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Spending More on the Poor? A Comprehensive Summary of State-Specific Responses to School Finance Reforms from 1990–2014

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ABSTRACT

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Spending More on the Poor? A Comprehensive Summary of State-Specific Responses to School Finance Reforms from 1990–2014

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Abstract

Sixty-seven school finance reforms (SFRs) in 26 states have taken place since 1990; however, there is little empirical evidence on the heterogeneity of SFR effects. We provide a comprehensive description of how individual reforms affected resource allocation to low- and high-income districts within states, including both financial and non-financial outcomes. After summarizing the heterogeneity of individual SFR impacts, we then examine its correlates, identifying both policy and legislative/political factors. Taken together, this research aims to provide a rich description of variation in states' responses to SFRs, as well as explanation of this heterogeneity as it relates to contextual factors.

Keywords: School Finance, Synthetic Controls

Spending More on the Poor? A Comprehensive Summary of State-Specific Responses to School Finance Reforms from 1990–2014

Introduction

There has been a spate of school finance reforms (SFRs) since 1990: sixty-seven reforms in 26 states. One reason for this activity is that SFRs have been shown to be an effective policy for increasing spending in lower-income districts. Indeed, recent national studies show that SFRs increase spending in poorer districts (Candelaria & Shores, 2019; Jackson, Johnson, & Persico, 2016; Lafortune, Rothstein, & Schanzenbach, 2018; Sims, 2011) and improve student outcomes, including graduation rates (Candelaria & Shores, 2019; Jackson et al., 2016), test scores (Lafortune et al., 2018) and adult earnings (Jackson et al., 2016). These national audits of SFRs are likely to overlook important variation among states. Specifically, state responses to SFRs can vary based on the magnitude of the change in spending to low-income districts and by how much and by the type of expenditure and resource that states allocate to low-income districts. Further, this variation in the magnitude of changes to spending and type of resource emphasized may be explained by a state's socio-political context—that is, the composition of its SFR, its socioeconomic and demographic composition, and the political make-up of its legislature and citizenry.

We expect heterogeneity in state-level responses to SFRs because the story of SFRs during this period is one of diversity. Some states changed their finance system through court order; others changed it because of legislative activity; still others changed it in response to both court and legislative activity. Some states had a single SFR during this period; others had multiple. Some states responded to SFRs by changing their funding formula; others kept the funding formula structure but changed its components or weights. Finally, some states were sued because facilities were deemed inadequate; others were sued because aggregate spending was inadequate.[1] The context in which an SFR takes place is also highly variable: both Democratic and Republican governors and legislatures have adopted legislative SFRs (Howard, Roch, & Schorpp, 2017; Wood & Theobald, 2003). Moreover, multiple states across the country have undergone an SFR, which suggests that the average income, demographic composition, and levels of income and racial inequality are highly variable as well.

Despite this diversity, much of what we know about the impact of SFRs comes from recent national studies (e.g., Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018; Sims, 2011), and only five (Kansas, Kentucky, Maryland, Massachusetts, and Vermont) of 26 states with an SFR during this period have been evaluated. National audits and this small sample of state-level case studies are likely to overlook important variation among states. In this paper, we are interested in three research questions related to the heterogeneity of SFRs: whether the effects of SFRs on school spending varied among states, whether states varied in the types of resources they purchased, and, finally, whether a state's socio-political context (e.g., its adopted funding formula, political make-up of the legislature, or level of socioeconomic inequality) is predictive of SFR impact.

Our study is motivated by the idea that understanding the variability of SFR effects across different contexts can be useful for policymakers. As we demonstrate, SFRs exhibit substantial variability in terms of their effects on spending and resource allocation; therefore, pursuing a reform can be a risky option for policymakers, even if SFRs, on average, have positive effects. For instance, to accommodate spending increases in public education spending required by SFRs, state lawmakers may need to disrupt financial budgets by reallocating funds among public expenditure categories or raise taxes to accommodate spending increases in public education (Baicker & Gordon, 2006; Liscow, 2018). Further, knowing which factors predict SFR effect sizes could mitigate some of the

[1] Many papers overview SFRs in this period (see, for example, Corcoran & Evans, 2015; Jackson, 2018; Roch & Howard, 2008; West & Peterson, 2007).

uncertainty associated with reform outcomes and can guide policy if some of these factors are levers over which the state has some control.

Because low-income districts are differentially impacted by SFRs relative to high-income districts, on average (Candelaria & Shores, 2019; Lafortune et al., 2018; Sims, 2011), we describe SFR effect-size variability across states by separately examining the bottom and top income terciles defined by district-level household income from the 1990 Decennial Census. Specifically, whenever resource outcome variables are measured at the district level, we compute the average level of resources across districts in the bottom tercile of the income distribution and across districts in the top tercile within each state. This approach accounts for heterogeneity of SFR impacts within states while also providing a way to assess the progressivity of reforms between terciles.

Our analytic strategy involves two steps: first, obtain causal estimates of effect size heterogeneity at the state-by-income tercile level; second, link these estimated effect sizes to covariates for purposes of descriptive analysis. To estimate state-by-income tercile impacts, we adopt the method of synthetic controls (Abadie, Diamond, & Hainmueller, 2010; Doudchenko & Imbens, 2017) to obtain time-invariant weights, enabling us to construct a hypothetical comparison group (that is, the synthetic control group). We then use these weights in a difference-in-differences framework (Arkhangelsky, Athey, Hirshberg, Imbens, & Wager, 2018) to estimate state-by-income tercile-specific responses to SFRs through the period of 1990 to 2014. This approach provides an estimate of how a state's SFR changed resource patterns, both in terms of levels and type of resource provided. Further, with these state-by-income tercile estimated effects, we can conduct descriptive analyses by linking these effects to state-level covariates. Based on a review of the literature, we examine a host of predictors, which include SFR-related policies, political partisanship, and socio-demographic variables.

The short version of our findings is as follows: in the aggregate, summarizing all the state-specific responses to SFRs, expenditures increased by 8 percent for low-income districts and 2 percent for high-income districts. Results from a traditional difference-in-differences model (i.e., one in which unit-specific counterfactuals are substituted for unit and year effects) are 1.3 to 1.9 times larger. In general, states that increased spending after reform increased spending to low-income districts in greater amounts relative to high-income districts, meaning that SFRs tend to have progressive effects on school resource allocation. Time spent in school marginally increased following SFRs, but kindergarten expansion did not.

At the same time, the heterogeneity of responses may temper enthusiasm for SFRs. First, 10 of 26 SFRs in this period resulted in spending losses to low-income districts. Further, placebo tests indicate that, in most cases, states without SFRs are as likely to increase spending to low-income districts as states with SFRs. Finally, increases to capital spending far outpace increases to instructional spending, suggesting SFRs reflect, in part, a demand for improved facilities. Insofar as the effect of capital spending on student achievement is uncertain (e.g., as suggested by Jackson, 2018), SFR-induced spending shocks may not consistently translate to student achievement gains.

Despite the prevalence of SFR activity, expectations that SFRs will have heterogeneous effects, and significance of this variability for disadvantaged students, a comprehensive evaluation of these reforms has not been conducted. Our study fills this gap and illustrates how familiar methodological approaches (synthetic controls and difference-in-differences) can be used to evaluate treatment effect variation in settings where randomization is impossible.

The paper proceeds as follows: (1) Previous Literature; (2) Data; (3) Research Methods; (4) Results; (5) Discussion; and (6) Conclusion.

Previous Literature

To date, studies of the effects of SFRs on revenues, expenditures, and student outcomes have either yielded (a) an aggregate effect combining SFRs across states or (b) an SFR-specific effect, based on a single reform in a given state. In the aggregate, recent studies leveraging the timing of an SFR as an exogenous shock to school spending have found consistently positive relationships between spending increases and student outcomes (Candelaria & Shores, 2019; Hyman, 2017; Jackson et al., 2016; Lafortune et al., 2018). These findings contrast from earlier studies, which did not provide consistent causal evidence that education spending increases improved student outcomes (e.g., Burtless, 1997; Greenwald, Hedges, & Laine, 1996; Hanushek, 1997)

Among aggregate studies, there has been limited attention to the mechanisms through which school resource shocks improve student outcomes. Jackson et al. (2016) find that states undergoing SFRs increased the number of new teachers hired per student, suggesting that smaller class sizes are driving results, a mechanism supported by prior literature (Chetty et al., 2011; Fredriksson, Öckert, & Oosterbeek, 2012; Krueger, 1999). One challenge to this interpretation of mechanisms is that many SFRs specifically target capital expenditures, resulting in capital expenditure increases (Jackson et al., 2016), which have no direct impact on class sizes. However, capital expenditures may improve student outcomes by increasing the time students spend in schools, for example by encouraging greater attendance, a result supported by evidence from facilities investments in California and Connecticut that boosted student achievement while increasing attendance (Lafortune & Schönholzer, 2017; Neilson & Zimmerman, 2014). Reviewing SFRs that took place between 2003–2013, Klopfer (2017) also finds that improvements to academic achievement are explained by increases in the length of the school day and not from increases to academic efficiency. At the same time, the evidence of a causal relationship between capital spending, time in school, and achievement is mixed. Of the seven studies reporting causal effects of capital spending (as summarized by Jackson, 2018), three report null findings on achievement and two of those three also find no direct effect on student attendance.[2]

[2] Cellini, Ferreira, and Rothstein (2010) do not include attendance as an outcome measure; Goncalves (2015) and Martorell, Stange, and McFarlin Jr (2016) directly test for attendance effects from increases to capital spending and find nothing.

SFR-specific impact evaluations would be useful for understanding the heterogeneity of impacts on total spending, as well as for understanding variability in possible mechanisms through which different resource allocations could affect student outcomes. Unfortunately, only 5 (of 26) states have been evaluated during this period, and among these studies, there has been little attention to mechanisms. Researchers have evaluated Kansas' 1992 School District Finance and Quality Performance Act (Duncombe & Johnston, 2004; Johnston & Duncombe, 1998), Kentucky's 1990 Kentucky Education Reform Act (Clark, 2003), Maryland's 2002 Bridge to Excellence in Public Schools Act (Chung, 2015), Massachusetts' 1993 Education Reform Act (Dee & Levine, 2004; Guryan, 2001), and Vermont's 1997 Equal Educational Opportunity Act (Downes, 2004).[3] Among these, results range from moderate spending increases with little improvement to student outcomes (Kansas, Kentucky, and Maryland) to increases in both spending and academic outcomes (Massachusetts and Vermont).

The limited study of these reforms and the heterogeneity of results provide impetus for a comprehensive study across multiple states. Further, given the variability in linkages between resource gains and academic improvements (e.g., Jackson, 2018), it suggests that variation in the type of resources states pursue resulting from SFRs is important as well. Therefore, we evaluate the impacts of SFRs in multiple domains, including per pupil total expenditures, teacher salaries, capital expenditures, class sizes, full day kindergarten enrollment, and the length of the school year. Interest in these non-fiscal outcomes is based on prior literature. Class sizes, on average, have decreased as a result of SFRs (Jackson et al., 2016) and are an important mediator of student academic outcomes (Chetty et al., 2011; Fredriksson et al., 2012; Krueger, 1999). Because full day kindergarten enrollment

[3] Michigan's 1994 Proposal A has been studied by multiple authors (Chaudhary, 2009; Cullen & Loeb, 2004; Hyman, 2017; Papke, 2008; Roy, 2011). Following, (Lafortune et al., 2018), we exclude this case because it was not an SFR, but instead came to a vote at the state level and was approved by voters as an amendment to the state constitution. Evaluations of New Jersey's 1997 *Abbott* and New York's 2003 *Campaign for Fiscal Equity* rulings are available as unpublished conference proceedings and dissertations (see Resch (2008) and Atchison (2017), respectively).

increased between 1990–2014 (Gibbs, 2017), we test whether this enrollment expansion can be linked to SFRs. And because SFRs have also resulted in students and teachers spending additional time in school, on average, (Jackson et al., 2016; Klopfer, 2017), we test whether the number of days in school or the number of minutes in the school day increased for individual states undergoing reform.

In addition to understanding the heterogeneity of SFR-impacts among states, these data also allow us to understand whether SFR-related policies, political and legal factors, and socio-demographic contexts influence SFR progressivity. We classify SFR-related policy factors as the school finance context in which SFRs take place. Variability in funding formulas will determine how much aid is allocated to low-income districts, as some formulas, for example, provide targeted aid based on student characteristics while others place limits on local revenues contributions (Card & Payne, 2002; Hoxby, 2001). Further, we look at whether the SFRs were induced by the courts or the legislature, and whether the state was subjected to multiple court rulings, which would indicate the state's compliance with court mandates.

To our knowledge, existing school finance research has not addressed whether political factors, legal factors, or socio-demographic contexts predict SFR progressivity.[4] Given this gap in the literature, we examine research that documents which factors and contexts predict the progressivity of a state's educational spending. Whether these predictors apply to the SFR landscape is an empirical question that we try to address here.

We classify political and legal factors as the ideological composition of the electorate and legislature. States with more liberal citizens and institutions contribute more state revenues to low-spending districts, and are more responsive to judicial mandates to restructure state education finance systems (Burbridge, 2002; Wood & Theobald, 2003). Polarization of US legislatures is associated with gridlock and a decrease in legislative

[4] While there have been a few studies that attempt to use political and legal factors to predict whether and (to a lesser extent) when an SFR occurs within a state (Dumas, 2017; Roch & Howard, 2008), these studies do not predict whether an SFR will increase spending to low-income districts.

capacity, which can impede implementation of changes to the school finance system (Voorheis, McCarty, & Shor, 2015). We also include the strength of the state's collective bargaining agreements as a political factor that can influence SFR-induced spending changes (Brunner, Hyman, Ju, et al., 2018).

We classify socio-demographic variables as the state's socioeconomic and demographic characteristics. For example, a state's ability to raise revenues for progressive spending will be greater if the state has a larger tax base (Baker, Sciarra, & Farrie, 2014). Higher levels of socioeconomic inequality may increase spending (Alesina & Rodrik, 1994; Boustan, Ferreira, Winkler, & Zolt, 2013; Corcoran & Evans, 2010), but these effects likely interact with the state's funding formula (Loeb, 2001). Racial segregation and composition may also reduce the progressivity of SFRs (Alesina, Glaeser, & Sacerdote, 2001; Ryan, 1999).

Taken together, these factors may be associated with observed heterogeneous effects of school finance reforms. The current study explores the association between these political and legal factors and finance reform outcomes. A summary of the predictors included in our study is shown in Table 1, along with the predicted direction of the relationship between the covariate and SFR progressivity.

Data

To understand how SFRs varied by state and resource, our analysis requires a tabulation of SFRs, a time series of dependent variables measured at the state-by-income tercile level (i.e., total expenditures and expenditure categories such as instructional and capital) and, when data are unavailable at this level, dependent variables measured at the state-level (i.e., kindergarten enrollment and time in school). To understand which variables are then predictive of SFR effect size variation, we compile a time series, when possible, of state-level variables theorized to be predictive of SFR progressivity.

Tabulation of School Finance Reforms

We compile a list of all major school finance reforms beginning in 1990 by leveraging recent lists compiled by Jackson et al. (2016) and Lafortune et al. (2018). In cases where there was a disagreement between our two sources, we privileged Lafortune and colleagues because they provided supplemental research on case histories and because they have a more recent list. We made two substantive changes to the cases provided by Lafortune and colleagues. First, resolutions of court cases and legislative enactments were recorded in calendar years, but these calendar years need not align with academic years (e.g., an event occurring in December of 2012 would be recorded as 2012, but would likely apply to the Fall and Spring of academic year 2012–13). We gathered the months and years in which cases were resolved or bills signed into law, and converted these events into academic calendar time. Second, in a few instances, a state had a court ruling and legislative bill passed in the same fiscal year but, based on the month, the ruling and bill occurred in adjacent academic years. In these cases, we separated the combined events into two events occurring in subsequent years. Appendix Table A1 lists the school finance reform events under consideration.[5]

Dependent Variables

In our analyses, we examine the impact of SFRs on both fiscal and non-fiscal outcomes. Fiscal outcomes are measured at the district level, and we transform them into state-by-income tercile level outcome variables. Non-fiscal outcomes are measured at both the district and the state level; only those measured at the district level are transformed into state-by-income tercile measures, and those measured at the state level are left as

[5] While we tabulate all court cases and legislative bills in Table A1, we require at least four year of pre-SFR outcome data before employing the synthetic control method we describe in the Research Methods section. For this year, cases that occurred before academic year 1992–93 are excluded, affecting four states, and appear in bold typeface in Table A1. We do not estimate an effect for Kentucky because the state had only one SFR during this period. The remaining states had multiple SFRs, and we use the first SFR beginning in 1992–1993 as the first event.

state-level descriptors. In what follows, we discuss our two sets of dependent variables and outline the steps we take to prepare them for analysis.

Fiscal Outcomes. With respect to fiscal outcome data, our primary data source is the Local Education Agency Finance Survey (F-33), which has been collected annually by the U.S. Census Bureau since 1989–90 and is distributed by the National Center for Education Statistics (NCES). From the F-33, we extract total revenues and total expenditures. We also obtain the following expenditure subcategories: current expenditures on elementary and secondary education, instructional staff support services, capital outlays, and teacher salaries. The panel data set of fiscal outcomes we assemble spans academic years 1989–90 to 2013–14.[6] In our analyses, we scale these data by total district enrollment and all dollar values are in 2013 USD using the Consumer Price Index.

Large fluctuations in district enrollment from one year to the next result in volatile outcome measures when enrollment is in the denominator. To address this issue, we follow Lafortune et al. (2018) and apply sample restrictions directly to district enrollment *before* scaling our fiscal variables by enrollment. Because one can make different choices regarding the stringency of any given data restriction, we generate two sets of enrollment variables—R1 and R2—that reflect different choices. We outline the restrictions that we apply and the differences between R1 and R2 below:

1. Remove small districts in which the total enrollment is less than α_1: $\alpha_1^{R1} = \alpha_1^{R2} = 100$.

2. Remove district-year observations in which enrollment exceeds mean district enrollment by scale factor α_2: $\alpha_2^{R1} = \alpha_2^{R2} = 2$.

3. Remove district-year observations in which enrollment is greater than α_3%: $\alpha_3^{R1} = 15$; $\alpha_3^{R2} = 12$.

[6] During schools years 1990–91, 1992–93, and 1993–94, the full universe of school districts were not surveyed and are not included in the NCES release of data; however, we were able to obtain the data directly from the U.S. Census Bureau.

4. Remove district-year observations in which enrollment is greater than α_4% above or below the district's constant growth rate trend: $\alpha_4^{R1} = 10$; $\alpha_4^{R2} = 8$.

5. Remove an entire district from the analytic sample if the restrictions (1) to (4) above cause the district to have more than α_5% of its observations removed: $\alpha_5^{R1} = \alpha_5^{R2} = 33$.

Once we generate restricted enrollment variables R1 and R2, we take each of the outcome measures that are to be scaled by district enrollment and create two new sets of variables: one set is divided by R1; the other is divided by R2. All fiscal variables are then log transformed and non-fiscal variables remain in levels. The two sets of outcome variables are then subjected to an outlier procedure that trims each variable based on its state average in a given year. Specifically, if a given district observation is less than 20 percent or more than 500 percent of the state average, it is dropped (Lafortune et al., 2018).

We then place districts into income terciles based on their state's 1989 median income levels, which comes from the 1990 U.S. Decennial Census. These income data precede all reforms under consideration in this study. Districts in the bottom tercile are the poorest in the state; districts in the top tercile, the richest. The state-specific terciles remain fixed throughout all analyses to help mitigate bias from potential Tiebout sorting induced by school finance reforms. For each state-specific tercile and year, we then compute the weighted median of our outcome variables of interest, where the weights are based on the annual district enrollment using R1 and R2 above. Finally, because identifying synthetic counterfactuals can be biased if there is measurement error or volatility in the dependent variable (Abadie, Diamond, & Hainmueller, 2015; Powell, 2018), we smooth the data by taking three-year moving averages as a final data transformation.

Using these tercile measures, we can examine the extent to which school finance reforms improved outcomes, on average, in the poorest districts in a state; moreover, we can examine the extent to which reforms were progressive by seeing whether bottom-tercile districts benefited more from school finance reform relative to top-tercile districts in the same state for a given outcome.

Non-Fiscal Outcomes. We also collect several non-fiscal outcome measures. From the NCES Local Education Agency Universe Survey, we obtain teachers per student ratios at the district level. As this outcome is at the district level we compute these at the state-by-income tercile level and smooth them as discussed above. From the Current Population Survey (CPS), which is administered by the U.S. Census Bureau, we extract data on the percentage of children that attend full-day kindergarten in each state over time. Both the teachers per student ratio and kindergarten enrollment data span academic years 1989–90 to 2013–14.

Finally, from the Schools and Staffing Survey (SASS), administered by NCES, we obtain the length of the school day in minutes and the number of days in the school year. Survey years used from the SASS include 1987–88, 1990–91, 1993–94, 1999–2000, 2003–04, 2007–08, and 2011–12. For each state, intervening years between SASS survey waves were predicted using linear interpolation. Given that we do not extrapolate data outside of the survey years, these data span academic years 1989–90 to 2011–12.

Summary statistics for all outcome variables are shown in Table 2. Sample means and standard deviations are computed among states that had a court-ordered or legislative reform. For outcomes measured at the income tercile level, we provide statistics for terciles 1 and 3, corresponding to lower-income and higher-income districts, respectively; the remaining outcomes have statistics reported at the state level. Because data from the CPS and SASS have sampling designs that provide only state-level representation, outcomes extracted from those surveys cannot be used to compute weighted-median terciles within the state; instead, we have only state-level average effects.

Predictors of SFRs

To understand heterogeneity based on the nature of the SFR context, we generate variables to indicate whether the courts or legislature induced the SFR and whether the

SFR was the first in the state. Further, we generate a panel dataset of funding formula for each state and year for the period 1990–2014. Because funding formula terminology varies by study and has changed over time, we develop a funding formula dictionary comprised of five common definitions of funding formula components: foundation plan, flat grant, equalization, power equalization, centralization, spending limits, and categorical aid. We identify two additional "add-on" components of the state funding formulas that are always used in conjunction with one or more of the five core formula: spending limits and categorical aid. States generally adopt "hybrid" funding formula, combining elements from each. For instance, at the time of a state's first SFR, 14 unique funding formula combinations were in place. Despite this heterogeneity, 22 of 26 states included, as at least one component of their funding formula, a foundation plan. Funding formula in states without SFRs are similarly hybridized and reliant on foundation plans: in 2014, 16 unique funding formula combinations are present in the 23 states without an SFR, and 19 of these states include at least a foundation plan as part of their formula. Additional details about the construction of the funding formula panel can be found in Appendix B; tabulations of states with SFRs and the funding formulas present in the state following an SFR are shown in Appendix C1.

Data for political and legal predictors of SFR heterogeneity come from multiple sources. State polarization data come from the Shor-McCarty legislative ideology data set[7], based on individual-level legislator roll call data. We use a continuous variable from this data set that represents the distance between Democratic and Republican party medians, within Senate and House of Representatives or Delegates.Citizen and legislature ideology is measured using data from congressional district voting patterns (Berry, Ringquist, Fording, & Hanson, 1998).[8] Larger values indicate more liberal citizens or legislatures, on average (Berry et al., 1998). We gather state-level indicators of teacher union strength from the

[7] Retrieved from https://doi.org/10.7910/DVN/BSLEFD

[8] Retrieved from https://rcfording.wordpress.com/state-ideology-data/

Thomas B. Fordham Institute, which are generated through a combination of factors including union resources and membership, involvement in politics, the scope of collective bargaining strength, state policies, and perceived union influence (Brunner et al., 2018; Winkler, Scull, & Zeehandelaar, 2012). Higher values on the index indicate stronger union status. Teacher union strength data used in the current analysis come from these reports (Brunner et al., 2018; Winkler et al., 2012), which give ratings for all states but are only available for the year 2011-12. In contrast, both state partisanship and citizen ideology are time-varying and available for all states.

Socioeconomic and demographic variables also come from multiple sources. We obtain state-level income inequality from a data set compiled by Sommeiller and Price (2018). For our analyses, we use the share of income held by the top 10 percent and the top 1 percent of earners in a state-year. From Sommeiller and Price (2018), we also obtain per capita personal income, as it provides a rough measure of the state's tax base. From the CCD school and district level universe files, we obtain the proportion of students that are free lunch eligible (FLE) as well as race and ethnicity information. Using the CCD variables aggregated at the district level, we then construct state-level measures of segregation by computing the information theory segregation index (Reardon & Firebaugh, 2002) among the following group pairs: white and black, white and Hispanic, and FLE and non-FLE. Higher values of the index imply that the group pair under consideration is becoming more segregated. All of these variables are time-varying for each state and were shown previously in Table 1.

