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Evaluating Education Governance: Does State Takeover of School Districts Affect Student Achievement?

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Local school boards have primary authority for running educational systems in the U.S. but little is known empirically about the merits of this arrangement. State takeovers of struggling districts represent a rare alternative form of educational governance and have become an increasingly common response to low performance. However, limited research explores whether this effectively improves student outcomes. We track all takeovers nationwide from the late 1980s, when the first takeovers occurred, through 2016 and describe takeover districts. While these districts are low performing, we find academic performance plays less of a role in predicting takeover for districts serving larger concentrations of African American students. We then use a new data source allowing for cross-state comparisons of student outcomes to estimate the effect of takeovers that occurred between 2011 and 2016. On average, we find no evidence that takeover generates academic benefits. Takeover appears to be disruptive in the early years of takeover, particularly to English Language Arts achievement, although the longer-term effects are less clear. We also observe considerable heterogeneity of effects across districts. Takeovers were least effective in districts with higher baseline achievement and least harmful in majority Latinx communities. Leaders should be cautious about using takeover without considering local context and a better understanding of why some takeovers are more effective than others.

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Abstract: Local school boards have primary authority for running educational systems in the U.S. but little is known empirically about the merits of this arrangement. State takeovers of struggling districts represent a rare alternative form of educational governance and have become an increasingly common response to low performance. However, limited research explores whether this effectively improves student outcomes. We track all takeovers nationwide from the late 1980s, when the first takeovers occurred, through 2016 and describe takeover districts. While these districts are low performing, we find academic performance plays less of a role in predicting takeover for districts serving larger concentrations of African American students. We then use a new data source allowing for cross-state comparisons of student outcomes to estimate the effect of takeovers that occurred between 2011 and 2016. On average, we find no evidence that takeover generates academic benefits. Takeover appears to be disruptive in the early years of takeover, particularly to English Language Arts achievement, although the longer-term effects are less clear. We also observe considerable heterogeneity of effects across districts. Takeovers were least effective in districts with higher baseline achievement and least harmful in majority Latinx communities. Leaders should be cautious about using takeover without considering local context and a better understanding of why some takeovers are more effective than others.

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EVALUATING EDUCATION GOVERNANCE: DOES STATE TAKEOVER OF SCHOOL DISTRICTS AFFECT STUDENT ACHIEVEMENT?

WHY STUDY STATE TAKEOVER?

School boards hold a special place in the American political landscape. They have been described by some as the purest expression of American democratic ideals (Iannaccone & Lutz, 2006; Maeroff, 2010; Resnick & Bryant, 2010; Hess & Meeks, 2013) and by others as institutions outdated and dysfunctional enough to be jettisoned entirely (Miller, 2008; Manna & McGuinn, 2013; Henig, Jacobsen, Reckhow, 2019). Historically, education has been a largely local affair with elected school boards holding the primary responsibility for governing our nation's schools and wielding the authority to hire and fire the superintendent responsible for school system operations (Howell, 2005). Progressive Era reformers created this system of locally elected boards with the goal of separating out education from the rest of urban governance, shielding schools from corruption and patronage, and placing educational experts at the helm (Finn & Keegan, 2004; Iannaccone & Lutz, 2006). However, very little empirical evidence speaks to whether this unique form of governance represents an effective arrangement for students.

Whereas most domestic public policy issues in the U.S. are handled by general-purpose institutions that cover a wide range of issues, education is an unusual policy area given its heavy reliance on single-purpose institutions, including school boards, that operate separately from the rest of local government (Kirst, 2004; Kirst & Wirt, 2009; Henig, 2013). However, this arrangement has been shifting in recent years as general-purpose governments (e.g., municipalities) and their executives (e.g., mayors and governors) have claimed greater authority

in the governance of school systems (Wong, Shen, Anagnostopoulos & Rutledge, 2007; Henig, 2013; Wong, 2013; Peterson, 2016). Public education has become more centralized as states and the federal government have taken on a greater role in funding and running educational systems. These changes have led political scientist Jeffrey Henig (2013) to argue that we are in the midst of the end to an era of "educational exceptionalism" where education policy is treated differently from other domestic issues.

An increased reliance on state takeovers of struggling local school districts represents a key example of this shift away from educational exceptionalism in that it removes school board authority, increases centralization, and often involves a greater role for general-purpose governments and executives. In 2015 alone, eleven states either debated or passed legislation that would allow for the creation of state-run districts (Layton, 2016) and at least 34 states now have the explicit authority to take over the management of schools, districts, or both (Jochim, 2016). Twenty states have laws authorizing state interventions into the finances of municipal governments (Huh, Fehr & Murphy, 2013) or the declaration of a fiscal emergency (Beckett-Camarata, 2004), which a state could theoretically use to take over a school district.

Typically, state takeover involves a shift in decision-making power from a locally elected school board to the state which then has the authority to do one or all of the following: run the school system directly, transfer authority to another official such as the mayor, abolish the school board, appoint all or some of a new school board, and/or appoint a new superintendent, receiver, or emergency manager to operate the system. State takeover, along with mayoral control, is therefore one of the few alternatives to the traditional school board system of educational governance that has been attempted in practice. However, there have been limited systematic

efforts to study the nature of these reforms, such as the frequency and length, as well as the types of districts that are targeted for takeover (for exceptions, see Jochim, 2016; Morel, 2018).

State takeover reforms have important equity implications. Takeovers tend to target low-performing systems where low-income learners and students of color are concentrated (Wong & Shen, 2003; Morel, 2018). Therefore, the impact of takeover on student achievement outcomes could contribute to either narrowing or widening race- and/or class-based achievement gaps. Additionally, school boards have historically served as key avenues for people of color to gain entry into public office (Henig, Hula, Orr & Pedescleaux, 1999). As a result, the removal of school board authority could have an impact on the descriptive representation of these communities' elected officials. Indeed, Morel (2018) finds that state takeovers of majority-Black districts have been followed by a decrease in the representation of African Americans in local government. However, he finds the opposite is true for majority-Latinx districts. Takeovers in these contexts have actually been followed by an increase in local Latinx political representation. In other words, we know these governance reforms have important political consequences but we know less about their effects on educational equity.

A key challenge for studying the effect of districtwide takeovers on student academic outcomes is that they occur relatively infrequently within any single state. Furthermore, each state has historically deployed its own standardized tests to measure student achievement. These two factors make cross-state comparisons both essential and difficult. Fortunately, we are able to capitalize on a new data source that allows us to compare academic outcomes across states to provide what we believe is the first available evidence on the question of whether and how state takeover of school districts affects the students served by these systems. More specifically, we estimate the effect of state takeover of districts nationally between 2011 and 2016 on test-based

English Language Arts and mathematics achievement among third through eighth grade students as well as educational inputs, including class size, the size of the charter sector, and educational spending. In the process, we examine and describe the characteristics of the full universe of districts that have undergone state takeover from 1988 to 2016.

WHAT IS THE RATIONALE AND EXISTING EVIDENCE FOR TAKEOVER?

Proponents of state takeover argue that it is a necessity in the case of persistent underperformance, financial mismanagement, noncompliance, or safety concerns, given that states have a constitutional obligation for providing public education (Parker, 2016; Ziebarth, 2002). They further point to school board dysfunction (e.g., Henig, Hula, Orr & Pedescleaux, 1999; Hess & Leal, 2004; Payne, 2008) and the low voter turnout that plagues school board elections, minimizing the degree to which these bodies are democratically representative and maximizing the influence of organized interest groups in these venues (Berry & Howell, 2005; Moe, 2005; Moe, 2011; Anzia, 2014). State takeover is therefore viewed as a governance arrangement that shields state level actors from local political pressures that make it otherwise difficult to enact change and shifts the venue of decision-making to one in which interest groups may have less power. In some cases, takeover is seen as a tool to remove officials who fail to follow basic rules (e.g., record keeping, education regulations) or local actors who break the law (e.g., fraud) (Jochim, 2016).

Critics of state takeover, on the other hand, argue that it is an ineffective strategy for improving school systems given state-level leaders' distance from students and limited capacity for directly running educational organizations. Instead, they make the case that local decision-makers who are closer to the teachers and students they serve are in the best position to turn

around failing school districts (Greenblatt, 2018). In contrast, takeover can get in the way of the development of local "civic capacity"—collective problem solving—to sustain school improvement efforts (Stone, Henig, Jones & Pierannunzi, 2001; Morel, 2018). They warn that state takeover often comes with disruptive changes, including turnover of leaders and teachers, that can get in the way of student learning. Furthermore, these skeptics caution that, beyond the effects on student outcomes, takeover usurps transparent, local, democratic decision-making, representation, and often the economic and political power of communities of color. Opponents have even at times gone so far as to compare state takeover to colonialism in cases where mostly white state-level leaders have seized power from communities composed primarily of and led by people of color (Oluwole & Green, 2009; Morel, 2018).

Recent national survey evidence suggests majority public support for state takeovers of troubled school systems in the case of persistently low academic performance and even higher levels of support in the case of financial mismanagement, at least when considered in the abstract. However, those citizens most likely to be directly affected by takeovers—teachers, residents of low-performing districts, and Black respondents—express significantly lower levels of support and higher rates of opposition (Schueler & West, 2019). This suggests that resistance to state takeover arises, at least in part, due to the loss of political power, autonomy, and job security that these stakeholders often experience when a takeover occurs in their community.

Importantly, the effect of alternatives to the traditional school board governance arrangement, let alone state takeovers, on student academic outcomes is unclear. Examining state takeovers and attempted turnarounds of districts from 1992 to 2000, Wong and Shen (2002, 2003) found that states have had some success at improving the financial management of struggling school systems but have had less success at improving student academic outcomes.

Given the data limitations at the time of publication, these researchers compared proficiency rates for districts across states. Unfortunately, this method has known drawbacks due to the fact that effects on these outcomes are dependent on underlying performance distributions (Ho, 2008). Furthermore, these studies predate the No Child Left Behind (NCLB) Act and thus the interventions under examination did not occur in a policy context with standardized performance information and high-stakes testing nationwide. As a result, there is a need to examine state takeovers in a more contemporary accountability climate.

More recent research on post-NCLB state takeovers is made up primarily of a series of case studies of districts under takeover, with bright spots in terms of student achievement outcomes from cities including New Orleans, Louisiana (Harris & Larsen, 2016; Harris & Larsen, 2018) and Lawrence, Massachusetts (Schueler, Goodman & Deming, 2016). However, researchers have also uncovered more mixed results from contexts such as Philadelphia, Pennsylvania (Gill, Zimmer, Christman & Blanc, 2007), Tennessee's Achievement School District (Zimmer, Henry, & Kho, 2017), and Newark, New Jersey (Chin, Kane, Kozakowski, Schueler & Staiger, 2017). The case studies reveal that state takeover can result in a diverse range of interventions designed to turn around school performance, even among those cases that have produced similar outcomes. For example, in New Orleans, Louisiana, collective bargaining was discontinued, tenure protections eliminated, nearly all teachers fired, and all schools eventually converted to charters. In contrast, in Lawrence, Massachusetts, although the state-appointed receiver had the authority to ignore the collective bargaining agreement and fire all teachers, he chose to re-negotiate a new contract with the union and to retain a majority of teachers. The district partnered with charter operators to manage a small number of schools, but did not convert any schools to charter status. While this literature is valuable in providing proof

points for the field about the viability of state-led, district-wide turnaround, it suffers from a key limitation: the case selection process is unclear. It could be that the districts chosen for case study are exceptional on a range of dimensions, and some states may have greater capacity for takeover than others, leaving the broader question on average nationwide takeover effects unanswered.

