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Does Centralization Promote Fiscal Health? The Effect of State Takeover of Local School Districts on Education Finance

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Abstract

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Introduction

Fiscal health is a critical precondition for local governments to effectively carry out their essential functions (McDonald 2018; Jimenez 2009). A major challenge for states is determining how to best support lower levels of government when localities experience fiscal or performance challenges without incentivizing future financial mismanagement. Would shifting greater authority to higher levels of government be a helpful or harmful response?

Scholars from the classical liberal economics tradition argue that decentralization encourages fiscal responsibility because of increased responsiveness to citizens and greater accountability and competition at lower levels of government (Hayek 1939; Tiebout 1956). However, more recent work on fiscal federalism suggests that decentralization could instead encourage fiscal irresponsibility because localities may perceive the potential for a bailout from a higher level of government (Rodden, 2006; Berry, 2008). The theoretical debate about whether centralization harms or improves fiscal health of local communities is far from settled (Manna 2013; Moffitt et al. 2021).

We study this topic in the context of U.S. public education. This area is ripe for contribution to centralization theory given education governance has historically been notably decentralized but has experienced greater centralization in recent decades as the federal government, state leaders, and executives at all levels have taken on a larger role (Henig 2013). Additionally, public schools have proven a helpful context for understanding important theoretical questions regarding public affairs, management, and governance broadly speaking such as managerial networking (Meier and O'Toole 2003; 2001; Nicholson-Crotty et al. 2016), public service motivation (L. B. Andersen, Heinesen, and Pedersen 2014), performance information (Heinrich 1999; Holbein and Hassell 2019; Barrows et al. 2016; Valant and Newark

2020), private sector competition (S. C. Andersen and Serritzlew 2007), and the determinants of organizational performance (Meier and O'Toole Jr 2002; W. Wang and Yeung 2019).

More specifically, we examine the effect of one particular form of political centralization—state takeover of local school districts—on fiscal outcomes. State takeover represents a shift in decision-making authority away from a locally elected school board and to the state. Historically state takeover has often occurred in response to perceived fiscal distress, fiscal mismanagement, or even fiscal crisis. Although it is hardly a regular occurrence, it has become an increasingly common response to local performance challenges since the first documented case in the late 1980s (Schueler and Bleiberg 2021). State takeovers also provide a unique opportunity to study issues of political centralization in the context of state-local dynamics, which is an important area for research given much of the literature on the topic has focused on devolution from federal to state levels of government (Sosin 2012) or administrative rather than political centralization (Kogan 2017).

The existing literature on state takeovers of school systems is relatively small. Previous studies find no evidence that, on average, it benefits student academic achievement (Schueler and Bleiberg 2021; Wong et al. 2007). Studying the political implications, Morel (2018) finds that takeover decreases descriptive representation of Black citizens when it occurs in majority-Black communities but increases descriptive representation when implemented in majority-Latino communities. In terms of fiscal outcomes, Wong and Shen (2003) find that takeovers in the 1990s are associated with lower overall per pupil spending while Schueler and Bleiberg (2021) find no evidence that takeovers from 2011 to 2016 affected expenditures. Additional evidence is needed to help reconcile these findings, examine takeovers over a longer period of time, and to explore a broader set of fiscal outcomes representing the financial health of local

governments. Additionally, it remains unknown why takeovers have become more common over the last 30 years if they do not seem to benefit students, on average, and are known to generate resistance from members of the targeted communities which tend to be home to disproportionate numbers of low-income residents of color (Schueler and West 2021).

In this paper, we examine the effect of political centralization on the fiscal condition of lower levels of government with an empirical study of state takeovers of local school districts over the past three decades. We use an original dataset that tracks all state takeovers of school districts combined with district-level finance information primarily from the National Center for Education Statistic's Common Core of Data and the U.S. Census. We observe 98 first-time takeovers during the 29-year period in which school finance data (i.e., F-33 surveys) is available (1990 to 2019). The data cover a near census of takeovers, which began in 1988 (Oluwole and Green 2009). The finance data allow us to examine district financial condition, defined as the ability of a government to provide adequate services while meeting both current and future financial obligations (X. Wang, Dennis, and Tu 2007). We also analyze the sources of revenues and the types of expenditures for each district over time. Our analytical approach is an event study model that allows us to isolate the effect of takeover on fiscal outcomes by comparing the change in outcomes for takeover districts before versus after takeover to the change in outcomes for districts not taken over in the same period.

We find that, on average, state takeovers increased school spending substantially and led to improvements in financial condition. Specifically, state takeover increased annual school spending by over $2,000 per student after five years, leading to improvements in both budgetary solvency and long-run solvency. Takeover increased expenditures to a larger degree when combined with performance accountability measures. Takeover effects were also strongest in

large school districts and those with high levels of initial debt. Takeover effects were weakest in majority Black communities. A deeper investigation into how the money was spent suggests that takeover led to increased expenditures on legacy costs such as employee benefits and debt retirement rather than raising salaries or lowering class sizes. We also provide evidence that state takeover represents a form of bailout of the district by the state in that takeover increased state revenues to targeted districts.

Despite the increased expenditures and overall improvements to fiscal condition, takeover may remain an undesirable form of bailout because the additional funding is accompanied by the removal of political autonomy. This is particularly true in majority-Black communities (Morel 2018) where our results suggest takeovers are less impactful than in communities serving lower concentrations of Black students. In sum, we find that political centralization, at least in the form of state takeover of school districts, can improve local fiscal condition, although not without substantial costs to both state budgets and local leaders.

Centralization and Fiscal Condition

Fiscal condition "describes a government's ability to meet its financial and service obligations" (Jimenez 2009) and is therefore an essential precondition for effective public management. Scholars have yet to resolve an enduring debate over whether centralization—the concentration of control under a single authority—improves or harms fiscal condition within localities (Krane, Ebdon, and Bartle 2004). K-12 school systems in the U.S. provide a useful context to examine this topic given the historically decentralized nature of education governance, a responsibility delegated by the U.S. Constitution to the states, of which the states have typically delegated substantial authority to local decisionmakers at the school district level (Kirst 2004). Despite this tradition of local control, observers have noted a trend toward greater centralization

in the most recent decades, with the passage of major federal education legislation, executives at the city and state level taking on a greater role in education policymaking, and state takeovers of local school systems with financial or academic troubles becoming more common (Henig 2013; Morel 2018; Peterson 1995). The Every Student Succeeds Act (ESSA) of 2015 then delegated some authority back to states and local governments (Wong 2020). These shifts illustrate the ongoing and policy-relevant debate about the proper role of federal, state, and local authorities in governing school systems. They also provide rare but useful opportunities to study variation in the degree to which education governance is centralized. Though it is certainly possible that centralization has no systematic impact on fiscal health (Jimenez 2009), there are strong theoretical reasons to suggest that centralization could either harm or improve fiscal condition.

How Centralization Could Harm Fiscal Condition

On the one hand, centralization—particularly from the local to the state level—might harm fiscal condition within localities. First, scholars from the classical liberal economics tradition have argued that decentralization encourages fiscal responsibility because of increased responsiveness and accountability at lower levels of government, as well as greater competition between localities (Hayek 1939; Tiebout 1956). In short, state leaders may have fewer incentives to tax efficiently because they perceive that there is less of a risk that residents will exit their jurisdiction compared to local leaders, given it is typically more challenging to move across state lines than across city, county, or school district boundaries.

Second, state leaders may be less representative of local communities than local leaders and therefore less responsive to constituents or not as incentivized to foster fiscal health. For these reasons, proponents of "progressive federalism" argue that minoritized groups can gain greater power and influence at more localized levels of government, particularly in contexts

where they represent a minority at the state but not at the local level (Gerken 2012). In the case of state intervention into school systems, state takeover of school districts has been shown to decrease the descriptive representation of Black citizens in local elected office when it occurs in majority-Black school districts (Morel 2018).[1] Decreased descriptive representation could be harmful to fiscal well-being, given that scholars have documented that decreases in non-White political representation on school boards leads to decreased spending on non-White students (Fischer 2020; Kogan, Lavertu, and Peskowitz 2021).

Furthermore, state intervention may be destabilizing or disruptive to local communities (Henig et al. 2001) and is typically most unpopular among members of the targeted local communities because local actors are likely to lose political and economic power as a result of such reforms (Morel and Nuamah 2020; Schueler and West 2021). Additionally, shifting the venue of decision-making from the local to the state level could reduce the power of interest groups with established influence at the current level of government, such as teachers' unions (Anzia 2013). This may, in turn, decrease overall spending on education in the local district, given unions are some of the most powerful advocates for increased education spending (Burns 2003). If spending increases are helpful for promoting fiscal health, centralization could therefore be detrimental.

How Centralization Could Improve Fiscal Condition

On the other hand, there are several reasons to believe that centralization might improve fiscal condition. First, scholars of "fiscal federalism" (Oates 1999) suggest that decentralization could encourage fiscal irresponsibility among localities because local leaders believe they will be bailed out by higher levels of government if they run into major financial or performance

[1] Although the reverse is true when takeover occurs in majority-Latino communities.

challenges (Rodden 2006). More specifically, when lower-level governments like school districts rely on grants and revenue sharing, they may expect to be bailed out by central governments (e.g., states) when things go wrong and therefore act with less fiscal discipline. Importantly, their creditors may also expect them to be bailed out and will therefore be more willing to take on risky endeavors. Even when centralized governments have a "no bailout" policy, these policies are not always credible. In contrast, states may act more fiscally responsible than localities because there is no equivalent bailout option for states.

Second, fiscal irresponsibility among localities may be especially likely to arise in the case of special function jurisdictions—which in the U.S. are most commonly school districts—because shared fiscal authority among a variety of functions of local governments representing a range of priorities can lead to "overfishing"—or overtaxing and overspending—as each authority seeks to maximize funding for its own priority (e.g., school boards for education; Berry 2008). In contrast, more centralized state policymakers must negotiate a broader agenda of priorities reducing ambiguity and the likelihood of overfishing.

Third, to the extent that higher spending levels promote fiscal health, centralization could facilitate greater financial well-being at the local level through redistribution across localities. This may be true given redistribution is more limited in scope at lower levels of government both because there are greater amounts of inequity across than within localities and because localities do not have the authority nor incentive to distribute resources outside of their own boundaries. Localities may also be unlikely to pursue policies that distribute resources to low income individuals out of a concern that high income (and therefore high tax-paying) individuals may exit the local community as a result, whereas migration across state borders is less common (Peterson 1981). Indeed, school finance reforms from 1989 to 2010, which were intended to

address unequal levels of funding across local districts due to inequity in local property wealth, centralized and redistributed resources across localities (Candelaria and Shores 2019). State funding formulas have counteracted regressive local funding schemes, on average, raising spending in traditionally disadvantaged communities (Chingos and Blagg 2017).

A fourth reason that centralization could promote fiscal health is if the interests of residents are better represented at the state than local level. In contrast to the progressive federalism argument, McDermott (1999) among others, argues that local school district governance has not lived up to its promise of allowing for citizen voice in the policymaking process. Centralization can shift the venue of decision-making to one in which teachers' unions have less influence, and some argue that this shift would improve fiscal condition in cases where union influence has led to inefficient or unsustainable resources expenditures (Burns 2003). Fifth and finally, states may simply have greater administrative capacity than local governments to manage fiscal issues effectively and therefore centralizing to the state level could promote fiscal health (Andrews et al. 2009; Brown and Potoski 2003; Hong 2017; Kogan 2017).

