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Getting Ahead by Spending More? Local Community Response to State Merit Aid Programs

Rajashri Chakrabarti
Nicole Gorton
Joydeep Roy

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Abstract

In more than half of U.S. states over the past two decades, the implementation of merit aid programs has dramatically reduced net tuition expenses for college-bound students who attend in-state colleges. Although the intention of these programs was to improve access to enrollment for high-achieving students, it is possible that they had unanticipated effects. We analyze whether state funding for higher education and K-12 education changed as a result of program implementation, and whether local school districts attempt to counter any such changes. We employ two methodologies to study whether this has been the case: a difference-in-differences model and a synthetic control estimation strategy. We find robust evidence that implementation of state merit aid programs led to an economically (and statistically) significant decline in state funding for K-12 education, which was mostly offset through increases in local revenues by school districts. These results have important implications for understanding how merit aid policies could have unintended consequences for the students they aim to support.

Key words: merit aid, school finance, incentives

Chakrabarti: Federal Reserve Bank of New York (emails: rajashri.chakrabarti@ny.frb.org, nicole.gorton@ny.frb.org). Roy: Columbia University (email: jr3137@columbia.edu). This paper was written when Nicole Gorton was a research analyst at the Federal Reserve Bank of New York. The authors thank George Bulman, Damon Clark, Peter Hinrichs, Mark Long, Michael Lovenheim, Maxim Pinkovskiy, Miikka Rokkanen, Wilbert van der Klaauw, and seminar participants at Brown University, Columbia University, American Economic Association meetings, and at conferences held by the Association for Education Finance and Policy, the Association for Public Policy Analysis and Management, and the Econometric Society for valuable comments. The authors also thank Robert Dent for excellent research assistance.

1 Introduction

Over the last two decades, the explosion of merit aid programs in various states has radically transformed the higher education landscape in the United States. In more than half of U.S. states today, college-bound students who attend in-state colleges, particularly public colleges, have witnessed a large reduction in their net college tuition expenses. Prior research has examined various aspects of state merit aid programs, including their effects on college enrollment, persistence and completion, on migration and brain drain, on choice of college and majors, and on high school outcomes[1]. In this paper we deviate from the existing literature and analyze a hitherto unexplored aspect of state merit aid programs. Did state merit aid programs affect kindergarten through twelfth grade (K-12) school funding? When the implementation of state merit aid programs forced an increase in state funding for higher education, was state funding for K-12 crowded out? Did local funding increase in an attempt to compensate for that change?

State merit aid programs could create upward pressures on state funding for higher education for two reasons. First, merit aid programs markedly increase the number of college-going students, forcing the state to put more funding towards colleges and universities. Second, merit aid programs minimize the out-of-pocket payments for families towards tuition, so a larger portion of each student's tuition cost is footed by the state.[2] Increases in state funding for higher education can potentially crowd out state funding for K-12 education, and we investigate whether this has been the case. Relatedly, we also ask whether local communities and school districts in these states increased their school spending in an effort to compensate any declines in K-12 state funding as well as potentially to make their students academically ready to take advantage of the generous merit aid. With college education becoming increasingly salient in our current economy, families and school districts may be induced to respond positively to such overtures. This is even more likely to be the case given that prior literature has documented significant responses both of school districts to incentives from higher levels of government (Dye and Reschovsky (2008), Chakrabarti, Livingston and Roy (2013)), as well as of students and families improving their high school credentials to take advantage of the tuition discounts. We study empirically the fiscal responses of families and governments to the adoption of state merit aid programs.

[1] Cornwell, Mustard and Sridhar (2006), Dynarski (2004); Cornwell, Lee and Mustard (2005), Dynarski (2008), Scott-Clayton (2011); Zhang and Ness (2010), Sjoquist and Winters (2014); Chakrabarti and Roy (2013), Cohodes and Goodman (2014) and Sjoquist and Winters (2015); Henry and Rubenstein (2002).

[2] While most merit programs were funded by the state, there were some that were funded by lotteries.

The answers to these questions have important implications not only for students in school at the time of merit aid program implementation, but also for education quality in the long run. If merit aid programs expand access to higher education at the expense of funding for K-12 education, we may be concerned that the students eligible to take advantage of these merit aid programs may be less well prepared over the long run if they faced revenue declines while enrolled in school. Similarly, there may be fewer students eligible to take advantage of merit aid programs if decreases in funding for K-12 education manifest in poorer academic outcomes. While we don't investigate these academic outcomes directly, they motivate our analysis fo

We employ two estimation strategies to investigate these responses. First, we use a difference-in-differences estimation strategy; then, we use a novel synthetic control strategy to estimate overall average treatment effects of these programs, as well as the path of treatment effects over time. Throughout, we take advantage of the staggered introduction of merit aid programs across the states (Figure 1). We investigate whether the introduction of such programs with generous tuition discounts affected state and local funding for K-12 education. We exploit a long time-series of data spanning 22 years, from 1989-90 academic year till 2010-11 academic year, during which 27 U.S. states enacted merit aid programs. The long time-span allows us not only to control adequately for pre-reform trends but also to capture medium-run and long-run responses.

We find that merit aid programs led to a statistically and economically significant increase in state funding for higher education.[3] In contrast, state funding for K-12 education fell markedly, in keeping with the crowd-out hypothesis. Consistent with the incentives outlined above, local government funding as captured by property tax revenue and local revenue increased. Despite an increase in local government funding, we find evidence that institution of merit aid programs led to a small net decline in total K-12 revenue in these states.

One concern with our estimation strategy is whether merit aid programs were implemented at random. Their implementation could reflect a broader, state-wide shift in public opinion towards higher education that could also have affected state and local funding. For example, such a shift in priorities could have mobilized the state government to implement additional policies then; in that case, our results could represent the combined effects of all these changes, rather than just the effect of the merit aid program itself. Alternatively, their implementation could

[3] As we will discuss, this result is not surprising but is rather a necessary condition if we expect any changes from state and local governments in response to these programs. Changes in state funding for higher education is the mechanism through which those responses would occur.

be a function of local economic conditions; for example, a high unemployment rate may prompt the state government to implement policies that would broaden the educational backgrounds of the state's residents. To investigate these potential concerns, we test for changes in the composition of the state legislature, changes in which party controlled the governorship around the time that the program was implemented, as well as changes in macro variables around the implementation year. In addition, we test whether the presence of a merit aid program and timing of its implementation can be predicted by observable characteristics of the state. If merit aid programs were systematically implemented by one particular party or were associated with a change in state government composition, we might be concerned that other policies or public opinion towards higher education were shifting simultaneously. However, we do not find evidence that one particular party, either in the governorship or in the state legislature, was more commonly associated with implementation of a merit aid program. This makes it unlikely that program implementation occurred alongside significant changes in public attitudes towards higher education or changes in other education policies. Similarly, we do not find any evidence of changes in macro variables around the time of implementation or evidence that observable characteristics can predict the timing of implementation.

The findings of this paper have important policy implications. While policymakers and academics have been emphasizing a focus on college readiness, college enrollment and completion, there has been limited research on how changes in higher education funding may affect incentives of different levels of government, school districts, and families. The institution of merit aid programs provides a natural experiment in which to analyze the incentives and responses of state governments, the willingness of local communities to take advantage of these potentially large discounts in tuition and the responses of the school districts to these various funding changes. We review in detail how our paper contributes more broadly to the burgeoning literature on state merit aid programs in the following section. The rest of our paper is structured as follows. We begin in section II by providing some background on merit aid programs and reviewing related research on such programs. In section III, we explain in detail the nature of our data and why it is well-suited to answering these questions. Section IV details our methodology and results using a differences-in-differences strategy. Section V describes the methodology for and results using a synthetic control estimation strategy. In section VI, we show some robustness checks related to the exogeneity of program implementation. Finally, in section VII, we conclude and discuss the implications of our results.

2 Background & Motivation

The first state merit aid program, instituted in Arkansas in 1991, was a relatively small initiative. It was quickly followed by the HOPE (Helping Outstanding Pupils Educationally) scholarship program in Georgia in 1993, the canonical program that was then widely replicated by other states. As of 2016, 27 U.S. states had implemented some type of a merit aid program. For a comprehensive list of these programs and the years they were implemented, see table 1.

Merit aid programs have many objectives, many of which have been documented by Cornwell, Lee and Mustard (2005). First, these programs intend to increase college enrollment by promoting access to higher education. Second, they provide a greater incentive for students to remain in-state for their post-secondary schooling.[4] Third, these programs reward and promote academic achievement, and may allow high-achieving students who may not have had the means to attend college to do so. Often, the scholarships are only available to students who meet certain academic benchmarks in high school, and renewal during college is contingent on a satisfactory rate of progression. Most merit aid programs award scholarships for attending both private and public in-state colleges, and at both 2-year and 4-year colleges. However, the awards are often lower at private colleges than in public ones, an institutional detail consistent with the stated goals of such programs.

There is a large literature exploring how effectively merit aid programs have produced these desired outcomes; the majority of which has highlighted the impact of these programs on college enrollment and completion. There is evidence that state merit aid programs lead to increases in overall college enrollment in the adopting state (Dynarski (2000) and Cornwell, Mustard and Sridhar (2006)), a result likely driven in part by sharp increases in in-state college enrollment (Dynarski (2000), Cornwell, Mustard and Sridhar (2006), Zhang and Ness (2010)). The evidence on completion is less robust; for example, Dynarski (2008) had found evidence of significant improvements in college completion in merit-aid adopting states but Sjoquist and Winters (2012) find that merit aid has no meaningful effect on the likelihood that an individual ever finishes college. The evidence in favor of a positive impact on academic outcomes is also mixed. Both Pallais (2009) and Scott-Clayton (2011) find positive impact of the merit aid programs in Tennessee and West Virginia on the probability of completion, GPA, credits

[4] One potential objective of merit aid programs may have been to encourage high-achieving, college educated students to remain in-state after graduation, since they would have remained in-state to attend college. However, there is little evidence that this objective has been reached. For example, Groen (2004) finds only modest effects of college location choice on future residential decisions.

earned, and other academic outcomes. But Cohodes and Goodman (2014), studying a merit aid program in Massachusetts, find that scholarship use actually lowered college completion rates because students chose lower quality colleges to take advantage of tuition discounts. Similarly, Cornwell, Lee and Mustard (2005) shows that the Georgia HOPE program led to a decrease in full-load enrollments and increases in both course withdrawals among resident freshmen and summer school credits, with the effects concentrated among those most likely to lose the scholarship otherwise. Fitzpatrick and Jones (2016) provide evidence on the long-term effects of merit-based scholarships on residential mobility. They find eligibility for merit aid programs slightly increases the propensity of state natives to live in-state. However, the magnitude of the impact is small enough to conclude that merit aid programs do not meaningfully alter migration behavior.

In a recent contribution, Rockoff (2010) finds a significant fiscal response by school districts to changes in the tax price of local school spending. Analyzing the impact of a property tax-relief program in New York State that lowered the marginal cost of school expenditure to homeowners, he finds that a typical school district, which received 20% of its revenue through the program in the school year 2001-2002, raised expenditure by 4.1% and local property taxes by 6.8% in response to the program. Other studies have also documented significant responses to incentives at the school district level Reback (forthcoming) finds that tax-price reductions offered to elderly homeowners moderate their effect on local public school revenues. Chakrabarti, Livingston and Roy (2014) find significant offsetting responses by local school districts in New York State in response to state aid declines in the post-Great Recession period - responses that varied by region, property wealth, and extent of state aid. In this paper we ask whether such fiscal responses can occur when the instrument is not fiscal or monetary in nature, at least not directly, though tuition discounts for college should in principle lead to considerable savings by families down the line.

Evidence on whether and how merit aid programs alter the type and quality of colleges attended is also mixed. Goodman (2008) and Cohodes and Goodman (2013) provide evidence that potential college students are often willing to trade-off school quality for a more generous scholarship and that this substitution adversely affects college outcomes. But Chakrabarti and Roy (2013) find that in the aftermath of the HOPE program, Georgia freshmen attended relatively more-selective colleges overall. This was true both of students enrolled in in-state colleges, as well as students attending out-of-state colleges, the latter most likely due to an increase in the reservation price to go to out-of-state colleges following HOPE. In general, evidence on the how successfully merit aid programs have reached their stated goals and whether or not they have benefitted students and states is mixed, and varies based on the particular program being studied.

College students are not the only ones affected by merit aid programs: there is evidence that high school students have significantly responded to these initiatives by either improving their performance or by taking other steps that would make them eligible for merit-based aid. Bugler, Henry and Rubenstein (1999) document an increase in SAT scores of affected Georgia high schoolers that is significantly greater than that of students nationwide. Similarly, Henry and Rubinstein (2002) find that the percentage of Georgia high school students graduating with a 3.0 GPA increase by about 5 percentage points from 55 to 60 percent. Further, they conclude that the HOPE program has improved both the academic skills of college-bound students as well as the equity of educational outcomes in Georgia their findings suggest that African American students, both male and female, have responded most strongly to the incentive. Pallais (2009) finds that in Tennessee the introduction of state merit aid program raised student ACT scores. In Tennessee eligibility for the HOPE scholarship depended partly on ACT scores Bruce and Carruthers (2014) show that students who did not have a high enough GPA in high school, retook the ACT in order to qualify. This underlines the fact that students strategically respond to the incentives embodied in merit aid programs something that has also been documented elsewhere. Steve Harkreader, John Hughes, Melanie Hicks Tozzi, and Gary Vanlandingham (2008) argue that the merit aid program in Florida (Bright Futures Scholarship) contributed to educational improvements by encouraging high school students to take academically challenging courses and attend college in the state, with low-income and minority students showing the largest improvements. The percentage of high school graduates taking required Bright Futures Scholarship courses increased from 54 percent in 1997 to 65 percent in 2001, while 30 percent of all high school graduates qualified for Bright Futures scholarships in 2001, up from 26 percent in 1997 (Office of Program Policy Analysis and Government Accountability, 2003).[5] Finally, there is robust evidence that these programs, with their threat of loss of scholarship unless specific thresholds are met, lead to improved college performance. Hernandez-Julian (2010) finds that

[5] At the same time, though, SAT, ACT, and College Placement test scores of students actually declined from 1996-97 to 2000-01. There is also some evidence of gaming. The Office of Program Policy Analysis and Government Accountability (2003) suggests that some grade inflation occurred in Florida, primarily affecting students who were at or near the Bright Futures GPA cutoff points.

incentives created by the LIFE scholarship in South Carolina increased college GPAs by as much as 1.01 on a four-point scale.