DATA

Tracking State Takeovers

To both track and describe state takeovers of school districts over time, and to estimate the effect of takeover on academic and policy outcomes, we generated an original dataset that identifies all districts that have ever been under state takeover since the first takeover occurred in 1988 up until 2016. Specifically, we track whether a district was under takeover by year, when the district transitioned to takeover, and if/when it was returned to local control. We define "takeover" as an instance when the state assumes control of an entire district and responds by either replacing all or part of the locally elected school board or superintendent with another decisionmaker or decision-making body. We exclusively track districtwide takeovers not takeovers of individual schools. The dataset also tracks variation in the type of takeover and governance shift (e.g., whether it led to mayoral control, a state appointed board, a state appointed superintendent, a jointly appointed board, etc.), as well as the rationale for the takeover (e.g., low academic performance, financial mismanagement, allegations of corruption, safety issues).

To create this dataset, we began by collecting existing studies of state takeover of districts (Jochim, 2016; Morel, 2018; Oluwole & Green, 2009; Ziebarth, 2002) and used them to

generate an initial list of districts that scholars who have previously studied this topic have identified as having ever been taken over. Each of these pre-existing reports focused on different periods of time and none provided comprehensive coverage of the time period we study, particularly the most recent takeovers in our study window that ultimately end up contributing to our estimates of takeover effects.

We validated the dates that districts entered and exited takeover, the type of takeover, and rationale for takeover using media reports and publicly available governmental records. Specifically, we used Google News Advanced Search and Education Week Archives to search for each individual takeover on our initial list. We then searched within these sources for state takeovers more generally over the entire period and thoroughly combed through all news articles to identify any takeovers that our other sources may have missed. Generally, we were able to confirm the tracking provided by the other secondary reports. However, we made a handful of changes based primarily on differences in the way we defined takeover or counted takeover start and end dates (which we describe below).

Finally, we verified our list of takeover districts and entry and exit dates via email communication with state education officials responsible for school accountability, turnaround, or state takeover. We emailed education officials in states where we observed at least one takeover or states that had takeover laws. This excluded states that did not have laws authorizing district takeover (Jochim, 2016; Ziebarth, 2002). We emailed officials in 26 states and received responses from 69 percent of these offices. These officials generally confirmed the accuracy of our tracking, but we did learn of and add to our list three additional districts that we had not included in our original tracking.

To define takeover timing, throughout the paper, we refer to an academic year by its spring year. For example, 2014 refers to the 2013-14 school year. Similarly, we identify takeover year as the spring of that academic school year. In other words, if the first year of takeover was the 2011-12 school year, we refer to the first takeover year as 2012. More specifically, if the takeover occurred between July 2011 and June 2012, we consider 2012 to be the first year of takeover. However, it is possible that takeovers announced late in a school year will have a low likelihood of being implemented until the next school year. Therefore, we confirm that our results do not fundamentally shift if we code the next school year to be the first takeover year if a takeover occurred between January and June (e.g., if we code the first takeover year as 2013 for takeovers that occurred between January 2012 and June 2012).

Academic Outcomes and District Characteristics

Historically, a key challenge of estimating the effect of state takeover on student learning has been the difficulty of making credible comparisons of district-level academic achievement outcomes across states. This is because individual states have used a variety of annual assessments – varying in form and difficulty – to measure annual progress. Measures of the percentage of students considered proficient on these exams have known limitations given that effects on these outcomes are dependent on underlying performance distributions (Ho, 2008).

The Stanford Education Data Archive (SEDA) (Reardon, Kalogrides, Ho, Shear, Shores & Fahle, 2016) provides a solution to this problem since these data have been normed to the National Assessment of Education Progress (NAEP) exam, allowing for cross-state achievement comparisons. SEDA currently includes measures of third to eighth grade academic performance in mathematics and English Language Arts (ELA) at the grade-district level across all states for 2009 to 2016. These test scores are standardized to the nationwide population of school districts.

In addition to academic outcomes, the SEDA also includes data compiled from the federal Common Core of Data (CCD), American Communities Survey (ACS), and Small Area Income Poverty Estimate (SAIPE). We use these data for our measures of district demographics and educational inputs such as pupil-teacher ratio, total per pupil expenditures, the share of expenditures spent on instruction, and the percent of public school students in charter schools.

For our analysis of takeover impact, we merge the SEDA Version 3.0 data with the subset of our tracking data covering the time period included in the SEDA data (2009 to 2016). We also merge in information on whether a district has ever been taken over by a state prior to 2016 (even if this occurred in the pre-2011 period). The resulting data are at the district-grade-year level and include over 500,000 district-grade-year observations, covering over 15,000 districts spread out over the fifty states. For our analysis, we ultimately exclude districts in states without statutes authorizing takeover, as well as districts that were taken over prior to 2011 and therefore could be considered "treated" though we confirm that our findings are not a function of these sample restrictions.

Finally, we supplement these data with information on additional state and district characteristics. We construct an indicator for whether a state had passed a state takeover authorization law, drawn from secondary sources (Jochim, 2016; Ziebarth, 2002; Morel, 2018; Rutgers, 2006) and our own policy tracking. We also added state political characteristics including measures of the partisanship of control over state legislatures and the governor's office from the National Council of State Legislators (2018) and Ballotpedia (2018). To examine the relationship between takeover and fiscal factors, we merged data from the Annual Survey of School System Finances (Census Bureau, 2018). These data allowed us to calculate the percent of a district's expenditures that came from state funding sources. Finally, to test whether district-

level policy changes could be driving results, we use data tracking the early implementation of teacher evaluation systems from Bleiberg and Harbatkin (2020).

DESCRIBING STATE TAKEOVERS 1988 – 2016

We begin by describing the full universe of state takeovers. In Figure 1, we display the number of takeovers that occurred in each year since the earliest takeovers in the late 1980s through 2016, as well as the average length of takeovers by the first year of takeover. This figure includes all takeovers but a version excluding takeovers that are ongoing post-2016 reveals the same patterns. The top panel shows that takeover is a relatively rare event, occurring between once and nine times per year. However, the use of takeovers has increased somewhat over time. For example, we observe an average of 3.9 takeovers per year (1988-2010) and 5.8 takeovers per year (2011-2016).

The bottom panel of Figure 1 illustrates that the average takeover length in a given year ranges from one to 14 years. The only exception is 1990 in which a single long-lasting takeover of New Jersey's Jersey City Public Schools began. Across all years, the average takeover lasts 6.31 years, ranging from one to 28 years. The average length of takeovers in our analysis window is somewhat shorter (five or fewer years) than the averages for previous years which are more likely to be between five and 10 years (although in part this could be because the takeovers in our analysis window are more likely to be ongoing).

In Figure 2 we show the states and regions where takeovers have been geographically concentrated. In the top panel, we show that historically, takeovers have occurred in all major regions of the U.S. (Northeast, South, Midwest, and West) but have been least common in the West—particularly the Mountain West (i.e., Montana, Wyoming, Colorado, New Mexico, Utah)

and the Great Plains (i.e., the Dakotas, Nebraska, Kansas, Oklahoma). The greatest number of takeovers have occurred in Mississippi (19) and Arkansas (11). There have also been a significant number in California (9), Kentucky (9), Alabama (9), West Virginia (9), Illinois (8), Pennsylvania (7), and Connecticut (6).

We further describe the sample in Table 1. Here we split up the complete universe of takeover districts into those that were taken over before the 2011 – 2016 window covered by the SEDA data (2009-2016) and those that occurred during that window and therefore contribute to our estimates of the effect of takeover. We do this to assess the extent to which our analytic sample of takeover districts is representative of the full population of takeover districts. Therefore, the first column represents the 79 districts that were taken over prior to 2011 and the second column includes the 35 districts that make up the treatment group in our analysis sample. These treatment group districts are also listed by name and year of takeover in Appendix Table A1. We do not include districts taken over in 2009 or 2010 in this group in order to preserve two years of baseline measures for our treated districts used in the analytic strategy we describe below. Column three includes only those districts that have never been taken over. For all characteristics, we average across all years in the SEDA panel (2009-2016).

Takeover districts vary in important ways from districts that have never experienced takeover. First, students of color and low-income students are overrepresented in these contexts. For instance, on average, 57 percent of the students in takeover districts in our analysis sample are Black compared to only 7 percent in the non-takeover districts. Similarly, 22 percent of treated students in the analysis sample are Hispanic versus only 13 percent of students in the non-takeover districts. On average, the recent takeover districts served a student population in which 86 percent of students received free or reduced-price lunch while the average non-

takeover district served a population in which 48 percent received reduced-price lunch. Not surprisingly given that state takeovers are known to target low-performing districts, on average, these districts score a little more than a half of a standard deviation lower than the typical district nationwide. All of these differences are statistically significant, as shown in column four.

We provide further visual evidence to explore the relative role of race and achievement in predicting takeover. In Figure 3, we show the density of various percentages of Black students among the takeover districts, illustrating that a large share of these takeover districts served student populations in which three-quarters or more of the students are Black. In Figure 4, we plot the percent of students in a district that are Black on the x-axis against baseline ELA (in the top panel) and math (in the bottom panel) achievement on the y-axis, for takeover districts (in black) and non-takeover districts (in grey). For the non-takeover districts, we have plotted a random sample of districts so that the takeover districts can be visually identified more easily but the patterns hold when we plot the full sample. Not surprisingly, overall, we observe a negative relationship between achievement and the percent of a district student population that is Black (shown on Figure 4 with a line of best fit), suggesting lower average levels of test-based performance among districts serving high concentrations of Black students, on average. However, this relationship is stronger in non-takeover than takeover districts. Among majority-Black districts, we observe takeovers throughout the performance distribution whereas in majority-non-Black districts, takeovers are concentrated in the bottom half of the performance distribution. In other words, in districts serving fewer Black students, academic performance is more predictive of takeover than it is in districts serving large populations of Black children. This suggests that race is playing an important role in the process of selection for takeover,

above and beyond the academic performance of a given district, with majority-Black communities at greater risk of being taken over.

Beyond their demographic makeup, takeover districts also tend to be situated in unique policy and political contexts. For instance, they are more likely to receive a large share of their funding from state sources. In our analysis sample, the average district received 61 percent of its revenue from their state compared to 47 percent for the non-takeover districts. Takeover districts are more likely to be found in urban contexts (25 percent of takeover districts versus 6 percent of non-takeover districts). Takeover districts have a larger charter school sector, serving 7 percent of students on average, compared to non-takeover districts, serving 1 percent. Finally, these districts are more likely to be both led by a Republican governor and to have unified government—the same party controlling both chambers of the legislature and the executive branch—at the state level. Again, all of these differences are statistically significant. Our descriptive results are generally consistent with previous research on this question that has examined some of these characteristics of takeover districts such as share of revenue coming from the state (e.g., Morel, 2018).