Research Methods

Our analytic strategy is designed to solve two identification problems associated with state-specific case studies with multiple events. First, when estimating treatment effects of individual states using a traditional difference-in-differences design, pre-SFR trends in the dependent variable may substantially differ from comparison, non-treated states;

consequently, the causal warrant of the estimates is questionable. Second, many states had multiple SFRs; to ensure that estimated effects are not attributed to subsequent SFRs and to ensure that effects of subsequent SFRs are not attributed to prior events, we need a model that adjusts for these multiple events. Our methods therefore combine the advantages of synthetic controls, which generate weights to identify control units most resembling the treated unit in terms of pre-treatment levels and trends in the dependent variable, with a difference-in-differences estimator, which leverages the synthetic weights while controlling for multiple SFR events.[9] Arkhangelsky et al. (2018) show that combining these methods results in less bias than either synthetic controls or difference-in-differences alone. We begin by reviewing the synthetic controls framework, and then discuss how we choose an optimal model, apply the difference-in-differences estimator and conduct inference.

Synthetic Controls Overview

Having constructed a panel data set in which the unit of observation is defined by a state, year, and income tercile tuple, we now wish to estimate the state-income tercile effects for all states undergoing an SFR. To do this, we implement a case studies approach using synthetic control methods (Abadie et al., 2010). For each SFR, the state undergoing reform is the treatment state, and the remaining states serve as a potential pool of controls. Following the notation of Abadie et al. (2010), we observe data for $S + 1$ states, where $s \in \{1, \ldots, S + 1\}$. Without loss of generality, we designate the first state to be the treatment state undergoing reform; therefore, there are S states that serve as potential controls. With respect to the time dimension, any given SFR has T_0 years of pre-treatment data (i.e., the number of years before an SFR) and a total of T years of data, where $1 \leq T_0 < T$. Because SFRs occur in different years, T_0 will vary across reforms.

We denote outcomes (for example, log total expenditures per pupil) as $Y_{st}^{Treated}$ and

[9] The difference-in-differences approach to multiply treated states was suggested by (Klopfer, 2017).

$Y_{st}^{Control}$ for the treated and control states, respectively. In the years before the reform, where $t \in \{1, \ldots, T_0\}$, we model outcomes to produce $Y_{st}^{Treated} = Y_{st}^{Control}$. In the years after, we model the difference between treatment and control by defining $\gamma_{st} = Y_{st}^{Treated} - Y_{st}^{Control}$. Combining notation, we describe outcome data for any state with the following equation:

$$Y_{st} = \gamma_{st} SFR_{st} + Y_{st}^{Control},$$

where SFR is a binary indicator that takes value one when the state undergoing reform (i.e., $s = 1$) is in year $t > T_0$. The goal of the synthetic controls method is to estimate $\gamma_{1T_0+1}, \ldots, \gamma_{1T}$, which corresponds to the dynamic treatment effect. Because $s = 1$ is the only treated state by construction, we can write

$$\gamma_{1t} = Y_{1t}^{Treated} - Y_{1t}^{Control} \quad \text{for } t > T_0.$$

Although we observe $Y_{1t}^{Treated}$, we need to estimate $Y_{1t}^{Control}$, which is the counterfactual for the treated state.

To estimate $Y_{1t}^{Control}$, we implement a minimzation procedure that finds weights w_s^* for each state in the control group such that

$$\begin{gathered}
\sum_{s=2}^{S+1} w_s^* Y_{s1}^{Control} = Y_{11}^{Treated} \\
\sum_{s=2}^{S+1} w_s^* Y_{s2}^{Control} = Y_{12}^{Treated} \\
\vdots \\
\sum_{s=2}^{S+1} w_s^* Y_{sT_0}^{Control} = Y_{1T_0}^{Treated},
\end{gathered}$$

where the system of equations above shows that these weights are estimated by matching exclusively on all the pre-treatment outcomes (Doudchenko & Imbens, 2017)—for each $t \in \{1, \ldots, T_0\}$—with the purpose of constructing differences between treatment and control equal to zero.[10] Then, we use these weights and apply them to the outcomes of the S members of the control group, which gives us

$$\hat{Y}_{1t}^{Control} = \sum_{s=2}^{S+1} w_s^* Y_{st}^{Control}.$$

Because $\hat{Y}_{1t}^{Control}$ describes what would have happened to the state undergoing an SFR for years $t > T_0$, it is a "synthetic" counterfactual group. Therefore, we can easily define the estimate of the dynamic treatment effects for $s = 1$ as

$$\hat{\gamma}_{1t} = Y_{1t}^{Treated} - \hat{Y}_{1t}^{Control} \quad \text{for } t > T_0.$$

Synthetic Controls Implementation. Within the synthetic control framework, we can scale the dependent variables for both treatment and control states to be value one at the timing of treatment. Transforming the data in this way forces the algorithm to match strictly on trends as opposed to levels (Cavallo, Galiani, Noy, & Pantano, 2013a). In total, for variables that are scaled by student enrollment, up to four models are available, indexed by data restrictions and trends: $(\text{Trends On}, \text{Trends Off}) \times (R1, R2)$. For variables not scaled by student enrollment, only two models are available, indexed by trends. All synthetic controls specifications for all state-terciles and outcome combinations are implemented using `synth_runner` in Stata by Galiani and Quistorff (2017).

Choosing an Optimal Model. Given four models from which to choose, we estimate each model combination and select the model that provides superior pre-treatment matches between treatment and control. We define superiority as the model that produces the minimum mean absolute effect size in years prior to treatment. We use the absolute effect size because we care about absolute differences from zero (where zero indicates a perfect match between treatment and control).

[10] Multiple papers have pointed out that including all lagged dependent variables effectively cancels out any additional lagged covariates (e.g., Kaul, Klößner, Pfeifer, and Schieler (2015)).

We summarize the results of our synthetic control efforts in Table 3 for the logarithm of expenditures per pupil. For Terciles 1 (low-income) and 3 (high-income), we present four statistics: the cumulative absolute pre-SFR effect size of the minimum model (i.e., "Min Abs(Effect)"); the ratio of the maximum to minimum cumulative absolute pre-SFR effect size of the maximum model (i.e., "Max to Min Abs(Effect)"; the pre-SFR mean of log per pupil expenditures (i.e., "Dep. Var."); and the mean pre-SFR effect size of the minimum model (i.e., "Min. Effect"). "Min Abs(Effect)" is comparable to the root-mean-squared-error (RMSE) and represents the total deviation in the dependent variable between the treated state and the synthetic control states from the model that minimizes (indexed by trends and data restriction) that deviation. "Max to Min Abs(Effect)" compares the minimizing model (of the four) to the one that maximizes the cumulative pre-SFR effect size. "Dep. Var." allows us to benchmark "Min Abs(Effect)" against the actual value of the dependent variable. "Min. Effect" is useful as an indicator of the difference-in-differences assumptions, namely, that there are no observable differences between treatment and control prior to treatment.

For nearly all states in Tercile 1 (low-income districts), the cumulative absolute effect size from the minimizing model is never greater than 0.06 and is, in many cases, less than 0.01. Alaska has the worst pre-treatment match at 0.060, which is less than 0.6 percent of the pre-SFR dependent variable mean. The ratio of the maximum to minimum cumulative absolute effect size ranges from 1.114 to 459.444. This means that pre-SFR match quality in some cases varies little by the selected model type; in other cases, the pre-SFR match quality varies dramatically. Finally, for all states, the average pre-SFR effect size from the minimizing model is never greater than 0.034 and is, in most cases, less than 0.01. This last result suggests that placing a linear restriction on the pre-SFR period to be equal to zero is defensible with these synthetic controls. For Tercile 3 (high-income districts), the results are comparable. The states that are included as synthetic controls and their accompanying weights are shown for all dependent variables and aggregations (i.e., Terciles

1 and 3 and the state average) in Appendix Tables G1, G2, and G3.

Difference-in-Differences

While the prior discussion addresses concerns about building proper counterfactuals using pre-treatment information about the dependent variable for individually treated units, it does not address the identification problem that arises when states undergo multiple SFRs. Specifically, the issue with building synthetic controls for $J+1$ SFRs is that the counterfactuals will be constructed to mimic changes in the dependent variable that resulted from the initial SFR. In the synthetic controls context, this is an example of conditioning on post-treatment variables and would result in bias (Montgomery, Nyhan, & Torres, 2018). At the same time, we do not wish to attribute effects of any subsequent reforms to prior reforms. To address these two issues, we employ a modified difference-in-differences estimator to summarize results. The model takes the form:

$$Y_{st} = \alpha_0 \sum_{j=1}^{j=J} D_{s,j} + \delta_s + \delta_t + \varepsilon_{st} \tag{1}$$

In this equation, s indexes state-terciles, t indexes time, and j indexes each SFR. $D_{s,j}$ is an indicator variable equal to unity in the year after a SFR takes place and zero otherwise. Multiple $D_{s,j}$ indicators are available for some states, and so this equation estimates the conditional effect of a subsequent SFR net of the effect of the prior SFR. The variable α_0 summarizes these coefficients.[11] Effectively, this model provides an estimate of the cumulative impact of an initial SFR and all subsequent reforms. When estimating the model, we weight the regression using the optimal choice model weights generated by the synthetic controls algorithm. Thus, the unit effects (δ_s) include only the treated state and states for which pre-treatment trends resemble trends in the treated state, and the year effects (δ_t) model the synthetically generated counterfactual trend. Finally, Arkhangelsky

[11] This method is suggested by Klopfer (2017).

et al. (2018) show that combining these methods results in less bias than either synthetic controls or difference-in-differences alone.

Results from synthetic controls and the difference-in-differences model for log total expenditures Tercile 1 (low-income districts) are shown in Figure 1. Results for Tercile 3 (high-income) districts are in the appendix Figure D1. The solid black line corresponds to effect sizes from synthetic controls. The dashed gray line corresponds to a dynamic difference-in-differences model that includes an indicator variable for each year subsequent to an SFR. The horizontal solid gray line corresponds to α_0 from Equation 1. For the two difference-in-differences estimates, we include the weights derived from the synthetic controls procedure and restrict the sample to states included from the donor pool.

Three points are worth noting from this figure. First, effect sizes prior to an SFR are very close to zero for nearly all states. This result, which conforms to the minimum absolute effect size and mean effect size columns from Table 3, gives us confidence that the linear restriction placed on the pre-period by setting it equal to zero is defensible. Second, results from the the non-parametric difference-in-differences model nearly perfectly replicate the effect sizes from synthetic controls. This result gives us confidence that the difference-in-differences estimator can be applied to the data using the weights derived from the synthetic controls routine. Finally, estimates for α_0 from Equation 1 are consistent with the pattern of results from the synthetic controls effects. This last result indicates that the control data are rarely located outside the convex hull, which would give rise to bias if the difference-in-differences estimator was not applied (Arkhangelsky et al., 2018). In sum, the results give us confidence that we can summarize the data effectively with a single statistic using the difference-in-differences estimator combined with synthetic controls weights.

Inference

For hypothesis testing, we construct placebo p-values (Abadie et al., 2010) designed to answer the following question: how often would we obtain results of the same magnitude or higher if we had chosen a state at random for the study instead of the state undergoing an SFR? We begin by applying the synthetic control method to each of the states that did not have an SFR (i.e., the donor pool of non-treated states). The donor pool for these placebo states include the remaining states without SFRs. The pre-period for the placebo tests is based on the pre-period of each treated state for which the placebo test is being conducted. For example, in Alaska, the placebo states are matched for the pre-period 1990–1999 (before Alaska had its SFR); whereas, for Arizona, the placebo states are matched for the pre-period 1990–1994 (before Arizona had its first SFR). Counterfactual units from the donor pool then receive a vector of weights. With these weights, we re-estimate Equation (1) for each placebo state. Because this placebo p-value can generate incorrect inferences if the placebo units are poorly matched to their counterfactuals (Ferman & Pinto, 2017), we rescale the effect size for each abs(α_0^*) (where * indexes non-treated states) and abs(α_0) by dividing by the pre-SFR root-mean-square prediction error (RMSPE). In effect, this technique shrinks those estimated effect sizes with poor pre-period match to zero. The p-value is then calculated as the proportion of placebo states with a scaled absolute effect size (i.e,. abs$(\alpha_0^*)/RMSPE^*$) greater than the scaled absolute effect size (i.e., abs$(\alpha_0)/RMSPE$) of a state with an SFR.

We complement these placebo p-values with conventional p-values derived from heteroskedastic robust standard errors, adjusted for finite samples (i.e., HC3 standard errors), as suggested by Arkhangelsky et al. (2018). We do not prioritize them, however, as these methods rely on the assumption of no autocorrelation within clusters, which Bertrand, Duflo, and Mullainathan (2004) and others have shown to be an implausible assumption in difference-in-differences applications. In all cases, p-values using these methods are much smaller than those from placebo-based methods. [12]

[12] Many other methods for conducting inference with synthetic controls have been suggested, though none are well-suited to our data. Arkhangelsky et al. (2018) suggest the leave-one-out clustered jackknife (which Cameron and Miller (2015) refer to as the “cluster generalization of the HC3 variance estimate,” (p. 342)); however, this method requires multiple clusters, and in many cases, we have fewer than five. The wild bootstrap works with few clusters but requires multiple treated units within each cluster (MacKinnon,

Results

Our results proceed as follows: (1) we first describe the average of α_0 among all states undergoing SFRs; this average across the population of treated states is then compared to estimates obtained from a traditional differences-in-differences estimator; (2) we then describe heterogeneity in these average estimated effects for multiple resource types among income-terciles one and three (i.e., low- and high-income districts) and state averages; (3) we conclude with an exploratory descriptive analysis, leveraging the vector of estimated α_0 as outcome variables in prediction models, to better understand the contexts in which SFRs are productive.

Main Effects of SFRs: Comparison of Synthetic Controls and Difference-in-Differences

Table 4 compares estimates for Terciles 1 (low-income) and 3 (high-income) for two different specifications and multiple outcomes. The first specification is the "Non-Synth" difference-in-differences model, which is identical to Equation (1) with four differences: (1) it includes all state-tercile-years in the sample (or state-years for data with state-level averages); (2) it clusters standard errors at the state level; (3) it does not leverage the vector of weights derived from the synthetic controls procedure; and (4) α_0 is calculated as $\sum_{j=1}^{j=J} D_{s,j} \times S/26$, where $S/26$ weights each $J+1$ effect by the number of states S contributing to the effect.[13] The second specification is a summary of the "synth" estimates, which is the mean of the vector α_0 for each state undergoing an SFR.

Nielsen, Roodman, Webb, et al., 2018; MacKinnon & Webb, 2017), and we have only one treated unit per cluster. Finally, Ferman and Pinto (2018) propose a method for calculating standard errors with few clusters and few treated units; however, this approach requires many counterfactual units, and in our case, there are many instances when we have fewer than five. The p-values for these methods are available upon request, though placebo-based methods are nearly always the most conservative.

[13] Weighting each $D_{s,j}$ is done so as to penalize multiple SFR events for which few states contribute. For instance, New Hampshire is the only state with seven SFRs; without weighting, New Hampshire's singular seventh SFR would contribute one-seventh of the weight to α_0. For the synthetic controls case, this was unnecessary as each state was estimated separately.

Both the traditional "non-synth" and synthetic results indicate a statistically significant effect for total spending among Tercile 1 (low-income) districts and a smaller and not consistently significant effect for log total spending among Tercile 3 (high-income districts). Results from the non-synth model are 1.3 times larger for Tercile 1 effects and 1.9 times larger for Tercile 3 effects compared to the synthetic controls mean, indicative of the bias reduction one gets from applying synthetic controls to the difference-in-differences estimator (Arkhangelsky et al., 2018). The non-synth difference-in-differences identifies a large effect for instructional spending; when unit-specific counterfactuals are included in the synthetic context, that effect approaches zero. For capital spending and class size reductions, both the synthetic and non-synthetic estimates are similar and significant. Further, aggregate synthetic results show that SFRs increased teacher salary expenditures for Tercile 3 districts more than for Tercile 1 districts (0.061 and 0.044, respectively). SFR effects on capital spending, however, were larger for Tercile 1 compared to Tercile 3 districts (0.386 and 0.101, respectively). Thus, SFRs tend to be more progressive with respect to their effects on capital spending than teacher salaries.

For the non-expenditure outcomes, the non-synthetic difference-in-differences fails to identify any effect on kindergarten expansion or increases to time spent in school. The standard errors for each of these estimates are larger than the estimated effect size in three cases and 57 percent of the estimate for one outcome. Synthetic and non-synthetic results are most inconsistent for these non-expenditure outcomes, a likely symptom of the large standard errors (e.g., the synthetic mean never falls outside a range that includes the non-synthetic mean and +/− 1 standard error). Aggregate synthetic estimates show no indication of kindergarten expansion and increases to time spent in school are trivial.

Aggregating unit-specific estimates identified from unit-specific counterfactuals provides important complementary information to traditional difference-in-differences estimates. In this context, we generally find evidence that the traditional methods overstate the overall effect of SFRs. We now turn to the heterogeneity of results. We first discuss unit-specific effects and then turn to inference.

Heterogeneity of SFR: Expenditures

State-specific effect sizes for total expenditures, capital expenditures, salary expenditures, and teachers per 100 students are shown in Figure 2. Results for Tercile 1 (low-income districts) are shown in the first panel and results for Tercile 3 (high-income districts) are shown in the second panel; for each outcome variable, states are sorted according to estimated effect sizes in Tercile 1. The unweighted correlation between Tercile 1 and Tercile 3 results are included in the bottom right-hand corner of the Tercile 3 panel of each outcome. The vertical dashed line shows the average of the point estimates and is identical to the "synth" average shown in Table 4. We calculate standard errors as $\frac{\alpha_0}{Z}$, where Z equals the inverse cumulative standard normal distribution of the placebo p-value. The displayed error bars indicate $+/-$ 1 standard error, which corresponds to the 68.2 percent confidence interval.

Effect sizes in Terciles 1 and 3 are moderately correlated ($\rho = 0.72$), meaning that, on average, SFR-induced changes to Tercile 1 spending also induced changes to Tercile 3 spending. SFRs caused sixteen of 26 states to increase spending; therefore, more than a third of states undergoing SFRs did not increase or reduced spending relative to synthetic counterfactuals. Among states that increased spending to Tercile 1 districts, the correlation between Tercile 1 and Tercile 3 effect sizes is $\rho = 0.38$; in contrast, among states in which spending was less than or equal to zero in Tercile 1 districts, the correlation between Tercile 1 and Tercile 3 effect sizes is $\rho = 0.66$. Thus, when SFRs fail to increase spending relative to counterfactuals, both low- and high-income districts tend to be negatively affected.

Correlations between Terciles 1 and 3 effect sizes for capital and salary expenditures are smaller, at $\rho = 0.50$ and $\rho = 0.59$, respectively. Among Tercile 1 districts, 17 states increased capital spending, and 14 states increased salary spending (results are identical for instructional spending). Among states that increased capital spending, the correlation between Terciles 1 and 3 is 0.47; for states that saw no increase or lost spending, the correlation is 0.07. Similarly, when SFRs increase salary expenditures, the correlation between Terciles 1 and 3 is 0.52, but when SFRs have no effect on salary expenditures, the correlation is 0.15. Thus, when states increase capital and salary spending to Tercile 1 districts as a result of SFRs, Tercile 3 districts are more likely to increase capital and salary spending as well.

The results described here are comparable to the limited number of prior case studies conducted on state-specific SFRs. For Massachusetts, Maryland and Vermont, the positive impact of their states' respective SFRs match work by Chung (2015, Maryland), Dee and Levine (2004) and Guryan (2001) (Massachusetts) and Downes (2004, Vermont). Studies of Kansas identified limited impacts of their respective SFRs, and our results are the same. The fact that our results mostly align with prior case-studies—studies that relied on different methodologies and counterfactuals—lend credibility to our analytic strategy.

For total expenditures in Tercile 1, using placebo p-values, 5 of 26 states had SFRs with effects statistically significantly different at the $p < 0.1$ level (see Table 5), of which three (Ohio, New York, and North Dakota) were positive and two (North Carolina and Texas) were negative. For 9 of 26 states, at least 50 percent of states that never had an SFR had effect sizes (in absolute terms) at least as large as the state with an SFR (i.e., $p < 0.5$ for 9 states). This means that for many of states undergoing an SFR in this period (9 of 26), states that never had an SFR increased spending to low-income districts as much or greater than these states with an SFR. For Tercile 3 (high-income) districts, only 3 (of 26) states had effects at the $p < 0.1$ level (Appendix Table E1). Heteroskedastic robust (HC3) standard errors are much smaller; effect sizes for Tercile 1 districts are significant at the $p < 0.1$ level for 18 or 26 states.

These placebo tests reveal that, in most cases, a state without an SFR was nearly as likely to increase spending in low-income districts as a state undergoing an SFR. Our interpretation of this result is that during this period from 1990–2014 many states experienced significant changes to their school finance regimes. In Michigan, for example, spending in low-income districts increased dramatically after 1994 (Chaudhary, 2009; Cullen & Loeb, 2004; Hyman, 2017; Papke, 2008; Roy, 2011); however, this change did not result from an SFR but from a referendum to the state constitution voted on by the electorate. The 2002 Florida Class Size Amendment is an example of another non-SFR piece of educational legislation that increased both spending and decreased class sizes (Chingos, 2012).

Assessing Spending Preferences. To understand spending preferences of states undergoing SFRs we regress ${\hat{\alpha_0}}^E$ on ${\hat{\alpha_0}}^{tot}$, where E indexes capital, instructional, and salary expenditures and *tot* indexes total expenditures. Because our estimated effect sizes are in log units, we interpret the regression coefficient on ${\hat{\alpha_0}}^{tot}$ as an elasticity.[14] Results from these log-log models are shown in Table 6. The top panel shows results from the regression-calibrated log-log models; results in the bottom panel are from the unadjusted ${\hat{\alpha_0}}^{tot}$ and are, as expected, attenuated.

Across Terciles 1 and 3, elasticities for capital spending are much larger than for instrucitonal or salary spending (top panel, Table 6). Our preferred regression calibrated estimates indicate that a 1 percent increase in total spending results in a 2.7 to 3.6 percent increase in capital spending, whereas a 1 percent increase in total spending results in only a 0.5 to 0.84 percent increase in salary spending. Thus, new construction is the expenditure of choice for states undergoing SFRs. Given that the evidence of capital spending's effects on student achievement is mixed (for an overview, see Jackson, 2018), the overall impact of SFRs on student achievement may be weakened.

[14] Because measurement error on the right-hand side of the equation can result in attenuation bias (Abel, 2017; Garber & Klepper, 1980; Griliches & Hausman, 1986), we use regression calibration to replace the estimated ${\hat{\alpha_0}}^{tot}$ with its best linear prediction (Carroll, Ruppert, Crainiceanu, & Stefanski, 2006; Pierce & Kellerer, 2004). Regression calibration takes advantage of the observed error variance in the right-hand side variables, which we estimate using the HC3 robust standard errors. The method replaces the error-prone variable with its best linear prediction, which can be estimated as a random effect (or empirical Bayes estimate).

Heterogeneity of SFR: Programmatic Changes

Next, we turn to state-specific effect sizes for the number of days in school, the number of minutes in the school day, and kindergarten enrollment. We present effect sizes and 68.2 percent confidence intervals in Figure 3.[15] Because these variables are measured solely at the state level, we do not have a comparison between terciles 1 and 3; however, as with the results discussion on expenditure heterogeneity, the vertical dashed line shows the average of the point estimates and is identical to the "synth" average shown in Table 4.

With respect to measures of the number of instructional days in the school year and the number of minutes in the school day, there are 25 states in the sample.[16] In 15 and 16 of these states, there were increases to minutes and days spent in school, respectively; of these, 2 and 5 were statistically significant at the $p < 0.1$ level based on placebo tests. In general, the magnitudes for these effects are fairly small. Of states with a positive increase to minutes or days in school, the average increase is roughly 5 minutes and 0.5 days, respectively.

In terms of full-time kindergarten enrollment, states are evenly divided between positive and negative effects. For part-time kindergarten, enrollment in part-time kindergarten decreased in 19 states (and increased in 7). There is some evidence that the decline in part-time kindergarten can be explained by states switching to full-day kindergarten: of the 19 states that saw part-time enrollment decline, 10 (of the 13 that expanded to full-time kindergarten) increased enrollment in full-time kindergarten. In general, these estimates are imprecise. Among the positive full-time kindergarten effects, only North Dakota and Washington have point estimates that are statistically significant at the 10 percent level, and among the states with negative effects, Arkansas and Pennsylvania are statistically significant at the 10 percent level. Five of the states that saw

[15] Appendix Table E2 provides placebo and heteroskedastic robust (HC3) p-values.

