books at home is correlated with increased educational attainment. Having physical books in the home at a young age may lead to increased literary development later in students' academic careers (Feng et al., 2014). Additionally, having books around the home may encourage a more positive learning environment and may encourage parents to spend more time reading with their children, which can have positive effects on reading achievement (Bradley et al., 2001; Hood et al., 2008; Yarosz and Barnett, 2001).

Second, the program aimed to engage parents in the reading process. Parental involvement in education has a large positive effect on achievement that is large relative to the effect of school resources (Houtenville and Conway, 2008). Family learning activities, such as book reading, are shown to improve childrens' literacy and reading achievement (Bus et al., 1995).

The program also aimed to engage teachers in the reading progress through implementation of evidence-based reading instruction at school. Much of the current work on the effectiveness of evidence-based reading instruction examines school-level interventions, where the effects tend to be mixed. For example, the Reading First Initiative, implemented in 2002-2003 through the No Child Left Behind Act, is a national program aimed at using evidence-based reading instruction in elementary

[3] The same storybook, titled "Acoustic Rooster and His Barnyard Band", was given to students in the classrooms that received awards.

schools. Some studies find positive effects on reading achievement (Ratcliff et al., 2011), whereas other studies find no difference in reading comprehension scores between students at schools using the Reading First Initiative reading instruction and students at schools using traditional reading instruction (Gamse et al., 2011).

Identifying causal impacts of school resources on student achievement is often difficult. For example, if the allocation of school resources is nonrandom with respect to student or school characteristics, we stand to mistakenly credit resource provision when any omitted variable that co-varies with those resources and ELA test scores could explain observed differences. Differences in local income levels could explain differences in ELA scores, for example, and if resources are systematically available in schools that just happen to serve lower-income families, then we may be identifying a relationship between income and student achievement instead of resources and ELA scores. Thus, exploiting cross-sectional differences could confound the effect of resources on reading achievement with school or household characteristics that may also impact achievement, such as fixed school inputs, the characteristics of peers, or neighborhoods. To retrieve causal estimates of resources' effects, such as the effect of books on student achievement, we seek plausibly exogenous variation in the timing of the provision of those resources. The Michigan Culture of Reading program yields a quasi-experimental setting in which there is a plausibly exogenous assignment of resources to students. Assuming there are no unobservable differences between classrooms selected for the program and classrooms not selected that change over time, this setting will allow us to estimate the causal impact of receiving books and evidence-based reading instruction on third-grade ELA test scores.

There has yet to be an evaluation of the Culture of Reading program and the potential benefits that may come from such low-cost interventions. I find large and significant gains in third-grade performance among students who were given a free book, family reading activities, and evidence-based reading instruction while enrolled in a Pre-K school. I find no such effects for students who received similar treatment even one year later in kindergarten, or in first or second grades.

In Section 2 I describe my data and empirical methodology. In Section 3 I report the results for the analysis of the Culture of Reading Program on third-grade achievement and explore heterogeneity in the effect for various subgroups. In Section 4 I draw conclusions.

IV.2 Empirical Design

IV.2.1 Data

The data used in this paper are from public schools in Michigan and are provided by the Michigan Education Research Institute.[4] The data contains yearly records of student-level demographic data for students who were enrolled in Pre-K through third-grade from 2014-2015 through 2018-19. The data also contains yearly records of third grade standardized test scores from 2014-2015 through 2018-2019.

My sample contains information on public school students and therefore I only observe students who attended a public Pre-K program. In 2014-2015, there were 46,114 Pre-K students in enrolled in a Michigan public school. In the following school year, 37,774 of those students stayed enrolled in a Michigan public school—14,391 students continued on to attend kindergarten and 23,198 remained enrolled in a public Pre-K program. Due to availability of data, the analysis for Pre-K students only include students who were enrolled in a public Pre-K program in 2014-2015 and progressed to third grade in 2018-2019.

I measure student achievement using standardized test scores from Michigan's

[4]This research result used data structured and maintained by the MERI-Michigan Education Data Center (MEDC). MEDC data is modified for analysis purposes using rules governed by MEDC and are not identical to those data collected and maintained by the Michigan Department of Education (MDE) and/or Michigan's Center for Educational Performance and Information (CEPI). Results, information and opinions solely represent the analysis, information and opinions of the author(s) and are not endorsed by, or reflect the views or positions of, grantors, MDE and CEPI or any employee thereof.

M-STEP standardized test. The M-STEP test was implemented in 2014-2015 and third-grade public school students in Michigan take the exam in the spring. The test covers English-language arts and mathematics. The Michigan Department of Education reports student-level scaled scores and proficiency levels. I standardize the scale scores within subject and year to a mean of zero and a standard deviation of one. Thus, the estimated effects are in student-level standard deviations and can be interpreted as effect sizes; a one unit increase in achievement can be interpreted as the average student scoring approximately one standard deviation higher than the reference cohort in that same grade.

Descriptive statistics are reported in Table IV.1. The table presents average values of student-level demographic variables used for each grade. Column (1) represents the sample of treated observations, column (2) represents the sample of untreated observations, and column (3) represents the difference between the treated sample and untreated sample. As the Culture of Reading program was targeted at low-income students, we would expect to see that the treated sample is composed of a higher percentage of low-income students. This is the case for each grade in the sample. Students that are able to access the Culture of Reading program are also more likely to be in an at-risk program and are more likely to be African-American/Black.

IV.2.2 Estimating the Effect on Student Achievement

The distribution of books and reading instruction to students was determined by the Michigan Department of Education. Teachers in early childhood programs and elementary school classrooms were able to apply for the program. Even though classrooms selected into the application process, the students did not select into the program. Culture of Reading awards were given to 23 kindergarten classrooms, 23 first grade classrooms, 21 second grade classrooms, 24 third grade classrooms, and 24

Table IV.1
Descriptive Statistics by Grade

	Treatment	Control	Difference
Pre-Kindergarten			
Low-Income	.789	.608	.178
Male	.519	.582	-.063
English Learner	.039	.065	-.026
At-Risk Program	.615	.530	.085
African-American/Black	.435	.178	.257
White	.351	.660	-.309
Hispanic/Latino	.134	.088	.046
School Locale - City	.367	.222	.145
School Locale - Rural	.059	.223	-.164
Kindergarten			
Low-Income	.633	.524	.109
Male	.519	.517	.002
English Learner	.042	.089	-.047
At-Risk Program	.514	.472	.042
African-American/Black	.163	.158	.006
White	.731	.679	.052
Hispanic/Latino	.054	.081	-.027
School Locale - City	.165	.228	-.063
School Locale - Rural	.391	.208	.183
Grade 1			
Low-Income	.653	.514	.139
Male	.511	.511	.000
English Learner	.139	.097	.042
At-Risk Program	.475	.432	.043
African-American/Black	.163	.166	-.008
White	.708	.670	.038
Hispanic/Latino	.070	.082	-.012
School Locale - City	.279	.241	.038
School Locale - Rural	.280	.201	.079
Grade 2			
Low-Income	.723	.508	.215
Male	.503	.509	.006
English Learner	.161	.090	.071
At-Risk Program	.560	.418	.142
African-American/Black	.284	.159	.125
White	.529	.678	-.149
Hispanic/Latino	.135	.082	.053
School Locale - City	.540	.233	.307
School Locale - Rural	.335	.204	.131

early childhood programs.[5]

Fundamentally, the design compares a group of treated students (i.e., those who received books) to a group of control students (i.e., those who did not receive books). Thus, I measure the effect of treatment within cohorts. That is, I identify the treatment effect by comparing second-graders to second-graders, first-graders to first-graders, and etc. As all students in the same grade in the same school were treated (or not treated) by the program, I compare students within the same grade across different schools. Across these cohorts of students, I consider the potential heterogeneity in performance gains that may relate to how far ahead of the third-grade ELA test students were when they received books.

The program was short-lived, and treatment only occurred in 2014. I restrict the sample to students who did not switch schools in 2014 to avoid mislabeling students as treated when they were not and vice versa because the timing of treatment is not known more precisely than the fall of 2014. For this reason, I also do not estimate treatment effects for students who were in third-grade in 2014-2015.

The outcome variable of interest is third-grade ELA standardized test scores. I control for school-level characteristics and student demographic characteristics to ensure the comparison between the treated and untreated group is confounded with as little outside variation as possible. Specifically, to examine whether the Culture of Reading Program led to significant improvements in third-grade test scores, I estimate for each grade level in 2014 (the year of the program):

$$T_{ics} = \alpha + \beta 1(Treated_{ics}) + X_i\gamma + K_s\delta + \varepsilon_{ics}$$

where T_{ics} is student i's test score, having been in class c in school s in 2014. Recalling that treatment fell at the classroom level within schools, $1(Treated_{ics})$ equals one if

[5] See https://www.michigan.gov/mde/0,4615,7-140-37818_34785-342225–,00.html for a list of classrooms that received books.

student i was in a classroom that received books as part of the Culture of Reading Program. In X_i I include student level demographic controls (i.e., gender, race, and indicators for whether student i qualified for free or reduced-price lunch, was an English-second language learner). I also control for whether students ever switch schools between kindergarten and grade three, or repeat a grade.[6] In K_{sy} I include school-level controls (i.e., enrollment, the percent of students who are white, Black or Hispanic/Latino, the percent who receive free or reduced-price lunch, and the percent of students who are English-language learners). ε_{ics} is the error term; standard errors are clustered at the school-level.

Assuming there are no unobservable differences between classrooms selected for the program and classrooms not selected that change over time, this setting will allow us to estimate the causal impact of the Culture of Reading Program on third-grade test scores. As teachers applied for the program and priority was given to classrooms with higher shares of low-income students, there are ways in which classrooms that received the program may differ from classrooms that did not. Among them, we can control for time-varying observable characteristics such as free/reduced priced lunch eligibility, race, or gender. While differences in unobservable characteristics cannot be controlled for, as long as those differences persist over time, the effect of the program is still well-identified.

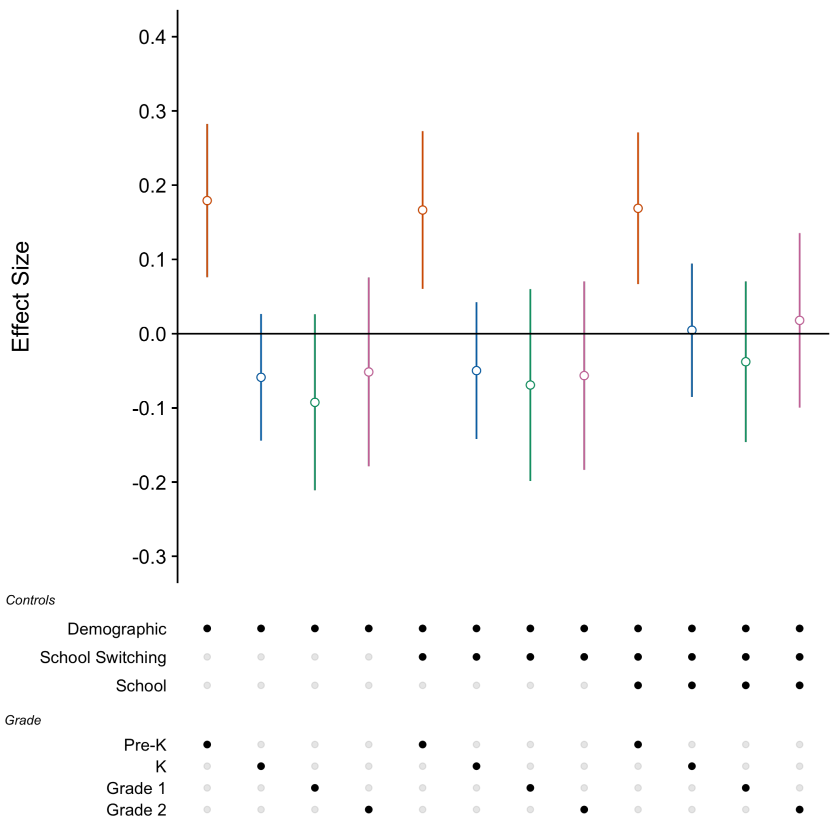
IV.3 Results

I begin with estimates for the full sample of students, regardless of their racial or socioeconomic composition. I consider two main outcome variables: third grade English-language Arts and third grade mathematics standardized test scores. The outcome variables are standardized within school year to a mean of zero and standard deviation of one.

[6] I do not include a switch from Pre-K to K as a switch because very few elementary schools include an early childhood program.