Potential Variation in the Effect of Centralization

The resolution of this puzzle about whether centralization promotes or hinders fiscal health may depend on a few factors. First, it may depend on the unit of centralization (federal versus state; Peterson, 1995; Peterson et al., 2010). Here we focus on centralization from the local to state level, though centralization from the states to the federal level could have very different consequences. Second, the effect of centralization on fiscal health may depend on the type of centralization being deployed. Scholars have outlined three primary types of centralization: (1) political, (2) administrative, and (3) fiscal (Manna 2013; Treisman 2007). In short, political centralization refers to the locus of decision-making power. Administrative

centralization concerns the extent to which a central authority relies on bureaucracies to implement its policies. Finally, fiscal centralization describes the degree of responsibility for collecting, distributing, and spending public revenues assigned to different levels of government. Definitionally, state takeover primarily involves political centralization, though it could be accompanied by forms of fiscal centralization (or decentralization) given that takeovers are typically enacted in response to fiscal distress.

Third, the effects of centralization may depend on the policy or political context. One important policy factor may be the extent to which higher levels of government have mechanisms to hold local systems accountable for the outcomes they produce, so called "performance-based federalism" (Kogan, Lavertu, and Peskowitz 2021; Wong 2013). For example, greater accountability may make states more willing to provide funding to relatively low resource communities and focus takeover efforts on improving academic achievement rather than improving fiscal condition.

The effect of centralization on fiscal outcomes may also vary depending on the size of the locality in question. State governments may be uncomfortable with authorizing the takeover of very large school districts because of the sheer logistical difficulty of running large systems. Larger districts may also have more success mounting a political opposition to takeover efforts which could complicate the implementation of reforms. Though states are the primary authorities controlling public education, state education agencies have tended to delegate that authority to localities and in turn tend to have relative low levels of administrative and technical capacity (Moffitt et al. 2021). Additionally, larger districts likely have the capacity to borrow more debt and therefore may have more substantial fiscal troubles that states are less able to address, given the sheer magnitude of the debt that a district like the New York City Department of Education

would be able to take on. Thus, the nature of takeover may be very different in a district serving millions of students, relative to one serving hundreds of students.

Finally, the effect of centralization may vary depending on the demographic composition of local communities. For example, state leaders, particularly in majority-White states, may be less responsive to the preferences of residents in localities that are home to large concentrations of people of color. Therefore, centralization may be less likely to improve fiscal condition in these districts. Relatedly, if centralization differentially affects local descriptive representation depending on the racial or ethnic makeup of a given locality, this may lead to differing impacts on fiscal outcomes. This may be especially likely in the case of state takeover of school systems where scholars have documented more positive effects on student outcomes when takeover occurs in majority-Hispanic than majority-Black districts (Schueler and Bleiberg 2021). Similarly, takeover appears to decrease descriptive representation when enacted in majority-Black districts but increase it when undertaken in majority-Latino contexts (Morel 2018). Given non-White representation on school boards increases educational spending on non-White students (Fischer 2020; Kogan, Lavertu, and Peskowitz 2021), takeover could differentially impact fiscal outcomes depending on the racial or ethnic composition of the targeted district, operating through changes in local descriptive representation.

State Takeover of Local School Districts

As we have described, state takeover is a shift in decision-making power from a locally elected school board to the state. The state then has the authority to implement a variety of interventions including direct administration of the district; transferring authority to another official such as the mayor; abolishing the school board; and appointing all or some of a new school board or a new superintendent, receiver, or emergency manager to operate the system.

Regardless of what the state does with its authority, takeover always involves a shift in political power from the local board to the state.

States often invoke multiple justifications for takeover including poor academic performance and fiscal condition. Fiscal distress or mismanagement had historically been the most frequent justification for district takeover (Oluwole and Green 2009), and state takeover of local governments occurs most frequently after recessions when fiscal stress is the most acute (Kogan 2015). It can also occur when there are other forms of mismanagement and, in more recent years with the shift toward universal test-based accountability policy, takeover of school districts has increasingly been undertaken to address persistently poor student academic performance (Jochim 2016).

Thirty four states have laws that specifically authorize the takeover of school districts (Jochim 2016). Every state has laws that allow for fiscal interventions; however, the scope, trigger for state action, and responsible government agencies varies widely (Bowman and Zuschlag 2022). Legislatures in some states have authorized dozens of fiscal interventions while other states permit a narrower set of responses. Twenty states have passed laws that allow the state to intervene in the finances of a local government (Huh, Fehr, and Murphy 2013) or declare a fiscal emergency (Beckett-Camarata 2004). States can also use these authorities to regulate local governments to take over school districts. Most states do not have defined indicators to measure fiscal distress, which provides flexibility to take over schools due to a wide variety of fiscal conditions (Kloha, Weissert, and Kleine 2005).

Takeover is a relatively rare intervention reserved as a reform of last resort for state education leaders but has become more common over time. Figure 1 describes the number of

districts taken over by states across three decades based on our own tracking data.[2] About three to four districts are taken over in a typical year. The first wave of state takeovers began in the late 1980s. Takeovers increased in the periods around the passage of the national school accountability legislation No Child Left Behind (NCLB) in 2001 and after the school-improvement-focused Race to the Top reforms in 2010.

We study three decades of state takeovers of local school systems to understand the impact of takeover in education governance, as a form of political centralization, on local finances. More specifically, we examine the effect of takeover on education spending in the targeted districts and on local fiscal condition. We next test whether the impact of takeover varies depending on the presence of accountability mechanisms, fiscal centralization, district size, and racial/ethnic characteristics. Finally, we extend our analysis with an investigation into how new funds are spent and whether state takeover functions as a fiscal bailout of the district.

Data and Sample

We analyze district-by-year data on state takeover, school spending, and fiscal condition for the years 1990 to 2019 using an original dataset of state takeovers covering this entire 29-year panel. Our sample includes 11,034 traditional school districts.[3] All outcome measures are from district F-33 survey data downloaded from the Common Core of Data (CCD) and the U.S. Census (2019 data only). Table 1 displays baseline averages for districts that experience takeovers and those that do not. Baseline years for all outcomes are also shown in Table 1. We restrict our analyses to include the years from 1998 to 2019 for five outcomes that are only

[2] We describe the methods for collecting these data in the "Data and Sample" section of the paper below.
[3] We show that our results are not driven by sample missingness or sample inclusion with a series of falsification tests (see Appendix Figure B1 and Appendix C for more details).

observable in those years (cash on hand, debt service coverage ratios, salary expenditures, benefit expenditures, and debt retirement).

Tracking State Takeovers

To track state takeovers we follow the procedure outlined in detail by Schueler and Bleiberg (2021). In short, we began by compiling pre-existing lists of takeovers that had been collected by other scholars for subsets of the time period we covered. We then used Google News Advanced Search and Education Week Archives to verify this tracking and to supplement years when we had no coverage based on other researchers' work. We tracked the district, state, and first and last year of takeover for each individual takeover event.

Ultimately, we observe 98 first-time takeovers in the panel from 1990 to 2019 and 84 takeovers from 1998 to 2019 (the period for which more detailed school spending information is available). Appendix Table A1 describes the number of districts that we observe across relative time that we use to identify the effect of takeover. Our event study has sufficient power to identify the effect of takeover for up to 10 years after state intervention. Five years after treatment we observe outcomes for 73 treated districts and 10 years after treatment, we observe outcomes for 57 treated districts. Additionally, in Appendix B we demonstrate that findings are very similar if we analyze only the takeover districts that we observe during the entire period of study (i.e., we balance the panel on relative time instead of calendar time).

We observe some districts that enter, exit, and then re-enter takeover. For our analysis, once a district is taken over, we consider it to be taken over in every post-treatment year. That definition of treatment avoids the potentially confounding relationship between subsequent changes to takeover status (i.e., exiting, re-entering) with district financial outcomes. In auxiliary analyses we find that our results are robust to alternative definitions of takeover and to

controlling for the number of years since exit from takeover and since beginning subsequent takeovers (see Appendix B).

We compare the baseline characteristics of takeover districts to non-takeover districts in Table 1. Takeover districts tend to be much larger than non-takeover districts, at baseline. Consistent with prior work, takeover districts also, on average, had a much lower share of White students (38% versus 83%) and a larger share of Black students (50% versus 7%) relative to non-takeover districts. However, at baseline, takeover and non-takeover districts had similar proportions of students with special educational needs and students who were eligible for free meals at school.

Measures of School Spending and Fiscal Condition

Capturing organizational fiscal condition is challenging because it demands a complex and multidimensional approach (Finkler, Calabrese, and Smith 2022; Gorina, Maher, and Joffe 2018; Hendrick 2004; Johnson, Kioko, and Hildreth 2012; Stone et al. 2015; X. Wang, Dennis, and Tu 2007), particularly when examining public school districts (Kim and Sorensen 2019; Ladd 1996). In this study, we consider four key ratios as our primary measures of district spending and fiscal condition: per-pupil expenditures, cash held per capita, the operating ratio, and the Debt Service Coverage Ratio (DSCR). We recognize that the process of financial condition ratio creation and analysis "is more of an art than a science" (Finkler, Calabrese, and Smith 2022, 787), and so we use contextual knowledge about local school districts and public education financial reporting to focus on a series of ratios that are most consequential, relevant to the public education, and informative. In the section that follows we detail each of these ratios, paying particular attention to how each ratio is calculated, as well as the information that it does and does not imply.

Our first fiscal outcome measure is per pupil educational expenditures, measured by average annual public school spending divided by total enrollment. We use a district-level measure of yearly expenditures per student in real 2019 dollars. As we show in Table 1, at baseline, takeover districts spent slightly less ($297 per pupil per year) than the average district that was never taken over.

To measure fiscal health, we consider cash solvency, budgetary solvency, and long-run solvency respectively (X. Wang, Dennis, and Tu 2007). First, we conceptualize cash solvency as a measure of liquidity and effective cash management. To operationalize this, we use a measure of cash held per capita at the end of a given fiscal year. Specifically, the F-33 surveys ask districts to report "the total amount of cash on hand and on deposit and investments in federal government, federal agency, state and local government and nongovernment securities." This is further defined as "cash, deposits, and government and private securities (bonds, notes, stocks, mortgages, etc.), except holdings of agency and private trust funds." They report investments at market value. Cash on hand does not include accounts receivable, value of real property, or non-security assets. We use this as our measure of cash on hand and divide it by the total district student enrollment in a given year to get an indicator of cash held per capita. Higher values correspond with more cash on hand, which may not necessarily translate to improved financial condition. Though more cash on hand increases cash solvency, too much cash on hand could indicate a lack of long-term investments.

To adjust the positive skew of the distribution of cash per capita, we use an inverse-hyperbolic-sine (IHS) transformation of our cash solvency measure. This measure can be interpreted similarly to a natural log transformed measure (i.e., roughly as percent change). Unlike a natural log transformation, the IHS-transformation does not require us to drop any

district-years in which zero cash was reported. As shown in Table 1, baseline average cash solvency was nearly identical for takeover and non-takeover districts (12.9 to 13.4).

We conceptualize budgetary solvency as the capacity for a district to generate sufficient revenues to run its operations. To measure this, we use a standard operating ratio of revenues to expenses in real 2019 dollars, as recommended by Wang and colleagues (2007). Higher values on this measure generally correspond with improved fiscal condition. As shown in Table 1, on average, in 1990 both takeover and non-takeover districts generated roughly $1.10 in revenues for every dollar spent.

Finally, we conceptualize long-run solvency as a government's ability to pay for existing liabilities (X. Wang, Dennis, and Tu 2007). This construct is particularly difficult to measure as school districts are government agencies for which revenues come primarily from taxes across multiple sources. We rely on the comprehensive debt service coverage ratio to measure the ability of a given school district to meet required debt service (Finkler, Calabrese, and Smith 2022). Specifically, we measure the DSCR as net operating income (total revenue – current expenditures) divided by the total debt obligations at the end of the year, including both short-term and long-term debt. Higher values on this measure generally correspond with improved fiscal condition. Again, we use an IHS transformation to adjust the positive skew of the distribution.