However, large scale initiatives like state-wide policies often have indirect or unintentional consequences, and state merit aid programs are no exception. In this paper, we highlight a potential consequence of state merit aid programs that has not been examined. We ask whether and how state governments, local communities and school districts in these states responded to the implementation of merit aid programs. Incentives matter: comprehensive state merit aid programs, which bestow a large tuition discount on residents of the state if they satisfy the eligibility criteria and enroll in an in-state college, might induce students and families to invest more in K-12 education or at least seek to compensate any declines in state funding. The relevant institutional structures will mediate the impact of the program. For example, there might be a higher response in states with less stringent eligibility rules and more generous scholarships, and in states which allow local school districts a higher discretion over raising revenues for their schools. But we should see districts encouraged to respond to the large pay-offs to having a higher share of their students be college-ready by making a commitment to improving the state of K-12 schools in their jurisdictions. Note that districts can invest not only to induce more of their students to go to any college, but also to make more of their students go to 4-year colleges relative to 2-year colleges (that is expand both the intensive and extensive margins). Not only are labor market outcomes much higher for students who complete a bachelors degree, it is also typically the case that state merit aid programs disproportionately lower the cost of attending 4-year colleges.

The fact that school districts as entities respond significantly to incentives, along with the fact that the generosity of merit aid scholarships have been documented to induce large responses from high school students, suggests that we might expect school districts across the merit aid states to have also behaved accordingly, expending effort to burnish the academic credentials of their students. We study whether this has indeed been the case.

3 Data

We employ data from multiple sources in the analyses that follow. Data on district spending, both aggregate data and various subcomponents, come from the annual F-33 school finance surveys collected by the National Center for Education Statistics (NCES), an agency of the

U.S. Department of Education, under its Common Core of Data (CCD) program.[6] Revenues at the district level are audited after the close of the fiscal year and are then submitted to NCES by each state education agency. Our sample includes detailed fiscal data on revenues and expenditures for all school districts providing public education to pre-kindergarten through grade 12 students beginning with the 1989-1990 academic year and extending through the 2010-2011 academic year. Because data were collected only on a subsample of school districts in the 1990-91, 1992-93 and 1993-94 school years, we exclude these years from our analysis. We use the following measures of school revenue: state revenue, local revenue, property tax revenue, and total revenue. We consider these variables in per student terms; we obtain these measures by dividing the revenue or expenditure in the corresponding category in a given school district and year by the number of students enrolled in that school district and year. Data on state support for higher education come from the State Higher Education Executive Officers (SHEEO) and begins in the 1999-2000 academic year at the state level. To put this variable into terms comparable to the rest of our variables, we adjust it using a cost of living adjustment to account for differences in purchasing power across states, the higher education cost adjustment for inflation to account for changes in inflation over time, and an enrollment mix index, to account for differences in the mix of institutions across states.

District-level control variables, which include district enrollment and district-level race/ethnic makeup data, come from district-level non-fiscal data collected as part of the CCD Local Education Agency Universe surveys. State-level data on population, share of population that is elderly, and race/ethnic makeup data come from the Census Bureau. We use annual data on state populations, drawing from the decennial censuses as well as the state intercensal estimates released by the Bureau. State-level data on median income comes from the U.S. Department of Commerce's Bureau of Economic Analysis. State-level data on the unemployment rate comes from the Bureau of Labor Statistics. All the nominal variables, including the adjusted state appropriation variable, have been deflated using the BLS Consumer Price Index (U.S. City Average) for Education and Communication (base year = December 1997). Summary statistics for all of these variables in the base year (1989), broken down by merit aid states, non-merit aid states, as well as overall can be found in Table 3.

Some states created lotteries to fund state merit aid programs, but we do not differentiate

[6] The CCD is a program of the NCES; it annually collects fiscal and non-fiscal data about all public schools, public school districts and state education agencies in the United States.

merit aid programs by breadth of program or source of funding in our analysis.[7] There are a few reasons for this. First, these lottery funds are often fungible and can be directed to fund other higher education expenses. In addition, the magnitude of funding raised by lotteries is unpredictable and often falls short of projections and of the funding required for a merit program, forcing states to utilize other fund sources. As a result, lotteries often supplant, rather than supplement, other state resources. Second, successfully teasing out the differential effects of program types and funding sources requires a substantial number of states in each group. Of the 27 states implementing merit aid programs, only 7 out of 27 states instituting merit aid programs used lotteries as a source of funding. 9 states implemented 'strong' merit aid programs, but 7 of these 9 had programs that used lottery funds. 15 states had merit aid programs considered to be broad-reaching, but 9 of these are states that also had both lottery programs and strong programs. Due to these limitations, we focus in this paper on the overall effect of merit aid programs in general.

4 Methodology and Results

4.1 Difference-in-Differences

4.1.1 Methodology

Our first identification strategy exploits the staggered introduction of merit aid programs by using a difference-in-differences estimation strategy. We first estimate the effects of the introduction of these programs on funding and spending patterns of school districts. Specifically, we start by estimating the following district-level model:

$$y_{ist} = \alpha_0 + \alpha_1 merit_{st} + \alpha_2 X_{ist} + \mu_i + \eta_t + \epsilon_{ist} \tag{1}$$

Here y_{ist} represents a district-level measure of revenue for school district i in state s at time t; $merit_{st}$ is a dummy variable that takes the value of 1 for state s in year t if that state had a merit aid program in operation that year, 0 otherwise.[8] The coefficient of interest is α_1, which captures any differential patterns in school districts impacted by the existence of merit aid policies, as compared to both the pre-program patterns as well as patterns in peer districts

[7] States with programs funded by lottery (as classified by Sjoquist and Winters) include Florida, Georgia, Kentucky, New Mexico, South Carolina, Tennessee, and West Virginia.

[8] This variable turns back to 0 if a state discontinues a merit aid program, as four states in our sample did during our time period of analysis. Results remain similar if we keep the dummy variable on in these cases.

in other states not exposed to such initiatives. μ_i are district-level fixed effects which control for any time-invariant district-level heterogeneity such as district location, urbanicity; these fixed effects also control for specific institutional features of the respective districts and states.[9] We include year fixed effects (η_t) to control for any secular changes during this period that might affect all districts. Finally, we include a vector of time-varying district and state-level characteristics (X_{ist}) to help control for other factors likely to affect spending patterns, which we detail later on.

In addition to looking at the effect on district funding, we also study whether merit aid programs affected state funding for higher education. State funded merit aid programs may lead to a larger state budget for higher education, which may in turn crowd out state funding for K-12 education. To understand whether this has been the case, we study the impact on higher education state appropriations and link that to the picture for state funding for K-12 education in the merit aid states. It is a necessary condition that merit aid programs increased state funding for higher education: without any response in state funding for higher education, local school district funding could not have been crowded out. To test this, we estimate model 1 above where the dependent variable is state appropriations for higher education obtained from SHEEO (see section 3).

To circumvent potential serial correlation of error terms in such panel data models, we cluster our standard errors. The merit programs and hence our intervention and identifying variation are at the state level, so one may argue in favor of clustering standard errors at the state level. However, the market under consideration is often looked upon as the effective unit to cluster standard errors at (Finkelstein 2007). In our case, we can think of the school district or the county in which the school district lies as the educational market. The idea behind market level clustering is that we may expect observations/entities within a market face common conditions (in our case, funding, instructional practices, etc.) and hence it is reasonable to think that error terms can be serially correlated within a market. In this case, standard errors should be clustered at the market level. It is also worth noting that while the programs under consideration are interventions at the state level, different school districts were effected differently depending on the presence of colleges within their boundaries or close to

[9] The district fixed effects will absorb state fixed effects, so these regressions do not separately include state fixed effects as in Zhang and Ness (2010). As these authors note, U.S. states vary greatly in their higher education systems (e.g., the level of state appropriations, public and private sectors (and the importance of 2-year versus 4-year colleges within the public sector), number and types of institutions, etc.). However, these characteristics are mostly stable over time and thus should be captured by state fixed effects.

them. The main results of this paper are presented with county clusters and school district clusters, both of which are reasonable approximations of the educational market. Clustering at the state level multiplies our standard errors as may be expected and some of our results are rendered statistically insignificant, indicating our identifying variation may be too small relative to the residual variation in this case. We follow the market argument here as we can reasonably expect standard errors to be correlated much more within the market and much less so outside.

Conley and Taber (2011) argue that clustered standard errors can be downwardly biased when the number of policy changes is small. In our case we have 27 states with merit aid policies, so this concern is less pertinent[10]. In our basic specification, we include all the 50 states and the District of Columbia, and look at the time period beginning in the 1989-1990 academic year and ending in the 2010-2011 academic year. This allows us to not only control for pre-program spending patterns but also allows us to capture medium-term and long-term effects (as Table 1 shows, most merit aid programs date from the mid-1990s and later).

We use different measures of per pupil revenues and spending as our dependent variable. We begin by comparing the patterns in total revenues, then disaggregate this into federal, state and local revenues. We pay particular attention to the changes in local revenues, looking separately at property tax revenues to see whether school districts in merit-aid states changed their fiscal efforts in response to such policies. We next look at various spending indicators in addition to overall spending per pupil, we examine patterns in instructional expenditures and in teacher salaries. As mentioned in the data section, all the district financial indicators have been deflated to 1997 dollars the base period used by the Bureau of Labor Statistics (BLS) - using the BLS Consumer Price Index (U.S. City Average) for Education and Communication.

To ensure that our estimates are not biased by time-varying changes in observable characteristics that could affect district spending, we include several time-varying covariates. Changes in demographic characteristics of a school district change the demand for spending. In our regressions we control for the share of students belonging to different races. Similarly, the demographic make-up of a state might influence school spending; we control for changes in state populations as well as their racial/ethnic make-up. Earlier literature has found some evidence that the share of elderly people in the population impacts school spending, so this variable is included as a control variable.[11] Finally, a states' economic condition might dictate whether its

[10]In their analysis of the impact of merit aid programs on collegiate attainment (2012) report similar findings whether they use conventional clustered standard errors or the Conley-Taber confidence intervals their preferred specification included 9 merit aid states

[11] Using district-level panel data, Harris, Evans, and Schwab (2001) estimate the elasticity of local-level expen-

residents are willing to pay more to support their schools; we include the unemployment rate in the state and state median income as proxies for the prevailing economic climate.

Controlling for district fixed effects, year-specific common shocks and time-varying covariates both at the state-level and at the district-level, the above specifications explore whether school districts located in merit aid-adopting states witnessed changes in their K-12 spending and allocation of the same. The underlying assumption in these specifications is the absence of other simultaneous changes - in policy or the environment in which districts operate - that might also affect school financing patterns and decisions of school districts. Note that our specification exploits the staggered introduction of merit aid programs across the U.S. states, which insulates against our findings against biases caused by shocks unique to merit aid states at a particular point in time.

4.1.2 Results

The results from this difference-in-differences analysis are presented in table 4.[12] The odd numbered columns allow for clustering at the district level, while the even numbered columns allow for county level clustering. The first and second columns report results from regressions that include district fixed effects and year fixed effects (in addition to the merit dummy). The third and fourth columns add various state-level covariates, while the fifth and sixth columns further add district-level covariates. Table 4 shows the effects of merit status, as estimated with specification (1), on K-12 school funding. We find that merit aid programs led to a steep decline in state revenue per student by around $200 (or by 5%).[13] Increased higher education funding due to merit aid programs seems to have diverted resources away from K-12 education. This decline was partly compensated by an increase in local revenue (by 3%). No net change was perceived in total revenue as changes in the two components of revenue largely offset each other.[14]

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ditures with respect to the share of elderly residents to be around -0.10. Figlio and Fletcher (2012) also estimate a negative impact of elderly shares on suburban school expenditures.

[12]Results displaying the effects of merit aid program implementation on state funding for higher education can be found in table A.1. Merit aid programs led to an economically and statistically significant increase in state appropriations for higher education, with the state appropriations per student in merit aid states increasing by 5% in the post-program period.

[13]Percentage effects are obtained by dividing coefficients by the overall period mean of the dependent variable. For local revenue, the period mean is $4,316. For state revenue, the period mean is $4,464. For total revenue, that value is $9,497.

[14]We omit federal revenues because these constitute only a small portion of total revenues and are also based on formulas rather than discretion. State revenues are also often based on formulas, but generally there is more discretion involved.

4.2 Synthetic Controls

4.2.1 Methodology

The previous analyses used a difference-in-difference estimation strategy to estimate the overall treatment effect of merit aid programs on school funding. In this section, we use a "synthetic control" estimation strategy which arguably leads to a more compelling identification of these treatment effects. We construct, for each treated state and each outcome of interest, a synthetic control state based on the pretreatment characteristics of each treated state, following the method pioneered by Abadie et al. (2010).[15] For each state and each outcome variable, we find a combination of weights among the group of control states, states that never implemented a merit-aid program during our period of analysis, such that the dependent variable paths for the treated state and its synthetic control are as close as possible in the pretreatment period. In other words, we find a set of weights to minimize the pre-treatment period root mean squared prediction error (RMSPE). We then use this weighted combination of control states (the 'synthetic control') to forecast the path of the dependent variable in the post-treatment period. The identifying assumption of this method is that, conditional on finding a combination of control states such that our treated state and its synthetic counterpart have the same path in the pretreatment period, any difference between them in the post-period is purely a function of the treatment effect.

We select weights by matching, one at a time, each treated state with the group of control states on the basis of the value of the dependent variable in each pre-treatment year, as well as the pretreatment average of state-level demographic variables. After selecting weights and generating synthetic controls for each state and outcome variable, we plot out the path of the treatment state and its synthetic control in the post-program period for each outcome variable of interest. We then calculate each state's average treatment effect by finding the average difference between the treated state outcome and the synthetic state outcome over the post-treatment period. For each of 24 merit aid states and each outcome variable, we generate synthetic controls as described above.[16] The complete set of results for each treated state and

[15] We perform this analysis at the state level, rather than at the district level, to allow us to understand the state-level effects as well as the overall effects. Because there are more than 10,000 school districts, we can't study each school district individually (nor would we want to).

[16] There are, in fact, 27 merit aid states but we are forced to omit 3 of them: Arkansas (1991), Georgia (1993), and North Dakota (1994). These states implemented merit aid policies early on, such that we do not have sufficient pretreatment years in our data to generate a suitable synthetic control state. In general, we do not group treated states into a single treated group because the implementation year is state-specific, rather than country-wide. In addition, grouping states together ignores valuable variation and heterogeneity across states.

each outcome variable are provided in figures A.1-A.3. These figures show that most treated states and their synthetic counterpart match closely in the years before the policy was implemented, then diverge in the post-period. For every treated state, these post-period divergences are the treatment effects that we are interested in measuring.

Table 5 summarizes outcome variables for each pre-treatment year for treated states, untreated states, and synthetic control states. For treated and synthetic averages, each year includes only states for which that year was in fact a pre-implementation year, while every untreated state is included in the average for every year. There are almost no statistically significant differences between synthetic and treated states in terms of each outcome variable each year. Most notably, the differences in magnitude and statistical significance between synthetic and treated states are much smaller than the differences between treated and untreated states, providing some suggestive evidence that a synthetic control method is an improvement over a differences-in-differences model.