[16] Indiana has no effect size because it had a reform in 2012, which falls outside the scope of the SASS data from which school length and minutes data come.

declines in part-time kindergarten are also statistically significant: California, Missouri, New Mexico, Ohio and Washington.

Similar to the expenditure results, Figure 3 reveals substantial heterogeneity in terms of programmatic changes made after SFR. There are two key limitations of these findings, however. First, because of data constraints we do not have within-state, district-level information about how these changes might have been more or less prominent in low-income districts. Such data would be useful to test, for example, whether capital expenditures are associated with increases to time spent in school. Second, we only have a subset of potential programmatic changes that states may have pursued. Despite these limitations, however, these results provide insight into the ways states pursue changes in terms of time in school and early childhood education.

Predictors of SFR

Leveraging the 26 point estimates for total expenditures among Terciles 1 (low-income) and 3 (high-income) districts, we now perform a descriptive analysis to assess the extent to which SFR-related policies, political and legal factors, and socio-demographic contexts predict the heterogeneity in effect sizes across states. The descriptive analysis is conducted as a sequence of bivariate regressions between the estimated effect size and the state-level predictor value indexed either to the year immediately after the first SFR or the year immediately prior to the first SFR. We use post-SFR covariate values for SFR-related policy variables, such as funding formula and descriptions of the SFR landscape, and we use pre-SFR covariate values for political and socio-demographic variables. In this way, each estimated effect size is linked to the political and socio-demographic context prior to the SFR and the school finance landscape that emerged after the SFR began. Continuous variables are standardized. Figures 4 and 5 present the results of this analysis. Both figures include separate plots for Tercile 1 and Tercile 3. Given the small sample size, we report point estimates and an error band that contains $+/-$ the standard error, which corresponds to a 68.2 percent confidence interval.

From Figure 4, we see variability in effect sizes by funding formula and funding formula modifiers. Among the funding formulas, states with flat grants, foundation plans, and equalization plans increased spending to low-income districts. States with power equalization plans and Washington, the only state with a centralization plan the first year following an SFR, did not increase spending to low-income districts. Among the funding formula modifiers, states with spending limits increased spending to low-income districts, and states with categorical aid did not. There is no corresponding evidence that these same components increase expenditures per pupil in high-income districts.[17] Regarding the SFR-policy context, we find that effect sizes are larger in situations where a court ruling precedes a legislative action. In states with a single legislative or court event, and in states with multiple events, average estimated effect sizes are close to zero.

Examining the antecedent socio-political correlates in Figure 5, we see that income inequality (especially the top 1 percent income share), liberal citizen ideology, and union strength are associated with positive increases to spending in low-income districts. Demographic variables that include state-level racial and income segregation, average income, and racial composition are uncorrelated with increased spending to low-income districts. Political variables that include institutional ideology and house and senate polarization are also uncorrelated with low-income spending increases. However, spending did decline in high-income districts following SFRs in states with greater senate and house polarization.

Discussion

We now address four related points to provide context for these results. First, many studies have leveraged SFRs (e.g., Brunner et al., 2018; Candelaria & Shores, 2019; Jackson

[17] In Appendix Figure F1, we plot the distribution of "hybrid" funding formula by state (i.e., the first funding formula we observe, by state, after the SFR takes place) combined with the average estimated effect of the SFR for each funding formula combination.

et al., 2016; Klopfer, 2017; Lafortune et al., 2018) to recover exogenous variation in spending that can, in turn, be linked to student outcomes. One possible conclusion from this literature might be that SFRs are an especially useful way to increase spending to low-income districts. Our results suggest that they are effective in the aggregate, but individual states do not consistently increase spending relative to randomly selected states similarly matched to counterfactuals. Other routes, such as demonstrated by Michigan and Florida, which did not have SFRs, appear to be available to increase spending in low-income districts.

Second, we can address where our findings converge (and diverge) from previous studies. First, our aggregate results are mostly in keeping with prior work. Effects are large for low-income districts and larger than effects in high-income districts. Further, expenditure preferences—i.e., in states undergoing SFRs, the percent increase for capital spending is larger than it is for salary spending—has not been tested previously. The main difference between our results and prior studies is that our placebo tests indicate states without SFRs had similar response patterns to states with SFRs, once matched to counterfactuals.The 1990–2014 period is one in which multiple states, with and without SFRs, were increasing spending to low-income districts.

Third, though the placebo tests show that a randomly selected state is, in many cases, as likely to increase spending for low-income districts during this period, this does not mean that in the absence of SFRs spending would have increased similarly for lower income districts. Indeed, one explanation for the results from these placebo tests is that SFRs are effective at convincing states to adopt more progressive school spending policies. In other words, states may copy or adopt SFR-related funding formula in response to other states going through SFRs, either because they wish to avoid litigation or because states recognize that these formula changes are useful. Currently, we do not have counterfactuals for addressing whether the large-scale changes in school finance can be attributed, at least in part, to SFRs, but it remains a possibility.

Finally, heterogeneity in treatment effects is important for multiple reasons. Either because of the risks and costs of adopting certain reforms, or because reforms may be more effective in some contexts than others—heterogeneity is a key feature of policy evaluation. However, much if not all of the research documenting treatment effect heterogeneity has come from randomized controlled trials. Many important phenomenon, like SFRs, are not subject to randomization and, up until recently, have not been evaluated with heterogeneity in mind. Our application of synthetic controls in this context is therefore an important methodological contribution. When sufficiently long time-series data are available, the methods used here provide one pathway for providing deeper understanding of the heterogeneity in the causal impacts of policies, as well as descriptive information about the variables predictive of this variation.

Conclusion

Consistent with recent studies in the public finance of education literature, this paper finds that school finance reforms (SFRs) increased spending per pupil more in low-income districts relative to high-income districts (Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018). We show that this result holds using two different methods. First, we estimate a standard difference-in-differences model and find that SRFs increased expenditures per pupil by about 9.5 percent, on average, in low-income districts. Second, we implement an estimation strategy that combines the synthetic controls method (Abadie et al., 2010) with multiple treated units (e.g., Acemoglu, Johnson, Kermani, Kwak, & Mitton, 2016; Billmeier & Nannicini, 2013; Cavallo, Galiani, Noy, & Pantano, 2013b) in a difference-in-differences framework (Arkhangelsky et al., 2018) and find that the average estimate across states is approximately 7.5 percent in low-income districts. Overall, these point estimates are qualitatively comparable to recent school finance studies. (Candelaria & Shores, 2019; Jackson et al., 2016; Lafortune et al., 2018).

More importantly, this paper provides novel, compelling evidence about the substantial heterogeneity of state-specific responses to SFRs. By using the synthetic control method in a difference-in-differences framework (Arkhangelsky et al., 2018), we estimate effect sizes at the state-by-income tercile level for each state that had an SFR. This enables us to quantify how expenditure allocations varied across states. In 13 states with an SFR in this period, both low- and high-income districts increased spending, while in 8 states, both low- and high-income districts decreased spending. States further varied in their spending preferences and programmatic implementation. States increased spending more to capital than to salaries; however, 8 (of 17) states that increased capital spending did not increase personnel spending and 5 (of 14) states that increased personnel spending did not increase capital. Programmatic changes at the state level were also variable; however, many of these outcomes were imprecisely estimated. One important takeaway from this analysis is that average effects mask heterogeneity; therefore, leveraging methods that provide state-specific estimates, such as synthetic controls, is useful to better understand the distribution that underlies the average.

Finally, to our knowledge, this paper is the first to leverage the variability in estimated effects for prediction purposes. Most research describing effect size heterogeneity is limited to randomized controlled trials (RCT) (see, e.g., Connors & Friedman-Krauss, 2017; Weiss et al., 2017). However, many socially relevant programs are not subject to randomization, and generalizing evidence from RCTs to external populations is challenging, even in cases where randomization is possible (Deaton & Cartwright, 2018). Using quasi-experimental methods, such as synthetic controls, we estimate unit-specific effects to conduct descriptive analysis in the context of effect size heterogeneity. Though this prediction analysis is both exploratory and descriptive, this type of research provides information about the contexts in which SFRs are most effective.

Because SFRs are costly and consequential for both educational and non-educational expenditures (Baicker & Gordon, 2006), it is useful to know which reforms worked and to be able to describe the contexts in which SFRs were most productive. With more evidence suggesting that money matters for educational outcomes, researchers will need to better understand the conditions and contexts in which money is most productive. By unmasking the heterogeneity underlying an average treatment effect, researchers should be able to better guide policy.

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Tables

Table 1
Predictor Variables and Expected Relation to Progressivity

Variable	Expected Relationship	Citation
Panel A: SFR-Related Policies		
Funding Formulas	+/−	Card & Payne (2002); Hoxby (2001)
Foundation Plan		
Flat Grant		
Equalization		
Power Equalization		
Centralization		
(+) Categorical Aid		
(+) Spending Limits		
Funding Formula Variability	+/−	Card & Payne (2002); Hoxby (2001)
Single Legislative Act	+/−	Langer & Brace (2005); Roch & Howard (2008)
Legislation Following Court Ruling	+/−	Langer & Brace (2005); Roch & Howard (2008)
Single Court Rulings	+/−	Langer & Brace (2005)
Panel B: Political and Legal Factors		
Citizen Ideology	+	Wood & Theobald (2003)
State Congressional Polarization	+/−	Voorheis et al. (2015)
State Congressional Partisanship	−	Voorheis et al. (2015)
Teacher Union Strength	+	Brunner et al. (2018)
Panel C: Socioeconomic and Demographic Variables		
Income per capita	+	Baker et al. (2014)
Income Inequality	+	Loeb (2001)
Racial Segregation	−	Ryan (1999)
Racial Composition	−	Alesina et al. (2001)

Table 2
Dependent Variable Summary Statistics Among States with SFRs

Panel A: Tercile-Level Variables	Tercile 1		Tercile 3	
	Mean	S.D.	Mean	S.D.
log(Revenues per Pupil)	9.34	0.33	9.27	0.27
log(Expenditures per Pupil)	9.34	0.34	9.28	0.27
log(Instructional Expenditures per Pupil)	8.71	0.33	8.64	0.27
log(Tch. Salary Expenditures per Pupil)	8.73	0.27	8.69	0.24
log(Capital Expenditures per Pupil)	6.43	0.77	6.50	0.64
Teachers per 100 Students	6.53	1.15	6.02	0.85

Panel B: State-Level Variables	State	
	Mean	S.D.
Pct. Full Day Kindergarten	0.56	0.25
Pct. Part-Time Kindergarten	0.44	0.25
Days in School	179.0	2.36
Minutes in School Day	394.8	14.9

Notes: Summary statistics are computed for all states that had a court-ordered or legislative school finance reform. All variables, except Days in School and Minutes in School Day, span academic years 1989–90 to 2013–14. Days in School and Minutes in School Day, which come from the Schools and Staffing Survey, span academic years 1989–90 to 2011–12.

Table 3
Match Quality Summary Statistics for Synthetic Controls: Log(Expenditures per Pupil)

	Tercile 1				Tercile 3			
	Min Abs(Effect)	Max to Min Abs(Effect)	Dep. Var.	Min. Effect	Min Abs(Effect)	Max to Min Abs(Effect)	Dep. Var.	Min. Effect
AK	0.060	10.522	10.093	0.012	0.024	2.484	9.398	0.002
AR	0.005	4.688	8.752	0.002	0.005	2.825	8.773	-0.001
AZ	0.004	4.582	8.918	0.000	0.018	2.348	8.929	-0.004
CA	0.027	1.201	9.164	-0.001	0.007	1.837	8.970	-0.002
CO	0.010	1.910	9.038	0.001	0.006	1.628	9.129	-0.000
ID	0.000	318.093	8.650	0.000	0.007	1.895	8.681	-0.001
IN	0.014	1.355	9.326	-0.004	0.021	1.524	9.278	0.002
KS	0.002	3.847	9.042	-0.001	0.001	17.710	9.050	0.001
MA	0.006	1.248	9.123	-0.001	0.002	1.895	9.265	0.000
MD	0.003	3.520	9.154	0.000	0.023	5.121	9.316	0.018
MO	0.001	13.745	8.729	-0.000	0.000	33.489	8.986	0.000
MT	0.000	70.545	9.013	-0.000	0.002	7.275	8.905	-0.000
NC	0.009	1.280	9.007	-0.009	0.007	1.475	9.074	-0.002
ND	0.014	1.764	9.153	0.000	0.020	1.660	8.971	-0.009
NH	0.013	1.413	9.181	0.002	0.004	2.191	9.125	0.001
NJ	0.010	9.507	9.539	0.003	0.001	126.461	9.631	0.000
NM	0.021	2.011	8.931	0.007	0.011	2.082	8.887	0.010
NY	0.007	10.684	9.614	0.000	0.010	23.231	9.735	-0.001
OH	0.001	11.248	9.031	0.000	0.002	4.659	9.058	0.000
PA	0.012	1.607	9.381	-0.003	0.008	1.978	9.491	-0.002
TN	0.001	8.788	8.823	-0.000	0.000	693.889	8.734	-0.000
TX	0.000	459.444	8.920	-0.000	0.000	113.499	8.933	-0.000
VT	0.015	1.114	9.388	0.001	0.005	4.737	9.429	-0.001
WA	0.013	1.848	9.209	0.000	0.034	1.268	9.249	0.016
WV	0.009	1.864	9.107	-0.001	0.019	1.283	9.116	0.000
WY	0.011	1.511	9.141	0.001	0.006	1.791	9.152	-0.001
Overall	0.013	15.127	9.218	0.000	0.013	18.019	9.200	0.002

Notes: Column "Min Abs(Effect)" is the cumulative sum of the absolute effect size for the pre-SFR period for the model that minimizes this statistic; the statistic indicates how well the counterfactual units match the treated unit. Column "Max to Min Abs(Effect)" is the ratio of the cumulative sums of the models that have the maximum and minimum absolute effect sizes for the pre-SFR period; the statistic indicates whether the minimizing model has better match quality than the maximizing model. Column "Dep. Var" is the mean of the dependent variable for the pre-SFR period; this value can be used to benchmark the cumulative absolute effect size from column 1. Column "Min. Effect" is the mean effect size for the pre-SFR period for the model that minimizes the cumulative sum of the absolute effect size in this period; the statistic is useful to evaluate the difference-in-differences assumption that restricts the pre-SFR period to be equal to zero.

Table 4
Difference-in-Differences with and without Synthetic Controls

Panel A: Tercile-Level Outcomes	Tercile 1		Tercile 3	
	Non-Synth	Synth	Non-Synth	Synth
log(Expenditures per Pupil)	0.095 (0.022)	0.075 [0.029]	0.031 (0.019)	0.016 [0.022]
log(Instructional Expenditures per Pupil)	0.081 (0.026)	0.014 [0.027]	0.038 (0.016)	0.003 [0.016]
log(Tch. Salary Expenditures per Pupil)	0.081 (0.025)	0.044 [0.030]	0.038 (0.019)	0.061 [0.020]
log(Capital Expenditures per Pupil)	0.319 (0.127)	0.386 [0.147]	0.012 (0.077)	0.101 [0.118]
Teachers per 100 Students	0.368 (0.128)	0.402 [0.218]	0.196 (0.080)	0.246 [0.097]

Panel B: State-Level Outcomes	State	
	Non-Synth	Synth
Pct. Full Day Kindergarten	0.039 (0.045)	-0.015 [0.078]
Pct. Part-Time Kindergarten	-0.039 (0.045)	-0.065 [0.028]
Days in School	-0.064 (0.324)	0.175 [0.225]
Minutes in School Day	2.973 (1.529)	2.570 [1.149]

Notes: Columns "Non-Synth" correspond to a version of Equation (1) that includes all treated and non-treated states and weights each non-treated state equally. Cluster robust standard errors in parentheses. Columns "Synth" correspond to the mean of the vector α_0 from Equation (1). Sample standard deviation computed across state-by-income terciles in brackets.

Table 5

Difference-in-Differences P-values, Placebo and Heteroskedastic-Robust (HC3) Methods, by State: Tercile 1 (Low-Income)

	Total Expenditures		Capital Expenditures		Salary Expenditures		Teachers per 100 Students	
State	Placebo	HC3	Placebo	HC3	Placebo	HC3	Placebo	HC3
AK	0.870	0.348	0.478	0.003	0.913	0.000	0.619	0.049
AR	0.227	0.000	0.391	0.000	0.545	0.000	0.000	0.000
AZ	0.174	0.000	0.174	0.782	0.261	0.000	0.900	0.006
CA	0.609	0.000	0.087	0.000	0.783	0.020	0.700	0.343
CO	0.348	0.144	0.870	0.528	0.591	0.186	0.476	0.008
ID	0.435	0.064	0.045	0.005	0.773	0.087	0.300	0.000
IN	0.652	0.764	0.818	0.858	1.000	0.990	0.143	0.000
KS	0.864	0.849	0.545	0.053	0.591	0.001	0.524	0.028
MA	0.500	0.000	0.565	0.000	0.261	0.000	.	.
MD	0.217	0.000	0.522	0.001	0.391	0.000	0.200	0.000
MO	0.773	0.562	0.227	.	0.591	0.228	0.381	0.009
MT	0.318	0.000	0.130	0.030	0.091	0.000	0.550	0.000
NC	0.087	0.000	0.455	0.008	0.045	0.000	0.238	0.004
ND	0.000	0.000	0.227	0.049	0.043	0.000	0.150	0.000
NH	0.652	0.000	0.591	0.000	0.304	0.000	0.000	0.000
NJ	1.000	0.845	0.783	0.392	0.826	0.190	0.550	0.111
NM	0.348	0.000	0.348	0.000	0.545	0.000	0.714	0.000
NY	0.000	0.000	0.364	0.000	0.182	0.000	0.095	0.000
OH	0.087	0.000	0.087	0.000	0.522	0.491	0.524	0.010
PA	0.318	0.000	0.455	0.438	0.227	0.006	0.050	0.000
TN	0.364	0.061	0.955	0.944	0.636	0.059	.	.
TX	0.087	0.106	0.545	0.839	0.091	0.161	0.048	0.037
VT	0.565	0.000	0.455	0.000	0.227	0.000	0.619	0.000
WA	0.130	0.008	0.261	0.000	0.261	0.479	0.150	0.066
WV	0.783	0.735	0.565	0.057	0.217	0.603	0.000	0.003
WY	0.391	0.000	0.565	0.000	0.000	0.000	0.714	0.576

Table 6

Elasticities: Total per Pupil Expenditures and Spending Sub-Categories

	Regression Calibrated Estimates			
	Tercile 1		Tercile 3	
	Capital	Salaries	Capital	Salaries
Beta EB	2.717*	0.836***	3.638**	0.505**
	(0.998)	(0.163)	(0.988)	(0.179)
	OLS Estimates			
	Tercile 1		Tercile 3	
	Capital	Salaries	Capital	Salaries
Beta	2.578**	0.762***	3.294**	0.477**
	(0.896)	(0.148)	(0.895)	(0.160)

Notes: Standard errors in parentheses. Beta EB (top panel) displays results from a log-log model in which $\hat{\alpha_0}^{tot}$ is replaced with its best linear unbiased predictor (empirical Bayes estimate). Beta (bottom panel) displays results using the error-prone variable. Coefficients with superscript a are significant at the 0.1% level; b at the 1% level; c at the 5% level; and d at the 10% level.

Figures

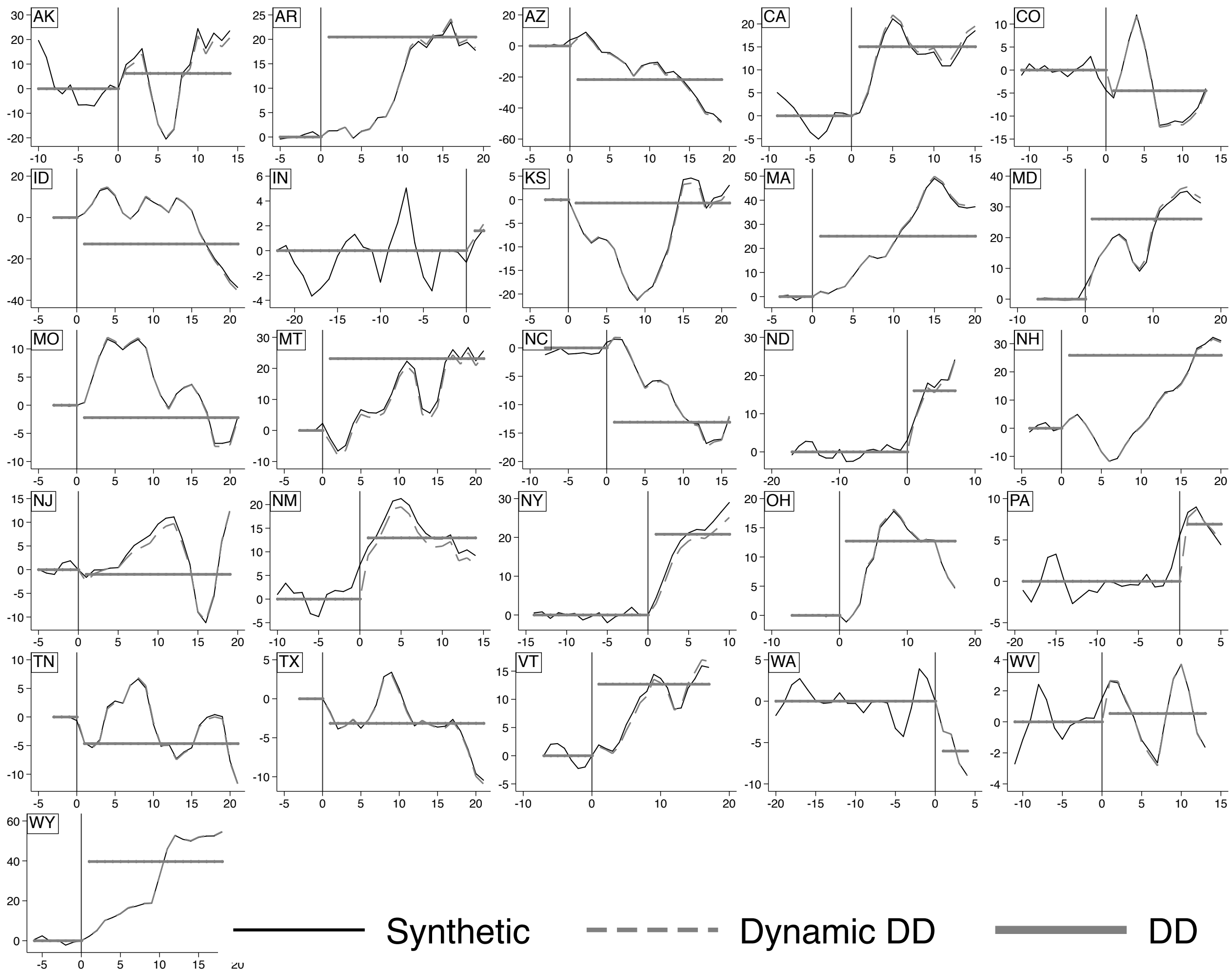

Figure 1. Synthetic Controls and Difference-in-Differences Effect Sizes: Tercile 1 Log Per Pupil Total Expenditures

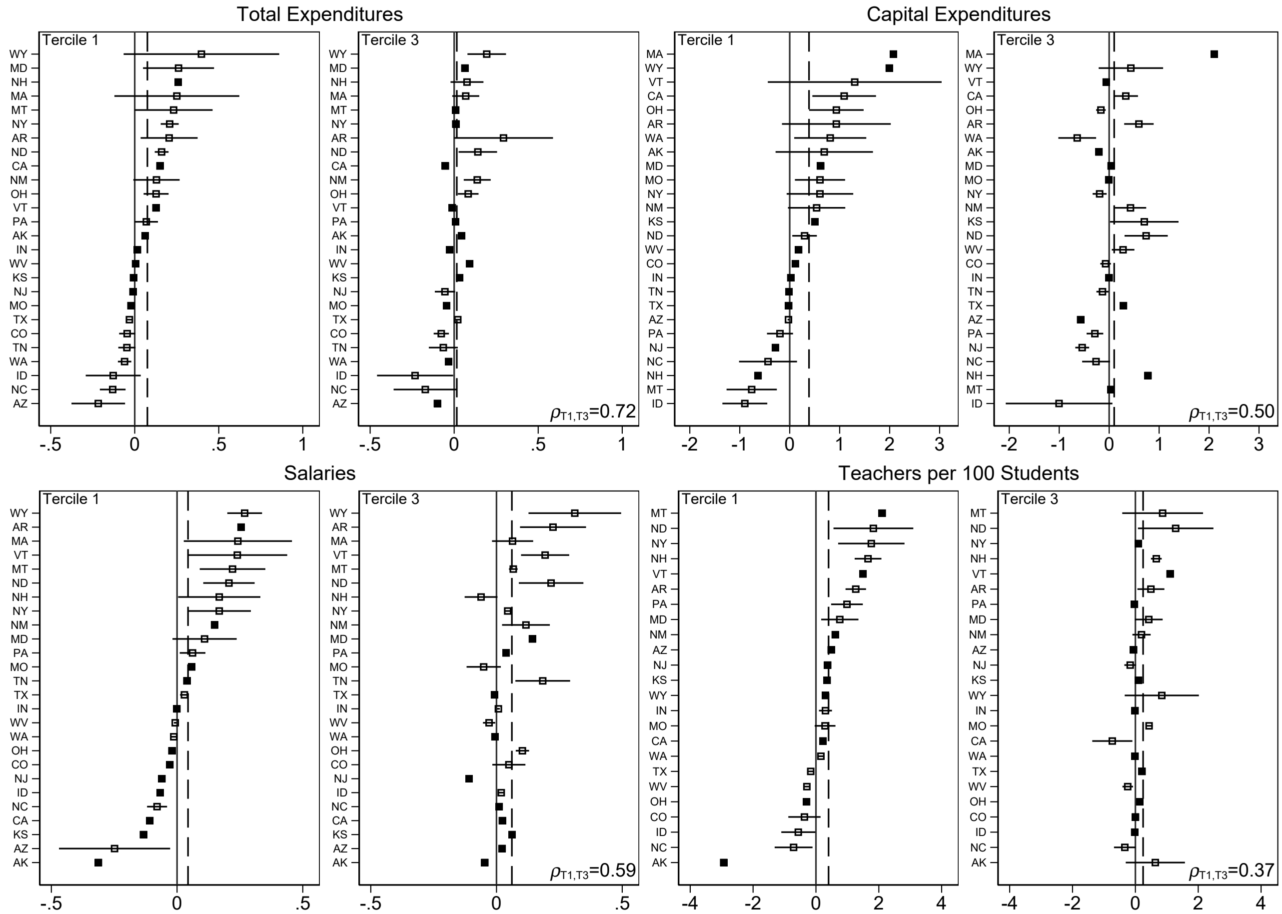

Figure 2. Synthetic Controls Results: Terciles 1 (Low-Income) and 3 (High-Income) Multiple Outcomes

Estimates of α_0 from Equation (1) are shown for each state with an SFR. Range caps are 68.2% confidence intervals based on placebo p-values converted to standard errors. Solid black markers indicate cases where $p > 0.5$; confidence intervals not shown in those cases. Values of ρ are unweighted correlation coefficients between Terciles 1 (low-income) and 3 (high-income) point estimates.