In addition to these financial condition ratios, we extend our analysis to understand how education funds are spent in takeover districts and the source of the funds (i.e., state or local). We examine major categories of educational spending including per-pupil expenditures on salaries and benefits as well as debt. To examine how much districts spent on paying off their long-term debt, we use a measure of debt service reported on F-33's as the amount of long-term

debt retired (IHS-transformed). Notably, debt service (loan repayments and interest) is considered as an expenditure in school district budgets. We also examine pupil-teacher ratios as a proxy for spending on class-size reduction efforts. As shown in Table 1, all measures were very similar in takeover and non-takeover districts at baseline.

To understand where funds came from, we examine revenue levels with measures of revenues from both state and local sources per pupil in real 2019 dollars. Notably, revenues could come from state or local taxes or debt taken out by a state or school district. As shown in Table 1, takeover districts had lower average local revenues and higher state revenues than non-takeover districts in 1990 (a pre-takeover year for all districts). This is consistent with Schueler and Bleiberg's (2021) finding that greater state contributions to a district's spending are predictive of takeover.

Analytic Approach

We estimate how state takeover affects school finances with an event study approach. Specifically, we use variation in state takeovers from two sources: (1) within-district differences in takeover status over time and (2) cross-district differences in whether and when districts were taken over. We specify the model as follows:

$$Y_{dt} = \sum_{r=-5}^{10} \beta_r \mathrm{I}(t - t_d^* = r) + X_{dt} + \pi_d + \delta_t + \varepsilon_{dt}$$

where, Y is a fiscal outcome for district d in year t, and t_d^* is the year of the first takeover within a given district. In this specification, β_r represents the effect of takeover r years after it began (or before if r is less than zero) relative to the year before enactment, r=-1, which is excluded (r=0 in the first year of takeover). We also include district (π_d) and year (δ_t) fixed effects that control for characteristics of districts that did not change during the period of study and any year-specific

events.

The term, X_{dt}, is a vector of control variables. In the main model specifications, this is simply a measure of a given fiscal outcome measured at baseline and interacted with a linear time trend. In Appendix Tables B2 and B3, we show that our models are robust to the exclusion of this time trend. We also show that the effect of takeover is robust to controlling for time varying demographic characteristics (the percent of Black students, White students, Hispanic students, students eligible for free lunch, and students with special needs). Additionally, we demonstrate the robustness of our results to controlling for contemporaneous centralizing events (school finance reforms). An additional concern is that districts might react to the threat of takeover. We find our results are robust to controlling for a measure of the prevalence of takeover within each state, which serves as a proxy for a credible takeover threat. Appendix B describes the full set of robustness checks in more detail, demonstrating strong support for the validity of our approach.

The parameters of interest are β_0 through β_{10}, which estimate the effects of takeover in each of the ten years after enactment. We trim years for treated districts that are more than five years before treatment and 10 years after treatment to fully saturate the model with event times. We show the full set of β estimates in figures, and we present estimated effects at prototypical years in Appendix Tables B2 and B3.

Our event study estimates build upon the traditional "two-way fixed effects" (TWFE) estimate by explicitly modeling dynamism in treatment effects over a long period of time (Goodman-Bacon, 2021). However, it is possible that some dynamic treatment effects may lead to biased estimates due to negative weights assigned to some of the average treatment effects on the treated (ATT) for each group and period (de Chaisemartin & D'Haultfoeuille, 2020). In

Appendix Figure B3, we present diagnostic results demonstrating that our findings are not affected by bias from such negative weights.

The key identifying assumption of our approach is that the trends in fiscal outcomes prior to state takeover were the same for districts that were taken over as they were for comparison districts that were not taken over in that period. β_{-5} through β_{-1} tests this assumption directly by estimating whether there were any differences in baseline measures of the outcomes between the treated and un-treated districts in any of the pre-treatment years. Visual inspection of the collection of these estimates allows us to look for evidence of whether the trends for the treatment and comparison groups were parallel. Significant trend differences in outcomes between the treatment and comparison groups prior to state takeover suggest an unobserved confounding factor other than takeover could explain results. Nonsignificant pre-treatment estimates from the event study help build a case that the parallel trends assumption is met.

Results

State Takeover Increased Educational Expenditures

State takeover caused a large and durable increase in per-pupil expenditures (PPE), on average. Figure 2 shows that the effect of takeover on PPE increased monotonically. After three years, takeover increased expenditures by about $800 per student, and seven to ten years after takeover PPE was over $2,500 higher than we would expect had the districts not been taken over. The magnitude of this increase is about twice the size of the change in spending caused by school finance reforms in the post-1990 “adequacy” era of reform (Lafortune, Rothstein, and Schanzenbach 2018). The pre-treatment estimates are jointly nonsignificant ($p>.10$) and quite small in magnitude when compared to the size of the effect several years after treatment. The similarity in pre-takeover PPE trends for districts that were taken over and comparison districts

that were not provides strong evidence that no unobserved policy changes account for the post-takeover spending increases that we observe.

Centralization Improved Financial Condition

The spending increases caused by state takeover improved the financial condition of school districts, on average. Figure 3 shows the effect of takeover on three measures of financial condition: cash held per capita (cash solvency), the operating ratio of revenues to expenses (budgetary solvency), and the debt service coverage ratio (long-run solvency). The post-takeover estimates suggest that state takeover increased budgetary and long-run solvency. The pre-takeover estimates are all individually and jointly nonsignificant, supporting our conclusion that unobserved district capacity to improve financial condition cannot explain the effects of state takeover and provides some empirical evidence in favor of the parallel trends assumption.

Figure 3 Panel A shows that takeover did not have a detectable effect on district's liquidity and effective cash management (i.e., cash solvency). That is not necessarily surprising, given that takeover is a policy of last resort to address fiscal distress. Districts under state management might spend all available assets to pay off debts and improve the general financial condition of the district. Relatedly, many state and federal grants require that districts return unspent funding at the end of the fiscal year, creating a disincentive for districts—particularly those that are more dependent on state revenues—to retain cash on hand (Odden and Picus 2007).

In Figure 3, Panel B and C we show the effect of state takeover on budgetary and long-run solvency respectively. First, we find that takeover increased budgetary solvency, on average. Specifically, takeover increased the ratio of revenues to expenditures (operating ratio) by about eight cents per dollar. The positive effect of takeover on budgetary solvency grows over time and

implies greater capacity to generate sufficient revenues to support district operations. Second, Figure 3, Panel C shows that state takeover also improved districts' capacity to pay off debts (i.e., long-run solvency). Takeover increased districts' DSCR by about 15 percent eight to ten years after takeover.

Impacts Varied Based on Accountability and District Characteristics

We examine whether the effects of state takeover on school district spending and financial condition vary depending on the presence of performance accountability mechanisms, fiscal centralization, district size, debt prior to takeover, and the racial and ethnic composition of the district. Overall, we find that state takeover increased education spending to a greater extent when combined with federal performance accountability. We also find that the effects of takeover on spending and financial condition tended to be positively correlated with district size, and, as hypothesized, we find that this relationship is driven by the fact that larger districts tended to have greater amounts of debt per capita. Finally, the benefits of takeover on fiscal outcomes are smaller in districts serving larger shares of Black students, while takeover effects do not vary depending on the size of a district's Hispanic population.

In Table 2, Panel A, we show results from difference-in-differences models for which we pool all post-takeover years. Results are consistent with the event study estimates previously presented, though the estimated effect on DSCRs is somewhat higher. In this pooled model with more precision, we estimate that takeover increased long-run solvency by about 35%.

No Child Left Behind (Performance Accountability)

Next, in Panel B, we test whether the effect of takeover varies when combined with performance accountability. More specifically, we estimate the effect of takeover before and after 2002 when federal test-based performance accountability was implemented nationwide

after the passage of No Child Left Behind (NCLB). These two separate terms allow us to test whether the effect of takeover was different in years after the passage of NCLB relative to prior to its passage. NCLB provided every state the authority to take over individual schools or entire districts for weak academic performance. States built new departments explicitly to monitor achievement and were required to assess students annually. NCLB increased the organizational focus on test score measures of student academic achievement outcomes and required that states increase their capacity for administering performance accountability.

We find that takeover increased spending by $828 more in the post-NCLB period than it did before its implementation. We also find that takeover increased cash on hand per capita by roughly 69 percent in the period after NCLB, though we do not find that this increase was statistically different from the effect in the period before NCLB (as illustrated by the non-significant F-test of equivalence in Table 2). Additionally, we find that the effects of state takeover on operating ratios and DSCRs were comparable before and after NCLB.

School Finance Reforms (Fiscal Centralization)

We test whether takeover impacts vary depending on whether a district was in a state where school finance reforms (SFR) had been implemented and provide results in Panel C of Table 2. SFRs were designed to use pooled, centralized state resources to redistribute education funds more equitably across districts within states than they had been under conventional funding systems that were largely determined by local property wealth and taxation effort. We consider SFRs to be a form of fiscal centralization. We use a similar model specification as the NCLB analysis above with data from Brunner and colleagues (2020) to identify which states underwent SFRs and when.

We find suggestive evidence that takeover led to somewhat larger increases in school spending in districts under SFR ($826) relative to those that were not under SFR ($613), though the difference between these two estimates is not statistically significant (p>.10). We also find that takeovers that occur alongside SFR led to more cash on hand at the end of the year, a result that is consistent with the mechanisms of SFR (providing greater resources to less-advantaged districts). Finally, we find that the effects of state takeover on operating ratios were comparable in districts for which takeover occurred alongside SFRs versus those that did not. We also find suggestive evidence of variation in the effect of takeover on long-run solvency, but the variation works in the opposite direction as cash solvency (i.e., takeover has a larger effect on long-run solvency in districts not under SFRs). As a result of these mixed findings, we are reluctant to draw strong conclusions about how SFR moderates the impact of takeover on fiscal health.

District and Baseline Debt Size

The takeover of large school districts that serve many thousands of students may differ substantially from smaller districts that serve a few hundred students. State governments tend to have relatively low levels of administrative and technical capacity (Moffitt et al. 2021) and may struggle with overseeing the finances of a very large school district. On the other hand, large districts are much more visible and may find it easier to mount political opposition to state takeovers. The superintendents of large school districts, for example, are often better known than the leaders of state departments of education. This visibility could theoretically increase the incentives for a given state to demonstrate improvements. Relatedly, large districts likely have a greater borrowing capacity based on their magnitude and importance, meaning that fiscal distress may increase exponentially in relation to district size, requiring a more substantial intervention in districts with both larger student enrollments and larger baseline debt.

We display heterogeneity in the effects of takeover based on district size and baseline debt per pupil in Panels D and E of Table 2. We find that spending, budgetary solvency, and long-run solvency increased more in larger districts and districts with greater levels of baseline debt per pupil than smaller districts and districts with less debt (all but one of these differences is statistically significant). We read this as suggestive evidence that takeover had a more potent effect in larger districts. However, we urge caution in the interpretation of these results given that district size and baseline debt are potentially endogenous because states may be less willing to take control of larger districts and those with more debt in the first place.