4.2.2 Estimating State-Level Treatment effects

We summarize these state-level results by computing the treatment effect for each state and each outcome variable, and plotting the distribution of effects. Figure 2 shows the state-specific average treatment effects of a merit aid policy on each of our primary outcome variables of interest. States with strong merit aid programs are highlighted in a darker blue. We find that most states saw a substantial decline in state revenue per student following the implementation of a merit aid policy; the median treatment effect is -\$404. In terms of local revenue, most states experienced only a modest change, but there is some heterogeneity across states. For example, Louisiana saw a decline of around -\$4,200 per student post-implementation, while Maryland and Wyoming each saw increases, of around \$750 and \$2,000 respectively. Of the states with strong merit aid programs, more experienced an increase in local revenue than experienced a negligible change or a decline. The median effect on local revenue per student was an increase of \$60, but a few of the states – in particular, New Jersey (+\$2,000), Wyoming (+\$1,050), and South Carolina (+\$1,000)— saw a much larger rise in local revenue after the implementation of a merit aid policy. Finally, in terms of total revenue, most states saw a decline; the median change was a decrease of \$441. In these figures, states that implemented strong programs are colored differently than those that implemented weak programs. Most of the strong states appear to have experienced a decrease in state revenue, though there is some heterogeneity in terms of the magnitude of that decline. The effects on total revenue and local revenue are somewhat more spread out amongst the strong states. Some states experienced an increase in local revenue (South Carolina), while others experienced no effect, or even a decline. In terms of total revenue, Tennessee and Florida experienced significant declines in total revenue, while South Carolina, Louisiana, and New Mexico actually experienced slight increases.

4.2.3 Estimating Overall Treatment Effects

Although the synthetic control procedure lends itself well to state-specific analysis, our primary goal is to understand the overall effects of merit aid policies, rather than state-specific ones.[17] To find the overall treatment effect, we begin by constructing an average treatment effect for each outcome by finding the average of the state-level treatment effects. We do this with three different weighting schemes: (i) un-weighted, where each state is treated equally in constructing the overall average, (ii) weighted by inverse root mean square prediction error (RMSPE), where states with a poor pretreatment fit are down-weighted, and (iii) weighted by base enrollment, where states with more students are weighted more heavily.[18] We construct standard errors on these average treatment effects that reflect the within-state, across-time variance, as well as the across-state variance.[19]

In Table 6 column (1) all states are treated equally in constructing the average. We find that state revenue decreases by \$701 per student, local revenue increases by \$205 per student, and total revenue decreases by \$308 per student. All of these effects are significant at the 1% level. When we weight by the inverse of the pre-period root mean squared error, the effect on local revenue nearly doubles, suggesting that many of the states where the treatment had little or negative effect on local revenue did not have a suitable synthetic counterpart. The effects on state and total revenue remain very similar (around -\$690 and -\$380, respectively). When we weight by base enrollment, the effects on local revenue and total revenue are no longer

[17] Because merit aid policies were implemented in different years in different states, we cannot aggregate our treated states into a single treated group as others have done (for example, Mazumder et al. 2016).

[18] Base enrollment is fall enrollment as of the fall of 1989, when our data begins.

[19] The variance of the overall ATEs is calculated as follows:

$$Var(ATE_y) = Var\Big[\frac{\sum_{s\in t}^{N} w_s \times ATE_{s,y}}{\sum_{s\in t} w_s}\Big] = \frac{\sum_{s\in t}^{N} w_s^2 \times \sigma_{s,y}^2}{[\sum_{s\in t} w_s]^2}.$$

for outcome y where w_s is the weight applied to state s. In the unweighted case, $w_s = 1 \forall s$. The results we present here assume that these state ATEs are independent ($cov(ATE_{s_1,y}, ATE_{s_2,y}) = 0$). However, the precision of these effects is robust to relaxing this assumption. We also estimate overall ATEs where we add in pairwise covariances to take into account that there may be non-zero covariance across state ATEs since each state's synthetic control is chosen from the same sample of donor states. The results remain statistically very similar and are available on request.

significant, likely because they were driven by states with small base enrollment (for example, Wyoming and Idaho). However, we still see a significant and robust decline in state revenue per student, and the magnitude of the effect is similar across weighting schemes.

These effects are a bit different in magnitude than those we estimated using a difference-in-differences method. There, we found that local revenue per student increased by around $114, state revenue per student decline fell by about $196, and total revenue fell by a (non-statistically significant) $22. Our synthetic control procedure yielded in a local revenue increase of $205, a state revenue decline of about $701, and a total revenue decline of $308. But regardless of method, the story is the same: state revenue fell, and local revenue rose – though the extent to which local governments were able to make up for the decline in state revenue is a bit different between the two methods, as the magnitude of the decline in state revenue is quite different. There are a number of potential explanations for this discrepancy. First, our difference-in-differences analysis is at the school district level, while our synthetic control analysis is at the state level. Second, the counterfactual comparison group between the two methods is very different. With the difference-in-difference, we compare the treated states to the universe of untreated school districts, while the synthetic control compares each treated state to its unique set of control states.

4.3 Estimating Time Paths of Overall Treatment Effects

These overall effects, however, obscure any heterogeneity in the magnitude of the effect over time and do not allow us to study the time path of the treatment effect. To estimate the time paths of the treatment effect for each outcome, we find the average treatment effect over all of the treated states at each point in time relative to the year the merit aid policy was implemented.[20] This process yields a single time series for each dependent variable, showing the average treatment effect in each period. To understand whether an effect of this magnitude could have occurred by random chance or is truly a function of the treatment, we perform placebo tests. We do this in the style of Abadie et al. 2010's placebo test methodology. For each treated state, we generate "placebo treatment effects" using the control states by performing, for each control state, the same synthetic matching procedure that we used for the treated state, and then plotting the trajectory of the "treatment effect" for the control states. Because the treatment did not really

[20]We exclude in the treated line states whose pre-period RMSPEs fall in the top quartile of the distribution to minimize noise in the overall effects generated by states with very poorly fit synthetic counterparts.

occur in these states, we would expect to see an effect that hovers around zero both before and after the treatment year. Each treated state will have 23 placebo lines, one corresponding to each of the control states.[21]

In order to aggregate these placebo tests and understand the probability that the overall treatment effect could have occurred by random chance, we need to generate a distribution of aggregated placebo test time series and compare these to our aggregated treated state time series. We do this in the following method, similar to that used by Cavallo et al. (2013). For each outcome variable, we randomly pick a placebo line from the placebo lines generated for each treated state, for a total of 24 placebo lines corresponding to a placebo line from each of the 24 merit-aid states. We then bootstrap the process of generating the "overall placebo treatment lines" and plot each "overall placebo treatment line" alongside the overall average treatment effect line to generate a distribution of placebo treatment lines. We filter these average placebo lines by comparing the pre-period RMSPE of the treated state to that of each control state and dropping placebo lines where the pre-period RMSPE is more than twice that of the treated state (Abadie et al. 2010).[22] Using this distribution and following Abadie et al. (2010), we calculate a p-value associated with each outcome variable, which represents the probability of 'randomly' generating a treatment effect where the post- to pre-RMSPE ratio is at least as large as that created by our treated state. In figures 3 through 5 which show the results of this procedure, the effects on local revenue and state revenue are significant at the 5% level (as indicated on the relevant figure) based on the Abadie p-value procedure. Although the magnitude of the effect on local revenue appears positive but relatively small, the significance of this estimate is quite high because of the low pre-period RMSPE. We see a large and robust decline in both state revenue and total revenue, though the effect on total revenue is not statistically significant at conventional levels because of the poor pre-period fit as compared to that of the placebo lines.

One advantage of this analysis is that we observe the timing and duration of the response to the implementation of the merit aid program. In terms of state revenue, we find that the decline begins immediately and continues throughout the post-period. Though we have no way to directly test this, this pattern is likely due to the increasing burden that the merit aid program placed on state finances as more and more students began to enroll in the program.

[21] For brevity, and because the state-specific placebo tests are not of particular interest to us, we do not present all of these results in our main results. They are available upon request.

[22] For local revenue, we only drop placebo lines where the pre-period RMSPE is more than 10 times that of the treated state. This is because the pre-period RMSPE for the treated states for local revenue is so small that if we cut by any more than that, we are left with almost no placebo lines.

Each year following program implementation, an additional cohort of students is eligible for funding, meaning the state had to expend more dollars to support the program. In terms of local revenue, we see that there was no effect for the first year after the policy was implemented. This is likely a function of the time required for local communities to institute changes in response to the program.

4.4 Synthetic Control Robustness

The primary identifying assumption of the synthetic control method is that, in lieu of the treatment, each treated state would have followed the same path as the chosen combination of control states. However, there are some scenarios in which this assumption could potentially be violated. For example, if a state is hit by a shock right at the time of the program or right after the program that is not experienced by one of the control states and is correlated with an outcome variable, our estimate of the treatment effect would include both the true effect plus the effect of this shock. This could also occur if an event happens a few years after program implementation, but within our time frame of post-period analysis because any shift in the path of treated state as compared to the path of the synthetic control in the post-period will be considered a function of the treatment effect. While it seems fairly unlikely that this would happen, there are a few state-specific events that occurred around the timing of program implementation and could have also affected our outcomes of interest. For example, Hurricane Katrina hit Louisiana in 2005. While Louisiana implemented a merit aid program in 1998, up until this point, we've examined the post-period for as long as our data allow so any deviations in state, total, or local revenue caused by Hurricane Katrina could be captured in the treatment effect. Other events that could cause similar issues are the housing boom and subsequent bust in states like California, Florida, and Nevada. New York experienced a significant boom on Wall Street between 1998-2000, followed by 9/11 in 2001. These state-specific events are more likely to occur as we examine post-treatment years increasingly far from the implementation date. Events occurring in control states are equally likely to affect our results, since they could invalidate our post-treatment period forecast. To ensure our results are not a function of these such events, we perform a few robustness checks.

First, we check that our results are not a function of a change in the composition of years included in calculating the overall treatment effect. In our primary results, we use every post-period year available in our data. But because we do not see every state for more than 5 years post-treatment, this means we include a different number of years in the average treatment effect depending on a state's implementation year.[23] To ensure this is not biasing our results, we perform our analysis using a 3-, 5- and 6-year period following the implementation year, rather than using all available post-treatment years. Tables 7, 8 and 9 show these results using a 3-, 5-, and 6-year window respectively. These results are qualitatively very similar to those presented in our main results. Importantly, the 3-year window and 5-year window results include all of our treated states in each post-period.

Second, we compute the overall average treatment effect for each outcome variable leaving one treated state out each time, to ensure that our results are not driven by a single state's effects, particularly given the concerns outlined above that may affect single states. The results of this exercise, using a three-year post-treatment window are presented in table 10.[24] Regardless of which state is left out of our analysis, our overall treatment effects remain qualitatively similar to the average treatment effect computed over the universe of treated states, suggesting that our results are not driven by a large effect in a single state. Finally, we perform our synthetic control analysis leaving one control state out of the donor pool at a time, and re-computing the overall average treatment effects. Table 11 presents these results, with the control state that was left out listed in the left-hand column. While there are some slight changes to point estimates as we vary the pool of control states, our estimates remain qualitatively very similar both to each other, as well as to the figures calculated with the universe of control states.

5 Robustness

5.1 Exogeneity of Program Implementation

While many papers have taken the implementation of merit aid to be exogenous, it is possible that merit aid programs were not randomly assigned to states at random times. Rather, the implementation of a merit aid policy could reflect the prevailing priorities of the state government, and by extension, the people who elected that government around the time of program implementation. Alternatively, program implementation could reflect a shift in state economic conditions. Of particular importance to our analysis is whether the implementation of these

[23]Figure 6 shows the number of states in our sample for each number of years post-treatment

[24]Note that for local and state the inverse RMSPE weighted treatment effects are nearly identical to the overall treatment effect no matter which state we leave out. This is because the weights are skewed; one state has an extremely small RMSPE compared to the other states. Thus, these results only change when this state is left out of the analysis.

programs coincided with changes in the state's educational priorities or economy that may have also affected our outcome variables. If this were the case, our point estimates of the effect of the merit aid policy would be the cumulative sum of the effect of the merit aid policy and the other changes that occurred and thus would violate the parallel trends assumption of our difference-in-differences model. In lieu of the treatment (the implementation of the merit aid programs), the treated and untreated states still would have followed different treatment paths because the treatment did not occur in a vacuum. We test for evidence of these explanations of implementation in the analyses that follow.

5.1.1 Selection on Observables

One test of program exogeneity is to identify whether observable factors, if any, can predict the implementation of a merit aid program as well as which factors can predict the timing of program implementation among states that implement a program. If observable characteristics can predict whether and/or when a program is introduced, there may be concerns that implementation changes reflect broader, state-wide changes that could be correlated with our outcomes. First, to identify whether observable characteristics can predict *which* states implemented a merit aid program, we estimate equations of the following form:

$$merit_s = totalrev_{s,1989} + staterev_{s,1989} + localrev_{s,1989} + X_{s,1989} + \epsilon_{s,1989} \tag{2}$$

where $merit_s$ is a dummy taking on a value of 1 if state s ever implemented a merit aid program, $X_{s,1989}$ is a vector of state-level demographic controls used in the synthetic control matching algorithm as of 1989, $totalrev$ is total revenue per student, $staterev$ is state revenue per student, and $localrev$ is local revenue per student. Because there is no pre-year for untreated states, we use values of our variables as of 1989, which is a pre-year for every state in our sample. Second, to test whether observable characteristics can predict the timing of program implementation among all program-implementing states, we estimate the below:

$$implement_{st} = totalrev_{s,t-1} + staterev_{s,t-1} + localrev_{s,t-1} + X_{s,t-1} + \epsilon_{s,t-1} \tag{3}$$

where $implement_{st}$ is a dummy taking on a value of 1 if state s implemented a merit aid program in year t and $X_{s,t-1}$ is a vector of state-level demographic controls used as of the previous period. Here, we take our independent variables in the previous period to model whether observed state-level variables affected program implementation the following year.

Results from estimating equations 2 and 3 are shown in table 12. There are some small demographic differences between treated and untreated states (column (1)) as well as between treated and synthetic states (column (2)) in terms of population, the unemployment rate, and enrollment, but there are no statistically (or economically) significant differences between our outcome variables in the base year. While we will never be able to rule out all potential sources of endogeneity, this provides some suggestive evidence that, for example, it was not the case that states historically investing more in education were more likely to implement merit aid programs. If this were the case, we could expect to see a large and statistically significant coefficient on state, local, or total revenue per student here. But our analysis takes advantage of the staggered introduction of merit aid programs, rather than simply comparing implementing states to non-implementing states. However, we also find no evidence that any of our demographic variables can predict the timing of program implementation. All point estimates of equation (2), shown in column (3) are statistically insignificant and most are close to zero.