In Figure IV.1, I plot the effect sizes of treatment on ELA test scores for each grade. The last four estimate bars represent the model with the full set of demographic and school controls. I find no evidence that having access to the Culture of Reading Program had significant effects on third-grade test scores for students who were in kindergarten, first, or second grade in 2014-2015, as these effects are estimated as a null effect. For those students enrolled in Pre-K in 2014-2015 and took the third-grade standardized test in 2018-19, I find an 0.17 standard deviation ($p = 0.001$) increase in ELA achievement associated with receiving the book.[7]

Figure IV.1
Effect of Treatment on Third Grade ELA Scores

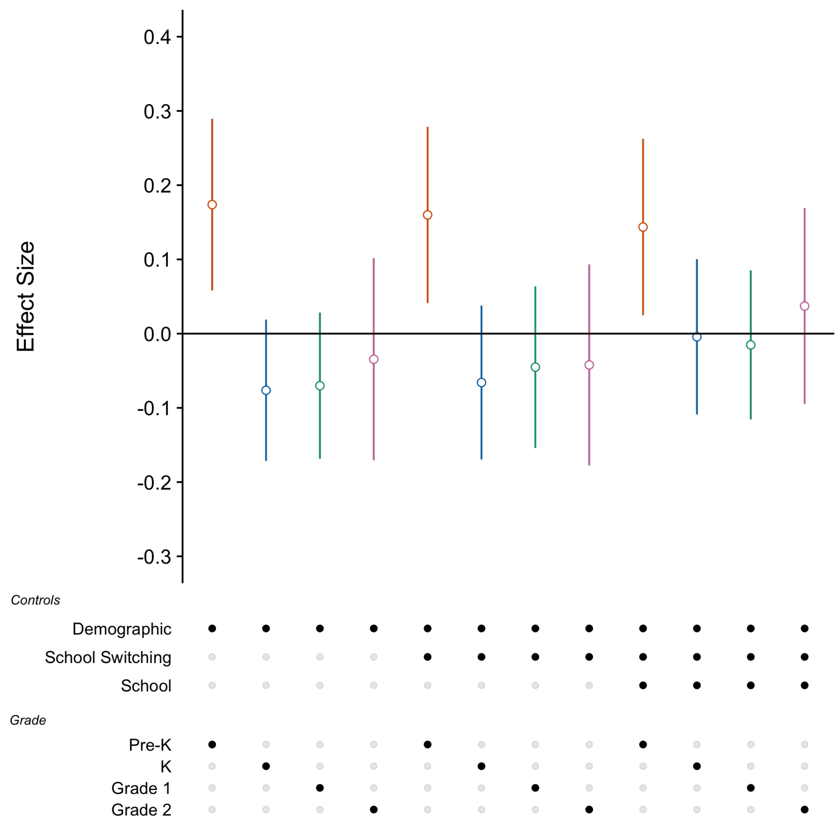


Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression. Standard errors are clustered at the school-level. There are 9,292 Pre-K observations, 95,849 K observations, 90,702 Grade 1 observations, and 95,745 Grade 2 observations.

[7]Results from regressions using school-level fixed effects are provided in Figures A.22 and A.23.

As higher ELA achievement is known to correlate with higher mathematics achievement (Martin and Mullis, 2013; Shin et al., 2013; Thurber et al., 2002), I also consider the potential for mathematics scores to vary systematically with treatment. In Figure IV.2, I plot the effect sizes of treatment on mathematics scores for each grade. Similar to ELA achievement, having access to the Culture of Reading Program has a null effect for students who were in kindergarten, first grade, or second grade in 2014-2015. For students who were enrolled in Pre-K in 2014-15, I find a significant and positive effect (0.14σ, $p = 0.018$).

Figure IV.2
Effect of Treatment on Third Grade Mathematics Scores

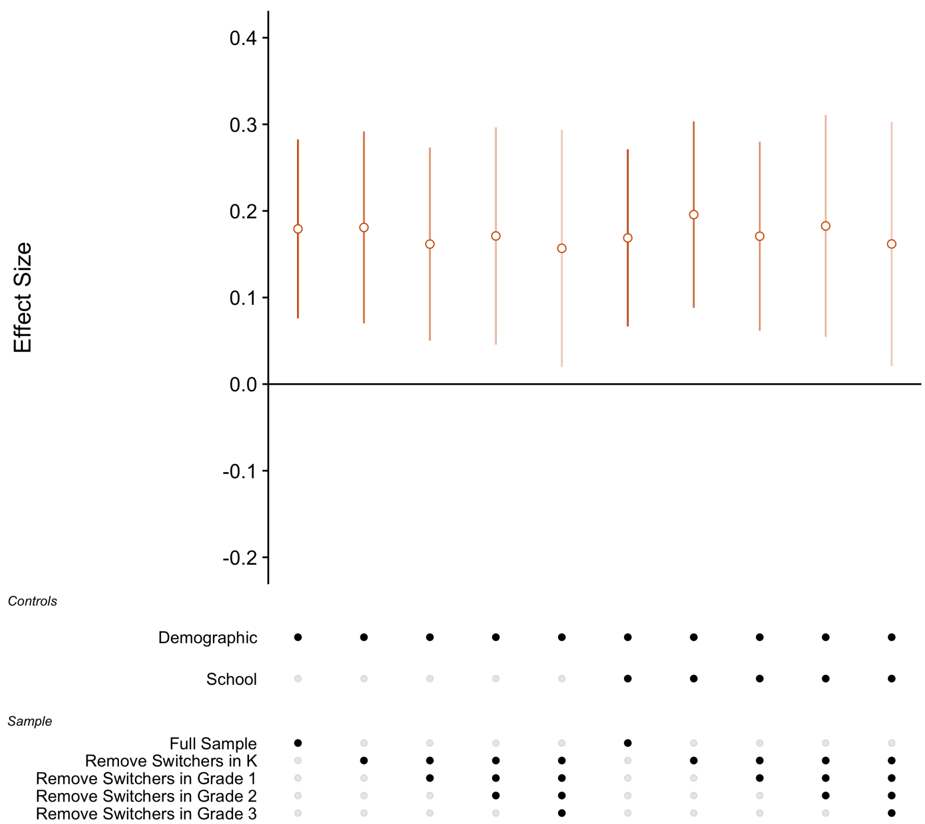


Note: Each bar represents the average effect of the Culture of Reading program on third-grade mathematics test scores. Each effect size is estimated from a linear regression. Standard errors are clustered at the school-level. There are 9,292 Pre-K observations, 95,849 K observations, 90,702 Grade 1 observations, and 95,745 Grade 2 observations.

IV.3.1 Pre-K

In Figure IV.3, I restrict the analysis to Pre-K students in 2014-2015 who took the standardized test in 2018-2019. I systematically drop the students who switched in each grade. The effect size is robust to dropping students who switch schools. In Figure IV.4, I repeat this procedure with mathematics scores and I also find that the effect size is robust to excluding students who switch schools. These findings indicate that the result is not being driven by students who switch schools.

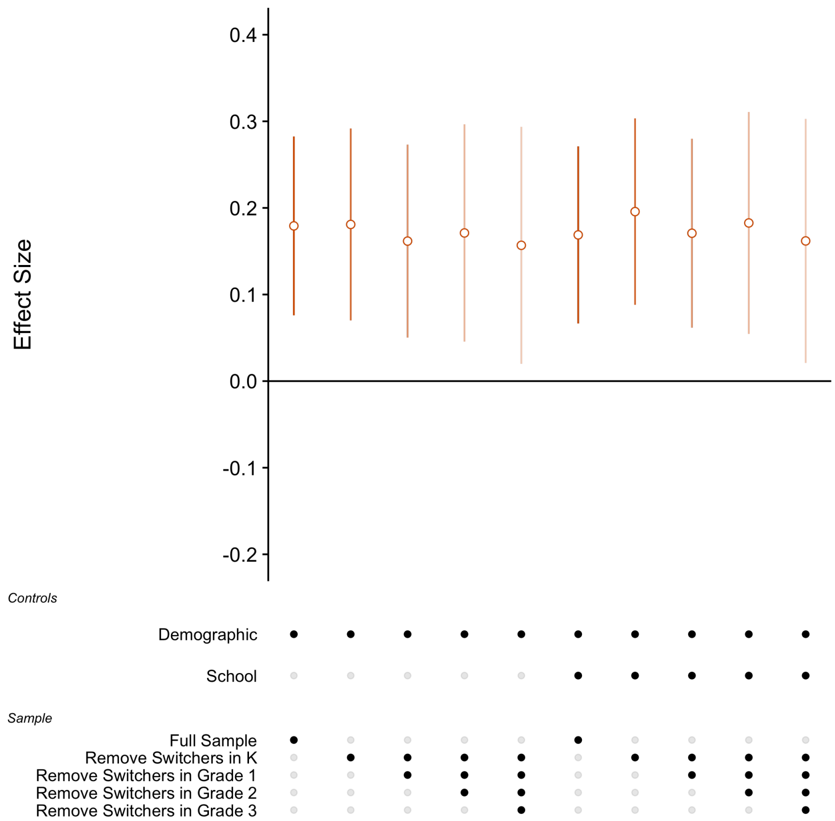
Figure IV.3
Effect of Treatment on Third Grade ELA Scores for Pre-K Students



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression. Standard errors are clustered at the school-level.

One concern is the selection out of public Pre-K options and into private Pre-K. The demographic characteristics of Pre-K students enrolled in Michigan public schools

Figure IV.4
Effect of Treatment on Third Grade Mathematics Scores for Pre-K Students



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression. Standard errors are clustered at the school-level.

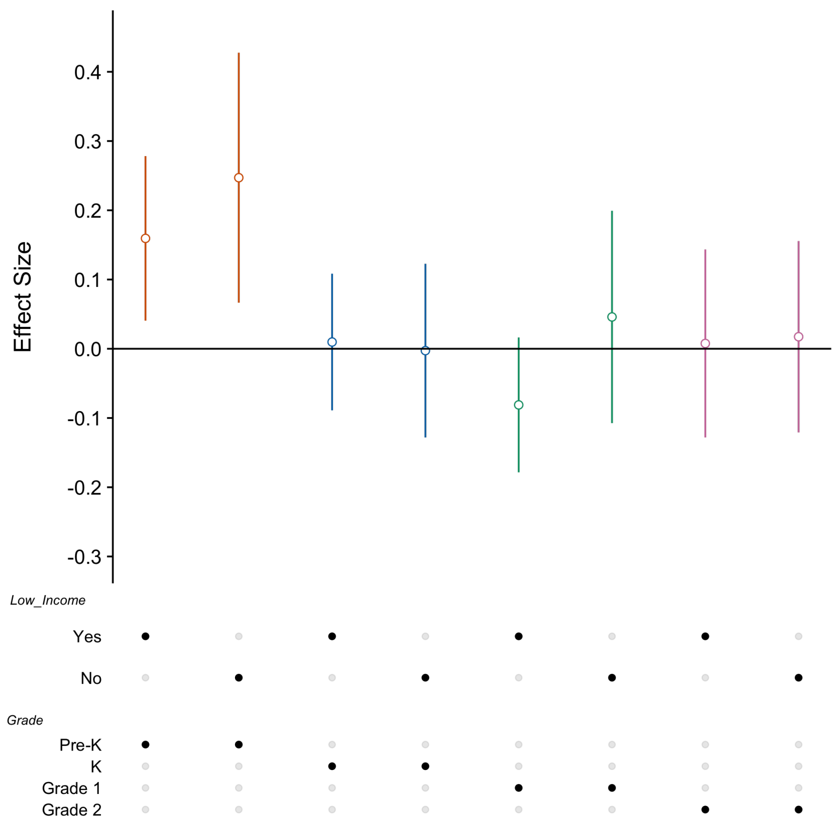
in 2014 is similar to the demographic characteristics of kindergarten through third-grade students enrolled in public schools in 2014. Therefore, based on observable characterstics of students, there is not sufficient evidence of large seleciton out of public Pre-K in favor of private options. However, the treatment group for Pre-K consists of more low-income and more African-American students than the rest of the sample. Because income and test scores are positively correlated, the Pre-K coefficient estimates represent a lower-bound on the treatment effect.

IV.3.2 Heterogeneity

The Culture of Reading program gives a free book, family reading instructions, and access to evidence-based reading instruction to all eligible students, regardless of family income. However, one might expect students from low-income families to benefit greater from participation in the program, as these families may have less access to books and other reading programs due to financial constraints. To estimate the effects across income status, I estimate the effect of receiving access to the program on ELA achievement scores for students who are eligible for free and reduced priced lunch and for students who are not, for each grade in the sample. Figure IV.5 presents the results on third grade test scores. I find null effects for students who were in kindergarten, first grade, or second grade at the time of treatment. For Pre-K students, I find a large, positive effects for students in both income groups. The average treatment effect for free/reduced lunch students is 0.16 standard deviations ($p = 0.009$). For Pre-K students who are not eligible for free/reduced price lunch, I find an effect size of 0.24 standard deviations ($p = 0.007$).

Next, I assess how the Culture of Reading program affects students of different races or ethnicities. I find that the positive effect of treatment on ELA achievement in Pre-K is largely driven by the effect of the program on African-American/Black students. The effect of the program on ELA achievement is 0.23 standard deviations

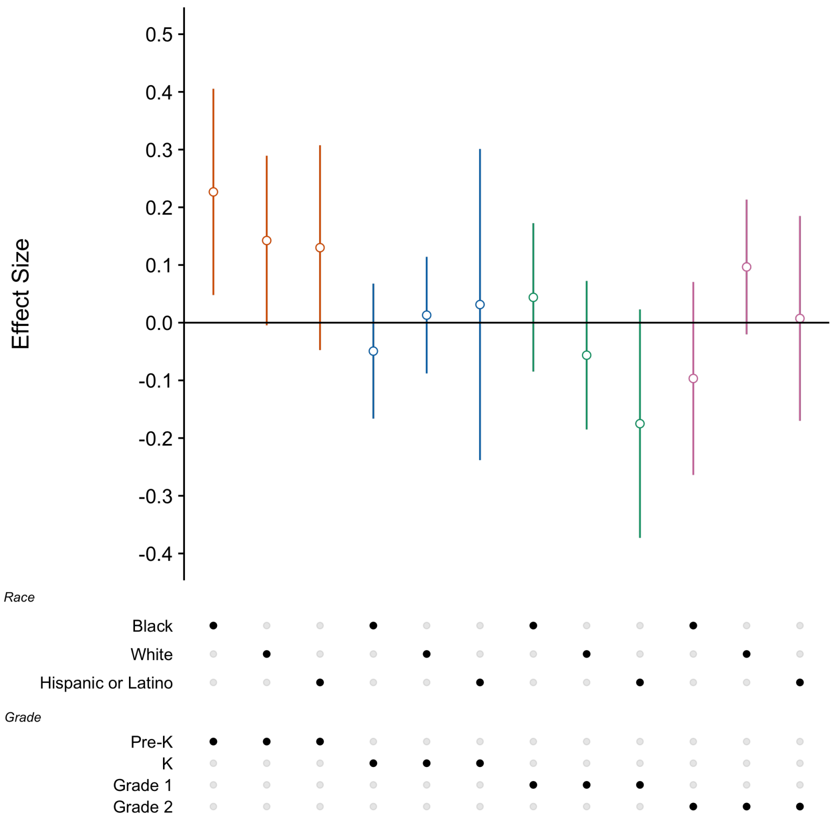
Figure IV.5
Effect of Treatment on Third Grade ELA Scores by Income Status



Note: Income status refers to whether or not the student qualifies for free/reduced priced lunch. Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression with controls for student demographics and school characteristics. Standard errors are clustered at the school-level.

for Black students in Pre-K in 2014. I find a marginally significant positive effect for white Pre-K students (0.14σ, p = .058) and null effect for Hispanic Pre-K students. I find null effects for all races/ethnicities in kindergarteners, first graders, and second graders.