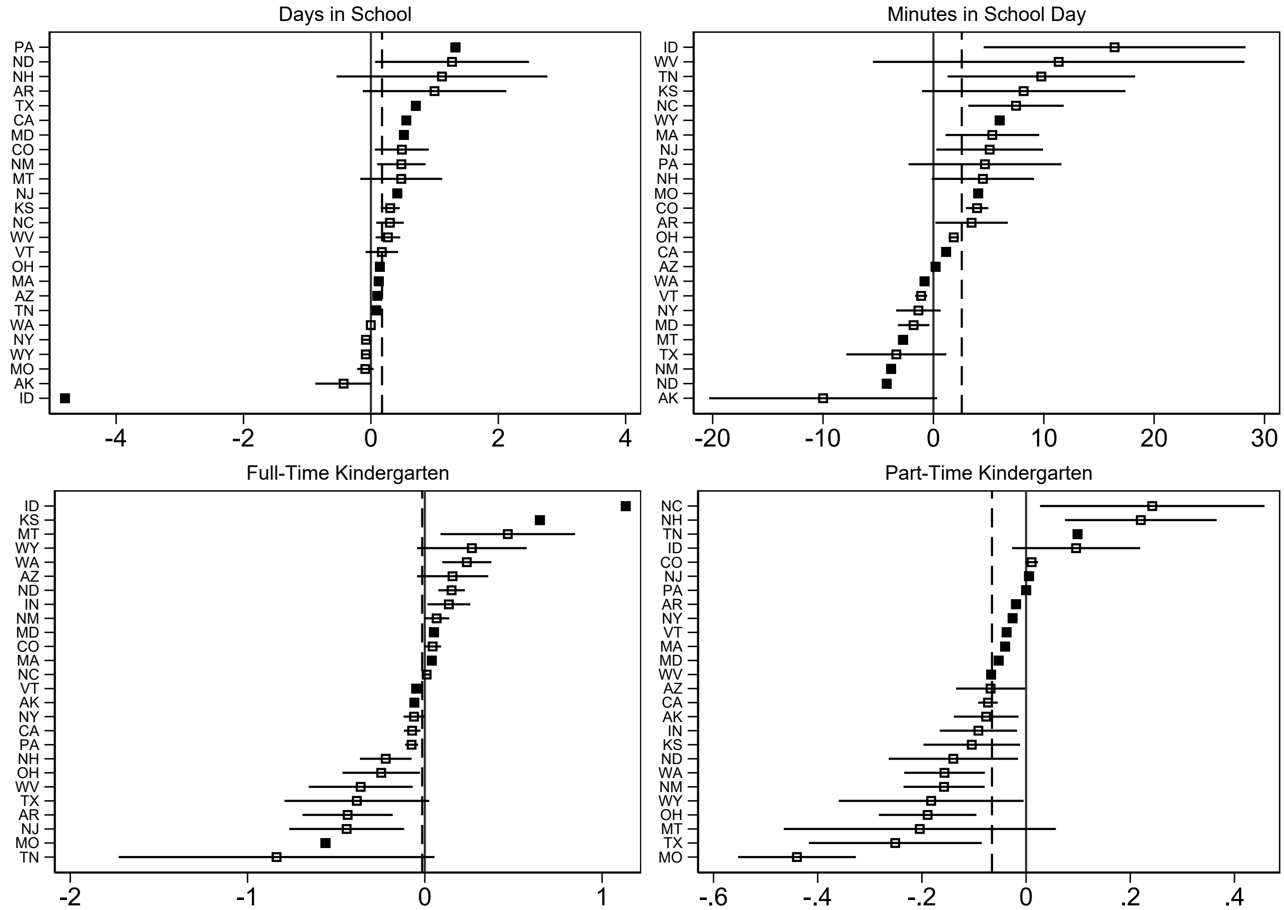

Figure 3. Synthetic Controls Results: State-Average Multiple Outcomes
Notes: Estimates of α_0 from Equation 1 are shown for each state with an SFR. Range caps are 68.2% confidence intervals (i.e., plus or minus one standard error) based on placebo p-values converted to standard errors. Solid black markers indicate cases where $p > 0.5$; confidence intervals not shown in those cases.

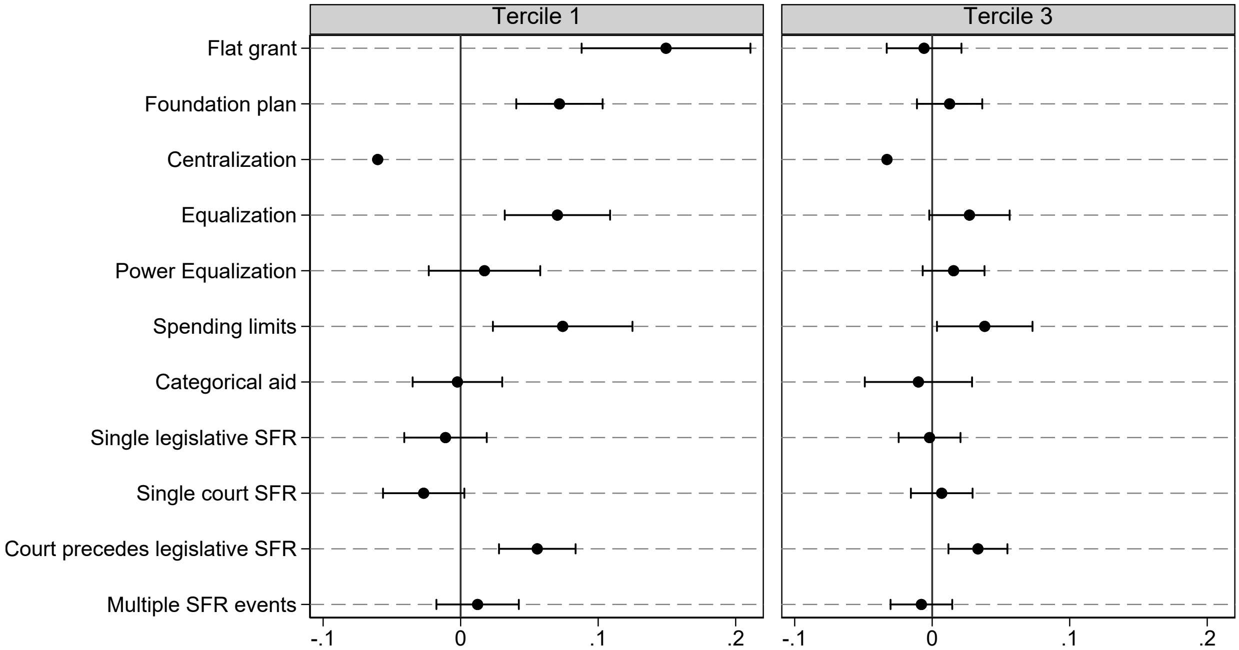

Figure 4. Predictors of SFR Heterogeneity: Log Total Expenditures
Notes: List of predictors and data sources are taken from Table 1. Range caps are 68.2% confidence intervals based on placebo p-values converted to standard errors. Solid black markers indicate cases where $p > 0.5$.

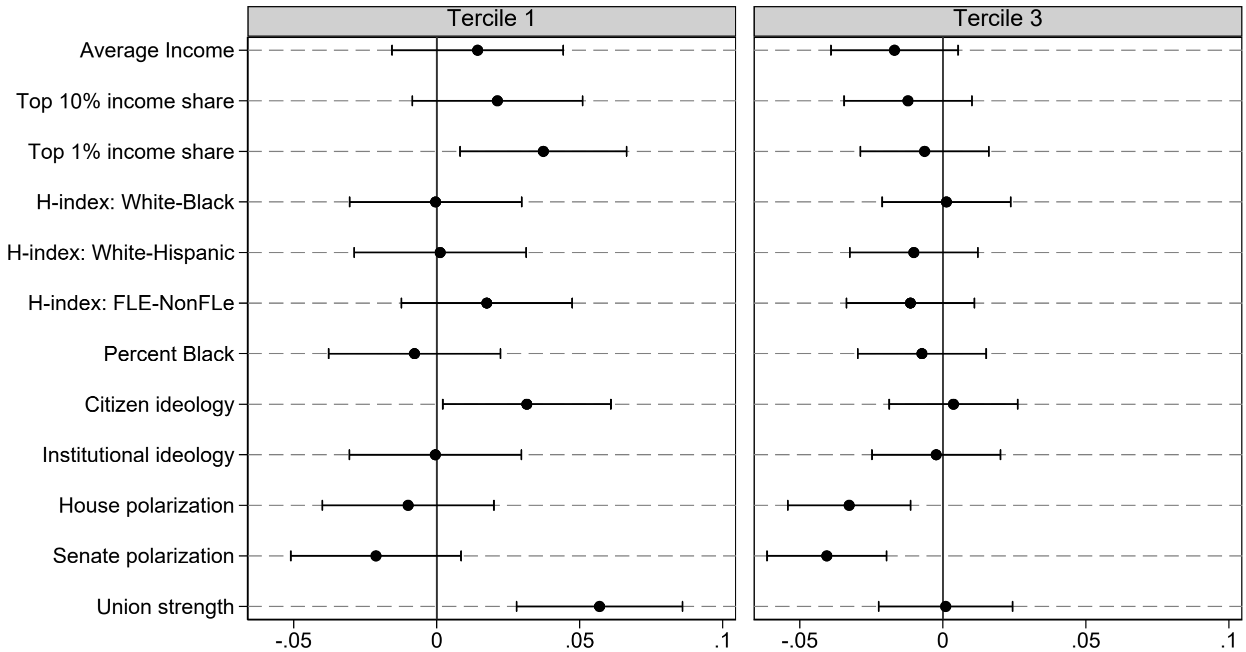

Figure 5. Predictors of SFR Heterogeneity: Log Total Expenditures
Notes: List of predictors and data sources are taken from Table 1. Range caps are 68.2% confidence intervals based on placebo p-values converted to standard errors. Solid black markers indicate cases where $p > 0.5$.

Appendix A

List of Reforms

Table A1
Court-Ordered and Legislative School Finance Reforms

State	Court Case or Legislative Bill	Historical Decision Date	Converted Academic Year
Alaska	Kasayulie v. State of Alaska	1-Sep-99	2000
Arizona	Roosevelt v. Bishop	21-Jul-94	1995
Arizona	Hull v. Albrecht	23-Dec-97	1998
Arizona	Hull v. Albrecht	18-Feb-98	1998
Arkansas	Lake View v. Arkansas	1-Dec-94	1995
Arkansas	Approved Equitable School Finance Plan (Acts 917, 916, and 1194)	1-Feb-95	1996
Arkansas	Lake View v. Huckabee	21-Nov-02	2003
Arkansas	Lake View v. Huckabee	5-May-05	2005
Arkansas	Various acts resulting from Master's Report findings	5/31/07	2007
California	Leroy F. Greene School Facilities Act of 1998	27-Aug-98	1999
California	Senate Bill 6, Senate Bill 550, Assembly Bill 1550, Assembly Bill 2727, and Assembly Bill 3001	1-Aug-04	2005
Colorado	Bill 181; ; Various Other Acts	1-Jul-00	2001
Idaho	Idaho Schools for Equal Educational	18-Mar-93	1993
Idaho	Opportunity v. Evans (ISEEO) Senate Bill 1560	1-Mar-94	1994
Idaho	Idaho Schools for Equal Educational Opportunity v. Evans (ISEEO V)	21-Dec-05	2005
Indiana	HB 1001 (Pl229)	1-Jul-11	2012
Kansas	The School District Finance and Quality Performance Act	1-Jul-92	1993
Kansas	Montoy v. State; Montoy v. State funding increases	3-Jan-05	2005
Kentucky	**Rose v. Council for Better Education, Inc.**	**28-Sep-89**	**1990**
Kentucky	**Kentucky Education Reform Act (HB 940)**	**24-Mar-90**	**1990**
Maryland	Bradford v. Maryland State Board of Education	18-Oct-96	1997

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Table A1 – continued from previous page.

State	Court Case or Legislative Bill	Historical Decision Date	Converted AY
Maryland	EducationBridge to Excellence in Public Schools	6-May-02	2002
Massachusets	McDuffy v. Secretary of the Executive Office of Education; Massachusetts Education Reform Act	15-June-93 (court); 18-June-93 (bill)	1994
Missouri	Committee for Educational Equality v. State of Missouri	1-Jan-93	1993
Missouri	Outstanding Schools Act (S.B. 380)	1-Aug-93	1994
Missouri	Senate Bill 287	29-Jun-05	2006
Montana	House Bill 667	1-Apr-93	1993
Montana	Columbia Falls Elementary School v. State	22-Mar-05	2005
Montana	M.C.A. Âğ 20-9-309	1-Oct-07	2008
Montana	Montana Quality Education Coalition v. Montana	15-Dec-08	2009
New Hampshire	Claremont New Hampshire v. Gregg	30-Dec-93	1994
New Hampshire	Claremont School District v. Governor	17-Dec-97	1998
New Hampshire	Claremont v. Governor (Claremont III); RSA chapter 193-E	15-Oct-99	2000
New Hampshire	Opinion of the Justices–School Financing (Claremont VI)	7-Dec-00	2001
New Hampshire	Claremont School District v. Governor	11-Apr-02	2002
New Hampshire	Londonderry School District v. New Hampshire	8-Sep-06	2007
New Hampshire	SB 539	21-Apr-08	2008
New Jersey	**The Quality Education Act; Abbot v. Burke**	**05-Jun-90 (court); July-90 (law)**	**1991**
New Jersey	Abbott v. Burke	12-Jul-94	1995
New Jersey	Comprehensive Educational Improvement and Financing Act of 1996	20-Dec-96	1997
New Jersey	Special Master's Report; Abbott v. Burke	14-May-97	1997
New Jersey	Abbott v. Burke	21-May-98	1998
New Jersey	Abbott v. Burke	7-Mar-00	2000
New Jersey	The School Funding Reform Act of 2008	1-Jan-08	2008
New Mexico	Zuni School District v. State	14-Oct-99	2000
New Mexico	Deficiencies Corrections Program; Public School Capital Outlay	5-Apr-01	2001

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Table A1 – continued from previous page.

State	Court Case or Legislative Bill	Historical Decision Date	Converted AY
New York	Act Campaign for Fiscal Equity, Inc. v. State	26-Jun-03	2004
New York	Campaign for Fiscal Equity, Inc. v. State	20-Nov-06	2007
New York	Education Budget and Reform Act	1-Apr-07	2007
North Carolina	Leandro v. State	24-Jul-97	1998
North Carolina	Hoke County Board of Education v. State	30-Jul-04	2005
North Dakota	SB 2200	3-May-07	2007
Pennsylvania	Act 61	9-July-08	2009
Ohio	DeRolph v. Ohio	25-Apr-97	1997
Ohio	DeRolph v. Ohio	11-May-00	2000
Ohio	Increased school funding (see 93 Ohio St.3d 309)	14-Sep-00	2001
Ohio	DeRolph v. Ohio	11-Dec-02	2003
Tennessee	**The Education Improvement Act**	**11-Mar-92**	**1992**
Tennessee	Tennessee Small School Systems v. McWherter	22-Mar-93	1993
Tennessee	Tennessee Small School Systems v. McWherter	16-Feb-95	1995
Tennessee	Tennessee Small School Systems v. McWherter	8-Oct-02	2003
Texas	**Edgewood Independent School District v. Kirby**	**22-Jan-91**	**1991**
Texas	**Carrolton-Farmers Branch ISD v. Edgewood Independent School District**	**30-Jan-92**	**1992**
Texas	Senate Bill 7	31-May-93	1993
Vermont	Brigham v. State	5-Feb-97	1997
Vermont	Revisions to Act 68; H.480	18-Jun-03	2004
Washington	McCleary v. State	1-Feb-10	2010
West Virginia	Tomblin v. Gainer	1-Aug-00	2001
Wyoming	Campbell County School District v. State	8-Nov-95	1996
Wyoming	The Education Resource Block Grant Model	April, 1997	1997
Wyoming	Wyoming Comprehensive Assessment System	June, 1997	1998
Wyoming	Campbell II; Recalibration of the MAP model	23-Feb-01	2001

Notes: Cases and bills in bold typeface are excluded from the analysis, as they are early in the sample. We require the first case to occur in academic year 1992–1993 (i.e., academic year 1993) in order to establish a sufficient baseline trend for synthetic control matching.

Appendix B

Generating Panel Dataset of Funding Formula

Linking the adopted funding formula to the time at which an SFR takes place requires a panel dataset of funding formulas, one that varies by state and year. We construct such a state-by-year dataset of funding formulas compiled from multiple studies and reports (Card & Payne, 2002; Hightower, Mitani, & Swanson, 2010a; Jackson et al., 2016; Lafortune et al., 2018; Sielke, Dayton, Holmes, Jefferson, & Fowler, 2001; Verstegen, 2017).

To build the panel dataset of state funding formula, we catalog state funding formulas identified by the sources (Card & Payne, 2002; Hightower et al., 2010a; Jackson et al., 2016; Lafortune et al., 2018; Sielke et al., 2001; Verstegen, 2017). These studies and reports include funding formula information across time from 1989-2008. Obtaining contemporary descriptions of state-level funding formula is challenging because public records of these formula vary in their degree of specificity and availability online. Obtaining these descriptions for the entire sample period is more challenging due to the limited availability of these formula in the public record. To obtain a historical record of state-level funding formula, we access prior studies and surveys that have indexed these formula. We do not access information directly from state archives; rather, we evaluate reports of funding formula that have been published over time, and additionally take advantage of studies that have collated these publications and historical archives.

Jackson et al. (2016) provide the only compendium of funding formula for multiple states and years. We use this data source for all states and years included in our study. We complement this database with additional years and states by taking state funding formula descriptions from Card and Payne (2002); Hightower, Mitani, and Swanson (2010b); Sielke et al. (2001); Verstegen (2017). Specifically, we obtain descriptions of funding formula for all available states in 1990 from Card and Payne (2002) and for 1998 from Sielke et al. (2001) and for 2006 from Verstegen (2017) and for 2008 from Hightower et al. (2010b).

Survey methods for collecting data vary across studies and lead to inconsistencies in information and terminology. In some cases this is minor, such as differences in describing foundation plans as“minimum foundation plan” (Card & Payne, 2002), “foundation program” (Verstegen, 2017), or “foundation aid” (Hoxby, 2001). However, particularly when discussing equalization plans, there is a wider variety of terms used to describe what we call “equalization” and “power equalization.” For example, power equalization is referred to as “local effort equalization” (Hightower et al., 2010b), “district power equalization” (Verstegen, 2017), and “variable guarantee” (Card & Payne, 2002). We observed some degree of inconsistent classifications in all of the funding formula we classified.

Although we find differences in how researchers label formula components, we are able to identify common definitions across studies. We reviewed the literature and collated funding formula terms and definitions across sources. Through an analysis of definitions and descriptions we were able to combine like terms into four main categories: flat grant, equalization, foundation, and other. Through this exercise we cross-checked definitions to ensure that, for example, the usage of “flat grant” or “foundation aid” was consistent across studies, regardless of specific nomenclature. Further analysis indicated that equalization needed to be split into two categories: equalization and power equalization, where power equalization indicates a direct relationship between level of local effort and level of state aid. The “other” category included full state funding, and what we came to call “add on” components, including spending limits and categorical aid. Next we provide the funding formula terms and definitions that emerged from our synthesis.

Foundation plans provide a guaranteed amount of funding per pupil in each district. Under this plan, the state utilizes block grants to supplement a district’s expected contribution so that the guaranteed minimum is met. Flat grants are used to provide a statewide uniform dollar amount per pupil, with any additional spending provided by local revenue. Flat grants do not rely on local spending effort, and are often combined with other funding formula adjustments. Equalization is a block grant that varies based on district tax base or tax revenue. These are differentiated from foundation grants in that they are not meant to standardize to a minimum level of spending per pupil, but rather intended to add to district spending based on some observable characteristic, such as local income level. These are distinct from power equalization grants. Power Equalization is a matching grant, wherein local effort and tax levels are directly tied to additional state funding. Power equalization grants ensure that districts with the same tax rate have the same amount of money to spend per pupil, regardless of taxable wealth in their district. Finally, centralization plans are those in which the state assesses, levies, and distributes all tax funding related to school financing.

We identify two additional “add-on” components of the state funding formulas. These are always used in conjunction with one or more of the five core formula types. Spending limits create a ceiling on property taxes or the amount of funding that can be added to state foundation plans. Categorical aid components may be distributed on a per-pupil basis for students who fit in a defined category. Special education spending by the state is often in the form of categorical aid, where a set dollar amount is provided for each pupil served by the program.

Appendix C

Funding Formulas

Table C1

Funding Formula Distribution

State	Funding Formula Components	FF Changes
AK	FP+EQ	0
AR	FP+EQ+SL	0
AZ	FP+EQ+PE+SL	0
CA	FP+FG	0
CO	FP+EQ+CA	0
ID	FP	0
IN	FP+PE+CA+SL	0
KS	FP+EQ+PE+SL	0
MA	FP	0
MD	FP+PE+FG	0
MO	FP+EQ+PE+FG	0
MT	FP+EQ+SL	0
NC	FP+EQ+CA	0
ND	EQ+PE	0
NH	FP	0
NJ	FP+EQ	3
NM	FP+EQ+CA	0
NY	EQ+FG	0
OH	FP	0
PA	FP+EQ+PE+CA	0
TN	FP	4
TX	FP+EQ+PE+SL	3
VT	FP+EQ+SL	0
WA	PE+CA+SL+CN	0
WV	FP+PE+CA+SL	0
WY	FP+EQ+SL	0
WY	FP+EQ+SL	0

Notes: Funding formulas listed correspond to the funding formula in place at the start of the first reform.