5.1.2 Trends in Macro Variables

In addition, we test whether there are trend breaks in macro variables around the time of program implementation. Drawing from Deshpande and Li (2017), we estimate equations of the following form, at the state (program) level:

$$y_{st} = \alpha + \sum_{\tau} \delta_\tau D_{s\tau} \cdot merit_s + \gamma_t + \delta_s + \epsilon_{st} \tag{4}$$

where y_{st} is one of the following: unemployment rate, K-12 fall enrollment, share of the population over age 65, share of population that is African-American, share of population that is White, share of population that is Hispanic, and share that is Asian. D_{st} are fixed effects corresponding to each time index τ. γ_t is a vector of year fixed effects and δ_s is a vector of state fixed effects. For untreated states, we consider the time index to always be 0. We cluster standard errors at the state level. We plot the coefficients δ_τ as shown in figure 7. For each of these variables, we do not see any evidence of trend breaks correlated with the timing of the merit aid program implementation.[25]

[25]Note that standard error bars expand over time because there are fewer states with data for 8, 9, and 10 years post-implementation.

5.1.3 State Governments

We test for patterns in state government control around program implementation by examining trends in state legislature composition and governorships over time. Data on state legislatures come from the National Conference of State Legislatures and data on party control of governorships come from The Washington Post.[26] Table 13 shows the fraction of merit aid states with various parties in charge of the state legislature and governorship as of the year before the merit aid policy was implemented. The majority of states that implemented merit aid programs had Republican governors at the time of implementation (58%), but it is fairly evenly balanced between the two parties. However, of these states with Republican governors, most had either split or democratic-controlled state legislatures at the time of implementation. In fact, when we split by both governorship and state legislature, the government that most commonly implemented merit aid implementing states are those states with a democratic governor and statehouse. However, because none of these statistics are significantly different from each other, it does not appear that merit aid implementation may not be associated with a change in political party that altered priorities throughout the state.

As a more rigorous test, we check to see if the party in control of the state legislature and/or of the governorship has any predictive power in terms of when a merit aid program will be implemented. To capture the effects of the political party associated with the governor in charge, we estimate:

$$implementation_{st} = \beta_1 gov_{st}^{rep} + \beta_2 gov_{st}^{dem} + \beta_3 gov_{st}^{other} + \epsilon_{st} \qquad (5)$$

where $implementation_{st}$ is an indicator variable taking on a value of 1 if year t is the year that state s implemented a merit aid policy. gov_{st}^{rep} is an indicator variable taking on a value of 1 if state s had a republican governor in year t (and similarly for democratic governors and governors from other parties). To capture the effects of the political party in control of the state legislature, we estimate:

$$implementation_{st} = \alpha_2 legislature_{st}^{rep} + \alpha_2 legislature_{st}^{dem} + \alpha_3 legislature_{st}^{split} + \epsilon_{st} \qquad (6)$$

where $legislature_{st}^{rep}$ is an indicator variable taking on a value of 1 if state s had majority republican state legislature in year t (and similarly for majority democratic state legislatures

[26]These data are only available in even years (when most state-level elections take place). Our primary results fill in for the odd years using data from the preceding even year, but are robust to filling in using data from the following even year, or to using only the available years of data.

and split legislatures). To capture the combined effects of the political party in control of the state legislature as well as the party of the state's governor, we estimate:

$$\begin{aligned} implementation_{st} = \gamma_1(legislature_{st}^{dem} \times gov_{st}^{dem}) + \gamma_2(legislature_{st}^{rep} \times gov_{st}^{dem}) \\ +\gamma_3(legislature_{st}^{split} \times gov_{st}^{dem}) + \gamma_4(legislature_{st}^{dem} \times gov_{st}^{rep}) \\ +\gamma_5(legislature_{st}^{rep} \times gov_{st}^{rep}) + \gamma_6(legislature_{st}^{split} \times gov_{st}^{rep}) \\ +\gamma_7(legislature_{st}^{dem} \times gov_{st}^{other}) + \gamma_8(legislature_{st}^{rep} \times gov_{st}^{other}) + \epsilon_{st} \end{aligned} \tag{7}$$

where the right hand side variables are the exhaustive set of combinations of governors and state legislature majorities. We include state-level controls for changes in local economic conditions that could affect program implementation where indicated. Equation 5 identifies whether the timing of merit aid policy implementation can be predicted by the presence of a governor from each type of party. Equation 6 does the same, but with state legislatures: does having a majority of one particular party predict the timing of program implementation? Finally, equation 7 combines both to study whether the timing of implementation depends on both the party in charge of the state legislature and in charge of the governorship.[27] Throughout our main analysis, we examine state governments in period t instead of $t-1$, since the year of implementation was likely the year the policy was signed into law. This analysis then captures the political parties in power in that year. As an additional test, we check to see if lagged party indicator variables make a difference in predicting the timing of program implementation: maybe it's not the people in charge the year the policy was implemented, but rather the people in charge the year before. Results are presented in appendix table A.2; we again see no evidence that these lagged indicators have any predictive power, either.

The results of the above are presented in table 14. We see no evidence of systematic patterns between political party and merit aid policy adoption. No matter the specification, our point estimates are statistically insignificant and close to 0. If we cannot predict the implementation of a merit aid policy based on the party in power in the year prior, it is unlikely that merit aid programs were systematically implemented as part of broad and wide-reaching changes in agendas.

[27]Within merit aid states, there are no observations of a state with split legislature and a governor from a non-major party.

5.2 Trends in Migration Patterns

The introduction of state-wide merit aid programs has the potential to influence local and state funding for education through changes in migration patterns. Change in school age population and income distribution of families can affect state funding for K-12 education through the state funding formula. Change in a state's age distribution and population composition could influence parents' willingness to direct resources to K-12 education through property taxes, which would affect local revenue. Families of high school-aged individuals and younger children (who will one day be high-school aged) could be motivated to move to a merit aid state to take advantage of a merit aid program in the future. Older, college-aged students could be motivated to move to merit-aid implementing states and attend institutions which offer merit aid. If such changes in migration did happen following the merit aid programs, then demographic and socioeconomic compositional changes could have contributed to some of the results above. To investigate the role of migration as a contributor, we estimate equations of the form of equation 4, where our dependent variables are (separately) total population, college-aged (defined as all individuals ages 18-44) population, young (ages 5-15) population, and the high-school graduating population (15-20). Results of this exercise are shown figure 8. We see no economically (or statistically) significant effects on the population of any age group, suggesting that the effects we find of these programs on funding are not generated through differential migration to (or from) merit aid states following program implementation.

6 Conclusion

There is no study so far that analyzes these ripple effects of merit aid programs; this paper aims at addressing this gap in our understanding. Previous literature has explored multiple aspects related to the academic and economic consequences of merit aid programs including postsecondary enrollment, persistence and completion, migration of students, choice of colleges and majors. But one potential consequence of these programs that has been unexplored thus far is their impact on K-12 funding and resource allocation. There are multiple ways in which the introduction of merit aid programs, particularly ones that are significant in size and generosity, can affect school funding. These not only include changes in state aid to local school districts due to higher spending on postsecondary education but also local community responses because of their incentives to spur increased college-going through higher rates of academic achievement of students. Both changes in intergovernmental aid and school district responses to changes in state aid have the potential to significantly affect educational outcomes, particularly among K-12 students.

To understand these effects, we used two different estimation strategies: first, we used a standard difference-in-differences model and second, we examined state-specific effects, overall effects, and the time paths of these treatment effects, on funding using synthetic controls. In both cases, we found that local revenue increased while state and total revenue fell. Our findings reveal a significant rise in state support for higher education in merit aid states following their implementation, as we would expect. In response, state funding for elementary and secondary education fell, underlining a potential trade-off in the face of limited state resources and competing priorities. There is evidence that K-12 school districts to some extent made up for this decline in state aid by raising property taxes and local revenues. Examining patterns of resource allocation, a small decline in the share of instructional expenditures was offset by a small increase in pupil support expenditures, while the share of administrative expenditures remained essentially at the same level or slightly increased indicating that the incentives and families and school districts may not always be similarly aligned. In states with strong merit aid programs, the effects on state funding for K-12 education were even more pronounced: state revenue per student declined significantly more in strong states than it did in weak merit states. We see an increase in local revenue per student in the strong states that is slightly less than the increase we observe for all states that instituted merit aid programs but the difference is not statistically different from zero.

These results have important implications. To the extent that the main rationale for merit aid programs is to improve postsecondary education, educators and policy makers should be aware of unintended consequences that might undercut the positive benefits. Both the graying of the population and potential growth in college enrollment, the latter bolstered by the increasing importance of postsecondary credential in today's economy, will be placing greater demands on state resources in the years to come. Our findings suggest that local school districts fiscally respond to incentives from higher levels of governments, but there may be a limit to their resilience. This in turn might hamper achievement and college-readiness at the K-12 level, with adverse implications for future educational attainment and economic growth.

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Figure 1: Mapping Roll-Out Of Merit Aid Programs Across States