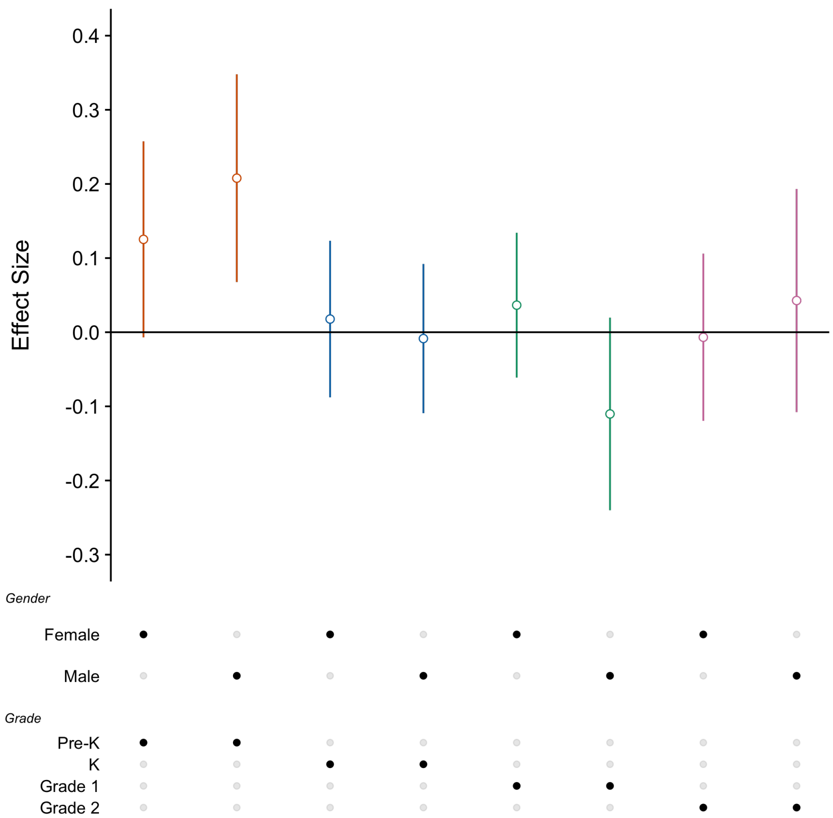
Figure IV.6
Effect of Treatment on Third Grade ELA Scores by Race/Ethnicity



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression with controls for student demographics and school characteristics. Standard errors are clustered at the school-level.

I then explore heterogenous treatment effects across gender. Figure IV.7 presents the results. Similar to the previous analyses, I find null effects for kindergarteners, first graders and second graders. I find that male students in Pre-K benefit more from the Culture of Reading program than female students in Pre-K. I estimate an effect size of 0.22 standard deviations (p = 0.002)for males and 0.12 standard devations (p = 0.082) for females.

Figure IV.7
Effect of Treatment on Third Grade ELA Scores by Gender



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression with controls for student demographics and school characteristics. Standard errors are clustered at the school-level.

IV.3.3 Matching

Students who had access to books and additional reading instructions through the Culture of Reading Program are more likely to be low-income, more likely to be African-American/Black, and are more likely to be in an at-risk program. As these are observable attributes, in the analysis above I control for any systematic variation in outcomes by these student characteristics. However, due to the imbalance in treatment and control groups, one can imagine that other unobservable attributes also differ across treatment and control groups. In the absence of randomization, effects can be estimated by comparing two individuals who have the same attributes, one who is treated and one who is not.

One method of matching involves pairing a treated student to an untreated student using all observable characterstics available. However, as the number of covariates gets large, it becomes increasingly more difficult to find an exact match, leading to a decreased sample size and increased susceptibility to bias. When exact matching on all characteristics is not possible, we can use propensity score matching to identify the treatment effect. This method matches treated and untreated observations using the conditional probability of being treated. In this case, the propensity score is the conditional probability of receiving access to the Culture of Reading Program. Propensity score matching will allow me to identify the effect of having received access to the Culture of Reading Program on third-grade standardized test scores.

I calculate propensity scores using logistic regression and then match propensity scores using nearest-neighbor matching. Nearest-neighbor matching matches one treated student to one untreated student with a similar propensity scores.[8] In other words, I match one 2nd grade student who received access to the Culture of Reading Program to one 2nd grade who has a similar propensity score but did not receive access to the program, for example. One consequence of this method is that my sample size

[8] I use the MatchIt R package (Ho et al., 2018) to perform the propensity score matching.

decreases significantly because one treated student must be matched to one untreated student with a similar propensity score. The final matched sample includes 874 Pre-K students, 1,806 kindergartners, 3,178 first-graders, and 1,832 second-graders.

Using the nearest-neighbor matching approach, I have pairs of students who have similar propensities to have received access to the Culture of Reading Program in 2014, one treated and one untreated. This method allows me to estimate the effect of having received access to the program on third-grade ELA scores. Results from matching are given in Table IV.2. I find that for Pre-K students, the effect of being eligible for the Culture of Reading program increases third-grade ELA test scores by 0.196 standard deviations. Propensity score matching yields null results for kindergarteners and second graders. I find a negative estimate of -0.086 standard deviations for first graders.

Table IV.2
Propensity Score Matching Estimation Results

	Normalized ELA Score			
	Pre-K	K	Grade 1	Grade 2
	(1)	(2)	(3)	(4)
Effect Size	0.194***	−0.050	−0.063**	−0.017
	(0.059)	(0.040)	(0.030)	(0.039)
Observations	874	1,806	3,178	1,832
Note:		*p<0.1; **p<0.05; ***p<0.01		

However, King and Nielsen (2019) show that propensity score matching attempts to approximate a completely randomized experiment, and in doing so increases imbalance, inefficiency, and bias. Therefore, I also perform coarsened exact matching to estimate the treatment effect (Iacus et al., 2012). Coarsened exact matching approximates a fully blocked randomized design, leading to lower imbalance and lower bias than traditional propensity score matching. This method also retains many more observations than 1:1 nearest neighbor propensity score matching.

Using coarsened exact matching, I find an effect on third-grade ELA test scores of 0.214 standard deviations ($p < 0.001$) for students who received access to the Culture of Reading while enrolled in Pre-K. I estimate null effects on test scores for students who received access to the program in kindergarten, first grade, or second grade. Estimation results are reported in Table IV.3.

Table IV.3
Coarsened Exact Matching Estimation Results

	Normalized ELA Score			
	Pre-K	K	Grade 1	Grade 2
	(1)	(2)	(3)	(4)
Effect Size	0.214***	−0.056	−0.031	0.024
	(0.051)	(0.039)	(0.041)	(0.131)
Observations	7,711	88,857	85,095	71,146
Note:		*p<0.1; **p<0.05; ***p<0.01		

IV.4 Conclusion

In this paper, I provide preliminary evidence of the effects of a multifacted reading program on third-grade achievement. My findings suggest that receiving access to a storybook, family reading activities, and evidence-based reading instruction in Pre-K has significant, positive effects on both ELA and mathematics achievement.

If the identifying assumptions hold, receving access to this program increased third-grade ELA achievement by 0.17 standard deviations, on average, for students who were treated in Pre-K. Compared to other studies on reading interventions, this effect size is larger. In a meta analysis, Cooper et al. (2000) finds a 0.14 standard deviation increase in reading achievement for students who participated in classroom-based summer reading programs. Kim (2007) finds an average effect of 0.08 standard deviations for fourth-grade students. In terms of pure capital-investments, Holden (2016) estimates a 0.15 standard deviation increase in achievement at the school-level, or 0.07 student-level standard deviations.

Overall, we are unable to conclude strongly that these increases in third grade achievement are due to the Culture of Reading Program. Further data and analysis is needed. In particular, restricting the sample to students who ever attended Pre-K would help to determine if the effects estimated here are due to students attending Pre-K or due to the reading program. There is also more we can learn about characteristics of selected classrooms in the years prior to the Culture of Reading program that could inform the results. Additionally, due to program limitations, this paper does not speak to the complementarities of the three components of the program. Understanding which features of a program–capital inputs, parental time, classroom curriculum, or a combination of the these–would be valuable to the policymakers designing and implementing early childhood interventions to promote literacy at a young age.

CHAPTER V

DISSERTATION CONCLUSION

Using applied econometric techniques, I provide insight on a number of education policies aimed at lower-income and traditionally underrepresented students.

In Chapter II, I provide early evidence on whether Louisiana's policy to require FAFSA submission prior to high school graduation translates into increased college enrollments. Using a synthetic control approach, I do not find large increases in college enrollments in the aggregate. However, I find suggestive evidence that enrollments may have increased at large, public four-year universities. This is particularly true for Black students, where my results suggest some substitution away from public two-year schools towards public four-year schools. Overall, this work shows that it is still too early to declare Louisiana's policy a success and that more efforts beyond mandatory FAFSA may be needed to induce students on the margin of enrolling in college into attending.

In the next chapter, I evaluate the academic impact of the USDA Fresh Fruits and Vegetables Program in Illinois elementary schools. I estimate negative effects of FFVP on achievement–specifically a 0.04 to 0.09 standard deviation decrease in ELA scores and a 0.07 to 0.14 standard deviation decrease in mathematics scores at the school-level. However, these effects are not distinguishable from a null effect, thus a cautious interpretation of the results would be that FFVP does not impact school-level achievement. I also find no impacts on attendance or disciplinary actions.

Finally, in Chapter IV, I find that receiving access to a reading program that engages students, parents, and teachers in the reading process is associated with increased third grade reading achievement for students who were exposed to the program while enrolled in Pre-K. Specifically, I find a 0.17 standard deviation in third grade test scores for students who received the Culture of Reading Program in

Pre-K. I also find larger effects for Black students, male students, and students who are not eligibile for free/reduced price lunch. Overall, more work is needed to draw strong conclusions about the effects of this program. Understanding which features of a program–capital inputs, parental time, classroom curriculum, or a combination of the these–would be valuable to the policymakers designing and implementing early childhood interventions to promote literacy at a young age.