Perhaps surprisingly, state takeover is not exclusively a big city phenomenon. Only 25 percent of takeover districts are classified as urban. The average district in our analysis window includes 3,972 students. This is not very large but is still larger than the average non-takeover district which has 1,539 students. None of the districts in our analysis sample come close to being in the top 100 largest districts in the country though such districts have historically been subject to takeover in years prior to our analysis window (e.g., Detroit, MI).

Nearly all of the takeovers that occurred within our analysis window resulted in the replacement of or loss of decision-making power for both the superintendent and the local school

board. Only eight takeovers resulted in either one or the other losing authority. The reasons for takeover that were publicly provided in news coverage included fiscal mismanagement, poor academic performance, noncompliance with state mandates, safety concerns, and under-enrollment. However, there were very few districts in which there seemed to be a single stated reason for takeover. Nearly all were described as having a host of these issues. Furthermore, there's a possibility that the avowed reason for a state takeover is related to the state authorizing law regardless of state officials' true motivations. In Appendix Table A2, we list all 28 states that have passed laws allowing the takeover of entire districts and the rationale(s) for takeover that each law permits. All but two of these states allow takeover in the event of persistent low academic performance. Half allow takeover due to either poor academic performance or financial mismanagement and two states only permit takeover in the event of financial issues.

The takeover districts in our analysis sample (2011 - 2016) are generally representative of the full universe of takeover districts (1988 - 2016). In Table 1, we show that the main difference between takeovers occurring after 2010 is that they targeted districts that were smaller (3,972 students on average) than the districts taken over prior to 2011 (serving 14,910 students), however, this difference is not statistically significant (as shown in column five of Table 1). Districts taken over prior to our window had a somewhat lower concentration of students qualifying for subsidized lunch (81 versus 86 percent) and a higher concentration of Asian students (2 versus 1 percent). They were also slightly lower achieving in ELA (-0.53 standard deviations) than the pre-2011 takeover districts (-0.46 standard deviations), though this is hard to assess given we are reliant on achievement data from the SEDA window (2009-2016) even for those districts taken over in prior decades (i.e., before 2009) and this difference is not statistically significant.

The bottom panel of Figure 2 shows that all four regions are represented in the analysis sample but that there are seven takeover states not represented. Importantly, our analysis sample does not include New Orleans, the one district that has experienced takeover in Louisiana and that serves as a notable example of a takeover and districtwide turnaround that has been rigorously studied and shown to have generated significant student achievement improvements (Harris & Larsen, 2016; Harris & Larsen, 2018). New Orleans is not in our analysis sample because this takeover occurred prior to 2011. Tennessee's Achievement School District is also not in our analysis sample because it includes schools from throughout the state and is not identified as a district in the SEDA data. However, our analysis sample does include Massachusetts' Lawrence Public Schools, the other major positive proof point of state takeover that has been previously evaluated (Schueler, Goodman & Deming, 2017).

ANALYZING THE EFFECT OF TAKEOVER 2011 – 2016

To study the overall effect of state takeover on academic achievement for the period between 2011 and 2016, we conduct difference-in-differences analyses comparing the achievement trends of takeover districts to the trends of comparable districts not experiencing state takeover. In other words, we test whether the change in achievement for takeover districts before versus after takeover was different than the change in achievement for districts that were not under state control over the same time period. We rely on the following model:

$$Y_{gdsy} = \beta_0 + \beta_1 Takeover_{dsy} + \gamma_d + \delta_{sy} + \varphi_g + \beta_2 X_{dg} + \varepsilon_{gdsy}$$

where Y is an outcome for grade g in district d in state s and year y. $Takeover_{dsy}$ is a binary indicator that equals one if a district was ever under takeover within our 2011-2016 analysis window and if the year is a post-takeover year. Therefore, β_1 is the coefficient of

interest that allows us to assess the effect of takeover on our outcomes. We include district fixed effects (γ_d) which flexibly provide the main effect of whether a district was ever taken over within our analysis window (2011-2016) for the purpose of our difference-in-differences model and control for time-invariant characteristics of school districts. State-by-year fixed effects (δ_{sy}) control for state-level factors that change over time, other than takeover, that could influence student achievement such as changes to state policy or collective bargaining laws, flexibilities granted to states from the federal government, the rigor of state standards, and state-level economic shocks. They also provide the main effect of year for the purpose of our difference-in-differences model. Grade fixed effects (φ_g) ensure that we are comparing students in the same grade levels as our data are at the district-grade-year level (though our results are not sensitive to the inclusion of grade fixed effects). Given takeover districts are likely selected on the basis of low-performance we control for pre-2011 baseline math and ELA performance at the district-grade level (X_{dg}). In short, we estimate whether the change in outcomes before and after takeover is different for takeover districts than for similarly achieving districts that were not subject to takeover over the same period. We cluster standard errors at the district level given this is the unit assigned to treatment. We run all models on a sample excluding those previously-treated districts that were taken over prior to 2011 as well excluding districts in states without takeover laws (where takeover is therefore not possible).

Importantly, the above model pools all post-takeover years to calculate the average effect of takeover. These pooled estimates are accompanied by results based on event-study models where we replace the main effect of takeover with separate indicators for each year leading up to and after takeover interacted with an indicator for whether the district was taken over. The event-study models take the following form:

$$Y_{gdsy} = \beta_0 + \sum_{r=-6}^{6} Takeover_{dsy} + \gamma_d + \delta_{sy} + \varphi_g + \beta_2 X_{dg} + \varepsilon_{gdsy}$$

Here, $\sum_{r=-6}^{6} Takeover_{dsy}$ represents a series of indicators for the number of years since takeover, centered on and omitting the year prior to takeover, for all six cohorts of takeovers that occur within our analytic window between 2011 and 2016. We observe a maximum of seven pre-takeover years for the 2016 cohort (pre-takeover year -6 through pre-takeover year 0) and a maximum of six post-takeover years for the 2011 cohort (takeover year 1 through takeover year 6). These post-takeover indicators allow us to assess the effect of takeover by the number of years of exposure to the reform. We include and highlight the event study results because the question of whether the effect of takeover changes over time is substantively interesting and because the event study specification allows us to transparently test whether pre-trend assumptions hold by examining the coefficients on the pre-takeover indicators.

Additionally, the methodological literature on difference-in-differences approaches to policy evaluation has rapidly progressed in recent years, suggesting that bias can arise in situations with multiple time periods and variation in treatment timing, particularly when relying on models estimating only two time periods ("pre" and "post") (e.g., Callaway and Sant'Anna, 2020; Sun and Abraham, forthcoming). First, such pooled difference-in-difference estimates can be biased when treatment effects vary across time (Goodman-Bacon, 2018; Baker, Larcker & Wang, 2021; Sun and Abraham, forthcoming). Therefore, rather than simply presenting pooled difference-in-differences estimates with only two time periods ("pre" and "post"), we present and indeed privilege event study models that estimate effects by year of treatment, overall and separately by cohort. An additional source of bias stems from the use of already-treated units as controls (Goodman-Bacon, 2018). We address this possibility by first excluding districts from our analytic sample that were taken over prior to our analysis window. Finally, we estimate our

models separately for each of the six cohorts of takeover districts (2011-2016), excluding all other ever-treated districts from our sample and examine the extent to which patterns are consistent across cohorts.

STATE TAKEOVER IMPACTS 2011 – 2016

Academic Achievement Effects

Overall, we find no evidence that state takeover improves academic achievement. We display results from our difference-in-differences estimates pooling all post-takeover years in Table 2. For ELA achievement, takeover has a moderate negative effect on ELA achievement on the order of 0.05 standard deviations, though this estimate does not quite achieve statistical significance ($p=0.10$). Given we know that these pooled estimates can be biased away from the true effects when there is variation in treatment timing, we turn to the results from our event study specification in which we include separate indicators for each pre- and post-takeover year (displayed in Table 3). Here we find negative effects concentrated in years two and three. There is some evidence of a bounce back to pre-takeover levels by year five, though not a statistically significant improvement relative to the pre-takeover period, and a suggestive decline again in year six. The estimate for year one is also negative in direction (-0.03) but does not achieve statistical significance.

We have greater confidence in our estimates for the first three years of takeover than for years four through six given we have an unbalanced panel and larger samples of treated districts contributing to these results. In other words, for all 35 takeover districts in our window, we observe the first year of takeover. However, for takeovers that occurred toward the end of our window (e.g., 2015), we do not observe outcomes in the later years of takeover. In the last

column of Table 3 we show the number of treated districts that contribute to each of our estimates. For years one through three, this is 24 districts or more. In year four, this declines to 18 districts and continues declining through year six for which we observe only four districts that were taken over in 2011 and were therefore under takeover for six years within our window.

We do not find strong evidence of a takeover effect on math achievement. When pooling all post-takeover years, we find takeover decreases test performance on the order of 0.04 standard deviations. However, this effect does not achieve statistical significance. When we break out the results by year using the event study approach, we again observe small, non-significant, negative effects in years one through four of takeover ranging from -0.03 to -0.04 depending on the number of years of takeover exposure. There is suggestive evidence of improvement in math by year five (0.07) and six (0.03) but neither of these coefficients reach statistical significance and again are based on a smaller number of treated districts (ten for year five and four for year six).

In Figure 5 we plot the coefficients from our event study specification which—for ELA—provide visual evidence of a break from the pre-treatment trend in outcomes after takeover, a recovery of test scores in the fifth year of takeover and a decline again in year six. The bump in year five has visual impact but, again, it is important to keep in mind that this estimate is both based on a mere ten treated districts and is not statistically different from zero. We find the scores in years one to three more informative of average takeover effects given the larger sample size of treated districts contributing to these means. For math, Figure 5 again provides some evidence of a small initial decline followed by recovery in years five and six but also illustrates that these coefficients are all estimated imprecisely (the farther out from takeover, the more imprecise).

To formally test whether or not we observe pre-takeover achievement trends, we examine the coefficients on the pre-takeover indicators displayed in Table 2 and plotted in Figure 5. We do not observe any statistically significant "effects" in the years leading up to takeover in either subject. Furthermore, in ELA, the magnitude of the pre-takeover coefficients is never larger than the magnitude of effects in the post-takeover years. The one exception is year negative six for which we find evidence of a pre-treatment "effect" in both subjects. However, we do not view this as a major threat to the validity of the post-takeover estimates as it is based on the single cohort for which we observe year negative six (the cohort of six districts taken over in 2016). This is also a cohort for which we only observe one post-takeover year (and therefore does not contribute to the estimates of years two through six of takeover). We discuss the cohort models in more detail below.

We detect no visual evidence in either subject of a substantial short-term decline in test scores in the year prior to takeover—an "Ashenfelter's dip"—which could be inflating our results due to mean reversion or suggesting that takeover districts were already experiencing performance declines relative to similar districts leading up to treatment (Ashenfelter, 1978). We also conduct a test in the spirit of a Granger causality check (Angrist & Pischke, 2009) by running an event study model without controls and find that none of the coefficients on the pre-treatment year estimates for year negative five to negative one approach statistical significance. The coefficients in the pre-takeover years farthest away from takeover do increase somewhat (from year negative five to negative three) in both subjects. However, the pre-takeover years leading up to intervention (years negative three to negative one) remain relatively consistent in magnitude, suggesting that the post-takeover effects are not an artifact of a pre-trend in the years leading up to takeover.