Appendix D

Synthetic Controls Effects Tercile 3

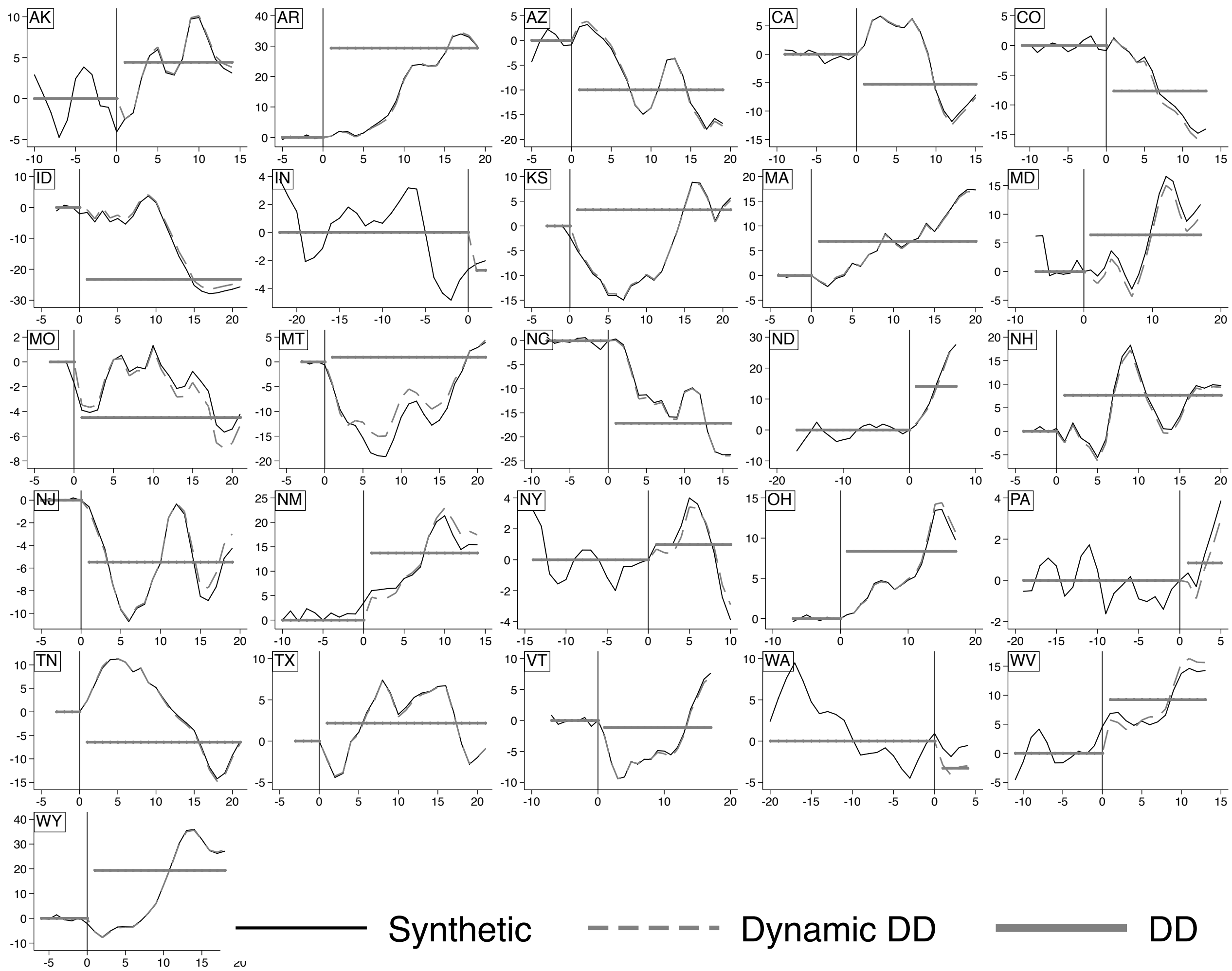


Figure D1. Synthetic Controls and Difference-in-Differences Effect Sizes: Tercile 3 Log Per Pupil Total Expenditures

Appendix E

Difference-in-Differences Placebo P-values, Tercile 3 and State Averages

Table E1

Difference-in-Differences p-values, Multiple Versions, by State: Tercile 3 (High-Income)

	Total Expenditures		Capital Expenditures		Salary Expenditures		Teachers per 100 Students	
State	Placebo	HC3	Placebo	HC3	Placebo	HC3	Placebo	HC3
AK	0.727	0.043	0.870	0.213	0.636	0.008	0.500	0.000
AR	0.318	0.000	0.043	0.000	0.087	0.000	0.250	0.000
AZ	0.783	0.004	0.739	0.000	0.591	0.266	1.000	0.661
CA	0.565	0.157	0.174	0.001	0.909	0.134	0.250	0.001
CO	0.091	0.001	0.500	0.387	0.455	0.000	0.857	0.929
ID	0.304	0.000	0.348	0.000	0.174	0.490	0.952	0.744
IN	0.636	0.388	1.000	0.990	0.182	0.883	0.952	0.896
KS	0.818	0.204	0.304	0.000	0.864	0.005	0.700	0.377
MA	0.391	0.024	0.609	0.000	0.435	0.014	.	.
MD	0.826	0.029	0.652	0.633	0.739	0.000	0.333	0.000
MO	0.652	0.014	1.000	0.951	0.455	0.047	0.000	0.000
MT	0.652	0.850	0.909	0.896	0.000	0.000	0.500	0.001
NC	0.364	0.000	0.348	0.004	0.909	0.549	0.333	0.025
ND	0.217	0.009	0.087	0.040	0.091	0.000	0.286	0.000
NH	0.435	0.002	0.870	0.033	0.348	0.019	0.000	0.001
NJ	0.364	0.051	0.000	0.000	0.682	0.000	0.381	0.309
NM	0.087	0.000	0.174	0.001	0.217	0.000	0.500	0.021
NY	0.913	0.614	0.174	0.046	0.000	0.010	0.850	0.488
OH	0.174	0.000	0.087	0.164	0.000	0.000	0.810	0.071
PA	0.727	0.745	0.087	0.025	0.609	0.041	0.850	0.755
TN	0.455	0.027	0.273	0.037	0.091	0.000	.	.
TX	0.130	0.177	0.818	0.076	0.909	0.727	0.619	0.000
VT	0.826	0.612	0.696	0.833	0.043	0.000	0.700	0.000
WA	0.727	0.377	0.091	0.005	0.727	0.828	0.667	0.784
WV	0.545	0.001	0.217	0.016	0.227	0.003	0.150	0.000
WY	0.091	0.000	0.500	0.103	0.091	0.000	0.476	0.000

Table E2

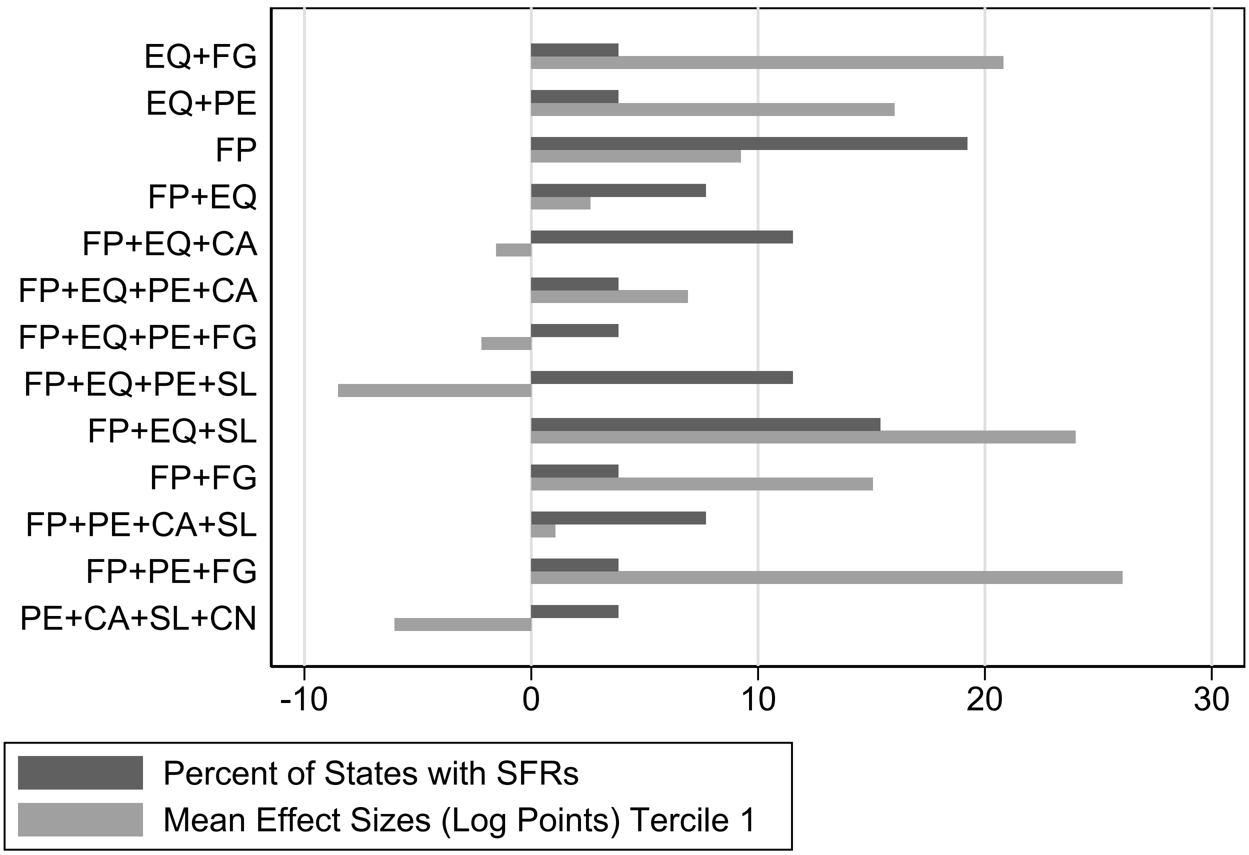
Difference-in-Differences p-values, Multiple Versions, by State: State Average

	Days in School Year		Minutes in School Day		Full-Time Kinder		Part-Time Kinder	
State	Placebo	HC3	Placebo	HC3	Placebo	HC3	Placebo	HC3
AK	0.333	0.111	0.333	0.000	0.522	0.149	0.217	0.015
AR	0.375	0.031	0.292	0.026	0.087	0.000	0.870	0.728
AZ	0.917	0.701	0.875	0.932	0.435	0.001	0.304	0.197
CA	0.625	0.159	1.000	0.591	0.130	0.001	0.000	0.043
CO	0.250	0.130	0.000	0.004	0.348	0.120	0.391	0.679
ID	0.625	0.000	0.167	0.000	0.739	0.000	0.435	0.004
IN	.	.	.	.	0.261	0.047	0.217	0.392
KS	0.042	0.490	0.375	0.000	0.652	0.000	0.261	0.000
MA	0.583	0.395	0.208	0.000	0.609	0.143	0.609	0.143
MD	0.583	0.124	0.208	0.101	0.739	0.120	0.739	0.120
MO	0.500	0.848	0.625	0.007	0.522	0.000	0.000	0.000
MT	0.458	0.363	0.583	0.397	0.217	0.000	0.435	0.000
NC	0.167	0.069	0.083	0.000	0.478	0.586	0.261	0.002
ND	0.292	0.034	0.625	0.295	0.043	0.000	0.261	0.018
NH	0.500	0.034	0.333	0.035	0.130	0.001	0.130	0.001
NJ	0.542	0.080	0.292	0.006	0.174	0.000	0.826	0.868
NM	0.208	0.207	0.792	0.015	0.348	0.263	0.043	0.000
NY	0.333	0.548	0.500	0.340	0.304	0.052	0.739	0.700
OH	0.583	0.359	0.000	0.427	0.261	0.000	0.043	0.000
PA	0.542	0.006	0.500	0.069	0.043	0.038	1.000	0.991
TN	0.708	0.718	0.250	0.000	0.348	0.000	0.522	0.068
TX	0.792	0.051	0.458	0.000	0.348	0.000	0.130	0.002
VT	0.500	0.527	0.042	0.542	0.609	0.100	0.652	0.214
WA	0.500	0.998	0.833	0.868	0.087	0.006	0.043	0.020
WV	0.167	0.136	0.500	0.000	0.217	0.000	0.783	0.206
WY	0.167	0.561	0.708	0.000	0.391	0.000	0.304	0.082

Appendix F

Average Effects of School Finance Reforms by Funding Formula

Figure F1. Funding Formula Typology: Distribution and Effects



Appendix G

Synthetic Control Weights

Table G1

Tercile 1 Control Weights by Dependent Variable and Treated State

Treated State	Control States
Dependent Variable: log(Capital Expenditures per Pupil)	
Alaska	Connecticut (0.121), Delaware (0.313), Florida (0.444), Maine (0.122)
Arizona	Alabama (0.021), Connecticut (0.010), Delaware (0.052), Florida (0.015), Georgia (0.018), Illinois (0.008), Iowa (0.019), Kentucky (0.025), Louisiana (0.019), Maine (0.048), Michigan (0.029), Minnesota (0.026), Mississippi (0.021), Nebraska (0.030), Nevada (0.126), Oklahoma (0.094), Oregon (0.033), Rhode Island (0.316), South Carolina (0.014), South Dakota (0.016), Utah (0.021), Virginia (0.020), Wisconsin (0.021)
Arkansas	Maine (0.774), Nevada (0.049), Oklahoma (0.177)
California	Alabama (0.444), Florida (0.070), Georgia (0.001), Maine (0.255), Mississippi (0.035), Nevada (0.008), Oklahoma (0.006), South Dakota (0.181)
Colorado	Illinois (0.124), Maine (0.619), Nevada (0.256)
Idaho	Alabama (0.028), Connecticut (0.026), Delaware (0.024), Florida (0.021), Georgia (0.023), Illinois (0.018), Iowa (0.031), Kentucky (0.031), Louisiana (0.040), Maine (0.030), Michigan (0.030), Minnesota (0.025), Mississippi (0.032), Nebraska (0.027), Oklahoma (0.094), Oregon (0.032), Rhode Island (0.045), South Carolina (0.027), South Dakota (0.340), Utah (0.017), Virginia (0.023), Wisconsin (0.035)
Indiana	Delaware (0.022), Georgia (0.008), Maine (0.085), Oregon (0.010), Rhode Island (0.173), South Dakota (0.122), Utah (0.128), Virginia (0.386), Wisconsin (0.066)
Kansas	Florida (0.742), Georgia (0.202), South Dakota (0.056)
Maryland	Illinois (0.388), Maine (0.227), South Dakota (0.385)

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Table G1 – continued from previous page.

Treated State	Control States
Massachusetts	Minnesota (0.921), Oklahoma (0.079)
Missouri	Oklahoma (0.450), Rhode Island (0.550)
Montana	Florida (0.232), Oklahoma (0.246), South Dakota (0.522)
New Hampshire	Illinois (0.199), Maine (0.135), Virginia (0.666)
New Jersey	Delaware (0.569), South Dakota (0.339), Utah (0.092)
New Mexico	Alabama (0.462), Michigan (0.329), Oregon (0.126), Utah (0.083)
New York	Mississippi (0.217), Oklahoma (0.094), South Carolina (0.219), South Dakota (0.354), Utah (0.116)
North Carolina	Illinois (0.328), Maine (0.579), South Dakota (0.093)
North Dakota	Florida (0.172), Iowa (0.277), Michigan (0.034), Minnesota (0.104), Nebraska (0.004), Oklahoma (0.007), Rhode Island (0.293), South Dakota (0.039), Virginia (0.069)
Ohio	Connecticut (0.095), Maine (0.154), Rhode Island (0.413), South Dakota (0.337)
Pennsylvania	Connecticut (0.544), Kentucky (0.228), Maine (0.091), Michigan (0.098), Oklahoma (0.039)
Tennessee	Maine (0.246), Rhode Island (0.394), South Dakota (0.360)
Texas	Alabama (0.009), Connecticut (0.012), Delaware (0.107), Florida (0.010), Georgia (0.015), Illinois (0.005), Iowa (0.012), Kentucky (0.020), Louisiana (0.017), Maine (0.006), Michigan (0.015), Minnesota (0.013), Mississippi (0.022), Nebraska (0.015), Oklahoma (0.006), Oregon (0.009), Rhode Island (0.639), South Carolina (0.013), South Dakota (0.009), Utah (0.021), Virginia (0.010), Wisconsin (0.017)
Vermont	Illinois (0.034), Louisiana (0.115), Rhode Island (0.852)
Washington	Connecticut (0.279), Florida (0.154), Maine (0.034), Minnesota (0.342), Oklahoma (0.191)
West Virginia	Delaware (0.023), Michigan (0.720), Virginia (0.257)

Continued on next page.

Table G1 – continued from previous page.

Treated State	Control States
Wyoming	Delaware (0.062), Nevada (0.178), Oklahoma (0.429), Oregon (0.331)
Dependent Variable: log(Instructional Expenditures per Pupil)	
Alaska	Alabama (0.180), Nevada (0.820)
Arizona	Alabama (0.007), Connecticut (0.012), Delaware (0.008), Florida (0.013), Georgia (0.293), Illinois (0.006), Iowa (0.009), Kentucky (0.009), Louisiana (0.010), Maine (0.008), Michigan (0.004), Minnesota (0.004), Mississippi (0.148), Nebraska (0.007), Nevada (0.053), Oklahoma (0.007), Oregon (0.010), Rhode Island (0.010), South Carolina (0.058), South Dakota (0.007), Utah (0.289), Virginia (0.020), Wisconsin (0.007)
Arkansas	Minnesota (0.541), Nebraska (0.311), South Dakota (0.148)
California	Georgia (0.285), Nevada (0.715)
Colorado	Florida (0.609), Iowa (0.302), Nebraska (0.088)
Idaho	Alabama (0.673), Mississippi (0.327)
Indiana	Delaware (0.163), Michigan (0.396), Oregon (0.395), Wisconsin (0.046)
Kansas	Connecticut (0.003), Delaware (0.005), Florida (0.003), Georgia (0.007), Illinois (0.008), Iowa (0.008), Kentucky (0.204), Louisiana (0.003), Maine (0.006), Michigan (0.007), Minnesota (0.014), Mississippi (0.002), Nebraska (0.015), Nevada (0.002), Oklahoma (0.004), Oregon (0.005), Rhode Island (0.004), South Carolina (0.003), South Dakota (0.521), Utah (0.002), Virginia (0.004), Wisconsin (0.167)
Maryland	Connecticut (0.444), Kentucky (0.168), South Carolina (0.388)
Massachusetts	Michigan (0.390), Minnesota (0.515), Nevada (0.096)
Missouri	Louisiana (0.321), Nevada (0.039), Utah (0.640)
Montana	Michigan (0.453), South Dakota (0.318), Utah (0.229)
New Hampshire	Georgia (0.079), Mississippi (0.325), South Carolina (0.221), South Dakota (0.376)

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Table G1 – continued from previous page.

Treated State	Control States
New Jersey	Kentucky (0.187), Wisconsin (0.813)
New Mexico	Georgia (0.250), Louisiana (0.374), South Carolina (0.376)
New York	Louisiana (0.839), South Dakota (0.161)
North Carolina	Connecticut (0.033), Georgia (0.001), South Carolina (0.441), Virginia (0.525)
North Dakota	Georgia (0.009), Louisiana (0.090), Minnesota (0.267), Nebraska (0.078), Nevada (0.313), South Dakota (0.244)
Ohio	Kentucky (0.004), Michigan (0.285), Mississippi (0.007), Nebraska (0.363), South Dakota (0.276), Wisconsin (0.065)
Pennsylvania	Connecticut (0.005), Florida (0.284), Nebraska (0.630), Oregon (0.081)
Tennessee	Alabama (0.026), Connecticut (0.034), Delaware (0.036), Florida (0.043), Georgia (0.024), Illinois (0.023), Iowa (0.040), Kentucky (0.016), Louisiana (0.054), Maine (0.024), Michigan (0.017), Minnesota (0.016), Mississippi (0.038), Nebraska (0.034), Nevada (0.161), Oklahoma (0.025), Oregon (0.039), Rhode Island (0.033), South Carolina (0.057), South Dakota (0.095), Utah (0.035), Virginia (0.033), Wisconsin (0.098)
Texas	Alabama (0.145), Connecticut (0.006), Delaware (0.015), Florida (0.026), Georgia (0.070), Illinois (0.016), Iowa (0.018), Kentucky (0.036), Louisiana (0.032), Maine (0.017), Michigan (0.022), Minnesota (0.016), Mississippi (0.333), Nebraska (0.014), Nevada (0.036), Oklahoma (0.031), Oregon (0.019), Rhode Island (0.010), South Carolina (0.027), South Dakota (0.020), Utah (0.059), Virginia (0.021), Wisconsin (0.011)
Vermont	Florida (0.026), Georgia (0.228), Louisiana (0.493), Nebraska (0.253)
Washington	Connecticut (0.008), Florida (0.091), Georgia (0.003), Illinois (0.182), Iowa (0.366), Louisiana (0.010), Nebraska (0.107), Oklahoma (0.010), Oregon (0.223)
West Virginia	Illinois (0.057), Kentucky (0.530), Michigan (0.345), Minnesota (0.015), South Dakota (0.053)

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Table G1 – continued from previous page.

Treated State	Control States
Wyoming	Georgia (0.142), Louisiana (0.440), Nevada (0.310), South Carolina (0.108)
Dependent Variable: log(Expenditures per Pupil)	
Alaska	Nevada (1.000)
Arizona	Minnesota (0.057), Mississippi (0.238), South Carolina (0.175), South Dakota (0.530)
Arkansas	Kentucky (0.135), Wisconsin (0.865)
California	Florida (0.848), Louisiana (0.152)
Colorado	Alabama (0.125), Louisiana (0.152), Nebraska (0.041), Oregon (0.682)
Idaho	Alabama (0.013), Connecticut (0.008), Delaware (0.005), Florida (0.009), Georgia (0.011), Illinois (0.006), Iowa (0.010), Kentucky (0.003), Louisiana (0.163), Maine (0.006), Michigan (0.004), Minnesota (0.003), Mississippi (0.006), Nebraska (0.015), Nevada (0.007), Oklahoma (0.005), Oregon (0.035), Rhode Island (0.008), South Carolina (0.009), South Dakota (0.649), Utah (0.006), Virginia (0.013), Wisconsin (0.005)
Indiana	Alabama (0.051), Delaware (0.196), Michigan (0.617), Oregon (0.136)
Kansas	Kentucky (0.262), Michigan (0.524), South Dakota (0.214)
Maryland	Nevada (0.106), Oregon (0.598), Rhode Island (0.284), Utah (0.013)
Massachusetts	Michigan (0.985), Mississippi (0.015)
Missouri	Florida (0.131), Louisiana (0.464), Mississippi (0.405)
Montana	Alabama (0.026), Connecticut (0.004), Delaware (0.014), Florida (0.012), Georgia (0.013), Illinois (0.012), Iowa (0.021), Kentucky (0.477), Louisiana (0.018), Maine (0.016), Michigan (0.018), Minnesota (0.017), Mississippi (0.134), Nebraska (0.016), Oklahoma (0.025), Oregon (0.017), Rhode Island (0.011), South Carolina (0.016), South Dakota (0.082), Utah (0.023), Virginia (0.013), Wisconsin (0.016)

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Table G1 – continued from previous page.

Treated State	Control States
New Hampshire	Georgia (0.033), Oregon (0.417), South Dakota (0.550)
New Jersey	Illinois (0.022), Kentucky (0.275), Wisconsin (0.703)
New Mexico	Alabama (0.519), Illinois (0.076), Oregon (0.142), South Carolina (0.263)
New York	Florida (0.039), Georgia (0.044), Louisiana (0.279), Maine (0.052), Minnesota (0.075), Nevada (0.044), South Dakota (0.275), Virginia (0.192)
North Carolina	Connecticut (0.100), Florida (0.165), Georgia (0.050), Louisiana (0.512), Oregon (0.167), South Dakota (0.005)
North Dakota	Delaware (0.320), Georgia (0.075), Louisiana (0.502), Nevada (0.103)
Ohio	Georgia (0.064), Maine (0.195), Minnesota (0.017), Oklahoma (0.263), South Dakota (0.161), Wisconsin (0.299)
Pennsylvania	Delaware (0.038), Georgia (0.150), Rhode Island (0.145), South Dakota (0.150), Wisconsin (0.518)
Tennessee	Kentucky (0.291), Mississippi (0.243), South Dakota (0.466)
Texas	Alabama (0.019), Connecticut (0.016), Delaware (0.025), Florida (0.012), Georgia (0.028), Illinois (0.020), Iowa (0.033), Kentucky (0.264), Louisiana (0.015), Maine (0.026), Michigan (0.034), Minnesota (0.064), Mississippi (0.017), Nebraska (0.027), Nevada (0.008), Oklahoma (0.046), Oregon (0.019), Rhode Island (0.019), South Carolina (0.015), South Dakota (0.195), Utah (0.027), Virginia (0.013), Wisconsin (0.061)
Vermont	Connecticut (0.803), Georgia (0.197)
Washington	Connecticut (0.481), Georgia (0.005), Nebraska (0.278), Oklahoma (0.236)
West Virginia	Kentucky (0.609), Michigan (0.006), Minnesota (0.045), South Dakota (0.132), Wisconsin (0.208)
Wyoming	Florida (0.157), Nevada (0.003), Oregon (0.840)
Dependent Variable: log(Revenues per Pupil)	
Alaska	Nevada (1.000)

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Table G1 – continued from previous page.