APPENDIX A

SUPPLEMENTAL FIGURES

Figure A.1
FTF Enrollment in Louisiana and Other States, by Race/Ethnicity

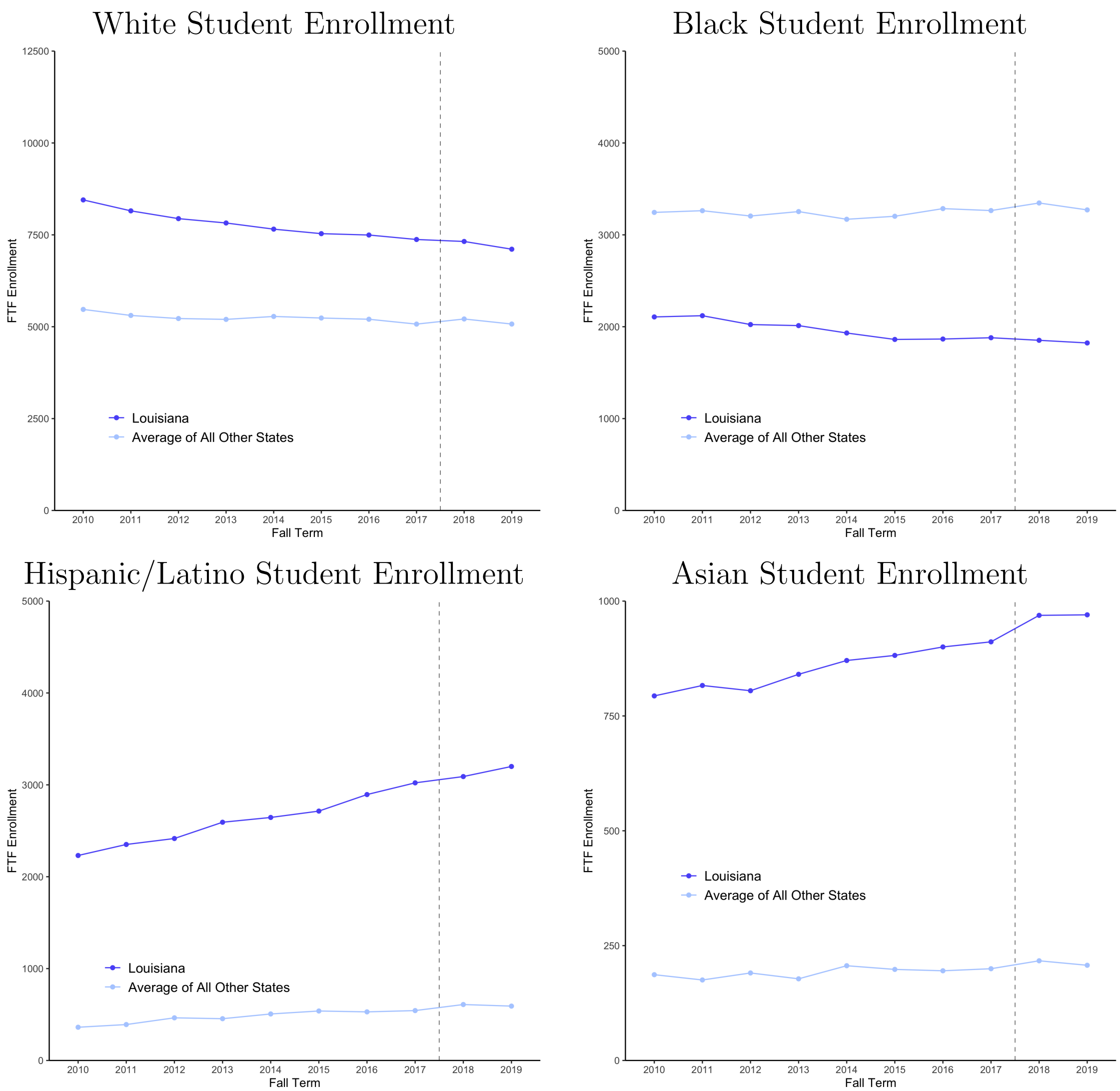


Figure A.2
Post-Secondary Enrollment in Louisiana - Excluding Neighboring States

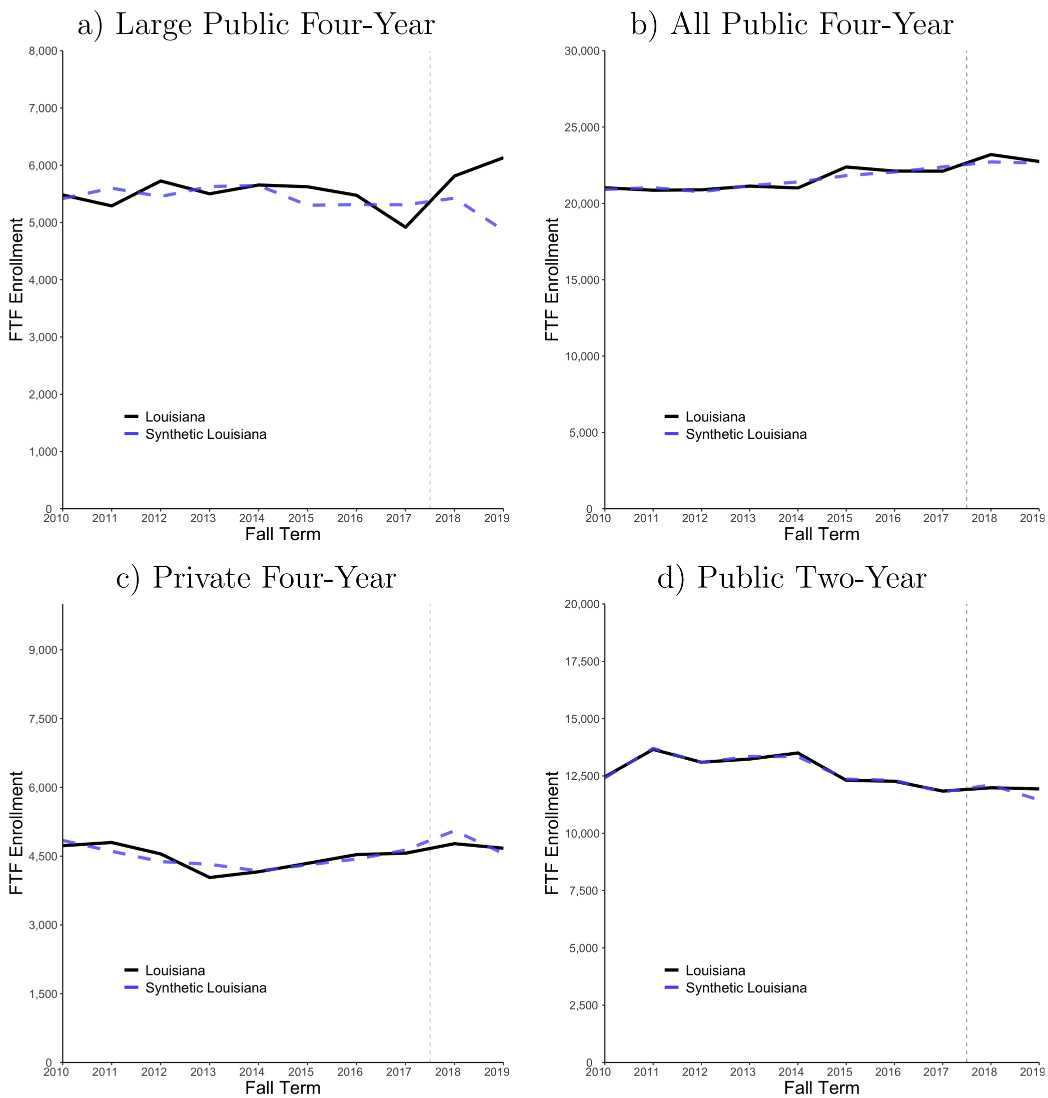


Figure A.3
Post-Secondary Enrollment in Louisiana - Excluding SEC States

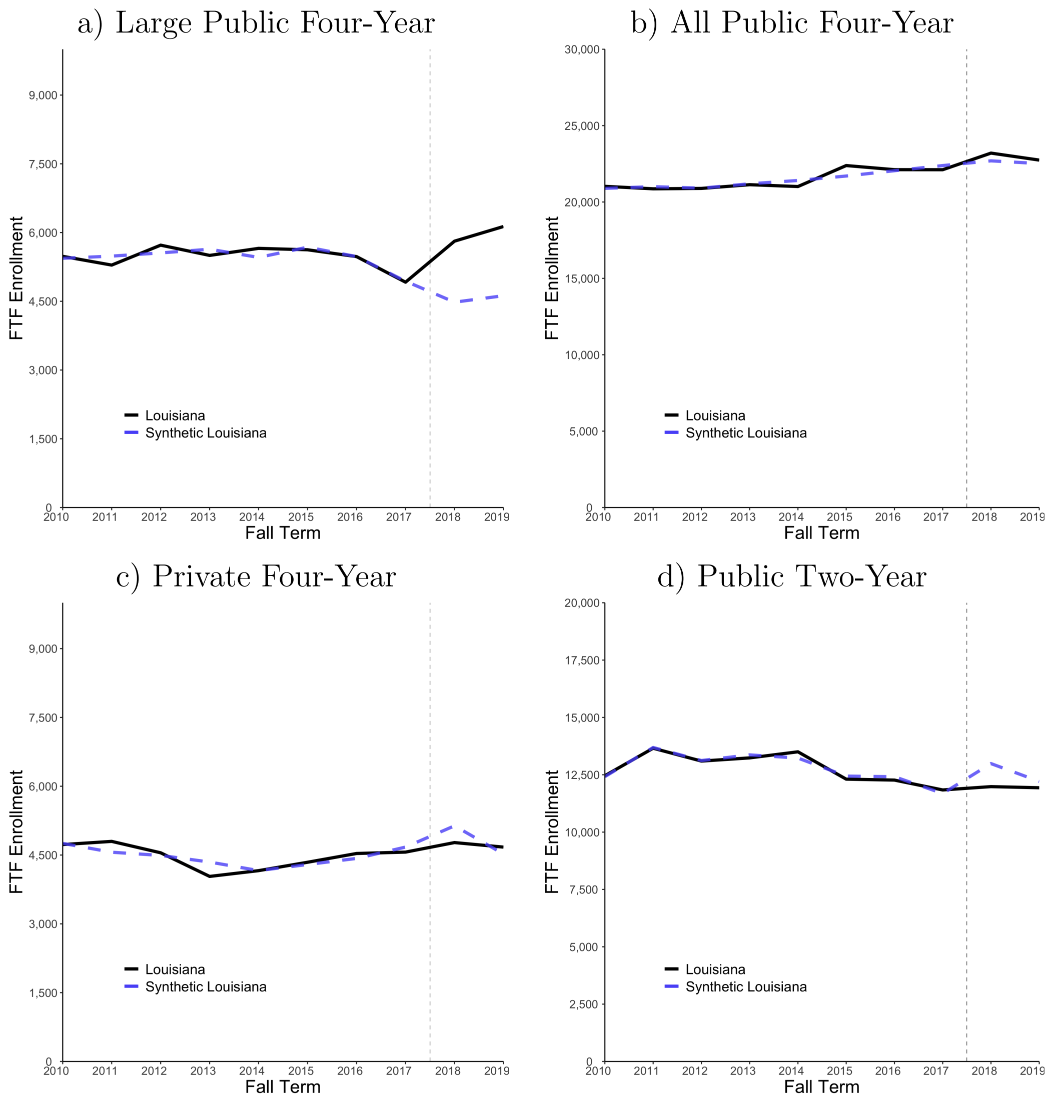


Figure A.4
White Student Enrollment in Louisiana - Excluding Neighboring States

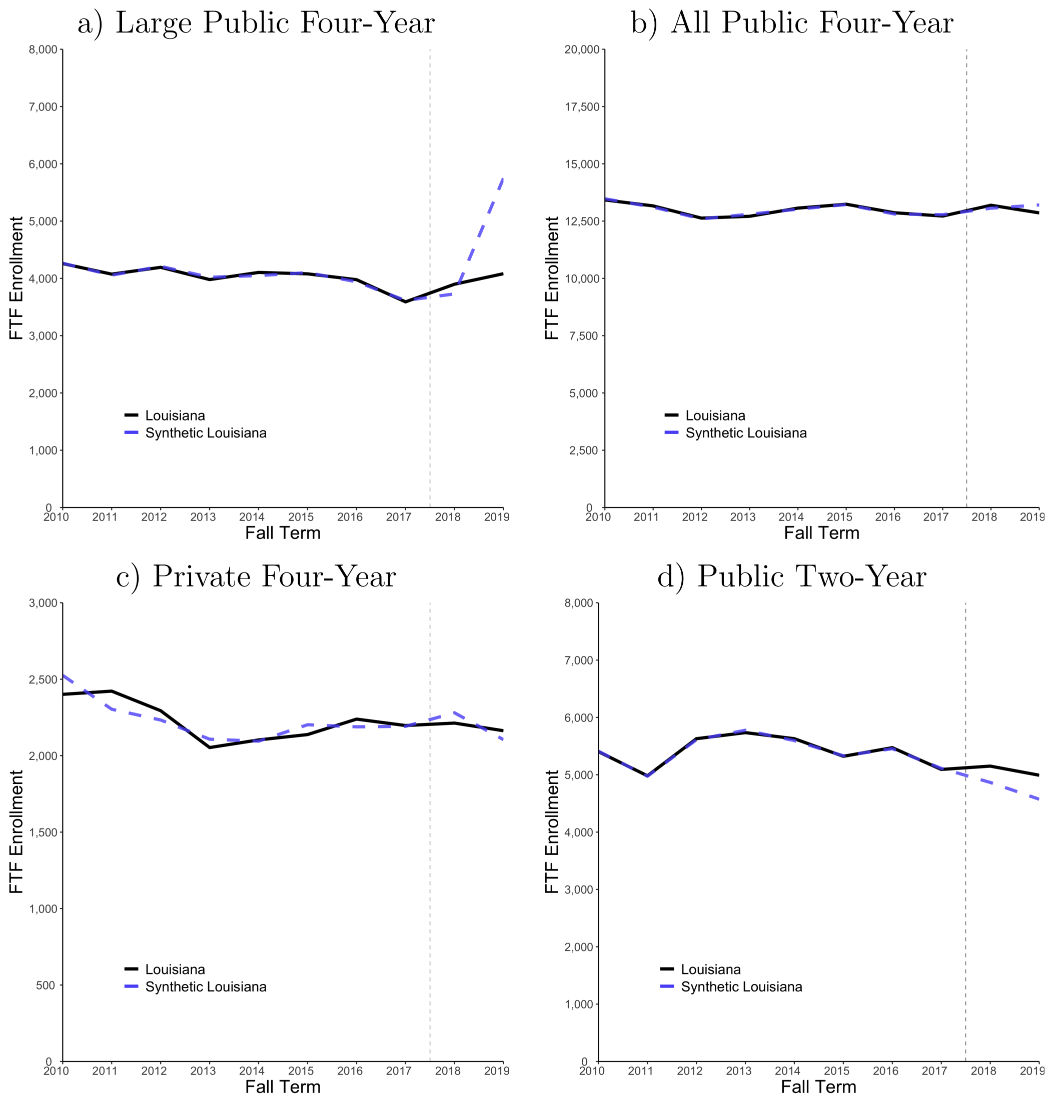


Figure A.5