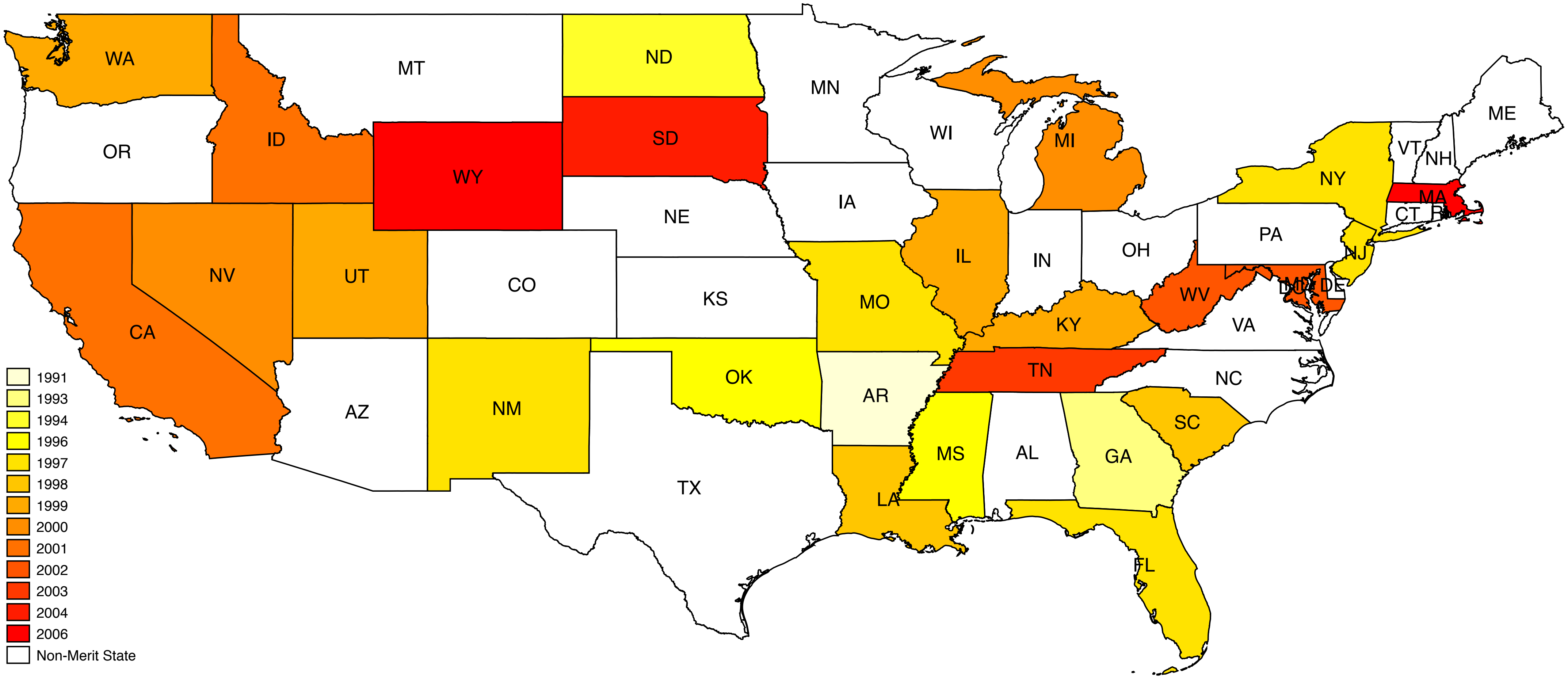


Figure 2: Average Treatment Effects by State and Outcome

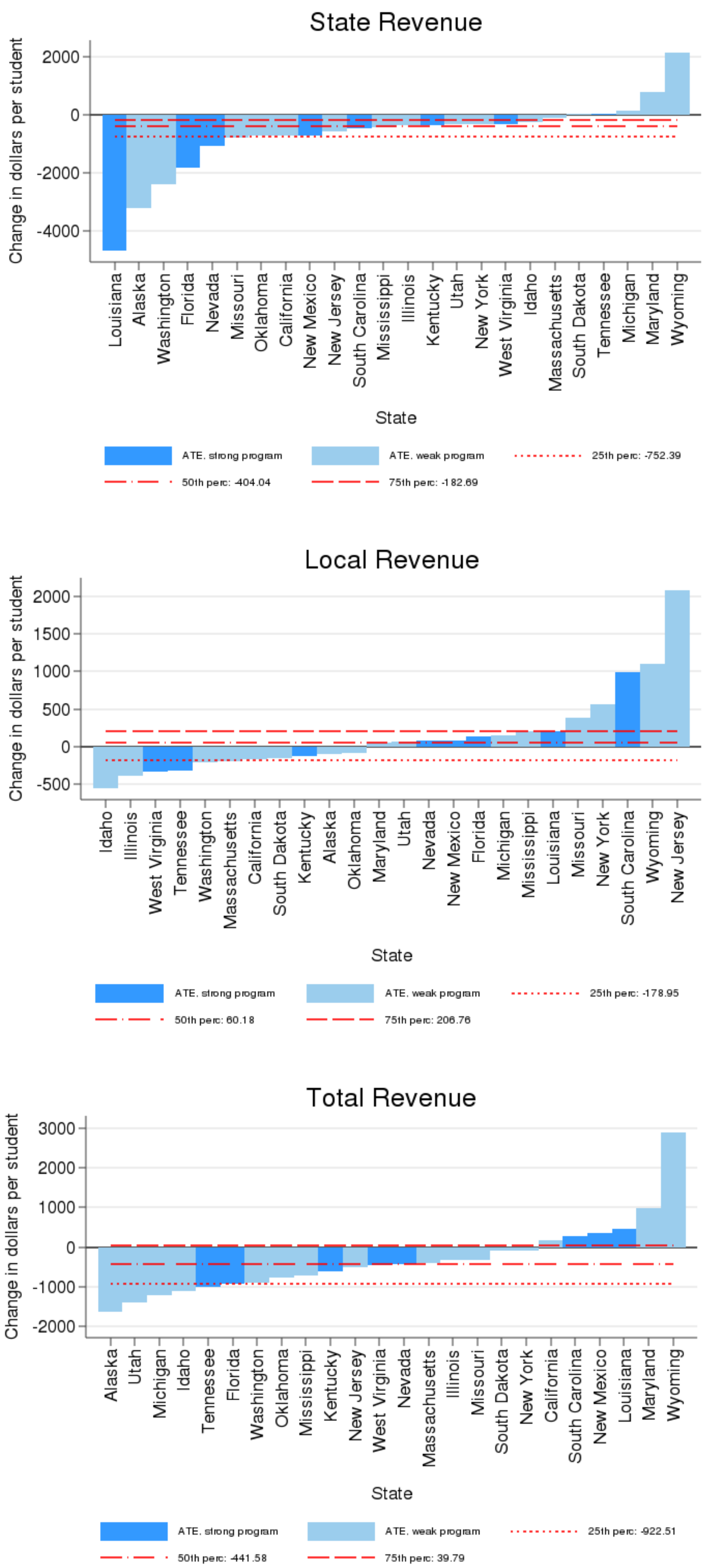


Figure 3: State Revenue

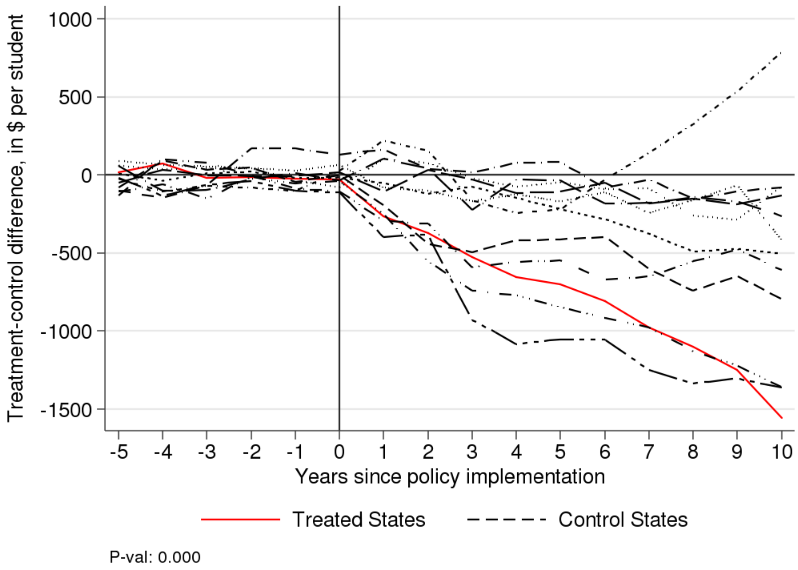


Figure 4: Local Revenue

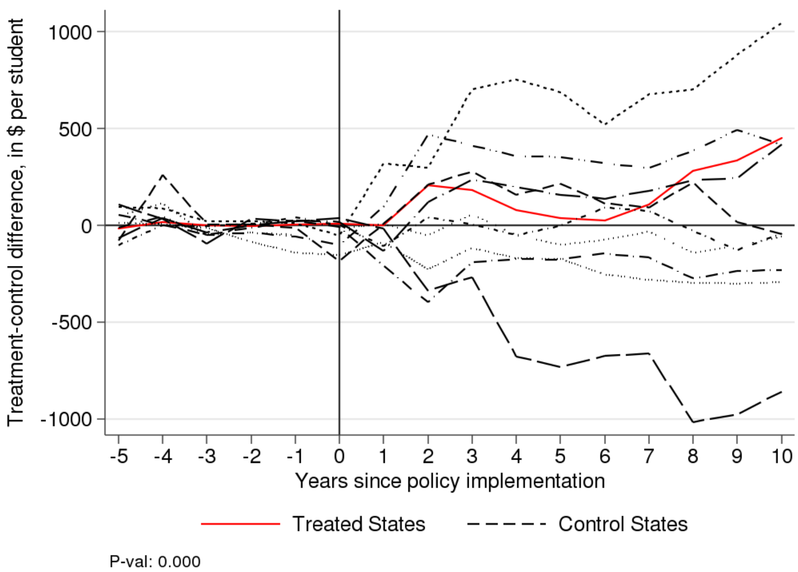


Figure 5: Total Revenue

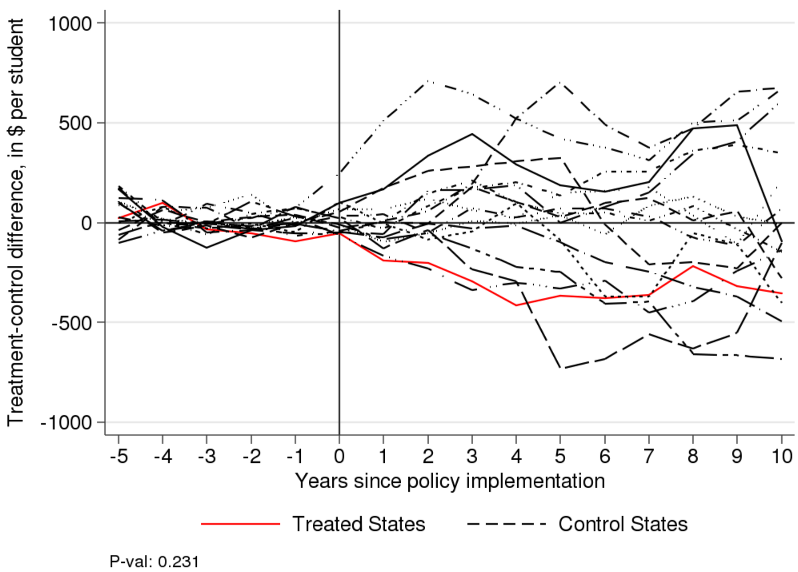
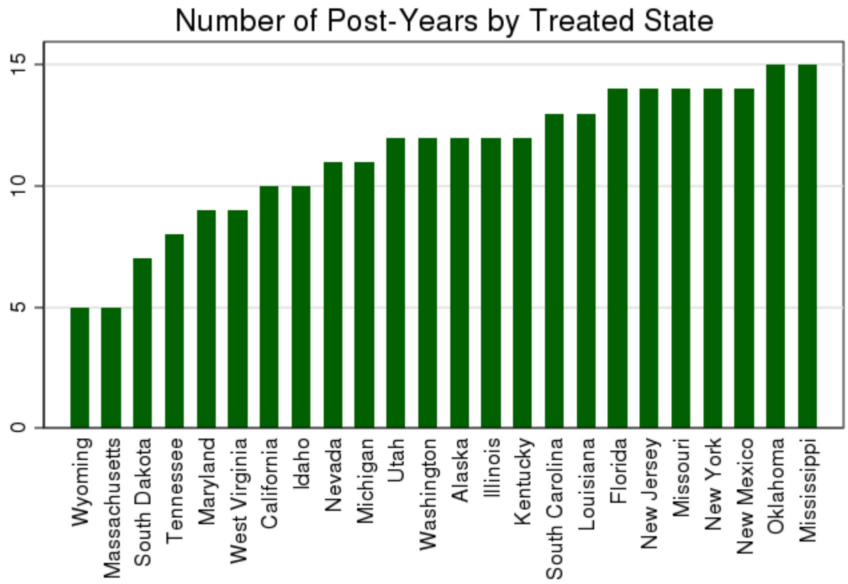