Treated State	Control States
Arizona	Alabama (0.173), Florida (0.024), Nebraska (0.106), Nevada (0.166), Wisconsin (0.530)
Arkansas	Florida (0.220), Georgia (0.009), Illinois (0.002), Kentucky (0.273), Wisconsin (0.496)
California	Georgia (0.123), Nebraska (0.002), Nevada (0.694), Virginia (0.181)
Colorado	Connecticut (0.129), Georgia (0.016), Minnesota (0.062), Nebraska (0.503), Nevada (0.113), Virginia (0.179)
Idaho	Alabama (0.978), Florida (0.013), Louisiana (0.009)
Indiana	Florida (0.057), Georgia (0.022), Michigan (0.199), Oregon (0.270), Utah (0.452)
Kansas	Alabama (0.003), Connecticut (0.002), Delaware (0.005), Florida (0.001), Illinois (0.004), Iowa (0.004), Kentucky (0.484), Louisiana (0.001), Maine (0.001), Michigan (0.362), Minnesota (0.002), Mississippi (0.101), Nebraska (0.003), Oklahoma (0.003), Oregon (0.001), Rhode Island (0.003), South Carolina (0.001), South Dakota (0.001), Utah (0.011), Virginia (0.001), Wisconsin (0.007)
Maryland	Florida (0.001), Georgia (0.053), Iowa (0.001), Louisiana (0.001), Maine (0.001), Michigan (0.001), Minnesota (0.001), Mississippi (0.002), Nebraska (0.209), Nevada (0.227), Oklahoma (0.001), Oregon (0.166), Rhode Island (0.001), South Carolina (0.031), South Dakota (0.001), Utah (0.001), Virginia (0.131), Wisconsin (0.172)
Massachusetts	Connecticut (0.114), Michigan (0.500), Minnesota (0.151), Nevada (0.235)
Missouri	Alabama (0.012), Connecticut (0.014), Delaware (0.009), Florida (0.083), Georgia (0.004), Illinois (0.014), Iowa (0.011), Kentucky (0.003), Louisiana (0.493), Maine (0.012), Michigan (0.181), Minnesota (0.012), Mississippi (0.013), Nebraska (0.007), Oklahoma (0.008), Oregon (0.013), Rhode Island (0.016), South Carolina (0.019), South Dakota (0.017), Utah (0.014), Virginia (0.038), Wisconsin (0.007)

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Table G1 – continued from previous page.

Treated State	Control States
Montana	Alabama (0.026), Connecticut (0.031), Delaware (0.035), Florida (0.031), Georgia (0.021), Illinois (0.035), Iowa (0.037), Kentucky (0.241), Louisiana (0.025), Maine (0.030), Michigan (0.028), Minnesota (0.033), Mississippi (0.027), Nebraska (0.034), Nevada (0.027), Oklahoma (0.025), Oregon (0.027), Rhode Island (0.032), South Carolina (0.025), South Dakota (0.025), Utah (0.026), Virginia (0.027), Wisconsin (0.154)
New Hampshire	Georgia (0.001), Nebraska (0.670), Virginia (0.329)
New Jersey	Georgia (0.174), Kentucky (0.222), Wisconsin (0.604)
New Mexico	Florida (0.203), Louisiana (0.536), Oregon (0.076), Rhode Island (0.084), Virginia (0.098), Wisconsin (0.004)
New York	Maine (1.000)
North Carolina	Florida (0.011), Georgia (0.061), Louisiana (0.567), Nevada (0.251), Oregon (0.084), South Dakota (0.028)
North Dakota	Connecticut (0.010), Florida (0.107), Maine (0.510), Mississippi (0.372)
Ohio	Alabama (0.120), Georgia (0.202), Kentucky (0.224), Wisconsin (0.454)
Pennsylvania	Connecticut (0.257), Kentucky (0.020), Maine (0.265), Minnesota (0.328), Nevada (0.130)
Tennessee	Alabama (0.719), Florida (0.028), Nebraska (0.252)
Texas	Alabama (0.004), Connecticut (0.008), Delaware (0.016), Florida (0.009), Georgia (0.095), Illinois (0.012), Iowa (0.012), Kentucky (0.640), Louisiana (0.007), Maine (0.012), Michigan (0.009), Minnesota (0.012), Nebraska (0.028), Oklahoma (0.010), Oregon (0.011), Rhode Island (0.011), South Carolina (0.009), South Dakota (0.009), Utah (0.007), Virginia (0.009), Wisconsin (0.068)
Vermont	Florida (0.654), Georgia (0.215), Nebraska (0.041), Virginia (0.090)
Washington	Connecticut (0.563), Illinois (0.056), Kentucky (0.068), Michigan (0.114), Wisconsin (0.200)

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Table G1 – continued from previous page.

Treated State	Control States
West Virginia	Delaware (0.088), Georgia (0.027), Kentucky (0.520), Utah (0.077), Wisconsin (0.289)
Wyoming	Connecticut (0.170), Florida (0.154), Nevada (0.548), Wisconsin (0.129)
Dependent Variable: log(Tch. Salary Expenditures per Pupil)	
Alaska	Nevada (1.000)
Arizona	Georgia (0.058), Minnesota (0.016), Nevada (0.177), South Dakota (0.702), Wisconsin (0.047)
Arkansas	Delaware (0.123), Oklahoma (0.473), Wisconsin (0.403)
California	Connecticut (0.069), Georgia (0.162), Nevada (0.768)
Colorado	Florida (0.537), Kentucky (0.025), Michigan (0.120), Mississippi (0.024), South Carolina (0.294)
Idaho	Georgia (0.109), South Carolina (0.516), Wisconsin (0.375)
Indiana	Alabama (0.162), Connecticut (0.312), Florida (0.074), Michigan (0.440), Nebraska (0.011)
Kansas	Delaware (0.481), Wisconsin (0.519)
Maryland	Alabama (0.253), Georgia (0.025), Nevada (0.069), South Carolina (0.653)
Massachusetts	Delaware (0.636), Mississippi (0.241), Nevada (0.123)
Missouri	Florida (0.347), Mississippi (0.462), Nebraska (0.191)
Montana	Alabama (0.026), Connecticut (0.021), Delaware (0.023), Florida (0.009), Georgia (0.068), Illinois (0.036), Iowa (0.025), Kentucky (0.269), Louisiana (0.022), Maine (0.022), Michigan (0.020), Minnesota (0.019), Mississippi (0.012), Nebraska (0.029), Oklahoma (0.045), Oregon (0.024), Rhode Island (0.013), South Carolina (0.018), South Dakota (0.034), Utah (0.021), Virginia (0.018), Wisconsin (0.226)

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Table G1 – continued from previous page.

Treated State	Control States
New Hampshire	Alabama (0.013), Connecticut (0.020), Delaware (0.025), Florida (0.015), Georgia (0.062), Illinois (0.018), Iowa (0.018), Kentucky (0.010), Louisiana (0.015), Maine (0.012), Michigan (0.028), Minnesota (0.366), Mississippi (0.012), Nebraska (0.017), Nevada (0.109), Oklahoma (0.016), Oregon (0.022), Rhode Island (0.014), South Carolina (0.015), South Dakota (0.019), Utah (0.015), Virginia (0.020), Wisconsin (0.141)
New Jersey	Kentucky (0.679), Minnesota (0.321)
New Mexico	South Carolina (0.550), Utah (0.450)
New York	Connecticut (0.227), Florida (0.023), Georgia (0.346), Oklahoma (0.077), South Dakota (0.327)
North Carolina	Alabama (0.050), Florida (0.231), Georgia (0.027), Louisiana (0.174), Maine (0.060), Mississippi (0.134), Rhode Island (0.068), South Carolina (0.257)
North Dakota	Florida (0.126), Georgia (0.119), Minnesota (0.279), Nevada (0.152), Oklahoma (0.095), South Dakota (0.228)
Ohio	Georgia (0.013), Kentucky (0.224), Louisiana (0.015), Oklahoma (0.160), South Dakota (0.489), Wisconsin (0.099)
Pennsylvania	Kentucky (0.230), Louisiana (0.004), Maine (0.079), South Dakota (0.113), Wisconsin (0.572)
Tennessee	Alabama (0.050), Connecticut (0.004), Delaware (0.016), Florida (0.017), Georgia (0.024), Illinois (0.013), Iowa (0.019), Kentucky (0.331), Louisiana (0.028), Maine (0.014), Michigan (0.016), Minnesota (0.017), Mississippi (0.068), Nebraska (0.018), Oklahoma (0.020), Oregon (0.023), Rhode Island (0.010), South Carolina (0.022), South Dakota (0.023), Utah (0.244), Virginia (0.016), Wisconsin (0.008)

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Table G1 – continued from previous page.

Treated State	Control States
Texas	Alabama (0.027), Connecticut (0.026), Delaware (0.002), Florida (0.033), Georgia (0.029), Illinois (0.021), Iowa (0.023), Kentucky (0.015), Louisiana (0.023), Maine (0.023), Michigan (0.471), Minnesota (0.026), Mississippi (0.051), Nebraska (0.019), Oklahoma (0.018), Oregon (0.032), Rhode Island (0.032), South Carolina (0.024), South Dakota (0.020), Utah (0.030), Virginia (0.040), Wisconsin (0.013)
Vermont	Delaware (0.331), Florida (0.097), Mississippi (0.035), Rhode Island (0.507), South Carolina (0.030)
Washington	Connecticut (0.217), Florida (0.172), Georgia (0.003), Louisiana (0.113), Minnesota (0.142), Oklahoma (0.027), Oregon (0.139), Wisconsin (0.187)
West Virginia	Alabama (0.089), Florida (0.010), Georgia (0.038), Iowa (0.292), Kentucky (0.346), Maine (0.001), Michigan (0.162), Minnesota (0.051), Nevada (0.011)
Wyoming	Connecticut (0.106), Delaware (0.121), Florida (0.433), Nebraska (0.165), Virginia (0.174)
Dependent Variable: Teachers per 100 Students	
Alaska	Kentucky (0.261), Nevada (0.739)
Arizona	Alabama (0.254), Kentucky (0.746)
Arkansas	Alabama (0.024), Connecticut (0.030), Delaware (0.031), Florida (0.033), Georgia (0.027), Illinois (0.026), Iowa (0.038), Kentucky (0.023), Louisiana (0.027), Maine (0.027), Minnesota (0.020), Mississippi (0.026), Nebraska (0.070), Nevada (0.295), Oklahoma (0.023), Oregon (0.077), Rhode Island (0.041), South Carolina (0.027), South Dakota (0.017), Utah (0.030), Wisconsin (0.088)
California	Minnesota (0.854), Wisconsin (0.146)
Colorado	Kentucky (0.003), Maine (0.318), Oklahoma (0.137), South Carolina (0.112), Utah (0.431)

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Table G1 – continued from previous page.

Treated State	Control States
Idaho	Alabama (0.049), Connecticut (0.032), Delaware (0.029), Florida (0.031), Georgia (0.036), Illinois (0.051), Iowa (0.032), Kentucky (0.048), Louisiana (0.037), Maine (0.045), Minnesota (0.031), Mississippi (0.033), Nebraska (0.031), Oklahoma (0.076), Oregon (0.019), Rhode Island (0.046), South Carolina (0.026), South Dakota (0.302), Utah (0.021), Wisconsin (0.025)
Indiana	Delaware (0.012), Illinois (0.185), Louisiana (0.174), Minnesota (0.084), Nebraska (0.049), Nevada (0.008), Oklahoma (0.158), Oregon (0.137), South Dakota (0.055), Utah (0.138)
Kansas	Alabama (0.790), Delaware (0.069), Kentucky (0.141)
Maryland	Alabama (0.363), Delaware (0.123), Illinois (0.354), Oregon (0.160)
Missouri	Alabama (0.036), Connecticut (0.038), Delaware (0.034), Florida (0.031), Georgia (0.050), Illinois (0.035), Iowa (0.035), Kentucky (0.048), Louisiana (0.036), Maine (0.038), Minnesota (0.037), Mississippi (0.034), Nebraska (0.013), Nevada (0.029), Oklahoma (0.088), Oregon (0.023), Rhode Island (0.034), South Carolina (0.031), South Dakota (0.281), Utah (0.024), Wisconsin (0.025)
Montana	Nebraska (0.099), Oregon (0.687), Wisconsin (0.214)
New Hampshire	Alabama (0.007), Connecticut (0.386), Delaware (0.059), Florida (0.010), Georgia (0.016), Illinois (0.007), Iowa (0.025), Kentucky (0.014), Louisiana (0.013), Maine (0.014), Minnesota (0.011), Mississippi (0.009), Nebraska (0.245), Nevada (0.104), Oklahoma (0.015), Oregon (0.007), Rhode Island (0.015), South Carolina (0.017), South Dakota (0.012), Utah (0.004), Wisconsin (0.011)
New Jersey	Connecticut (0.015), Delaware (0.332), Kentucky (0.304), Nebraska (0.344), Rhode Island (0.005)
New Mexico	Minnesota (0.297), Utah (0.318), Wisconsin (0.385)
New York	Minnesota (0.073), Mississippi (0.234), Oregon (0.296), Wisconsin (0.398)

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Table G1 – continued from previous page.

Treated State	Control States
North Carolina	Delaware (0.016), Iowa (0.433), Kentucky (0.129), Maine (0.285), Utah (0.137)
North Dakota	Delaware (0.030), Nebraska (0.187), South Carolina (0.031), South Dakota (0.753)
Ohio	Delaware (0.468), Illinois (0.300), Kentucky (0.108), South Dakota (0.124)
Pennsylvania	Utah (0.236), Wisconsin (0.764)
Texas	Alabama (0.369), Connecticut (0.013), Delaware (0.016), Florida (0.010), Georgia (0.010), Illinois (0.017), Iowa (0.013), Kentucky (0.142), Louisiana (0.014), Maine (0.014), Minnesota (0.009), Mississippi (0.011), Nebraska (0.007), Nevada (0.007), Oklahoma (0.216), Oregon (0.004), Rhode Island (0.012), South Carolina (0.010), South Dakota (0.098), Wisconsin (0.007)
Vermont	Delaware (0.168), Louisiana (0.462), Nebraska (0.370)
Washington	Delaware (0.087), Illinois (0.014), Kentucky (0.186), Louisiana (0.218), Nebraska (0.158), Oregon (0.279), Rhode Island (0.032), South Dakota (0.026)
West Virginia	Alabama (0.074), Delaware (0.053), Kentucky (0.070), Maine (0.207), Mississippi (0.122), Oklahoma (0.074), Oregon (0.080), South Carolina (0.198), Utah (0.034), Wisconsin (0.088)
Wyoming	Nevada (0.211), Oregon (0.078), Utah (0.405), Wisconsin (0.306)

Table G2

Tercile 3 Control Weights by Dependent Variable and Treated State

Treated State	Control States
Dependent Variable: log(Capital Expenditures per Pupil)	
Alaska	Alabama (0.407), Georgia (0.021), Nevada (0.267), Utah (0.305)
Arizona	Delaware (0.137), Maine (0.092), Wisconsin (0.770)
Arkansas	Alabama (0.012), Connecticut (0.015), Delaware (0.042), Florida (0.013), Georgia (0.012), Illinois (0.020), Iowa (0.015), Kentucky (0.027), Louisiana (0.016), Maine (0.062), Michigan (0.032), Minnesota (0.014), Mississippi (0.012), Nebraska (0.020), Nevada (0.010), Oklahoma (0.014), Oregon (0.017), Rhode Island (0.057), South Carolina (0.015), South Dakota (0.113), Utah (0.017), Virginia (0.014), Wisconsin (0.432)
California	Alabama (0.230), Kentucky (0.278), Maine (0.013), Mississippi (0.099), Oklahoma (0.013), Rhode Island (0.233), South Dakota (0.092), Utah (0.042)
Colorado	Alabama (0.070), Connecticut (0.058), Georgia (0.203), Illinois (0.480), Kentucky (0.072), Oregon (0.082), Wisconsin (0.035)
Idaho	Nebraska (0.029), South Dakota (0.700), Wisconsin (0.271)
Indiana	Iowa (0.024), Kentucky (0.061), Maine (0.092), Nebraska (0.129), Oklahoma (0.325), Virginia (0.136), Wisconsin (0.233)
Kansas	Alabama (0.028), Connecticut (0.034), Delaware (0.046), Florida (0.026), Georgia (0.028), Illinois (0.028), Iowa (0.039), Kentucky (0.053), Louisiana (0.025), Maine (0.077), Michigan (0.068), Minnesota (0.036), Mississippi (0.033), Nebraska (0.025), Nevada (0.033), Oklahoma (0.088), Oregon (0.136), Rhode Island (0.021), South Carolina (0.030), South Dakota (0.020), Utah (0.046), Virginia (0.029), Wisconsin (0.050)
Maryland	Alabama (0.267), Georgia (0.354), Maine (0.008), Nevada (0.131), Rhode Island (0.239)
Massachusetts	Nevada (1.000)

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Table G2 – continued from previous page.

Treated State	Control States
Missouri	Alabama (0.516), Louisiana (0.230), South Dakota (0.254)
Montana	Alabama (0.516), South Dakota (0.484)
New Hampshire	Alabama (0.842), Maine (0.158)
New Jersey	Alabama (0.025), Connecticut (0.035), Delaware (0.045), Florida (0.019), Georgia (0.025), Illinois (0.029), Iowa (0.034), Kentucky (0.038), Louisiana (0.049), Maine (0.019), Michigan (0.034), Minnesota (0.029), Mississippi (0.039), Nebraska (0.035), Nevada (0.017), Oklahoma (0.024), Oregon (0.105), Rhode Island (0.129), South Carolina (0.029), South Dakota (0.130), Utah (0.035), Virginia (0.016), Wisconsin (0.058)
New Mexico	Connecticut (0.057), Delaware (0.078), Georgia (0.543), Mississippi (0.127), Nebraska (0.163), South Dakota (0.031)
New York	Connecticut (0.325), Delaware (0.054), Illinois (0.202), Iowa (0.100), Maine (0.070), Mississippi (0.008), Rhode Island (0.200), Utah (0.042)
North Carolina	Georgia (0.604), Louisiana (0.085), Mississippi (0.216), Nebraska (0.079), Nevada (0.015)
North Dakota	Delaware (0.605), Kentucky (0.033), Maine (0.041), South Carolina (0.073), South Dakota (0.216), Wisconsin (0.031)
Ohio	Delaware (0.060), Louisiana (0.403), Nebraska (0.075), Rhode Island (0.240), Wisconsin (0.221)
Pennsylvania	Alabama (0.289), Connecticut (0.023), Illinois (0.279), Kentucky (0.087), Louisiana (0.023), Maine (0.018), Rhode Island (0.078), South Carolina (0.073), South Dakota (0.130)

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Table G2 – continued from previous page.

Treated State	Control States
Tennessee	Alabama (0.022), Connecticut (0.053), Delaware (0.030), Florida (0.035), Georgia (0.032), Illinois (0.250), Iowa (0.030), Kentucky (0.046), Louisiana (0.027), Maine (0.053), Michigan (0.037), Minnesota (0.034), Mississippi (0.025), Nebraska (0.033), Oklahoma (0.031), Oregon (0.026), Rhode Island (0.039), South Carolina (0.036), South Dakota (0.027), Utah (0.031), Virginia (0.025), Wisconsin (0.077)
Texas	Connecticut (0.020), Illinois (0.268), Wisconsin (0.712)
Vermont	Connecticut (0.123), Delaware (0.358), Illinois (0.266), Maine (0.001), Nebraska (0.227), Wisconsin (0.026)
Washington	Maine (0.094), Oklahoma (0.766), Wisconsin (0.139)
West Virginia	Kentucky (0.208), Maine (0.173), Oregon (0.010), Rhode Island (0.215), Utah (0.045), Wisconsin (0.348)
Wyoming	Florida (0.144), Georgia (0.856)
Dependent Variable: log(Instructional Expenditures per Pupil)	
Alaska	Delaware (0.427), Virginia (0.573)
Arizona	Georgia (0.429), Virginia (0.571)
Arkansas	Alabama (0.911), Michigan (0.080), Nebraska (0.009)
California	Georgia (0.009), Louisiana (0.209), Virginia (0.781)
Colorado	Florida (0.218), Iowa (0.219), Oregon (0.029), Rhode Island (0.137), Virginia (0.397)
Idaho	Alabama (0.019), Connecticut (0.005), Delaware (0.002), Florida (0.003), Georgia (0.003), Illinois (0.596), Iowa (0.017), Kentucky (0.004), Louisiana (0.007), Maine (0.007), Michigan (0.017), Minnesota (0.003), Mississippi (0.003), Nebraska (0.013), Nevada (0.066), Oklahoma (0.006), Oregon (0.006), Rhode Island (0.004), South Carolina (0.003), South Dakota (0.204), Utah (0.006), Virginia (0.003), Wisconsin (0.004)
Indiana	Maine (0.042), Michigan (0.496), South Carolina (0.278), Utah (0.184)
Kansas	Connecticut (0.045), Iowa (0.871), Maine (0.084)
Maryland	Virginia (1.000)

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Table G2 – continued from previous page.

Treated State	Control States
Massachusetts	Delaware (0.241), Illinois (0.140), Maine (0.288), Michigan (0.331)
Missouri	Alabama (0.089), Connecticut (0.008), Delaware (0.010), Florida (0.017), Georgia (0.017), Illinois (0.042), Iowa (0.037), Kentucky (0.014), Louisiana (0.456), Maine (0.018), Michigan (0.019), Minnesota (0.014), Mississippi (0.025), Nebraska (0.030), Oklahoma (0.020), Oregon (0.019), Rhode Island (0.012), South Carolina (0.018), South Dakota (0.057), Utah (0.050), Virginia (0.013), Wisconsin (0.015)
Montana	Georgia (0.248), Kentucky (0.444), Maine (0.308)
New Hampshire	Connecticut (0.175), Illinois (0.062), Maine (0.072), Virginia (0.691)
New Jersey	Illinois (0.030), Nebraska (0.970)
New Mexico	Florida (0.170), Georgia (0.264), Kentucky (0.239), Mississippi (0.327)
New York	Connecticut (0.628), Minnesota (0.214), Virginia (0.157)
North Carolina	Delaware (0.036), Mississippi (0.183), Oklahoma (0.127), Virginia (0.654)
North Dakota	Louisiana (0.497), Maine (0.136), Mississippi (0.368)
Ohio	Connecticut (0.088), Iowa (0.403), Nebraska (0.020), Nevada (0.017), Rhode Island (0.018), South Dakota (0.454)
Pennsylvania	Connecticut (0.465), Michigan (0.502), Oregon (0.033)
Tennessee	Delaware (0.727), South Dakota (0.273)
Texas	Delaware (0.468), Michigan (0.367), South Dakota (0.165)
Vermont	Alabama (0.190), Georgia (0.013), Illinois (0.382), Iowa (0.132), Nevada (0.283)
Washington	Connecticut (0.448), Iowa (0.267), Oklahoma (0.063), Oregon (0.222)
West Virginia	Alabama (0.731), Michigan (0.269)
Wyoming	Illinois (0.240), Nevada (0.449), Virginia (0.311)
Dependent Variable: log(Expenditures per Pupil)	

Continued on next page.

Table G2 – continued from previous page.