White Student Enrollment in Louisiana - Excluding SEC States

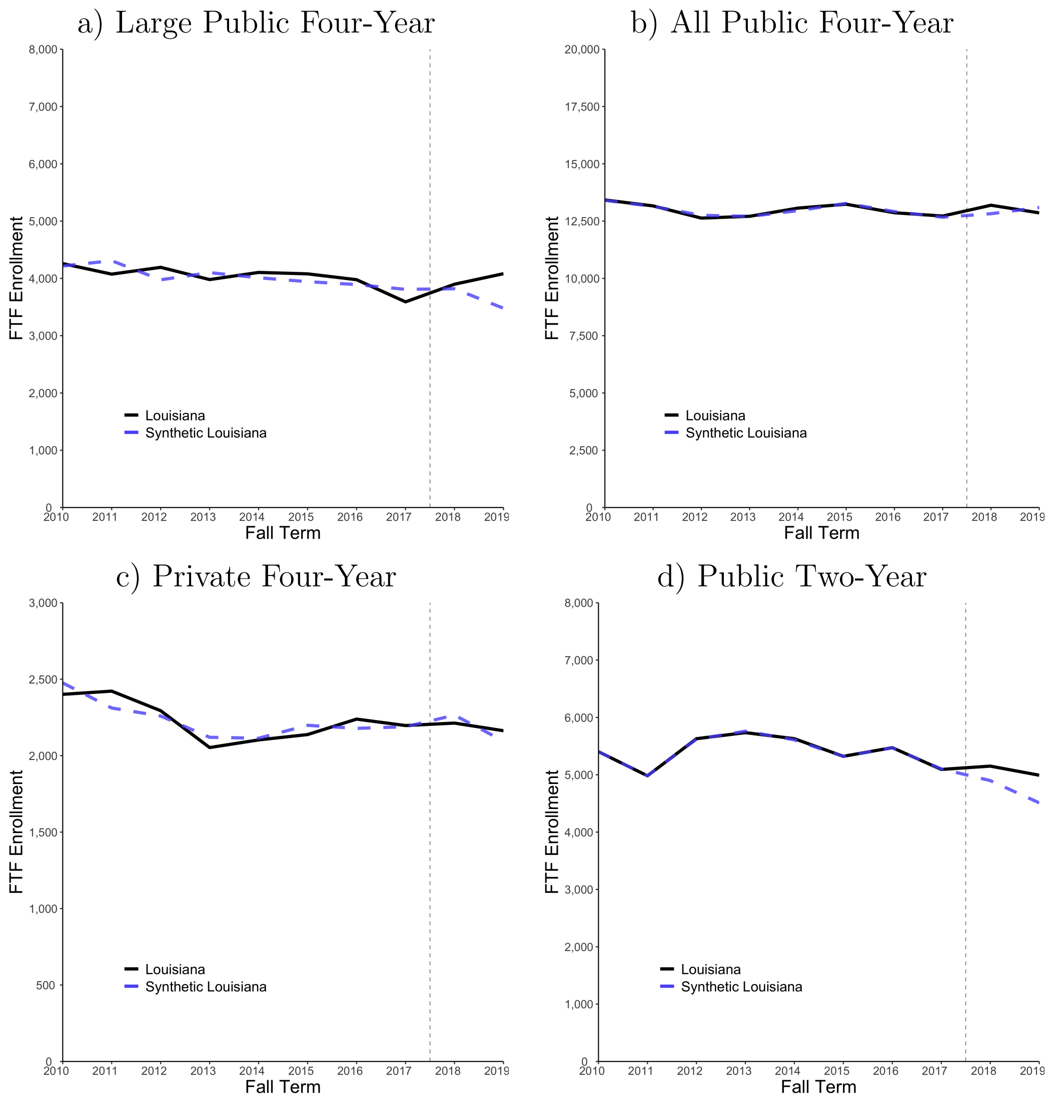


Figure A.6
Black Student Enrollment in Louisiana - Excluding Neighboring States

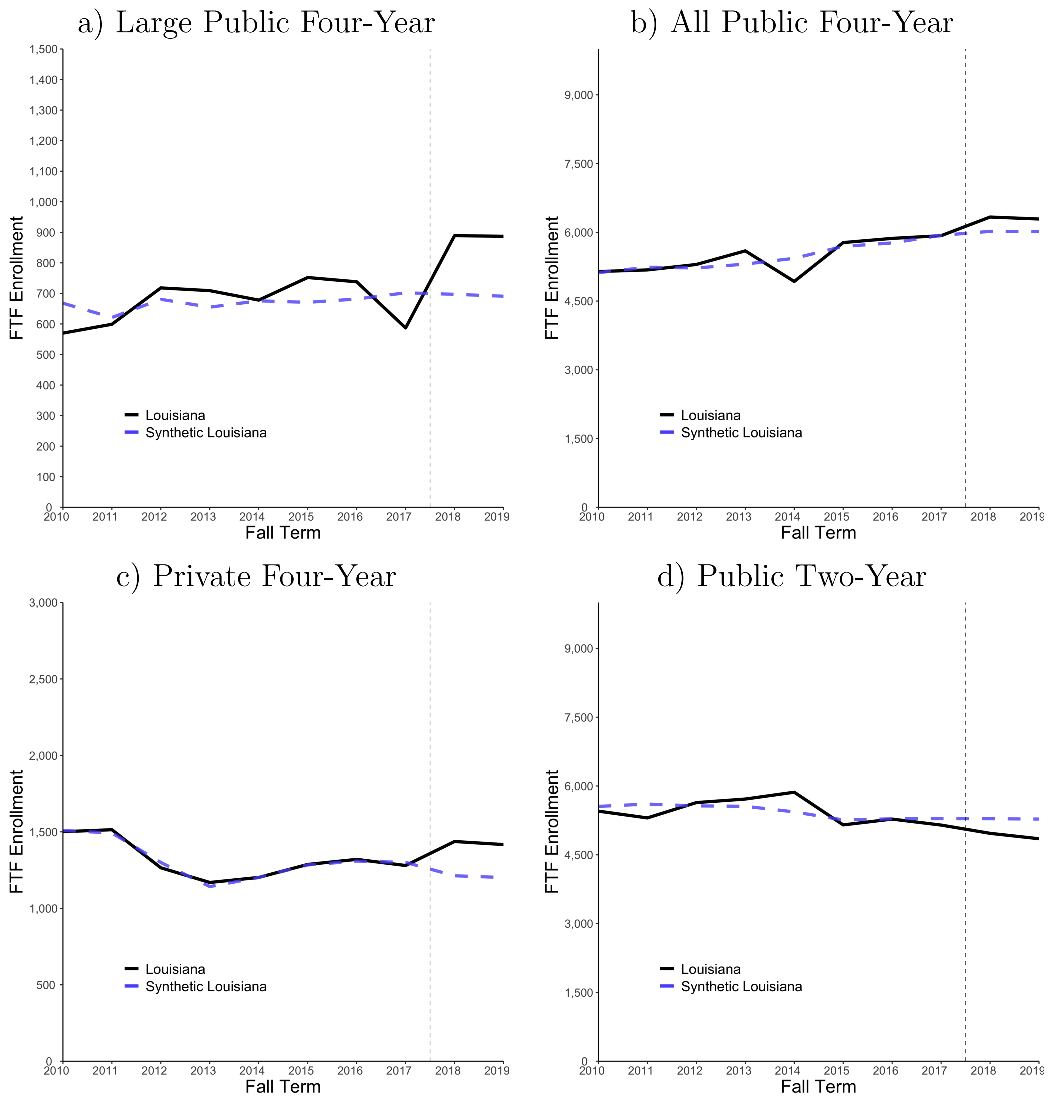


Figure A.7
Black Student Enrollment in Louisiana - Excluding SEC States

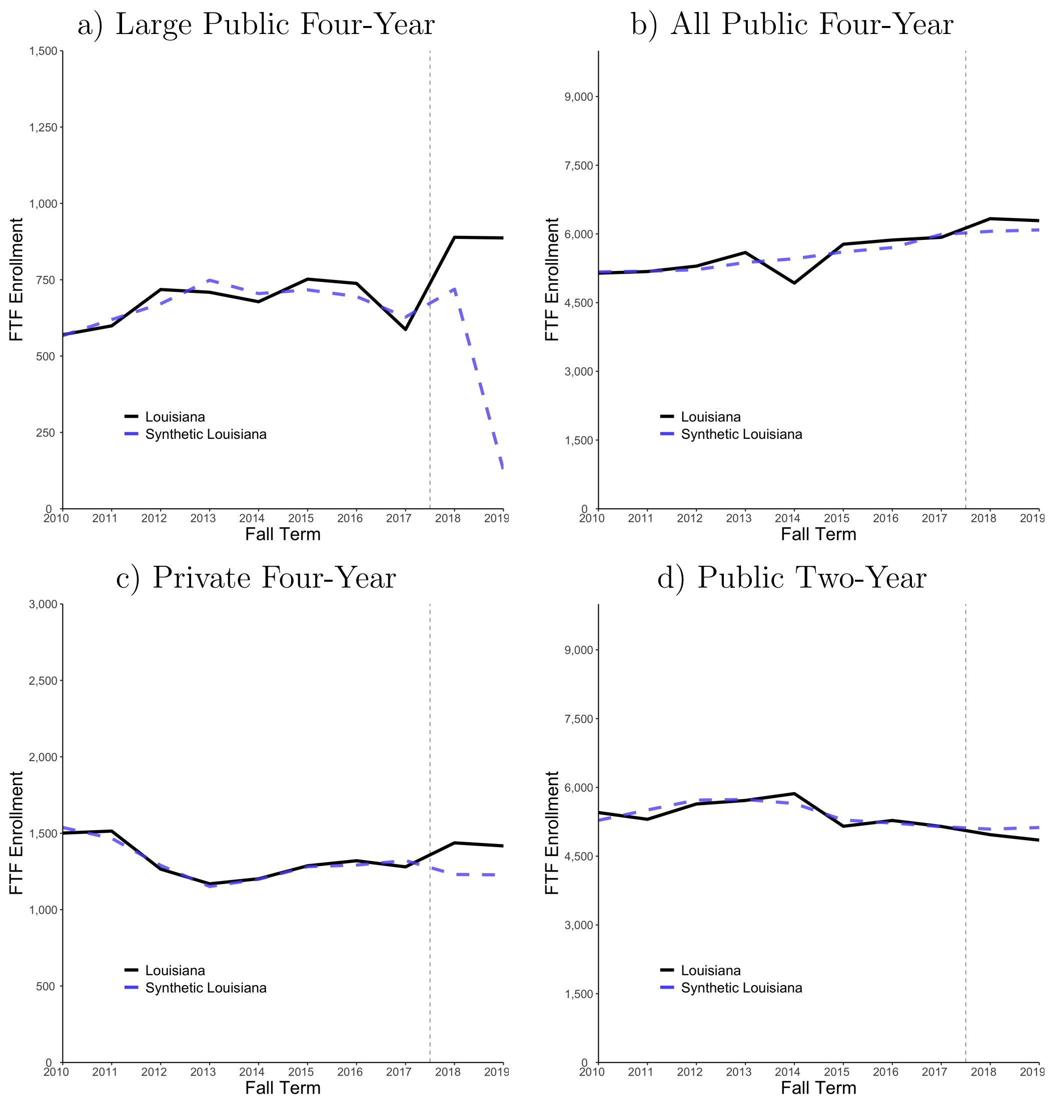


Figure A.8
Hispanic/Latino Student Enrollment in Louisiana - Excluding Neighboring States

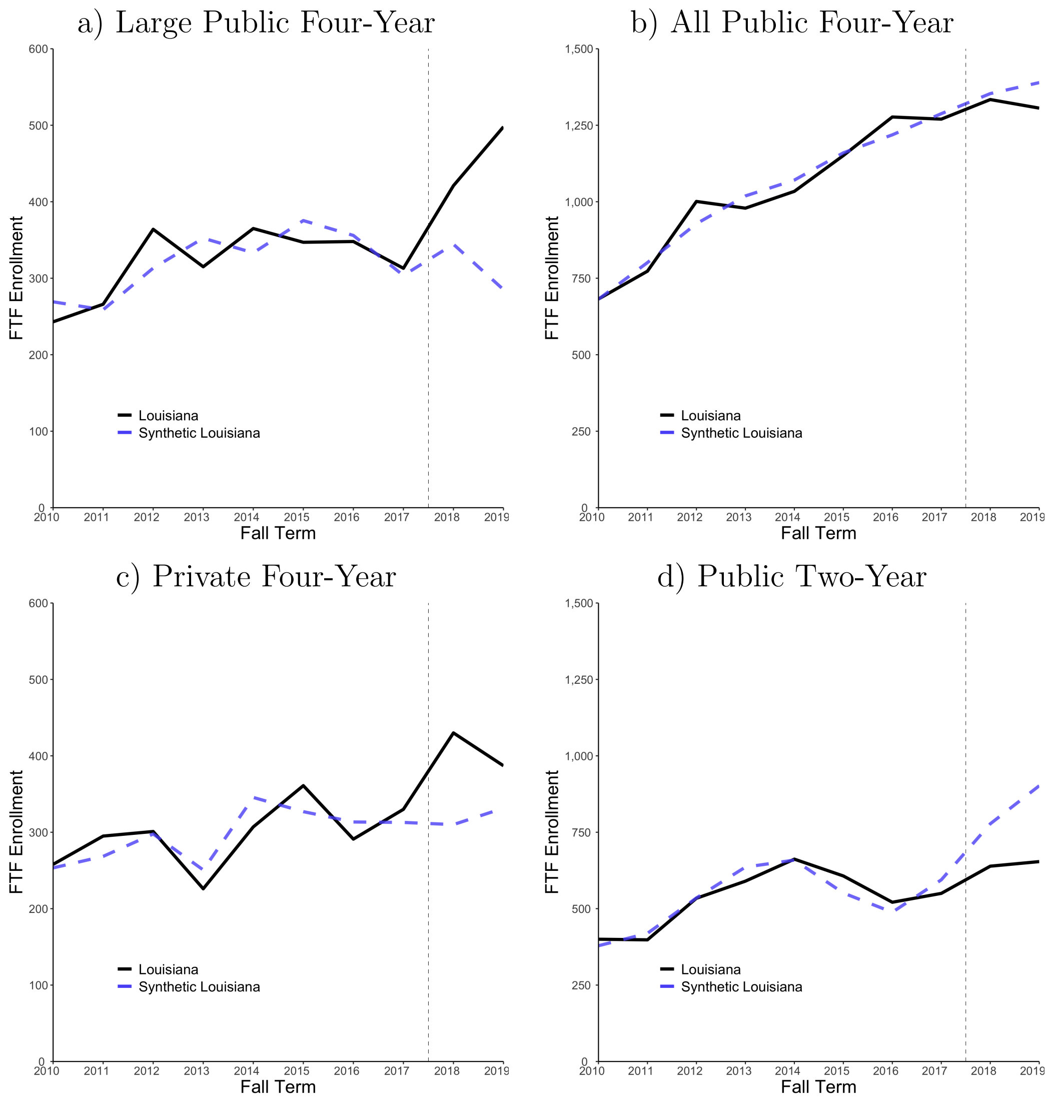


Figure A.9
Hispanic/Latino Student Enrollment in Louisiana - Excluding SEC States