Figure 6: Number of Post-Treatment Periods Observed



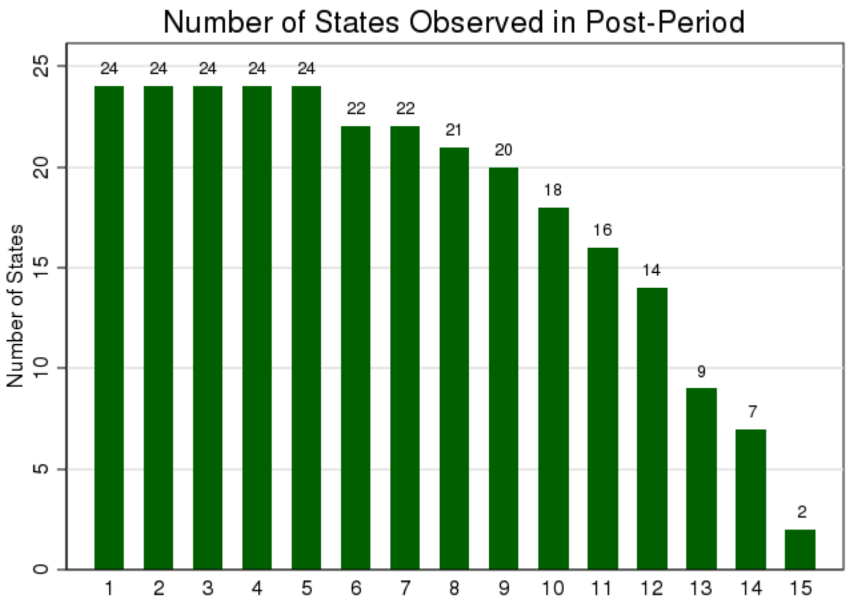


Figure 7: Robustness: Trends in Macrovariables

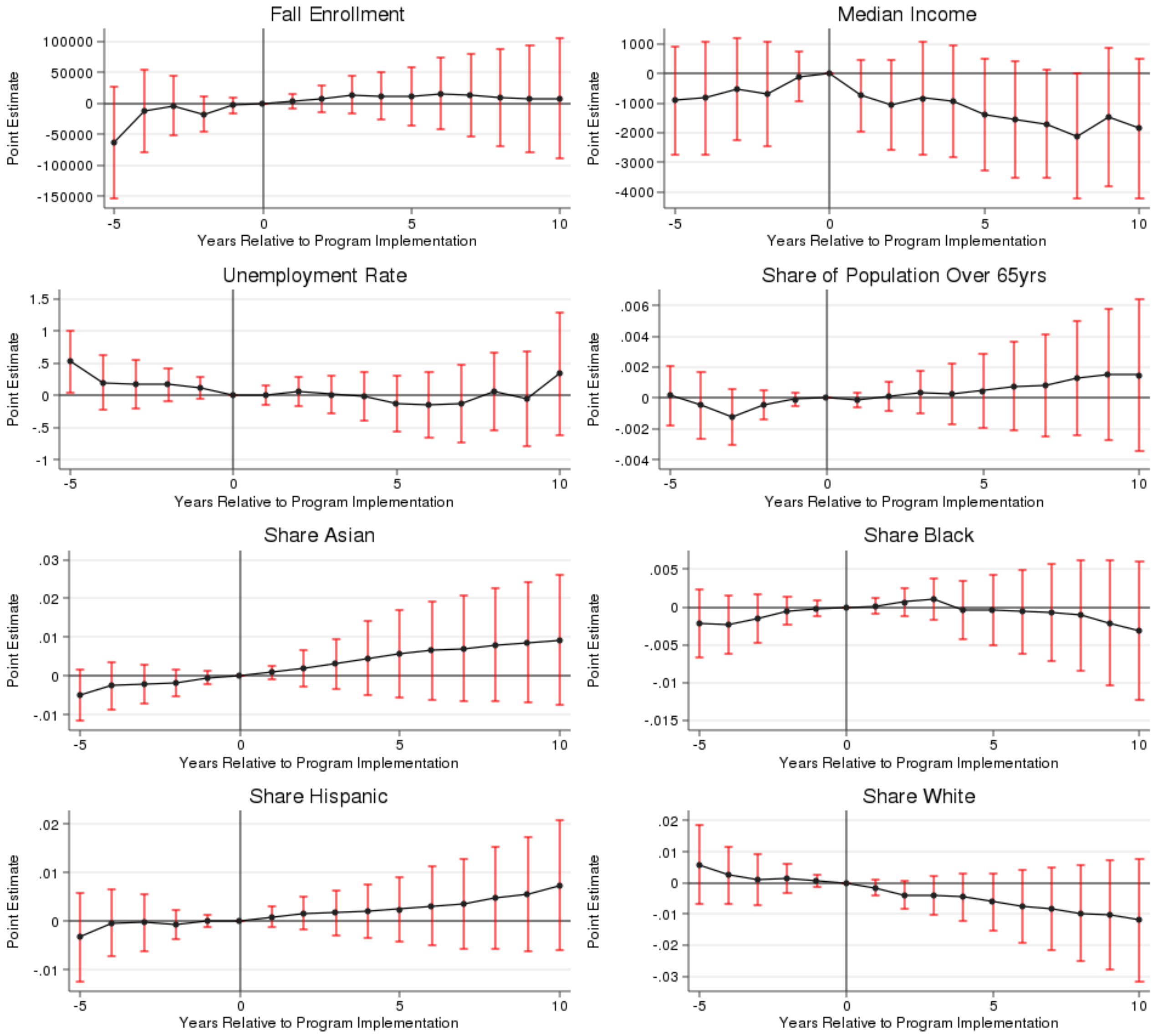


Figure 8: Robustness: Trends in Migration

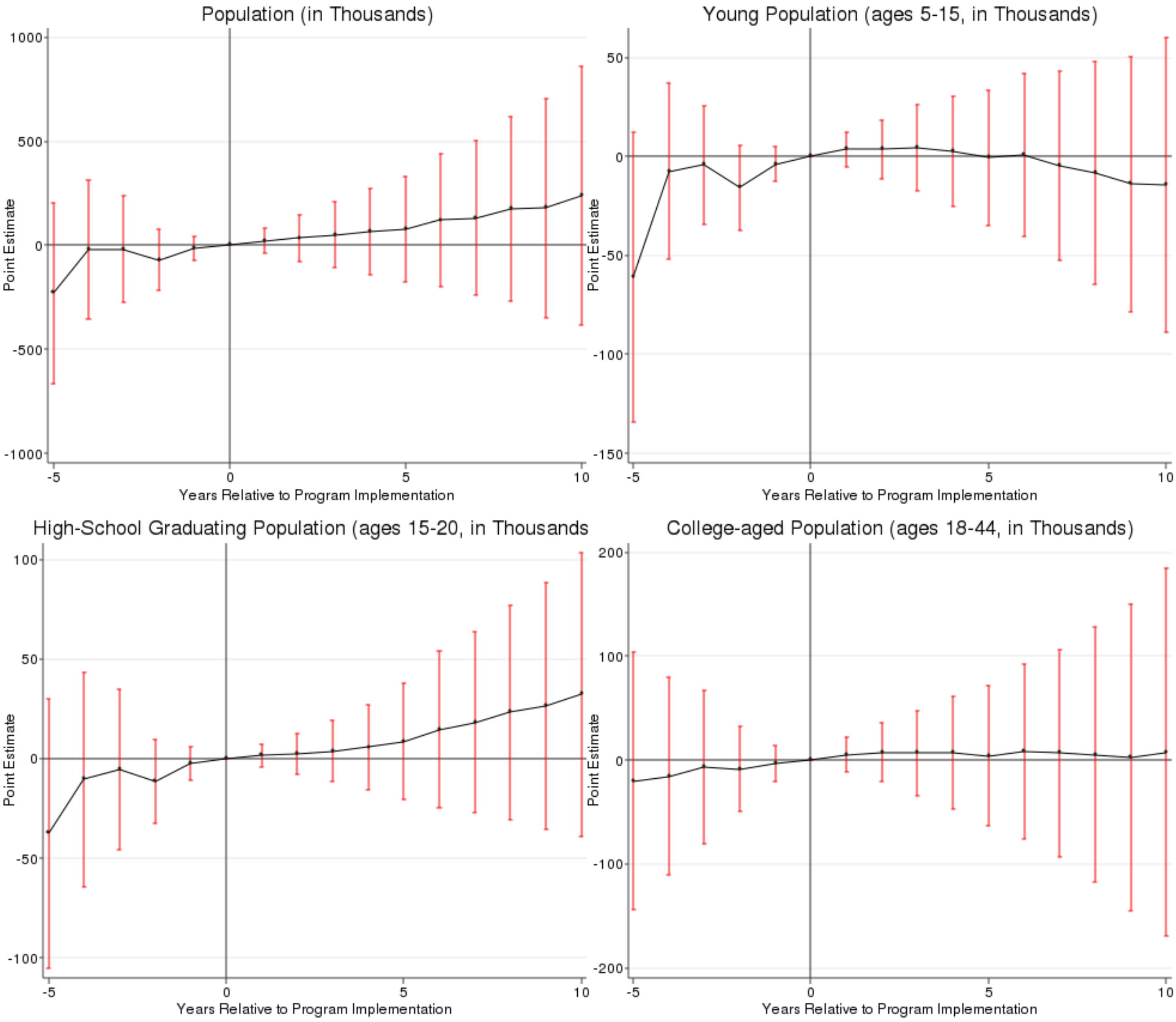


Table 1: States with Merit Aid Programs, 1990-2010

State	Implementation	Scholarship Name	Strong	Lottery	Broad
Arkansas	1991	Arkansas Academic Challenge Scholarship	No	No	Yes
Alaska	1999	Alaska Scholars	No	No	No
California	2001	Competitive Cal Grant Program	No	No	No
Florida	1997	Florida Bright Futures Scholarship	Yes	Yes	Yes
Georgia	1993	Georgia HOPE Scholarship	Yes	Yes	Yes
Idaho	2001	Robert R. Lee Promise Scholarship	No	No	No
Illinois	1999-2004	Illinois Merit Recognition Scholarship	No	No	No
Kentucky	1999	Kentucky Educational Excellence Scholarship	Yes	Yes	Yes
Louisiana	1998	Louisiana TOPS Scholarship	Yes	No	Yes
Maryland	2002-2005	Maryland HOPE Scholarship	No	No	Yes
Massachusetts	2006	John and Abigail Adams Scholarship	No	No	No
Michigan	2000-2008	Michigan Merit and Promise Scholarship	No	No	Yes
Mississippi	1996	Mississippi TAG and ESG	No	No	Yes
Missouri	1997	Missouri Bright Flight Scholarship	No	No	No
Nevada	2000	Nevada Millennium Scholarship	Yes	No	Yes
New Jersey	1997 (2004)	New Jersey OSRP (STARS)	No	No	No
New Mexico	1997	New Mexico Lottery Success Scholarship	Yes	Yes	Yes
New York	1997	NY Scholarships for Academic Excellence	No	No	No
North Dakota	1994	North Dakota Scholars Program	No	No	No
Oklahoma	1996	Oklahoma PROMISE Scholarship	No	No	No
South Carolina	1998	South Carolina LIFE Scholarship	Yes	Yes	Yes
South Dakota	2004	South Dakota Opportunity Scholarship	No	No	Yes
Tennessee	2003	Tennessee HOPE Scholarship	Yes	Yes	Yes
Utah	1999	New Century Scholarship	No	No	No
Washington	1999-2006	Washington PROMISE Scholarship	No	No	No
West Virginia	2002	West Virginia PROMISE Scholarship	Yes	Yes	Yes
Wyoming	2006	Hathaway Scholarship	No	No	Yes

The classification of merit aid states into strong and broad programs is borrowed from Sjoquist and Winters (2014).

Table 2: Institutional Details of State Merit Aid Programs

State	Lottery Program	Strong Program	Broad Program	Program Discontinued
Florida	x	x	x	-
Georgia	x	x	x	-
Kentucky	x	x	x	-
Louisiana	-	x	x	-
Nevada	-	x	x	-
New Mexico	x	x	x	-
South Carolina	x	x	x	-
Tennessee	x	x	x	-
West Virginia	x	x	x	-
Michigan	-	-	x	x
Illinois	-	-	-	x
Maryland	-	-	x	x
Washington	-	-	-	x
Wyoming	-	-	x	-
Mississippi	-	-	x	-
Michigan	-	-	x	-
Arkansas	-	-	x	-

States under ‘Program Discontinued’ are those that started a merit aid program, then ended it within our sample period. Table includes only merit aid states that fall into at least one of these four categories (strong, lottery, broad, or program discontinued). The classification of merit aid states into strong and broad programs is borrowed from Sjoquist and Winters (2014).

Table 3: Summary Statistics in Base Year (1989)

	Merit States		Non-Merit State		All States	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
State Revenue per Student	3823.53	2680.27	3495.91	2130.91	3663.33	2432.68
Local Revenue per Student	4470.28	4686.70	4970.87	3974.68	4715.17	4359.99
Property Tax Revenue per Student	3380.04	3957.74	3891.56	3123.00	3628.89	3585.06
Total Revenue per Student	8816.28	5089.17	8874.49	4771.77	8844.76	4936.36
State Support for Public and Independent Higher Education per Student	9560.13	1843.29	8423.71	2239.72	8940.35	2150.01
State Share of White Residents	0.77	0.11	0.86	0.12	0.81	0.12
State Share of Hispanic Residents	0.08	0.08	0.06	0.08	0.07	0.08
State Share of Black Residents	0.11	0.07	0.06	0.05	0.09	0.07
State Share of Asian Residents	0.03	0.03	0.01	0.01	0.02	0.02
State Share of Residents over the age of 65	0.12	0.02	0.13	0.02	0.13	0.02
State Total Population (in thousands)	9997.49	8832.11	6075.40	5428.21	8058.14	7605.68
State Median Income	53650.96	9165.34	50787.86	6131.20	52235.25	7943.53
State Unemployment Rate	5.55	1.03	4.79	1.19	5.17	1.18
District Fall Enrollment	3106.36	15559.03	2221.43	6641.74	2673.41	12058.76
District Share of White Residents	0.81	0.24	0.88	0.20	0.85	0.22
District Share of Hispanic Residents	0.07	0.15	0.06	0.16	0.07	0.15
District Share of Black Residents	0.08	0.17	0.04	0.11	0.06	0.15
District Share of Asian Residents	0.02	0.04	0.01	0.01	0.01	0.04

Sources: National Center for Education Statistics, State Higher Education Executive Officers, and Census. All nominal variables have been deflated using the 1997 BLS Price Index for Education Services. State-level variables are weighted by the number of districts in each state.

Table 4: Did State Merit Aid Programs Affect K-12 School Funding?
Impact of Policy on State Revenues per Student per Student, Local Revenues, Property Tax Revenues per Student, and Total Revenues per Student

	No Controls		State Controls		All Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: State Revenue per Student						
Merit	-144.3*	-144.3***	-184.9***	-184.9***	-195.7***	-195.7***
	(58.16)	(25.63)	(44.71)	(21.73)	(43.81)	(21.16)
Observations	284687	284687	284687	284687	278485	278485
R^2	0.603	0.603	0.611	0.611	0.648	0.648
Clusters	County	District	County	District	County	District
Panel B: Local Revenue per Student						
Merit	238.6***	238.6***	112.9*	112.9***	114.8**	114.8***
	(54.62)	(26.49)	(44.10)	(21.91)	(43.86)	(21.53)
Observations	283881	283881	283881	283881	277653	277653
R^2	0.719	0.719	0.722	0.722	0.810	0.810
Clusters	County	District	County	District	County	District
Panel C: Property Tax Revenue per Student						
Merit	134.6**	134.6***	52.01	52.01**	31.60	31.60
	(45.34)	(21.54)	(41.83)	(18.62)	(41.18)	(17.10)
Observations	240712	240712	240712	240712	236130	236130
R^2	0.705	0.705	0.707	0.707	0.847	0.847
Clusters	County	District	County	District	County	District
Panel D: Total Revenue per Student						
Merit	166.3**	166.3***	-16.97	-16.97	-22.37	-22.37
	(53.97)	(44.54)	(48.86)	(35.82)	(49.87)	(36.12)
Observations	285376	285376	285376	285376	279119	279119
R^2	0.653	0.653	0.655	0.655	0.730	0.730
Clusters	County	District	County	District	County	District

Please see equation 1 in the text. Each regression includes district and year fixed effects and, where indicated, state and district-level controls. State-level controls include population, median income, unemployment rate, share of elderly residents and shares of residents belonging to various races. District-level controls include district enrollment and shares of students belonging to various races. Standard errors are clustered at either the county or district level, as indicated. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 5: Synthetic Control Comparisons

Variable	Untreated States	Treated States	Synthetic Control States
State Revenue per Student			
1989	3,450.62	4,005.62	3,794.46
1991	3,253.08	3,664.49	3,617.41
1994	3,180.77	3,399.34	3,442.94
1995	3,194.42	3,478.24	3,452.50
1996	3,223.88	3,535.70	3,495.98
1997	3,294.52	3,576.20	3,512.31
1998	3,636.53	3,728.44	3,764.06
1999	4,054.28	3,745.77	3,877.71
2000	4,238.08	3,900.25	4,011.28⋆
2001	4,289.00	3,873.82	3,884.83
2002	4,352.66	3,767.73	3,749.69
2003	4,379.21	4,143.47	4,175.45
2004	4,553.36	5,709.07	5,238.20
2005	4,830.96⋆	5,491.03	5,663.26
Local Revenue per Student			
1989	4,713.12	3,546.62	3,610.63
1991	4,480.30	3,327.01	3,331.84
1994	3,918.94	3,067.07	3,022.82
1995	3,866.62	2,941.34	2,948.85
1996	3,901.65	3,045.22	3,085.21
1997	3,865.00	2,867.52	2,828.69
1998	3,785.13	2,977.55	2,986.87
1999	3,811.13	3,405.87	3,369.71
2000	4,156.67	3,539.34	3,570.58
2001	4,239.35	4,031.82	4,045.37
2002	4,231.91	4,140.52	4,160.55
2003	4,477.31	4,628.71	4,727.93
2004	4,585.34	5,562.01	5,472.57
2005	4,746.48	6,088.67	5,665.22
Total Revenue per Student			
1989	8,638.16	8,130.04	7,968.20
1991	8,222.48	7,549.35	7,553.37
1994	7,539.67	6,976.52	7,006.38
1995	7,484.68	6,914.76	6,920.59
1996	7,558.05	7,082.52	7,237.03⋆
1997	7,646.63	6,979.34	7,070.24
1998	7,943.36	7,272.69	7,494.80⋆
1999	8,465.75	7,731.14	7,945.59
2000	8,990.59	8,086.83	8,198.35
2001	9,201.23	8,643.93	8,703.66
2002	9,319.55	8,739.19	8,825.41
2003	9,675.17	9,741.68	9,906.54
2004	9,992.09**	12,201.82	11,726.07
2005	10,418.25	12,525.85	12,512.04

Asterisks in the synthetic column indicate statistically significant differences between treated and synthetic states, while asterisks in the untreated column indicate differences between treated and untreated states. Treated states include only those for which we compute a synthetic counterpart (we exclude Georgia, Arkansas, and North Dakota for which we have too few pre-treatment years).

Table 6: Overall Average Treatment Effects

	Unweighted (1)	Weighted by Inverse RMSPE (2)	Weighted by Base Enrollment (3)
State Revenue	-701.32***	-687.40***	-607.01***
	(28.10)	(58.67)	(113.69)
Local Revenue	205.36***	413.60***	271.68
	(18.32)	(30.65)	(145.45)
Total Revenue	-308.30***	-383.61***	-155.68
	(22.89)	(23.27)	(152.91)

Table 7: Overall Average Treatment Effects: 3 year window around implementation

	Unweighted (1)	Weighted by Inverse RMSPE (2)	Weighted by Base Enrollment (3)
State Revenue	-329.78***	-159.26***	-253.18***
	(14.70	(27.43)	(38.23)
Local Revenue	160.79***	227.18***	204.65***
	(13.63)	(32.18)	(65.02)
Total Revenue	-210.20***	-236.90***	-159.87***
	(10.13	(13.99)	(41.085)

Table 8: Overall Average Treatment Effects: 5 year window around implementation

	Unweighted (1)	Weighted by Inverse RMSPE (2)	Weighted by Base Enrollment (3)
State Revenue	-463.69***	-313.24***	-365.42***
	(16.91)	(37.60)	(53.40)
Local Revenue	111.81***	299.90***	59.72
	(12.78)	(32.56)	(88.84)
Total Revenue	-301.40***	-309.98***	-240.81***
	(15.20)	(16.40)	(47.42)

Table 9: Overall Average Treatment Effects: 6 year window around implementation

	Unweighted (1)	Weighted by Inverse RMSPE (2)	Weighted by Base Enrollment (3)
State Revenue	-515.98***	-383.46***	-437***
	(18.158)	(43.32)	(65.98)
Local Revenue	101.81***	314.43***	33.84
	(12.736)	(30.021)	(82.861)
Total Revenue	-319.91***	-328.4555***	-259.04***
	(16.82)	(17.732)	(49.68)