To further probe the pre-trends assumption, we estimate a comparative interrupted time series model where we include a linear time trend ("Year") as well as our treatment indicator and an interaction between an indicator for being taken over and the linear time trend. We display the results in Appendix Table A3, showing that our estimates of the treatment effects are robust to this specification. With this specification, the negative results become larger (-0.07 standard deviations) and statistically significant for both subjects. The insignificance and small magnitude of the interaction between the takeover indicator and the linear time trend imply there is no significant difference in the pre-treatment trend between the treatment and control group.

To examine the role that each cohort plays in driving the post-takeover effects, we estimate a version of our model where we include separate pre- and post-takeover indicators by year for each of the six cohorts contributing to our estimates and display the results in Table 4. In ELA, we observe no significant pre-treatment coefficients for four of the six cohorts. One of the two cohorts with significant pre-takeover "effects" is the 2016 cohort, which again, only contributes to our estimates of the first year of takeover. All but one cohort (2012) reflects a pattern of negative post-takeover effects and the apparent ELA improvement in year five is driven entirely by the 2012 cohort. In short, these results provide confidence that the results for years one through three of takeover are not driven by any one cohort. In math, the patterns of results are more mixed by cohort, but overall, Table 4 supports our conclusion that there is no strong evidence that state takeover, on average, has a significant effect on math scores in one direction or the other.

We also illustrate that our results are not due to changes in missingness for our outcomes or district-grade observations over time. In Appendix Table A4, we show that state takeover does not predict a change in the likelihood of a district-grade observation missing an ELA or math test

score value when using the pooled difference-in-differences model with test score missingness as the outcome. When we examine the effect of takeover on test score missingness using the event study model, we do not find evidence of a differential change in missingness. The one exception is takeover year five for ELA in which we observe a slight increase in missingness. Interestingly, year five is the only year for which we observe a positive (though statistically insignificant) coefficient on ELA test scores.

Finally, we explore whether other time varying district-level policies, besides state takeover, could be explaining our results. While most major relevant policy changes over this period were occurring primarily at the state level (e.g., adoption of Common Core State Standards), one exception was the introduction of a new generation of teacher evaluation systems that were encouraged and piloted in certain districts in response to the federal Race to the Top competitive grant program. We estimate a version of our models where we replace state-year fixed effects with separate state and year fixed effects and then control for time-varying indicators of district-level teacher evaluation policy adoption based on Bleiberg and Harbatkin's (2020) tracking of teacher evaluation policy diffusion. Results are displayed in Appendix Table A5. Our findings remain unchanged. Furthermore, given that state takeover is a relatively all-encompassing reform, to the extent that it is correlated in time with other district-level policy changes, it is likely that those policy changes are the result of takeover itself. We would view these other policy changes as a mechanism through which takeover influences student outcomes and therefore not a threat to the validity of our inferences about takeover.

Heterogeneity of Achievement Effects

In Figure 6 we illustrate that the effect of state takeover on academic achievement is far from uniform. To generate this figure, we ran a modified version of our pooled model with

indicators for each individual treated district interacted with an indicator for whether it was a post-takeover year. Our model is otherwise the same as the model described above. We then plot the coefficients for each takeover district sorted from most negative to most positive for ELA in the top panel and math in the bottom panel.

While roughly half of districts saw negative effects in ELA, there were several districts that experienced virtually no change in outcomes. In both subjects, a handful of districts saw non-trivial positive impacts in the aftermath of takeover. We would caution against attaching much meaning to the specific rank order of districts given the precise order is sensitive to modelling choices and sample restrictions. However, Lawrence, Massachusetts is an interesting case where we observe positive gains in both subjects (though larger in math than ELA). This finding is notable because Lawrence is one of the few takeover districts in our sample that has been studied in the period overlapping our analysis window and previous studies have identified it as a positive proof point for state takeover, consistent with our findings here (Schueler, Goodman & Deming, 2017; Schueler, 2018).

To test for patterns in the heterogenous effects, we examined whether effects varied by a number of baseline district-level characteristics, including the racial and ethnic makeup of a district, the percent of students qualifying for subsidized meals and English language learner services, baseline achievement and district size (i.e., number of students). We display the results in Table 5, showing that, in both ELA and math, takeovers had smaller effects in districts serving a greater share of Hispanic students. In other words, these findings suggest that takeover has a greater potential to be beneficial in districts serving larger concentrations of Latinx students. The effects are also smaller in districts with a larger share of students classified as English Language Learners (though only the interaction for ELA is statistically significant). In both subjects, we

find that the negative effects of takeover were larger in districts with higher baseline test scores. The effect of takeover does not appear to vary based on the percent of students qualifying for subsidized lunch, the size of the district, or whether the stated rationale for takeover was unrelated to academic performance based on our tracking.

Effects on District Enrollment and Demographics

We also examine whether the effects of takeover are driven by changes to the composition of the districts under study. In other words, we wanted to know whether—for example—these effects could be due to families leaving or entering takeover districts due to takeover. In Table 2, we show the results from a series of regressions where we run our pooled difference-in-differences models but replace test score outcomes with a variety of district characteristics. Overall, we find no strong evidence that changes in the size or demographic makeup of treated districts are driving our conclusions. We observe a small effect of takeover on the number of students enrolled in a district (by approximately193 students on average), on the percent of students who are Black or classified as English Language Learners (by half a percentage point in both cases), and the percent of students qualifying for subsidized lunch (by 1.75 percentage points). However, when we examine the effect of takeover on demographic characteristics in an event study framework (results displayed in Table 6), it is not clear whether or not these changes are due to pre-trends leading up to takeover. We find no evidence takeover significantly shifted the percent of Hispanic students served by takeover districts relative to comparison districts or altered the rate at which districts were classifying students as in need of special education services.

Effects on Educational Inputs

To examine possible mechanisms for the effect of state takeover, we examine the extent to which takeover in the 2011 to 2016 period had an effect on major education inputs. We display results from pooled difference-in-differences estimates in Table 2 and from event study estimates in Table 7. We find no strong evidence that takeover changed average class sizes, the size of the charter sector or overall per pupil expenditures. For all three outcomes, the coefficients are positive both overall and for any given takeover year but are rarely statistically significant. The one exception is that takeover appears to have increased the charter sector share by 2.83 percentage points (when pooling all post-takeover years), however, we also observe statistically significant "effects" on the size of the charter sector in several pre-takeover years and therefore cannot rule out the possibility that these impacts are due to pre-trends. In other words, there is no strong evidence that the results are due to a major increase in class sizes, the charter market share, or major reductions in educational spending.

DISCUSSION

State takeover of struggling school systems has become an increasingly common policy response in the event of persistent districtwide underperformance, particularly as a range of observers have pointed to school board dysfunction as a potential cause for chronic low achievement. However, there has been little empirical evidence to shed light on the average impact of governance reforms that shift authority from local school boards to state governments. This is a particularly concerning gap in the literature given that the Every Student Succeeds Act now provides states with greater autonomy over turnaround policies and requires them to use federal funds for turnaround policies that are backed by federally-defined evidence standards

(Levesque, 2016; West, 2016). States therefore have a substantial need for research on state-level takeover and turnaround policy.

To address this need, we provide a systematic tracking and description of state takeovers nationally from the first takeovers in the late 1980s through 2016. We show that takeovers have become more common and shorter over time, are not an exclusively big city phenomenon, and tend to occur in districts with larger charter sectors. The latter finding could suggest that there is overlap in the political forces and community of educational reform-oriented entrepreneurs exerting influence in these districts. Perhaps most interestingly, districts serving larger concentrations of African American students tend to be more likely targets for takeover regardless of the academic performance of these systems. This is something previous studies have not been able to examine given the lack of comparable performance metrics across states.

Beyond describing takeovers, this study examines the effect of state takeover on academic achievement and key educational inputs in the post-No Child Left Behind era. On average, we find no strong evidence that takeover produces benefits for student academic achievement in ELA or math, at least in the short run, and evidence that it is typically disruptive for student ELA achievement in the early years of takeover. The magnitude of the effects on short term ELA performance are not trivial—about ten percent of the gap between students eligible and ineligible for subsidized lunch in eighth grade reading on the National Assessment of Educational Progress (NAEP) (Hill, Bloom, Black, Lipsey, 2007). We find no strong evidence of causal impacts on class size, charter market share, or educational spending. While we argue that the negative effects are non-trivial in size, even a null finding is important in this case given the extreme step of state takeover is often made on the promise of rapid performance improvements. Additionally, we observe suggestive evidence that takeover could produce

benefits in the longer term but are less confident in these results given they are based on a small subset of districts and more sensitive to modelling choices and sample restrictions. Future research should therefore shed additional light on the longer-term impacts of takeover which could potentially improve performance in the long-run despite short-term disruptions.

Importantly, we find a nontrivial amount of heterogeneity from district to district in the effect of takeover on academic achievement. Unfortunately, the existing case study literature of district takeovers does not provide much in the way of consistent patterns that would help to explain the heterogeneity of effects. This is in part because there is variation across these cases studies in terms of the characteristics of the districts under takeover, the policy interventions that leaders took in the aftermath of takeover, and the academic outcomes these cases produced. For instance, Henry, Pham, Kho and Zimmer (2020) find that high teacher turnover in Tennessee's state-run Achievement School District helped to explain why the effects weren't more positive, and in Lawrence, leaders erred on the side of retaining teachers and generated benefits. In contrast, in New Orleans, nearly all teachers were let go and the reforms resulted in large academic gains. One might wonder about the role of charter operators. New Orleans converted all schools to charter status and produced positive results. Similarly, in Newark, shifting enrollment from low-performing traditional public schools to higher-performing charter schools improved district outcomes. However, Lawrence was able to make notable improvements without converting schools to charter status and charters played a major role in the Tennessee Achievement School District reforms which produced disappointing outcomes. In terms of district baseline characteristics, Lawrence is the one impact evaluation case study featuring a majority-Latinx district, although the other positive proof point, New Orleans, serves a majority-Black student population. In both subjects, our study finds that the negative effects were smaller

in the districts that were the lowest performing at baseline but with the existing data we cannot provide much information about the mechanisms at play. A key direction for future research would be to better explain this heterogeneity. One possibility is that there is an interaction between the contextual characteristics of districts and the intervention features needed to generate improvement. Another possibility is that the case studies are not representative of average takeovers.

Interestingly, the heterogeneity in takeover effects seems to be at the district rather than state level. For example, Mississippi and Arkansas are home to both positive and negative outliers in terms of the achievement effects of its various takeovers. This suggests that state capacity or other state level factors may not be the primary (or at least are not the only) factors that explain why some takeovers are successful and others are not. Unpacking these questions may require the collection of new data describing these districts and the features of the reforms that were undertaken in the aftermath of takeover. One promising approach would be to study these states with both positive and negative cases in more detail to try to understand why some were more successful than others.

We do find that these negative takeover effects were smaller in districts serving larger Hispanic student populations. Unfortunately, we are unable with the existing data to say much about why this is the case, but previous research may provide some clues. Our finding on the differences in the effectiveness of takeovers of districts serving majority-African American vs. majority Latinx student populations aligns with Morel's (2017) findings on the effects of takeover on local descriptive political representation. He finds that takeovers of majority-Black districts lead to a loss of local Black political power whereas takeovers of majority-Latinx districts are more often followed by an increase in Latinx local political representation. It is

possible that this dynamic could help explain the mechanisms through which takeover harms academic outcomes given our parallel findings, especially since others have found that increases in non-white political representation on school boards leads to increased spending on and academic achievement of non-white students (Kogan, Lavertu & Peskowitz, 2020; Fischer, 2020). Interestingly, in a meta-analysis of studies of reforms designed to improve low-performing schools, Schueler, Asher, Larned, Mehrotra and Pollard (2020) also find that the benefits from these reforms have been concentrated in majority Latinx contexts. Regardless of the mechanisms, these two studies together suggest that takeover is particularly risky for both the adults and children in majority-African American communities.