District Racial and Ethnic Composition

We find communities that were home to a greater share of Black residents were less likely to experience the benefits of takeover on education spending and improved fiscal health. In Panels F and G of Table 2, we show how the effect of takeover varies depending on the percent of Hispanic and Black students, respectively. We find that the predicted takeover-induced increases in per-pupil expenditures and long-run solvency decline as the proportion of Black students increases. Specifically, we find that the positive effect of takeover on per-pupil expenditures in districts that are predominantly Black (75 percent) is about $1,700 lower than a district that is predominantly non-Black (25 percent), and the effect on DSCRs is roughly 40 percent lower. Figure 5 provides visual intuition for these interactions, demonstrating that, although the effects of takeover are very large in districts with zero percent Black students, takeover has no significant effect on DSCRs and per-pupil expenditures in districts with at least 40 and 50 percent Black students, respectively. In contrast, we find no evidence of significant interactions between state takeover and the proportion of Black students for other measures of

fiscal health. We also find that the effect of state takeover on financial condition does not vary depending on the proportion of Hispanic students in the district.

Extensions

Additional Expenditures Funded Legacy Costs

The additional expenditures caused by takeover were primarily spent on legacy costs such as staff benefits and debt retirement. Figure 4 displays the effect of state takeover on pupil-to-teacher ratios and categories of school spending (i.e., salary, benefits, debt). We find no effect of takeover on pupil-teacher ratios suggesting that takeover did not cause districts to reduce class sizes. Expenditures on teacher salaries increased after takeover, but that trend appears to have begun prior to state intervention. This implies districts may have been trying to retain teachers, perhaps to avoid state takeover related to academic performance issues.

In contrast, we find that takeover led to increases in district spending on benefits, steadily rising to about $1,500 per pupil higher 10 years after takeover. Estimates suggest that about three quarters of the increase in school spending went to benefits. We also find evidence suggesting that takeover led districts to spend more on retiring long-term debt. Prior to takeover, we observe no differences in benefits spending or debt retired between districts that were taken over and those that were not. Then, our estimates suggest that takeover caused districts to spend about 200 percent more on retiring long-term debt after takeover, though these estimates are somewhat imprecise.

Takeover Increased State Contributions

Our results suggest that takeover involved a fiscal bailout of school districts on the part of states. In Figure 5 we show the effect of takeover on state and local revenues. We find convincing evidence that takeover increased average state revenues spent on education. In the

years leading up to state intervention, districts that would be taken over and comparison districts received about the same amount of state revenues per pupil. State revenues began to increase immediately after takeover and were about $1,400 higher five years after takeover. In contrast, local revenues increased somewhat three years prior to takeover, though the estimated effects in the five years before takeover are jointly nonsignificant. Local revenues also increased after takeover; five years after takeover, local revenues had increased by about $1,800 per pupil. Overall, the state revenue increases caused by takeover are consistent with a fiscal bailout.

Conclusion

A major challenge for states is determining how to support lower levels of government with fiscal challenges without incentivizing future financial mismanagement. Though classical liberal economics tradition argues that decentralization encourages fiscal responsibility, more recent work on fiscal federalism suggests that decentralization could instead encourage fiscal irresponsibility. Such a situation leaves states with a major predicament: what are they to do when lower levels of government run into major financial or performance challenges?

Our study suggests that one form of political centralization in the context of public education governance—state takeovers of local school districts—can improve the fiscal condition of the targeted districts. This seems to operate through a channel in which centralization facilitates greater financial well-being at the local level through greater state spending on the relatively disadvantaged districts that are targeted for reform. We find that takeovers increased school spending by roughly $2,000 per pupil after five years and led to improvements in cash and budgetary solvency. Takeover increased education spending to a greater extent when combined with federal accountability mechanisms, when implemented in larger districts and those with larger initial amounts of outstanding debt, and when occurring in

districts with fewer Black students. Takeover also appeared to improve fiscal condition more when undertaken in larger districts, those with larger amounts of initial debt, and those with a lower portion of Black students. Our examination of mechanisms suggests that increased funding came primarily from state sources and was used for employee benefits and debt retirement.

These findings contribute to a growing literature on centralization and decentralization in the context of public education (Burns 2010; Henig et al. 2001; Manna 2013; McDermott 1999; Morel 2018; Morel and Nuamah 2020; Peterson 1981). Much of the previous theoretical and empirical work has focused on centralization and redistribution, rather than fiscal performance or condition. This research has been helpful for understanding the consequences of local control and local funding of U.S. schools, generally showing that decentralization tends to make redistribution more difficult in theory, but not always in practice. Our results are consistent with this line of work, suggesting that shifts in authority from the local to the state level can increase the resources flowing to the locality; we also expand upon this work to show that they also improve measures of fiscal well-being.

That said, though our findings suggest improvements to fiscal condition, they do not necessarily imply improvements in efficiency or performance. Instead, it is possible that states are simply leveraging their more robust set of resources to fund the legacy costs that the localities alone struggled to fund, perhaps in response to pressure from powerful creditors or because states themselves have no bailout option for public education. Increases in revenues from the states are also persistent even after 10 years suggesting that these increases in funding become institutionalized.

Our research may help to explain why states have increasingly enacted takeovers over the past three decades—despite the mixed evidence of effectiveness for improving student

outcomes—but does not necessarily suggest that states should rush to take over all districts with fiscal challenges. Takeovers likely persist, at least in part, because they provide states a mechanism to improve fiscal condition of local districts and to address the demands of creditors. However, we see several reasons why states may seek to keep takeovers as a measure of last resort.

First, previous research shows that takeovers, on average, do not improve the school district performance as measured by the academic achievement of students in the targeted districts (Schueler and Bleiberg 2021). This paper provides some insight into how this could be true, despite the average spending increases that accompany takeovers and the substantial literature on the positive effect of increased spending on student achievement (Jackson, Johnson, and Persico 2015). With takeover, we observe that new funds go toward legacy costs rather than expenditures that might be more likely to result in student academic improvements (e.g., class size reduction). Previous work also suggests that takeovers do not dramatically increase within-district funding equity (Bleiberg, Lyon, and Schueler 2022).

Furthermore, takeovers tend to generate political opposition within the districts that are targeted for reform largely due to concerns over the resulting loss of local economic and political power (Schueler and West 2021; Morel and Nuamah 2020; Marsh et al. 2021; Welsh et al. 2019). Indeed, when enacted in majority-Black communities, takeovers decrease the descriptive representation of local elected officials and may therefore exacerbate race-based political inequality (Morel 2018). However, the reverse is true when takeover occurs in majority-Hispanic communities (where takeover seems to pave the way for greater Latino descriptive representation), though the mechanisms underpinning these racial and ethnic differences are not well understood. Our finding that the fiscal benefits of takeover are smallest in districts serving

the greatest share of Black residents provides further evidence that takeover may be particularly risky when enacted in majority-Black communities. Likewise, we also find that takeover-induced increases to spending and improvements to fiscal health are similar in districts serving large and small shares of Hispanic students.

In sum, the improvements in fiscal condition that accompany takeover come at a significant cost to both states and localities, in different ways. For one, states are providing a large share of the funding increases. Thus, the fiscal costs are somewhat diffuse; the costs of the increases in state revenues are distributed across the state (rather than in a smaller locality). Second, however, takeover comes with political costs for state leaders who are called to address local opposition to takeover and for local communities through the loss of the elected school board's decision-making power. Therefore, this form of political centralization increases expenditures and improves fiscal condition, on average, but does so at a non-trivial cost to both the centralizing bodies of government and, in many cases, to the local communities targeted for intervention.

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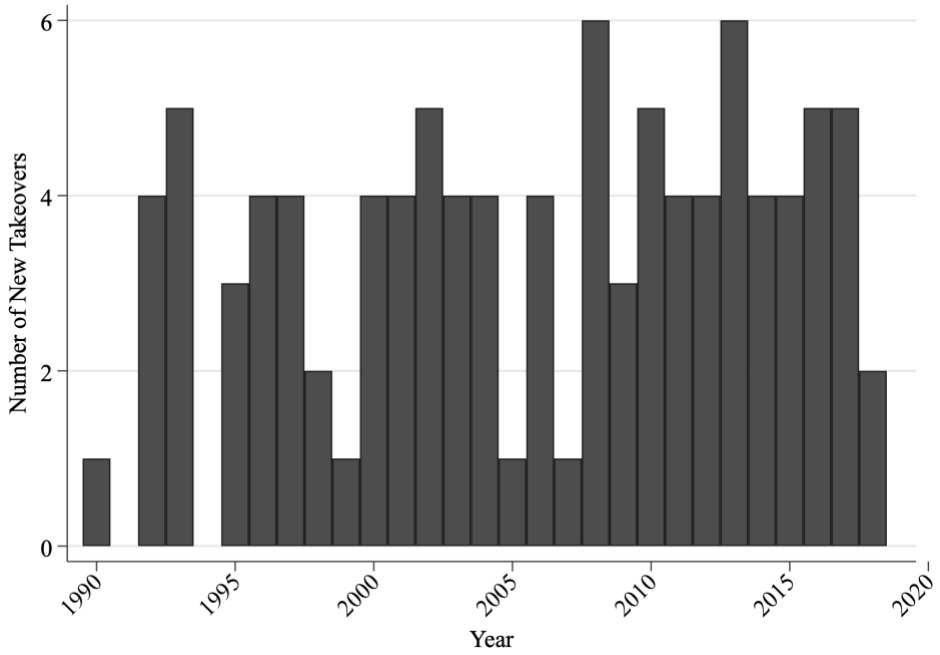


Figure 1. Number of New Takeovers over Time

Note: During the period of study we observe state takeovers from 1990 to 2018.

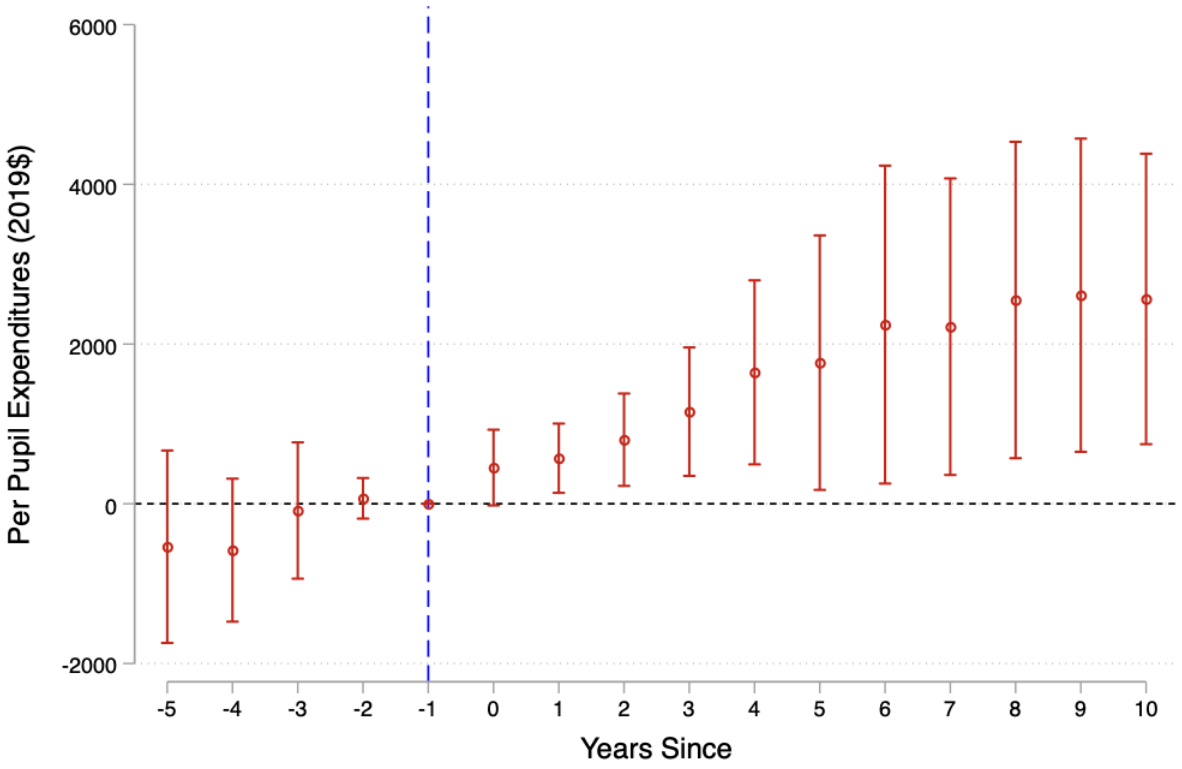


Figure 2. The Effect of State Takeover on Per-Pupil Expenditures, 2019$

Note: Error bars represent 95% confidence intervals. Robust standard errors clustered at the district level.