Table 10: Leave-One-Out Treatment Effects

State	State Revenue			Local Revenue			Total Revenue		
Weighting	Unweighted	Inverse RMSPE	Enrollment	Unweighted	Inverse RMSPE	Enrollment	Unweighted	Inverse RMSPE	Enrollment
Overall ATE	-329.78***	-159.27 ***	-253.19***	160.79***	227.19***	204.65**	-210.21***	-236.91***	-159.88**
	(14.71)	(27.44)	(38.23)	(13.64)	(32.19)	(65.03)	(10.13)	(14.00)	(41.09)
State Left Out									
Alaska	-247.65***	-159.27 ***	-229.91***	166.54***	227.19***	206.74**	-152.80***	-231.79***	-143.65**
	(11.34)	(28.03)	(38.23)	(14.51)	(32.88)	(66.46)	(10.48)	(14.35)	(41.95)
California	-322.70***	-159.27 ***	-202.47***	161.17***	227.19***	215.79**	-241.01***	-253.63***	-299.27**
	(15.59)	(28.03)	(38.99)	(14.53)	(32.88)	(75.62)	(10.46)	(14.55)	(33.70)
Florida	-327.96***	-159.27 ***	-242.78***	166.73***	234.20***	220.48**	-197.06***	-229.58***	-128.92**
	(15.11)	(28.03)	(33.36)	(14.54)	(34.02)	(68.16)	(10.40)	(14.57)	(40.22)
Idaho	-331.40***	-159.27 ***	-252.93***	171.11***	227.19***	206.48**	-202.66***	-234.70***	-158.42**
	(15.61)	(28.03)	(39.06)	(14.53)	(32.88)	(66.44)	(10.79)	(14.51)	(41.98)
Illinois	-343.62***	-187.84 ***	-285.55***	167.46***	227.19***	231.07***	-216.46***	-259.80***	-172.40***
	(15.68)	(33.44)	(41.33)	(14.51)	(32.88)	(69.91)	(10.49)	(14.17)	(39.46)
Kentucky	-325.85***	-159.27 ***	-248.69***	168.54***	227.19***	210.62**	-196.53***	-232.73***	-150.06**
	(15.67)	(28.03)	(39.15)	(14.53)	(32.88)	(66.60)	(10.66)	(14.49)	(41.98)
Louisiana	-193.44***	-159.27 ***	-175.26***	165.27***	227.19***	208.21**	-209.74***	-237.28***	-158.40**
	(15.54)	(28.03)	(39.01)	(14.53)	(32.88)	(66.57)	(10.68)	(14.61)	(41.99)
Maryland	-346.83***	-159.27 ***	-263.24***	171.20***	227.19***	213.68**	-225.01***	-250.11***	-169.13**
	(15.66)	(28.03)	(39.18)	(14.53)	(32.88)	(66.66)	(10.55)	(14.69)	(41.88)
Massachusetts	-341.04***	-159.27 ***	-256.05***	164.00***	227.19***	206.50**	-209.28***	-237.01***	-158.75**
	(15.52)	(28.03)	(39.03)	(14.15)	(32.88)	(66.41)	(10.57)	(14.53)	(41.95)
Michigan	-371.99***	-159.27 ***	-302.50***	165.49***	227.19***	213.03**	-209.28***	-237.05***	-155.93**
	(15.63)	(28.03)	(39.27)	(14.52)	(32.88)	(67.11)	(10.61)	(14.62)	(41.88)
Mississippi	-331.14***	-100.28 **	-252.75***	162.81***	227.19***	205.52**	-178.46***	-227.35***	-152.35**
	(15.63)	(37.90)	(39.06)	(14.53)	(32.88)	(66.45)	(10.78)	(14.49)	(41.98)
Missouri	-333.93***	-159.20 ***	-253.43***	157.60***	24.26***	204.27**	-207.67***	-228.16***	-158.49**
	(15.45)	(28.05)	(39.02)	(14.46)	(3.59)	(66.46)	(10.65)	(14.33)	(41.97)
Nevada	-303.46***	-159.27 ***	-230.68***	169.13***	227.19***	212.42**	-200.82***	-224.17***	-151.09**
	(15.62)	(28.03)	(39.11)	(14.50)	(32.88)	(66.65)	(10.79)	(15.25)	(42.13)
New Jersey	-343.94***	-159.27 ***	-256.69***	101.44***	227.19***	186.05**	-232.49***	-239.64***	-166.38**
	(15.65)	(28.03)	(39.08)	(6.68)	(32.88)	(65.48)	(10.60)	(14.37)	(41.96)
New Mexico	-346.96***	-159.27 ***	-259.93***	164.28***	227.19***	207.28**	-204.89***	-235.28***	-156.22**
	(15.57)	(28.03)	(39.04)	(14.53)	(32.88)	(66.53)	(10.78)	(14.54)	(42.03)
New York	-344.68***	-159.27 ***	-348.16***	144.20***	227.19***	84.03***	-207.58***	-236.31***	-120.32***
	(15.62)	(28.03)	(45.85)	(14.02)	(32.88)	(23.52)	(10.78)	(14.55)	(59.29)
Oklahoma	-338.61***	-197.40 ***	-254.69***	176.71***	227.19***	209.52**	-196.13***	-207.73***	-155.44**
	(15.53)	(19.08)	(39.04)	(14.52)	(32.88)	(66.46)	(10.53)	(14.73)	(41.95)
South Carolina	-336.84***	-159.27 ***	-254.41***	150.34***	227.19***	201.85**	-217.26***	-247.52***	-161.47**
	(15.55)	(28.03)	(39.04)	(14.49)	(32.88)	(66.47)	(10.31)	(14.52)	(41.90)
South Dakota	-344.00***	-159.27 ***	-254.30***	170.14***	227.19***	205.80**	-213.31***	-242.87***	-159.97**
	(15.68)	(28.03)	(39.06)	(14.50)	(32.88)	(66.43)	(10.63)	(14.94)	(41.97)
Tennessee	-342.98***	-159.27 ***	-260.25***	173.30***	227.19***	214.97**	-190.37***	-231.67***	-144.10**
	(15.67)	(28.03)	(39.18)	(14.51)	(32.88)	(66.64)	(10.47)	(14.45)	(41.80)
Utah	-335.13***	-159.27 ***	-254.23***	162.08***	227.19***	206.31**	-171.79***	-230.45***	-138.85**
	(15.68)	(28.03)	(39.13)	(14.54)	(32.88)	(66.55)	(10.57)	(14.40)	(41.93)
Washington	-286.68***	-159.27 ***	-240.48***	168.44***	227.19***	207.26**	-200.62***	-211.62***	-156.65**
	(14.75)	(28.03)	(38.87)	(14.53)	(32.88)	(66.46)	(10.69)	(15.48)	(41.98)
West Virginia	-338.86***	-159.27 ***	-254.51***	178.71***	227.19***	209.23**	-213.88***	-239.24***	-160.22**
	(15.67)	(28.03)	(39.07)	(14.50)	(32.88)	(66.45)	(10.73)	(14.58)	(41.98)
Wyoming	-435.11***	-159.27 ***	-262.00***	112.36***	227.19***	200.63**	-349.90***	-276.31***	-171.76**
	(14.36)	(28.03)	(39.03)	(14.32)	(32.88)	(66.43)	(9.63)	(14.40)	(41.95)

These reflect averages over the three years following implementation.

Table 11: Leave-One-Control State-Out Treatment Effects

State	State Revenue			Local Revenue			Total Revenue		
Weighting	Unweighted	Inverse RMSPE	Enrollment	Unweighted	Inverse RMSPE	Enrollment	Unweighted	Inverse RMSPE	Enrollment
Overall ATE	-329.78***	-159.27 ***	-253.19***	160.79***	227.19***	204.65**	-210.21***	-236.91***	-159.88**
	(14.71)	(27.44)	(38.23)	(13.64)	(32.19)	(65.03)	(10.13)	(14.00)	(41.09)
Control State Left Out									
Alabama	-329.21***	-140.68 ***	-253.32***	194.71***	166.95***	244.37**	-226.46***	-188.29***	-171.68**
	(14.71)	(19.65)	(38.24)	(13.83)	(21.38)	(66.42)	(9.50)	(15.49)	(41.06)
Arizona	-329.03***	-198.50 ***	-252.87***	161.97***	248.32***	206.07**	-222.29***	-246.94***	-165.75**
	(14.70)	(29.89)	(38.23)	(13.64)	(33.64)	(65.03)	(10.15)	(14.17)	(41.06)
Colorado	-321.96***	-147.84 ***	-248.90***	159.08***	94.15***	203.98**	-214.26***	-241.60***	-161.02**
	(14.68)	(23.26)	(38.23)	(13.64)	(8.75)	(65.02)	(10.33)	(14.84)	(41.12)
Connecticut	-329.68***	-200.95 ***	-252.90***	163.06***	260.84***	205.71**	-327.36***	-265.15***	-309.90**
	(14.70)	(29.82)	(38.23)	(13.81)	(35.54)	(65.06)	(13.91)	(14.14)	(42.65)
Delaware	-357.37***	-276.94 ***	-277.16***	151.85***	161.72***	209.06**	-206.54***	-216.36***	-175.20**
	(14.82)	(26.84)	(38.25)	(13.64)	(21.95)	(65.46)	(10.04)	(14.03)	(41.20)
Hawaii	-272.84***	-291.54 ***	-149.69***	52.36***	203.88***	151.10*	-200.61***	-230.96***	-158.49*
	(11.91)	(28.95)	(27.91)	(13.65)	(28.86)	(64.99)	(9.89)	(13.80)	(41.18)
Indiana	-327.29***	-201.80 ***	-248.93***	172.93***	199.77***	205.67**	-214.97***	-247.85***	-171.30**
	(14.71)	(25.65)	(38.23)	(13.64)	(26.60)	(64.98)	(10.17)	(13.49)	(38.70)
Iowa	-328.96***	-143.80 ***	-252.98***	159.04***	250.14***	202.46**	-210.74***	-234.97***	-159.17**
	(14.71)	(24.88)	(38.24)	(13.64)	(32.80)	(65.05)	(10.14)	(14.07)	(41.10)
Kansas	-331.41***	-182.85 ***	-254.25***	160.96***	117.58***	203.97**	-211.83***	-239.16***	-160.05**
	(14.69)	(33.84)	(38.42)	(13.64)	(15.28)	(65.03)	(10.14)	(14.01)	(41.09)
Maine	-329.46***	-207.84 ***	-253.46***	163.02***	246.93***	206.77**	-210.21***	-236.91***	-159.88**
	(14.71)	(28.87)	(38.24)	(13.65)	(33.46)	(65.03)	(10.13)	(14.00)	(41.09)
Minnesota	-345.13***	-129.28 *	-254.05***	148.01***	53.34***	179.65***	-210.21***	-236.91***	-159.88***
	(15.22)	(46.49)	(38.28)	(13.34)	(5.31)	(44.86)	(10.13)	(14.00)	(41.09)
Montana	-329.68***	-192.69 ***	-253.52***	167.06***	144.51***	209.54**	-207.38***	-232.40***	-155.94**
	(14.72)	(28.18)	(38.23)	(13.65)	(15.62)	(65.03)	(10.11)	(14.14)	(41.01)
Nebraska	-317.71***	-285.13 ***	-249.68***	203.35***	247.05***	391.22*	-210.21***	-236.91***	-159.88*
	(14.62)	(27.90)	(38.21)	(17.93)	(29.87)	(154.80)	(10.13)	(14.00)	(41.09)
New Hampshire	-330.36***	-258.14 ***	-251.34***	166.37***	240.62***	204.71**	-191.86***	-229.16***	-152.61**
	(14.70)	(24.38)	(38.23)	(13.65)	(28.45)	(65.06)	(12.06)	(14.11)	(41.14)
North Carolina	-359.38***	-201.11 ***	-308.34***	159.78***	79.37***	204.87**	-200.65***	-216.88***	-189.51**
	(14.86)	(26.62)	(39.17)	(13.61)	(9.68)	(65.03)	(10.07)	(12.52)	(34.38)
Ohio	-328.90***	-154.24 ***	-253.13***	167.22***	198.23***	204.58**	-196.52***	-208.99***	-142.57**
	(14.71)	(30.05)	(38.23)	(13.78)	(21.65)	(65.07)	(9.89)	(11.27)	(37.73)
Oregon	-324.59***	-93.84 **	-249.46***	134.50***	88.83***	172.71*	-210.03***	-239.95***	-143.95*
	(15.13)	(30.28)	(38.60)	(13.69)	(4.72)	(65.03)	(9.68)	(13.92)	(39.26)
Pennsylvania	-330.37***	-196.31 ***	-260.72***	191.38***	254.99***	283.93***	-220.52***	-243.10***	-168.09***
	(15.34)	(29.16)	(38.36)	(13.76)	(34.09)	(67.09)	(10.17)	(14.46)	(40.93)
Rhode Island	-328.75***	-182.83 ***	-252.52***	160.15***	249.87***	204.42**	-199.53***	-231.74***	-154.61**
	(14.70)	(29.53)	(38.23)	(13.65)	(33.74)	(65.03)	(10.11)	(14.15)	(41.62)
Texas	-332.52***	-163.03 ***	-255.26***	161.67***	241.27***	205.19**	-211.38***	-233.06***	-160.16**
	(14.71)	(38.16)	(38.24)	(13.64)	(32.17)	(65.03)	(10.23)	(14.49)	(41.10)
Vermont	-198.19***	-188.74 ***	-182.62***	137.20***	188.71***	125.22**	-142.95***	-226.44***	-39.75**
	(16.78)	(21.92)	(38.88)	(17.04)	(25.79)	(43.23)	(9.97)	(14.20)	(41.19)
Virginia	-283.87***	-125.39 ***	-230.27***	162.04***	241.41***	205.12**	-209.34***	-236.80***	-156.22**
	(14.27)	(31.65)	(36.65)	(13.65)	(32.03)	(65.03)	(10.29)	(14.03)	(46.06)
Wisconsin	-319.59***	-172.22 ***	-247.55***	148.87***	226.22***	187.96**	-210.21***	-236.91***	-159.88**
	(14.67)	(23.58)	(38.23)	(12.15)	(31.56)	(57.84)	(10.13)	(14.00)	(41.09)

These reflect averages over the three years following implementation.

Table 12: Exogeneity of Program Implementation: Selection on Observables

	Treated vs. Untreated	Treated vs. Synthetic	Timing
	(1)	(2)	(3)
State Revenue per Student	-0.000383	-0.000306	0.0000512
	(0.000463)	(0.000456)	(0.0000521)
Local Revenue per Student	-0.000380	-0.000339	0.0000391
	(0.000440)	(0.000423)	(0.0000468)
Total Revenue per Student	0.000233	0.000132	-0.0000515
	(0.000416)	(0.000396)	(0.0000455)
Population (in Thousands)	0.000375**	0.000422**	-0.000000397
	(0.000112)	(0.000145)	(0.0000242)
Unemployment Rate	0.194**	0.235***	-0.00713
	(0.0666)	(0.0648)	(0.00976)
Fall Enrollment	-0.00000229**	-0.00000260**	8.20e-09
	(0.000000694)	(0.000000917)	(0.000000145)
Median Income	0.0000226	0.0000371*	0.00000251
	(0.0000134)	(0.0000154)	(0.00000232)
Share Population Over 65	-6.057	-2.271	0.194
	(4.629)	(5.004)	(0.982)
Share Hispanic	0.442	-0.874	0.00101
	(1.510)	(1.420)	(0.251)
Share Black	-0.0362	-1.447	0.0864
	(1.091)	(1.114)	(0.206)
Share Asian	-0.667	-2.531	-0.340
	(1.482)	(1.510)	(1.218)
Share White	0.137	-0.727	0.0277
	(1.152)	(1.216)	(0.214)
Observations	48	48	384

Please see equations 2 and 3 in the text. Standard errors in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 13: Exogeneity of Program Implementation: State Government Control

	(1)
	Fraction Merit Aid States
Democratic Governor	0.42
Republican Governor	0.58
Democratic Statehouse	0.46
Republican Statehouse	0.33
Split Statehouse	0.21
Democratic Governor and Statehouse	0.25
Democratic Governor and Republican Statehouse	0.12
Democratic Governor and Split Statehouse	0.04
Republican Governor and Democratic Statehouse	0.21
Republican Governor and Statehouse	0.21
Republican Governor and Split Statehouse	0.17
Other Party Governor and Democratic Statehouse	0.00
Other Party Governor and Republican Statehouse	0.00
Other Party Governor and Statehouse	0.00
Observations	24

Source: The Washington Post and the National Conference of State Legislatures

Table 14: Exogeneity of Program Implementation: State Government Control

	Governorship		Legislature		Governor and Legislature	
	(1)	(2)	(3)	(4)	(5)	(6)
Democratic Governor	-0.00605	-0.00382				
	(0.0212)	(0.0214)				
Other Party Governor	-0.0556	-0.0319				
	(0.159)	(0.166)				
Democratic Legislature			-0.0114	-0.00665		
			(0.0241)	(0.0277)		
Split Legislature			0.0156	0.0193		
			(0.0290)	(0.0300)		
Democratic Governor and Statehouse					-0.0194	-0.0130
					(0.0297)	(0.0324)
Democratic Governor and Republican Statehouse					0.00357	0.00942
					(0.0435)	(0.0442)
Democratic Governor and Split Statehouse					0.0264	0.0312
					(0.0382)	(0.0387)
Republican Governor and Democratic Statehouse					0.000776	0.00877
					(0.0316)	(0.0359)
Republican Governor and Split Statehouse					0.00893	0.0141
					(0.0387)	(0.0405)
Other Party Governor and Statehouse					-0.0536	-0.0199
					(0.160)	(0.168)
Controls	No	Yes	No	Yes	No	Yes
Observations	456	456	456	456	456	456
R^2	0.000	0.015	0.002	0.017	0.004	0.018

Please see equations 6, 5 and 7 in the text. Governor and State Legislature data is available only in even years. We fill in for odd years based by carrying forward from the previous even year to the following odd year. Results are robust to carrying backwards to the previous odd year, as well as to using only available years of data. Standard errors in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

A Appendix

Table A.1: Did State Merit Aid Programs Affect State Appropriations for Higher Education?