From a policy perspective, although our findings suggest that there are some cases in which state takeover can result in immediate gains for kids, this does not appear to be true on average, at least in the period from 2011 to 2016. These results do not provide support for the theory that school board governance is the primary cause of low academic performance in struggling school districts. Notably, we find that the main takeover that both occurred within our analysis window and that has been previously evaluated, Massachusetts' Lawrence Public Schools, is an outlier demonstrating positive effects of takeover in both reading and math. This finding illustrates why researchers should remain cautious about drawing conclusions from single case studies about state takeover as a reform strategy more generally. Similarly, before learning what makes a state takeover more or less successful, policymakers should be careful about deploying takeover as a strategy for improving academic achievement, particularly in contexts that differ on important dimensions from those rare districts that have demonstrated positive results.

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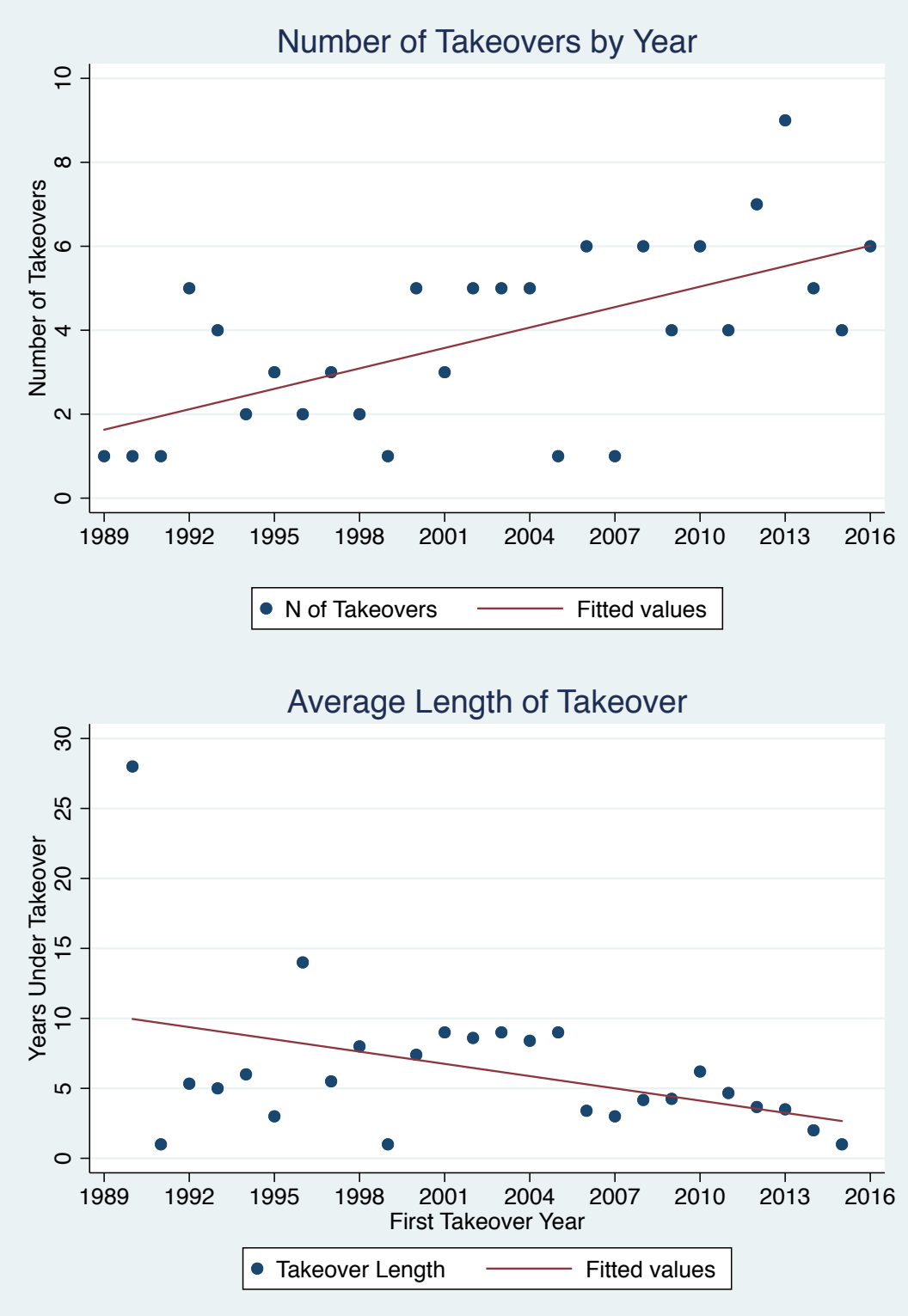


Figure 1. Number of Takeovers and Average Length of Takeover by Year.

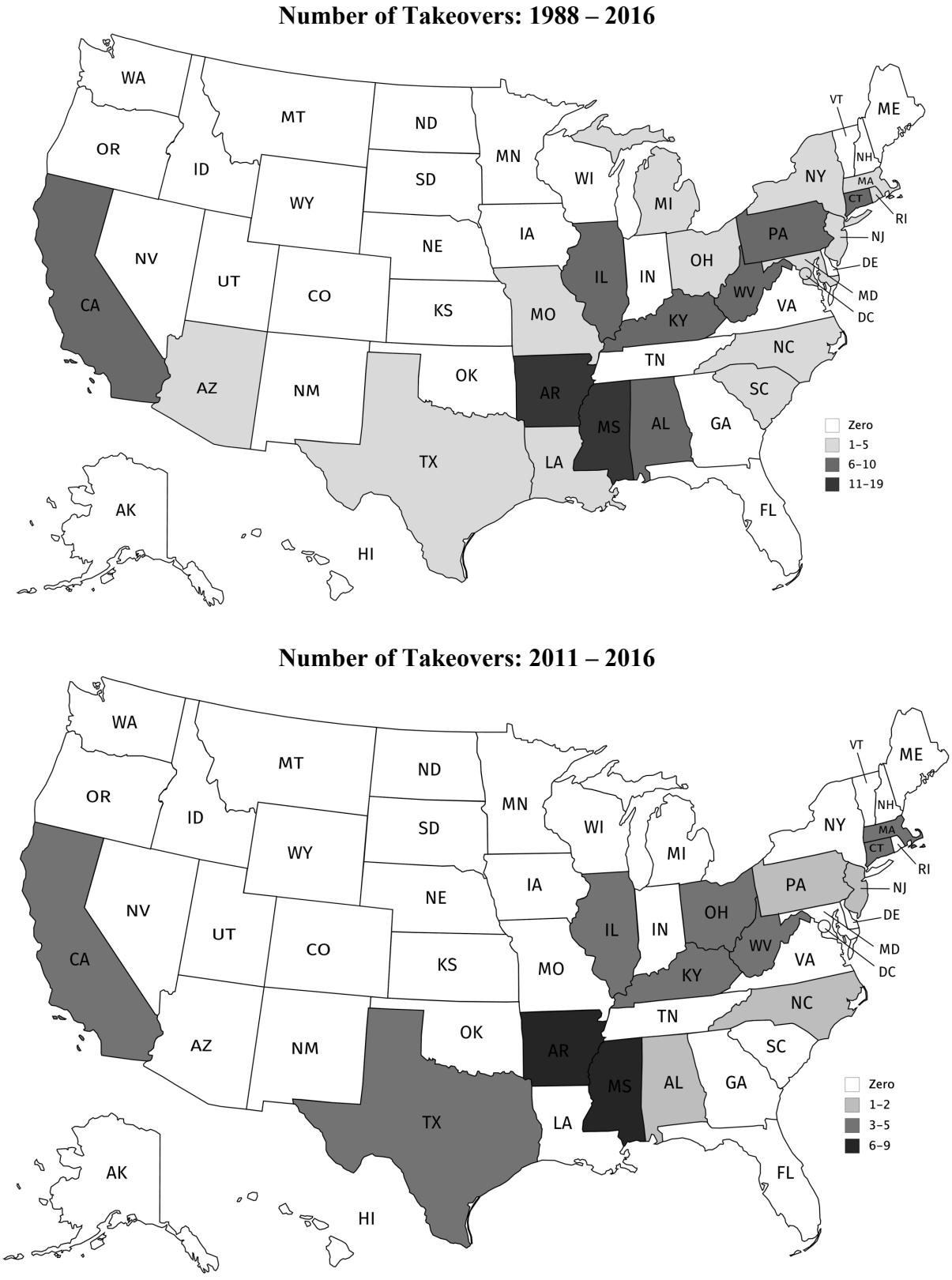


Figure 2. Number of Takeovers by State.

Table 1. Characteristics of Districts by Takeover Status and Timing

	(1)	(2)	(3)	(4)	(5)
	Taken Over 1988-2010	Taken Over 2011-2016	Never Taken Over	Diff. (2) - (3)	Diff. (1) - (2)
Sample Size					
N of states	23	14	50		
N of districts	79	35	13,561		
District Student Demographics					
Asian	0.02	0.01	0.02	-0.01	0.01*
Black	0.47	0.57	0.07	0.49***	-0.10
Hispanic	0.18	0.22	0.13	0.09**	-0.04
Native American	0.01	0.00	0.03	-0.03	0.01
White	0.31	0.21	0.74	-0.54***	0.10
Subsidized Lunch	0.81	0.86	0.48	0.37***	-0.05**
English Language Learner	0.07	0.08	0.04	0.03**	-0.01
Special Education	0.15	0.14	0.15	-0.00	0.01
District Characteristics					
N of tested students	14,910	3,972	1,539	2,433**	10,938
Percent in charter schools	0.09	0.07	0.01	0.06***	0.02
Pupil teacher ratio	15.20	14.77	14.18	0.59	0.43
Per pupil expenditures	15,688	14,315	15,190	-875	1,373
Percent revenue from state	0.54	0.61	0.47	0.13***	-0.07
Urban	0.23	0.25	0.06	0.19***	-0.02
Percent of adults with B.A.	0.17	0.15	0.23	-0.08***	0.02
Percent unemployed	0.12	0.12	0.08	0.05***	0.00
District Average Achievement					
Standardized math scores	-0.53	-0.55	0.02	-0.57***	0.02
Standardized ELA scores	-0.46	-0.53	0.03	-0.56***	0.07
Percent missing test scores	0.08	0.03	0.05	-0.02	0.05
State Characteristics, 2009					
Unified government	0.29	0.40	0.27	0.13**	-0.11
GOP governor	0.50	0.46	0.53	0.07	0.04
GOP governor, unified gov't	0.20	0.40	0.14	0.25***	-0.20

Note: Student and district characteristics represent district averages over all years in our SEDA panel (2009 through 2016). Per pupil expenditures represent average annual spending in dollars. Percent missing test scores represent the percent of districts in each category missing districtwide test scores. *** p<0.01, ** p<0.05, * p<0.10.