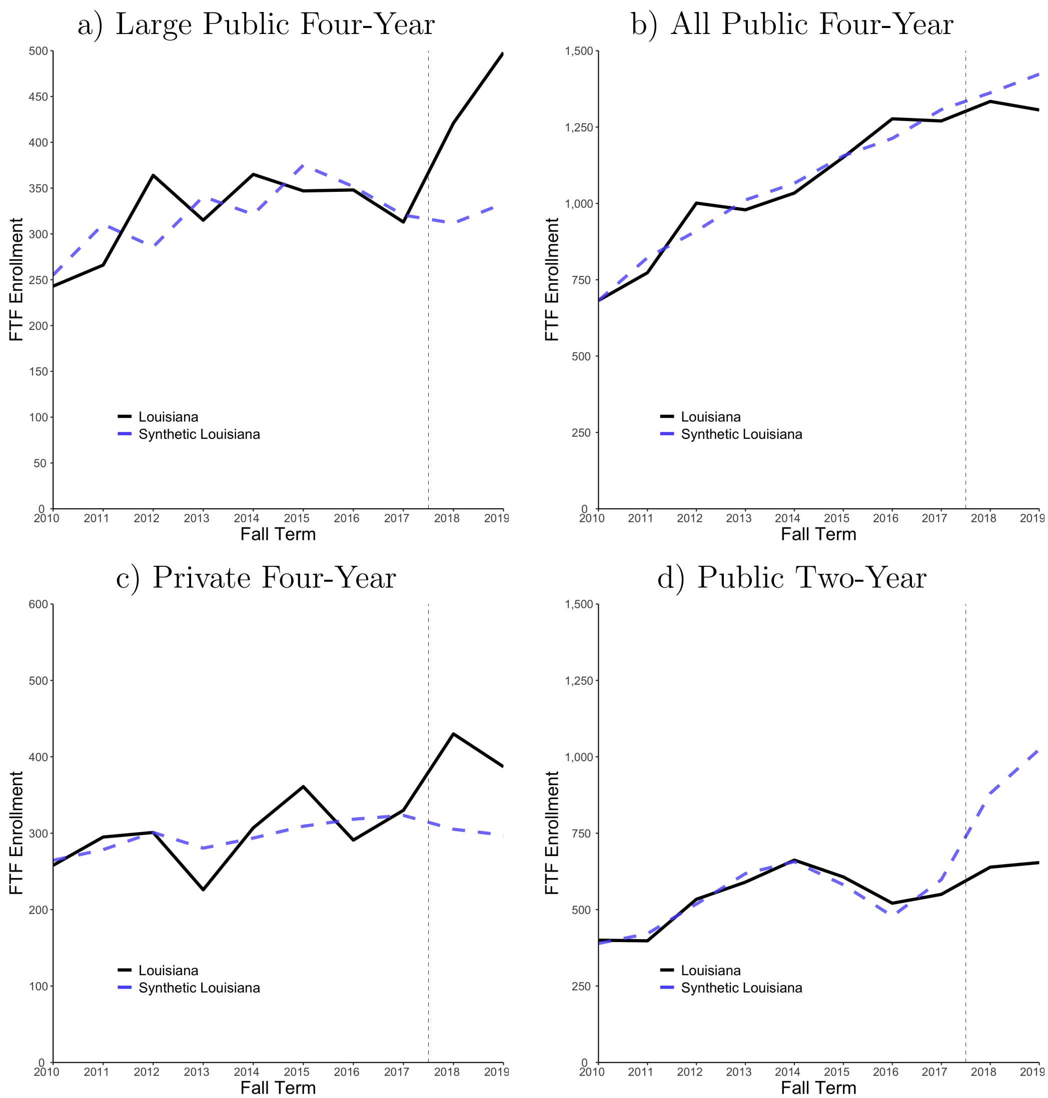


Figure A.10
Asian Student Enrollment in Louisiana - Excluding Neighboring States

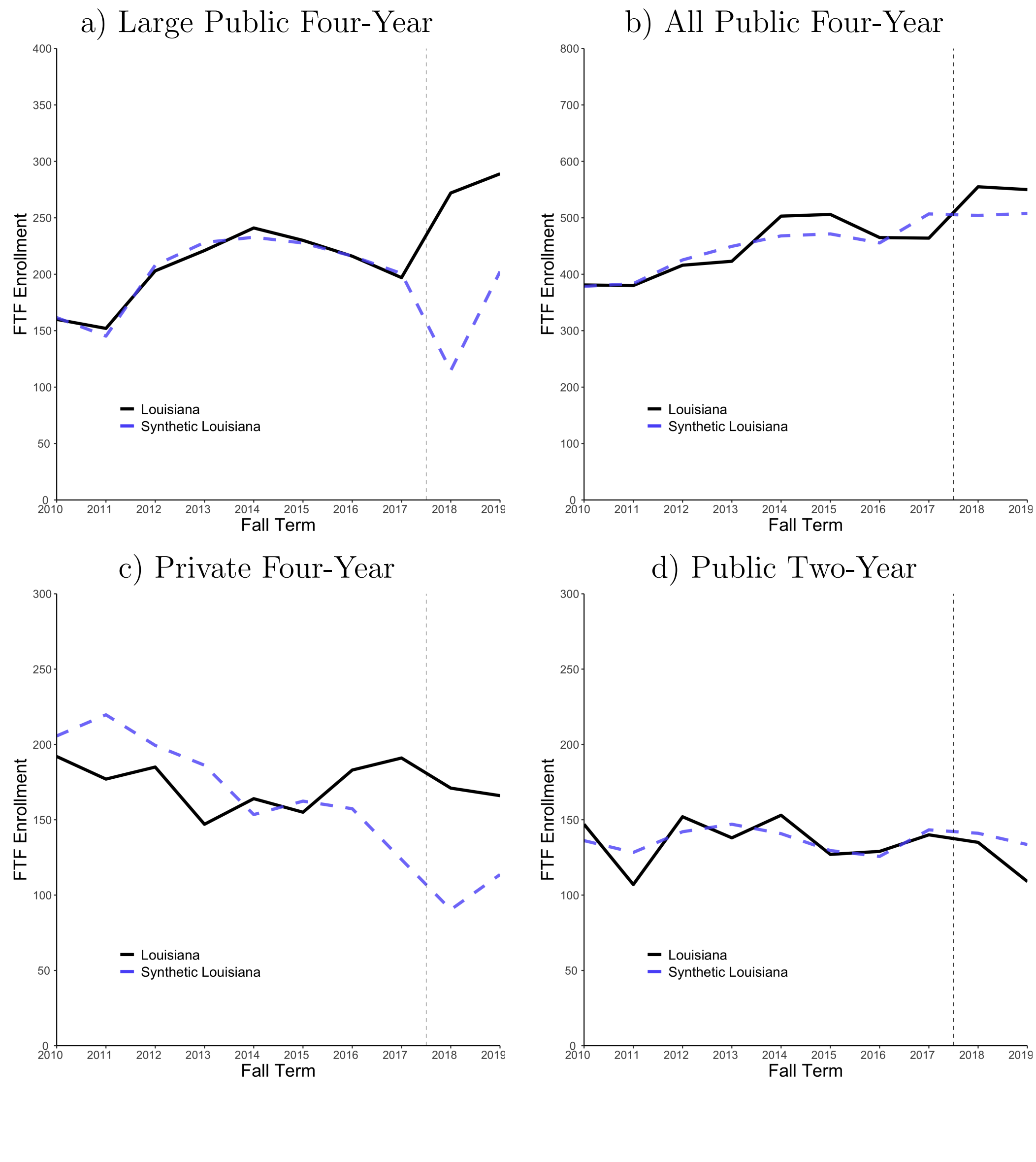


Figure A.11
Asian Student Enrollment in Louisiana - Excluding SEC States

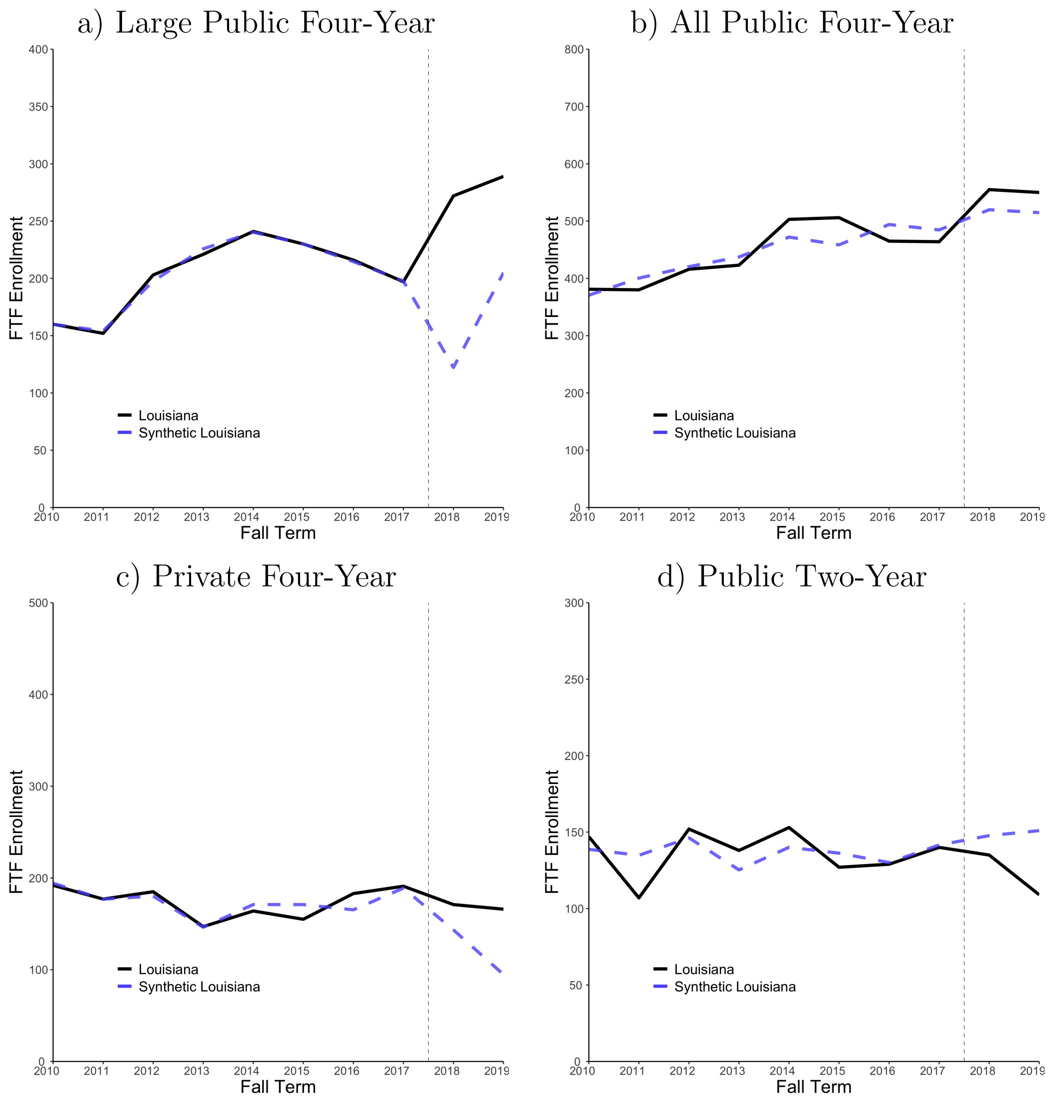


Figure A.12
Total Pell Grant Awards in Louisiana - Excluding Neighboring and SEC States

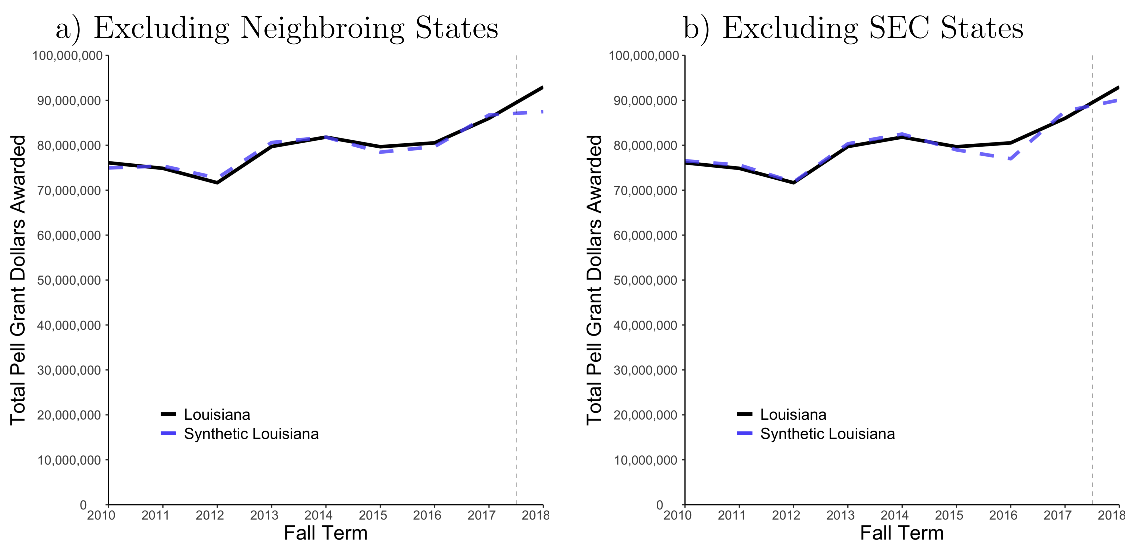


Figure A.13
Total Pell Grant Awards for Louisiana by Tuition - Excluding Neighboring States