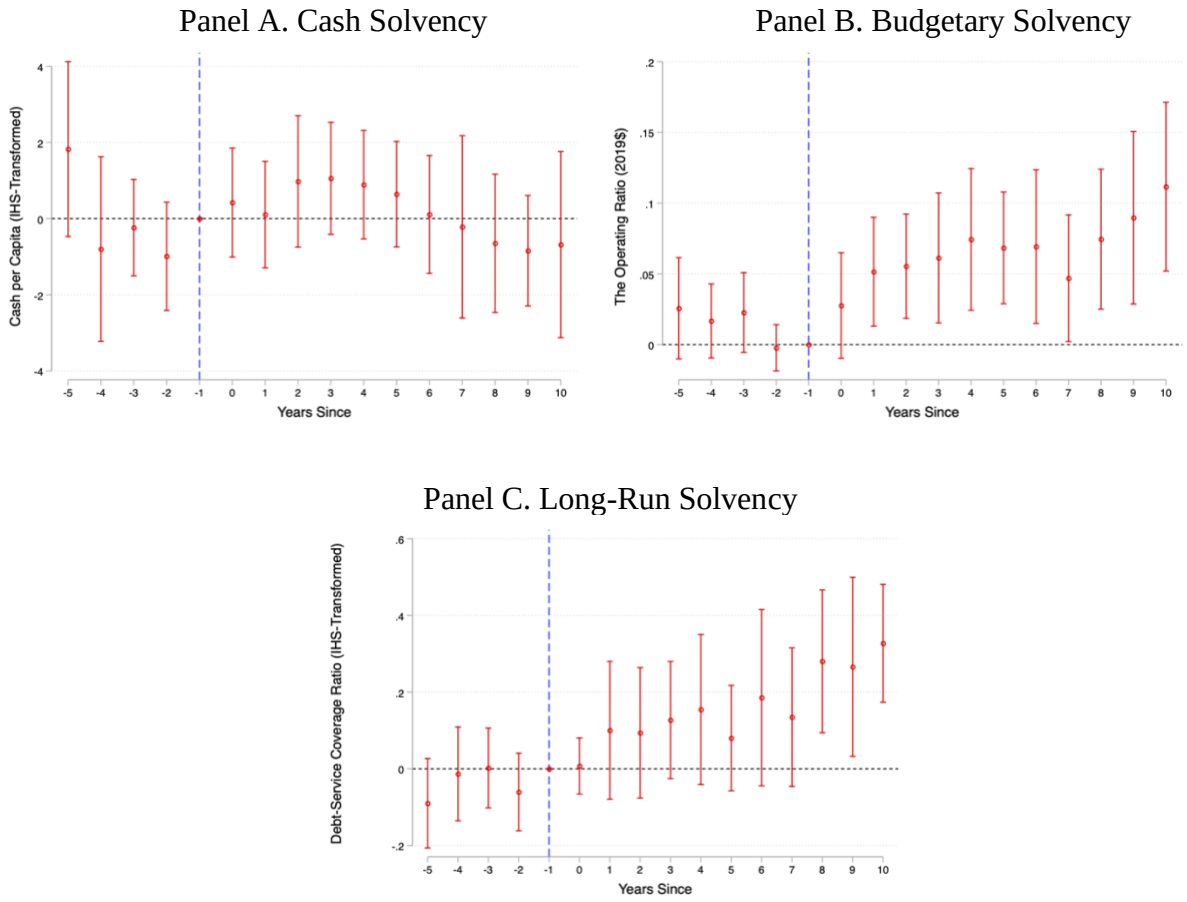


Figure 3. The Effect of State Takeover on Financial Condition

Note: Error bars represent 95% confidence intervals. Robust standard errors clustered at the district level.

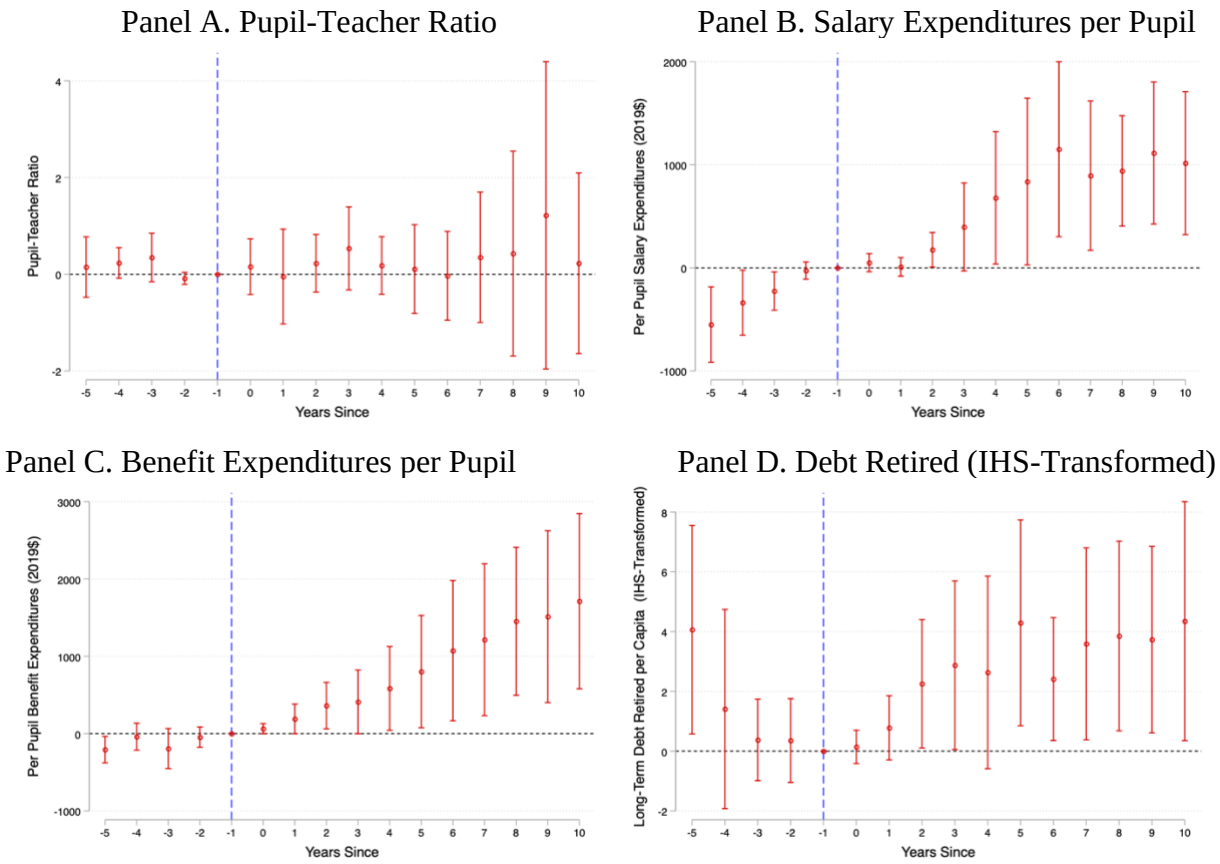
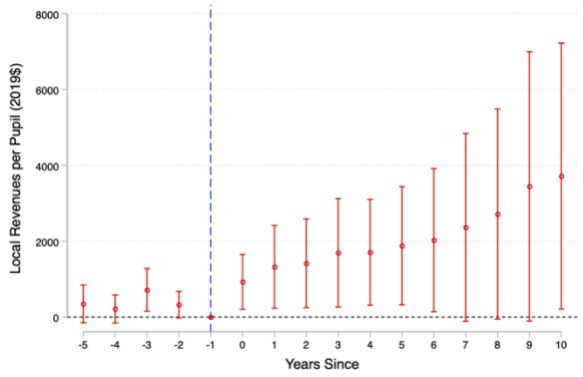


Figure 4. The Effect of State Takeover on Forms of Educational Spending

Note: Error bars represent 95% confidence intervals. Robust standard errors clustered at the district level.

Panel A. Local Revenues per Pupil, 2019$



Panel B. State Revenues per Pupil, 2019$

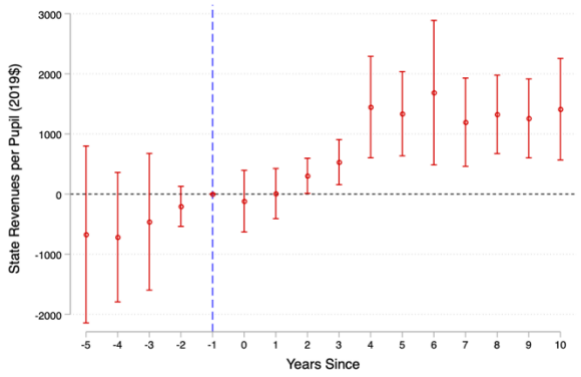
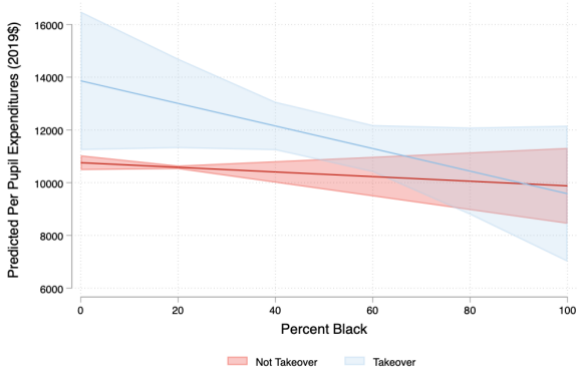


Figure 5. The Effect of State Takeover on Revenues

Note: Error bars represent 95% confidence intervals. Robust standard errors clustered at the district level.

Panel A. Per-Pupil Expenditures, 2019$



Panel B. Long-Run Solvency

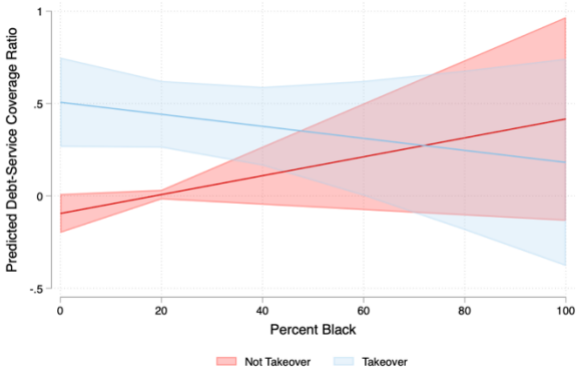


Figure 5. Heterogeneity in the Effect of State Takeover by the Proportion of Black Students

Note: Error bars represent 95% confidence intervals. Robust standard errors clustered at the district level.