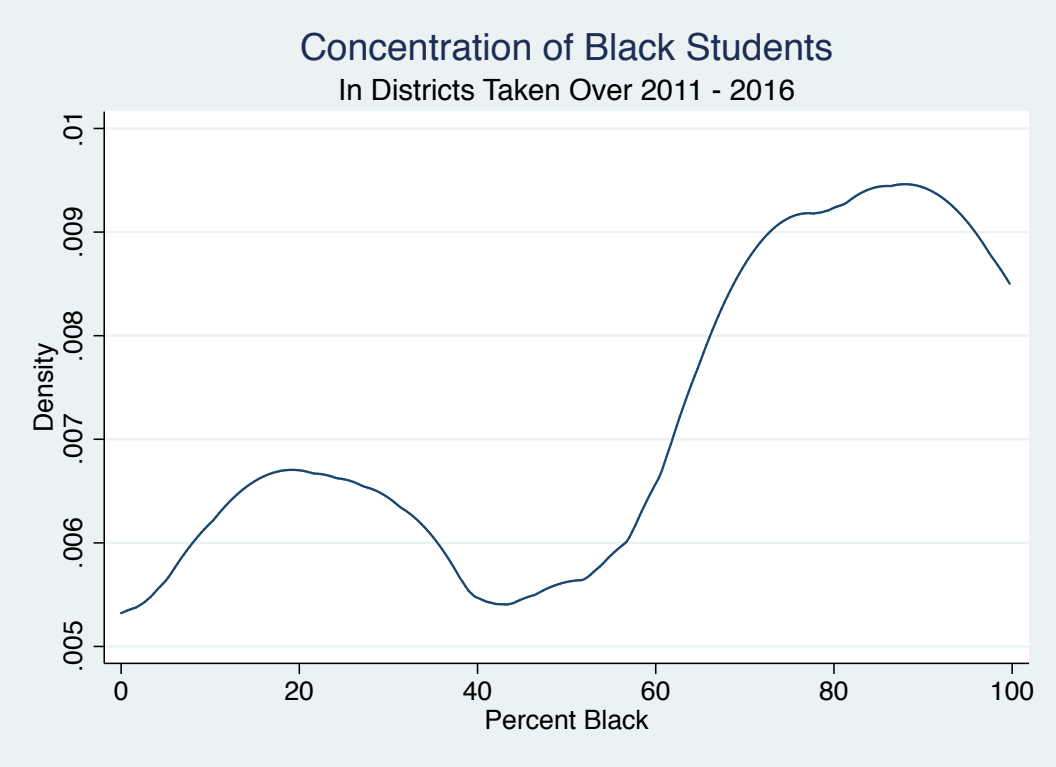


Figure 3. Density of Percent Black Students in 2011 – 2016 Takeover Districts.

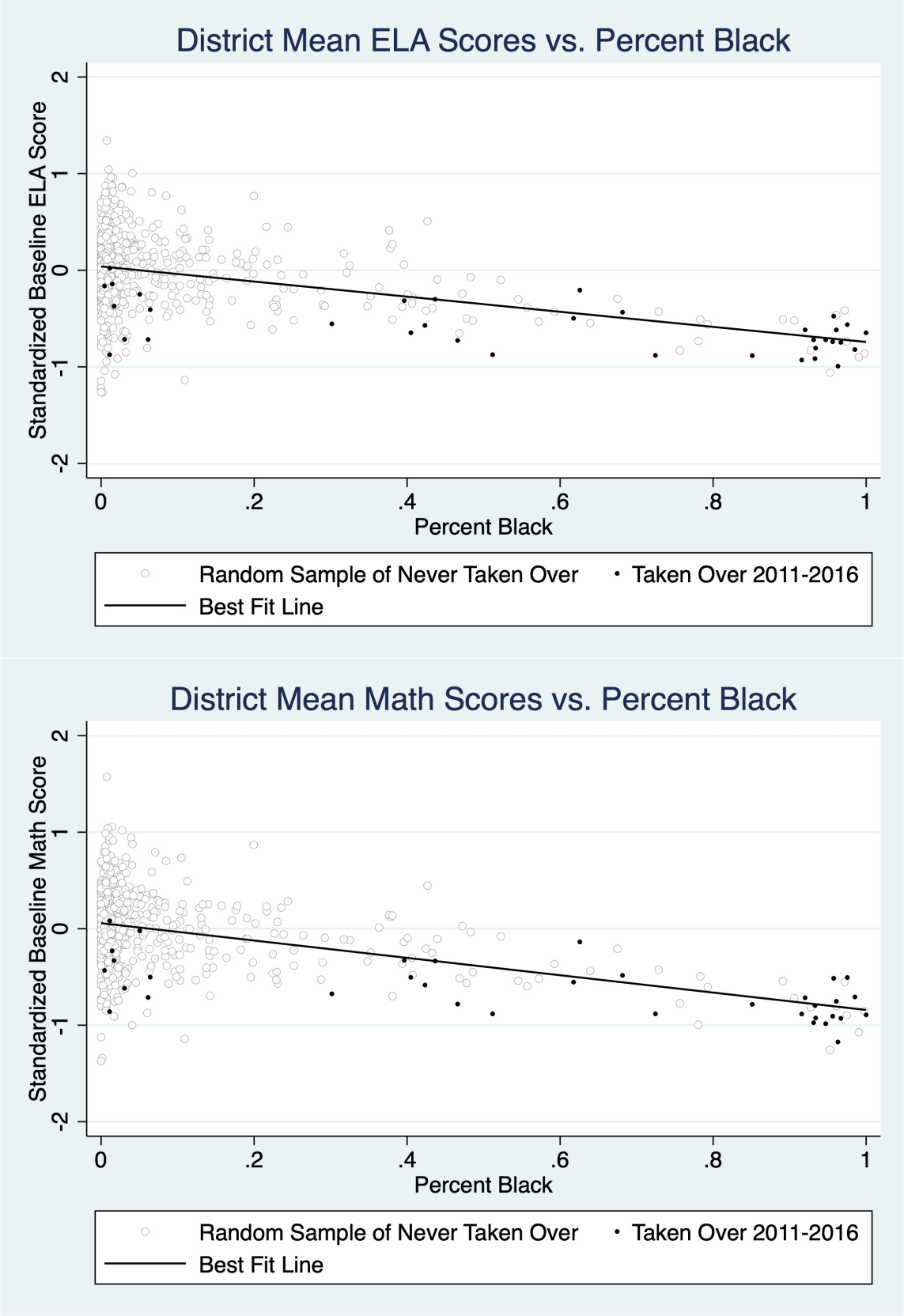


Figure 4. Performance Versus Percent Black in Takeover and Never Taken Over Districts.

Table 2. The Effect of State Takeover, Difference-in-Difference Estimates

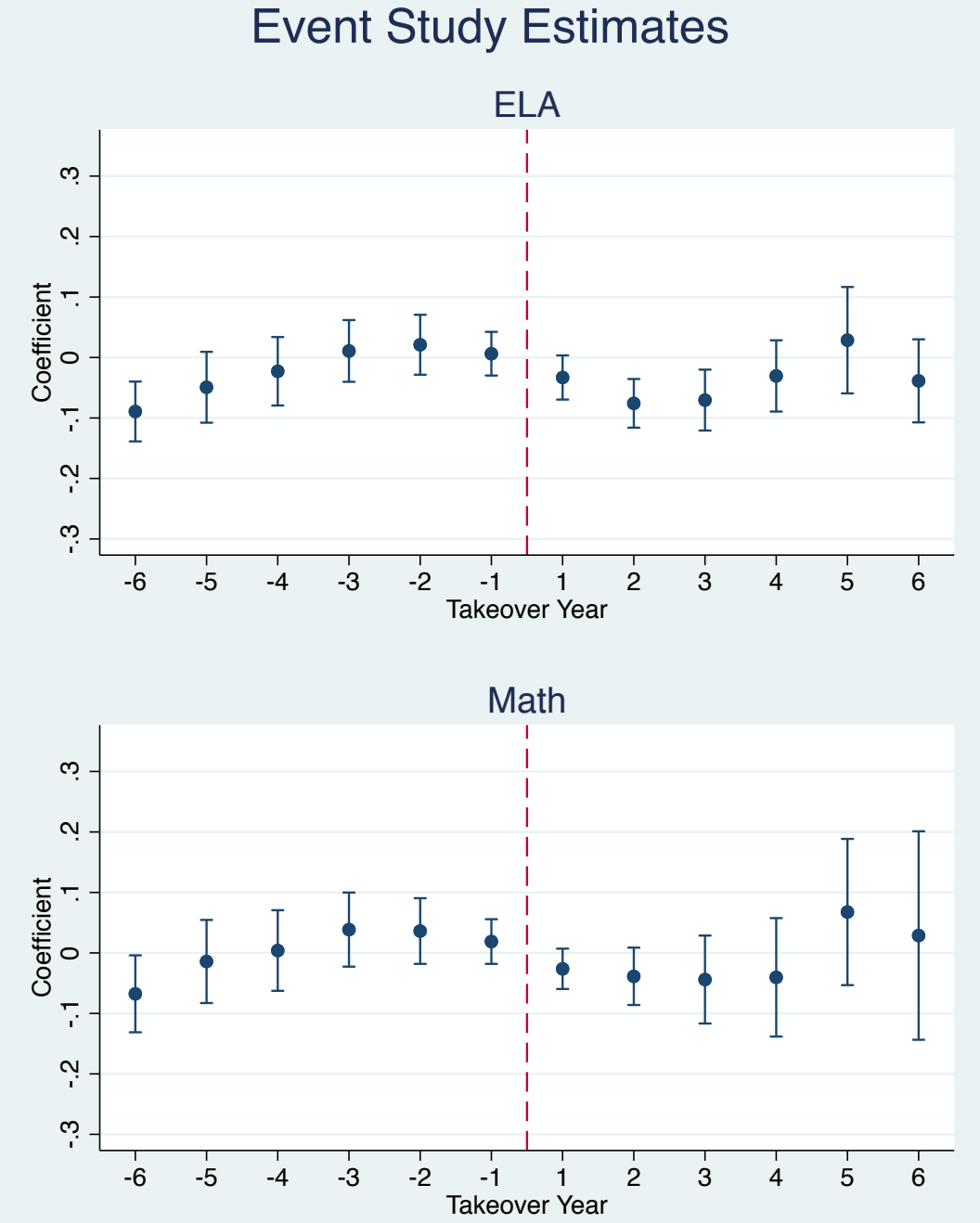
	ELA	Math	Enrollment (N of Students)	Percent Black	Percent Hispanic	Percent Subsidized Lunch	Percent English Language Learners	Percent Special Education	Class Size (N of Students)	Percent in Charter Schools	Per Pupil Spending
Takeover	-0.05	-0.04	-192.60*	-0.50*	0.12	-1.75***	0.50**	-0.01	0.37	2.83***	658.90
	(0.03)	(0.04)	(108)	(0.30)	(0.33)	(0.56)	-0.23	(0.25)	(0.24)	(1.04)	(685.70)
N of Observations	337,386	326,064	338,540	338,540	338,540	338,540	321,392	333,687	327,995	298,301	335,589
Comparison Mean	-0.01	-0.01	1,772.27	8.89	13.17	43.69	5.03	14.00	14.60	1.03	13,056.91