Treated State	Control States
Alaska	Connecticut (0.676), Nebraska (0.324)
Arizona	Maine (0.594), Michigan (0.140), Oregon (0.265)
Arkansas	Kentucky (0.126), Michigan (0.621), Nebraska (0.042), Utah (0.211)
California	Louisiana (0.490), Oregon (0.173), Virginia (0.337)
Colorado	Alabama (0.240), Connecticut (0.046), Michigan (0.043), Oregon (0.403), Rhode Island (0.195), Virginia (0.073)
Idaho	Kentucky (0.499), Michigan (0.008), Utah (0.493)
Indiana	Michigan (0.764), Oregon (0.041), South Carolina (0.038), Utah (0.157)
Kansas	Alabama (0.033), Connecticut (0.010), Delaware (0.024), Florida (0.026), Georgia (0.031), Illinois (0.029), Iowa (0.032), Kentucky (0.058), Louisiana (0.029), Maine (0.032), Michigan (0.384), Minnesota (0.025), Mississippi (0.034), Nebraska (0.031), Oklahoma (0.040), Oregon (0.029), Rhode Island (0.027), South Carolina (0.032), South Dakota (0.034), Utah (0.034), Virginia (0.021), Wisconsin (0.003)
Maryland	Connecticut (0.633), Virginia (0.367)
Massachusetts	Delaware (0.415), Nevada (0.433), Oregon (0.075), Wisconsin (0.078)
Missouri	Alabama (0.018), Connecticut (0.014), Delaware (0.018), Florida (0.020), Georgia (0.018), Illinois (0.020), Iowa (0.040), Kentucky (0.022), Louisiana (0.319), Maine (0.127), Michigan (0.021), Minnesota (0.051), Mississippi (0.027), Nebraska (0.046), Nevada (0.016), Oklahoma (0.021), Oregon (0.023), Rhode Island (0.017), South Carolina (0.018), South Dakota (0.061), Utah (0.048), Virginia (0.018), Wisconsin (0.017)
Montana	Alabama (0.361), Kentucky (0.189), Nevada (0.450)
New Hampshire	Connecticut (0.069), South Carolina (0.396), Virginia (0.535)
New Jersey	Connecticut (0.145), Louisiana (0.124), Michigan (0.406), Oregon (0.214), Wisconsin (0.112)

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Table G2 – continued from previous page.

Treated State	Control States
New Mexico	Alabama (0.086), Georgia (0.255), Kentucky (0.099), Louisiana (0.033), Michigan (0.066), Utah (0.461)
New York	Connecticut (0.761), Virginia (0.239)
North Carolina	Alabama (0.030), South Carolina (0.577), South Dakota (0.042), Virginia (0.350)
North Dakota	Delaware (0.320), Louisiana (0.680)
Ohio	Iowa (0.067), Kentucky (0.075), Michigan (0.324), Minnesota (0.534)
Pennsylvania	Connecticut (0.331), Florida (0.261), Illinois (0.066), Kentucky (0.066), Maine (0.062), Nebraska (0.089), Wisconsin (0.125)
Tennessee	Alabama (0.006), Connecticut (0.002), Delaware (0.005), Georgia (0.003), Illinois (0.002), Iowa (0.003), Kentucky (0.002), Louisiana (0.844), Maine (0.002), Michigan (0.003), Minnesota (0.005), Mississippi (0.006), Nebraska (0.044), Oklahoma (0.003), Oregon (0.004), Rhode Island (0.005), South Carolina (0.003), South Dakota (0.004), Utah (0.003), Virginia (0.048), Wisconsin (0.003)
Texas	Alabama (0.003), Connecticut (0.004), Delaware (0.008), Florida (0.003), Georgia (0.004), Illinois (0.003), Iowa (0.014), Kentucky (0.011), Louisiana (0.010), Maine (0.028), Michigan (0.323), Minnesota (0.009), Mississippi (0.006), Nebraska (0.235), Nevada (0.002), Oklahoma (0.010), Oregon (0.007), Rhode Island (0.005), South Carolina (0.003), South Dakota (0.044), Utah (0.258), Virginia (0.002), Wisconsin (0.007)
Vermont	Alabama (0.127), Oregon (0.155), South Carolina (0.209), Virginia (0.510)
Washington	Connecticut (0.329), Florida (0.305), Wisconsin (0.366)
West Virginia	Kentucky (0.311), Michigan (0.530), Utah (0.158)
Wyoming	Connecticut (0.158), Florida (0.055), Louisiana (0.368), Virginia (0.419)
Dependent Variable: log(Revenues per Pupil)	
Alaska	Connecticut (0.423), Florida (0.577)

Continued on next page.

Table G2 – continued from previous page.

Treated State	Control States
Arizona	Kentucky (0.230), Louisiana (0.723), Oregon (0.047)
Arkansas	Kentucky (0.573), Louisiana (0.242), Michigan (0.059), Oregon (0.126)
California	Louisiana (0.733), Virginia (0.267)
Colorado	Connecticut (0.119), Oklahoma (0.063), Oregon (0.753), Virginia (0.065)
Idaho	Kentucky (0.406), Louisiana (0.449), Oregon (0.145)
Indiana	Delaware (0.070), Kentucky (0.259), South Dakota (0.073), Wisconsin (0.599)
Kansas	Alabama (0.161), Louisiana (0.511), Michigan (0.328)
Maryland	Florida (1.000)
Massachusetts	Connecticut (0.413), Maine (0.390), Virginia (0.197)
Missouri	Alabama (0.037), Connecticut (0.009), Delaware (0.016), Florida (0.017), Georgia (0.025), Illinois (0.021), Iowa (0.023), Kentucky (0.022), Louisiana (0.321), Maine (0.018), Michigan (0.017), Minnesota (0.020), Mississippi (0.064), Nebraska (0.022), Nevada (0.026), Oklahoma (0.031), Oregon (0.020), Rhode Island (0.016), South Carolina (0.028), South Dakota (0.125), Utah (0.085), Virginia (0.021), Wisconsin (0.016)
Montana	Alabama (0.236), South Dakota (0.526), Virginia (0.239)
New Hampshire	Alabama (0.002), Connecticut (0.004), Delaware (0.002), Florida (0.116), Georgia (0.003), Illinois (0.003), Iowa (0.002), Kentucky (0.001), Louisiana (0.028), Maine (0.002), Michigan (0.002), Minnesota (0.003), Mississippi (0.002), Nebraska (0.505), Nevada (0.003), Oklahoma (0.002), Oregon (0.297), Rhode Island (0.002), South Carolina (0.002), South Dakota (0.002), Utah (0.002), Virginia (0.011), Wisconsin (0.002)
New Jersey	Oklahoma (0.293), Oregon (0.707)
New Mexico	Florida (0.036), Michigan (0.201), Mississippi (0.286), Oklahoma (0.045), South Dakota (0.432)
New York	Connecticut (0.761), Florida (0.058), Virginia (0.180)

Continued on next page.

Table G2 – continued from previous page.

Treated State	Control States
North Carolina	Nebraska (0.052), Oregon (0.064), Virginia (0.884)
North Dakota	Delaware (0.041), Georgia (0.238), Louisiana (0.197), Mississippi (0.524)
Ohio	Kentucky (0.228), Louisiana (0.057), Maine (0.575), Nevada (0.057), Oklahoma (0.026), Oregon (0.056)
Pennsylvania	Connecticut (0.585), Delaware (0.162), Kentucky (0.218), Maine (0.035)
Tennessee	Kentucky (0.083), Michigan (0.109), Utah (0.807)
Texas	Kentucky (0.478), Michigan (0.522)
Vermont	Connecticut (0.306), Mississippi (0.055), Oregon (0.446), South Dakota (0.012), Virginia (0.181)
Washington	Connecticut (0.562), Kentucky (0.276), Michigan (0.035), Oregon (0.126)
West Virginia	Alabama (0.427), Kentucky (0.265), Oklahoma (0.309)
Wyoming	Connecticut (0.153), Georgia (0.190), Louisiana (0.185), Virginia (0.471)
Dependent Variable: log(Tch. Salary Expenditures per Pupil)	
Alaska	Connecticut (0.503), Delaware (0.009), Florida (0.186), Nebraska (0.303)
Arizona	Delaware (0.267), Florida (0.214), Nebraska (0.059), Oregon (0.437), South Carolina (0.023)
Arkansas	Alabama (0.072), Kentucky (0.001), Nebraska (0.038), Oklahoma (0.890)
California	Florida (0.304), Minnesota (0.079), South Carolina (0.617)
Colorado	Connecticut (0.057), Florida (0.258), Oregon (0.243), Rhode Island (0.075), South Carolina (0.367)

Continued on next page.

Table G2 – continued from previous page.

Treated State	Control States
Idaho	Alabama (0.028), Connecticut (0.016), Delaware (0.014), Florida (0.009), Georgia (0.018), Illinois (0.023), Iowa (0.038), Kentucky (0.360), Louisiana (0.016), Maine (0.019), Michigan (0.044), Minnesota (0.014), Mississippi (0.013), Nebraska (0.017), Nevada (0.162), Oklahoma (0.066), Oregon (0.020), Rhode Island (0.021), South Carolina (0.011), South Dakota (0.026), Utah (0.025), Virginia (0.016), Wisconsin (0.024)
Indiana	Florida (0.075), Michigan (0.511), Minnesota (0.046), South Carolina (0.219), Wisconsin (0.148)
Kansas	Alabama (0.014), Connecticut (0.021), Delaware (0.023), Florida (0.018), Georgia (0.020), Illinois (0.025), Iowa (0.019), Kentucky (0.347), Louisiana (0.017), Maine (0.020), Michigan (0.028), Minnesota (0.022), Mississippi (0.015), Nebraska (0.023), Oklahoma (0.007), Oregon (0.255), Rhode Island (0.030), South Carolina (0.018), South Dakota (0.015), Utah (0.007), Virginia (0.020), Wisconsin (0.037)
Maryland	Connecticut (0.658), Florida (0.342)
Massachusetts	Connecticut (0.342), Delaware (0.102), Florida (0.164), Maine (0.392)
Missouri	Florida (0.140), Louisiana (0.317), Mississippi (0.543)
Montana	Alabama (0.034), Connecticut (0.005), Delaware (0.084), Florida (0.073), Georgia (0.026), Illinois (0.017), Iowa (0.019), Kentucky (0.022), Louisiana (0.021), Maine (0.015), Michigan (0.014), Minnesota (0.046), Mississippi (0.126), Nebraska (0.060), Oklahoma (0.025), Oregon (0.032), Rhode Island (0.014), South Carolina (0.067), South Dakota (0.027), Utah (0.232), Virginia (0.019), Wisconsin (0.019)
New Hampshire	Georgia (0.316), Louisiana (0.080), Maine (0.596), Nevada (0.007)
New Jersey	Oklahoma (0.603), Oregon (0.195), Virginia (0.202)
New Mexico	Alabama (0.956), Utah (0.044)

Continued on next page.

Table G2 – continued from previous page.

Treated State	Control States
New York	Connecticut (0.261), Florida (0.259), Kentucky (0.045), Michigan (0.014), Nebraska (0.075), Oregon (0.016), South Dakota (0.329)
North Carolina	Florida (0.556), Minnesota (0.221), Oklahoma (0.147), Oregon (0.037), Virginia (0.039)
North Dakota	Georgia (0.484), Mississippi (0.516)
Ohio	Illinois (0.162), Kentucky (0.369), Michigan (0.231), Oklahoma (0.238)
Pennsylvania	Connecticut (0.083), Kentucky (0.560), Louisiana (0.029), Michigan (0.170), Nebraska (0.112), Nevada (0.046)
Tennessee	Alabama (0.026), Connecticut (0.009), Delaware (0.012), Florida (0.005), Georgia (0.017), Illinois (0.025), Iowa (0.020), Kentucky (0.125), Louisiana (0.010), Maine (0.012), Michigan (0.379), Minnesota (0.010), Mississippi (0.007), Nebraska (0.202), Oklahoma (0.025), Oregon (0.016), Rhode Island (0.016), South Carolina (0.006), South Dakota (0.023), Utah (0.018), Virginia (0.008), Wisconsin (0.026)
Texas	Maine (0.408), Mississippi (0.058), South Carolina (0.534)
Vermont	Connecticut (0.157), Illinois (0.003), Nebraska (0.025), Nevada (0.521), Oklahoma (0.193), Virginia (0.101)
Washington	Connecticut (0.304), Illinois (0.112), Kentucky (0.032), Louisiana (0.206), Michigan (0.198), Nebraska (0.027), Oklahoma (0.084), Virginia (0.037)
West Virginia	Alabama (0.055), Georgia (0.178), Kentucky (0.354), Maine (0.010), Minnesota (0.037), Nebraska (0.120), South Dakota (0.028), Wisconsin (0.218)
Wyoming	Florida (0.759), Louisiana (0.111), South Carolina (0.131)
Dependent Variable: Teachers per 100 Students	
Alaska	Connecticut (0.253), Oregon (0.065), Utah (0.681)
Arizona	Alabama (0.678), Oklahoma (0.322)

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Table G2 – continued from previous page.

Treated State	Control States
Arkansas	Florida (0.346), Georgia (0.314), Oregon (0.099), South Dakota (0.225), Utah (0.015)
California	Maine (0.521), Minnesota (0.390), Utah (0.089)
Colorado	Connecticut (0.015), Florida (0.061), Iowa (0.367), Kentucky (0.095), Louisiana (0.063), Minnesota (0.093), Nevada (0.005), Wisconsin (0.301)
Idaho	Oklahoma (0.542), Rhode Island (0.018), Utah (0.440)
Indiana	Nevada (0.172), Oregon (0.238), Utah (0.193), Wisconsin (0.397)
Kansas	Alabama (0.005), Connecticut (0.004), Delaware (0.007), Florida (0.006), Georgia (0.006), Illinois (0.009), Iowa (0.007), Kentucky (0.009), Louisiana (0.705), Maine (0.009), Minnesota (0.005), Mississippi (0.007), Nebraska (0.008), Oklahoma (0.118), Oregon (0.004), Rhode Island (0.064), South Carolina (0.006), South Dakota (0.007), Utah (0.004), Wisconsin (0.010)
Maryland	Connecticut (0.041), Louisiana (0.192), Oregon (0.264), Rhode Island (0.130), South Dakota (0.373)
Missouri	Alabama (0.051), Connecticut (0.036), Delaware (0.045), Florida (0.048), Georgia (0.045), Illinois (0.053), Iowa (0.044), Kentucky (0.050), Louisiana (0.076), Maine (0.043), Minnesota (0.048), Mississippi (0.054), Nebraska (0.047), Oklahoma (0.056), Oregon (0.047), Rhode Island (0.036), South Carolina (0.045), South Dakota (0.048), Utah (0.086), Wisconsin (0.042)
Montana	Alabama (0.382), Oregon (0.574), Utah (0.044)
New Hampshire	Connecticut (0.559), Oregon (0.441)
New Jersey	Delaware (0.551), Louisiana (0.015), Rhode Island (0.039), South Dakota (0.395)
New Mexico	Alabama (0.867), Kentucky (0.119), Utah (0.014)
New York	Connecticut (0.800), Maine (0.005), Oregon (0.194)
North Carolina	Illinois (0.779), Nevada (0.008), Utah (0.214)
North Dakota	Alabama (0.831), Utah (0.169)

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Table G2 – continued from previous page.

Treated State	Control States
Ohio	Louisiana (0.069), Oklahoma (0.656), Rhode Island (0.239), Utah (0.036)
Pennsylvania	Connecticut (0.292), Florida (0.391), Maine (0.317)
Texas	Alabama (0.017), Connecticut (0.008), Delaware (0.013), Florida (0.011), Georgia (0.011), Illinois (0.018), Iowa (0.015), Kentucky (0.019), Louisiana (0.107), Maine (0.014), Minnesota (0.012), Mississippi (0.016), Nebraska (0.019), Nevada (0.029), Oklahoma (0.484), Oregon (0.010), Rhode Island (0.133), South Carolina (0.013), South Dakota (0.022), Utah (0.013), Wisconsin (0.017)
Vermont	Oklahoma (0.984), South Dakota (0.016)
Washington	Illinois (0.036), Kentucky (0.132), Nevada (0.038), Oklahoma (0.247), Rhode Island (0.013), Wisconsin (0.534)
West Virginia	Alabama (0.105), Connecticut (0.029), Florida (0.060), Kentucky (0.089), Maine (0.060), Mississippi (0.107), Nebraska (0.408), South Carolina (0.125), Utah (0.017)
Wyoming	Alabama (0.416), Oregon (0.476), South Dakota (0.109)

Table G3

State Average Control Weights by Dependent Variable and Treated State

Treated State	Control States
Dependent Variable: Pct. Full Day Kindergarten	
Alaska	Alabama (0.210), Mississippi (0.090), Nevada (0.131), Oklahoma (0.072), Oregon (0.015), South Dakota (0.031), Utah (0.047), Virginia (0.404)
Arizona	Maine (0.200), Nevada (0.194), South Carolina (0.319), Utah (0.287)
Arkansas	Alabama (0.353), Connecticut (0.012), Delaware (0.022), Florida (0.037), Georgia (0.024), Illinois (0.017), Iowa (0.016), Kentucky (0.009), Louisiana (0.045), Maine (0.014), Michigan (0.012), Minnesota (0.018), Mississippi (0.030), Nebraska (0.014), Nevada (0.119), Oklahoma (0.109), Oregon (0.009), Rhode Island (0.022), South Carolina (0.011), South Dakota (0.013), Utah (0.021), Virginia (0.029), Wisconsin (0.042)
California	Alabama (0.115), Delaware (0.196), Georgia (0.001), Iowa (0.061), Nevada (0.138), Oklahoma (0.119), Oregon (0.134), South Dakota (0.026), Utah (0.212)
Colorado	Delaware (0.117), Kentucky (0.361), Michigan (0.016), Nevada (0.001), Rhode Island (0.083), Utah (0.423)
Idaho	Delaware (0.140), Nevada (0.527), Wisconsin (0.333)
Indiana	Georgia (0.004), Maine (0.409), Nevada (0.364), South Dakota (0.080), Utah (0.143)
Kansas	Delaware (0.258), Nevada (0.531), Wisconsin (0.211)
Maryland	Georgia (0.143), Louisiana (0.039), Maine (0.103), Nebraska (0.088), Oklahoma (0.462), South Carolina (0.003), South Dakota (0.162)
Massachusetts	Alabama (0.019), Connecticut (0.023), Delaware (0.014), Florida (0.019), Georgia (0.016), Illinois (0.486), Iowa (0.019), Kentucky (0.017), Louisiana (0.016), Maine (0.146), Michigan (0.018), Minnesota (0.018), Mississippi (0.017), Nebraska (0.018), Nevada (0.012), Oklahoma (0.020), Oregon (0.022), Rhode Island (0.016), South Carolina (0.026), South Dakota (0.023), Virginia (0.019), Wisconsin (0.015)

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Table G3 – continued from previous page.

Treated State	Control States
Missouri	Alabama (0.006), Connecticut (0.005), Delaware (0.005), Florida (0.006), Georgia (0.005), Illinois (0.007), Iowa (0.007), Kentucky (0.007), Louisiana (0.007), Maine (0.002), Michigan (0.010), Minnesota (0.002), Mississippi (0.006), Nebraska (0.007), Nevada (0.345), Oklahoma (0.428), Oregon (0.004), Rhode Island (0.007), South Carolina (0.004), South Dakota (0.005), Utah (0.111), Virginia (0.007), Wisconsin (0.007)
Montana	Nevada (0.242), Utah (0.758)
New Hampshire	Georgia (0.129), Minnesota (0.871)
New Jersey	Alabama (0.058), Connecticut (0.019), Delaware (0.085), Florida (0.040), Georgia (0.036), Illinois (0.032), Iowa (0.024), Kentucky (0.021), Louisiana (0.043), Maine (0.040), Michigan (0.021), Minnesota (0.043), Mississippi (0.037), Nebraska (0.026), Nevada (0.068), Oklahoma (0.142), Oregon (0.024), Rhode Island (0.025), South Carolina (0.019), South Dakota (0.052), Utah (0.014), Virginia (0.029), Wisconsin (0.102)
New Mexico	Kentucky (0.046), Nevada (0.023), Oregon (0.011), Rhode Island (0.275), South Dakota (0.355), Utah (0.184), Wisconsin (0.105)
New York	Georgia (0.287), Illinois (0.298), Louisiana (0.254), Nebraska (0.124), South Dakota (0.037)
North Carolina	Alabama (0.201), Georgia (0.246), Louisiana (0.553)
North Dakota	Alabama (0.091), Maine (0.371), Nevada (0.364), Rhode Island (0.173)
Ohio	Iowa (0.146), Kentucky (0.027), Michigan (0.324), Nevada (0.299), Oklahoma (0.199), Utah (0.006)
Pennsylvania	Delaware (0.098), Georgia (0.143), Maine (0.118), Minnesota (0.183), Nevada (0.004), South Dakota (0.232), Utah (0.162), Wisconsin (0.060)

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Table G3 – continued from previous page.

Treated State	Control States
Tennessee	Alabama (0.035), Connecticut (0.040), Delaware (0.056), Florida (0.036), Georgia (0.035), Illinois (0.034), Iowa (0.039), Kentucky (0.051), Louisiana (0.037), Maine (0.087), Michigan (0.041), Minnesota (0.040), Mississippi (0.038), Nebraska (0.051), Nevada (0.035), Oklahoma (0.028), Oregon (0.034), Rhode Island (0.105), South Carolina (0.045), South Dakota (0.030), Utah (0.023), Virginia (0.040), Wisconsin (0.040)
Texas	Alabama (0.036), Connecticut (0.032), Delaware (0.035), Florida (0.035), Georgia (0.037), Illinois (0.036), Iowa (0.037), Kentucky (0.035), Louisiana (0.037), Maine (0.019), Michigan (0.040), Minnesota (0.028), Mississippi (0.036), Nebraska (0.034), Nevada (0.064), Oklahoma (0.159), Oregon (0.033), Rhode Island (0.034), South Carolina (0.028), South Dakota (0.039), Utah (0.093), Virginia (0.034), Wisconsin (0.040)
Vermont	Maine (0.457), Minnesota (0.543)
Washington	Georgia (0.065), Nevada (0.313), Oklahoma (0.385), Oregon (0.207), Virginia (0.030)
West Virginia	Delaware (0.032), Georgia (0.174), Maine (0.032), Michigan (0.339), Nebraska (0.067), Oklahoma (0.196), Oregon (0.161)
Wyoming	Kentucky (0.072), Oregon (0.861), Utah (0.067)
Dependent Variable: Pct. Part-Time Kindergarten	
Alaska	Florida (0.116), Louisiana (0.056), Mississippi (0.098), Nevada (0.251), Oklahoma (0.113), South Dakota (0.027), Virginia (0.339)
Arizona	Georgia (0.043), Illinois (0.218), Kentucky (0.163), Louisiana (0.022), South Carolina (0.554)
Arkansas	Louisiana (0.864), Nevada (0.032), Virginia (0.104)
California	Alabama (0.081), Connecticut (0.012), Delaware (0.184), Florida (0.067), Georgia (0.027), Illinois (0.343), Iowa (0.039), Louisiana (0.080), Utah (0.168)

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Table G3 – continued from previous page.

Treated State	Control States
Colorado	Delaware (0.245), Iowa (0.100), Kentucky (0.071), Mississippi (0.210), Nevada (0.166), Oregon (0.066), Rhode Island (0.006), South Carolina (0.082), Wisconsin (0.053)
Idaho	Alabama (0.017), Connecticut (0.037), Delaware (0.052), Florida (0.013), Georgia (0.072), Illinois (0.027), Iowa (0.034), Kentucky (0.039), Louisiana (0.007), Maine (0.056), Michigan (0.032), Minnesota (0.046), Mississippi (0.028), Nebraska (0.034), Nevada (0.026), Oklahoma (0.026), Oregon (0.033), Rhode Island (0.038), South Carolina (0.251), South Dakota (0.030), Utah (0.029), Virginia (0.032), Wisconsin (0.041)
Indiana	Delaware (0.038), Georgia (0.172), Louisiana (0.017), Maine (0.299), Michigan (0.022), Nevada (0.451)
Kansas	Alabama (0.012), Connecticut (0.030), Delaware (0.355), Florida (0.024), Georgia (0.006), Illinois (0.025), Iowa (0.032), Kentucky (0.047), Louisiana (0.009), Maine (0.030), Michigan (0.029), Minnesota (0.030), Mississippi (0.035), Nebraska (0.031), Nevada (0.026), Oklahoma (0.024), Oregon (0.028), Rhode Island (0.058), South Carolina (0.037), South Dakota (0.026), Utah (0.026), Virginia (0.033), Wisconsin (0.047)
Maryland	Georgia (0.143), Louisiana (0.039), Maine (0.103), Nebraska (0.088), Oklahoma (0.462), South Carolina (0.003), South Dakota (0.162)
Massachusetts	Alabama (0.019), Connecticut (0.023), Delaware (0.014), Florida (0.019), Georgia (0.016), Illinois (0.486), Iowa (0.019), Kentucky (0.017), Louisiana (0.016), Maine (0.146), Michigan (0.018), Minnesota (0.018), Mississippi (0.017), Nebraska (0.018), Nevada (0.012), Oklahoma (0.020), Oregon (0.022), Rhode Island (0.016), South Carolina (0.026), South Dakota (0.023), Virginia (0.019), Wisconsin (0.015)

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Table G3 – continued from previous page.