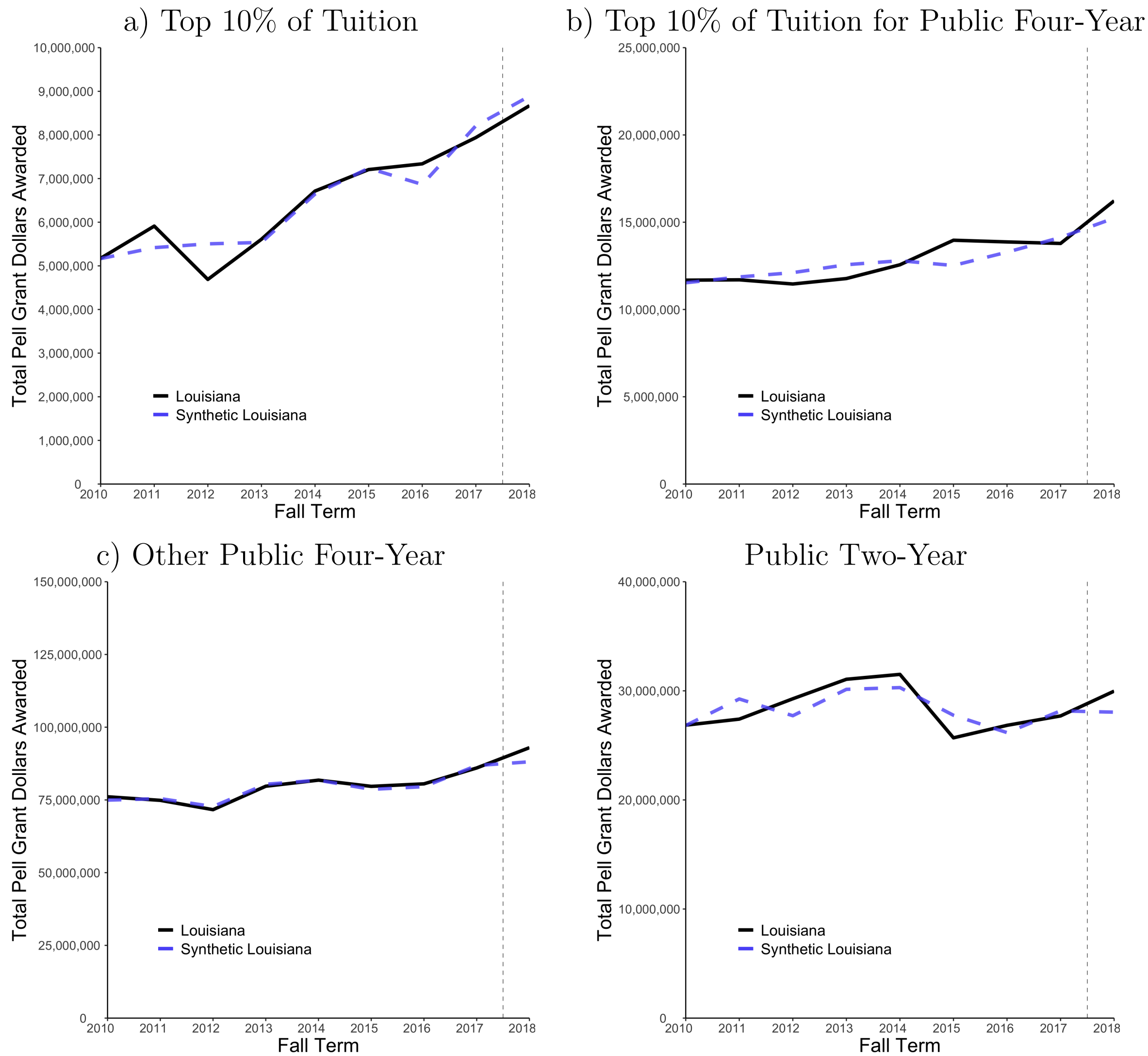


Figure A.14
Total Pell Grant Awards for Louisiana by Tuition - Excluding SEC States