	No Controls		State Controls		All Controls	
	(1)	(2)	(3)	(4)	(5)	(6)
State Support for Public and Independent Higher Education per Pupil						
Merit	378.5***	378.5***	375.0***	375.0***	325.0***	325.0***
	(45.05)	(10.53)	(37.84)	(10.79)	(39.68)	(10.14)
Observations	186581	186581	186123	186123	182250	182250
R^2	0.942	0.942	0.953	0.953	0.954	0.954
Clusters	County	District	County	District	County	District

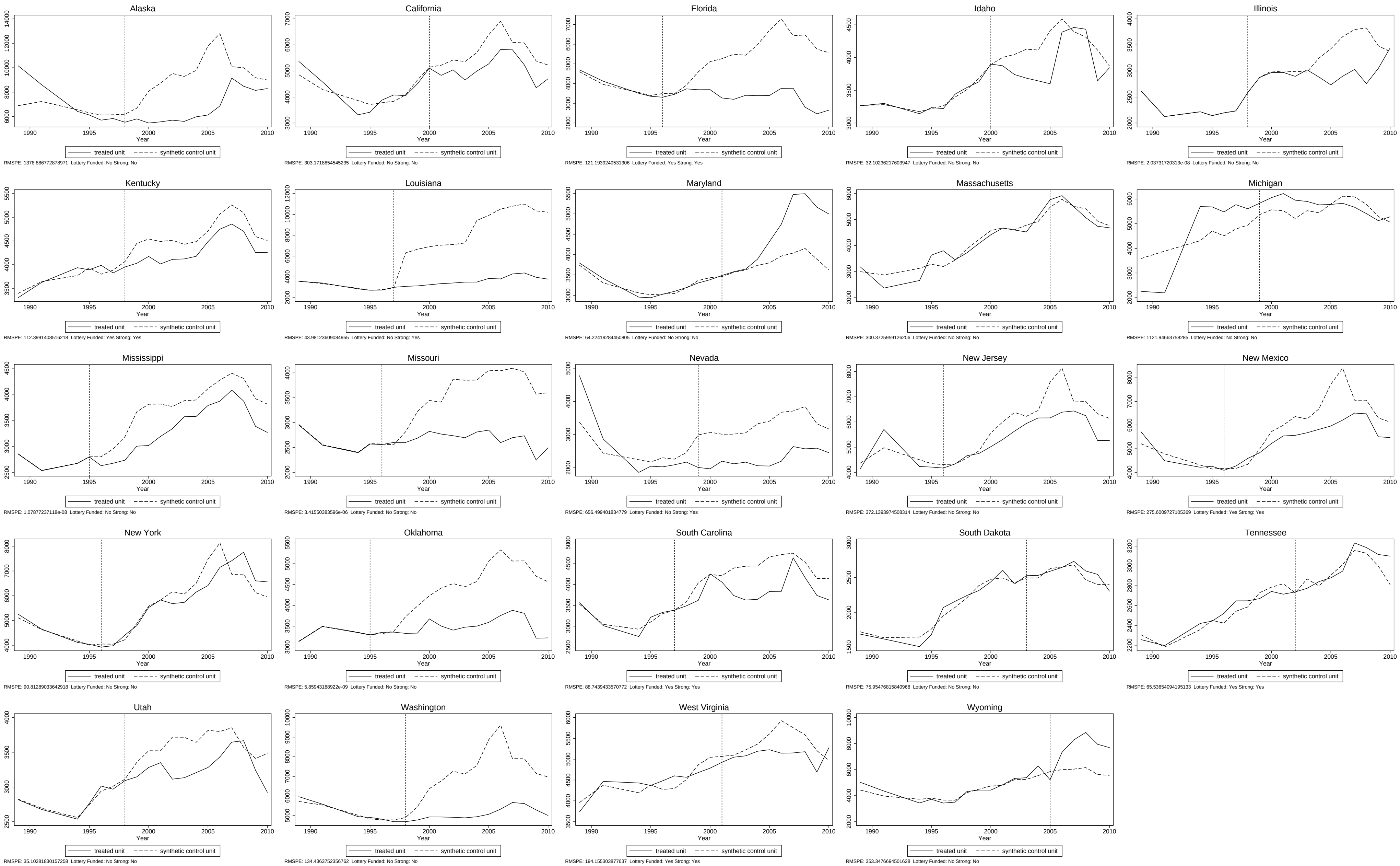
Please see equation 1 in the text. Each regression includes district and year fixed effects and both state and district-level controls. State-level controls include population, median income, unemployment rate, share of elderly residents and shares of residents belonging to various races. District-level controls include district enrollment and shares of students belonging to various races. Standard errors are clustered at either the county or district level, as indicated. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.2: Exogeneity of Program Implementation: State Government Control

	Democrats		Republicans		Democrats		Republicans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic Gov. in Implementation Year OR 1 OR 2 Years Prior	-0.00270	0.00450						
	(0.0211)	(0.0216)						
Republican Gov. in Implementation Year OR 1 OR 2 Years Prior			0.00583	0.00148				
			(0.0218)	(0.0223)				
Legislature	0.0130	0.0129	0.0130	0.0126	0.0130	0.0127	0.0134	0.0131
	(0.0134)	(0.0142)	(0.0133)	(0.0142)	(0.0134)	(0.0142)	(0.0133)	(0.0142)
Democratic Gov. in Implementation Year OR Year Prior					-0.00287	0.00132		
					(0.0211)	(0.0214)		
Republican Gov. in Implementation Year OR Year Prior							-0.00356	-0.00766
							(0.0214)	(0.0217)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	456	456	456	456	456	456	456	456
R^2	0.002	0.016	0.002	0.016	0.002	0.016	0.002	0.017

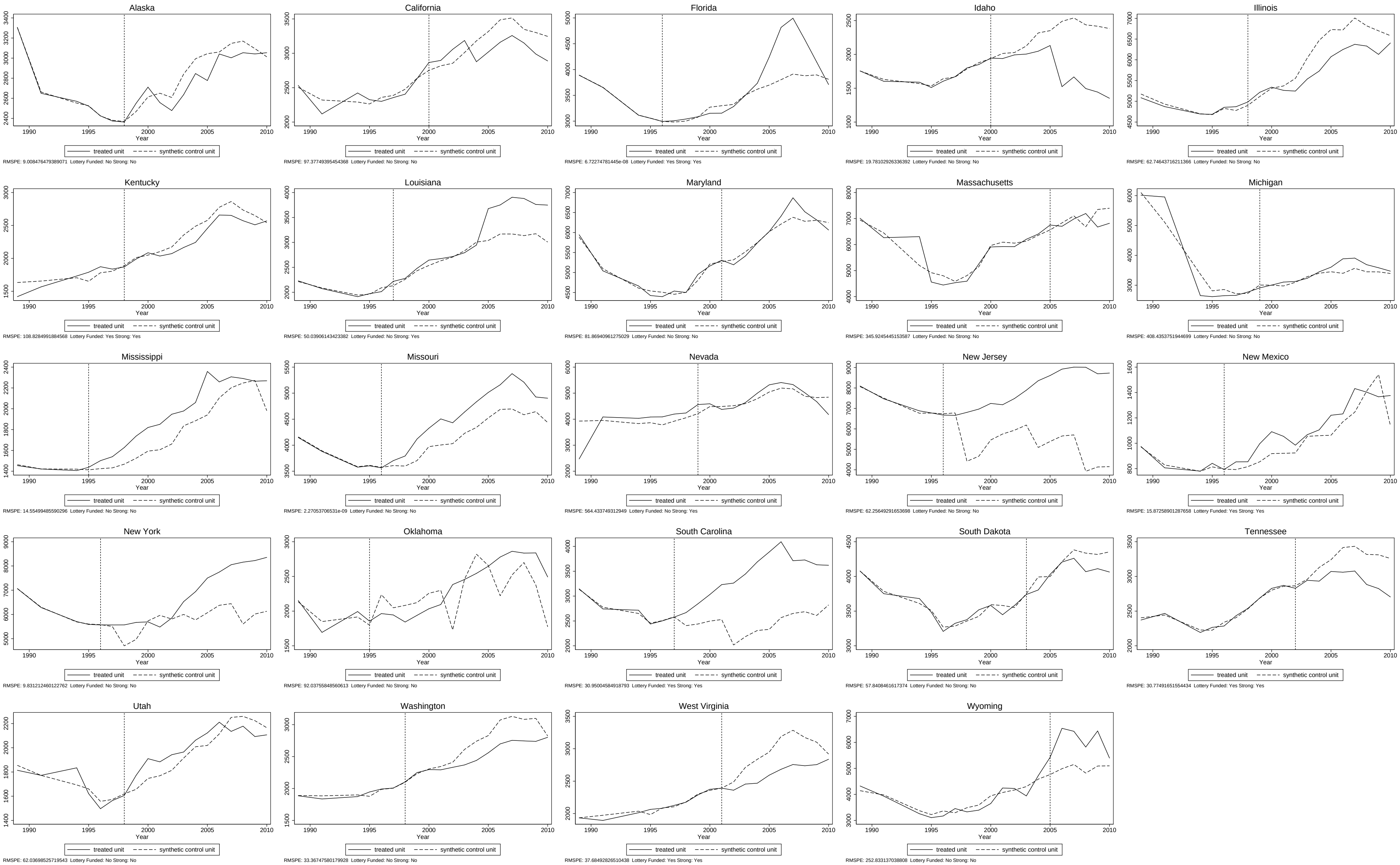
Please see equations 6, 5 and 7 in the text. Governor and State Legislature data is available only in even years. We fill in for odd years based by carrying forward from the previous even year to the following odd year. Results are robust to carrying backwards to the previous odd year, as well as to using only available years of data. Standard errors in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.1: State-Level Synthetic Control Results: State Revenue



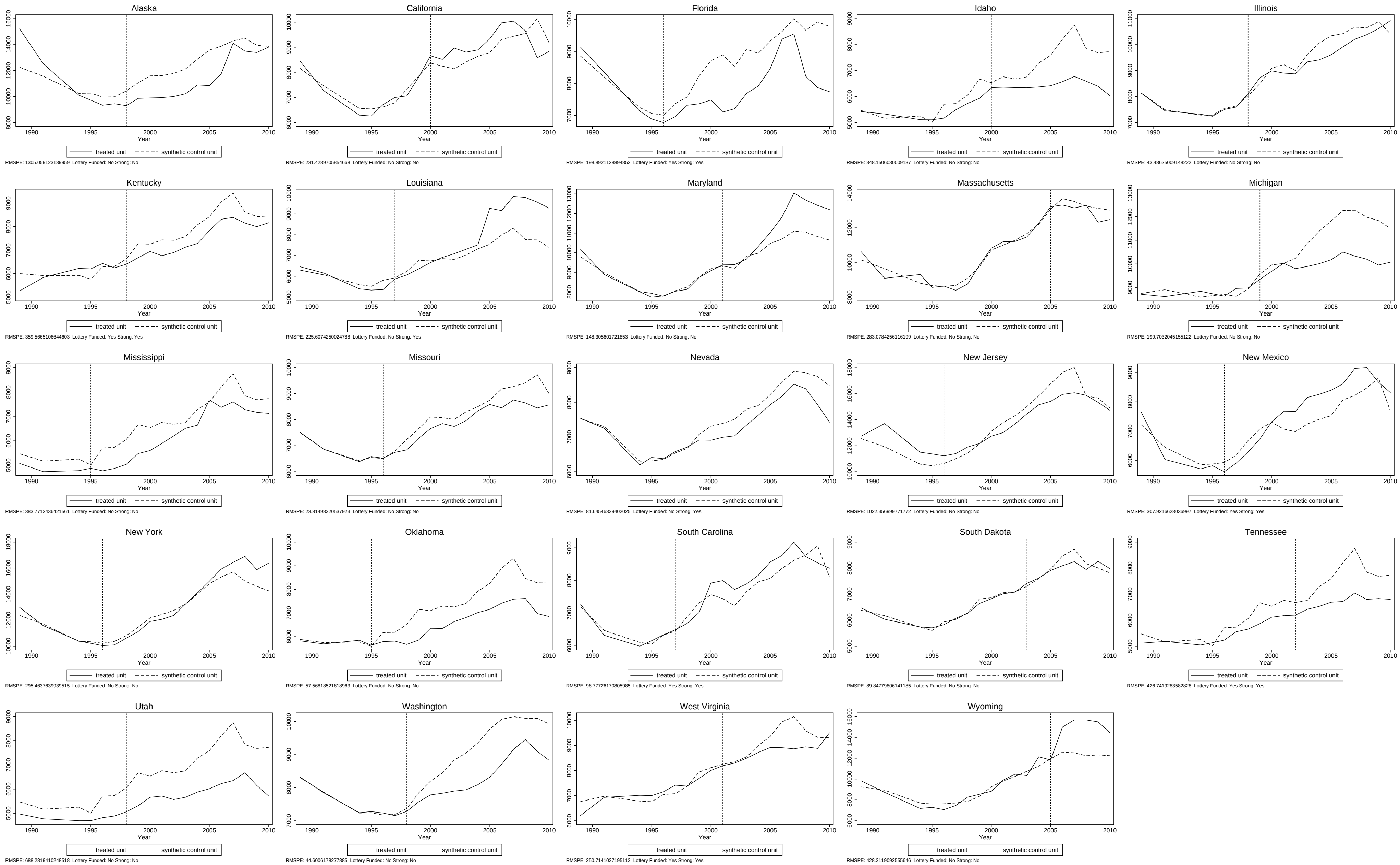
Note that year indicates year corresponding to fall semester.

Figure A.2: State-Level Synthetic Control Results: Local Revenue



Note that year indicates year corresponding to fall semester.

Figure A.3: State-Level Synthetic Control Results: Total Revenue



Note that year indicates year corresponding to fall semester.