Note: Estimates come from a regression of the listed outcome on an indicator for being in a district that was taken over in a post-takeover year. Here, the ending year is the last post-takeover year that we observe. These estimates pool effects from all post-takeover years and from all 35 takeovers in our analytic window. Models include baseline demographic and achievement controls, district fixed effects, grade fixed effects and state-year fixed effects. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. The Effect of State Takeover on Test Scores, Event Study Estimates

	ELA	Math	N of Takeover Districts
Pre-Takeover Year -6	-0.09***	-0.07*	6
	(0.03)	(0.04)	
Pre-Takeover Year -5	-0.05	-0.01	10
	(0.04)	(0.04)	
Pre-Takeover Year -4	-0.02	0.00	15
	(0.03)	(0.04)	
Pre-Takeover Year -3	0.01	0.04	24
	(0.03)	(0.04)	
Pre-Takeover Year -2	0.02	0.04	31
	(0.03)	(0.03)	
Pre-Takeover Year -1	0.01	0.02	35
	(0.02)	(0.02)	
Takeover Year 1	-0.03	-0.03	35
	(0.02)	(0.02)	
Takeover Year 2	-0.08***	-0.04	28
	(0.02)	(0.03)	
Takeover Year 3	-0.07**	-0.04	24
	(0.03)	(0.04)	
Takeover Year 4	-0.03	-0.04	18
	(0.04)	(0.06)	
Takeover Year 5	0.03	0.07	10
	(0.05)	(0.07)	
Takeover Year 6	-0.04	0.03	4
	(0.04)	(0.11)	
N of Observations	337,386	326,064	

Note: Estimates come from a regression of the listed outcome on indicators for being in a district that was taken over and interactions between that indicator and indicators for each pre- and post-takeover year (with Year 0 as the omitted year). Here, the ending year is the last post-takeover year that we observe. Models include baseline demographic and achievement controls, district fixed effects, grade fixed effects and state-year fixed effects. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. The final column represents the number of treated districts that contribute to each estimate. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.



Note: Estimates come from a regression of the listed outcome on indicators for being in a district that was taken over in a given pre- or post-takeover year, using year zero as the omitted year. All models include district fixed effects, grade fixed effects, state-year fixed effects, and baseline achievement controls. We exclude districts taken over before 2011 and states without takeovers. Standard errors clustered at the district level.

Figure 5. Pre-Takeover Effects on Test Scores.

Table 4. The Effect of State Takeover on Test Scores, Event Study Estimates Separately by Cohort

	ELA						Math					
Cohort	2011	2012	2013	2014	2015	2016	2011	2012	2013	2014	2015	2016
Pre-Takeover Year -6						-0.19***						-0.10*
						(0.05)						(0.06)
Pre-Takeover Year -5					-0.00	-0.16***					0.00	-0.07
					(0.07)	(0.06)					(0.08)	(0.07)
Pre-Takeover Year -4				-0.02	0.04	-0.12				-0.04	0.05	-0.02
				(0.04)	(0.07)	(0.08)				(0.05)	(0.08)	(0.09)
Pre-Takeover Year -3			0.02	0.08*	0.08	-0.12			0.05	0.07	0.06	-0.05
			(0.05)	(0.04)	(0.06)	(0.08)			(0.06)	(0.07)	(0.10)	(0.07)
Pre-Takeover Year -2		0.00	0.08	0.15*	0.03	-0.15***		-0.01	0.08	0.11	0.01	-0.03
		(0.02)	(0.05)	(0.08)	(0.08)	(0.05)		(0.04)	(0.07)	(0.08)	(0.10)	(0.05)
Pre-Takeover Year -1	-0.00	-0.02	0.08	0.02	0.03	-0.10*	-0.07***	-0.02	0.11*	-0.01	0.01	0.00
	(0.03)	(0.01)	(0.054)	(0.02)	(0.04)	(0.06)	(0.02)	(0.03)	(0.06)	(0.02)	(0.06)	(0.03)
Takeover Year 1	-0.08***	-0.02	-0.01	0.02	-0.06	-0.08	-0.04	0.00	-0.06	0.04	-0.07	-0.02
	(0.02)	(0.04)	(0.04)	(0.06)	(0.08)	(0.06)	(0.02)	(0.02)	(0.05)	(0.04)	(0.07)	(0.06)
Takeover Year 2	-0.10***	-0.01	-0.03	-0.10*	-0.10*		-0.01	0.03	-0.05	-0.03	-0.11***	
	(0.02)	(0.03)	(0.05)	(0.05)	(0.05)		(0.03)	(0.03)	(0.07)	(0.06)	(0.04)	
Takeover Year 3	-0.07*	0.05*	-0.09**	-0.02			0.04	0.08	-0.11	0.00		
	(0.04)	(0.03)	(0.04)	(0.08)			(0.05)	(0.07)	(0.08)	(0.07)		
Takeover Year 4	0.04	0.07	-0.09**				0.14***	0.08	-0.16**			
	(0.04)	(0.04)	(0.04)				(0.05)	(0.07)	(0.08)			
Takeover Year 5	-0.01	0.08					0.09	0.09				
	(0.09)	(0.06)					(0.12)	(0.09)				
Takeover Year 6	-0.05						0.05					
	(0.05)						(0.12)					
N of Observations	336,071	336,158	336,290	336,122	336,087	336,183	324,775	324,864	325,000	324,823	324,786	324,891

Note: Reference category is year 0 (the last pre-treatment year). Each column describe a different regression where the sample is restricted to include a specified cohort and all comparison districts. Models include baseline demographic and achievement controls, district fixed effects, grade fixed effects and state-year fixed effects. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Variation in Effect of State Takeover by Baseline District Characteristics

	ELA						
	Percent Black	Percent Hispanic	Percent FRPL	Percent ELL	Baseline Test Scores	Student Enrollment	Non-Academic Takeover
Takeover X Characteristic	-0.07	0.18***	-0.10	0.34**	-0.30***	0.03	-0.04
	(0.07)	(0.06)	(0.21)	(0.17)	(0.08)	(0.02)	(0.05)
Takeover	-0.00	-0.08**	0.04	-0.07**	-0.20***	-0.06*	-0.04
	(0.05)	(0.04)	(0.17)	(0.03)	(0.05)	(0.03)	(0.04)
N of Observations	337,358	337,358	337,358	336,435	337,386	337,358	337,386
	Math						
	Percent Black	Percent Hispanic	Percent FRPL	Percent ELL	Baseline Test Scores	Student Enrollment	Non-Academic Takeover
Takeover X Characteristic	-0.01	0.17*	0.04	0.32	-0.35***	-0.03	0.03
	(0.12)	(0.09)	(0.28)	(0.21)	(0.11)	(0.05)	(0.07)
Takeover	-0.04	-0.08	-0.08	-0.07	-0.24***	-0.03	-0.05
	(0.08)	(0.05)	(0.24)	(0.04)	(0.08)	(0.05)	(0.06)
N of Observations	326,036	326,036	326,036	325,120	326,064	326,036	326,064

Note: The student enrollment variable has been rescaled to represent increments of 10,000 students. Here, the ending year is the last post-takeover year that we observe. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. The Effect of State Takeover on Enrollment and Demographics, Event Study Estimates

	Enrollment (N of Students)	Percent Black	Percent Hispanic	Percent Subsidized Lunch	Percent English Language Learners	Percent Special Education
Pre-Takeover Year -6	54.46***	0.49	-1.27	3.88**	-1.02*	-0.74
	(18.20)	(0.93)	(0.94)	(1.52)	(0.57)	(1.72)
Pre-Takeover Year -5	48.42***	1.46***	-1.10	3.27**	-0.89**	-0.52
	(14.24)	(0.48)	(0.74)	(1.28)	(0.41)	(0.72)
Pre-Takeover Year -4	37.90***	1.32***	-0.89*	-1.38	-0.22	-0.67
	(9.34)	(0.40)	(0.48)	(3.32)	(0.35)	(0.52)
Pre-Takeover Year -3	43.57***	1.18***	-0.18	1.13	-0.29	-0.06
	(15.82)	(0.38)	(0.45)	(0.87)	(0.25)	(0.36)
Pre-Takeover Year -2	21.69*	0.04	-0.31	1.27*	-0.54**	-0.06
	(11.99)	(0.63)	(0.24)	(0.77)	(0.24)	(0.22)
Pre-Takeover Year -1	14.64*	0.27	-0.16	-0.15	-0.21	0.08
	(8.54)	(0.23)	(0.15)	(0.92)	(0.18)	(0.17)
Takeover Year 1	-6.68	0.19	-0.08	0.34	0.15	0.09
	(4.70)	(0.23)	(0.11)	(0.54)	(0.21)	(0.18)
Takeover Year 2	-12.36	0.06	-0.11	-2.27***	0.06	0.10
	(11.20)	(0.24)	(0.21)	(0.81)	(0.28)	(0.29)
Takeover Year 3	-25.35	-0.17	-0.34	-1.85	0.13	-0.52
	(17.37)	(0.35)	(0.29)	(1.47)	(0.39)	(0.34)
Takeover Year 4	-10.04	-0.57	-0.21	-2.11	0.42	-0.46
	(21.95)	(0.45)	(0.49)	(1.39)	(0.36)	(0.40)
Takeover Year 5	54.82**	-0.55	-0.53*	-3.97**	1.17	-0.19
	(24.32)	(0.41)	(0.30)	(1.87)	(0.81)	(0.45)
Takeover Year 6	65.02	0.08	-1.07***	-2.61*	1.32	-0.07
	(43.35)	(0.37)	(0.33)	(1.40)	(1.09)	(0.56)
N of Observations	338,503	338,503	338,503	338,540	321,392	333,687

Note: Estimates come from regressions with indicators for being in a district that was taken over in a given post-takeover year. All models include district fixed effects, grade fixed effects, state-year fixed effects, and baseline achievement controls. All samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. The Effect of State Takeover on Educational Inputs

	Class Size (N of Students)	Percent in Charter Schools	Per Pupil Expenditures
Pre-Takeover Year -6	0.21	-2.37	-1,190.00
	-0.33	(1.61)	(1,031.00)
Pre-Takeover Year -5	-0.16	-1.91**	-121.90
	-0.36	(0.86)	(595.60)
Pre-Takeover Year -4	-0.25	-0.96**	319.40
	-0.3	(0.49)	(670.20)
Pre-Takeover Year -3	-0.41*	-1.53**	-171.90
	(0.24)	(0.70)	(643.00)
Pre-Takeover Year -2	-0.22	-1.11***	-334.40
	(0.30)	(0.41)	(531.70)
Pre-Takeover Year -1	-0.30	-0.19	174.50
	(0.31)	(0.16)	(369.20)
Takeover Year 1	0.047	1.08**	859.20
	(0.20)	(0.54)	(811.70)
Takeover Year 2	0.51*	2.57**	496.60
	(0.28)	(1.16)	(815.30)
Takeover Year 3	0.25	4.01**	596.20
	(0.30)	(1.59)	(626.80)
Takeover Year 4	0.02	3.66**	-65.80
	(0.45)	(1.67)	(780.50)
Takeover Year 5	0.30	1.19	764.80
	(0.35)	(1.02)	(1,417.00)
Takeover Year 6	0.76		237.40
	(0.56)		(633.30)
N of Observations	327,995	298,301	335,589