Table 1. Baseline Characteristics of Takeover and Never Takeover Districts

	Takeover		Never Takeover		
	Mean	SD	Mean	SD	Baseline Year
Panel A. Student Characteristics					
Enrollment	27,992.2	101,226.6	2,960.8	9,570.7	1990
Percent White	37.9%	34.2%	83.2%	22.6%	1990
Percent Hispanic	11.2%	21.5%	7.0%	15.7%	1990
Percent Black	49.9%	35.2%	6.8%	15.2%	1990
Percent Special Education	11.3%	5.4%	10.4%	4.6%	1990
Percent Free Lunch	10.8%	20.9%	7.7%	13.7%	1990
Panel B. Measures of School Spending and Fiscal Condition					
Per-Pupil Expenditures, 2019$	8,112.28	2,609.77	8,409.22	2,726.02	1990
Cash Held per Capita, IHS-Transformed	12.89	5.29	13.41	5.11	1998
Operating Ratio, 2019$	1.11	0.12	1.14	0.14	1990
Debt Service Coverage Ratio, IHS-Transformed	0.28	1.24	0.44	1.49	1998
Salary Expenditures per Pupil, 2019$	3,836.28	1,059.17	3,868.71	1,030.59	1998
Benefit Expenditures per Pupil, 2019$	1,030.11	370.61	962.77	372.75	1998
Pupil-Teacher Ratio	17.61	2.95	16.20	4.02	1990
Debt Retired, IHS-Transformed	5.30	1.23	5.62	1.21	1998
Local Revenues, 2019$	2,782.61	1,954.08	4,319.41	2,608.25	1990
State Revenues, 2019$	5,135.10	1,663.60	4,696.91	2,091.94	1990
Observations	104		11,034		

Notes: Data are from F-33 Surveys and summarized at the district level. Cash held per capita is a ratio of total cash on hand at the end of a given year over student enrollment and is used to measure cash solvency. The operating ratio is a measure of revenues to expenditures and is used to operationalize budgetary solvency. The debt service coverage ratio is a ratio of net income over long-term liabilities and is used to operationalize long-run solvency. Inverse Hyperbolic Sine (IHS) Transformations are used in place of natural log transformations to preserve the values of zero.

Table 2. Variation in the Effect of Takeover on School Expenditures and Fiscal Condition

	Per Pupil Expenditures	Cash Held per Capita	Operating Ratio	Debt Service Coverage Ratio
Panel A. Main Model				
Effect of Takeover (all post years)	3478.5*	0.144	0.0810***	0.358***
	(1478.1)	(0.629)	(0.023)	(0.109)
Panel B. Federal Performance Accountability				
Takeover Effect Pre-NCLB	-90.14	0.475	0.079***	0.513
	(360.5)	(0.657)	(0.023)	(0.420)
Takeover Effect Post-NCLB	737.6**	0.687**	0.060***	0.199*
	(272.7)	(0.253)	(0.011)	(0.098)
F-Test of Equivalence	*0.004*	*0.714*	*0.384*	*0.440*
Panel C. School Finance Reforms				
Takeover Effect, Under SFR	844.1*	1.495**	0.051***	0.011
	(364.7)	(0.478)	(0.014)	(0.094)
Takeover Effect, No SFR	628.7+	0.144	0.067***	0.353*
	(362.0)	(0.235)	(0.015)	(0.148)
F-Test of Equivalence	*0.662*	*0.010*	*0.423*	*0.041*
Panel D. District Size				
Takeover Effect, Big Districts	4417.2**	-0.453	0.088**	0.446***
	(1453.6)	(0.979)	(0.031)	(0.087)
Takeover Effect, Small Districts	1014.2+	0.650	0.062***	0.122
	(569.2)	(0.452)	(0.0120)	(0.079)
F-Test of Equivalence	*0.026*	*0.273*	*0.406*	*0.010*
Panel E. Baseline Debt				
Takeover Effect, Large Baseline Debt	4295.2**	-0.193	0.113***	0.426***
	(1323.1)	(0.684)	(0.026)	(0.088)
Takeover Effect, Small Baseline Debt	516.4	1.194	0.037**	0.037
	(600.8)	(1.192)	(0.014)	(0.014)
F-Test of Equivalence	*0.011*	*0.314*	*0.013*	*0.010*
Panel F. Percent Hispanic Students				
Takeover Effect	1332.1+	0.632	0.0705+	0.148+
	(682.9)	(0.685)	(0.0368)	(0.0820)
Takeover Effect*Percent Hispanic	749.9	-1.097	-0.0365	0.272
	(1765.0)	(1.283)	(0.0646)	(0.340)
Panel G. Percent Black Students				
Takeover Effect	2773.1*	-0.612	0.0723+	0.520***
	(1086.6)	(0.710)	(0.0379)	(0.108)
Takeover Effect*Percent Black	-3402.0+	2.486	-0.0323	-0.837***
	(2036.2)	(1.762)	(0.0423)	(0.220)
District Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X
Baseline Outcome Linear Time Trends	X	X	X	X

Notes: Per-pupil expenditures are in real 2019 dollars. Cash held per capita is a ratio of total cash on hand at the end of a given year over student enrollment (IHS-transformed) and is used to measure cash solvency. The operating ratio is a measure of revenues to expenditures in real 2019 dollars and is used to operationalize budgetary solvency. The debt service coverage ratio (DSCR) is a ratio of net income over long-term liabilities (IHS-transformed) and is used to operationalize long-run solvency. Panels F and G include control variables for the percent of Hispanic and Black students in the district, respectively. The sample size varies across models because for some measures the baseline year is 1998 (see Table 1). Robust standard errors clustered at the district level are in parentheses. + p<0.1, * p<0.05, ** p<0.01, *** p<0.001.

Appendix A. Takeover Frequencies and Samples

Table A1 below describes the number of districts that we observe across relative time that we use to identify the effect of takeover. School spending outcomes our available from 1990 and detailed spending information is available since 1998. We observe 98 takeovers in the panel from 1990 to 2019 and 84 takeovers from 1998 to 2019. Five years after treatment we observe outcomes for 73 treated districts and 10 years after treatment, we observe outcomes for 57 treated districts. Our event study has sufficient power to identify the effect of takeover for up to 10 years after state intervention. We find similar results if we analyze only the takeover districts that we observe during the period of study (i.e., we balance the panel on relative time instead of calendar time; see Appendix Tables B2 and B3 below).

Table A1. Number of Takeover Districts for Each Year Relative to Takeover

Year Relative to Takeover	Treated Districts	
	1990+ Sample	1998+ Sample
-5	89	69
-4	84	70
-3	91	76
-2	93	81
-1	99	84
0	98	82
1	93	82
2	92	84
3	88	78
4	86	76
5	80	73
6	74	73
7	68	68
8	65	65
9	61	60
10	58	57

Notes: The column "Years Relative to Takeover" displays the number of years before or after the first takeover in each first takeover district. The "Treated Districts" column displays the number of Takeover Districts that contribute to the estimate of the effect of takeover in a given year relative to takeover.

Appendix B. Robustness

The key identifying assumption of our approach is that the trends in expenditures and fiscal health prior to state takeover were the same for districts that were taken over and comparison districts that were not taken over. The pre-takeover estimates in our event study models provide compelling, initial evidence for the interval validity of our empirical specification. As noted throughout the text, we further probe the robustness of our approach with a number of additional tests. In this section, we first display results from a series alternative model specifications that represent potential other, reasonable methods for analyzing the effect of takeover, as described in the main text. In Table B1 below, we first show that our results are robust to alternative definitions of takeover.

In Tables B2 and B3, we also show the results from additional specifications that remove the baseline linear time trends, control for years since exit from takeover and time relative to second takeovers, control for time-varying demographic changes, balance the panel on relative time instead of calendar time, control for concurrent school finance reforms, and control for the prevalence of takeover across the state. In short, the model in column (2) that removes baseline linear time trends presents a model with no control variables at all, which some researchers prefer for event study models. The model in column (3) accounts for the fact that some of the takeover districts are removed from state takeover (i.e., local control is returned), and some even are removed and then return to state takeover. The model in column (4) includes control variables to account for any racial or ethnic changes in the study body population before or during takeover. These include the percent of Black students, White students, Hispanic students, students eligible for free lunch, and students with special needs. The model in column (5) accounts for the changes in the sample of treated states that contribute to estimates of the effect

of takeover, as shown in Table A1 above. Instead of allowing the sample to change, we only use districts for which we have data for the 5 years before and ten years after takeover. This limits our sample of takeover districts but ensures that dynamic effects are not driven by changes to the sample composition (instead of the effect of state takeover). The model in column (6) controls for concurrent shocks by adding a control for statewide school finance reform using the school finance reforms documented by Brunner and colleagues (2019). The model in column (7) accounts for potential district reactions to the threat of takeover. We find our results are robust to controlling for a measure of the prevalence of takeover within each state which serves as a proxy for a credible takeover threat. It is clear from these models that our results are very consistent even when we use these other approaches.

In Figure B1, we also display findings from a series of falsification tests examining the effect of takeover on missingness across each of our outcome variables. For all of these measures, the figure below demonstrates that takeover is not endogenously related to the likelihood of a given district-year observation being missing. In Figure C1 below, we also show and describe the results of our examination of the effect of state takeover on sample inclusion.

Finally, we also present analytic and diagnostic results that demonstrate that our findings are not affected by bias from heterogenous treatment effects with staggered adoption recently highlighted in the methodological literature. Our event study estimate improves upon the traditional "two-way fixed effects" (TWFE) estimate by explicitly modeling dynamism in treatment effects over a long period of time (Goodman-Bacon, 2021). However, it is still possible that some dynamic treatment effects may lead to biased estimates due to negative weights assigned to some of the average treatment effects on the treated (ATT) for each group and period (de Chaisemartin & D'Haultfoeuille, 2020). In Figure B2 below, we present the

weights assigned to each ATT in a simpler, generalized differences-in-difference approach that just uses the pooled estimate of the effect of state takeover to demonstrate that even in this less dynamic setting, none of the weights are negative. Together, these robustness checks provide strong support for the validity of our approach.

Table B1. The Effect of State Takeover on District Finance, Three Conceptualizations of Takeover Timing

	Per-Pupil Expenditures	Cash Held per Capita	The Operating Ratio	Debt Service Coverage Ratio	Pupil-Teacher Ratio	Per Pupil Salary Expenditures	Per Pupil Benefit Expenditures	Long-Term Debt Retired per Capita	Local Revenues per Pupil	State Revenues per Pupil
Effect of Takeover (all post years)	3178.48*	-0.05	0.06**	0.28*	-0.07	957.81	1329.76+	0.93***	2733.92*	1525.77*
	(1620.09)	(0.53)	(0.02)	(0.14)	(0.31)	(592.91)	(696.43)	(0.26)	(1380.35)	(706.60)
Effect of Takeover (on/off)	3478.52*	0.14	0.08***	0.36***	0.06	1119.06*	1482.76*	0.94***	2887.44*	1898.40***
	(1478.13)	(0.63)	(0.02)	(0.11)	(0.41)	(518.71)	(631.49)	(0.27)	(1304.55)	(542.74)
Effect of Takeover (censored)	3678.95*	-0.13	0.08***	0.36**	-0.02	1145.30*	1548.12*	1.01***	3103.43*	1948.33***
	(1537.88)	(0.58)	(0.02)	(0.12)	(0.27)	(536.64)	(640.64)	(0.25)	(1335.71)	(565.76)
Observations (uncensored)	295215	229360	295215	198404	281939	228522	228522	239680	295406	295406
R-squared	0.896	0.792	0.393	0.440	0.173	0.919	0.839	0.527	0.883	0.809
Adjusted R-squared	0.891	0.779	0.363	0.404	0.131	0.914	0.830	0.500	0.877	0.800

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors, clustered at the district level in parentheses. Each cell shows the results of a separate regression. All models have district and year fixed effects. Models include no control variables. The first row of results (all post years) displays the overall effect of takeover from the main model using a generalized differences-in-differences framework. The second row (on/off) displays the effect of takeover when the takeover term is only coded 1 during the time of takeover and is coded 0 otherwise. The third row of results (censored) displays results from a specification that drops takeover districts after they exit from the first takeover. The number of observations does not account for this censoring. Per pupil expenditures are in real 2019 dollars. Cash held per capita is a ratio of total cash for debt service, bonds, and other cash over student enrollment (IHS-transformed) and is used to measure cash solvency. The operating ratio is a measure of revenues to expenditures in real 2019 dollars and is used to operationalize budgetary solvency. The debt service coverage ratio (DSCR) is a ratio of net income over long-term liabilities (IHS-transformed) and is used to operationalize long-run solvency.