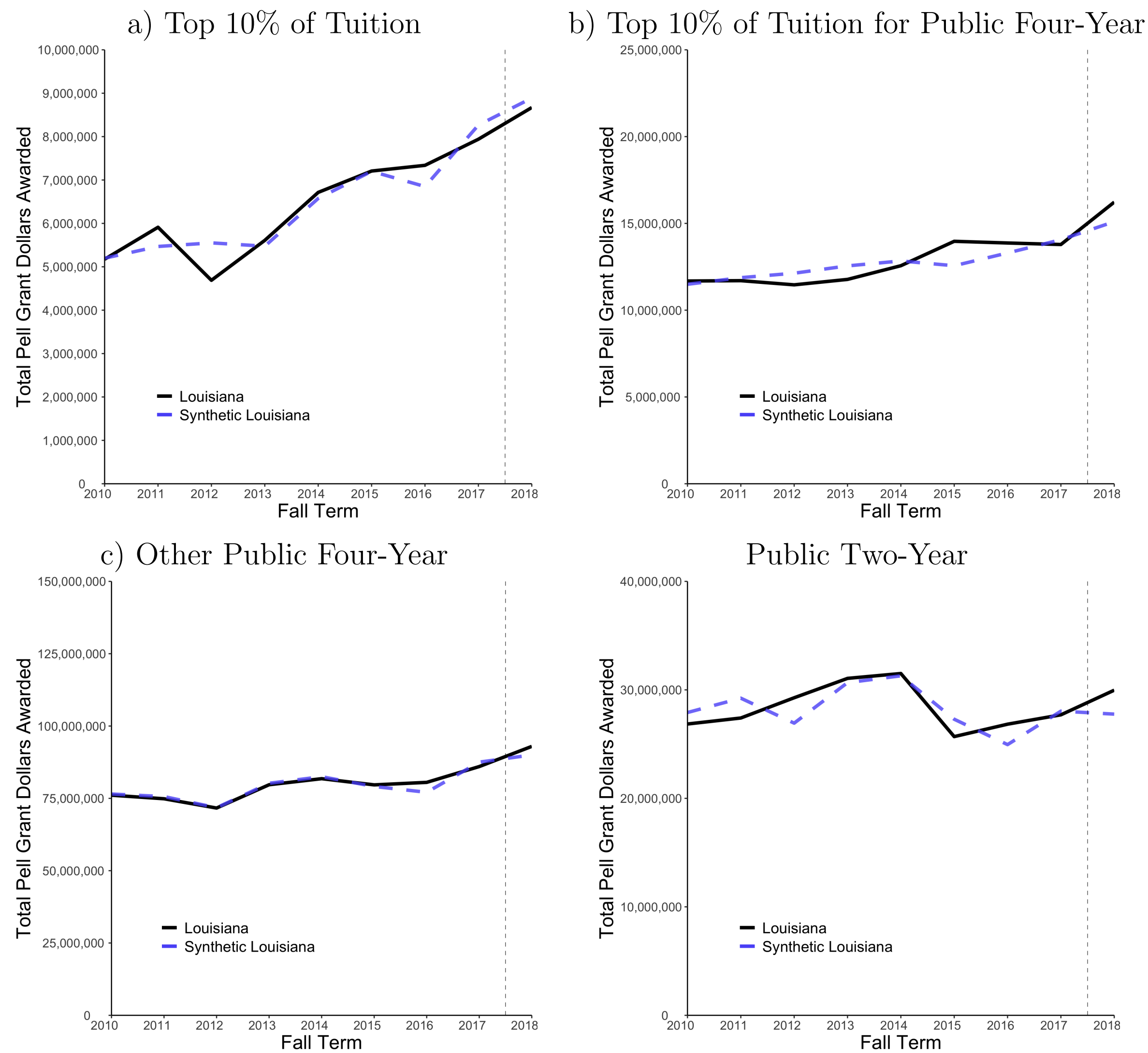


Figure A.15
Pre-Trends Test for ELA Scores - Full Sample

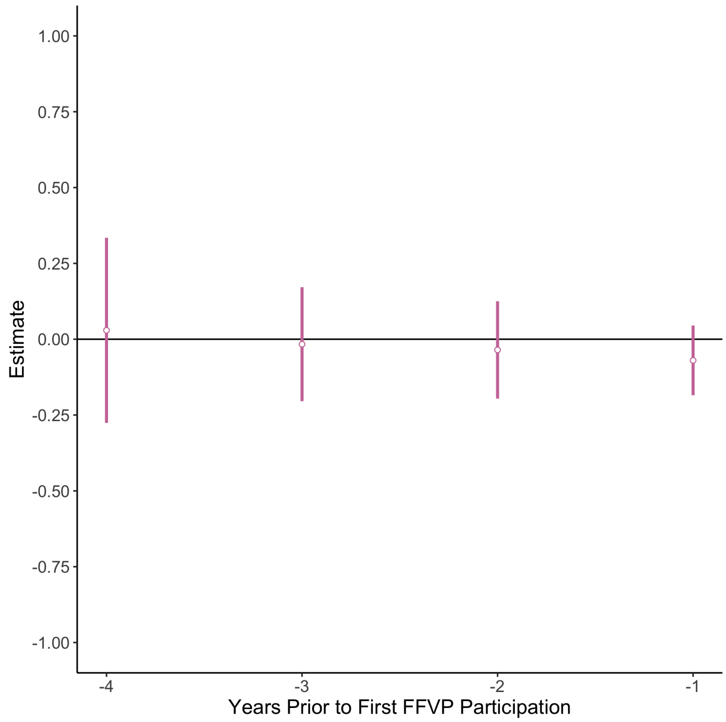


Figure A.16
Pre-Trends Test for ELA Scores - Treated Schools Sample

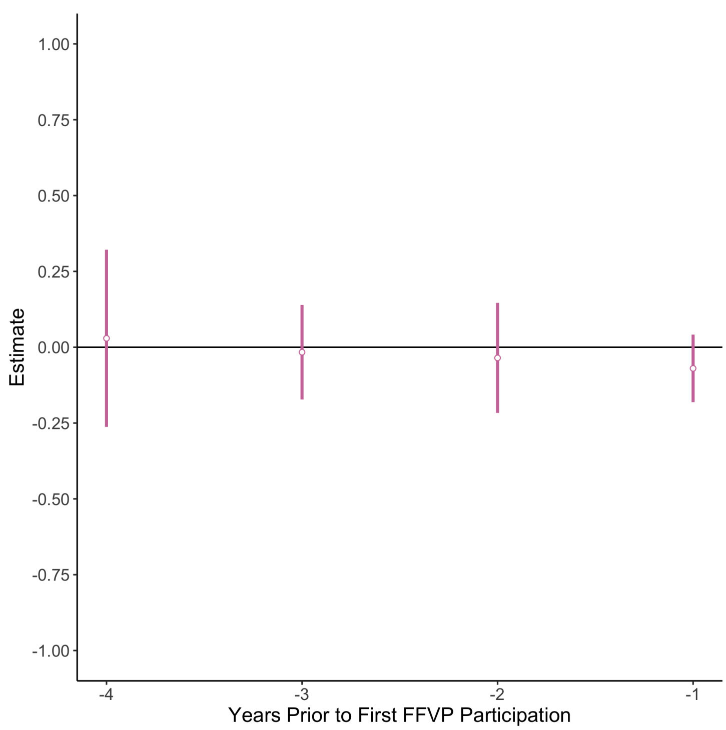


Figure A.17
Pre-Trends Test for ELA Scores - Stay Treated Sample

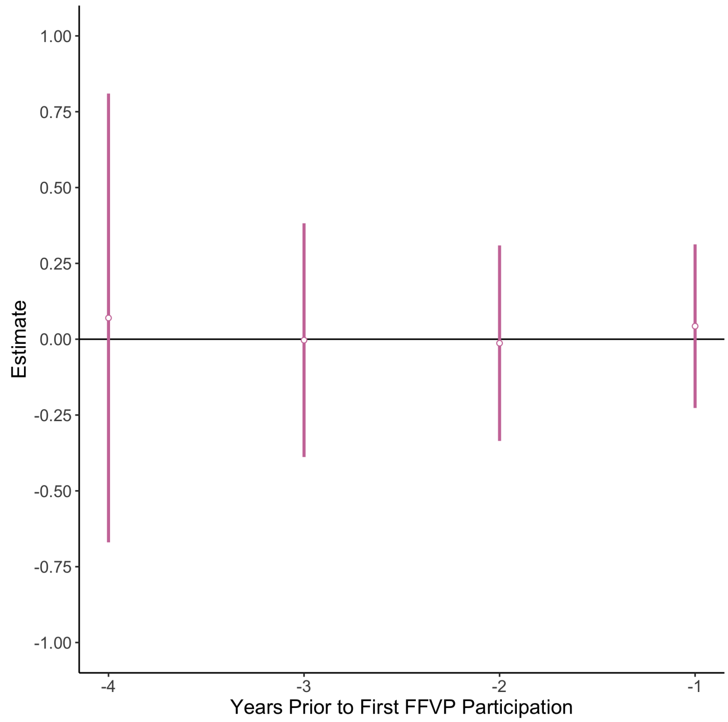


Figure A.18
Pre-Trends Test for Mathematics Scores - Full Sample

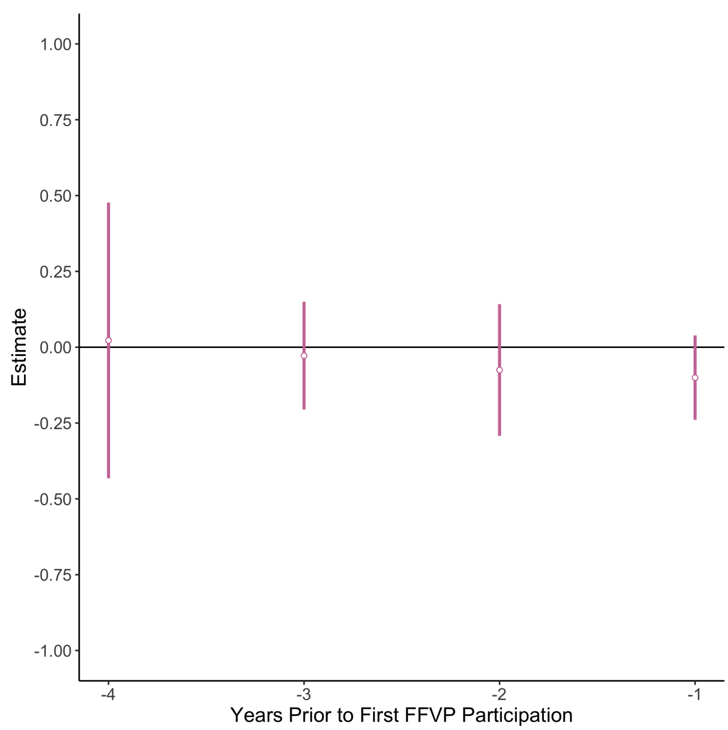


Figure A.19
Pre-Trends Test for Mathematics Scores - Treated Schools Sample

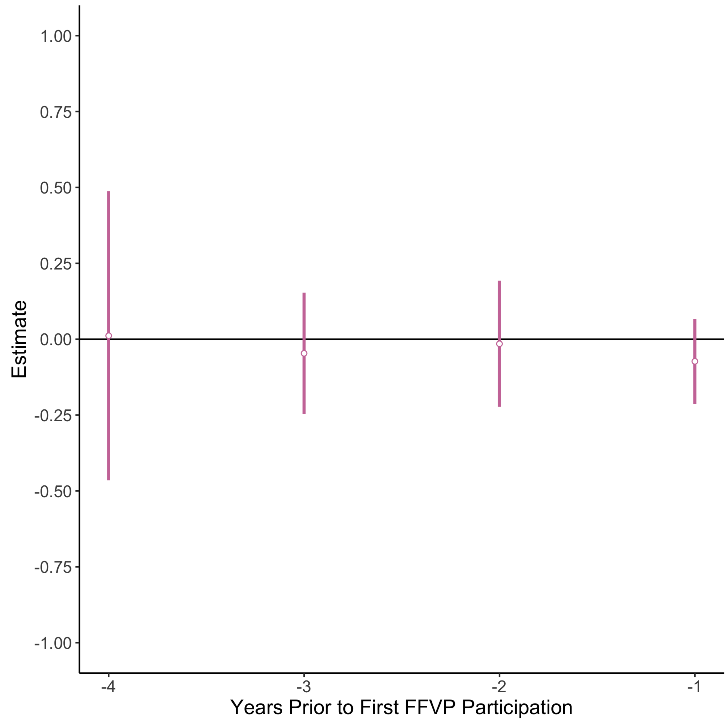


Figure A.20
Pre-Trends Test for Mathematics Scores - Stay Treated Sample

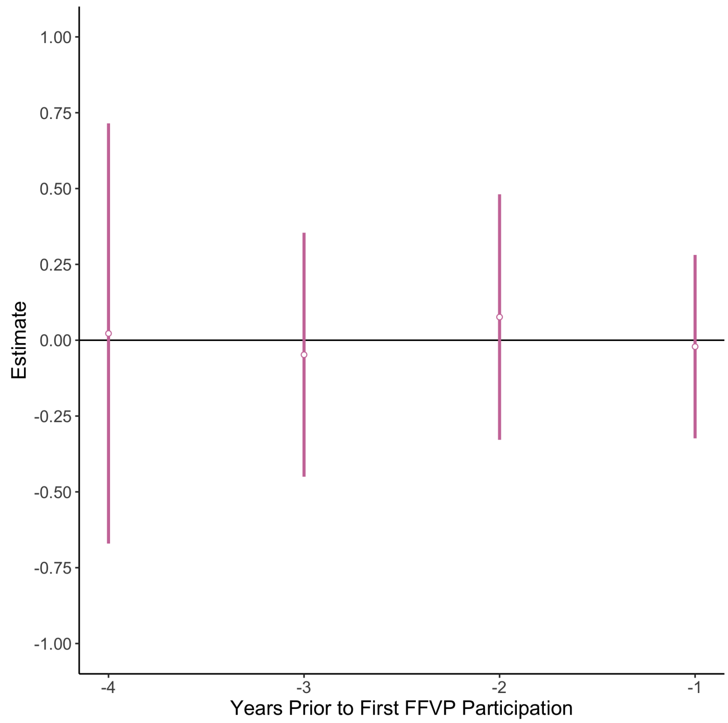


Figure A.21
Overlapping Density Plots

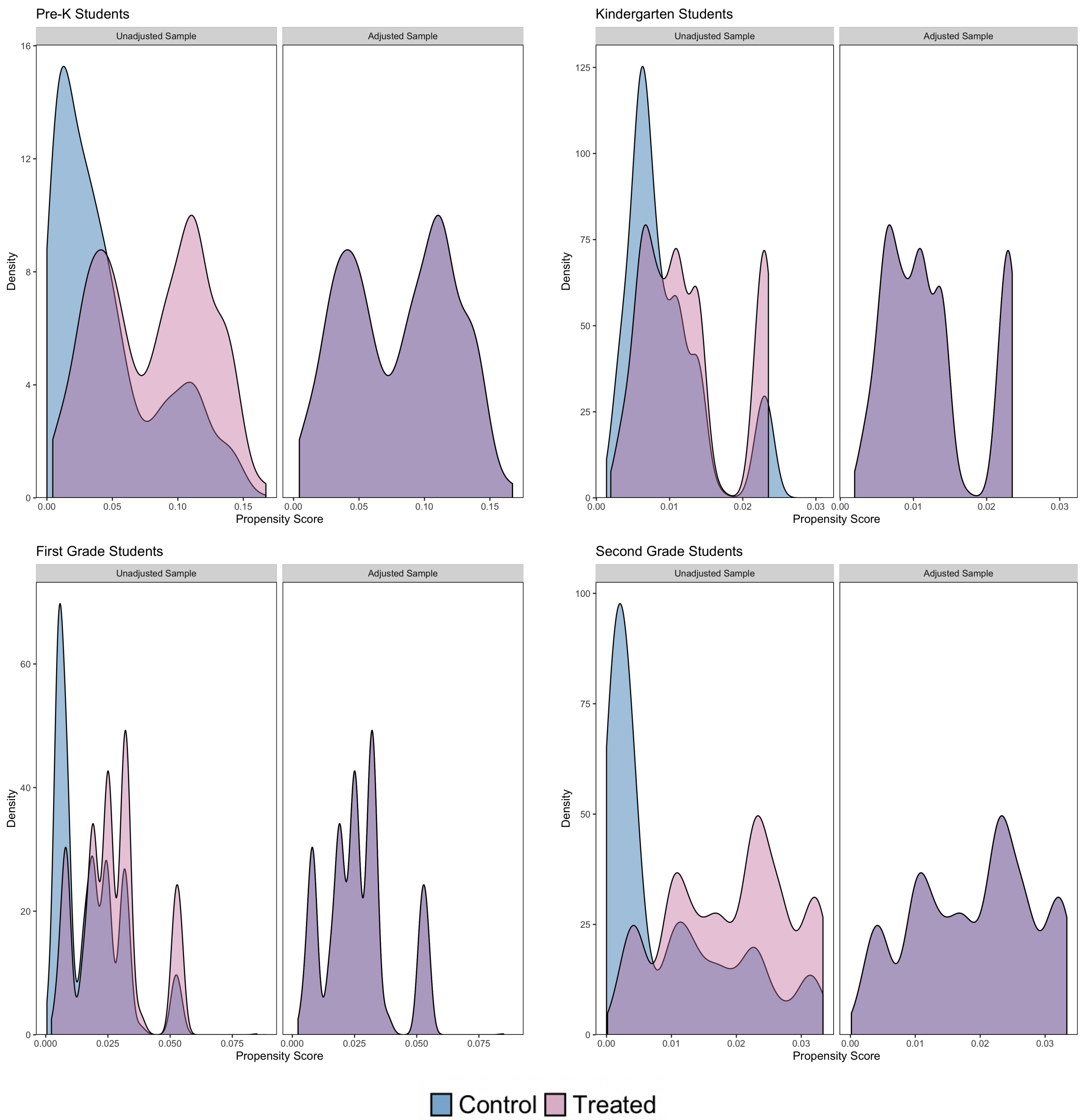
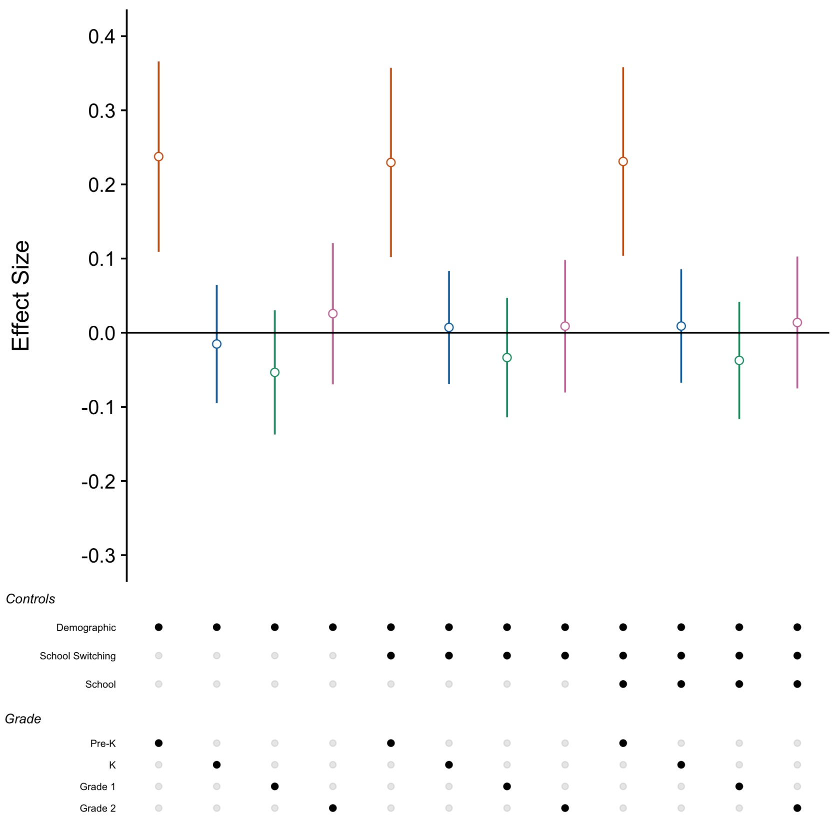
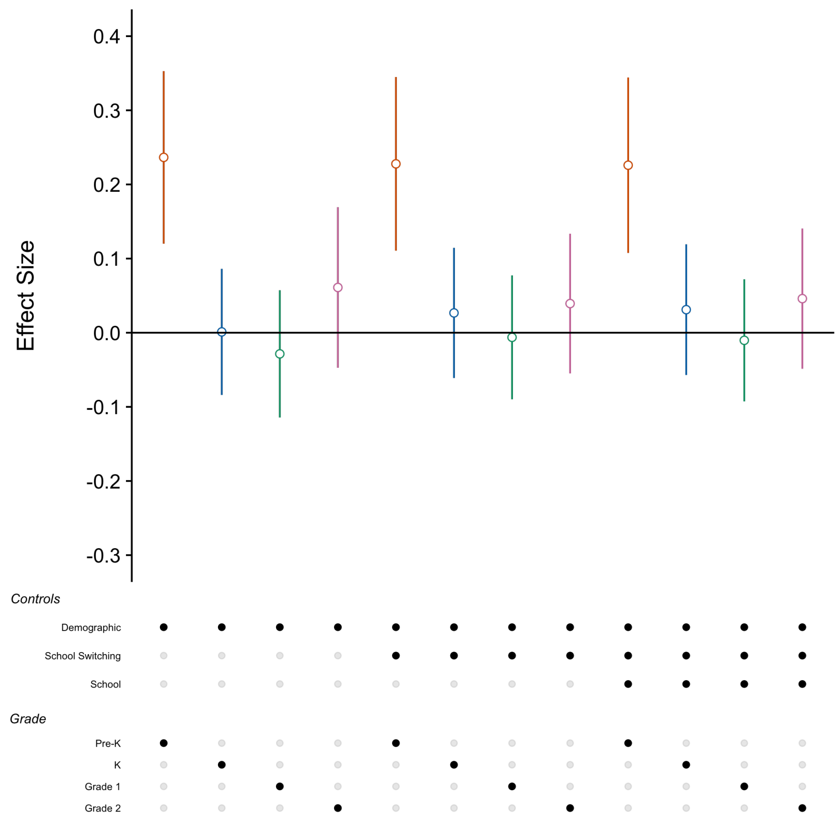


Figure A.22
Effect of Treatment on Third Grade ELA Scores, with School Fixed Effects



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression with controls for student demographics and school characteristics and school-level fixed effects. Standard errors are clustered at the school-level.

Figure A.23
Effect of Treatment on Third Grade Mathematics Scores, with School Fixed Effects



Note: Each bar represents the average effect of the Culture of Reading program on third-grade ELA test scores. Each effect size is estimated from a linear regression with controls for student demographics and school characteristics and school-level fixed effects. Standard errors are clustered at the school-level.

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