Treated State	Control States
Missouri	Alabama (0.326), Connecticut (0.026), Delaware (0.017), Florida (0.027), Georgia (0.007), Illinois (0.037), Iowa (0.019), Kentucky (0.021), Louisiana (0.049), Maine (0.033), Michigan (0.023), Minnesota (0.021), Mississippi (0.012), Nebraska (0.025), Nevada (0.027), Oklahoma (0.021), Oregon (0.023), Rhode Island (0.023), South Carolina (0.031), South Dakota (0.020), Utah (0.026), Virginia (0.172), Wisconsin (0.015)
Montana	Alabama (0.008), Connecticut (0.017), Delaware (0.019), Florida (0.012), Georgia (0.010), Illinois (0.011), Iowa (0.016), Kentucky (0.021), Louisiana (0.006), Maine (0.029), Michigan (0.014), Minnesota (0.017), Mississippi (0.014), Nebraska (0.016), Nevada (0.013), Oklahoma (0.012), Oregon (0.014), Rhode Island (0.141), South Carolina (0.101), South Dakota (0.013), Utah (0.013), Virginia (0.467), Wisconsin (0.016)
New Hampshire	Georgia (0.129), Minnesota (0.871)
New Jersey	Alabama (0.024), Connecticut (0.020), Delaware (0.026), Florida (0.019), Georgia (0.162), Illinois (0.024), Iowa (0.028), Kentucky (0.024), Louisiana (0.045), Maine (0.017), Michigan (0.028), Minnesota (0.020), Mississippi (0.028), Nebraska (0.025), Nevada (0.168), Oklahoma (0.143), Oregon (0.023), Rhode Island (0.025), South Carolina (0.017), South Dakota (0.027), Utah (0.026), Virginia (0.022), Wisconsin (0.058)
New Mexico	Iowa (0.121), Michigan (0.377), Oregon (0.015), Rhode Island (0.218), South Dakota (0.042), Utah (0.080), Virginia (0.148)
New York	Georgia (0.257), Kentucky (0.025), Louisiana (0.306), Mississippi (0.050), Nebraska (0.182), South Carolina (0.179), Virginia (0.002)
North Carolina	Alabama (0.556), Delaware (0.225), Georgia (0.073), Louisiana (0.076), Mississippi (0.070)
North Dakota	Alabama (0.137), Kentucky (0.016), Louisiana (0.116), Maine (0.581), Nevada (0.151)

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Table G3 – continued from previous page.

Treated State	Control States
Ohio	Florida (0.037), Georgia (0.057), Louisiana (0.110), Nevada (0.428), Oklahoma (0.045), South Carolina (0.213), Virginia (0.110)
Pennsylvania	Florida (0.009), Georgia (0.198), Illinois (0.009), Iowa (0.020), Maine (0.077), Minnesota (0.242), Nebraska (0.006), Nevada (0.101), South Dakota (0.112), Utah (0.089), Wisconsin (0.138)
Tennessee	Alabama (0.427), Connecticut (0.004), Delaware (0.002), Florida (0.364), Illinois (0.005), Iowa (0.003), Kentucky (0.002), Maine (0.008), Michigan (0.002), Minnesota (0.002), Mississippi (0.003), Nebraska (0.002), Nevada (0.001), Oklahoma (0.002), Oregon (0.002), Rhode Island (0.002), South Carolina (0.160), South Dakota (0.002), Utah (0.001), Virginia (0.006), Wisconsin (0.002)
Texas	Alabama (0.248), Connecticut (0.026), Delaware (0.019), Florida (0.030), Georgia (0.004), Illinois (0.040), Iowa (0.021), Kentucky (0.021), Louisiana (0.019), Maine (0.033), Michigan (0.023), Minnesota (0.023), Mississippi (0.016), Nebraska (0.024), Nevada (0.024), Oklahoma (0.023), Oregon (0.024), Rhode Island (0.020), South Carolina (0.030), South Dakota (0.023), Utah (0.027), Virginia (0.264), Wisconsin (0.018)
Vermont	Alabama (0.104), Connecticut (0.025), Florida (0.088), Maine (0.172), Minnesota (0.518), South Carolina (0.093)
Washington	Georgia (0.071), Mississippi (0.011), Nevada (0.184), Oklahoma (0.134), Oregon (0.206), Utah (0.393)
West Virginia	Georgia (0.487), Kentucky (0.128), South Carolina (0.385)
Wyoming	Alabama (0.220), Georgia (0.055), Kentucky (0.621), Louisiana (0.054), South Dakota (0.050)
Dependent Variable: Days in School	

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Table G3 – continued from previous page.

Treated State	Control States
Alaska	Alabama (0.022), Connecticut (0.029), Delaware (0.026), Florida (0.024), Georgia (0.030), Illinois (0.027), Indiana (0.022), Iowa (0.033), Kentucky (0.026), Louisiana (0.028), Maine (0.031), Michigan (0.039), Minnesota (0.026), Mississippi (0.020), Nebraska (0.195), Nevada (0.032), Oklahoma (0.036), Oregon (0.161), Rhode Island (0.031), South Carolina (0.032), South Dakota (0.040), Utah (0.029), Virginia (0.031), Wisconsin (0.030)
Arizona	Alabama (0.892), Minnesota (0.020), Nebraska (0.088)
Arkansas	Alabama (0.027), Connecticut (0.036), Delaware (0.031), Florida (0.028), Georgia (0.037), Illinois (0.047), Indiana (0.026), Iowa (0.042), Kentucky (0.031), Louisiana (0.041), Maine (0.039), Michigan (0.052), Minnesota (0.031), Mississippi (0.025), Nebraska (0.044), Nevada (0.041), Oklahoma (0.044), Oregon (0.136), Rhode Island (0.038), South Carolina (0.040), South Dakota (0.050), Utah (0.036), Virginia (0.039), Wisconsin (0.038)
California	Florida (0.002), Michigan (0.268), Utah (0.730)
Colorado	Alabama (0.001), Connecticut (0.004), Delaware (0.003), Florida (0.001), Georgia (0.002), Illinois (0.369), Indiana (0.003), Iowa (0.003), Kentucky (0.002), Louisiana (0.010), Maine (0.003), Michigan (0.010), Minnesota (0.002), Mississippi (0.005), Nebraska (0.002), Nevada (0.004), Oklahoma (0.004), Oregon (0.554), Rhode Island (0.003), South Carolina (0.003), South Dakota (0.007), Utah (0.002), Virginia (0.003), Wisconsin (0.003)
Idaho	Michigan (0.621), Oregon (0.379)

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Table G3 – continued from previous page.

Treated State	Control States
Kansas	Alabama (0.029), Connecticut (0.027), Delaware (0.024), Florida (0.040), Georgia (0.039), Illinois (0.024), Indiana (0.019), Iowa (0.041), Kentucky (0.026), Louisiana (0.023), Maine (0.024), Michigan (0.036), Minnesota (0.022), Mississippi (0.017), Nebraska (0.073), Nevada (0.035), Oklahoma (0.031), Oregon (0.270), Rhode Island (0.039), South Carolina (0.038), South Dakota (0.016), Utah (0.039), Virginia (0.038), Wisconsin (0.032)
Maryland	Michigan (0.196), Nebraska (0.218), Utah (0.586)
Massachusetts	Alabama (0.089), Connecticut (0.037), Delaware (0.038), Florida (0.059), Georgia (0.043), Illinois (0.029), Indiana (0.033), Iowa (0.040), Kentucky (0.041), Louisiana (0.031), Maine (0.041), Michigan (0.032), Minnesota (0.048), Mississippi (0.030), Nebraska (0.062), Nevada (0.038), Oklahoma (0.038), Oregon (0.031), Rhode Island (0.042), South Carolina (0.040), South Dakota (0.034), Utah (0.043), Virginia (0.041), Wisconsin (0.039)
Missouri	Alabama (0.003), Connecticut (0.001), Delaware (0.001), Florida (0.001), Georgia (0.001), Illinois (0.002), Iowa (0.001), Kentucky (0.002), Louisiana (0.001), Maine (0.006), Michigan (0.001), Minnesota (0.372), Mississippi (0.317), Nebraska (0.003), Nevada (0.001), Oklahoma (0.004), Oregon (0.004), Rhode Island (0.001), South Carolina (0.001), South Dakota (0.273), Utah (0.001), Virginia (0.001), Wisconsin (0.001)
Montana	Alabama (0.009), Connecticut (0.014), Delaware (0.010), Florida (0.034), Georgia (0.032), Illinois (0.009), Indiana (0.007), Iowa (0.079), Kentucky (0.010), Louisiana (0.008), Maine (0.007), Michigan (0.330), Minnesota (0.005), Mississippi (0.004), Nebraska (0.075), Nevada (0.021), Oklahoma (0.009), Oregon (0.023), Rhode Island (0.024), South Carolina (0.065), South Dakota (0.007), Utah (0.150), Virginia (0.051), Wisconsin (0.017)
New Hampshire	Michigan (0.200), Nebraska (0.218), Utah (0.582)

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Table G3 – continued from previous page.

Treated State	Control States
New Jersey	Alabama (0.075), Connecticut (0.032), Delaware (0.030), Florida (0.049), Georgia (0.041), Illinois (0.025), Indiana (0.023), Iowa (0.040), Kentucky (0.033), Louisiana (0.025), Maine (0.040), Michigan (0.031), Minnesota (0.042), Mississippi (0.020), Nebraska (0.156), Nevada (0.036), Oklahoma (0.038), Oregon (0.034), Rhode Island (0.042), South Carolina (0.038), South Dakota (0.034), Utah (0.041), Virginia (0.039), Wisconsin (0.036)
New Mexico	Alabama (0.005), Connecticut (0.020), Delaware (0.015), Florida (0.007), Georgia (0.016), Illinois (0.036), Indiana (0.014), Iowa (0.021), Kentucky (0.013), Louisiana (0.031), Maine (0.018), Michigan (0.039), Minnesota (0.011), Mississippi (0.016), Nebraska (0.015), Nevada (0.022), Oklahoma (0.025), Oregon (0.550), Rhode Island (0.018), South Carolina (0.020), South Dakota (0.035), Utah (0.016), Virginia (0.018), Wisconsin (0.019)
New York	Alabama (0.053), Connecticut (0.039), Delaware (0.043), Florida (0.048), Georgia (0.039), Illinois (0.035), Indiana (0.052), Iowa (0.037), Kentucky (0.043), Louisiana (0.037), Maine (0.038), Michigan (0.034), Minnesota (0.043), Mississippi (0.089), Nebraska (0.037), Nevada (0.038), Oklahoma (0.036), Oregon (0.031), Rhode Island (0.039), South Carolina (0.038), South Dakota (0.035), Utah (0.040), Virginia (0.038), Wisconsin (0.039)
North Carolina	Alabama (0.016), Connecticut (0.087), Delaware (0.034), Florida (0.045), Georgia (0.028), Illinois (0.034), Indiana (0.034), Iowa (0.027), Kentucky (0.023), Louisiana (0.044), Maine (0.014), Michigan (0.044), Minnesota (0.010), Mississippi (0.324), Nebraska (0.014), Nevada (0.029), Oklahoma (0.016), Oregon (0.015), Rhode Island (0.026), South Carolina (0.032), South Dakota (0.012), Utah (0.032), Virginia (0.031), Wisconsin (0.029)

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Table G3 – continued from previous page.

Treated State	Control States
North Dakota	Alabama (0.005), Connecticut (0.010), Delaware (0.007), Florida (0.006), Georgia (0.014), Illinois (0.005), Indiana (0.006), Iowa (0.026), Kentucky (0.007), Louisiana (0.011), Maine (0.008), Michigan (0.340), Minnesota (0.005), Mississippi (0.006), Nebraska (0.066), Nevada (0.014), Oklahoma (0.011), Oregon (0.373), Rhode Island (0.014), South Carolina (0.019), South Dakota (0.003), Utah (0.015), Virginia (0.017), Wisconsin (0.011)
Ohio	Alabama (0.034), Connecticut (0.029), Delaware (0.026), Florida (0.032), Georgia (0.037), Illinois (0.028), Indiana (0.020), Iowa (0.040), Kentucky (0.028), Louisiana (0.026), Maine (0.037), Michigan (0.036), Minnesota (0.033), Mississippi (0.018), Nebraska (0.216), Nevada (0.036), Oklahoma (0.041), Oregon (0.061), Rhode Island (0.039), South Carolina (0.037), South Dakota (0.040), Utah (0.036), Virginia (0.037), Wisconsin (0.033)
Pennsylvania	Michigan (0.183), Nebraska (0.027), Utah (0.790)
Tennessee	Alabama (0.032), Connecticut (0.033), Delaware (0.028), Florida (0.032), Georgia (0.039), Illinois (0.034), Indiana (0.020), Iowa (0.043), Kentucky (0.030), Louisiana (0.031), Maine (0.040), Michigan (0.043), Minnesota (0.034), Mississippi (0.017), Nebraska (0.085), Nevada (0.040), Oklahoma (0.045), Oregon (0.132), Rhode Island (0.041), South Carolina (0.040), South Dakota (0.046), Utah (0.038), Virginia (0.040), Wisconsin (0.037)
Texas	Alabama (0.571), Minnesota (0.274), Nebraska (0.155)
Vermont	Alabama (0.310), Connecticut (0.029), Delaware (0.037), Florida (0.053), Georgia (0.031), Illinois (0.014), Indiana (0.048), Iowa (0.026), Kentucky (0.039), Louisiana (0.019), Maine (0.029), Michigan (0.014), Minnesota (0.041), Mississippi (0.059), Nebraska (0.031), Nevada (0.025), Oklahoma (0.022), Oregon (0.009), Rhode Island (0.029), South Carolina (0.027), South Dakota (0.017), Utah (0.032), Virginia (0.029), Wisconsin (0.029)

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Table G3 – continued from previous page.

Treated State	Control States
Washington	Alabama (0.029), Connecticut (0.035), Delaware (0.032), Florida (0.027), Georgia (0.031), Illinois (0.023), Indiana (0.029), Iowa (0.040), Kentucky (0.031), Louisiana (0.035), Maine (0.037), Michigan (0.047), Minnesota (0.028), Mississippi (0.027), Nebraska (0.040), Nevada (0.040), Oklahoma (0.039), Oregon (0.152), Rhode Island (0.037), South Carolina (0.038), South Dakota (0.037), Utah (0.032), Virginia (0.099), Wisconsin (0.035)
West Virginia	Alabama (0.002), Connecticut (0.002), Delaware (0.002), Florida (0.235), Georgia (0.010), Illinois (0.002), Indiana (0.001), Iowa (0.012), Kentucky (0.002), Louisiana (0.002), Maine (0.001), Michigan (0.023), Minnesota (0.001), Mississippi (0.001), Nebraska (0.008), Nevada (0.005), Oklahoma (0.002), Oregon (0.002), Rhode Island (0.007), South Carolina (0.012), South Dakota (0.001), Utah (0.650), Virginia (0.013), Wisconsin (0.004)
Wyoming	Alabama (0.038), Connecticut (0.029), Delaware (0.027), Florida (0.035), Georgia (0.038), Illinois (0.027), Indiana (0.021), Iowa (0.041), Kentucky (0.029), Louisiana (0.026), Maine (0.038), Michigan (0.035), Minnesota (0.035), Mississippi (0.018), Nebraska (0.207), Nevada (0.036), Oklahoma (0.041), Oregon (0.056), Rhode Island (0.040), South Carolina (0.037), South Dakota (0.039), Utah (0.037), Virginia (0.037), Wisconsin (0.034)
Dependent Variable: Minutes in School Day	
Alaska	Michigan (0.595), Rhode Island (0.058), South Carolina (0.347)
Arizona	Delaware (0.190), South Dakota (0.036), Utah (0.774)

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Table G3 – continued from previous page.

Treated State	Control States
Arkansas	Alabama (0.033), Connecticut (0.022), Delaware (0.019), Florida (0.029), Georgia (0.020), Illinois (0.057), Indiana (0.029), Iowa (0.034), Kentucky (0.052), Louisiana (0.022), Maine (0.037), Michigan (0.015), Minnesota (0.227), Mississippi (0.035), Nebraska (0.015), Nevada (0.066), Oklahoma (0.023), Oregon (0.026), Rhode Island (0.040), South Carolina (0.017), South Dakota (0.044), Utah (0.067), Virginia (0.025), Wisconsin (0.045)
California	Nebraska (0.207), South Dakota (0.656), Utah (0.137)
Colorado	Alabama (0.020), Connecticut (0.003), Delaware (0.032), Florida (0.003), Georgia (0.005), Illinois (0.006), Indiana (0.007), Iowa (0.003), Kentucky (0.006), Louisiana (0.003), Maine (0.002), Michigan (0.002), Mississippi (0.006), Nebraska (0.525), Nevada (0.003), Oklahoma (0.006), Oregon (0.004), Rhode Island (0.002), South Carolina (0.003), South Dakota (0.343), Utah (0.006), Virginia (0.003), Wisconsin (0.007)
Idaho	Alabama (0.027), Connecticut (0.021), Delaware (0.015), Florida (0.012), Georgia (0.016), Illinois (0.021), Indiana (0.020), Iowa (0.076), Kentucky (0.020), Louisiana (0.033), Maine (0.031), Michigan (0.021), Minnesota (0.017), Mississippi (0.031), Nebraska (0.019), Nevada (0.444), Oklahoma (0.020), Oregon (0.015), Rhode Island (0.019), South Carolina (0.015), South Dakota (0.025), Utah (0.029), Virginia (0.023), Wisconsin (0.031)
Kansas	Alabama (0.037), Connecticut (0.032), Delaware (0.022), Florida (0.027), Georgia (0.026), Illinois (0.030), Indiana (0.029), Iowa (0.045), Kentucky (0.029), Louisiana (0.052), Maine (0.038), Michigan (0.031), Minnesota (0.025), Mississippi (0.054), Nebraska (0.027), Nevada (0.076), Oklahoma (0.029), Oregon (0.024), Rhode Island (0.033), South Carolina (0.026), South Dakota (0.043), Utah (0.055), Virginia (0.033), Wisconsin (0.177)

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Table G3 – continued from previous page.

Treated State	Control States
Maryland	Alabama (0.019), Connecticut (0.036), Delaware (0.022), Florida (0.043), Georgia (0.039), Illinois (0.015), Indiana (0.020), Iowa (0.025), Kentucky (0.017), Louisiana (0.021), Maine (0.024), Michigan (0.082), Minnesota (0.015), Mississippi (0.017), Nebraska (0.026), Nevada (0.017), Oklahoma (0.021), Oregon (0.030), Rhode Island (0.025), South Carolina (0.417), South Dakota (0.015), Utah (0.014), Virginia (0.027), Wisconsin (0.015)
Massachusetts	Alabama (0.029), Connecticut (0.029), Delaware (0.121), Florida (0.031), Georgia (0.039), Illinois (0.035), Indiana (0.035), Iowa (0.023), Kentucky (0.035), Louisiana (0.027), Maine (0.025), Michigan (0.034), Minnesota (0.047), Mississippi (0.027), Nebraska (0.042), Nevada (0.026), Oklahoma (0.036), Oregon (0.181), Rhode Island (0.026), South Carolina (0.040), South Dakota (0.029), Utah (0.027), Virginia (0.029), Wisconsin (0.026)
Missouri	Alabama (0.013), Connecticut (0.017), Florida (0.145), Georgia (0.020), Illinois (0.012), Indiana (0.015), Iowa (0.008), Kentucky (0.015), Louisiana (0.010), Maine (0.010), Michigan (0.014), Minnesota (0.009), Mississippi (0.010), Nebraska (0.012), Nevada (0.009), Oklahoma (0.014), Oregon (0.581), Rhode Island (0.013), South Carolina (0.029), South Dakota (0.010), Utah (0.009), Virginia (0.016), Wisconsin (0.009)
Montana	Alabama (0.031), Connecticut (0.023), Delaware (0.184), Florida (0.016), Georgia (0.027), Illinois (0.050), Indiana (0.035), Iowa (0.017), Kentucky (0.037), Louisiana (0.035), Maine (0.018), Michigan (0.039), Minnesota (0.043), Mississippi (0.042), Nebraska (0.061), Nevada (0.033), Oklahoma (0.041), Oregon (0.030), Rhode Island (0.017), South Carolina (0.024), South Dakota (0.064), Utah (0.059), Virginia (0.025), Wisconsin (0.049)

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Table G3 – continued from previous page.

Treated State	Control States
New Hampshire	Alabama (0.022), Connecticut (0.015), Delaware (0.011), Florida (0.009), Georgia (0.014), Illinois (0.021), Indiana (0.019), Iowa (0.015), Kentucky (0.019), Louisiana (0.032), Maine (0.014), Michigan (0.021), Minnesota (0.015), Mississippi (0.040), Nebraska (0.015), Nevada (0.034), Oklahoma (0.019), Oregon (0.014), Rhode Island (0.012), South Carolina (0.012), South Dakota (0.001), Utah (0.429), Virginia (0.016), Wisconsin (0.182)
New Jersey	Alabama (0.019), Connecticut (0.016), Delaware (0.021), Florida (0.007), Georgia (0.016), Illinois (0.021), Indiana (0.018), Iowa (0.010), Kentucky (0.016), Louisiana (0.286), Maine (0.010), Michigan (0.057), Minnesota (0.014), Mississippi (0.035), Nebraska (0.223), Nevada (0.024), Oklahoma (0.024), Oregon (0.013), Rhode Island (0.008), South Carolina (0.016), South Dakota (0.038), Utah (0.043), Virginia (0.016), Wisconsin (0.049)
New Mexico	Iowa (0.199), Louisiana (0.404), Michigan (0.397)
New York	Maine (0.430), Michigan (0.106), Minnesota (0.066), Mississippi (0.168), Oregon (0.230)
North Carolina	Alabama (0.024), Connecticut (0.016), Delaware (0.163), Florida (0.011), Georgia (0.018), Illinois (0.055), Indiana (0.027), Iowa (0.013), Kentucky (0.031), Louisiana (0.025), Maine (0.013), Michigan (0.020), Minnesota (0.041), Mississippi (0.036), Nebraska (0.031), Nevada (0.027), Oklahoma (0.030), Oregon (0.022), Rhode Island (0.012), South Carolina (0.015), South Dakota (0.188), Utah (0.113), Virginia (0.018), Wisconsin (0.050)
North Dakota	Minnesota (0.256), Rhode Island (0.744)

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Table G3 – continued from previous page.

Treated State	Control States
Ohio	Alabama (0.007), Connecticut (0.015), Delaware (0.178), Florida (0.013), Georgia (0.011), Illinois (0.019), Indiana (0.013), Iowa (0.007), Kentucky (0.014), Louisiana (0.009), Maine (0.009), Michigan (0.268), Minnesota (0.279), Mississippi (0.007), Nebraska (0.006), Nevada (0.005), Oklahoma (0.021), Oregon (0.051), Rhode Island (0.011), South Carolina (0.011), South Dakota (0.009), Utah (0.014), Virginia (0.014), Wisconsin (0.008)
Pennsylvania	Connecticut (0.169), Iowa (0.283), Utah (0.547)
Tennessee	Alabama (0.005), Connecticut (0.008), Delaware (0.002), Florida (0.407), Georgia (0.019), Illinois (0.004), Indiana (0.006), Iowa (0.002), Kentucky (0.006), Louisiana (0.004), Maine (0.004), Michigan (0.005), Minnesota (0.006), Mississippi (0.003), Nebraska (0.004), Nevada (0.003), Oklahoma (0.005), Oregon (0.444), Rhode Island (0.005), South Carolina (0.043), South Dakota (0.002), Utah (0.002), Virginia (0.008), Wisconsin (0.003)
Texas	Alabama (0.016), Connecticut (0.022), Florida (0.185), Georgia (0.026), Illinois (0.014), Indiana (0.018), Iowa (0.010), Kentucky (0.017), Louisiana (0.013), Maine (0.014), Michigan (0.016), Minnesota (0.011), Mississippi (0.013), Nebraska (0.013), Nevada (0.013), Oklahoma (0.016), Oregon (0.471), Rhode Island (0.017), South Carolina (0.041), South Dakota (0.012), Utah (0.011), Virginia (0.021), Wisconsin (0.011)
Vermont	Alabama (0.009), Connecticut (0.046), Delaware (0.008), Florida (0.023), Georgia (0.012), Illinois (0.009), Indiana (0.009), Iowa (0.268), Kentucky (0.008), Louisiana (0.022), Maine (0.079), Michigan (0.299), Minnesota (0.008), Mississippi (0.009), Nebraska (0.011), Nevada (0.015), Oklahoma (0.011), Oregon (0.010), Rhode Island (0.061), South Carolina (0.033), South Dakota (0.008), Utah (0.010), Virginia (0.022), Wisconsin (0.009)
Washington	Florida (0.451), Indiana (0.129), Minnesota (0.420)
West Virginia	Maine (0.042), Minnesota (0.561), Mississippi (0.397)

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Table G3 – continued from previous page.

Treated State	Control States
Wyoming	Delaware (0.378), Michigan (0.367), Nebraska (0.254)