Table B2. Robustness for Spending and Fiscal Health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Per Pupil Expenditures, 2019$						
5 Years Pre	-538.323	-713.579	-490.416	-695.761	-596.328	-619.554	-550.471
	(614.395)	(711.471)	(611.834)	(560.657)	(692.078)	(586.281)	(612.275)
Year of Takeover	452.738+	519.544*	479.205+	330.607*	563.467+	444.436*	445.741+
	(241.738)	(260.059)	(248.354)	(131.191)	(294.401)	(206.198)	(238.111)
5 Years Post	1761.504*	2101.781*	1870.202*	-228.316	2006.297*	1332.769*	1726.971*
	(809.765)	(930.688)	(818.027)	(385.358)	(787.121)	(629.428)	(837.211)
10 Years Post	2563.839**	3186.769**	3083.020**	314.596	2708.448**	2182.951**	2509.517**
	(928.091)	(1131.568)	(1081.743)	(564.872)	(911.202)	(749.932)	(956.782)
	Cash Held per Capita, IHS-Transformed						
5 Years Pre	1.826	0.627	0.951	-0.789	2.737***	2.035+	1.823
	(1.171)	(0.986)	(1.142)	(1.153)	(0.546)	(1.204)	(1.171)
Year of Takeover	0.423	0.297	0.325	0.378	-0.073	0.434	0.423
	(0.730)	(0.769)	(0.789)	(0.802)	(0.392)	(0.722)	(0.731)
5 Years Post	0.644	0.555	0.206	0.606	0.996**	0.664	0.650
	(0.707)	(0.747)	(0.776)	(0.778)	(0.325)	(0.701)	(0.708)
10 Years Post	-0.682	-0.018	-2.255+	0.148	-0.698	-0.736	-0.679
	(1.246)	(1.330)	(1.300)	(2.100)	(0.459)	(1.237)	(1.248)
	The Operating Ratio, 2019$						
5 Years Pre	0.026	0.022	0.027	0.028+	0.027	0.025	0.026
	(0.018)	(0.020)	(0.018)	(0.017)	(0.021)	(0.018)	(0.018)
Year of Takeover	0.028	0.028	0.028	0.024	0.033	0.028	0.028
	(0.019)	(0.019)	(0.020)	(0.019)	(0.023)	(0.019)	(0.019)
5 Years Post	0.068***	0.075***	0.072***	0.064+	0.072**	0.065**	0.068***
	(0.020)	(0.020)	(0.020)	(0.035)	(0.022)	(0.021)	(0.020)
10 Years Post	0.112***	0.122***	0.127***	0.090+	0.111***	0.108***	0.111***
	(0.030)	(0.032)	(0.034)	(0.051)	(0.032)	(0.031)	(0.031)
	Debt Service Coverage Ratio, IHS-Transformed						
5 Years Pre	-0.090	-0.163**	-0.086	-0.071	-0.019	-0.085	-0.089
	(0.059)	(0.059)	(0.059)	(0.052)	(0.042)	(0.058)	(0.060)
Year of Takeover	0.008	0.026	0.007	0.002	0.043*	0.008	0.007
	(0.037)	(0.040)	(0.037)	(0.044)	(0.020)	(0.037)	(0.037)
5 Years Post	0.080	0.175+	0.086	0.008	0.168***	0.043	0.082
	(0.070)	(0.090)	(0.069)	(0.069)	(0.029)	(0.067)	(0.070)
10 Years Post	0.327***	0.497***	0.356***	0.306*	0.381***	0.291***	0.330***
	(0.078)	(0.109)	(0.074)	(0.154)	(0.040)	(0.076)	(0.079)
Baseline Outcome Linear Time Trends	X		X	X	X	X	X

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors, clustered at the district level in parentheses. Each column shows the results of a separate regression. All models have district and year fixed effects. Column (1) shows the main model discussed in the text. Column (2) removes the baseline linear time trends from this model. Column (3) adds controls for years since exit, years since 2nd takeover, years since 2nd takeover exit to the main model. Column (4) adds controls for time-varying student racial and ethnic demographic changes. Column (5) balances the district-year panel on relative time instead of on calendar time. Column (6) adds control variables for SFR using the Brunner et al. (2021) timing. Column (7) adds a time-varying control variable accounting for the portion of districts in a given state under takeover.

Table B3. Robustness for How Money Was Spent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Pupil-Teacher Ratio			
5 Years Pre	0.114	0.051	0.130	0.046	0.166	0.196	0.168
	(0.392)	(0.416)	(0.373)	(0.419)	(0.430)	(0.300)	(0.318)
Year of Takeover	-0.007	-0.077	0.016	-0.019	-0.017	0.153	0.196
	(0.328)	(0.408)	(0.282)	(0.397)	(0.368)	(0.278)	(0.283)
5 Years Post	-0.011	0.109	0.054	0.278	-0.253	0.227	0.251
	(0.461)	(0.480)	(0.550)	(0.575)	(0.530)	(0.414)	(0.437)
10 Years Post	-0.616	-0.702	-0.359	0.026	-0.692	0.307	0.425
	(0.665)	(0.702)	(1.105)	(0.745)	(0.685)	(0.951)	(0.951)
				Per-Pupil Salary Expenditures, 2019$			
5 Years Pre	-550.158**	-724.713**	-525.350**	-381.976+	-569.426***	-518.612*	-550.336**
	(186.325)	(228.755)	(185.144)	(224.299)	(130.986)	(203.192)	(186.402)
Year of Takeover	50.384	78.495	49.865	29.408	85.271	63.437	50.376
	(44.680)	(51.733)	(44.770)	(42.440)	(54.220)	(47.691)	(44.803)
5 Years Post	837.925*	1030.074*	882.739*	-289.820	1261.050***	462.948+	834.638*
	(412.887)	(482.311)	(403.980)	(299.646)	(233.174)	(278.782)	(412.645)
10 Years Post	1016.006**	1393.223**	1222.515***	26.910	1343.026***	682.041**	1012.521**
	(353.588)	(458.592)	(303.115)	(306.701)	(248.500)	(229.437)	(352.476)
				Per-Pupil Benefit Expenditures, 2019$			
5 Years Pre	-207.109*	-351.590**	-160.551+	17.280	-38.382	-176.796*	-209.914*
	(87.034)	(109.012)	(85.297)	(75.886)	(54.776)	(83.916)	(87.428)
Year of Takeover	63.447+	99.014**	62.444+	123.585*	119.308***	71.332*	64.372+
	(33.461)	(36.389)	(33.948)	(59.331)	(23.797)	(35.704)	(35.280)
5 Years Post	794.804*	1022.264*	877.792*	-170.264**	1249.457***	544.351*	775.739*
	(367.451)	(401.499)	(347.245)	(63.133)	(164.675)	(272.047)	(371.492)
10 Years Post	1713.122**	2155.412***	2095.990***	122.794	2359.115***	1475.075**	1677.728**
	(578.032)	(633.396)	(480.053)	(190.178)	(277.161)	(481.042)	(563.956)
				Long-Term Debt Retired, IHS-Transformed			
5 Years Pre	2.163*	2.286**	2.209**	2.392**	3.090***	2.171*	2.163*
	(0.850)	(0.887)	(0.843)	(0.793)	(0.522)	(0.848)	(0.849)
Year of Takeover	0.057	0.032	0.055	0.015	0.183	0.058	0.057
	(0.146)	(0.135)	(0.147)	(0.138)	(0.137)	(0.146)	(0.146)
5 Years Post	2.313**	2.146**	2.391**	1.013*	3.308***	2.261**	2.310**
	(0.824)	(0.753)	(0.798)	(0.485)	(0.455)	(0.791)	(0.824)
10 Years Post	2.114*	1.843*	2.488**	0.549	3.241***	2.062*	2.107*
	(0.887)	(0.756)	(0.774)	(0.449)	(0.402)	(0.857)	(0.885)
Baseline Outcome Linear Time Trends	X		X	X	X	X	X

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors, clustered at the district level in parentheses. Each column shows the results of a separate regression. All models have district and year fixed effects. Column (1) shows the main model discussed in the text. Column (2) removes the baseline linear time trends from this model. Column (3) adds controls for years since exit, years since 2nd takeover, years since 2nd takeover exit to the main model. Column (4) adds controls for time-varying student racial and ethnic demographic changes. Column (5) balances the district-year panel on relative time instead of on calendar time. Column (6) adds control variables for SFR using the Brunner et al. (2021) timing. Column (7) adds a time-varying control variable accounting for the portion of districts in a given state under takeover.

Table B4. Robustness for Revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Local Revenues per Pupil, 2019$			
5 Years Pre	347.288	328.355	420.580	250.247+	442.167	309.198	342.680
	(255.335)	(255.831)	(259.293)	(148.336)	(302.090)	(231.913)	(254.364)
Year of Takeover	928.537*	904.500*	967.482*	579.395**	1031.265*	920.932**	924.885*
	(368.979)	(367.302)	(382.486)	(204.848)	(429.232)	(347.712)	(368.539)
5 Years Post	1880.367*	1870.812*	2040.334*	432.909*	2005.367*	1636.884*	1861.695*
	(794.737)	(796.739)	(795.705)	(181.915)	(831.015)	(680.671)	(809.097)
10 Years Post	3717.169*	3687.053*	4489.977*	522.841+	3944.678*	3491.588*	3690.431*
	(1789.262)	(1794.817)	(1949.250)	(308.317)	(1833.528)	(1675.403)	(1808.418)
				State Revenues per Pupil, 2019$			
5 Years Pre	-672.354	-669.053	-658.713	-725.933	-777.411	-727.066	-684.869
	(750.750)	(746.406)	(751.877)	(743.846)	(842.046)	(712.534)	(744.626)
Year of Takeover	-118.114	-123.602	-110.949	-134.034	-69.352	-130.174	-133.192
	(261.597)	(258.477)	(264.134)	(231.683)	(327.257)	(248.965)	(248.539)
5 Years Post	1336.234***	1352.400***	1365.364***	134.941	1483.434***	944.013***	1256.065***
	(357.521)	(366.571)	(349.623)	(555.407)	(324.946)	(254.305)	(378.570)
10 Years Post	1412.118**	1423.867***	1553.261**	940.648	1394.998**	1042.940*	1295.201**
	(430.851)	(424.378)	(475.025)	(902.161)	(454.671)	(480.694)	(396.407)
Baseline Outcome	X		X	X	X	X	X
Linear Time Trends							

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors, clustered at the district level in parentheses. Each column shows the results of a separate regression. All models have district and year fixed effects. Column (1) shows the main model discussed in the text. Column (2) removes the baseline linear time trends from this model. Column (3) adds controls for years since exit, years since 2nd takeover, years since 2nd takeover exit to the main model. Column (4) adds controls for time-varying student racial and ethnic demographic changes. Column (5) balances the district-year panel on relative time instead of on calendar time. Column (6) adds control variables for SFR using the Brunner et al. (2021) timing. Column (7) adds a time-varying control variable accounting for the portion of districts in a given state under takeover.