Note: Estimates come from regressions with indicators for being in a district that was taken over in a given post-takeover year. Models include district fixed effects, grade fixed effects, state-year fixed effects, and baseline achievement controls. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. We do not estimate year six effects on charter share because we do not have charter share data for 2016 (all year six estimates are based on 2016 outcomes). Expenditure variables are in dollar units. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

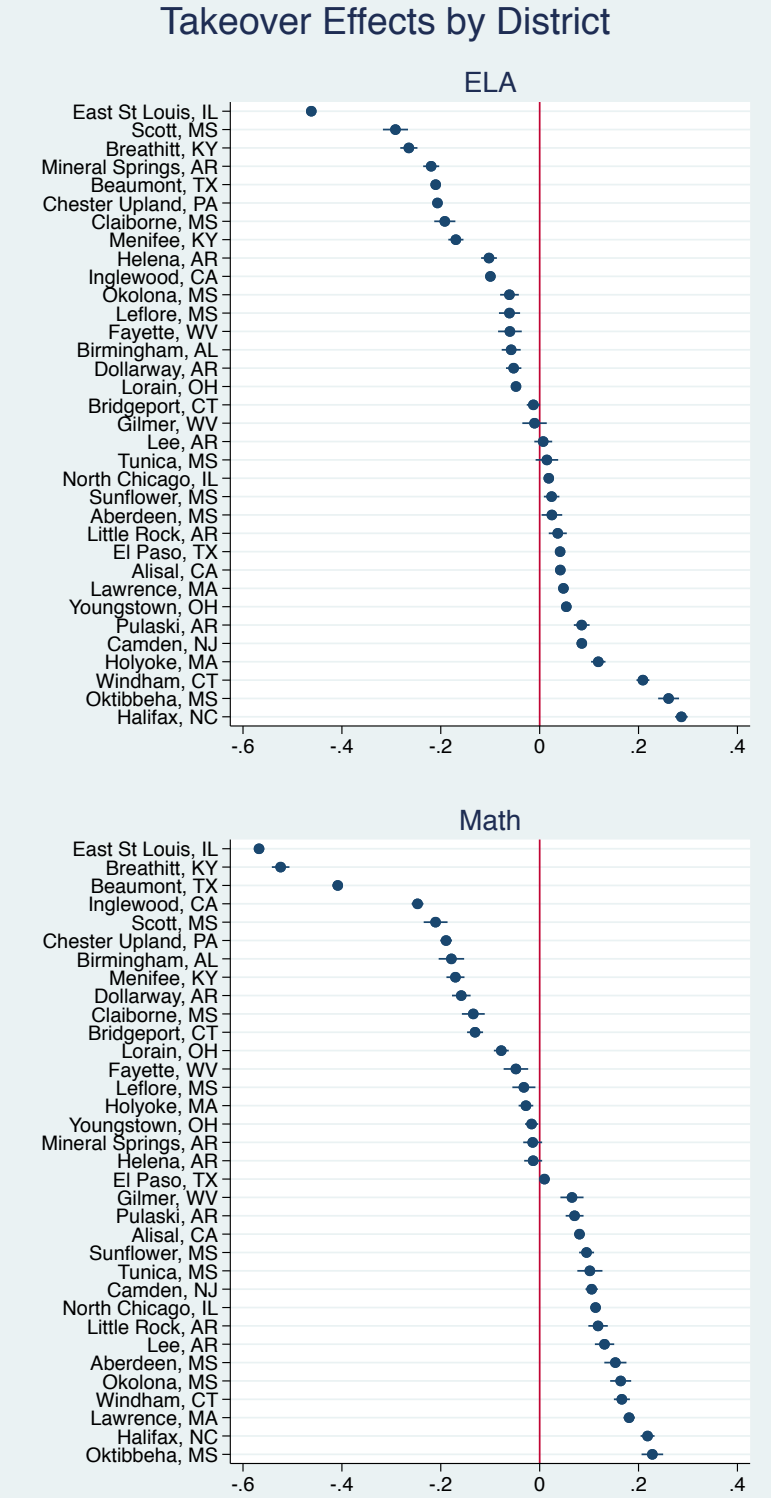


Figure 6. Takeover Effects by School District.

Appendix Table A1. Districts Taken Over by Year, 2011 to 2016

2010-11	Alisal Union	CA
	Fayette County	WV
	Okolona Separate	MS
	Sunflower County	MS
2011-12	Bridgeport	CT
	Drew	MS
	Gilmer County	WV
	Helena/West Helena	AR
	Lawrence	MA
	Pulaski County Spec.	AR
	Windham	CT
2012-13	Aberdeen	MS
	Birmingham City	AL
	Breathitt County	KY
	Chester-Upland	PA
	Dollarway	AR
	East St. Louis	IL
	Inglewood Unified	CA
	North Chicago	IL
	Oktibbeha County	MS
2013-14	Camden City	NJ
	Claiborne County	MS
	El Paso	TX
	Leflore County	MS
	Mineral Springs	AR
2014-15	Beaumont	TX
	Lee County	AR
	Scott County	MS
	Youngstown City	OH
2015-16	Halifax County	NC
	Holyoke	MA
	Little Rock School District	AR
	Lorain City	OH
	Menifee County	KY
	Tunica County School District	MS

Appendix Table A2. Rationale for Takeover by State, Among States with Laws Authorizing District Takeover

State	Year Law Passed	Academic	Financial
Alabama	1995	X	
Arizona	2008	X	
Arkansas	1983	X	X
California	1999	X	X
Colorado	2000		X
Connecticut	1999	X	X
Idaho	2006	X	
Illinois	1980	X	X
Iowa	1990	X	
Kentucky	1984	X	X
Maryland	1999	X	X
Massachusetts	1989	X	
Michigan	1990	X	
Mississippi	2000	X	
Missouri	1999	X	X
New Jersey	1988	X	
New Mexico	2002	X	X
New York	2002	X	X
North Carolina	1998	X	X
Ohio	1998	X	
Oklahoma	1999	X	X
Pennsylvania	1998	X	
Rhode Island	1997	X	X
South Carolina	1998	X	X
Tennessee	2002	X	
Texas	2000	X	X
Vermont	2001		X
West Virginia	1998	X	

Note: Table includes all states that have a law or regulation that authorize takeover of a district. Law describes the first year a law authorizing takeovers was passed. Some states that authorize district takeover have never used their law. Achievement indicates a state may take over a district for weak academic outcomes and financial indicates a state may take over a district for fiscal mismanagement. Source: Jochim, 2016; Morel, 2018; Rutgers, 2006; Ziebarth, 2002.

Appendix Table A3. Effects on Test Scores Using a Comparative Interrupted Time Series Model

	ELA	Math
Takeover	-0.07***	-0.07**
	(0.03)	(0.03)
Year	0.01***	0.00***
	(0.00)	(0.00)
Ever Takeover X Year	0.01	0.01
	(0.01)	(0.01)
Observations	337,386	326,064

Note: Estimates come from a regression of the listed outcome on indicators for being in a district that was taken over in a post-takeover year, a linear time trend, and an interaction between the indicator for being in a takeover district and the linear time trend. Here, the ending year is the last post-takeover year that we observe. All models include district fixed effects, grade fixed effects, state fixed effects, and baseline achievement controls. We exclude districts taken over before 2011 and states without takeovers. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A4. Takeover Effect on Test Score Missingness

	Missing ELA	Missing Math
Takeover	0.01	0.00
	(0.01)	(0.00)
N of Observations	338,624	338,624
	Missing ELA	Missing Math
Pre-Takeover Year -6	0.00**	-0.00
	(0.00)	(0.00)
Pre-Takeover Year -5	0.01***	0.00
	(0.00)	(0.00)
Pre-Takeover Year -4	0.00	0.00
	(0.00)	(0.00)
Pre-Takeover Year -3	0.00	0.00
	(0.00)	(0.00)
Pre-Takeover Year -2	0.00	0.00
	(0.00)	(0.00)
Pre-Takeover Year -1	0.01	0.00
	(0.01)	(0.00)
Takeover Year 1	-0.00*	0.00
	(0.00)	(0.00)
Takeover Year 2	0.01	-0.00*
	(0.01)	(0.00)
Takeover Year 3	0.00	0.02*
	(0.01)	(0.01)
Takeover Year 4	0.01	-0.01**
	(0.01)	(0.00)
Takeover Year 5	0.09*	0.01
	(0.05)	(0.02)
Takeover Year 6	-0.01	-0.01
	(0.01)	(0.01)
N of Observations	338,624	338,624

Note: Estimates in the top row come from a regression of the listed outcome on an indicator for being in a district that was taken over in a post-takeover year and an interaction between that indicator and an indicator for whether it was a post-takeover year. Here, the ending year is the last post-takeover year that we observe. Estimates in the rest of the rows come from a separate regression with indicators for being in a district that was taken over and interactions between that indicator and indicators for each year (with Year 0 as the omitted year). In other words, the top row pools all post-takeover years while the rest separate out each post-takeover year. Models include district, grade, and state-year fixed effects, and baseline achievement controls. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. The final column represents the number of treated districts that contribute to each estimate. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A5. Examining Robustness to Controlling for District Policy Changes

	ELA	Math	N of Takeover Districts
Takeover	-0.05	-0.04	35
	(0.03)	(0.04)	
Teacher evaluation control	x	x	
N of Observations	337,386	326,064	

	ELA	Math	N of Takeover Districts
Pre-Takeover Year -6	-0.09***	-0.07*	6
	(0.03)	(0.04)	
Pre-Takeover Year -5	-0.05	-0.02	10
	(0.04)	(0.04)	
Pre-Takeover Year -4	-0.02	0.00	15
	(0.03)	(0.04)	
Pre-Takeover Year -3	0.01	0.04	24
	(0.03)	(0.04)	
Pre-Takeover Year -2	0.02	0.04	31
	(0.03)	(0.03)	
Pre-Takeover Year -1	0.01	0.02	35
	(0.02)	(0.02)	
Takeover Year 1	-0.03	-0.03	35
	(0.02)	(0.02)	
Takeover Year 2	-0.08***	-0.04	28
	(0.02)	(0.03)	
Takeover Year 3	-0.07**	-0.04	24
	(0.03)	(0.04)	
Takeover Year 4	-0.03	-0.04	18
	(0.04)	(0.06)	
Takeover Year 5	0.03	0.07	10
	(0.05)	(0.07)	
Takeover Year 6	-0.04	0.03	4
	(0.04)	(0.11)	
Teacher evaluation control	x	x	
N of Observations	337,386	326,064	

Note: Estimates in the top row come from a regression of the listed outcome on an indicator for being in a district that was taken over in a post-takeover year and an interaction between that indicator and an indicator for whether it was a post-takeover year. Here, the ending year is the last post-takeover year that we observe. Estimates in the rest of the rows come from a separate regression with indicators for being in a district that was taken over and interactions between that indicator and indicators for each year (with Year 0 as the omitted year). In other words, the top row pools all post-takeover years while the rest separate out each post-takeover year. Models include district, grade, and state-year fixed effects, baseline achievement controls, and controls for time varying policy changes at the district level. Samples exclude districts taken over before 2011 and in states without takeovers. Standard errors clustered at the district level. The final column represents the number of treated districts that contribute to each estimate. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.