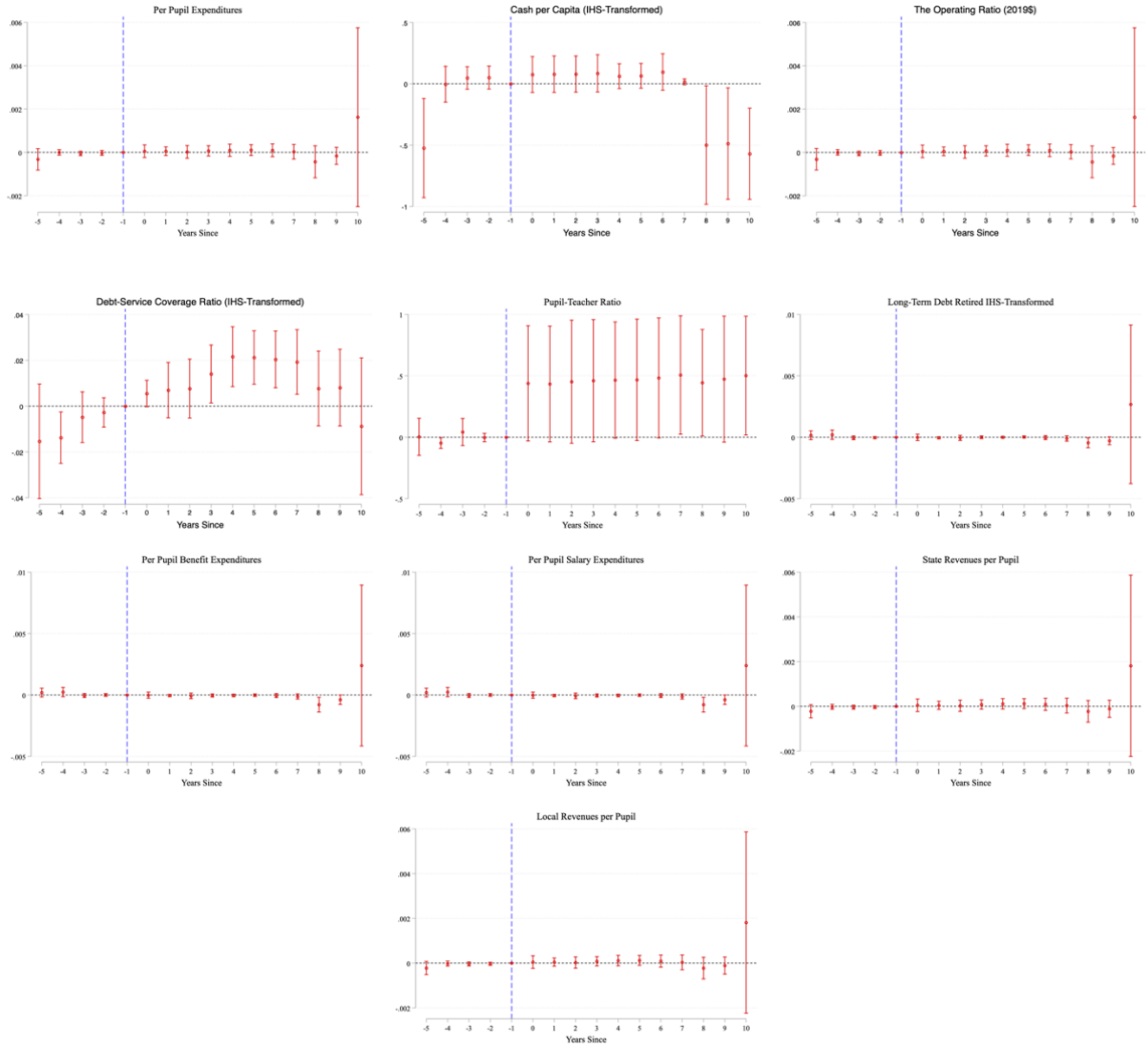


Figure B1. The Effect of State Takeover on Missingness of Outcome Measures

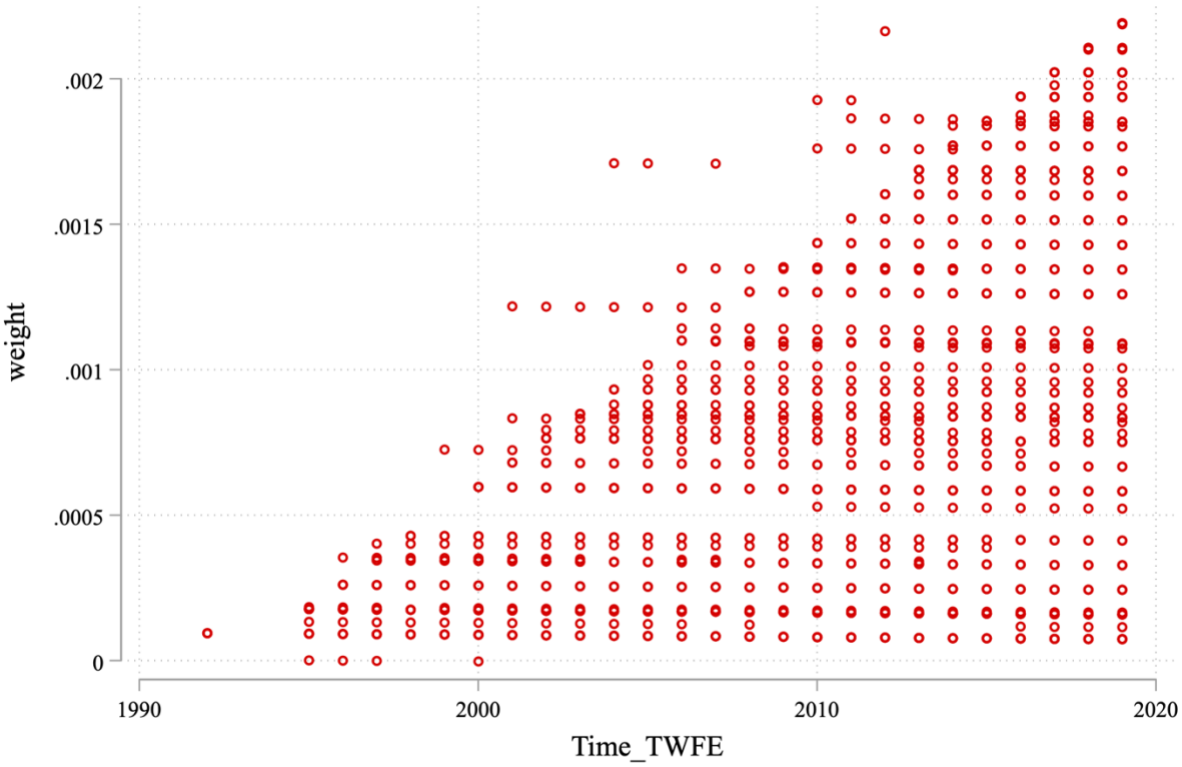


Figure B2. Weights Assigned to Average Treatment Effects on the Treated (ATT) for Each Group and Period

Appendix C. School District Financial Data

School District Financial Data Our primary data source is the Local Education Agency Finance Survey (F-33) maintained by the National Center for Education Statistics (NCES) for the period 1990 through 2018. We augment this data with one later version of the F-33 survey provided by the U.S. census for the year 2019. We limit the sample to traditional school districts. This includes elementary, secondary and unified school systems, and does not include charter schools, college-grade systems, vocational or special education systems, non-operating school systems and educational service agencies.

We also drop a small number of observations associated with the following types of educational agencies: 1) Districts with only kindergarten or pre-kindergarten; 2) Regional education services agencies, or county superintendents serving the same purpose; 3) State-operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; 4) Federally operated institutions charged, at least in part, with providing elementary and/or secondary instruction or services to a special-needs population; and 5) other education agencies that are not a local school district. We also drop Hawaii from the sample because there is a single school district under the control of the state.

We also impose a few more restrictions to reduce the noise in the F-33 finance data. We drop districts with fewer than 50 students, including those with zero students. Second, drop districts in which more than half of the students have special needs. Third, we drop observations if a district reported spending zero dollars per pupil in a given year. Finally, following Brunner et al (2019) and Lafortune et al. (2018), we drop district-year observations for finance outcomes that were at least five times greater or five times smaller than the state-by-year mean of the

variable. Also following Brunner et al (2019) and Lafortune, Rothstein, and Schanzenbach (2018), we exclude any district-year observation with enrollment more than double the district's average enrollment over the entire sample period.

In Appendix Figure C1 below, we show that we find no evidence of endogeneity between these sample restrictions and takeover by examining the "effect" of takeover on sample inclusion. In other words, we use our main model specification without any sample restrictions, replacing the finance outcome with a dummy variable indicating whether or not a given district-year observation was included in the sample.

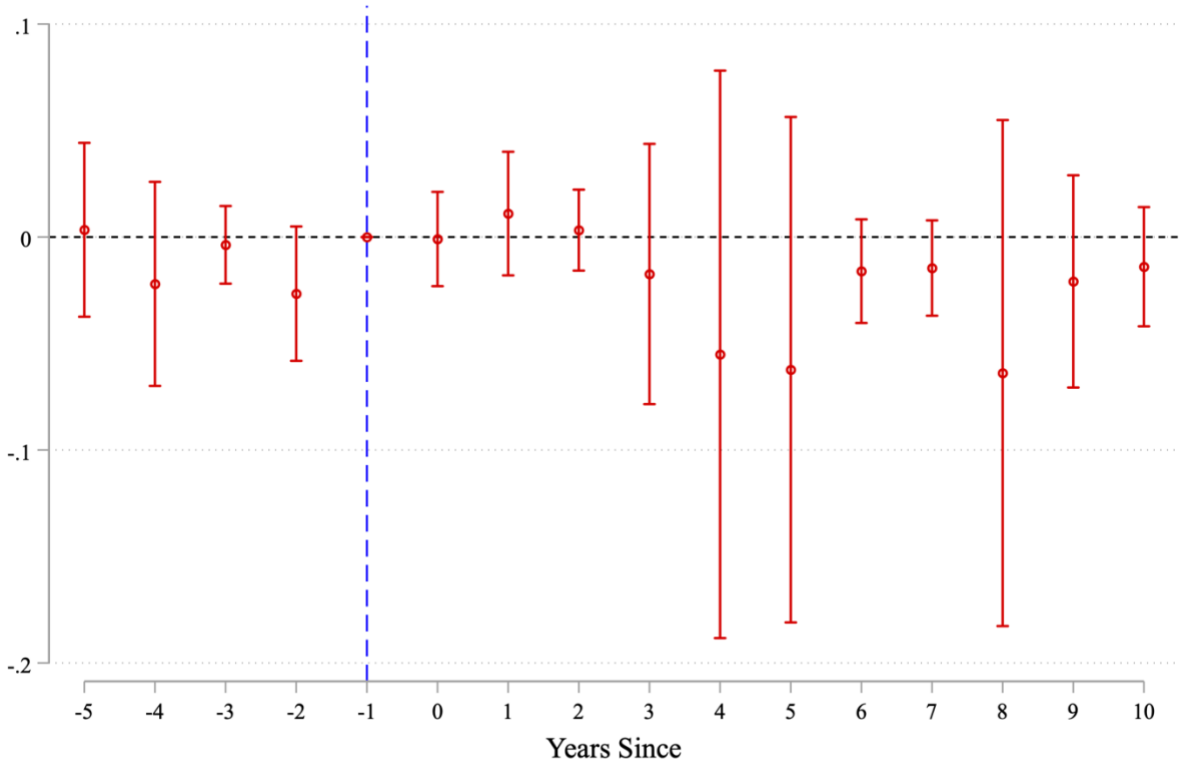


Figure C1. The "Effect" of State Takeover on Sample Inclusion

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